with the view currently being expressed by the Overseas Development Council (*3*) that there has been a fundamental turn in the long-term food situation.

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Advantages of Surface Mining

Having spent the last 23 years working in and around both surface and underground mines, I have a different view of strip mines than is expressed in the article by Robert Gillette (News and Comment, 2 Nov., p. 456). No mention is made of the effect on the miners of working underground as opposed to that of working on the surface.

Black lung and silicosis are rare diseases among strip miners. Very few men are injured by rockfalls in surface mines. In an underground mine, if something goes wrong, there is nowhere to go; the miner is surrounded by rock. Explosions of pockets of methane can kill or injure miners in an underground mine. In a surface mine the methane has a better chance to leak off or blow harmlessly into the air. Rock bursts, coal dust explosions, and fire are deadly hazards underground, and the bodies of miners are often never recovered when the ocean, a lake, or a river breaks into an underground mine.

Strip mines can and should be reclaimed. That is not to say that the terrain should be put back as it was. The character of the rock is changed by mining, and a different drainage pattern might be more desirable.

Coal seams sometimes serve as aquifers, but their permeability does not approach that of the spoil pile which is left by strip mining. The large rocks roll to the bottom of the pile, and the fine material stays at the top. Thus the spoil is segregated according to size and forms an excellent aquifer near the bottom.

Why not transport the coal by slurry pipeline rather than suffer the high electrical transmission losses of long-distance power lines? Slurry transport would require much less water than electric power generation at the mine site.

Underground mining can recover only 60 percent of the mineral that can be recovered by strip mining the same deposit. Which resources are we most concerned about conserving—human lives, the terrain, the vegetation, or the mineral?

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Citation Analysis

The *Science Citation Index* is a valuable and powerful tool when used for the purpose for which it was intended, as an aid in literature search. It also invites a variety of statistical investigations, which must, however, be considered with prudence, since they may lead to misleading results. No matter how cautiously the authors express themselves, the casual readers, that is the majority, will treat the results as established facts and forget about the assumptions underlying them. This is also happening with the computer output for economic models, which is accepted as if it were experimental observation.

An example is the article by Jonathan R. Cole and Stephen Cole (27 Oct. 1972, p. 368) in which the authors conclude that only a few elite scientists contribute to scientific progress, contrary to the generally accepted "Ortega hypothesis" that the majority of active scientists contribute to the advance. Although the authors carefully consider possible weaknesses in their argument, their article proves merely that citation statistics give a distorted picture of the way in which physics advances. Every physicist knows that in his research he uses a multitude of contributions made by others, some important, many minor but nevertheless essential. Only a few of those are cited; others are taken for granted. A striking example is the article by Edwin D. Becker and T. C. Farrar (27 Oct. 1972, p. 361) just preceding the article by Cole and Cole. It describes the basic features of Fourier transform spectroscopy. One gathers that its authors consider "Fellgett's advantage" and the "Jacquinot advantage" to be significant factors in this research technique, but the article carries no footnotes referring to Fellgett and Jacquinot. In fact, all experimental papers mention techniques without a reference to their origin. Scintillation counters and photomultipliers are generally used in experiments in nuclear and particle physics, but their inventors and the dozens of researchers who have improved these essential tools to their present perfection are rarely cited. Many other examples of this kind can be found both in experimental and theoretical physics. The reason for citing a paper is primarily for possible support of the author's contentions and only secondarily in recognition of previous work. A closer study of the referencing habits of physicists is needed before one can draw reliable conclusions from counting footnotes. It is certainly unwarranted to accept Cole and Cole's recommendation for a reduction in the size of science on that basis.

Cole and Cole refer to one of the early citation studies of M. M. Kessler (*1*). However, they fail to cite an important warning in another report by Kessler and F. E. Heart (*2*). The warning reads: "CAUTION! Any attempt to equate high frequency of citation with worth or excellence will end in disaster; nor can we say that low frequency of citation indicates lack of worth." This conclusion was drawn from a citation analysis of 36 volumes of *Physical Review* covering 9 years, 1950 through 1958, containing 8,521 articles with 137,108 references.

There is a rumor afoot that the promotion of some faculty members is now based on the frequency with which their work appears in the *Science Citation Index*. I hope that this is just a rumor. One way to get cited more often than average is to publish an apparently important paper that is demonstrably wrong.

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Cole and Cole clearly show that the physics papers receiving the most citations are the ones that receive the most citations. Their other conclusions are less convincing and appear to be based on a mixture of questionable assumptions and non sequiturs. For example,

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after a lengthy discussion of the fact that a garbage collector can be replaced more easily than, say, a brain surgeon, they say, "Within science some men are more easily replaced than others. . . . It may not be necessary to have 80 percent of the scientific community occupied in producing 15 or 20 percent of the work that is used in significant scientific discoveries, if perhaps only half their number could produce the same work." Thus the fact that some men could be replaced becomes an indication that the work could be done even if these men were removed from the work force *without being replaced.*

Faulty logic is apparent in the introduction of evidence that a great deal of work in physics is not cited at all. Even if "cited" is equivalent to "used," this evidence is irrelevant to the main thesis. To say that some obscure physicists do not contribute is not to deny that many others do contribute. Obviously, in physics as in garbage collecting or sociology, some workers are unproductive, and that was true even when we had one-tenth as many physicists as we have today. It might be more relevant to see if the *percentage* of uncited work has increased as the number of physicists has grown. In any event, because of the way the grant system works, it is unlikely that unproductive physicists absorb much of our research budget.

Many factors that could have a bearing on Cole and Cole's interpretation of their data are either ignored completely or else dismissed by the introduction of some questionable hypothesis.

1) They do not mention that some fields of research are more popular than others. A person working in acoustics will receive fewer citations than a worker in high energy physics.

2) The possibility of more than one "generation of influence" on a paper is dismissed with a hypothesis that a search of further generations would not add many names that "appear more than once." But these names are the very names that the Ortega hypothesis is all about. It seems odd to set out to test a hypothesis concerning the effects of obscure researchers, then to say we do not have to look very far for these effects because these men can be replaced, and finally to say that we could therefore have progress without them or without any replacements for them.

3) Although Ortega specifically re-

ferred to "experimental science," Cole and Cole make no distinction between experimental and theoretical work, and they use the words "work" and "ideas" interchangeably. Consider a 1968 paper by Gell-Mann, Oakes, and Renner (*1*) in the 1971 *Science Citation Index*. It cited 26 papers; *all* were by theorists. A check of the cited papers shows that almost all of the papers cited in them were also by theorists. The only references to experimental work were "second-generation" citations of books or review articles. But review articles were excluded from Cole and Cole's study because they would "distort . . . [their] results." Thus their methods must lead to the conclusion that Gell-Mann *et al.* are not influenced by experimental results!

Recognition of the difference between theory and experiment makes the "Pointilliste" analogy more understandable. We may say that experimentalists fill in points on the canvas, while theorists try to recognize the picture that emerges. Eventually a theorist will say, "Aha, it's a giraffe (or an octet)." Maybe the theorist does not need every point in order to recognize the giraffe; maybe some experimenters fill in more points than others; maybe some workers are filling in some obscure cloud in the background instead of parts of the giraffe. But if too many points are missing, the picture is unrecognizable; theorists are already asking for points that are not being filled in because of budget limitations. It is difficult to decide, before the picture is recognized, which points will be significant, and thus where to place our dwindling resources, but it must be better to base the decision on analysis of the physics rather than on mere numerology.

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Being intrigued by the hypothesis of Cole and Cole that the "contribution to scientific progress" of an individual can be determined by "citation analysis," I decided to engage in the exercise in ego gratification that this hypothesis suggests. Looking up my own work in the 1972 *Science Citation Index*, I found, among other things, two references to a humorous letter I wrote to the editor of *Physics Today* (*1*) about what physics would have been like if the

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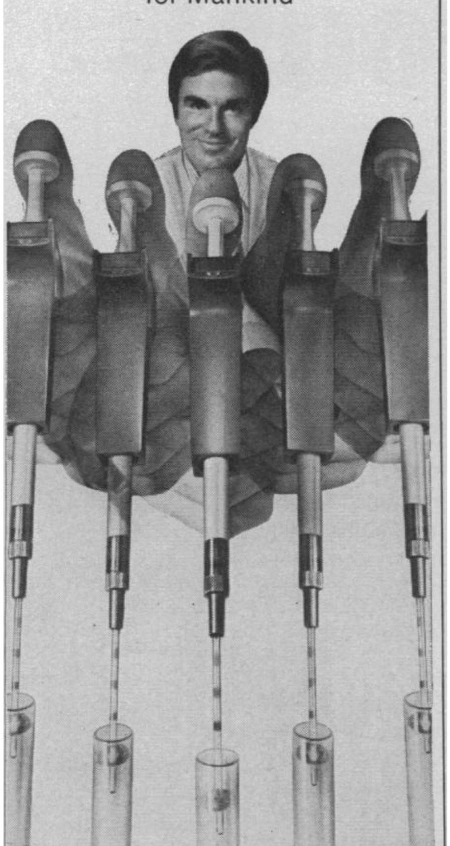

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present mechanisms for research funding and publication had existed in the time of Copernicus and Kepler, making this letter one of my "major contributions to scientific progress" for that year. These references were due to two letters to the editor that were critical of mine. Nevertheless, they provided me with two citations, just as this letter will provide a citation for Cole and Cole. Hence, if a couple of more critical letters are published on the article by Cole and Cole, their work can, by its own standards, be said to have contributed about as much to scientific progress as my joke about Copernicus.

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The criticisms of our article fall into two categories: (i) Citations are an inadequate way to measure the quality of scientific work or intellectual influences on it; and (ii) the conclusions we reach concerning the size of science are not warranted by the data.

First consider the specific criticisms that are made of the use of citations.

1) The number of citations received by an article is dependent upon the "popularity" of the specialty (McGervey). This is only partially true. The sheer size of a field is not, in fact, closely related to the number of citations papers receive. While large specialties have more participants, they also have more literature to draw upon. Papers published in the larger specialty of solid-state physics, for example, do not receive more citations than those published in the smaller specialty of high energy physics. Further, McGervey assumes that the popularity of a specialty has nothing to do with the current opinion by scientists of the relative importance of work done in that specialty. Is acoustics merely a less popular specialty than high energy physics, or does high energy physics, more than acoustics, address a set of questions which are seen by physicists as more central to the advance of physics in general?

2) Experimentalists "fill in points on the canvas, while theorists try to recognize the picture that emerges" (McGervey). Two things are implied here: that experimentalists make many minor (infrequently cited) discoveries that contribute to the syntheses of the theoreticians and that theoreticians frequently do not cite the work of experimentalists that they have used. Important experimental work and technical innovations are frequently cited. For example, the most cited scientist in the 1971 *Science Citation Index* (SCI), O. H. Lowry, is cited for the development of a technique. Also consider the paper by J. H. Christenson *et al.* (*1*) reporting the violation of CP (charge and parity) conservation. In the first 5 years after it was published, this article received a total of 369 citations. Although Christenson, a graduate student at the time, was the first author, the two senior authors, J. W. Cronin and V. L. Fitch, received a total of 261 and 160 citations, respectively, to other papers on which they were the first author. On the matter of influential work going uncited, this certainly happens in *specific* papers. What is important, however, is whether or not there is significant work which received few or no citations in the entire body of literature. One only has to consider the example used by Goudsmit himself. Although Fellgett and Jacquinot are not cited by Becker and Farrar, Fellgett received 33 citations and Jacquinot 42 (not counting self-citations) in the 1972 SCI. These totals would put them in the top 10 to 15 percent of all scientists. Although any one paper may fail to cite a paper that has been influential in its genesis, the critical point, which is missed by Goudsmit, is that the probability of an important paper going uncited in the entire body of literature is low.

3) One of the most frequent criticisms of the use of citation counts to measure the quality of work is that it is impossible to tell the difference between a "positive" and a "negative" citation. This criticism is based upon an incorrect definition of high-quality work. "Correctness" is only one of the criteria we use in evaluating scientific work. Much trivial work is "correct," and much important work turns out in historical retrospect to have been "incorrect." If we take Kuhn's (*2*) argument seriously, then the work of most of the great figures in the history of science was in a sense "incorrect." A paper which is important enough to receive a large number of critical citations is probably a significant contribution. Why would a large number of scientists waste their time pointing out a trivial error? In fact, they do not.

Papers which are trivial and receive critical citations will not accumulate large numbers of citations. Thus, Yaes' letter, which received two critical citations, is not, even by our own rough empirical measure, a significant contribution.

4) Counting citations in an inadequate way to evaluate individual scientists when tenure or other similar decisions are made. We cannot emphasize strongly enough that we totally agree with this point. Nowhere have we ever suggested that citations be used as a basis for rewards. Sociologists use citation analysis to study the *community* of scientists, not individual scientists per se. In any large aggregate of scientists there will be a relatively high correlation between the number of citations received and other methods of evaluation. There will always be individual cases, of course, where the rough statistical measure is inaccurate. Using citation counts to determine the future of a scientist's career would be committing the "fallacy of misplaced concreteness," would be reifying the statistical indicator, and would be grossly unfair to the individuals involved. Although counting citations is indeed a rough way to measure quality and influence, it has allowed us to address a whole range of substantive problems which, heretofore, were not negotiable because there was no adequate measure of research performance. Max Delbruck was well aware of the need at times for adopting less than perfect measures, as long as the scientist is aware that his measures are crude. His "principle of limited sloppiness" (*3*) does not, of course, excuse muddled thinking or poor logic. But his idea shows an acute awareness of the processes by which knowledge advances at various stages in the development of disciplines.

Finally a word about criticism by some natural scientists of work in the sociology of science. When a physicist publicly criticizes the work of another physicist, he is usually advised to be at least somewhat familiar with the literature on the subject of debate. When some natural scientists publicly criticize the work of sociologists of science, they do not appear to be familiar with literature on the topic. The criticisms about citation counts in the letters here are a case in point. There is now a substantial body of literature on the methodological problems involved in using citation counts. Virtually all criticisms raised in these letters have been analyzed in some detail in this literature. Critics of citation analysis have a responsibility to famaliarize themselves with this literature.

The second criticism of our paper suggests that even if most scientists are indeed rarely cited (accepting this indicator of influence as valid), this does not mean that scientific progress would be unaffected by a reduction in the number of scientists. Consider McGervey's implicit assumption that, since all research scientists might contribute some slight piece of relevant knowledge, they are therefore deserving of support. In a world in which resources were unlimited, we too would be in favor of the society supporting anyone who wanted to be a scientist. Science is certainly an intrinsically more interesting and worthwhile endeavor than many others. Unfortunately, we live in a world in which there is a limitation on available resources. In such a situation, rather than bemoan the sad state of science, it is the responsibility of the scientific community to consider how the limited resources we do have can be most effectively utilized.

Nowhere in our article do we suggest that there should be any cutback in the level of spending for scientific research and development. Our findings raise the issue, however, of whether limited resources might best be concentrated in support of the relatively small number of scientists who have the highest probability of making significant discoveries. We hypothesize that such a policy would not bring about a decline in the rate of scientific progress. We do not claim to have proved this conclusively. We claim to have presented enough data in support of hypothesis to merit its further consideration.

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