Cirker's letter is not wrongheaded, it is merely special pleading. He complains that "the major forces behind the bill are powerful publishing interests." He himself speaks for his individual powerful publishing interest—a special kind of publishing interest which doesn't like the idea of copyright in any form. He calls a copyright a "monopolistic grant," which is of course absurd, as the Department of Justice has discovered.

I mention one more of Cirker's special pleas: his implication that reversion of copyrighted material to the public domain makes the material available to consumers at cheaper prices. There is nothing in the history or statistics of publishing to support that statement.

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Many readers of *Science* are authors, working under the limitations of existing copyright law and at a disadvantage in protecting their investment as compared with their neighbors who have spent time developing real estate, grocery stores, or bowling alleys. They may well prefer supporting any move to extend copyright protection to joining Cirker in opposing it.

Cirker points out that books may be sold more cheaply if authors are deprived of their royalties at the earliest possible date. He is correct. What is surprising is that he fails to carry this argument to its logical conclusion by proposing that publishers, printers, bookbinders, and booksellers join the author in this act of abnegation, thereby providing the public with books at no cost at all!

Behind this thesis, and behind copyright law in general, is the interesting assumption that, after a certain time, the public has a "right" to benefit from an author's labor without paying him for it. An extension of the copyright period beyond the current 56 years is thus seen, by Cirker at least, as an instance of giving "public property to private interests." What should be made clear is that present copyright law gives away private property to the public in a manner that would provoke great uproar if it were applied in the field of real estate and other areas of the business world. To argue that the 56-year protection of the law covers the lifetime of most authors is absurd. If writing is to be treated like any other kind of labor, then its fruits should be secured to the writer and his estate in the same way that a title deed to his house is secured.

We are asked to believe, however, that the availability of books is of such vital public interest that the property rights of authors must be limited for the common good. Here again it is difficult to see why this principle is not even more applicable in other fields—to such commodities, say, as food, medical care, housing. The availability of these commodities at low cost is surely more crucial to the general welfare than the production of cheaper books. While it may be flattering to conclude that the written word takes primacy over the necessities of life, it is a little unrealistic.

But even if books *were* more important than bread, we move into an Alice-in-Wonderland kind of logic when we maintain that the more valuable something is to society, the less right there is for the man who produces it to receive his reward!

Does the royalty-rate factor seriously reduce the reading resources of the man in the street? British practice, as Cirker points out, provides much longer copyright protection for a writer—his lifetime plus 50 years. Yet, if some recent reports are to be believed, more people read more books in Britain than in the United States, and they do so within the framework of a lower standard of living.

The "censorship" issue is utterly misleading, and mention of the *Mein Kampf* affair irrelevant. How would Cirker resolve a problem of that kind? By unlimited pirating of foreign works in "the national interest," perhaps? Any author can prevent the public from knowing what he is thinking by not writing a book in the first place; or, having written it, by withholding the manuscript from publication. Curtailing copyright will not affect the tendency of governments, on both sides of the Atlantic, to keep some of their documents from the public eye.

Cirker has confused two issues, copyright and royalties. There is no reason why authors should not retain their right to royalties after copyright has expired. Copyright is concerned with the right to copy material. It is perfectly possible to construct copyright laws that provide for general permission to republish a book after a certain period of time has elapsed but that require anyone doing so to pay royalties at some statutory rate to the author or his estate. Why not? The reprinter of books in the public domain is already free of many of the risks taken by the original publisher. He is not investing in an unknown author; he can find out what the sales have been over the years and plan his printing with a smaller margin of error. It is not unreasonable to demand that he pay the writers who make his own profits possible.

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I.Q. Scores and Genetic Trends

Newcombe [*Science* **141**, 1104 (1963)] implies that a decrease in I.Q. scores in a large sample of subjects over generations would indicate a decline in intelligence. This, in turn, would support the hypothesis that the frequency of superior combinations of alleles in the collective pool of human genes is diminishing. I suggest that this line of reasoning will not stand up under closer scrutiny.

Let us, for the moment, ignore all other theoretical and methodological difficulties and focus on the problem of test instrument artifacts. What instruments could one select so as to obtain comparable data over two or more generations? If identical tests were used, any changes, up or down, would much more likely be due to cultural changes, and no test is completely culture-free. If there were a real decline in biological intelligence (whatever that may be), such a decline would be very small in any one generation. Indeed, it would be much smaller than the error of measurement for any one particular individual. It can be argued very strongly that even the most culture-free test imaginable would be subject to cultural changes over one generation at least as large as the largest changes that might be produced by hereditary factors.

Let us assume, then, what might happen if a different test or battery were used in each generation. Such a test would have to be standardized on a presumably representative sample of the generation to be tested. Now, theoretically, any factors affecting our original population sample should be affecting the entire population of which it is representative, and therefore also the new standardization sample drawn

therefrom. So if we administer the second-generation test to our experimental group, resulting scores do not, in fact, yield a comparison with the first-generation sample, but with its own generation, and thereby with itself. If sampling and standardization were adequate, the second-generation sample and its contemporary population should have identical distributions of test scores.

One might argue that one could circumvent this dilemma by choosing measures which reflect basic biological adaptation, such as physiological arousal, speed of motor responses, and so on. However, there are at least three objections to this proposal: (i) Even such basic adaptive mechanisms could be subject to cultural influences, particularly child-rearing practices. (ii) Those who believe a decline in "true" intelligence is taking place may not accept such measures as representative of the functions they have in mind. (iii) Even if such mechanisms showed changes over generations, such changes are neither necessarily maladaptive or correlated with higher mental processes.

I therefore propose that psychological and psychophysiological tests are not proper means to demonstrate changes in "true" intelligence taking place in the general population over a number of generations.

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I have no quarrel with Wolfensberger's point, except as it reflects a misunderstanding of that made in my own letter. Both of us have emphasized in our own ways that purely psychological tests will probably never be refined to the point of measuring the heritable component of intelligence, exclusive of the environmental contribution. It follows from this that the absolute values of scores from such tests, carried out over a number of generations, are not a proper means to demonstrate changes in the hereditary basis of intelligence. On this we are in complete agreement.

The point Wolfensberger has missed is that refinement in assessing a heritable component must depend largely on use of the pedigree method, in addition to any psychological test. For example, where high test scores are achieved by the parents, evidence of the extent of the hereditary component is best sought by observing the offspring to see if they show the parental trait. The evidence improves with increased knowledge of the environments in which members of the family were raised. Thus, in an extreme case where all members of the family achieved high test scores, in spite of great social adversity of a kind normally associated with low scores in the rest of the population (crowding in the home, low socio-economic status, and so on), one might reasonably infer an exceedingly strong hereditary component for high intelligence in these particular parents. If such parents were, in addition, more fertile than average, they might be regarded as contributing disproportionately to the underlying genetic basis of intelligence in the next generation.

Attempts to detect trends in the hereditary component of intelligence are presumably best based, as in the past, on studies of the correlations between fertility and intelligence. Success in such studies must necessarily depend upon use of the pedigree method to assess the hereditary component in test scores of individuals whose fertilities are being investigated.

The main point of my letter was that, although the desired high level of refinement is still a long way off, we have not yet taken some of the obvious first steps in the direction of greater refinement using genetic methods that are now open to us.

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Engineers and Their Efforts

It is difficult to disagree with most of the arguments presented in the fine essay on waste and duplication in scientific research [*Science* **142**, 625 (8 Nov. 1963]. However, the statement concerning engineers and their efforts, even though rather gentlemanly in its wording, is hard to swallow. Among engineering educators the most popular current explanation of the decreasing engineering enrollments (not shared by me) accuses the press and other news media of always talking about "scientific success" and "engineering failure" (especially in the field of space exploration). Is the editor absolutely sure that his words "but there is no reason why we should be blamed when some engineering effort goes awry. 'Scientists fail in effort to orbit space vehicle' is a typical headline" are definitely correct and fair? Besides, I always have been under the illusion that engineers are not second-rate citizens in the AAAS Kingdom, despite the fact that engineering papers or reports practically never can be found in the pages of *Science*.

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Clipping Conflict: Some Solutions

Regarding the recent suggestion by Jensen [*Science* **142**, 341 (18 Oct. 1963)] that articles in *Science* on the same subject should not be on the same sheet, in order to facilitate filing: Such a suggestion is frequently received by editors of technical journals; unfortunately, it does little to solve the real problem, which is that the typical article has several subjects; hence there is always the problem of how to file an article by subject. A makeshift solution for a file indexed by subject is to write the several topics on separate sheets—perhaps with a telegram-style abstract—and file each sheet under its subject.

A better solution is to number articles, index them by concept terms, and retrieve them by so-called "concept coordination" [see R. S. Casey *et al.*, Eds., *Punch Cards—Their Application to Science and Industry* (Reinhold, New York, 1958)]. Cards printed with document numbers in a matrix form, which can be punched manually, are available from several manufacturers. To retrieve any item one makes a little packet of pertinent subject cards and looks for coincidence of holes by holding the packet up to light.

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. . . I offer the simpler solution of printing articles and reports on odd-numbered pages and advertisements in those sections on even-numbered pages. This allows "clippers" to clip as they will and "clip-and-pasters" to do likewise.

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. . . Jensen's alternative will not be satisfactory to everyone, particularly to those with strange multidisciplinary interests. I have found photocopying of overlapping pages of two articles works satisfactorily. . . .

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