



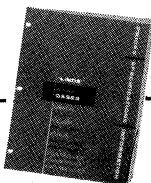
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or even erroneous "proof." It has taken centuries to provide calculus with a rigorous basis, and some say the job is not yet completely accomplished. Right now there are eminent men on both sides of the Kriebiozen contest. Only in the future can we find out whether the worthy doctors are standing against a dangerous fallacy or resisting medical progress.

In connection with the Abel incident cited in Barber's article, let it be said that the problem of the solution of the fifth-degree equation was in the air at the time. It was one of those problems that sounded easy enough to be tackled by incompetent enthusiasts. Gauss had probably received many manuscripts that were not worth the time it would have taken to decipher them. There was no way for him to recognize a genius from a cursory glance at the work of an unknown.

I wonder if investigation would not reveal that there has been more resistance to the ideas that contradicted accepted theory than to those that extended or complemented it.

RUTH M. BALLARD
*University of Chicago,
Chicago, Illinois*

The article by Barber was most interesting. However, I would like to point out that he utilized a common misconception to illustrate his thesis that substantive theories held by scientists prevent their acceptance of new ideas. The illustration used was the rejection of the heliocentric theory of Copernicus by the astronomers of his era because they firmly believed that the earth was motionless and at the center of the universe.

At the time of Copernicus the heliocentric theory could no more be justified than could the geocentric theory. Neither could be chosen as superior on the basis of observations accumulated up to that date. The rejection of the Copernican universe, therefore, did not imply a stubborn and blind clinging to outdated concepts on the part of the contemporaries of Copernicus. Indeed, it was only after the formulation of the Newtonian synthesis, which presented mechanical reasons for believing the heliocentric theory superior, that this theory began to gain wider acceptance. Both were taught at Harvard and Yale in the 18th century. The first observational evidence from which the motion of the earth could be inferred was not obtained until 1838, when several ob-

servers were able to determine annual stellar parallax.

Many new theories are rejected because there is no way in which a firm decision may be made at that particular time, due to lack of evidence in favor of either theory. Either may be satisfactory, and it is only after criticism, discussion, and further investigation have produced more definite evidence that the more adequate theory will survive.

ESTHER B. SPARBERG
*Hofstra College,
Hempstead, New York*

**Racial Differences and
Witch Hunting**

In a recent issue of *Science* (1), Santiago Genovés of the University of Mexico discourses at some length concerning a paper of mine published in the *Mankind Quarterly* last year (2). Genovés objects to my criticism of Klineberg's chapter "Race and psychology," included in the UNESCO publication *The Race Question in Modern Science* (ed. 2, 1956). He confuses the issues through bad logic and too much vehemence. What I actually did in my paper was to show, I think conclusively, that the evidence for no race differences presented by Klineberg is far too meager, too ambiguous, and too inconclusive to justify his sweeping assertion that "the scientist knows of no relation between race and psychology." My paper would have been "unscientific racism" (Genovés's term) only if its main purpose had been to support the doctrines of a "master race" or "chosen people." As its aim was simply to point out the flimsy nature of Klineberg's data, it is a legitimate enterprise, unless one considers any criticism of equalitarianism to be morally untenable.

Genovés is critical of my view that widespread Negro-white hybridization has in the past led to illiteracy, social and economic backwardness, and degeneracy. He assumes that I condemn all race mixing, which is untrue. Most racial hybrids are viable, and many are successful people, as witness the Hawaiian-Chinese and Japanese-American crosses in Hawaii. But one need go no farther afield than the West Indies, Central America, and parts of South America to be convinced of the bad effects of Negro-white crosses when these are numerous. My concern was

—and is—that American whites, under the prodding of the equalitarians and moralistic social reformers, will become convinced that it is their duty to absorb the 18 or so million Negroes and Negroids in this country. No matter how low (in a socioeconomic sense) an American white may be, his ancestors built the civilizations of Europe; and no matter how high (again in a socioeconomic sense) a Negro may be, his ancestors were (and his kinsmen still are) savages in an African jungle. Free and general race mixture of Negro-

white groups in this country would inevitably be not only dysgenic but socially disastrous.

Genovés becomes quite vehement in his denunciation of a review by A. James Gregor (3) of Juan Comas's *Racial Myths* (published by UNESCO). He calls it "a review which is full of totally unjustified personal attacks and insinuations of a political type, without basis and completely outside the framework of the problem under discussion." This statement so blatantly distorts Gregor's review as to be a com-

plete caricature of what he said. Gregor pointed out, with appropriate references and in relevant detail, that that Comas is guilty of shoddy and pretentious scholarship, is unfamiliar with the movements and people whom he criticizes, does not know his primary sources, borrows (often inaccurately) from secondary sources, and is ill informed even in those matters in which he claims special competence. Gregor's paper is a masterly piece of scientific debunking.

Genovés's letter would be pathetic were it not at once tragic and dangerous—tragic because it represents the utterly illogical attitude of many present-day social scientists toward race; dangerous because it implies that equalitarianism is somehow sacrosanct and that its denial is heresy. When racial differences are found, especially those between Negro and white groups, they must be explained away, distorted, belittled, or ignored. And scientists reporting such differences must be discredited and maligned.

HENRY E. GARRETT

University of Virginia, Charlottesville

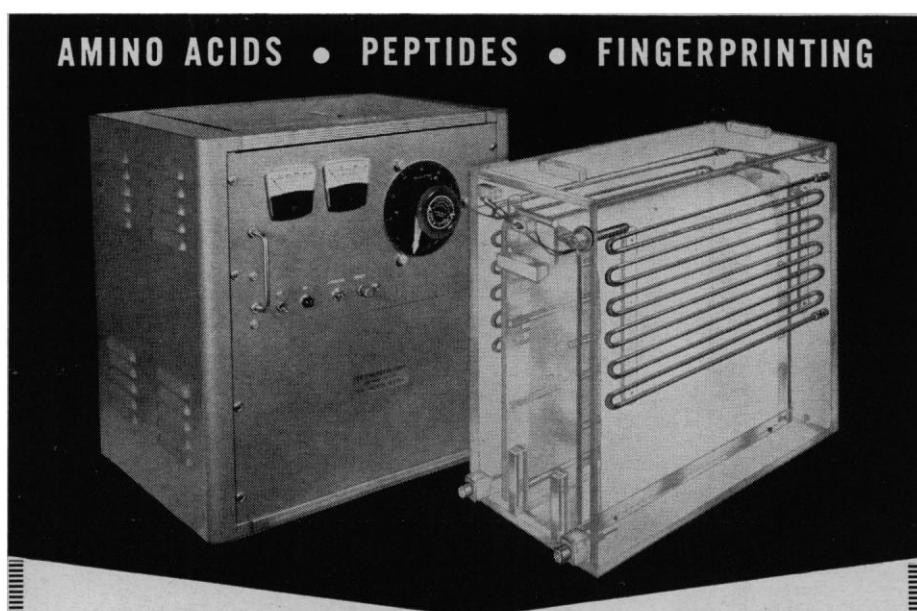
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1. S. Genovés, *Science* 134, 1928 (1961).
2. H. E. Garrett, *Mankind Quart.* 1, 15 (1960).
3. A. J. Gregor, *ibid.* 2, 30 (1961).

Santiago Genovés took the time, at the close of his long communication to *Science*, to gratuitously add his judgment concerning my review of Juan Comas's pamphlet *Racial Myths*, carried in a recent issue of the *Mankind Quarterly* (1). In his considered judgment, the review was full of (i) "totally unjustified personal attacks" and (ii) "insinuations of a political type, without basis and completely outside the framework of the problem under discussion."

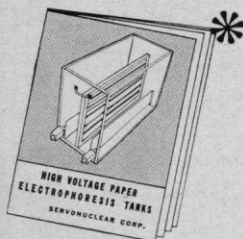
Ultimate appeal, of course, should be made to the review itself, but for the benefit of the readers of *Science*, who will not, in all probability, ever see the review, I should like to briefly consider Genovés's contentions.

If Genovés understands a "totally unjustified personal attack" to mean the exposure of gross errors in scholarship on the part of a man selected to write a brochure to be given international circulation, then my review constituted such an "attack." Comas, in the course of his brief pamphlet, misspelled the proper names of at least ten of the authors he was ostensibly "criticising." He was cavalier in the use of dates, arch in his criticism of



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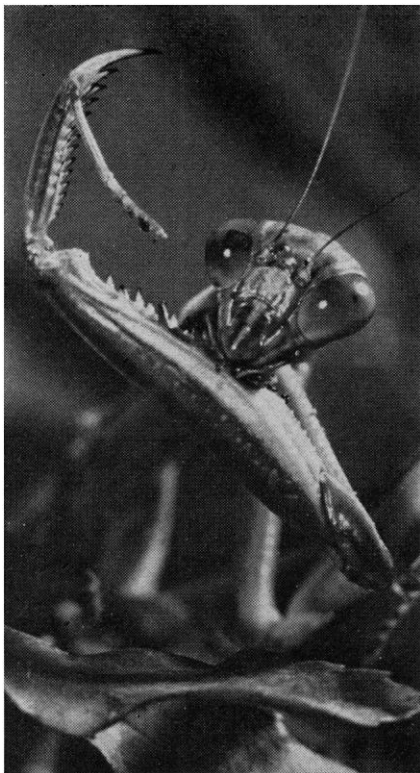
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men whose books he had obviously not read, and given to broad and unqualified pontifications about matters of fact and theory still in legitimate dispute. These are, of course, serious charges. I have documented them in my review. I was shocked that a scholar of Comas's reputation could have produced so shoddy a piece of work.

To anticipate, perhaps, I should like to say that my professional concern has been with critical analysis of theory constructions in the social sciences. My objections to Comas's work arise from this concern. Similarly, the current issue of *Phylon* will carry my critical discussion of the "racist" theories of Hans F. K. Guenther (whom Comas deals with only through secondary references). Furthermore, *Sociological Quarterly* will, in the near future, carry my review of Ludwig Winter's *Der Begabungsschwund in Europa* (Paehl, Hohe Warte, 1959), an attempted "racist" analysis of the decline of European potential. My criticisms are, in both cases, as sharp as those I directed against Comas's "antiracist" pamphlet. My concern, fundamentally, has been with respectable scholarship rather than "racism" or "antiracism" (whatever those expressions might mean).

Concerning Genovés's second point, I should like to say that there was very little in the way of insinuation in my review. I said specifically that Comas had rested a substantial part of his case on a rather trivial book written by a notorious Communist, whose work, in itself, has been characterized as "completely lacking in academic self-respect" (the expression is that of Dobzhansky).

Both Genovés and Comas know that orthodox Marxism conceives of science (both natural and social science) as "partisan." Science, for the Marxist, is an element of the "ideological superstructure" of society (2, pp. 11, 39, 54, 78) and as such serves the interests of determinate classes (3). One of the criteria for the validation of a scientific theory is, therefore, its utility in the class struggle (2, p. 87; 4). We know what effect such conjectures have had on genetics, anthropology, psychology, biology, and even nuclear physics in the Soviet Union. The influence of this kind of thinking in the social sciences is particularly deleterious (5).

While it would not be legitimate to simply dismiss Marxist works in this specific area, their materials are to be employed with more than a little circumspection. Comas, on the other hand,



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employed the analysis of an avowed and orthodox Communist (in fact, he did little more than paraphrase his conjectures) to level the most serious charges against a number of academicians (Alexis Carrel among them).

All these things, I am convinced, are quite to the point. Comas's little pamphlet was, and is, objectionable for a number of reasons: its evident lack of scholarship, its devious documentation, its theoretical poverty, and its almost abject dependence upon as questionable a source as Marcel Prenant.

A. JAMES GREGOR

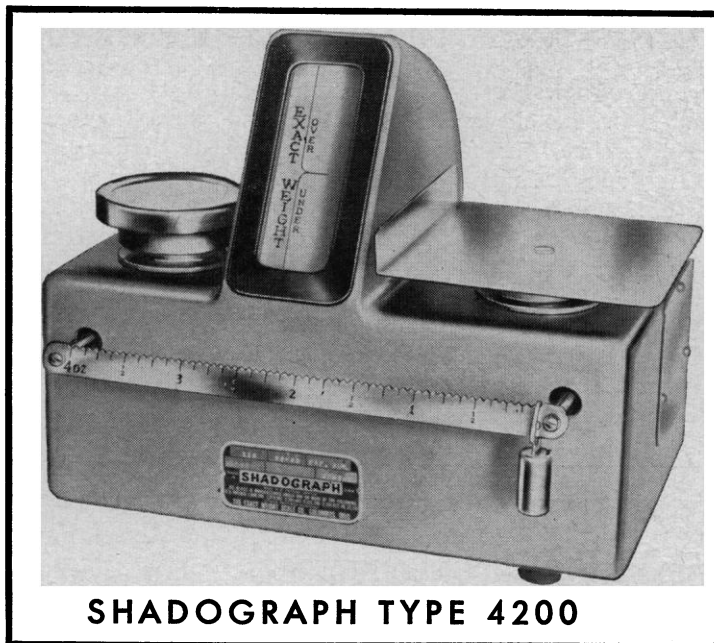
Philosophy Department,
University of Hawaii, Honolulu

References and Notes

1. A. J. Gregor, *Mankind Quart.* 2, 30 (1961).
2. See P. Judin and M. Rosental, *Diccionario de Filosofia y Sociologia Marxista* (Seneca, Buenos Aires, 1959).
3. See G. Wetter, *Dialectical Materialism* (Praeger, New York, 1958), pp. 268-74; F. V. Konstantinov, *Los Fundamentos de la Filosofia Marxista* (Grijalbo, Mexico, 1959), p. 529, note 1; I. M. Bochenski, *Die dogmatischen Grundlagen der sowjetischen Philosophie* (Reidel, Dordrecht, 1959), p. vi.
4. See N. Lenin, *On Socialist Ideology and Culture* (Foreign Languages, Moscow), p. 53.
5. See my review of I. A. Gejewski, *Die Rassenpolitik des amerikanischen Imperialismus* (Dietz, Berlin, 1956), A. I. Demidenko, *Die Rassentheorien im Dienste der amerikanischen Imperialisten* (Dietz, Berlin, 1954), and A. P. Gagarin, *Die amerikanische buergerliche Philosophie und Soziologie im Dienste des Imperialismus* (Dietz, Berlin, 1953), in *Problems of the Peoples of the USSR*, No. 11 (1961).

This matter has come to such a point that review of a little of its history is justified. On the appearance in the first number of the *Mankind Quarterly* of Garrett's criticism of Klineberg's work (1), several biologists and anthropologists expressed their amazement and disagreement (2, 3). Moreover, several scientists on the honorary advisory board of the *Mankind Quarterly* resigned.

What happens then? The editors (4) of the *Mankind Quarterly*, and others in sympathy with its views (5), devote themselves by all available means to discrediting those who from a strictly scientific point of view agree in their opposition to the *Quarterly's* position—all this without presenting a single scientifically proved fact to support their position. Furthermore, they ascribe to us sympathy for political ideas which we simply do not have, gratuitously making us partisans of what they call "the equalitarian dogma." Disagreement with Garrett's second article (4) was wide and explicit (6). Now Garrett tells us that his racism is not "un-



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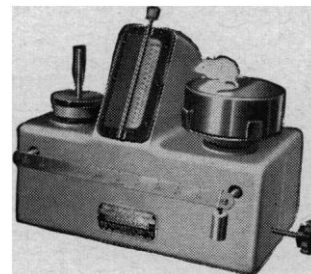
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scientific" because he does not explicitly propose a "master race" or a "chosen people." However, a few lines later, without any factual basis, he alleges the bad effects of Negro-white hybridization in the West Indies, Central America, and parts of South America, stating that the white, "no matter how low (in a socio-economic sense)," will always be better than the Negro, "no matter how high (again in a socio-economic sense)."

I may point out that the major percentage of delinquency found among

the Negroes in the United States was interpreted by Garrett as caused by racial characters (1). That is, according to Garrett and others, Negroes possess a genetic factor that induces prostitution, crime, robbery, and so on: "Haiti is an unhappy example of what the Negro can do when left to govern himself"; "The weak, disease-ridden population of modern Egypt offers dramatic evidence of the evil effects of a hybridization which has gone on for 5000 years" (1), and so on and on (7). According to Garrett, the Negro is in-

capable of governing himself, and the same holds for the Indian. Hitler *et al.*, supported by similar theories—which sprouted from the views of pseudoscientists—murdered many millions of Jews because their hybridization with "Aryans" were also not "viable," to use Garrett's expression. We all remember the result, but if Garrett has forgotten it, he should read the impressive story by Miklos Nyiszli, *Médecin à Auschwitz*, which appeared in October 1961.

Garrett misquotes me in affirming that I subscribe to "racial equalitarianism." No anthropologist or biologist with average knowledge maintains such a position. I repeat what I said: "Racial differences exist. These differences should be and are being studied. A whole branch of anthropology is concerned with this study in an effort to determine what the differences are and how they may be used—to understand them from every possible angle (genetic, morphological, social, and so forth) but always within the framework of serious and scientific investigation. The 1952 UNESCO Statement on Race is quite clear on this point" (3).

I hold that from the viewpoint of human biology and of human genetics it is just as unscientific to support an equalitarian dogma as it is to maintain that the actual cultural superiority or inferiority of a certain human group is due to morphological or possibly psychological differences. I do not belong to the group that takes the first position, but Garrett does belong to the group that takes the second.

At the last meeting of the American Anthropological Association, in November 1961, the assembled anthropologists approved, by a vote of 192 to 0, the following resolution (9):

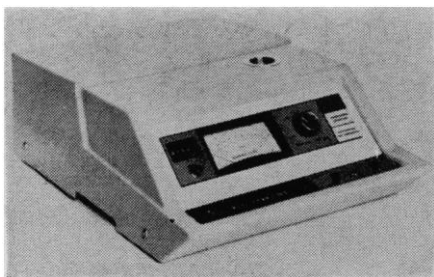
The American Anthropological Association repudiates statements now appearing in the United States that negroes are biologically and in innate mental ability inferior to whites. . . . According to present knowledge there is no proof that the groups of mankind differ in their innate mental characteristics, whether in respect of intelligence or temperament. The scientific evidence indicates that the range of mental capacities in all ethnic groups is much the same. No doubt the myth of racial inferiority will continue to be believed by those who are able to find in it a fortification against their personal insecurity and lack of status. But for the good of mankind in general the myth ought to be recognized for what it is—a pretext for bigotry and discrimination.

No further comment is needed. But I should like to repeat what I said in

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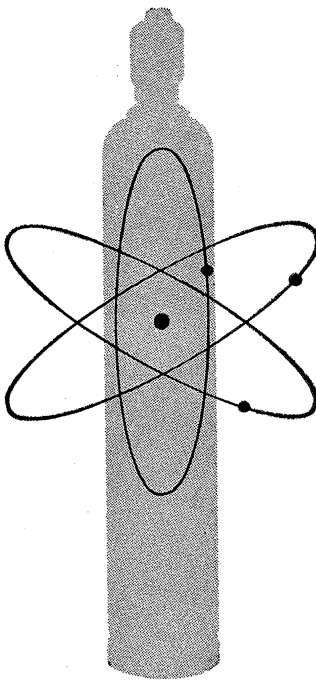
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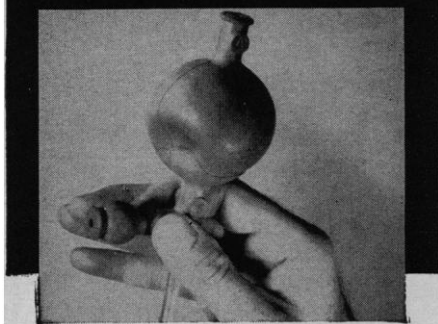
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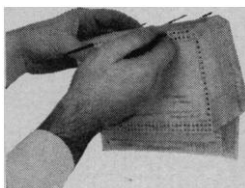
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my earlier note (3): "I am now formally addressing *Science*, as the official voice of the American Association for the Advancement of Science, to ask that the Association urge the scientists who are accessible to it (I would suggest, among others, Medawar, Haldane, Simpson, Huxley, Neel, Wright, Dunn, Dobzhansky, and de Beer), United States citizens or not, to take action, in the name of the Association, against this unwelcome, ill-founded, unbiological outgrowth of racism."

I accorded Gregor only 12 meager lines in my earlier letter (3). My judgment, however, was justified. I have carefully read Comas's work and Gregor's criticism and have also seen Comas's reply to Gregor, to be published shortly. Because of this, not only do I continue to maintain my expressed opinion of Gregor's trivial criticism of Comas's brochure but, on reading his present comments on my letter to *Science*, note that he as well as Garrett attributes to me, in an effort to influence the reader, political ideas which I do not support. The "witch-hunting" of "McCarthyism"—to quote the expression of Garrett, his colleague—fortunately has disappeared. Gregor, utilizing as a pretext various typographical errors in the last edition of *Racial Myths*, for which Comas is not responsible (this edition was published in the United States, without the knowledge or revision of the author; the original manuscript, in Spanish, was commissioned and published by UNESCO in 1951), proceeds to discredit it, as well as Comas's competence in the field of anthropology. I refer Gregor and the reader to Comas's reply, which will appear in a forthcoming issue of *Current Anthropology*.

SANTIAGO GENOVÉS

Institute of History, University of Mexico, Mexico City

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1. H. E. Garrett, *Mankind Quart.* 1, 15 (1960).
2. B. Skerlj, *Man* 60, 172 (1960); J. Comas, *Current Anthropol.* 2, 303 (1961); G. A. Harrison, *Man* 61, 189 (1961); M. J. Herskovits, *Negro Digest* 12, 43 (1961).
3. S. Genovés, *Science* 134, 1928 (1961).
4. H. E. Garrett, *Mankind Quart.* (Apr. 1961); *Perspectives in Biol. and Med.* 4, 480 (1961).
5. A. James, *Mankind Quart.* 2, 30 (1961).
6. M. J. Herskovits, *Perspectives in Biol. and Med.* 5, 122 (1961); A. Montagu, *ibid.* 5, 135 (1961); "Statement on race and intelligence from the Society for the Psychological Study of Social Issues," *ibid.* 5, 129 (1961) (the society is a part of the American Psychological Association; this public statement was made 6 Sept. 1961, on behalf of the executive council of the society, by its president, Isidor Chein); S. Garb, *Perspectives in Biol. and Med.* 5, 135 (1961); W. Hartman, *ibid.* 5, 136 (1961); M. H. Frank, *ibid.* 5, 139 (1961); N. H. Eisen, *ibid.* 5, 139 (1961); D. J. Ingle, *ibid.* 5, 142 (1961).

7. There is no necessity for further discussion of the subject, but I wish to cite D. D. Jackson's comments in *Science* [134, 1910 (1961)] with reference to another article: "Indeed, Passamanick . . . has recently suggested that his studies show the human brain to be so highly adaptable that the lower end of the I.Q. scale is related not to heredity but to opportunity—including the circumstances of birth and early rearing."
8. M. Nyiszli, *Medecin à Auschwitz* (Julliard, Paris, 1961).
9. American Association of Anthropologists, *Fellow Newsletter* 3, 1 (Jan. 1962).

The Search for Other Civilizations

The article by Sebastian von Hoerner, "The search for signals from other civilizations" [*Science* 134, 1839 (1961)], contains at least one statement that seems to be so out of line with observed facts that it should not go unchallenged. This is the statement that the ancient Greeks could have found the distances between neighboring stars to within 5 percent.

Let us accept, for the moment, von Hoerner's statement that by assuming the earth to be an "average" planet—that is, comparable to the average for the five planets visible to the naked eye—the Greeks could have obtained a value for the distance of the sun too large by a factor of 2, and thus a value for its luminosity too high by a factor of 4. By subsequently comparing the brightness of the "ten brightest" stars (presumably the brightest ten are meant) with that of the sun and assuming the average luminosity for these stars equal to the luminosity of the sun, the Greeks could have "determined" the average distance of these stars—but they would have been way off. The brightest ten stars visible from Athens 2400 years ago would have been the same as the brightest ten in the sky now, except for Achernar which was then not really visible north of Khar-toum; the ten would have been Sirius, Canopus, Alpha Centauri, Arcturus, Vega, Capella, Rigel, Procyon, Beta Centauri, and Betelgeuse. At about 450 B.C. Alpha and Beta Centauri had declinations around -49° ; Canopus had a declination of -52° , but since the Greeks presumably named this star, they must have been aware of its existence, and it must have been easily visible from Alexandria. If, however, one does not include it, Altair would be added to the list.

Using the latest available data—and for at least four stars these are none too good even now—we find the average luminosity of the brightest ten to



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