As one justification of his use of phenotype frequencies, Lahovary speaks of "the AB group conforming to expectation in the whites and, to a more limited extent, in the blacks, whereas it is much higher than the expected values in the Mongoloids and in the populations with a Mongoloid or Eurasiatic admixture." He states that "in the white fringe, a higher frequency of AB than the expected value is a diagnostic indication of Asiatic admixture" and (without giving any reference) that "Myslaveč assumes that this racial trait is probably due to a slightly different position of the respective genes in the yellow races, favoring certain linkages." I have found no evidence of this supposed excess of AB; moreover, of the five sets of observations on "Eurasiatics and Mongoloids" quoted in Table 1 of Lahovary's paper, four show a deficiency of AB, the fourth set being 100 percent group O. If, however, any bodies of data should show such a significant and consistent excess, the first thought of any serologist would be a suspicion of technical errors. If such errors could be eliminated from consideration, various genetical possibilities would have to be considered and explored, and until the purely genetical problem had been solved it would be necessary to refrain from drawing any anthropological conclusions from the material.

A corollary of Lahovary's attitude to phenotypes in isolation from the underlying genetical situation is his use of the "index of deviation" and the incorporation of such indices in a special type of deviation diagram. The index of deviation, though apparently a simple mathematical quantity, has no clear statistical meaning, and it incorporates and confuses the true differences between populations together with the sampling errors for each of them (as well as technical errors in the testing of them).

It is desirable, for certain purposes, to have a quantitative expression of the difference between two populations. As far as the ABO groups are concerned, this is very simply given in both direction and magnitude by the line on a Streng triangular diagram, which joins the two points representing the populations concerned. A single Streng diagram can thus incorporate with a high degree of statistical efficiency the information that is much less efficiently expressed by a number of Lahovary diagrams one less in number than the total number of points on the Streng diagram.

Where the number of independent variables concerned is greater than two, the mathematical expression of differences between populations poses a highly complex problem that is not solved by Lahovary's device of summing indiscriminately the differences in percentage frequencies of all phenotypes.

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Rejoinders to W. C. Boyd and A. E. Mourant

In answer to my reply in *Science* to certain criticisms of my work by W. C. Boyd, who alleges that I do not give due recognition to the independence of the genes, Boyd maintains that I still commit this error. However, our points of view are not as far apart as Boyd seems to believe. The difference is not fundamental but lies more in stressing what *distinguishes* the various races or what they share *in common.*

The reader may judge for himself. I wrote: "There is a general specificity working for the unity of each organism and of each racial entity . . . we cannot deny [that is, notwithstanding the theoretical independence of the genes] the existence of *parallel trends* making for harmony." What does Boyd answer? "It is true that the characteristics of any individual must form a more or less harmonious whole. . . . What is characteristic of a race is a 'certain constellation of characters'." It does not appear to me that the difference between "parallel trends making for harmony" and a "constellation of characters forming a more or less harmonious whole" is very easy to perceive.

It might even be contended that, on the whole, "parallel trends" leave more to the independence of the genes than a "constellation of characters," a constellation implying, in the cosmos, a permanent and, judged by human measures, an irrevocable relationship. In slightly different terms, we therefore express a nearly similar opinion. Boyd's assertion that I am "paying lip service to the independence of the genes" is consequently somewhat gratuitous. What is unfortunately still more gratuitous is his saddling me with the strange theory that each individual has only his own racial genes, that, for instance, I presume that the ABO or Rh genes of the Negro or *everything* about a member of one race is different from the corresponding feature in a member of another race. This accusation is all the more surprising since my definition of "parallel trends," previously mentioned by Boyd, excludes such an interpretation. Moreover, this would also imply that I deny the existence of a human species, for it is clear that if every race had *only* its specific genes, there would be not *one* human species subdivided into various races but *several* different species of "humans." Needless to say, I never have harbored, nor could be supposed to harbor, so wildly revolutionary an idea.

The real difference between Boyd's conceptions and mine lies in the difference in emphasis. Whereas he stresses the *similarities* among the races and thus, indirectly, the independence of the genes, I stress their *distinctiveness* and, thereby, the racial correlations of the genes, because, as stated by F. v. Eickstedt, the great German anthropologist: "All characters are not inherited independently; on the contrary, most of them [I would say personally, at any rate many of them] are transmitted in correlated groups. . . . There could be otherwise no racial types, nor even any definite types at all." [*Rassenkunde und Rassengeschichte*

der Menschheit (Stuttgart, 1942), p. 592]. Without the transmission of the genes in more or less correlated groups, there would be no zoology or anthropology, as we know them, for no classification would ever have been possible.

If I stress racial specificity and thus, implicitly, the existence of correlated systems of genes, it is because I think it is also a more constructive approach to racial problems and more conducive to further progress than the opposite tendency. If we lay the stress on the characters (ergo, genes) that are inherited in common (that is, independently) by the greyhound, the bulldog, the pekinese, or the pointer, and so forth, and make them all belong to the canine species, this will not bring us very far in the study of the various races of dogs. What will be, on the contrary, much more fruitful will be to study the *distinctive* characters that make these various breeds differ.

The parallel trends or constellations of characters proving the very frequent correlations of genes, which find their expression in racial specificity, are evident in all the biologic potentialities of mankind, and the works devoted to their study are legion. I mention as a small sample, for the specificity of the skeleton (*1–5*), for the racial specificity of the brain, that is, correlation of the genes governing the development of the brain (*6–11*), for the racial specificity and correlation of genes in the functions of the brain (*12–19*), for the racial specificity, or constellation of characters directing the chemical constitution of the blood, blood pressure, and heart, basal metabolism and temperature regulation (*20–28*), for racial specificity, that is, correlated transmission of genes concerning organic functions and morbidity (*29–37*), for various other genetic and racial correlations (*38–41*), and standard works, among many, giving numerous instances of racial correlations (*42–44*).

H. Vallois has stated his position in unequivocal terms: "The differences between the various races are much greater than was generally admitted . . . they effect the composition of our very tissues, all their intimate functions, for they affect indeed the whole being." ["La découverte des races," Sem. des Hôpitaux, Mar. 1952]

Racially correlated characters are thus found in the whole field of human biology, and even in psychology. This implies necessarily the existence of racial genic trends working in correlated harmony, the independent genes completing the picture by insuring the unity of the species. (The independent and the correlated genes evidently do not form two distinct classes but are transmitted independently, or in correlated groups, according to circumstances.) In this way, the independence and the interdependence of the genes are two complementary and necessary conditions of life as we know it.

With regard to the similarity observed by Boyd in the ABO frequencies of Eskimos and Australian aborigines, this detracts nothing from the specificity of each group, considering that in the other blood systems they are completely different. I agree, however, that no definite conclusion can be reached at present regarding the B subgroups.

A. E. Mourant's contribution to genetics and serology has been so considerable that any observation of his deserves great consideration. I will therefore attempt to reply to his objections as fully as space permits.

To his first indictment, that I apply to anthropological problems the crude phenotypic classifications of the serologist, while rarely making use of the accepted methods of genetical research, I would answer that the arguments for the preference to be given, in racial and ethnical studies, to the phenotypic or, vice versa, to the genetic data, have been already exhaustively dealt with, in an ample discussion between F. Lenz and J. Schwidetzky [Lenz, *Z. Morphologie und Abstammungslehre* (1940–41); Schwidetzky, and a last rejoinder by Lenz, *ibid.* **40**, No. 1 (1942–43)]. I need not therefore thrash out once again the *pros* and *contras*. The conclusion seemed to me at the time to be that, while for clinical and purely scientific studies preference should be given to the genetic approach, the phenotypic data were more adequate for ethnical problems; and it is this opinion that has guided me since.

With regard to Myslaveč's assertion concerning the frequency of the AB group, I had it verified by K. Dürr, author of *Blut Auswertung* and a skilled mathematician, who obligingly compared a large number of phenotypic and genotypic series and arrived at the same conclusion as Myslaveč, namely, that the AB percentage conformed to expectations in the whites, was slightly, but not significantly, lower, in the Negroes, while it was often significantly above expectations in various Mongoloid series. I was then confirmed in my impression of its symptomological importance for ethnical classifications.

In Mongoloid series the frequencies for A, B, or O may vary considerably. For A, the variation is from 17 to 41 percent, or more; for B, from 19 to nearly 40 percent; for O, from less than 20 to around 40 percent. Corresponding percentages for these blood groups can be found in all the other races of mankind. If we had only the data for A and B, we would have to admit the absence of any common link, of any specific character that we could term "Mongoloid" in these so different series (not to consider the subgroups). If we observe, however, the AB group, this specific character is immediately apparent. Whatever be the variations for the other blood groups, the percentage of AB is uniformly high and varies comparatively little, between 10 and 15 percent, or sometimes a little more. It is thus, on an average, 5 times the mean value of AB in the whites, and 4 times this value in the Negroes. This is therefore, the common Mongoloid denominator, and we can tell at once simply by glancing at the frequency of AB, whether or not we have to do with a Mongoloid series. The racial significance of the AB group thus cannot be overrated, and consideration simply of the gene frequencies loses sight of this most valuable aid for ethnical

classification and analysis. Another instance of this is to be seen in two phenotypic series of Germans and Swedes:

	AB	A	B	O
	(percent)			
Germans (E. Friesland, Buchner, 646 cases)	1.5	52.3	13.3	32.9
Swedes (Stockholm, 500, Folgren, Vin.)	7.2	50.0	9.1	33.7

The most notable difference between these two series is that the percentage of AB in the Swedes is fivefold that of the Germans. The reason is that the Finns, owing to their Ural and Siberian origins, have a considerable Mongoloid component. The divergence in the Swedes, as to the proportion of AB, is thus clearly due to Finnish infiltration, naturally lacking in the Germans.

This interesting detection of the Finnish component in the Swedish series would not be obtainable with the Streng method, favored by Mourant. Transcribed into allelic frequencies, the two foregoing series would become the following:

	p	q	r	Total
Germans	32	7.8	57.4	97.6
Swedes	34.6	8.6	48.1	101.3

From this comparison no such useful indication, of the kind extracted from the phenotypes, could be drawn. Many similar instances could be given.

The AB group also permits one to distinguish the North African populations of the western Mediterranean basin from those of the eastern end of the Mediterranean (Egyptians and Arabs). The percentage of AB in the latter group is, on an average, nearly treble that of the western Mediterraneans.

All the more interesting is the geographic correlation between a high frequency of AB and an Asiatic origin, independent in large measure of race, inasmuch as Arabs, Iranians, and many Indians often have as high a percentage of AB as the Mongoloids. Many more instances of the great value of AB for ethnical research could be given. Disregard of this group and exclusive reliance on gene frequencies is doubtless the reason why Streng's results are, notwithstanding Mourant's preference, so often unsatisfactory, if not directly absurd. Streng utilizes for his ethnic index, the formula $I = (2p + q)/\sqrt{3}$. This yields such indices, for instance, as [O. Streng in *Die Bluteigenschaften der Völker,* Festschrift für H. Hirt (Heidelberg, 1936)]:

North Germans	38.2		Chinese	32.9
Abyssinians	38.4	or	Dutch	33.5
Norwegians	38.5		Iranians	34.3

The strangeness of these results needs no further comment and can only perplex and mislead the layman. But the prestige of what seems mathematical efficiency is so great, that undaunted, many other authors have also adopted procedures more or less similar to those utilized by Streng. I mention, as only one unfortunate instance, the graphical representation in J. Avias' study, "Les groupes sanguins des Néocalédoniens et des Océaniens, en général du point de vue de l'anthropologie raciale" [*L'Anthropologie* 53 (1949)]. This representation shows Hindus and Japanese, Caucasian peoples and Australian natives, African blacks and the oceanic inhabitants of the Admiralty Islands, Australian natives and Eskimos, and so forth, paired in the most brotherly way. If a method is to be judged by its results, we cannot agree that this one is "efficient."

Moreover, the Streng method can deal only with three variables at most. It is thus entirely inadequate to give full expression in its triangular diagrams to the various frequencies of the Rh system, although this system is fast becoming, probably, the most important one for human classifications. On the contrary, my simple method can cope with any number of variables, by the rough and ready process of summing up all the differences deriving from the various frequencies observed in any given blood system. Thus, notwithstanding the similarity in the ABO system, say of Australians and Eskimos, we can immediately see by their very high "index of deviation," both in the MN and in the Rh systems, that these populations are really entirely unconnected. The direction of the deviations can also be easily expressed.

A tree should be judged by its fruits. My method, however open to objections, avoids the fallacies noted in the foregoing paragraphs and works out better, in practice, than the other methods.

This may perhaps be because, as von Eickstedt, the German anthropologist, writes (*Rassenkunde und Rassengeschichte der Menschheit,* p. 544):

> . . . as all the investigations made so far are but samplings, it is an illusion to seek more than approximate truths . . . the more so, as biological, like physical phenomena, cannot be determined, as a rule, with mathematical precision. . . . The examination of a single series of characters may also lead to absurd results . . . the rigidity of algebraic and complicated mathematical constructions tends to obscure the essential sampling origin of our data, and the basic fluidity of biological factors, transforming provisional approximations into hard and fast truths—which they are not. . . .

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The foregoing communications from W. C. Boyd, A. E. Mourant, and N. Lahovary have been greatly delayed in publication because Dr. Lahovary did not observe editorial restrictions on the length permissible for his rejoinder.

The rejoinder to W. C. Boyd in its present form is printed without curtailment, except for the list of 44 references cited in the text and with which Dr. Lahovary desired to document his reply; those interested may obtain this list in mimeographed form from the American Documentation Institute, Library of Congress, Washington, D.C. The rejoinder to A. E. Mourant appears in somewhat condensed form, with one lengthy table of data omitted entirely.