

CONCERNING THE COMMENT BY B. A. SHULYAK IN HIS
BOOK "PHYSICS OF WAVES ON THE SURFACE OF A
LOOSE BED OR A LIQUID"*

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In 1971 Nauka has published a book by B. A. Shulyak "Physics of Waves on the Surface of a Loose Bed or a Liquid," the second part (Chaps. 2-5) of which deals with the theory of similarity and dimensional analysis. Without discussing it in substance, I only feel compelled to focus my attention here on the following items.

The author refers to a certain place in my book "Introduction to the Theory of Similarity" (Vysshaya Shkola, Moscow, 1963) listed in the bibliographies to Chaps. 2, 3, and 5. The author views this item quite critically, as clearly indicated by his comments ("rather significant drawbacks," "erroneous premises," "this is absolutely wrong," etc.). In the text the author mentions my book several times and always with some slighting comment.

As the author of a book which has been subjected to criticism, I am obligated to either affirm the correctness of these comments or disagree with them. In this case, however, I find it necessary to depart from that rule. The fact of the matter is that the said comments about my work are entirely irrelevant. This may appear surprising, or even unlikely. The plain facts leave no doubt about it, however.

Let us consider one by one all the comments referring to my book.

1. On p. 38 we read that the erroneous premises are due to "an introduction to the theory of similarity of elements taken from dimensional analysis (the π -theorem)." But my book has been organized so that the entire theory of similarity (including all premises) is presented in the first four chapters, while dimensional analysis is covered in the fifth chapter. Consequently, it is quite impossible that elements of dimensional analysis could have been introduced into the fundamentals of similarity theory. This can be easily ascertained; one only has to look at the Table of Contents, where the titles of chapters appear heavily underlined.
2. The idea is suggested, furthermore, that the introduction of an "insufficiently rigorous" concept of the field of a physical quantity will, in the final analysis, lead "to an ambiguity in the determination of the number of dynamic critical groups and to a necessity of defining them 'artificially,' i.e., apart from the very equations of motion." But I arrive at the final result in a direct way, intentionally without digressing into rather vague considerations. Indeed, there is nothing to be said for "an ambiguity in the determination of the number" of critical groups and their artificial definition. But I would like to know where in my book and how have critical groups been derived outside the equations. I certainly do not know about it, inasmuch as such groups are always based on the very equations of the problem.
3. On p. 74 the author refers to my book directly, saying that "... a representation of π by $\Phi(\pi_i)$ is called a criterial equation, considering that he (probably should be it?) relates the π_i -complexes

*In this published letter Professor A. A. Gukhman answers only those comments by B. A. Shulyak in his book "Physics of Waves on the Surface of a Loose Bed or a Liquid" (Nauka, Moscow, 1971), where he refers to A. A. Gukhman's book "Introduction to the Theory of Similarity" (Vysshaya Shkola, Moscow, 1963). As to B. A. Shulyak's critique of the work by Academician M. V. Kirpichev's group in the theory of similarity, the editors of Inzhenerno-Fizicheskii Zhurnal consider it inappropriate to open a discussion on that subject.

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with one another. This is absolutely wrong." Nothing even remotely resembling such a statement appears in my book (and could not, in the vein it has been written).

4. On p. 89 the author discusses the conditions under which a similarity group loses its significance with regard to a process, and he emphasizes that the range of values within which it continues to characterize the given process is limited. While on the subject, he mentions (again referring to my book directly) that "without justification, an important role is assigned" to the Reynolds number in cases where it is not significant. The following must be said to this. The problem has been thoroughly analyzed in my book in several places, as the Table of Contents will indicate (see §§ 12, 20, 21, 25, and 33). That applies also to the Reynolds number specifically. It would be impossible to have overlooked this.
5. The following reference to my book (on the same p. 89) corresponds just as little to the actual contents of it: "There is also a reverse trend in evidence, namely to describe identical processes by entirely different similarity groups which include viscosity."
6. Finally, without any particular reference to my book, my name is mentioned on p. 50 and it is said that a procedure has been proposed for setting up similarity criteria which yields criterial groups not corresponding to the fundamental equations. Of course, nothing of that sort can be found in my latest book. What I have said is that the determining criterial groups (at that time I still used this terminology) which are arguments of universal equations must be made up of quantities known from the problem conditions. For this reason, in setting up the similarity groups (which, of course, are found from the fundamental equations of the problem and which fully correspond to these equations), one must introduce here values of the variables based on initial and boundary conditions.

It is obvious from all this that the "criticism" of my book "Introduction to the Theory of Similarity" by B. A. Shulyak in his book "Physics of Waves on the Surface of a Loose Bed or a Liquid" is entirely irrelevant.