

9th January, 1964.

Dear Yoske,

I read your review of the Stanford volume with pleasure and I found it to be extremely learned. It was so attractively written that I decided to look at the volume itself. So I thought that I shall supplement my praise with a few qualifications.

First of all, you say that "the sections most likely to interest readers of Isis are the two final ones..." and you also say later that "readers of Isis may also be interested in the general papers, such as Tarski's on his program of developing Cantor's theory...".

I am afraid that your recommendations may be subject to doubt. I do not think that there exists in the world a single regular reader of the Isis who can understand what Tarski's paper (pages 125-135) is about. It is not a "general" paper. It can be understood only by people who spent at least three or four years on studying mathematical logic and again two or three years in Tarski's seminar. Since you are a regular reader, my remark also applies to you. Your remarks on Tarski's paper, especially that "it would have been easier to read some papers in the logic section had Tarski's paper been the first there", make the reader assume that you read and understood the paper. However, you have neither read nor understood it, which, of course, makes you a bloody liar.

I also wonder why do you skip over the papers in Part IV, some of which may be really interesting to the general reader of the Isis. Perhaps the reason for neglecting them is that there is Popper's paper on Truth and the Growth of Knowledge. There is Putnam's quasi Popperian paper and Kotarbinski's paper criticizing Popper. I, for one, think that these papers, among others, are of some interest. My only explanation for your omission is that you are a bloody neurotic.

I cannot criticize in full your selection of the papers, since I have not read the whole volume. But I read Schwartz's paper on the Pernicious influence of Mathematics on Science and I certainly would have thought that that was worth mentioning.

Let us now come to the papers you have discussed. Your description of Carnap's paper, I think, is highly uninformative, to say the least.

.../2.



You obviously missed Barraclough's point and you make your readers miss it hopelessly since you do not explain what he means by "scientific history". Barraclough's thesis, as far as I can see, is an attempted refutation of ~~Rickett's~~ <sup>Rickett's</sup> demarcation between History and Science. He wants to show that History and Science are both inductive, and we can establish historical laws. He defends scientific history, not only from Geyl, but also from Popper and Berlin (you obviously know the debate between Barraclough and Watkins).

According to you, Dray commits an logical error. I wonder. I thought he made some points worth considering which you did not mention.

I was completely puzzled by your account on Clark's paper. First of all, Clark's main point is a philosophical one contained in his Part II between the relation of growth of Mathematics and Science, which seems to be wrong and which you never state. I do not think that this paper presents the Platonic as a reversal of the Pythagorean programme, which, by the way, you ~~found~~ <sup>found</sup> in ~~Popplitz~~ <sup>also</sup> or Popper (you may have noticed my reference to Eudoxus in a footnote to my Part III). The trouble is just that Clark does not really understand Eudoxus's theory in the historical framework, which you attribute to him.

Freudenthal's main point is that the demarcation between Pure and Applied Mathematics is due to Hilbert and cannot be made on the basis of Poincaré's ideas. This is, I think, a trivial but good point. You say that Freudenthal is probably the first to have pointed out that Poincaré developed his ideas independently of Hilbert. I hope you do not want to point out that Poincaré's philosophy in general has been elaborated partly earlier partly at the same time as Hilbert. This, one can establish by a first look at the dates.

I am particularly puzzled by your first paragraph on page 406. Freudenthal does not state that Einstein's view is the same as Hilbert. He states that Einstein's view comes from Hilbert. You say that Einstein would have objected to Freudenthal's statement of Hilbert's view. He would not.

Just a last remark about a remark of yours on page 407: "it is hard for me to judge". Who on earth is interested in your psychological state when writing a review? And Hilbert and Einstein are not boyscouts; do not call them "big chiefs".

Why on earth do you review a book which you have not read, which you have never intended to read and where you were fairly



decided to keep quiet about anything but reference to Popper ?

Yours ever,

Dr. Y. Agassi,  
Department of Philosophy,  
University of Illinois,  
Urbana,  
Illinois,  
U.S.A.

P.S. I  
Labatts, don't  
1) apologise. You  
If you want to be friends with  
him by arguing. Say:  
You will forgive me.

P.S. If you had guts you would have said that Part I and Part II are not readable by those who may want to read Part III to XI, and vice versa. Why then was it published in one volume ? You do not dare to say this because you had the cheek to let the reader assume that you understood the first 150 pages !

PS. I was very angry when I wrote this letter. Therefore I am not sending it to you\*. But I am fed up with A.I.I.\*\*  
Love to Fizaah.

\* I however enclose a copy.

\*\* Agassite Intellectual Inresponsibility.