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C. H.

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# ANALYSIS

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## SOME REMARKS ON EMPIRICISM

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IN this note, I want to deal with the objections which Dr. v. Juhos has raised in this periodical<sup>1</sup> to certain considerations concerning the language of science, which are mainly due to Carnap, Neurath and Popper, and some of which I have outlined in an earlier paper.<sup>2</sup>

We may distinguish three main points to which v. Juhos's criticism refers: (I) The "behavioristic" interpretation of psychological statements such as "I see blue"; (II) the view that in our empirical science even statements of the kind just mentioned might be "altered" or "abandoned" under certain conditions; (III) The proposal to express "epistemological" considerations in the formal mode of speech.

Von Juhos maintains that each of these points is a thesis or a postulate of physicalism: he speaks of "the physicalist mode of speech which demands the alterability of all statements" (l.c. p. 89; in this passage, elements of each of the three independent points are combined), and he declares the recommendation of the formal mode to be "a special postulate of Physicalism" (l.c. p. 90). For the sake of clarity, it ought to be noticed, that only (I) is a thesis of physicalism, whilst (II) and (III) are fully independent of it.

<sup>1</sup> B. v. Juhos, *Empiricism and Physicalism*; *Analysis*, 2, 81.

<sup>2</sup> Hempel (1): *On the logical positivists' theory of truth*; *Analysis*, 2, 49.



Indeed, physicalism<sup>3</sup> asserts that the language of physics is a universal language of science ; i.e. that "every sentence of any branch of scientific language is equipollent to some sentence of the physical language, and can therefore be translated into the physical language without changing its content."<sup>4</sup>

This thesis obviously entails the special assertion that the statements of psychology can be translated into the physical language.<sup>5</sup> In the Vienna Circle, this was originally called the *thesis of behaviorism*. At present, this designation is rarely employed, in order to avoid confusing this logical thesis, which deals with the syntax of scientific language, with the psychological statements and the methodological principles of American behaviorism. But as v. Juhos employs the term "behaviorism" in order to denote the syntactical thesis under consideration, it may be suitable to do the same in the following discussion.

So (I) is in fact, a thesis of physicalism. (III) is independent of physicalism : the thesis of behaviorism as well as many other theses of this kind can be formulated both in the material<sup>6</sup> and in the formal mode. And obviously point (II) is not a consequence of physicalism either ; this is illustrated by the fact that in earlier papers serving to establish the thesis of physicalism (especially in (1)), Carnap started from the assumption that science is based on protocol-statements which need no confirm-

<sup>3</sup> See e.g.

Carnap (1) : *The Unity of Science* ; London 1934.

(2) : *Philosophy and Logical Syntax* ; London 1935 (pp. 88-97).

(3) : *Testability and Meaning* ; (will be published in *Philosophy of Science*, 1936).

(4) : *Über die Einheitssprache der Wissenschaft* ; (will be published in the Transactions of the International Congress of Scientific Philosophy, Paris 1935. Publishers, Hermann & Cie, Paris.)

Neurath (1') : *Physicalisme* ; *Scientia* 1931, p. 117.

(1'') : *Physikalismus* ; *Scientia* 1931, p. 297.

(2) : *Physicalism* ; *The Monist* 1931.

(3) : *Empirische Soziologie*, Wien 1931.

(4) : *Soziologie im Physikalismus* ; *Erkenntnis* 2, p. 393.

Schlick (1) : *De la relation entre les notions psychologiques et les notions physiques* ; *Revue de Synthèse* X, p. 5 (1935).

<sup>4</sup> Carnap (2), p. 89. In the meantime, Carnap has developed a certain modification of physicalism (see his papers (3), (4)). For our purposes it is, however, not necessary to refer to this refined form of the physicalistic thesis.

<sup>5</sup> For a detailed account of this thesis see e.g.

Carnap (5) : *Psychologie in physikalischer Sprache* ; *Erkenntnis* 3, 107.

(6) : *Les concepts psychologiques et les concepts physiques sont-ils foncièrement différents ?* *Revue de Synthèse* X, 43 (1935).

Hempel (2) : *Analyse logique de la psychologie* ; *Revue de Synthèse* X, 27 (1935). (Here much further literature is indicated).

Neurath (5) : *Einheitswissenschaft und Psychologie*, Wien 1931.

<sup>6</sup> See e.g. Carnap (7) : *Erwiderung auf die . . . Aufsätze von E. Zilsel und K. Duncker* ; *Erkenntnis* 3, 177 (especially pp. 183-187, pointing out the dangers which arise from employing the material mode in the discussion of the syntactical thesis of behaviorism.)



ation (and are, therefore, not iterable) ; and that Schlick in a recent paper<sup>7</sup>) expressly acknowledges physicalism though rejecting the assertion (II).

On (I). Let us now consider first those of v. Juhos's objections which really concern physicalism, viz. those directed against (syntactical) behaviorism.

(a) In a rough formulation<sup>8</sup>, the thesis of behaviorism (physicalism) asserts, that for any statement speaking of 'feelings,' 'thoughts,' 'acts of will', etc. of a person, there is an equipollent statement which speaks exclusively of the "bodily" behaviour (movements, sounds pronounced, physiological reactions, etc.) of the person in question ; and v. Juhos is right in asserting that, according to behaviorism, the indications of a man about the state of his own feelings (e.g. his saying or writing "I feel pain") have the same logical function for testing a psychological hypothesis (: "This person is in the pain-state") as the pointed, written or spoken indications of a barometer have for testing a physical hypothesis (: "In the interior of this instrument, the air-pressure is now 1 atmosphere").

As a consequence, incorrect indications of the human "pain-indicator" correspond, for behaviorism, to incorrect indications of an air-pressure-indicator or some other instrument. But saying, as v. Juhos does, that for this reason "the physicalist . . . must in cases of the above-mentioned type reproach the instruments in question with a lie or an error" (l.c. p. 87) is evidently running just contrary to the fundamental idea of behaviorism : that attitude would mean applying to physics the point of view of (a rather primitive form of every-day-life) psychology, whilst behaviorism tends to proceed vice versa.

In fact—and this is accentuated by behaviorism—the methodological principles of scientific psychology are the same as those of physics ; and particularly in the case of incorrect indications one blames neither the instrument nor the test-person : one rather tries to *explain* the occurring "deviations," e.g. by showing that certain disturbing factors are working. Formally, such an explanation consists of the establishment of a hypothesis concerning disturbing factors.

(b) In psychology, two particularly important kinds of hypothesis of this type are error-hypotheses and lie-hypotheses.

Now, v. Juhos thinks (l.c. p. 84, ff.) that for explaining

<sup>7</sup> Schlick (1), p. 14.

<sup>8</sup> See Hempel (2).



incorrect indication of a man about his own state of feeling, physicalism demands (or at least admits) the introduction of an error-hypothesis ; and he asserts that this " has no meaning for the Empiricist " (l.c. pp. 85/86). It ought to be noticed, however, that *physicalism makes not the least allegation concerning the question of lie and error*, as may be seen from the above formulations and from the papers cited. Therefore, even if v. Juhos were right in asserting that scientific psychology does not admit of error-hypotheses in the considered cases, this would not have consequences for physicalism.

Besides that, it seems to me, that his assertion is not right :

Allowing, or generally forbidding, the introduction of error-hypotheses concerning the results of psychological self-observation (" introspection ") is, I think, a question of syntactical convention, which cannot be true or false, but only more or less practical. In the case of allowing, the convention mainly consists of specifying how to test such an error-hypothesis. Hereby, the error-hypotheses are *given* a " meaning."

As to actual scientific psychology, I think that it would *not* generally exclude error-hypotheses concerning the results of self-observation. E.g. Any psychologist would admit the possibility (and even the frequent occurrence) of error when a man states through self-observation the motives of his actions ; e.g. when he says : " I change my political party out of conviction ; I am sure that I have not got the least desire for material advantages." (In cases of this type, psychoanalysis, e.g., furnishes us with certain testing-methods.) And it seems to me that there is only a difference of degree between this case and that of a person saying " I feel pain " or " I see blue."<sup>9</sup>

And a more general point of view : if lie-hypotheses were not admitted in the latter case, a sentence of the form " When pronouncing the statement S, the person A committed an error " would be meaningful or meaningless (not false !)—i.e. it would belong or not belong to scientific language—according to the words occurring in S ; and this would be so even if S itself (" I

<sup>9</sup> v. Juhos says that " while formulating a proposition such as ' I feel pain ', I know already whether I have made a true or a false statement " (l.c.p. 84). I think, that with equal justice, one could say : " When formulating a statement about the real motives of my action, I know already if I am right or wrong."

In quite a similar connexion, v. Juhos asserts that, concerning the falseness of contradictory statements, an error is impossible, and that somebody who pretends that a certain contradiction is true, must purposely make a false statement (l.c. pp. 89/90). From the experience I acquired when teaching mathematics at school, I think that on this point v. Juhos is certainly not right.



feel pain ") is a formally correct and meaningful statement of the language under consideration. This would obviously be very little expedient. Therefore, I think that scientific psychology would not exclude once and for all error-hypotheses concerning statements like "I see blue."

However, this question cannot be discussed here in more detail, because it is independent of the problems of physicalism.

(c) Von Juhos further objects to physicalism leading to such "absurd" "consequences" (l.c. p. 85) as the assertion that people who do not know the pain-state-criteria of contemporary psycho-physiology, do not mean anything when speaking about pains. This is "absurd," indeed, but it is *not* a "consequence" of physicalism. Physicalism only refers to the fact that all the methods employed in every-day life and by science for testing statements about feelings, thoughts, and such like of a man consist in examining his behaviour. In every-day life, one refers principally to such characteristics of behaviour as play of features, gestures, blushing, crying, etc. Scientific psychology, which has at its disposal many more empirical laws than every-day life, knows that those characteristics are empirically connected with other ones (e.g. certain nervous reactions) which, consequently, may likewise serve for testing the statement in question. But the introduction of new testing-methods does not render the older ones "meaningless" (though they may turn out to be less exact)—just as in physics the introduction of a new (e.g. electrical) method of testing a temperature-indication does not imply the rejection of the former methods (such as using a mercury-thermometer or even mere judging by touching the body in question) as "meaningless."

(d) Finally, v. Juhos gives an example (l.c. p. 88/89) meant to illustrate the possibility of testing certain psychological statements directly, i.e. without referring to the "bodily" behavior of the person in question. Without discussing this generally question here (it had better be put into the formal mode before being examined), I want to remark, that the example given by v. Juhos seems to me not to stand its ground. For how can the blind man (B) first know that his colour-experiences occur in regular connexion with those of the seeing man (S), and that they may therefore serve him in future for "directly" controlling S's colour-statements? At first, S must tell him one or several times, when he, S, is seeing blue. And B, when noticing that



each time he himself has likewise a colour-experience, may state a general law : Each time I have a colour-experience, S has one, too. And this law may serve him for further controlling S. But B would *not* be able to establish the law *without* knowing first the indications of S, which are at the same time *a part of S's bodily behaviour*.

Concerning (II). (e) It first has to be noticed that the conception of certain statements as unalterable or the stipulation that no statement of empirical science has to be maintained at all costs, is a matter of convention between scientists ; this has been accentuated particularly by Carnap.<sup>10</sup>

On the other hand, one may ask, which of the two possible conventions is in better congruence with the actual methodological attitude of our empirical science. As to this question, first Neurath<sup>11</sup> and Popper,<sup>12</sup> later joined by Carnap in their view, have insisted upon the alterability of *any* statement in empirical science ; but they do *not* pretend that " we are allowed to alter the propositions obtained by observation as it pleases us " (v. Juhos, l.c. p. 83) ; on the contrary : a statement which is once acknowledged (and in particular an observation-statement) can only be abandoned if it is incompatible with another statement which is confirmed by observation in a very high degree.<sup>13</sup>

(f) As to the self-observation statements, there exists at least the possibility of introducing a lie-hypothesis, and therefore it is possible, that even a statement of this kind should be abandoned. v. Juhos himself admits : " In stating ' I feel pain ' I may have said something wrong . . . because I wanted to lie " (l.c. p. 84). And this evidently invalidates his assertion that statements such as, I feel pain, " are unalterable, for the possibility of error concerning them does not existst " (p. 84). No : such a statement might be altered—at least by adopting a lie-hypothesis. As to the question of this being the only possibility, see (b).

Moreover I wish to emphasize that when discussing the

<sup>10</sup> See Carnap (8) : *Über Protokollsätze* ; *Erkenntnis* 3, 215 ; and (3).

<sup>11</sup> See e.g. Neurath (6) : *Protokollsätze* ; *Erkenntnis* 3, 204.

(7) : *Radikaler Physikalismus und "wirkliche Welt"* ; *Erkenntnis* 4, 346.

(8) : *Pseudorationalismus der Falsifikation* ; *Erkenntnis* 5, 353.

(Here, Neurath expressly accentuates that observation-statements—for which he proposes a certain common form—possess a "greater stability" (p. 362) than many other statements in empirical science : they need not be altered so often as the latter ones.)

<sup>12</sup> See K. Popper (1) : *Logik der Forschung*, Wien 1935. This book contains a detailed theory of the principles of testing in science and in particular an important and original account of the alterability of any empirical statement.

<sup>13</sup> For more detail, see particularly Popper (1).



possibilities of lie and error in this connexion, v. Juhos seems to start from a misunderstanding similar to that concerning physicalism (see (b)) : the adherents of the alterability of any scientific statement do *not* pretend that the alteration of the considered psychological statements must always be carried out by assuming an error ; the distinction between lie and error is quite independent of the problem in question.

On (III). (g) Finally, v. Juhos develops a very interesting objection against the proposal of employing the formal mode of speech<sup>14</sup> in epistemological discussion (l.c. pp. 90-92). He refers to a formulation given by me : " The system of protocol-statements which we call true . . . may only be characterized by the historical fact, that it is . . . actually adopted . . . by the scientists of our culture circle." Dr. v. Juhos translates this into the formal mode as follows : " the system of protocol-statements that we call true is characterized by the quality that certain protocol-statements belong to it, which assert that this very system of statements is acknowledged as true by the scientists of our cultural circle " (p. 91) ; from this remark, v. Juhos deduces the " objection . . . that an infinite number of systems of protocol-statements which are not to be contradicted might be quoted, all of which contain those particular statements, which characterize as true the system of protocol-statements of our science, but for the rest are incompatible with this system. And all these imaginable systems have to be considered true exactly in the same sense as our true science " (l.c. p. 91).

However, the translation given by v. Juhos is not adequate. The term "*historical fact*" serves to express a *reference* to that which is *acknowledged* as factual *by our science*. For the translation into the formal mode, we have to replace " It is a fact, that . . . " by : " The statement . . . is sufficiently confirmed by the protocol-statements adopted in our science." So, the following translation of the cited passage results : " The following statement is sufficiently confirmed by the protocol-statements adopted *in our science* : ' Amongst the numerous imaginable consistent sets of protocol-statements, there is in practice exactly one which is adopted by the vast majority of instructed scientific observers ; at the same time, it is just this set which we generally call true '." The whole is by no means a contradictory pro-

<sup>14</sup> See e.g. Carnap (1), p. 37 ff. ; (2), p. 68 ff.

(10) : *On the character of philosophic problems ; Philosophy of Science I*, p. 5 (1934).



position, nor can one deduce from it the consequences indicated by v. Juhos.

In the end, v. Juhos's objection furnishes us with a new example in favour of the proposal to use the formal mode of speech in logical discussion ; for, as we saw, my earlier material formulation caused a misunderstanding which is now, I hope, cleared up.

Brussels, December 1935.