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Ajdukiewicz's Theory of Meaning

Biographical Note. Ajdukiewicz was born on the 12th of December, 1890 in Tarnopol. Studying at the Jan Kazimierz University, Lwow, he found himself amongst the students of Kazimierz Twardowski, the distinguished psychologist and philosopher, and founder of the intellectual movement known today as the *Lwow-Warsaw School*. His teachers at university were also: Jan Łukasiewicz, the outstanding logician, widely known as the creator of many-valued logic; Marian Smoluchowski, one of the most distinguished Polish physicists and Waclaw Sierpinski, a mathematician of world renown.

His doctoral dissertation (*On the Relation of the Aprioricity of Space in Kant to the Genesis of the Spatial Imagination* - in Polish) - written under the direction of Kazimierz Twardowski - was devoted to the Kantian conception of time and space.

After obtaining his doctorate he went for a year to Goettingen, where at this time Edmund Husserl was preparing *The Idea of a Pure Phenomenology* for publication, and David Hilbert was developing his program of research into the foundations of mathematics. These contacts, especially that with Hilbert, had an undoubted influence on the shaping of Ajdukiewicz's academic development. Hilbert's views on the nature of mathematics found expression in the discussions which Ajdukiewicz included in his habilitation dissertation, *On the Methodology of the Deductive Sciences*, published in 1921.

During the First World War, he was awarded a medal for bravery as a soldier of the Austrian army. After Poland regained its independence, Ajdukiewicz took part in the struggles which the recreated country led concerning guarantees of its independence and borders. He left the army, with the rank of Captain, on the 2nd of November, 1920, seven months after his marriage to Maria Twardowska, the daughter of Kazimierz.

He returned to academic work in 1921, and so after reaching the age of thirty. In 1926 he became Professor at the University of Warsaw, but only two years later he moved to Lwow University, where he would stay until 1939 and so until the outbreak of the next world war. Like many teachers, university teachers in particular, he joined in with conspiratorial activity organizing underground teaching. After the end of the war he settled in Poznan, taking up work in the Adam Mickiewicz University, Poznan. In 1948 he was chosen as Rector of that university. He moved to Warsaw in 1956 and during the years 1956 to 1961 - and so until the time of his retirement - he was Director of one of the two departments of logic which existed at that time in Warsaw University.

He was the founder and the first Director of the Section of Logic of the Institute of Philosophy and Sociology, Polish Academy of Sciences. He was also a contributor and the first Editor-in-Chief of the journal *Studia Logica*. He took an active part in the activities of many academic organizations, both national and international. During the celebrations of the three-hundredth anniversary of the death of Pascal, the University of Clermont-Ferrand presented him with the title of Doctor, *honoris causa*. He was a member of the Polish Academy of Learning (Polska Akademia Umiejętności), later the Polish Academy of Sciences. He died in 1963.

\$1. Logician, Philosopher, Theorist of Language. Among philosophers Ajdukiewicz is often considered to be a logician. However, he was not a logician and he certainly was not a logician in the contemporary sense of the word. He did possess a vast knowledge of logic, he taught logic and was the author of an university textbook on logic and a monograph entitled *Pragmatic Logic*. But despite its title, *Pragmatic Logic* is a work mainly in the area of philosophy of science and only fragments of it touch on logical questions. Ajdukiewicz was closest to being a logician in his habilitation dissertation. For included in it are results representing an important step for logical research conducted at that time. In all Ajdukiewicz's publications his profound philosophical knowledge and philosopher's temperament become apparent. The nucleus of Ajdukiewicz's interests was the theory of language, a discipline lying at the crossroads of logic, theory (or, sticking to the traditional vocabulary for Poland, *methodology*) of science, philosophy, and of course, linguistics.

Ajdukiewicz was interested in exactly those aspects of linguistic communication which can be the subject of purely theoretical considerations, and therefore of research whose 'direct' object are not ethnic languages but 'theoretical models' which reconstruct the chosen properties of a natural language. A natural language and its theoretical model are two fundamentally different things. Their mutual dependence reflects as much the intentions of the model's creator, as the degree of adequacy with which the attributes of linguistic communication, which the model was to recreate, have in fact been recreated. From the formal point of view, the relations a model and the language modeled indicates the appropriate code of interpretation for the given model. Ajdukiewicz's interests in the problems of the theory of language coincided in a clear way with the dominant tendencies in philosophy at the beginning of the twentieth century. These tendencies included, first of all, the turn towards positivism as a doctrine limiting the fields of philosophical interests to those problems which can be formulated, and solved, through the application of the same linguistic means and research techniques which are used in science. Secondly, there was the discovery of logic as a tool of theoretical analysis, whose subject is the process of development of knowledge - scientific knowledge in particular. Linguistic communication is an immensely important element of this process, and at the same time logic, in a natural and direct way, is the source of theoretical models of various aspects of linguistic communication.

The philosophy of the Lwow-Warsaw School, of which Ajdukiewicz was one of the most distinguished representatives, not only remained in agreement with the above tendencies but, alongside the Vienna Circle, represented one of the main centers promoting 'scientific philosophy'. The work of Polish philosophers, and of Ajdukiewicz in particular, was an important element of the research led in accordance with the basic assumptions of the doctrine of the scientific nature of philosophy, at that time a doctrine gaining in significance and influence. This work was published in *Erkenntnis*, the theoretical journal of the Vienna Circle and presented at international philosophical conferences; and at the close of the inter-war period, it was made known through publication in *Studia Philosophica*, a journal created especially with the idea of presenting Polish philosophy in international languages. Due to all this, it became known in the world and had an important influence on the development of twentieth-century philosophy.

A separate question, which I do not intend to take up here, is to what extent was this 'scientific philosophy' a branch of positivism, 'neopositivism'; to what extent was it 'logical empiricism' and therefore a specific epistemological doctrine, which need not necessarily be connected with the positivistic world-view; and to what extent was it 'analytical philosophy' - a current in philosophical investigations based on the conviction of the purely linguistic nature of all philosophical problems. I cannot agree with Jan Wolenski (See Wolenski 1985), undoubtedly the greatest expert on the philosophy of the Lwow-Warsaw School, when he tries to see this philosophy as one of the branches of analytical philosophy. I consider this classification to be excessively narrow, and from a certain point of view simply confusing. One of the main theses of analytical philosophy was the claim of the distinguished role of natural language. Revealing the contents which have supposedly been preserved in natural language through generations of its development, getting rid of misunderstanding which gives rise to the misguided use of the

vocabulary of the language - these were to be particularly important tasks for philosophers. This conviction about the special role of natural language was not shared by the representatives of the Lwow-Warsaw School. And this conviction was certainly not shared by Ajdukiewicz, who amongst all the students of Twardowski most closely connected his interests to the problems of the theory of language.

\$2. Three Levels of Semiotic Analyses. The idea of creating a theory of language, as an area of considerations completing linguistic research, was born together with the development of logic. One can see this in many authors, especially those who, like Peirce, Frege or Russell, made an important contribution to the development of contemporary logic. But we can equally find this in the work of those linguists (e.g. de Saussure) worried by the one-sidedly empirical character of their discipline. Two conceptions form the theoretical background of this study.

One of these is the Fregean opposition of *Sinn* and *Bedeutung* - what an expression *means* and what it *refers to*. Frege's merit is not so much the distinction of sense (conceived, putting it most generally, as a way of identifying the referent appropriate for a given expression), and reference (conceived as assigning an expression its 'denotation'- and so the referent appropriate for a given expression). His achievement is not so much this as the creation of the conception of *extensional* languages i.e. languages in which effective linguistic communication does not demand familiarity with the meanings of terms - familiarity with the referents suffices.

The second theoretical conception comes to us from C. Morris' division of 'semiotics' understood exactly as theory of language in three research disciplines: *syntax*, *semantics*, and *pragmatics*.

Syntax, according to Morris, is that branch of semiotic considerations in which linguistic analyses reduce to 'structural' relationships between expressions. He has here in mind those relations to describe which it suffices to get to know the 'basics' of a given expression (e.g. the type and order of syllables of which it consists); on the other hand it is unnecessary to penetrate into the 'content' or, sticking to the terminology already introduced, the *meaning* of expressions. A structural property, in the sense here recalled, of the term 'cat' is e.g. that it is made up of the three letters 'c', 'a', and 't'. The structural relationships between the terms 'cat' and 'cathedral' are that the first term is the beginning segment of the second, that the first is shorter than the second and even that both are nouns. This last example may provoke some doubts, to which we must devote some attention.

Not without justification would a linguist point out that 'cat' and 'cathedral' are nouns not because they include such and not other letters, but because they serve as the signs of given things. And so it is their meaning and not their structure which decides about their belonging to this rather than another grammatical category. This remark ceases to be appropriate when we pass from research of languages in their natural form to research of 'structural grammars' serving as theoretical models of actual grammatical relationships. Structural grammars are grammars in which the syntactic categories of expressions are described without any consideration of the content of those expressions. When creating a language we decree that 'cat' - to recall the discussed example - and 'cathedral' are nouns (the term 'noun' is usually exchanged for another, e.g. 'single-argument predicate') which is synonymous with including them under the category of terms for which these and not other syntactical rules are in force. Such decisions permit the description of rules which delimit the permissible ways of building compound expressions in a given sentence.

Since we have already reminded ourselves about structural grammars, it is the right moment to note that the first structural grammar drawn up in a precise and complete way was Ajdukiewicz's grammar presented in 'Die syntaktische Konnexitaet', *Studia Philosophica* 1 (1936), pp. 1 - 27. The grammars of Lambek, Chomsky, Montague and many others, which today represent the standard research tools of 'mathematical linguistics' (the branch of the theory of language which looks to mathematics for its research tools)- these grammars were created significantly later. What is more, as was shown by the

course of related formal analyses, it turned out that they were equivalent to Ajdukiewicz's grammar (the equivalence of two grammars here depends on the translatability of assertions expressed on the basis of one of the grammars into a language of the second).

Ajdukiewicz's grammar is not only a universally acknowledged theoretical achievement, but is equally an achievement which became the point of departure for vast theoretical research. In particular Lambek's grammar represents a natural modification of Ajdukiewicz's grammar with interesting mathematical properties.

Semantics, in Morris' sense, is that branch of semiotics in which the object of our analyses are the meanings of expressions, including the relations connecting expressions with their referents. Running ahead a little, let us note that Ajdukiewicz's work in which he takes up the task of defining the concept of meaning is thought of as a bridge between syntax and semantics. Therefore Ajdukiewicz, as we shall see, looked for possibilities of defining the concept of meaning by invoking syntactic concepts, amongst others. Not only syntactic concepts, but, as we will discuss further, even when Ajdukiewicz's considerations go beyond the conceptual apparatus of syntax, he does not reach for semantic concepts. This is understandable - seeking a definition of meaning, Ajdukiewicz could not take advantage of terms whose sense depends on the term to be defined.

It's a significant fact that the earliest attempts to build a formal theory of language were at the same time attempts to limit semiotic considerations to syntactic discussions. This was connected to the positivist orientation of theoreticians of language (the early Wittgenstein, Carnap, and in Poland - though with many reservations - Ajdukiewicz), which required these thinkers to avoid all 'metaphysical' problems, understood as problems not decidable with the means used in science.¹ All questions concerning the existence of material reality, and therefore equally semiotic questions concerning the relations between language and its referents, were treated by the positivists as typical 'metaphysical' questions. To get rid of 'metaphysics' from his considerations, Carnap proposed to exchange expressions in the 'material mode' for expressions in the 'formal mode'. The essence of this idea was to exchange analyses of causal relations between states of affairs with analyses of formal relations (such as e.g. logical implication) connecting sentences, which, in the material interpretation, had represented a description of states of affairs.

Semantics as a fully legitimate branch of theory of language, was not created until Tarski, publishing his work devoted to the theory of truth at the end of the thirties. In the same period Carnap also began to reach the limits of a purely syntactical conception of problems of language. It is not surprising, therefore, that next to Polish philosophers (Kotarbinski, Ajdukiewicz, Łukasiewicz and others) he was one of the first to appreciate the path-breaking significance of Tarski's researches. Realizing the importance of the semantic perspective brought about a radical change of theoretical orientation of the thinkers of this period. Carnap published *Testability and Meaning*, a work of a decidedly semantic character and Ajdukiewicz abandoned the conception of radical conventionalism, which had been a natural consequence of his early semiotic investigations.

Since we have already distinguished semantics and syntax, let us note the following. In 'Die syntaktische Konnexitaet', Ajdukiewicz did not use the term 'syntactic category', on the other hand it was exactly with the meaning which we give that concept today that he used the term 'semantic concept'. This was not an accidental terminological shift. The division of expressions into syntactic categories has, as we have already noted, a semantic basis. By using the term 'semantic category', Ajdukiewicz accented the relationships which the duly constructed language must show, between its syntactical structure and the ontological structure of the domain which the referents of the linguistic expressions make up. Exactly this relationship between syntax and ontology became the basis for introducing the

¹ Ajdukiewicz's scientism never took an extreme form. He not only did not question the meaningfulness of philosophical issues, but was the author of several works counted as 'metaphysics' by the representatives of the Vienna Circle.

concept of a language *attached to* a given domain (to a given 'semantic model') which played an important role in the semiotic analyses of authors associated with Ajdukiewicz e.g. R. Suszko.

Pragmatics is in turn that branch of semiotics in which language is conceived as a tool of communication of a given group of people - a linguistic community. So pragmatic research includes all those investigations which relate properties of a language with the behavior of its users.

For a long period of time pragmatics was, to a considerable extent, the name of a discipline more projected than existent. Only since the fifties has this area of research had a speedy and intensive development. This research is led, however, by the assumption (fully natural if pragmatics is to be conceived as a theoretical discipline) that to understand the behavior of the users of a language, what is essential is that which can be conceived in some general schema, the same for all users of the language. And so the pronoun 'I' used by Peter refers to Peter, but when used by Anne refers to Anne. Though the referent of the term 'I' therefore changes in various contexts, the way in which this pronoun is used can be described with the help of a simple and fully universal semantic directive: 'I' used by person A refers exactly to that person A.

There is a certain paradox in the fact that a pragmatics thus constructed, reduces to semantics. To give semantic sense to the pragmatic assertions, it suffices to assume that a linguistic expression (a sentence in particular) is not only one or another conjunction of linguistic signs but a conjunction of linguistic signs together with a context in which it is used. In this way pragmatics becomes semantics of 'contextual languages'.

As we shall see when considering Ajdukiewicz's conceptions, we need a pragmatics which concentrates on linguistic phenomena which give birth to the relationship of the content of communication to context. But as well as that necessary are pragmatic analyses which take into account the fact that members of a linguistic community are not only users of a certain common means of communication, but are also its active creators. Perhaps the apt term for pragmatics thus conceived would be *diachronic pragmatics*². The distinctions discussed above are to represent the theoretical background of the considerations to which we shall proceed presently. Their main part will be a discussion of the theory of meaning presented by Ajdukiewicz in 'Sprache und Sinn', an article published in the journal *Erkenntnis* 4 (1934), pp. 100 - 38.

§3. Ajdukiewicz's Theory of Meaning - Meaning Directives. Ajdukiewicz's theory of meaning is a theory today almost completely forgotten. Still, it is a conception which with all certainty deserves attention and whose connections with many lively and important trends in contemporary semiotic research are both deep and far reaching. Before we try to understand why, in contrast to 'Die Syntaktische Konnexitaet' (counted amongst the basic works in the field of theory of language), 'Sprache und Sinn' belongs to those works almost never recalled, we must get to know the theoretical propositions contained in that paper.

The main problem with which someone who attempts to construct a theory of meaning is faced, is the question of what the meaning of linguistic expression consists in beyond reference. That expressions which refer to different objects cannot mean the same thing is obvious. Let's add: it is obvious on the basis of the purely intuitive sense we give to the term 'meaning' (perhaps Ajdukiewicz caught it most aptly with the definition - for use in informal analyses - of meaning as a 'way of understanding expressions'); it is obvious on the basis of the distinction introduced by Frege; it is obvious on the basis of the conception which identifies meaning with the content of a term (i.e. with a set of constitutive character-

² This is a reference to a conception, originating in de Saussure, which distinguishes between synchronic and diachronic theory of language. In contrast to synchronic considerations, diachronic considerations treat a language as a unit undergoing constant changes. Cf. F. de Saussure (1967 - 1971).

istics of the objects to which a given term refers); and it is obvious on the basis of many other conceptions of meaning. In any case it is obvious that meaning is not identical with reference.

If we wanted to identify the concept of meaning with the concept of reference, we would meet a well known difficulty. Ajdukiewicz recalls it in one of the opening paragraphs (see § 3) of his paper: that firstly an expression which does not refer to anything (e.g. 'glass mountain') nevertheless possesses a definite meaning and secondly, two expressions which refer to the same thing (e.g. 'the highest peak in Europe' and 'the highest peak in Switzerland') are not understood in the same way, and so do not have the same meaning.

In constructing his theory of meaning, Ajdukiewicz took up the following point of view: it is not the referents of expressions, but the rules by which we are lead - when considering sentences including a given expression - which decide about the meaning of that expression. This attitude, though surely not the same as, was definitely inspired by Frege's conception. But whereas Frege presented himself in rather general statements, Ajdukiewicz took up the task of expressing his intuitions in a way at once precise and complete. In presenting his conception I will limit myself to a sketch of its fundamental assumptions, trying to avoid entering into technical details, in so far as this is possible without importantly limiting the completeness of the lecture.

In Ajdukiewicz's opinion the acceptance of a sentence always depends on the - not always explicit - invocation of 'meaning directives' valid for the language in which the judgment requiring justification is articulated. And a *meaning directive* amounts to a rule of behavior, requiring the users of the language to accept a certain sentence in certain circumstances. Let us add: on the basis of the language in which it applies, such a directive not only requires acceptance of a sentence, but it makes that acceptance a justified one. Ajdukiewicz did not stress the connection between the concept of meaning and the concept of justification (he preferred to work with the concept of acceptance, without a clear assertion that we are here concerned with justified acceptance), but these connections are obvious. They are also an important element of the perspective in which we should conduct an evaluation of Ajdukiewicz's theory of meaning.

Ajdukiewicz distinguished three kinds of meaning directive:

4.1. The first group is made up of *empirical directives*. In Ajdukiewicz's conception these are rules of behavior with which we conduct ourselves when accepting sentences of a given kind as a result of certain psychological perceptions. These perceptions include both internal experience, introspection, as well as external experiences, that is perceptions of the senses.

4.2. The second group is made up of *axiomatic directives*. These are rules requiring 'unconditional' acceptance of certain sentences, and so acceptance without reference to any reason. We accept the sentence *If today is Tuesday then tomorrow will be Wednesday* on the basis of an axiomatic directive. We accept it, not because we have taken into account some reason which supports its acceptance, but because it is a *meaning postulate* i.e. a sentence whose acceptance is required by an axiomatic rule applying in the language. Of course, to one who insists on an explanation of the basis on which we have been convinced by the sentence mentioned, we can say, "because that's how the terms *Tuesday* and *Wednesday* are understood in English." But to give such an explanation is, in accordance with Ajdukiewicz's conception of meaning as much as to say: "Because in English an axiomatic directive applies requiring acceptance of all sentences of the form *If day x is Tuesday then the next day after x will be Wednesday.*" To refer to the meaning of given expressions is, according to Ajdukiewicz, to refer to the appropriate meaning directives.

Let us note at once that this line of thought leads to the problem of what determines what here: do the meanings determine the meaning directives applying to the language, or conversely do the meaning

directives determine meanings? The answer to this question depends on whether we treat the formulation of meaning directives as a procedure whose goal is to reconstruct a property already existing in the language (in this case the formulation of meaning directives should reconstruct already existing meanings), or, as a procedure which is but one element in the creation of the language. If the latter is the case, by formulating rules we determine the meaning of those terms to which the rules apply.

Let us return to the main trail of our considerations. For we have still to characterize one more group of directives - 'deductive directives'.

4.3. Deductive directives are directives requiring acceptance of a given judgment, when others were accepted earlier. So, if e.g. we accept that today is either Sunday or Monday and then, in some or other way, we realize that it is a weekday and so not a Sunday, we must accept that today is Monday. In a fully general schema the above mentioned directive is so-called *modus tollens*: from *A or B* and *not-B* conclude *A*.

\$4. Ajdukiewicz's Theory of Meaning - Definition of Synonymy. For a directive to concern an expression *W* 'in an essential way', it does not suffice that it can be used to justify a sentence including the expression *W*. Such a directive might not distinguish the expression *W* from other expressions of the same grammatical category, e.g. the axiomatic directive requiring the acceptance of all sentences of the form $a = a$ does not distinguish any name which *a* might represent from any other name from the same set. The only term which this directive concerns in an essential way is the symbol for the relation of identity. In the sense here recalled, the directive *modus tollens* concerns the word *or*, the word *not* and it does not essentially concern any other word.

So, is the meaning of the expression *W* given by all the directives which concern it in an essential way and which are furthermore formulated in a 'fully general' way? The requirement that our attention concentrate on rules formulated in a 'fully general' way cannot be left out when answering this question. If we were to formulate the principle of identity $a = a$ in the form of a series of particular principles referring to various kinds of objects: to towns (*a* might be limited to towns), to people (*a* might be limited to people) it would turn out that each of these particular principles essentially concerns not only the sign of identity but all the expressions of a given kind (e.g. all the names of towns). We are here concerned with a certain problem - the problem is to explain on what this 'full generality' depends - and Ajdukiewicz solved this problem in a particular way. But to understand the basic assumptions of his theory, we need not penetrate into this rather technical question.

Let us return to our opening question: is the meaning of the expression *W* given by all the directives which concern the expression in an essential way and are furthermore formulated in a 'fully general' way? Not completely. For it can be that the meaning of the expression *W* depends on the meaning of another expression *V*, e.g. in the sense that both these expressions appear in an essential way in the same directive. That a cat is an animal, we may consider a meaning postulate - thus the term *cat* and the term *animal* turn out to be semantically related. Next, it is obvious that the relation between expressions may be either direct or indirect: if *W* is semantically related to *V* and *V* to *U*, then *W* is semantically related to *U*, though not necessarily directly. If we now remember about the relationships of the kind here described, it becomes clear that the meaning of the expression *W* is given not only by the directives in which the expression appears, but also by those directives in which appear expressions semantically related to it.

All such expressions and directives related to them together create a certain well demarcated fragment of the language - Ajdukiewicz calls this fragment *isolated*. So, if the language under consideration includes some expression *V* which does not appear in this fragment, then that expression does not enter into any semantic relationship with expressions of the distinguished fragment.

We can imagine languages falling into many isolated fragments not connected with each other. A language is *coherent* if none of its proper parts is an isolated fragment. As we might expect Ajdukiewicz's attention is concentrated on coherent languages - non-coherent languages are conglomerates of various languages. Nevertheless many of the analyses completed by Ajdukiewicz concern both coherent and non-coherent languages.

If in a language **J** we replace the expression *W* everywhere with the expression *W'*, thus creating a changed language **J'**, it is 'clear' that *W* in **J'** will mean the same as *W* in **J**. The sense of clarity, to which we have here referred, demands a certain reflection. The assertion about the synonymy of *W* and *W'* appears here as a consequence of the given way of constructing the language **J'**. On what is this replacement of *W*, appearing in **J**, with *W'* to depend? If simply on this, that we decree that *W'* is to take over all the properties of *W*, and so also its meaning, then obviously it turns out that *W'* is synonymous with *W*. Ajdukiewicz's conception of meaning allows us to assert something decidedly less trivial. Replacing *W* with *W'*, in accordance with this conception and in such a way as to ensure the synonymy of the two expressions, amounts to replacing *W* by *W'* in all the meaning directives. Therefore, the meaning determines the meaning directives.

If this is so, the concept of the synonymy of two expressions can be identified with the complete interchangeability of these expressions in their meaning directives. Putting the matter a little more strictly: two expressions *W* and *W'* of a language **J** are *synonymous* in this language if their exchange of places in their meaning directives transforms these sets of directives into (respectively) equivalent sets. I have not here quoted Ajdukiewicz's definitions in their original formulation. However, I hope that, while allowing myself a rather loose presentation of them, I have preserved their sense at the same time avoiding the troubles of defining a series of subsidiary terms.

From the logical point of view defining synonymy is identical with defining the concept of meaning; we can, through so-called definition through abstraction, identify the meaning of a term *W* with the class of all terms synonymous with *W*. However, omitting the purely logical aspects of the discussed relationship, it is clear that if we know when two terms have identical and when they have different meaning, we thus possess a certain basic knowledge of what meaning is.

So giving a definition of synonymy would finish the work, for the goal of the considerations taken up by Ajdukiewicz was exactly the definition of the concept of meaning. Now, there has been a rather significant number of philosophers supporting the thesis that the meaning of expressions is given by the ways of verifying sentences³ appropriate for a given language - let us add: there is a not insignificant number of philosophers of language who tirelessly support this thesis until today. In contrast to them, Ajdukiewicz went to the trouble of going beyond generalizations and of specifying on what the relationship between meaning and justification is to depend. The results of this scrupulousness were for him damaging.

\$5. Tarski's Criticism of Ajdukiewicz's Theory of Meaning. Tarski pointed out to Ajdukiewicz that even though in some language, two expressions *W* and *W'* are indistinguishable by the appropriate meaning directives of the language, despite what we might expect, this fact does not at all guarantee that *W* and *W'* have the same reference. This will not be so in the case when e.g. among the meaning directives we find the axiomatic directive $W \neq W'$. This directive does not reduce the symmetry of *W* and *W'* as far as the meaning directives are concerned, but it excludes the possibility of using *W* and *W'* as signs of the same objects. The example given by Tarski was thought of as an example proving that Ajdukiewicz's theory of meaning does not fulfill the hopes its author held for it. Regardless of the goals this example serves, the thesis that meaning directives (understood as Ajdukiewicz wanted) cannot demarcate referents in an unequivocal way is worth our attention in itself.

³ The slogan 'the meaning of a sentence is its method of verification' appears in different versions in the work of the representatives of the Vienna Circle.

The basic question to which this observation gives rise is of the nature of the revealed difficulty. The reason for the lack of the expected relationship between justification and reference might be the irreducibility of the concept of justification to meaning directives, in Ajdukiewicz's sense. Perhaps the understanding of justification demands reference to something else alongside meaning directives, understood as Ajdukiewicz wanted. The reason might also be the irreducibility of the concept of meaning to the concept of justification. So we would then have to assume that familiarity with the procedures used in justification of sentences does not mark out the meanings of those sentences nor the meanings of the terms from which they are built. Whether or not this is so, the opening intuitions, on which Ajdukiewicz built his theoretical constructions demand radical rethinking.

The question around which our next considerations will revolve, is the question in what way, in the process of the development of a language, it happens that terms are assigned their referents. The considerations which we will conduct will be neither exhaustive nor sufficiently penetrating. The considerations which I will try to develop will be conducted in connection with Ajdukiewicz's conception, and their main goal will be an analysis of both the limits and virtues of that conception.

In independently published papers, S. Kripke and H. Putnam have constructed a so-called causal theory of reference. Since the question which I intend to take up belongs to exactly the area of problems which concerned these two authors, I will refer to their conception on various occasions. I will do this trying to limit my presentation of their ideas to certain key theses, and avoiding the rather sophisticated conceptual apparatus they use. In any case I do not think that the concept of a possible world or the concept of 'transworld' identity - 'transidentity' - would be a necessary element of an accurate account of the behavior of the users of a language (both these concepts play an important role in the considerations of the creators of the causal theory of reference.)

Before we take up these considerations as promised, let us consider a little further the following: To what branch of semiotics does Ajdukiewicz's conception belong? The attempt at classification should cast a little additional light on the nature of this conception, facilitating its further development. The following observation has a key significance: in Ajdukiewicz's conception of meaning the user of a language is treated as a machine - a robot which picks up impulses from its surroundings (it registers changes in input), analyses them and enters the given data into its memory. The situation which the user of the language identifies is the equivalent of these impulses. The empirical rules are the equivalent of the counting algorithms which the machine uses to assign the gathered data to the received impulses. Finally the sentences accepted as a result of this application of empirical rules are the equivalent of this data.

Doubtless the machine does not carry out any semantic analyses - it does not consider any semantic relations connecting data and the states of affairs (changes of input) corresponding to it. The machine does not think, it only realizes the algorithm input by the programmer. I will term exactly this kind of behavior, *procedural*, regardless of whether this is the behavior of a machine or a person. Ajdukiewicz's conception of meaning has, in the sense given here, the character of a procedural conception. It is not a purely syntactic conception, because the directives specifying the behavior of the users of the language are defined in reference to states of affairs, as well as expressions. However it is not a semantic conception, because in setting it up we do not refer to any analyses of content - we do not demand from the user of the meaning directives any consideration of the meaning of expressions included in sentences which she accepts in the given situations.

Tarski's example seems to prove its point without leaving room for doubt that a conception of meaning cannot be a purely procedural conception. So in what direction should the development of of this conception proceed to satisfactorily explain what understanding of linguistic expressions is based on? In

other words, what decides that they mean such and such for the users of the language, and not something else?

\$6. Cognitive Schemes. Our arguments will certainly gain in clarity if, instead of conducting them in a general way, we concentrate on a chosen example. Let's consider how the users of the term 'Warsaw' (thought of as the name of the present capital of Poland) connect this term with the referent corresponding to it. Our goal will not be to open the history of the appearance of this name, but rather what it amounts to that for the users of English, 'Warsaw' is the name of just this and not another object. The term 'Warsaw' need not be tied to the English language - the same name could be used by the speaker of any other natural language. So it does not seem that our projected analyses should be conducted with a concentration on some specific language. On the other hand to go beyond the borders of general considerations and take up some particular questions might impose a relativization of our considerations to a specific 'linguistic community' - in our case the specific group of the users of the name 'Warsaw'. The knowledge and experience of the users of the term determines their linguistic behavior - in our case this is their recognition of whether or not a given use of the term 'Warsaw' is correct. So the considerations which we intend to take up lead us into the area of pragmatics.

To competently work with a language including the names of places, we need to possess a certain basic topographical knowledge, taking the form of a system of beliefs and ideas. The elements of this knowledge may be specific ascertainments e.g. that Warsaw is situated on the Vistula or that the building situated in the center of Warsaw is the Palace of Culture. Facts known by one of the users of the term 'Warsaw' may be unknown to another, but despite this their ways of using this term, and generally the names of cities, need not cause difficulties in communication. The condition of effective communication is to a lesser degree familiarity with these or other facts than agreement on basic topographical ideas. The system of these ideas creates a certain *cognitive scheme*, to use a concept known from cognitive science⁴. With regard for its character we can call it a scheme of *topographical relations*, or *topographical scheme*, for short.

What comprises the elements of a topographical scheme thus understood? Above all ideas of 'topographical space' - of all the possible 'topographical regions' in which the various elements of topography may be located: hills, rivers ('waterways'), lakes (water reservoirs), vegetation of given kinds, buildings etc.. These regions are situated in relation to each other in a certain definite way, so for example one lies to the left or right of the second, to the North or East, farther or closer and so on. These relations are the next examples of elements of the scheme. With time, the objects situated in the discussed region of topographical space are subject to transformations. These transformations determine the next list of topographical ideas. The transformations are either gradual (e.g. water rising in a river), or sudden (e.g. destruction of a flood dam). What is more important is that observing the topographical changes demands from the observer the ability to distinguish two situations: firstly when we are concerned with the same object, despite the changes taking place (e.g. a river which rises is still the same river); secondly when the result is the disappearance of certain elements of the topography (e.g. a dam is destroyed by a flood) or the appearance of other elements (e.g. a new river bed).

What are these 'topographical ideas' about which we have just spoken? It is a lot easier to describe the function they fulfill than their ontological nature. Speaking most generally, they are means of spatial orientation: mental models of objects which occur or may occur in reality; arrangements of actual or possible relations between such objects; and also certain beliefs about the limits to which the actual behavior of such objects are subject or may be subject. Those topographical ideas fulfilling the roles of models, may be compared with graphs, maps photographs, mock-ups, drawings etc., etc... The belief elements may be compared to fragments of texts, thus to objects whose natural mode of presentation is

⁴ I will use the term 'cognitive scheme' under the guidance of the intuitions which were expressed in authors dealing with the issues of the representation of knowledge. Cf. Hayes (1974).

the sentence. So, we need to distinguish among the elements of cognitive schemes: *iconographic ideas* and *presuppositions*. Cognitive schemes are made up of both of these.

The point at which we find ourselves is perhaps the right one for a short word about the relationships which we can find between the concept of an cognitive scheme and the concept of a possible world, in the understanding which Kripke has given possible worlds. We may make transformations in an actual topographical space, a physical region, the effect of which is a shift in it, getting rid of some of its elements, adding others, however in such a way as to maintain all the relations which the elements of the topographical space must exhibit, in accordance with the presuppositions of the scheme. An object thus constructed will be a *possible world*. So an cognitive scheme can be conceived as a specific 'frame' serving the construction of possible worlds.

The ideas representing the basis of systems of communication certainly have foundations in the nature of the species - they are to some extent biologically conditioned. Comparing the spatial orientation of man with that of other species reveals these differences in a way not leaving room for doubt about this. Researchers of bees have managed to decipher the means of communication of these insects, because they have established what directs the bees by discerning the locations of objects important for them. Knowledge of the scheme of topographical relations permitted the understanding of the system of signals used by these animals - the bees' language. Perhaps there exist beings, perhaps at a level of intelligence not lower than the human, for whom the 'finding of a common language' would turn out to be impossible. It is not sure whether man is able to find out all the regularities which govern the course of natural phenomena and which are the basis of living creatures' orientation in the world, thus their communicative systems.

These ideas are also to some extent a product of civilization - we possess them thanks to the abilities and knowledge passed on from generation to generation. Finding a common language may be made difficult not only by inter-species barriers but also, as we very well know, by civilizational barriers. We may not only prove unable to correctly interpret the signals which others species use - we may be unable to interpret the languages used by the representatives of other cultures. But there is unfortunately something deceptive in these obvious yet sketchy assertions.

\$7. So-called Cultural Differences. One of the basic traits of postmodern philosophy is an emphasis on the significance of cultural differences and their influence on the process of communication. Since we have already taken up this question, let's take note of the following. Firstly, cultural differences need not in the least create unconquerable obstacle to mutual understanding. Nothing indicates that (the basic level of) our contemporary topographical ideas would be out of agreement with the ideas of people living in other epochs. Elementary topographical ideas - 'topographical space', 'topographical region', 'on the right' and 'on the left', 'further - nearer', 'water reservoir', 'hill', 'region inhabited by man', 'group of trees' and many others - are certainly common to all the representatives of the species 'Homo sapiens', from the moment of the appearance of the species.

That the ideas of contemporary man are, from certain points of view, richer than the ideas of people of other historical periods, and from other points of view, poorer, does not change the fact that their elementary part (that particularly connected with the biological character of the species) is practically unchanging. There is no reason to suppose that our elementary topographical ideas differ from the ideas occurring in the representatives of those species of animals whose means of orientation on the ground is similar to that of humans. Knowledge of animal behavior may certainly be a good source of knowledge about man. The thesis of the constancy of rudimentary ideas may be based on exactly such considerations.

Besides overrating the significance of inter-cultural differences, postmodern philosophy fails to fully appreciate the significance of intra-cultural differences, individual differences in particular. Coperni-

cus' positioning of the Sun in the center of the planetary system represented a dramatic transformation of contemporary ideas (the concept of planet changed its meaning, the concept of the backward movement of the planets lost its sense, and above all the Aristotelian distinction of two spheres lost its sense too: the sub-lunar and that of the eternal stars). So it was a proposition rejecting an old cognitive scheme, and not merely the exchange of one description of the movement of the bodies of the planetary system, for another. This change was not compelled by any newly discovered facts. There is also no basis for searching for the source of the Copernican revolution in any cultural particulars of Copernicus' epoch. It is not the cultural transformation which explains Copernicus' work, but conversely - Copernicus' work represents one of the essential factors stimulating this transformation. Copernicus' theory foretells the close of the Middle Ages and the development of a new period: the Renaissance.

To show the differences of individual cognitive schemes, we hardly need examples as spectacular as Copernicus' system. No-one has any doubt that in every area the ideas of professionals are different from the ideas of layman; the ideas of people possessing particular insight (e.g. the inborn ability to correctly interpret others' behavior) are different the ideas of one who doesn't possess such abilities, etc., etc..It's an uncontroversial matter that differences in ideas- and in the cognitive systems determined by them - may represent an important obstacle in shared communication, regardless of whether these are differences between people belonging to different cultures or between individuals belonging to the same cultural formation and yet differing in their ways of perceiving things. Intra-cultural differences can be stronger and more far going than inter-cultural differences.

I have drifted very far from the discussions which are the direct object of our interest, but not without reason. The purpose I had was to emphasize the theses that the basis of linguistic communication, within the confines of a specific community, is the existence of common ideas for the given community and common cognitive schemes based on those ideas. The more numerous or the more differentiated this community is the greater is the probability that these common ideas exist only at a rudimentary level. And these ideas may not suffice for the correct reception of a communication based on ideas richer, more refined or simply different. This is exactly why the process of communication does not, as a rule, reduce to the invocation of common ideas, but, rather, to a significant degree depends on the 'search for a common language '. Communications directed towards a specific person must either be so built that their intended sense would be intelligible to the receiver with his linguistic habits (cognitive schemes mastered by him), or, these communications must include an 'interpretative key' as an essential element. Some part of the utterance may not carry any information connected with the exact content of the communication, and may only give the recipient indications necessary for the correct interpretation of the communication.

Let us return for a moment to our example. Warsaw is not only a 'topographical unit'. It is also an 'administrative unit'. We might fear that without reference to administrative criteria we will not manage to mark out the borders of Warsaw and thus to demarcate Warsaw from the places surrounding it. Doesn't that mean that that the correct interpretation of the term 'Warsaw' demands, alongside topographical ideas, familiarity with administrative relations: possession of a certain conception of an administrative division and a certain conception of the relations which occur between elements of such a division.

A moment's thought suffices to realize that that administrative concepts do not play any role if we treat the problem of defining the borders of Warsaw as a purely semantic problem, and if we therefore ignore its administrative implications and conditions. So to correctly use the term 'Warsaw' no conceptual apparatus which would make up an 'administrative scheme' is necessary. It is necessary, on the other hand, to treat certain persons (institutions), as authorities for the marking of the city's borders, and thus for determining the referent of its name. If such institutions do not exist or do not exercise the prerogatives to which they are entitled, the city may not possess any clearly defined borders. The *topographi-*

cal scope belonging to the name of this city, i.e. the collection of all regions which lie within its borders, may remain blurred.

§8. Terminological Conventions. So, how can we assign the term 'Warsaw' to Warsaw? Ajdukiewicz's conception leads to a quite different answer to this question than do the considerations of Kripke and Putnam. In their opinion the connection of 'Warsaw' with Warsaw began when the place known until today as Warsaw, was thus 'christened'. Kripke and Putnam treat exactly this act of naming as an act which once and for all decides the assignment connecting the individual object and its name. The formal expression for this fact is the 'transidentity' of Warsaw: in all possible worlds the name 'Warsaw' refers to the same object. Next, this time following in Ajdukiewicz's footsteps, we should say that the assignment of 'Warsaw' to the city which this term refers to, is carried by the acceptance of such and not other meaning directives. Each of these conceptions includes a correct element, yet both are - as I will try to show - from certain important points of view, problematic.

I should perhaps justify my intention of taking up - very briefly after all - the Kripke-Putnam theory. The justification is obvious: though our interests are concentrated on Ajdukiewicz, coming to realize the limits of his conception, and at the same time some of its virtues, will be easier if we go through the virtues and faults of an alternative conception.

The problem which confronts the supporters of the 'act of naming' conception, as an act which sets up a permanent obligation for all users of the name (it connects the name with the object in all possible worlds), is the problem of the changeability of things. The object, taken as the referent, changes its properties with the course of time: its shape, internal structure, the relations which connect it with its surroundings, etc.. However, if the object changes, the question which we cannot avoid is the question: are we still concerned with the same object or is it perhaps a quite different object to which the old name no longer applies? Before we take up this problem, let's have a closer look at the 'ceremony' of naming itself.

Despite the well known conception of ostensive definition (or perhaps: despite a certain excessively simplified interpretation of this conception), naming an object is not exhausted by the physical act of pointing to an object and simultaneously uttering the formula: 'I name thee N'. If during the times when faraway countries were still undiscovered, a brave captain who was sailing up to a new land declared, indicating with the movement of his hand the emerging outlines of land, I name this land 'The Isle of Saint Anne', in honor of our patron saint,' then for all those who do not doubt the semantic rights of the captain, the discovered territory will bear the name 'The Isle of Saint Anne'. However with an important reservation. This 'Island' must be an island. If it is not, then the captain's gesture did not determine which part of the land is to bear the given name.

The moral of this story is the following: the demonstration of an object is not carried out by a pointing gesture, but rather through a procedure which is comprised of two things: the demarcation of a certain class of objects (here it is the class of islands) and the distinguishing from just this class, the exact object which we are concerned with. This last must be done in a way understandable for others. This second component of the christening ceremony need not demand the demonstration of the object in its entirety. Those who understand the word island, know which land the captain has in mind, though they see only a small fragment of it. Naming is always connected with the invocation of the appropriate cognitive schemes; naming topographical objects demands reference to topographical ideas. The case of 'Warsaw' is similar. Let's imagine that the legendary ruler, Piast Kolodziej, of prehistorical Poland, having climbed a hill with his company, pointed to a set of buildings situated below by moving his arm and said: 'It is our will that this be called Warsaw'. I hope the reader will forgive these historical and topographical confabulations. If, however the story has described actual events, the chronicler of the ruler could not know whether the word 'Warsaw' covered only the city or the city together with the surrounding boroughs.

The island and the city acquired their names. What next? The island is exposed to action of the ocean waves and tectonic tremors. The city grows, new fortifications appear and port areas are extended. At other times it depopulates, is destroyed by fires and the overflows of the Vistula. All these events seem not to spoil the transidentity of our objects. Is it really so? It is after all possible that in one of the possible worlds, the name 'Warsaw' is transmitted to a city built near the old Warsaw, destroyed by flood. In a different world the name 'Warsaw' is still born by the remains of the old Warsaw and the new city is called 'New Warsaw'. Similarly with the island. It was broken in two by the tremors. Since one of the parts is smaller, in one of the possible worlds it is treated as a part of the main island whose name hasn't changed, but in a different world it was decided that the Isle of Saint Anne ceased to exist and two new islands took its place. And these were called Big and Small.

The natural order of the course of events (it's exactly an invocation of this which justifies the name of the Kripke-Putnam conception - *causal*) does not exclude something happening which breaks the continuity of semantic behavior based on that order. The unclarity which emerges will have to be removed with the help of new, often contingent, terminological conventions.

\$9. Meaning Directives. The questions which we have considered have also another aspect. In both the case of the island and the city, the correct application of the names of these objects demands certain specific knowledge about these objects, in other words familiarity with their spatial borders. This is knowledge of a special kind, especially in the case of the city which is a human creation. To correctly operate with the term Warsaw, we need not know whether it lies by a river, or not, or how many bridges connect its left and right banks. In contrast to these and many other properties of Warsaw which we do not treat as essential for the correct understanding of the term 'Warsaw', we treat its shape as a constitutive characteristic and thus one of those which decides about the correct understanding of the term.

The criterium distinguishing certain characteristics of Warsaw as constitutive is the ease with which they are grasped. That Wilanow is (presently) a part of Warsaw (it was not always), and Konstancin is not such a part is not - in contrast to knowledge about the river and bridges - empirical knowledge. No ground research explains this fact. The beliefs mentioned here owe their justification to the decision of the fathers of the city (or in any case some institution empowered to determine the city's borders). They do not owe their justification to some or other physical characteristics of the conglomeration made up by Warsaw together with its surroundings. The (mediaeval) problem: was Warsaw just the city or also its boroughs, has its contemporary counterpart.

Ajdukiewicz's conviction that the meaning directives determine the referents of terms is clearly an intuition to some extent correct. It is not groundless even with respect to individual name 'Warsaw', thus to one of the type of names which become signs of these and not other objects as an implication of a terminological declaration: the act of naming.

The meaning directives, to which the user of the name should conform if he is to respect the official meaning of this term, are the directives requiring acceptance of sentences of the form:

The present borders of Warsaw are such and such.

So these are axiomatic directives and precisely articulated in the language with which we operate in the relevant documents. The demonstrative adjective 'present' needs to be replaced by a clear description of the period of time corresponding to it.

In order that the legal decisions determining the borders of Warsaw (these are the basis of the axiomatic directives discussed above) help geodesists, marking out the borders on the ground, these deci-

sions must be expressed with the help of concepts used by geodesists. In particular they must determine the arrangement of the referents relative to which refer the positions of topographical points and their mutual spatial orientation.

Of course, not only does the borders of Warsaw have its definition but also the topographical concepts necessary to their description have theirs. And so the way in which these concepts should be applied regulate the appropriate meaning postulates. However with certain exceptions: Some of the concepts discussed here refer to individual objects, serving as points of orientation. This could be the Polar Star or selected points on the ground, from which geodesists start marking out a distance-grid. Their description requires ostensive procedures (procedures including as an essential element the demonstration of the defined object). In other words, to construct an arrangement of concepts necessary for defining the borders of Warsaw requires us to go beyond a series of purely linguistic descriptions. Could that extra-verbal part of the terminological constructions, in the course of the appropriate theoretical analyses, be treated as equivalent to the existence of certain empirical directives? Let us put this question aside for the moment.

The accuracy - by the applicable contemporary standards, both geodesic and legal - with which the borders of the city are to be determined means that the rudimentary scheme of topographical relations does not suffice. We have to refer to concepts of Euclidean geometry and its postulates. Then operating with the postulates (all of those we have mentioned) and operating with the results received from geodesic measurements demands an appropriate system of deductive directives.

The step by step analysis of the procedures necessary to determine the borders of Warsaw (and thus the object which the term 'Warsaw' refers to) - invokes all three kinds of meaning directives which Ajdukiewicz distinguished. Ajdukiewicz's conception is not then a purely theoretical construction having no equivalent in actual linguistic practice. The discussed example shows certain essential relations which connect his conception to actual semantic procedures. However the nature of these relations requires more precise description. In our further analyses, we will devote special attention to the role of empirical directives.

\$10. Diagnostic Indicators. It would be a misunderstanding to imagine that operating with language can always be reduced to constructions as perfect as contemporary acts normalizing territorial relations. It is surely not so. Let's have a look a situation in which the precision characterizing rigorously described topographical systems is an unobtainable ideal.

Let us look at the example discussed from a slightly different point of view. Instead of asking how (on the basis of the relevant legal documents and professional knowledge) the geodesic authorities mark out the borders of the city, let's ask how a user of the language operates with the term 'Warsaw' lacking both copies of documents and theodolite. Which procedures allow him to accept or reject assertions demanding knowledge of the city's borders, such as, e.g. verification of assertions of the form *Here is (here was) Warsaw?*

Apart from particular circumstances, in a situation described here we do not refer to any historical sources or currently applicable legal documents. We refer rather to knowledge, gained in the past, usually in circumstance that cannot be recreated. We owe it's greater part to participation in *the social transmission of information*. It consists of things we once heard, read, observed (when the means of passing information is a picture), and also what we have managed to infer from information gained in this way. We also acquire knowledge about the borders of the city by studying maps of the city, reading newspapers, recalling signs on main roads read when leaving Warsaw and probably by many other means. Buildings, streets, squares which we find in the city are recognized by comparing them with what we once saw, often only in the form of a reproduction, or even comparing a picture with a remem-

bered description. So we reach the resources of ideas we possess: iconographic models and presuppositions.

It's always an open question how far we can be sure of the adequacy of the ideas to which we refer. For the iconographic models we possess can be imprecise, and the presuppositions may be false. It is a separate problem which I wouldn't like to discuss, though it is a particularly significant problem. In contrast to Ajdukiewicz's meaning directives, which by definition ensure the correctness of all the reasoning based on them, cognitive schemes do not ensure such correctness. If our ideas are faulty, the inferences based on them may also be mistaken.

However, in spite of this it doesn't seem to me that the use of linguistic expressions necessitates the existence of established empirical directives binding all the users. If linguistic utterances are to be correlated in some way with the states of affairs which they describe, the users of the language should be able to use the appropriate - let's bring in this term - 'diagnostic indicators'. A *diagnostic indicator* of an object Q amounts to a certain fragment or aspect q of the object Q which allows a properly trained observer to recognize it. The voice of a person hidden from us, her way of moving, the specific arrangement of her fingerprints (to remind us of reading detective novels), and, of course, their appearance, these are only a few widely known diagnostic indicators applied when we recognize people. There are many more of such indicators, and if we don't confine our observations to 'direct' observation (i.e. allowing the use of instruments, use of testimony, experiment) there will be, practically speaking, an unlimited number.

In the cases in which linguistic communication serves mutual understanding of matters being the possible subject of empirical verification, efficient communication does not demand operating with commonly accepted empirical directives. It is enough if the partners in communication refer the terms they use to the same objects. If X says: 'Here is Warsaw' (he recognizes a certain fragment as a fragment of Warsaw), then for this communication to be correctly understood by Y, it's not at all necessary for X and Y to know any empirical directives. It's completely sufficient that the criteria which X and Y apply in deciding whether a certain area or object is within the borders of Warsaw are not mutually contradictory. X need also not know what indicators Y makes use of and, conversely, Y need not know X's indicators. Furthermore the ranges of applicability of X's indicators need not be identical with the ranges of Y's - a fragment of Warsaw known to X, need not be known to Y.

Linguistic communication whose basis is shared topographical ideas and *consistent* (in the sense that their applications do not result in contradictions), though not necessarily common, topographical indicators will be conducted without conflict. Problems only appear if the interlocutors begin to operate either with different ideas or inconsistent indicators. If the findings of X and Y contradict each other they must compare their methodologies and check if, through the revision of one or both of them, it is possible to get rid of the discrepancies which have arisen.

\$11. Reference Without Meaning. If we stand by the Fregean intuition, which requires that to know the meaning of a term we must understand the way in which the users of the language determine the referent of the term, then our observations seem to lead to the assertion that effective communication need not at all demand from speakers an identical understanding of the terms used by term (giving them the same meaning). It is enough if they ascribe to them the same referents. This is actually, however, quite obvious. Our intention was to say something more. Let's invoke a slightly different example.

According to Kripke and Putnam a tiger is called a 'tiger' because once this kind of animal was thus named. Natural kinds are, alongside individual objects, another example of transidentical objects - they are the same objects in all possible worlds. At the same, as the authors mentioned managed to show convincingly, all the proposed definitions of the tiger are nothing other than certain lists of empirical indicators facilitating the distinguishing of tigers from other animals. If we imagine tigers being the

product of some different, peculiar, genetic transformations, it turns out that a tiger does not have to be a big animal - there could be a small one, it does not have to be striped - there could be a spotted one and it does not have to be a predator - there could be one eating grass. Every definition could, in certain circumstances, be incorrect and seen to be so. So, it could turn out that it does not include 'tigers' living in a certain, previously unknown, region, 'tigers' which once existed or appear in the future or - these authors use the method of thought experiments with great enthusiasm - 'tigers' living on 'twin earth'. In short „everyone sees what a 'tiger' is” , and with that finishes the definition of a tiger.

The implications which are drawn from these imaginative exercises by Kripke and Putnam do not necessarily agree with those I am trying to formulate. First of all, the fact that mental experiments allow us to change a tiger beyond recognition, only bears witness to the fact that in our (contemporary) cognitive scheme of the animal word is a built-in belief of the great plasticity of kinds. This is exactly why we are disposed to treat the proposed definition of tiger as a definition corresponding to the actual state of knowledge. Secondly, this naturalness of the kind *tiger*, which is supposedly to ensure that a tiger is a tiger in every possible world, is nothing other than existence of indicators which allow us to identify tigers in a highly consistent way. These diagnostic indicators - regardless of whether we use the same ones or not - do not have the character of definitions. This is exactly why our cognitive scheme does not exclude the existence of tigers with quite different characteristics.

We do not know how the future will go for tigers, or to what changes this kind will succumb, if it manages to survive. However, this does not have great significance for contemporary ways of using the term. We can describe these in the following way. The term 'tiger', within the scope with which it is used today, possesses well determined referents (because the diagnostic procedures with which we operate are consistent). At the same time there does not exist any official meaning directives which would determine the sense of this term, or even the scope of this term, and which could be treated as definitive. Let's spell out the point: the term 'tiger' refers to a specific kind of animal, yet at the same time this term does not possess any universally valid meaning. So there is nothing like the meaning of the term 'tiger', if by meaning we understand - as Ajdukiewicz intended - something which is an universal characteristic valid for all users of this term.

This negative conclusion does not necessarily prove that, in searching for a definition of meaning, Ajdukiewicz was doomed to failure. After all this conclusion concerns actual languages, whereas Ajdukiewicz's analyses are concerned with theoretical models. Is there any obstacle to our taking given diagnostic indicators as a basis for defining the empirical directives appropriate for a given language? The language thus created would not be identical with the original one, but the terms of both languages would have exactly the same referents. We had exactly this under consideration at the end of paragraph **\$10**, which finished with an unanswered question. It ran thus: Can we, in the course of the appropriate theoretical analyses, treat the extra-verbal part of our terminological constructions as equivalent to the existence of certain empirical directives?

Before we take up this problem in the next paragraph, let us finish this fragment of our considerations with the following observation. Since the discussion we have led concerned Ajdukiewicz, we gave the concept of an empirical directive a rather narrow interpretation. Ajdukiewicz's empirical directives were thought of as directives determining modes of linguistic behavior in the face of uncomplicated perceptions of the senses. However, there is no good reason to limit empirical findings to simple situations which the observer can recognize without difficulty and without resorting to any special procedures. We can assume that such findings may demand longer and more penetrating observation, sophisticated apparatus and complicated analysis. What is more, we may assume that they need not concern physical objects. If, reading a book, I make observations referring to its content, these observations deserve to be called empirical though this decision disagrees with the assumed understanding of the term 'empirical'; we used to treat as empirical only the contact with the book and not with its content. Equally a mathematician who analyses examples of certain formal constructions, looking for that train

of ideas which allows him to solve the problem he set himself, he often would, not without reason, describe these kind of analyses as empirical. The role of empirical procedures in the process of communication, amongst those empirical procedures in a possibly wide sense, would demand separate study.

\$12. Alleged and Actual Objective Referents. Let us break the main train of thought for a moment to consider on what basis we can expect a term to possess a relatively well defined referent. And let us assume that this is a term whose correct applications are recognized exclusively with the help of diagnostic indicators. The attempt to deal with this question leads to several, not easy, problems. We will not be in a position to take them up, let alone solve them, yet they should be at least adverted to.

When we try to ‘grasp the sense’ of a term unknown to us, as well as when we propose a certain term as the name of a newly discovered phenomenon, our task is the correct grasping of the characteristics which serve as diagnostic indicators of the objects to which the term is, or is to be, applied. In the first case, this correct grasp of the characteristics depends on the discovery of characteristics which allow us to apply the term in just those situations in which competent users - in our opinion competent - apply it. In the second case, the correct grasp of the characteristics depends on finding such diagnostic indicators which ensure the application of the term consistent with our knowledge about the objects to which it is to refer. Whether our grasp of these characteristics is easier or more difficult to achieve depends on many factors. It is above all decided by how easily the relevant objects are recognized. This, of course, does not depend only on the objects, but also on our abilities, acquired skills, our knowledge and often technical possibilities.

Let us notice that speaking about the objects which we want to learn to recognize may have sense even when no-one is able to recognize them. A hypothesis about the existence of objects of a certain specific kind, may be a more or less well justified consequence of theoretical considerations. Such a supposition need not always be based on convincing argumentation. Even then, however, it can be well enough formulated to take up empirical research which confirms or falsifies it. In the first case we may be able to find certain diagnostic indicators corresponding to and complementary to the purely theoretical characteristics of the objects, included in the opening hypothesis.

Objects whose existence is proved with the help of theoretical analyses can turn out to be artifacts - constructs to which nothing answers in reality. ‘Factor causing this and that’, because this is the form a hypothetical definition usually takes, may not exist. The ‘this and that’ may be caused by many mutually independent factors, not forming any homogenous set and, what is more, belonging to a quite different region than the one where the supposed factor was to be found. However, it happens that ‘imaginary’ objects, are for a rather long time taken to be empirically identifiable and, furthermore, amongst people possessing the appropriate professional knowledge.

The problem of distinguishing ‘theoretical artifacts’ from actual objects, is not directly connected with the questions discussed in this article. But this problem has its ‘empirical’ correlate. It can happen that the characteristics which we treat as diagnostic indicators of a certain object (or class of objects) does not refer to any actual object. The object which these indicators supposedly mark out is an artifact of the empirical procedures applied. So, in which cases are we allowed to assert that the diagnostic indicators determine referents, and in which are we not? We may reach the same question by a slightly different road.

Let’s suppose that the diagnostic indicators which X applies when using the term T are not identical with the diagnostic indicators used by Y. They are, however, consistent - they do not lead to any contradictory results. From the semantic point of view, this could amount to two different things. It could mean in other words that the term T possesses a definite referent, **T**, which however is identified differently by X than by Y (they use different diagnostic indicators). In our discussion until now we have concentrated our attention on exactly this possibility. But there is another. This circumstance can indi-

cate the existence of some consistency in the linguistic behavior of X and Y (and possibly of the remaining users of the term T), for which we need not necessarily assume the existence of any referent **T** of the term T.

A number of theoreticians of language, among them such distinguished members as Quine and Davidson, cannot see what could justify operating with the concept of reference and do not feel the need to introduce this concept into semantic considerations. This position represents a rather far going cognitive skepticism which I do not share and for which I cannot find good justification. However, this article is not the right place to develop this question. But we should agree that whenever we attribute the users of the term T the ability to identify the referent **T** of this term, we support a certain thesis representing an interpretation and not merely a description of their behavior. Unfortunately very few things in general, and human behavior in particular, can be understood if we limit ourselves to description and avoid any theory going beyond established facts.

In any case, even if we were inclined to treat the discussion of referents as an alternative way of speaking about the behavior of the users of the language, we know perfectly well about which behavior we are concerned. In particular, we would surely say that when we assert of a certain term T that, for its users, it possesses an established referent, then really we assert that the diagnostic procedures are not only consistent at present, but will preserve this consistency in the future. So, regardless of whether we give the concept of reference a literal sense or treat it as a convenient metaphor, from the assertion that a certain term possesses an established referent we cannot infer - in accordance with Ajdukiewicz, Frege or many other theoreticians of language - that this referent (actual or merely purported) is marked out as something which would deserve the name *meaning* of the term.

It's not the meaning of a term which determines its referent, but rather all the 'meanings', differing among themselves, which its users associate with it. To speak about 'linguistic' meaning for all the users of a language, and so about meaning which is not relativised to a given person, is to create an entity which, as everything seems to show, is a good example of a theoretical artifact.

\$13. Gestalts and Constatations. Let's return to Ajdukiewicz. We were trying to show that whenever the users of a certain term T apply it using consistent, though not necessarily identical, diagnostic indicators, these are the basis for judging that they use it in reference to (relatively) well recognized objects. In other words this term has a relatively well determined reference. Why shouldn't we treat such behavior as behavior in accordance with appropriately formulated empirical directives ? Here I repeat the earlier formulated question, to which this time will try to find an answer.

Certain of our abilities elude any verbal analysis. In particular verbal reactions can have the character of spontaneous reactions, which we are not able to simplify and describe. I am in pain, I am hungry, this is green, hot - all the assertions of this kind, when we express them on the basis of observational beliefs, have the character of spontaneous reactions, not based on further analysis. It seems that Ajdukiewicz sought theoretical expression for exactly such procedures of accepting judgments, in constructing his conception of empirical directives. Schlick (1934) also gave a distinguished theoretical significance, in introducing his concept of 'constatation', to those observational judgments whose further analysis is not in any way possible. But in any case, in the situation when X and Y use the term T consistently, based on their private diagnostic indicators (e.g. they consistently assert 'we are entering Warsaw'), their linguistic behavior need not be unanalysable. If asked: 'On what basis do you think so?', X and Y can give themselves more or less exhaustive explanations. This is one of the ways in which models used by some people are assimilated by others, but on other occasions are subjected to critical evaluation and are disqualified.

Now, there was something Ajdukiewicz, Schlick and many representatives of logical empiricism in its early version did not notice. This was that our diagnostic abilities are nearly always a conjunction of certain spontaneous reactions, not depending on further analysis, *and* certain relatively well realized and verbalized procedures. If it were possible to separate the first of these from the second, a conception like Ajdukiewicz's conception of meaning or Schlick's conception of founding knowledge on constataions exempt from further analysis would be defensible. However this is not so. Our reactions to stimuli are nearly always holistic. So, it is not so that first of all we see the color and shape of the flames, hear the crackling of burning branches, feel heat, smoke stings our eyes and only then we put together these perceptions and judge: the wood is on fire. What we perceive is a 'gestalt'⁵, a certain whole in which we are able to distinguish - *ex post* - many elements but which we need not be able to characterize by listing those elements and describing their mutual relations. If the empirical directives concern these elements, we are unable to infer our linguistic behavior caused by the observation of the gestalt. If we formulate the directive in terms of the gestalt it will not have the elementary character which Ajdukiewicz wanted to ensure for his empirical directives.

\$14. Standardized Systems of Beliefs. Conceiving language as a system of well defined meaning directives is a rather imposing generalization of the procedure of the formalization of the theory of mathematics - and so giving them the form of a deductive system⁶ based on clearly described axioms and directives. It's clear that the languages of mathematics does not include empirical directives. A natural question, to which this observation gives birth, is the question whether empirical theories can be conceived as deductive systems, built within languages equipped with all three kinds of directive. In developing his theory of meaning Ajdukiewicz also gave his attention to this intuition.

Ajdukiewicz certainly realized that empirical directives, described as he described them, could not be seriously treated as tool serving to justify the main assertions of any empirical theory worthy of attention. Essentially not only empirical directives, described as Ajdukiewicz did, but also all empirical procedures which can be the object of machine implementation fail to offer the possibility of justifying empirical assertions in such a compass that would suffice for the development of empirical theories⁷. But Ajdukiewicz was not at all convinced that such assertions demand justification. He only expected that they must be consistent with the empirical knowledge we can gather with the help empirical directives. This conviction appears to be a variant of the thesis of the distinguished role of observational sentences, representing the foundation of logical empiricism. We shall see that this is not so in the closing paragraph of this article.

Ajdukiewicz expressed his point of view in two works: 'Das Weltbild und die Begriffsapparatur' (*Erkenntnis* 4(1934), pp. 259-87) and 'Die wissenschaftliche Welperspektive' (*Erkenntnis* 5(1935), pp. 22-30), formulating in these papers the doctrine of 'radical conventionalism'. This doctrine represents an important part of Ajdukiewicz's theoretical considerations in the early thirties and in an essential way completes his conception of meaning. To correctly grasp the intuitions of this conception, and at the same time present Ajdukiewicz's considerations as a fragment of a certain wider problem context, we must begin from the introduction and preliminary discussion of certain concepts.

Let us call *standardized* all those procedures of gathering empirical data (observation, measurement, test, experiment), which are conceived in the form of well defined algorithms. Geodesic measurements may serve as an example of such procedures, together with all the diagnostic procedures used in marking out the borders of a city. Research conducted in the area of artificial intelligence, and contemporary knowledge concerning the methodology of empirical procedures, both allow us to treat the concept of a

⁵ The concept of form, 'gestalt', and the Gestalt psychology connected with it was developed at the beginning of the 20th century in works by Max Wertheimer, Wolfgang Koehler and Kurt Kofka.

⁶ The concept of a deductive system will appear again in our considerations in \$14.

⁷ In the light of the impressive results obtained by H. Simon and his students concerning the automatic discovery of laws, this statement demands more precise formulation.

standardized empirical procedure as a concept which, in many areas of human epistemological activity, has an adequately clearly defined content.

In the light of the discussion which we conducted earlier, the introduction of standardized empirical procedures in place of empirical directives is not merely a cosmetic procedure. This procedure removes certain 'ideological' limitations which existed in the conception of empirical directives, making it parallel to Schlick's conception of constataion. What is more important, however, is that it seems that we may assign an equivalent (leading to the acceptance of the same sentences) standardized empirical procedure to each empirical directive. We would here be concerned with what would be less a correspondent than a certain generalization of Ajdukiewicz's conception of directives.

Next, let's call any set of beliefs B of which it is assumed that the only possible ways of transformation are the following a 'standardized system of beliefs': adding to B sentences derived from B with help of previously accepted directives Δ , and, adding sentences which can be justified by invoking previously accepted standardized empirical procedures Ξ . Thus each standardized set of beliefs may be expressed as a triple of the form (B, Δ, Ξ) .

In order that the above definition be sufficiently general, we must assume that the set Δ includes axiomatic directives alongside the deductive directives. Invoking axiomatic directives in the derivation of new sentences from the opening set of beliefs depends on adding one of the sentences within the scope of this directive (one of the meaning postulates corresponding to this directive).

We shall call a (*possible*) *history* of a standardized set of beliefs (B, Δ, Ξ) any (finite or not) sequence of sets of beliefs B, B', B'', \dots , for which each successive element is obtained from its immediate predecessor set by the 'single application' of a permissible transformation with directives Δ or procedures Ξ . The concept of a *single application* which has appeared here demands explanation. Axiomatic-deductive directives, as well as empirical procedures are, as we have assumed, formulated in the form of schemata for behavior: in such and such circumstances ω accept such and such sentence α . Every concrete example of such a schema, which leads to the acceptance of a concrete sentence α represents a single application of a rule or directive corresponding to the schema.

In the light of the above given definition a possible history B, B', B'', \dots of a standardized set of beliefs (B, Δ, Ξ) yields an increasing sequence: $B < B' < B'' \dots$ for which each successive element is richer than its predecessor in the sequence by exactly one belief. We will call the set-theoretic sum of such a sequence the *content of a possible history*. On the other hand we will call the original set the *axiomatica* (set of *axioms*) of the history. The use of these names has a fairly obvious explanation: the beliefs belonging to the set B are the only beliefs not possessing any justification covered by the concept of an admissible transformation of the original system.

The concept of a standardized set of beliefs is a natural generalization of the concept of a deductive system. A *deductive system*, in the sense we are concerned with, is any pair of the form (B, Δ) or, putting it a little differently, it is any standardized system of beliefs (B, Δ, Ξ) , such that Ξ is here the empty set - so the only way of widening the set is by invoking the deductive directives Δ . The concept of a deductive system thus defined was the subject of Tarski's (1935) research and is sometimes called a *generalized* one. A distinguished significance is given to those deductive systems in which the axiomatic-deductive directives form a collection equivalent to the set of directives of classical logic (classical predicate calculus or, if the language in which the beliefs B are articulated does not include quantifiers, classical sentential calculus). We will call these deductive systems *classical*. We will also include under this name those standardized systems of beliefs (B, Δ, Ξ) whose deductive part (B, Δ) is classical.

The concepts introduced here may form the subject of interesting formal analyses. The theory of deductive systems is itself a rich and interesting mathematical theory (I here treat metamathematics as a part of mathematics, see e.g. Wójcicki 1988). However, in the context of this article, these concepts are only a convenient means of presenting certain discussions and conceptions of their solution.

Let's finish this section with an important explanation. The term 'belief' used in the above definitions serves to underline the pragmatic orientation of the planned analyses. And this is in accordance with the character of Ajdukiewicz's work discussed here, in particular his theory of meaning. Invoking the concepts with which he operates, we can essentially describe a *belief* as a sentence which is accepted by someone. We shall also give this sense to the concept of belief.

\$15 Inconsistent Systems. A mathematician is introducing his considerations in the area of 'formalized theory', and so in the area of a certain deductive system (B, Δ) . What happens when he discovers that this system is inconsistent: the axioms of the theory, with the help of the directives Δ , allow the proof of a pair of 'mutually inconsistent' assertions. The concept of inconsistent sentences has a well known sense on the basis of classical logic. For deductive systems of an arbitrary kind, the concept demands definition. We'll assume that in the case of generalized systems it signifies that they allow the proof of a pair of assertions α and β from which we can derive an arbitrary assertion γ , by applying the directives Δ ⁸. Using the terminology introduced in the previous paragraph, we can treat the discovery made by the mathematician as the discovery that in one of the histories of the system (B, Δ) - furthermore a history which the mathematician is able to indicate - appears, as an element of this history, an inconsistent system (a system including inconsistent sentences).

Let us note in the margins of the above definitions that in mathematical logic the concept of proof corresponds to the concept of a history of a deductive system (B, Δ) , more exactly the concept of a finite history of this system. The sequence of formulae setting the subsequent widenings of the original set of beliefs B is a proof of the last of these formulae on the basis of the assumptions B and the directives ('rules of inference') Δ .

A theory which has turned out to be inconsistent must be subject to a revision which removes the inconsistency. At least from the formal point of view there are two possible changes. The first depends on making a review of the axioms of the theory and either simply removing some of them, or, replacing them with others. The second change depends on a revision of the logic of the system (the axiomatic-deductive directives). Of course it's conceivable to apply a revision of both the axioms and the logic of the system.

The concept of an inconsistent system transfers, without any important changes, to standardized systems of beliefs. A standardized system of beliefs is *inconsistent* when the content of one of its possible histories includes a pair of inconsistent sentences. The difference between a situation depending on the uncovering of an inconsistency in a deductive system and that depending on the uncovering of an inconsistency in a system of beliefs which admits the application empirical rules, this difference reduces to the fact that apart from 'axiomatic' (beliefs of the original system) and deductive directives, the source of the contradiction may be empirical procedures. Wanting to liquidate inconsistency we must give an analysis, not only of the beliefs which the system includes, and not only of the logic of the system, but also of the standardized empirical procedures appropriate for a given system.

\$16. Revision Criteria. It is clear that getting rid of inconsistency cannot be a goal in itself. Inconsistency of beliefs reveals the existence of some defect in the way of conceiving whatever the inconsistent beliefs were concerned with. Thus it is a signal indicating the necessity of a complete rethink of the

⁸ This definition of contradiction maintains a certain important property of the classical contradiction. In so-called paraconsistent logic this concept is weakened yet more.

questions which our beliefs were about. The source of the inconsistency should be removed and at the same time the whole of the 'healthy' part of the system should be kept. The thus formulated recommendation is as obvious as it is unclear. So the problem becomes one of finding more precise criteria to which a well carried out revision of the system should answer.

We have found ourselves in an area of problematics which is the subject of quite lively research, though it includes within its boundaries systems of beliefs defined in a slightly different way. The research I have here recalled is conducted in connection with the so-called AMG theory of belief systems (the theory created by three authors: Alchurron, Makinson and Garderfors). A short mention in connection with this theory may be a quite natural and needed development of our considerations.

So, first of all, in this theory the research carried out is limited, in a highly natural way, to systems based on classical logic and possibilities for revision of this logic are not considered at all. Secondly, the systems investigated in this theory are not fully defined - from the point of view of the definitions we have assumed above. In AMG-theory we are not concerned with any standardized system of empirical procedures. Neither this concept, nor any corresponding to it appears in this theory. And yet the considerations carried out in AMG-theory can easily be interpreted as referring to standardized systems of beliefs. We must only assume that to every system of beliefs in the sense of AMG-theory is connected a certain system of empirical procedures. *Implicitly* connected - the procedures making up a part of this arrangement/system are not described and neither are any of their properties. This obviously indicates that no assertions are formulated in which the properties of empirical procedures must be taken into consideration. In particular, research conducted on the basis of AMG-theory concerning revisions of systems of beliefs, does not include questions of the revision of empirical procedures.

Let's mention one more characteristic of AMG-theory. In this theory the sought-after strategies of revision are those not causing any 'narrowing' of the content of the system. Let us notice at once that since the strategies of revision in AMG-theory do not anticipate revision of either the logic (only the use of classical logic is admitted) or empirical procedures (they are not the subject of any research), then the only way of revising the system is through revision of the original beliefs. In the light of these remarks, it becomes clear on what would depend the unnecessary narrowing of the content of the system: that e.g. we have removed from the system the sentences α , β and γ while to remove the inconsistency plaguing the system it would suffice to remove the first two.

The recommendation for conducting revisions of systems so as not to remove more beliefs than is necessary for the elimination of inconsistency is a formal rule. Formulating it does not in essence demand content-related or conceptual difficulties. Let's notice at once that the removal of inconsistency by narrowing the content of the system, according to the rule, may be carried out in many inequivalent ways. The rule itself - not to make unnecessary eliminations - does not unequivocally determine the way the system may be revised.

\$17. Beliefs More Or Less Justified. The rule of avoiding unnecessary narrowing of the content of the system is - as we have said - a formal rule. Let's devote some attention to the strategies of revision of belief systems which have a content-related character. Only the confrontation of content-related and formal criteria will allow us to correctly characterize the views which Ajdukiewicz concluded with his doctrine of radical conventionalism.

One of the central questions troubling theoreticians of science, is the question whether the correctness of decisions, taken up in the modification of a system, can be evaluated with reference to the concept of truth. If we treat the concept of truth as a legitimate tool of analysis for the theory of knowledge, then an obvious requirement which every good revision of the system should fulfill is the preservation of all those beliefs we know to be true. Secondly, all those procedures which we know to be *valid* (their correct use does not allow the justification of a false sentence) should be preserved.

Well, on the question of the legitimacy of using the concept of truth we find a dramatic division of opinion amongst both the representatives of science and those whose profession is the investigation into science. Furthermore, often one and the same person takes up now one, now the other point of view. That we shall not be able to decide about this problem in this article, is obvious. However, let's try to have a look at it.

The crowning argument against operating with the concept of truth is the following. You want to know if the belief α is true - true in the classical sense, determined by Aristotle. In order to know this you must know whether things are as α says. So you must compare what the sentence α says with the corresponding state of affairs and check if this occurs. But in order to do this you must know what relations connect expressions with elements of reality. On the other hand, everything you know about reality, about the states of affairs from which that reality is supposedly built, is included in these beliefs. So you are limited to the comparison of belief α with these other beliefs. And you can only do this much, nothing more. Any discussion about the comparison of α with reality, with the states of affairs described α is an illusion. The sooner we are cured of it, the better.

I'm afraid that the argumentation quoted here belongs to the same category as that in whose thinking the hare will never catch up with the tortoise because, when it finds itself at the point where the tortoise is now, the tortoise will already be further on. We could mention an almost infinite number of sentences whose logical value we establish by carrying out procedures depending on some form of check on how things are in reality. And now we find out that in observing, measuring, weighing, subjecting things to experiment and test we are not conducting any comparison of beliefs with the state of facts, but only multiplying the number of our beliefs to be compared with our earlier knowledge. Of course, reality is a source of inspiration for our beliefs, but of what sort we don't know. What we reach is only our observations and not reality. About reality itself and thus the relations connecting our beliefs and reality we are not able to say anything. We are like that hare which chases the tortoise - regardless of how much we can reduce the distance by, the tortoise is already further.

I'm deliberately reaching for metaphor and not argument. The philosophical literature is full of arguments in defense of epistemological realism - as we can call the position of the defenders of truth - and arguments proving the opposite thesis. However, both sides - those who believe that there are questions in reference to which it's possible to grasp the truth and those who believe that truth is a concept devoid of any well defined content - sustain their points of view. So let us stop with what has been said.

We have even more reason to stop on that since the dispute about the legitimacy of using the concept of truth seems not to have a great significance in view of the fact that both sides of the dispute:

- (a) agree to accept certain sentences as better justified than others,
- (b) agree that empirical procedures play a special role among procedures serving to justify sentences and they
- (c) agree that any kind of justification with absolute certainty is not possible.

The convergences of opinion seem more important than the divergences and we might wonder whether the dispute between realists and their adversaries on the question of truth does not sometimes have the character of a verbal dispute: that for the realists 'justification' equals 'indication of truth' whereas their opponents do not feel the need to suspect any relationship between justification and truth. For them, to justify is to 'legitimize acceptance'.

\$18. The Doctrine of Radical Conventionalism. Let's base our further discussions on the concept of justification rather than truth, resisting comment on the mutual relations between these concepts.

The fundamental thesis of logical empiricism is of the distinguished epistemological role of empirically decidable sentences. Exactly these sentences, and no others, can be justified to a particularly high degree. So these sentences should be eliminated from a system undergoing revision only as a last resort. But the empiricists also went further. Namely they asserted that, with respect to those sentences which are not empirically decidable, the concept of justification loses its sense. Their presence in scientific theories is justified only by the fact that they represent a convenient way of ordering empirical knowledge. Such sentences do not concern the objects being subject of empirical investigation but are rather only certain abstract objects - theoretical constructs - which we create to facilitate our defining the relationships which connect the objects to which we have experiential access.

Knowledge, in the opinion of empiricists, divides into empirical and theoretical. A more precise presentation of this point of view would demand the introduction of a number definitions. And so, for example, definitions setting out the relationships between empirical knowledge and observational sentences. A great part of the work carried out by logical empiricists concerned exactly questions connected with explaining the following: On what do the differences depend, between observational and empirical knowledge, and between empirical and theoretical knowledge (theoretical 'knowledge'). Playing a purely instrumental role, this supposed theoretical knowledge was not treated as knowledge in the proper sense of the word.

The train of thought I have tried to sketch, in discussing the position of logical empiricism, belongs to a quite different conceptual order than the considerations of Ajdukiewicz devoted to the theory of meaning. Empiricists ask: What role do such and such linguistic expressions play in epistemological procedures? Ajdukiewicz - as a theorist of language - asked: what role do such and such linguistic expressions play in the process of communication. The change in perspective causes a change in terminology. Instead of treating theoretical assertions as epistemological instruments - and so as something which is not in itself an element of knowledge - Ajdukiewicz preferred to treat these assertions, and especially the chief assertions amongst them (being the main principles of empirical theories), as linguistic postulates. So they are for Ajdukiewicz certain terminological conventions.

There is no important difference between instrumentalism and conventionalism, as long as we treat empirical knowledge as distinguished knowledge. The difference reduces to the use of different words, accenting different aspects of the same thing. Let us further explain with the following. The doctrine of conventionalism is associated with the names of Poincare and Duhem. It is an earlier doctrine than that of logical empiricism and certainly is not identical with it. We have already established in what point the doctrines converge, but there is no space here to consider in what aspects they differ.

From the thirties on Ajdukiewicz was a conventionalist without being an instrumentalist. He was not an instrumentalist because he rejected the conviction of the distinguished character of empirical knowledge. He rejected that conviction because he recognized that in situations when there appears the necessity of a revision of the theory, empirical directives are not distinguished among the linguistic directives in any way. A crisis in science, which is the appearance of an inconsistency which cannot be removed by showing that an 'error of art' has been committed, must be overcome. (An 'error of art' is when one of the directives or procedures has been badly applied - so some supposed proof is not a proof, so one of the supposedly justified empirical theses has been incorrectly acquired.) Undoubtedly, when overcoming a crisis, a revision of the system (theory) should be carried out respecting the rule to avoid changes which are unnecessary to the removal of the inconsistency. But this is probably the only limitation which Ajdukiewicz would have been disposed to accept.

It is exactly because he did not treat empirical directives as in need of special care that Ajdukiewicz called his conventionalism *radical conventionalism*. There exists a far going similarity between Ajdukiewicz's conventionalism and Quine's, but this is a subject which we cannot here develop. There also exists a clear kinship of ideas between Ajdukiewicz's conventionalism and the doctrine of incom-

measurability, not so much proposed as suggested by Kuhn. The incommensurability of two theories depends on the lack conceptual relations which would allow an assertion of one theory to be translated into an assertion of the second. A change of meaning directives of a language may, as Ajdukiewicz stressed, cause the untranslatability of languages and thus the incommensurability of theories formulated within them.

It is a peculiar paradox that Ajdukiewicz's work appeared in a journal dedicated to logical empiricism, as *Erkenntnis* was in the thirties, without provoking any strong polemic. This phenomenon is probably explained by the fact that the anti-empiricist consequences of Ajdukiewicz's doctrines were disguised by his own terminology. In any case the whole theoretical perspective (concentrating attention on problems of communication and not epistemological problems) represented a rather effective obstacle to the recognition of the Trojan horse, as which Ajdukiewicz's work was able to appear in the stronghold of empiricism. However it is not Ajdukiewicz but Kuhn who brought about the ultimate breaking of the fashion which told philosophers at the beginning of our century to adopt the empiricist point of view.

Since we have already spoken about it, let's cite the last sentence of *Die wissenschaftliche Weltperspektive*. It is connected with the basic assumptions of the doctrine of radical conventionalism and reads: 'Such a conception of the historical development of science would represent the main body of the humanistic understanding of the process of the evolution of science.' The humanistic aspects of the development of science found themselves at the center of philosophers attention only thanks to the work of Kuhn. Ajdukiewicz was the precursor of research taking into account the humanistic (pragmatic) aspects of the problems. Yet in the minds of those who classify philosophy and philosophers using stereotypes they themselves produce, he is an example of a philosopher who, in his epistemological considerations, remained in a sterile world of purely formal ideas.

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