reconstructions of the ordinary language within constructed systems (this is the case, e.g., for sentences like "all ravens are black" or "space is three-dimensional", and similar ones). On the other hand, at the present moment I cannot think of any term in a relatively clear language, for which I would be in doubt whether to count it as logical or as descriptive. For example, I would count as logical every constant which is either primitive, or defined, or definable in the system of *Principia Mathematica* or in that of Quine's *Mathematical Logic;* and this covers all constants of pure mathematics. (I do not include, of course, the axiomatic primitive constants of a mathematical axiom system, since they are uninterpreted and thus the distinction does not apply to them at all; but I include the explicit predicate of any axiom system, compare [1929-2] §30b, [1954-3] §42d, and [1958-2] §42d). As an example of the difficulty, Beth refers to the sign "=". He agrees that this sign, if defined as the sign of identity as in Russell's system, is clearly logical. But then he refers to the same sign as occurring within the axiom system of the elementary theory of simple order. It is true that within this system the sign "=" can be defined on the basis of the axiomatic primitive sign "<". But obviously here the question of the classification of the signs can only be raised when an interpretation for the primitive is specified. It is certainly possible to choose the interpretation in such a way that this primitive becomes a descriptive sign; and in this case, the sign "=" is likewise descriptive. But then this sign is not a sign of identity; it is only factually equivalent, not logically equivalent, to the sign of identity, which is logical. (This situation is analogous to the following. If we define "*np*" as "the number of planets", then it is a descriptive sign and is only factually equivalent, not logically equivalent, to the logical sign "9".)

I entirely agree with Beth that the difference between Tarski's method and my method of semantics is to a large extent to be explained by the fact that Tarski deals chiefly with languages for logic and mathematics, thus languages without descriptive constants, while I regard it as an essential task for semantics to develop a method applicable to languages of empirical science. I believe that a semantics for languages of this kind must give an explication for the distinction between logical and descriptive signs and that between logical and factual truth, because it seems to me that without these distinctions a satisfactory methodological analysis of science is not possible.

Beth's criticism (in §9) of the loose use of the words "calculus", "semantical system", and related terms in my books is correct and fair. I thought originally that the context would always make it sufficiently clear whether I meant to refer to expressions of the object language (e.g., with "a sentence of the calculus") or of the metalanguage (e.g., with "a rule of the calculus"). But my experience in teaching has shown that this ambiguous use is often misleading. Beth's suggestions for a terminology seem to me good and helpful. I now use a similar terminology in my courses and in the book [1958-2].

In §10 Beth discusses the problem of so-called *ontological commitments.* I believe today that there is a good deal of truth in his remark that the problems of traditional metaphysics (disregarding here the often anti-scientific attitude in the movement of German idealism) are often closely related to problems of logic and semantics. Beth himself has in other publications clearly pointed out these connections. The earlier anti-metaphysical formulations in our movement, especially during the Vienna period, were often too general. On the other hand, it seems to me still very important to make a clear distinction between genuine questions and pseudo-questions, both in traditional and in contemporary philosophical discussions. It is not quite clear what exactly Beth means by "ontological commitments", and he does not mention explicitly an example of such a commitment. I have the impression that he means existential statements on general kinds of entities (and perhaps also negations of such statements), e.g., "there are classes of classes of objects". More specifically, I presume he is thinking of those statements which I have called *external* existential statements. I have discussed the problem of their interpretation in §4B.

19. P. F. Strawson on Linguistic Naturalism

At the beginning of his essay, Strawson outlines the two competing methods proposed for philosophical clarification, the method of rational reconstruction in a formal language system with exact rules, and the method of describing and analyzing the actual usage of words in everyday language. Strawson calls the adherents of the first method constructionists; the adherents of the second method may be called linguistic naturalists or, within this section for short, naturalists.

By an explication I understand the replacement of a pre-scientific, inexact concept (which I call "explicandum") by an exact concept ("explicatum"), which frequently belongs to the scientific language. (For more details see ch. I of my book on probability [1950-4].) Although explications are often given also by scientists, it seems to me particularly characteristic of philosophical work that a great part of it is devoted to proposing and discussing explications of certain basic, general concepts. The first, preparatory step in an explication consists in the informal *clarification of the explicandum.* (Strawson correctly describes my characterization of the procedure of explication and refers to the chapter mentioned above. He says, however, that my term for this procedure was "clarification" and does not mention the term "explication". Therefore it is not clear in some parts of his discussion of my conception whether

he uses the term "clarification" to refer to the explication or to its first step, viz. the clarification of the explicandum.)

Strawson then discusses the specific question, which indeed touches the central point of the controversy, whether an explication given, not in the ordinary language, but in a scientific, technically constructed language could possibly be useful for the solution of a philosophical problem. He declares that the negative answer to this particular question seems "evident" to him and adds that "it seems to require no argument". However, I am firmly convinced of the affirmative answer. But this view does not seem immediately evident to me; I would rather call it an insight, gained on the basis of extended work concerning explications of various kinds. Therefore, arguments on either side do not seem superfluous to me. Frequently, what seems to be a clash between two opposing positions, is actually a difference in the interpretation of a concept. I have the impression that Strawson's view is based on the conception of a sharp separation, perhaps even a gap, between everyday concepts and scientific concepts. I see here no sharp boundary line but a continuous transition. The process of the acquisition of knowledge begins with common sense knowledge; gradually the methods become more refined and systematic, and thus more scientific. To my example of the explication of the qualitative concept Warm by the quantitative concept Temperature, Strawson remarks that the latter concept does not help in solving puzzling questions like "Does it follow from the fact that the same object can feel warm to one man and cold to another that the object really is neither cold nor warm nor cool nor has any such property?" In order to solve this puzzle, we have first to distinguish between the following two concepts: (1) "the thing x feels warm to the person y" and (2) "the thing x is warm", and then to clarify the relation between them. The method and terminology used for this clarification depends upon the specific purpose we may have in mind. First, it is indeed possible to clarify the distinction in a simple way in ordinary language. But if we require a more thorough clarification, we must search for explications of the two concepts. The explication of concept (1) may be given in an improved version of the ordinary language concerning perceptions and the like. If a still more exact explication is desired, we may go to the scientific language of psychology. The explication of concept (2) must use an objective language, which may be a carefully selected qualitative part of the ordinary language. If we wish the explicatum to be more precise, then we use the quantitative term "temperature" either as a term of the developed ordinary language, or as a scientific term of the language of physics.

In his section on pseudo-questions, Strawson ascribes to me the view that "philosophical questions and perplexities cannot really be taken seriously". If this were my view, I would not have devoted the greater part of my life's work to attempts at solving or clarifying such problems. Nor do I "ignore the role of conceptual explanation in resolving philosophical difficulties". A very large part of my philosophical work actually consists just in developing and applying the methods of conceptual explanation—in my terminology, explication. Strawson believes that philosophical problems are raised by people "who know very well how to use the expressions concerned". I should rather say that these people usually believe they know this very well, but often deceive themselves. The first step in helping these people consists in leading them to the insight that something is wrong with their use of certain expressions, that it involves confusions or even inconsistencies. Frequently the puzzle concerns expressions of ordinary language, for example, numerical terms, spatial and temporal terms for the description of motion, terms like "true", "entailment", and the like. These may be terms which in most cases are used without any difficulty. But then it may occur that in certain critical contexts the ordinary usage leads to difficulties, unanswerable questions, even contradictions, demonstrating the surprising fact that people are not completely clear about the correct usage. A misinterpretation of the expressions describing motion led to Zeno's paradoxes; in the antinomy of the liar, the term "true" led to difficulties. With respect to the numerical words "one", "two", etc., the situation might seem different. For thousands of years, many people used these words adequately for all practical purposes, and for several centuries the mathematicians have had a systematically constructed theory involving these words. But even in this case, complete clarity was lacking. Before Frege, nobody was able to give an exact account of the meanings of these words in non-arithmetical terms. By Frege's explication of the numerical words, which I would regard as one of the greatest philosophical achievements of the last century, the logical connection between these words and logical particles like "there is", "not", "or", and "the same as" became completely clear for the first time. Therefore we have to say that in spite of practical skill in usage, people in general, and even mathematicians before Frege, were not completely clear about the meaning of numerical words. Clarity is here understood in a stricter sense than in ordinary language; this sense does not require the ability to give a definition, but it requires that the usage does not lead to logical paradoxes.

An explication replaces the imprecise explicandum by a more precise explicatum. Therefore, whenever greater precision in communication is desired, it will be advisable to use the explicatum instead of the explicandum. The explicatum may belong to the ordinary language, although perhaps to a more exact part of it. Or it may be that it did not belong to the ordinary language originally but was introduced as a

scientific term. Even such a term will frequently be accepted later into the everyday language, such as "at 4:30 P.M.", "temperature", "speed" as a quantitative term. In other cases, the explicatum is chiefly used in technical, scientific contexts. The only essential requirement is that the explicatum be more precise than the explicandum; it is unimportant to which part of the language it belongs. However, since exact concepts are more easily found in the scientific part of our language, it will often be useful to define the explicatum in this part. Furthermore, exactness and clarity are best achieved by a certain degree of systematization. Therefore the explicatum usually belongs to a systematic conceptual framework. But the system may be of a rather elementary kind as, for instance, the system of numerical words in everyday language. The use of symbolic logic and of a constructed language system with explicit syntactical and semantical rules is the most elaborate and most efficient method. For philosophical explications the use of this method is advisable only in special cases, but not generally.

The explicatum is intended to take the place of the explicandum, and that means, of course, that it is to be used for the same purpose as the explicandum. Again misled by his sharp distinction between scientific language and ordinary language, Strawson seems to misunderstand this point. He believes that the explicatum is meant to serve a scientific purpose, in distinction to the explicandum which serves a pre-scientific purpose. Suppose the statement "it will probably be very hot tomorrow at noon" is made for the purpose of communicating a future state to be expected, perhaps with regard to practical consequences. The use of the explicatum "temperature" instead of "very hot" in the above statement makes it possible to fulfil the same purpose in a more efficient way: "the temperature tomorrow at noon will probably be about so and so much".

The aim of naturalists and constructionists is basically the same: clarifications and solutions of philosophical problems and perplexities. The two schools would also agree in the point, emphasized by Wittgenstein, that some of these problems and most of these perplexities result from an inappropriate use of language. Here we may distinguish between two kinds of cases, which, however, are not separated by a sharp line. In cases of the first kind, the unsuitable usage does not occur in everyday language, but is introduced by a philosopher in an intentional or unintentional deviation from ordinary language. A philosopher may, for instance, use a certain term of ordinary language in a much more general sense; or he may make a wrong assimilation, as Strawson has explained. In a case of this kind, the method of the analysis of ordinary language may be sufficient for the solution of the ensuing puzzle; the deviation is pointed out, and thereby the perplexity disappears. The constructionist may agree with this procedure; but in some cases of this kind he would

suggest an additional step. For example, a philosopher, needing a more general concept for his particular purposes, may change the sense of a term of everyday language because in this language there is no term for the more general concept. In this case, the constructionist may regard it as advisable to choose a different term for the new concept, and to provide more exact directives for its use. The new expression may be formed by the addition of a qualifying adjective to the old term, or by the addition of a subscript, or even a newly coined term may be proposed. (If the reader is shocked or offended by the two last-mentioned procedures, I hasten to assure him that they occur, of course, only in the more barbarous regions, like America, where the sacrilege of tampering with the holy tradition of language is sometimes connived at.)

In cases of the second sort, the misuse of certain expressions occurs in everyday language. Here again, both the naturalist and the constructionist may agree in the diagnosis of the inappropriate usage in question. It may be that both also agree with regard to the therapy, which may consist in the suggestion that the unsuitable way of speaking be replaced by a more suitable one, still belonging to ordinary language. But here it may occur that the constructionist prefers the use of a newly constructed term not belonging to ordinary language. How far he will move away from ordinary language will depend upon what he regards as useful in the given case. I should like to emphasize again that this is a matter of degree. The constructionist may, for example, propose to use, in certain philosophical contexts (not in contexts of everyday life), certain words of everyday language according to certain rules (e.g., to use the word "or" only in the non-exclusive sense), or he may propose a symbol for the new sense. For the rules or the definition of the explicatum—whether it be represented by an old word in a new sense, or by a new word, or by a symbol—he may use either ordinary language, or in addition some scientific terms frequently used in ordinary language, or purely scientific terms. He may merely state a few simple rules, or he may prefer a more or less elaborate procedure, and for this he may or may not use an artificial language.

In my view, the extent to which artificial and elaborate means are used depends on the nature of the philosophical problem in question, and also on the aim of the therapy. The aim may merely be to eliminate an isolated minor difficulty in the simplest way possible. In this case, simple means will suffice. Or the aim may be a more thoroughgoing reform in order to overcome a larger group of interconnected philosophical difficulties. In this case it may be necessary to use more elaborate means and a more comprehensive systematization.

One of Strawson's main arguments for the thesis of naturalism is the following. Since the roots of philosophical difficulties lie in ordinary

language, the difficulties must be eliminated by the analysis of ordinary language. To propose for this aim an artificial language or a new scientific explicatum, would be "to do something utterly irrelevant", and to deflect our attention from the original difficulties to entirely different concepts. As I emphasized above, an explicatum serves primarily for the same purpose as the explicandum; therefore, an artificial language as a whole may at first serve the same purpose as ordinary language. Later, of course, the new term or the new language may be used within new contexts. In my view, a language, whether natural or artificial, is an instrument that may be replaced or modified according to our needs, like any other instrument. For the naturalists, ordinary language seems to have an essentially fixed character and therefore to be basically indispensable, just like our body with its organs, to which we may add accessories like eyeglasses, hearing aides, and the like, but which we cannot essentially change or replace. However, a natural language is not an unchangeable function of our body, but something we have learned; therefore we can replace it by another language. Some naturalists seem to think that it is in principle impossible to learn an artificial language in any other way than by a translation into our mother tongue. Some formulations by Strawson might also be interpreted in this sense; but I do not know whether he actually holds this view. At any rate, this view is certainly wrong. The method of learning by translation is indeed sometimes practically convenient, and therefore usually applied in learning an artificial language, especially one used in logic or mathematics. But just as we can learn another natural language without the use of our mother tongue as a metalanguage (e.g., in the Berlitz method), so we can learn a language of the kind used in symbolic logic (but with pronounceable words instead of merely graphic symbols, and with a sufficient vocabulary of non-logical constants) without the help of our mother tongue. Later, after such a language has been learned by the practical, direct method, we might learn explicit rules for it, formulated in this language itself, just as a child in school learns grammatical rules of his mother tongue, formulated in the same language. In this way the artificial language would become a regulated language system. This is explained in detail by Bohnert (§II). My intention in making this point is not, of course, to propose the actual use of this method for learning a logical language, but merely to point out the theoretical possibility of such a procedure, and thereby to refute the wide-spread view that constructed languages are not autonomous, but essentially parasitic, based on natural languages.

A natural language is like a crude, primitive pocketknife, very useful for a hundred different purposes. But for certain specific purposes, special tools are more efficient, e.g., chisels, cutting-machines, and finally

the microtome. If we find that the pocket knife is too crude for a given purpose and creates defective products, we shall try to discover the cause of the failure, and then either use the knife more skillfully, or replace it for this special purpose by a more suitable tool, or even invent a new one. The naturalist's thesis is like saying that by using a special tool we evade the problem of the correct use of the cruder tool. But would anyone criticize the bacteriologist for using a microtome, and assert that he is evading the problem of correctly using a pocketknife?

Bertrand Russell,[25] from whom most of us have learned the use of a symbolic language for the clarification and solution of philosophical problems, has recently shown in a delightful way the futility of the tendency to stick to the customary language at any price.

The choice of a method for the solution of a given philosophical problem should be decided in each case by practical considerations. We constructionists should not claim that our method is the only one for the solution of philosophical problems, or necessarily the best in all cases. But naturalists likewise should not make such claims for their method, as some of them, though not Strawson, do.

Let us consider some of the examples mentioned earlier. Frege was able to give his explications of the numerical words "one", "two", etc. in the natural language (*Die Grundlagen der Arithmetik,* 1884). But later he found it advisable and in a certain sense even necessary to formulate these explications in a newly constructed logical language system (*Grundgesetze der Arithmetik,* two volumes, 1893 and 1903). To demonstrate the adequacy of his explications, he had to show that the numerals and the other arithmetical signs, as defined by him, had the properties customarily ascribed to them in arithmetic. For this purpose, it was necessary to show that the basic laws of arithmetic could be proved for his explicata. And in order to assure the cogency and purity of the proofs, it was necessary to formulate them in a system with fixed axioms and rules of inference. It would hardly have been advisable, although theoretically possible, to use the ordinary word language for these operations. The situation is different in the case of Zeno's paradoxes. For their solution, certain parts of mathematics are needed which go far beyond elementary arithmetic, such as the theory of real numbers, the concept of the limit of a series, and finally the proof that certain infinite series are convergent, i.e., that every member of the series is greater than zero and nevertheless the sum of the whole series is finite. In this case, the perplexities were formulated in the natural language. But the diagnosis consists in the demonstration that certain apparently valid forms of inference involving the infinite are fallacious and lead to contradic-

[25] B. Russell, "Logic and Ontology", *J. Phil.* LIV (1957), 225-230.

tions. The therapy consists in the use of a new language, with terms suitable for the formulation of the problem and with rules of deduction preventing the old contradictions. In this case, the old knife and the simple chisel are inadequate; we have to use a more elaborate tool.

I agree with Strawson in the view expressed in his present essay and at another place[26] that the naturalist and the constructionist methods are not necessarily competitive, but rather mutually complementary, since each of them fulfills a certain purpose. To this appeal for cooperation instead of controversy Strawson adds the remark that he himself is partisan; and so am I, on the other side.

A view similar to that in which Strawson and I agree, is held by Nelson Goodman. In the third section of his essay, which deals with constructionism, he gives first a concise, clear exposition of the aims of conceptual constructions. His comparison of this kind of construction with the drawing of a map clears up misunderstandings which are the basis of many criticisms of constructionism. He emphasizes correctly that the reconstruction of a single concept, or of a conceptual system, or of a total language is not intended to copy or picture reality either as a whole, or in part, or on a diminished scale, but to represent the relations among the objects in question by an abstract schema. Then he likewise expresses the view that the activities of the constructionists and the linguistic naturalists (or verbal analysts, as he calls them) are not incompatible but rather complimentary. I share Goodman's feelings when he says that the verbal analyst appears to him "as a valued and respected, if inexplicably hostile ally". (This does not, of course, apply to Strawson.)

It is certainly more fruitful, instead of wasting time in deprecating the method of the other side, to work out some mode of peaceful coexistence of the two movements, and if possible, to cooperate. We all agree that it is important that good analytic work on philosophical problems be performed. Everyone may do this according to the method which seems to be the most promising to him. The future will show which of the two methods, or which of the many varieties of each, or which combinations of both, furnishes the best results.

20. Yehoshua Bar-Hillel on Linguistics and Metatheory

Bar-Hillel makes an appeal for closer collaboration between logicians and linguists. He explains that it would be desirable for logicians to study the logical structure of natural languages too, as he himself, Quine, Reichenbach, and others have begun to do. On the other hand, linguists could use certain methods and results of modern logic to advantage for

[26]Strawson, "Construction and Analysis" in: Ayer *et al., The Revolution in Philosophy* (London, 1957). Ayer too regards both methods as legitimate and fruitful (*ibid.*, 86).

their own purposes. I am in full sympathy with this appeal. The fact that I myself have given no more than short indications at various places is, as Bar-Hillel remarks, only due to the necessity of a division of labor.

I think, however, that we should be cautious in the specification of those items in logic which might be fruitful for linguistics. I believe that we should less emphasize special results than general points of view and general characteristic traits of the methods of syntax and semantics as developed in logic. These traits would include, e.g., the possibility of representing in a formal way logical meaning relations by syntactical rules of transformation, as Bar-Hillel has pointed out. I myself am not in a position even to try to make these matters clear to the linguists; this is the task of those who, like Bar-Hillel, are familiar with the contemporary methods and tendencies in linguistic investigation. It is always difficult to build a bridge between two fields of knowledge which have developed their methods and terminologies separately, so that even elementary communication is not easy. Bar-Hillel's paper of 1954 (see his footnote 13) seems to have found little echo among linguists so far, although this paper, in contrast to my publications, is written in a generally comprehensible language, is published in a linguistic periodical, and makes direct references to the works of the structural linguists. I was not surprised to find that Chomsky[26a] in his reply to Bar-Hillel's article does not agree with Bar-Hillel's views; I think that Chomsky is to some extent right, because Bar-Hillel claims too much when he speaks about the *immediate* importance of my investigations for linguistics. But, on the other hand, I have the impression that Chomsky failed to grasp the meaning of Bar-Hillel's appeal and also the aim and nature of my theories of syntax and semantics, and this shows the great difficulty of communication between the two fields.

I am in agreement in all essential points with Bar-Hillel's explanation of the replacement of ontological controversies about entities of various kinds by discussions of different language forms. These discussions would include the semantical and pragmatical properties of the language forms and, above all, the usefulness of certain language forms for given purposes. My view on this question is set forth in greater detail in §4A. Bar-Hillel suggests not only to *replace* the ontological theses of the existence of certain kinds of entities by a discussion of practical questions concerning the choice of forms of language, but rather to *interpret* those theses as assertions of the expediency of corresponding language forms for certain purposes. It is true that this procedure would have the advantage that the allegedly theoretical theses of ontology would be in-

[26a]Noam Chomsky, "Logical Syntax and Semantics. Their Linguistic Relevance", *Language*, XXXI (1955), 36-45.