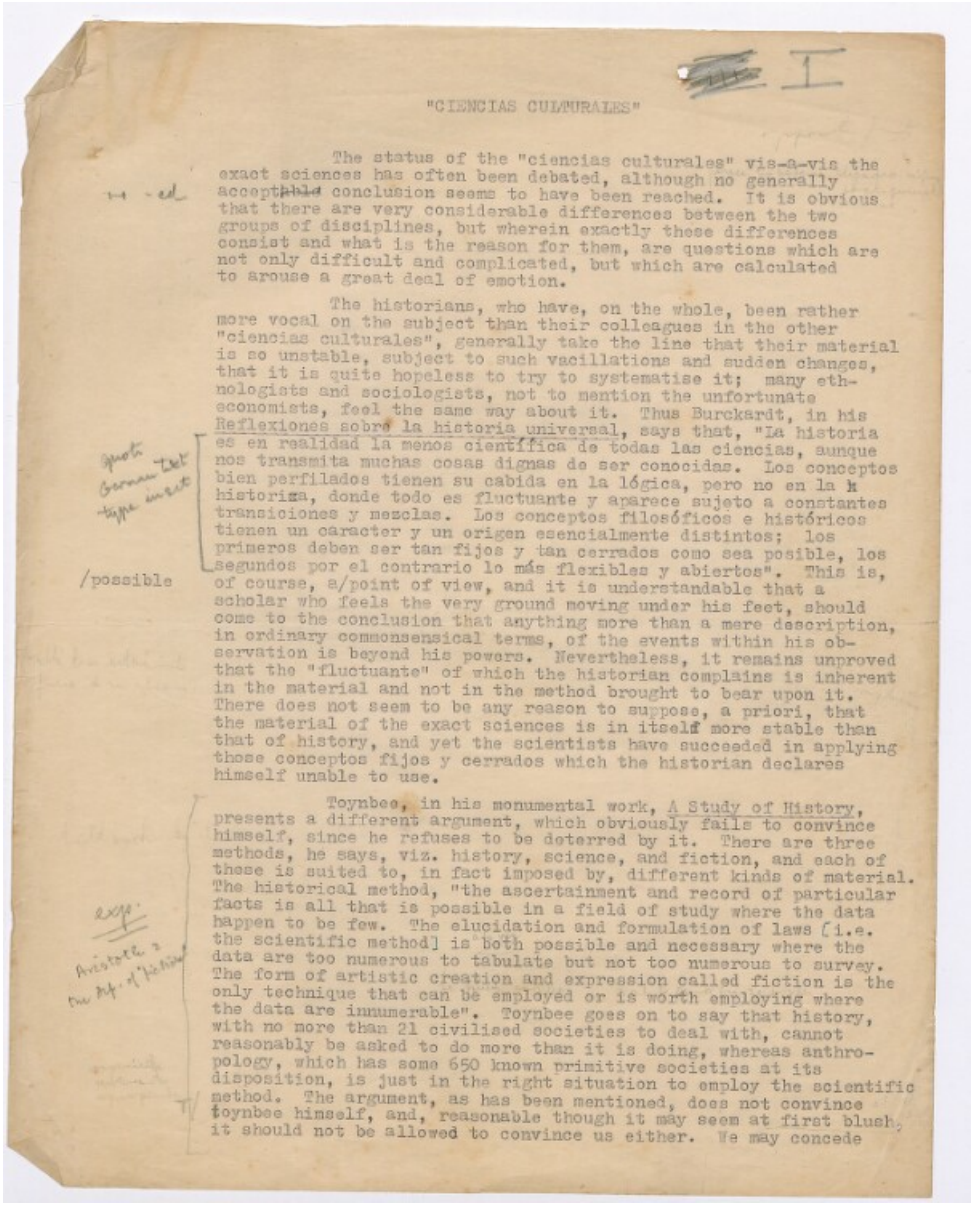


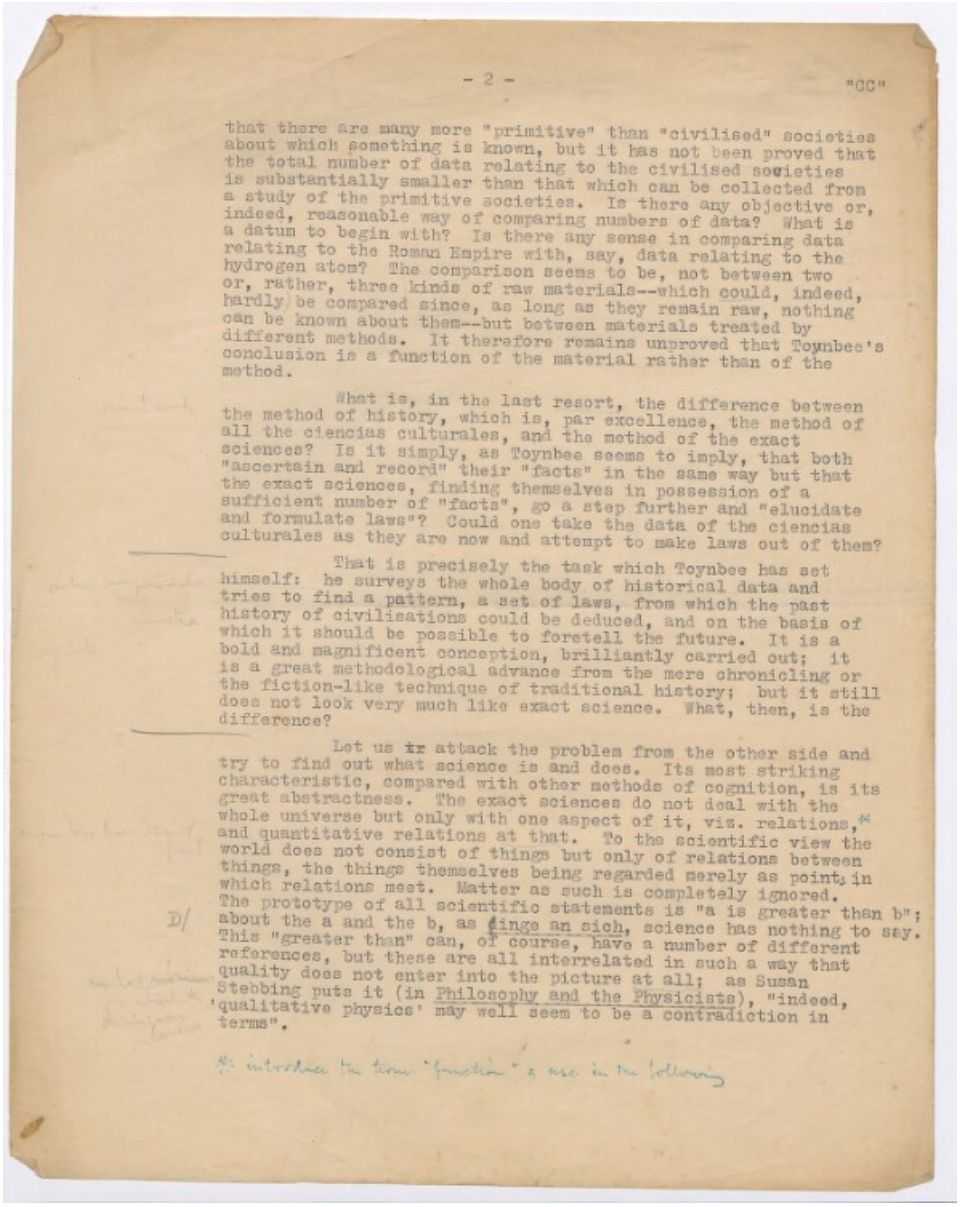
I

"CIENCIAS CULTURALES"

The status of the "ciencias culturales" vis-a-vis the exact sciences has often been debated, although no generally acceptable conclusion seems to have been reached. It is obvious that there are very considerable differences between the two groups of disciplines, but wherein exactly these differences consist and what is the reason for them, are questions which are not only difficult and complicated, but which are calculated to arouse a great deal of emotion.

The historians, who have, on the whole, been rather more vocal on the subject than their colleagues in the other "ciencias culturales", generally take the line that their material is so unstable, subject to such vacillations and sudden changes, that it is quite hopeless to try to systematise it; many ethnologists and sociologists, not to mention the unfortunate economists, feel the same way about it. Thus Burckardt, in his Reflexiones sobre la historia universal, says that, "La historia es en realidad la menos científica de todas las ciencias, aunque nos transmita muchas cosas dignas de ser conocidas. Los conceptos bien perfilados tienen su cabida en la lógica, pero no en la historia, donde todo es fluctuante y aparece sujeto a constantes transiciones y mezclas. Los conceptos filosóficos e históricos tienen un caracter y un origen esencialmente distintos; los primeros deben ser tan fijos y tan cerrados como sea posible, los segundos por el contrario lo más flexibles y abiertos". This is, of course, a point of view, and it is understandable that a scholar who feels the very ground moving under his feet, should come to the conclusion that anything more than a mere description, in ordinary commonsensical terms, of the events within his observation is beyond his powers. Nevertheless, it remains unproved that the "fluctuante" of which the historian complains is inherent in the material and not in the method brought to bear upon it. There does not seem to be any reason to suppose, a priori, that the material of the exact sciences is in itself more stable than that of history, and yet the scientists have succeeded in applying those conceptos fijos y cerrados which the historian declares himself unable to use.

Toynbee, in his monumental work, A Study of History, presents a different argument, which obviously fails to convince himself, since he refuses to be deterred by it. There are three methods, he says, viz. history, science, and fiction, and each of these is suited to, in fact imposed by, different kinds of material. The historical method, "the ascertainment and record of particular facts is all that is possible in a field of study where the data happen to be few. The elucidation and formulation of laws [i.e. the scientific method] is both possible and necessary where the data are too numerous to tabulate but not too numerous to survey. The form of artistic creation and expression called fiction is the only technique that can be employed or is worth employing where the data are innumerable". Toynbee goes on to say that history, with no more than 21 civilised societies to deal with, cannot reasonably be asked to do more than it is doing, whereas anthropology, which has some 650 known primitive societies at its disposition, is just in the right situation to employ the scientific method. The argument, as has been mentioned, does not convince Toynbee himself, and, reasonable though it may seem at first blush, it should not be allowed to convince us either. We may concede



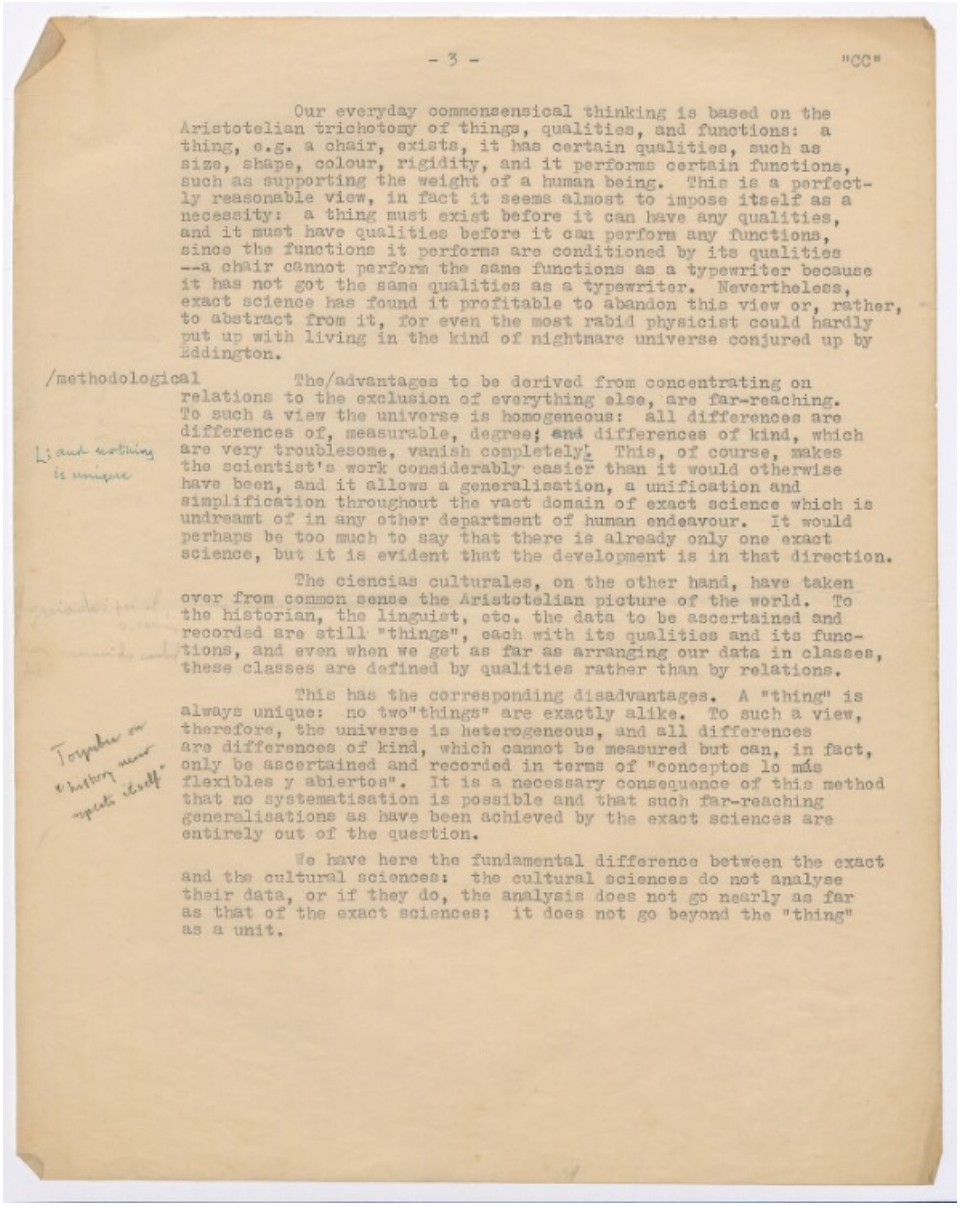
that there are many more "primitive" than "civilised" societies about which something is known, but it has not been proved that the total number of data relating to the civilised societies is substantially smaller than that which can be collected from a study of the primitive societies. Is there any objective or, indeed, reasonable way of comparing numbers of data? What is a datum to begin with? Is there any sense in comparing data relating to the Roman Empire with, say, data relating to the hydrogen atom? The comparison seems to be, not between two or, rather, three kinds of raw materials--which could, indeed, hardly be compared since, as long as they remain raw, nothing can be known about them--but between materials treated by different methods. It therefore remains unproved that Toynbee's conclusion is a function of the material rather than of the method.

What is, in the last resort, the difference between the method of history, which is, par excellence, the method of all the ciencias culturales, and the method of the exact sciences? Is it simply, as Toynbee seems to imply, that both "ascertain and record" their "facts" in the same way but that the exact sciences, finding themselves in possession of a sufficient number of "facts", go a step further and "elucidate and formulate laws"? Could one take the data of the ciencias culturales as they are now and attempt to make laws out of them?

That is precisely the task which Toynbee has set himself: he surveys the whole body of historical data and tries to find a pattern, a set of laws, from which the past history of civilisations could be deduced, and on the basis of which it should be possible to foretell the future. It is a bold and magnificent conception, brilliantly carried out; it is a great methodological advance from the mere chronicling or the fiction-like technique of traditional history; but it still does not look very much like exact science. What, then, is the difference?

Let us ~~ir~~ attack the problem from the other side and try to find out what science is and does. Its most striking characteristic, compared with other methods of cognition, is its great abstractness. The exact sciences do not deal with the whole universe but only with one aspect of it, viz. relations,* and quantitative relations at that. To the scientific view the world does not consist of things but only of relations between things, the things themselves being regarded merely as points in which relations meet. Matter as such is completely ignored. The prototype of all scientific statements is "a is greater than b"; about the a and the b, as Dinge an sich, science has nothing to say. This "greater than" can, of course, have a number of different references, but these are all interrelated in such a way that quality does not enter into the picture at all; as Susan Stebbing puts it (in Philosophy and the Physicists), "indeed, 'qualitative physics' may well seem to be a contradiction in terms".

*: introduce the term "function" & use in the following



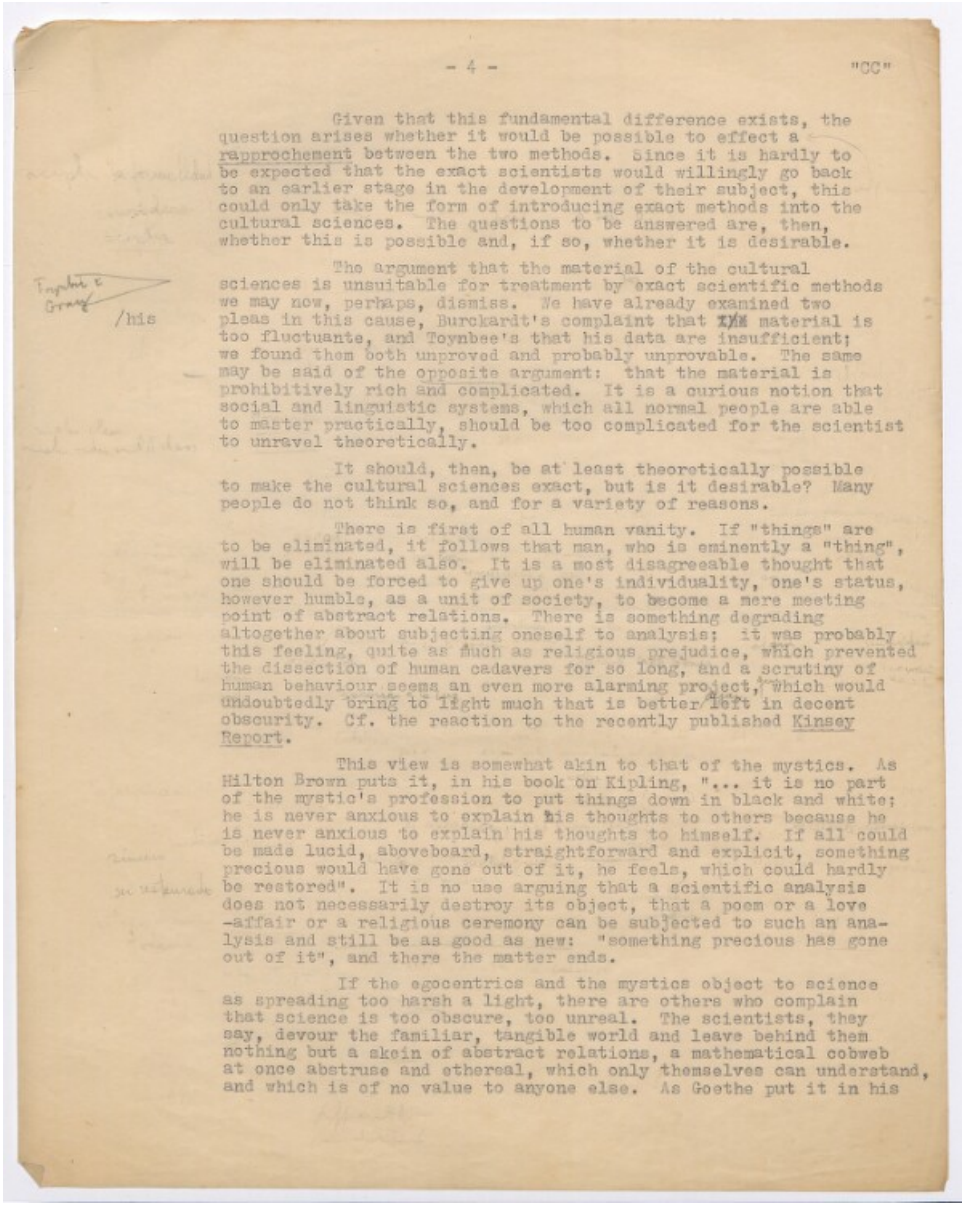
Our everyday commonsensical thinking is based on the Aristotelian trichotomy of things, qualities, and functions: a thing, e.g. a chair, exists, it has certain qualities, such as size, shape, colour, rigidity, and it performs certain functions, such as supporting the weight of a human being. This is a perfectly reasonable view, in fact it seems almost to impose itself as a necessity: a thing must exist before it can have any qualities, and it must have qualities before it can perform any functions, since the functions it performs are conditioned by its qualities --a chair cannot perform the same functions as a typewriter because it has not got the same qualities as a typewriter. Nevertheless, exact science has found it profitable to abandon this view or, rather, to abstract from it, for even the most rabid physicist could hardly put up with living in the kind of nightmare universe conjured up by Eddington.

/methodological The/advantages to be derived from concentrating on relations to the exclusion of everything else, are far-reaching. To such a view the universe is homogeneous: all differences are differences of, measurable, degree; and differences of kind, which are very troublesome, vanish completely. This, of course, makes the scientist's work considerably easier than it would otherwise have been, and it allows a generalisation, a unification and simplification throughout the vast domain of exact science which is undreamt of in any other department of human endeavour. It would perhaps be too much to say that there is already only one exact science, but it is evident that the development is in that direction.

The ciencias culturales, on the other hand, have taken over from common sense the Aristotelian picture of the world. To the historian, the linguist, etc. the data to be ascertained and recorded are still "things", each with its qualities and its functions, and even when we get as far as arranging our data in classes, these classes are defined by qualities rather than by relations.

This has the corresponding disadvantages. A "thing" is always unique: no two"things" are exactly alike. To such a view, therefore, the universe is heterogeneous, and all differences are differences of kind, which cannot be measured but can, in fact, only be ascertained and recorded in terms of "conceptos lo más flexibles y abiertos". It is a necessary consequence of this method that no systematisation is possible and that such far-reaching generalisations as have been achieved by the exact sciences are entirely out of the question.

We have here the fundamental difference between the exact and the cultural sciences: the cultural sciences do not analyse their data, or if they do, the analysis does not go nearly as far as that of the exact sciences; it does not go beyond the "thing" as a unit.



Given that this fundamental difference exists, the question arises whether it would be possible to effect a rapprochement between the two methods. Since it is hardly to be expected that the exact scientists would willingly go back to an earlier stage in the development of their subject, this could only take the form of introducing exact methods into the cultural sciences. The questions to be answered are, then, whether this is possible and, if so, whether it is desirable.

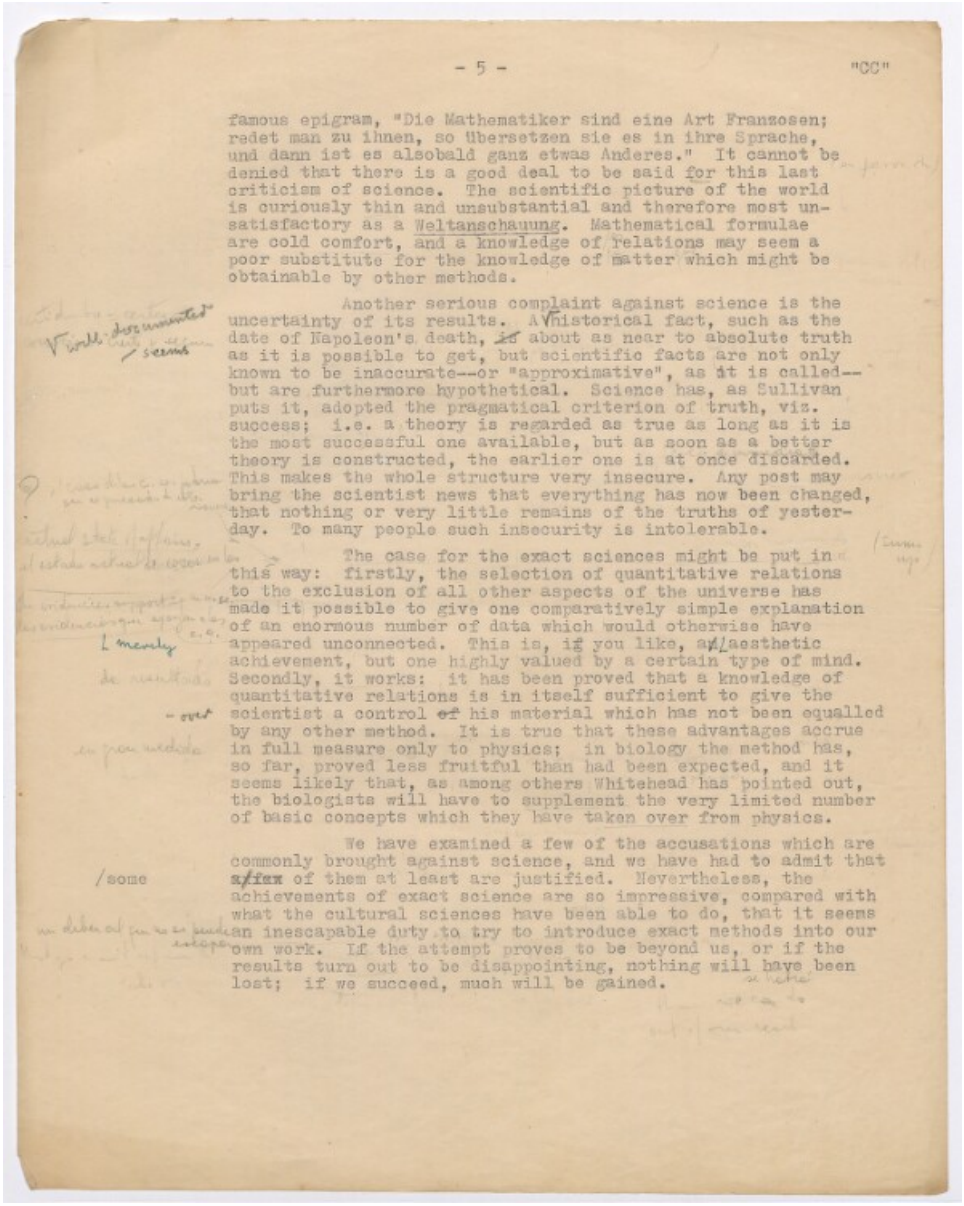
The argument that the material of the cultural sciences is unsuitable for treatment by exact scientific methods we may now, perhaps, dismiss. We have already examined two pleas in this cause, Burckardt's complaint that ~~the~~ material is too fluctuante, and Toynbee's that his data are insufficient; we found them both unproved and probably unprovable. The same may be said of the opposite argument: that the material is prohibitively rich and complicated. It is a curious notion that social and linguistic systems, which all normal people are able to master practically, should be too complicated for the scientist to unravel theoretically.

It should, then, be at least theoretically possible to make the cultural sciences exact, but is it desirable? Many people do not think so, and for a variety of reasons.

There is first of all human vanity. If "things" are to be eliminated, it follows that man, who is eminently a "thing", will be eliminated also. It is a most disagreeable thought that one should be forced to give up one's individuality, one's status, however humble, as a unit of society, to become a mere meeting point of abstract relations. There is something degrading altogether about subjecting oneself to analysis; it was probably this feeling, quite as much as religious prejudice, which prevented the dissection of human cadavers for so long, and a scrutiny of human behaviour seems an even more alarming project, which would undoubtedly bring to light much that is better left in decent obscurity. Cf. the reaction to the recently published Kinsey Report.

This view is somewhat akin to that of the mystics. As Hilton Brown puts it, in his book on Kipling, "... it is no part of the mystic's profession to put things down in black and white; he is never anxious to explain his thoughts to others because he is never anxious to explain his thoughts to himself. If all could be made lucid, aboveboard, straightforward and explicit, something precious would have gone out of it, he feels, which could hardly be restored". It is no use arguing that a scientific analysis does not necessarily destroy its object, that a poem or a love-affair or a religious ceremony can be subjected to such an analysis and still be as good as new: "something precious has gone out of it", and there the matter ends.

If the egocentrics and the mystics object to science as spreading too harsh a light, there are others who complain that science is too obscure, too unreal. The scientists, they say, devour the familiar, tangible world and leave behind them nothing but a skein of abstract relations, a mathematical cobweb at once abstruse and ethereal, which only themselves can understand, and which is of no value to anyone else. As Goethe put it in his

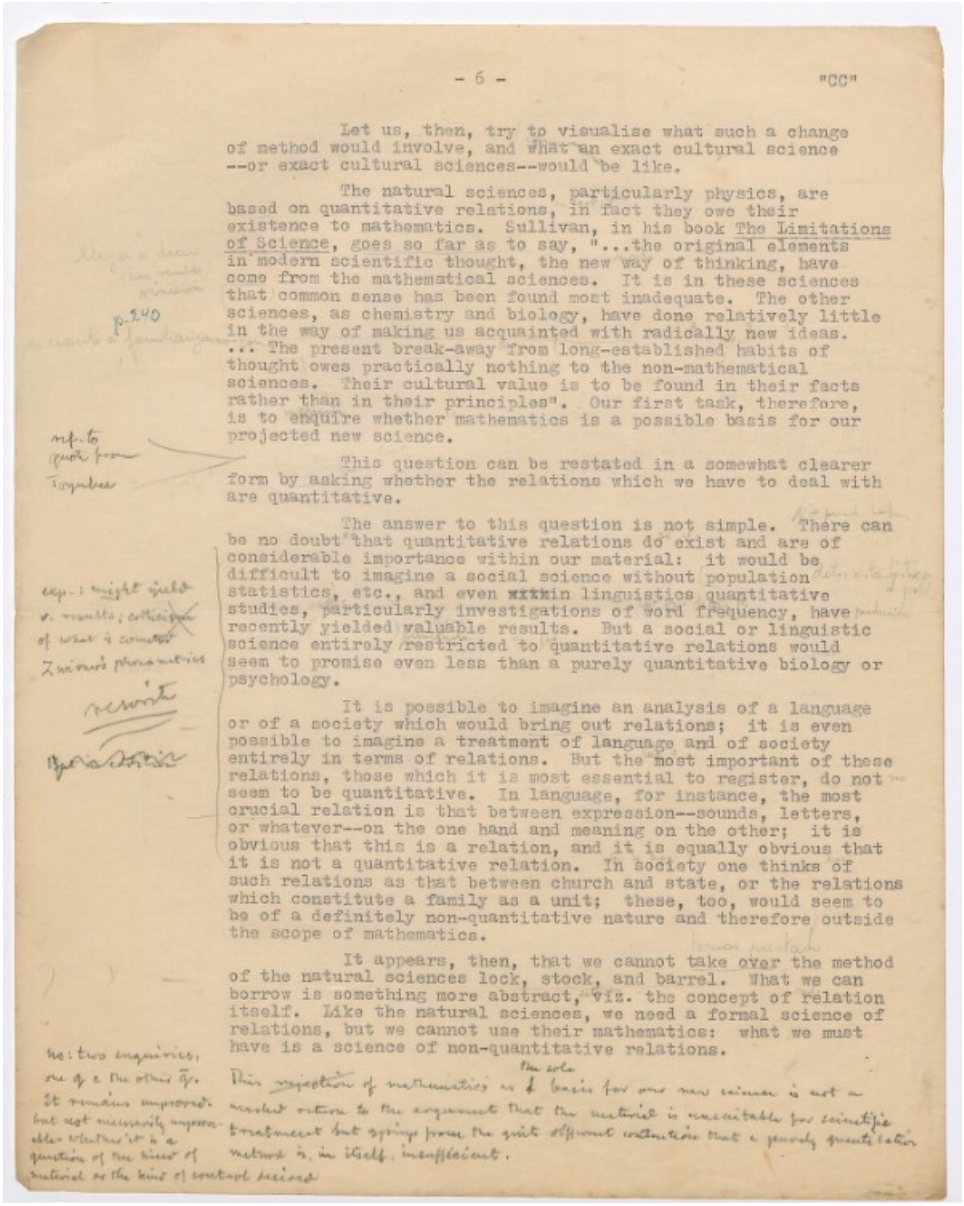


famous epigram, "Die Mathematiker sind eine Art Franzosen; redet man zu ihnen, so übersetzen sie es in ihre Sprache, und dann ist es alsobald ganz etwas Anderes." It cannot be denied that there is a good deal to be said for this last criticism of science. The scientific picture of the world is curiously thin and unsubstantial and therefore most unsatisfactory as a Weltanschauung. Mathematical formulae are cold comfort, and a knowledge of relations may seem a poor substitute for the knowledge of matter which might be obtainable by other methods.

Another serious complaint against science is the uncertainty of its results. A historical fact, such as the date of Napoleon's death, is about as near to absolute truth as it is possible to get, but scientific facts are not only known to be inaccurate--or "approximative", as it is called--but are furthermore hypothetical. Science has, as Sullivan puts it, adopted the pragmatical criterion of truth, viz. success; i.e. a theory is regarded as true as long as it is the most successful one available, but as soon as a better theory is constructed, the earlier one is at once discarded. This makes the whole structure very insecure. Any post may bring the scientist news that everything has now been changed, that nothing or very little remains of the truths of yesterday. To many people such insecurity is intolerable.

The case for the exact sciences might be put in this way: firstly, the selection of quantitative relations to the exclusion of all other aspects of the universe has made it possible to give one comparatively simple explanation of an enormous number of data which would otherwise have appeared unconnected. This is, if you like, an aesthetic achievement, but one highly valued by a certain type of mind. Secondly, it works: it has been proved that a knowledge of quantitative relations is in itself sufficient to give the scientist a control of his material which has not been equalled by any other method. It is true that these advantages accrue in full measure only to physics; in biology the method has, so far, proved less fruitful than had been expected, and it seems likely that, as among others Whitehead has pointed out, the biologists will have to supplement the very limited number of basic concepts which they have taken over from physics.

We have examined a few of the accusations which are commonly brought against science, and we have had to admit that some of them at least are justified. Nevertheless, the achievements of exact science are so impressive, compared with what the cultural sciences have been able to do, that it seems an inescapable duty to try to introduce exact methods into our own work. If the attempt proves to be beyond us, or if the results turn out to be disappointing, nothing will have been lost; if we succeed, much will be gained.



Let us, then, try to visualise what such a change of method would involve, and what an exact cultural science --or exact cultural sciences--would be like.

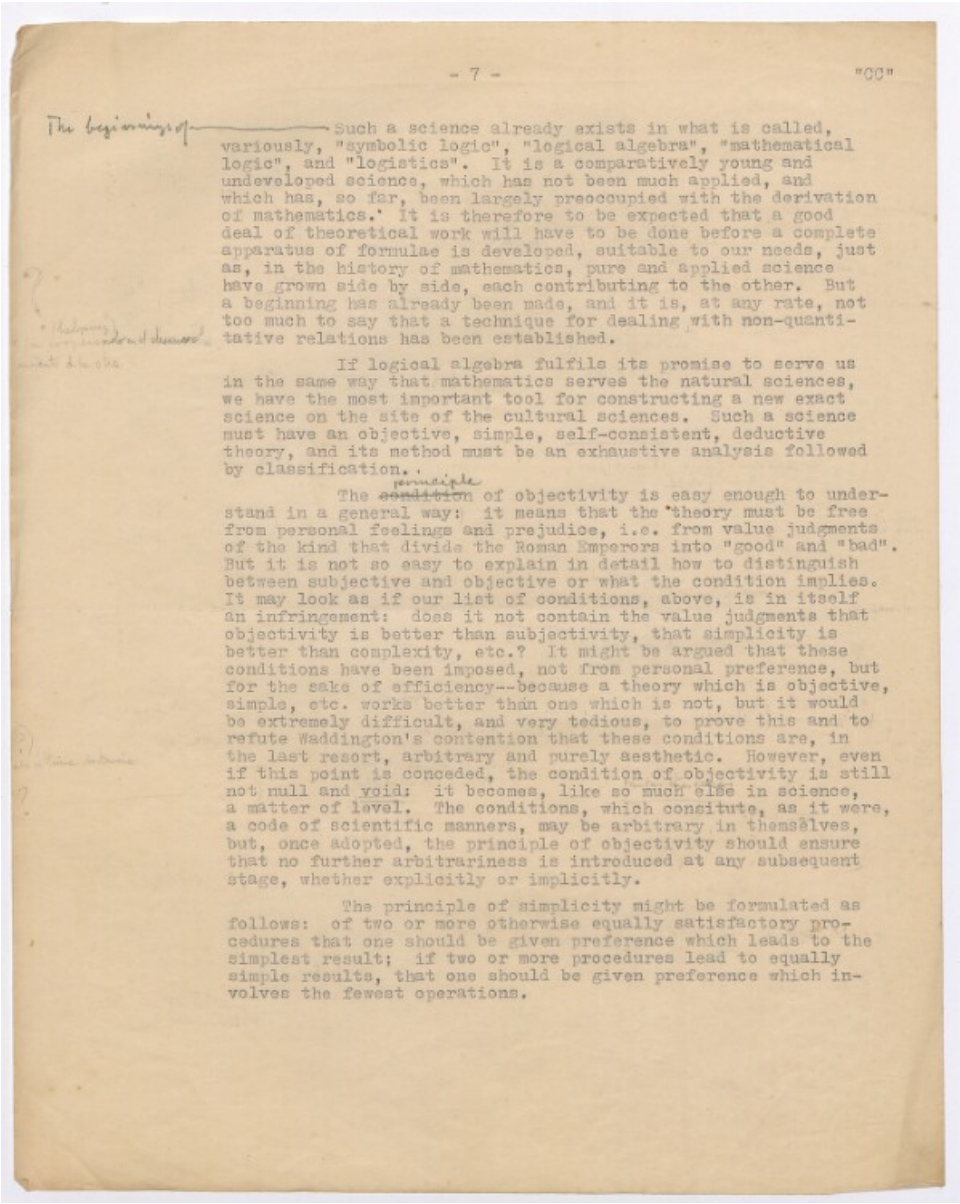
The natural sciences, particularly physics, are based on quantitative relations, in fact they owe their existence to mathematics. Sullivan, in his book The Limitations of Science, goes so far as to say, "...the original elements in modern scientific thought, the new way of thinking, have come from the mathematical sciences. It is in these sciences that common sense has been found most inadequate. The other sciences, as chemistry and biology, have done relatively little in the way of making us acquainted with radically new ideas. ... The present break-away from long-established habits of thought owes practically nothing to the non-mathematical sciences. Their cultural value is to be found in their facts rather than in their principles". Our first task, therefore, is to enquire whether mathematics is a possible basis for our projected new science.

This question can be restated in a somewhat clearer form by asking whether the relations which we have to deal with are quantitative.

The answer to this question is not simple. There can be no doubt that quantitative relations do exist and are of considerable importance within our material: it would be difficult to imagine a social science without population statistics, etc., and even ~~xxx~~in linguistics quantitative studies, particularly investigations of word frequency, have recently yielded valuable results. But a social or linguistic science entirely restricted to quantitative relations would seem to promise even less than a purely quantitative biology or psychology.

It is possible to imagine an analysis of a language or of a society which would bring out relations; it is even possible to imagine a treatment of language and of society entirely in terms of relations. But the most important of these relations, those which it is most essential to register, do not seem to be quantitative. In language, for instance, the most crucial relation is that between expression--sounds, letters, or whatever--on the one hand and meaning on the other; it is obvious that this is a relation, and it is equally obvious that it is not a quantitative relation. In society one thinks of such relations as that between church and state, or the relations which constitute a family as a unit; these, too, would seem to be of a definitely non-quantitative nature and therefore outside the scope of mathematics.

It appears, then, that we cannot take over the method of the natural sciences lock, stock, and barrel. What we can borrow is something more abstract, viz. the concept of relation itself. Like the natural sciences, we need a formal science of relations, but we cannot use their mathematics: what we must have is a science of non-quantitative relations.

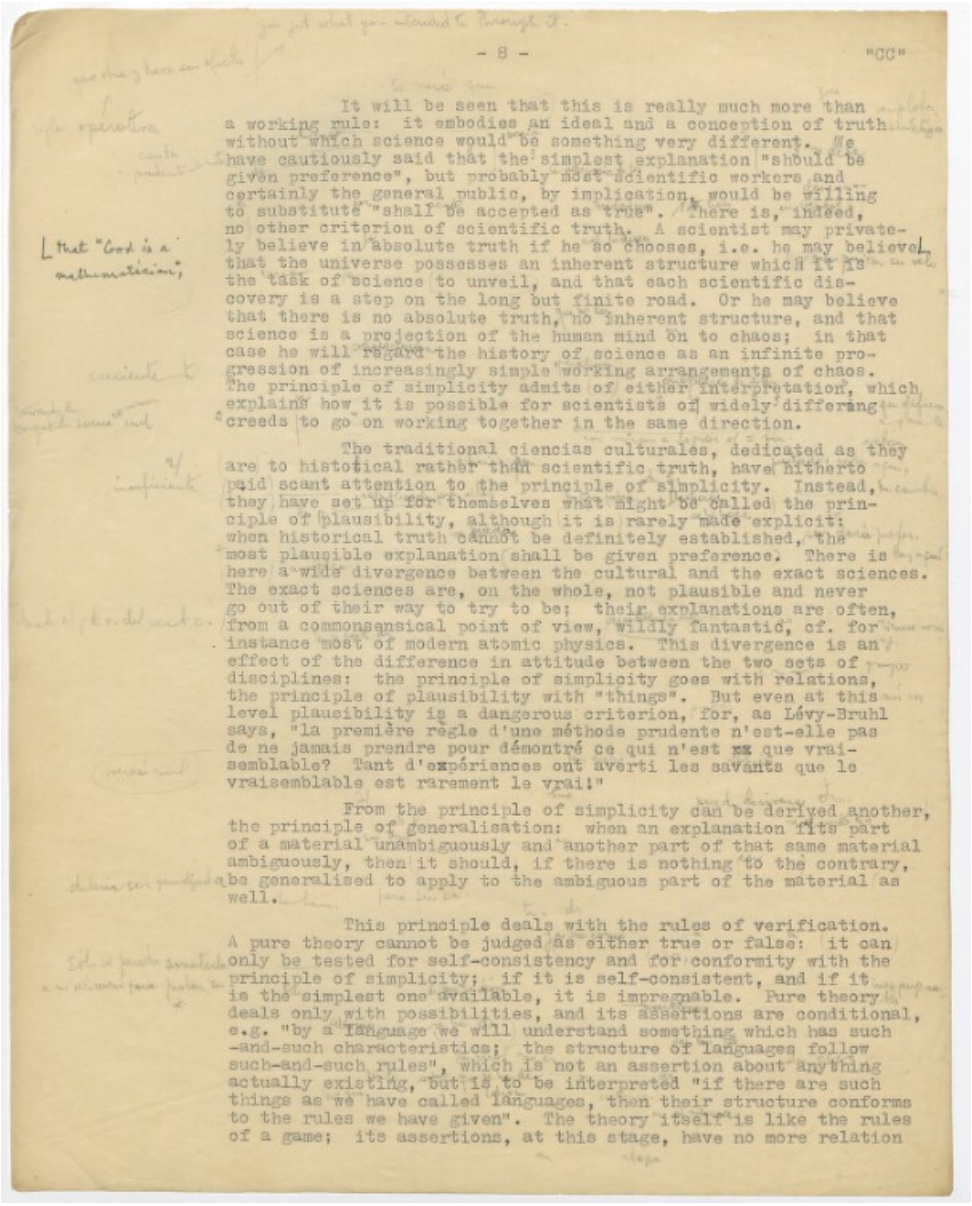


The beginnings of Such a science already exists in what is called, variously, "symbolic logic", "logical algebra", "mathematical logic", and "logistics". It is a comparatively young and undeveloped science, which has not been much applied, and which has, so far, been largely preoccupied with the derivation of mathematics.' It is therefore to be expected that a good deal of theoretical work will have to be done before a complete apparatus of formulae is developed, suitable to our needs, just as, in the history of mathematics, pure and applied science have grown side by side, each contributing to the other. But a beginning has already been made, and it is, at any rate, not too much to say that a technique for dealing with non-quantitative relations has been established.

If logical algebra fulfils its promise to serve us in the same way that mathematics serves the natural sciences, we have the most important tool for constructing a new exact science on the site of the cultural sciences. Such a science must have an objective, simple, self-consistent, deductive theory, and its method must be an exhaustive analysis followed by classification.'

The ~~condition~~ principle of objectivity is easy enough to understand in a general way: it means that the theory must be free from personal feelings and prejudice, i.e. from value judgments of the kind that divide the Roman Emperors into "good" and "bad". But it is not so easy to explain in detail how to distinguish between subjective and objective or what the condition implies. It may look as if our list of conditions, above, is in itself an infringement: does it not contain the value judgments that objectivity is better than subjectivity, that simplicity is better than complexity, etc.? It might be argued that these conditions have been imposed, not from personal preference, but for the sake of efficiency--because a theory which is objective, simple, etc. works better than one which is not, but it would be extremely difficult, and very tedious, to prove this and to refute Waddington's contention that these conditions are, in the last resort, arbitrary and purely aesthetic. However, even if this point is conceded, the condition of objectivity is still not null and void: it becomes, like so much else in science, a matter of level. The conditions, which consitute, as it were, a code of scientific manners, may be arbitrary in themselves, but, once adopted, the principle of objectivity should ensure that no further arbitrariness is introduced at any subsequent stage, whether explicitly or implicitly.

The principle of simplicity might be formulated as follows: of two or more otherwise equally satisfactory procedures that one should be given preference which leads to the simplest result; if two or more procedures lead to equally simple results, that one should be given preference which involves the fewest operations.



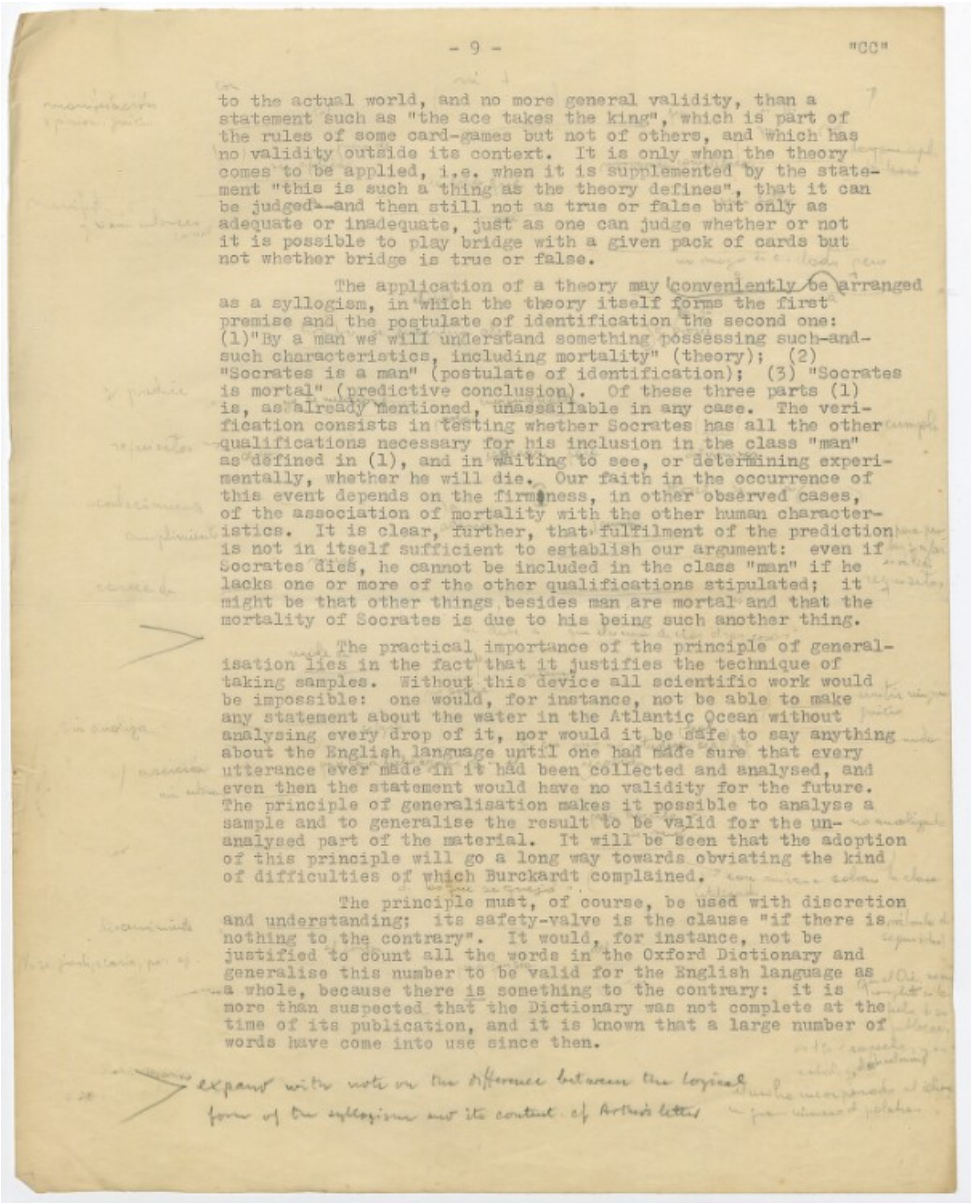
It will be seen that this is really much more than a working rule: it embodies an ideal and a conception of truth without which science would be something very different. We have cautiously said that the simplest explanation "should be given preference", but probably most scientific workers and certainly the general public, by implication, would be willing to substitute "shall be accepted as true". There is, indeed, no other criterion of scientific truth. A scientist may privately believe in absolute truth if he so chooses, i.e. he may believe that the universe possesses an inherent structure which it is the task of science to unveil, and that each scientific discovery is a step on the long but finite road. Or he may believe that there is no absolute truth, no inherent structure, and that science is a projection of the human mind on to chaos; in that case he will regard the history of science as an infinite progression of increasingly simple working arrangements of chaos. The principle of simplicity admits of either interpretation, which explains how it is possible for scientists of widely differing creeds to go on working together in the same direction.

that "God is a mathematician";

The traditional ciencias culturales, dedicated as they are to histotical rather than scientific truth, have hitherto paid scant attention to the principle of simplicity. Instead, they have set up for themselves what might be called the principle of plausibility, although it is rarely made explicit: when historical truth cannot be definitely established, the most plausible explanation shall be given preference. There is here a wide divergence between the cultural and the exact sciences. The exact sciences are, on the whole, not plausible and never go out of their way to try to be; their explanations are often, from a commonsensical point of view, wildly fantastic, cf. for instance most of modern atomic physics. This divergence is an effect of the difference in attitude between the two sets of disciplines: the principle of simplicity goes with relations, the principle of plausibility with "things". But even at this level plausibility is a dangerous criterion, for, as Lévy-Bruhl says, "la première règle d'une méthode prudente n'est-elle pas de ne jamais prendre pour démontré ce qui n'est ~~ss~~ que vraisemblable? Tant d'expériences ont averti les savants que le vraisemblable est rarement le vrai!"

From the principle of simplicity can be derived another, the principle of generalisation: when an explanation fits part of a material unambiguously and another part of that same material ambiguously, then it should, if there is nothing to the contrary, be generalised to apply to the ambiguous part of the material as well.

This principle deals with the rules of verification. A pure theory cannot be judged as either true or false: it can only be tested for self-consistency and for conformity with the principle of simplicity; if it is self-consistent, and if it is the simplest one available, it is impregnable. Pure theory deals only with possibilities, and its assertions are conditional, e.g. "by a language we will understand something which has such-and-such characteristics; the structure of languages follow such-and-such rules", which is not an assertion about anything actually existing, but is to be interpreted "if there are such things as we have called languages, then their structure conforms to the rules we have given". The theory itself is like the rules of a game; its assertions, at this stage, have no more relation



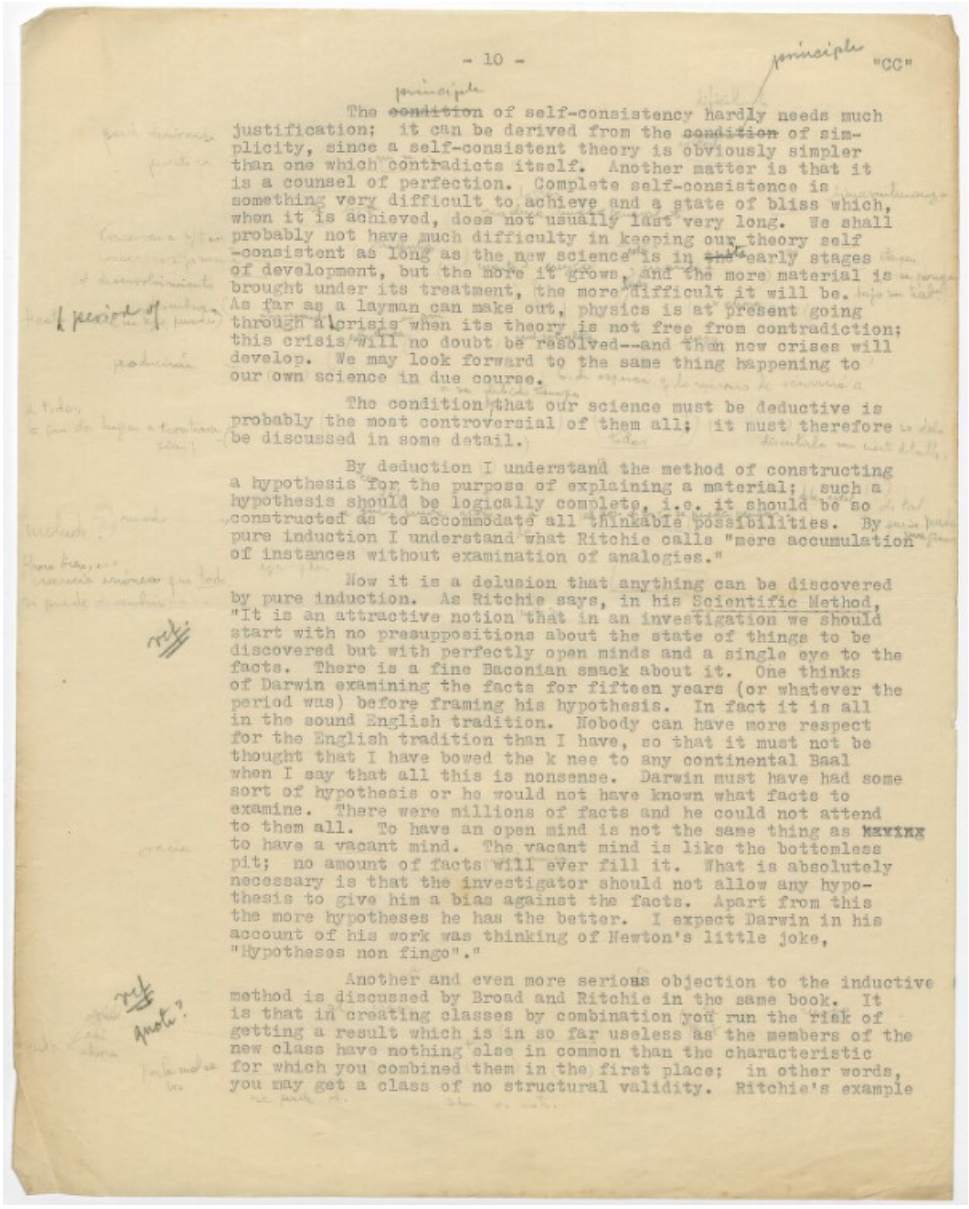
to the actual world, and no more general validity, than a statement such as "the ace takes the king", which is part of the rules of some card-games but not of others, and which has no validity outside its context. It is only when the theory comes to be applied, i.e. when it is supplemented by the statement "this is such a thing as the theory defines", that it can be judged - and then still not as true or false but only as adequate or inadequate, just as one can judge whether or not it is possible to play bridge with a given pack of cards but not whether bridge is true or false.

The application of a theory may conveniently be arranged as a syllogism, in which the theory itself forms the first premise and the postulate of identification the second one: (1)"By a man we will understand something possessing such-and-such characteristics, including mortality" (theory); (2) "Socrates is a man" (postulate of identification); (3) "Socrates is mortal" (predictive conclusion). Of these three parts (1) is, as already mentioned, unassailable in any case. The verification consists in testing whether Socrates has all the other qualifications necessary for his inclusion in the class "man" as defined in (1), and in waiting to see, or determining experimentally, whether he will die. Our faith in the occurrence of this event depends on the firmness, in other observed cases, of the association of mortality with the other human characteristics. It is clear, further, that fulfilment of the prediction is not in itself sufficient to establish our argument: even if Socrates dies, he cannot be included in the class "man" if he lacks one or more of the other qualifications stipulated; it might be that other things besides man are mortal and that the mortality of Socrates is due to his being such another thing.

The practical importance of the principle of generalisation lies in the fact that it justifies the technique of taking samples. Without this device all scientific work would be impossible: one would, for instance, not be able to make any statement about the water in the Atlantic Ocean without analysing every drop of it, nor would it be safe to say anything about the English language until one had made sure that every utterance ever made in it had been collected and analysed, and even then the statement would have no validity for the future. The principle of generalisation makes it possible to analyse a sample and to generalise the result to be valid for the unanalysed part of the material. It will be seen that the adoption of this principle will go a long way towards obviating the kind of difficulties of which Burckardt complained.

The principle must, of course, be used with discretion and understanding; its safety-valve is the clause "if there is nothing to the contrary". It would, for instance, not be justified to count all the words in the Oxford Dictionary and generalise this number to be valid for the English language as a whole, because there is something to the contrary: it is more than suspected that the Dictionary was not complete at the time of its publication, and it is known that a large number of words have come into use since then.

expand with note on the difference between the logical form of the syllogism and its content. cf Arthur's letter



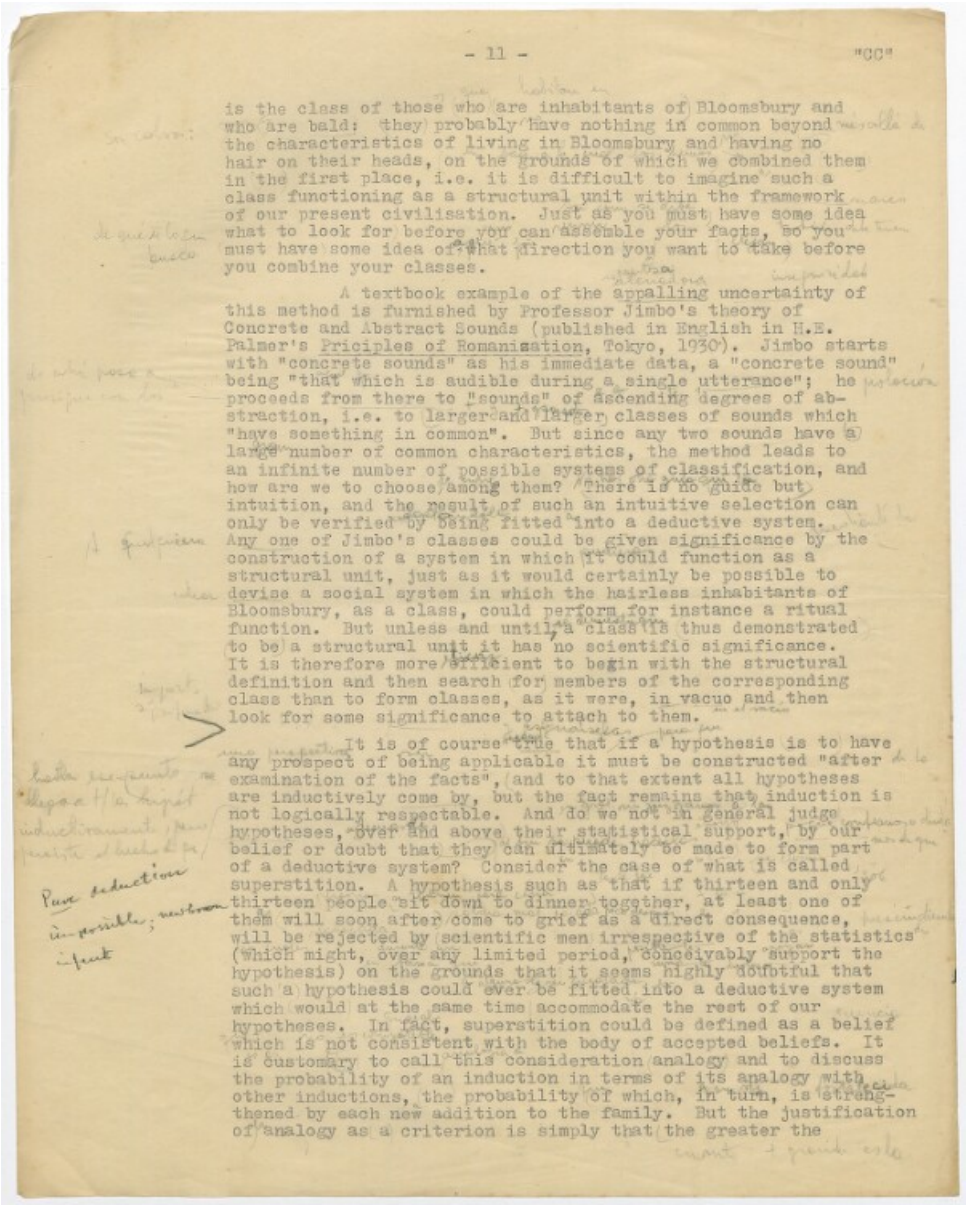
The ~~condition~~ principle of self-consistency hardly needs much justification; it can be derived from the ~~condition~~ principle "CC" of simplicity, since a self-consistent theory is obviously simpler than one which contradicts itself. Another matter is that it is a counsel of perfection. Complete self-consistence is something very difficult to achieve and a state of bliss which, when it is achieved, does not usually last very long. We shall probably not have much difficulty in keeping our theory self-consistent as long as the new science is in ~~the~~ early stages of development, but the more it grows, and the more material is brought under its treatment, the more difficult it will be. As far as a layman can make out, physics is at present going through a period of crisis when its theory is not free from contradiction; this crisis will no doubt be resolved--and then new crises will develop. We may look forward to the same thing happening to our own science in due course.

The condition that our science must be deductive is probably the most controversial of them all; it must therefore be discussed in some detail.

By deduction I understand the method of constructing a hypothesis for the purpose of explaining a material; such a hypothesis should be logically complete, i.e. it should be so constructed as to accommodate all thinkable possibilities. By pure induction I understand what Ritchie calls "mere accumulation of instances without examination of analogies."

Now it is a delusion that anything can be discovered by pure induction. As Ritchie says, in his Scientific Method, "It is an attractive notion that in an investigation we should start with no presuppositions about the state of things to be discovered but with perfectly open minds and a single eye to the facts. There is a fine Baconian smack about it. One thinks of Darwin examining the facts for fifteen years (or whatever the period was) before framing his hypothesis. In fact it is all in the sound English tradition. Nobody can have more respect for the English tradition than I have, so that it must not be thought that I have bowed the k nee to any continental Baal when I say that all this is nonsense. Darwin must have had some sort of hypothesis or he would not have known what facts to examine. There were millions of facts and he could not attend to them all. To have an open mind is not the same thing as HAVING to have a vacant mind. The vacant mind is like the bottomless pit; no amount of facts will ever fill it. What is absolutely necessary is that the investigator should not allow any hypothesis to give him a bias against the facts. Apart from this the more hypotheses he has the better. I expect Darwin in his account of his work was thinking of Newton's little joke, "Hypotheses non fingo"."

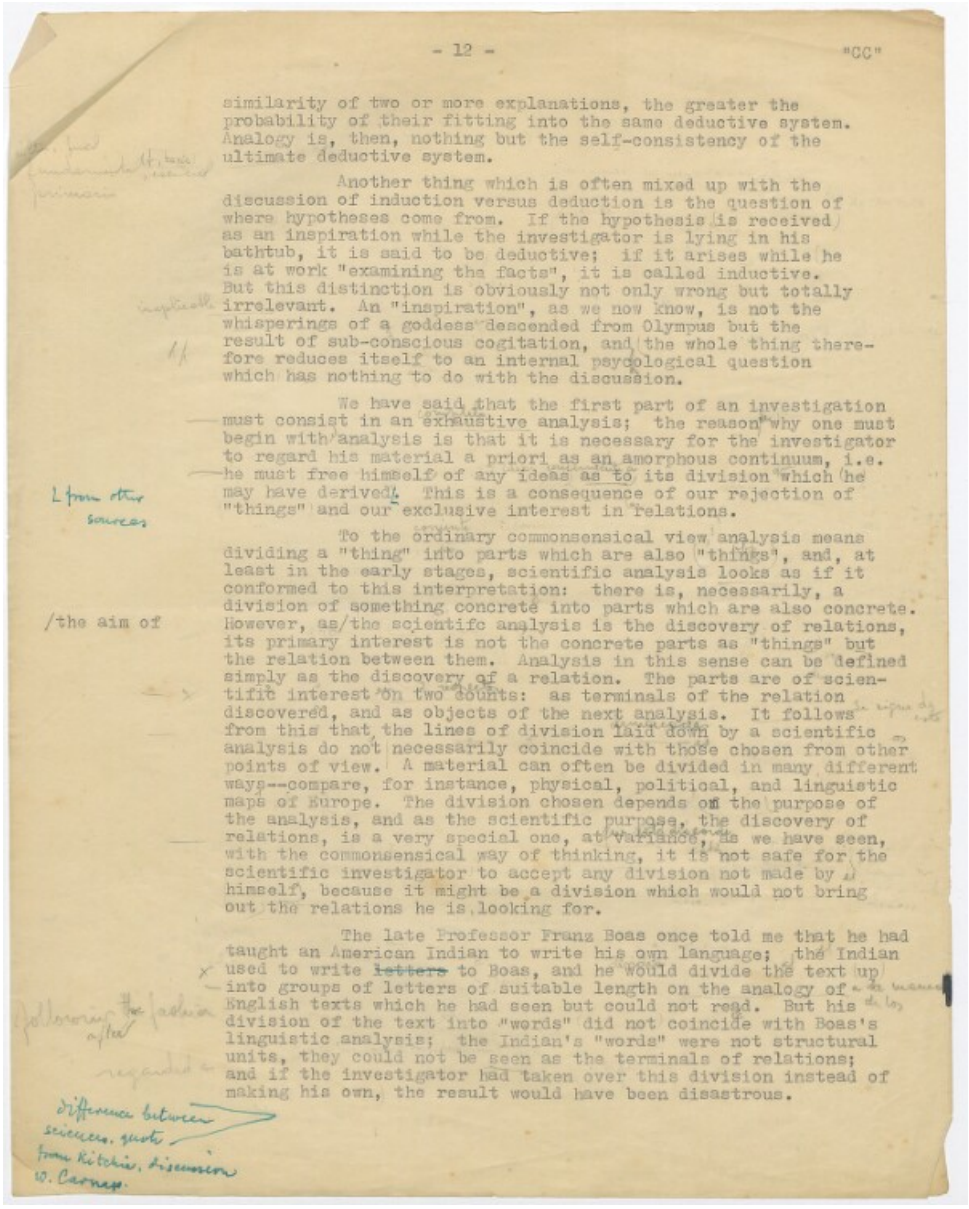
Another and even more serious objection to the inductive method is discussed by Broad and Ritchie in the same book. It is that in creating classes by combination you run the risk of getting a result which is in so far useless as the members of the new class have nothing else in common than the characteristic for which you combined them in the first place; in other words, you may get a class of no structural validity. Ritchie's example



is the class of those who are inhabitants of Bloomsbury and who are bald: they probably have nothing in common beyond the characteristics of living in Bloomsbury and having no hair on their heads, on the grounds of which we combined them in the first place, i.e. it is difficult to imagine such a class functioning as a structural unit within the framework of our present civilisation. Just as you must have some idea what to look for before you can assemble your facts, so you must have some idea of what direction you want to take before you combine your classes.

A textbook example of the appalling uncertainty of this method is furnished by Professor Jimbo's theory of Concrete and Abstract Sounds (published in English in H.E. Palmer's Principles of Romanisation, Tokyo, 1930). Jimbo starts with "concrete sounds" as his immediate data, a "concrete sound" being "that which is audible during a single utterance"; he proceeds from there to "sounds" of ascending degrees of abstraction, i.e. to larger and larger classes of sounds which "have something in common". But since any two sounds have a large number of common characteristics, the method leads to an infinite number of possible systems of classification, and how are we to choose among them? There is no guide but intuition, and the result of such an intuitive selection can only be verified by being fitted into a deductive system. Any one of Jimbo's classes could be given significance by the construction of a system in which it could function as a structural unit, just as it would certainly be possible to devise a social system in which the hairless inhabitants of Bloomsbury, as a class, could perform for instance a ritual function. But unless and until a class is thus demonstrated to be a structural unit it has no scientific significance. It is therefore more efficient to begin with the structural definition and then search for members of the corresponding class than to form classes, as it were, in vacuo and then look for some significance to attach to them.

It is of course true that if a hypothesis is to have any prospect of being applicable it must be constructed "after examination of the facts", and to that extent all hypotheses are inductively come by, but the fact remains that induction is not logically respectable. And do we not in general judge hypotheses, over and above their statistical support, by our belief or doubt that they can ultimately be made to form part of a deductive system? Consider the case of what is called superstition. A hypothesis such as that if thirteen and only thirteen people sit down to dinner together, at least one of them will soon after come to grief as a direct consequence, will be rejected by scientific men irrespective of the statistics (which might, over any limited period, conceivably support the hypothesis) on the grounds that it seems highly doubtful that such a hypothesis could ever be fitted into a deductive system which would at the same time accommodate the rest of our hypotheses. In fact, superstition could be defined as a belief which is not consistent with the body of accepted beliefs. It is customary to call this consideration analogy and to discuss the probability of an induction in terms of its analogy with other inductions, the probability of which, in turn, is strengthened by each new addition to the family. But the justification of analogy as a criterion is simply that the greater the



similarity of two or more explanations, the greater the probability of their fitting into the same deductive system. Analogy is, then, nothing but the self-consistency of the ultimate deductive system.

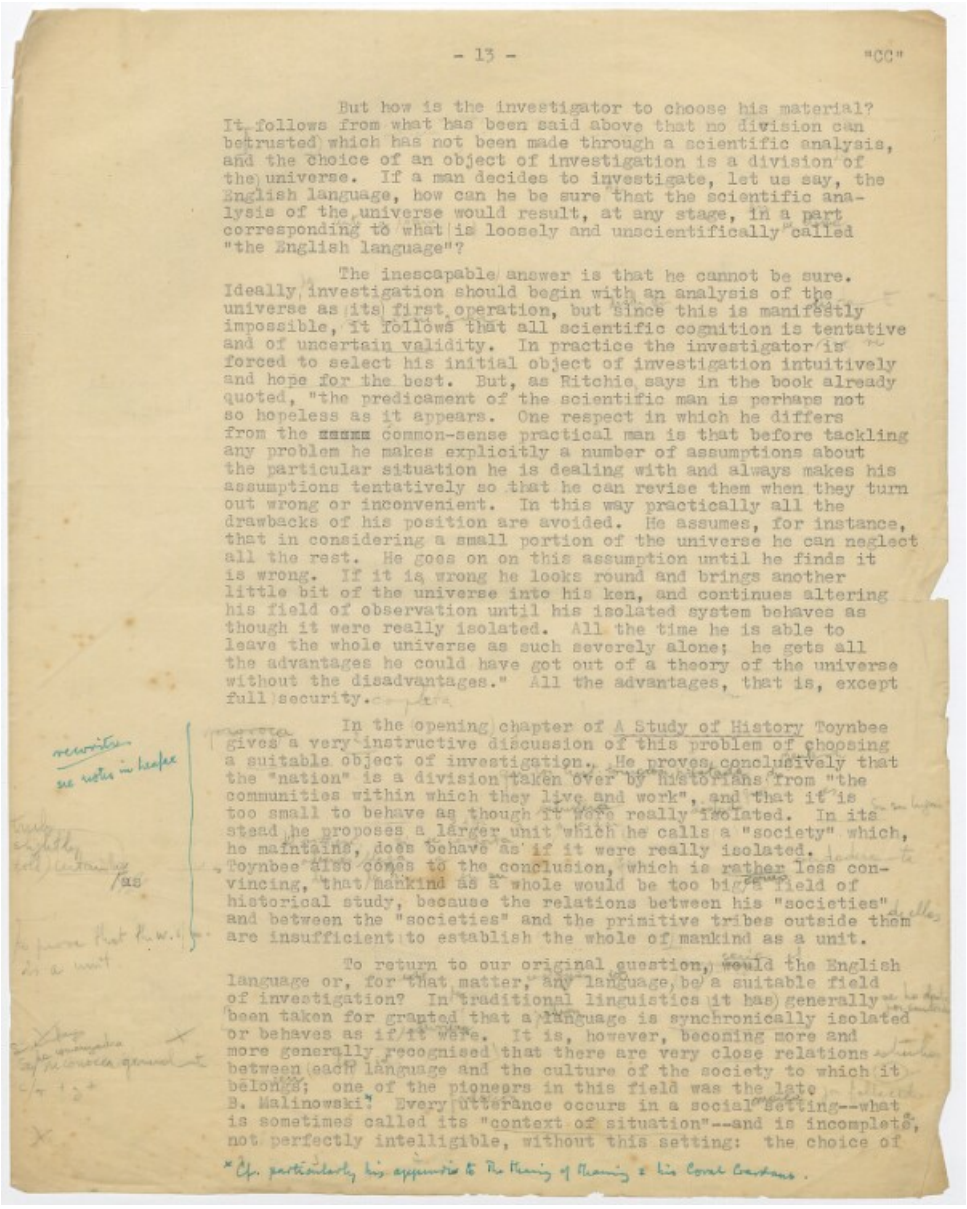
Another thing which is often mixed up with the discussion of induction versus deduction is the question of where hypotheses come from. If the hypothesis is received as an inspiration while the investigator is lying in his bathtub, it is said to be deductive; if it arises while he is at work "examining the facts", it is called inductive. But this distinction is obviously not only wrong but totally irrelevant. An "inspiration", as we now know, is not the whisperings of a goddess descended from Olympus but the result of sub-conscious cogitation, and the whole thing therefore reduces itself to an internal psycological question which has nothing to do with the discussion.

We have said that the first part of an investigation must consist in an exhaustive analysis; the reason why one must begin with analysis is that it is necessary for the investigator to regard his material a priori as an amorphous continuum, i.e. he must free himself of any ideas as to its division which he may have derived from other sources. This is a consequence of our rejection of "things" and our exclusive interest in relations.

To the ordinary commonsensical view analysis means dividing a "thing" into parts which are also "things", and, at least in the early stages, scientific analysis looks as if it conformed to this interpretation: there is, necessarily, a division of something concrete into parts which are also concrete. However, as the aim of scientifc analysis is the discovery of relations, its primary interest is not the concrete parts as "things" but the relation between them. Analysis in this sense can be defined simply as the discovery of a relation. The parts are of scientifit interest on two counts: as terminals of the relation discovered, and as objects of the next analysis. It follows from this that the lines of division laid down by a scientific analysis do not necessarily coincide with those chosen from other points of view. A material can often be divided in many different ways--compare, for instance, physical, political, and linguistic maps of Europe. The division chosen depends on the purpose of the analysis, and as the scientific purpose, the discovery of relations, is a very special one, at variance, as we have seen, with the commonsensical way of thinking, it is not safe for the scientific investigator to accept any division not made by himself, because it might be a division which would not bring out the relations he is looking for.

The late Professor Franz Boas once told me that he had taught an American Indian to write his own language; the Indian used to write ~~letters~~ to Boas, and he would divide the text up into groups of letters of suitable length on the analogy of English texts which he had seen but could not read. But his division of the text into "words" did not coincide with Boas's linguistic analysis; the Indian's "words" were not structural units, they could not be seen as the terminals of relations; and if the investigator had taken over this division instead of making his own, the result would have been disastrous.

difference between sciences, quote from Kitchis, discussion w. Carnap.



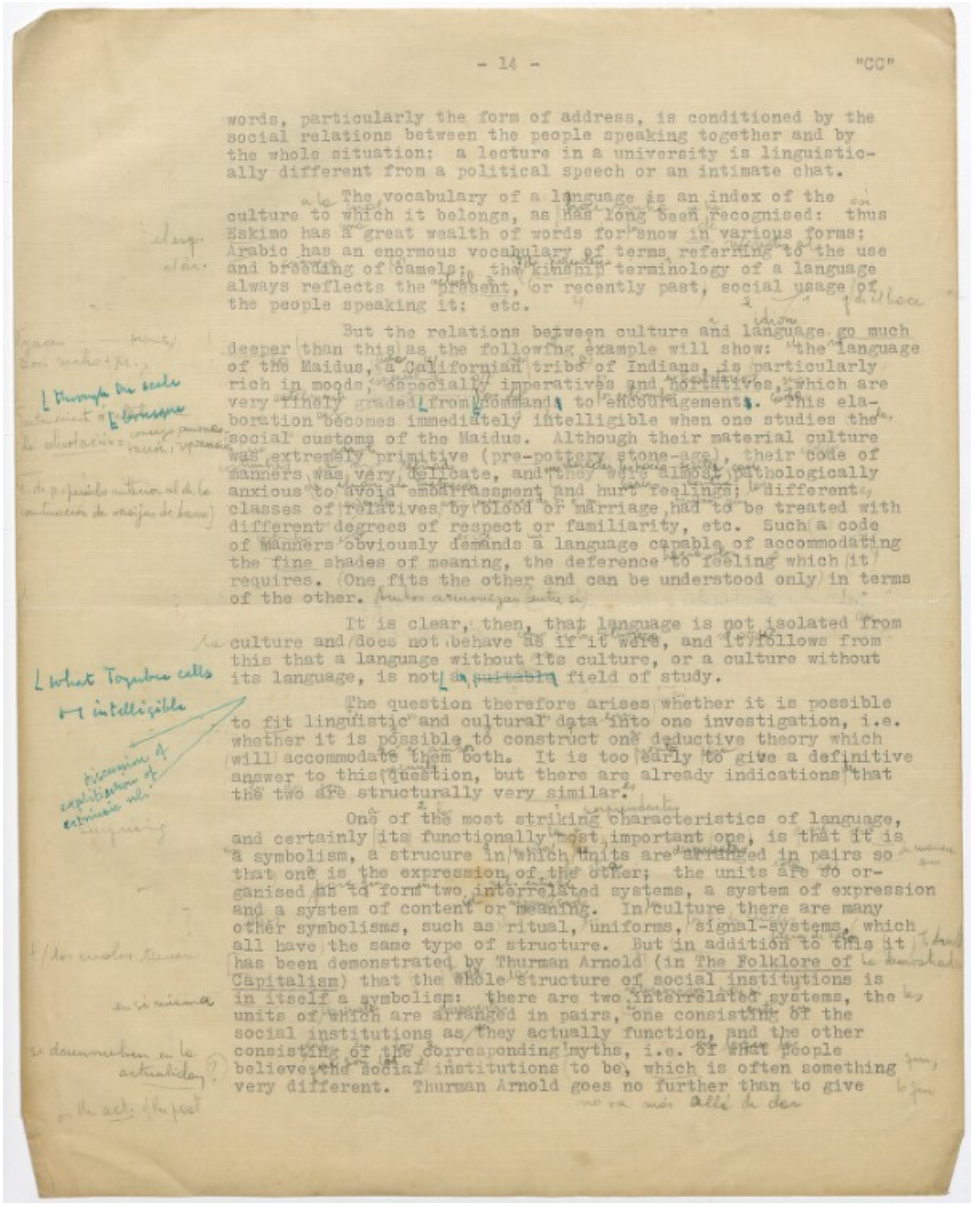
But how is the investigator to choose his material? It follows from what has been said above that no division can betrusted which has not been made through a scientific analysis, and the choice of an object of investigation is a division of the universe. If a man decides to investigate, let us say, the English language, how can he be sure that the scientific analysis of the universe would result, at any stage, in a part corresponding to what is loosely and unscientifically called "the English language"?

The inescapable answer is that he cannot be sure. Ideally investigation should begin with an analysis of the universe as its first operation, but since this is manifestly impossible, it follows that all scientific cognition is tentative and of uncertain validity. In practice the investigator is forced to select his initial object of investigation intuitively and hope for the best. But, as Ritchie says in the book already quoted, "the predicament of the scientific man is perhaps not so hopeless as it appears. One respect in which he differs from the ~~xxxxx~~ common-sense practical man is that before tackling any problem he makes explicitly a number of assumptions about the particular situation he is dealing with and always makes his assumptions tentatively so that he can revise them when they turn out wrong or inconvenient. In this way practically all the drawbacks of his position are avoided. He assumes, for instance, that in considering a small portion of the universe he can neglect all the rest. He goes on on this assumption until he finds it is wrong. If it is wrong he looks round and brings another little bit of the universe into his ken, and continues altering his field of observation until his isolated system behaves as though it were really isolated. All the time he is able to leave the whole universe as such severely alone; he gets all the advantages he could have got out of a theory of the universe without the disadvantages." All the advantages, that is, except full security.

In the opening chapter of A Study of History Toynbee gives a very instructive discussion of this problem of choosing a suitable object of investigation. He proves conclusively that the "nation" is a division taken over by historians from "the communities within which they live and work", and that it is too small to behave as though it were really isolated. In its stead he proposes a larger unit which he calls a "society" which, he maintains, does behave as if it were really isolated. Toynbee also comes to the conclusion, which is rather less convincing, that mankind as a whole would be too big a field of historical study, because the relations between his "societies" and between the "societies" and the primitive tribes outside them are insufficient to establish the whole of mankind as a unit.

To return to our original question, would the English language or, for that matter, any language, be a suitable field of investigation? In traditional linguistics it has generally been taken for granted that a language is synchronically isolated or behaves as if it were. It is, however, becoming more and more generally recognised that there are very close relations between each language and the culture of the society to which it belongs; one of the pioneers in this field was the late B. Malinowski* Every utterance occurs in a social setting--what is sometimes called its "context of situation"--and is incomplete, not perfectly intelligible, without this setting: the choice of

* Cf. particularly his appendix to The Meaning of Meaning : his Coral Gardens.



words, particularly the form of address, is conditioned by the social relations between the people speaking together and by the whole situation; a lecture in a university is linguistically different from a political speech or an intimate chat.

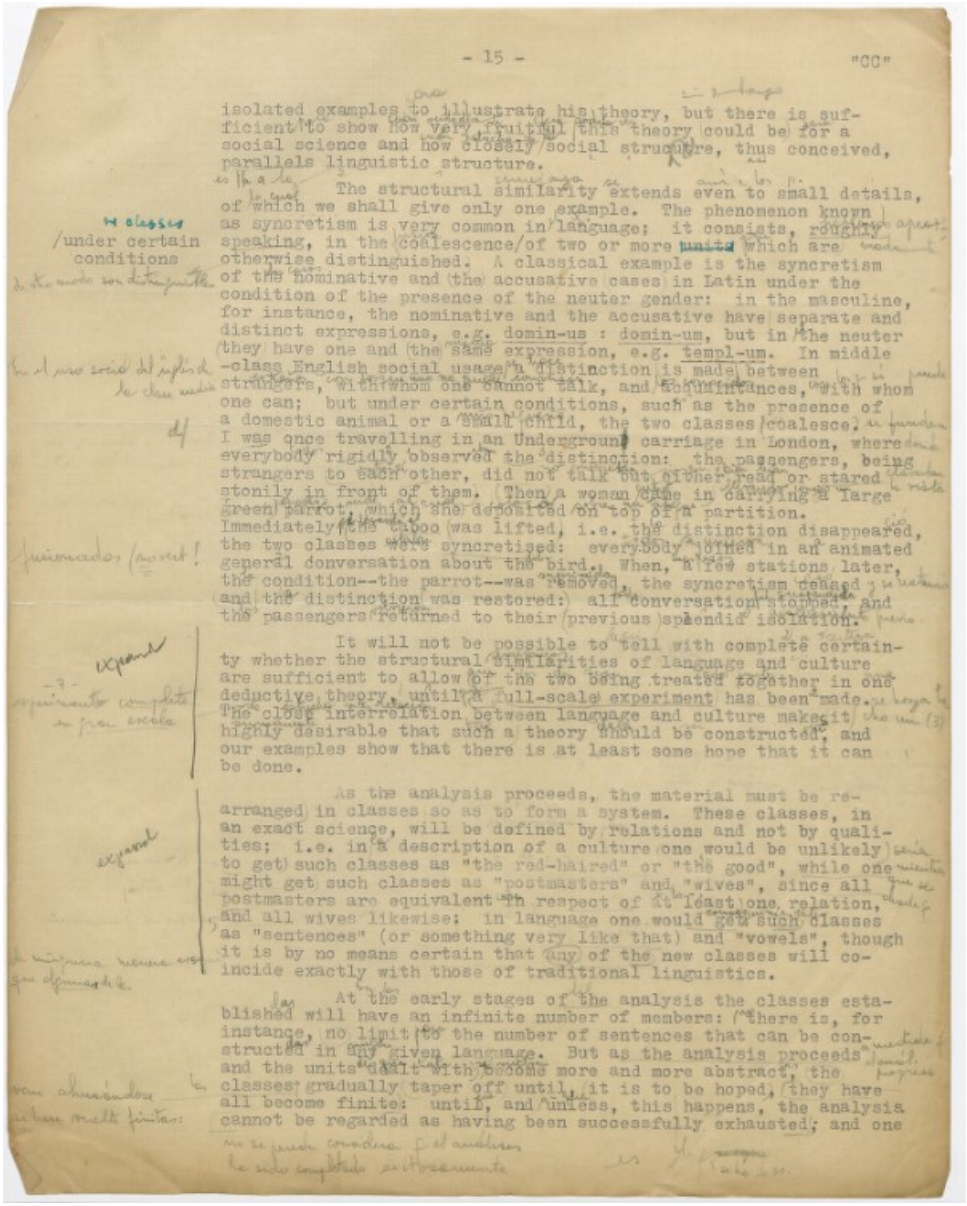
The vocabulary of a language is an index of the culture to which it belongs, as has long been recognised: thus Eskimo has a great wealth of words for snow in various forms; Arabic has an enormous vocabulary of terms referring to the use and breeding of camels; the kinship terminology of a language always reflects the present, or recently past, social usage of the people speaking it; etc.

But the relations between culture and language go much deeper than this as the following example will show: the language of the Maidus, a Californian tribe of Indians, is particularly rich in moods, especially imperatives and optatives, which are very finely graded from command to encouragement. This elaboration becomes immediately intelligible when one studies the social customs of the Maidus. Although their material culture was extremely primitive (pre-pottery stone-age), their code of manners was very delicate, and they were almost pathologically anxious to avoid embarrassment and hurt feelings; different classes of relatives by blood or marriage had to be treated with different degrees of respect or familiarity, etc. Such a code of manners obviously demands a language capable of accommodating the fine shades of meaning, the deference to feeling which it requires. One fits the other and can be understood only in terms of the other.

It is clear, then, that language is not isolated from culture and does not behave as if it were, and it follows from this that a language without its culture, or a culture without its language, is not a suitable field of study.

The question therefore arises whether it is possible to fit linguistic and cultural data into one investigation, i.e. whether it is possible to construct one deductive theory which will accommodate them both. It is too early to give a definitive answer to this question, but there are already indications that the two are structurally very similar.

One of the most striking characteristics of language, and certainly its functionally most important one, is that it is a symbolism, a strucure in which units are arranged in pairs so that one is the expression of the other; the units are so organised as to form two interrelated systems, a system of expression and a system of content or meaning. In culture there are many other symbolisms, such as ritual, uniforms, signal-systems, which all have the same type of structure. But in addition to this it has been demonstrated by Thurman Arnold (in The Folklore of Capitalism) that the whole structure of social institutions is in itself a symbolism: there are two interrelated systems, the units of which are arranged in pairs, one consisting of the social institutions as they actually function, and the other consisting of the corresponding myths, i.e. of what people believe the social institutions to be, which is often something very different. Thurman Arnold goes no further than to give



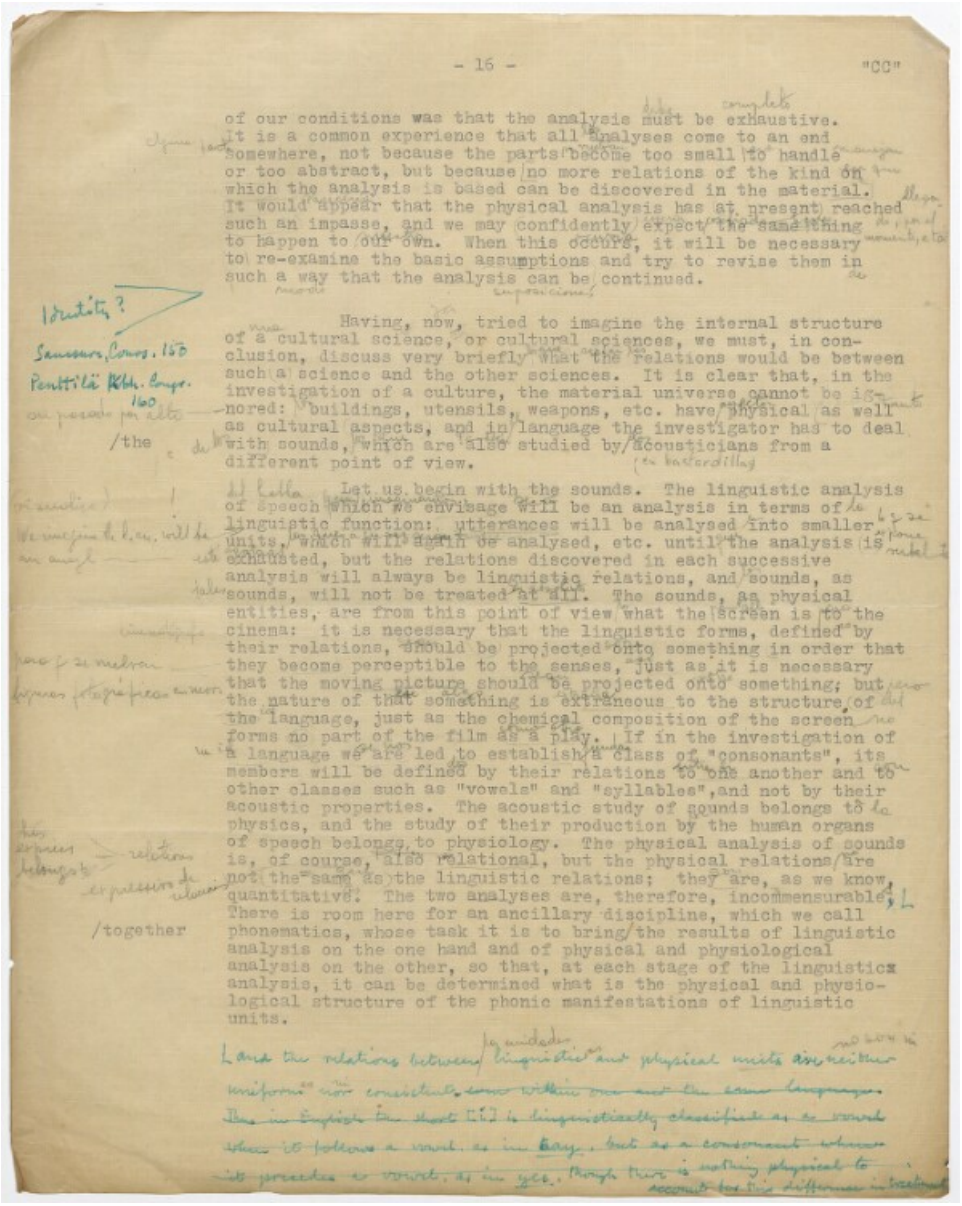
isolated examples to illustrate his theory, but there is sufficient to show how very fruitful this theory could be for a social science and how closely social structure, thus conceived, parallels linguistic structure.

The structural similarity extends even to small details, of which we shall give only one example. The phenomenon known as syncretism is very common in language; it consists, roughly speaking, in the coalescence of two or more units which are otherwise distinguished. A classical example is the syncretism of the nominative and the accusative cases in Latin under the condition of the presence of the neuter gender: in the masculine, for instance, the nominative and the accusative have separate and distinct expressions, e.g. domin-us : domin-um, but in the neuter they have one and the same expression, e.g. templ-um. In middle-class English social usage a distinction is made between strangers, with whom one cannot talk, and acquaintances, with whom one can; but under certain conditions, such as the presence of a domestic animal or a small child, the two classes coalesce. I was once travelling in an Underground carriage in London, where everybody rigidly observed the distinction: the passengers, being strangers to each other, did not talk but either read or stared stonily in front of them. Then a woman came in carrying a large green parrot, which she deposited on top of a partition. Immediately the taboo was lifted, i.e. the distinction disappeared, the two classes were syncretised: everybody joined in an animated general conversation about the bird. When, a few stations later, the condition--the parrot--was removed, the syncretism ceased and the distinction was restored: all conversation stopped, and the passengers returned to their previous splendid isolation.

It will not be possible to tell with complete certainty whether the structural similarities of language and culture are sufficient to allow of the two being treated together in one deductive theory, until a full-scale experiment has been made. The close interrelation between language and culture makes it highly desirable that such a theory should be constructed, and our examples show that there is at least some hope that it can be done.

As the analysis proceeds, the material must be rearranged in classes so as to form a system. These classes, in an exact science, will be defined by relations and not by qualities; i.e. in a description of a culture one would be unlikely to get such classes as "the red-haired" or "the good", while one might get such classes as "postmasters" and "wives", since all postmasters are equivalent in respect of at least one relation, and all wives likewise; in language one would get such classes as "sentences" (or something very like that) and "vowels", though it is by no means certain that any of the new classes will coincide exactly with those of traditional linguistics.

At the early stages of the analysis the classes established will have an infinite number of members: there is, for instance, no limit to the number of sentences that can be constructed in any given language. But as the analysis proceeds and the units dealt with become more and more abstract, the classes gradually taper off until, it is to be hoped, they have all become finite: until, and unless, this happens, the analysis cannot be regarded as having been successfully exhausted; and one

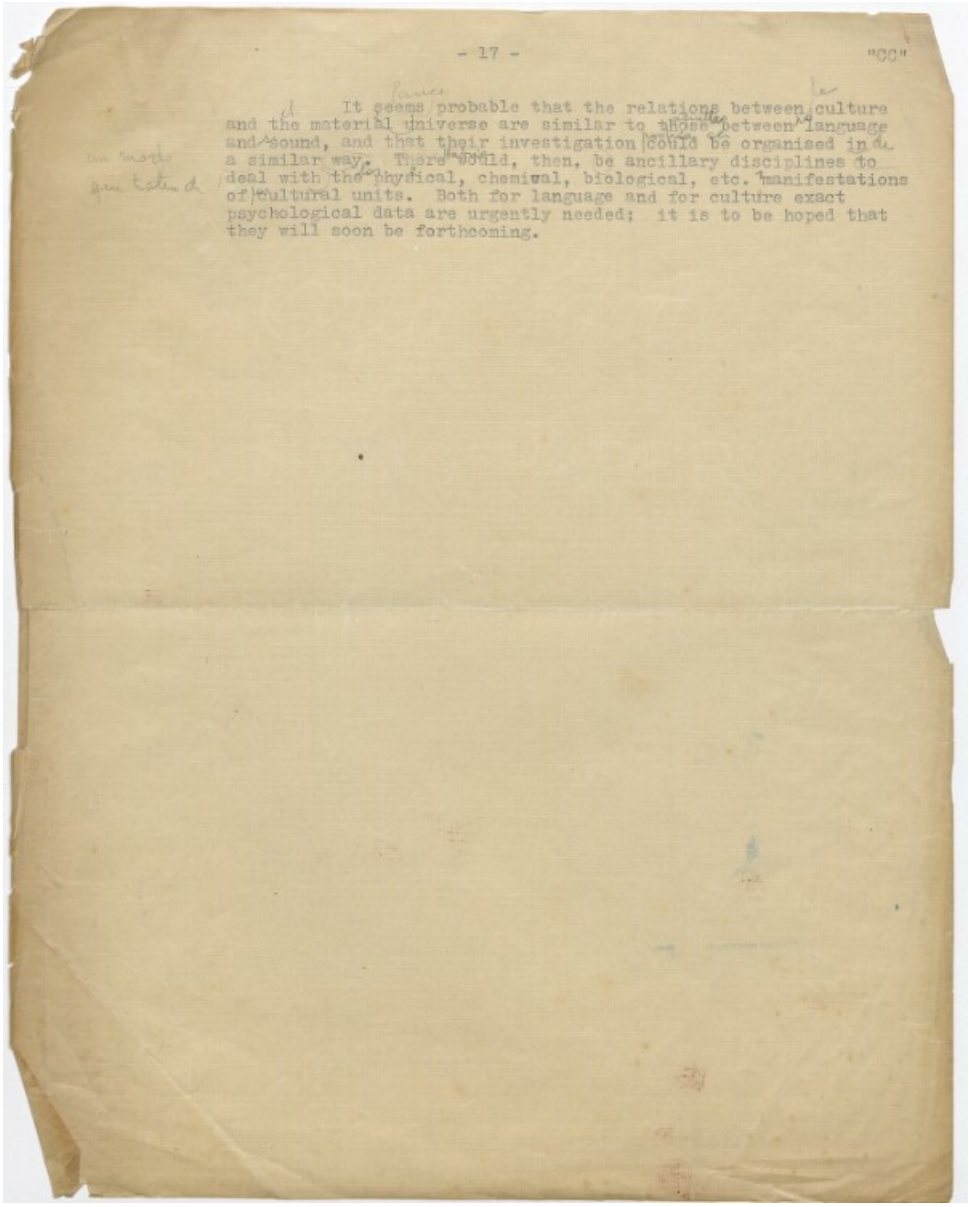


of our conditions was that the analysis must be exhaustive. It is a common experience that all analyses come to an end somewhere, not because the parts become too small to handle or too abstract, but because no more relations of the kind on which the analysis is based can be discovered in the material. It would appear that the physical analysis has at present reached such an impasse, and we may confidently expect the same thing to happen to our own. When this occurs, it will be necessary to re-examine the basic assumptions and try to revise them in such a way that the analysis can be continued.

Having, now, tried to imagine the internal structure of a cultural science, or cultural sciences, we must, in conclusion, discuss very briefly what the relations would be between such a science and the other sciences. It is clear that, in the investigation of a culture, the material universe cannot be ignored: buildings, utensils, weapons, etc. have physical as well as cultural aspects, and in language the investigator has to deal with sounds, which are also studied by acousticians from a different point of view.

Let us begin with the sounds. The linguistic analysis of speech which we envisage will be an analysis in terms of linguistic function: utterances will be analysed into smaller units, which will again be analysed, etc. until the analysis is exhausted, but the relations discovered in each successive analysis will always be linguistic relations, and sounds, as sounds, will not be treated at all. The sounds, as physical entities, are from this point of view what the screen is to the cinema: it is necessary that the linguistic forms, defined by their relations, should be projected onto something in order that they become perceptible to the senses, just as it is necessary that the moving picture should be projected onto something; but the nature of that something is extraneous to the structure of the language, just as the chemical composition of the screen forms no part of the film as a play. If in the investigation of a language we are led to establish a class of "consonants", its members will be defined by their relations to one another and to other classes such as "vowels" and "syllables", and not by their acoustic properties. The acoustic study of sounds belongs to physics, and the study of their production by the human organs of speech belongs to physiology. The physical analysis of sounds is, of course, also relational, but the physical relations are not the same as the linguistic relations; they are, as we know, quantitative. The two analyses are, therefore, incommensurable. There is room here for an ancillary discipline, which we call phonematics, whose task it is to bring the results of linguistic analysis on the one hand and of physical and physiological analysis on the other, so that, at each stage of the linguistic analysis, it can be determined what is the physical and physiological structure of the phonic manifestations of linguistic units.

and the relations between linguistic and physical units are neither uniform nor consistent. Even within one and the same language. Thus in English the sound [j] is linguistically classified as a vowel when it follows a vowel, as in *boy*, but as a consonant when it precedes a vowel, as in *yes*. Though there is nothing physical to account for this difference in treatment



It seems probable that the relations between culture and the material universe are similar to those between language and sound, and that their investigation could be organised in a similar way. There would, then, be ancillary disciplines to deal with the physical, chemical, biological, etc. manifestations of cultural units. Both for language and for culture exact psychological data are urgently needed; it is to be hoped that they will soon be forthcoming.