compounds and plant residues in defined ratios". Moreover, although it has been repeatedly suggested that the coking propensities of bituminous coal reside in the 'vitrain', we here disbelieve it.

It seems to me that a scientific classification should not only be applicable to all types of coals but also have a more fundamental basis than any yet proposed.

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April 29.

AN adequate and reasoned reply to Prof. Bone's letter would require more space than, as I am given to understand, NATURE can afford; I must, therefore, let the statements in my article, which I endeavoured to make in a critical and impartial spirit, speak for themselves.

I may, however, be permitted to refer to two points raised in Prof. Bone's letter. A perusal of the literature on the formation and the chemical and petrographic constitution of coal and its commercial preparation and utilisation during the last ten years provides evidence of the wide use of the terms proposed by Stopes and of the acceptance of what they are intended to signify. I tried to make it clear in my article that only qualified acceptance has been accorded to them in some quarters.

Prof. Bone's experience that the isolated coal components are not different and typical in their chemical composition is contrary to the results of hundreds of analyses and carbonising tests published by workers in Great Britain and many other countries. My own work on the behaviour of the coal components during carbonisation, and on the composition and distribution of the mineral matter in coal, furnishes ample proof for the statement made in my article. However, by its very nature the composite character of coal does not permit of ready generalisation, and if Prof. Bone is aware of cases in which typical differences in composition between coal components or their ashes cannot be recognised, he would earn the thanks of other workers interested in the subject for bringing these exceptions to their notice.

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Philosophy and Modern Science

WHEN I read Dr. H. Dingle's book "Science and Human Experience" I found that I agreed with nearly all of it; now I find myself in disagreement with most of his article in the Jubilee issue of NATURE. I realise that he may not be expressing his own views, but be trying to summarise those of others, and that most of those he expresses are prevalent; but I cannot convince myself that they are right. The differences begin with what he calls the fundamental principle of the rejection of unobservables. No distinction is made between sensations and concepts. Dr. Dingle makes general observability part of his criterion; since each sensation is private to one individual, he thereby leaves the whole basis of our experience out of science. The principle cannot be applied to concepts, because in fact they are not observed by anybody. If we are realists we may say that they are inferred; if we are phenomenalists we may say that they are constructed. If there is any change in scientific thought in this respect, it is that our realists have now a greater disposition to modify their ideas of *what* is real when new data derived from sensation become available.

The scientific validity of a concept in fact depends on quite different criteria; it depends on whether the concept and the postulated laws that it satisfies help to co-ordinate our sensations. If different people find the same concepts useful, that is because to a considerable extent they have similar sensations and similar processes of thought; but what are sensations to one are concepts to another. The rejection of unnecessary concepts is not a fundamental principle at all; it is a practical rule of method, like not putting six pairs of knives and forks on the table for a two-course dinner. Thus I cannot agree that the rejection of absolute position was the great feature of the principle of relativity; the important thing was the statement of the laws satisfied by relative position. Admittedly the method made the detection of the law easier; that is why it was a good method. But the important thing was the application of the principle that a formally simple law has an appreciable *a priori* probability. I have shown in my "Scientific Inference" that this principle is fundamental, and that without it we could never attach a high probability to any quantitative law however often it is verified; but though it is universally used, people seem to have a curious reluctance to admit that they are using it. Let us respect the broom; but there is no need to be ashamed of the electric light.

The confusion between sensations and concepts again vitiates Dr. Dingle's answer to the question 'Do things exist when they are not observed?' Sensations obviously do not; but would Dr. Dingle return the answer 'No' to the question, 'Did Neptune exist before it was observed?' The fact is that when we say we observe a thing we do nothing of the sort; we have certain sensations and we assert the result of a long chain of inference from them, which is not the shorter because we have made inferences of the same type so often that we carry them out rapidly and often forget that they are there. The perturbations of Uranus afforded just the same kind of ground epistemologically for inferring the existence of Neptune that a telescopic observation does.

The 'principle of causality', again, has no scientific status. As has been repeatedly pointed out, nobody has ever succeeded in stating it in such a way that it will help us to say *what* laws are causal. We know of many actual causal laws, and there is reason to believe that many others remain to be discovered; but the notion of a universal principle of causality is by nature incapable of verification and in practice useless. What is used in practice, consciously or not, is the simplicity postulate.

I am mystified by Dr. Dingle's statement that a probability is the ratio of two integers. Given that a point is equally likely to be anywhere within a length a, what is the probability that it lies within distance $a/\sqrt{2}$ of one end? Or does he contemplate a field such that all distances between possible positions are integral multiples of some universal length, so that continuous variation is excluded?

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