INTRODUCTION.

OUR PROBLEM DEFINED BY ITS RELATIONS TO LOGIC, PSYCHOLOGY AND MATHEMATICS.

1. GEOMETRY, throughout the 17th and 18th centuries, remained, in the war against empiricism, an impregnable fortress of the idealists. Those who held—as was generally held on the Continent—that certain knowledge, independent of experience, was possible about the real world, had only to point to Geometry: none but a madman, they said, would throw doubt on its validity, and none but a fool would deny its objective reference. The English Empiricists, in this matter, had, therefore, a somewhat difficult task; either they had to ignore the problem, or if, like Hume and Mill, they ventured on the assault, they were driven into the apparently paradoxical assertion that Geometry, at bottom, had no certainty of a different *kind* from that of Mechanics—only the perpetual presence of spatial impressions, they said, made our experience of the truth of the axioms so wide as to *seem* absolute certainty.

Here, however, as in many other instances, merciless logic drove these philosophers, whether they would or no, into glaring opposition to the common sense of their day. It was only through Kant, the creator of modern Epistemology, that the geometrical problem received a modern form. He reduced the question to the following hypotheticals: If Geometry has apodeictic certainty, its matter, *i.e.* space, must be *à priori*, and as such must be purely subjective; and conversely, if space is purely subjective, Geometry must have apodeictic certainty. The latter hypothetical has more weight with Kant, indeed it is ineradicably bound up with his whole Epistemology; nevertheless it has, I think, much less force than the former. Let us

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accept, however, for the moment, the Kantian formulation, and endeavour to give precision to the terms *à priori* and *subjective.*

2. One of the great difficulties, throughout this controversy, is the extremely variable use to which these words, as well as the word *empirical,* are put by different authors. To Kant, who was nothing of a psychologist, *à priori* and *subjective* were almost interchangeable terms[1]; in modern usage there is, on the whole, a tendency to confine the word *subjective* to Psychology, leaving *à priori* to do duty for Epistemology. If we accept this differentiation, we may set up, corresponding to the problems of these two sciences, the following provisional definitions: *à priori* applies to any piece of knowledge which, though perhaps elicited by experience, is *logically* presupposed in experience: *subjective* applies to any mental state whose immediate cause lies, not in the external world, but within the limits of the subject. The latter definition, of course, is framed exclusively for Psychology: from the point of view of physical Science all mental states are subjective. But for a Science whose matter, strictly speaking, is *only* mental states, we require, if we are to use the word to any purpose, some differentia among mental states, as a mark of a more special subjectivity on the part of those to which this term is applied.

Now the only mental states whose immediate causes lie in the external world are *sensations.* A pure sensation is, of course, an impossible abstraction—we are never wholly passive under the action of an external stimulus—but for the purposes of Psychology the abstraction is a useful one. Whatever, then, is not sensation, we shall, in Psychology, call subjective. It is in sensation alone that we are directly affected by the external world, and only here does it give us direct information about itself.

3. Let us now consider the epistemological question, as to the sort of knowledge which can be called *à priori.* Here we have nothing to do—in the first instance, at any rate—with the cause or genesis of a piece of knowledge; we accept knowledge as a datum to be analysed and classified. Such analysis will reveal a formal and a material element in

[1] Cf. Erdmann, Axiome der Geometrie, p. 111: "Für Kant sind Apriorität und ausschliessliche Subjectivität allerdings Wechselbegriffe."

knowledge. The formal element will consist of postulates which are required to make knowledge possible at all, and of all that can be deduced from these postulates; the material element, on the other hand, will consist of all that comes to fill in the form given by the formal postulates—all that is contingent or dependent on experience, all that might have been otherwise without rendering knowledge impossible. We shall then call the formal element *à priori*, the material element empirical.

4. Now what is the connection between the subjective and the *à priori*? It is a connection, obviously—if it exists at all—from the outside, *i.e.* not deducible directly from the nature of either, but provable—if it can be proved—only by a general view of the conditions of both. The question, what knowledge is *à priori*, must, on the above definition, depend on a logical analysis of knowledge, by which the conditions of possible experience may be revealed; but the question, what elements of a cognitive state are subjective, is to be investigated by pure Psychology, which has to determine what, in our perceptions, belongs to sensation, and what is the work of thought or of association. Since, then, these two questions belong to different sciences, and can be settled independently, will it not be wise to conduct the two investigations separately? To decree that the *à priori* shall always be subjective, seems dangerous, when we reflect that such a view places our results, as to the *à priori*, at the mercy of empirical psychology. How serious this danger is, the controversy as to Kant's pure intuition sufficiently shows.

5. I shall, therefore, throughout the present Essay, use the word *à priori* without any psychological implication. My test of apriority will be purely logical: Would experience be impossible, if a certain axiom or postulate were denied? Or, in a more restricted sense, which gives apriority only within a particular science: Would experience as to the subject-matter of that science be impossible, without a certain axiom or postulate? My results also, therefore, will be purely logical. If Psychology declares that some things, which I have declared *à priori*, are not subjective, then, failing an error of detail in my proofs, the connection of the *à priori* and the subjective, so far as those things are concerned, must be given up. There will be no discussion, accordingly, throughout this Essay, of the relation of the *à priori* to the subjective—a relation which cannot determine what pieces of knowledge are *à priori*, but rather depends on that determination, and belongs, in any case, rather to Metaphysics than to Epistemology.

6. As I have ventured to use the word *à priori* in a slightly unconventional sense, I will give a few elucidatory remarks of a general nature.

The *à priori*, since Kant at any rate, has generally stood for the necessary or apodeictic element in knowledge. But modern logic has shown that necessary propositions are always, in one aspect at least, hypothetical. There may be, and usually is, an implication that the connection, of which necessity is predicated, has some existence, but still, necessity always points beyond itself to a *ground* of necessity, and asserts this ground rather than the actual connection. As Bradley points out, "arsenic poisons" remains true, even if it is poisoning no one. If, therefore, the *à priori* in knowledge be primarily the necessary, it must be the necessary on some hypothesis, and the *ground* of necessity must be included as *à priori*. But the ground of necessity is, so far as the necessary connection in question can show, a mere fact, a merely categorical judgment. Hence necessity alone is an insufficient criterion of apriority.

To supplement this criterion, we must supply the hypothesis or ground, on which alone the necessity holds, and this ground will vary from one science to another, and even, with the progress of knowledge, in the same science at different times. For as knowledge becomes more developed and articulate, more and more necessary connections are perceived, and the merely categorical truths, though they remain the foundation of apodeictic judgments, diminish in relative number. Nevertheless, in a fairly advanced science such as Geometry, we can, I think, pretty completely supply the appropriate ground, and establish, within the limits of the isolated science, the distinction between the necessary and the merely assertorical.

7. There are two grounds, I think, on which necessity may be sought within any science. These may be (very roughly) distinguished as the ground which Kant seeks in the

Prolegomena, and that which he seeks in the *Pure Reason*. We may start from the existence of our science as a fact, and analyse the reasoning employed with a view to discovering the fundamental postulate on which its logical possibility depends; in this case, the postulate, and all which follows from it alone, will be *à priori*. *Or* we may accept the existence of the subject-matter of our science as our basis of fact, and deduce dogmatically whatever principles we can from the essential nature of this subject-matter. In this latter case, however, it is not the whole empirical nature of the subject-matter, as revealed by the subsequent researches of our science, which forms our ground; for if it were, the whole science would, of course, be *à priori*. Rather it is that element, in the subject-matter, which makes *possible* the branch of experience dealt with by the science in question[1]. The importance of this distinction will appear more clearly as we proceed[2].

8. These two grounds of necessity, in ultimate analysis, fall together. The *methods* of investigation in the two cases differ widely, but the *results* cannot differ. For in the first case, by analysis of the science, we discover the postulate on which alone its reasonings are possible. Now if reasoning in the science is impossible without some postulate, this postulate must be essential to experience of the subject-matter of the science, and thus we get the second ground. Nevertheless, the two methods are useful as supplementing one another, and the first, as starting from the actual science, is the safest and easiest method of investigation, though the second seems the more convincing for exposition.

9. The course of my argument, therefore, will be as follows: In the first chapter, as a preliminary to the logical analysis of Geometry, I shall give a brief history of the rise and development of non-Euclidean systems. The second chapter will prepare the ground for a constructive theory of Geometry, by a criticism of some previous philosophical views; in this chapter, I shall

[1] I use "experience" here in the widest possible sense, the sense in which the word is used by Bradley.

[2] Where the branch of experience in question is essential to all experience, the resulting apriority may be regarded as absolute; where it is necessary only to some special science, as relative to that science.

endeavour to exhibit such views as partly true, partly false, and so to establish, by preliminary polemics, the truth of such parts of my own theory as are to be found in former writers. A large part of this theory, however, cannot be so introduced, since the whole field of projective Geometry, so far as I am aware, has been hitherto unknown to philosophers. Passing, in the third chapter, from criticism to construction, I shall deal first with projective Geometry. This, I shall maintain, is necessarily true of any form of externality, and is, since some such form is necessary to experience, completely *à priori*. In metrical Geometry, however, which I shall next consider, the axioms will fall into two classes: (1) Those common to Euclidean and non-Euclidean spaces. These will be found, on the one hand, essential to the possibility of measurement in any continuum, and on the other hand, necessary properties of any form of externality with more than one dimension. They will, therefore, be declared *à priori*. (2) Those axioms which distinguish Euclidean from non-Euclidean spaces. These will be regarded as wholly empirical. The axiom that the number of dimensions is three, however, though empirical, will be declared, since small errors are here impossible, exactly and certainly true of our actual world; while the two remaining axioms, which determine the value of the space-constant, will be regarded as only approximately known, and certain only within the errors of observation[1]. The fourth chapter, finally, will endeavour to prove, what was assumed in Chapter III., that some form of externality is necessary to experience, and will conclude by exhibiting the logical impossibility, if knowledge of such a form is to be freed from contradictions, of wholly abstracting this knowledge from all reference to the matter contained in the form.

I shall hope to have touched, with this discussion, on all the main points relating to the Foundations of Geometry.

[1] I have given no account of these empirical proofs, as they seem to be constituted by the whole body of physical science. Everything in physical science, from the law of gravitation to the building of bridges, from the spectroscope to the art of navigation, would be profoundly modified by any considerable inaccuracy in the hypothesis that our actual space is Euclidean. The observed truth of physical science, therefore, constitutes overwhelming empirical evidence that this hypothesis is very approximately correct, even if not rigidly true.

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CHAPTER I.

A SHORT HISTORY OF METAGEOMETRY.

10. WHEN a long established system is attacked, it usually happens that the attack begins only at a single point, where the weakness of the established doctrine is peculiarly evident. But criticism, when once invited, is apt to extend much further than the most daring, at first, would have wished.

> "First cut the liquefaction, what comes last,
> But Fichte's clever cut at God himself?"

So it has been with Geometry. The liquefaction of Euclidean orthodoxy is the axiom of parallels, and it was by the refusal to admit this axiom without proof that Metageometry began. The first effort in this direction, that of Legendre[1], was inspired by the hope of deducing this axiom from the others—a hope which, as we now know, was doomed to inevitable failure. Parallels are defined by Legendre as lines in the same plane, such that, if a third line cut them, it makes the sum of the interior and opposite angles equal to two right angles. He proves without difficulty that such lines would not meet, but is unable to prove that non-parallel lines in a plane must meet. Similarly he can prove that the sum of the angles of a triangle cannot exceed two right angles, and that if any one triangle has a sum equal to two right angles, all triangles have the same sum; but he is unable to prove the existence of this one triangle.

11. Thus Legendre's attempt broke down; but mere failure

[1] V. Mémoires de l'Académie royale des Sciences de l'Institut de France, T. XII. 1833, for a full statement of his results, with references to former writings.

could prove nothing. A bolder method, suggested by Gauss, was carried out by Lobatchewsky and Bolyai[1]. If the axiom of parallels is logically deducible from the others, we shall, by denying it and maintaining the rest, be led to contradictions. These three mathematicians, accordingly, attacked the problem indirectly: they denied the axiom of parallels, and yet obtained a logically consistent Geometry. They inferred that the axiom was logically independent of the others, and essential to the Euclidean system. Their works, being all inspired by this motive, may be distinguished as forming the first period in the development of Metageometry.

The second period, inaugurated by Riemann, had a much deeper import: it was largely philosophical in its aims and constructive in its methods. It aimed at no less than a logical analysis of all the essential axioms of Geometry, and regarded space as a particular case of the more general conception of a *manifold*. Taking its stand on the methods of analytical metrical Geometry, it established two non-Euclidean systems, the first that of Lobatchewsky, the second—in which the axiom of the straight line, in Euclid's form, was also denied—a new variety, by analogy called spherical. The leading conception in this period is the *measure of curvature*, a term invented by Gauss, but applied by him only to surfaces. Gauss had shown that free mobility on surfaces was only possible when the measure of curvature was constant; Riemann and Helmholtz extended this proposition to n dimensions, and made it the fundamental property of space.

In the third period, which begins with Cayley, the philosophical motive, which had moved the first pioneers, is less apparent, and is replaced by a more technical and mathematical spirit. This period is chiefly distinguished from the second, in a mathematical point of view, by its method, which is projective instead of metrical. The leading mathematical conception here

[1] This bolder method, it appears, had been suggested, nearly a century earlier, by an Italian, Saccheri. His work, which seems to have remained completely unknown until Beltrami rediscovered it in 1889, is called "Euclides ab omni naevo vindicatus, etc." Mediolani, 1733. (See Veronese, Grundzüge der Geometrie, German translation, Leipzig, 1894, p. 636.) His results included spherical as well as hyperbolic space; but they alarmed him to such an extent that he devoted the last half of his book to disproving them.

is the Absolute (*Grundgebild*), a figure by relation to which all metrical properties become projective. Cayley's work, which was very brief, and attracted little attention, has been perfected and elaborated by F. Klein, and through him has found general acceptance. Klein has added to the two kinds of non-Euclidean Geometry already known, a third, which he calls elliptic; this third kind closely resembles Helmholtz's spherical Geometry, but is distinguished by the important difference that, in it, two straight lines meet in only one point[1]. The distinctive mark of the spaces represented by both is that, like the surface of a sphere, they are finite but unbounded. The reduction of metrical to projective properties, as will be proved hereafter, has only a technical importance; at the same time, projective Geometry is able to deal directly with those purely descriptive or qualitative properties of space which are common to Euclid and Metageometry alike. The third period has, therefore, great philosophical importance, while its method has, mathematically, much greater beauty and unity than that of the second; it is able to treat all kinds of space at once, so that every symbolic proposition is, according to the meaning given to the symbols, a proposition in whichever Geometry we choose. This has the advantage of proving that further research cannot lead to contradictions in non-Euclidean systems, unless it at the same moment reveals contradictions in Euclid. These systems, therefore, are logically as sound as that of Euclid himself.

After this brief sketch of the characteristics of the three periods, I will proceed to a more detailed account. It will be my aim to avoid, as far as possible, all technical mathematics, and bring into relief only those fundamental points in the

[1] Klein's first account of elliptic Geometry, as a result of Cayley's projective theory of distance, appeared in two articles entitled "Ueber die sogenannte Nicht-Euklidische Geometrie, I, II," Math. Annalen 4, 6 (1871—2). It was afterwards independently discovered by Newcomb, in an article entitled "Elementary Theorems relating to the geometry of a space of three dimensions, and of uniform positive curvature in the fourth dimension," Crelle's Journal für die reine und angewandte Mathematik, Vol. 83 (1877). For an account of the mathematical controversies concerning elliptic Geometry, see Klein's "Vorlesungen über Nicht-Euklidische Geometrie," Göttingen 1893, I. p. 284 ff. A bibliography of the relevant literature up to the year 1878 was given by Halsted in the American Journal of Mathematics, Vols. 1, 2.

mathematical development, which seem of logical or philosophical importance.

First Period.

12. The originator of the whole system, *Gauss*, does not appear, as regards strictly non-Euclidean Geometry, in any of his hitherto published papers, to have given more than results; his proofs remain unknown to us. Nevertheless he was the first to investigate the consequences of denying the axiom of parallels[1], and in his letters he communicated these consequences to some of his friends, among whom was Wolfgang Bolyai. The first mention of the subject in his letters occurs when he was only 18; four years later, in 1799, writing to W. Bolyai, he enunciates the important theorem that, in hyperbolic Geometry, there is a maximum to the area of a triangle. From later writings it appears that he had worked out a system nearly, if not quite, as complete as those of Lobatchewsky and Bolyai[2].

It is important to remember, however, that Gauss's work on curvature, which *was* published, laid the foundation for the whole method of the second period, and was undertaken, according to Riemann and Helmholtz[3], with a view to an (unpublished) investigation of the foundations of Geometry. His work in this direction will, owing to its method, be better treated of under the second period, but it is interesting to observe that he stood, like many pioneers, at the head of two tendencies which afterwards diverged.

13. *Lobatchewsky*, a professor in the University of Kasan, first published his results, in their native Russian, in the proceedings of that learned body for the years 1829—1830. Owing to this double obscurity of language and place, they attracted little attention, until he translated them into French[4]

[1] Veronese (op. cit. p. 638) denies the priority of Gauss in the invention of a non-Euclidean system, though he admits him to have been the first to regard the axiom of parallels as indemonstrable. His grounds for the former assertion seem scarcely adequate: on the evidence against it, see Klein, Nicht-Euklid, I. pp. 171–174.

[2] V. Briefwechsel mit Schumacher, Bd. II. p. 268.

[3] Cf. Helmholtz, Wiss. Abh. II. p. 611.

[4] Crelle's Journal, 1837.