HUGO DINGLER'S PHILOSOPHY OF GEOMETRY

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1. The Connection between Geometry and Reality

Hugo Dingler (1881-1954) graduated from Göttingen with a dissertation on differential geometry[1] at a time when Felix Klein, David Hilbert and Hermann Minkowski taught in that university. He had studied earlier with Wilhelm Röntgen in Munich and, while at Göttingen, he also attended Edmund Husserl's courses on philosophy.

During twenty years he taught physics in Munich, first, from 1912 to 1920, as *Privatdozent* at the Technische Hochschule, later, from 1920 to 1932, as Extraordinary Professor at the University. However, he made no contributions to the exciting physical discoveries of the time. His numerous books and articles dealt almost exclusively with what we now call foundational problems. Moreover, they did not discuss the foundations of the new theories of Relativity and Quanta, but sought to consolidate the old physics, based on Euclidean geometry and Newtonian dynamics, into an impregnable and everlasting system.

In 1932, Dingler was appointed to the chair of Philosophy at the TH Darmstadt. Two years later he was dismissed from it "on political

1 Dingler (1907). A list of Dingler's publications will be found in J. Willer, *Relativität und Eindeutigkeit*, pp. 205-209. Willer's book is the most complete monograph about Dingler in existence, but I have found it not altogether reliable on some points. In my opinion the best critical study of Dingler's epistemology is the introduction by K.Lorenz and J.Mittelstrass to Dingler, EW2. (Abbreviated references are decoded on pp. 127 ff.) Wagner (1955/56) is a useful sketch, but Krampf (1956) and (1971), like Krampf's book, PHD, are too partisan. On the specific subject of geometry, A.Kamlah (1976) is excellent, though its main emphasis lies rather on P.Lorenzen's reformulation of Dingler's theory (see below, pp. 116 ff.). Literature in English is very scarce. I am acquainted only with a somewhat misleading article by H.Sandborn (1952) and the encyclopedia articles in the *Dictionary of Scientific Biography* and in Edwards' *Encyclopedia of Philosophy*.

Diálogos, 32 (1978), p. 85-128.

grounds" stemming apparently from the publication of a book on Jewish culture fifteen years before.[2] He subsequently sought to make amends for his un-German behavior by writing pro-Nazi trash, but—as it generally happens to intellectuals who fawn on the leaders of a totalitarian party—he labored in vain, for he never succeeded in ingratiating himself with the new masters of his country. Dingler's political antics are probably no less responsible than his negative stance towards modern physics for the almost total oblivion into which his work fell outside Germany and Italy after the Second World War.

The persistent aim of Dingler's philosophical endeavors was to vindicate and secure our exact knowledge of nature. He came to think of it as an ever growing system, continually bringing new patches and aspects of reality under its sway, but resting on the indestructible, inalterable foundations provided by the four "ideal sciences" of logic, arithmetic, geometry and dynamics. Both historically and systematically, geometry played a key role in this construction. Euclid's *Elements* were for a long time the paradigm of exact science, until the 19th century discovery of alternative systems of geometry upset the traditional notions of rationality and brought the foundations of knowledge into confusion. This discovery, more than any other historical event, motivated Dingler's own critical enterprise. In his system of science, geometry, as a discipline at once "logical" and "manual", bridges the chasm between thought and reality, paves the way for physics and provides a cue for understanding the true nature and scope of this science.

Dingler discusses geometry and its foundations in many of his books and in several articles. Two books are specifically devoted to the subject: *The Foundations of Applied Geometry* (GAG, 1911) and *The Foundations of Geometry* (GG, 1932).[3] The former deals with it in the setting of Dingler's earlier epistemology and provides a suitable introduction to his work. We shall examine it forthwith. The latter will be studied in Section 4.

The discovery of non-Euclidean geometries put an end to the illusion that geometrical truth can be established by sheer deductive reasoning. Infinitely many internally consistent yet mutually incompatible axiomatic systems can be devised that bind what are traditionally regarded as geometrical words into seemingly geometri-

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[2] Dingler, *Die Kultur der Juden. Eine Versöhnung zwischen Religion und Wissenschaft*. Leipzig: 1919.

[3] See also the articles: Dingler (1907b), (1920b), (1925), (1934), (1935), (1955).

cal propositions. But each of these systems is by itself an empty "logical edifice" *(logisches Gebäude)*,[4] which does not deserves the name of geometry unless it is actually "connected with reality."

The connection would be ensured for a particular "logical edifice" if its axioms could be said to provide an accurate description of actual facts. The "logical edifice" would then express "the true geometry of our world." This presupposes, however, that the primitive or undefined terms that occur in those axioms be assigned definite physical referents, which is not such a simple matter as naïve empiricists are wont to believe. Take, for example, Hilbert's axiom system for Euclidean geometry. Its axioms speak about three kinds of entities called *points, straight lines* and *planes*. What on earth are these entities? One might feel tempted to say that a (Euclidean) straight line is any object satisfying all the conditions that Hilbert's axioms prescribe for straight lines. But surely the said conditions do not even make sense unless one knows already what are points and planes, and what is to be understood by incidence, betweenness and congruence. We see, by the way, how silly it is to claim that an axiom system such as Hilbert's "implicitly defines" its primitive terms, when as a matter of fact it does not even supply fixed criteria for picking out their referents.

Though Dingler, as we shall see, eventually succumbs in GAG to a temptation akin to the one we have just rejected, when discussing the referents of Hilbert's primitives he comes up with a different and very important idea. He recalls that straight lines and planes are being daily constructed and used by architects and masons, astronomers and engineers. These everyday straights and planes are not usually tested for their conformity with Hilbert's axioms but are immediately acknowledged as such if they agree, within the accepted range of tolerance, with other previously given straights and planes, embodied in diverse tools and instruments (e.g. rulers, drawing-boards, the carpentry tool called "plane"). They all derive ultimately from prototypes made and kept in the factories of precision instruments.

Prototype planes are originally produced according to the following procedure: Take three hard bodies A,B,C and rub them by turns, pairwise, against one another—that is, rub A against B, B against C, C against A, A against B again, etc.—until three polished surfaces are obtained, each of which fits exactly the other two. The surfaces are viewed as more or less perfect planes, according to the

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[4] Dingler, GAG, p.5. The expression is defined on GAG, p.44, as synonymous with *axiomatic theory*.

degree of exactness with which they fit each other. A prototype straight line can be obtained by making two such plane surfaces with a common edge. The procedure described constitutes what Dingler calls an "empirical definition" or "realization" of the plane—and, indirectly, of the straight line.[5] The name is justified insofar as any surface Σ will be regarded as plane if a prototype plane Π can be applied on it and made to slide over it, within its boundaries, in any direction, while every part of Π remains in full coincidence with some part of Σ. Also, if Σ fits any part of a plane surface. (Similar statements concerning straight lines will be easily made by the reader.) Dingler emphasizes the peculiar value of this method of definition.

> I ask someone: What is a plane. He does not answer a word, he does not give a logical definition of the plane, expressed through concepts; neither does he produce an object that has a plane surface, while pointing at the latter [. . . . . .]; but he takes three rigid bodies and starts the process of making a plane that was described above. If he had just pointed at a prototype plane [*Normalebene*] I could not have known that its colour or the minor irregularities of its texture, etc. do not belong to it as such. But through the method chosen by him I know this at once, I have at once "a notion" of what a plane is, or rather of what it *ought* to be.[6]

It is clear that we can only get a notion of the plane from watching our man making one if we understand what he is doing, i.e., if we grasp the rule that guides his behavior. We must be able to see, e.g., that wearing a green jacket is not prescribed by that rule. But Dingler believes that rules of human behavior are more or less immediately intelligible to us (while natural kinds and laws are not). This is a point he sets great store by in his mature philosophy: generality can never be attained by merely opening our eyes and unbiasedly contemplating the events of nature. Universals enter our world and shape our surroundings only through our understanding of rules and our acting by them. Dingler does not pursue the matter any further in this book.

Another question that he fails to discuss, though it is suggested immediately by his exposition, concerns the consistency of the "empirical definition" of the plane. We do not doubt that a surface that is plane when measured up against a prototype manufactured by

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[5] Dingler, GAG, p.19.

[6] Dingler, GAG, pp. 22 f.

the above procedure from three bodies A, B and C will also turn out to be plane when compared with a prototype obtained from three other bodies A,′ B′ and C.′ Does our certainty rest only on an empirical regularity, inductively ascertained? Or does it stem from some deeper necessity? It is obvious at any rate that sheer conceptual analysis cannot guarantee our expectations.[7] We shall return to the matter later on, when dealing with Dingler's GG, where it looms larger than in GAG.

In the book we are now examining, "empirical definitions", notwithstanding their great philosophical interest and their unquestionable practical usefulness, are ultimately excluded from the foundations of applied geometry. Dingler notes that by picking out the referents of "plane" and "straight line" we do not yet succeed in setting up a connnection between reality and a "full" system of geometry in which congruence and distance are defined. For this we need, according to him, a realization of the rigid body. Dingler does not even consider the possibility of a physical geometry in which rigid bodies are lacking. (He does in fact mention Riemann's condition of "independence of lines from the way they lie in space" but does not dwell on its significance.) But he is well aware that their existence is compatible with any geometry of constant curvature. Depending on the actual deportment of rigid bodies, the "geometry of our world" may be Euclidean or hyperbolic, spherical or elliptic. Evidently, one will be able to know how rigid bodies behave only if one can tell what real bodies are rigid. Dingler is never tired of emphasizing that this is something that one cannot learn from experience. In order to establish that a given body is rigid we must measure the distances between its points and verify that they do not change as the body moves. But in order to measure distances we must have an acknowledgedly rigid body at our disposal. A prototype rigid body appears thus to be necessary for establishing a connection between a metric geometry and reality. Dingler observes that if the plane and the straight line have been already introduced as above, the construction of the rigid body must take their "empirical definition" into account, for planes and straights must be preserved in every rigid motion. This dependence of the full conception of the rigid body on that of the plane and the straight line is responsible, according to Dingler, for the complicated, seemingly unintuitive form of Euclid's fifth postulate.[9] On the other hand—he says—there

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7 Cf. Bopp (1958), p.64.

8 Dinlger, GAG, p.36.

9 Dingler, GAG, p. 35.

exists the alternative of introducing the rigid body directly first, and then defining the plane and the straight line by means of it.[10] Such is the method preferred by him in GAG. If it could be successfully carried out in the way he proposes, it would automatically yield a positive solution to the consistency problem we raised above regarding the "empirical definition" of the plane. (This might be the reason why Dingler ignores this problem in GAG—it is not worth mentioning, if his theory is correct.)

2. *The rigid body*

Dingler's "Foundations of Applied Geometry" of 1911 rest on the theory of science developed a year earlier in a small book on the *Limits and Aims of Science* (GZW). According to Dingler science has a double task, namely "(a) *to explain*, to make understandable whatever exists [. . .] (b) to dominate reality effectively, that is, to actually produce definite things which did not exist previously."[11] Fulfilment of this task in any specific domain of reality must begin by pointing out the "elementary phenomenon" *(Elementarvorgang)* into which all phenomena of the domain can be analyzed in thought and out of which they can be built in fact. In stark opposition to 19th century empiricism, Dingler contends that such elementary phenomena cannot be discovered inductively but must be fixed for each field of research by a rational choice. This choice is guided by a principle that Dingler, after Mach, calls the Principle of Economy: the elementary phenomenon is the simplest conceivable phenomenon in the field.[12] Scientific theory teaches how complex phenomena are constructed from the elementary phenomenon. If actual practice deviates from our theoretical predictions, the anomalies are attributed to unforeseen perturbations following a method Dingler calls Exhaustion.[13] Little by little science succeds in accounting for all

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10 Dingler, GAG, p.33. The reader is presumably aware of the fact that 19th century mathematicians had succeeded in characterizing Euclidean geometry and the classical non-Euclidean geometries by their respective groups of motions. Cf. Klein (1871), Poincaré (1887), Lie (1890).

11 Dingler, GAG, p.48. The theory of science sketched in Dingler, GZW, is presented with greater detail in the second chapter of GAG.

12 Thus, for example, the elementary phenomenon of optics is illumination from a point-source. (Dingler, GAG, pp.61 ff.)

13 Exhaustion may be regarded as the mainstay of Dingler's methodology. Without it "a strict systematic and orderly procedure in the formation of physical concepts and their realization" would not be possible (Dingler, MP, p.145; cf. pp.171 ff.) The following passage gives the gist of it: "[We demand that] all our laws have absolute and lasting validity. What shall we do if we meet

such perturbations with increasing accuracy, that is to say, in explaining them in terms of elementary phenomena.[14] GAG describes the rigid body as the elementary phenomenon of geometry.[15] The requirement of maximum simplicity is achieved by postulating that the rigid body obeys the laws of the simplest conceivable geometrical system, namely, the Euclidean system.

> Thus if we see some real thing that changes its spatial appearance, we ask: Is this change a geometric change? We obtain the answer by *observing whether the thing changes in accordance with the laws of Euclidean geometry* (of Euclidean motion). *If such is the case we call this thing a rigid body.* If it is not the case, some perturbing circumstances are to blame for it and the extent of their influence is measured by the magnitude of the thing's deviations from the laws of Euclidean geometry.[16]

Dingler's meaning will become clearer as we discuss some of the difficulties encountered by his doctrine and examine the emendation by which he believed he could overcome those difficulties. First of all, we note that natural phenomena do not of themselves fall into neatly distinguished domains or fields of study, but are roughly parcelled out into them by men. If, following Dingler, we correlate each field of study with an elementary phenomenon it makes more sense to characterize the former as the set of all phenomena that can be analyzed into the latter, rather than to define the latter as the ultimate "building block" of phenomena in the former. This approach implies, of course, that a particular phenomenon may belong to two or more fields and be given a different explanation in each in terms of their respective elementary phenomena. Such inconsistencies can only be avoided if fields of study are linearly ordered and science is built stepwise so that the theory of the prior and more fundamental fields can never be contradicted by that of the posterior and less basic ones. Some such idea of science is found already in Dingler's first book (GKTW, §4) but it is not fully

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any objects, to which a law should apply, but which are not themselves realizations of the law? In such case we will nevertheless uphold the law by ascribing every deviation from it which the objects exhibit to other circumstances or causes (so called perturbing circumstances). This is the *principle of exhaustion.*" (Dingler, GP², p.38: see also MP, pp.146, 282 f.) The method of exhaustion is of course strongly reminiscent of the much decried method of *ad hoc* hypotheses; see below, pp. 93f., n.19, and p. 124.

[14] See the eloquent passages in Dingler, GZW, pp.73, 81.

[15] Dingler, GAG, p.73; cf. pp.89,116. This perplexing terminology was subsequently abandoned by Dingler.

[16] Dingler, GAG, p.132.

developed until much later, in *The Foundations of Physics* (GP, first edition 1919; second, much revised edition, 1923) and in *Physics and Hypothesis* (PH, 1921). Exact science is described in these books as proceeding from a prescientific situation, in which no valid general statements can be made, toward a practically unattainable but indefinitely approachable goal, in which each particular statement of fact would be understood as an instance of some universal law, inferrable from a few simple axioms. This edifice of exact science, whose foundations were laid by Euclid and Newton, is called by Dingler "the pure synthesis" (subsequently: "the System"). Dingler contrasts this slowly growing but rock-firm core of human knowledge with the peripheral, more volatile forms of scientific enterprise. Here fast advances are made by proposing hypotheses that are experimentally tested in the manner explained in standard textbooks of scientific methodology. But no meaningful tests can be carried out, no reliable data can be extricated from the wavering, drifting store of human perceptions, unless we can resort to a firm, unambiguous theoretical framework, regulating the construction of scientific instruments and the interpretation of their readings. According to Dingler, such a framework can only be supplied by the "pure synthesis," whose results can never therefore be undermined by experiment or contradicted by a viable scientific hypothesis. Consequently, the several components of "the System"—logic, arithmetic, geometry, mechanics—can be built by postulation and exhaustion, in the way described in GAG and cursorily sketched above, provided only that each discipline is careful not to contradict the preceding ones. Geometry, ever faithful to logic and arithmetic, need not fear a clash with other scientific theories, for all the rest are subordinate to it. Like a majority of his contemporaries, Dingler believed that every experimental measurement involves a measurement of distance and consequently depends on the accepted geometry and specifically on the standard of rigidity. The difficulty mentioned at the beginning of this paragraph does not therefore exist for geometry.

In fact, the method of exhaustion, by which experimental results that appear to deviate from the theory are interpreted as the effect of uncontrolled perturbations and classed as "errors," is beautifully illustrated in the field of applied geometry. Take the case of surveyors. Dingler quotes the following pasage from Jordan's *Handbuch der Vermessungskunde:*

> If the three angles of a triangle are measured with equal precision, there will arise, because of errors of measurement, a difference between the

angle sum and 180°. This difference is distributed by equal parts between the three measured angles.[17]

Clearly, surveyors do not look upon Euclidean geometry as an empirical science, for *they correct their experience by this geometry.* In the same book (PH, pp. 26 ff.) and in a separate article on "The rigid body" (1920a), Dingler takes up the question of the manufacture of rigid bodies in the factories of precision instruments. Rigid bodies sold around the world to scientists and industrialists are made and judged according to some ultimate standards of rigidity that Dingler calls autogenous rigid bodies. The latter are required to comply with Euclidean geometry. Suppose now that the exactest available autogenous rigid body appears, on examination, to violate the laws of that geometry. Suppose all the more obvious sources of error have been eliminated. What can one do under such circumstances? Dingler says that one can either (1) regard the fact as the experimental proof that real space is non-Euclidean or (2) ascribe the deviation from Euclidean geometry to an unknown cause or to an unknown combination of known causes. Dingler proposes with tongue in cheek that a questionnaire be sent to the better known manufacturers of precision instruments asking them what their reaction would be if the above came to pass. He is quite certain that they will all in practice follow alternative (2) even though, if they have been reading too much philosophy of science, they might not confess it openly.[18] Moreover, he is certain that the manufacturers will not hesitate to use Euclidean geometry in all the *calculations* required for checking the properties of the autogenous rigid body. Dingler believes that, living as we do in a vast and broadly unknown world, we are under no obligation to determine the true cause of the anomalous (i.e. counter-Euclidean) behavior of a body and to correct the anomaly within a fixed length of time. On the other hand, it is clear that, if such anomalies arise, there will never be any dearth of hypotheses for explaining them. Dingler concludes that the rigid body is manufactured in the factories that supply all scientific laboratories and observatories with their research equipment "not empirically but by exhaustion of Euclidean geometry".[19] Hans

[17] Jordan, *op. cit.*, vol.II, 8th ed. (Stuttgart, 1914), p.22, quoted in Dingler, PH, p.41n. Dingler also remits to vol.I of Jordan's work (6th ed., 1910), pp.32 and 208 f.

[18] Dingler (1920a), p.490.

[19] Dingler (1920a), p.492. See also Dingler, GP², p.140. In GP², p. 136, he bids us take a look at a worker making a fine precision instrument. "*He continually resorts to geometrical controls drawn from Euclidean geometry.* He

Reichenbach objected to the proposed geometrical definition of the rigid body, asserting that the latter can also be defined dynamically, namely as a closed system, in which case it is an open, empirical question whether rigid bodies are actually Euclidean or not.[20] For Reichenbach a closed system is one that is not subject to the influence of external *differential* forces[21] and he apparently thought that such systems can be detected by mutual comparison, without having to resort to a previously established geometry. But, since no body is ever totally immune to external differential forces for a significant length of time, if Reichenbach's criterion of rigidity is adopted the question that must be answered in actual practice is not which bodies are rigid, but which are more nearly so, i.e., which bodies are subject to a smaller deformation due to the presence of such forces. According to Dingler the magnitude and sense of the deformation suffered under given circumstances by different bodies can only be determined with the aid of the geometrical standard of rigidity. This standard provides the "zero-point" to which all comparisons of shape must be referred.[22]

One can hardly conceive a stronger defense of the conventionality of geometry. But if Dingler is right any geometry or at any rate any three-dimensional geometry of constant curvature can be likewise upheld by exhaustion. It is merely a matter of choosing its motions as characteristc of rigidity. Why then Dingler's unmitigated preference for the Euclidean system? We read in GAG that "it is generally agreed that Euclidean geometry is the simplest."[23] Dingler, however, does not just accept it from hearsay, but he seeks to prove it. In several occasions he gives different arguments for the maximal simplicity of zero-curvature geometry which, if valid, would of

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demands that the end surfaces of a screw remain parallel to itself, that all circles and rectangles satisfy certain well-known geometrical relations, etc. If he notices in his device any deviations from these rules he will not claim that he has found the empirical proof of non-Euclidean geometry but he will say that 'there is something wrong' and he will manipulate his device until it is 'right', i.e. until the greatest possible agreement with Euclidean geometry is achieved. But this is precisely the *method of exhaustion* . . ."

20 Dingler (1922), p.52.

21 According to Reichenbach, a *differential* or *physical* force is one which acts differently on different bodies and can be neutralized by shielding; he constrasts such forces with universal or metric forces, which act equally on all bodies and are not stopped by insulators. Reichenbach maintains that the latter can always be equated to zero by suitably adjusting geometry. (Reichenbach, PST, pp. 10 ff.)

22 See Dingler, GP[2], pp. 133, 141 f.

23 Dingler, GAG, p.132.

course, by the Principle of Economy, uniquely prescribe the choice of it.

In GAG and again twenty years later (Dingler (1934)) he claimed that Euclidean n-space is simpler than every other n-dimensional Riemannian manifold because it alone possesses a chart u with coordinate functions $u^1, \dots, u^n$, that fulfils the following requirement: for every integer i $(1 \leq i \leq n)$ and every pair of real numbers a and b in the range of u^i, the mapping that assigns to each point P on the hypersurface $u^i=a$ the point P′ on the hypersurface $u^i=b$ which has, except for the i-th coordinate, the same coordinates as P, is an isometry of the former hypersurface onto the latter. I fail to see why a Riemannian manifold that meets this condition should in any sense be simpler than one that does not. Moreover, Dingler's proof of his alleged characterization of Euclidean space is defective. He argues more or less as follows: Let the components of the metric tensor relative to chart u be designated by g_{jk} $(1 \leq j,k \leq n)$. u will meet the stated condition if and only if the line element $g_{jk}du^j du^k$ is identical for every hypersurface $u^i=$ const. This implies that the g_{jk} do not depend on u^i, so that

$$(1) \qquad \frac{\partial g_{jk}}{\partial u^i} = 0 \qquad (1 \leq i,j,k, \leq n)$$

Since this must hold for every value of the index i the components g_{jk} of the metric tensor (relative to the chart u) must be constant. This can hold only if the manifold is Euclidean. The flaw in Dingler's argument is obvious. Take two hypersurfaces $u^i=a$ and $u^i=b$, and let their line elements be represented respectively by $g_{jk}du^j du^k$ and $\bar{g}_{jk}du^i du^k$ Since $du^i=0$ on either case, differences between g_{ij} and $\bar{g}_{ij}$ $(1 \leq j \leq n)$ will make no difference in the line elements. Consequently the condition (1) need hold only for $j \neq i \neq k$ and we are left entirely in the dark concerning the behavior of the derivatives $\partial g_{ij}/\partial u^i$ $(1 \leq i,j \leq n)$. It follows that the g_{ij} need not be constant and the manifold might not be Euclidean. The flaw in Dingler's proof was pointed out to him by Kurt Reidemeister. Dingler responded by imposing a further requirement on the chart u: the "parameter curves" (i.e., the integral paths of the $\partial/\partial u^i$) must meet orthogonally. This implies that $g_{jk} = 0$ for $j \neq k$ $(1 \leq j,k \leq n)$ on all the domain of u.[24] I am not sure that this will save the foregoing argument but it certainly suffices to characterize Euclidean n-space. For a chart u meeting *both* the stated requirements is what is usually called a

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24 Dingler (1935), p.674.

Cartesian coordinate system and it is a well-known fact that such systems can only be defined on Euclidean spaces. But who would venture to assert that a spherical surface is less simple than a cylinder because Cartesian coordinates can be introduced on the latter, while on the former one must make do, say, with latitude and longitude?

In his article "On the concept of maximal simplicity"[25] Dingler proposed an exact criterion of maximal simplicity by which, he claimed, the Euclidean metric turns out to be simpler than any other Riemannian metric. Dingler's criterion of maximal simplicity is of course essential for all applications of the Principle of Economy, on which his "pure synthesis" is made to rest, at least until the early twenties. It may be profitably compared with other such criteria proposed by simplicity-loving contemporary epistemologists. Dingler's criterion is intended to single out a "logical formulation" within a series or "group" of such formulations. Logical formulations *(logische Formulierungen)* are identified by their features *(Bestimmungen).* Features shared by all formulations in the group are called group-features. All other predicates of a formulation are said to be individual features. Two or more features are said to be independent if the negation of any of them is compatible with the conjunction of the others.[26] A feature is said to be negative if it consists in the absence of a feature; otherwise, it is said to be positive.[27] A feature is said to be elementary (relative to a group of formulations) if it is not a conjunction of independent features or, if it is such a conjunction, all its components jointly determine every formulation of the group which shares any of them. The simplest logical formulation of a group is defined to be the one that posseses the least number of positive, independent and elementary individual features (relative to the group).[28] Dingler's proof that Euclidean geometry is, according to this definition, the simplest Riemannian geometry, is less clear and straightforward than one might wish, but, if I understand it well, it ultimately rests on the following consideration: the metric tensor of Euclidean *n*-space is fully

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[25] Dingler (1920b); reproduced in Dingler, GP², pp. 101-113.

[26] Dingler defines independence only for a pair of determinations: they are independent if the negation of one is not incompatible with the other. But he subsequently uses this concept as if it were an *n*-ary relation between determinations (for any $n > 1$). I presume that he would have accepted the definition I gave above.

[27] "Eine negative Bestimmung ist die Aussage, dass die Formulierung eine gewisse Bestimmung nicht hat." (Dingler (1920b), p.427.) I confess that I am unable to make any sense of this defintion.

[28] Dingler (1920b), p.428.

determined by the $n(n+1)/2$ constant values of its components relative to a suitable chart, whereas no other metric tensor of a Riemannian n-space is fully determined by such a small set of numbers.[29] The argument is conclusive only if the constant value of a component of a metric tensor relative to a chart of the respective n-manifold constitutes an elementary feature of the tensor, in Dingler's sense. Now such a component, even if it happens to be constant, is not just a number, but a scalar field. The statement that it is everywhere (on the chart's domain) equal to a fixed real number k can be analyzed into the statement that its value at an arbitrary point of its domain is k and n statements to the effect that its partial derivatives with respect to the coordinate functions are all, at that point, equal to zero. These statements are not jointly true of every metric tensor of which any of them is true. Consequently the statement that was analyzed into them does not express an elementary feature (relative to the group of Riemannian metric tensors). The metric of Euclidean n-space does not therefore exhibit only $n(n+1)/2$ positive, independent, elementary individual features, but at least $(n+1)^2 n/2$—provided that the features brought to light in the preceding analysis are indeed elementary. But in that case the Euclidean metric is not the simplest Riemannian metric but only one of the simplest, according to Dingler's criterion. For the metric tensor of any maximally symmetric n-space can be specified in terms of a suitable chart by giving the value of its components relative to that chart at an arbitrary point of the chart's domain and the value of its partial derivatives at that point with respect to the coordinate functions. And this makes $(n+1)^2 n/2$ features, exactly as in the Euclidean case. We need not probe into this matter any further. I have dwelt on it so long chiefly because it serves to illustrate the pitfalls one is bound to run into when discussing the comparative simplicity of complex theories. In *Das Experiment* (1928) and after, Dingler no longer looks on simplicity as the decisive criterion that guides the construction of scientific theories. He expects instead that the scientist's will to refer all the glittering and ever changing appearance of nature to a few definite, stable, unambiguously reproducible forms shall suffice to determine the system of exact fundamental science. Thus, he writes,

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[29] The complications in Dingler's actual argument are partly due to the fact that he tries to prove a stronger proposition, namely, that Euclidean space is the simplest metric space (or, as he puts it, that the Euclidean distance function is the simplest such function). However, he makes some additional assumptions that restrict the metric spaces under comparison to the class of Riemannian n-spaces. See Dingler (1920b), pp.431-433.

"when we strive to find *absolutely unambiguous forms* (and such is the unavoidable foundation of all exact science) we are led perforce to Euclidean geometry."[30] However, as one of his critics did not fail to point out, Dingler often uses his new favorite term "unambiguousness" *(Eindeutigkeit)* much in the same sense in which he earlier used "simplicity" *(Einfachheit)*. For instance, he characterizes uniformly accelerated motion as "the most unambiguous" *(eindeutigste)* motion that starts from rest, because it "contains the least number of defining features" *(die wenigsten Bestimmungen)*.[31]

The deadliest objection against the definition of the rigid body by "the laws of Euclidean geometry" was raised by Dingler himself. He called it "Wellstein's objection," because he drew it from Joseph Wellstein's article on the foundations of geometry in the Weber-Wellstein Encyclopedia of Mathematics. (The article was published several years before GAG). Wellstein's objection stems from the essentially abstract nature of deductive theories. Any consistent theory admits several "models", i.e. interpretations under which all its theorems come true, and if the theory is not too simple it can be modelled in many incompatible ways in one and the same domain of objects. Moreover, any of the models can provide a framework for setting up others. Consider the case in point of Euclidean geometry, that is, the set of all logical consequences of Hilbert's axioms. Let E denote a model of Euclidean geometry. In other words, E is a set of objects, called "points", structured as a Euclidean space. Consider the collection of all spheres of E that pass through a fixed point P. Excise P. There remains a collection of punctured spheres. Call each punctured sphere a "plane" and the intersection of two such "planes" a "straight". If we understand "point" and "incidence" as before and contrive a suitable interpretation of congruence the axioms of Euclidean geometry will all be satisfied on $E-\{P\}$, on the

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30 Dingler, EW², p.134; he adds further on that a unique geometry, i.e. "so-called Euclidean [geometry]", results "einfach aus der Absicht heraus, die überhaupt erst zur Gründung einer Geometrie hintreibt, aus der Absicht, in dem vielgestaltigen und fliessenden Wirklichen feste, eindeutig bestimmte Formen aufzustellen." (*Ibid.*, p. 138).

31 Dingler, E, p. 117; see Th. Vogel's remarks in v.Aster and Vogel (1931), p. 11. Subsequently Dingler regarded the quest for maximal simplicity as a consequence of the supreme scientific principle of unambiguousness (*Eindeutigkeit)*. He proves it as follows: "Der Geist ist fähig, ungezählte Arten von zu realisierenden Plänen (Ideen) zu fassen (Grundfähigkeit). Um unter diesen eine eindeutige Auswahl zu treffen, bedarf es eines formalen Prinzips. Da sich aber die Definition einer Idee stets durch Angabe einer (endlichen) Zahl von Bestimmungen vollzieht, die beliebig gross werden kann, so ist das einzige Mittel einer eindeutigen Festlegung formaler Art ein *Minimumprinzip für diese Anzahl.*" (Dingler, MP, p.197.)

proposed interpretation.[32] Let E′ denote the set E − {P}, structured as a Euclidean space according to this interpretation. The Euclidean group of motions acts of course on E′ in a definite way. But a "parallel translation" or a "rotation about a point" in E′ have next to nothing in common with their namesakes in E. Our example shows clearly that "Euclidean geometry is unable to define the rigid body unambiguously."[33] In order to characterize the rigid body as a "thing [that] changes in accordance with the laws of Euclidean geometry (of Euclidean motion)"[34] we must first indicate how the Euclidean system is to be understood in its applications to physical things. Giving up the opinion he had voiced in GAG, Dingler now acknowledges that the connection between geometry and reality must be secured before the rigid body is defined and cannot be brought about by its definition alone. The step-by-step construction of physical geometry, brooched in the second edition of *Die Grundlagen der Physik* (GP2, 1923) and much furthered in *Das Experiment* (E, 1928), is carried out in full in *Die Grundlagen der Geometrie* (GG, 1933), and again, in a different, seemingly more rigorous manner, in *Der Aufbau der gesamten Fundamentalwissenschaft* (AEF, completed in 1944, published posthumously in 1964).[35]

3. *"The System" and "the Untouched".*

Before presenting the final version of Dingler's philosophy of geometry it is advisable to say a few words about his mature conception of the system of exact science to which geometry belongs. This is developed in *Das Experiment* and *Das System* (S, 1930), and again in *Die Methode der Physik* (MP, 1938) and in his posthumous works *Die Ergreifung des Wirklichen* (EW, 1955) and *Aufbau der exakten Fundamentalwissenschaft.*[36] I take it that Dingler's philosophy of science remained essentially the same throughout this period, although in each new book he shifted perspectives and changed the terminology, apparently with the aim of making his teachings more perspicuous and persuasive.

Dingler never tired of proclaiming that security is the essence of science.[36] At the frontier posts of knowledge, one may indeed

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32 Wellstein, GG, pp.34 ff.

33 Dingler, GP2, p.147.

34 Dingler, GAG, p.132; quoted above on p. 7.

35 See Dingler, GP2, pp. 147-164; E, pp. 56-109; GG, pp.5-31; AEF, pp.153-187; see also MP, pp. 95-113, 164 ff.; EW2, pp. 131-142.

36 Dingler, MP, p.39; cf. GKTW, pp.4, 76; GP2, p.32; S, p.60, etc.

venture hypotheses, in the interest of a quick advance. But such hypotheses could not be measured up against facts and would even lack a definite meaning if science did not possess an indestructible core of absolutely certain knowledge. Dingler never reconciled himself with the modern vision of the boat of science being unbuilt and rebuilt while out at sea.[37]

He motivates the scientific quest for certainty by an appeal to the supreme ends of mankind. No matter what these ends are man must seek to fulfil them in reality, and this implies, according to Dingler, that he must wish to learn more and more ways "of unambiguously exerting on this reality unambiguously preconceived effects." The ability to exert such effects he aptly calls "the domination of nature" *(Die Beherrschung der Wirklichkeit oder der Natur)* which is to him "the universal goal of all science." For the stated reason, the pursuit of this goal is a "prerequisite of man's ethical activity."[38] The "domination of nature" in the above sense must rest on stable rules and laws stating what has to be done in order to obtain the desired effects. To establish such laws, with the widest possible scope, is according to Dingler the chief task of science.[39] How can one achieve a secure knowledge of general laws? Dingler rejects the classical rationalist conception of "self evident" universal truths that are "intuitively" grasped.[40]

All universal statements are questionable for they refer to an unlimited number of cases, which cannot all be perceived at once. There are only two kinds of statements that Dingler holds to be indubitable: (a) Statements describing whatever is directly experienced or "lived through", without any explanation or interpretation *(Erlebnisaussagen);* and (b) statements concerning feasible actions "that can become and can have become actual or real at any time" *(Handlungsaussagen).*[42] Consequently, if science is to be thoroughly

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[37] I have not found evidence that Dingler was familiar with Otto Neurath's metaphor, but he does mention with obvious dislike Popper's comparison of science with a building erected above a swamp on piles "of which one knows that they must founder again and again" (Dingler, MP, p.14; cf. Popper, LSD, p.111.)

[38] Dingler, MP, p.42; cf. S, p.31. The highest goal of mankind is the subject-matter of ethics. Dingler devoted to it his book *Das Handeln im Sinne des höchsten Zieles* (1935).

[39] Dingler, MP. p.44.

[40] See, e.g., Dingler, PH, p.118; S, pp. 50f.

[41] Dingler, S, p.33.

[42] Dingler, MP, pp.34,34. In *Das System* these two kinds of statements are called, respectively, (a) *Hic-et-nunc-Aussagen* or *Intellektuationssätze* and (b) *Willensaussagen* or *Realisationssätze.* (Dingler, S, pp.33,34,40,41.)

and securely grounded, all scientific statements must be shown to rest on either of these two kinds of uncontroversial "carefree" statements.[43] However, since induction is a hoax and no universal laws can be based on particular statements of fact, *Erlebnisaussagen* cannot provide alone the sought for foundation of science.[44] Therefore science must ultimately rest on *Handlungsaussagen*, that is, on proposals—and programs—for action. Each such proposal, if at all feasible, can be carried out innumerable times, "semper et ubique (in intention at least)", and thus possesses a kind of inbuilt universality, stemming "from our decision to apply [it] wherever it is appropriate."[45] Or, as Dingler puts it in AEF:

> In the sphere of that which is *subject to our will* we can in fact [. . .] *bestow universal validity on* some statements simply by voluntarily carrying them out.[46]

The fundamental "action statement" or "first proposition of the will" *(erster Willenssatz)* from which all science depends is carefully articulated into a system of 22 principles in AEF (pp. 38-48), under the general title of "The Plan." For our present purposes it will suffice to quote a much shorter collection of principles, listed in an earlier work as the "demands" of an "ideal methodology"

> (i) Science must provide an absolute and gapless foundation for each of its statements *(Prinzip der Begründung).*
>
> (ii) Its several stages and steps must be so ordered that none is presupposed by its predecessors *(Prinzip der Ordnung,* also called *Prinzip der pragmatischen Ordnung,* because it forbids "pragmatic vicious circles").
>
> (iii) It must be possible to reconstruct science in its entirety at any time, so that, by following the same method again from the beginning one arrives at exactly the same results *(Prinzip des beliebigen Neuaufbaues).*[47]

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[43] Dingler, MP, p.60.

[44] On induction, see for example, Dingler, PH, pp. 102, 132, 137 ff.; GP^2, pp.39 ff.; S, p.50; MP, pp.340 ff.; EW^2, pp.141 f.; also Dingler (1920c), p.130, (1923), pp.23,27.

[45] Dingler, S, pp.41, 51.

[46] Dingler, AEF, p.22.

[47] Dingler, MP, p.71. The first and the last principle are common to all rationalist epistemologies, but the principle of pragmatic order is characteristic of Dingler. A typical violation of this principle is "the circle in the empirical foundation of geometry", which arises when geometrical propositions are empirically tested by means of instruments built in accordance with a definite geometry. (See Dingler (1925).) Because all exact scientific experiments require instruments embodying the choice of a geometry, Euclidean geometry is

The principles of ideal methodology, especially the last two, clearly imply that science must have a definite starting point. This must be found in a prescientific stage of human consciousness, in which no scientific laws, and, generally speaking, no universal statements can be held to be valid. This stage or sphere of life Dingler called "the pre-universal standpoint" and also "life's standpoint," because it is the standpoint that we ordinarily take in everyday life.[48] Within this sphere we distinguish naturally between that which depends on our own initiatives, our own "will", and "the Given", i.e., "that part of my world in which I have not undertaken any deliberate intellectual changes *(bewusste geistige Aenderungen)*".[49]

As a technical term for the given, Dingler (1942) coined the expression *das Unberührte,* the Untouched.[50] This is the rock bottom ground beyond which no foundational inquiry can go, the "zero-point" from which the methodical construction and reconstruction of science must begin. In agreement with the third methodological principle, the Untouched must therefore be recoverable at any time. Dingler believes that its recovery can be effected by abstaining from all general assumptions and theoretical interpretations, from "every deliberate intellectual addition" to what is purely and simply perceived.[51] We need not dwell here on the familiar difficulties that beset philosophical conceptions of the given. We must mention however than in Dingler's mature view, the Untouched almost coincides with the world of unsophisticated common sense. When all theories are put aside, he says, things about me are simply seen to be "out there", quite apart from any considerations regarding their causal action on my senses, such as, say, that I can

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absolutely to be preferred over other geometries of constant curvature (which Dingler regards, like Helmholtz, as the sole conceivable physical geometries, being the only ones compatible with the existence of rigid bodies). Since every geometry of non-zero constant curvature involves a parameter—namely, the curvature—which, in practice, must be fixed by measurement, any attempt to construct the fundamental instruments of measurement according to such a geometry would be pragmatically circular for it would necessitate the use of those very instruments for measuring the parameter (Dingler, EW^2, p.164).

48 "Vorallgemeinstandpunkt" in Dingler, GP^2, p.8, "Lebensstandpunkt" in PH, pp.114 f., 122 f.; AEF. p.34; "Voreindeutigkeitsstandpunkt" in S, p.54; MWL, pp. 34-38. The earliest formulation, still strongly influenced by 19th century mentalism, in GKTW, pp.24 ff.

49 Dingler, AEF, p.30.

50 See also Dingler, AEF, pp.30 ff.; EW^2, p.80. In E, S and MP Dingler had often spoken of "untouched nature" *(die unberührte Natur).*

51 Dingler, EW^2, p.81.

only see them through my eyes and that I am not therefore really in touch with them but only with their sensuous appearance. Other persons I meet as such, i.e. as living and thinking people, and it were madness to describe them as perceived bodies whose behaviour intimates that they are probably governed by rational souls. The given comprises all the "basic faculties" *(Grundfähigkeiten)*—including the faculty of speech—that are necessary for the construction of science. Dingler repeatedly emphasizes that the givenness of such basic faculties is not a *logical* but a *practical* presupposition of science; they are not a premise to its deductions but a source of its actual development. Because of this, it is neither necessary nor advisable to analyze them before proceeding to the rigorous reconstruction of science. It is enough to exercise them as required as we go forward.[52]

Foremost among the basic faculties needed for science is the capacity to conceive what Dingler calls *ideas (Ideen)*.[53] He describes them as mental forms or structures *(geistige Gebilde)* that cannot be had as direct perceptions of reality nor as memory images of such perceptions.[54] Such is, for example, the geometric idea of a widthless line. Ideas are schematic and can be totally grasped and analyzed for they contain only what we have deliberately put into them. They contrast with perceived realities, with their unlimited wealth of detail. Indeed, the infinite fullness of the latter, as against the essential meagreness of the former, is singled out by Dingler as the main criterion for distinghishing between the "outer world" of reality and the "inner world" of ideas.[55] The very poverty of ideas is what makes them into the mainstay of exact science. In order to secure its goals one must be able to extract from "ever flowing nature" unambiguously defined, reproducible forms. But "in nature itself there are no such univocally definite elements, whose identity can always be verified with the highest available precision."[56] That is why the scientific domination of nature necessarily depends on the conception and the deliberate, ever imperfect but ever improving realization of ideas. However, not any arbitrary idea, not any "free

52 That all the natural abilities demanded for the construction of science must be given at the outset was pointed out by Dingler in his earliest book (GKTW, p.33). He subsequently returned several times to the subject. See Dingler, MWL, pp.64 ff.; MP, p.31; AEF, pp.33 ff.

53 Dingler, AEF, p.54; EW², p. 62.

54 Dingler, EW², pp. 109f.; cf. p.61 and AEF, p.41.

55 Dingler, EW², p. 75.

56 Dingler, AEF, p.54.

creation of the human mind" will do for this purpose. The ideas of science must be built following a strict order, in accordance with the relevant principles of the Plan, beginning with the fourfold primordial idea we shall discuss below. Ideas built synthetically and methodically from this one are called by Dingler *ideas of s-science* or *s-forms* (where *s* stands for *secure*). "Every attempt at determining something in nature in a truly unambiguous and reliably reproducible way can only be carried out by means of the realization the ideas of *s-science* [. . .] Indeed, every exact measuring instrument (from the simple meter rod to the interferometer, the quartz clock, etc.) is such a realisation of *s*-forms. *S*-forms are therefore the only available means of achieving unambiguous statements, exact concepts and laws in the natural sciences."[57]

The methodical synthesis of *s-forms* is presented by Dingler in AEF. One of the principles of the Plan or "first proposition of the will" developed at the beginning of that book prescribes that science, in its intellectual part, must work exclusively with ideas.[58] These ideas must be perfectly definite and it must be possible to reproduce them (mentally) without ambiguity and to recognize them without hesitation. They must also be realizable with increasing precision and their realizations ought to be recognizable as such. The latter requirement implies that scientific ideas must be specified by properties or relations drawn from actual experience *(das wirkliche Erleben).* Consider any such property. It is indeed unambiguous as an experience, while it is experienced. But there is no way of fixing it unambiguously in our minds or outside them; for mere recollection does not certify identity through time and there is no guarantee for the constancy of empirical circumstances. (None, at any rate, before exact science has been erected.)[59] Scientific ideas can therefore only be specified by perceived relations *(Relationserlebnisse).* But not any such relation will do, either. "Only those perceived relations are relevant which are wholly independent of the peculiar nature of the related terms."[60] According to Dingler only one perceived relation or "perception of relation" satisfies this requirement, and that is the perception of *difference.* This is had whenever we perceive an "unqualified something" *("überhaupt Etwas"),* which is thereby singled out and characterized as "different from the rest, the

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[57] Dingler, AEF, p.55.
[58] Dingler, AEF, p.40.
[59] Dingler, AEF, p.44.
[60] Dingler, AEF, p.45.

'background'.[61] This relation is perfectly unambiguous, for there is only one sort of difference. It can be unambiguously fixed in the mind, and is unambiguously reproducible and recognizable at all times.[62] Dingler concludes that the primary *s*-idea, from which all other such ideas must be synthesized, is the idea of an unqualified, distinct something *(Etwas Unterschiedenes überhaupt).* This can be regarded (I) *in itself* and (II) as *limited by the background.* Both the something and its boundary can be viewed as (a) *constant* and (b) *variable.* We thus obtain a fourfold idea, which Dingler calls the Schema:

(Ia) Something distinct, considered in itself, constant.
(Ib) Something distinct, considered in itself, variable.
(IIa) Something distinct, considered with regard to its boundary, constant.
(IIb) Something distinct, considered with regard to its boundary, variable.

"The specific kind of something that is given by the Schema in each of these four cases becomes the fundamental element of a science, when the corresponding case is dealt with according to the principles of the Plan."[63] The four sciences in question are:

(Ia) The science of number (Arithmetic)
(Ib) The science of time and variables (Analysis)
(IIa) The science of space (Geometry)
(IIb) The science of motion and its causes (Mechanics).

It is not possible to sketch here Dingler's construction of Arithmetic and Analysis (AEF, pp. 57-137, 188-196; a similar, though greatly improved construction, is given in Paul Lorenzen's book, *Differential und Integral*, of which there is an English translation.) However, before brooching our chosen subject of geometry, it will be useful to paraphrase Dingler's discussion of the realization of the fundamental idea of arithmetic, namely, the idea of an unqualified, distinct, constant something. A natural object must be produced in which the idea is brought out as clearly as possible. Such an object necessarily possesses infinitely many irrelevant properties, which must be maximally inconspicuous, while its

61 Dingler, AEF, p. 45.
62 Dingler, AEF, p. 46.
63 Dingler, AEF, p.57.

property of being different (from the background) is suitably enhanced. This is achieved as follows: we take a maximally uniform background, such as a white leaf of paper, and produce in it a difference of the sort described, e.g. a small black spot. Such a spot meets the stated conditions. One pays no attention to its limits, its colour, etc. One sees only that an unqualified something is here singled out.[64] One can subsequently consider another realization of the same idea, and view them both as belonging together. By repeating this procedure one can obtain a realization of any positive integer.

4. The Foundations of Geometry

We have seen that young Dingler, in the wake of Ueberweg and Helmholtz, regarded the twin concepts of rigid body and rigid motion as the central concepts of gemetry. In GAG he sought to define these concepts by means of the Euclidean axioms, which, he maintained, determined the simplest conceivable distance function. A rigid body, in this view, is any body such that the (Euclidean) distance between any two of its points remains constant as the body moves in space. Dingler changed this approach when he became aware that, depending on the interpretation given to the Euclidean axioms, one could obtain many different, mutually incompatible, realizations of the rigid body. (It is indeed surprising that Dingler should have taken so long in discovering this, for the abstract—and hence systematically ambiguous—nature of axiom systems was duly stressed in his earliest book.)[65]

In GG he claims to build geometry, "for the first time since Euclid", on entirely new, differently conceived, foundations.[66] The new method consists in defining the rigid or, as he now says, the "deformation-free" body *(deformations-freier Körper* or *Df.K.)* by gradual specification of the original, admittedly unaccountable idea of a "space" that can be divided by "surfaces".[67] The specification is done by means of a series of "definitionlike statements", "employing the language of everyday life and certain expressions [. . .] that designate immediately intuitable contents *(unmittelbare*

64 Dingler, AEF, p.58.

65 Dingler, GKTW, pp. 6 ff.

66 Dingler, GG, p.iii; cf. pp.27 ff.

67 Dingler, GG, pp. 5 ff. On p.6 Dingler emphasizes that it is useless to try to account for such ideas. "We must start here—he writes—from the simple fact that we have them, and use their givenness as an instrument."

Anschaulichkeiten) from our ordinary everyday sphere of action."[68] Such expressions enable us to prescribe unambiguously the actions leading to the realization of the relevant geometrical forms. The definitionlike statements in question are certainly not nominal definitions that introduce a shorter substitute for a complex, but well understood expression. In the new geometry they do indeed play the role of axioms; but they differ, say, from Euclid's or Hilbert's axioms in that they "merely take note of the existence of a basic faculty of direct comparison" and they "possess a direct unambiguous reference to reality, for they always express performable actions and have no meaning apart from that."[69]

On the other hand, in AEF geometry is presented as the theory of unchanging boundaries, and consequently all kinds of motion, rigid or otherwise, lie beyond its pale.[70] Dingler's construction no longer aims therefore at defining the Euclidean rigid body, but at justifying the Euclidean axioms (in Hilbert's version) as necessary consequences of the only viable unambiguous specification (by means of the relation of difference) of the primordial idea of an *unqualified something, considered with regard to its constant boundary.* Notwithstanding this important difference of aim, and a few no less important differences in execution, Dingler follows in AEF the same steps as in GG, successively specifying the topological ideas of *surface, line* and *point*, the affine ideas of *plane, straight line* and *parallellism*, and the metric idea of congruence; and there is little doubt that he regarded the theory developed in AEF only as a more perfect version of the one presented in GG. In either book, anticipating the objection that, in the end, he has merely produced a new-fangled axiom system, he emphatically asserts that his system differs from all its predecessors insofar as by the very manner of its generation it is naturally endowed with a definite interpretation.

The starting-point of geometry is very clearly described by Dingler in his latest summary of the matter (in EW): "We call something bounded, a body; the boundary, we call its surface. [. . .] The experience *(Erlebnis)* of a body bounded on all sides must be obtained from the Untouched, so to speak; otherwise we cannot begin at all."[71] It is clear that we do have the said experience. But is it our

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68 Dingler, GG, p.7.

69 Dingler, GG, p.13; cf. pp.30 f.

70 In the light of this development it is ironic to recall Dingler's earlier rejection of "the Platonic view, shared by Euclid (and Hilbert), that movement may not occur in geometry". Dingler had declared that this view rested "on false epistemological assumptions" (GG, p.23).

71 Dingler, EW^2, p.131.

only experience of something bounded? Cannot we perceive a surface bounded by lines or a line bounded by points? Moreover, though such experiences may indeed suggest our *idea* of something bounded does not the latter go far beyond them? We cannot visualize, perhaps, but we can certainly conceive with the utmost precision an n-dimensional solid for any integer n greater than 3. To such questions Dingler would have probably replied that any bounded object we might make or handle is a (3-dimensional) body and its boundary is what we ordinarily call a surface.[72] Dingler chooses to regard bodies as distinct from their surfaces. (In other words, a Dinglerian body is always identical with its *interior*.) Both in GG and in AEF, Dingler introduces at the outset the idea of *qualitative space* or *Q-space (analysis situs space* or *AS-space* in GG), as the background from which bodies are separated by their surfaces. There follow in GG a few seemingly straightforward definitions: Q-space can always be divided by a surface into two separate parts; such a surface is called a *separation surface.* A separation surface T may in its turn consist of parts that lie in different regions of space divided by a separation surface T′ which does not meet T. If such is not the case, T is called a *running surface (Lauffläche).* Dingler notes that such a surface always has two sides, corresponding to the two parts into which it divides space. A running surface can always be divided by a line into separate parts. Such a line is called a *separation line.* A *running line* can be defined analogously. A running line can always be divided into two separate parts by a so called *separation punctual.* If P is a punctual such that no two parts of P lie on different sides of a running surface, P is called a *point.* In AEF this simple development is replaced by twelve dense pages of definitions, assumptions and theorems, amounting to a clumsy, skimpy, but for Dingler's purposes probably sufficient topological theory of surfaces. Its declared aim is to eliminate the ambiguities in the everyday concept of a surface.[73] In the time elapsed after publishing GG,

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[72] In a curious passage of MP, p.105, Dingler sought to justify the choice of a 3-dimensional space by observing that it was the simplest space in which arbitrarily many bounded regions can simultaneously stand in mutual contact, each with all. This, he felt, was required in order to fulfil "the desire for establishing arbitrarily many reciprocal causal actions". It is indeed well-known that in a 2-dimensional space homeomorphic with the Euclidean plane five or more regions cannot be mutually in contact each with all, except through a point. But it is hard to see why contact through points should not be good enough for transmitting causal influence. One may also take exception with the assumption that the "simplest" space is the one with the smallest admissible dimension number.

[73] Dingler, AEF, p.160.

Dingler obviously became aware that there are unspeakably many ways of exactly defining that concept. He consistently prefers what he takes to be the simpler, more natural alternatives, barring such monsters as a surface with isolated points or with protruding hairs or blades, that might be shaved off, so to speak, by a separation surface which does not meet the body of which the surface in question is the boundary. The definitions of GG are essentially preserved, but further qualifications and distinctions are now introduced, such as that between open and closed running lines or between simply connected and multiply connected running surfaces. If we are allowed to take a look at the collection of all points discernible in Q-space, we shall see that Dingler's theory furnishes it with a natural topology, in which Dinglerian bodies are the open sets. That it is a Hausdorff topology follows at once from the very definition of a point. Does Dingler's theory succeed in specifying the topology of Q-space any further? It is hard to tell. We may assume that a body whose boundary is a simply connected running surface is always homeomorphic to R^3, so that Q-space is a three-dimensional topological manifold. But the only hint we are given with regard to its global topology is not easy to interpret. Statement 1,22 says that space is "unboundable" *(unbegrenzbar)*. This can be taken to mean merely that to space as such no boundary can be set; that no surface can be found at which, so to speak, every running line will stop. On this interpretation, 1,22 is innocuous and not very illuminating. Indeed, every topological manifold is unboundable in this sense. Dingler adds, however, that if space is divided by a separation surface into two bodies K and K,′ K will be said to be finite if it "contains nothing of the unboundable of space".[74] In this case K′ is said to be "fully infinite". On the other hand, if neither K nor K′are finite they both contain something of the unboundable of space, and are said to be "partially infinite". There is only one way in which I can make sense of these baffling definitions, and that is by interpreting 1,22 to mean that space is non-compact, in other words, that not every collection of bodies comprising all of space includes a finite subcollection that also comprises it. Without doing much violence to the foregoing definitions one can then reformulate them as follows: K is finite if it is contained in a compact region of space; otherwise K is infinite ("fully" or "partly", as before). The assumption that space is non-compact is evidently not so indifferent as the assumption that

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74 "In einem Körper K kann [. . .] vom Unbegrenzbaren des R[aumes] nichts enthalten sein; dann heisst K endlich." (Dingler, AEF, p.161.)

it has no boundary, and one is entitled to ask what justifies it. In the methodological comments that follow upon the section on topology Dingler observes that his assumptions in that section are only intended to "*select and fix* among the unencompassable multitude of possibilities that are hidden in the concepts of everyday speech, those that are appropriate."[75] He claims moreover that the choice made in each case is the only one that could be unambiguously determined at its respective stage. With the means available when the choice was exercised no other stipulation could have been formulated. One may indeed wonder whether this can be said of 1,22, when interpreted in the manner proposed by me. For evidently the same means that enable one to postulate that space is non-compact can be used for pronouncing it compact, and I cannot see that the former stipulation is more definite, or simpler, or in any sense more appropriate than the latter. These reflections will appear to discredit my interpretation of 1,22. On the other hand, only on that interpretation does Dingler's theory preclude space from having the topology of projective space, a result that is taken for granted in the sequel. Of course, one may add, any postulate implying this result would seem no less arbitrary than the assumption that space is non-compact, within a theory whose axioms purportedly lack "ontological power", and which, as Dingler puts it, "do not say something about the constitution of reality, but only choose some among its countless possibilities, in order to determine certain concepts unambiguously, step by step, in every respect."[76]

The theory of Q-space is followed by the all-important definition of the plane. This is very much the same in either book. In AEF it reads as follows:

> **4,5. [. . .] A running surface whose two sides are indistinguished (1) in their entirety and (2) with regard to each of their points, is called a *plane*.**

Dingler claims to show from his topological assumptions, that every plane is simply connected (4,54) and divides space into two connected (4,53) "partially infinite" regions (4,55). Also that two different planes cannot share a piece of surface (4,6) and *hence*, if they meet at all, they meet on a separation line (4,62). Such a line is called a *straight* (4,63). Dingler maintains that no further assump-

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[75] Dingler, AEF, p.173.
[76] Dingler, AEF, p.175.

tions are necessary in order to prove Hilbert's axioms of incidence and betweenness. However, most of his proofs are objectionable.[77] We must fix our attention on Definition 4,5 itself, which is clarified by Dingler's own paraphrase in EW: a plane is a surface whose two sides can be wholly superposed at all places, and are thus spatially indistinguishable.[78] Dingler is quick to point out that, in accordance with the Plan, only the relation of difference has any bearing on this specification of the general idea of a boundary. We note however that not just any difference—or lack of difference—is relevant here, not, say, difference in colour or electric charge, but specifically *spatial* difference. Again, not every difference that we would ordinarily term spatial can be taken into consideration; for many such differences—e.g. differences in size or distance—have not yet been defined and will be defined later with the aid of the concept of a plane. Strictly speaking only topological differences stand at our disposal. But then, of course, if only such differences are considered, every simply connected open separation surface in a topologically Euclidean space satisfies Dingler's definition of a plane.[79] That this was not what he had in mind, we gather at once from what he says about the practical procedure for the manufacture of plane surfaces, which, he maintains, is justified by Definition 4,5. This is none other than the three-plate method we described on p. 87. Now, it is essential for this method that the three plates be stiff. But how are we to define stiffness? Indeed we would expect to judge of this property—by virtue of which an object resists *bending*, not stretching—by standards based on our idea of a plane.

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[77] Allow me to paraphrase the "proof" of 4,54: A plane is a simply connected running surface. If the plane E is not simply connected and P is a point on E there is a neighbourhood S of P on E which can be embedded in a simply connected running surface. A simple running line L_1 can therefore be drawn which cuts S into two parts and hence *also cuts* E *into two parts.* On the other hand (by the definition of multiple connectedness), there must be a simple running line L_2 on E which does not cut E in two. But then the two regions of E can be distinguished by means of these two lines L_1 and L_2, and E does not satisfy the definition of a plane (4,5). The concluding sentence is hard to understand: What two regions of E are being spoken of? The two regions into which it is allegedly cut by L_1? But no mention is made in 4,5 of such regions *on* a plane, but only of the regions *outside* it into which the plane divides space. However we need not probe further into this matter, for the italicized clause "*also cuts* E *into two parts*" is plainly a *non sequitur.*

[78] "Eine [. . .] Fläche, bei der ihre beiden Flächenseiten im ganzen und an allen Stellen aufeinanderlegbar, d.h. räumlich ununterscheidbar sind." Dingler, EW^2, p. 133.

[79] The two regions into which such a surface divides space are homeomorphic and hence topologically indistinguishable.

A few simple group-theoretical considerations can help us understand what is wrong with Dingler's definition of the plane. Let R designate Q-space, with its natural topology. Let G be a group acting transitively and effectively on R. Let S be a simply connected surface which divides R into two connected regions R_1 and R_2 (in other words, R—S is a topological space consisting of two components). Let $H \subset G$ be the stability group of S. Dingler's definition of the plane is then equivalent to the following: S is a plane if and only if (i) for each $h \in H$, either $h(R_1) = R_1$ or $h(R_1) = R_2$; (ii) for each pair of points x, y in S, there is an $h \in H$ such that $h(x) = y$. Every such group G determines a set of spatial properties and relations, the so-called G-invariants.[80] Obviously, if S is a plane by the definition just given and if $h \in H$, a set of points $X \subset S$ cannot be distinguished from $h(X)$ with regard to the G-invariants; and the same can be said of R_1 and $h(R_1)$. Consequently, if we take spatial predicates to mean G-invariants and superposition of S on itself to mean any mapping $x \longmapsto h(x)$ where h belongs to $H \subset G$, the stability group of S, we may indeed say with Dingler that the two sides of S can be wholly superposed at all places and are indistinguishable with respect to spatial predicates. But no indication concerning the nature of such predicates can be gathered from the foregoing definition of the plane, for it depends essentially on the as yet undetermined group G and its action on space. We cannot expect, therefore, that our definition or Dingler's definition 4,5 can of themselves contribute to specify geometry and geometrical predicates beyond the topological level. Dingler grants as much when he observes, at the end of the section on planes and straights, that "the concept of indistinguishability, on which everything rests, has not yet been defined in any way."[81] We can derive but little confort from this remark, for Dingler does not define this concept in the sequel. In fact, he resorts again to it, as it stands, in his definitions of paralellism, orthogonality and congruence.

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80 A group G is said to act on a space R if there is a mapping F: G x R⟶R such that for any $g,h \in G$ and any $x \in R$, if gh designates the product of g and h in G, $F(g,F(h,x) = F(gh,x)$. For brevity, we write gx instead of $F(g,x)$. G acts transitively on R if for every $x,y \in R$ there is a $g \in G$ such that $gx=y$. G acts effectively on R if $gx=x$ for every $x \in R$ only if g is the neutral element of G. The stability group of a part M of R is the subgroup of G whose underlying set is $\{ g | g \in G$ and for each $x \in M$, $gx \in M \}$. An n-ary relation P on R is invariant under G or G-invariant if for every $g \in G$ and $x_1, \ldots, x_n \in R$, $P(x_1, \ldots, x_n)$ implies $P(gx_1, \ldots, gx_n)$.

81 Dingler, AEF, p.183. Contrast with this his earlier reliance on our everyday perception of difference (Dingler (1925), p.323, quoted below in note 95.)

To these we must now turn. GG defines parallel line as follows: let *g* be a straight on plane E, and let P be a point on E outside *g*; if a line *g´* on E passes through P and the strip limited by *g* and *g* shows no difference "on either extreme to the left and to the right of P", *g* and *g´* are said to be parallel. The Dingler parallel to *g* through P is obviously unique. If we grant that two straights do not meet at more than one point, it is clear that two Dingler parallels cannot meet at all (their intersection would occur on one side of P). But there is no apparent reason why any two coplanar lines must intersect if they are not Dingler-parallel. Dingler, of course, is well aware of it. Boylai-Lobachevsky geometry—in which through any point outside a given straight line *m* we shall find a single Dingler parallel to *m*, but also two Bolyai-Lobachevsky and infinitely many Euclid parallels to *m*—is excluded only by Dingler's definition of the deformation-free body.[82] This can be rendered as follows: We shall say that a body K glides along a straight line or along a plane if it moves in such manner that every point of K that lies on the said line (plane) at any time continues to lie on it throughout the movement; we shall likewise say that K rotates about a line *g* that passes through it if it moves in such manner that any point of K that lies on *g* remains on the same location throughout the movement; finally, we shall say that two straight lines *g* and *h* meet orthogonally if the figures formed on either side of *g* are indistinguishable. A body K is said to be *deformation-free* if and only if (I) whenever K glides along any straight line *g* through K and any plane E through *g*, (a) K also glides along every other plane through *g* and along every parallel to *g* that passes through K and belongs to any of those planes; (b) if H is the set of points of K which lie at some time during the motion on a given straight line *h* through *g*, then, as K glides along *g* and E, H lies at each moment on a parallel to *h*; (II) whenever K rotates about a straight line *g* through it (a) K eventually returns to its initial position; (b) if G is at same time during this motion the intersection of K with a half-plane F bounded by *g*, G comes to lie once on each half-plane bounded by *g* before returning to F; (c) if P is a point of K outside *g* which lies at same time during the motion on a line *h* that meets *g* orthogonally at Q, then, if P_1 and P_2 denote any two locations of P during the motion, the segments QP_1 and QP_2 are

[82] Let *g* be a straight line, P a point outside it, *g´* a line through P on the plane determined by P and *g*. *g´* is parallel to *g* according to Euclid's definition if and only if *g* and *g´* do not meet; it is parallel to *g* according to Bolyai's and Lobachevky's definition if and only if every line between it and the perpendicular from P to *g* meets *g*.

indistinguishable except by their position. Two segments AB and CD are said to be *congruent* if a given pair of points on a deformation-free body can be carried into coincidence first with AB and then with CD.[83]

Dingler's definition of a deformation-free body is plainly an attempt to characterize Euclidean translations and rotations. We shall allow it to be satisfactory and shall not discuss the necessity (or redundancy) of its requirements. We would rather wish to learn what justified Dingler's belief that such a complex and restrictive set of stipulations was uniquely determined and preferable to every conceivable alternative. From Helmholtz he acquired the persuasion that only deformation-free motions can induce a physically meaningful distance function on Q-space. This restricts the choice to the four classical geometries of constant curvature: Elliptic, Spheric, Hyperbolic and Euclidean. The first two are excluded if Q-space is non-compact. (Hence the need for our proposed interpretation of 1,22). But how does Dingler justify his preference for Euclidean and against Hyperbolic or Bolyai-Lobachevsky geometry? He often notes that the latter is essentially ambiguous, for it can only be determined up to a constant (we may take this to be the Schweikart constant, i.e. the distance between the vertex of a right angle and a straight line parallel to both its sides; or the constant negative Riemannian space curvature, etc.)[84] Euclidean geometry is not affected by a similar indeterminacy but, as Dingler had learnt from Wellstein, in the familiar axiomatic formulation it leaves room for a wide variety of interpretations. It was his declared aim in GG to overcome this difficulty by his novel, allegedly nonaxiomatic approach. He may claim success if, but only if, his definition of the deformation free body is truly unambiguous. This, as I shall now show, is far from being the case. Let R designate Dingler's Q-space with its natural topology and let G be the (abstract) group of Euclidean motions. If Dingler's definition of the deformation-free body is correct there must be a mapping Φ: G x R$\rightarrow$R through which G acts transitively and effectively on R, such that, if B is any deformation-free body and H and H′ are two locations filled by B at two moments of its history, there is a $g \in$ G such that $\Phi(g,\mathrm{H}) = \mathrm{H}'$. Let f: R$\longrightarrow$R be an arbitrary homeomorphism. Let Ψ : G x R$\longrightarrow$R be defined by $\Psi(g,x) = f \circ \Phi(g, f^{-1}(x))$. It can be easily shown that G acts transitively and

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[83] Dingler, GG, pp. 23-25.

[84] See note 47.

effectively on R through Ψ.[85] A body any two of whose locations K and K´ meet the condition

$$\Psi(h, K) = K' \text{ for some } h \in G$$

will then satisfy Dingler's definition of the deformation-free body. In particular, if f agrees with the identity on H, while f(H´) is a proper subset of H´, Dingler's definition can be simultaneously exemplified by the above mentioned body B that successively occupies H and H´ and by another body B´ that successively fills H and only part of H´. Of course the planes and straights and parallels mentioned in the definition of the deformation-free body will not be the same in either case. This is only natural since we have shown that Dingler's characterization of these geometrical entities depends on the choice of a group and its action on Q-space. The group is here in both cases the same, namely, the Euclidean group of motions, but its action is different. Since there are no a priori grounds for preferring Ψ to Φ or Φ to Ψ, there is no reason for holding that B is more properly a deformation-free body than B´, or vice versa.

In AEF the treatment of parallellism and congruence is rather different. Parallellism is defined for planes. Let E_1 be a plane that divides space into two regions R_1' and choose a point P in R_1. Let E_2. be a plane through P that lies entirely in R_1 and divides space into two parts R_2 and R_2'. E_1 must them lie entirely in one of these parts, say in R_2. The intersection of R_1 and R_2 is a body—a slab—limited by E_1 and E_2. If all the directions in which this body is unlimited are indistinguishable, E_1 and E_2 are said to be *parallel.* Two straight lines, g_1 and g_2 are parallel if they are the intersections of two parallel planes with a third plane. It is clear that parallel planes—in the foregoing sense—do not meet. Dingler *postulates* that any two planes meet unless they are parallel (7,2). He claims to derive the usual parallel postulate from this one (8,4;8,5). Let AB and CD be two segments, lying on two parallel lines. If AC and BD, or if AD and BC also lie on parallel lines, AB and CD are said to be *congruent* (8,6). Now, if AB and CD are two arbitrary segments there always exists, on Dingler's assumptions, a segment CB´ which is congruent to AB by the foregoing definition.[86] Congruence, or lack of it, between

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85 We must show that for any g and h in G and any x in R, $\Psi(gh,x) = \Psi(g,\Psi(h,x))$. Now $\Psi(g,\Psi(h,x)) = f \circ \Phi(g, f^{-1} \circ f \circ \Phi(h,f^{-1}(x))) = f \circ \Phi(g,\Phi(h,f^{-1}(x))) = f \circ \Phi(gh,f^{-1}(x)) = \Psi(gh,x)$.

86 Draw the parallel to AB through C. Draw the parallel to AC through B. Let B´denote their intersection. (The latter must exist, for both lines lie on plane ABC and they are parallel, respectively, to two lines through A).

two arbitrary segments will therefore be defined if we can provide a criterion of congruence for segments with a common endpoint. The criterion supplied by Dingler is disappointing: two segments PQ and PR, with a common endpoint P, are said to be congruent if they are indistinguishable! (9,1).[87]

It must be clear by now that Dingler's construction of geometry as a branch of exact fundamental science does not fulfil the high hopes fostered by his ambitious programme. Paul Lorenzen has sought to vindicate Dingler's approach to the subject in a very illuminating article on "the problem of the foundation of geometry as a science of spatial order", apparently written while he was working on the posthumous edition of AEF.[88] We owe to Lorenzen an exact definition of the key concept of indistinguishability: Two objects are indistinguishable if every sentence that says something about one implies the sentence which says the same about the other. Since implication is exactly defined only for sentences belonging to formalized or canonic languages, Lorenzen's concept of indistinguishability is relative to the choice of such a language. In his discussion of geometry, he considers a first-order language with three sorts of individual constants and variables (standing for points, straights and planes, respectively) and four primitive predicates: "lies on" (a binary relation between a point and a straight or a point and a plane, or between a straight and a plane), "lies between" (a ternary relation between a plane and two points), "is parallel to" (a binary relation between two planes) and "is perpendicular to" (a binary relation between a line and a plane). We need not worry here about the limitations of first-order language as a vehicle for geometry.[89] Let x and y be two individual constants of Lorenzen's language. We may say that the objects denoted by x and y are geometrically indistinguishable in Lorenzen's sense if every sentence S of the language, in which y does not occur, implies the sentence S´ obtained by substituting y for x in every ocurrence of the latter in S. Guided by this general criterion Lorenzen achieves a rigorous formulation of Dingler's definition of the plane. Let E denote a plane, while P and Q denote points. Let S(E,P) be any sentence in which the only individual constants are E and P and let S(E,Q/P) be the sentence obtained by substituting Q for P in every ocurrence of the latter in

[87] However, it is instructive to read Dingler's definition in the light of Nevanlinna's treatment of congruence in Nevanlinna and Kustaanheimo, GG, part I.

[88] Lorenzen (1961). Lorenzen's edition of Dingler's AEF appeared in 1964.

[89] See Tarski, "What is elementary geometry?"

S(E,P). Planes are then characterized by the following two "principles of homogeneity":

1. *Inner homogeneity.* If P and Q lie on E, S(E,P) implies S(E,Q/P).
2. *Outer homogeneity.* If neither P nor Q lie on E, S(E,P) implies S(E,Q/P).[90]

Evidently the two principles will suitably restrict the extension of the concept of a plane only if the predicates of the language are specified further. As usual, incidence and betweenness are characterized by axioms. Lorenzen does not list them but quotes the following two examples:

If E lies between P and Q, then, for any point X not on E, E lies between X and P or E lies between X and Q.

If E lies between P and Q and both P and Q lie on the straight g there is a point X at which g meets E.[91]

Parallellism and orthogonality are characterized by axiom schemata:

I. If point P lies on straight *g* and point Q lies on straight *g´* and *g* and *g´* lie on plane E and E is parallel to plane F, S(E,F,P, *g)* implies S(E,F,Q/P, *g´/g).*

II. If point P lies on the straights *g*, *h* and *k*, and *g* is perpendicular to plane E and *h* and *k* lie on E, S(E, *g*, *h*) implies S(E, *g*,*k/h).*[92]

Lorenzen adds the following existence postulate: If E is any plane and P is any point there is a unique plane X through P which is parallel to E and a unique straight through P which is perpendicular to E. In Lorenzen's geometrical language congruence between segments or point-pairs can be easily defined. We say that two point-pairs (A,B), (C,D) are parallel, if there is a plane E on which points A,B,C, and D lie and there are two parallel planes F_1 and F_2, distinct from E, such that A and B lie on F_1 and C and D lie on F_2. Two point-pairs (A,B), (C,D) are said to be orthogonal if (A,B) lies on a straight perpendicular to some plane through (C,D). Four distinct points A,B,C and D, such that (A,B) is parallel to (C,D), are said to form a *parallellogram* ABCD if (A,C) is parallel to (BD) or if (A,D) is parallel to (B,C). If say, (A,C) and (B,D) are parallel, (A,D) and (B,C) are called the diagonals of ABCD. We can now define:

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90 Lorenzen (1961), p.134 gives only the first of these two principles. The second one is added in the more popular exposition in Lorenzen and Schwemmer, KLEW, p.227.

91 Lorenzen (1961), p.136. "P ∈ E" in the first axiom is plainly a misprint for "P ∉ E".

92 Lorenzen (1961), pp. 136, 135.

Two point-pairs (A,B) and (C,D) are congruent if and only if one of the following conditions is fulfilled:

(i) (A,B) = (C,D): (ii) a point of (A,B), say A, is identical with a point of (C,D), say C, and there exists a point X such that ABXD is a parallellogram with orthogonal diagonals (A,X) and (B,D); (iii) ABCD is a parallellogram and (A,B) is parallel to (C,D); (iv) there exists a point-pair (X,Y) which is congruent with both (A,B) and (C,D).

Lorenzen's postulates imply that any perpendicular to a plane E is also perpendicular to every plane parallel to E. For let F be such a plane and h the perpendicular to E through P ϵ E. h meets F on Q. If $h \neq k$, h and k determine a plane G which intersects E along the straight g. g is thereby distinguished among the lines through P on E. This, however, is incompatible with axiom schema I.[93] It follows immediately that any two points A and B on a plane E and the points C and D (where the perpendicular to E through A and B meets a plane F parallel to E) determine a rectangle. Max Dehn (1900) showed that the existence of rectangles in a space will not suffice to mark it out as Euclidean unless it is asserted jointly with the Archimedean axiom.[93b] Lorenzen obligingly adds this axiom to his assumptions. Lorenzen's work certainly throws a new light on the fundamental concepts of geometry, placing Dingler's true merits in a proper perspective, and may even procure a fairer hearing to such discredited items of mathematical tradition as Euclid's definition of the straight line as a line which lies equally (ἐξ ἴσου κεῖται) with regard to its points. But it does not vindicate Dingler's attempt to do away with geometric axiom systems. Lorenzen's system may claim greater perspicuity than Hilbert's. It would certainly be no mean achievement to have rid geometry of the concept of congruence.

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93 This can be shown more precisely as follows. Let x and y be two individual constants or variables standing for points, straight lines or planes. Let $x \sqcap y$ denote the intersection or 'meet' of x and y. Let $x \sqcup y$ denote their 'join', by which we mean (i) the plane determined by them, if x and y are two straights or a straight and a point outside it, or (ii) the straight determined by them if x and y are two points, or (iii) x itself, if $y=x$ or if y lies on x, or (iv) y itself, if x lies on y. We now consider an instance of Axiom Schema I, in which the antecedent reads: "If P lies on g and P lies on g' and g and g' lie on E and E is parallel to F". The sentence S(E,F,P,g) is: "For every straight h and every straight k, if h is perpendicular to E and k is perpendicular to F and $h \sqcap$ P=P and $h \sqcap$ F=$k \sqcap$ F then $((h \sqcup k) \sqcap E) \sqcup g=g$". Under the aforesaid condition this sentence will universally imply the sentence obtained from it by substituting g' for g, only if for every h and k meeting the stated requirements, $(h \sqcup k) \sqcap$E=P, that is to say, only if $h=k$. This necessary condition can only be met if E and F have a common perpendicular through P. Since P is arbitrary, two parallel planes have a common perpendicular through each point on either.

93b The Archimedean Axiom says that given two segments r and s, with r less

But Lorenzen's system is an axiom system all the same, no less ambiguous than the rest, fraught with all the equivocations attendant on a first order theory with an infinite model.

Two more attempts at vindicating a Dinglerian view of geometry may be mentioned here. Bopp (1969) allows his case for the uniqueness of Euclidean geometry to rest ultimately on experience, thus entirely missing Dingler's main philosophical point, namely, that geometry, the fundamental theory of measurement, is presupposed by every scientific observation. Janich (1976) tries to show that an "operationally grounded" practical geometry must necessarily be Euclidean. Some essential steps, such as the constructive formulation of the Archimedean axion, are—he admits—still pending. I am afraid, moreover, that even within the ground actually covered by Janich he overlooks some possibilities. Janich operationally defines *corners*, i.e. solid bodies with three polished flat faces meeting orthogonally at a vertex. Let ABC designate a corner with polished faces A,B and C. Let face A rest on a sufficiently large flat surface E. We press a corner FGH against face B, so that face H lies opposite E, leaving an empty space between them. Now apply one face, say P, of a small enough corner PQR, against H, while faces Q and R fall towards E. If the geometry were Euclidean it should then be possible to place still another corner with one face pressed against Q or R, while another fits snugly upon E. But Janich's operational definitions cannot provide any assurance that this must be so. Since the standards set for corners have nothing to do with the foregoing configuration it is just a contingent, empirical fact that such corners as might be produced in a carpenter shop do fit into the configuration rather well.

5. Concluding Remarks

Dingler's foundation of geometry amounts in the end, as we have seen, to just another, not remarkably neat and perhaps even insufficient axiomatization of Euclid's theory. Like all such axiomatizations it admits infinitely many mutually incompatible interpretations in a suitable domain. Given one such interpretation I, with underlying topological space R, the rest can be derived from it through the automorphisms of R in the manner sketched in our discussion of the definition of the rigid body in Dingler's GG (pp. 115f.). It is a familiar fact that among the many possible physical interpretations of Euclidean geometry there is one according to

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than *s*, there exists a positive integer *n* such that *s* is less than *n* times *r*.

which a human body, or at any rate a human bone, behaves approximately like a Euclidean rigid body. We are encouraged to give special attention to this interpretation by Dingler's remark that any "axiom system of geometry designed to yield an unambiguous physical geometry *(eine eindeutige Geometrie in der Realität)* should not only display relations of the fundamental concepts to one another, but must also contain their *relations* to us."[94] Indeed, if there were only one such interpretation it would in all likelihood be singled out unambiguously by the exercise of our ordinary powers of discrimination, on which Dingler once relied for fixing the meaning of "indistinguishable" as used in his axiom system.[95] But of course the said interpretation is not unique. Consider a physical interpretation I of Euclidean geometry. Choose a unit of length in I. Let η be a positive rational number. We say that a body B is rigid in I within η if any segment of unit length marked in B continues to measure $1 \pm \eta$ units while B remains under certain standard physical conditions (of temperature, stress, etc.) no matter how it is moved about in space.[96] Obviously there are infinitely many different interpretations of Euclidean geometry that agree with I in the choice of the unit of length, in all of which B is rigid within η. This fact forces us to take a new look at Dingler's theory of measurement. He held, as we know, that the manufacture and improvement of precision instruments is guided and controlled by Euclidean geometry.[97] In a sense, he is right. But we cannot understand this to mean, as he believed, that there is a unique idea of the deformation-free body being progressively realized, with increasing approximation, in the factories of precision instruments. On the contrary, the preceding remarks show that at each stage in the history of metrology there is a whole family of interpretations of Euclidean geometry that are embodied, within the then

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94 Dingler (1925), pp.322 f. My italics.

95 "This axiom system yields a geometry which naturally depends on what one means by "different", but which is completely unambiguous if we understand this word in the sense determined by our immediately given knowledge of the relation thus called (the very considerable agreement of different men on this matter is a physiological *hic et nunc* fact which opens the possibility of defining the concept of human "normality" with regard to it)." Dingler (1925), p.323.

96 For our discussion we need only this abstract, purposely impractical definition of rigidity. We need not analyze the exact operational and statistical meaning of the statement that such and such a segment, marked on a body, measures 1 or $1 \pm \eta$ units of length.

97 Dingler (1920a). See above, p.10.

accepted approximation, in the standards and methods of measurement. Every technical advance, allowing a decrease in the fraction within which meter sticks can be required to be rigid, or prescribing a change in the material from which meter sticks are made, or in the methods by which readings are taken on them, involves a modification of that family. Historically, it would seem, such changes have always involved a reduction of the previously accepted family, not its replacement by another one not contained in it. But metrologically acceptable interpretations of Euclidean geometry cannot be said to converge towards one of them according to a priori laws. Improvements in measurement inevitably involve some tacit, usually unconscious choice. The considerations by which such choices are guided deserve some attention. At any given time measurements performed with the best means available agree with one another within some specifiable range. This sets an infimum to the rational numbers within which standards of length can be required to be rigid in some interpretation of Euclidean geometry. Any interpretation in which the standards of length are rigid within that infimum is then metrologically acceptable. At some moment in history wooden yardsticks were substituted for the anatomic standards whose memory still lingers in the names of some units of length (e.g. *hand, foot, cubit;* the French *pouce*, etc.) The main reason for this change was not that human limbs were insufficiently rigid within the then attainable precision, but rather that they were not uniform enough to yield consistent measurements even with that low precision. English metrology was tied to the King's foot, but its length had to be preserved and reproduced in some hard, durable, transportable stuff in which sections of equal length could be easily marked. Wooden yardsticks were later replaced by metallic meter rods mainly for the sake of increased exactness. Let η designate the rational number within which the best wooden standards could be required to be rigid. Let $F(\eta)$ be the family of interpretations of Euclidean geometry in which wooden standards were rigid within η under specific "normal" conditions (u.n.c.). More exact measurements could only be achieved by means of standards of length rigid within a number $\delta < \eta$. For sufficiently small δ not even the best wooden standards will all be rigid within δ, u.n.c., in an interpretation of Euclidean geometry. There was thus a limit beyond which no improvements in precision could be achieved except by changing the method of measurement. In the case in point the change was fairly easy: it was a matter of making the standards in another material. There obviously exists, for some number δ, considerably smaller than our hypothetical η, a family $F(\delta) \subset F(\eta)$, such that, say, steel rods, under appropriate conditions, are rigid within δ in any member of $F(\delta)$. $F(\delta)$ is certainly a proper subset of $F(\eta)$. A further increase in precision coupled with a further reduction in the family of metrologically admissible interpretations can be attained by replacing steel by the platinum-iridium alloy used in the meter rod at Sèvres. The important thing is that each such reduction—say from $F(\eta)$ to a specific $F(\delta) \subset F(\eta)$—is determined by the choice of the material out of which the standards are made, which is bound to depend in its turn on the materials actually available. Generally speaking any advance in metrology involves a transition from one range of error η to a lesser range of error δ. This brings about a reduction of the family of admissible physical interpretations of geometry. In our notation, one passes from a family $F(\eta)$ to another family $F(\delta) \subset F(\eta)$. The choice of $F(\delta)$ within $F(\eta)$ is determined by the nature of the new metrical procedures which are responsible for the advance. We see at once that, since it is assumed that $F(\delta)$ must be a subset of $F(\eta)$, the previous history of metrology conditions at any of its stages the step that will be taken next. This step, however, is not arbitrary within the range allowed by that history, for it is severely limited by what is materially and technically possible. Analogous considerations apply to enlargements of the spatial scope of exact distance measurements. Suppose that the best meter rods are required to be rigid within η, in a family of interpretations $F(\eta)$. The elements of $F(\eta)$ are presumably indistinguishable in everyday life or in the laboratory, where distances are measured directly by rods. However, discernible differences must inevitably arise on the astronomical or even on the geographical scale. Obviously not every interpretation of Euclidean geometry in which the best rods are rigid, under standard conditions, within some small real number η, is compatible with the classical assumption that starlight travels along straight lines outside the atmosphere of the Earth. Let $H(\eta)$ be the subset of $F(\eta)$ which is compatible with this assumption. $H(\eta)$ is preferred to the remaining interpretations, not because of any specifically geometrical reason, but because light rays are, as far as one can see, the only available means for keeping track of the geometry in outer space. This last fact enables us to guess which would have been the reaction of the scientific community had it been verified that astronomical triangulations resting on the assumption just mentioned consistently yielded measurements deviating from Euclidean predictions by more than the admissible error. Far from refurbishing optics in order to preserve their Euclid, scientists would in all likelihood have gone through the trouble of learning non-Euclidean geometry and would have proclaimed its validity in the physical world.[98] According to Dingler such behaviour would have constituted a violation of the principle of pragmatic order, for a geometry may not base its claims on measurements performed with instruments designed according to another geometry. However, what really matters here is not the geometry by which the instruments in question have been built but the geometries that they actually satisfy (with the attainable exactness). And there can hardly be any doubt that if, say, Lobachevsky had verified that astronomical data sustained the claims of a Bolyai-Lobachevsky geometry G(k), with characteristic parameter k, meter rods would have been found to be rigid in G(k) within the same number within which they were required to be rigid in Euclidean geometry.[99]

Though I cannot countenance Dingler's thesis that physical geometry is determined uniquely by the will to do science, and is Euclidean, I am persuaded that his philosophical study of geometry throws an important light on the physically more significant features of Euclidean geometry and helps explain its success in providing a starting-point for the rational elaboration of experience. I cannot bestow such praise on his attempted foundation of Newtonian mechanics as the fourth branch of exact fundamental science. While his theory of geometry depends heavily on Helmholtz's and owes much to the work of Klein, Poincaré and Hilbert, his philosophy of mechanics stands all by itself as an idiosyncratic and seemingly far-fetched creation.[100] We cannot deal with it here, but a few brief indications may give the reader an inkling of it. Its main tenet is that the phenomena of change can be given a final, intelectually satisfying scientific explanation only by analyzing them into interactions of the simplest conceivable kind. Now the simplest conceivable interaction can be unambiguously defined, according to Dingler, as that between two mass-points acting on each other according to Newton's Law of Gravitation. That the interacting points or "infinitesimal balls"[101] must have a mass is proved as follows:

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98 My guess is based on what has actually happened in the 20th century. However, in order to keep the discussion, as far as possible, in Dingler's own terms, I refrain from introducing considerations involving the relativistic notion of a space-time metric. I therefore speak of physical geometry as if it could be kept neatly separate from chronometry. This simplifies matters and does not weaken my methodological argument.

99 Lobachevsky's astronomical calculations are given in Lobachevsky, ZGA, p.24. See also Gauss' letter to Gerling of March 16th, 1819 (Gauss, WW, vol.8, p.182).

100 It does not lack followers, however. See Thüring, GPGP.

101 *Differentialkugel* (Dingler, EW2, p.147).

A purely geometrical figure can neither exert nor receive an action because it has a purely ideal nature. In order that [the infinitesimal ball] K may receive an action an additional property should be ascribed to it (a property not expressed in its geometrical form). We denote this property of K by m.[102]

Dingler seeks to show that Newton's Axioms of Motion also pertain necessarily to the simplest schema of interaction. Since the same characteristic constant occurs in the Second and Third Axiom and in the Law of Gravitation, Dingler thinks that the identity of gravitational and inertial mass is automatically secured.[103] Since the Law of Gravitation is a principle of exact fundamental science, no observation or experiment can be held against it. Indeed in our vast, complex and mostly unknown world, there is no dearth of occult causes for explaining away any apparent deviation of nature from Newtonian orthodoxy. Thus, Dingler praises H. von Seeliger for explaining the precession of Mercury's perihelion as caused by interplanetary dust.[104]

From Dingler's standpoint, Einstein's Theory of Relativity was totally unacceptable. Physicists may indeed propose imaginative hypotheses for the temporary, heuristically profitable yet intellectually unsatisfactory explanation of phenomena that resist analysis in terms of Newton's laws. But such hypotheses should by no means contradict the principles of exact fundamental science or inextricable chaos will ensue. Dingler acknowledges the mathematical beauty of Einstein's creation but deplores the philosophical confusion from which it stems. He criticized the Theory of Relativity in the fourth part of *Physik und Hypothese* (PH, 1921) and in several papers written in the early twenties.[105] In contrast with many early

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102 Dingler, EW², p.148. For alleged proofs that the law of interaction must be formally identical with Newton's law of gravitation, see Dingler, GP², p.112; E, pp. 116 ff; MP, pp. 136 ff.; AEF, pp. 212 ff. A good analysis of the presuppositions of the proof in E will be found in Aster and Vogel (1931), pp. 10 ff.

103 Dingler, GP², pp.240 ff.

104 Dingler (1923), p.49. He refers to Seeliger, "Das Zodiakallicht und die empirischen Glieder in der Bewegung der inneren Planeten", in *Sitzungsber. der bay.Akad.*, 36 (1906). In this work the origin of zodiacal light is ascribed to an ellipsoidal mass of cosmic dust surrounding the sun, and also causing the precession of Mercury's perihelion.

105 Dingler, PH, pp.150-188, RO, (1920d), (1923), E, pp.135-145 (on the possibility of non-Newtonian mechanics) and pp.154 f. (on the definition of simultaneity, criticized by Vogel in Aster and Vogel (1931), pp.18-20), MP, pp.165f., 186-191, 262, 390-394. Dingler's criticism of relativity is neatly summarized in Willer, RE, pp.112-129. Reichenbach (1921) is a very apt reply to Dingler (1920d).

adversaries of Einstein, he never questions the consistency of the theory. His main objection is that such logically consistent mathematical structures can be had by the dozen, and that to fit into them any collection of data within the admissible range of experimental error is merely a matter of ingenuity, of rightly choosing the relevant parameters and boundary conditions.[106] By losening its ties with the uniquely privileged, unambiguously definable theories of Euclid and Newton, science has been left adrift. Dingler's detailed criticisms of Einstein's theory are few in number and do not bespeak a very deep acquaintance with it. He never comes to grips with the fundamental conception of a spacetime metric, for neglect of which he can arrive at the quaint conclusion that Special Relativity violates either the Principle of Sufficient Reason or the Principle of the Homogeneity of Space, when it predicts that two materially indistinguishable natural processes beginning simultaneously may not end at the same time.[107] Dingler's most striking objection is directed against Galileian, not Einsteinian relativity. The Galileian principle, which is usually regarded as an integral part of Newtonian physics, can be formulated saying that no mechanical experiments will ever enable us to decide whether our laboratory is at rest or moving with uniform rectilinear velocity. Dingler believes that a scientific and hence unambiguous description of phenomena requires that we distinguish neatly between motion and rest. This can be done by referring all observed positions to a single spatial coordinate system. Dingler believes such system can be determined uniquely and has been indeed so determined by astronomers, from the average positions of the stars. The Galileian principle is therefore irrelevant to Newtonian physics. Were it not so, the consequences would be disastrous to the latter. For mechanical experiments are useless not only for discerning uniform rectilinear motion from rest but also for distinguishing uniformly accelerated rectilinear motion from uniform rectilinear motion. It will be countered that this claim is plainly false since, for example, a man drinking beer on a smoothly flying aeroplane might for aught he knows be at rest yet will instantly notice the slightest jerk of the aeroplane. However, this familiar counterexample is not fair to Dingler's claim, for the comparison between rest and uniform

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[106] Compare Imre Lakatos' claim that "the most admired scientific theories simply fail to forbid any observable state of affairs" (Lakatos and Musgrave, CGK, p.100) and Popper's criticism in Schilpp, PKP, pp.1004-1009.

[107] Dingler (1920d), p.21; cf. pp. 18 ff. The prediction in question, illustrated by the Paradox of Twins, merely shows that natural clocks in a relativistic world do not measure *time* but *proper time*, i.e. the spacetime interval along their respective world-lines.

rectilinear motion and that between the latter and uniformly accelerated rectilinear motion are not based on strictly analogous assumptions. In the former case, the same constant velocity is initially imparted to all the elements of a system. Observation of the relative positions of those elements does not then disclose the velocity. In the latter case the same constant acceleration is not simultaneously imparted to every element of the system but only to some, viz. to the body of the aeroplane (which, for simplicity's sake, we may regard as perfectly rigid), but not to the drinker and the beer. No wonder then that observation of their relative positions reveals the acceleration. Suppose, however, that K is an inertial frame and K′ is a system of objects moving with the same uniform rectilinear acceleration a relative to K. It is impossible to tell from the sole observation of the motion whether the relation between K and K′ is that described above or whether K falls freely with acceleration -a in a homogeneous gravitational field in which K′ is at rest. Dingler's claim turns out to be merely a reformulation of Einstein's (weak) Principle of Equivalence. It is ironic that when levelling his gun against Galileian Relativity he should have thus unwittingly lighted on one of the chief buttresses of Einstein's General Theory.[108]

Though Dingler's conceptions, like so many other at first blush exciting philosophical projects, have not been and in all likelihood cannot be successfully worked out in detail, one cannot easily forget the rich suggestions of his original vision:

> Nature does not of itself provide any element or properties of exactly constant constitution (within the momentarily accepted range of imprecision), nor are there any means intrinsic to it for discerning them. That is why we procure ourselves in the absolutely unambiguous ideal concepts and their realizations a solid scaffolding in nature. Only that which belongs to this scaffolding or is tightly bound to it can be exactly reproduced; hence only in these elements can truly exact "natural laws" be found.[109]

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108 In fact, the gist of Dingler's remarks can be elicited from Corollary VI to Newton's Laws of Motion: "If bodies, moved in any manner among themselves, are urged in the direction of parallel lines by equal accelerative forces, they will all continue to move among themselves, after the same manner as if they had not been urged by those forces."

109 Dingler, EW2, pp. 162 f.

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