The Problem of the Invariance of Dimension in the Growth of Modern Topology, Part I

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Introduction

> Die hohe Bedeutung bestimmter Probleme für den Fortschritt der mathematischen Wissenschaft im Allgemeinen und die wichtige Rolle, die sie bei der Arbeit des einzelnen Forschers spielen, ist unleugbar. ... Wie überhaupt jedes menschliche Unternehmen Ziele verfolgt, so braucht die mathematische Forschung Probleme. Durch die Lösung von Problemen stählt sich die Kraft des Forschers; er findet neue Methoden und Ausblicke, er gewinnt einen weiteren und freieren Horizont.
> – HILBERT, 'Mathematische Probleme' *(1900: 253–254)*[1]

[1] References such as 'HILBERT ... *(1900: 253–254)*' denote items in the bibliography and page numbers or sections in these items. I have used this system of references throughout. The quotation from HILBERT may be translated as follows:

> The great significance of specific problems for the progress of mathematics in general and the important role which they play in the work of individual researchers is undeniable. ... Just as every human undertaking strives towards goals, so mathematical research needs problems. The power of the researcher is tempered through the solution of problems; by them he discovers new methods and points of view and opens up a wider and more expansive horizon.

Problems, above all, motivate the progress of mathematics and one of the most interesting groups of problems in mathematics surrounds the apparently simple idea of dimension. Indeed dimension is not so simple an idea after all. For in 1877 when GEORG CANTOR showed that the points of geometrical figures like squares, 'clearly 2-dimensional', can be put into one-one correspondence with the points of straight line segments, 'obviously 1-dimensional', he thereby rendered the 'simple' idea problematic. Difficulties such as this one which CANTOR uncovered demand explanations. In the past they have fed mathematicians with a wide variety of challenging research problems. The long term result has been the development of an entire branch of topology: dimension theory.

Topological dimension theory has a long and fascinating history and, in essence, three problems have been fundamental to its growth into a mature theory: (1) the problem of defining the concept of dimension itself and some closely related concepts, such as the concept of curve; (2) the problem of explaining the number of dimensions of physical space; and (3) the problem of proving the invariance of the dimension numbers of mathematical spaces under some restricted type of mapping. The first and third problems, mathematical in nature, have been the most important direct influence on the growth of the topological theory of dimension. The second, a problem of physics or cosmology, has provided an indirect but significant motivation for the development of the theory from outside the domain of mathematics. Let us look a little closer at these three problems which have formed the basic problem core of dimension theory.

The definition problem has ancient roots which can be detected in some brief passages in the writings of Greek philosophers and mathematicians. However, dimension theory is primarily a modern subject; its main historical roots start in the nineteenth century. At the very beginning of that century BERNARD BOLZANO examined several facets of the definition problem and proposed some interesting tentative solutions to it. I have dealt with BOLZANO's contributions elsewhere *(1977)*. After BOLZANO a few nineteenth-century mathematicians took up the definition problem, but they did not resolve it very satisfactorily. Interest in the problem arose through the creation of the mathematical theory of higher spaces and multi-dimensional geometries and the consequent methodological changes in the whole field of geometry. In the twentieth century HENRI POINCARÉ was the first to construct a definition of dimension, one based on 'cuts' (1903, 1912). It was soon criticised by L.E.J. BROUWER who replaced it by an improved version (1913). In the early 1920's PAUL URYSOHN and then KARL MENGER published independently of one another further definitions of dimension which turned out to be equivalent and which progressed a step beyond BROUWER's definition. These definitions of BROUWER, URYSOHN, and MENGER became the foundation for a wide-ranging theory of dimension which began a rapid development in the twenties.

The cosmological problem of explaining the dimension number of physical space, like the definition problem, arose in the speculations of the ancient Greeks, and over the centuries many thinkers have puzzled over it. In modern times IMMANUEL KANT considered it in his very first published work. For

POINCARÉ this problem was the chief motive for his proposing his cut definition of dimension. There can be little doubt that this problem has had a significant role in the development of the mathematical theory of dimension.

The origin of the invariance problem has a precise date, 1877, the year when GEORG CANTOR discovered – to his own astonishment! – that geometrical figures of different dimension numbers can have their points arranged in one-one correspondence. CANTOR's paradoxical result is a direct blow to our naive concept of dimension. When he and RICHARD DEDEKIND discussed it in an exchange of letters, they formulated the invariance problem for dimension, that is, the problem of showing that for certain continuous mappings dimension remains an invariant concept. Solution of the invariance problem quickly became regarded as the way to resolve CANTOR's strange result. As soon as CANTOR published his paradoxical correspondence in 1878 a number of mathematicians tried to demonstrate the invariance, but the problem proved to be refractory. GIUSEPPE PEANO dealt another blow to the naive concept of dimension in 1890 when he published the first example of a space-filling curve, a continuous mapping of a line segment onto an entire square. By the turn of the present century still no one had found a general proof of dimensional invariance. With hindsight we can see why the problem was so difficult to solve. At the time there were virtually no adequate tools for the task. Topology was a young subject. At last L.E.J. BROUWER achieved success by constructing a full and rigorous proof in 1910, which was published in 1911. Two years later he published another proof which he rested on his definition of dimension. Undoubtedly the key to BROUWER's success was the new and fruitful ideas that he himself introduced into topology.

The two problems of defining dimension and proving its invariance were the primary influences on the creation and growth of modern dimension theory. They are the central foci of its history. However, the problem of explaining the dimension number of space was also at times a motive from outside mathematics for the theory's development. Other problems as well had some influence on its development and, when the theory came to maturity in the 1920's, further problems became the centre of attention. Yet in the period up to about 1925 the twin problems of definition and invariance were the central motive forces with the cosmological problem as a pervading background force.

In the present historical work I shall be concentrating on the invariance problem, although at times the other two problems will of necessity come to the fore. The present work takes dimension theory from the time before CANTOR, when it consisted of a few loosely connected ideas and theories, to 1913, the year when BROUWER published his second invariance proof. Chapter 1, a prelude so to speak, is devoted to the pre-Cantorian ideas. Then chapters 2 through 7 take the history of the invariance problem from its conception to its solution by BROUWER. Chapter 8 ties up some loose ends, gives a glimpse of later developments, and then draws some conclusions about the history. The present part I consists of chapters 1–4; chapters 5–8 will constitute part II.

In this work I examine the mathematics of dimension in considerable detail. I think the topic deserves a full history. The mathematical ideas remain important to us and some of the greatest mathematicians of the latter half of the nineteenth and the first part of the twentieth centuries helped develop them. Moreover, the rise of dimension theory coincides with the early growth of topology. Indeed during the period under review the two subjects were in strong interaction. Dimension problems were partly responsible for the birth of modern topology. Some of the best-known early topologists were also concerned with the problems of dimension, for example, CANTOR, SCHOENFLIES, POINCARÉ, and BROUWER. So the history of dimension theory throws a great deal of light on the history of topology. Hence, it is worth while to look closely at the development of the former.

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Chapter 1. Ideas concerning Dimension before Cantor

My aim in this chapter is twofold. First I wish to describe some important Greek ideas about dimension. Without doubt these are significant when viewed in relation to the modern development of dimension theory. Second I want to examine the origins of theories of hyperspaces and higher-dimensional geometries. In these theories and the accompanying changes in the philosophy of geometry lies the most important background to CANTOR's discovery that figures of differing dimensions can be put into one-one correspondence.

We look to the Greeks for the principal ancient source of the science of mathematics. Modern practitioners of the science have a continuing debt to these ancient philosophers and mathematicians for its fundamentals. Hence, it may come as no surprise to discover that we can follow the roots of dimension theory back to them. To be sure, the Greeks did not formulate a detailed theory of dimension. Nevertheless, they thought and argued about the conceptual problems involved in dimension and so produced ideas and informal theories which have had a lasting influence on generations of mathematicians, including some in our own century. ARISTOTLE, EUCLID, and PTOLEMY all thought about dimension. But even before these men the oldest sect of mathematicians, the Pythagoreans, grappled with dimension-theoretic problems in the context of their cosmogony. Thus from the mists of early Pythagoreanism to twentieth-century topology there stretches a thread of geometrical ideas concerned with the concept of dimension. Let us begin by examining the Greek end of this thread.[1]

When trying to solve their fundamental cosmogonical problem of explaining the generation of physical things from numbers (*i.e.*, the positive integers) in accordance with their distinctive mathematical philosophy, the early Pythagoreans described how the basic geometrical figures, the point, line, surface, and solid, could be produced from numbers as an intermediate stage in the genetic process. It was natural for the Pythagoreans to touch upon the dimension concept at least incidentally, inasmuch as their problem and resulting theory involved a discussion of things having different dimensions. According to modern scholars[2] we must distinguish two versions of the intermediate stage, an older one and a more recent one which probably constituted an improvement on the first. Although the evidence for these pre-Socratic theories is fragmentary, the modern rational reconstructions of them do seem convincing.

Let us have a look at the primitive version of the intermediate stage. The most pertinent evidence comes in the following fragments. First there is an extensive passage on the Pythagorean ideas in *Theologumena Arithmeticae*, a passage which is ascribed to SPEUSIPPUS[3] (THOMAS *(1939: 80–83)*):

> For 1 is a point, 2 is a line, 3 is a triangle and 4 is a pyramid; all these are elements and principles of the figures like to them. In these numbers is seen the first of the progressions, that in which the terms exceed by an equal amount, and they have 10 for their sum. In surfaces and solids there are the elements – point, line, triangle, pyramid. ... The same result is seen in their generation. For the first principle of magnitude is point, the second is line, the third is surface, the fourth is solid.

Supplementing this text are two passages referring to the Pythagoreans from ARISTOTLE (*Metaphysics*, VII.2, 1028b16; XIV.3, 1090b5; quoted in GUTHRIE *(1962: 259)*):

> Some think that the limits of bodies, such as surface and line and point or unit, are substances, rather than body and the solid.

> There are some who, because the point is the limit and end of a line, the line of a surface and the surface of a solid, hold it to be inescapable that such natures exist.

The intermediate stage of the theory may be described more fully as follows.[4] There is a progression from point to line to surface to solid which is the pattern

[1] I do not claim to be an expert on Greek investigations concerning the dimension concept, but I hope that the following constitutes an improvement of my note 4 in *(1977: 266–267)*.

[2] I am relying particularly on KIRK/RAVEN *(1957: 253–256)* and GUTHRIE *(1962: 256–265)*.

[3] SPEUSIPPUS was the son of PLATO's sister and his immediate successor as head of the Academy. He was strongly influenced by Pythagorean philosophy.

[4] I am following KIRK/RAVEN *(1957: 254–256)* and GUTHRIE *(1962: 259–262)*. *Cf.* the discussion of SEXTUS EMPIRICUS *(1936: 346–347)*.

of generation. In particular, we have the special figures, the point, the line segment, the triangle, the tetrahedron, and these are associated with the numbers 1, 2, 3, 4, in arithmetic progression. In fact, the geometrical figures can be described by the minimum number of independent points required to contain them: one or the unit for the point itself, two for the line segment, three for the triangle, and four for the tetrahedron. According to the passages from ARISTOTLE the key concept is limit (or extremity, boundary).

The neo-Pythagorean author NICOMACHUS of Gerasa (ca. A.D. 100) provides an embroidered account of the old Pythagorean theory in his *Introduction to Arithmetic*. In this later account the dimension concept is explicitly named (*(1926)*, quoted in GUTHRIE *(1962: 261)*):

> Unity, then, occupying the place and character of a point, will be the beginning of intervals and of numbers, but not itself an interval or a number, just as the point is the beginning of a line, or an interval, but is not itself line or interval. Indeed, when a point is added to a point, it makes no increase, for when a non-dimensional thing is added to another non-dimensional thing, it will not thereby have dimension. ... Unity, therefore, is non-dimensional and elementary, and dimension first is found and seen in 2, then in 3, then in 4, and in succession in the following numbers; for 'dimension' is that which is conceived of as between two limits. The first dimension is called 'line', for a line is that which is extended in one direction. Two dimensions are called 'surface', for a surface is that which is extended in two directions. Three dimensions are called 'solid', for a solid is that which is extended in three directions.
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> ...
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> The point, then, is the beginning of dimension, but not itself a dimension, and likewise the beginning of a line, but not itself a line; the line is the beginning of surface, but not surface, and the beginning of the two-dimensional, but not itself extended in two directions. Naturally, too, surface is the beginning of body, but not itself body, and likewise the beginning of the three-dimensional but not itself extended in three directions. Exactly the same in numbers, unit is the beginning of all number that advances unit by unit in one direction; linear number is the beginning of plane number, which spreads out like a plane in more than one dimension; and plane number is the beginning of solid number, which possesses a depth in the third dimension besides the original ones.

In the second passage NICOMACHUS describes a strong link between the dimensions and the Pythagorean figurate numbers, the latter in a sense being a part of Greek geometrical algebra.

The primitive theory of the intermediate stage might be described as static. There is no indication of how generation is accomplished in the sequence, point, line, surface, solid. Indeed the static concept of limit is the basis of the explanation. In what seems to be a later Pythagorean theory[5] we find a more

[5] GUTHRIE *(1962: 262)* casts some doubt on whether this theory is really Pythagorean.

dynamic explanation of generation, for the key concept is motion. This later theory is succinctly described in two passages, one from ARISTOTLE (*On the Soul*, I.4, 409a4; quoted in KIRK/RAVEN (*1957: 254)*):

> For they say that the movement of a line creates a plane and that of the point a line; and likewise the movements of units will be lines. For the point is a unit having position.

and one from SEXTUS EMPIRICUS *(1936: 346–349)*:

> But some assert that the body is constructed from one point; for this point when it has flowed produces the line, and the line when it has flowed makes the plane, and this when it has moved towards depth generates the body which has three dimensions. But this view of the (later) Pythagoreans differs from that of the earlier ones. For these latter formed the numbers from two principles, the One and the Indefinite Dyad, and then, from the numbers, the points and the lines and both the plane and the solid forms; but the former build up all of them from a single point. For from this the line is produced, and from the line the plane, and from this the body.

In this unified motion theory of generation the basic geometrical sequence, point, line segment, triangle, tetrahedron, is replaced by the sequence, point, line segment, square, cube. This more advanced theory is usually called the fluxion theory. It has had a profound impact on later mathematicians, such as NEWTON and RIEMANN (see below).[6]

Moving on to the times of ARISTOTLE (384–322 B.C.) and EUCLID (ca. 300 B.C.) we find the following definitions concerning the basic objects of geometry in EUCLID's *Elements* (EUCLID/HEATH *(1926: I,153; III,260)*):

Book I

1. A *point* is that which has no part.
2. A *line* is breadthless length.
3. The extremities of a line are points.
5. A *surface* is that which has length and breadth only.
6. The extremities of a surface are lines.

Book XI

1. A *solid* is that which has length, breadth, and depth.
2. An extremity of a solid is a surface.

We may distinguish two rudimentary 'theories' of dimension suggested by these definitions: a direct theory given by definitions I.1, I.2, I.5, and XI.1 and an indirect theory hinted at by definitions I.3, I.6, and XI.2. Following his usual way of compromise, EUCLID presents both theories, although other sources separate them.

The first theory suggests a direct link between the basic geometrical figures and dimension. The definitions in effect nominate and enumerate the dimensions

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[6] *Cf.* KIRK/RAVEN *(1957: 254–255)* and GUTHRIE *(1962: 262–265)*. I am not convinced by GUTHRIE's explanation of why the motion theory was proposed. On the later influence of the fluxion theory *cf.* EVANS *(1955)*.

of the various figures. Points have no dimension ('no part'); while lines have one ('length'); surfaces, two ('length and breadth'); and solids, three ('length, breadth, and depth').[7] However, EUCLID only touches on the concept of dimension implicitly. Neither does he provide any explanation of the concept, nor does he use any general term for it. Hence, these definitions hardly add up to a genuine theory of dimension.

In the *Topics* (VI.5, 142b22-29; VI.6, 143b11-144a4; see HEATH *(1949: 87–91)*) ARISTOTLE by implication criticises the main definitions of EUCLID's direct theory because of their negative character. He gives his own direct theory of dimension based on the notions of divisibility and continuity (linked concepts in his system) in *On the Heavens* (I.1, 268a4-13, 268a20-268b5) and *Metaphysics* (V.6, 1016b23-31; V.13, 1020a7-14) (*cf.* HEATH *(1949: 159–160, 206–207)*). He even has a special term for dimension. Part of the passage in *On the Heavens* is of special interest (HEATH *(1949: 159)*):

> Of things constituted by nature some are bodies and magnitudes, some possess body and magnitude, and some are the principles of things which possess these. That is continuous which is divisible into parts continually divisible and that which is divisible every way is body. Of magnitude that which (extends) one way is a line, that which (extends) two ways a plane, and that which (extends) three ways a body. And there is no magnitude besides these, because the three dimensions are all that there are, and thrice extended means extended all ways. For, as the Pythagoreans say, the All and all things in it are determined by three things; end, middle and beginning give the number of the All, and these give the number of the Triad.

In this text ARISTOTLE explicitly adopts a Pythagorean metaphysical argument for the 3-dimensionality of physical bodies,[8] although the argument is hardly convincing. The fact that ARISTOTLE relies on Pythagorean ideas suggests that possibly these earlier philosopher-mathematicians took an interest in this special problem of dimension. According to SIMPLICIUS in a commentary on ARISTOTLE's work, PTOLEMY (ca. A.D. 150) later wrote an entire book *On Dimension* in which he put forward a better argument for the 3-dimensionality of bodies and the universe (THOMAS *(1941: 410–413)*). Unfortunately PTOLEMY's book is not extant.[9]

EUCLID's indirect theory, comprised in his subsidiary definitions I.3, I.6, and XI.2 and based on the concept of extremity, has an affinity with the modern recursive definition of dimension using the concept of boundary. EUCLID's subsidiary definitions suggest a progression from points to lines to surfaces to solids, or more accurately, the reverse progression. In modern dimension theory

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[7] The Euclidean definition of point seems to have derived ultimately from the Pythagorean one: the point is a unit (monad) having position. The property of having no part is like the indivisibility of the unit. However, EUCLID does not say anything about position. *Cf.* EUCLID/HEATH *(1926: I, 155–157)*.

[8] We would say in a more abstract way that it is an argument for the 3-dimensionality of *space*.

[9] For a history of the problem of explaining the three dimensions of space *cf.* WHITROW *(1956)* and JAMMER *(1969: 174–186, 205–207)*.

a set or space is n-dimensional according to a recursive definition, whereby n is the least integer for which every point has arbitrarily small neighbourhoods with boundaries at most $(n-1)$-dimensional (and the empty set is assigned dimension -1). Creators of the modern theory, such as POINCARÉ and MENGER, have stressed the connection between the modern and Euclidean definitions, but I do not think we should read too much of the modern theory into the ancient texts. The ancient mathematicians did not operate with genuine recursive definitions, even if their progressions have a semblance of recursion.

According to HEATH (EUCLID/HEATH *(1926: I,155–156)*) the subsidiary definitions are older than EUCLID's main ones and he points to a passage in ARISTOTLE (*Topics*, VI.4, 141b20; HEATH *(1949: 85–86)*) in which the philosopher speaks of the former as *the* definitions. Moreover, ARISTOTLE gives us a supplement to them (*Metaphysics*, XI.2, 1060b12-17; HEATH *(1949: 224)*):

> If we suppose lines or what immediately follows them (I mean the primary surfaces) to be principles, these are at all events not separable substances but are sections and divisions, the one of surfaces, the other of bodies (as points are of lines); they are also extremities or limits of the same things; but all of them subsist in other things, and no one of them is separable.

Thus the various geometrical objects can be sections and divisions as well as extremities of the next higher ones in the hierarchy.

ARISTOTLE is, in fact, critical of all the definitions dependent upon the concepts of extremity and division (*Topics*, VI.4, 141b21; HEATH *(1949: 85–86)*):

> All these definitions explain the prior by means of the posterior, for they say that a point is an extremity of a line, a line of a plane, and a plane of a solid.

To ARISTOTLE's mind the progression from solid down to point, from posterior to prior in the older definitions, is mistaken and so these are unscientific.

In spite of ARISTOTLE's criticism of the logic of the extremity definitions these are the most interesting from the modern dimension-theoretic viewpoint. The downwards progression from solid to point is what makes them seem recursive to us. HEATH remarks *(1926: I,155–156)* that PLATO may well have been an object of ARISTOTLE's criticism. Going beyond this criticism, we may look for the origins of the Euclidean extremity definitions. It seems very likely to me that ultimately these derive from the first Pythagorean theory of generation. In that theory the essential concept was limit or extremity and, in particular, the line segment was viewed as dependent on its two endpoints.[10] I would conjecture that there is an indirect historical link between the very early Pythagorean theory and the extremity definitions. It would certainly be interesting to trace this link further through the intermediate generations of philosophers and mathematicians.[11] However, it seems clear to me that just as the early Pythagoreans sought to explain the generation of physical things through numbers then points, lines, surfaces, and solids, the later extremity definitions constitute an attempt to explain the connections among the geometrical figures and to

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[10] See the texts quoted above, especially those of ARISTOTLE.

[11] This task is beyond the scope of the present work.

explain in a rudimentary way the dimension concept. Thus I think the ancient origins of dimension theory lie in a cosmogonical theory aiming at explaining the beginnings of physical things.

From the brief history just given it should be clear that the Greeks had wide-ranging interests in problems connected with dimension. On the cosmological side their speculations went from the Pythagorean theories of generation to PTOLEMY's proposed demonstration that there are not more than three dimensions. On the more mathematical side there is the evidence collected in the Euclidean definitions. Let me sum up the main theories. The direct theories of EUCLID (naming the dimensions of geometrical figures) and ARISTOTLE (based on divisibility and continuity) do not offer much information on the dimension concept itself. However, the indirect extremity theory found in EUCLID and ARISTOTLE is much more explanatory. It tells us how the geometrical figures are connected by dimension. With hindsight we recognise something of the modern theory in this. It seems likely that this theory goes back to the earliest Pythagorean cosmogonical theory. Of course, this latter theory was intended as an explanation, specifically of the beginnings of physical bodies and indirectly of dimension itself. The later fluxion theory is similarly explanatory.

Both the Greek extremity theory and fluxion theory have been very influential. Not surprisingly we find ISAAC NEWTON (1642–1727) calling upon the motion theory as a support for his fluxional principles of the calculus. In his *Tractatus de Quadratura Curvarum* (dating from 1693; first published in Latin in 1704) he begins by relying on its ancient authority *(1964: 141)*:

> I don't here consider Mathematical Quantities as composed of Parts *extreamly small*, but as *generated by a continual motion*. Lines are described, and by describing are generated, not by any apposition of Parts, but by a continual motion of Points. Surfaces are generated by the motion of Lines, Solids by the motion of Surfaces, Angles by the Rotation of their Legs, Time by a continual flux, and so in the rest. These *Geneses* are founded upon Nature, and are every Day seen in the motion of Bodies.
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> And after this manner the Ancients by carrying moveable right Lines along immoveable ones in a Normal Position or Situation, have taught us the Geneses of Rectangles.

Later in the eighteenth century we find the two Greek theories given an airing in the great *Encyclopédie* of DENIS DIDEROT and JEAN LE ROND D'ALEMBERT *(1751-80)* and again in the collected mathematical articles of the subsequent *Encyclopédie Méthodique. Mathématiques (1784–89).*[12] In his article 'Point' D'ALEMBERT (1717–1783) gives most prominence to the extremity theory, because he espoused an abstractionist philosophy of geometry. Only 3-dimensional solids really exist; points, lines, and surfaces merely exist by abstraction and are the boundaries of their respective higher-dimensional figures. However, he quite naturally includes a statement of the fluxion theory *(1751-80: XII,871)*:

[12] See the articles 'Dimension', 'Point', 'Ligne', 'Surface', 'Solide', and 'Géométrie'.

> Si l'on se représente qu'un *point* coule, il tracera une ligne; & une ligne qui couleroit engendreroit une surface, &c. Cette maniere de considérer la génération des dimensions ou des propriétés des corps, paroît être le premier fondement de la Géométrie moderne, c'est-à-dire, de la Géométrie analytique qui fait usage du calcul différenciel & intégral ....
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> (If one represents a point as flowing, it will trace a line; and a line which flows will engender a surface, *etc.* This way of considering the generation of dimensions or the properties of bodies can be the first foundation of modern geometry, *i.e.*, of the analytical geometry which uses the differential and integral calculus ... .)

In the nineteenth century the Greek theories continued to attract the attention of certain mathematicians, for example, BOLZANO and RIEMANN.[13] I have already mentioned that POINCARÉ and MENGER in the twentieth century have looked back to the Greeks for the original source of dimension ideas. Thus throughout the entire history of mathematics the ancient knowledge about dimension has been a part of geometrical thinking.

I shall now turn to the history of theories of abstract spaces having dimensions greater than three, the subject of the second part of this chapter. The main history of such theories starts in the 1840's and '50's with the publication in rapid succession of works by GRASSMANN, CAYLEY, RIEMANN, and several others. The theories in these works clearly demonstrated that geometry and its spaces ought to be separated from the usual physical space of our perception. Moreover, they forced a radical change in the philosophy of geometry. These changes in geometry and its accompanying philosophy are an essential part of the trend of arithmetisation of the nineteenth century. They form a prime background to CANTOR's paradoxical discovery about dimension. Yet even before 1840 we find a long prehistory of hyperspaces scattered in many sources which is intrinsically interesting and worthy of our attention first.

In a few Greek mathematical works there are traces of higher-dimensional thinking. Recall that in Greek geometrical algebra as, for example, expounded by EUCLID one has linear, square, plane, and solid numbers and magnitudes. Given this geometrical view of quantity there is an automatic prohibition to multiplying, say, rectangular by square quantities to get a 4-dimensional quantity, since space is apparently at most 3-dimensional. Nevertheless, the later mathematicians HERON and DIOPHANTUS of Alexandria did such calculations in their works. In the case of DIOPHANTUS' algebra he specifically introduced higher-dimensional unknowns: *δυναμοδύναμις* (dynamodynamis, square-square) for x^4, *δυναμόκυβος* (dynamocubos, square-cube) for x^5, and *κυβόκυβος* (cubocubos, cube-cube) for x^6. Seemingly commenting on these developments, PAPPUS (ca. A.D. 300) says in connection with the famous locus problem of five or six lines (THOMAS *(1941: 600–603)*):

> ... since no figure can be contained in more than three dimensions. It is true that some recent writers have agreed among themselves to use such

[13] *Cf.* my *(1977)* and below.

> expressions, but they have no clear meaning when they multiply the rectangle contained by these straight lines with the square on that or the rectangle contained by those.

Thus PAPPUS' advice is negative: stick to tradition and avoid the impossible! In any case it does not seem likely that HERON or DIOPHANTUS really conceived of a geometrical space of four or more dimensions. Instead they occasionally just abandoned the tradition of geometrical algebra.

Avoidance of the impossible is characteristic of the prehistory of higher spaces. In the fourteenth century when NICOLE ORESME (1323?–1382) proposed a theory dealing with the quantitative measure or graphical representation of the intensities of qualities in things in his *Tractatus de configurationibus qualitatum et motuum* (probably composed in the 1350's), he expressly denied the need to consider a 4-dimensional space even though his ideas led in that direction. In chapter 4 of part I of his magnificent work (ORESME/CLAGETT *(1968:172–177)*) graphical representations of intensities of qualities in various geometrical subjects are introduced. For a point subject a line segment will represent a quantitative measure of a quality of that subject. A bounded surface will represent the measure of a quality in a linear subject, while a bounded solid will do for a planar subject. In the case of a solid subject ORESME suggests that it be broken into an infinite sequence of surfaces, each with a solid 'graph', such that the graphs interpenetrate or are in mathematical superposition. However, he asserts categorically that this representation does not take place in the fourth dimension, since that does not exist and cannot be imagined. Nevertheless, ORESME in effect has described a 3-dimensional image of a 4-dimensional solid. In all probability this was the first time that anyone did so.[14]

Following the lead of DIOPHANTUS' algebra, many Arab and early Western algebraists spoke of supersolid magnitudes, although they did not intend to introduce spaces of dimensions greater than three. A noteworthy remark on this tradition comes in MICHAEL STIFEL's (ca. 1487–1567) commentary to CHRISTOFF RUDOLFF's Coss (algebra) (RUDOLFF/STIFEL *(1553–54:folio 9)*). STIFEL points out that whereas in geometry we cannot progress beyond the 3-dimensional cube or solid, in algebra we can indeed have a progression (through multiplication) which goes beyond the cube to a 'solid line' ('corporliche lini', which is 4-dimensional), a 'solid surface' ('corporliche superficies', 5-dimensional), and so forth. In this possibility lies the great advantage of algebra over geometry. Pure algebra can break the bonds of the ancient geometrical formulation of the subject. However, a century later the Englishman JOHN WALLIS (1616–1703) inveighed against the mixing of any geometrical terms or ideas with algebra in his important *Treatise (1685:126)*:[15]

> For whereas Nature, in propriety of Speech, doth not admit of more than *Three* (Local) Dimensions, (Length, Breadth and Thickness, in Lines, Surfaces and Solids;) it may justly seem very improper, to talk of a Solid (of three Dimensions) drawn into a Fourth, Fifth, Sixth, or further Dimension.

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[14] *Cf.* ORESME's earlier *Quaestiones super geometriam Euclidis* in ORESME/CLAGETT *(1968:530–531, 544–547)*.

[15] *Cf. (1685:103)*.

> A Line drawn into a Line, shall make a Plane or Surface; this drawn into a Line, shall make a Solid: But if this Solid be drawn into a Line, or this Plane into a Plane, what shall it make? a *Plano-plane?* That is a Monster in Nature, and less possible than a *Chimera* or *Centaure*. For Length, Breadth and Thickness, take up the whole of *Space*. Nor can our Fansie imagine how there should be a Fourth Local Dimension beyond these Three.

WALLIS' more forceful words carry the same message as PAPPUS' of many centuries before.

Notwithstanding WALLIS' rhetoric and good common sense a few men in the seventeenth century dared to conceive of the geometrical fourth dimension. No less than DESCARTES and PASCAL were among their number, although they only flirted with the concept (see WIELEITNER *(1925)* for references). In quite another sphere the Cambridge Platonist HENRY MORE (1614–1687) was attracted by the idea of a fourth dimension, since it provided a place for spirits (Spissitudo essentialis), as he describes in his *Enchiridion Metaphysicum (1671:384)*. In the nineteenth century J.K.F. ZÖLLNER (1834–1882) held a view similar to MORE's that led to some amusing consequences.[16]

Around the middle of the eighteenth century a more serious view of the possibility of higher-dimensional spaces began to take shape. IMMANUEL KANT (1724–1804) touched upon the possibility of a science of hyperspaces in his very first publication, *Gedanken von der wahren Schätzung der lebendigen Kräfte... (1749)*. In this work dealing mainly with the celebrated question of *vis viva*[17] KANT examines the cosmological problem of the 3-dimensionality of physical space, but he feels unable to provide an absolutely convincing explanation for this fact of nature. Nonetheless he conjectures that it is probably explained by the inverse-square law governing the forces between substances (masses) in the universe. However, in another possible world this special law of action could be different, so that to cover all possible worlds one would need a general science of spaces of diverse dimensions *(1749:13)=(1929:12)*:

> Eine Wissenschaft von allen diesen möglichen Raumes-Arten, wäre ohnfehlbar die höchste Geometrie die ein endlicher Verstand unternehmen könnte. Die Unmöglichkeit, die wir bey uns bemerken, einen Raum von mehr als drey Abmessungen uns vorzustellen, scheinet mir daher zu rühren, weil unsere Seele ebenfalls nach dem Gesetze der umgekehrten doppelten Verhältniss der Weiten die Eindrücke von draussen empfängt, und weil ihre Natur selber dazu gemacht ist, nicht allein so zu leiden, sondern auch auf diese Weise ausser sich zu würken.

> (A science of all these possible kinds of space would undoubtedly be the highest enterprise which a finite understanding could undertake in the field of geometry. The impossibility, which we observe in ourselves, of representing a space of more than three dimensions seems to me to be due to the fact that our soul receives impressions from without according to the law of the

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[16] On MORE *cf.* JAMMER *(1969:180–181)*. On ZÖLLNER's views *cf.* his *(1878)* and KLEIN *(1926:169–170)*.

[17] *Cf.* HANKINS *(1965)*. Unfortunately HANKINS does not discuss KANT's work.

inverse square of the distances, and because its nature is so constituted that not only is it thus affected but that in this same manner it likewise acts outside itself.)

In the century before KANT both GALILEO and LEIBNIZ had discussed the problem of space's three dimensions.[18] KANT was the next important philosopher to grapple with the difficult problem. That his thoughts on the subject led him to conceive of a science of *n*-spaces was certainly prophetic.

In both his precritical and critical periods KANT had a continuing interest in philosophical problems about space and geometry. One may recall his penetrating observation in his essay 'Vom den ersten Grunde des Unterschiedes der Gegenden im Raume' *(1768)* that in our 3-dimensional world there exist bodies which are similar and have the same distances among all their respective parts, and yet they cannot be made directly congruent by superposition on the same space. The left and right hands give a simple example of this paradoxical phenomenon. We know, unlike KANT, that in 4-space this will no longer happen. Finally, before leaving KANT, we should note that for him geometry was primarily a science of space as we know it. Although he had some conception of other, more abstract geometries, as clearly evidenced by his prediction of a science of *n*-spaces, he still mainly thought of geometry as linked to physical space.

Just a few years after KANT speculated about a geometry of hyperspaces in his first published work JEAN D'ALEMBERT inserted a more concrete suggestion of a space of more than three dimensions into his article 'Dimension' in the *Encyclopédie* (1754) (DIDEROT/D'ALEMBERT *(1751-80: IV,1010)*):

> J'ai dit ... qu'il n'étoit pas possible de concevoir plus de trois *dimensions*. Un homme d'esprit de ma connoissance croit qu'on pourroit cependant regarder la durée comme une quatrieme *dimension*, & que le produit du tems par la solidité seroit en quelque maniere un produit de quatre *dimensions;* cette idée peut être contestée, mais elle a, ce me semble, quelque mérite, quand ce ne seroit que celui de la nouveauté.
>
> (I have said ... that it is not possible to conceive of more than three dimensions. An intelligent man of my acquaintance believes nevertheless that one can regard time as a fourth dimension, and that the product of time by a solid in this way will be a product of four dimensions; this idea can be disputed, but it seems to me to have some merit even though this may only be that of novelty.)

JOSEPH LOUIS LAGRANGE (1736–1813) later followed in the spirit of this suggestion, when he asserted in his *Théorie des fonctions analytiques (1797: 223)* that mechanics could be taken to be a geometry of four dimensions, employing x, y, z as coordinates for space and t for the fourth dimension of time. Thus by the end of the eighteenth century some truly great thinkers had pronounced on the desirability of considering hyperspaces, although no one had yet fashioned a genuine science of such unimaginable geometrical objects.

[18] *Cf.* WHITROW *(1956)* and JAMMER *(1969)*.

The nineteenth century saw the development of the theory of higher-dimensional spaces and geometry to maturity. However, as already mentioned this did not really begin until nearly half way through the century. During the first forty years contributions to the subject were still sporadic and not well publicised. In his summary 'Essai' on imaginary quantities in Gergonne's *Annales* JEAN ROBERT ARGAND (1768–1822), after sketching a theory in three dimensions, suggests the possibility of extensions to higher dimensions *(1813:146)*. Yet he never produced anything beyond this brief remark. Both CARL GUSTAV JACOB JACOBI (1804–1851) and GEORGE GREEN (1793–1841) used n-dimensional generalisations in the course of their analytical researches (*e.g.*, JACOBI *(1834)*, GREEN *(1835)*). For example, GREEN, after reducing the physical problem of the attraction of ellipsoids with variable densities to mathematical analysis, says *(1835)=(1871:188)*:

> The original problem being thus brought completely within the pale of analysis, is no longer confined as it were to the three dimensions of space.

Thenceforth he develops his theorems of analysis quite naturally in a space of arbitrary finite dimension.

CARL FRIEDRICH GAUSS (1777–1855) was the most significant mathematician to consider the geometry of hyperspaces during the first forty years of the nineteenth century. However, he published little on the subject and even the notes and letters, which only came to light after his death, probably do not represent the full scope of his thoughts on this matter. Nevertheless, the hints that we do have indicate a lifelong interest in multi-dimensional spaces and it is clear that his interest was related to many of his other concerns in mathematics. It is appropriate for us to regard GAUSS' thoughts on hyperspace as a bridge between the prehistory of the concept and its mature development. He thought often and deeply about the abstract foundations of geometry and his last work dealing with higher space (1850–51) falls within the initial period of mature growth of multi-dimensional geometry.

The earliest record of GAUSS' knowledge of higher spaces comes from 1816 in a letter of FRIEDRICH LUDWIG WACHTER to GAUSS of 12 December (see STÄCKEL *(1901)*, GAUSS *(1917:481–482)*). This letter refers to a conversation which WACHTER had with GAUSS during April of that year. It demonstrates GAUSS' (as well as WACHTER's) early understanding of multi-dimensional analytic geometry. WACHTER specifically mentions infinite-dimensional spaces as well as finite-dimensional ones, so he and GAUSS were apparantly well advanced in the theory.

We find GAUSS' most important references to the problem of defining 1-, 2-, and n-dimensional manifolds (Mannigfaltigkeiten) in his two works of 1831 on the theory of biquadratic residues: 'Theoria residuorum biquadraticorum. Commentatio secunda' *(1832)=(1863:93–148)* and especially the accompanying 'Selbstanzeige' *(1831)=(1863:169–178)*. The reference to higher-dimensional developments consists of just a single remark. One of GAUSS' concerns in these works is the 'metaphysics', *i.e.*, foundations, of the complex number system. Although he describes the usual planar representation of complex numbers, feeling even as late as 1831 that this number system needed some sort of justification by means of a geometrical model *(1863: 109–110, 174)*, in his 'Anzeige' he puts more emphasis on an abstract approach to the 'true metaphysics of imaginary quantities' *(1863: 175–178)*. He only treats the complex ('Gaussian') integers in detail but presumably an extension to all complex numbers can be made easily. The kernel of GAUSS' abstract theory springs from his observation that numeration consists in a set of relations. If we have a linear sequence of objects, it is the relations among them and their inverse relations that are important. From these we can pick an origin, derive the inverse units $+1$ and -1, and then obtain the usual integers. If we have a sequence of sequences or, in other words, a manifold of two dimensions, we must examine the relations between one sequence and an immediately neighbouring sequence and thence derive the inverse units $+i$ and $-i$ and then all the complex integers. In this way we abstract the complex integers from a twofold variety of sequence of sequences.

For GAUSS then, number systems in the most proper mathematical sense of the term are about relations, their comparison and 'numbering', and not about the objects numbered. But to see these relations intuitively we need to use geometrical models. The plane, a well-understood manifold of two dimensions, serves this purpose for the complex number system. GAUSS regarded his theory of complex quantities as a way of clearing up the 'mysterious obscurity' falsely attributed to the imaginary numbers. His abstract theory is a genuine ancestor of the rigorous logical theories of numbers proposed during the latter half of the nineteenth century.

At the very end of the 'Anzeige' GAUSS *(1863: 178)* mentions manifolds of more than two dimensions, but he does not discuss them. However, it seems certain that his specific theory for the complex integers and 2-dimensional manifolds is related to a more general theory of manifolds which he held. Such a general theory would cover hypernumber systems and n-dimensional manifolds and would constitute a branch of abstract geometry. We find a brief indication of the more general scope of such a theory in a letter of GAUSS to HANSEN of 11 December 1825 *(1929: 8)*:

> Ich habe mich in diesem Herbst sehr viel mit der allgemeinen Betrachtung der krummen Flächen beschäftigt, welches [sic] in ein unabsehbares Feld führt. ... Jene Untersuchungen greifen tief in vieles andere, ich möchte sogar sagen, in die Metaphysik der Raumlehre ein, und nur mit Mühe kann ich mich von solchen daraus entspringenden Folgen, wie z.B. die wahre Metaphysik der negativen und imaginären Grössen ist, losreissen. Der wahre Sinn des $\sqrt{-1}$ steht mir dabei mit grosser Lebendigkeit vor der Seele, aber es wird sehr schwer sein, ihn in Worte zu fassen, die immer nur ein vages, in der Luft schwebendes Bild geben können.

> (During this autumn I have concerned myself very much with the general consideration of curved surfaces, which leads into an immense field. ... Those investigations penetrate deeply into many others, I may even say into the metaphysics of the theory of space, and only with difficulty can I tear myself away from such results arising therefrom, as, for example, the true metaphysics of negative and imaginary quantities. The true meaning of $\sqrt{-1}$

> stands very vividly before my soul, but it will be very difficult to put it into words, which can only give but a vague fleeting image.)

It is clear that GAUSS' ideas on curved surfaces, negative and imaginary quantities, and manifolds and space were all linked.

During the last years of his life GAUSS did explicitly consider arbitrary n-dimensional manifolds in his lectures, 'Über die Methode der kleinsten Quadrate', delivered during the winter semester of 1850–51. Concerning the content of these we have the records of his student AUGUST RITTER (1826–1908) (in GAUSS *(1917: 469–482)*).[19] From these records it is easy to see why GAUSS chose abstract manifolds or analytic spaces of n dimensions as the natural vehicle for his investigations into the method of least squares. Such spaces involve a generalised analytic geometry based on n variable coordinates and the usual Euclidean metric which can be applied to the solution of the minimum problem.

GAUSS' ideas on multi-dimensional spaces bring us close to his philosophical thoughts on geometry. The 'Anzeige' of 1831 and the letter to HANSEN of 1825 give definite signs of his abstract view of the subject. There are further indications of this view from the latter part of his life. In his *Jubiläumsschrift* of 1849, 'Beiträge zur Theorie der algebraischen Gleichungen' *(1850)*=*(1876: 71–102)*, containing his important revision of his very first attempt to prove the fundamental theorem of algebra in his *Inaugural Dissertation* of 1799, he excuses himself for a reliance on geometrical reasoning[20] *(1876:79)*:

> Ich werde die Beweisführung in einer der Geometrie der Lage entnommenen Einkleidung darstellen, weil jene dadurch die grösste Anschaulichkeit und Einfachheit gewinnt. Im Grunde gehört aber der eigentliche Inhalt der ganzen Argumentation einem höhern von Räumlichem unabhängigen Gebiete der allgemeinen abstracten Grössenlehre an, dessen Gegenstand die nach der Stetigkeit zusammenhängenden Grössencombinationen sind, einem Gebiete, welches zur Zeit noch wenig angebauet ist, und in welchem man sich auch nicht bewegen kann ohne eine von räumlichen Bildern entlehnte Sprache.

> (I shall represent the proof in clothing borrowed from the geometry of position, because this yields the greatest lucidity and simplicity. However, the real content of the entire argument belongs fundamentally to a higher domain of the general abstract theory of quantity, independent of spatial things, whose subject is the combinations of quantities connected according to continuity; a domain which at this time is still little cultivated, and in which one cannot express oneself without a language based on spatial images.)

The view of an abstract geometry divorced from spatial intuition expressed in this passage must be related to GAUSS' longstanding desire for a development of 'geometria situs' (see STÄCKEL *(1922–33: 46–49)*). At the end of his life he persisted in his hope for a flowering of this subject, as WALTER SARTORIUS VON

[19] *Cf.* also DEDEKIND *(1931: 293–306)*.

[20] This geometrical reasoning is related to certain complex functions.

WALTERSHAUSEN has reported from conversations which he had with GAUSS during his last years, 1847–1855 *(1856: 88)*:

> ... eine ausserordentliche Hoffnung setzte er aber auf die Ausbildung der Geometria situs, in der weite gänzlich unangebaute Felder sich befänden, die durch unsern gegenwärtigen Calcul noch so gut wie gar nicht beherrscht werden könnten.
>
> (... yet he held an extraordinary hope for the development of geometria situs, in which there might be extensive but entirely uncultivated fields that could hardly be controlled by our present methods.)

GAUSS thus connected abstract geometry with geometria situs – topology, as we would call it. He published virtually nothing on this subject, nor did he even write much down on it. Yet he had a vision of its importance. It remained for others to transform this vision into a reality.[21]

To be sure, GAUSS felt it was necessary to relate abstract geometry to the concrete geometry[22] of physical space. Abstract geometry is concerned with n-dimensional manifolds in general, while spatial geometry has a special 3-dimensional manifold for its subject. He thought that our knowledge of this special manifold is not entirely *a priori* (letters to BESSEL, GAUSS *(1900: 200–201)*). Consequently, he did not subscribe to KANT's view of space as an outer form of our intuition and explicitly said so on a number of occasions (GAUSS *(1863: 177) 1900: 224)*). He believed that space is real and our knowledge of space is at least partly empirical. One could even prove this fact from the writings of KANT himself:[23]

In spite of the fact that GAUSS disagreed fundamentally with KANT on the nature of space and geometry, it is possible that he borrowed one thing from the great philosopher of Königsberg: the word 'Mannigfaltigkeit' ('manifold'). This word and the related term 'Mannigfaltige' occur frequently in KANT's writings, especially in the *Critik der reinen Vernunft*. Perhaps GAUSS decided to use the Kantian term, although not in the way KANT employed it. In GAUSS' papers the term 'Mannigfaltigkeit' denotes the most general objects of geometry. After him and his successor RIEMANN it became widely used by mathematicians.[24]

GAUSS' general attitude towards geometry marks the beginning of a new philosophical view of the subject which became established during the nineteenth century. For him geometry in large part became freed from physical space. On one level it became a branch of 'pure mathematics'. One may rely on

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[21] For comments on GAUSS' work in topology and the foundations of complex numbers *cf.* STÄCKEL *(1922–33: 46–68)*, FRAENKEL *(1920)*, SCHLESINGER *(1922–33: 53–57, 202–210)*. See GAUSS *(1917: 106–107, 396–397, 407–412, 436–437)*.

[22] We would say, 'applied geometry'.

[23] *Cf.* GAUSS *(1863: 177)* and SCHLESINGER *(1922–33: 177–181)*.

[24] STALLO *(1960: 268–269)* suggests that GAUSS borrowed the term 'Mannigfaltigkeit' from HERBART, who uses it in his metaphysical works. However, I can find no evidence that GAUSS ever took an interest in HERBART's philosophy. On the contrary, it seems much more likely that GAUSS borrowed the term from KANT, whom he criticised. Nevertheless, it is only a conjecture that GAUSS borrowed the term; I cannot find any direct evidence in GAUSS' writings to confirm that he took it from KANT.

spatial intuition and images, but in the final analysis these can be eliminated from abstract geometrical theories. While GAUSS cannot be said to have completely envisaged the purely logical treatment of geometry, so masterfully advocated at the end of the century by the Italian school and HILBERT, he did see the subject as encompassing much more than the metrical relations of physical space. His notion of n-dimensional manifolds as well as his theory of non-Euclidean geometry were a part of this view. As we shall see in a moment, GAUSS' philosophical views about the nature of geometry were soon to influence the young RIEMANN.

A host of works published during the 1840's and '50's dealing with hyperspaces mark the beginnings of the mature development of multi-dimensional geometry. A wide variety of algebraic, analytic, and geometrical problems spurred mathematicians to consider such spaces.[25] The mathematicians who started to grapple with the problems of hyperspace geometry at this time were ARTHUR CAYLEY (1821–1895), HERMANN GRASSMANN (1809–1877), JULIUS PLÜCKER (1801–1868), AUGUSTIN LOUIS CAUCHY (1789–1857), JAMES JOSEPH SYLVESTER (1814–1897), LUDWIG SCHLÄFLI (1814–1895), and BERNHARD RIEMANN (1826–1866). Most of these men simply introduced hyperspaces through n-fold systems of coordinates and then investigated the metric and projective geometry of the resulting coordinate spaces with analytic techniques. In his first paper on the subject, 'Chapters in the Analytical Geometry of (n) Dimensions' *(1844)* (read in 1843), the young CAYLEY proceeds in this way. In this paper as elsewhere he is not concerned with giving an explanation of the possible 'metaphysics' of higher space.[26] CAUCHY's very short 'Mémoire sur les lieux analytiques' *(1847)*, in a similar vein to CAYLEY's first paper, merely defines some geometrical terms for n-dimensional analysis. Presumably following the lead of CAYLEY, SYLVESTER began to work with n-dimensional spaces in 1850 *(1850) (1851) (1851a)* (*etc.*). In one of his early papers he makes the following apposite methodological remark justifying the geometry of n-spaces *(1851a:120)=(1904:219)*:

> If the impressions of outward objects came only through the sight, and there were no sense of touch or resistance, would not space of three dimensions have been physically inconceivable? The geometry of three dimensions in ordinary parlance would then have been called transcendental. But in very truth the distinction is vain and futile. Geometry, to be properly understood, must be studied under a universal point of view; every (even the most elementary) proposition must be regarded as a fact, and but as a single specimen of an infinite series of homologous facts.
>
> In this way only (discarding as but the transient outward form of a limited portion of an infinite system of ideas, all notion of extension as essential to the conception of geometry, however useful as a ,suggestive element) we may hope to see accomplished an organic and vital development of the science.

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[25] *Cf.* SEGRE *(1921)* for an encyclopedic view of the mathematics and an overview of the history of hyperspaces. *Cf.* also his *(1904)*.

[26] Some of CAYLEY's other papers on the subject are *(1846) (1846a) (1854) (1870)*.

GEORGE BOOLE (1815–1864) subscribed to much the same view as SYLVESTER, as shown in a brief note in his *Laws of Thought (1854:175)*.

LUDWIG SCHLÄFLI, proceeding along the same methodological path as CAYLEY, SYLVESTER, and CAUCHY, wrote one of the most extensive accounts of the metrical geometry of n coordinate spaces during the years 1850–1852: *Theorie der vielfachen Kontinuität (1901)*. However, he ran into difficulties with the publication of this immense work, so that it was not published in its entirety until after his death (*cf. (1950:388–390)*). The excerpts which did appear soon after composition *(1855) (1858–60)* unfortunately did not attract much attention.

JULIUS PLÜCKER's mode of introducing hyperspaces was quite different from the methods of CAYLEY, CAUCHY, SYLVESTER, and SCHLÄFLI. In a remark in his *System der Geometrie des Raumes ... (1846:322–323)* he shows how to create a 4-dimensional geometry in ordinary 3-dimensional space through a system of line coordinates. Straight lines in ordinary space defined by four parameters become the elements of a hyperspace. This innovative and distinctly geometrical way of introducing higher spaces through basic elements other than points gave PLÜCKER an entire programme of research which culminated in his postumously-published *Neue Geometrie des Raumes gegründet auf die Betrachtung der geraden Linie als Raumelement (1868) (1869)*.

I now wish to consider the work of GRASSMANN and RIEMANN in more detail, because their philosophical attitudes to the foundations of hyperspace geometry typify some of the essential changes in ideas concerning space, geometry, and dimension which were taking place around the middle of the nineteenth century.

Die lineale Ausdehnungslehre (1844) of HERMANN GRASSMANN has often been criticised for its difficult 'metaphysical' style. Unquestionably the mathematical theory contained in the book is much obscured by the cloak of philosophy which surrounds it. Perhaps GRASSMANN's father influenced the son in his general philosophical view of mathematics a little too strongly.[27] Possibly his theological studies at Berlin University had a detrimental effect on his mathematical ideas. Indeed he acquired his mathematics without the benefit of formal university instruction. It certainly did not aid the first readers of his book that GRASSMANN chose to fit his original mathematical theory into an unclear philosophical framework, an all-too-metaphysical framework in the German idealistic tradition. Partly for this obscurity in presentation he failed to gain early recognition for his new 'theory of extension' and a much desired university post. Even the revised version of his book *(1862)*, far less impregnated with philosophy, did not win him full success and praise from the mathematical community. However, in spite of the apparent bad features of his mixture of philosophy and mathematics in the *Ausdehnungslehre* we must recognise a most valuable philosophical element, giving us a clear sign of the attitudes of some mid-nineteenth-century mathematicians to geometry and hyperspaces. It seems that it was partly for this element that GAUSS was able to give a measure of praise to GRASSMANN's work, for there is a marked affinity between GAUSS'

[27] *Cf.* SCHLEGEL *(1878)*, who suggests this explanation to me.

ideas on the metaphysics of complex quantities and the philosophical tendency towards abstraction of GRASSMANN's book.[28]

When attempting to place his new theory of extension among the mathematical sciences, GRASSMANN expresses the key philosophical judgment of his *Ausdehnungslehre* – that the theory is logically separate from geometry *(1844: Vorrede, Einleitung) = (1894: 10–11, 22–32)*. Up to the time of GRASSMANN virtually all mathematicians and philosophers[29] looked upon geometry as a science concerned with physical space and therefore derived from our knowledge of the external world and limited to the three dimensions of space. But in the *Ausdehnungslehre* GRASSMANN conceives of mathematics as a science of pure forms of thought, logically independent of experience.[30] The theory of extension as a branch of pure mathematics with continuous quantities as its subject matter is divorced from spatial intuitions and free from the constraints of 3-dimensionality. Under this view it transcends traditional geometry, which is only a small application of the much more general theory of extension. Thus in an abstract of his work he writes *(1845) = (1894: 297)*.

> Meine Ausdehnungslehre bildet die abstrakte Grundlage der Raumlehre (Geometrie), das heisst, sie ist die von allen räumlichen Anschauungen gelöste, rein mathematische Wissenschaft, deren specielle Anwendung auf den Raum die Raumlehre ist.
>
> ...
>
> Dadurch geschieht es nun, dass die Sätze der Raumlehre eine Tendenz zur Allgemeinheit haben, die in ihr vermöge ihrer Beschränkung auf drei Dimensionen keine Befriedigung findet, sondern erst in der Ausdehnungslehre zur Ruhe kommt.
>
> (My theory of extension forms the abstract foundation of the theory of space (geometry), *i.e.*, it is a pure mathematical science independent of all spatial intuitions, whose special application to space is geometry.
>
> ...
>
> Thus it is the case that the theorems of geometry tend towards generality, but in virtue of their limitation to three dimensions this generality cannot be achieved; it can be achieved first in the theory of extension.)

Although GRASSMANN's style may be somewhat opaque, his line of thought is transparent. *Ausdehnungslehre*, or abstract geometry, is not confined to our knowledge of physical space, but is prior to such knowledge. It comes before physical magnitude. Furthermore, it is even prior to number and arithmetic as such, although abstract concepts of number and magnitude are immediately derivable from the continuous quantities which are the subject matter of the

[28] See the letter of GAUSS to GRASSMANN, dated 14 December 1844, in GAUSS *(1917: 436–437)*.

[29] GAUSS and the non-Euclidean geometers excepted.

[30] Note that GRASSMANN took a psychologistic view of mathematics. This was a common view in the nineteenth century, but it is now much discredited.

theory. Starting from this assumption about priority, we can envisage geometry developing towards its natural generality according to its own internal logic.[31]

Consequently, in his *Ausdehnungslehre* GRASSMANN goes beyond the ordinary 3-dimensional space to treat higher-dimensional abstract spaces *(1844: §§13, 14, 16)=(1894: 46–49, 51–53)*. His definition of such spaces is based on an analogy with the ancient fluxion theory of the generation of figures, whereby a moving point generates a line, a moving line produces a surface, *etc.* Strictly speaking, he tries to draw a sharp boundary between this 'concrete' motion theory and his own abstract theory, although I do not think we can regard this attempt as very successful. When considering the generation of an extension-form of first order (Ausdehnungsgebilde erster Stufe), he conceives an abstract element (Element, like a geometrical point) undergoing changes (Aenderungen, like the motion of a point) in a specified direction, thereby producing the first-order form (like a straight line segment) *(1894: 48)*:

> Unter einem Ausdehnungsgebilde erster Stufe verstehen wir die Gesammtheit der Elemente, in die ein erzeugendes Element bei stetiger Aenderung übergeht ... .
>
> (We understand by an extension-form of first order the totality of elements into which a generating element passes through continuous change ... .)

The totality of all such possible elements extended along one dimension is then a system or domain (System, Gebiet) of first order (like an infinite line). Proceeding to higher-order forms, he thinks of two distinct and independent changes operating on an element. First one change produces a first-order form from the element and then the second change produces a sequence of (parallel) forms. The infinite set of elements thus generated is a system of second order. With additional types of changes it is possible to derive systems of third, fourth, ..., and any finite order.

Although GRASSMANN was a founder of modern vector analysis, we may well find it difficult to look upon his definition of systems of finite order —presumably finite-dimensional vector spaces—as very revealing. He must have had a hard time trying to put his 'inductive' definition of his fundamental spaces into words, since he could find hardly anything more than the ancient fluxion theory to rely upon. However, in the second edition of his work the definition of n^{th}-order domains is significantly improved and much more straightforward *(1862: §14)=(1896: 16)*. A domain of n^{th} order is simply dependent on coordinates associated with n units (unit vectors).

BERNHARD RIEMANN's principal statement concerning the domain of higher-dimensional geometry is his celebrated *Habilitationsvortrag*, 'Über die Hypothesen, welche der Geometrie zu Grunde liegen'. Like GRASSMANN RIEMANN unfortunately couched his work on hyperspace in rather muddy philosophical language. Yet in spite of the obscuring mode of expression this lecture, delivered in 1854 but not published until 1868 after his death, is well known for

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[31] KLEIN *(1926: 177–178)* emphasises the methodological link between the views of GRASSMANN and the mathematicians (such as HILBERT) who developed logical theories of pure (nonmetrical) geometry at the end of the century.

its profound ideas, so tightly packed into its two dozen paragraphs: the generalisation to n dimensions of Gaussian curvature, the foundations of 'Riemannian' geometry and the elliptic variety of non-Euclidean geometry, and the discussion of the relationship between pure geometry and physical space. We now recognise in RIEMANN's short work the seeds for a tradition which led to the mathematics behind EINSTEIN's theory of relativity.

For the present discussion we need only be concerned with the first section of the lecture. It is in this section that RIEMANN tries to explain his notion of an n-dimensional manifold. When the lecture was first published in 1868, philosophers took most interest in this section, because the treatment of the manifold concept in it is largely philosophical. In fact, the philosophers quickly made objections to RIEMANN's unclear methods and presentation. Yet there is a key point in the first section which all the early readers of the published lecture failed to grasp. RIEMANN primarily aimed at defining n-dimensional manifolds as *topological* objects. His fundamental concern was topology, or analysis situs, as he would say. With topology in its infancy it is not surprising that early readers did not understand RIEMANN's aim. Still it is worth while for us to examine this very early statement of a topological programme, especially in order to understand some of the difficulties faced by even a great mathematician when presenting original ideas.

No doubt part of the difficulty with RIEMANN's presentation was the result of the hasty preparation of the lecture which was enforced on him. How he came to lecture on the foundations of geometry for his habilitation to *Privatdozent* is a curious tale, worth repeating (DEDEKIND in RIEMANN *(1953: 547–549)*).[32] By the end of 1853 RIEMANN had finished and submitted his *Habilitationsschrift*, 'Über die Darstellbarkeit einer Function durch eine trigonometrische Reihe'; the *Habilitationsvortrag* still had to be arranged. Concerning the prospects for this he wrote to his brother WILHELM on 28 December 1853 (RIEMANN *(1953: 547)*):

> Mit meinen Arbeiten steht es jetzt so ziemlich; ich habe Anfangs December meine Habilitationsschrift abgeliefert und musste dabei drei Themata zur Probevorlesung vorschlagen, von denen dann die Facultät eines wählt. Die beiden ersten hatte ich fertig und hoffte, dass man eins davon nehmen würde; Gauss aber hatte das dritte gewählt, und so bin ich nun wieder etwas in der Klemme, da ich dies noch ausarbeiten muss.
>
> (My work is now progressing tolerably well; at the beginning of December I submitted my *Habilitationsschrift* and besides had to propose three subjects for the trial lecture, from which the faculty could then choose one. I had the first two complete and hoped that they would choose one of them; Gauss, however, chose the third and so now I am in something of a quandary, since I must still work this one out.)

Poor RIEMANN! GAUSS passed over the first two of the suggested themes, one on the history of the representation of functions by trigonometric series, the second on the solution of two equations of the second degree in two unknowns, in order

[32] RICHARD DEDEKIND's 'Bernhard Riemann's Lebenslauf' in RIEMANN *(1953: 539–558)* is the standard biography of RIEMANN; *cf.* FREUDENTHAL *(1975a)*.

that he might hear something on the foundations of geometry, a subject close to his heart.[33] During the first part of 1854 RIEMANN was ill much of the time; he overworked and suffered from the inclement weather. It was not until a fortnight after Easter that he began to prepare his trial lecture in earnest, but nevertheless it was complete by Whitsuntide. The preparation time was certainly remarkably short, when we consider the stature of the final product. GAUSS himself wanted to examine the young geometer, but he was in poor health. Many even thought his death was imminent. So he asked that RIEMANN's lecture be postponed until his own health might improve and RIEMANN accepted the inevitable delay. But then without warning GAUSS decided to let RIEMANN give the lecture, as the latter told his brother in a letter of 26 June 1854 *(1953: 548)*:

> Da entschloss er sich plötzlich auf mein wiederholtes Bitten, „um die Sache vom Halse los zu werden", am Freitag nach Pfingsten Mittag das Colloquium auf den anderen Tag um halb elf anzusetzen und so war ich am Sonnabend um eins glücklich damit fertig.
>
> (Then suddenly, after my repeated requests 'to take the yoke from my neck', he decided at noon on the Friday after Whitsun to set the colloquium for the next day at 10.30 and so on Saturday at one o'clock I was fortunately finished with it.)

After the lecture, which was given so suddenly on the 10th of June, GAUSS highly praised RIEMANN's work to the physicist WILHELM WEBER on their way back to the faculty meeting. He expressed himself with unaccustomed excitement on the depth of RIEMANN's thought. In the eyes of the Prince of Mathematicians the young man was a success.

Considering the way it was prepared, we must conclude that the lecture was only an extended sketch of RIEMANN's geometrical ideas. Moreover, insofar as he had to deliver it to the entire Philosophical Faculty of Göttingen, he needed to try to make his ideas clear to nonmathematicians (DEDEKIND in RIEMANN *(1953: 549)*). Probably if he had had time in his brief life to revise and expand the lecture for publication, he would have expressed himself differently on many matters. As it was, his only further treatment of part of the material of the lecture was in his Paris Academy essay of 1861 *(1953: 391–404)*.[34] Thus his friend DEDEKIND almost certainly published a work which RIEMANN himself would have preferred to put into a better form. It was, of course, wise for DEDEKIND to publish the original lecture, but we must be prepared for its confusions and infelicitous expressions.

The overall theme of RIEMANN's *Habilitationsvortrag (1868) = (1953: 272–287)* is to determine the logical relationship between geometry conceived, more or less, as a discipline of 'pure mathematics' and its application to physical space. Within the scope of this theme he examines three special problems. The first of these is the important one for us *(1953: 272)*:

> Ich habe mir daher zunächst die Aufgabe gestellt, den Begriff einer mehrfach ausgedehnten Grösse aus allgemeinen Grössenbegriffen zu construiren.

[33] *Cf.* RIEMANN *(1953: Nachträge, 112)*.

[34] *Cf.* the 'Fragment aus der Analysis Situs' *(1953: 479–482)*.

(In the first place I have thus set myself the task of constructing the concept of a multiply extended magnitude from the general concepts of magnitude.)

RIEMANN thought that a clarification of the notion of multiply extended magnitude was a crucial prerequisite for improving our understanding of the relationship of geometry to the concept of space. Contained in his discussion of n-fold magnitudes given in section I of his lecture is an informal theory of topological manifolds and dimension. He intended this theory to serve as a general framework ('Vorarbeit', 'preparation') for contributions to analysis situs. A statement of this intention is tucked away in a footnote at the end of the published version of the lecture *(1953:286)*.[35]

When embarking on his discussion of the general notion of manifold, RIEMANN asks the indulgence of his audience, for – as he puts it – the difficulty lies more in the philosophical nature of the subject and he is not practised in such investigations *(1953:273)*. He claims besides that there was little background for him to rely upon – only a few remarks in papers of GAUSS and some philosophical researches of HERBART. He explicitly cites those papers of GAUSS which he found useful: 'Theoria residuorum biquadraticorum. Commentatio secunda' *(1832)=(1863:93–148)*, the accompanying 'Selbstanzeige' *(1831) =(1863:169–178)*, and the *Jubiläumsschrift* of 1849, 'Beiträge zur Theorie der algebraischen Gleichungen' *(1850)=(1876:71–102)*. Thus GAUSS' hints on the abstract and geometrical foundations of the complex number system, on the definition of 2- and higher-dimensional manifolds, and on the broader need to develop the subject of analysis situs[36] were a direct stimulus for RIEMANN's ideas about manifolds. In his discussion RIEMANN goes so far as to borrow some expressions from GAUSS. In the case of the reference to HERBART's works he does not say which he found helpful. However, it is known that this German philosopher had a significant, if (as we might be inclined to think) detrimental influence on the young mathematician. Let us then have a brief look at this influence.

While a university student RIEMANN took a special interest in the philosophy of JOHANN FRIEDRICH HERBART (1776–1841). In his biography DEDEKIND reports that during three semesters at Göttingen from Easter 1849 through 1850 RIEMANN attended some philosophical lectures and, in particular, made a study of HERBART's thought (RIEMANN *(1953:544–545)*). In some fragments which RIEMANN left among his papers there is ample evidence of HERBART's influence on his thoughts about psychology, epistemology, metaphysics, and natural philosophy *(1953:509–538)*. It is even possible that his difficult 'metaphysical' style of writing derived partly from HERBART. Unfortunately his style often follows the example of the worst German philosophical tradition. Yet, while HERBART's ideas were the starting point for his own, he did not remain a Herbartian. In the end he criticised HERBART and developed his own philosophical viewpoint, as an undated fragment in his *Nachlass* reveals *(1953:508)*:

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[35] Many apparently missed this statement of intention. For example, in his English translation CLIFFORD fails to include it.

[36] See the discussion above.

Der Verfasser ist Herbartianer in Psychologie und Erkenntnisstheorie (Methodologie und Eidolologie), Herbart's Naturphilosophie und den darauf bezüglichen metaphysischen Disciplinen (Ontologie und Synechologie) kann er meistens nicht sich anschliessen.

(The author is a Herbartian in psychology and epistemology (methodology and eidolology), but for the most part he cannot subscribe to Herbart's natural philosophy and the related metaphysical disciplines (ontology and synechology).)

HERBART was KANT's successor at Königsberg, but he developed his own distinct philosophy. He also taught at Göttingen from 1833 until his death in 1841, hence his spiritual influence there over RIEMANN. Strongly interested in mathematics throughout his career, HERBART propounded an extensive philosophy of space and geometry in his works on psychology and metaphysics. Judging by the statement of RIEMANN just quoted, we can assume that HERBART's psychological theory of space and geometry had more influence over RIEMANN's ideas than his metaphysical theory. However, after examining the *Habilitationsvortrag* for traces of HERBART's philosophy, I think that the general views of HERBART exerted greater influence on RIEMANN's philosophico-geometrical ideas than specific points of doctrine.[37]

The most important statements of HERBART's philosophy of space and geometry occur in his two treatises *Psychologie als Wissenschaft (1824) (1825)* and *Allgemeine Metaphysik, nebst den Anfängen der philosophischen Naturlehre (1828) (1829)*, ponderous works in the German metaphysical tradition. In the first HERBART investigates the psychological origins of our ideas of 'sensible' space (sinnlicher Raum) *(1824: §100) = (1890: 409–420) (1825: §§109–116, 139, 143) = (1892: 86–112, 191–193, 222–224)*. An essential part of his doctrine is that our idea of space is but one example of a sequence or sequence-form (Reihe, Reihenform). We can also regard time, the colours, and the musical tones as sequences, though not all as linear sequences. Sensible space is a sequence of sequences of sequences; hence, it is 3-dimensional. This characterisation has some similarity to GAUSS' theory of 2- and higher-dimensional manifolds in his 'Anzeige' of 1831. RIEMANN seems to have been attracted to HERBART's theory of sequence-forms, as is evident when he too compares the multi-dimensional space continuum with the colours. Moreover, RIEMANN, like HERBART, is very much an empiricist in his views about space.

In his *Allgemeine Metaphysik* under his 'Synechologie' HERBART proposes a metaphysical theory of intelligible space and geometry *(1829: Drittes Abschnitt) = (1893: 110–186)*. In his system synechology is one of the four branches of metaphysics and concerns the theory of the continuous – the continuous as exhibited in time, space, matter, and motion. By trying to resolve the contradiction inherent in the concept of the continuous (synechology's basic problem or pseudoproblem!), he successively constructs the straight line, the plane, and geometrical space. In effect the resulting philosophy is a defence of

[37] On HERBART's influence over RIEMANN *cf.* ERDMANN *(1877: 29–31)* and RUSSEL *(1897: 14–15, 62–63)*.

traditional Euclideanism, but HERBART is often wrong in his mathematics – even according to the standards of his day. In particular, he gives a metaphysical proof of the impossibility of 4-dimensional intelligible space *(1893:152–153)*. Obviously RIEMANN did not rely very much upon HERBART's metaphysics. However, the programme of *constructing* the geometrical objects of various dimensions and the view of them as *sequences* certainly had an impact on RIEMANN, when he was trying to define his manifolds in a recursive or inductive manner.

Apart from the background researches of GAUSS and HERBART, RIEMANN's own work in mathematics, especially in the theory of complex functions and algebraic functions, must have motivated his attempt in the *Habilitationsvortrag* to provide a possible framework for contributions to analysis situs through a definition of manifold. His investigations in complex function theory as presented in his *Dissertation*, 'Grundlagen für eine allgemeine Theorie der Functionen einer veränderlichen complexen Grösse' *(1851)=(1953:3–48)*, had led him to consider the topology of surfaces, *i.e.*, 2-dimensional manifolds, and so it was natural for him to contemplate a study of the general case. Thus in his lecture of 1854 he briefly refers to his own special way of dealing with multi-valued analytic functions – by means of RIEMANN surfaces – in order to persuade his listeners that a topological treatment of manifolds is important and even necessary for an understanding and further development of some parts of mathematics. In his magnificent papers collectively known as his 'Theorie der Abel'schen Functionen' *(1857) (1857a) (1857b) (1857c)=(1953:88–144)*, published three years after delivering the lecture, RIEMANN provides a fairly clear statement of his conception of topology *(1953:91)*:

> Bei der Untersuchung der Functionen, welche aus der Integration vollständiger Differentialien entstehen, sind einige der analysis situs angehörige Sätze fast unentbehrlich. Mit diesem von Leibnitz, wenn auch vielleicht nicht ganz in derselben Bedeutung, gebrauchten Namen darf wohl ein Theil der Lehre von den stetigen Grössen bezeichnet werden, welcher die Grössen nicht als unabhängig von der Lage existirend und durch einander messbar betrachtet, sondern von den Massverhältnissen ganz absehend, nur ihre Orts- und Gebietsverhältnisse der Untersuchung unterwirft.

> (In the investigation of functions which arise from the integration of total differentials several theorems belonging to analysis situs are almost indispensable. With this name, used by Leibniz, though perhaps not entirely with the same significance, one may be permitted to denote a part of the theory of continuous quantities which considers such quantities not as existing independently of their position or measurable by one another, but, on the contrary, which investigates only their local and regional properties entirely divorced from measure-relations.)

With this background in mind we may proceed to examining the details of the first section of his lecture, trying to give a sympathetic reading to some of his obscure expressions.

When attempting to construct the concept of a multiply extended magnitude (mehrfach ausgedehnte Grösse) from general concepts of magnitude, RIEMANN has a great deal to say about the concept of manifold (Mannigfaltigkeit). A close reading of the text shows that he regards the manifold concept as more general than the concept of magnitude. Most of his early expositors and critics did not recognise this important point, probably because it is not declared explicitly. JACOBSON *(1883)* was an exception. By trying to say something about the concept of manifold apparently RIEMANN was struggling to define a notion like our modern topological manifold or even our general topological space, although in his work there remains an implicit link between his manifold concept and the narrow domain of algebraic functions. In general his results on manifolds are not fully successful.

RIEMANN speaks of discrete and continuous manifolds with elements and points as corresponding specialisations (Bestimmungsweisen) *(1953: 273–274)*. In essence the discrete manifolds are quantified by the integers. However, it is the continuous manifolds which are of prime interest to him. But in trying to capture their essential nature he gets mixed up with philosophical terms and outmoded scholastic methods of concept analysis. Perhaps it is best to say that his discussion is just a reflection of his poor philosophical training at Göttingen and the pernicious influence of HERBART. His distinctions do not seem to work.[38]

RIEMANN suggests that in most examples manifolds are measured quantities, *i.e.*, magnitudes. In this case measure consists in the superposition of the magnitudes to be compared. However, there are important examples when this method does not apply. He says *(1953: 274)*:

> Fehlt dieses, so kann man zwei Grössen nur vergleichen, wenn die eine ein Theil der andern ist, und auch dann nur das Mehr oder Minder, nicht das Wieviel entscheiden. Die Untersuchungen, welche sich in diesem Falle über sie anstellen lassen, bilden einen allgemeinen von Massbestimmungen unabhängigen Theil der Grössenlehre, wo die Grössen nicht als unabhängig von der Lage existirend und nicht als durch eine Einheit ausdrückbar, sondern als Gebiete in einer Mannigfaltigkeit betrachtet werden.

> (In the absence of this, two magnitudes can only be compared when one is a part of the other; in which case also we can only determine the more or less and not the how much. The researches which can in this case be instituted about them form a general part of the science of magnitude in which magnitudes are regarded not as existing independently of position and not as expressible in terms of a unit, but as regions in a manifold.)

This is the main passage in which RIEMANN points to the science of analysis situs. It is very similar to the one quoted above from his work of 1857 on Abelian functions. It is clear that he is referring to topological manifolds, because he immediately cites the cases of multi-valued analytic functions and problems related to complex analysis. The RIEMANN surfaces are uppermost in

[38] *Cf.* especially the critiques of STALLO *(1960: 259–279)* and RUSSELL *(1897: 13–16, 63–70)*.

his mind. Although he uses the term 'Grösse', it is apparent that he does not mean 'magnitude' in the usual measured sense, but rather in the sense of general and not necessarily metrical extension. Perhaps he could have used GRASSMANN's term 'Ausdehnung', but given it a broader connotation.

RIEMANN chooses to examine only two aspects of his general topological manifolds. He offers a construction method for producing multi-dimensional manifolds and a reduction method for determining points in such manifolds by sets of quantities, *i.e.*, real number coordinates.

RIEMANN's construction method is hardly more than the ancient fluxion theory extended to n dimensions *(1953: 275)*. His description has something of the form of an inductive definition. The true character of a simply extended or 1-dimensional manifold (eine einfach ausgedehnte Mannigfaltigkeit) is that continuous progress (motion; stetiger Fortgang) is only possible in two directions, forwards and backwards (terms borrowed from GAUSS). If we suppose that an entire simply extended manifold passes over into a series of such manifolds in point-to-point correspondence, then we obtain a doubly extended (2-dimensional) manifold. This construction is exactly analogous to the ancient idea that a continuously moving line produces a surface. In general we can continue the process to give an n-fold or n-dimensional manifold. RIEMANN concludes *(1953: 275)*:

> Wenn man, anstatt den Begriff als bestimmbar, seinen Gegenstand als veränderlich betrachtet, so kann diese Construction bezeichnet werden als eine Zusammensetzung einer Veränderlichkeit von $n+1$ Dimensionen aus einer Veränderlichkeit von n Dimensionen und aus einer Veränderlichkeit von Einer Dimension.
>
> (If instead of considering the concept as determinable, one considers its objects as a variable, then this construction can be described as a composition of a variable of $n+1$ dimensions from a variable of n dimensions and a variable of one dimension.)

Judging by this passage, one will probably conclude that RIEMANN has only succeeded in making the simple and intuitive fluxion theory quite opaque. His philosophical expressions do not add much clarity to his thoughts. Yet perhaps we can sympathise with his struggle to make his topological objects abstract.

In order to show how to reduce the positions in an n-dimensional manifold to the determination of n real quantities, RIEMANN reverses the construction process *(1953: 275–276)*. He assumes that there is a 1-dimensional manifold which we can use as a device to determine the positions of points in a given n-dimensional manifold: a 1-dimensional measuring stick, so to speak. A point on the 1-manifold is taken as the origin; other points on it are assigned appropriate distances from the origin. Corresponding to any given point on the 1-manifold, we assign a whole set of points in the n-manifold in such a way that different points on the 1-manifold correspond to distinct submanifolds of the n-manifold. Expressing this idea in the language of function theory, RIEMANN says that we take a continuous function of positions within the n-manifold to values on the 1-manifold, assuming that the function is not constant over an entire (n-dimensional?)[39] region of the manifold. Accordingly every system of points for which the function has a constant value forms a continuous submanifold of fewer than n dimensions, in fact, of $n-1$ dimensions. As the function value on the 1-manifold changes these $(n-1)$-dimensional manifolds pass over continuously into one another. Hence, we may think of one of them as the original $(n-1)$-manifold from which the others proceed, such that each point on the original manifold corresponds (in a one-one onto manner) to a distinct point on each image manifold. This correspondence is part of the construction process. RIEMANN casually mentions possible exceptional cases, *i.e.*, singularities in the correspondence; he is apparently thinking of branch points on a RIEMANN surface. In fact, most of his ideas about the functional reduction of manifolds can be related to complex, or even algebraic, functions.

The process just described shows that we can reduce the determination of a position in an n-manifold to the determination of a number and an $(n-1)$-manifold. Continuing the process, we see that a point in an n-manifold can normally be determined by n real numbers: $x_1, x_2, \ldots x_n$. However, RIEMANN asserts that sometimes n steps/numbers will not suffice to determine a point in a manifold. Here we have infinite-dimensional manifolds, and he mentions function spaces as examples!

In brief RIEMANN characterises an n-dimensional manifold as a 1-parameter family of $(n-1)$-dimensional manifolds. His manifolds are Euclidean in their smallest parts and even globally Euclidean (apart from possible singularities which he decides to ignore in his lecture). He assumes differentiability in the later sections of the *Habilitationsvortrag*, so apparently Riemannian manifolds are intended to be differentiable manifolds.[40]

Certainly RIEMANN sought a manifold concept of great generality. Nevertheless, his all-too-brief informal explanations are fraught with several difficulties. For example, what are we to understand by 'continuity' or the implicit use of motion in his abstract characterisation? He does not tell us. One may suppose that these concepts are to have the informal meanings current around 1850. Not surprisingly later mathematicians following the critical principles of the arithmetisation programme, especially as laid down by WEIERSTRASS, found it necessary to probe more deeply into these vague points in RIEMANN's lecture.

On a very profound level there is a serious problem related to RIEMANN's entire approach. Ultimately he links his basic topological objects with numbers and coordinate systems. In other words, a Riemannian manifold is always reducible to a 'number-manifold' ('Zahlenmannigfaltigkeit', a term often used by LIE and KLEIN). Consequently, when trying to construct a framework for *nonmetrical* analysis situs, he nonetheless seems to fall back on concepts of measurement and ordinary analytic geometry. In this way he appears to complete a logical circle. If the coordinates of the points in a manifold are given by the usual Euclidean distance measure, then a specific metric is already

[39] Presumably the region must be n-dimensional or, as we might say, it must contain an open set.

[40] ENRIQUES *(1898)* attempts to reconstruct RIEMANN's theory of manifolds and dimension.

assumed for the supposedly nonmetrical structure. How can he then go on to consider different possible measure-relations in the second section of his lecture? At the heart of this logical problem is a deep issue in the foundations of geometry of which RIEMANN's later followers and critics became acutely aware: for example, FELIX KLEIN *(1897)=(1921: 388–389)* and BERTRAND RUSSELL *(1897: 30–33)*.[41] Towards the end of the nineteenth century mathematicians discerned a directly related problem in the case of projective geometry, ostensibly nonmetrical. They wondered: Is it really permissible to use a coordinate system, normally defined by a metric, to study this geometry in a purely projective way? KLEIN pointed the way to a solution avoiding the vicious circle, through VON STAUDT's quadrilateral construction. By means of this construction we can avoid the usual metric. We can coordinatise the structures investigated in projective geometry in such a way that their points are consistently assigned numbers by convention without bringing in a metric.[42] By analogy what Riemannian manifolds need is a method for arbitrarily assigning coordinates consistent with the topologies of their structures. In his work RIEMANN gives no indication of such a method, although he most probably thought of his coordinates as arbitrary.[43] They constitute parametrisations of the manifolds. Still, the solution by arbitrary coordinates is only a partial one. What we find conspicuously lacking in RIEMANN's work is the notion of a topological mapping. For modern mathematicians topology is inseparable from homeomorphisms. RIEMANN never contemplated these in his programme of analysis situs.

In a broader context mathematicians working towards the end of the nineteenth century began to feel the need for a deeper analysis of the foundations of nonmetrical geometry, in particular, of topology. KLEIN especially saw the logical difficulty in basing geometry on number-manifolds and pressed for a solution *(1897)=(1921: 388–389)*.[44] In effect this difficulty was the immediate motivation for the development of the concept of a topological space by HILBERT *(1903)*, WEYL *(1913)*, and HAUSDORFF *(1914)*. With respect to RIEMANN and his much earlier work, we must conclude that he did not found the subject of general topology, but only groped towards it.[45]

In spite of the multiplicity of obscure points in RIEMANN's entire *Habilitationsvortrag*, the work became quite significant in the development of geometrical thought after it was published in 1868. It was primarily HERMANN VON HELMHOLTZ (1821–1894) who made mathematicians and philosophers aware of RIEMANN's brilliant ideas. Since about 1866 HELMHOLTZ had been working on a philosophico-geometrical problem closely related to RIEMANN's,

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[41] *Cf.* LIE/ENGEL *(1893: 394–395, 486)*.

[42] *E.g., cf.* RUSSELL *(1897: 118–119, 123–126)* for an exposition of this solution to the problem.

[43] Perhaps he had in mind a generalisation of GAUSS' 'arbitrary' parametric coordinates for surfaces as presented in the 'Disquisitiones generales circa superficies curvas'.

[44] *Cf.* LIE/ENGEL *(1893: 535–537)*.

[45] Thus RIEMANN's main achievements in the *Habilitationsvortrag* really lie in the second and third sections. His ideas on differential geometry are especially important and soon after publication of the work other mathematicians began investigating them further.

but derived from a different source.[46] When reading accounts of RIEMANN's work in two obituaries by ERNST SCHERING, HELMHOLTZ was surprised – and dismayed – to find that RIEMANN had previously explored fundamental questions about space and the geometrical axioms. He quickly obtained a copy of the *Habilitationsvortrag* from SCHERING. Shortly thereafter he presented a paper to the Naturhistorisch-medicinischer Verein at Heidelberg on 22 May 1868 *(1868–69)* and he sent a further paper to the Königliche Gesellschaft der Wissenschaften at Göttingen, which was presented on the third of June. The second paper, 'Über die Thatsachen, die der Geometrie zum Grunde liegen' *(1868a)*, is his most important on the subject.

In very general terms RIEMANN and HELMHOLTZ had attacked the same problem, the so-called RIEMANN-HELMHOLTZ (afterwards RIEMANN-HELMHOLTZ-LIE) space problem. However, closer analysis shows that RIEMANN had started from a far more general position, a position which we would characterise as partly topological. Indeed HELMHOLTZ' much more widely-read and influential papers tended to conceal the deeper springs of RIEMANN's thought.[47] Because of HELMHOLTZ' greater influence virtually all nineteenth-century thinkers lost sight of RIEMANN's general topological basis. In his second paper of 1868 HELMHOLTZ simply reduces RIEMANN's discussion of n-dimensional manifolds to a succinct assumption or axiom which declares that n real-valued continuous coordinates determine the points in such manifolds *(1868a: 197–198)*. Anyone reading RIEMANN and HELMHOLTZ on manifolds must regard the latter's account as simpler and clearer.

After the burst of activity on higher-dimensional geometry of the 1840's and '50's numerous mathematicians proceeded with further developments.[48] They investigated hyperspaces both from an analytical and more purely geometrical direction. Most attention was given to n-spaces of the Euclidean linear type, 'homoloidal spaces', like GRASSMANN's domains. Hence during the final third of the nineteenth century hyperspaces became a commonplace in mathematical works. Following GAUSS and RIEMANN many employed the term 'Mannigfaltigkeit' ('manifold', and in French, 'variété'), but giving it a great diversity of meanings. In his *Erlanger Programm* FELIX KLEIN (1849–1925) prominently uses the term to refer to the basic abstract spaces underlying his group-theoretic definitions of geometries *(1872: §10, Note IV)*. Mappings or transformations are also, of course, a feature of KLEIN's approach to geometry, something which was lacking in RIEMANN's analysis situs. KARL WEIERSTRASS (1815–1897) commonly used the term 'Mannigfaltigkeit' in his lectures on multi-variable analysis *(1927: 55–60)* (*cf.* PINCHERLE *(1880)*). He treated his n-fold manifolds as arithmetical continua (Euclidean spaces) founded on a logical theory of the real numbers and he introduced basic analytical concepts such as limit point, continuum, and interior in order to develop analysis rigorously. In this work lies

[46] HELMHOLTZ' source was the physio-psychological problem of spatial perceptions *(1868a: 193)*. On HELMHOLTZ' life and work *cf.* KOENIGSBERGER *(1902–03) (1906)*; see especially *(1906: 254–266)*.

[47] *Cf.* WEYL *(1923)* and FREUDENTHAL *(1960)*.

[48] *Cf.* SEGRE *(1921)*.

the true origin of general or analytic topology. CANTOR, taking his cue from WEIERSTRASS, then employed the term 'Mannigfaltigkeit' and the related topological concepts. However, in several of his important contributions the term merely means an arbitrary set of numbers or points; it can even mean just an abstract set (Menge). It was only at the very end of the nineteenth century and then in the first years of the twentieth that a few mathematicians started to propose rigorous definitions of the *topological concept of manifold.* WALTER VON DYCK, HENRI POINCARÉ, HERMANN WEYL, and L. E. J. BROUWER were the principal creators of this formal development. Nevertheless, there is some historical justice in linking RIEMANN's earlier 'primitive' ideas with the definitions of these later mathematicians.

The publications of RIEMANN and especially HELMHOLTZ initiated a wide-ranging philosophical discussion of the nature of space and the foundations of geometry including its non-Euclidean and multi-dimensional branches. In the 1870's and '80's the Germans were the most active participants in this discussion, while in the 1890's the French took the leading role with BERTRAND RUSSELL as a misguided English contributor.[49] From the beginning philosophers strongly criticised RIEMANN's entire way of proceeding and implicit philosophy, especially his starting point: coordinatised manifolds. The whole gamut of philosophical positions from empiricism to idealism, in particular, Kantianism, was represented in the discussion. HELMHOLTZ was the principal empiricist; his student, BENNO ERDMANN, expanded upon his philosophy at great length in the book *Die Axiome der Geometrie (1877).* Apart from a certain amount of almost inevitable polemizing, on the whole the standard of argument in this discussion was high.

The rapid developments in geometry forced philosophers to see the science in a new light. It became clear that many significant branches of geometry had to be separated from the notion of space. Physical space could no longer be the ultimate foundation for the entire discipline. Philosophers and mathematicians thus spoke of 'metageometry' and 'géométrie générale'. Geometry presented a more abstract face. To be sure, the geometry of our space still remained uppermost in the minds of many philosophers. If ERDMANN or RUSSELL discussed the axioms of geometry, in the final analysis they were concerned with those assumptions which characterise that 3-fold manifold which is our space. The more radical split between pure geometry as an uninterpreted deductive system and applied geometry as a way of modelling physical space did not become fully understood until after the later work of PASCH and the Italian school and the publication of HILBERT's *Grundlagen der Geometrie* (1899).

The approach to higher space through coordinate manifolds and analytical methods begun by geometers of the mid-nineteenth century presents us with a

[49] RUSSELL *(1897: 54–116)* gives a good critical review of philosophies of geometry from KANT to 1897. ERDMANN *(1877: 17–33)* also presents a review, covering a shorter period of time. For references to the vast philosophical literature concerning the problems of geometry and space that was generated principally by the publications of RIEMANN and HELMHOLTZ and also the non-Euclidean geometers consult the bibliographies of HALSTEAD *(1878–79)* and SOMMERVILLE *(1911).*

definite historical indicator of the developing cleavage between geometry and space. Abstract spaces took on a fundamental significance; 'space' became arithmetised. This development in geometry is part of the trend of nineteenth-century arithmetisation (*cf.* KLEIN *(1895)=(1922:232–240)*). The application of the new rigorous theories of real numbers (proposed around 1870) then brought about a synthesis between the programmes of arithmetisation in analysis and in geometry. Geometrico-mechanical continuity based on the intuitive idea of motion was thereby replaced by analytically-defined continuity. With WEIERSTRASS and CANTOR multi-dimensional coordinate manifolds became fully arithmetised.

Nearer the end of the century, however, a trend contrary to the analytic one developed. Several mathematicians argued that geometry ought to be constructed as a *pure* autonomous science and not on the basis of number-manifolds. This is the axiomatic-deductive trend. From it arose the criticism of RIEMANN, HELMHOLTZ, and LIE for basing geometry on *Zahlenmannigfaltigkeiten*, because ultimately such a use appears to complete a vicious circle. Hence, mathematicians initiated a deeper analysis of the foundations of geometry which had important consequences for the subsequent development of a genuinely nonmetrical topology.[50] RIEMANN (and others) had tried to develop this subject, but without success. Only in the twentieth century could this subject grow according to its natural principles.

To conclude this chapter let me turn specifically to the impact of the proposal and growth of analytical theories of hyperspaces and n-dimensional manifolds on ideas of dimension itself. Without doubt the growth of these theories made the dimension concept more easily comprehensible. Implicit in them is what we might appropriately call the coordinate idea of dimension. This is the simple, naive idea that the dimension of a space is merely the unique number of coordinates needed to determine a point in that space. Strictly speaking, neither GRASSMANN, nor RIEMANN, nor HELMHOLTZ ever espoused this theory of dimension, for they associated continuity and even differentiability with their abstract spaces. Yet it must be admitted that they were not very definite about their continuity and differentiability assumptions; GRASSMANN and RIEMANN really thought of continuity in terms of the ancient fluxion theory. However, when GEORG CANTOR probed deeper into the coordinate idea of dimension and discovered his paradoxical result showing that the simple idea is untenable, he forced mathematicians to take a fresh look at dimension. CANTOR's work could only have taken place in the context of the development of arithmetised hyperspaces. His discovery, the subject of the next chapter, is inconceivable without the prior growth of such ideas.

[50] This deeper analysis also affected the development of LIE group theory. Alongside the deeper analysis leading to nonmetrical topology some mathematicians developed metrical ideas related to topology further. Thus work in the calculus of variations and functional spaces motivated FRÉCHET to put forward a theory of (E) classes or metric spaces (the latter being HAUSDORFF's term) *(1906)*.

Chapter 2. Georg Cantor's 'Paradox' of Dimension

In 1877 GEORG CANTOR (1845–1918)[1] discovered to his own amazement that the points of a unit line segment can be put into one-one correspondence with the points of a unit square or more generally with the points of a ρ-dimensional cube. This counterintuitive result immediately called into question the concept of dimension. Was it well-defined or even meaningful in mathematics? CANTOR's discovery marks the origin of the most important problem of dimension, the problem of its invariance. Is dimension invariant with respect to any class of mappings? This problem drove mathematicians to conduct a search for an invariance proof for dimension, a search which lasted for over three decades. Without question CANTOR's discovery and the closely linked invariance problem together have been the single most important cause for the growth of modern topological dimension theory. In this chapter we shall see how CANTOR arrived at his surprising result and examine the implications of his find.[2]

CANTOR's work on set theory arose out of his investigations into the uniqueness of representing a function by a trigonometric series.[3] In 1874 he published his first purely set-theoretic paper, 'Über eine Eigenschaft des Inbegriffes aller reellen algebraischen Zahlen' *(1874)*, in which there are proofs that the set of real algebraic numbers can be conceived in the form of an infinite sequence

$$\omega_1, \omega_2, \ldots, \omega_\nu, \ldots$$

(*i.e.*, the set is countable, abzählbar, to use CANTOR's later term), while the set of *all* real numbers cannot be given in the form of an infinite sequence (*i.e.*, the set is uncountable, unabzählbar).[4] In fact, he proved the latter result in the stronger form: for every sequence of real numbers and every interval of the real line a number can be determined in the interval which is not in the given sequence. Through the medium of these results he saw a clear distinction between two types of infinite sets of numbers on the real line. It was then perfectly natural for him to wonder whether there are other types of infinite sets, say for the plane or for higher-dimensional spaces.

CANTOR discovered the results of his paper of 1874 late in 1873, as his correspondence with RICHARD DEDEKIND (1831–1916)[5] clearly shows. The letters from CANTOR to DEDEKIND between 29 November and 27 December reveal just how quickly he wrapped up his investigations (CANTOR/DEDEKIND *(1937: 12–20)*).

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[1] The standard biography of CANTOR is FRAENKEL *(1930)*. *Cf.* MESCHKOWSKI *(1967)* and GRATTAN-GUINNESS *(1971)*. I understand that Prof. J. W. DAUBEN intends to publish or has recently published a biography of CANTOR, although I have not yet seen it.

[2] The material in this chapter has also been covered by DAUBEN *(1974)*. However, my approach is somewhat different and I believe I have added some new insights. To be sure, the story of CANTOR's paradoxical discovery is a crucial part of the origins of topological dimension theory and must be included in a full history.

[3] *Cf.* DAUBEN *(1971)*.

[4] CANTOR's term 'abzählbar' first appears in his *(1882)*.

[5] For biographical material on DEDEKIND *cf.* LANDAU *(1917)* and DUGAC *(1976)*.

CANTOR had met DEDEKIND by chance in Gersau during a trip to Switzerland in 1872 (FRAENKEL *(1930: 196)*), and the famous exchange of letters which ensued[6] has become an invaluable source of information about the motivation for the mathematical work of these two men. One can almost see CANTOR in the act of creating set theory and topology through his letters to DEDEKIND.

Having investigated the power or cardinality[7] of two important linear sets, the real algebraic numbers and the real numbers, he turned to an examination of higher-dimensional sets. In a letter to DEDEKIND dated 5 January 1874 he posed a new research question (CANTOR/DEDEKIND *(1937: 20)*):

> Lässt sich eine Fläche (etwa ein Quadrat mit Einschluss der Begrenzung) eindeutig auf eine Linie (etwa eine gerade Strecke mit Einschluss der Endpunkte) eindeutig beziehen, so dass zu jedem Puncte der Fläche ein Punct der Linie und umgekehrt zu jedem Puncte der Linie ein Punct der Fläche gehört?
>
> (Can a surface (perhaps a square including its boundary) be put into one-one correspondence with a line (perhaps a straight line segment including its endpoints) so that to each point of the surface there corresponds a point of the line and conversely to each point of the line there corresponds a point of the surface?)

This problem is basic to the growth of dimension theory. From the start CANTOR was convinced of the importance and difficulty of his research question. He realised that some, indeed most, would regard a negative answer to it as so obvious that a proof would hardly be necessary. When he discussed it with a friend in Berlin some time during the first part of 1874, the friend explained that the matter was absurd, so to speak (CANTOR/DEDEKIND *(1937: 21)*):

> ... da es sich von selbst verstünde, dass zwei unabhängige Veränderliche sich nicht auf eine zurückführen lassen.
>
> (... since it is obvious that two independent variables cannot be reduced to one.)

In relating this encounter in a letter of 18 May 1874, the young CANTOR sought DEDEKIND's reassurance that he was not chasing after a delusion! However, we know that he was not deceived. Even in posing his research question he introduced something quite new and important into thinking about dimension. *He related mappings and correspondences to the dimension of figures and spaces.* For CANTOR this was a natural relation because he was interested in cardinality. In this way he was led to a novel treatment of the dimension concept, having surprising consequences.

We have no record of DEDEKIND's initial reaction to CANTOR's research

[6] The correspondence has recently been rediscovered; *cf.* KIMBERLING *(1972)*, GRATTAN-GUINNESS *(1974)*, DUGAC *(1976)*.

[7] CANTOR first began to use the term 'Mächtigkeit' in a letter to DEDEKIND of 20 June 1877 (CANTOR/DEDEKIND *(1937: 25)*) and then in his *(1878)*.

problem and, moreover, no written record of exactly how CANTOR dealt with it from May 1874 until April 1877. No letters concerning the matter have come to light covering this period; probably nothing on the subject was written. It is likely that he only worked intermittently on his research problem from mid-1874 to mid-1877 and indeed without success. However, he persisted in regarding it as important. For example, when he attended the *Gaussjubiläum* in Göttingen on 30 April 1877, the one-hundredth anniversary of GAUSS' birth, he told various colleagues (among them HEINRICH WEBER and RUDOLPH LIPSCHITZ) of his problem, which he felt was fundamental to geometry. Again most of these colleagues thought the answer was obvious; a one-one correspondence between geometrical figures of differing dimensions is not possible. Still CANTOR felt that a *proof* was needed.

Subsequent to the GAUSS Jubilee in April CANTOR switched his line of attack, employing the usual strategy of the mathematician which advises that if a proof of a conjecture seems difficult to find then try to find a counterexample. In a letter to DEDEKIND of 20 June 1877 there is a positive solution to the problem! In this letter he first states his solution in geometrical terms, namely (CANTOR/DEDEKIND *(1937: 25)*):

> ... dass Flächen, Körper, ja selbst stetige Gebilde von ρ Dimensionen sich eindeutig zuordnen lassen stetigen Linien, also Gebilden von nur *einer* Dimension, dass also Flächen, Körper, ja sogar Gebilde von ρ Dimensionen, dieselbe *Mächtigkeit* haben, wie Curven ....
>
> (... that surfaces, solids, even continuous figures of ρ dimensions can be put into one-one correspondence with continuous lines, thus figures of only one dimension; therefore, that surfaces, solids, even figures of ρ dimensions have the same power as curves ....)

Immediately CANTOR saw his result on the equal power or cardinality of sets of various dimensions as a criticism of some assumptions about dimension commonly held by geometers of his time. They casually spoke of simply infinite, twofold, threefold, ... ρ-fold infinite figures; they even regarded the infinity of points of a surface as the square of the infinity of points of a line or the infinity of points of a solid as the cube of the infinite set of points of the line. Such thoughts were prevalent at the time. However, CANTOR changed the situation entirely. He injected his new ideas about mappings and correspondences into thinking about dimension. Consequently, his proposed proof of equal power was to be an attack on the foundations of geometry, on the very concept of dimension which geometers were using uncritically.

However, in the letter of June 20th CANTOR presents a demonstration which is arithmetical, not geometrical, in character. First of all, he claims that all (connected) figures of the same dimension number can be mapped *analytically* onto one another. By 'figures' he seems to mean just a variety of relatively simple geometrical ones, for otherwise the claim would amount to a very broad generalisation of the 2-dimensional RIEMANN mapping theorem. But surely he does not intend to make such a generalisation, for he must have been well aware of the virtual impossibility of proving such an extreme—and false—generalisation.[8]

Yet CANTOR is really interested in figures as sets of points and of different dimensions. He tries to show that systems of values $(x_1, x_2, \ldots x_\rho)$, where $0 \leqq x_i \leqq 1$, can be put into one-one correspondence with values of a variable y, $0 \leqq y \leqq 1$. We might state this more simply by saying that the aim is to show that the points of a unit ρ-dimensional cube can be put into one-one correspondence with the points of a unit line segment. CANTOR's technique of proof rests on his assertion that every number x, $0 \leqq x \leqq 1$, can be represented uniquely in the form of an *infinite* decimal expansion:

$$x = \alpha_1 \frac{1}{10} + \alpha_2 \frac{1}{10^2} + \ldots + \alpha_\nu \frac{1}{10^\nu} + \cdots,$$

where the α's are digits, $0, \ldots 9$. Using simple decimal expansions instead of writing CANTOR's equivalent decimal series, we can write ρ decimal values for each point of the ρ-cube:

$$\begin{aligned} x_1 &= 0.\alpha_{11}\alpha_{12} \ldots \alpha_{1\nu} \ldots, \\ x_2 &= 0.\alpha_{21}\alpha_{22} \ldots \alpha_{2\nu} \ldots, \\ &\ldots\ldots\ldots\ldots\ldots\ldots \\ x_\rho &= 0.\alpha_{\rho 1}\alpha_{\rho 2} \ldots \alpha_{\rho\nu} \ldots \end{aligned}$$

and a single decimal for each value of y:

$$y = 0.\beta_1 \beta_2 \ldots \beta_\nu \ldots.$$

Now the x's and the y can be put into correspondence according to the following equations:

$$\begin{aligned} &\beta_{(n-1)\rho+1} = \alpha_{1n};\ \beta_{(n-1)\rho+2} = \alpha_{2n};\ \ldots \\ &\beta_{(n-1)\rho+\sigma} = \alpha_{\sigma n};\ \ldots\ \beta_{(n-1)\rho+\rho} = \alpha_{\rho n}. \end{aligned}$$

We have a kind of 'interlacing' of the x decimals to yield a y number so that:

$$y = 0.\alpha_{11}\alpha_{21} \ldots \alpha_{\rho 1}\alpha_{12}\alpha_{22} \ldots \alpha_{\rho 2}\alpha_{13}\alpha_{23} \ldots \alpha_{\rho 3} \ldots.$$

Conversely, a y number can be 'unlaced' to give x numbers.

DEDEKIND quickly responded in a letter of June 22nd with an objection to CANTOR's proof. First DEDEKIND assumed that CANTOR, by requiring *infinite* decimal expansions, wished to exclude decimal numbers with trailing 0's. Of course, these can always be replaced with trailing 9's (with the single exception of the number 0 itself). For example, the finite decimal

$$0.3000 \ldots = 0.3$$

[8] The DIRICHLET Principle, which RIEMANN used in his proof of the conformal mapping theorem *(1953: 40–42)*, was severely criticised by WEIERSTRASS *(1870)* (which was not printed until 1895). Around 1870 H. A. SCHWARZ and C. NEUMANN then tried to prove the mapping theorem without the DIRICHLET Principle. CANTOR must have known about these important mathematical developments, so that it is highly improbable that he would wish to generalise the difficult theorem without any proof.

can be replaced by the infinite decimal

$$0.2999\ldots.$$

With this assumption DEDEKIND's objection amounts to the following. Take the simplest case when we lace up two real numbers, x and y:

$$x = 0.\alpha_1\alpha_2\ldots\alpha_\nu\ldots,$$
$$y = 0.\beta_1\beta_2\ldots\beta_\nu\ldots$$

to derive a third number z:

$$z = 0.\gamma_1\gamma_2\ldots\gamma_\nu\ldots,$$

where

$$\gamma_1 = \alpha_1,\ \gamma_2 = \beta_1,\ \gamma_3 = \alpha_2,\ \gamma_4 = \beta_2, \ldots \gamma_{2\nu-1} = \alpha_\nu,\ \gamma_{2\nu} = \beta_\nu \ldots.$$

Now if we take a specific z number:

$$0.478310507090\alpha_7 0\alpha_8 0\alpha_9 0\ldots\alpha_\nu 0\ldots,$$

then the unlacing process will yield a y number,

$$y = 0.730000\ldots,$$

which does not have the required infinite decimal form. Moreover, the infinite decimal form for this y number does not correspond to the specified z number. In general, there are infintely many of these z numbers which yield inadmissible x or y decimal numbers. DEDEKIND concluded this letter with the sentence (CANTOR/DEDEKIND *(1937:28)*):

> Ich weiss nicht, ob mein Einwurf von wesentlicher Bedeutung für Ihre Idee ist, doch wollte ich ihn nicht zurückhalten.

> (I do not know whether my objection is of essential significance for your idea; however, I did not want to hold it back from you.)

CANTOR's reply was swift. In a card postmarked 23.6.77, the day after DEDEKIND wrote his objection, he accepted the criticism as a criticism of the proof but not of the theorem itself. CANTOR felt his result could be salvaged. His immediate reaction was to claim that he had 'proved' more than he intended. As given, his interlacing process brings a system of real variables $x_1, x_2, \ldots x_\rho$, with $0 \leqq x_i \leqq 1$, into correspondence with a variable y. Now the variable y does not take on all values in the unit interval, but only certain values y' which exclude those suggested in DEDEKIND's objection, *viz.*, those with certain patterns of 0's. In his postcard CANTOR suggests that to fix up his proof all that is needed is to show how to bring the y' into one-one correspondence with another variable t which takes on *all* real values between 0 and 1.

As it turned out, CANTOR was not immediately able to repair his proof as suggested in his postcard.[9] Instead in a letter of just two days later

[9] In reality it is quite easy to fix up CANTOR's first proof. We merely block 0's with nonzero digits to eliminate the difficulty which DEDEKIND pointed out. Thus we block the example of DEDEKIND as follows: 0.47831|05|07|09 .... We then treat blocks as single digits for interlacing and unlacing.

(25 June 1877) he presented DEDEKIND with a completely new proof, which, though overcoming DEDEKIND's objection, is not nearly as simple as the earlier proof. He must have been anxious to see his research problem through to an acceptable conclusion. In the letter he puts his theorem as follows (CANTOR/DEDEKIND *(1937:29)*):

(A) „Eine nach ρ Dimensionen ausgedehnte stetige Mannigfaltigkeit lässt sich eindeutig einer stetigen Mannigfaltigkeit von einer Dimension zuordnen, oder: (was nur eine andere Form desselben Satzes ist) die Puncte (Elemente) einer nach ρ Dimensionen ausgedehnten Mannigfaltigkeit lassen sich durch eine reelle Coordinate t so bestimmen, dass zu jedem reellen Werth von t im Intervalle $(0 \ldots 1)$ ein Punct der Mannigfaltigkeit, aber auch umgekehrt zu jedem Puncte der M. ein bestimmter Werth von t im Intervalle $(0 \ldots 1)$ gehört."

((A) A continuous manifold extended in ρ dimensions can be put into one-one correspondence with a continuous manifold of one dimension, or (what is only the same theorem in a different guise) the points (elements) of a manifold extended in ρ dimensions can be determined by one real coordinate t so that to every real value of t in the [closed] interval $(0 \ldots 1)$ there corresponds a point of the manifold and also conversely to every point of the manifold there is a definite value of t in the [closed] interval $(0 \ldots 1)$.)

To prove the theorem CANTOR begins by using the fact that every irrational number e between 0 and 1 can be represented by a unique infinite continued fraction:

$$e=\cfrac{1}{\alpha_1+\cfrac{1}{\alpha_2+\ddots+\cfrac{1}{\alpha_\nu+\ldots}}}\qquad=(\alpha_1,\alpha_2,\ldots,\alpha_\nu,\ldots),$$

where the α's are positive integers. Since we do not have the difficulty with 0 as with decimal numbers, it is now possible to salvage the interlacing argument. Thus if we have ρ irrational numbers between 0 and 1, represented by continued fractions:

$$\begin{aligned} e_1&=(\alpha_{11},\alpha_{12},\ldots,\alpha_{1\nu},\ldots),\\ e_2&=(\alpha_{21},\alpha_{22},\ldots,\alpha_{2\nu},\ldots),\\ &\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\ e_\rho&=(\alpha_{\rho 1},\alpha_{\rho 2},\ldots,\alpha_{\rho\nu},\ldots),\end{aligned}$$

then we can interlace these to derive another irrational number ∂, $0<\partial<1$:

$$\partial=(\beta_1,\beta_2,\ldots,\beta_\nu,\ldots),$$

according to the equations:

$$\beta_{(n-1)\rho+1}=\alpha_{1n},\ldots\beta_{(n-1)\rho+\sigma}=\alpha_{\sigma n},\ldots\beta_{n\rho}=\alpha_{\rho n}.$$

Consequently, ρ irrational numbers can be interlaced to make one irrational number and, conversely, one irrational number can be unlaced to give ρ irrationals.

The problem of giving a correspondence between points of the unit segment and points of the ρ-cube is now driven back to the problem of showing that there is a one-one correspondence between the set of irrational numbers in the unit interval and the set of *all* numbers in that interval, *i.e.*, the problem of proving (CANTOR/DEDEKIND *(1937:30)*):

> (B) „Eine veränderliche Zahl e, welche alle *irrationalen* Zahlenwerthe des Intervalles (0 ... 1) annehmen kann, lässt sich *eindeutig* einer Zahl x, welche *alle* Werthe dieses Intervalles ohne Ausnahme erhält, zuordnen."

> ((B) A variable number e which can take all irrational number values of the [closed] interval (0 ... 1) can be put in one-one correspondence with a number x which takes all values of this interval without exception.)

For us the proof of this theorem is not difficult, but for CANTOR, the first to explore the unknown territory of infinite sets, one-one correspondences, and cardinality, the proof did not come easily. The proof given in his letters to DEDEKIND of 25 and 29 June 1877 is far from simple. However, it does reveal to us how he first grappled with the problems of the cardinality of infinite sets.

To demonstrate (B) CANTOR begins by considering the rational numbers in the closed interval $[0,1]$[10] given in the form of a sequence:

$$r_1, r_2, \ldots, r_\nu, \ldots$$

and an arbitrarily chosen infinite sequence of irrational numbers ε_ν from the interval $[0, 1]$ which obey the conditions that $\varepsilon_\nu < \varepsilon_{\nu+1}$ (monotonically increasing) and $\lim \varepsilon_\nu = 1$. The sequence of rational numbers and the sequence of irrational numbers can easily be put into one-one correspondence in the obvious way: r_ν corresponds to ε_ν. On the basis of this correspondence we immediately have a correspondence between the real numbers of the unit interval minus the rationals (*i.e.*, the irrationals e) and the numbers of $[0, 1]$ minus the irrationals ε_ν and so theorem (B) is reduced to (CANTOR/DEDEKIND *(1937:31)*):

> (C) „Eine Zahl f, welche alle Werthe des Intervalles (0 ... 1) annehmen kann mit Ausnahme gewisser ε_ν, die an die Bedingungen gebunden sind: $\varepsilon_\nu < \varepsilon_{\nu+1}$ und $\lim \varepsilon_\nu = 1$ lässt sich einer stetigen Veränderlichen x eindeutig zuordnen, welche alle Werthe des Intervalles (0 ... 1) ohne Ausnahme erhält."

> ((C) A number f which can take all values of the interval (0 ... 1) with the exception of certain ε_ν, which are bound by the conditions $\varepsilon_\nu < \varepsilon_{\nu+1}$ and $\lim \varepsilon_\nu = 1$, can be put into one-one correspondence with a continuous variable x which takes all values of the interval (0 ... 1) without exception.)

---

[10] It is convenient to use the modern terminology and symbols, (x, y), $[x, y]$, $(x, y]$, $[x, y)$, for open, closed, and half-open intervals.

Reducing theorem (B) to (C) simplifies the task of finding a correspondence. The irrational numbers ε_ν just break up the unit interval $[0,1]$ into infinitely many subintervals. The first of these is half-open and the remaining are open intervals; additionally, there is the single point 1:

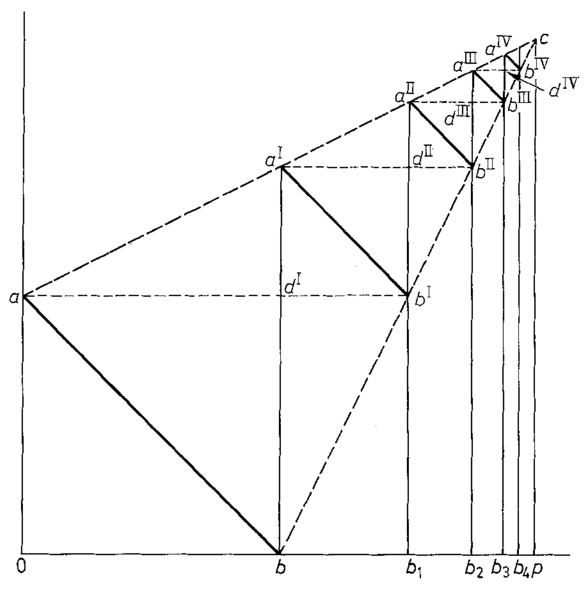
$$[0,\varepsilon_1), (\varepsilon_1,\varepsilon_2), \ldots (\varepsilon_\nu,\varepsilon_{\nu+1}), \ldots, 1.$$

These can be lined up with the complete closed interval $[0,1]$. Thus towards the end of his letter of 25 June CANTOR says the proof of (C) is to be carried out by successive applications of (CANTOR/DEDEKIND *(1937:32)*):

(D) „Eine Zahl y, welche alle Werthe des Intervalles $(0\ldots 1)$ mit alleiniger Ausnahme des Werthes 0 annehmen kann, lässt sich einer Zahl x eindeutig zuordnen, welche alle Werthe des Intervalles $(0\ldots 1)$ ohne Ausnahme erhält."

((D) A number y which can take all values of the interval $(0\ldots 1)$ with the single exception of the value 0 can be put into one-one correspondence with a number x which takes all values of the interval $(0\ldots 1)$ without exception.)

Hence we want to show that the half-open interval $(0,1]$ can be put in one-one correspondence with the closed interval $[0,1]$. CANTOR's proof of (D) proceeds by means of a complex diagram defining a curve (CANTOR/DEDEKIND *(1937:32)*):



The curve consists of the parallel segments $\overline{ab}, \overline{a'b'}, \overline{a''b''}, \ldots$, excluding the endpoints $b, b', b'', \ldots$, plus the point c. The lengths are defined as:

$$\overline{op}=\overline{pc}=1;\ \overline{ob}=\tfrac{1}{2};\ \overline{bb_1}=\tfrac{1}{4};\ \overline{b_1b_2}=\tfrac{1}{8};\ \overline{b_2b_3}=\tfrac{1}{16}\ldots;$$
$$\overline{oa}=\tfrac{1}{2};\ \overline{a'd'}=\tfrac{1}{4};\ \overline{a''d''}=\tfrac{1}{8};\ \overline{a'''d'''}=\tfrac{1}{16}\ldots.$$

Clearly the curve maps the closed unit interval [0, 1] on the x-axis onto the half-open unit interval (0, 1] on the y-axis.

The curve and theorem (D) do not quite complete the proof of (C), because, first, (D) needs to be generalised to intervals with arbitrary endpoints and, second, it is necessary to show the related result that an open interval can be put into correspondence with a closed interval. CANTOR soon realised the need to fill out his proof of (C) and sent a completion to DEDEKIND in a letter of 29 June. In this he makes heavy weather of the completion (CANTOR/DEDEKIND *(1937: 34–37)*).[11] Yet we must remember that in this work he was first coming to grips with correspondences and cardinal equivalences, a subject he was 'creating out of nothing'.

During the summer of 1877 CANTOR wrote out the results that he had communicated to DEDEKIND in a paper, 'Ein Beitrag zur Mannigfaltigkeitslehre' *(1878)*. In the course of writing this paper he discovered a much simpler proof of his main theorem (B) and told DEDEKIND of it in a letter of 23 October 1877. The proof of this theorem, that the irrational numbers in the unit interval (denoted by the variable e) can be put into one-one correspondence with all the numbers of the unit interval (variable x), or in CANTOR's notation:

$$e \sim x,$$

is simply carried out as follows (CANTOR/DEDEKIND *(1937: 40–41)*). Let ϕ_ν be the general member of the sequence of all rational numbers in [0, 1] and let η_ν be the general member of a sequence of irrational numbers in the same interval, *e.g.*,

$$\eta_\nu = \frac{\sqrt{2}}{2^\nu}.$$

Also let h represent all the other values of the interval [0, 1], except those represented by ϕ_ν and η_ν. Then:

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[11] Briefly the completion of (C) is accomplished as follows. First we can generalise theorem (D) to intervals with arbitrary endpoints in a trivial way by using linear substitutions (theorem (F), CANTOR/DEDEKIND *(1937: 35)*). Second we can prove the following theorem with the help of (F) by aligning appropriate intervals (CANTOR/DEDEKIND *(1937: 35)*):

> (G) „Eine Zahl w, welche alle Werthe des Intervalles $(\alpha \ldots \beta)$ mit Ausnahme der beiden Endwerthe α, β erhält, lässt sich einer veränderlichen Zahl u eindeutig zuordnen, die alle Werthe des Intervalles $(\alpha \ldots \beta)$ annimmt."
>
> ((G) A number w which takes all values of the interval $(\alpha \ldots \beta)$ with the exception of the two endpoints α, β can be put into one-one correspondence with a variable number u which takes all values of the interval $(\alpha \ldots \beta)$.)

Finally we can arrive at a one-one correspondence between all the intervals

$$[0, \varepsilon_1), (\varepsilon_1, \varepsilon_2), \ldots (\varepsilon_\nu, \varepsilon_{\nu+1}), \ldots, 1$$

and the entire closed interval [0, 1] by using theorem (G) to give correspondences between $(\varepsilon_1, \varepsilon_2)$ and $[\varepsilon_1, \varepsilon_2]$, $(\varepsilon_3, \varepsilon_4)$ and $[\varepsilon_3, \varepsilon_4], \ldots (\varepsilon_{2\nu-1}, \varepsilon_{2\nu})$ and $[\varepsilon_{2\nu-1}, \varepsilon_{2\nu}], \ldots$.

$$x \equiv \{h, \eta_\nu, \phi_\nu\},$$
$$e \equiv \{h, \eta_\nu\} \equiv \{h, \eta_{2\nu-1}, \eta_{2\nu}\}.$$

But

$$h \sim h; \quad \eta_\nu \sim \eta_{2\nu-1}; \quad \phi_\nu \sim \eta_{2\nu}.$$

Hence, it follows that

$$x \sim e.$$

What an elegant proof!

Having made the paradoxical discovery that 1- and ρ-dimensional figures can be put into one-one correspondence, CANTOR was quick to draw out some mathematical and philosophical consequences of his unexpected find. In his correspondence with DEDEKIND we get the first reactions of the two men to CANTOR's result. In the last paragraphs of his letter of 25 June 1877 (CANTOR/DEDEKIND *(1937:33–34)*) he remarks about the implications of his discovery. He says that he has followed with interest the efforts of GAUSS, RIEMANN, HELMHOLTZ, and others directed towards understanding the foundations of geometry, but his result has now made him doubt the validity of their work (CANTOR/DEDEKIND *(1937:33)*):

> Dabei fiel mir auf, dass alle in dieses Feld schlagenden Untersuchungen *ihrerseits* von einer unbewiesenen Voraussetzung ausgehen, die mir nicht als selbstverständlich, vielmehr einer Begründung bedürftig erschienen ist. Ich meine die Voraussetzung, dass eine ρ fach ausgedehnte stetige Mannigfaltigkeit zur Bestimmung ihrer Elemente ρ von einander unabhängiger reeller Coordinaten bedarf, dass diese Zahl der Coordinaten für eine und dieselbe Mannigfaltigkeit weder vergrössert noch verkleinert werden könne.

> (It strikes me that all investigations taken up in this field begin for their part from an unproved assumption which does not appear to me to be obvious, but rather seems to need a proof. I mean the assumption that a ρ-fold extended continuous manifold requires ρ independent real coordinates for the determination of its element and that this number of coordinates can be neither increased nor decreased for one and the same manifold.)

We have seen that at first he had thought that this assumption of geometers was correct, but he had differed from them in that he had thought it required a rigorous proof and so persisted in trying to find one. However, he was completely surprised when he finally found a counterexample: the assumption is false. A ρ-fold manifold can be 'coordinatised' by a single coordinate. He ascribed this strange result to the 'wonderful power in the usual real and irrational numbers'. Moreover, he recognised that his result can be easily extended from ρ-dimensional manifolds to infinite-dimensional manifolds, assuming that their infinitely many dimensions have the form of a simple infinite sequence (*i.e.*, that the dimension is countably infinite).

CANTOR's letter ends with the following paragraph (CANTOR/DEDEKIND *(1937:34)*):

> *Nun scheint es mir*, dass alle philosophischen oder mathematischen Deductionen, welche von jener irrthümlichen Voraussetzung Gebrauch machen, unzu-

lässig sind. Vielmehr wird der Unterschied, welcher zwischen Gebilden von *verschiedener* Dimensionszahlen liegt, in ganz anderen Momenten gesucht werden müssen, als in der für characteristisch gehaltenen Zahl der unabhängigen Coordinaten.

(Now it seems to me that all philosophical or mathematical deductions which make use of this mistaken assumption are inadmissible. Rather the distinction which exists between figures of different dimension numbers must be sought in entirely different aspects than in the number of independent coordinates, which is normally held to be characteristic.)

So he saw his result as a direct and devastating blow to the 'coordinate concept of dimension'. Another dimension concept is needed.

DEDEKIND responded to CANTOR's observations as well as to his proof, first in a postcard (now lost) and then more fully in a letter of the second of July. DEDEKIND had gone over the second proof and reported that he could find no mistakes. However, he was not able to agree with CANTOR's reading of the consequences of his theorem. In spite of the theorem or rather because of considerations occasioned by it, he still affirmed his conviction (CANTOR/DEDEKIND *(1937:37)*):

... dass die Dimensionenzahl einer stetigen Mannigfaltigkeit nach wie vor die erste und wichtigste Invariante derselben ist ....

(... that the dimension number of a continuous manifold is now as before the first and most important invariant of a manifold ... .)

In the light of CANTOR's paradoxical result the constancy of the dimension number certainly required proof, and as long as this proof was lacking dimensional invariance was in doubt. But DEDEKIND was convinced that a proof is possible.

In his important letter of July 2nd he goes on to give a very penetrating *explanation* of the mathematical situation surrounding dimension and CANTOR's result. Giving the benefit of the doubt to earlier writers and thereby possibly deferring to his friend RIEMANN, now deceased, he says that these writers clearly (!) made the implicit but quite natural assumption that, when giving a new coordinate system to the points of a continuous manifold, one assumes the new coordinates are *continuous* functions of the old coordinates, so that what is continuously connected according to the first coordinate system is also connected according to the second. In consequence, DEDEKIND arrives at the following theorem (CANTOR/DEDEKIND *(1937:38)*):

Gelingt es, eine gegenseitige eindeutige und vollständige Correspondenz zwischen den Puncten einer stetigen Mannigfaltigkeit A von a Dimensionen einerseits und den Puncten einer stetigen Mannigfaltigkeit B von b Dimensionen andererseits herzustellen, so ist diese *Correspondenz selbst*, wenn a und b *ungleich* sind, nothwendig eine *durchweg unstetige*.

(If one succeeds in setting up a one-one and complete correspondence between the points of a continuous manifold A of a dimensions on the one hand and the points of a continuous manifold B of b dimensions on the other, then this correspondence itself must necessarily be discontinuous throughout if a and b are unequal.)

In his letter DEDEKIND probably imputes too much to the 'implicit assumptions' of previous geometers. Surely CANTOR's paradoxical discovery forced a new understanding of dimension and the basic assumptions of geometry. Indeed the result drove DEDEKIND to conjecture a *new* theorem, which in reality is a very good statement of the invariance of dimension theorem. This is a very apposite conjecture for DEDEKIND to make. As we shall see the conjectured theorem became the central issue in the subsequent development of dimension theory. Unfortunately DEDEKIND himself had no proof of it.

In his letter DEDEKIND tries to give a specific explanation of CANTOR's correspondence result on the basis of his conjectured theorem. Concerning CANTOR's first attempt using decimal expansions, DEDEKIND claims (albeit somewhat hesitantly; see CANTOR/DEDEKIND *(1937:38)*) that if it had been possible to complete the direct interlacing correspondence from the ρ-dimensional cube to all the points of the unit segment, then the correspondence would have been continuous. This claim is wrong, as a quick inspection of even the incomplete correspondence based on decimals shows.[12] However, he gives a good explanation of CANTOR's second way of setting up a correspondence. He asserts that the correspondence between irrational numbers based on continued fractions is continuous and this is indeed correct.[13] But then CANTOR's extra machinery to extend the correspondence to include the rationals makes it thoroughly discontinuous.

At the end of his letter DEDEKIND warns CANTOR not to polemize openly against the original 'article of faith' of manifold theory unless he (CANTOR) examines the conjectured invariance theorem. Clearly DEDEKIND did not read the same significance into CANTOR's counterintuitive discovery as CANTOR himself had. In DEDEKIND's eyes the task now was to save dimension by means of an invariance proof and not to criticise the deficiencies of the investigations of previous geometers. I think DEDEKIND is a little overprotective of the 'great men' of the past. In effect he counsels a search for 'hidden lemmas' in the writings of earlier mathematicians. On the other hand, his topological understanding of the geometrical situation is very acute; his idea of dimensional invariance based on continuity is full of insight. He appears somewhat blinded to what is new in his own thinking by his refusal to take a more critical attitude to the growth of mathematics.

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[12] For example, if we consider the correspondence of the unit segment with the square and consider on the line the point $0.5 = 0.4999\ldots$ and any point close to 0.5 but slightly greater in value, then in the correspondence these points are unlaced to give points in the square which are far apart. So the partial correspondence is not continuous in the sense in which DEDEKIND thought of it, *viz.*, in the sense that nearby points are mapped to nearby points.

[13] In fact, the correspondence between the irrationals is bicontinuous and, hence, a homeomorphism. However, dimensional invariance prohibits the extension of this homeomorphism to include the rationals.

CANTOR swiftly responded to DEDEKIND's letter two days later on July 4th. In his reply he immediately concedes DEDEKIND's points over the interpretation of the correspondence result. He says that he overstated his case and did not intend to criticise the concept of ρ-fold extended continuous manifold as such, but only to clarify it. He did not intend to deny the 'definiteness of the dimension number', but only the invariance of the number of coordinates, which some authors seemed to accept too easily as the same as the dimension number. So to CANTOR's mind coordinate dimension is not viable in mathematics. It must be distinguished from some other notion of dimension, not yet made explicit.

CANTOR readily accepted DEDEKIND's conjectured invariance theorem as a possible way out of the difficulty over dimension, but, of course, he wanted a proof. In his letter he immediately points to some apparent difficulties likely to be encountered in any attempt to demonstrate the conjectured theorem. In the first place he forsees a problem in defining the concept of continuous correspondence in full generality; yet all hangs on such a definition. In the second place he recognises that there may be some difficulties with the approach to dimension number through an invariance proof if we take into account figures which are not continuous throughout (*i.e.* in modern terms, not connected), but which may reasonably be assigned a dimension number. This latter point is deep. Not only does CANTOR think an invariance proof is required, but also he hints at the need for an independent definition of dimension to cover arbitrary figures (or, as we would say now, arbitrary point sets). At this stage in their critical discussion of the dimension problem we can see that CANTOR and DEDEKIND achieved a very high level of understanding and insight.

CANTOR's important paper, 'Ein Beitrag zur Mannigfaltigkeitslehre' *(1878)*, written during the early summer of 1877 and published in the following year, is an exposition of his discoveries about correspondence and dimension and incorporates all his results largely in the form in which he had related them to DEDEKIND. However, certain general points are emphasised in the paper. From the beginning he uses the term 'Mannigfaltigkeit' ('manifold') to mean virtually any 'well-defined' set of elements; so we are nearly in the realm of general set theory. He devotes the first few paragrahs to a delineation of his idea of power (Mächtigkeit) based on one-one correspondence, giving definitions of equal power (gleiche Mächtigkeit; or äquivalent), lesser and greater power, and providing several significant mathematical examples. He emphasises the sharp distinction between the power of finite sets and the power of infinite sets. The concept of power for finite sets is equivalent to that of positive whole number, so that two finite sets are of equal power if and only if they have the same number of elements and a proper subset (Bestandteil) of a finite set always has smaller power than the set itself. However, the situation is fundamentally different for infinite sets. An infinite set can be put into one-one correspondence with certain of its infinite subsets, and CANTOR gives the simple example of the sequence of positive integers and the sequence of positive even integers which can be put into one-one correspondence and which are therefore of equal power.

Although in his paper CANTOR briefly deals with sets of the power of the positive integers, sets of the smallest infinite power (*i.e.*, countable sets), he, of course, concentrates on the power of 'so-called continuous n-fold manifolds' (*i.e.*, the power of the continuum). Near the beginning of the paper he mentions RIEMANN, HELMHOLTZ, and others[14] who first dealt with manifolds, but after the discussion in the correspondence with DEDEKIND his criticism of these writers is toned down. He only points to the 'tacit' assumption of most of these that the basic correspondence of the elements of an n-dimensional manifold with n real coordinates must be *continuous* and promises in a footnote to return to this matter on another occasion. In fact, a year later he did consider the matter, the fundamental problem of dimension. Then he launches into his demonstration that the elements of an n-fold extended continuous manifold can be determined by a single coordinate or, more specifically, that the points of an n-dimensional cube can be put into one-one correspondence with the points of a unit line segment. Thus all continuous manifolds have the same power. His proof is essentially as he communicated it to DEDEKIND. However, one very important new thing is added at the end of the paper: a mention – the first by CANTOR – of the celebrated continuum hypothesis! Here he formulates it by saying that the infinite linear manifolds (*i.e.*, sets of real numbers) divide into two classes according to whether they can be brought into the form of the sequence of the positive integers or into the form of the real numbers in the unit interval. In other words, there are only two powers of infinite linear sets. In the light of his recent discoveries it is a perfectly natural way to state the continuum hypothesis. Thus far he had found only two kinds of infinite sets even among the n-dimensional and infinite-dimensional manifolds.[15]

CANTOR submitted his paper to *Crelles Journal (Journal für die reine und angewandte Mathematik)* on 11 July 1877. It is well known that he felt frustrated by delays with its publication (FRAENKEL *(1930:198)*, GRATTAN-GUINNESS *(1974:111–113)*). BORCHARDT, the managing editor of the journal, seemed to impede its progress; as he learned from his old friend LAMPE who dealt with printer's proofs for the journal, BORCHARDT put other papers submitted later before CANTOR's in the order for publication.[16] After three months he complained to DEDEKIND and sought his advice about publishing the work as a separate pamphlet. DEDEKIND thought this was not a wise course, when he considered his own difficulties with publishing his pamphlet *Stetigkeit und irrationale Zahlen.* Finally in November CANTOR heard that his 'Beitrag' would be published after all.

In his biography of CANTOR FRAENKEL *(1930:198)* suggests that KRONECKER may have had a hand in the delay over publication in *Crelle*, although the

[14] He also mentions ROSANES, LIEBMANN, and ERDMANN.

[15] In the paper he implies that he has a proof of the hypothesis. However, we know that he struggled for many years to find a proof and in the end failed to find one which satisfied him.

[16] However, when one looks at the submission dates of the papers in the 84th volume of the *Journal*, one realises that the publication of CANTOR's paper was scarcely delayed at all. Nevertheless, there certainly was some difficulty with the paper. DUGAC *(1976:122–123, 125)* seems to think otherwise, but he does not take account of a letter from CANTOR to DEDEKIND of 10 November 1877 (GRATTAN-GUINNESS *(1974:112)*).

evidence is not clear on this point.[17] Whatever the case may be, CANTOR's paper certainly offered a surprising and counterintuitive result in the emerging field of set theory. It seems the editors of the journal, most probably including KRONECKER, doubted the validity of CANTOR's (admittedly long) proof of the result. However, if they were not able to detect a specific error in the proof, they still must have found the result unsatisfactory. To them it was a monster and accordingly to be rejected!

To conclude this chapter I shall sum up the achievements concerning dimension which CANTOR and DEDEKIND made up to 1878. Prior to their work RIEMANN and HELMHOLTZ, among others, had put forward a very informal theory of continuous manifolds or n-dimensional geometrical objects which included an implicit definition or theory of dimension. RIEMANN and HELMHOLTZ only intended their theory of manifolds and dimension as a general framework for their investigations into geometry. Objectively one cannot but regard the theory as vague in its mathematical details (see chapter 1).

A few years after the appearance of the publications of RIEMANN, HELMHOLTZ, and other nineteenth-century mathematicians with an early interest in multi-dimensional geometry CANTOR approached the basic concept of dimension from a position outside of geometry, from the point of view of his work on one-one correspondences and power, hence, from the viewpoint of the set theory he was in the midst of creating. Consequently, he brought an entirely new set-theoretic approach to problems of geometry. After his initial stumblings over a proof, he was able to demonstrate that a ρ-dimensional cube can be put into one-one correspondence with a unit line segment, certainly a surprising and counterintuitive result. Apparently the editors of *Crelles Journal* found this result bizarre. They wished to ignore or reject it; to use LAKATOS' *(1976)* term, they were monster-barrers. In contrast, CANTOR took the bull by the horns. Initially he read into his result a strong criticism of the dimension concept implicit in the work of RIEMANN, HELMHOLTZ, and others: the concept of dimension based on the number of coordinates needed to determine a point. DEDEKIND, while accepting CANTOR's counterintuitive result whole-heartedly, did not read the same message in it. He immediately saw a way out of the difficulty through continuity and gave credit to the older geometers for this means of escape from the consequences of CANTOR's result. To a certain extent it is true that RIEMANN and HELMHOLTZ included continuity (and differentiability) in their concept of dimension. However, I think DEDEKIND imputed a little too much to their informal theory. He wanted to find 'hidden lemmas' in the work of the great men of the past. Surely CANTOR's discovery put an entirely new light on the coordinate theory of manifolds and dimension. A fresh examination of the vague informal concept of dimension was now an absolute necessity. DEDEKIND even realised this: he quickly saw that a proof of some kind of theorem about dimensional invariance incorporating the idea of continuity was needed. Hence, he came to state very clearly the crucial problem of dimension which the paradoxical correspondence result forced upon mathema-

[17] DUGAC *(1976: 125)* cites evidence to the contrary.

ticians. At once CANTOR accepted that DEDEKIND's reading of the problem situation was better than his own initial one. However, he also recognised possible difficulties lurking in the background of the proposed dimensional invariance theorem. In fact, the subsequent history of the search for a proof of invariance of dimension has shown that the theorem was far easier to state than to prove. Nevertheless, with hindsight we can see that CANTOR and DEDEKIND pinpointed the sensitive spot in the problematic notion of dimension.

The critical discussion which CANTOR and DEDEKIND had concerning the dimension problem was unquestionably of a very high standard. They clearly discerned the problems involved in the situation and immediately sought to explain the difficulties. Above all their attempted *explanation* of the paradoxical result was acute and must be admired. In this way DEDEKIND was led to formulate very succinctly the next problem to be tackled after CANTOR's important discovery. Should we not look upon the CANTOR/DEDEKIND discussion as a paradigm for good critical discussions of mathematical or scientific problems?

Chapter 3. Early Efforts to Prove the Invariance of Dimension, 1878–1879

The publication of CANTOR's 'Beitrag' towards the beginning of 1878 immediately caused a flurry of mathematical activity. The objective now was to save the concept of dimension; the paradoxical correspondence had to be explained (one is tempted to say, explained away). During the months July to October 1878, when CANTOR's paper had barely left the presses, five mathematicians attempted to demonstrate the invariance of dimension through a consideration of continuity—just as DEDEKIND had suggested in his letters to CANTOR. These were JAKOB LÜROTH, JOHANNES THOMAE, ENNO JÜRGENS, EUGEN NETTO, as well as CANTOR himself. Then during 1879 both JÜRGENS and CANTOR came back to the problem. JÜRGENS published a much fuller account of his solution and CANTOR gave an entirely new and general approach to dimensional invariance after discussing it in correspondence with DEDEKIND. These early efforts towards showing dimensional invariance of 1878–79 were only partially successful. LÜROTH and JÜRGENS (and also CANTOR in his first work of 1878) only aimed at demonstrating invariance for low dimension numbers. The proofs, though interesting, are extremely complex. THOMAE, NETTO, and CANTOR all tried to prove dimensional invariance generally, but subsequent criticism has revealed flaws in their proofs. Without doubt the greatest difficulty which all these mathematicians came up against was the primitive state of topology. Topology, at least the part which we now know to be most relevant, was virtually nonexistent at the time. The proofs from this period mainly use methods of real analysis and simple geometry, since the topological tools were not yet available. These tools were only created in the later decades of the nineteenth century and the first decades of the twentieth (see chapter 4). Thus we cannot entirely blame the mathematicians of this period for their lack of complete success. Yet it is worth while examining this early work to see how these mathematicians struggled with a difficult problem. In the history of mathematics 'failures' can be interesting.[1]

JAKOB LÜROTH (1844–1910)[2], in 1878 a young professor at the Technische Hochschule in Karlsruhe, was the first off the mark to make a contribution towards solving the dimensional invariance problem. He had a paper presented to the Physikalisch-medizinische Sozietät in Erlangen as early as 8 July 1878. In this brief note there are proofs of dimensional invariance for two special cases only. Later in his career he returned to the problem and extended his results. These later contributions date from 1899 and 1906. In all of his papers LÜROTH's procedure is to assume that there is a one-one continuous correspondence between coordinate or number manifolds M_m and M_n, with $n<m$, and then to show that this assumption leads to a contradiction by deducing that the correspondence cannot be one-one. In his note of 1878 he does this for the cases $n=1$ and $n=2$, while in his note of 1899 he extends his results to $n=3$. Finally in a long paper of 1906 which appeared in *Mathematische Annalen* he collected all his results and presented them in a more complete form.

Let us look at some of the details of LÜROTH's first note 'Ueber gegenseitig eindeutige und stetige Abbildung von Mannigfaltigkeiten verschiedener Dimensionen aufeinander' *(1878)*. Letting $x_1, x_2, \dots x_m$ denote the coordinates of points x of the m-dimensional manifold M_m and $y_1, y_2, \dots y_n$ denote the coordinates of points y of the n-dimensional manifold M_n, with $n<m$, we assume that there is a one-one correspondence between points x and y and that the correspondence is continuous. We can think of each y coordinate, y_i, as a function of the coordinates of x.

The proof for the case $n=1$ and $m \geqq 2$ is easy. First let the coordinates $x_3, \dots x_m$ in M_m all be constant so that we consider only a plane section of M_m. Moreover, let $x_1 = r\cos\theta$ and $x_2 = r\sin\theta$, where r is small, so that we just consider a little circle in this plane section. Let A and B be any two points on this circle, for which y_1 has the values a and b, respectively. Since y_1 is a continuous function of points on the circle, y_1 must take the value $\frac{1}{2}(a+b)$ at least once on each of the two arcs from A to B according to the intermediate value theorem and, hence, it must take this value at least twice on the circle. But this result contradicts the supposition that the function y_1 is one-one. According to LÜROTH *(1878:191)* CANTOR discovered this same simple proof and told him of this fact in a letter.

The mathematicians trying to prove dimensional invariance at this time commonly used the intermediate value theorem as an important element in their proofs. We can formulate the theorem, which goes back as far as BOLZANO *(1817)* and CAUCHY *(1821:460, 463)*, in the following way:

Let $y=f(x)$ be a continuous real function defined on an interval of R (the real numbers). If the function takes two values $f(x_1) \neq f(x_2)$ at two points

[1] The material covered in this chapter has also been dealt with by DAUBEN *(1975)*. However, DAUBEN's paper lacks some topological insights. For example, he does not give JÜRGENS' work on domain invariance its proper due.

[2] For biographical material on LÜROTH *cf.* BRILL/NOETHER *(1911)* and VOSS *(1911)*.

$x_1, x_2, x_1 < x_2$, in the interval, then for any number c between $f(x_1)$ and $f(x_2)$ there exists x_3 such that $x_1 < x_3 < x_2$ and $f(x_3) = c$.

In the argument given above LÜROTH used the theorem in a more general version for a continuous function defined on the arcs of a circle. Others employed versions of the theorem covering continuous functions defined on curves or arcs of curves. In general the mathematicians of this time recognised – somewhat vaguely – the topological significance of this theorem. We now see the intermediate value theorem as a special case of a theorem on connectedness:

If the mapping $f\colon X \to Y$ is a continuous mapping from the topological space X to the space Y and X is connected, then $f(X)$ is also connected.

To be sure, the mathematicians of about 1880 did not possess anything as general as this theorem. However, they chose the special case, the intermediate value theorem, as the best tool available for attacking the problem of dimensional invariance.[3]

Returning to LÜROTH's note of 1878, we find that his proof for $n=2$ and $m \geqq 3$ is considerably harder. The rest of his note is devoted to it *(1878:191–195)*. The bare bones of the proof are as follows. First of all we confine our attention to a 3-dimensional section of M_m by holding the coordinates $x_4, x_5, \ldots x_m$ constant. We take two points of this section, A and B, for which y_1 has different values, a_1 and b_1, respectively, where we assume $a_1 > b_1$. LÜROTH considers the sphere with diameter AB and thinks of it as a globe representing the surface of the earth with A as the North pole and B, the South pole. Under this representation we regard y_1 and y_2 as functions of position on this globe. The objective of the proof is to show that there are two points on the globe for which y_1, y_2 take the same pair of values. Hence, the correspondence is not one-one.

Because of the continuity of the function y_1 it is possible to describe circles around the poles A and B so that the variation of the function is less than $\delta_1 = \frac{1}{4}(a_1 - b_1)$ within these circles; that is, in the circle around A $y_1 > a_1 - \delta_1$ and in the circle around B $y_1 < b_1 + \delta_1$. Thinking of the sphere as a geographical globe, we give it lines of latitude and longitude so as to divide it up into $2q^2$ surface pieces.[4] If we take q equal to some minimal p, then the two sets of triangular pieces close to the North and South poles, describing two circular regions, fall within the circles around the poles. Beginning from the circular region covering the North pole, we can construct a surface over the pole for which $y_1 \geqq c_1 = \frac{1}{2}(a_1 + b_1)$ by adding little square geographical regions with this property so long as they are connected on their sides with regions already added. We are interested in the southernmost boundary of this covering surface: it is a connected path, having no self-intersections and consisting of pieces of meridians and parallel circles, and it goes around the North pole, touching every meridian as it passes.

[3] There is an excellent paper by H. HOPF *(1953)* on the deeper topological significance of the intermediate value theorem.

[4] To give this subdivision of the globe we need $q-1$ parallel circles of latitude (rather than q circles as given in LÜROTH *(1878)*) and $2q$ equidistant meridians of longitude.

We can carry out the construction just described for every q of form p, $2p$, $4p, \dots 2^i p, \dots$, thereby deriving covering surfaces $R_0, R_1, R_2, \dots$ with southernmost boundary curves $G_0, G_1, G_2, \dots$. Since each surface R_i includes its predecessors, the boundary curves G_i in general approach the south or at least do not go north. Hence, we can always find a boundary curve such that the points of it and all succeeding curves in the sequence are less than a prescribed small distance from points for which $y_1 < c_1$.

Similarly we can construct connected covering surfaces and boundary curves F_i around the South pole for every q with form $2^i p$. In constructing these surfaces covering the South pole we join little square regions on the sides of previously added regions under the conditions that none of these touch any of the curves G_k and that each of the resulting boundary curves F_i does not intersect itself. With increasing index the curves F_i proceed northwards or at least not southwards and their points come as close as we please to points of surfaces R_k for sufficiently large k. Yet the curves F_i and G_k do not intersect, so that between each pair of these there is a ring or 'stream' around the globe whose 'banks' do not cut one another.

If for each F_i we consider its northermost point lying on the 0° meridian, then with increasing i these points proceed north and approach a limit position C. There is a similar limit position D on the 180° meridian. LÜROTH claims without proof that $y_1 = c_1$ for these points. Hence, we have points on 'opposite sides' of the globe with the same y_1 value c_1. Supposing that the function y_2 has value a_2 at C and value b_2 at D and setting $c_2 = \frac{1}{2}(a_2 + b_2)$, LÜROTH gives in the remainder of his paper a lengthy proof that there are two distinct points within the rings with the same y_1, y_2 values, c_1, c_2. Consequently, he concludes *(1878:195)*:

> ... wenn auf einer Kugel zwei eindeutige und stetige Functionen gegeben sind, so gibt es stets Werthe, welche gleichzeitig von beiden Functionen in zwei Punkten der Kugel angenommen werden....
>
> (... if on a sphere two single-valued and continuous functions are given, then there are always values which are taken simultaneously by both functions in two points of the sphere....)

Hence, we must conclude that a one-one continuous correspondence between a manifold M_m, $m > 2$, and a manifold M_2 is not possible.

LÜROTH's proof with its strong geometrical flavour is quite interesting. Moreover, the tools of real analysis are used fairly carefully to prove the existence of the two points on the sphere with the same pair of values.[5]

When LÜROTH returned to the problem of dimensional invariance in his further note of 1899 with a proof for the case $n = 3$ and $m \geqq 4$ *(1899)*, he generalised the basic idea of his earlier note. Additionally, in the new case he employed GAUSS' integral for the linking of two curves in space (GAUSS *(1867:605)*), although he did not use this important topological idea in any brilliant way. In general he found it extremely difficult to extend his work to higher-dimensional cases and as a result his proof in 1889 much more complex.

[5] By later standards of rigour some aspects of the proof are open to criticism.

In a sense his *Mathematische Annalen* paper *(1906)*, in which he set out proofs in full for all the cases he had considered earlier, is an admission of defeat. LÜROTH was convinced that his basic idea for a proof could be extended to a general demonstration of dimensional invariance, but he could not see how to make the extension. His 'geometrical intuition' did not permit him any insight into the general case.

At virtually the same time as LÜROTH was first proving special cases of dimensional invariance, JOHANNES THOMAE (1840–1921)[6], a prolific writer of mathematical books and papers and then *ordentlicher Professor* at Freiburg, tried to prove the invariance in full generality. He presented a note *(1878)* to the Königliche Gesellschaft der Wissenschaften at Göttingen in August 1878, just a month after LÜROTH's first note. His general proof is based on an assumption from analysis situs 'against whose general validity there can be no real objections'[7] *(1878: 466–467)*:

> Eine zusammenhängende continuirliche Mannigfaltigkeit M_n von n Dimensionen kann durch eine oder mehrere Mannigfaltigkeiten von $n-2$ oder weniger Dimensionen ($M_\nu, M'_{\nu'}, M''_{\nu''}, \ldots$; $\nu, \nu', \nu'', \ldots \leqq n-2$) nicht in getrennte Stücke zerlegt werden.
>
> Dabei muss allerdings vorausgesetzt werden, dass nicht die Anzahl der Mannigfaltigkeiten $M_\nu, M'_{\nu'}, M''_{\nu''}, \ldots$ in jedem noch so kleinen Stücke einer continuirlichen Mannigfaltigkeit von $n-1$ Dimensionen abzählbar unendlich gross sei.

> (A connected continuous manifold M_n of n dimensions cannot be divided into separate pieces by one or several manifolds of $n-2$ or fewer dimensions ($M_\nu, M'_{\nu'}, M''_{\nu''}, \ldots$; $\nu, \nu', \nu'', \ldots \leqq n-2$).
>
> However, it must be assumed that the number of manifolds $M_\nu, M'_{\nu'}, M''_{\nu''}, \ldots$ in each small region of a continuous manifold of $n-1$ dimensions is not countably infinite.)

Briefly, THOMAE's proof runs as follows. We suppose that there is a one-one continuous correspondence between manifolds M_m (with points x having coordinates $x_1, x_2, \ldots x_m$) and M_n (with points y having coordinates $y_1, y_2, \ldots y_n$), $m > n$, and consider just one coordinate y_1 as a continuous function of $x_1, x_2, \ldots x_m$ for a bounded region of M_m. If A and B are points of this region giving maximum and minimum values a and b to y_1, then, by a generalisation of the intermediate value theorem which THOMAE assumes as proved, on every curve connecting A and B there is a point for which $y_1 = c$, c being an intermediate value between a and b. In view of his main topological assumption he concludes that the infinite set of points for which $y_1 = c$ must form a continuous (*i.e.*, connected) manifold of $n-1$ dimensions in at least one place. Hence, a continuous function y_1 of a manifold M_m of m dimensions takes a certain value along a continuous manifold M_{m-1} of $m-1$ dimensions. Similarly

[6] On THOMAE's life *cf.* H. LIEBMANN *(1921)*.

[7] In the original German: 'deren allgemeiner Giltigkeit keine erheblichen Bedenken entgegen stehen dürften'.

a continuous function y_2 of a manifold of $m-1$ dimensions takes some fixed value along a manifold M_{m-2} of $m-2$ dimensions, and so on. In the end the n functions $y_1, y_2, \dots y_n$ of points in some region of M_m take at least one system of values in a continuous manifold of $m-n$ dimensions. Thus there is a fixed y in M_n corresponding to infinitely many x points in M_m and the correspondence is certainly not one-one.

THOMAE must have derived his fundamental topological assumption from a fragment on analysis situs by RIEMANN *(1953:481)* (*cf.* CANTOR/DEDEKIND *(1937:44)*). RIEMANN's separation property for dimension is indeed interesting. Something like it figures in the later history of dimension theory beginning with POINCARÉ and BROUWER.[8] However, THOMAE's contemporaries were quick to point out that the separation property was no less in need of proof than the invariance of dimension. At a session of the 51st Versammlung Deutscher Naturforscher und Aerzte held in Cassel in September 1878 LÜROTH rightly criticised THOMAE's 'proof' of dimensional invariance, declaring that the separation property and invariance were on the same footing with respect to their importance and difficulty of proof (*cf.* JÜRGENS *(1878:139–140)*). THOMAE's 'proof' was a nonstarter.[9] Still, in defence of THOMAE it is significant that he chose to put the problem of dimensional invariance squarely in the domain of analysis situs.

At the same session of the Versammlung Deutscher Naturforscher und Aerzte, held on the 13th of September, ENNO JÜRGENS (1849–1907)[10] sketched an alternative proof for the 2-dimensional case of invariance *(1878)*. A year later he published a full version of his proof. JÜRGENS was hardly an important figure in nineteenth-century mathematics. The bibliography of his publications contains only eight items. During 1873 and 1874 he had studied at Berlin, where he developed an interest in function theory through WEIERSTRASS. The latter advised him to go to Halle University for his habilitation, where he remained from 1875 until 1883. Halle was, of course, CANTOR's university, so it is not surprising that JÜRGENS became interested in the problem of dimensional invariance. The endproduct of his investigations was a proof whose method is especially interesting in the light of later developments. Unfortunately he chose to publish the full version of his alternative proof in a separate pamphlet, *Allgemeine Sätze über Systeme von zwei eindeutigen und stetigen reellen Functionen von zwei reellen Veränderlichen (1879)*, with the result that it cannot have received wide circulation.

The most significant feature of JÜRGENS' proof of the 2-dimensional case of dimensional invariance is the fact that he laid its foundation on a rigorous proof of the invariance of domain for the plane. He was the first mathematician to discern the close connection between domain invariance and dimensional in-

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[8] A version of it is provable in modern dimension theories; *e.g. cf.* HUREWICZ/WALLMAN *(1948:48)*.

[9] LÜROTH also criticised THOMAE's assertion that the point set for which $y_1 = c$ must form a *connected* $(n-1)$-dimensional manifold in at least one place. *Cf.* also CANTOR/DEDEKIND *(1937:44)*.

[10] On JÜRGENS' life *cf.* KRAUSE *(1908)*.

variance – an important observation which gave him a better insight into the problem situation than his contemporaries possessed.[11] JÜRGENS' planar domain invariance theorem is a little more general than usual *(1879: 3)*:

> Wenn zwei unabhängige reelle Veränderliche x_1 und x_2 als rechtwinklige Punktcoordinaten in der Ebene aufgefasst alle Stellen im Inneren und auf einem Kreise durchlaufen, wenn von ihnen zwei andere reelle Veränderliche y_1 und y_2 eindeutig und stetig abhängen und dabei dasselbe Werthepaar y_1, y_2 zu einer endlichen Anzahl von Werthepaaren x_1, x_2 gehört, so enthält, indem auch die Veränderlichen y_1 und y_2 in einer zweiten Ebene als rechtwinklige Punktcoordinaten angesehen werden, der von den Punkten y_1, y_2 gebildete Theil dieser Ebene ein zweifach ausgedehntes Stück der Ebene, etwa die ganze Fläche eines Kreises, in sich.
>
> (If two independent real variables x_1 and x_2, thought of as rectangular coordinates in the plane, range over all points in the interior and on a circle, and if two other real variables y_1 and y_2 are single-valued and continuous functions of them whereby the same value pair y_1, y_2 belongs to a finite number of value pairs x_1, x_2, then, when the variables y_1 and y_2 are thought of as rectangular coordinates in a second plane, the part of this latter plane formed by the points y_1, y_2 contains a twofold extended piece of the plane, say the entire surface of a circle.)

The usual domain invariance thorem is a corollary deducible from this theorem.

JÜRGENS' demonstration of the theorem is complicated and a little clumsy. Nevertheless, it is an adequate proof based on Weierstrassian ε, δ methods of real analysis. Because of the importance of domain invariance in the history of dimension theory and topology generally, it is worth while having a closer look at the proof (in *(1879: 3–9)*).

Let a_1, a_2 denote the centre of the given circular region in the x_1, x_2 plane and let A_1, A_2 be the corresponding y_1, y_2 point. Because the point A_1, A_2 corresponds to only a finite number of x_1, x_2 points, there must exist a circle, say with equation

$$(1) \qquad (x_1 - a_1)^2 + (x_2 - a_2)^2 = r^2,$$

within the given circular region none of whose points is mapped to the point A_1, A_2. Let ρ be the minimum value which the expression

$$(*) \qquad +\sqrt{(y_1 - A_1)^2 + (y_2 - A_2)^2}$$

takes for points on circle (1). If R is a positive number $< \rho$, then every radius emanating from a_1, a_2 to a point on the circumference of circle (1) has a point which is mapped by y_1, y_2 to a point which is at a distance R from A_1, A_2 – a fact guaranteed by the intermediate value theorem. Thus we find that on the circle

$$(2) \qquad (\eta_1 - A_1)^2 + (\eta_2 - A_2)^2 = R^2$$

[11] DAUBEN *(1975)* does not mention this important point.

(where η_1, η_2 are the coordinates for the second plane) there are infinitely many points that are images of points of the surface within circle (1). JÜRGENS' first objective is to show that these y_1, y_2 points fill up an entire connected arc of circle (2).

In order to achieve this objective he borrows LÜROTH's method of cartographical subdivision and divides the surface of circle (1) into $4q^2$ little elements.[12] We are only interested in those elements all of whose points are mapped to y_1, y_2 points which lie within circle (2). Provided q is sufficiently large, all the little elements around a_1, a_2 will be of this type. As LÜROTH *(1878)* did we construct a surface within circle (1) by successively adding elements of the appropriate type to those around a_1, a_2, provided they are adjacent on a side to previously added elements. We end up with a surface F all of whose points are mapped to y_1, y_2 points within circle (2). JÜRGENS is concerned with the outermost boundary C of F and he takes the trouble to prove that the curve C traces a continuous circuit around the point a_1, a_2. C has the property that its points are mapped to points y_1, y_2 which are less than distance R from A_1, A_2, but within a well-defined small distance of its points[13] there are points with images at a distance $\geqq R$ from A_1, A_2.

For an appropriate sequence of increasing q's we derive a sequence of curves $C_1, C_2, \ldots$, such that each curve encloses its predecessors unless they coincide. Any limit point of the curves C_i (*i.e.*, a point x_1, x_2 which comes arbitrarily close to points of the curves C_i) falls within circle (1) and for it the expression (*) has the value R. The objective is to show that the y_1, y_2 images of these limit points form a whole connected arc on circle (2). JÜRGENS achieves this first objective by giving a rigorous limit argument based on the y_1, y_2 images of the curves C_i and the intermediate value theorem.

In the first part of his proof he only required that the radius R of circle (2) be less than ρ. Consequently, he really showed that the image points y_1, y_2 fill up a connected arc on every circle with centre A_1, A_2 and radius $<\rho$. JÜRGENS' second objective is to show that these arcs fill up an entire piece of surface. This he accomplishes through a modified limit argument, hence, completing his demonstration of the invariance theorem.

JÜRGENS' argument, though of a high standard of rigour, is not very attractive. Its complicated applications of methods of Weierstrassian analysis do not reveal in a clear way why the theorem should work. Moreover, it is difficult to imagine extending the argument to higher dimensions. For us, having the advantage of several more generations of mathematical experience, the proof lacks topological insight. Of course, we cannot blame JÜRGENS for not using mathematical tools which did not exist at the time. We must give him credit for his skillful handling of the tools he did have at his disposal. A significant feature of his proof is its dependence upon the intermediate value theorem, a theorem with topological import concerning connectedness. As we have seen LÜROTH and THOMAE also employed this theorem in their arguments. Thus all these mathematicians recognised an importance in this quasi-topological result.

[12] JÜRGENS uses $q-1$ equidistant concentric circles and $4q$ radii at equal angles to obtain this subdivision.

[13] distance $(r/q)(\pi/2+1)$.

In his pamphlet JÜRGENS *(1879:9)* gives an alternative way of expressing his result: the system of image points y_1, y_2 of the circular region in the first plane must contain an *interior point* in the second plane. However, this does not mean that interior points of the region in the first plane are necessarily mapped to interior points in the second. A simple example – a circle folded along one of its diameters – shows that this need not be the case. In this example some interior points become boundary points. Yet the JÜRGENS theorem does guarantee that there are interior points arbitrarily close to images of interior points. Motivated by these considerations, he strengthens the conditions of his theorem in order to prove the usual domain invariance theorem for the plane as a corollary *(1879:11)*:

> Wenn zwei eindeutige und stetige reelle Functionen von zwei reellen Veränderlichen dasselbe Werthepaar nicht wiederholt annehmen, so entspricht einer inneren Stelle des Gebietes der unabhängigen Veränderlichen eine innere Stelle im Gebiete der abhängigen Veränderlichen.
>
> (If two single-valued and continuous real functions of two real variables do not take the same pair of values more than once, then to an interior point of the domain of the independent variables there corresponds an interior point in the domain of the dependent variables.)

In other words, a one-one continuous mapping of a region in one plane to another takes interior points to interior points. Moreover, as JÜRGENS adds *(1879:11)* the inverse mapping must be continuous: so in modern terms we have a topological (*i.e.*, one-one bicontinuous onto) mapping.

It is an easy step to deduce the invariance of dimension theorem for $n=2$ from the planar domain invariance theorem *(1879:17–18)*. JÜRGENS expresses the former theorem in the following way *(1879:17)*:

> Kein Theil des dreifach ausgedehnten Raumes, welcher eine Kugel ganz enthält, kann auf irgend einen Theil der Ebene eindeutig und stetig abgebildet werden.
>
> (No part of threefold extended space which contains an entire sphere can be mapped onto any part of the plane in a single-valued and continuous way.)

For the proof suppose that we can map a sphere continuously onto a part of the plane and assume that the mapping is one-one.[14] Then an equatorial planar section of the sphere will be mapped onto a part of the plane in such a way that interior points are mapped to interior points. Hence, the centre of the sphere, a_1, a_2, a_3, which is an interior point in the equatorial plane, will be mapped to an interior point, B_1, B_2, in the image plane. But then points not in the equatorial plane but close to a_1, a_2, a_3 cannot be mapped to points close to

[14] The assumption that the mapping is one-one is a slight simplication of JÜRGENS' argument. He only assumes that the mapping is of the type described in his first theorem, *viz.*, that the mapping is such that each image point corresponds to at most finitely many preimage points.

B_1, B_2 – contrary to the assumption of continuity. Thus from this contradiction the invariance of dimension for dimension two follows.[15]

Following close on the heels of the contributions of LÜROTH, THOMAE, and JÜRGENS, EUGEN NETTO (1846–1919)[16] completed a general proof of dimensional invariance in October 1878 which was published in *Crelles Journal* at the very end of the year. NETTO is best remembered for his contributions to the development of group theory; his textbook *Substitutionentheorie und ihre Anwendung auf die Algebra* (1882) is a minor classic in the subject. From 1866 until 1870 he had studied at Berlin under the great masters KUMMER, KRONECKER, and WEIERSTRASS; WEIERSTRASS had been the chief examiner for his dissertation. During the '70's he was a teacher at the Friedrich-Werder'sches Gymnasium in Berlin. While there he wrote his paper on dimensional invariance, virtually his only contribution to *Mannigfaltigkeitslehre.*

NETTO's 'Beitrag zur Mannigfaltigkeitslehre' *(1879)*, setting out his attempt to prove dimensional invariance in general, is an important paper for its time. Even though the attempt fails, it possesses interesting facets. NETTO put the problem firmly in the realm of topology and in a much better way than THOMAE had. In particular, NETTO's topological concepts are largely borrowed from WEIERSTRASS, who used them in function-theoretic work.

NETTO's proof is inductive. He starts by examining the three simplest cases *(1879:264–265)*. First, a one-one correspondence between a 1-dimensional manifold, a line, and a 0-dimensional manifold, which he takes to be a single point, is obviously not possible. So clearly a one-one correspondence between a manifold $M_n (n \geqq 1)$ and M_0 is impossible. Second, a correspondence between manifolds $M_n (n \geqq 2)$ and M_1 is not possible either. Suppose we can map a 2-dimensional manifold M_2 one-one continuously onto a simple manifold M_1. Consider a simple closed line $\mathfrak{A}$ in M_2 and its image A in M_1. Since A cannot be a point, NETTO asserts it must be a certain piece of the line M_1, although it cannot be the entire line. Consider an interior point of A. In order to pass from this point to a point not belonging to A, we must pass over one of the boundary points (Grenzpunkte) of A, say α_1. Let α_1 correspond to a_1 in M_2. Now in M_2 in order to pass from points of $\mathfrak{A}$ to points not belonging to this figure it is by no means necessary to pass through a_1. But given the continuous correspondence, it should be necessary. Hence, the supposed correspondence between M_1 and M_2 is impossible, and also one between M_1 and $M_n (n > 2)$. Third, consider the possibility of mapping a continuous threefold manifold M_3 one-one continuously onto a continuous twofold manifold M_2. Choose a surface $\mathfrak{A}$ in M_3 which will be mapped to a surface A in M_2. Surface A will be bounded by one or more curves $\alpha_1, \alpha_2, \ldots$ such that it is impossible to go from a point of A to a point not in A without passing through one of these curves. Yet the corresponding curves $a_1, a_2, \ldots$ in $\mathfrak{A}$ do not have this bounding property. It is always

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[15] JÜRGENS *(1879)* contains another interesting application of his main theorem: a proof of the fundamental theorem of algebra. The connection between this theorem and domain invariance is now well known.

[16] On NETTO's life *cf.* BIERMANN *(1974)*.

possible to go from a point of a surface in space immediately to a point not on the surface. Hence, a correspondence between M_2 and $M_n (n \geqq 3)$ is not possible.

The modern topologist will certainly find the proofs above unsatisfactory. Indeed they are not proofs at all from our standpoint. NETTO just accepts ideas and facts related to the JORDAN curve theorem (1887) and its analogues for other dimensions. Yet in his defence we should recognise that he displays a good intuitive understanding of the topological situation, although he expresses no need to prove his most significant assumptions about separation. It must be emphasised again that topology was in a very primitive state when NETTO wrote his paper. He could only indicate the topological problem situation without really proving anything as we would expect today.

In his paper NETTO proceeds to the inductive step of his general invariance proof, trying to demonstrate the impossibility of a one-one continuous mapping from an n-dimensional manifold M_n onto an $(n-1)$-dimensional manifold M_{n-1}. His demonstration continues the line of thought of his first three cases which he connects with the following principle *(1879: 265)*:

> In einer Mannigfaltigkeit νten Grades wird jedes Gebilde νten Grades durch ein anderes von geringerem Grade begrenzt; in einer Mannigfaltigkeit $(\nu+1)$ten Grades fällt jedes Gebilde νten Grades mit seiner Grenze zusammen.
>
> (In a manifold of ν^{th} degree [*i.e.*, dimension] every figure of ν^{th} degree is bounded by another of lesser degree; in a manifold of $(\nu+1)^{\text{th}}$ degree every figure of ν^{th} degree coincides with its boundary.)

In tackling the general case he clearly recognises the need to define rigorously the geometrico-topological concepts to be used. Hence, he adopts Weierstrassian neighbourhood topology, although he does not credit WEIERSTRASS explicitly.[17] To be sure, these topological ideas were the best tools which NETTO had available. His use of them probably constitutes the most important feature of his entire paper. Thus he gives reasonable definitions of interior point (ein Punkt im Innern) and boundary point (ein Punkt auf der Grenze) and takes connectedness in the Weierstrassian sense that a figure will be connected if every pair of points of it can be joined by a line belonging entirely to the figure. Unfortunately these basic tools from general topology were quite insufficient means for NETTO to construct a cogent inductive step for his proof.

JÜRGENS was very quick to detect an important error in NETTO's general invariance proof. He included a very astute criticism of the proof in his pamphlet of 1879 *(1879: 17)*, which appeared a few months after NETTO's paper. Having *proved* the 2-dimensional domain invariance theorem himself, JÜRGENS realised quite correctly that NETTO merely *assumed* the general invariance of domain theorem in his proof. For example, in his third special case

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[17] *Cf.* WEIERSTRASS *(1894: 70–71, 83–84) (1880: 721) = (1895: 203) (1927: 56–58)*, PINCHERLE *(1880: 234–237)*. In my opinion DAUBEN *(1975)* wrongly credits these topological ideas to NETTO. WEIERSTRASS certainly used such ideas in his lectures around 1879. However, in the case of connectedness NETTO's definition appears to be slightly more general than that of WEIERSTRASS.

he took it for granted that the surface in M_3 will be mapped to a surface in M_2 *which possesses interior points.* Yet only a proof of something like JÜRGENS' planar domain invariance theorem will guarantee that this is the case. Consequently, JÜRGENS was fully justified in further condemning NETTO's proof *(1879:17)*:

> ... der ... Inductionsschluss ist nur dann richtig, wenn auch die Unmöglichkeit einer eindeutigen und stetigen Abbildung einer stetigen Mannigfaltigkeit *n*ter Dimension auf einen unstetigen Theil einer zweiten stetigen Mannigfaltigkeit *n*ter Dimension, d.h. auf einen Theil ohne innere Punkte, nachgewiesen wird.
>
> (... the induction step is then only correct if the impossibility of a single-valued and continuous mapping [of the type JÜRGENS had considered] of a continuous manifold of the n^{th} dimension onto a discontinuous part of a second continuous manifold of the n^{th} dimension, *i.e.*, onto a part without interior points, is also proved.)

JÜRGENS, equipped with a demonstration of a special domain invariance theorem, had a good insight into the close connection between any attempt to prove dimensional invariance in general and the invariance of domain.[18]

During the time that LÜROTH, THOMAE, JÜRGENS, and NETTO were busily trying to prove dimensional invariance in the last months of 1878, CANTOR did not remain inactive over the problem to which his own paradoxical discovery had given birth. Several times in his correspondence he gently urged DEDEKIND to prove the conjecture on the invariance of dimension which DEDEKIND himself had put forward. But DEDEKIND did not seem to have the time or inclination to think about a proof (CANTOR/DEDEKIND *(1937:37,40,41,42)*, GRATTAN-GUINNESS *(1974:111)*). CANTOR also kept up with the publications on the subject and cited all that had appeared thus far in a letter to DEDEKIND of the 29th of December 1878. Yet he was not completely satisfied with the published proofs (CANTOR/DEDEKIND *(1937:43)*):

> ... es scheint mir jedoch die Sache noch nicht ganz fertig gestellt zu sein.
>
> (... it seems to me, however, that the situation is still not entirely resolved.)

NETTO's proof interested him most, undoubtedly because it was the only reasonably good attempt at a general proof. Nevertheless, it did not make him feel that the problem was solved, as he admitted in a letter of 5 January 1879 (GRATTAN-GUINNESS *(1974:114)*):

> ... so dankenswerth auch dieser mit Scharfsinn unternommene Beweisversuch mir erscheint, so kann ich dennoch gewisse Bedenken an demselben nicht verscheuchen und fürchte, dass es *nur* ein Versuch ist, der zur Klärung über die Sache aber gewiss beitragen würde.
>
> (... No matter how commendable this penetrating attempt at a proof appears to me, I still cannot banish certain doubts about it and I fear that it is

[18] JÜRGENS *(1899:52–53)* also contains this criticism.

only an attempt, which nonetheless will certainly contribute to clarifying the situation.)

In view of these lingering doubts he presented his own proof of dimensional invariance to DEDEKIND in a letter date 17 January 1879.

In his letter CANTOR expresses the theorem to be proved in the following terms (CANTOR/DEDEKIND *(1937:45)*):

> Eine stetige M_μ und eine stetige M_ν lassen sich, falls $\mu<\nu$, nicht *stetig* so einander zuordnen, dass zu jedem Elemente von M_μ *ein einziges* Element von M_ν und zu jedem Elemente von M_ν *ein oder mehrere* Elemente von M_μ gehören.
>
> (A continuous [*i.e.*, connected] M_μ and a continuous M_ν, in case $\mu<\nu$, cannot be put into continuous correspondence with one another such that to each element of M_μ there belongs a single element of M_ν and to each element of M_ν there belongs one or more elements of M_μ.)

He claims that he had a proof of this for over a year, but previously had serious doubts about its validity since it depends upon a multi-valued correspondence. Only now has he resolved the doubts in his own mind.[19]

CANTOR's proof is complex (CANTOR/DEDEKIND *(1937:44–46)*). It proceeds by induction on ν. According to him the theorem is obvious for $\nu=1$, since like NETTO he thinks of M_0 as a single point. For the induction step we assume the case $\nu=n-1$ and try to deduce the case $\nu=n$. To this end we suppose that there can be a continuous many-one correspondence as described between M_μ and M_n, $\mu<n$. CANTOR's objective is to show that this supposition leads to a clash with the intermediate value theorem. As we have already seen the intermediate value theorem figures in nearly all the dimensional invariance proofs from the years 1878–1879. CANTOR's intention is to use the theorem in a modified form, as the 'JORDAN-BROUWER separation theorem' for the simple case of the hypersphere. Taking a distance function from the centre of the hypersphere relative to its surface so that points within have a negative distance, while points outside have a positive distance, and points on the sphere have zero distance, we derive the result that a connected figure with points on both the inside and outside must cut the sphere. This result follows, since a continuous function with a positive and a negative value must have a zero in between.

For the main body of CANTOR's proof let a and b be two interior points of M_μ with A and B the corresponding points of M_n. In M_n construct an $(n-1)$-dimensional sphere K_{n-1} around A sufficiently small to exclude B, while in M_μ construct a sphere $K_{\mu-1}$ around a sufficiently small so that it excludes b and so that its image in M_n, a figure $G_{\mu-1}$ (which is $(\mu-1)$-dimensional by the inductive hypothesis), lies entirely within K_{n-1}. The latter condition can be met according to CANTOR because of the continuity of the correspondence. Letting ζ be the

[19] CANTOR's invariance 'theorem' as stated above can be falsified by a counterexample: PEANO's space-filling curve. However, PEANO's example was not published until eleven years later and by then the problem situation in topology and dimension theory had changed considerably. *Cf.* chapter 4.

point (of $G_{\mu-1}$) corresponding to an arbitrary point z of $K_{\mu-1}$, draw a straight line segment from A to ζ and extend it so that it cuts the sphere K_{n-1} in a determined point Z. Carrying out this construction with respect to all points z of $K_{\mu-1}$ furnishes a continuous correspondence between points z of $K_{\mu-1}$ and certain points Z of K_{n-1}. The points Z cannot cover all of K_{n-1}, for otherwise the inductive hypothesis would be contradicted. Hence, there must be a point P of K_{n-1} which is not a Z point. If we draw segment AP, it does not touch the figure $G_{\mu-1}$. Moreover, if we connect P to the point B lying outside K_{n-1} by a curve, then we obtain a connected line APB which has no point in common with $G_{\mu-1}$. But this line corresponds to one or more curves in M_μ which run continuously from a to b and according to CANTOR's intermediate value/separation theorem these curves must cut the sphere $K_{\mu-1}$. However, it is impossible for the curves to cut $K_{\mu-1}$, since they correspond to line APB which does not cut $G_{\mu-1}$. Thus by *reductio ad absurdum* the result follows.

Two days after CANTOR wrote out his proof (*i.e.*, 19 January) DEDEKIND responded with two specific objections (CANTOR/DEDEKIND *(1937:46–48)*). First it is conceivable that a point ζ of $G_{\mu-1}$ corresponding to a point z of $K_{\mu-1}$ is A, since several points of M_μ can correspond to one and the same point of M_n. In this case the image Z would be indeterminate. DEDEKIND immediately saw a way out of this difficulty if it is assumed that the number of points a' in M_μ which correspond to point A in M_n is *finite*. Under this assumption we can make the sphere $K_{\mu-1}$ sufficiently small to exclude the other points a'. However, if the number of corresponding points can be *infinite*, then he could see no exit from the difficulty and, moreover, he then discerned a second difficulty. According to CANTOR the line APB corresponds to one or more continuous lines in M_μ going from a to b. But if B can correspond to infinitely many points b', then he was doubtful about the existence of a particular line from a to b which has the image APB. Hence, what concerned DEDEKIND most was the possibility of an infinite many-one correspondence in CANTOR's proof.

Beyond his specific criticisms DEDEKIND had some interesting programmatic ideas about *Mannigfaltigkeitslehre* or *Gebietslehre* (his preferred term). In his letter he calls for a more rigorous development of the subject, saying (CANTOR/DEDEKIND *(1937:47)*):

> ... es wäre sehr verdienstlich, wenn diese ganze ‚Gebietslehre' ab ovo dargestellt würde, ohne die geometrische Anschauung zuzuziehen, und dabei müsste z.B. der Begriff einer von dem Puncte a nach dem Puncte b innerhalb des Gebietes G stetig führenden Linie recht bestimmt und deutlich definiert werden.
>
> (... It would be very meritorious if this entire 'domain theory' were developed ab ovo without bringing in geometrical intuition and in doing that, *e.g.*, the concept of a line proceeding continuously from the point a to the point b within the domain G would have to be defined quite definitely and precisely.)

Is this not a proposal for a programme of topology based on arithmetical or analytical principles? Indeed DEDEKIND's remark foreshadows the subsequent development of point set topology which CANTOR above all initiated (see chapter 4).

In his letter DEDEKIND praises NETTO's paper, saying NETTO's definitions form a good kernel for further developments. At the same time he reveals to CANTOR some of his own attempts to elaborate the basic concepts of the theory of domains. He seems to be referring to a little paper, 'Allgemeine Sätze über Räume', probably written in the early 1870's but not published until 1931 *(1931:353–355)*. This paper is certainly brief; it contains just a few theorems on open sets (Körper) and their boundaries (Begrenzungen). In his letter DEDEKIND tells CANTOR that his motivation for writing such a paper was to shore up the DIRICHLET Principle for a contemplated edition of DIRICHLET's lectures on potential theory. He thought that an analytic theory of domains could be used to avoid WEIERSTRASS' devastating critique *(1870)*.[20]

CANTOR partly anticipated the objections which DEDEKIND included in his letter. This is evidenced by a postcard of 20 January (CANTOR/DEDEKIND *(1937:48)*). There CANTOR suggests that it would be better to think of the correspondence as proceeding from points A and B of M_n to points of M_μ. If we consider just one of the points a corresponding to A and yet all of the points $b, b', b'', \ldots$ corresponding to B, then the sphere $K_{\mu-1}$ around a can be made sufficiently small so that *all points* $b, b', b'', \ldots$ will fall outside $K_{\mu-1}$.[21] According to CANTOR this is possible because of the continuity of the correspondence which prevents the points $b, b', b'', \ldots$ from coming 'infinitely close' to a. Now there can be no doubt that one of the curves corresponding to APB goes from a to at least one of the points $b, b', b'', \ldots$ thus cutting the sphere $K_{\mu-1}$. Hence, DEDEKIND's second objection is overcome.

When CANTOR heard from DEDEKIND, he was not immediately sure how to overcome the first objection (CANTOR/DEDEKIND *(1937:49)*). Yet he was certain that he did not want to limit his theorem to *finite* many-one correspondences with the consequent loss of generality. Then a little while later he found a way around the first objection. This discovery led him to write out a fuller version of his proof in a paper which was presented to the Königliche Gesellschaft der Wissenschaften at Göttingen on 12 February 1879. Having only recently been elected a Corresponding Member of the society, he felt the society's *Nachrichten* was a good medium for publication. In this paper, 'Über einen Satz aus der Theorie der stetigen Mannigfaltigkeiten' *(1879)*, his theorem appears in a slightly more general form *(1932:136)*:

> Hat man zwischen zwei stetigen Gebieten M_μ und M_ν eine solche Abhängigkeit, dass zu jedem Punkte z von M_μ *höchstens* ein Punkt Z von M_ν, zu jedem Punkte Z von M_ν *mindestens* ein Punkt z von M_μ gehört, und ist ferner diese Beziehung eine stetige, so dass unendlich kleinen Änderungen von z unendlich kleine Änderungen von Z und auch umgekehrt unendlich kleinen

[20] In the end someone else edited DIRICHLET's lectures, *viz.*, P. GRUBE (P. LEJEUNE DIRICHLET, *Über Kräfte, die im umgekehrten Verhältnis des Quadrates der Entfernung wirken*, Leipzig, 1876 and 1887). However, DEDEKIND continued working out his ideas on analytic topology in another paper of about 1892, published in *(1931:356–370)*.

[21] We still insure that $G_{\mu-1}$ corresponding to $K_{\mu-1}$ falls within K_{n-1}.

Änderungen von Z unendlich kleine Änderungen von z entsprechen, so ist $\mu \geqq \nu$.

(If one has a correspondence between two continuous domains M_μ and M_ν such that to each point z of M_μ at most one point Z of M_ν corresponds and to each point Z of M_ν at least one point z of M_μ corresponds, and further if this relation is continuous so that infinitely small changes of Z correspond to infinitely small changes of z and also, conversely, infinitely small changes of z correspond to infinitely small changes of Z, then $\mu \geqq \nu$.)

The fact that in this version of the theorem CANTOR just assumes that the many-one correspondence from M_μ to M_ν is partial does not affect the proof in any way.[22] His published proof is in essence the same as the one he communicated to DEDEKIND by letter. It differs only by including attempts to overcome DEDEKIND's two objections. The second objection is dealt with in the way suggested above. With regard to the first he became convinced that he could *ignore* the possibility that points ζ may coincide with A (inasmuch as some of the a's associated with A may be on the sphere $K_{\mu-1}$). We still get a continuous correspondence between those points of $K_{\mu-1}$ which have corresponding points in M_n *and yet are not mapped to* A and certain points Z' on K_{n-1}. We can still form the line APB in M_n. It corresponds to a continuous domain N in M_μ that runs from a to one or more of the b's associated with B and that has no point in common with $K_{\mu-1}$. Indeed N includes no a points even if some of these happen to be on $K_{\mu-1}$, because the line APB does not return to A after leaving A. Hence, when it came to publication CANTOR had no qualms about the validity of his theorem and proof.

In spite of CANTOR's convinctions his final proof is still open to criticism; it is flawed. DEDEKIND put his finger on a sensitive place in the proof when he drew attention to the possibility of an infinite many-one correspondence, especially in his second objection. In this case he wondered whether there would always be a curve leading from a to b corresponding to APB. However, when CANTOR's paper appeared no one redeveloped or continued the line of criticism which DEDEKIND had put to CANTOR in their private correspondence. It seems the paper of 1879 attracted little attention. It was not until 1899 that JÜRGENS published a direct criticism of CANTOR's proof in a review of the problem situation concerning dimensional invariance *(1899:53)*. He refuted the intermediate value theorem upon which the proof is based by providing the following counterexample. Consider the mapping from points x in the open interval $(1,2)$ to all irrational numbers y for which $3/8x < y < 5/8x$. In an intuitive sense this function is continuous, although as JÜRGENS pointed out there are difficulties with defining continuity for multi-valued functions.[23] Yet the intermediate value theorem does not hold, because y does not take the rational values. Furthermore, if we consider the images of the interval $(1,2)$, there is no continuous path stretching from an image point of an x close to 1 to an image point of an x close

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[22] Note that in this version of the theorem he has now spelt out the requirement that his correspondence be continuous in both directions, *i.e.*, that it be bicontinuous.

[23] To be sure, CANTOR did not reckon with these difficulties explicitly.

to 2. However, CANTOR required a similar path in his proof, since he relied on the existence of a continuous path from a to one of the b's corresponding to the line APB. JÜRGENS' counterexample shows that such a path need not exist. So CANTOR's proof fails.

From our present vantage point the attempted proofs of dimensional invariance put forward during 1878 and 1879 do not appear very satisfactory. Having the great benefit of hindsight, we can see that the mathematicians of the period were struggling to subdue a difficult problem with inadequate weapons. Fully developed topological methods and ideas were not available to them. Their best means of attack lay in analysis. Indeed with only these means they handled their problem very skillfully. It must be conceded that the standard of rigour and critical argument among them was high, given the poverty of their methods. Of course, some topological ideas were implicit in the Weierstrassian analysis of their day. For example, there were reasonable definitions of interior point, boundary point, and connectedness. Also they could and did use the intermediate value theorem together with its implications with a certain amount of topological understanding. Yet we can see that the mathematicians of 1878–1879 frequently felt the need to go beyond the slender means at their disposal. The general proofs of THOMAE and NETTO give evidence of this fact. However, their contemporaries quickly sensed the lack of rigour in these proofs. They soon realised that much more was required to make them cogent.

In my judgment the best proof of all from this period is JÜRGENS'. His linking of a demonstration of the planar domain invariance theorem with dimensional invariance gave him a deep insight into the problem situation. Unfortunately he was not a first-class mathematician, so that he was not able to develop his important line of thought into more general results. He was no BROUWER. Perhaps if he had chosen a better way of publishing his research, his work might have received more attention. Then others might have taken it up and developed it further.

It is clear that around 1880 adequate topological tools in mathematics were badly needed. The attempts to prove invariance provided some motivation for these to be fashioned. Other parts of mathematics cried out for their development. DEDEKIND's letter to CANTOR of 19 January 1879 is a definite expression of the need for a full programme of analytic topology. Thus, before considering later efforts towards solving the dimensional invariance problem, we must take account of the earlier development of point set topology in the next chapter.

Chapter 4. The Rise of Point Set Topology

By the 1880's most mathematicians thought that CANTOR's paradox about dimension had been explained away by either NETTO's or CANTOR's proof of dimensional invariance. JÜRGENS was virtually the only one to voice dissent, but his acute criticism of the NETTO proof went largely unnoticed. The consensus was that the invariance problem for dimension had been solved. For example, we find FELIX KLEIN drawing attention to CANTOR's paradoxical result in his important little booklet of 1882, *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale (1882)=(1923:527)*, but he was not overly concerned about the difficulties that it presented. Indeed he was examining RIEMANN's function theory on the intuitive geometrical level. WALTER VON DYCK (1856–1934) in his first 'Beiträge zur Analysis situs' pointed to the result of CANTOR, but then he immediately held up LÜROTH's special proof and CANTOR's general proof of invariance as guarantees for the soundness of the dimension concept *(1888:457–458)*. In a short paper published in 1887 GINO LORIA (1862–1939) described the situation concerning dimension and its invariance for an Italian readership. In the paper he summarised CANTOR's correspondence result between the unit interval and the unit cube of n dimensions and then repeated NETTO's general invariance proof *(1887)*.[1] Hence, among mathematicians the generally held opinion during the last two decades of the nineteenth century was that the invariance of dimension had been proved. This opinion is echoed in ARTHUR SCHOENFLIES' *Encyklopädie* article, 'Mengenlehre', submitted in November 1898 *(1898:187)*. However, from our vantage point we know that the situation with regard to dimensional invariance was not as satisfactory as it then seemed to most.

It was a revolution which changed attitudes to the dimension problem – a revolution in topology brought about by the rise of the theory of sets and particularly the theory of sets of points. Point set theory (Punktmannigfaltigkeitslehre) is the creation of one man: GEORG CANTOR. Although he did not contribute very much to topology directly, his point set theory added an entirely new perspective to topological thinking. Today we regard dimension theory primarily as a branch of set-theoretic topology and so the development of point set theory eventually had the most profound effect on the subject. In this chapter we must go beyond the confines of the history of dimension theory and take account of the rise of set theory and point set theory.

CANTOR published his most important investigations into the theory of sets of points in a series of six papers entitled 'Ueber unendliche, lineare Punktmannichfaltigkeiten' *(1879a) (1880) (1882) (1883) (1883a) (1884)=(1932:139–246)*. These brilliant papers constitute the 'quintessence of CANTOR's lifework' (as ZERMELO has said in CANTOR *(1932:246)*). Much of the material concerns the founding of the theory of transfinite cardinals and ordinals. However, CANTOR's main object of study is linear point sets and point sets in the n-dimensional arithmetic continuum (Euclidean n-space) and their properties. The resulting

[1] *Cf.* also the invariance proofs of F. GIUDICE *(1891)* and M. DEL GIUDICE *(1904)*, which, however, are not very successful.

theory became the basis for point set topology. The fundamental concept is that of limit point (Grenzpunkt; or in later terminology, Häufungspunkt: accumulation point): a point p is a limit point of a set P if every neighbourhood (Umgebung) of the point, however small, contains several points of the set *(1932: 149) (cf. (1872)=(1932: 98))*. The fundamental theorem is the so-called BOLZANO-WEIERSTRASS theorem, used by WEIERSTRASS in his lectures and known to CANTOR from these, which states *(1932: 149)* that every infinite set of points in a bounded region of n-space possesses at least one limit point.[2] From this fundamental concept and theorem flow all of CANTOR's deep point set-theoretic concepts. Among these are the notions of a derived set (abgeleitete Punktmenge, Ableitung) *(1932: 98, 139–140)*, an everywhere dense set in a given interval (überall-dichte in einem gegebenen Intervalle Punktmenge) *(1932: 140–141)*, an isolated point set (isolierte Punktmenge) *(1932: 158)*, and a nowhere dense set (in keinem Intervalle überall-dichte Punktmenge) *(1932: 161)*. By introducing these notions CANTOR opened up a whole new world of ideas for the emerging field of topology.

As an example of his procedure let us see how CANTOR investigates the concept of continuum in the tenth section of his *Grundlagen einer allgemeinen Mannigfaltigkeitslehre (1883a)*. CANTOR was very proud of his definition of continuum *(1883a)=(1932: 190–194)* as he felt it solved an ancient problem of both mathematical and philosophical importance. In the *Grundlagen* he gives his solution within the scope of the 'n-dimensional plane arithmetic space G_n' ('der n-dimensionale ebene *arithmetische* Raum', *i.e.*, n-dimensional Eucludean space), consisting of the set of all value systems (points)

$$(x_1 \mid x_2 \mid \dots \mid x_n)$$

of real numbers x_i (under the definition of CANTOR) provided with the usual Euclidean metric or measure of distance between points. The question then is: When is a set of points of G_n a continuum or a continuous set?

In order to answer this question, CANTOR first introduces the concept of a perfect set (perfekte Punktmenge): a perfect set is one which is identical with the set of all its limit points, *i.e.*, its first derived set,

$$S \equiv S^{(1)},$$

and hence is identical with its derived sets of all orders. Very perceptively he realises that a perfect set need not be dense in any interval and in a footnote *(1932: 207)* he gives the classic example of a nowhere dense perfect set, the famous Cantor ternary set or discontinuum: the set of all real numbers of the form

$$z = \frac{c_1}{3} + \frac{c_2}{3^2} + \cdots + \frac{c_\nu}{3^\nu} + \cdots,$$

where the coefficients can only take the values 0 and 2. CANTOR then introduces the concept of connectedness (zusammenhängende Punktmenge). Previously

[2] *Cf.* BOLZANO *(1817)* and WEIERSTRASS *(1927: 56)*.

WEIERSTRASS had used the notion that a set is connected if every pair of points of it can be joined by a polygonal path in the set.[3] However, CANTOR wanted a concept within the domain of his new theory of sets. A set T is connected in CANTOR's sense if for every pair of points t, t' of T and every given arbitrarily small number ε we can choose a finite number of points $t_1, t_2, \ldots t_\nu$ in T such that the distances $\overline{tt_1}, \overline{t_1t_2}, \overline{t_2t_3}, \ldots \overline{t_\nu t'}$ are all less then ε. This form of metric connectedness, sometimes called the property of being well-chained, remained a part of point set theory for a long time after CANTOR, until RIESZ, LENNES, and HAUSDORFF put forward the 'modern' concept of connectedness in the first years of the twentieth century.

Finally CANTOR asserts that a point set of G_n will be a continuum (Kontinuum) if it is both perfect and connected. In a footnote he adds *(1932:208)* the definition of a semicontinuum as an imperfect connected set of the second number class ($\aleph_1$) with the property that every pair of points can be joined by a complete continuum. He boasts that his conception of continuum is superior to those of BOLZANO and DEDEKIND, although his criticism of the BOLZANO notion is founded on a misunderstanding of BOLZANO's ideas.[4] In any case, CANTOR's new set-theoretic idea of continuum was of major significance in the early development of analytic topology. It constituted a new insight into a traditional informal concept.

In the course of analysing the continuum concept CANTOR asserted a false proposition *(1932:193)* which the Scandinavian mathematician IVAR BENDIXSON (1861–1935) swiftly criticised.[5] The revision of the offending proposition led CANTOR to define the extremely important notion of a closed set (abgeschlossene Menge) *(1932:226)*: a closed set is one which contains all of its limit points. On the other hand, a dense-in-itself set (in sich dichte Menge) *(1932:228)* is one which is contained in its set of limit points or its first derived set. A perfect set is then both closed and dense-in-itself.

When investigating the concept of continuum, CANTOR *(1932:207–208)* noted that his notion was independent of the idea of dimension. He promised to take up the problem of defining the dimension concept for continua, but he never did. Perhaps his mental illness prevented him from doing so. Nevertheless, he had a clear interest in the definition problem for dimension motivated by his own paradoxical discovery.

CANTOR's imaginative ideas had the most profound effect on the growth of mathematics. Analysts were the first to see their usefulness. Point set theory offered wonderful new instruments for a detailed study of the nature of functions, with the result that the growth of real and complex function theory was greatly accelerated in the years after the publication of CANTOR's great papers. Applications of the Cantorian toolkit to the fundamental notions of geometry came a little later. GIUSEPPE PEANO and CAMILLE JORDAN were among the

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[3] *Cf.* WEIERSTRASS *(1895:203) (1927:57)*. However, he only defined connectedness in conjunction with his concept of continuum (connected open set).

[4] *Cf.* my *(1977:283–284)*.

[5] During 1883 CANTOR and BENDIXSON had a substantial correspondence concerning the difficulty. *Cf.* BENDIXSON *(1883)*.

first to take the Cantorian ideas into the domain of geometry, so their work became a vital background to the development of point set topology.

Among topologists CAMILLE JORDAN (1838–1922)[6] is best known for his celebrated theorem on closed curves in the plane. He first enunciated this result in a 'Note' at the end of the first edition of his *Cours d'Analyse.* In order to understand why JORDAN wanted to state and prove this theorem, we must consider a few aspects of his analysis text.[7]

The differences between the first and second editions of Jordan's *Cours d'Analyse* could hardly be greater. In terms of the state of mathematics and standards of rigour of the 1880's and '90's the editions are worlds apart. JORDAN's attempt to prove the curve theorem is bound up with these differences. The first edition, published in three volumes between 1882 and 1887, is very much a *Cours d'Analyse de l'Ecole Polytechnique (1882) (1883) (1887).* It is not a research treatise, but a textbook for students with an emphasis on methods. However, in the 'Preface' JORDAN writes *(1882*:v*)*:

> Nous avons apporté un soin particulier à l'établissement des théorèmes fondamentaux. Il n'en est aucun dont la démonstration ne soit subordonnée à certaines restrictions. Nous nous sommes efforcé d'apporter dans cette discussion, parfois délicate, toute la précision et la rigueur compatibles avec un enseignement élémentaire.
>
> (We have taken particular care in the establishing of fundamental theorems. There is none for which the demonstration has not been subordinated to certain restrictions. We have tried to bring into the discussion, at times delicate, all the precision and rigour compatible with an elementary education.)

In spite of this declaration of intended rigour[8] the first *Cours* is virtually pre-Weierstrassian on the fundamentals of analysis. Of course, JORDAN was well aware of the new Weierstrassian ideas, which by the 1880's were having a powerful impact in France, but apparently he did not wish to expose his students to their full force. Nevertheless, in order to assuage his conscience about matters of rigour JORDAN appended a 'Note sur quelques points de la théorie des fonctions' at the end of the last volume of the first edition *(1887:549–615).* In this place he revises proofs of the main text according to the highest standards of rigour then current. It is in this 'Note' that the closed curve theorem first appeared.

The second edition of the *Cours*, published between 1893 and 1896, contrasts markedly with the first in content and style. In 1883 JORDAN succeeded LIOUVILLE to the chair of mathematics at the Collège de France; he had been *suppléant* to SERRET at the Collège since 1875. At this ancient institution he could lecture on his own research interests and this fact is reflected in the second edition of his book. It should really be entitled *Cours d'Analyse du Collège de*

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[6] The standard biography of JORDAN is LEBESGUE *(1926).*

[7] I intend to examine the origins of the JORDAN curve theorem more fully in a future paper. GUGGENHEIMER *(1977)* has analysed some of the history of the theorem.

[8] JORDAN's declaration harks back to the opening of CAUCHY's *Cours d'Analyse.*

France, for it more accurately describes his analysis lectures at the Collège than at the École Polytechnique.[9] It is much more a research treatise; the standard of rigour is far higher. JORDAN incorporates the material of the earlier 'Note' into the main text of the first volume and includes much new research material.[10]

The curve theorem is a part of this radical transformation from the first to the second edition and, in particular, it is connected with proofs of CAUCHY's integral theorem in complex analysis. In the first edition JORDAN *(1884: 275–277)* proves the CAUCHY theorem in much the same way as CAUCHY himself had done in 1825 – by a variational calculus technique showing that the first variation of the integral is zero. This proof hardly comes up to standards being expected in the 1880's, when quite a few mathematicians were scrutinising the theorem and subjecting it to further proof analysis, although it is probably sufficient for a student text.[11] However, in the 'Note' JORDAN reexamines the CAUCHY theorem, stating it in the following form *(1887: 605)*:

> Soit C une ligne continue fermée et sans points multiples, ne contenant à son intérieur aucun point critique de la fonction $f(z)$. L'intégrale $\int f(z)\,dz$, prise le long d'une ligne fermée et rectifiable quelconque K intérieure à C, est nulle.

> (Let C be a closed continuous line without multiple points, not containing any critical points of the function $f(z)$ on its interior. The integral $\int f(z)\,dz$, taken along any closed and rectifiable line K, is zero.)

From the very statement of the theorem one can discern that JORDAN had himself subjected the theorem and its proof to a detailed analysis. For example, simple closed lines or curves, which are assumed to have interiors, figure in the restatement of the old theorem. Moreover, in the 'Note' he provides his own definition of rectifiability *(1887: 594–598)*, which he immediately links with his earlier defined concept of a function of bounded variation *(1881)*. He also takes the trouble to prove the existence of the integral of a complex function *(1887: 603–605)*.

Thus while previous mathematicians hardly saw the need to prove the obviously true statement that a simple closed curve divides the plane into an inside region and an outside region, JORDAN required a proof, and a lengthy one at that *(1887: 587–594)*, as a consequence of his analysis of the CAUCHY theorem.[12] In the 'Note' he defines a curve as the sequence of points represented by the equations

$$x = f(t), \qquad y = \phi(t),$$

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[9] The student editions of JORDAN's analysis courses at the École Polytechnique reveal that he never treated analysis there as rigorously as in his published second edition. The second edition owes much to a course which he gave at the Collège de France during the year 1891–92 entitled, 'Principes du calcul infinitésimal'.

[10] LEBESGUE *(1926)* essentially makes the point of this paragraph.

[11] Probably the most important new proof of the CAUCHY theorem to appear during the 1880's was GOURSAT *(1884)*. This led to his celebrated improvement of the theorem *(1900)*.

[12] BOLZANO was actually the first to state the JORDAN curve theorem and he did so many years before JORDAN, but he gave no proof. *Cf.* my *(1977)*.

where f and ϕ are functions of the parameter t. If the functions are continous, the curve is said to be continuous and if they have a common period, the curve is said to be closed. If for certain distinct values of t within the period the values of x are the same and the values of y are the same, then the closed curve has multiple points. However, JORDAN only considers continuous closed curves without multiple points, *i.e.*, simple closed curves, and so states the theorem *(1887: 593)*:

> ... toute courbe continue C divise le plan en deux régions, l'une extérieure, l'autre intérieure, cette dernière ne pouvant se réduire à zéro, car elle contient un cercle de rayon fini.
>
> (... Every continuous curve C divides the plane into two regions, the one exterior, the other interior; the latter cannot be reduced to zero, because it contains a circle of finite radius.)

JORDAN's proof is based on polygons approximating the curve C from the inside and from the outside. He assumes the theorem for simple polygons, *i.e.*, he assumes that a polygon without multiple points divides the plane into an interior and an exterior region. First, according to JORDAN we can construct a simple polygon P' which approximates C to any degree of accuracy. Then he offers a way to construct simple polygons S and S' from P' which are inside and outside of C, respectively. These approximate C and contain it within an annular region. Indeed an entire sequence of interior polygons S, $S_1, \ldots$, each inside the next, and an entire sequence of exterior polygons S', $S'_1, \ldots$, each outside the next, can be constructed. These approximate C with increasing accuracy and corresponding pairs form smaller and smaller annular regions which squeeze down to C. Hence, the theorem follows according to JORDAN.

Later generations of mathematicians have come to regard the closed curve theorem as a difficult part of elementary topology. As a first attempt JORDAN's proof is quite good, although it was not many years before mathematicians began to point out flaws.[13] ARTHUR SCHOENFLIES was the first critic, but initially he only criticised the proof for its complication. He followed his criticism with a proof of a less general version of the theorem – a proof which is not completely adequate *(1896)*. SCHOENFLIES was also the first to draw attention to the topological significance of the result. JORDAN was interested in the theorem for its immediate applications in analysis. In fact, the reason why he employed polygonal approximations in the proof is to be found in his application of the result to the CAUCHY integral theorem. For that theorem he wanted a polygonal path of integration on the interior of the simple closed curve *(1887: 605)*. Yet even if JORDAN did not fully understand the topological significance of the closed curve theorem, in the end we must credit him with stating and trying to prove something of great importance.

[13] It is now well known that the method of polygonal approximations is an inefficient way of proving the JORDAN curve theorem. VEBLEN *(1905)* initiated the direct method of proving the theorem; *cf.* BROUWER *(1910g)*. Early criticisms of JORDAN's proof are to be found in: SCHOENFLIES *(1896)*, VEBLEN *(1905)*, AMES *(1905)*, SCHOENFLIES *(1906) (1908: 169) (1925)*. *Cf.* GUGGENHEIMER *(1977)*.

Mathematicians thinking about the dimensional invariance problem soon realised that the JORDAN curve theorem could be useful to them, because they perceived that an n-dimensional generalisation – the theorem that states that an $(n-1)$-dimensional sphere and its homeomorphs divide an n-space into two regions – yields an n-dimensional domain invariance theorem and then the invariance of dimension. However, no one genuinely understood the difficulty of proving an n-dimensional JORDAN separation theorem until BROUWER.

In the first volume of the second edition of his *Cours d'Analyse* JORDAN included a slightly revised proof of the curve theorem *(1893: 90–100)*. This is where most mathematicians first came across the theorem. He also included a full section *(1893: 18–31)* on Cantorian set theory, incorporating material from a research paper on the integral of the previous year *(1892)*. He based the set theory on n-spaces provided with an *écart* (distance measure).[14] Within the section on set theory there is a new definition of connectedness (d'un seul tenant) *(1893: 25)*: a set is connected if it cannot be separated into several closed sets. This definition is equivalent to CANTOR's inasmuch as JORDAN only considered closed and bounded (*i.e.*, compact) sets. In this special case it is also equivalent to the usual definition (due to RIESZ, LENNES, and HAUSDORFF). JORDAN's overall contribution to point set topology was substantial, although he only saw the subject in the context of analysis. Numerous mathematicians read the second *Cours d'Analyse* and through it learned about the set-theoretic foundations of modern analysis.

GIUSEPPE PEANO (1858–1932)[15] was in many ways JORDAN's rival in mathematics. PEANO and JORDAN separately attacked the problems of measure and integration, but their results were remarkably close (PEANO *(1887)*, JORDAN *(1892)*). They proposed similar definitions of rectifiability (JORDAN *(1887: 594)*, PEANO *(1890b)*), although PEANO's is somewhat more general. It is interesting to find an early criticism by PEANO of a result in the first edition of JORDAN's *Cours*. The dispute over JORDAN's mistake occupies several pages of the 1884 volume of the *Nouvelles Annales de Mathématiques*.[16] From the point of view of the development of topology both saw the importance of applying Cantorian set theory to intuitive geometrical ideas.

PEANO's example of a space-filling curve, a curve which covers all the points of a square *(1890)*, is the most spectacular example he ever devised. It completely upset the 'geometrical intuitions' of the mathematicians of the day. It is not known precisely how PEANO came to devise such a counterintuitive curve, but we can see that much of his earlier work of the 1880's was directed towards a critical appraisal of commonly-held mathematical notions. His analysis text *Calcolo Differenziale e Principii di Calcolo Integrale* (1884) abounds with examples demonstrating the need to revise the fundamentals of the subject then taken for granted. In his *Applicazioni Geometriche del Calcolo Infinitesimale*

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[14] JORDAN's *écart* idea seems to have been an important source of inspiration for FRÉCHET when defining the concept of (E) class (metric space) in *(1906)*.

[15] PEANO *(1973)* contains a good biography by H.C. KENNEDY.

[16] *Cf. Nouvelles Annales de Mathématiques* (3) **3** (1884), 45–47, 153–155, 252–256, 475–482.

(1887: 152–259) PEANO devotes a chapter to the study of geometrical magnitudes, giving definitions of the interior and exterior measure of linear sets, plane areas, and spatial volumes. One would expect that the curve bounding an area would contribute nothing to the measure of that area, *i.e.*, that the curve itself could be squeezed within an arbitrarily small region. However, taking a set-theoretic view, PEANO realises that the interior area and the exterior area of a planar region can be different. Possibly these ideas motivated PEANO's investigation of planar curves defined by continuous functions.

In the short paper 'Sur une courbe, qui remplit toute une aire plane' *(1890)* PEANO defines his curve in a purely analytic way without reference to geometrical considerations. This procedure accords with his critical 'antigeometrical' attitude to analysis. For the construction we consider x and y as continuous functions of the parameter t in the interval $[0, 1]$; as numbers between 0 and 1, x, y, and t are given ternary (base 3) representations.[17] The aim is to put the ternary fractions

$$t = 0.a_1 a_2 a_3 \ldots,$$

representing points of the unit interval, into continuous correspondence with the ternary fractions

$$x = 0.b_1 b_2 b_3 \ldots,$$
$$y = 0.c_1 c_2 c_3 \ldots,$$

representing points in the unit square. To achieve this aim we let $\boldsymbol{k}a$ (the 'complement of a') denote the digit $2-a$, where a is one of the digits 0, 1, or 2. Thus

$$\boldsymbol{k}0 = 2, \quad \boldsymbol{k}1 = 1, \quad \boldsymbol{k}2 = 0.$$

$\boldsymbol{k}^n a$ is the result of applying the operation $\boldsymbol{k}$ n times to a and it has the value a, if n is even, and $\boldsymbol{k}a$, if n is odd. The correspondence between t and (x, y) is given by the following equations:

$$\begin{array}{ll}
b_1 = a_1, & c_1 = \boldsymbol{k}^{a_1} a_2, \\
b_2 = \boldsymbol{k}^{a_2} a_3, & c_2 = \boldsymbol{k}^{a_1 + a_3} a_4, \\
b_3 = \boldsymbol{k}^{a_2 + a_4} a_5, & c_3 = \boldsymbol{k}^{a_1 + a_3 + a_5} a_6, \\
\ldots & \ldots \\
b_n = \boldsymbol{k}^{a_2 + a_4 + \cdots + a_{2n-2}} a_{2n-1}, & c_n = \boldsymbol{k}^{a_1 + a_3 + \cdots + a_{2n-1}} a_{2n}.
\end{array}$$

It is easy to see that every point of the square is covered and that the correspondence is continuous by the very way the ternary representation proceeds. In his paper PEANO briefly indicates how the correspondence can be generalised to the cube. In fact, extending it to an n-cube is not difficult.

Although PEANO described his curve analytically, soon after the publication of his paper HILBERT *(1891)* presented a geometrical method for constructing a space-filling curve as the limit of a sequence of polygonal paths. Later SCHOEN-

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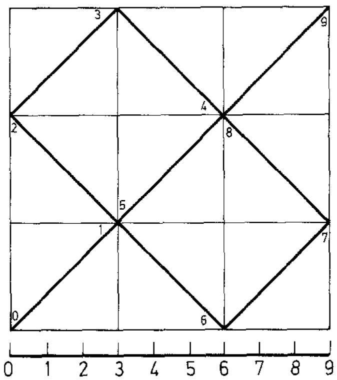
[17] PEANO carefully distinguishes between a numeral and the number it represents, but to simplify the exposition I have not emphasised this distinction.

FLIES *(1900: 121–123)* and the American mathematician E. H. MOORE *(1900: 77)* showed how to 'visualise' a construction of PEANO's original example. The basic figure is the following:



As the first step of the construction we divide the unit square into 9 subsquares and draw a polygonal line (corresponding to the unit segment) through the square, as shown in the figure. In the second step we subdivide each small square and draw similar polygonal lines in each. If we continue in this manner, the limit curve is PEANO's example; the entire square is covered.

PEANO's curve was a second blow to the naive concept of dimension after CANTOR's one-one correspondence, for it provides a *continuous mapping* between a 1-dimensional object and a 2-dimensional one. However, PEANO himself was quick to explain away the difficulty. Because some numbers have two distinct ternary representations, certain points of the square are covered twice and certain others are covered four times by the curve. Thus the PEANO curve describes a continuous, but not a one-one correspondence. PEANO interpreted this fact as a confirmation of NETTO's proof of the invariance of dimension, which is cited in his paper. Hence, we see that even PEANO, usually a very perceptive critic, detected no flaw in the earlier general invariance proofs.

HILBERT in his paper *(1891: 461)* noted that it is possible to devise space-filling curves which cover the points of the square at most three times. Much later LEBESGUE found that it is *not* possible to give such curves with the points covered at most twice. He discerned in this fact a key to proving dimensional invariance! (See chapter 7.)

PEANO's 'curve' clearly demonstrates that the usual way of defining a curve through a pair of continuous functions of a parameter leads to counterintuitive results. Presumably no one wants to think of a square as a curve. It is not surprising that PEANO's example immediately captured the attention of mathematicians and soon they were proposing new definitions of the curve concept in order to exclude the PEANO monsters. This work marks a starting place in the history of topological curve theory, a subject closely related to dimension theory. JORDAN, however, never publicly recognised PEANO's strange result. In the first *Cours (1887: 587)* he had defined curves through continuous functions and in subsequent editions he never bothered to change his old definition. Moreover, he had an interest in determining when a closed curve encloses a well-defined area in the sense of measure theory and proved in the 'Note' *(1887: 599–600)* that the area will certainly be defined in the case of a rectifiable curve. A few years later in 1894 he asked the following question in *L'Intermédiaire des Mathématiciens (1894a)*:

> Pourrait-on signaler une courbe
>
> $$x=f(t), \quad y=\phi(t)$$
>
> (f et ϕ étant des fonctions continues) dont l'aire fut indéterminée?
>
> (Can one describe a curve
>
> $$x=f(t), \quad y=\phi(t)$$
>
> (f and ϕ being continuous functions) for which the area is indeterminate?)

PEANO *(1896)* responded to JORDAN's question by pointing to his space-filling curve. If we take any arc of the curve and connect the endpoints with an ordinary curve so as to give a closed curve, then we have a curve with an interior area strictly less than its exterior area. But JORDAN never acknowledged PEANO's response and did not alter his definition of continuous curve in the third edition of the *Cours d'Analyse*.

PEANO's spectacular example is but one in a long list of strange curves and point sets which run counter to 'naive geometrical intuition'. Set theory has been the main breeding ground for such examples. Two other striking entries in this list may be mentioned. In 1903 WILLIAM OSGOOD *(1903)* and HENRI LEBESGUE *(1903–05)* each published an example of a JORDAN curve (*i.e.*, a simple closed curve) possessing a *positive* exterior measure, *i.e.*, the curve itself has a measurable area. This curve is even stranger than the one which PEANO suggested in his response to JORDAN's question, because it has no multiple points. In 1910 L. E. J. BROUWER *(1910d)* published the first examples of indecomposable continua and curves which divide the plane into three or more regions but which are the common boundaries of all the regions. One might say that in the years around 1900 set theory bred a plethora of topological monsters. An early catalogue of these is contained in the first book in English on set theory, WILLIAM H. and GRACE CHISHOLM YOUNG's *Theory of Sets of Points (1906)*.[18] Of course, for mathematicians well trained in Cantorian methods the monsters became manageable, indeed friendly, and they guided the way to quite a few new results. Thus pathological oddities were an important spur to growth in set-theoretic topology.

Given the wealth of results from the papers of CANTOR and many others, certain mathematicians recognised the need for an entire programme of exploration in set-theoretic topology. At the First International Congress of Mathematicians, held in Zürich from 9 to 11 August 1897, ADOLF HURWITZ (1859–1919) sketched such a plan of investigation *(1898)*. In his lecture reviewing

[18] *Cf.* SCHOENFLIES *(1906a)*.

progress in analytic function theory HURWITZ *(1898: 101–104)* tries to determine the precise domain of validity of the CAUCHY integral theorem and in so doing points to the important work of JORDAN. He asks the highly relevant general questions *(1898: 101)*:

> ... was ist eine einfach geschlossene Linie, was ist eine Linie, insbesondere eine geschlossene Linie überhaupt, und sind alle oder nur gewisse geschlossene Linien in dem Ausspruch des Cauchy'schen Satzes zulässig?
>
> (... What is a simple closed line, what is a line, especially, a closed line in general, and are all or only some closed lines admissible in the enunciation of the Cauchy theorem?)

In attempting to grapple with these questions he puts them into the general context of the topology of closed sets. In a way similar to CANTOR's definition of cardinals and ordinals, we can assign closed point sets to classes: each class containing those sets which can be mapped one-one continuously onto one another. The sets in each class will be called 'equivalent'. Then *(1898: 102)*:

> Diese Einteilung der Punktmengen in Klassen bildet ... die allgemeinste Grundlage der *Analysis situs*. Die Aufgabe der Analysis situs ist es, die Invarianten der einzelnen Klassen von Punktmengen aufzusuchen.
>
> (This distribution of point sets into classes forms ... the most general foundation of analysis situs. The task of analysis situs is to search for the invariants of the single classes of point sets.)

HURWITZ gives a specific example: a geometrico-set-theoretic definition of JORDAN's idea of a simple closed curve, as a point set (in the plane) which belongs to the class of the boundary of a square. In essence, he states the KLEIN *Erlanger Programm (1872)* for analysis situs in a sharper form by placing the central problem into the framework of point set theory.

HURWITZ never contributed to the programme adumbrated in his Zürich lecture. It was ARTHUR SCHOENFLIES (1853–1928)[19] who first embarked upon such a programme. At the turn of the century SCHOENFLIES was the foremost propagandist for CANTOR's set theory. He wrote the article on 'Mengenlehre' for the *Encyklopädie der mathematischen Wissenschaften (1898)* and composed *Berichte (1900) (1908)* on progress in the field in response to a commission by the Deutsche Mathematiker-Vereinigung. As noted above he was the first mathematician to draw attention to JORDAN's curve theorem by suggesting its topological significance and giving an alternative proof.

There is a short early paper by SCHOENFLIES, 'Ueber einen Satz der Analysis Situs' *(1899)*, in which he reproves the planar domain invariance theorem in the formulation *(1899: 282)*:

> ... dass das umkehrbar eindeutige und stetige Abbild der Fläche eines Quadrats wieder ein einfach zusammenhängendes Flächenstück ist, dass es

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[19] There is little biographical material on SCHOENFLIES. However, *cf.* BIEBERBACH *(1923)*.

nämlich aus der Gesamtheit aller Punkte besteht, die dem Innern einer geschlossenen Curve angehören.

(... that the one-one continuous image of the surface of a square is again a simply connected piece of surface, *viz.*, that it consists of the totality of all points which belong to the interior of a closed curve.)

At the time this paper was presented (13 January 1900) SCHOENFLIES was unaware of JÜRGENS' proof of 1878–79 of this same result. However, in contrast to JÜRGENS' way of proceeding SCHOENFLIES relies heavily on JORDAN's demonstration of the curve theorem.[20] At the end of his paper he deduces the special case of dimensional invariance from the planar domain invariance theorem *(1899: 289–290)*. For this he uses a theorem of CANTOR *(1882: 117)* =*(1932: 153)* to the effect that a collection of closed planar surfaces (having interior points) for which no two have interior points in common is at most countable. The goal is to show that a 3-dimensional or an n-dimensional cube cannot be mapped in a one-one continuous manner onto a planar surface. Suppose such a mapping is possible. Then by the domain invariance theorem parallel square slices of the cube will be mapped onto simply connected regions bounded by JORDAN curves, with interior points corresponding to interior points. Because the mapping is one-one, the images of the square slices cannot overlap. But as there are uncountably many square sections we have a contradiction with CANTOR's theorem.[21] In the last paragraph of his paper SCHOENFLIES suggests that his method of proof for domain invariance is capable of generalisation to arbitrary spaces; he hardly realises the difficulties of such a generalisation.

It was around 1902 that SCHOENFLIES began an intensive and rapid development of a programme of set-theoretic topology. In that year he published a paper containing a statement and proof of a converse of JORDAN's theorem *(1902)*. This little paper led to three long 'Beiträge zur Theorie der Punktmengen' *(1904) (1904a) (1906)* (*cf.* *(1908a)*), having as their central theme the topology of closed curves and simple closed curves in the plane. The opening paragraph of the first 'Beiträge' paper sets the scene *(1904: 195)*:

Als eines der allgemeinsten Probleme aus der Theorie der Punktmengen kann man die Aufgabe bezeichnen, die grundlegenden Sätze der *Analysis Situs* mengentheoretisch zu formulieren und zu begründen und die Beziehungen darzulegen, die zwischen den *mengentheoretisch-geometrischen* und den *analytischen* Ausdrucksweisen derselben Begriffe und Sätze obwalten. Die paradoxen Resultate, wie sie z.B. in der eineindeutigen Abbildung der Continua und in der Peanoschen Kurve vorliegen, haben die naiven Vorstellungen der Analysis Situs gründlich zerstört. Um so mehr muss man verlangen, dass die Mengentheorie wiederum Ersatz schafft und die geometrischen Grundbegriffe in einer Weise definiert, die ihnen ihren natürlichen für die Analysis Situs charakteristischen Inhalt wieder zurückgibt. Ist auch die

[20] *Cf.* two subsequent proofs by OSGOOD *(1900)* and BERNSTEIN *(1900)*.

[21] *Cf.* a similar earlier proof of MILESI *(1892)*; criticised by SCHOENFLIES *(1908: 165)*.

vielgeschmähte Anschauung keine Quelle des Beweises, so scheint es mir doch – wenigstens im Gebiet der Analysis Situs – ein Ziel der Forschung zu sein, den Inhalt der geometrischen Definitionen mit dem Anschauungs*inhalt* in Übereinstimmung zu bringen.

(As one of the most general problems of the theory of point sets we can point to the task of formulating and establishing the fundamental theorems of analysis situs set-theoretically and setting forth the relationships which exist between the set-theoretico-geometric and the analytic modes of expressing these concepts and theorems. The paradoxical results, as they occur, *e.g.*, in the one-one mapping of continua and in the Peano curve, have completely destroyed the naive ideas of analysis situs. All the more we must demand that set theory provide a substitute and define the basic geometrical concepts in a way that gives back to them their natural content characteristic of analysis situs. Even if the much maligned intuition is no source of proof, it still seems to me that it is a goal of research to reconcile the content of geometrical definitions with the content of intuition – at least in the domain of analysis situs.)

When submitting his first 'Beiträge', SCHOENFLIES put the following methodological point into the accompanying letter to his friend HILBERT (letter of 28 April 1903):[22]

Wir stehen mit dem, was den Inhalt der Analysis Situs ausmacht, grossenteils noch auf dem Boden der Anschauung, die zugleich als Beweisgrund dienen muss. Aber es dürfte nötig sein, auch auf diesem Gebiet nach grösserer Exactheit zu streben, und seine Sätze aus den geometrischen und mengentheoretischen Grundtatsachen abzuleiten. Dies allmählich zu tun, ist mein lebhafter Wunsch. Ich habe wieder Lust bekommen, hierüber intensiver zu arbeiten, und hoffe, es wird mir mit der Zeit gelingen.

(With regard to what constitutes the content of analysis situs, to a large extent we still rely on intuition which at the same time must serve as a basis of proof. Yet it may be necessary to strive for even greater exactness in this domain and derive its theorems from geometric and set-theoretic fundamentals. It is my keen wish to do this at last. I have felt like working on this more intensively and I hope that I shall succeed in time.)

SCHOENFLIES was certainly aware of the difficulties of 'naive geometrical intuition' and sought to put topology on a sound, purely set-theoretic footing. However, in view of the fate which befell his theory the methodological remark in the letter to HILBERT has an ironic ring.

In executing his programme SCHOENFLIES had most success with the converse to the JORDAN curve theorem. For this he introduced the concept of accessibility (Erreichbarkeit), at first implicitly *(1902) (1904)*, then explicitly *(1904b) (1906)*. If M is a connected domain (open set) and t is one of its boundary points, then we say that t is accessible from M if for every point m of

[22] This letter is in the HILBERT *Nachlass* in the Niedersächsische Staats- und Universitätsbibliothek, Göttingen (signature: HILBERT 355, no. 15).

M there is a simple path (einfacher Weg) in *M* leading from *m* to *t* *(1906: 296–297)*.[23] The example that motivates SCHOENFLIES' definition is the curve $\sin \frac{1}{x}$, $-1 \leqq x \leqq +1$, together with its limit points on the *y*-axis which lie between -1 and $+1$. The points on the y-axis, $-1 < y < +1$, are not accessible from an open domain surrounding the curve. However, SCHOENFLIES recognised that all the points of a JORDAN curve, *i.e.*, a homeomorphic image of a circle, are accessible from both the interior domain and the exterior domain and that this fact is the key to giving a converse to JORDAN's theorem. In SCHOENFLIES' terminology a closed curve is a bounded nowhere dense perfect connected point set which divides the plane into two connected domains having the point set as their common boundary *(1904a: 146–147)* and a simple closed curve is a closed curve with all of its points accessible from each of the two domains *(1904: 217)* *(1906: 305)*. Then the converse of the JORDAN curve theorem states that every simple closed curve can be mapped in a one-one continuous manner onto the points of a circle (proofs in *(1902)* *(1904)* *(1906)*). This result is the high point of SCHOENFLIES' topological investigations.

SCHOENFLIES gathered the results of his 'Beiträge' series and other papers *(1907)* *(1907a)* into the second part of his *Bericht* for the Deutsche Mathematiker-Vereinigung: *Die Entwickelung der Lehre von den Punktmannigfaltigkeiten* *(1908)*. In his eyes he had completed a beautiful chapter in topology, meeting the highest standards of set-theoretic rigour. However, it was scarcely a year after the appearance of this 'complete manual' of planar analysis situs that L. E. J. BROUWER delivered a resounding critique of the theory through a set of ingenious counterexamples *(1910d)*.[24] Nearly all of the theory of closed curves suffered destruction. Only the converse to the JORDAN theorem based on accessibility escaped unharmed. It turned out that the topology of the plane was more subtle than SCHOENFLIES had ever imagined.

In spite of the BROUWER critique we still must recognise SCHOENFLIES as a pioneer. He strove for a high standard of rigour and had a good set of Cantorian tools to aid him in this struggle. He also had some good insights into the pathology of curves. However, many times he was careless and disorganised in his research. 'Chaotic' is not an unfair description for some of his books and papers. More importantly, we now know that the backbone of his topological investigations, polygonal approximations, is a very inefficient device. Thus in the end the work of SCHOENFLIES in topology has been completely overshadowed by that of BROUWER and others.

To conclude this chapter I should like to describe the problem situation concerning dimensional invariance around the beginning of the twentieth century. As noted above, after the intensive work of 1878–79 most mathematicians forgot about the problem during the last two decades of the nineteenth century; they simply believed that it had been solved by NETTO and CANTOR. However,

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[23] In this definition a simple path is just a polygonal path which does not cross itself. It may have infinitely many edges, in which case the only limit point of the vertices is an endpoint.

[24] *Cf.* also DENJOY *(1910)*.

JÜRGENS had never been convinced by the earlier general proofs and so in 1899 he published his note on 'Der Begriff der *n*-fachen stetigen Mannigfaltigkeit' *(1899)* in which he thoroughly criticised the old proofs and thereby reopened the general problem. JÜRGENS was acutely aware of the difficulties in proving the invariance of dimension. As one of the major stumbling blocks he discerned the difficulty of providing an adequate definition of continuous connection and small displacement (stetiger Zusammenhang; kleine Verschiebung) for manifolds, *i.e.*, the property of nearness of points in manifolds. Thus he saw the need for a better definition of the very notion of continuous manifold. This is a problem which BROUWER later attacked and solved (see chapters 6 and 7). In a sense, only after this problem had been solved can we assign a dimension number to a manifold and then consider the invariance problem. Related to these questions is the problem of assigning dimension numbers to arbitrary subsets of a manifold. JÜRGENS realised that, apart from associating the numbers 1 through *n* to most subsets as their dimension numbers, there are certain subsets for which the notion of extension is inapplicable. He thought of these sets as consisting of infinite systems of separate points; we would call them 0-dimensional. By thus recognising the existence of 0-dimensional sets, however vaguely, he was able to criticise the proof of NETTO (see chapter 3) and also clarify the general definition problem for dimension. Overall in his short article JÜRGENS put the problems of dimension into sharper focus and so prepared the way for the work of the twentieth century topologists. In particular, he emphasised the close relation between domain and dimensional invariance.

In the same year as JÜRGENS published his critical note JACOB LÜROTH returned to the invariance problem and proved another special case *(1899)*. Then a few years later he brought together all of his results, fully worked out, in a final contribution to the subject *(1906)*. In the second part of his *Bericht* SCHOENFLIES *(1908: 164–168)* drew attention to the *open problem* of dimensional invariance, which just a few years before he had considered closed. He suggested in the second *Bericht* that the best way to tackle the problem would be through a general proof of domain invariance in *n* dimensions.

Solutions to the problems of dimensional and domain invariance were all the more urgent at this time, because mathematicians were recognising their relevance to certain 'higher' branches of mathematics. ROBERT FRICKE (1861–1930), the expert on automorphic functions at the beginning of the century and co-author with KLEIN of the classic treatise on the subject *(1897) (1912)*, underlined the pressing need to solve these problems, for their solution would help to shore up the continuity method of proof employed in the theory of automorphic functions and in uniformising analytic functions.[25] POINCARÉ and KLEIN had first introduced the method in the 1880's, but by about 1900 FRICKE became convinced that it required further justification. In a lecture delivered to the Third International Congress of Mathematicians held in Heidelberg during August 1904 FRICKE declared that the development of the theory of automorphic functions had come to a virtual standstill over this difficult point *(1905)*.

[25] *Cf.* the details in FRICKE *(1913: 445–452)*. Also *cf.* FREUDENTHAL's historical remarks in BROUWER *(1976: 572–573)* and the remarks of KLEIN in his *(1923: 731–741)*.

The method was used for proving some fundamental theorems on the existence of inverse functions to the automorphic functions (the so-called linear polymorphic functions). The leading idea of the method was to compare two manifolds of equal dimension, one of all the groups or fundamental domains of a given signature associated with a set of automorphic functions and the other of all the related RIEMANN surfaces of the same signature. The dimension of the manifolds was determined by counting the parameters given in the signature. Hence, the method partly rested on the simple coordinate or parameter idea of dimension. In the light of CANTOR's paradoxical discovery FRICKE saw that it was not completely rigorous. Domain or dimensional invariance was needed in some form. Although he did not expect that the continuity method would be invalidated over this point, he wanted to see a full topological foundation for the fundamental existence theorems. When he had struggled earlier to obtain partial proofs of the existence theorems *(1904)*, he had been reassured by SCHOENFLIES' proof of the planar domain invariance theorem that full rigour could be attained. Consequently, at Heidelberg he urged set theorists to bring the problems of domain and dimensional invariance to a satisfactory conclusion by proving the general n-dimensional cases.

We can now see that at the start of the twentieth century the problem of the invariance of dimension was wide open; it begged for solution. To be sure, mathematicians expected a positive solution. The rapid developments in set-theoretic topology seemed to give strong hints towards this end. The theory had become a very helpful tool for clarifying the basic notions and difficulties surrounding the problem. Partial solutions also seemed to open up the way to a full solution. Undoubtedly the most promising route appeared to be through invariance of domain. Yet in spite of the hopeful expectations which mathematicians held at the time, the road to success turned out to be more difficult than imagined, as we shall see in the ensuing chapters.

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