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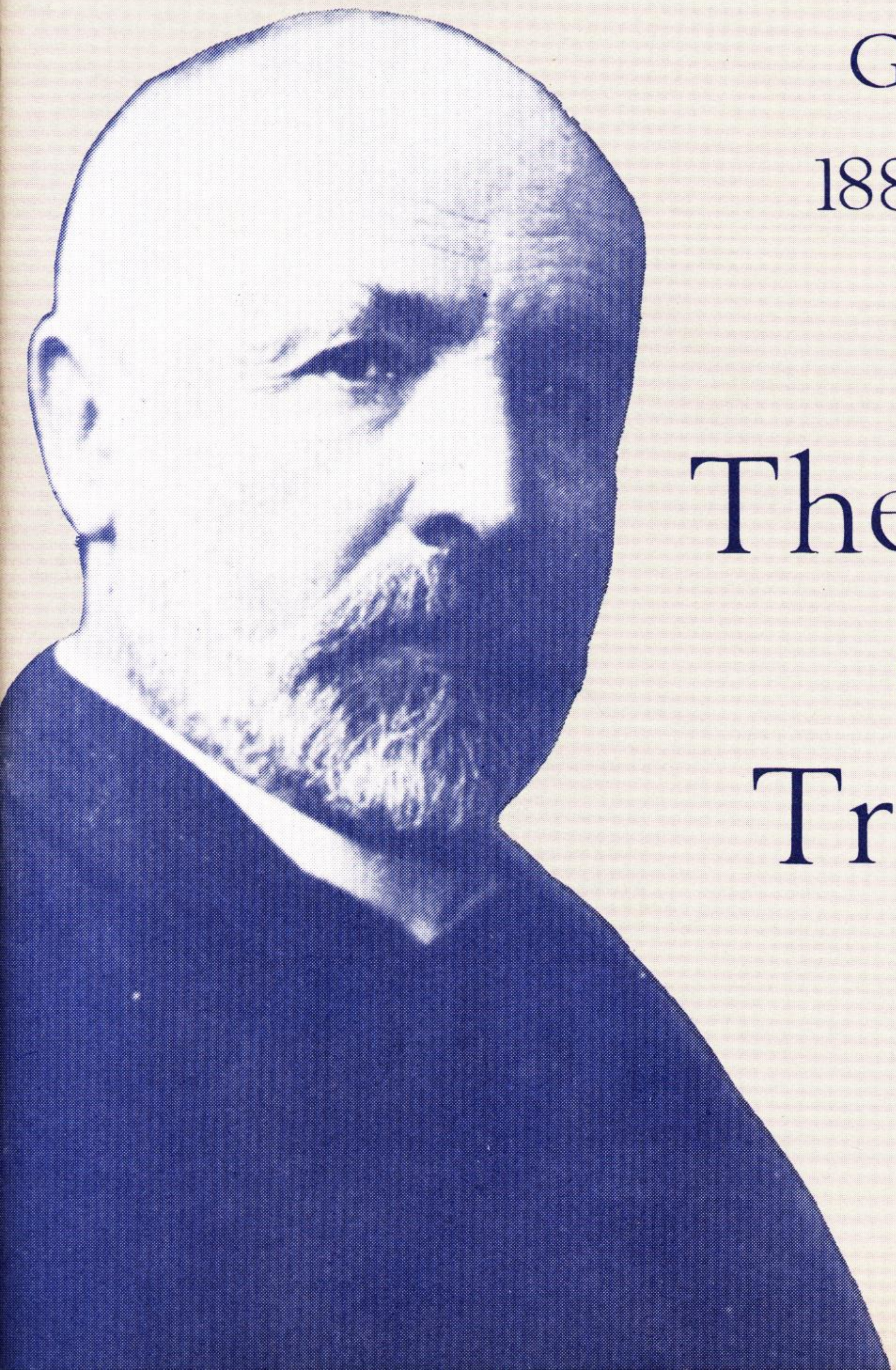

First English Translation

Georg Cantor's

1883 Grundlagen

with

The Concept of the Transfinite

by Uwe Parpart

Campaigner

Vol. 9 Nos. 1-2 Journal of the National Caucus of Labor Committees January-February 1976

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Preface

Physics and Economics

Although the contents of this issue are directly addressed to crucial problems centering around present-day physics, the more urgent and broader relevance of the material is located within present international discussions concerning the International Development Bank program (IDB) for a "new world economic order." As this material is being circulated in a political journal — rather than one of the standard physicists' publications — some explanation to the layman-reader of the broader implications is plainly required.

Summary reports concerning the items and their pre-publishing history sets the stage for the appropriate, clarifying remarks which then follow.

The contents of this issue are English translations of Georg Cantor's revolutionary 1883 *Grundlagen...* and some correlated selections from G. Riemann, together with an extended prefatory dissertation by Uwe Parpart. The included, most newsworthy feature of Parpart's dissertation is the demonstration that Einstein's blunders concerning General Relativity involve an included point of elementary illiteracy which ought to have been noted and corrected by any of a number of specialists during the past half-century.

To put the matter in concise, technical language: as Weyl includes the blunder in his authoritative *Space, Time, Matter*, although Einstein sets out the correct intention to adopt the standpoint of Riemannian non-Euclidean conceptions of manifolds, thus attempting to free physics of Euclidean-Lagrangian metaphysical corruptions, the attempt to erect a mathematical structure for that effort was itself flatly corrupted in two interconnected ways. First, the introduction of what are to be termed affine assumptions concerning physical microspace poisoned the entire effort with nothing but a devastating regression toward Euclidean (or, more relevantly, Kantian) forms of axiomatic apriorism — embedding in the structure a pervasive assumption absolutely antagonistic to the relativistic principle.

Second, although Einstein, Weyl, et al. presumed to premise the effort upon the preceding achievements of Riemann, they considered only the simple (Gauss-like) cases of a Riemannian manifold, ignoring the existence of higher-order manifolds and the special case of nested higher-order manifolds. This two-fold illiteracy respecting the sources of mathematical knowledge upon which they drew ensured the vicious failure of the entire enterprise.

Parpart's critical treatment of such outstanding blunders has the effect of clearing away a key part of the accumulated intellectual rubbish which has blocked fundamental progress in the study of physical fields.

Although that implication of the Riemann and Cantor material was recognized from the outset of Parpart's approximately five-year preparation of the paper published here, the production of a physics-oriented prefatory treatment was not the originally intended emphasis of the effort. Thereby, so to speak, hangs our tale.

Lyndon LaRouche's pioneering achievements in Marxian economics and epistemology of the 1955-1961 period were most significantly influenced by the conception of Riemann and Cantor underlined in this issue's contents. Not Riemann plus Cantor, but Riemann as understood retrospectively from the standpoint represented by Cantor's elaboration of the notion of the "transfinite." LaRouche's solution of the paradox of "extended simple reproduction" (the last chapter of Volume II of Marx's *Capital*), the relevance of the notion of nested higher-order manifolds to the decisive error in Hegel's *Phenomenology* and *Science of Logic*, and the redefinition of *negentropy* for uses in ecology and economics were all instances in which there was a large debt to Riemann-Cantor, both directly and because of provocative stimulus not otherwise so enjoyed. Since this influence of Riemann-Cantor is significantly embedded in LaRouche's economic work and so forth, it was the obvious duty of the Labor Committees to clarify public knowledge on this aspect of our theoretical development at some suitable opportunity.

This mandatory public clarification faced a special obstacle in the fact that Cantor's most significant writing was not available in English translation. Worse, to the extent that his writings were known among professionals, the interpretation was usually wretched. The respect generally accorded the epistemologically crude *Principia Mathematica* of Russell and Whitehead is in itself ample proof that most professionals lack the rudiments of competence concerning Cantor's notion of the transfinite and concerning cognate features of Riemann's work on manifolds. For related reasons, the reciprocal interdependence of Riemann and Cantor for a theory of manifolds is generally ignored in the same circles.

For these reasons, it would not have been appropriate to publish merely a translation of Cantor's 1883 *Grundlagen*. The translation required a preface which both emphasized Cantor's systematic connections to Riemann, and pointed directly to the effects of such Riemann-Cantor influence on critical aspects of Labor Committee theoretical work. Now, approximately five years ago, Parpart, a philosophy professor who had come up through advanced mathematical training, undertook the assignment of writing such a preface and completing a final editing of the translation of the *Grundlagen* itself.

There were two determining, evolving sets of circumstances which shifted the emphasis and scope of that preface from its originally projected objectives to the character of the Parpart dissertation published here. First, for reasons which are not in the slightest exaggerated nor properly mysterious, the conception of physical processes elaborated in the Labor Committees' economic theories represent a significantly more advanced epistemological conception than presently prevails in theoretical physics as such. This has obvious implications. Second, the development of the dissertation demanded an intensive comparison of the epistemological content of LaRouche's theoretical work with that of leading epistemological currents in the evolution of theoretical physics during the 19th and early 20th centuries. This second consideration was increased in importance and bias as a result of the Labor Committees' active and increasing support for broadly based basic research centered around controlled thermonuclear reactions. Hence, the dissertation has acquired the form of a contribution to theoretical physics in the tradition of Riemann and Cantor.

The Crucial Issue

In the history of modern science, two basic, mutually antagonistic approaches to defining a scientific method predominate. On the one side, there are several varieties of the so-called reductionist approach, for which the classical materialism of Euler and Lagrange or the empiricist and positivist currents are leading examples. This approach is premised on the attempt to fit material processes within analytical schemata defined by aprioristic aesthetic presumptions concerning space, time and scalar notions of magnitude — associated with aprioristic belief in self-evident discreteness. The axiomatic features of this first approach impel the analyst to define the fundamental qualities of nature in terms of elementary particles or (at least) ostensibly elementary discrete phenomena.

On the opposite side, only universals are considered primary or ontologically irreducible. That is, totalities which represent true universals are not defined as aggregations of particulars, but the particulars subsumed by such universals are understood to be elaborations of the universality as the corresponding primary existence. The apparent difficulty traditionally encountered by this approach is the fact that if totalities are measured in terms of homogeneous linear extension — hence, bringing Euclidean apriorism in through the cellar door — it is impossible to define a universality in such a way that subsumed, actually existing discrete phenomena have the determined quality of actual existence. Since discrete phenomena of some varieties stubbornly exist, the simplistic or linear conception of a universality (or, true universal manifold) must be rejected as incompatible with the physical evidence.

If this problem is situated within the empirical domain of the history of successive development of societies, the conceptual form of a satisfactory solution to the notion of universals can be made immediately apparent, with results which conform to the empirical evidence of modern economic developments. If the world economy is defined as an interdependent, indivisible totality (in terms of effective relationships), the uniquely acceptable characteristic feature of that universality is of the form of exponential functions in terms of $S'/(C+V)$, for the condition that S', C, V are determined in the way prescribed for this purpose in *Dialectical Economics*. This method, which has been uniquely verified by the crucial evidence of the 1958-1975 period, is also verified for not only the general case of a potentially multilinear social evolution, but also for the case of general ecology (the evolution of the biosphere).

Translating the results into concise language, we have a nested series of manifolds of generally successively higher orders. In this representation, the invariant feature of each manifold is a non-linear characteristic of changing, but ordered values which expresses both the self-development of the immediate manifold (economy or ecology) as a whole, and also the potential emergence of a successor manifold. The conception to be attributed to *negentropy* as such an invariant (as a world-line characteristic) is rigorously defined in those same terms.

Although this is implicitly Karl Marx's outlook in both economics and in the distinction between his dialectical method and that of Hegel, LaRouche's original contributions have the double significance of making that connection explicit and thereupon building somewhat further. It was the fact that the Riemann-Cantor approach to manifolds pointed to a physics in agreement with such a world view which prevented LaRouche from immediately discarding his such approaches to economy and then allowed him to appropriate theoretical apparatus for global policy-making in the period immediately ahead. Indeed, the basis employed by LaRouche and his collaborators in developing the IDB strategical approach to a new world economic order is just that.

Consequently, it should also be understood that the contents of the present issue have the following, inter-related urgent political significances. In general, the situating of the method used by the Labor Committees in respect to the leading problems of contemporary physics have the immediate benefit of making the method itself more readily comprehensible to a broad stratum of specialists. At the same time, the fact that this approach provides a common language of policy-making for the coordinated fostering of scientific development and economic development is of the utmost importance in minimizing the risk of detours into misallocation of resources. At the same time, through the elaboration of this material in the form of education programs among skilled and semi-skilled workers, this same approach provides such workers with a direct and efficient common language for participating directly in formulating and judging global development and related policies.

For the skilled and semi-skilled worker into whose hands this issue comes, we offer the consoling assurance that what he needs to understand concerning this subject-matter is not inaccessible through classes provided to an intelligent person of his probable education background. The secret of the whole matter involves the mastery of a few basic conceptions, after which other essential rudiments begin to fit quickly into place in comprehension. We shall certainly not deprecate the incontestable value of a broad and profound educational background in any profession. Such specialized education and experience pertain to the professional side of physics and so forth. What concerns us as politicians is not the elaborate cognitive architecture of professional physics, but only a handful of essential, fundamental principles which ought to be the common basis for collaboration between the skilled worker (as policy maker) and the working scientist.

Lyndon H. LaRouche Jr.
Dec. 30, 1975

attack crucial unresolved problems of Marxian economics and method from such an informed standpoint.

The necessary connection between economics and physics is efficiently understood by briefly considering the essential fallacy in Hegel's work. Hegel, as in the *Phenomenology*, understood that a comprehension of reality demanded the abandonment of simplistic notions of homogeneous continuity in favor of the notion of a principle of self-development specifically characteristic of universals. Hegel also correctly understood the problem of formal determination, that particularities must be shown to be necessary actualities elaborated by the process of self-development of wholes — that the existence of the particular has the analytical significance of mediation of the process of self-development of the universal. Hegel's treatment of true infinity (actual infinity) and merely potential infinity (bad infinity) in his *Science of Logic* coheres with this, and also has a direct connection to Cantor's later development of the notion of the transfinite.

Hegel also recognized that the physical universe must be of the order of a higher-order manifold (in the sense that the flawed Einstein version of General Relativity does not). However, Hegel balked at the speculation that the "nested manifolds" of successive social evolutions could be actually mediated through the physical domain — since this would require that the physical universe be not a single higher-order manifold, but a system of nested manifolds, such that human actions might *ultimately* be efficient means for altering the laws of that universe. At exactly that crisis-point of his conception, Hegel retained his dialectical method by rejecting "materialism," limiting his concern to the self-development of the Logos (e.g., "idealism"). As in Hegel's *Philosophy of History*, he rejects the conception of *labor* which treats transformation of the material conditions of existence as primary, and defines *labor* in respect of the administrative activities of "civil society." Thus, the executive "labor" of the Prussian monarch expresses a primary form of labor for Hegel.

When Marx's 1844-1845 writings are studied in that light, and from the standpoint of LaRouche's elaboration of economics and epistemology, a rigorous conception of the specific accomplishments and limitations of Marx's discoveries immediately follows. Marx apparently (and to some extent actually) evades the issues of physical scientific knowledge by the means underscored in the first and second of his "Theses on Feuerbach," freshly capitulated in the elaboration of *the single empirical premise of all human knowledge* in his "Feuerbach" section of *The German Ideology*. It is sufficient, in Marx's argument to this effect, that human history demonstrates the efficiency of man's necessary ability to increase the negentropy of existence, since the effectiveness of man's such achievements suffices also to demonstrate that the laws of the physical universe must be in conformity with the possibility of such successes.

Hence, Marx defines the *negentropic* transformation of materialized labor through scientific advances as the single, invariant, world-line principle upon which single rule all human scientific knowledge is properly, uniquely premised. LaRouche's essential original accomplishment has been to identify that connection and to employ that as the basis for the elaboration of economic science and a few correlated areas of endeavor.

The Ecological Case

The appropriateness of applying such an approach to the fundamental questions of ecology can not be competently resisted. The broad results are relatively immediate and incontestable in fact.

Reductionist biology locates the evolution of the species in the isolated biological individual, a hotly defended opinion in defiance of the relevant empirical evidence. An array of species (to put the point in broad terms) corresponds to a negentropic state of an entire ecology. The effect of shifts in populations of included varieties is to alter the negentropic state for better or worse. (The analogy to variations in specific commodities is slightly strained but not otherwise inappropriate.) An adverse result lowers the negentropic state, with corresponding effects on the plenum of species; an enhanced negentropy mediates further variations, which mediate further variations. Thus, the universal, the entire biosphere or specific ecology, mediates its own negentropy through the determination of the individual variety.

In economy, the determinant of negentropy is immediately located in the single creative individual. This individual is variously creative either in synthesizing new conceptions whose realization increases the negentropy of the economy as a whole, or "more passively" develops conceptions which enable him to master the conceptual innovations created by others. The material-cultural conditions of life determine the kinds of creativity of this import available to the society. In this way, realized inventions, by enhancing the negentropy of general material-cultural existence, foster the advancement of the creative powers of the population, which is then a potential for new inventions which are realized as further advances in the material-cultural negentropy of the society as a whole.

Although these points have been developed here only in summary pedagogical form, it should be clear enough that this systematical approach to the combined issues of ecology and economy represents the

The Concept of the Transfinite

by Uwe Parpart

Das eigentliche Studium der Menschheit ist der Mensch.

Goethe

I. "The Unity of the All"

Since the publication in 1910 of Russell and Whitehead's *Principia Mathematica*, Georg Cantor's set theory has played a central role in virtually all investigations into the foundations of mathematics. However, the "logicist" Russell-Frege *(1)* approach to the philosophy of mathematics, which attempts to derive the entirety of mathematical propositions in all the different branches of the field from a combination of set theoretical and formal logical principles, would have been completely alien to Cantor's overall intent. Rather than subcribing to the narrowness and reductionism of the Russell-Frege program, Cantor saw himself embarked upon the most broad-based epistemological project, not at all, or at least not principally, associated with set theory in the current mathematical-technical use of that term, but conceived of as an ambitious investigation into the concept of the infinite in continuation of the earlier efforts of Nicholas of Cusa, Giordano Bruno, Benedict Spinoza, and Gottfried Wilhelm von Leibniz. This is clear both from the direct evidence of Cantor's writings and from what we know about the intellectual origins of his work. Appropriately, the sub-title of the 1883 *Grundlagen*, which represents the result of ten years of intense struggle with the concept of the infinite, is "A Mathematical-Philosophical Study in the Theory of the Infinite." And the preface of the special 1883 Teubner edition of the *Grundlagen*, originally published as part five of a series of essays entitled "On Infinite Linear Point Manifolds" in the *Mathematical Annals*, contains the caution that

> this essay was written for philosophers who have followed the development of mathematics into the most recent period and for mathematicans who are familiar with the most important older and newer issues in philosophy.

More directly, in an 1885 review of Gottlob Frege's *The Foundations of Arithmetic* (1884), Cantor explicitly criticizes Frege's attempt to base the concept of number upon the notion of the "extension of a concept," which he regards as utterly imprecise, and counterposes his own approach of first settling the issue of determinate infinite numbers and then applying the insights thus gained into the number concept to the derivation of the principal predicates of number for both the finite and the infinite realm.

The study of the intellectual roots of Cantor's project reveals two principal types of problems which Cantor intended to address through his concept of transfinite number. On the one hand, he was thoroughly familiar with the principal lines of research in mathematical physics in the first 50 years of the 19th century and, in particular, with the results attained by Riemann both in his geometrical and his function-theoretical works. Through his investigation of the line continuum, Cantor set himself the task of further elaborating Riemann's concept of an n-dimensional manifold. On the other hand, in the *Grundlagen* and elsewhere, Cantor explicitly references the major problems with Spinoza's theory of the actual infinite and claims that the formal properties of the transfinite ordinal numbers imply a possible solution. According to the *Grundlagen*, the weakest and most difficult point of Spinoza's theory involves the relationship of the finite to the infinite modes of substance: "It remains unclear why and under what circumstances the finite is able to maintain itself vis-à-vis the infinite." The same problem had previously been identified by Hegel; thus at the beginning of the Spinoza-section of the *Lectures on the History of Philosophy*, he writes

> Spinoza died on February 21, 1677, in his 44th year of *consumption* (tuberculosis), of which he had suffered for some time — consonant with his system in which all specificity and singularity is *consumed* by the one substance.

It is through Cantor's discovery of a succession of definite transfinite numbers, his proof of the existence of an internal differentiation of the infinite and of its determinateness comparable to that of the finite realm, that Spinoza's problem is solved, and the ability of the finite individual to maintain itself in face of the consuming power of the infinite is explained. For the successive ordering of the transfinite requires the "creative intervention" of the finite and discrete. This also defines the deeper point of connection between Cantor's and Spinoza's theorizing. The reading of Spinoza's *Ethics* convinces one very directly that above all else this is a record or an account of the principal features of the psychology of the creative process. Spinoza gives us a direct insight, even if *more geometrico*, into his own understanding of the process of perfection of human knowledge. Similarly this is the one theme to which Cantor consistently returns in his published writings and letters, especially in his most productive 1878 through 1884 period. His concept of a successive ordering of transfinite numbers most beautifully expresses the unity of his insight into the mental process necessary to solve the problems he poses for himself in the theory of manifolds, and the unique appropriateness of the results of his reflection upon this process (i.e. the transfinite numbers) to the solution of the mathematical problems at hand. A comment on the process of concept formation immediately following the first actual definition of the sequence of transfinite ordinals demonstrates the point:

> Here we see a dialectical generation of concepts which always leads farther, and yet free from all arbitrariness remains in itself necessary and rigorous

— a precise formulation both of the way in which a higher order transfinite ordinal follows upon a previously defined number sequence, and at the same time of the process in which a unique new concept is determined through the way the sequence of preceding concepts "has modified in a definite way the substance of our mind."

My own principal use, in the following, of Cantor's transfinite numbers will be in defining how Marx's theory of knowledge, which remained incomplete in this respect, must be extended to an understanding of the necessary structure of the physical universe as a whole, i.e., as the basis for drawing the most general hylozoic conclusions from Marx's epistemology and economics. That such an extension is necessary is indicated negatively by the glaring inadequacy of especially Engels' studies in mathematical physics (as, for example, in the *Anti-Dühring* and the *Dialectics of Nature*), and otherwise by the demonstrated inability of contemporary theoretical physics to progress beyond the epistemologically pre-Marxian standpoint of Einstein's 1915 Theory of General Relativity, and of accomplishing its urgently necessary unification with quantum mechanics into one coherent physical theory of the micro- and the macrocosmos.

It is usually assumed today, (cf., e.g., the 1956 supplementary notes to Wolfgang Pauli's classic *The Theory of Relativity*), that the epistemological situation produced by developments in quantum mechanics especially since 1927 makes "a complete solution of the open problems of physics through a return to the classical field concepts impossible." Einstein's hopes for a "unified field theory,"

> this ambitious program of a theory which solves all problems regarding the elementary particles of matter with the help of classical fields which are everywhere regular (free of singularities),

are seen not only as extremely difficult to attain, but as unattainable and wrongheaded in principle. I will show that, on the contrary, Einstein's attempt at creating a unified field theory represents the absolutely indispensible rigor in the approach to the solution of the problems of particle physics and that Cantor's Theory of the Transfinite allows us to formulate a non-linear conception of the continuum and of the physical field which defines the necessary basis for advancing beyond relativity without giving up continuity.

The unresolved problem of Spinoza's *Ethics* — the role of the individual — and the scandal of contemporary physics — the incoherent side-by-side of relativity and quantum theory — both exemplify that most fundamental type of problem of human knowledge analyzed by Immanuel Kant in the *Critique of Pure Reason* under the rubric of "antinomies of pure reason." For theoretical physics the "second antinomy" is the most relevant one.

Kant demonstrates first (thesis) that "Every composite substance in the world is made up of simple parts, and nothing anywhere exists save the simple and what is composed of the simple" (*Discreteness*, elementary particle conception), and second (antithesis), with the same degree of cogency, that "No composite thing in the world is made up of simple parts and there nowhere exists in the world anything simple" (*continuity*, classical field conception). The antinomy is directly replicated in the field wave-particle duality. It will be resolved through the application of Cantor's theory of transfinite numbers to continuous Riemannian manifolds, generating the concept of a continuous nested sequence of such manifolds, characterized by a succession of transfinite numbers, and so that discrete particles are seen as necessary for the transition from one manifold to the next.

The solution derives from the following more general epistemological considerations. When we are confronted with one of the Kantian antinomies, the issue is certainly not finding a solution *within* logic to a logical paradox. Rather, precisely the kind of challenge to our knowledge has been thrown up which defies all attempts at *logical* resolution, and effects not just the coherence of our way of *looking at* the world, but of our practical intervention into it — hence of our very existence. The antinomy, which merely openly draws the contradictory conclusions from beliefs we know we cannot give up, is thoroughly infectious, and once it has gained access to the structure of knowledge it leaves nothing unaffected and threatens chaos and destruction wherever it appears. Finally, after it has turned everything around us into rubble, it directly attacks us, and our most cherished belief that a rational, lawful universe — the only kind knowable to us — must ultimately be penetrable by a logically consistent and complete set of universal laws. This is the most profound threat; for if we cannot maintain logical comprehensibility, how can we maintain lawfulness, and what is left standing between us and chaos?

Modern indifferentism recoils from the threat and has tried to evade the challenge. Thus, the hegemonic

"Kopenhagen interpretation" of quantum mechanics speaks of wave-particle "complementarity," as though contradictory existences would "complement" each other! The price that has been paid is the virtual destruction of all progress in theoretical physics. The antinomies are real, and, as Hegel points out, Kant's main accomplishment was to recognize that they are necessary; he adds that there exist not only the four Kantian antinomies, but a (potentially) infinite number of them. Barring the self-destructive evasiveness of indifferentism, the necessary existence of such a sequence of contradictions forces us to conceptualize the uniquely appropriate transfinite quality of the human mind and, coherently, of human existence and the existence of the physical universe in its entirety, for which the necessity of contradiction is not a mortal threat, but a productive condition of its existence.

Such a quality was first successfully isolated through the application of the results of the Feuerbach-Marx critique of Hegel's dialectical method to the study of the capitalist economy and defines the contents of the evolutionary social reproductive process. At any given stage of human social evolution, the process of the necessary appropriation of nature for man is governed by definite laws, production-technologies, and production-relations (social institutions) — i.e., by an historically specific "internal logic." On the other hand, for each fixed mode of global production-technology, mere quantitative expansion in that mode (expanded simple reproduction) will sooner or later incur the problem of limited natural resources for further development — the sooner, the more successful the expansion process in the given mode. We reach the point where the society under consideration must destroy and supersede the logic of its own previous existence or else destroy itself in short order. But this only defines the matter negatively. In positive terms, it does not suffice that the old mode of production at a certain point be augmented and ultimately qualitatively changed through the introduction of new technologies which define new types of resources for continued human existence; rather, that such qualitative changes, whose content must be that of forcing non-linear increases in social "free energy" rates S'/C+V (2), continually *have* to occur has to be recognized as the *"conditio humana."* For human society to gain self-conscious control over the process of its own future development, the mere *occurrence* of expanded reproduction proper (for example, as incidental by-product of the capitalist accumulation process) is utterly insufficient. Instead, the *necessity* of expanded reproduction has to become understood, and a species quality equal to the demand of continual technological innovations and exhibited as the definite quality of human individuals has to be determined. The required transfinite quality is circumscribed by the notion of human freedom and exhibited in the determinate (governed by the requirement of negentropy) activity of the creative individual. Here creativity must signify not just the spontaneous, isolated singular insight, which produces an accidental discovery, but is not conscious of the conditions of its own capacity to do so. Rather, it is the deliberately controlled process of positing new types of lawful connection, based uniquely in the self-conscious insight into one's own mental processes.

The fundamental premise of Marxian epistemology — that all human knowledge is based on human existence — can now be utilized to extend our understanding of the necessary invariant characteristics of the human evolutionary process beyond anthropology and to adduce the most general invariant features of the physical universe as a whole. Following Cantor's lead in the *Grundlagen* (paragraph eight), where the simultaneous *intrasubjective* (or *immanent*) and *trans-subjective* (or *transient*) validity — i.e., validity in the intellectual and the physical world — of mathematical concepts is defended by reference to "the unity of the all, which includes our own existence," the matter can be put as follows:

1. It is a necessary presupposition of the possibility of science that one and only one set of laws governs the physical universe in its entirety. In particular, *ad hoc* constructs brought in to account for empirical data or specialized parts of the whole not comprehended by the original set of laws must be strictly ruled out — the antithesis of pluralism. *(3)*

2. Human existence, which, as the final product of the process of natural history, is part of natural existence, is governed by the necessity of expanded reproduction or freedom.

Consequently, there must be a unitary process determining human existence, as well as organic and inorganic nature, and the laws of this process, notably also with respect to the inorganic, must be consistent with the principal invarient of human existence, the capacity for qualitative conceptual advances.

Note the following corollaries to this conclusion:

1. While expanded reproduction is a lawful process, its successive moments are governed by successively different sets of laws; otherwise we would have mere expanded *simple* reproduction. Similarly, we must admit successively different sets of laws for the evolutionary process of universal substance, i.e., changing laws of (inorganic) nature.

2. Despite changes in the "internal logic," successive moments of the expanded social reproduction process do not simply arbitrarily follow each other, but in each case represent the next higher level in "free energy" contents and tendencies toward non-

linear increases in "free energy" ratios. The physical universe as a whole must exhibit the same lawful ordering and overall negentropic quality.

3. The world (space-time) -manifold is not a simple continuum in the Kantian sense of infinite divisibility. At first sight, the postulate of the "unity of the all" appears to imply the opposite, for continuity is simply its ontological equivalent: discreteness or, equivalently, the existence of self-subsistent particles would have to allow for different functional relationships between different (separate) sets of particles, and no reason for the imposition of one coherent set of laws could be determined. This point, incidentally, was well understood by David Hume who, in the *Inquiry Concerning Human Understanding*, is forced to admit that in order to account for the empirically real oneness of the world, a "pre-established harmony" may have to be introduced.

But while unity implies continuity, this need not be the kind of continuity or connectedness of the world-manifold envisaged by Kant. The Kantian continuity condition is equivalent to the assumption of the metric homogeneity of the continuum, and Riemann has demonstrated that a continuous manifold admits of highly inhomogeneous metric relations, so that homogeneity (divisibility based upon *one* pre-assigned law of division) is shown to be an unwarranted *a priorism*. The metric and exact kind of connectedness of the world-manifold are empirical questions, to be determined within the conceptual framework of a geometrical formalism, which takes neither space (continuous fields) nor discrete existences (particles) in space as primary, but is adequate to the formulation of their process of interaction.

My argument that the world-manifold is a "non-linear" continuum characterized by lawfully changing internal laws ("laws of nature") is based on the premises of the necessity of expanded reproduction for human existence and of the "unity of the all," allowing the extension of what is necessary for *human* existence to the *existence* of substance *in general*. The necessity of expanded reproduction was discovered by Hegel and Marx and is the cornerstone of Marxian epistemology. The "unity"-premise was first explicitly asserted some 200 years earlier and brought with it the final destruction of the medieval-Aristotelean world system. Its conscious application, though based on theological considerations, marks the beginning of the development of modern science at the turn of the 16th tc the 17th century. Since the notion of the "one-ness" of substance is fundamental to my entire subsequent argument, I will now briefly review the first successful use of the unity-principle in Kepler's astronomical theories.

Historical Excursus: Kepler—the Unity of the All is Founded on the Rationality of God's Will

Johannes Kepler was born in 1571 in the southwest German province of Württemberg; in 1589 he enrolled as a theology student at Tübingen University — at the time the world center of Lutheran orthodoxy — and simultaneously entered the "Tübinger Stift," the same religious institution which was later to produce Hölderlin, Schelling, and Hegel. Kepler's purpose at that point was to become a minister, and the subsequent five years of study of philosophy and theology, according to his own testimony, defined for him the fundamental problems and convictions that became the mainspring for his later work in astronomy and physics.

Hegel calls it the "protestant principle"

> to transpose the world of intellect into the realm of one's own feelings and sensibilities (*Gemüt*) and, in one's own self-consciousness, to look at everything and to know and to feel all that formerly was beyond (this world).

This is the actual, active side of the principle of the "unity of the all," of which Renaissance philosophy had had a mere formal understanding. Kepler defines that unity by asserting the coincidence of the essential predicates of the minds of God and man: God endowed man with a rational soul, and that is what is meant by saying that He created man in His image. The notion of rationality is then explicated by way of geometry:

> Geometry is one and eternal, a reflection out of the mind of God. That mankind shares in it is one of the reasons to call man an image of God.

This notion of unity, that God created the world in accordance with knowable geometrical principles, is the basis of Kepler's *Mysterium Cosmographicum* (1597), reporting the discovery of close quantitative relations between the orbits and distances of the six planets known at the time and the five regular Euclidean solids. However, Kepler's use of the unity principle is not limited to the ultimately mistaken "geometrical interpretation." Its most profound application comes in the *Astronomia Nova* (1609) and is already indicated in the subtitle: *αἰτιόλογητος seu Physica Coelestis* (New Astronomy—based on causal explanations or Celestial Physics).

Physics and astronomy had been strictly kept apart in the medieval Aristotelean-Ptolemaic system, the former advancing causal explanations of terrestrial (sublunar sphere) phenomena, the latter providing purely geometrical descriptions of the motions of the heavenly bodies, constituted of a condensed *"quinta essentia."* Ptolemy (and similarly Copernicus) regarded the terrestrial and heavenly spheres as made of entirely different kinds of matter and abiding by equally different sets of laws. Thus for Ptolemy

> it is impermissible to consider our human conditions equal to those of the immortal gods and to treat sacred things from the standpoint of others which are entirely dissimilar to them...Thus we must form our judgment about celestial events not on the basis of occurrences on earth, but rather on the basis of their own inner essence and the immutable course of the heavenly motions. Then all those motions will appear simple to us and much simpler than those which occur in our own realm.

Kepler's concept of celestial physics (or mechanics) utterly destroys such dualism. From the time of his earliest astronomical studies when he was introduced to and accepted the Copernican heliocentric system, Kepler simultaneously entertained the idea that the sun was also to be seen as the seat of forces responsible for the motion of the planets. This was the crucial hypothesis allowing Kepler to exploit the wealth of the empirical observational material accumulated by his predecessor as imperial mathematician to Rudolph II, Tycho Brahe, and to establish the first two laws of planetary motion, based principally on investigations of the orbit of Mars. Tycho had left Kepler with a most interesting problem: on one hand, Tycho, like Copernicus, held on to the assumption of uniform circular motion of the planets; on the other, the very accuracy of Tycho's observations allowed Kepler to determine that in some crucial cases the position of the planet Mars calculated in accordance with the uniform circular motion assumption deviated by up to eight minutes from the actually observed position. At that point, rather than searching for alternative purely kinematical solutions to the problem, Kepler introduced, and with immediate success, his causal hypothesis about physical moving forces emanating from the sun. The specifics are not important; but the new force hypothesis allowed him to discard two critical features of the Ptolemy-Copernicus-Tycho theories:

1. that in order to deal with certain irregularities in the orbits of the "upper" planets (Mars, Jupiter, Saturn), the center of the universe had to be located not in the sun but in the center of the earth's orbit a certain distance away from the sun;

2. that while irregularities were permitted in the orbits of the "upper" planets, the axiom of uniform circular motion was to be rigorously upheld for the orbit of the earth.

Concerning the first, the force-hypothesis, of course, demands that the center of the earth's orbit is the real sun, rather than some "mathematical" or "mean" sun. Kepler had been greatly encouraged in this view through his reading of William Gilbert's *De Magnete* (1600), which defines the notion of a field of force (*orbis virtutis*) and in particular establishes that no mathematical point in a magnet, but the magnet as a whole generates the attractive (repulsive) forces. *(4)*

Kepler saw Gilbert's magnetic forces as analogous to the forces which he had postulated in his "*gravitas*" theory, developed to explain the interaction of heavenly bodies. In 1607, he took the decisive step and for the first time explained an empirically observable and quantitatively well-understood phenomenon on earth, the tidal fluctuations, by way of a mechanical (gravitational) interaction of the earth with a heavenly body, the moon *(5)*. The qualitative distinctness of phenomena in the sublunar (terrestrial) and the "aetherial" region had been eliminated and the principal obstacle for the development of a theory of universal gravitation was removed.

Concerning the second, once the determination of the planetary orbits by solar gravitational forces is admitted, the orbit of the earth no longer enjoys any exceptional status. With that crucial insight, Kepler explicitly transforms his force-hypothesis into a "relativity-thesis," which in short order leads to the pronouncement of his planet laws. He argues as follows: we observe the other planets from a moving earth; therefore any errors concerning the earth's orbit will necessarily introduce errors into the calculations of the orbits of the other planets. However, a symmetrical line of reasoning could be employed by somebody residing on Mars. So let us establish the orbit of the earth relative to an observation from the standpoint of Mars positions of several sun-earth distances. In this fashion, we will gain empirical data for the determination of both orbits and will no longer have to rely upon *a priori* assumptions about the geometrical shape of the one arbitrarily singled-out orbit of the earth. Thus, in Kepler's own words, "eight minutes showed the way to a renovation of the whole of astronomy" — provided their significance is understood and interpreted from a standpoint of the rigorous application of the epistemological unity principle.

The *Astronomia Nova* defines Kepler's most advanced standpoint of a unitary universe governed by mechanical cause-effect relationships —

> Mästlin (Kepler's teacher) used to laugh at my attempts to reduce everything to natural causes. However, it is my pride and my consolation that I succeeded in this.

Much like Descartes, who in his 1664 *Principles of Philosophy* presents a generally identical *"Weltbild"* (world picture), Kepler does not press on to consider the compatibility of human existence and freedom with such a mechanistic world view. Instead, in the *Harmonices Mundi (Harmonies of the World)*, which he finished a few days after the outbreak of the Thirty Years War, he reverts to the geometrical interpretation of the unity principle of the early *Mysterium Cosmographicum*. Once again the notion of God the geometer comes into the foreground, leading precisely to that view of the static perfection of God's mind which is subject to the Ficino paradox, *(6)* that an omniscient God is necessarily impotent, if omniscience is taken to imply the existence of a perfected body of knowledge constructed in accordance with a given set of laws; for the existence of a completed totality of that kind would entirely eliminate God's freedom to intervene into and change the future course of events.

Kepler's failure is understandable. After he leaves Prague and the imperial court in 1612, the conditions of his material existence become increasingly uncertain, and, with the beginning of the war in 1618, his life is drawn into and repeatedly threatened by the events of war and the devastation, destruction, and diseases it brings along. The *Harmonices Mundi* envisage the derivation in natural science of a world formula which can be applied to attain the moral progress of man. Under the impression of the utter disharmony of human existence, the order of knowing has been reversed and the serenity of astronomical and geometrical knowledge is put forth as the ideal for the regulation of human affairs. Like a man who is drowning, Kepler writes in 1629:

> When the storm rages and the shipwreck of the state threatens, we can do nothing more worthy than to sink the anchor of our peaceful studies into the ground of eternity.

A year later, in 1630, he died.

It is significant and serves to emphasize the point that fundamental conceptual advances first occur in the field of metaphysics rather than physics, that the first to come up with a conception of God which avoids the Ficino paradox and points toward a conception of the physical universe coherent with human existence was a contemporary of Kepler's, the Silesian cobbler and philosopher Jakob Böhme. Unlike the astronomer, Böhme does not abhor strife and dissonance, but sees in them the very principle of the life process. The first being God created was Lucifer. God is the "self-separating (-differentiating) unity of opposites," and the same goes for nature; for

> the entirety of nature together with all the forces that exist in nature, in addition width, depth, height, the heavens and the earth...(are) the body of God.

Now, why is such a process of self-differentiation of God-Nature through opposing qualities necessary? Böhme's most important answer amounts to explaining the necessary structure of the universe in terms of conditions for its knowability:

> No thing can without adversity come to know itself; for if it has nothing, which opposes itself to it, then for ever and ever it goes out of itself and does not return back into itself. If, however, it does not return back into itself as into that out of which originally it came, then it knows nothing of its substance.

Herein also lies the answer to Ficino: The world was not created all at once; rather creation is a continuous process of self-differentiation, and only through such a process does God come to know himself. If omniscience is interpreted from the standpoint of a process of perfection of knowledge, then it is no longer antithetical to omnipotence but presupposes it. These ideas will find a direct application to theoretical physics.

II From Fourier to Cantor

Hegel appropriately defines the concept of self-differentiating and self-developing substance as follows:

> The living substance, further, is the being which is truly *subject* or, what is the same thing, is truly real only insofar as it is the motion of the positing of itself or the mediation with itself of the becoming-different of itself.

But, much as in the different case of Marx, the concept remains bare — "the whole concealed and hidden within its simplicity" — and no application to the physical universe in its entirety is attempted. My procedure in the following will be to use the concept of substance as negentropic process of self-differentiation as a point of perspective and to organize the material of 19th century mathematics and physics from the standpoint of successive approximations to the concept. Conceptions which pass the test can then in turn be regarded as providing the "detailed expanse of content" and the "developed expression of form" without which "science has no general intelligibility."

Paralleling the separation in Chapter I of the "unity"- and the "expanded reproduction"-premise, the subsequent investigation into the notion of infinite, continuous substance, which should simultaneously be regarded as an historical exposition of the origins within mathematical physics of Cantor's theory of the transfinite, is divided into two parts: first, the notion that all relations of substance including its measure-relations are internal is set forth. This amounts to developing the idea of "relative space" and polemicizing against "absolute space" and the associated more far-reaching homogeneity assumption that all functions in nature are analytic ("smooth"). The result is the statement of the classical field conception in its most general form of a Riemannian manifold. However, it is seen that in the usual interpretation of that concept it is insufficient to accomodate the existence of discrete particles. The second part, therefore, introduces Cantor's transfinite numbers to provide continuous Riemannian manifolds with enough internal structure to make discrete existence *as determined by the continuous whole* possible.

1. Newton vs. Descartes

The notion of space as the integral whole of the process of nature, i.e., the notion of "relative space," was first developed by Descartes. In explicit opposition to the views of the Greek atomists, characterized by the irreducible dualism of "atoms" and "the void," Descartes denies the existence of both. There is no duality of space and matter: extension is the essence of material substance; matter and extension are identical. Changes in substance signify matter in motion, and the latter must be describable in purely geometrical terms.

The implied conception of a purely geometrical physics gives rise to Descartes' most significant achievement, the development of analytical geometry. The space of analytical geometry is the continuum of the positions of moving bodies. By way of contrast, the space of the older synthetic geometry is simply the space between the individual rigid bodies and figures which alone comprise the subject matter of geometrical investigations. The space of Descartes' analytical geometry is the first example of a continuous manifold, consciously conceived as such — the three-dimensional manifold of all possible paths or curves of particles in motion. Only the later 18th century French Newtonians managed to reduce it to the concept of the rectilinear Cartesian co-ordinate system, a fate from which it was not rescued until Gauss and Riemann made the Cartesian manifold the basis for their non-Euclidean geometries.

While we cannot in all fairness saddle Newton with the exaggerations of his interpreters, and while many of his specific criticisms of detailed aspects of Descartes' physics were undoubtedly well taken, there is at the same time no question that the overall impact of his mechanics (and underlying metaphysics) takes us back to pre-Cartesian positions. His abandonment of Descartes' Continuum Theory of Material Substance and re-establishment of an atomistic theory which regards absolutely hard, incompressible, indivisible particles as the ultimate constituent elements of matter are at the heart of the problem — a veritable Pandora's box of metaphysical horrors. The difficulties become immediately obvious when we take a close look at Newton's most important accomplishment, his Theory of Universal Gravitation.

The bold idea of the same general attraction force attaching indiscriminately to all physical bodies could have significantly advanced, as a unifying force defining the integrity of the physical process, Descartes' notion of space as the continuous manifold of physical events. Instead, pressed into the metaphysical framework of atomism, it gave rise to the abomination of "instantaneous action at a distance" and firmly re-established "absolute space."

In Newton's theory there exists a fixed number of particles (point masses) in the universe and there inheres in each, for reasons not otherwise explained, an attractive force, proportional to its mass, which acts instantaneously and in inverse proportion to the square of the distance upon every other particle in existence. This is described by the differential equations for the n-body problem

$$\text{(I)} \qquad \ddot{x}_i = \gamma \sum_{k=1}^{n}{}' m_k \frac{x_k - x_i}{r_{ik}^3} \quad (i = 1, \ldots, n)$$

(similarly for the y and z components).
The fact that the time t does not explicitly occur in these equations signifies the instantaneity of gravitational action. To the extent that gravitational interaction among particles defines a set of purely external relations for each gravitating mass which in no way modifies their internal being, space, of course, can be no more than the "uninvolved stage" for physical events, the reference body or unchanging yardstick in the background, the "innocent" infinite-size container of matter in motion — space in the "absolute" sense.

Newton himself, while holding fast to his atomist hypothesis (one place where his "*hypothesis non fingo*" might have been productive), certainly was not satisfied with its consequences. Thus he writes in a letter to Bentley:

> That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it.

Nor did he have the complete faith in the overall coherence and consistency of his system displayed by Lagrange in *Mécanique Analytique*, and by Laplace in *Mécanique Céleste*. The problem was that — principally for psychological reasons deriving from his personal and social circumstances that cannot be dealt with here — whenever he was confronted with a choice between atomism and continuum theories the metaphysical weight of the former proved superior. His relationship to the differential calculus which he himself developed is a case in point. While much of the research and calculations that went into the *Principia* were based upon and made use of the calculus, there is no trace of it in the final version of the *Principia* itself.

Lagrange, in full recognition of the continuum-theoretical implications of the calculus of infinitesimals, explicitly drew the consequences for mathematical theory. In his *Théorie des Functions Analytiques* of 1797, he purged the calculus and specifically the crucial concept of the limit altogether from the theory of functions and replaced it by a purely formal "calculus of derivations." Only functions which can be defined by a power series

$$f(x) = a_0 + a_1 x + a_2 x^2 + \ldots$$

are taken into consideration, and the differential quotient $f'(x)$ (Lagrange even avoids the use of those words and calls it the "derived function") is then defined in purely formal fashion by a second power series

$$f'(x) = a_1 + 2a_2 x + 3a_3 x^2 + \ldots$$

Ironically, if these series are to be applied in physics, we have to consider questions of their convergence which is possible only through utilization of the limit concept.

These considerations aside, there exists an essential feature of Newton's mechanics which begins to undermine the notion of "absolute" space which otherwise appears to be such an unshakable part of it. As the law of inertia makes no principle distinction between a body at rest and in uniform, rectilinear motion, the absolute significance of a specific point in space, which had been a cornerstone of Aristotelean physics (everything has its "natural" resting place), disappears. Formally, this finds its expression in the invariance of the system of equations (I) with respect to the group of so-called Galilei-Newton transformations

$$\text{(II)} \qquad \begin{aligned} x_i' &= x_i + \alpha_1, \quad y_i' = y_i + \alpha_2, \quad z_i' = z_i + \alpha_3 \\ x_i' &= a_1 x_i + b_1 y_i + c_1 z_i \\ y_i' &= a_2 x_i + b_2 y_i + c_2 z_i \\ z_i' &= a_3 x_i + b_3 y_i + c_3 z_i \\ t' &= \pm\, t + \alpha_4 \\ x_i' &= x_i + \beta_1 t, \quad y_i' = y_i + \beta_2 t, \quad z_i' = z_i + \beta_3 t \end{aligned}$$

of the (x,y,z,t) co-ordinate system into the (x',y',z',t') system. What we are left with is a concept of space whose "absoluteness" has been considerably diminished. Not only is no one point or region of space from the standpoint of Newtonian mechanics preferred to any other, but there does not even exist the possibility of determining whether or not space, which is presumably stationary, *(7)* may not in fact be in a state of uniform, rectilinear motion. Space, in the sense of inertial framework, has lost a great deal of its assumed independent reality, and for all its "absoluteness" even the properties of Newton's space can only be investigated by way of an investigation of physical reality in its entirety. Curiously enough Newton himself can be quoted to that effect. In the introduction to the *Principia* he writes:

> Therefore geometry is founded in mechanical practice, and is nothing but that part of universal mechanics which accurately proposes and demonstrates the art of measuring.

2. Gauss and Fourier: The Forerunners of Classical Field Theory

By the end of the 18th century the Newtonian system increasingly began to resemble the system of Ptolemaic astronomy at the end of the 16th: not disproved, but more and more cluttered with ad hoc constructions. Ironically, this was primarily the result of the convincing simplicity and success of the core of the system, the mechanics. The difficulties arose when, in the 18th century, its principles and assumptions were rapidly extended to the entire realm of non-mechanical physical phenomena such as light, heat, electricity and magnetism. In each case and in conformity with the metaphysical basis of the mechanics, fluids of corpuscles were invented to account for the observed effects: Lavoisier's "caloric" (heat fluid), Benjamin Franklin's fluids of positive and negative electricity, magnetic north pole and south pole fluids, etc. (Compare today's nonsense: quantum-field theory, "gravitons," etc.) This wholly uncritical extension of Newtonian mechanics on the part of experimental physicists to a multiplicity of non-mechanical phenomena, of course, did little to advance physical knowledge, and soon the entire structure began to crumble under its own weight.

On the positive side, it brought about a thorough re-examination of the ontological and epistemological basis of the theory. Kant's antinomies, discreteness vs. continuity in particular, have been discussed above. Beyond that, Kant, occasioned by the consideration of certain cosmological paradoxes of a more limited significance than those presented in the *Critique*, began to develop in the *Metaphysische Anfangsgründe der Naturwissenschaft* the outlines of a continuum theory of material substance which, via Schelling's *"Naturphilosophie,"* became the basis for all 19th century field-theoretical conceptions.

The Newtonian universe encounters the following difficulties:

1. Given the assumed spatial and temporal infinity of the universe, the amount of matter in the universe must also be infinite. Otherwise, through the effects of radiation and aberration from their course, the finite number of stars would long since have been dispersed into infinite space and the world as we know it could not exist.

2. If the amount of matter in the universe is infinite, then either (a) the average density of matter is everywhere the same if we compare large enough regions; or (b) there exists a kind of center of the universe where the density of matter is maximal and diminishes with growing distance from the center. Neither configuration is possible: (a) implies that from any given direction in space an infinite gravitational force must act upon a given body; (b) would lead to the gravitational collapse of all matter into the center which exerts an infinite strength attraction.

2a is closely related to the so-called Olbers paradox (named after the 18th century German astronomer with whom Gauss carried on an extended correspondence). Olbers had posed the question of why the night sky was dark, a phenomenon that cannot be explained on Newton's assumptions, since in every direction from the earth there exists an infinite number of stars, and even though the intensity of light decreases with distance, the smallest finite amount added up infinitely often must build up to an infinite intensity.

The cosmological difficulties of Newton's theory have a direct bearing upon problems of the structure of matter "in the small." If we interpret the result of this survey of possible cosmological configurations from the standpoint of the integrity of a "viable" configuration of universal matter ("viable" in the sense of being a possible vehicle for observed physical processes), and given that no stable "viable" configuration exists — only dispersion or collapse — what then of the integrity of ordinary physical objects?

Kant correctly observes that the atomistic theory has great difficulties in accounting for the cohesion of material objects and for their most basic quality of exhibiting varying degrees of resistance to penetration by other objects. Why do the atoms which make up this table not just diffuse into the surrounding space; and why is it easier for me to push my fist through air than through a brick wall? There is nothing in the conception of an atom per se that could explain these facts. Certainly gravitational forces on the microscopic level would be much too small to account for the integrity and relative impenetrability of most "things."

Newton's answer to the problem appears to be contained in the following passage from the *Opticks*:

> It seems to me, further, that these Particles have not only a *Vis inertiae*, accompanied with such passive Laws of Motion as naturally result from that Force, but also that they are moved by certain active Principles, such as is that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies.
>
> I had rather infer from their Cohesion, that their Particles attract one another by some Force, which in immediate Contact is exceeding strong, at small distances performs the chemical Operations abovementioned, and reaches not far from the Particles with any sensible Effect.

While these statements might fit right in with modern electron theory, they are hardly satisfactory from a rigorous mechanistic standpoint. A new force (or forces), inherent in the "inscrutable" atom, is introduced in an essentially ad hoc fashion in order to account for phenomena the theory is otherwise incapable of handling. Worse yet, given that atomic masses are not proportional to atomic volumes, different intensities of microscopic attractive force cannot be defined as quantities of the same force varying with the size of the atoms, and hence we need not just one Force of Cohesion, but a great many qualitatively different ones in order to explain observed differences in degrees of penetrability.

Kant overturns this entire hodgepodge and instead takes the resistance force of matter as primary, explaining it as the equilibrium state of two fundamental forces — attractive and repulsive — which are polar opposites of each other. Matter is thus conceived of as the filling — in continuous fashion — of space by force, which force-"field," in turn, defines the medium for the propagation of physical effects. These are the basic principles of classical field theory. Beyond Kant, they were elaborated in much greater detail in the writings of Schelling who particularly stressed the idea of regarding different physical phenomena (such as light, heat, electricity, etc.) as manifestations of the same underlying fundamental forces or, alternatively, as different states of the one basic, continuous force field. (It was an avid student of Schelling's "*Naturphilosophie,*" Hans Christian Oersted, who in 1820 discovered electromagnetism, the unity of electrical and magnetic phenomena.)

Gauss: Intrinsic Geometry

Hermann Weyl has pointed out that if Descartes' theory of motion, in which a fluidum which fills space continuously acts as the carrier of motion, is followed through consistently, a field theory results, in which the behavior of material substance is described by the differential equations of the hydrodynamics of incompressible non-viscous liquids. Kant's and Schelling's concepts of substance are field-theoretical in a more immediate sense. In all three cases the field concepts, of course, carry with them the implied notions of space as the manifold of physical events, and therefore of "relative" space. This is most explicit in the case of Kant and his concept of the "manifold of appearance" — except that he relativizes space with respect to the knowing subject.

(If he had taken the unorganized — not yet brought under a concept — sense manifold, derived from the manifold of things in themselves, in the sense of projective geometry, i.e., things "originally" bearing only qualitative relations to each other which we "later on" quantify, then the application of the *a priori* representation of space to the sense manifold would amount to the imposition of specific metric relations, and Kant would have anticipated, at least in general terms, the Cayley-Klein conception of projective geometry.)

However, in all three cases as well, the authors either explicitly or implicitly hold on to the Euclidean structure of the manifold, and thus more than just a tinge of "absolute" space is thereby retained.

(The extent to which a Euclidean structure permits one to get away from the notion of "absolute space" is the subject of the invariance theory of the Euclidean group, i.e., the group of congruent transformations of the (x,y,z) — coordinate system.)

In fact, acceptance of *any one* (not just the Euclidean) geometrical structure of the manifold, to the extent that it is not determined by the internal relations of the manifold, but imposed upon it "from the outside," is a form of "absolutism." To see its incompatibility with a rigorous relativist point of view, we merely have to note that one critical feature of that view is that it does not accept the distinction between internal and external relations of bodies. However, if a specific geometrical structure is imposed from the outside, then the spatial or external relations of bodies are — at least in part — determined from the outside and, by the continuity of external and internal relations, must affect the inner nature of the bodies in question. Through the acceptance of a fixed geometry, the relativist view has turned into its opposite.

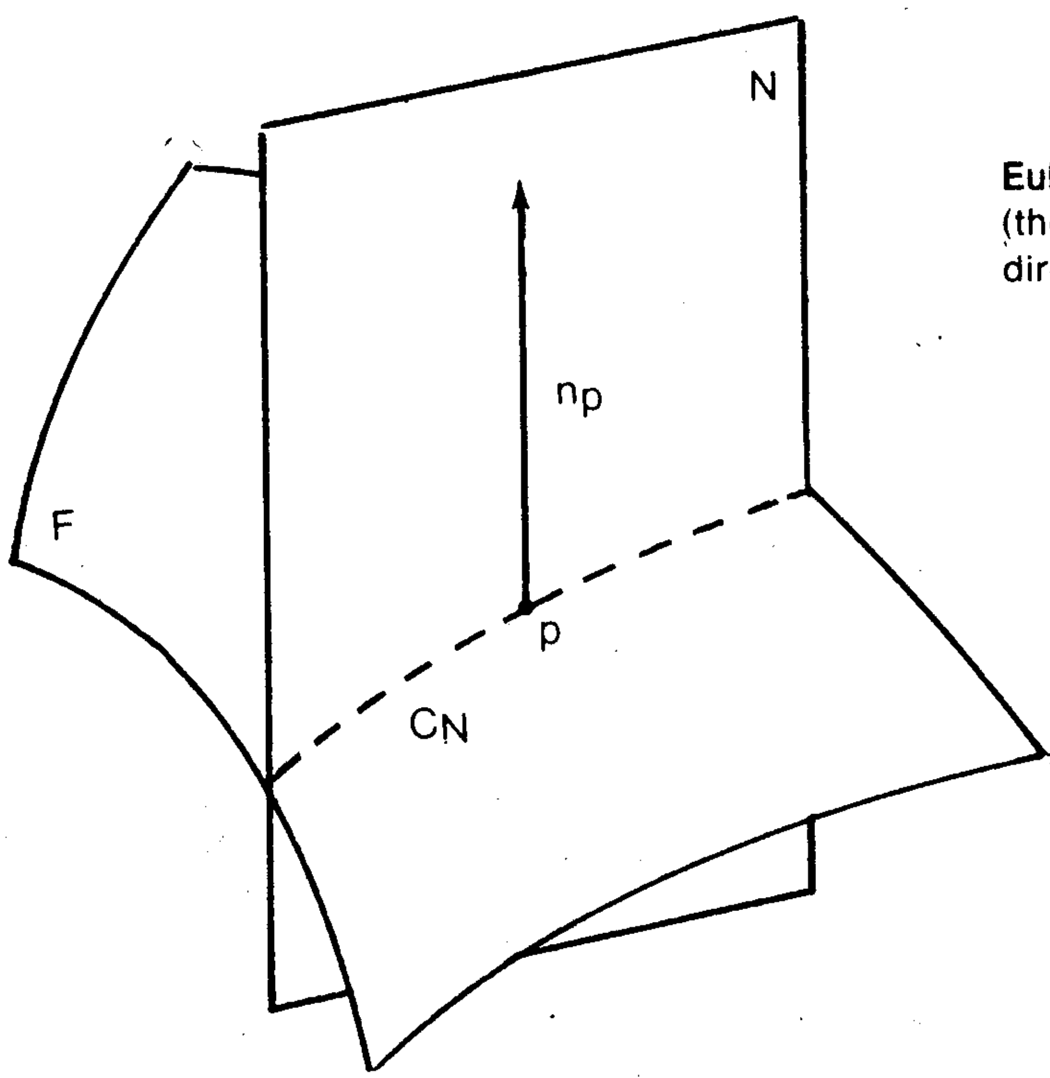
Schelling appears to have been aware of the problem and tried to deal with it by incorporating it as a positive feature of his theory. Thus in his *"Erster Entwurf der Naturphilosophie"* he concludes that "there ought to be discernible in experience something which, without being in space, would be the principle of all spatiality."

That such conclusions should be drawn simply indicates how firmly entrenched the notion of the "Euclideaness", as the one and necessary character, of space was at the time.

What was at issue was not simply or solely a metaphysical dogma. This becomes obvious when we pose the problem of measurement in the manifold. To carry the conception of relative space through to its conclusion and purge it of every tinge of "absolutism," a method of measurement had to be discovered which did not have to rely upon a fixed reference body, given once and for all, and brought to the manifold "from the outside." To have developed such a method was the principal achievement of Karl Friedrich Gauss, who was the first to study the "intrinsic geometry" of arbitrary surfaces (or manifolds), i.e., the intrinsic properties of surfaces independent of the manner in which they are embedded in surrounding space. With Gauss' method the conceptual basis for a radical relativism thus came into existence. As a consequence, the remaining vestiges of "absolutism," "Euclideaness" and the related, but more fundamental assumption of the homogeneity of physical space were swept aside in short order.

(To further put into perspective the broad epistemological significance of Gauss' discoveries — in his possession since the earliest years of the 19th century, but not published in formal detail until the appearance in 1827 of the *Disquisitiones Circa Superficies Curvas* — it will be useful to compare this exposition of his ideas with the remarks on the concept of "historical specificity," a key concept of Marxian anthropology, in Lyn Marcus, *Dialectical Economics*, Chapter 4, p. 107 ff.)

Gauss' method of *"geometria intrinsica"* is the following: "Arbitrary surfaces," i.e., surfaces other than the ones dealt with in elementary geometry — planes, spheres, curves, etc. — were studied before Gauss, with the first important results due to Euler (1760) and Meusnier (1776 — a general in the French revolutionary army who died in 1793 of wounds in battle). Neither of them, however, looked at surfaces directly; rather, they considered them as two-dimensional objects embedded in three-dimensional Euclidean space, defined analytically with respect to that space by a function $f(x,y) = z$ (for example: $x^2 + y^2 = z$, a paraboloid). Then, since they were in possession of a reasonably well-developed theory of curves in the plane, they proceeded to investigate surfaces by looking at the curves generated by intersecting the surfaces with planes at various different angles.



Euler's definition of curvature
(the *normal* n_p is a unit vector perpendicular to the direction of the tangent plane at point p.)

The curvature K of a surface F at a point p can then be defined as follows:

Let n_p be the normal to F at p and let N be a plane that cuts F and contains n_p The intersection of N and F is a curve C_N which at p has a *curvature* K_N. This would seem to be the definition we are looking for, if it were not for the fact that it depends on the choice of N. A plane N_1 containing n_p but cutting F at a different angle than N will in general define a curve C_{N_1} such that $K_N \neq K_{N_1}$

Euler rescued this otherwise simple and compact idea of defining a surface curvature by proving a theorem to the effect that:

1. If the K_{N_i} are not all equal, then there exists exactly one direction of the cutting plane for which the curvature of F at p has a minimum value $K_{N_1}=K_1$, and one for which it has a maximum value $K_{N_2}=K_2$.These two directions are mutually perpendicular.

2. If N makes an angle ϕ with N_1 (for which the curvature equals K_1), then

$$K_N = K_1 \cos^2\phi + K_2 \sin^2\phi.$$

To the extent that the theorem establishes a definite analytical relationship between the curvature in different directions, directional dependence, of course, ceases to be a problem.

Euler's concept of curvature has been presented at some length to make two related points: First re-emphasizing an earlier comment, the derivation and application of the concept depends entirely on the embedding of the surface in Euclidean three-space. To verify this explicitly, the reader should consult the standard calculus proof of Euler's theorem. Second, a point on concept formation. Euler's definition imports into the study of surfaces a concept developed for the study of curves in a plane and thus reduces surface-curvature to curve-curvature. But by looking at surfaces from the standpoint of so many curves, a most important intrinsic property of surfaces gets totally ignored: by bending (without stretching), certain surfaces can be transformed into a given surface, others can not. For example, a plane piece of paper can be rolled up into a cylinder or a cone (paper hat) but cannot be made to fit onto a spherical object as anyone who ever tried to wrap a ball can readily attest. Any curve, on the other hand, can be bent so as to fit any other curve; there are no interesting "deformation invariants" for curves, whereas in the case of surfaces the study of such invariants is what (after Gauss) the theory is all about. Gauss himself describes the matter as follows:

> When a surface is regarded, not as the boundary of a solid, but as a flexible, though not extensible solid, one dimension of which is supposed to vanish, then the properties of the surface depend in part upon the form to which we can suppose it reduced, and are in part absolute and remain invariable, whatever may be the form into which the surface is bent. To these latter properties, the study of which opens to geometry a new and fertile field, belong the measures of curvature and the integral curvature, in the sense which we have given to these expressions.

The sense Gauss gives to the notion of curvature is independent of the theory of curves on surfaces. According to his own abstract of the *Disquisitiones,* Gauss arrived at his definition of surface curvature by utilizing a "procedure which is contantly employed in astronomy, where all directions are referred to a fictitious celestial sphere of infinite radius." *(8)*

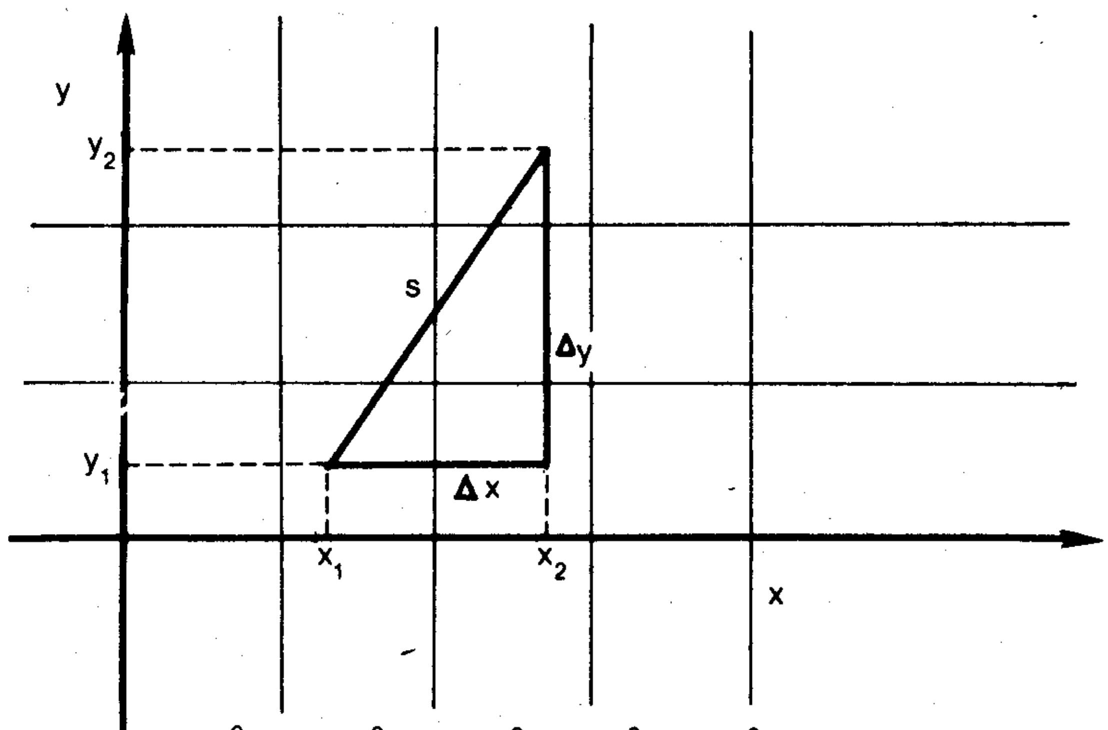
Let p be a point on a surface F, and S a segment of F containing p. Now erect a normal at each point of S and transfer the initial point of each normal to one point. Then the normals form a solid angle. Next construct a unit sphere with the vortex of the solid angle as its center. Call the segment of the surface of the sphere which intersects the solid angle n(S). Then the Gaussian measure of the curvature of F at p is

$$K(p) = \lim_{S \to p} \frac{\text{area } n(S)}{\text{area } S}$$

[$\lim\limits_{S\to p}$ = the limit of – as S shrinks to p]

If F is a plane, we can see immediately that $K(p)=0$ (for all p); for a sphere of radius r, a simple computation yields $K(p) = 1/r^2$ (for all p), etc. What establishes the significance of this measure of curvature — which can be proved to be equal to the product of the extreme curvatures K_1 and K_2 above — is Gauss' fundamental discovery that it is a deformation invariant and is *completely determined by the inner measure-relations of the surface.* This cannot be seen on the basis of the definition we have provided, which obviously relies upon the embedding of the surface in three-space and actually is *not* invariant under isometries. However, this closely follows Gauss' own procedure, who in his treatise gives the definition put forward above and only demonstrates the invariance of his measure of curvature after developing the analytical tools for carrying out measurements on surfaces without reference to surrounding space — a problem, by the way, which he had to confront in actual practice when in 1816 he was commissioned to make a geodetical survey of the Kingdom of Hanover.

Gauss' method is as follows: Instead of referring to a given point on a surface by means of its x,y,z, coordinates in Euclidean three-space, two parameters, u and v, are introduced on the surface itself, which determine a point in a manner familiar from the assignment — for navigational purposes, etc. — of latitudes and longitudes to points on the surface of the earth. In this way a coordinate net is thrown over the surface comparable to the net of Cartesian coordinates in the plane — except that the curves u = constant and v = constant will no longer, in general, be straight lines intersecting each other at right angles. The problem is that under these circumstances, i.e., given curvilinear rather than rectangular coordinates, we can no longer use the Pythagorean Theorem for distance measurements. The difficulty is overcome by assuming the "Euclideaness of the surface in the small" or, equivalently, the applicability of Pythagoras' Theorem for "infinitesimal distances."

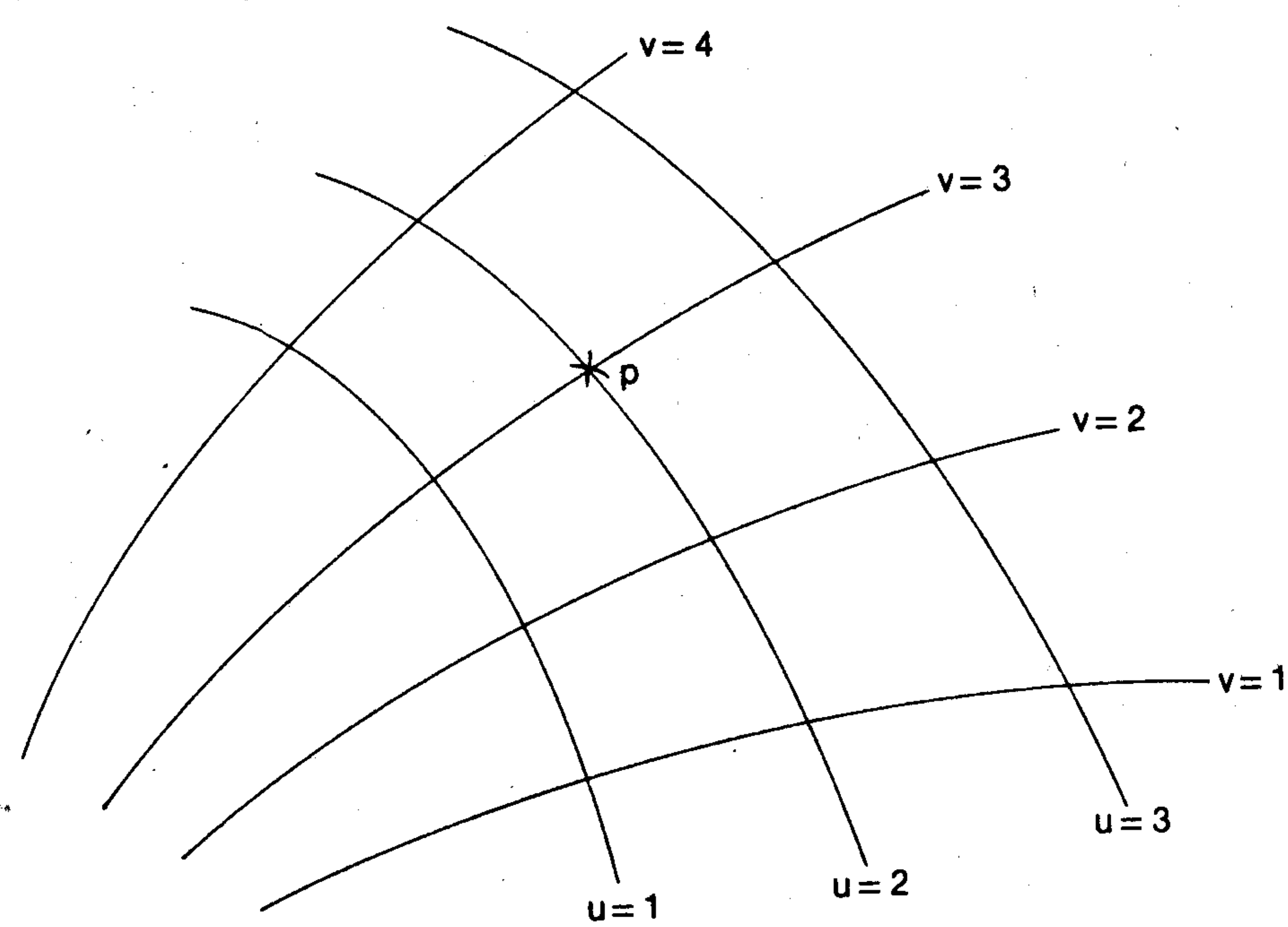


$s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 = (\Delta x)^2 + (\Delta y)^2$

THE THEOREM OF PYTHAGORAS

Given rectangular coordinates, the distance between two points can be determined by using the Pythagorean Theorem.

Given curvilinear coordinates, the Pythagorean Theorem cannot be used as such. In the case of two points which are infinitely close, equation (VI) is used to determine the distance between the two.(If u = x and v = y, then E = 1, G = 1, and F = 0 and equation (VI) reduces to a Pythagorean formulation.)



p = (2,3)

Assume that for a parameter t a curve c on the surface F is given by the equation $u = u(t)$, $v = v(t)$. We know that in Euclidean (x,y,z) -space the differential of the arc length s of a curve parametrized by t is given by

$$\text{(I)}\qquad \left(\frac{ds}{dt}\right)^2 = \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2$$

To make this formula applicable to our curve c on F, we consider c as a curve in (x,y,z,)-space by expressing x, y, and z as functions of u and v.

$$x = x(u, v), \quad y = y(u, v), \quad z = z(u, v).$$

Then c is represented by

$$\text{(II)}\qquad \begin{aligned} x &= x(u\,[t], v\,[t]), \\ y &= y(u\,[t], v\,[t])\ldots \end{aligned}$$

Forming the differentials of (II)

$$\text{(III)}\qquad \frac{dx}{dt} = \frac{\partial x}{\partial u}\frac{du}{dt} + \frac{\partial x}{\partial v}\frac{dv}{dt}, \ldots$$

and substituting them in (I) we obtain a quadratic differential form

$$\text{(IV)}\qquad \left(\frac{ds}{dt}\right)^2 = \left(\frac{\partial x}{\partial u}\frac{du}{dt}\right)^2 + 2\,\frac{\partial x}{\partial u}\frac{du}{dt}\cdot\frac{\partial x}{\partial v}\frac{dv}{dt} + \left(\frac{\partial x}{\partial v}\frac{dv}{dt}\right)^2 + \ldots$$

Introducing the so-called Gaussian coefficients

$$E = \left(\frac{\partial x}{\partial u}\right)^2 + \left(\frac{\partial y}{\partial u}\right)^2 + \left(\frac{\partial z}{\partial u}\right)^2,$$

$$F = \frac{\partial x}{\partial u}\frac{\partial x}{\partial v} + \frac{\partial y}{\partial u}\frac{\partial y}{\partial v} + \frac{\partial z}{\partial u}\frac{\partial z}{\partial v},$$

$$G = \left(\frac{\partial x}{\partial v}\right)^2 + \left(\frac{\partial y}{\partial v}\right)^2 + \left(\frac{\partial z}{\partial v}\right)^2,$$

equation (IV), the expression for the infinitesimal "line element" on the surface, becomes

$$\text{(V)}\qquad \left(\frac{ds}{dt}\right)^2 = E\cdot\left(\frac{du}{dt}\right)^2 + 2F\frac{du}{dt}\frac{dv}{dt} + G\left(\frac{dv}{dt}\right)^2,$$

or simply

$$\text{(VI)}\qquad ds^2 = E\,(du)^2 + 2F\,(du dv) + G\,(dv)^2\,.$$

For a given surface, E, F, and G will in general vary from point to point, in a sense defining in each case the extent to which the metric in the neighborhood of a point deviates from the Euclidean metric.

The Gaussian curvature K at a point p can be computed solely from the coefficients E,F, and G and their first and second order differential quotients on the basis of a rather complicated formula which is included here without explanation and simply for reference purposes:

$$\text{(VII)}\qquad \begin{aligned} 4\,(EG - F^2)K = {} & E\left(\frac{\partial E}{\partial v}\cdot\frac{\partial G}{\partial v} - 2\frac{\partial F}{\partial u}\cdot\frac{\partial G}{\partial v} + \left(\frac{\partial G}{\partial u}\right)^2\right) \\ & + F\left(\frac{\partial E}{\partial u}\cdot\frac{\partial G}{\partial v} - \frac{\partial E}{\partial v}\cdot\frac{\partial G}{\partial u} - 2\,\frac{\partial E}{\partial v}\cdot\frac{\partial F}{\partial v}\right. \\ & \left. + 4\,\frac{\partial F}{\partial u}\cdot\frac{\partial F}{\partial v} - 2\,\frac{\partial F}{\partial u}\cdot\frac{\partial G}{\partial u}\right) \\ & + G\left(\frac{\partial E}{\partial u}\cdot\frac{\partial G}{\partial u} - 2\,\frac{\partial E}{\partial u}\cdot\frac{\partial F}{\partial v} + \left(\frac{\partial E}{\partial u}\right)^2\right) \\ & - 2\,(EG - F^2)\left(\frac{\partial^2 E}{\partial v^2} - 2\,\frac{\partial^2 F}{\partial u \partial v} + \frac{\partial^2 G}{\partial u^2}\right) \end{aligned}$$

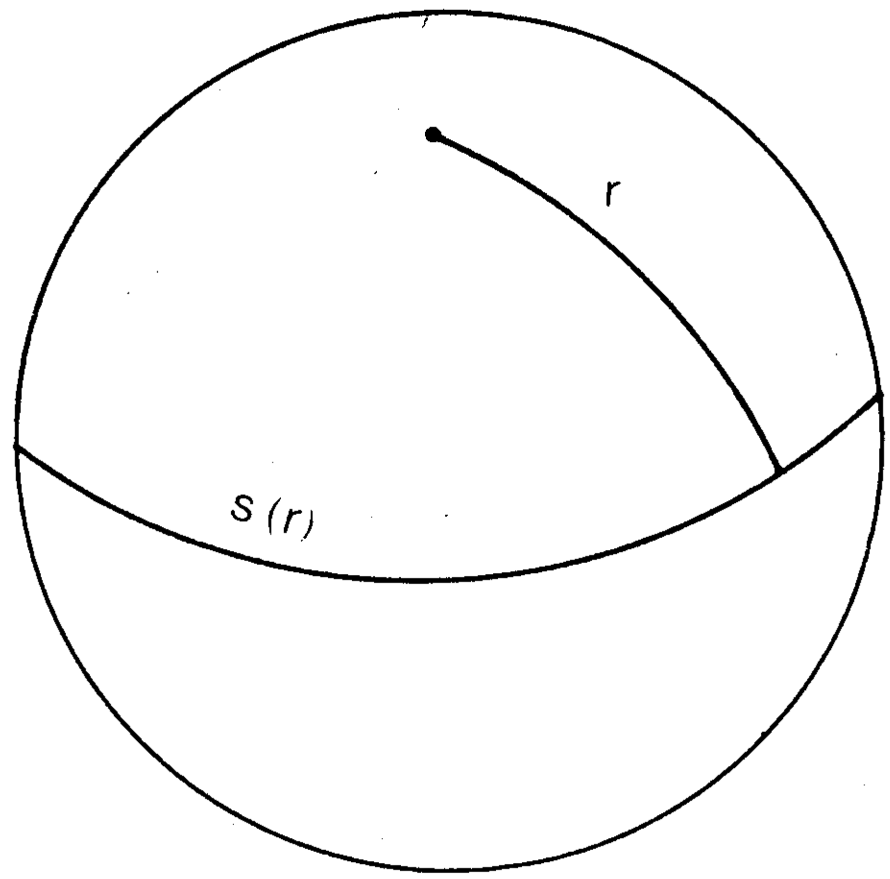
From (VII) it is clear that, if E,F, and G are constant, then $K = 0$, i.e., the constancy of the Gaussian coefficients defines a sufficient condition for the Euclidean character of a surface.

Since, furthermore, the metric coefficients, which only depend upon the surface and not its embedding in three-space, uniquely determine K, it should be possible to ascertain whether a surface is curved or not by measurements taken on the surface itself. That this is indeed the case is shown by the following example:

Let S be the surface of a sphere of radius R and let c be a circle on S with center p. Now, using a measuring chain, we can measure the lengths of the radii from p to c and the length of the circumference of c; let their lengths be r and s respectively. If their measurements had been taken on a plane, then we could express s as a function of r by the formula

$$s\,(r) = 2\,\pi\,r\;;$$

instead, however, we find that



Using the same radius r, s(r) is smaller when drawn on a sphere than when it is drawn on a plane.

$$s(r) = 2\pi R \sin(r/R).$$

Without making use of the third dimension we have, demonstrated that S is not a plane.

More generally, the coefficients of (VI), which from now on we shall call the *metrical groundform* and write as

$$\text{(VIII)} \qquad ds^2 = \sum_{i,k=1}^{n} g_{ik}\, du_i\, du_k$$

$$(u_1 = u, \text{and } u_2 = v,\ g_{ik} = g_{ki})$$

determine not only the Gaussian curvature K of the surface, but the intrinsic surface geometry in its entirety. In fact, through his discovery of the possibility of different intrinsically determined metrics for surfaces and his recognition of the fundamental significance of the coefficients of the metrical groundform for the determination of all other geometric magnitudes such as the length of curves and the size of angles and areas, after Gauss' *Disquisitiones* we are for the first time able to speak of *a* geometry rather than of *Geometry*, meaning the geometry of Euclid. Gauss actually indicates how a geometry, and trigonometry in particular, of arbitrary surfaces can be developed in exact analogy to plane geometry, if, instead of straight lines, we base it on the concept of the *geodesic* or shortest line (in terms of the metrical groundform) between two points of the surface. Then the "distance between two points" of plane geometry is replaced by their "geodetic distance," i.e., the length of the geodesic between them. All the other invariant geometric properties of a surface can be derived from this notion.

Parenthetically: Non-Euclidean Geometry

With the major elements of Gauss' theory of curved surfaces now before us, it is clear that his conceptions define a much more broad-based and radical departure from Euclidean notions than the so-called non-Euclidean geometries of Bolyai and Lobachevsky of 1832. In the Gaussian framework, the latter simply define the special case of a space of constant, negative curvature. That Gauss was well aware of the three-dimensional implications of his work on surfaces is apparent both from his correspondence and from his publications. As early as 1817, as evidenced by a letter to the astronomer Olbers, he had reached the conclusion that there is no inconsistency in the assumption of an "anti-Euclidean" geometry, and that therefore the question of the true geometry of space was an empirical one that had to be decided on the basis of experiments. The (negative) results of one such experiment, actually carried out by Gauss, are reported at the end of the *Disquisitiones*: the sum of the angles of the triangle formed by the mountain tops of Hoher Hagen, Brocken, and Inselsberg — the greatest side of which is more than 100 kilometers long — deviates from 180 degrees by an amount that lies within the limits of error of the measurement.

To make such measurements possible at all, Gauss constructed what he called a "heliotrop," an instrument which concentrates reflected sun rays virtually into a point, thus creating a highly visible target of minimal extension for long distance measurement. The measurement of large scale light ray triangles (rather than measurements on the surface) obviously would not have made any sense unless Gauss had pursued the idea that the three-dimensional surface, into which the two-dimensional surface of the earth is embedded, might be of the same complexity as that surface, or more generally, that the intrinsic geometry of three-space might be constructible along the same lines as that of two-space, and that therefore actual three-space might exhibit the same characteristics of metric inhomogeneity, changing (not merely non-zero) curvature, etc., as the actual two-dimensional surfaces contained in it.

In any case, there can be no doubt that a proper understanding of both the development and the significance (for physics, etc.) of non-Euclidean geometry can only be attained from the standpoint of a generalization to higher dimensions — first developed explicitly in Riemann's 1854 *Über die Hypothesen, welche der Geometrie zu Grunde liegen* — of Gauss' theory of curved surfaces, and not from the abstract standpoint of questions concerning the provability or independence of Euclid's "fifth (parallel) postulate," which, as Weyl correctly points out, "seems to us nowadays to be a somewhat accidental point of departure."

Gauss' point of departure was his astronomical investigations and the cosmological and epistemological concerns that grew out of them. He undoubtedly was familiar with the kind of cosmological problem typified by the Olbers paradox of which we gave a brief exposition above. Otherwise his theoretical grounding was in Leibniz' relativity doctrine of space, empirically reinforced by his growing involvement in the development of the mathematical theoretical framework for a unified and comprehensive understanding of the phenomena of electricity and magnetism — efforts which between 1838 and 1840 led to the publication of two fundamental treatises in this field: 1. General Theory of Earth Magnetism; and 2. General Theorems Relating to Attractive and Repulsive Forces Acting in an Inverse Proportion to the Square of the Distance.

Perhaps the, in brief, most striking example of his continuum approach to problems of physics and astronomy can be gleaned from the title of his 1818 treatise on secular disturbances of the orbits of planets, developed in conjunction with the computation of the orbit of the asteroid Pallas:

Determinatio attractionis quam in punctum quodvis positionis datae exerceret planeta, si eius massa per totam orbitam ratione temporis, quo singulae partes describuntur, uniformiter esset dispartita.
(Determination of the attraction which a planet would exercise upon an arbitrary point of a given position, if its mass were uniformly distributed over the entire orbit in proportion to the time in which the individual parts described run through the orbit. Assume that the mass of a planet be distributed along its orbit in inverse proportion to the orbital speed at a given point. Then compute the force of attraction of this ring upon a test body.)

This great amout of emphasis has been put on the epistemological and empirical science context of Gauss' work because in immediate continuation and amplification of the thrust of the relativity doctrine of space discussed above, his "anti-Euclidean" and differential geometry represent a major step towards the complete transformation of geometry from the study of fixed, abstract homogeneous space into the study of the changing, intrinsically determined configurations of the internal relations of the evolutionary process of substance. It is the germ of such a conception of space that Riemann discerns in Gauss' work and elaborates in his own writings.

The extent to which Gauss fails to free himself completely from the notion of absolute space finds its expression in the fact that in his explication of the concept of a metrically inhomogeneous surface the Euclidean metric remains essential — even though only "in the small." I shall demonstrate that in the case of Riemann this is not so, and that therefore the customary view that Riemann's geometry is sufficiently characterized by describing it as a generalization to n dimensions (arbitrary n) of Gauss' theory of curved surfaces is incorrect.

Fourier: "Arbitrary" Functions

Before entering into the description of some key elements of Fourier analysis — the analogue in the theory of functions of Gauss' achievements in geometry — it will be useful to briefly pose the antinomy of discreteness vs. continuity. The antinomy can only be overcome if the assumption of what one might call "unilegality" — the notion that one set of laws is given once and for all (when? why these?) — is discarded and replaced by an evolutionary conception of substance which embodies, through the creation of qualitatively new individuals, the idea of changes in the laws that govern the process of evolution, i.e. changes in the "laws of nature." For geometry, conceived of not axiomatically, but as the science of real space, this means discarding the notion of absolute space and that of *one* geometry characterized by *one* metric along with it. Thus the introduction of metric inhomogeneity, intrinsically determined, rather than the axiomatic introduction of the negation of the parallel-axiom, which simply leads to a metrically homogeneous space of different constant curvature than Euclid's and leaves the notion of absolute space completely intact, defines the significant advance over Euclideanism. Looking ahead, with Riemann this leads toward the systematic distinction between the topological and metric properties of space, and a given topological manifold is demonstrated to be susceptible of different metric relations — the very idea to which Cantor, by focusing on the invariance character of the metric, ultimately gives coherent epistomological expression in his notion of different orders of the transfinite.

A full understanding of the significance of Fourier's function-theoretical results must proceed from the idea of functions as representing the internal relations of substance and of space as the complex of such relations; or conversely, from viewing functions as dimensionally scaled down projections of the world geometry in a given region or over a given period of time. As we pointed out above, this was at least in principle the standpoint of Descartes in his development of analytical geometry, where space is regarded as composed of the entirety of curves traced out by mass points in motion. It is rigorously adopted in Riemann's investigations of functions of a complex variable, where the surface determined by a complex function is no longer simply a kind of visual aid guiding our intuition, but becomes an essential, indispensible part of the theory; and it finds its most developed expression in the key notion of Hermann Minkowski's formulation of Einstein's Relativity Theory, the notion of the "world line" of a particle in the four-dimensional space-time manifold.

On the basis of such a unified perspective on geometry and function theory, which leads Riemann to the establishment of an entirely new branch of mathematics — topology — we can describe the effect of Fourier's results as having demolished what is the precise analogue of "Euclideanism" in analysis: the assumption — formulated most explicitly in Lagrange's *Théorie des fonctions analytiques* (1797) — that the concept of a function $y = f(x)$ is identical with that of an analytical expression in x representable by a power series

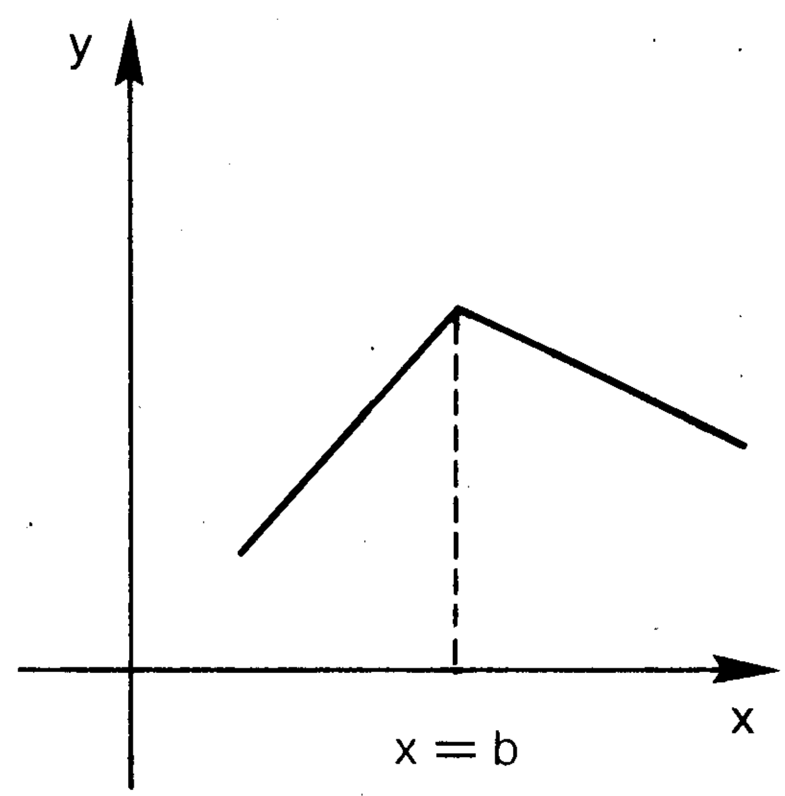
$$y = P(x) = a_0 + a_1 x + a_2 x^2 + \dots, \text{ etc.}$$

The analogy between "Euclideanism" (or, actually, more broadly, the homogeneity of space) and the requirement of analyticity holds in the sense that both, within the framework of Newtonian dynamics, are consequences of the hypothesis of "hard ball" particles as the ultimate constituents of matter. To show this for analytic functions, we first have to take a somewhat closer look at the latter. The following is a sufficiently precise definition of Lagrange's intended concept:

$f(x)$ is *analytic* in a neighborhood of $x = a$ if and only if it is representable there by a convergent Taylor series, i.e., a series of the form

$$f(x) = f(a) + \frac{f'(a)}{1!}(x - a) + \frac{f''(a)}{2!}(x - a)^2 + \dots \text{ etc.}$$

An analytic function, so defined, has a number of remarkable properties, most importantly for our present purpose it is "very smooth," i.e., it has an unlimited number of continuous derivatives. If we think of the graph of such a function as the path of a Newtonian particle in motion, it becomes immediately obvious why Lagrange regarded only analytic functions as relevant and admissable for the mathematical representation of natural phenomena. Assume, contrary to the requirement of analyticity, that at a point $x = b$ the function $f(x)$ representing the motion of a given particle, though continuous, is not differentiable; e.g. let $f(x)$ have a sharp corner at $x = b$.



Then f(x) does not have a unique tangent at b, or, in terms of the particle, at $x = b$ we cannot determine its momentary direction. This, of course, would violate the otherwise assumed completely deterministic character of the particle's behavior. Or, to express the whole affair in positive terms: for analytic functions, knowledge of the values of the ordinate for an arbitrarily small piece of the abscissa determines the course of the function in its entirety. Thus let the y-values be known for an arbitrarily small neighborhood around $x=0$. Then we know the values of the differential quotients of all orders at $x=0$ and, hence, because

$$f(o) = a_0 , \; f'(o) = a_1 , \; f''(o) = 2a_2 , \ldots \text{etc.}$$

for the coefficients $a_0, a_1, a_2, \ldots$ of the power series, know the whole function. It is precisely this deterministic quality of analytic functions which to Lagrange recommended their exclusive adoption for mathematical physics.

Interestingly enough, a much broader explication of the concept of a function had been given some 50 years before Lagrange's *Théorie* by Euler. In his investigations of the problem of the vibrating string he had been forced to consider functions $y = f(x)$ defined by an "arbitrary" curve (*"libero manus ducta"*) and subject only to the condition that any parallel to the y-axis intersects the curve only once. However, in the *Introductio in analysin infinitorum* (1748), Euler reverts to the restrictive identification of a function y of x with an "analytical expression" in x. This can hardly be surprising, for Euler's mathematics was fully grounded in the conceptual framework of Newtonian mechanics.

A systematic extension of the concept of a function beyond the analytic ones became possible only when physicists posed for themselves the problem of mathematical comprehension of phenomena which in principle transcended the scope of mechanical theories, or, more accurately, when they adopted a view of certain physical phenomena which was totally at variance with that of Newtonian mechanics.

Such was the case with Fourier's theory of heat which he had worked out as early as 1807 but did not fully publish until the appearance in 1822 of his *Théorie Analytique de la Chaleur*. While there is no immediate documentary evidence to this effect, Fourier's concept of heat appears to be derived as a direct application of Kant's notion, in the *Metaphysische Anfangsgründe*, of a continuum of attractive and repulsive forces. I quote from the *Théorie Analytique*, ch. I, sect. II, "Preliminary Definitions and General Notions":

> The free state of heat is the same as that of light; the active state of this element is then entirely different from that of gaseous substances. Heat acts in the same manner in a vacuum, in elastic fluids, and in liquid and solid masses, it is propagated only by way of radiation, but its sensible effects differ according to the nature of bodies.
>
> Heat is the origin of all elasticity; it is the *repulsive force* which preserves the form of solid masses, and the volume of liquids. In solid masses, neighboring molecules would yield to their mutual *attraction* if its effect were not destroyed by the heat which separates them.
>
> This elastic force is greater according as the temperature is higher; which is the reason why bodies dilate or contract when their temperature is raised or lowered.
>
> The equilibrium which exists, in the interior of a solid mass, between the repulsive force of heat and the molecules' attraction, is stable; that is to say it re-establishes itself when disturbed by an accidental cause.

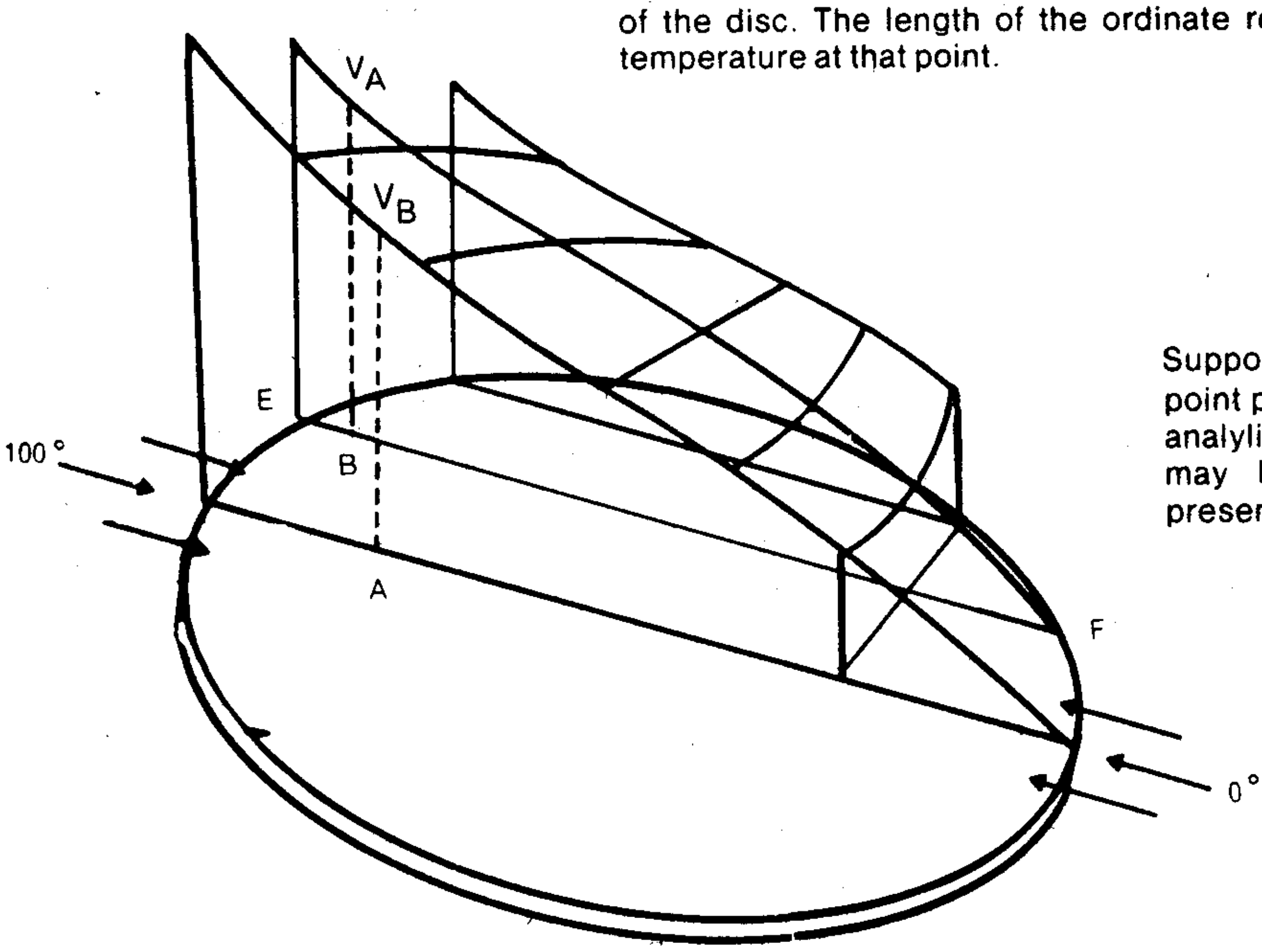
The field-theoretical implications define the strength of this conception and its obvious superiority over Lavoisier's "caloric" (heat fluid) theory. According to his own testimony in the "Preliminary Discourse" to his work, Fourier arrived at his notion of heat and of the action of heat through consideration of the effects of the sun on the biosphere; and, at one point, his field conception — even to the extent of regarding the field as primary and as determining the properties of the bodies immersed in it — becomes quite explicit, if only in a questioning, speculative manner:

> But independently of these two sources of heat (terrestrial and solar), is there not a more universal cause, which determines *the temperature of the heavens*, in that part of space which the solar system now occupies? Since the observed facts necessitate this cause, what are the consequences of an exact theory in this entirely new question; how shall we be able to determine that constant value of *the temperature of space*, and from it the temperature which belongs to each planet?

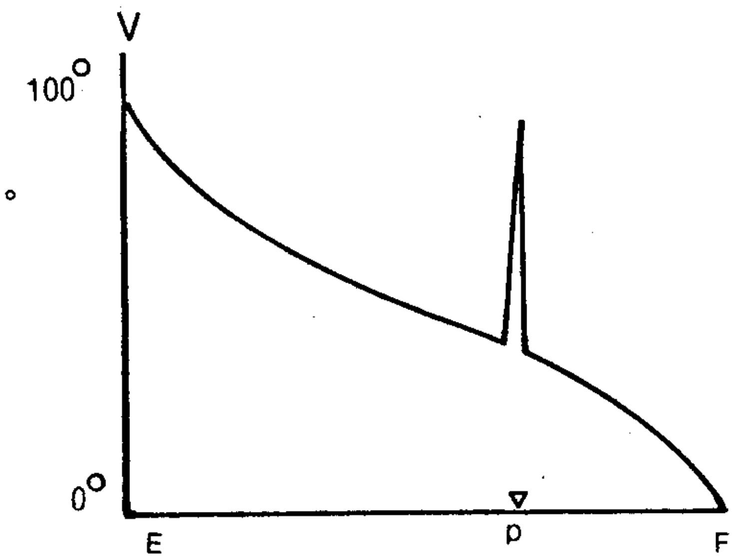
Beyond such specifics it is useful to take a brief look at the overall breadth and scope of Fourier's life activities to get a sense for the source of his accomplishments and the decision and self-confidence with which he puts forth his new ideas.

From 1796 to 1798, Fourier teaches at the *Ecole Polytechnique*, an institution of the French Revolution, founded for the purpose of training the officers of the revolutionary armies. At the time, the school was under the leadership of the geometer Monge, later Minister of the Navy under Napoleon. Both Monge and Fourier participate in Napoleon's expedition to Egypt, and after their return Fourier becomes prefect of the *Department Isère* in Grenoble (until 1817). What he expected from his work is clear from his own words:

A *temperature surface* is formed by connecting the ordinates which are extended from points on the surface of the disc. The length of the ordinate represents the temperature at that point.



Suppose that at time t = 0 the tip of a hot pin is placed at point p on line ds. Such a curve is not susceptible to the analylitical treatment required by Lagrange, but rather may be expressed by a trigonometric series as presented by Fourier.



> It is easy to judge how much these researches concern the physical sciences and civil society, and what may be their influence on the progress of the arts which require the employment and distribution of heat. They have also a necessary connection with the system of the world, and their relations become known when we consider the grand phenomena which take place near the surface of the terrestrial globe.

The principal problem Fourier considers is that of the propagation of heat in solids of different geometrical shapes and under arbitrary initial and boundary conditions. Take, for example, a metallic disc of large diameter which at one part of its boundary is exposed to a source of heat of constant temperature, say of 100 degrees, and at another part of its boundary is immersed in ice water. After a sufficient amount of time has elapsed, we can then ask what the stationary temperatures at each point of the surface of the disc will be as the result of the propagation of heat through the disc under the given *boundary conditions.* Now suppose that an ordinate be raised perpendicular to the plane of the disc whose length is proportional to the stationary temperature at that point. The end points of the ordinates will represent a curved surface extended above the plane of the disc. We arrive at the next type of propagation problem, if we assume that at a certain time $t=0$ the heat source is removed from the boundary of the disc, and we want to find an analytic expression for the continuous change with time of the shape of the curved surface under the *initial conditions* represented by the stationary temperatures at $t=0$.

As a general expression for the propagation of heat in three-dimensional homogeneous solids, Fourier wrote down the partial differential equation

$$\text{(I)} \qquad \frac{\partial v}{\partial t} = C\left(\frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2} + \frac{\partial^2 v}{\partial z^2}\right)$$

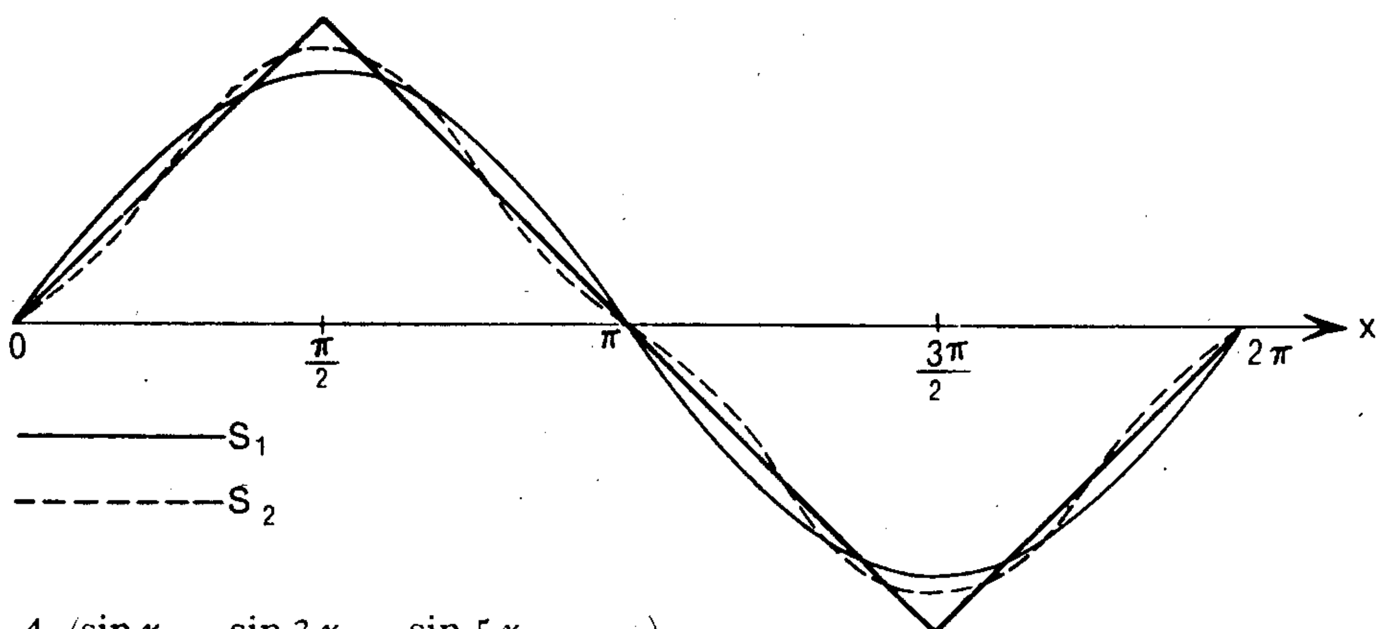
where $v = f(x,y,z,t)$ is the temperature at time t and point (x,y,z), and C a constant depending on the solid under consideration. The difficulties in integrating this equation arise from the fact that it lies in the nature of our initial value problem just discussed that v at $t=0$ may be an entirely arbitrary (continuous) function which we can trace as a curve through the end-points of the temperature ordinates, but which, in general, will not be given in the form of an analytical expression that can be subjected to the required analytical treatment. Fourier solves the problem by showing that any function whatsoever ("*des fonctions absolument arbitraires*"), including functions with a certain number of discontinuities in a given interval may be represented by one analytical expression for the entire interval, viz. by a convergent trigonometric series of the form

$$
\begin{aligned}
f(x) &= \tfrac{1}{2} a_0 + a_1 \cos x + a_2 \cos 2x + \ldots \\
&\quad + b_1 \sin x + b_2 \sin 2x + \ldots \qquad \text{(II)} \\
&= \frac{a_0}{2} + \sum_{n=1}^{\infty} (a_n \cos nx + b_n \sin nx)
\end{aligned}
$$

where in the most general case the coefficients a_n and b_n are given by

$$
\text{(III)} \qquad a_n = \frac{1}{\pi} \int_0^{2\pi} f(x) \cos nx \, dx, \qquad b_n = \frac{1}{\pi} \int_0^{2\pi} f(x) \sin nx \, dx.
$$

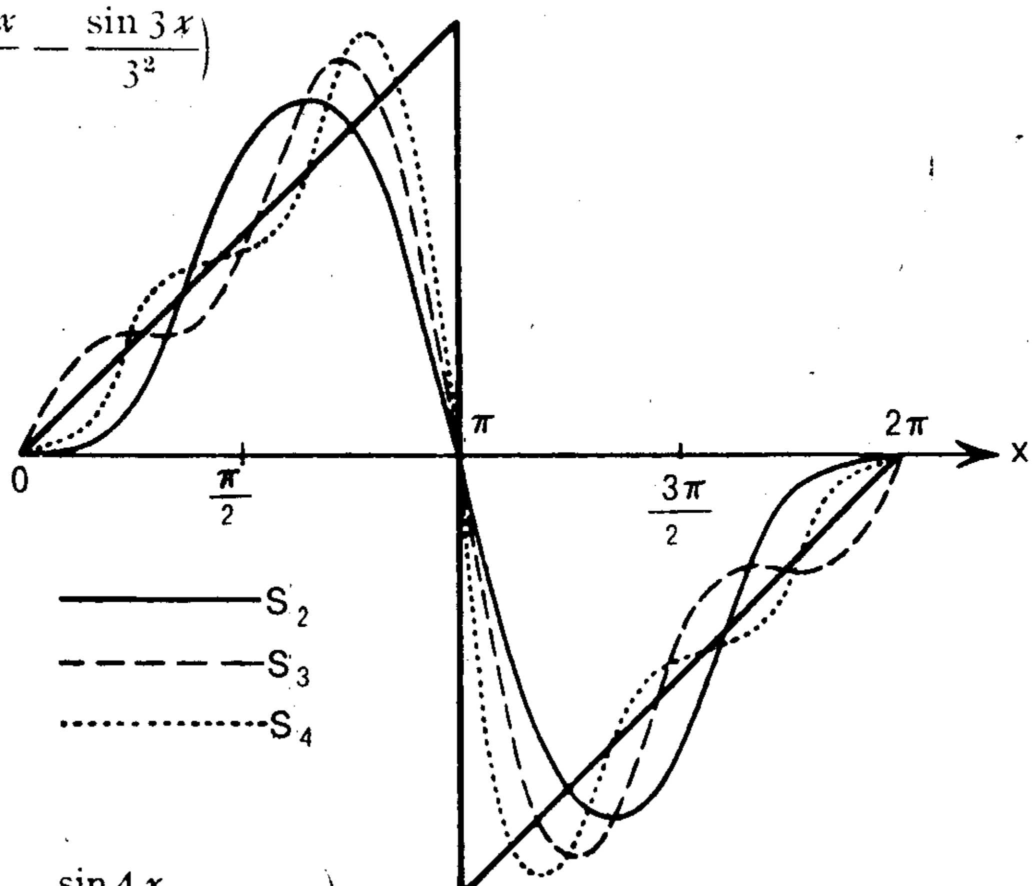
The idea behind the approximation of an arbitrary function by means of superimposition of different trigonometric functions is best explained graphically:



$$f(x) = \frac{4}{\pi}\left(\frac{\sin x}{1^2} - \frac{\sin 3x}{3^2} + \frac{\sin 5x}{5^2} - \cdots\right),$$

$$s_1(x) = \frac{4}{\pi}\frac{\sin x}{1^2},$$

$$s_2(x) = \frac{4}{\pi}\left(\frac{\sin x}{1^2} - \frac{\sin 3x}{3^2}\right)$$



$$S(x) = 2\left(\frac{\sin x}{1} - \frac{\sin 2x}{2} + \frac{\sin 3x}{3} - \frac{\sin 4x}{4} + - \cdots\right).$$

By means of the Fourier series representation of very general types of functions the scope of the concept of functional relationships susceptible of mathematical treatment (*mathematisch behandelbar*) was, of course, tremendously increased and Lagrange's world was shattered. In his own treatment of functions representable by a trigonometric series (Göttingen, 1854), Riemann reports the existence of a document in the archives of the French Academy which shows that when Fourier first presented his results to the Academy in 1807, his assertion that entirely arbitrary, graphically given functions may be expressed by a trigonometric series caught the aging Lagrange unaware to such an extent that he opposed it in a most virulent manner.

Such anecdotes aside, the broad epistemological consequences of Fourier analysis can readily be brought out as follows: I developed above the appropriateness of analytic functions to the thoroughgoing determinism of Newtonian mechanics, associated specifically with the discrete, "hard ball," particle conception of matter. However, even though I have counterposed Fourier's developing field theoretical notions and his introduction of arbitrary functions directly to Lagrange's brand of Newtonianism, it should not be assumed that it is the strict determinism of the latter which is tossed overboard in Fourier's theory and the field theories developed after him. Determinism is not the *differentia specifica* of Newtonian mechanics and there is nothing in the mathematics of classical field theory which is incompatible with a deterministic position. Thus Newton's theory of gravitation is actually most convincingly and elegantly formulated in the field theoretical framework of the mathematical potential theory of Poisson and Gauss. The difference lies elsewhere: consider again the arbitrary temperature function defining a typical Fourier initial or boundary value problem. In principle we will be confronted there with a very "bad" looking curve representing an extremely varied (but, in general, continuous) distribution of values. Still, we have to get an analytical grip on even the most inhomogeneous such "scalar field," if the heat propagation problem in that case is to be solved. In Newtonian dynamics, on the other hand, that kind of problem will, at least in principle, never arise. We will never have to face more than a finite or at most discrete distribution of phase quantities, and the necessity for the introduction of arbitrary initial value functions will never come up.

This is where the crucial ontological difference between field and particle conceptions of matter (or substance) directly comes to the fore. The important sense in which the field concept defines a critical advance over atomism is in its monistic conception of substance, so that all relations become empirical, internal relations, and through the mutual determination and interpenetration of the whole and its parts the possibility at least — which does *not* exist for the Newtonian system — for the introduction of the kind of "freedom" demanded by a coherent solution to the discreteness-continuity antinomy presents itself for the first time.

These are, of course, old problems, discovered well before Schelling attacked atomism and Hegel, in turn, Schelling's amorphous monism.

Parmenides, more astute than Schelling, had long ago concluded that if substance is one and homogeneous, then change cannot exist and the appearance of change must be an illusion. Atomistic conceptions were introduced by Democritus and Leucippus to save the appearances and to make change possible at least in the form of changes in the combination of atoms, i.e. in their external relations. Thus, change was reinstituted by, so to speak, pushing its illusory character beneath the threshhold of perception. Parmenides' problem had only been covered over; it still awaits its solution.

3. Riemann: The World Manifold

Discussing the mathematics of Einstein's relativity principle, Hermann Minkowski, in a lecture before the Göttingen mathematical society, Nov. 5, 1907, introduced his paper with the following remark:

> Mathematicians are particularly well pre-disposed to accept the new conceptions, because to do so is a matter of getting acclimated to a conceptual mold which has long been utterly familiar to them. Physicists, on the other hand, must now at least in part invent these concepts anew and with great effort cut a path through a jungle of unclarities, while quite close by the mathematician's well-constructed road built long ago leads comfortably forward. Indeed, all in all the new hypotheses, if they actually do represent the phenomena correctly, would almost mean the greatest triumph which the application of mathematics has ever shown. What is at issue — to put it as briefly as possible — is that the world in terms of space and time in a certain sense is a four-dimensional, non-Euclidean manifold. To the glory of mathematics and the boundless astonishment of the rest of mankind, it would become apparent that the mathematicians, purely in their fantasy, had created a whole large field to which one day, without this ever having been the intention of these idealist fellows, should accrue perfectly real existence.

On a separate occasion Minkowski even speaks of a kind of "pre-established harmony between pure mathematics and physics."

It is, of course, at first sight quite astonishing that more than 50 years before the establishment by Einstein of the Special and the General Theory of Relativity Gauss and particularly Riemann created *in full* the mathematical forms in which relativity theory is most appropriately cast. However, a mere cursory reading of the most important of Riemann's original treatises, first published in a complete edition including an appendix of philosophical fragments by H. Weber and Dedekind in 1876, makes clear that no pre-established harmony assumption needs to be introduced to explain this phenomenon. There is ample documentary evidence that ever since his days as a student in Berlin (1847-1849) Riemann, who, incidentally, started his career as a student of Protestant (Evangelical-Lutheran) theology, was embarked upon a most broad-based epistemological project of attempting to create one comprehensive mathematical-theoretical framework for the unified expression of the phenomena of light, electricity, magnetism, heat, and gravitation, then dealt with by almost totally separate branches of physics. Throughout his life he regarded this as his main work (*"meine Hauptarbeit"*) and therefore, if the question is asked why Riemann's "purely mathematical" developments are so wonderfully appropriate to the most advanced developments of general relativity, the answer is straight forward: not only was Riemann deeply involved in coping with the same general type of problems as Lorentz, Poincaré, Einstein, et al. half a century later, but the scope of his project of a "unified field theory," as it would now be called, undoubtedly exceeded that of Einstein, employing a methodological rigor which Einstein approximated only on a few rare occasions. In the following I shall prove this both directly and in terms of the indirect evidence provided by a unified analysis of Riemann's discoveries in what are nowadays too easily regarded as entirely separate fields of mathematical inquiry. In the process one thing in particular should become unquestionably clear — that the contemporary view of Riemannian geometry, based principally upon the interpretations of Christoffel, Ricci, Levi-Civitá, and more recently Hermann Weyl are altogether too narrow, and in their restrictiveness import precisely those problems into theoretical physics which presently stand in the way of expanding general relativity beyond its present limitations into a theory of the unified field.

I start with an account of Riemann's most explicit and detailed extant treatment of the unified field problem, included among the philosophical fragments under the title "New Mathematical Principles of Natural Philosophy." (A footnote in the text says: "Discovered on March 1, 1853.") Referencing the piece (though not necessarily the particular version contained in the appendix to the collected works), Riemann in a letter to his brother, dated Dec. 28, 1853, writes:

> My other investigation on the connection between electricity, galvanism, light, and gravity I had resumed immediately after the completion of my *Habilitationsschrift*, and I have gotten far enough with it so that without second thoughts I can publish it at this time.

The key elements of the theoretical sketch are as follows:

First, the methodological premise — "Let us try to deduce it (the internal state or constitution of ponderable bodies) by way of analogy from our own inner (mode of) perception."

There follows a number of specifics on the functioning of the soul based on the psychology of Herbart. These are not important. What matters is the methodological rigor embodied in the quote. Mind and body are of the same world abiding by the same sets of laws, and in these the laws of the mind are epistemologically prior. I have developed this in detail in Section I.

Second, the internal relations of substance — There exists a space-filling substance (*Stoff*) — later called aether — which has the properties of an incompressible homogeneous fluid without inertia. Both gravitational and electromagnetic effects are propagated through and explained as modifications of the space-filling aether.

The effects of ponderable matter upon ponderable matter fall into two classes: 1. attractive and repulsive forces, inversely proportional to the square of the distance; and 2. light and heat radiation. These effects are characterized by means of one unified action principle (*"Wirkungsgesetz"*), derived on the basis of the assumption that aether particles only act upon their immediate neighborhood.

The unified action finds its expression in a force which acts to change the form of the infinitesimal aether particle at point $O=(x,y,z)$ and can be thought of as resulting from forces which would effect a change in the length of the line element s ending at $O'=(x',y',z')$. The mathematical form of the action principle is the following:

If dV is the volume of an infinitesimal aether particle at point O and time t, and dV′ the volume of the same particle at t′ then the force resulting from the difference in the two states of the aether which acts to elongate ds is given by

$$a \cdot \frac{dV - dV'}{dV} + b \cdot \frac{ds - ds'}{ds}$$

This law can be thought of as split into two parts: 1. the resistance offered by an aether particle to a change in volume; and 2. the resistance offered by a physical line-element to a change in length. Gravitation and electrostatic attraction and repulsion are based on the former, propagation of light and heat and the electrodynamical or magnetic attraction and repulsion on the latter.

Riemann concludes his sketch as follows:

> Now there is no reason to assume that the effects of both causes change with time in accordance with the same laws: thus adding up the effects of all the earlier forms of substance-particle upon the change of the line-element ds at time t, then the value of
>
> $$\frac{\partial ds}{dt}$$
>
> which they attempt to bring about becomes
>
> $$= \int_{-\infty}^{t} \frac{dV' - dV}{dV}\, \psi\,(t - t')\, \partial t'$$
>
> $$+ \int_{-\infty}^{t} \frac{ds' - ds}{ds}\, \phi\,(t - t')\, \partial t'.$$
>
> How now must the functions ψ and ϕ be constituted so that gravitation, light and radiating heat can be propagated by the spatial medium?

Parenthetical Note on Riemann's "A Contribution to Electrodynamics" (1858)

The significance of the above sketch of a unified physical theory is underlined by the fact that the 1858 piece suggesting an electromagnetic theory of light is directly based upon the general ideas put forth in the sketch. Riemann writes in 1858:

> I have found that the electrodynamic effects of galvanic currents can be explained if one assumes that the action of one electrical mass upon the others does not occur instantaneously, but is propagated to them with a constant velocity (within the errors of observation equal to the speed of light). The differential equation for the propagation of force under this assumption becomes the same as that for the propagation of light and of radiating heat.

Riemann's mathematical elaboration of this idea is based upon introducing a time variable into the condition for the potential function U of electrical masses, replacing the usual condition for the simultaneous scalar potential

$$\text{(I)} \qquad \frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} + \frac{\partial^2 U}{\partial z^2} = -4\pi\rho$$

by the condition for what nowadays is called a "retarded" potential

$$\text{(II)} \qquad \frac{\partial^2 U}{\partial t^2} \Big/ c^2 - \left(\frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} + \frac{\partial^2 U}{\partial z^2} \right) = -4\pi\rho$$

where c is a constant (velocity).

Potential functions for (I) and (II) respectively will then be of the form

$$\frac{f(t)}{r} \quad \text{and} \quad \frac{f(t - \frac{r}{c})}{r}$$

Retarded potentials were, of course, to play a key role in H.A. Lorentz' theory of the electron, the direct forerunner of Einstein's Special Relativity. Furthermore, it can be proved (as was done by Levi-Civitá) that Riemann's results in the 1858 paper are quite sufficient to derive Maxwell's equation of the electromagnetic field without complicated recourse to displacement currents and what not. (Interestingly, Clausius, whom Maxwell quotes as his source in his dismissal of Riemann's ideas, regarded Riemann's formulas as "mathematically" (!) unsound.)

At this point, rather than proceeding to a more detailed analysis of Riemann's speculations on natural philosophy, I will merely emphasize two things for purposes of reference later on. First, the "unified field" and its carrier, the aether, have an independent existence and determine the behavior of ponderable matter. Second, different field actions are unified into one action principle which in turn determines the geometry of the field by determining the length of the infinitesimal line element ds for the neighborhood at any given point. Both points will be crucial as we proceed to Riemannian geometry and the question of the internal determinations of n-dimensional manifolds. Beyond this, of course, an outline of Riemann's attempt at a unified physical theory should be quite sufficient at this point to establish, at least in principle, the above claim concerning both the scope and the depth of Riemann's overall project.

Riemann and Faraday

In a famous 1894 Vienna lecture on "Riemann and his significance for the development of modern mathematics,"Felix Klein draws a direct parallel between Riemann's accomplishments in mathematics and Faraday's in physics, locating the similarity specifically in their "near-action" notions:

> What in physics is the banishing of far-actions, the explanation of the phenomena by means of the internal forces of a space-filling aether, this, in mathematics, is the understanding of functions on the basis of *their behavior in the infinitely small*, in particular, therefore, on the basis of the differential equations they satisfy."

There undoubtedly exists that parallel, but we need to explain why. The necessary point is most easily made by drawing the conceptual connection between Faraday's notion of the electromagnetic field and Riemann's concept of an n-dimensional manifold, further elucidating both.

Under the influence of the same *"Naturphilosophie"* conceptions which led Oersted to his discovery of electromagnetism — conceptions transmitted to England by Coleridge, et al. — Faraday arrived at the notion of the electrostatic and magnetic "line of force," basic to his full field-theoretical conceptions of electricity and magnetism. He arrived at this notion some ten years before his own researches — falling mainly into the two decades between 1830 and 1850 — in the 1831 discovery of the induction of an electrical current in a wire by a moving magnet yielded the first convincing empirical verification of the existsnce of such lines. The discovery of induction in the first place, of course, established an important kind of symmetry of electrical and magnetic forces: just as electrical currents produce a magnetic effect, so Faraday had now shown that magnetic forces can produce electrical effects. However — and this is where the discovery of magnetic induction points, well beyond itself — only the phenomenon of induction strictly requires the assumption that the actual energy giving rise to the inductive current is located — along the magnetic lines of force — in the medium surrounding the magnet, rather than being concentrated at the poles, acting upon bodies at a distance. Indeed, the strength of the current induced in the wire is directly proportional to the number of lines of force cut by the surface enclosed by the wire.

Faraday's case for the independent significance of the lines of force was greatly strengthened by the results of the application of his subsequent researches in electro-chemistry to electrostatic induction. Analyzing the phenomena of electrochemical decomposition he found that it was not — as the Newtonian theory had maintained — the (distance-) action of the poles (cathode, anode) upon the electrolytic solution, but the actual current flowing through the solution that produced the observed effects: "The metallic poles would appear to be mere terminations of the decomposable substance." The insignificance of poles was proved by passing electricity through a salt solution and then simply letting it discharge as a spark into the air. Even in the absence of poles decomposition occurred in the usual fashion. This elimination of the notion of poles as centers of force, and consequently the elimination of (straight line) action of central forces at a distance, of course, served to reinforce Faraday's earlier conclusions concerning the lines of force and to further focus his attention on the medium doing duty as the carrier of these lines. Such a focus by itself is an important departure from the Newtonian framework where knowledge of the position and momentum of a particle are regarded as sufficient to completely predict the entire future course of events.

In electrostatics, the near-action hypothesis associated with the line of force conceived of as a line of strain or tension in a medium immediately proved successful, leading to the discovery of the new material constant of specific inductive capacity and forcing a revision of Coulomb's Law of Electrostatic action

$$F = \epsilon \frac{Q_1 Q_2}{r^2}$$

which had been modeled on Newton's Laws of Gravitational Attraction, with ϵ assumed to be of the same universal character as the gravitational constant g in

$$F = g \frac{m_1 m_2}{r^2}$$

Still, the final step toward a "pure" field-theory conception of electricity and magnetism had yet to be made. Both in electrolysis and in electrostatics the line of force could be conceived of as a chain of particles polarized (deformed) by forces acting upon the endpoints of the chain. Not so in the case of magnetism. In 1845, Faraday demonstrated that the magnetic lines of force, rather than being composed of particles, actually acted upon the particles of any given substance so as to bring them in line with *(paramagnetics)* or set them across (*diamagnetics*) the lines of force of a given magnetic field.

Interestingly, the work that followed on trying to find an explanation for the phenomenon of diamagnetism brought Faraday into almost direct contact with Riemann who, after two years of study at the University of Berlin, had returned to Göttingen in the spring of 1849. There, he attended the lectures on experimental physics and became an active participant — notably also in the laboratory work — in the mathematical-physical seminar of Wilhelm Weber.

Now it was Weber who, after the publication by Faraday of his discovery of diamagnetism, had proposed and thought that he had experimentally verified the theory that diamagnetism, just like the much stronger paramagnetism, involved polarity, but a polarity opposite to that of paramagnetics. This would have explained Faraday's observation, and without taking any recourse whatever to the "lines of force" notion. Understandably Faraday looked for an alternative interpretation of Weber's experimental results, coming up in the process with the key concept of permeability and a way of reducing the notion of polarity to that of relatively high concentrations of lines of force. The way the concept of permeability took shape can be seen from the following note in which lines of force are thought of in analogy to rays of light:

> I cannot resist throwing forth another view of these phenomena (of para- and diamagnetism) which may possibly be the true one. The lines of magnetic force may perhaps be assumed as in some degree resembling the rays of light, heat, etc. and may find difficulty in passing through bodies, and so be affected by them, as light is affected.

Thus diamagnetics are relatively poor conductors (relatively impermeable) of magnetic lines of force, and deflecting them into the more easily permeable surrounding medium, will seek out points of least magnetic action in an inhomogeneous field. This, rather than reverse polarity, accounts for their apparent repulsion by a strong magnet. Paramagnetics react in the opposite fashion. Furthermore, since the permeability of a substance is measured relative to the surrounding medium, diamagnetics can be made to behave like paramagnetics (and vice versa) in the obvious way.

This leaves the lines of force as the only thing that has an absolute significance. They represent the magnetic force in the space surrounding a magnet, their strength — even in the case of permanent magnets — being determined not by some imaginary "amounts of magnetism" concentrated at the poles, but by the condition of the magnetic substance as a whole through which the lines of force run as continuous curves, converging at the endpoints and creating the appearance of polarity. They do not even require a carrier medium for their existence; they exist

> not by a succession of particles, as in the case of static electric induction...but by the condition of space free from such material particles. A magnet placed in the middle of the best vacuum we can produce, and whether that vacuum be formed in a space previously occupied by paramagnetic or diamagnetic bodies, acts as well upon a needle as if it were surrounded by air, water, or glass; and therefore these lines exist in such a vacuum as well as where there is matter.

While obviously a full account could not be given here, it is an exciting thing to follow the evolution of Faraday's ideas from the inception of the mere notion (*Vorstellung*) of the line of force through its development into a scientific hypothesis and finally a comprehensive theoretical concept (*Begriff*), with each step of the development consolidated and empirically anchored by select experiments. The final conception Faraday arrives at — put forth as early as 1846 — is that of a field of criss-crossing lines of force as the actual spatial location of physical energy, not itself in need of a substantial carrier, but capable of transporting other physical phenomena, such as light. Gravitational lines of force are later added to the magnetic and electrostatic ones, thus completing the picture so as to encompass all known types of physical force and action. As Maxwell — commenting here specifically on gravitation — wrote in a letter to Faraday in response to the latter's full presentation of his ideas in an 1857 paper:

> ...then your lines of force can "weave a web across the sky" and lead the stars in their courses without any necessarily immediate connection with objects of their attraction.

It was not, however, Maxwell, but, ironically, the then-assistant in the mathematical-physical seminar of Faraday's opponent Wilhelm Weber, Bernhard Riemann, who in 1854 in his *Habilitationsvortrag* (Inaugural Address), *"On the Hypotheses upon which Geometry is Based,"* and in pursuit of his own ideas for a unified physical theory, drew the radical conclusions from Faraday's work.

Note on Maxwell: Indifferentism

It is one of the more remarkable aspects of Maxwell's *Treatise on Electricity and Magnetism* that while throughout its two volumes the author claims to be an ardent follower and advocate of Faraday's views of electromagnetic phenomena, there nowhere appears a coherent exposition of these views; indeed the only physical theories elaborated at length are those of the "atomists" and "far-action" theorists, Ampère and Wilhelm Weber — Faraday's leading opponents. This led Felix Klein to state in his *Lectures on the Development of Mathematics in the 19th Century* that

> "there can be no doubt that Maxwell was an atomist at heart....If Maxwell in his *Treatise* nonetheless chooses exclusively the phenomenological mode of presentation, I regard this as a conscious act of resignation.

Klein is too polite, and matters are not that simple. The attitude displayed by Maxwell in his *Treatise* is precisely that mood of weariness and indifferentism, rightly castigated by Kant as the archenemy of scientific inquiry, "the mother, in all sciences, of chaos and night."

Citing from the preface of the *Treatise:*

> I was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the mathematicians, so that neither he nor they were satisfied with each other's language. I had also the conviction that this discrepancy did not arise from either party being wrong....For instance, Faraday, in his mind's eye, saw lines of force traversing all space where the mathematicians saw centers of force attracting at a distance; Faraday saw a medium where they saw nothing but distance: Faraday sought the seat of the phenomena in real actions going on in the medium, they were satisfied that they had found it in the power of action at a distance impressed on the electric fluids.
>
> When I had translated what I considered to be Faraday's ideas into a mathematical form, I found that, in general, the results of the two methods coincided, so that the phenomena were accounted for, and the laws of action deduced by both methods...

Observe that embodied in this is a virtual restatement of the discreteness-continuity antinomy, and then compare Kant's and Maxwell's reaction. The former regards the understanding of the necessity of such antinomies as crucial and indispensable to the progress of scientific knowledge, the latter perceives mere differences in formulation, nothing that could not be straightened out by choice of the appropriate mathematical method.

Faraday, in no way shared Maxwell's attitude. Especially in the reports on his later researches, and contrary to Maxwell's misrepresentations (cf., below) of his expicitly stated views, he regarded the electromagnetic and gravitational field, defined by the lines of force, as the primary physical entity. Far from being satisfied with letting fields and particles stand indifferently side by side, he actually attempted to interpret matter as a special field condition:

> Faraday: there are the lines of gravitating force, those of electrostatic induction, those of magnetic action...I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted.
>
> And now Maxwell: He (Faraday) even speaks of the lines of force belonging to a body as in some sense part of itself, so that in its action on distant bodies it cannot be said to act where it is not. This, however, is not a dominant idea with Faraday. I think he would rather have said that the field of space is full of lines of force, whose arrangement depends on that of the bodies in the field, and that the mechanical and electrical action on each body is determined by the lines which abut on it.

On the contrary, Faraday without a doubt had the astuteness and resolve to pursue his ideas to their necessary conclusions rather than being satisfied with Maxwell's suggested compromise. However, as a former bookbinder's apprentice and lacking all formal training in mathematics, the task of formulating his ideas with the necessary precision and, simultaneously, universality, which would have brought to the fore the implied deep-rooted epistemological problems of a "pure" field theory, was beyond his grasp.

This, parenthetically — and especially in comparison with Riemann — allows a brief but important point to be made on the role of mathematics in the development of scientific knowledge.

Certainly, it is not merely or primarily a kind of formulation aid which permits a more compact expression of an already existing, fully developed physical theory. Rather, it is involved in the formulation of a new theory starting with the earliest formative stages. There its principal task is to create concrete universals necessary to transform mere notions (*"Vorstellungen"*) into testable scientific hypotheses establishing at the same time a crucial link between the scientific problem under consideration and its broader epistemological context. In general, it is precisely its close affinity to epistemology which makes mathematics (not to be mistaken for a collection of more or less sophisticated computational devices) indispensable to the process of theory formation. No consistent mathematical framework can claim completeness (Gödel): thus appropriate mathematical rigor most directly leads to the detection of epistemological defects and the associated more fundamental antinomies necessarily embodied in any one relatively complete scientific concept. But back from Hegel to Maxwell.

The basic problem of the Faraday-Maxwell Theory of the electromagnetic field, as summarized by the familiar Maxwell-Hertz field equations, consists in the obviously paradoxical conclusion that the theory is incompatible with the existence of discrete electrical charges. This is best demonstrated in the theoretical context of H.A. Lorentz's Theory of Electrons; which is an immediate offspring of the Faraday-Maxwell Theory. The mathematical theory of electrons is based on the following assumptions (which either are direct consequences of or immediately cohere with the Faraday-Maxwell Theory):

An electron is an electrical charge distributed over a certain *finite* volume element of the aether with volume-density ρ *(9)*, thus separating the aether into an interior (to the electron) and an exterior space. Conversely, we can think of the electron as a specific local modification of the state of the aether. In either case, it is coherent to regard the electrons as movable while the aether remains at rest. To the extent that the Faraday-Maxwell Theory conceives of electromagnetic forces as conditions of stress in the aether, and the aether pervades the electron, the task of formulating the mathematics of the behavior of electrons, therefore, reduces to adapting the Maxwell-Hertz equations for the free (uncharged) aether to the case of positive volume-densities. Only minor modifications are required, so that the following system of equations governing the behavior of free electrons goes over into the usual Maxwell-Hertz equations, if in I and III we let the volume density of ρ go to zero:

$$\text{(I)} \qquad \operatorname{div} E = \rho,$$

$$\text{(II)} \qquad \operatorname{div} H = 0$$

$$\text{(III)} \qquad \operatorname{curl} H = \frac{1}{c}\left(\frac{\partial E}{\partial t} + \rho V\right),$$

$$\text{(IV)} \qquad \operatorname{curl} E = -\frac{1}{c}\,\frac{\partial H}{\partial t},$$

where E and H are the electric and magnetic field strength vectors, c a constant (speed of light) depending on the aether, and V a vector representing the velocity of the charge so that ρV is the convection current.

Now consider the force acting upon a charge (electron) moving with velocity V in an electromagnetic field characterized by (I)-(IV). According to Lorentz, this force ("Lorentz force") per unit volume is given by

$$\text{(V)} \qquad F = \rho\left[E + \frac{1}{c}(V \times H)\right],$$

where (VxH) is the vector - or cross-product of charge velocity and magnetic force. The principal defect of the Faraday-Maxwell Theory consists in the fact that we must demand that

$$\text{(VI)} \qquad \rho\left[E + \frac{1}{c}(V \times H)\right] = 0$$

at each individual point of the electron. Otherwise, since the charge elements of a given electrical charge are all of the same sign, and since no cohesive forces can be derived from (I)-(IV), the Coloumb repulsive forces for any finite volume element of the charge would simply blow the charge apart. On the other hand, if (VI) holds, then no stationary charge ($V=0$) can exist, since (VI) implies that $\rho = 0$. Hence, our conclusion, spelled out above, that the Faraday-Maxwell Theory is incompatible with the existence of charges.

From the epistemological standpoint of the discreteness-continuity antinomy this result is, of course, hardly surprising. As a "pure" field theory, the Faraday-Maxwell Theory has no need for discrete individuals, and we should not expect that the Maxwell-Hertz equations, which describe all physical actions in terms of continuous stress forces in a continuous medium, will allow us to derive the existence of forces making possible the packaging of the continuum forces into discrete bundles. On the contrary, it will be demonstrated in the treatment of Cantor's manifold theory below, that the assumption of the existence of such "counting forces" on the "same level" (order of the transfinite) as that of the "forces counted" will necessarily lead to contradiction and paradox *(10)*. The necessity of the existence of discrete individuals is intimately bound up with the concept of a "non-linear" continuum, and the problem will be taken up again after that concept has been defined.

The Faraday-Maxwell Theory's difficulties with the existence of charges may at first sight appear to be solvable, if, starting from the empirical evidence of the existence of electrons (e.g. cathode and β-rays), we postulate their existence in the form of perfectly rigid bodies, and then attempt to explain their integrity by means of cohesive forces of a non-electromagnetic character. A first attempt in that direction was made by Henri Poincaré in his 1905 paper *"Sur la dynamique de l'électron,"* where he introduces a cohesive pressure p without, however, defining it beyond specifying what its magnitude has to be. A later attempt is due to Einstein. In a 1919 paper *Do gravitational fields play an essential role in the structure of the elementary particles of matter?"* he proposes the hypothesis that the electron is held together by the gravitational attraction it exerts on its own charge-elements. The hypothesis derives a certain initial plausibility from — among other things — the fact, first pointed out by M. Abraham, that the electromagnetic energy of the stationary electron as given by the Lorentz equations makes up only three-fourths of its total energy. However, a very powerful empirical argument would appear to make the Einstein hypothesis untenable: a comparison of the gravitational attractive forces of the electron with the electrostatic Coulomb repulsive forces shows that their ratio

$$\frac{e^2}{km^2} \sim 10^{40}\,(!) \qquad (k = \text{gravitational constant})$$

is such that a counterbalancing of the repulsive electrostatic by the attractive gravitational forces (cf., Kant, *Anfangsgründe*) seems out of the question.

Actually more to the point, more indicative of the fundamental epistemological problems involved, and at the same time not altogether without irony is the fact that when we try to calculate the field of a particle from the field equations of the General Theory of Relativity, modified in accordance with the Einstein hypothesis, and supplemented by the equations of electron theory, this will leave us exactly *one* equation short for the determination of the unknown in the static spherically symmetrical case. With the result, in Einstein's own words,

> that any *spherically symmetrical distribution* of electricity appears capable of remaining in equilibrium. Thus the problem of the constitution of the elementary quanta cannot yet be solved on the immediate basis of the given equations.

Riemannian Geometry

I have chosen the somewhat anachronistic presentation of the key difficulties with Maxwell's theory in the previous section, because this will now make it possible to situate Riemann's indispensable mathematical contribution to a unified physical theory in the most effective manner and without getting lost in formal details. First, an important and indicative problem with the presentation of Riemannian geometry must be mentioned. As will become obvious from the critique of Hermann Weyl's interpretation of the *"Hypothesen"* below, what in most contemporary textbooks on differential geometry, etc. is referred to as Riemannian geometry (Riemannian manifolds, metrics and so on) represents a kind of false positivity — one of several possible formal elaborations of Riemann's concept of geometry falsely pinned down as the only one. In this formalism, Riemannian geometry reduces to no more than the generalization of the intrinsic geometry of a surface with two dimensions to an arbitrary number n. Such a reductive interpretation utterly ignores the scope of Riemann's overall project and misrepresents the principally epistemological purpose of the *"Hypothesen,"* which were not intended to create yet another geometrical formalism, but to lay bare and critically investigate the hypotheses upon which geometry is based.

I will develop the principle aspects of Riemann's ideas on geometry (so far as they are explained in the *"Hypothesen"*) in the form of three compact propositions:

1. Each multiply extended quantity (multidimensional manifold) is susceptible of several metric relations: Euclidean space constitutes only a special case of a three-dimensional manifold with a metric determined by the Pythagorean Theorem. An n-dimensional manifold M^n is characterized by the fact that there exists a one-to-one, continuous mapping from M^n to a coordinate system

$$\{(x_1, \ldots, x_i, \ldots, x_n)\}$$

2. On the assumption that lines have a length independent of their position, the metric relations of an n-dimensional manifold M^n are determined as follows: to each line element ds at $p \epsilon M_n$ a number

$$ds = fp\,(dx_1, \ldots, dx_i, \ldots, dx_n)$$

is assigned, where the functions f_p are continuous and positive homogeneous.

3. What specific functions f_p are to be assigned to the points of actual physical space, so as to distinguish it from other conceivable three-dimensional manifolds, is an empirical question. In general, for the purpose of assigning metric relations to a manifold, we must distinguish between *discrete manifolds*, the principle of whose metric relations is already contained in their concept, and *continuous manifolds*, where it must come from somewhere else:

> Either, therefore, the reality underlying space must form a discrete manifold, or the basis for its metric relations must be sought outside it, in binding forces acting upon it.
>
> (*"Hypothesen"*).

I will deal with some of the mathematical details involved in the determination of the metric functions (proposition *2*) at a later point. It is the conjunction of propositions *1* and *3* which most emphatically shows the extent to which Riemann's geometry is more than a mere generalization of Gauss' theory of surfaces; I will call the joint contents of these propositions Riemann's *principle of relativity*. Its overall significance is most easily demonstrated by returning to the problem of the granular structure of matter and the Maxwell equations.

Consider again Einstein's insight — which he wrongly regarded as simply another defeat for his efforts of constructing a unified field theory — of the *under-determination* of the field of a material particle by the equations of electron theory and of the gravitational field. If we free ourselves for a moment from its specific mathematical-technical measuring (not enough equations for the number of unknowns), then the notion of underdetermination becomes immediately suggestive of the crucial type of problem involved in Einstein's failure. I noted above the impossibility of deriving from the Maxwell-Lorentz theory the cohesive forces holding the electron together. The introduction of gravitational forces effects no basic change; underdetermination persists. I now want to show that the reason for this lies in the simplistic topology, geometry and type of continuum assumed to characterize the physical fields under consideration. This is brought out as soon as we shift attention from the question of *what* holds particles together and *how* this works to the more fundamental one of *why* particles should exist in the first place. From the standpoint of the linear Euclidean (or Euclidean-in-the-small) continua underlying the Maxwell-Lorentz and the Einstein theories this question is unanswerable. Indeed, a physical process modeled in a continuum characterized by simple progressive divisibility *ad infinitum* is not "in need of" discrete existence and actually "annihilates" *(11)* it. Riemann considers this problem in the form of the problem of measurement, and that — apart from more general anthropological-evolutionary considerations — defines the most direct access to it: In discrete manifolds, quantitative comparison reduces to counting, with the simplest parts functioning as counting units.

However, in continuous manifolds, there exists — by definition — no "natural" units and any arbitrarily internally chosen unit immediately gets destroyed again by the process of division. Thus, Riemann concludes that the basis for carrying out measurements in a continuous manifold must be imported into it (imposed upon it) from the "outside." But he also insists unconditionally — and this, as I will show in the next section, distinguishes him in principle from his later interpreters — that what has to be identified "on the outside" are certain *empirically determinable* "binding forces" acting upon the manifold rather than arbitrarily *a priori* chosen yardsticks of whatever description. The choice of such "yardsticks" independently of the internal constitution of the manifold would — in geometrical terms — simply amount to the re-introduction of (Newtonian) "absolute space," and, along with it, the asymmetry of the Newtonian world-manifold in which the once and for all given metrical structure is the source of the inertial forces but cannot itself be effected by any kind of empirical force.

"The metrical structure of the world-manifold is empirically determined," i.e., the world-metric is determined strictly *relative* to the empirical contents of the world — this is Riemann's *relativity principle*. Its discovery represents his major philosophical accomplishment and the minimum epistemological rigor to be applied in the solution of the problems of measurement and of the possibility of discrete existence. This once said, let us return to the "outside," "counting," or cohesive forces. What does it mean to say that they "act upon" the manifold? Riemann fails at this point. In his mathematical writings, he does not pursue the problem beyond identifying the necessity of "outside" forces, though in the philosophical fragments ("antinomies") the matter is discussed further in religious terms. The difficulty is that "binding forces" must be "outside" the simply continuous manifold as specified; this "outside" must still be *internal* to the empirical world-manifold in its entirety or else a contradiction to the relativity principle ensues.

But how can this be. Only if the world-manifold as constituted at this point is the result of a process which, though continuous, has gone through several qualitatively different stages defined by different modes of internal organization (different sets of laws), and in which no stage n is "linearly accessible" from stage n—1. The continuity-characteristics of this type of process are such that infinite divisibility is replaced by unlimited self-differentiation effected through the production of (thus necessary!) qualitatively new individuals. Lastly, the "outside binding forces" are "outside" only of any given stage, but need no longer be considered as counter-balancing by sheer intensity the internal repulsive forces of particularate matter, but, for example, through effecting distributions of internal energies in relatively stable geometrical configurations whose intrinsic metric relations bear no linear relationship to the gross metric of the external field.

Only in a linear continuum and the corresponding affine or affinely connected (affine in the small) manifolds is it necessary to treat material particles as boundary-singularities of an essentially homogeneous field and to attempt to achieve cohesion by "brute force." In an *essentially* non-Euclidean, stratified continuum, the stability of discrete existence is not absolute (determined *once* and for all, in *one* way, from here to eternity), but a kind of meta-stability attained in a principally *force-free* fashion through the changing ways in which the historical process of the evolution of the material universe (as a process of field-particle interaction) determines microscopic configurations of energy whose geometry enjoys relative stability with respect to a given stage of the process of evolution in the large.

While not advancing to the standpoint of a non-linear continuum, Riemann's approach to geometry — as opposed to that of his successors — is sufficiently open-ended to allow for it as a possibility. In particular, he explicitly envisages what I have presented as a consequence of the stratified continuum notion and what is the diametrical opposite of the axiomatic assumptions of contemporary differential geometry — non-Euclideaness in the small:

> ...it is therefore quite thinkable indeed that the measure-relations of space in the infinitely small do not conform to the presuppositions of (Euclidean) geometry, and this would in fact have to be assumed as soon as, on that basis, a simpler way of explaining the phenomenon becomes possible. The question of the validity of the presuppositions of (Euclidean) geometry in the infinitely small is connected with the question of the inner reason and basis (*dem innern Grunde*) of the measure-relations of space....
>
> (*Hypothesen*).

Such a view will be much less surprising if we consider the intimate connection between Riemann's geometrical investigations and his work on the theory of complex- and real-valued functions. Specifically, his function-theoretical studies certainly prevented him from adopting the simplistic and provably false view shared by Hermann Weyl, et al., that "through recourse to the infinitely small all problems become linearized" (Weyl, *Raumproblem*). On the contrary, Riemann regarded it as a most important task to investigate the *conditions* under which linearization might be justifiable and to determine the degree of *distortion* introduced through such a procedure. This defines the significance of his treatise "On the Representability of a Function by a Trigonometric Series" and the included dissertation on the concept of a definite integral and the scope of its validity.

Discussing the same general problem, Felix Klein — in his already cited 1894 Vienna lecture — notes that Riemann in his later years always pointed out to his students what he regarded as the most remarkable result of modern analysis: the demonstration of the existence of a continuous function which is nowhere differentiable, i.e., of a function which stubbornly refuses to become linear in the infinitely small at *any* of its points. More broadly, the connection between Riemann's geometrical and function-theoretical investigations, defining the widest possible scope for his geometry, is the following:

In his construction of a "Riemannian" manifold, Riemann starts with a rigorous relativist conception of space — that space is uniquely determined by the totality of the relative positions and causal connections between all physical objects and events and represents their overall mode of organization. It is the task of mathematical physics to investigate the conditions of the quantitative, mathematical representability of the functional relationships among "general extended quantities" obtaining in this (the world-) manifold, and to devise appropriate concepts for the formulation of empirical hypotheses and the subsequent mapping out of the actual constitution of the manifold based on subjecting these hypotheses to "crucial" experiments.

A "Riemannian" manifold is precisely that kind of concept, appropriate to the design of hypotheses by means of which the empirical world-geometry can be determined. Its technical definition proceeds in three stages: first of all it is simply a collection of "extended quantities" (or functional relations between them), each of which is fully determined through the (numerical) specification of n independent attributes, where n signifies the number of *dimensions* of the manifold. Thus, for example, the points of ordinary physical space are specified by means of determinate values for three independent variables x,y,z, so that the totality of these points forms a three-dimensional manifold. Similarly, the points on a line form a one-dimensional manifold, the possible positions of a rigid body, which has three translational and three rotational degrees of freedom, a six-dimensional manifold, etc. Interestingly, Riemann also allowed for infinite-dimensional manifolds — today's function spaces —, i.e.,

> manifolds in which the determination of position requires not a finite but either an infinite series or a continuous manifold of quantitative determinations.
>
> (*Hypothesen*).

In the *second* place, the now defined n-dimensional manifolds of "extended quantities" can be subjected to an investigation in which

> the quantities are regarded, not as existing independent of position or as expressible in terms of a unit, but as regions in a manifold. Such investigations have become a necessity for several parts of mathematics, in particular, for the treatment of the many-valued analytic functions,...
>
> (*Hypothesen*).

What is discussed here, the study of the internal relations of a manifold in abstraction from and preceding the assignment of definite metric-relations, Riemann, following Leibniz, calls *analysis situs*; today it bears the name of *topology*. Aside from the general concepts of discreteness, continuity, and dimension, no other topological properties of manifolds are dealt with in the *Hypothesen*, and even in his function-theoretical works, which contain a greater number of significant examples, Riemann does not arrive at a precise definition of the notion of topological property, which goes beyond the above general characterization. To my knowledge, an adequate definition was first achieved by the German geometer (and Gauss student) Moebius in an 1863 treatise where he classified figures which arise from each other by way of one-to-one, continuous transformations as *elementarily related*, leading to the definition of topological properties as invariants with respect to such transformations. Moebius also discovered a topological invariant of surfaces which Riemann had failed to identify; he demonstrated the existence of one-sided surfaces ("Moebius strip") and showed that no continuous function would tranform them into ordinary two-sided ones.

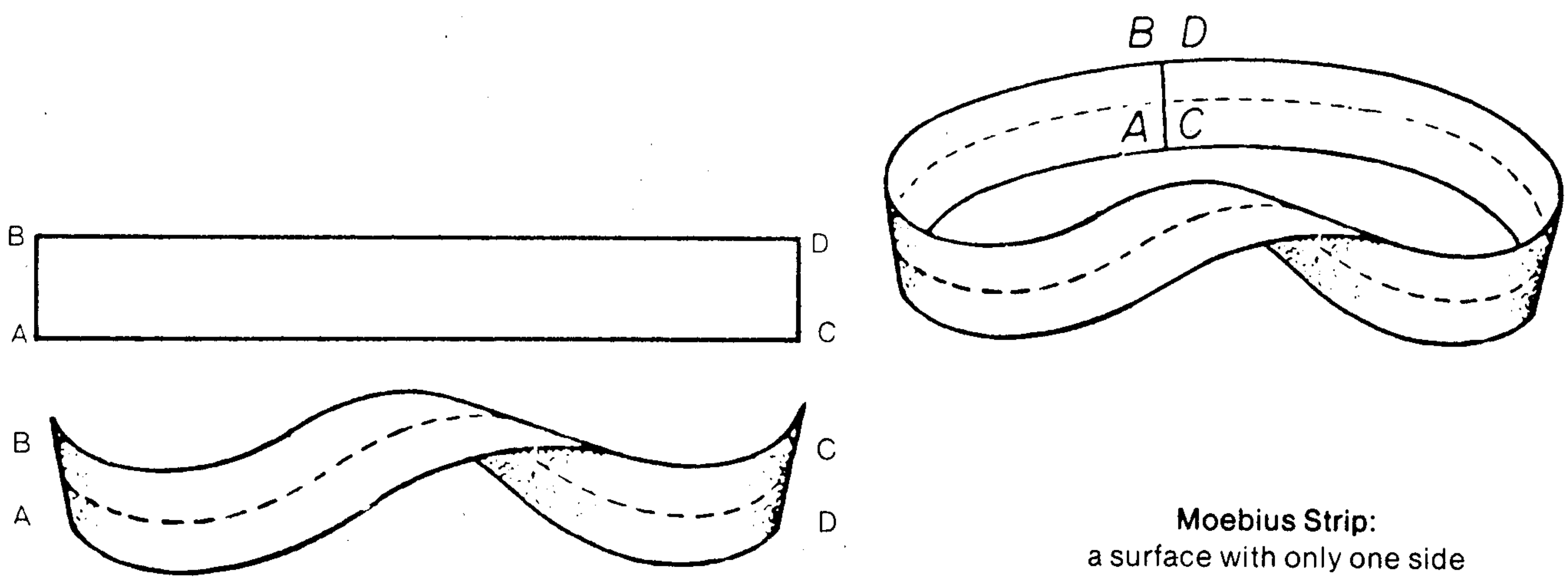
But whatever the degree of generality of Riemann's notion of the topological structure of a manifold, his introduction of the distinction between topological and metric properties and demonstration that the latter cannot be derived uniquely from the former was a major accomplishment. A given topological manifold (space) could now be seen to be susceptible of several different types of metric relations, and consequently empirical data had to be sought to determine which of these was the real one. The question of the world-metric had been removed from the realm of pure speculation and dogmatic assertion and transformed into a question of empirical science.

The *third* stage of the definition of a Riemannian manifold involves the identification of the conditions and methods of the assignment of specific metric relations (distance functions) between its elements. This takes up much of Chapter II of the *Hypothesen*, but the details are not important in this context; to some extent they will be discussed in the next section.

I have gone into this three-stage definition process at some length, because in it the dependency of Riemann's geometrical conceptions upon results obtained in his study of the "Foundations of a General Theory of Functions of a Complex Quantity (Variable)" is most clearly revealed.

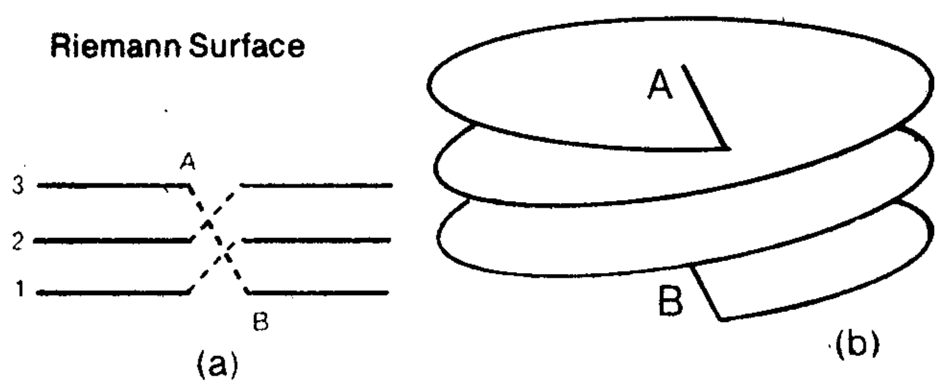
Much as in geometry he dissociates the three-dimensional manifold of space from its specific Euclidean metric and thus gains some entirely new insights into the essential attributes of the concept of space, so three years earlier, Riemann in his doctoral dissertation had proposed to dissociate complex function from their specific analytical expressions and instead investigate the degree to which they might be characterizable in terms of properties of their total domain of definition. This procedure led directly to the discovery of certain crucial topological invariants of the domain of existence of analytical functions and simultaneously the appreciation of the outstanding significance of the values of the function at singular and boundary points of the domain for the determination of its general character and overall behavior. (For example, if in a given domain a function is allowed to attain only isolated singularities of finite order, then the function is necessarily algebraic.)

The discovery of the significance of the topological properties of the domain was a direct outcome of the



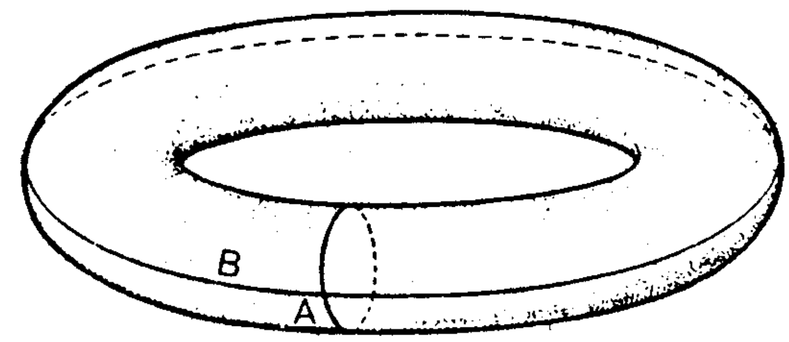
Moebius Strip:
a surface with only one side

fact that from the outset Riemann considered complex functions $f(z)=f(x+iy)=w=u+iv$ not merely in terms of their defining analytical expressions (polynomials, etc.) but — following the lead of Gauss — as distinctive types of mappings between the (x,y) -plane and the (u,v) -plane. He showed that these mappings are *conformal*, i.e., that they transform infinitesimal triangles of the (x,y) -plane into infinitesimal triangles of the (u,v) -plane which are similar to the given ones, and then actually turned this geometrical transformation property into the guiding conception for the further course of the investigation. This Copernican inversion immediately showed its fruitfulness leading to the notion of the "Riemann surface" of multi-valued functions and the kind of straight-forward determination of their properties and the properties of their integrals which previously had posed almost insuperable difficulties. Riemann's procedure was simple enough: in order to extend the correspondence between analytic functions and conformal maps — or, more pointedly, to extend the concept of analytic functions *as* conformal maps — to the multi-valued case ($f(z)=\log z$, $f(z)=\sqrt{z}$, etc.) it was necessary to conceive of the domain of these functions in such a way that they become single-valued at every point. This is made possible by covering the (x,y) -plane with the required number of copies of itself (infinitely many for $\log z$, two for $\sqrt{z}$, etc.), connecting them at the *branch points* (zero for both $\log z$ and $\sqrt{z}$)of the function, which then become the *winding points* of the spiral staircase-like Riemann surface. On



The function $\sqrt[3]{z}$ has three roots for each value of z. These three roots are considered coming from different parts of the z-plane by considering the z-plane to consist of three sheets (all joined at the branch point at the origin) that are distinct and co-planar. They are shown separated in (a) and (b). 1,2,3 in (a) are the sheets; the dotted lines indicate how the three sheets are joined across the cut: the edges of the cut labeled A and B are considered joined without interfering with the other surfaces.

these surfaces, all mathematical operations such as integration, etc. can be carried out in the same way as in the ordinary plane. More importantly for the purposes of this discussion of Riemannian geometry, the invariant properties of the Riemann surfaces of different analytic functions with respect to conformal, or, more generally, on-to-one, continuous maps can now be identified, and it turns out that they are uniquely topologically characterizable in terms of their *genus* p — the maximum number of non-intersecting cuts along closed curves of the surface which leave it in one piece. In other words, we have the theorem that two (closed) surfaces are topologically equivalent — related by a one-to-one, continuous transformation — if and only if they have the same genus. Note that $p=0$ defines *simply connected*, $p>0$ *multiply connected* surfaces.



The maximum number of closed curves that can be drawn on a surface, without cutting the surface intc two separate parts, is called the *order of connectivity*. For a sphere, this will be zero; for a torus there are two curves indicated by A and B above. The *genus* of a surface is defined as equal to one half the order of connectivity (ie., the genus p equals 1 for both a torus and a single-handled sphere).

Since the genus, in turn, bears a simple algebraic relationship to the number of winding points of the Riemann surface, whole sets of analytic functions can now be classified and their course be determined in advance in terms of the topological properties of their domains — and that is the type of result we were looking for. On the one hand, global topological properties — principally the distribution of singularities — have a determining influence on the behavior of the function, and this is reflected in its mapping properties. On the other hand, topology is about "the same distance away" from fully prescribing the specific course of the functions as it is from fixing the detailed metric properties of a given manifold. The local metric relations are consistent with a large variety of different kinds of interconnectedness of the manifold as a whole, so that, in particular, the world manifold, no matter what its empirically determined "local shape," on the large scale might exhibit any number of degrees of multiple connectedness. It would clearly be Riemann's sense that one must assume that this is the case if it leads to a more convincing explanation of the phenomena.

Given that Riemann's geometrical and his function-theoretical efforts were developed entirely in parallel, both motivated by his overall epistemological project of investigating the conditions for the development of a unified physical (field-) theory, and both converging upon the molding of indispensable topological concepts as the basis for further research, I think that the essential character of Riemannian geometry is best located relative to these topological inventions — not as defined by them, but by the investigation of the *interrelation* of topological and metric structures. Precisely the extent to which this is true defines the open-endedness and flexibility of the theory and the possibility of successfully reformulating it from the standpoint of Cantor's *Mannigfaltigkeitslehre* (doctrine of manifolds).

Weyl on Riemann

If it were necessary at this point to summarize our notion of Riemannian geometry, the following, though extremely condensed, would convey the essential idea: Riemann, prompted by his concern for developing a unified physical theory, developed a concept of relative space, the notion of an n-dimensional manifold, whose shape is determined by the interaction of "global" field forces with "local" points of condensation of these forces; this process of interaction is seen to create a more or less inhomogeneous metric field for the manifold as a whole. A most complicated case arises when we no longer assume even a relative independence of bodies from their position in the manifold, since then

> one can no longer draw any conclusions from the metric relations in the large for those in the infinitely small,...a still more complicated situation can occur, when the assumed representability of a line element by the square root of a differential expression of the second degree does not take place.

Noting further

> that the empirical concepts on which the metric determinations of space are based, the concept of a solid body and that of a light ray, apparently lose their validity in the infinitely small,

Riemann was clearly prepared to assume that especially in the immediate neighborhood or "at" the locations of particles in the field, extreme conditions might obtain occasioning radical alterations in the metric structure of the manifold.

The suggestive function-theoretical analogue of such a situation had been analyzed in detail by Riemann in his work on multi-valued complex functions, where the "singularities" (branch points, etc.) are the points of transition between the different sheets of the "Riemann surface" which geometrically represents the function under consideration. More broadly, Riemann's characteristic method of investigation in all branches of physics and mathematics that he touched upon, was to subject the object under investigation to certain "pathological" conditions in order to form a reliable judgment on what is essential to their behavior under "normal" circumstances. Thus, a key section of his "Theory of Abelian Functions" is entitled "Determination of a Function of a Complex Variable by Means of Boundary and Discontinuity Conditions." Interestingly enough, this epistemological trademark of Riemann's is well-known to Hermann Weyl, who in his *Space-Time-Matter* formulates it as follows:

> The principle of gaining knowledge of the external world from the behavior of its infinitesimal parts is the mainspring of...Riemann's geometry, and, indeed, the mainspring of all the eminent work of Riemann, in particular, that dealing with the theory of complex functions.

Riemann did not bring his work on the mathematical and empirical structure of space (or better: the world-manifold) to a satisfactory conclusion. The discreteness-continuity antinomy, the problem of the "geometry" of field-particle interaction, Riemann's question about the discreteness or continuity of the reality underlying space — these problems can only be solved from the standpoint of a world process that recognizes a level of internal differentiation of the continuum to which Riemann did not penetrate: not *one* manifold, no matter what the degree of inhomogeneity of its metric, but a succession of nested manifolds, internally characterized by different relative infinities (transfinite numbers), the transition from one to the subsequent one mediated by "exceptional individuals" (particle modes), defines the world process.

Riemann himself was fully aware of the tentative and highly incomplete nature of his geometrical and physical-philosophical investigations. Nothing he wrote in either field was published during his lifetime, though the *"Hypothesen"* was delivered in 1854 as a lecture before the Göttingen faculty. Still, his main published works on complex function theory, his notion of the "Riemann surface" of an analytic function, are most suggestive of the course his geometrical investigations could have taken; this and his method and epistemological principles make nonsense of the positivistic interpretation of his geometry by Weyl and others, who take what Riemann explicitly presented as only *one* — though admittedly the only one he elaborated somewhat — example and application of his geometrical method for the entirety of his geometry. Historically, this cut off any further fruitful development of differential geometry and turned it into a stale mathematical discipline; presently, the hegemonic, fundamentally "affine" conception of Riemannian geometry is symptomatic of the general methodological inadequacies that have prevented the extension of the general theory of relativity beyond the scope of a theory for the gravitational field.

Weyl alone cannot be saddled with the affine interpretation of the *"Hypothesen."* Christoffel, and then especially the Italian school of Ricci and Levi-Civitá, did the necessary preparatory work. However, it is through Weyl and his application of the interpretation to the "problem of space" — the problem of singling out from a variety of world-metric types a unique one satisfying certain analytical conditions which in turn correspond to empirical relations of the world manifold — that the affine conception came to dominate discussions of the application of Riemannian geometry to theoretical physics.

In the appendix to his 1919 new edition of Riemann's *"Hypothesen,"* Weyl formulates his standpoint as follows:

> It will furthermore be natural to assume that the different points of the manifold do not already differ with respect to the measure relations obtaining in each of them; analytically, this is expressed by the fact that the functions f_p corresponding to the points p all arise from one function f by way of linear transformation of the variables. This is the case when f_p^2 is a positive-definite quadratic form at each point:
>
> $$\text{(I)}\, f = \sqrt{(dx_1)^2 + \ldots + (dx_i)^2 + \ldots + (dx_n)^2}\,;$$
>
> however, it is in general not the case when f_p is the fourth root of a form of the fourth degree with coefficients varying from point to point.

Distance functions of the type (I) express the validity of the Pythagorean Theorem ("Euclidean-ness") in the infinitely small. Weyl proposes to solve the "problem of space" by establishing certain simple internal properties of spaces with a Pythagorean metric which distinguish them from all others and recommends that they alone be considered for empirical applications. This is done both in general physical-epistemological terms and in terms of detailed mathematical execution in the 1923 *mathematische Analyse des Raumproblems (Mathematical Analysis of the Problem of Space).* There three general arguments are advanced in favor of the exclusive characterization of the metric field by means of a quadratic differential form

$$ds^2 = g_{ik}\, dx^i dx^k \quad :$$

1. The effectiveness of near-action physics and Riemannian geometry is based on the principle of determining the form and contents of the physical world through an understanding of its behavior in the infinitely small. Such an understanding in turn is made possible by the fact that *through the recourse to the infinitely small, all problems become linearized.* (The emphasis is Weyl's.)

2. Commenting on Riemann's entertaining of the possibility that ds might be given as the fourth root of a homogeneous polynomial of the fourth degree, the sixth root of a rational form of the sixth degree, etc., Weyl writes:

> It seems to me that the higher cases adduced by Riemann for the purpose of comparison are constructed in accordance with an overly formal principle. Surely one must require at least the nature of the metric to be the same at every point of space.

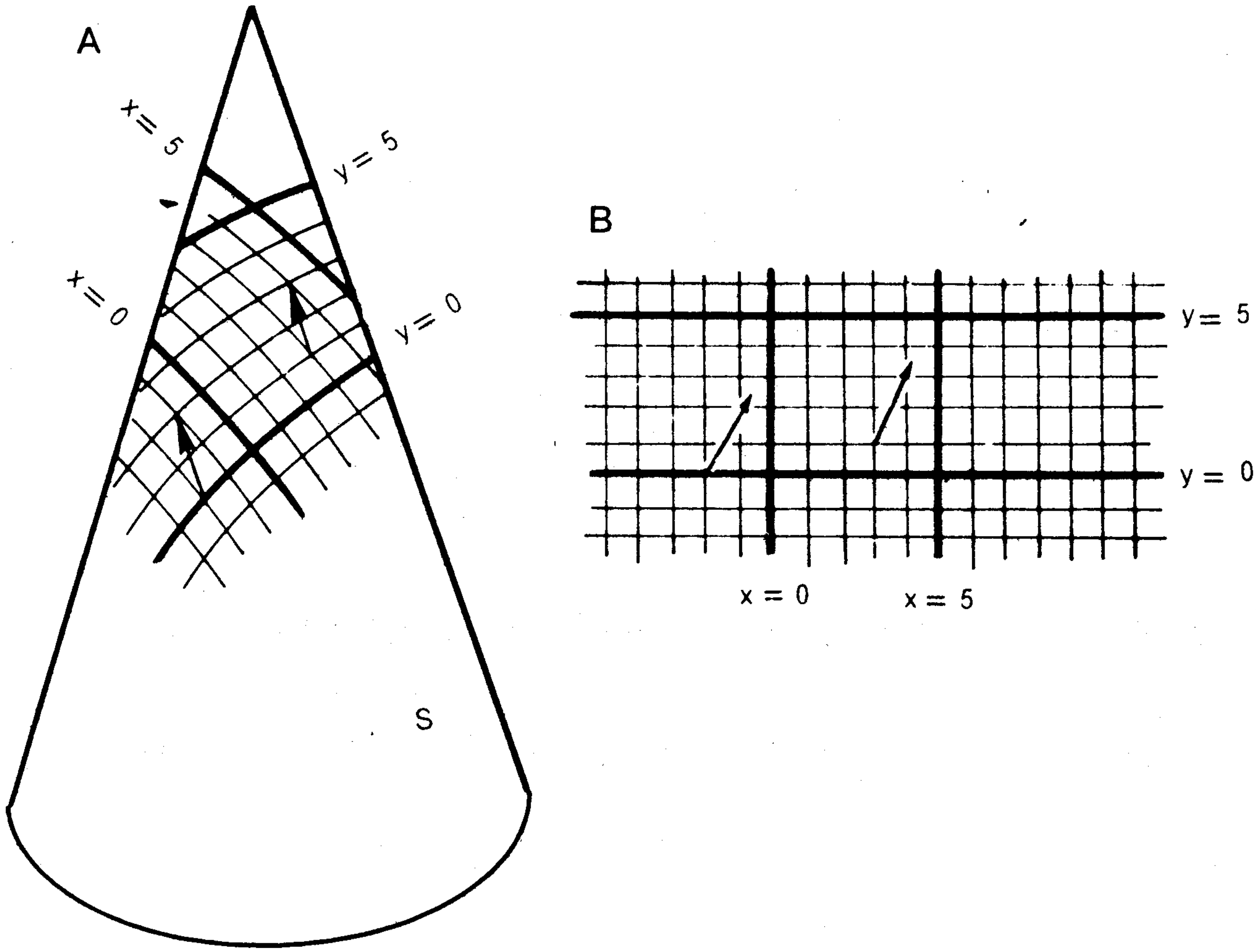
3. The real, four-dimensional world is an example of an affinely connected manifold. A body released in a definite world-direction carries out a uniquely determined natural motion from which it can only be deflected through external forces. The mathematical theorem that accompanies and elaborates these points is the following:

> The metric field of a given manifold M uniquely determines the affine connection of M if and only if at each point of M the metric field is characterized by a non-degenerate quadratic differential form
>
> $$g_{ik}\, dx^i dx^k,$$
>
> i.e., is of a Pythagorean nature.

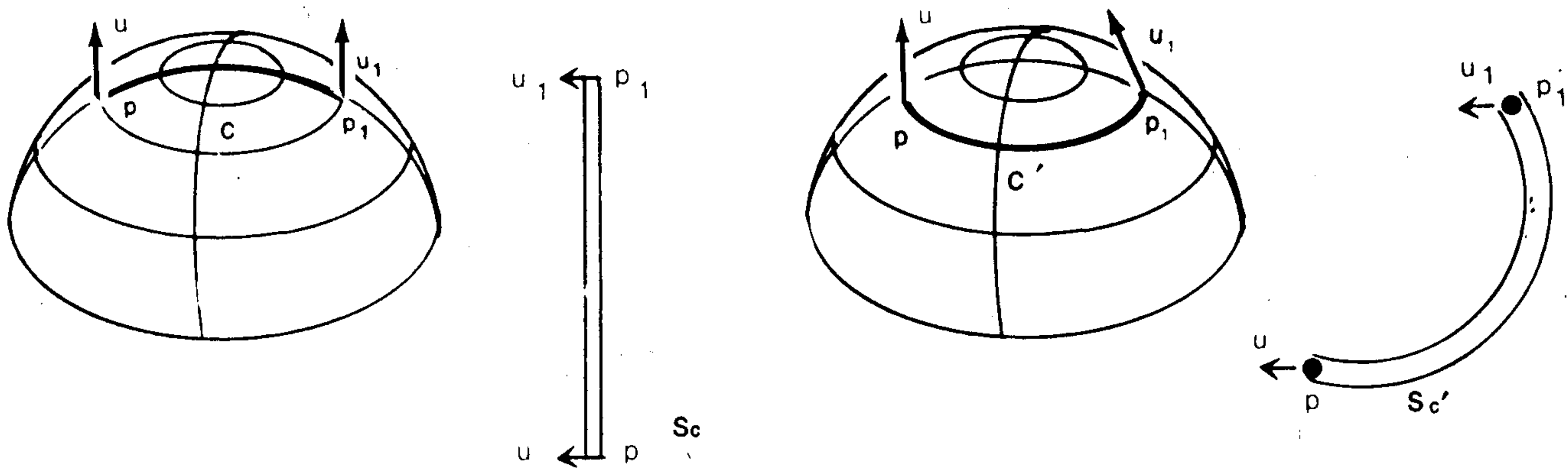
There is no reason to doubt the mathematical cogency of this proposition. However, it is precisely the fact that Pythagorean-type metrics uniquely determine the affine connection of the manifold they characterize which speaks against the exclusive adoption of such Pythagorean structures. To explain this it will be necessary to take a look at Weyl's proof and to some extent at least "unpack" the contents of his theorem through a series of definitions of key concepts.

1. The operation of *parallel displacement* is fundamental to both finite and infinitesimal affine geometry. In the Euclidean plane, for two points p and p_1 and a vector v at p there exists a unique vector v_1 at p_1 *parallel* to and of the same length as v. The Euclidean notion of parallelism can be extended to arbitrary sufaces S:

Let p and p_1 be in S, u a (unit) tangent vector at p, and T and T_1 the tangent planes at p and p_1. If S is *developable* (of the same intrinsic geometry as the plane), then u_1 at p_1 is called *parallel to u with respect to S,* if it is parallel to u in the ordinary sense after S has been developed onto the plane. If S is not developable, then parallel displacement has to be defined by way of a curve c connecting p and p_1: Consider the set of all tangent planes to S at points along c and their lines of intersection; the surface formed by the totality of these lines is called the *envelope* of S along c, and is clearly a developable surface S_c. Now, u_1 at p_1 is said to be *parallel to u along c on S* if it is parallel to u with respect to S_c.



Consider two vectors on the surface S of a cone and with respect to a coordinate patch (A). Since the surface of a cone is a *developable* surface, it (and the coordinate patch) can be laid flat onto a plane without stretching (B). The two vectors are then defined to be *parallel* with respect to the conic surface if they are parallel to the plane.



One consequence of defining parallel transfer in this way is that at point p_1, there is in general no unique vector that is parallel to a given vector at p with both vectors tangent to the surface S at their respective points. This can be seen here, with two curves, c and c', connecting p and p_1 on a sphere. Curve c is along longitudes and S_c can be developed into a straight line: c' is along a latitude and S_c' can be developed into an arc of a circle.

Finally, the *infinitesimal parallel displacement* of vectors in arbitrary *n*-dimensional manifolds must be defined. Infinitesimal displacement is the geometrical basis of the *tensor calculus* and was initially introduced in a 1917 paper by Levi-Civita in connection with the embedding of an n-dimensional Riemannian manifold (i.e., a manifold whose metric is given by the form

$$ds^2 = g_{ik}\,dx^i dx^k)$$

in a Euclidean space of

$$\frac{n\,(n+1)}{2}$$

dimensions (where

$$\frac{n(n+1)}{2}$$

corresponds to the number of the tensor components g_{ik}). The concept arises straightforwardly by way of first generalizing the notion of finite displacements on two-dimensional surfaces to the n-dimensional case, and then restricting the displacement operation to points in an infinitely small neighborhood of a given point p.

Independently of the embedding of the manifold in Euclidean space, an infinitesimal parallel displacement of the vector v at p to an infinitely near point p_1 can, in analogy to finite affine geometry where parallel displacements of a vector leave its components unchanged, be characterized analytically as an infinitesimal translation, for which there exists a coordinate system (for the immediate neighborhood of p) in which the components of v remain unaltered by the translation. In an arbitrary coordinate system x_i, the change of the vector components ξ^i brought about by infinitesimal parallel displacements is given by

$$\text{(I)}\quad d\xi^i = -\Gamma^i_{rs} dx^s \xi^r.$$

Here the quantities Γ^i_{rs}, called the *Christoffel symbols,* which determine the displacement process, depend only on the coordinates and satisfy the symmetry relation

$$\text{(II)}\quad \Gamma^i_{rs} = \Gamma^i_{sr}$$

Conversely, if the quantities Γ^i_{rs} satisfy (II) and a translation of the vector field at p to points in the infinitesimal neighborhood of p is defined by (I), then there exists a coordinate system in which the infinitesimal translation so defined leaves the vector components unchanged. Weyl calls this " a possible system of infinitesimal parallel displacements." If a manifold is such that at each of its points p among all possible systems of parallel displacements of the vector field to points in the immediate neighborhood of p one and only one can be singled out as "real," then the manifold is said to be *affinely connected* (or provided with an *affine connection*), and the Γ^i_{rs} are called the *components of the affine connection.*

Remark on Affine Spaces

Elementary (metric) geometry studies those properties of geometrical figures and relations which remain unchanged (*invariant*) under *congruence transformations* (parallel displacements, rotations), i.e., motions to which a *rigid body* can be subjected without changing its shape. *Affine geometry* is the theory of invariants of the considerably larger group of *affine* or whole linear *transformations*

$$x' = a_1 x + b_1 y + c_1 z + d_1$$

$$y' = a_2 x + b_2 y + c_2 z + d_2$$

$$z' = a_3 x + b_3 y + c_3 z + d_3 \ ,$$

among which the congruence transformations are contained as special cases. Thus certain elementary geometrical properties and relations of objects (such as shapes, angles, distances) will not be invariant under permissible coordinate transformations in affine spaces. However, the distinction between finite and infinitely distant points of space remains intact and along with it all those concepts that depend upon that distinction: the parallelism of straight lines, the classification of conic sections into ellipses, parabolas, and hyperbolas, etc.

"Affinities" are actually best interpreted as pure *homogeneous deformations* or as simultaneous *linear expansions* (contractions) of a given space in the mutually orthogonal directions of its coordinates.

Examples:

1. The *Galileo-Newton transformations,* expressing the equivalence of any two inertial frames (coordinate systems) and the invariance of the laws of Newtonian mechanics with respect to them, are affine transformations. Thus the space of Newtonian physics is a four-dimensional affine space, in which the velocity of a body in uniform motion is not of, whereas the force (represented by a vector) with which one body acts upon another is of the required invariant significance.

2. The transformations employed by Marx in the last chapter of Volume II of *Capital* in the diagrams intended to explicate the process of expanded reproduction are affine transformations. This signifies Marx's failure to fully conceptualize the expanded reproduction process: only expanded *simple* reproduction, i.e., linear expansion within a given mode of technology, is susceptible to affine treatment. If the space of expanded reproduction proper were an affine space, then, in accordance with Marx's discussion, we would arrive at the conclusion that productivity of labor (or certain linear productivity increases) is an invariant of the expanded reproduction process, whereas the opposite (i.e., non-linear increases occasioned by the introduction of qualitatively new technologies) is the case. This was first pointed out by Rosa Luxemburg in Chapter XXV of the *Accumulation of Capital.*

Affinely connected manifolds, as just defined above, can now simply be characterized as manifolds in which all the properties of affine spaces are explained and valid not globally, but only for an infinitesimal neighborhood of each of its points p.

By means of what intrinsic properties can the affinely connected manifolds be distinguished among arbitrary continuous n-dimensional manifolds, or, equivalently, what allows us to single out a unique system of parallel displacements among all possible ones at a given point of the manifold p?

This is the subject of what is sometimes called the *Fundamental Lemma of Riemannian Geometry* (which is identical to the if-condition of the above-stated basic theorem establishing the connection between affinely connected manifolds and manifolds with a Pythagorean-type metric); the lemma reads:

> On an n-dimensional manifold whose metric is given by the fundamental quadratic form $g_{ik}\, dx^i dx^k$ there exists for each of its points p a unique system of parallel displacements of the vector field at p to points in the infinitesimal neighborhood of p which leaves the lengths of all the vectors unchanged, i.e., for which
>
> $$d(g_{ik}\,\xi^i \xi^k) = 0.$$

This is a most important result, because it allows us to ask a question about the real world an unambiguous answer to which would apparently determine once and for all the nature of the metric of the world-manifold: Does there exist for every world-point p and for a given test-body at p a unique world-direction which determines its motion? The simplicity of the question is deceptive and the seemingly obvious "yes"-answer looses its obviousness as soon as we begin to make the first inquiries about the nature of the test-body and of the direction-determining "guidance field" under consideration. Is it true for an electron at a given place and time? According to Heisenberg (uncertainty) the very question is nonsensical. I contend that in order to coherently pose the above type of question, a unified physical theory of the world manifold would have to be available to us, but that the elaboration of such a theory could not conceivably involve a characterization of the world metric in terms of infinitesimal parallel displacements because this would imply a violation of the principle of relativity. To see this, the condition of the existence of unique systems of parallel displacements (or, as this is also expressed, unique affine connections compatible with the metric structure) must be investigated in greater depth. A good handle for doing this is provided in Weyl's proof of the converse of the fundamental lemma (the only if-condition of the *"Raumproblem"*-theorem), which for convenient reference shall now be briefly restated:

> Let f be a metric function defined on an n-dimensional manifold and such that at each point of the manifold f determines a unique system of congruent infinitesimal parallel displacements. Then f^2 is a non-degenerate quadratic differential form.

Weyl's proof proceeds from the notion that the metrical constitution of a manifold at a point p is known, if among the linear transformations of the vector field with the fixed point p (rotations) we can single out the congruent ones. This had been the leading idea of Helmholtz's 1868 paper "The Facts upon which Geometry is Based," in which he showed that "Riemannian spaces" (defined as spaces whose metric is given by a positive-definite quadratic form $g_{ik}\,dx^i dx^k$) can be uniquely characterized by the postulate of the free mobility of sufficiently small rigid bodies, or, more precisely, by the requirement that an infinitesimal body containing p can be freely rotated about p without undergoing deformation.

Helmholtz further demonstrated that if we demand the free mobility of arbitrary finite bodies, then the space in question must, in addition, be of constant Riemannian curvature. Formulated in terms of the theory of transformation groups, Helmholtz's principal result states that the group of homogeneous linear transformations of the differentials (line elements) dx^i at p determined by the infinitesimal free mobility condition consists exactly of all those linear transformations which leave the form $g_{ik}\,dx^i dx^k$ invariant. Similarly, though in considerably more general fashion, Weyl proves that an infinitesimal rotation group of the vector field at p satisfying analytical conditions that depend upon the degrees of freedom of the vector field and otherwise directly translate into group-theoretical language the unique determination of the affine connection by the metric field, comprises the entirety of infinitesimal linear transformations which transform a given non-degenerate quadratic form into itself.

Helmholtz was led to his theorems singling out "Riemannian spaces" among more general metric spaces, and spaces of constant curvature among the general "Riemannian" ones by what he regarded as indubitable facts concerning rigid bodies and processes of physical measurement. We carry out measurements, he argued, by moving calibrated measuring rods from one place in physical space to another. These measuring rods are three-dimensional rigid bodies, and unless they can be moved from here to there without undergoing changes in shape and size, all measurement is impossible. Free mobility of finite rigid bodies is a presupposition of physical measurement, and therefore the world-manifold is a "Riemannian" space of constant curvature.

Well, Helmholtz was proved wrong by Einstein and the general critique and re-evaluation of the concept of rigid body brought about by relativity theory. The kind of measurement Helmholtz regarded as the only possible one turns out to be possible only under special conditions; the General Theory of Relativity gives up finite free mobility and asserts it only for the infinitely small. This may or may not be the correct view, but — as Riemann, Helmholtz, and Einstein would all agree — whether or not it is is an empirical question. Not so for Weyl, — and this fact is the first important epistemological conclusion from his proposed solution to the *"Raumproblem."* He himself is quite open about the dogmatic implications of his theorem: "it is a matter," he says, "of rationally comprehending the *one* immutable *Pythagorean nature* of the metric, in which the *a priori* (my emphasis) essence (*Wesen*) of space manifests itself." And elsewhere, commenting on the distinction between his and the Euclidean point of view:

> That there is something a priori about the structure of the extensive medium of the exterior world is, therefore, not denied in principle; only the borderline between the a priori and the a posteriori is moved to a different place.

Weyl knows that to be consistent with Einstein's empirical discoveries, geometry cannot maintain the a priori metric homogeneity of space; however he clings to the a priori homogeneity of the *nature* of the metric — because otherwise "everything would be possible"?

We get a more definite sense of the implications of Weyl's apriorism by focusing on the general equivalence of his and Helmholtz's characterizations of metric spaces equipped with quadratic forms. Keeping in mind Helmholtz's concern about the possibility of measurement, to assume the necessity of the affineness of space in the infinitely small is simply to hold on to a last vestige of the "absoluteness" of space in the Newtonian sense; to import extraneous yardsticks — though infinitely small ones — for the purpose of quantitative determination of the world-manifold. This, however, is clearly antithetical to a rigorous interpretation of the principle of relativity, which demands that measurement be strictly based on the internal relations of substance and no aspect (moment) of the physical process be accorded invariant significance, which depends for its determination upon the introduction of "absolutist" (i.e., essentially subjectivist) assumptions. The reasons why Weyl felt it necessary to make a priori assumptions (as opposed to forming empirical hypotheses) about the structure of the world-manifold are straightforward.

The introduction of such assumptions (comparable to the Kantian categories) must appear as a condition of the very knowability of the universe — though it makes knowledge partial in the dual sense of that term — to anyone who is not in possession of a conception of the physical universe as a process of successive qualitative self-differentiation of which we ourselves (including the progressive perfection of our knowledge) are an integral part. Such a conception of progress through qualitatively distinct stages (both in the ontological and the epistemological sense), in which the necessity of reaching the next higher stage determines the adequacy — and thus is the sole permissible basis for the measurement — of processes in the here and now, is indispensable if a radical application of the principle of relativity and the coherent radically empirical outlook, which tolerates no dogmatic premises, are to be possible.

The assumption that the world-manifold is necessarily affinely connected is not only an intolerable apriorism, it also is responsible for the status of discrete existences (particles) as Kantian-type "things-in-themselves" and the consequent severing of the microscopic from the macroscopic physical realm; as follows:

In an affinely connected manifold the parallel displacements defined by the Γ^i_{rs} (the components of the affine connection) induce linear maps between the tangent spaces at different points of the manifold and, hence, linear functional relationships between the lengths of the line elements at any two points. In this situation there is no room for non-linear displacements anywhere, and if the unified physical field is such a linearly connected manifold permitting only linearly related variations in field intensities from one place to the next, then discrete existences necessarily acquire the status of "uncontrollable" intensities (unchecked by cohesive forces) or of unremovable singularities, which "disinterestedly" coexist with the field without being knowable from its standpoint, i.e., as "in-themselves"-existences. This, of course, is precisely the situation in theoretical physics today.

To avoid these difficulties, neither field nor particles can be taken as primary, but only the process of their continuous interaction. Then the topology and the "local" and large-scale metrical structure of the process must be mapped out — not as determined by an aprioristic epistemology but by the internal necessity of the progressive evolution of the process. This requires the introduction of Cantor's transfinite numbers and of the concept of a "non-linear" continuum.

4. Cantor: The Theory of the Transfinite

Prelude: Cantor as a student of Riemann and Weierstrass

I shall look at Cantor from the standpoint of the necessary further development of Riemann's project as defined in the previous section. In particular, Cantor's transfinite numbers can be viewed as a further elaboration of Riemann's distinction between discrete and continous manifolds, defining internal differentiation and distinct determinate infinities within the continuum itself. This yields the conception of not one, but of an ordered, continuous succession of manifolds — precisely the notion needed to apply the fundamental concept of Marxian economics, expanded reproduction, to the physical universe as a whole.

Cantor's mathematical discoveries, aside from their basis in his philosophical concerns, resulted from the convergence of two separate principle lines of investigation, both of Riemannian origin:

First — though this is not the chronological order of these developments — Cantor directly connects up with Riemann's work on n-dimensional manifolds in an 1878 paper entitled *A Contribution to the Theory of Manifolds.* This paper directly references Riemann's *"Hypothesen"* as well as Helmholtz' 1868 piece "The Facts Upon Which Geometry is Based." Cantor here discusses the concept of a continuous manifold and makes the discovery that in principle n-dimensional manifolds can be collapsed into manifolds of one dimension, so that dimensionality itself cannot be viewed as a way of developing a notion of higher orders of infinities. The theorem Cantor proves is the following:

> The elements of an n-dimensional manifold can be uniquely and completely determined by just one, continuous coordinate t, i.e. there exists a one-to-one correspondence between the elements of a continuous n-dimensional manifold and a continuous one-dimensional manifold (such as an arbitrary segment of the real line).

Or to restate this in terms of the concept of the *power* of a manifold or an aggregate, which is first developed in the introduction of this 1878 paper, the power of an n-dimensional continuous manifold is the same as that of a one-dimensional one. His ability to prove this theorem came as the greatest surprise to Cantor himself. He actually decided to communicate this proof to his colleague Dedekind before submitting it for publication, commenting in the Dedekind letter, *"Je le vois, mais je ne le crois pas."* The reason is only too obvious. Historically, Cantor had actually in the preceding three years made every attempt to prove the opposite — that is to show that no such one-to-one correspondence between manifolds of different dimensions was possible.

He was prompted to make this investigation by an earlier 1874 discovery in which we can locate at least one important strand of his development of the theory of aggregates in general and of different powers of the infinite in particular. In this 1874 paper, *On a Property of the Totality of All Real Algebraic Numbers,* he had succeeded in proving that there exists no one-to-one correspondence between the natural numbers sequence, the numbers one, two, three, four, etc., and the entirety of the real numbers, the numbers encompassing the natural numbers, the rationals and the irrationals. We can surmise that he then attempted to find higher-order infinities by advancing to higher dimensions — by advancing from the real line to the plane to three dimensional spaces, etc. Now the 1878 paper demonstrated that this was impossible. The necessary conclusion to be drawn from this was that it was only through a much more thorough and careful study of the properties of the one-dimensional continuum that progress might be made towards an understanding of the possibility of higher-order infinities.

We find the second major root of Cantor's theory of transfinite numbers in his work on certain key features of Fourier Analysis. Fourier's work of subjecting the so-called arbitrary functions to analytical treatment was, as we pointed out above, one of the most fruitful ways of making progress — not just with the integration of certain partial differential equations which without his method would have been impossible to handle, but also with the discovery of the fundamental topological properties of the manifold underlying functional relationships and determining their character. Thus, Fourier, faced with the problem of considering the permissible degrees of "misrepresentation" of arbitrary functions through trigonometric series, actually had to come to grips with the most general concept of a function. And it was this same general line of investigation which in subsequent decades was carried on successfully by Dirichlet in his 1829 paper, "On the Convergence of Trigonometric Series Representations of Arbitrary Functions" and in the work of Dirichlet's student Bernhard Riemann.

Cantor's work on Fourier Analysis began amost immediately after he received his PhD. at the University of Berlin and came to Halle-Wittenberg in 1869. His publications on the uniqueness of trigonometric series representations of real valued functions f(x) fall into the 1870-1872 period.

The principle theorem involved is the following: *Theorem:* Two trigonometric series

$$\tfrac{1}{2} b_0 + \sum (a_n \sin nx + b_n \cos nx) \text{ and}$$
$$\tfrac{1}{2} b'_0 + \sum (a'_n \sin nx + b'_n \cos nx)$$

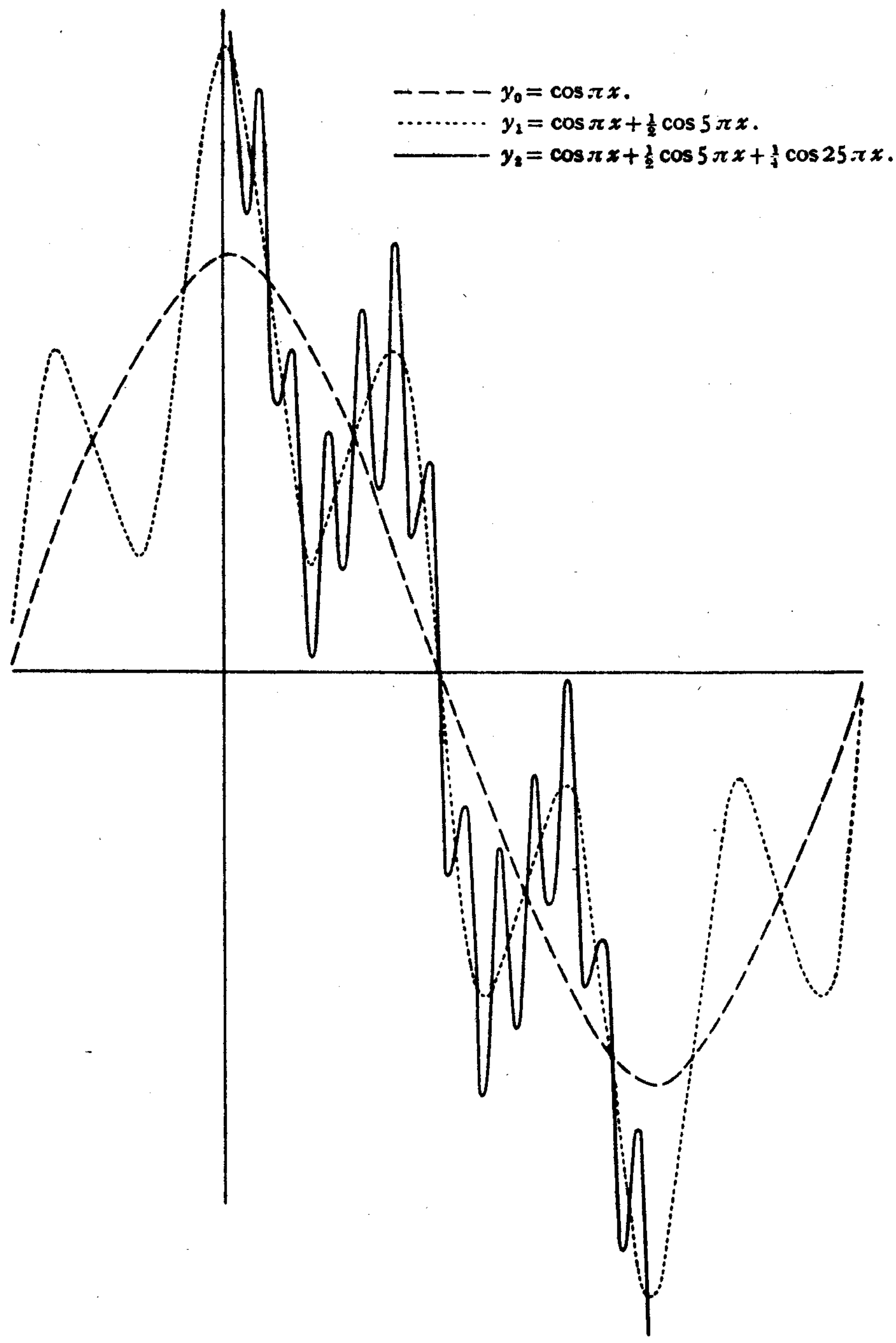
coincide in their coefficients even if for an *infinite* number of values of x of the interval $(0...2\pi)$ the series fail to converge. In his attempt to determine the precise size of the so-called exceptional, in this case, infinite sets of values where convergence of the series is permitted to fail, Cantor introduced his concept of the derived point set, a generalization of which straightforwardly led to the introduction of his transfinite ordinal numbers. In fact, the transfinite ordinals technically are nothing more than the indices of these derived sets. The details of this are well developed in Philip E.B. Jourdain's introduction to Cantor's 1895 *Contributions To The Founding of The Theory of Transfinite Numbers.* That Cantor himself actually began to understand the more far reaching significance of this notion of a derived set at that early point, that out of it the concept of a transfinite ordinal could be developed, was something which, according to his own testimony, he already saw in the 1870-1871 period.

This general line of investigation which, directly in the mathematical-technical sense, leads to the discovery of the transfinite ordinals is the same line first pursued by Fourier and then by Riemann in his 1854 piece "On the Representability of a Function by Means of a Trigonometric Series." This Riemann essay written contemporaneously with the *"Hypothesen"* contains a short compact chapter on the theory of the "Riemann Integral." Here Riemann, prompted by his investigation of the conditions under which a function is Fourier representable, found it necessary to investigate, in more general terms, the necessary and sufficient conditions of the integrability of an arbitrary function, and, specifically, to try to find an estimate on the tolerable number of discontinuities of such a function in a given interval. It is necessary to reiterate that it is precisely investigations of this kind which gave us the most detailed insight and most comprehensive understanding of the character of the continuum. Therefore, it should not really be that surprising that it is precisely investigations of this kind which despite their seemingly limited technical-mathematical character play a major contributing role in the development of Cantor's theories which, of course, had the broadest possible epistemological impact.

Now specifically for Cantor's intellectual development. It will be necessary to establish here that while Fourier, Dirichlet and Riemann and, to a certain extent, Gauss as well made contributions of major significance to the study of the topological structure of the continuum, the most general and far-reaching investigations, though focused much more specifically in the context of the theory of complex value functions, are due to Karl Weierstrass. Weierstrass came to Berlin after an unspectacular career at various small Catholic High Schools in the year 1859, when he was already in his 40s. Cantor was a student of Weierstrass' for four years (from 1863 through 1867 with a brief interruption when he spent the summer of 1866 in Göttingen). The principle features of Weierstrass' mathematical theories are best summarized by focusing on his method — the almost proverbial Weierstrassian rigor (*Weierstrass'sche Strenge*). He pushed to the extreme the method of subjecting functional relationships to "pathological" situations in order to determine their principle characteristics. The purpose was to attain the most precise demarcation of concepts which previously had not been established in their exact meaning. Thus, for example, it was generally assumed that continuous functions would also be differentiable at every point, precisely because of the apparently implied smoothness of a continuous function. Weierstrass utterly destroyed this belief by actually exhibiting, to the great surprise of all his contemporaries, a function which is continuous but not differentiable at any point. The conceptual demarcation between continuity and differentiability on the basis of the inspection of this example then became clearly understood.(see next pg.)

More broadly, his investigations and the construction of such pathological examples allowed him to make the greatest amount of progress with regard to the determination of the topological characteristics of a manifold which must be presupposed in order for certain mathematical qualities to be exhibited by functions within that manifold. Here it was specifically his investigations of a variety of so-called existence theorems which were of the greatest value. A case in point is his discussion of the "Dirichlet Principle," a method of deduction borrowed from the calculus of variations by means of which the unique existence of solutions to certain boundary value problems in the theory of partial differential equations is established. Riemann time and again availed himself of this method in his theory of Abelian functions. The simplest form for the problem under consideration is the following:

Consider a set of boundary values to be given by a function $U(\psi)$ where ψ is the angle and the values $U(\psi)$ are continuously distributed over the boundary



Approximation of the Weierstrass curve $\sum_{0}^{\infty} b^{\nu} \cos a^{\nu} \pi x$ where $b = \frac{1}{2}$, $a = 5$.

of a circular disc. Then we want to prove the following existence theorem: that in the interior of the disc there exists one-and-only-one continuous function u which is continuous with the given boundary values and satisfies the equation

$$\Delta u = \frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0.$$

Presumably on the basis of the calculus of variations it can now be established that such a unique function u exists as the lower limit of the value of a double integral extending over the entire disc.

In 1869, Weierstrass published his conclusive refutation of this method of argument by means of the following type of example: Assume that among all continuously curved curves between A and B passing through C we want to determine the one whose length is a minimum. The lower limit of all possible curves that must be considered in this case, is given by the straight lines AC and BC which, however, at C no longer fit together in a continuously curved fashion. Thus, the apparent lower limit does not belong among the functions that could be considered a solution.

What is of importance here is not the disproof of the Dirichlet Principle and the apparent, at the time, invalidation of much of Riemann's work on algebraic functions, but rather the method by which Weierstrass accomplished this. Parenthetically, it should be mentioned that Hilbert at a later point established that if the proper restrictions are placed upon the cases considered by Riemann, the restricted Dirichlet Principle is entirely valid and none of Riemann's results become false. In any case, it is Weierstrass' *method* which is at issue. Here, as well as in the more celebrated case of his critique of Cauchy's definition of irrational numbers, Weierstrass establishes the necessity for a constructive definition of mathematical concepts, thus focusing his attention on the process that establishes a concept, rather than on the result of that process. Previous to Weierstrass, these methods of construction had remained almost entirely obscure and existence theorems of the relevant kind, for example, had generally to be established by way of recourse to physical and geometrical "plausibility." Cauchy defined the irrational numbers as follows: "An irrational number is the limit of the various fractions which furnish more and more appropriate values for it."

As Weierstrass points out, this definition presupposes the existence of such limits and, as such, is a circular definition because the concept of limit, in turn, presupposes the antecedent definition of irrational numbers. In the *Grundlagen* Cantor remarks that Weierstrass was probably the first to avoid this logical error in his own construction. He defined the new irrational numbers strictly as specifiable aggregates of numbers (rationals, in this case) which had been previously defined. And he gives a specification of precisely how such aggregates are to be constructed.

Weierstrass' method, however, culminates in his rigorous application of "nesting processes" which were first used by Bernhard Bolzano in 1817. The important point is that through the use of these nesting procedures, arithmethic construction processes can be found which allow us to eliminate loose assumptions concerning the existence of limits, etc. and thereby give us a much greater insight into the contents of the limit concept itself. Aside from the definition of the irrationals, Weierstrass applied a nesting process in his proof of the "Bolzano-Weierstrass" theorem, one of the most fundamental theorems in general topology, which establishes the existence of "points of condensation" in every closed interval of the real line. Interestingly, Cantor's derived point sets actually are, for any given set, the sets of points of condensation or limit points of that set. Beyond the Bolzano-Weierstrass theorem, Weierstrass then actually went on to discover and develop much of what is now known as point set topology and certainly most of what is needed as a basis in topology for function-theoretical investigations. Clearly, Cantor's method of defining transfinite ordinals owes a great deal to Weierstrass' procedures. Cantor draws the relevant parallel between transfinite ordinals and irrational numbers in an 1884 letter:

> The transfinite numbers are, in a sense, new *irrationalities* and, indeed, in my eyes, the best method of defining *finite* irrational numbers is the same in principle as my method of introducing transfinite numbers. We can say that the transfinite numbers stand or fall with finite irrational numbers; in their inmost being they are alike, for both are definitely marked-off modifications of the actually infinite.

While Weierstrass' method and influence were a major contributing factor to Cantor's development, we should add here at least one word of caution. Weierstrass, as opposed to Riemann, had only a most tentative relationship to physics and consequently the scope of his investigations was hemmed-in from two sides — the side of physics and that of epistemology — because it is precisely through its contribution to the problems that arise in physics that mathematics develops or makes significant contributions to the development of broader epistemological problems. While indeed Cantor was more of a student of Weierstrass' than of Riemann's and while this left its positive mark on his work, it also, in a sense, became his downfall later on when in his own investigations he began to focus more and more on the formal structural aspects of his theory and away from the broadest philosophical implications of his 1870-1884 discoveries.

The Sequence of Omegas

We begin the discussion of Cantor's own theory of infinite aggregates, as developed principally in the 1883 *Grundlagen* with a brief exposition of the formal aspects in the process of the definition of the transfinite ordinals. In a sense we can think of them from the standpoint of asking ourselves the question of how one would extend the process of counting beyond the finite numbers one, two, three, four, etc., and what principles of the counting process one would have to adduce in order to understand how its extension beyond the realm of finite sets is possible. According to his own testimony, this was Cantor's own way of thinking about his new numbers. While they could not possibly have the same characteristics in every respect as the finite numbers, there was in all cases the need for adducing a new and sufficiently general concept whose comprehension would be large enough so as to encompass both the realm of the finite and of the infinite.

What is essential to the counting process is the following: it is first and foremost a process of ordering — a process by means of which the different elements of a given set are put, if you will, next to each other, placed one before the other, the other behind the one, so that we can then use that ordering and the process of counting can commence. Clearly, if we think of counting as principally ordering or based on ordering in this sense, then the initial difficulty of conceiving of a process of counting when there is no longer a clearly defined sequence, as in the case of the natural numbers, tends to disappear. Cantor believed that he could actually prove that every set, no matter what its size, could be "well ordered." This well-ordering theorem, which in contemporary axiomatic set theory is seen to be the equivalent of the so-called axiom of choice, would then be a sufficient basis for the extension into the *trans*-finite of the counting process.

Specifically, the transfinite series of the ω's is constructed in accordance with what Cantor calls two principles of generation, two constructive principles, and one limiting principle which imposes certain restrictions upon the otherwise untamed process of contruction as defined by the two generation principles. The first of these principles is familiar to everyone. It is simply the instruction: "add one"; the very way in which we proceed in the process of counting in the realm of the finite. Now consider the set of all such numbers which have been generated by means of the first principle of generation. This set does not contain a greatest number. We can however imagine a new number ω which precisely references the specific order of succession in which the first set, "the first number-class," has been formed.

The first infinite ordinal number ω references the order type of the set of all numbers preceeding it, but is not itself a member of that set. Once ω is given we can again apply the first principle of generation to form new numbers $\omega+1$, $\omega+2$,...etc: Again we will be in a position of not arriving at a greatest number in this sequence. But, as in the case of ω, another number 2ω can be formed to reference the set of all numbers $\omega+1$, $\omega+2$, ..., etc. which preceed it.

Now let us try to adduce the general principle on the basis of which ω, 2ω, etc. have been constructed. This principle is clearly different from the first generation principle. What Cantor calls the second generation principle is spelled out with precision as follows: If we are given a set of integers in any definite succession such that there exists no greatest number in this set, then on the basis of the second principle of generation a new number is formed which is defined as the next greater number to all the preceeding ones.

The significant new element introduced by Cantor at this point consists of using the concept of power, which he had developed in the above quoted 1878 piece, and which now comes to intersect with the ordering and counting process defined by the two generation principles. This gives rise to the limiting principle which decrees, in the specific case in our construction of infinite numbers that we have reached at this point, that all the numbers of the second number class, numbers such as ω, $\omega+1$, 2ω, etc. can only be of the power of the entirety of the set which they are counting, in this case the power of the first number class. This limits the otherwise absolutely infinite progression of integers and introduces steps into their continuous succession, so that we obtain natural segments, which Cantor calls number classes, in the progression of the entire sequence of the transfinite numbers.

It is an easy matter to prove that indeed the number classes I, II, III, etc. so formed follow each other in order of power, in such a way that the power of the second number-class is in fact the next higher power to the first and so on and so forth. The proof of this is straight forward and Cantor provides it in the *Grundlagen.* We will not, at this point, enter into Cantor's technical development. Rather, we will make the transition to the discussion of its epistemological import, after a brief accounting to ourselves of the new and most powerful analytical tools that are now in our possession in the form of the sequence of the transfinite ordinals.

With the ω's themselves, we have before us a sequence of relative infinities definitely related to a specific process of construction or generation. The entire sequence of these relative infinities in turn is "packaged" in such a way that we gain a succession of number-classes of increasing power. The word "power" can be interpreted, given that we know what is on the inside, if you will, of a given number-class, not only from the standpoint of what in contemporary set theory is called the cardinality of the relevant sets, i.e., their size, but actually from the standpoint of the increasing internal differentiation of these infinite sets. In this sense, the ℵ's, that is the names of the number-classes referencing definite powers, not only express concepts of undifferentiated size, but, through the way in which the concept of power is merged with the ordering process, increasing powers actually mean increasing orders of internal differentiation.

The relevant "absolute" in all of this is none of these aggregates of ordinal numbers as such, nor some putative ordinal referencing the set of all ordinals. Cantor has pointed out, in advance of the well-known statement to this effect by Burali-Forti, that the concept of a set of all ordinals to which a definite ordinal can be assigned would be a contradictory set. However, there does exist an absolute in this, and it has to be identified precisely as that generating process or, if you will, the process of counting, of "organizing" the different levels of the infinite. When we focus on this process function rather than the completed product or outcome of the counting or generating, this process can never be conceived as a completed infinite. To put it another way, it would clearly be a contradiction in terms to attempt to conceive of an open-ended process as being completed at a given point. So there is really no mystery in the Burali-Forti paradox. Cantor's own failure to point out why no such contradiction exists simply points again to the fact that Cantor's own comprehension of the full impact and implications of his work, especially in the later parts of his life, became more and more clouded — clouded precisely through his own increasingly formal interpretation of the results of his work.

Toward the Continuum Hypothesis

With the 1883 publication of the *Grundlagen,* Cantor's work reached a critical point. We shall explain in the following how most of the critical questions concerning the further elaboration of his theory revolve around the "continuum hypothesis." This question further serves as a convenient focal point for the discussion of the broader epistemological implications of the theory of the transfinite.

Let us review the mathematical results and the battery of analytical tools available to Cantor at the conclusion of the *Grundlagen.*

First, as early as 1874, Cantor discovered the existence of point sets of different cardinalities — the countable sets of the natural and rational numbers and the uncountable set of the real numbers which can be regarded as one-dimensional coordinates of the line continuum. Significantly, in his subsequent mathematical researches, no other cardinalities than these two, the countable and that of the so-called perfect sets (defined via his notion of derivative), ever arose.

Second, the definition of the transfinite ordinals led to the simultaneous definition of successive number-classes and the result that the set of countable ordinals is of the size of the second number-class. The transfinite ordinals are formed by the same type of limiting process which generates the irrational numbers which, in turn, are of the power of the continuum.

On the basis of juxtaposing the result of the two, Cantor was almost naturally led toward the formation of the hypothesis, first explicitly formulated in the already-mentioned 1878 paper, that the line continuum is of the power of the second number class, i.e., that there are as many points on the real line as there are countable ordinals. By the end of 1883, Cantor appeared to be certain that the proof of the continuum hypothesis was virtually at hand. Thus, at the end of the sixth essay in the *Mathematical Annals* series "On Infinite Linear Point Manifolds" (the *Grundlagen* piece was number five in the series), he writes "From this will be deduced with the help of the theorems *proved* in No. 5, paragraph 13, that the (real) line continuum has the power of the second number class (II)."

But it was not just the "natural course" of his mathematical researches which led Cantor to formulate the hypothesis and expect its early proof. It was precisely this kind of theorem which appeared to him to be necessary to affect the transition from the *immanent* reality of his new numbers to their *transient* significance. The continuum hypothesis once proved would connect the abstract second number-class of his infinite "counting" numbers to the empirical continuum.

There can be no question that Cantor, in the process of the invention of his new numbers, had precisely such an empirical application in mind. This becomes clear from a letter he wrote to the Swedish mathematician Mittag-Leffler on Oct. 20, 1884. Interestingly, in this letter he also promises to Mittag-Leffler an essay on quadratic forms, exactly the type of topic to which he would have had to apply himself to reconnect his work to geometry and, specifically, Riemannian manifolds. The contents of the letter coincide with the conclusion to an 1885 article in the *Acta Mathematica* Vol. VII, entitled, "On Several Theorems of the

Theory of Point Sets in an n-fold Extended Continuous Space G_n"

> The above investigation on *point sets* I undertook from the outset not simply out of speculative interest, but with a view toward applications which I expected to be made of them in *mathematical physics* and in other sciences.
> The hypothesis upon which most of the theoretical investigations into *natural phenomena* are based, have never been very satisfactory to me, and I thought that I had to ascribe this to the fact, that *theoreticians* generally either leave questions about the *ultimate* elements of matter in a state of complete indeterminacy or assume these, as so-called *atoms*, to be of a *very small* but not *entirely vanishing volume.* I had no doubt that in order to arrive at a satisfactory *explanation of nature,* the *ultimate* or genuinely *simple* elements of matter must be presupposed to be of an *actually infinite number* and as regards the *spatial* aspect must be regarded as *entirely without extension* and strictly punctual.
> I was strengthened in this view, when I noticed that in recent times such eminent *physicists* as *Faraday, Ampère, Wilhelm Weber,* and of the *mathematicians* among others *Cauchy* have stated the same conviction...
> I start from the view, which I believe to coincide with that of present day physics, that we have to presuppose *two specifically different types of matter acting the one upon the other* and accordingly also *two* different coexisting classes of *monads,* i.e. *body-matter, body-monads* and *aether-monads* ....From this standpoint there arises as the *first question* what *powers* regarding their elements are to be ascribed to these two types of matter, insofar as they are viewed as *body* - and *aether*-monads respectively in this regard I already several years ago formed the *hypohesis* that the *power* of *body-matter* is that which in my investigations I call the *first* power; that on the other hand the power of the *aether-matter* is the *second* power.

We have quoted this most explicit of Cantor's statements concerning his view of what he called the transient significance of the transfinite numbers at length because in it two contradictory elements are combined. On the one hand it is clear that precisely from its empirical validity must ultimately derive what we judge to be the significance of Cantor's theory. To the extent that he saw that, and sought to establish this validity, he was surely moving in the right direction. Yet, the specific way in which he suggests his numbers connect-up with the real world is not only factually wrong but, more significantly, incorrect in principle and points to the major weakness in Cantor's own epistemological development.

What would have been necessary to investigate was not some kind of correspondence of the static structure of Cantor's transfinite system with an equally static and given structure of ultimate elements of reality, but rather a correspondence between the processes of generation defining both of these structures considered as finished product. In other words, the basis in reality for Cantor's numbers would have had to be identified by trying to find in nature the *Erzeugungs Prozess,* the process of generation which coheres with the *"Erzeugungs Prozess"* upon which the magnificent structure of the transfinite ordinals is based.

It is this correspondence between processes of generation which was the real kind of continuum hypothesis which Cantor would have had to discover if he wanted to proceed beyond the point reached in 1883. It is, at the same time, ironical and tragic that Cantor actually had nearly all the necessary evidentiary material at hand which would have allowed him to establish such a correspondence. However, he would have had to look not directly into nature, but first and foremost into his own mind — at himself as the subject of the creative process which can and must find itself first in itself and only secondarily, or by implication, in the negentropic process of universal evolution.

Parenthetically, if Cantor had come to understand that his structure of the transfinite simply maps the trace of the creative process, then he would have been able to directly utilize the principle of the "unity of the all" which he announced in the *Grundlagen* in order to establish the direct coherence between his mathematical-theoretical investigations and the evolutionary process of the physical universe. On the basis of the presently available biographical evidence, based principally on letters from Cantor to Mittag-Leffler and a short biographical study published in 1930 by Frankel, it appears that certain unresolved questions concerning the nature of religious belief prevented Cantor from identifying what one might call the secular basis of which the continuum hypothesis was the false ideological representation in his own mind. Both from a note on the concept of the absolute in the appended notes to the *Grundlagen* and also from a similar note in a communication from Cantor to a certain Enestroem in Stockholm on Nov. 4, 1885, the same point emerges — while Cantor correctly distinguishes between the absolute and the transfinite modes, and points out that the failure to make such a distinction represents the Achilles heal of Spinoza's *Ethics*, his religious mystification of the contents of the absolute becomes the identifiable point where his system fails. We quote briefly from the Enestroem letter:

Cantor argues that the uncritical rejection of the legitimate notion of the actual infinite is a kind of myopia which takes away the ability to

> see the actual infinite even though in the form of its highest and absolute bearer it has created us and maintains us and in its secondary transfinite form it surrounds us everywhere and even lives in our own mind.

> Another frequent mixup occurs between the two forms of the actual infinite through the way in which the transfinite gets intermingled with the absolute even though these two concepts are strictly distinct in so far as the former (i.e. the transfinite) is an infinite, but nonetheless can still be added to, while the latter (i.e. the absolute) is essentially such that it cannot be added to (cannot be made more plentiful) and thus is to be thought of as mathematically indeterminable.

The problem here ought to be obvious. Cantor identifies the absolute, that which produces the different transfinite modes or relative infinities, with God rather than with the self-subsisting creative process of his own mind. He then identifies the transfinite modes with concepts in the mind which is correct as far as it goes. It becomes wrong when an actual identity of mind and such concepts appears to be postulated. To identify substance as subject is the final necessary step which tragically Cantor was unable to take. Symptomatic of this and, more specifically, of his mistaken focus on the structure of the process rather than on the process itself is his developing intellectual relationship with Edmund Hussel which begins almost as soon as Hussel arrives as a teacher at the University Halle-Wittenberg in 1887. But before we draw out the implications of this in a concluding biographical sketch of Cantor's, we shall briefly interpose a few concluding remarks on the subject of the continuum hypothesis.

In his famous address to the Paris International Mathematicians Congress in 1900, Hilbert *(12)* listed the continuum hypothesis as the first of the major unsolved problems of mathematics. In 1908, prompted by the succession of paradoxes generated on the basis of a purely formal conception of Cantor's set theory, Zermelo undertook to formulate the theory in axiomatic form. The question of the continuum hypothesis then became posed analogously to the famous earlier question of the independence of the parallel postulate from the remaining axioms of Euclid's geometry: Is the continuum hypothesis itself to be added as an axiom of set theory or is it a provable theorem?

The problem was solved in two steps. First in 1938, Kurt Gödel proved the consistency of the continuum hypothesis with the remaining axioms, then in 1963 Paul J. Cohen proved the consistency of the negation of the continuum hypothesis with the remaining axiom thus establishing its independence from the axioms. Moreover, it was a by-product of Cohen's work that not only the continuum hypothesis as stated by Cantor, but an infinite number of other possibilities of assigning $\aleph$'s to the line continuum emerged as a consistent possibility. This utterly destroys axiomatic set theory as a viable mathematical theory and that was precisely the conclusion which a large number of mathematicians drew at the time. What they failed to see was that it was not Cantor's theory that had been destroyed, but only the assumption that it could be comprehensively formulated as a formal axiomatic theory. Cohen himself, in the conclusion of his 1966 book *Set Theory and the Continuum Hypothesis,* suggests the following:

> A point of view which the author feels may eventually come to be accepted is that the continuum hypothesis is obviously false. $\aleph_1$, is the set of countable ordinals and this is merely a special and the simplest way of generating a higher cardinal. The set C, that is the continuum in contrast is generated by a totally new and more powerful principle, namely the power set axiom. It is unreasonable to expect that any description of a larger cardinal which attempts to build up that cardinal from ideas deriving from the replacement axiom can ever reach C. Thus
>
> $$C > \aleph_n, \aleph_\omega, \aleph_\alpha,$$
>
> where
>
> $$\alpha = \aleph_\omega$$
>
> etc. This point of view regards C as an incredibly rich set given to us by one bold new axiom which can never be approached by any piecemeal process of construction. Perhaps later generations will see the problem more clearly and express themselves more eloquently.

Indeed. However we need not accept Cohen's reasons and still can in principle agree with him that the continuum hypothesis is false. In general terms, we have already stated this above. To briefly further elaborate the point, the continuum and, if there is going to be any significance for that concept, the continuum of the real world manifold clearly cannot be assigned any one cardinality because this continuum is constantly in a process of further evolution. There exists a continual process of generation, *Erzeugungs Prozess,* which produces a nested sequence of manifolds each internally characterized by a specific relative infinity or transfinite number and in such a fashion that we can conceive of every new manifold characterized by a new order of the transfinite. Each successor manifold is seen to function as a "counting manifold," which bears the same relationship to the preceeding manifold as ω bears to the preceeding natural numbers. Such a counting manifold embodies the principle of internal differentiation and organization of that preceeding manifold. We shall see in the final section of this paper how this allows us to give, at least in outline or in programmatic form, a new interpretation to the General Theory of Relativity which removes the principal objectionable features of that theory, in particular, the assumption of a necessary cosmological singularity or "Big Bang" if you prefer.

Biographical Notes

Georg Ferdinand Ludwig Philipp Cantor was born on March 3, 1845 in St. Petersburg. His father was a Jewish merchant who, by the time Georg Cantor was born, had already converted to the Evangelical Lutheran faith. Cantor's mother, Maria Eoehm, was a Catholic and came from a family of practicing musicians. In 1856, the family moved to Frankfurt where Cantor attended various schools in Frankfurt, Wiesbaden and Darmstadt.

He began his university studies in Zürich in the fall of 1862, but then moved to Berlin in the fall of 1863 to attend lectures and seminars by Kummer, Weierstrass, and Kronecker. Apart from the summer semester of 1866, which was spent in Göttingen, Cantor spent the major part of his studies at the University of Berlin where he received his PhD. in Dec. 1867. In 1869, he took a teaching job at the University in Halle-Wittenberg. The subsequent decade or more, precisely the period from 1871 through 1884, defines the most productive period of his life. He became a full professor in Halle in 1879, and for health reasons was forced to retire from his professorship in 1905. He died on Jan. 6, 1918, in the psychiatric clinic in Halle.

We have already referred to the tragic turn of Cantor's life shortly after he had concluded the *Grundlagen,* and a few less significant subsequent papers. An almost clinical record of the decisive crisis in his life which developed early in the year 1884 led to a period of temporary insanity in the spring and early summer of 1884. The subsequent deep and recurring depression is contained in a sequence of 52 letters which, in 1884 alone, Cantor wrote to his only friend among his mathematician colleagues, the Swedish mathematician Mittag-Leffler.

The appended excerpts from some of these letters should give a relatively clear picture of the succession of events and of their contents. The two immediate proximate causes for Cantor's mental collapse are to be sought in, first, the circumstances surrounding his inability to come to grips with the problem of the continuum hypothesis and the repeated failure to formulate a proof, and, second, and more fundamentally, his relationship to the Berlin mathematics Czar, Leopold Kronecker.

Cantor is by no means exaggerating when in various letters he charges that for the entire period of the 1870s and early 1880s, in which he publishes the major results of his scientific effort, Kronecker is making it his personal business to discredit Cantor's work among the largest conceivable circle of mathematicians throughout Europe. For example, he wrote a series of letters to the eminent French mathematician, Hermite, charging that Cantor's results on transfinite numbers were nothing but entirely baseless speculations, in order to obviate any influence of Cantor's work, not only in Germany, but in France as well.

There can be no question that at least until the mid-1880s Kronecker's efforts were only too successful. Cantor consequently found himself in a social environment where, among those whom he regarded as his peers, he found absolutely no resonance for his scientific efforts. His later lapse into an almost structuralist interpretation and reformulation of his earlier work must be regarded as a direct consequence of this extreme form of social isolation. There can be no doubt that under these circumstances he began to develop certain paranoid fantasies which, by the spring of 1884, led to a complete nervous breakdown. It also appears that in this situation he received little or no support from his immediate family.

Acute periods of insanity were followed by periods of depression, a sense of utter worthlessness and increasing guilt feelings concerning his relationship with Kronecker. He began to accuse himself of having gone too far, of doing injustice to Kronecker in his accusations. Finally, in the fall of 1884, Cantor decided "to go to Canossa," or Berlin as it were, to attempt some form of reconciliation with Kronecker. It is clear from his correspondence with Mittag-Leffler in this period that, possibly under family pressure, what preceeded the actual visit with Kronecker in Berlin was nothing short of a process of brainwashing which Cantor inflicted upon himself. After the actual meeting with Kronecker, Cantor at least partially replaced his own identity with that of the Berlin controller. In fact, Cantor's growing formalism after 1884 amounts to a rejection of his own earlier identity and the adoption, at least in part, of the outlook characteristic of Kronecker's views. By the end of 1884, it is clear that much of Cantor's creative genius was destroyed. We have the record in his own recounting of a visit to Halle by Mittag-Leffler in that period. This only friend of Cantor's found him engaged in the obsessive effort to prove the identity of Shakespeare and Francis Bacon. Cantor never fully recovered from his 1884 collapse.

Letters

Excerpts of letters from Cantor to the Swedish mathematician Mittag-Leffler written in the year 1884; quoted and translated from A. Schoenflies, *"Die Krisis in Cantor's Mathematischem Schaffen"* ("The Crisis in Cantor's Mathematical Creative Work"), *Acta Mathematica*, 50 (1928), pp. 1-23

January 1, 1884

Your perception of the meaning of my application (for a professorship) is *entirely correct*; I did not think in the least that I would already acquire the Berlin position at this point.

Since, however, I am interested in getting it in due time, and since I know that Schwarz and Kronecker for years now have been concocting horrible intrigues to discredit me for fear I might get there at some point or other, I thought it my duty to take the initiative myself and apply to the minister.

The exact effect this would have I knew entirely in advance, namely that Kronecker would jump as if he had been stung by a scorpion and along with his auxiliary troops would begin to howl such that Berlin would think itself displaced into the African deserts with its lions, tigers and hyenas. It appears that I have actually achieved this purpose.

January 6, 1884

The idea conveyed in your letter of December 28, that even the French mathematicians are now beginning to show an interest in my work, does not quite strike me as plausible....for as long as I have been engaged in scientific work, Kronecker has systematically attacked my work and declared it as suspect, as empty fantasies without any real basis.

January 21, 1884

My dearest friend!

In answer to your letter of the 17th, let me tell you that I am in agreement with everything you intend to do.

Your letter contains many interesting matters; I would *very much welcome it*, if Kronecker would carry out his intention and put his grudge against function theory and the general theory of aggregates, of which the latter is a part, into written words...

Now I am eager to see what he will send you for the *Acta*; if he should really do this, then his essay will be full of malice against the neighbor in Halle-Wittenberg; who knows if his natural cleverness will not at the last moment win the upper hand so that, as up till now, he will keep his arms in hiding, something which, at any rate, has brought him more success than open enmity would...

Many greetings and I remain *de tant mon coeur votre ami dévoué*

George Cantor

January 25, 1884

My dear friend!

...With respect to this man (i.e., Kronecker) take to heart the words *"timeo Danaos et dona ferentes."*...

It is *highly suspicious* that he would offer the product of the *passion* he has collected inside of himself against function theory and the theory of aggregates specifically to *you* and *your journal*; I suspect that the *only purpose* he pursues with this is to drive me or rather my essays out of the *"Acta"* much as he has entirely succeeded in doing with respect to *"Crelle's Journal."*

The reason why I have not sent *anything there* for seven years is none other than that I *forever* preclude any communality with Herr Kronecker; he knows this quite well and now also wants to force me to stop publishing in the *Acta*.

I'll see if I am not right.

Many sincere greetings from your faithfully devoted friend

George Cantor

January 26, 1884

My dear friend.

...So the *Acta* are supposed to be good enough to spread around this filth, his own journal he does not want to use for it.

How does Kerr Kronecker *dare* tell you that "he hopes you will accept his work for the *Acta* with the same *impartiality* as in the case of the investigations of your friend Cantor"?

Maybe *his* concoctions require *impartiality* and tolerant treatment in protection of that little bit of a transitory power position which he has been able to create for himself; for *my* work I *demand partiality*, but not partiality for my own transitory person, but partiality for the *truth*, which is *eternal* and with the most sovereign disdain looks down upon those moles who dare imagine that their miserable scribblings will be able to adversely affect it in the long run.

Sincere greetings from your eternally faithful friend

George Cantor

September 9, 1884

It is very interesting to me to gather from your post card today that at least for the moment your paper has only done harm to you in France, since in it you take recourse to my own work...

I suspect that the tone of this affair was set in Berlin and Göttingen and that the good Frenchmen only go along with it out of politeness.

Even Weierstrass probably is not innocent in this; *even he does not like it*, that you have joined me in friendship.

August 18, 1884

My dear friend!

....The excitements I have had this summer and of which I have written to you, lie behind me. They had their basis, as I can now tell you, in the differences into which, not without my own fault, I had gotten through my scientific work. Perhaps you had already correctly suspected this.

Not exhaustion from my work, but frictions, which I reasonably could have avoided, *were the cause for my distemper.*

The fact that Kronecker so sharply spoke against my work should not have inflamed me against him to such an extent as you saw this last winter; in my eagerness I actually went *to the point of injustice* in the letters I wrote to you about this. *I truly regret this.* And even if Kronecker initiated all this, I have nonetheless come to the decision to hold my hand out to him and *attempt* a reconciliation with him...

With sincere greetings, your devoted friend

G. Cantor

September 9, 1884

Then I will also look up Herr Kronecker and we'll see if your positive opinion of my persuasiveness will be proved correct with him; I do not expect this, for this is, so to speak, a *question of power, and that kind of question can never be decided by way of persuasion;* the question is *which* ideas are more powerful, comprehensive and fruitful, Kronecker's or mine; only success will in time decide our struggle!!

August 26, 1884

I am now in the possession of an extremely simple proof for the most important theorem of the theory of aggregates, that the continuum has the power of the number class II.

November 14, 1884

My dear friend,

You know that frequently I thought to be in possession of a rigorous proof that the line continuum possesses the power of the second number class; time and again there were gaps in my proofs and always I exerted myself anew and in the same direction and when once again I thought I had reached the most desired goal, I suddenly rebounded, because in some hidden corner I noticed a faulty deduction.

And when in these last days I once again exerted myself to the same purpose, what did I find? I found a rigorous proof that the continuum does *not* possess the power of the second number class and moreover that its power is not given by any specifiable number.

However fatal an error may have been, especially one that has been harbored for so long, its final elimination, in turn, is so much more of a gain.

November 15, 1884

My dear friend.

The reasons of which I wrote yesterday against the theorem of the *second* power of the continuum, I have again *disproved* today; thus all the reasons in favor of the second power of the continuum once again come into the foreground unconquered....

December 17, 1884

Perhaps it will interest you that in the course of the studies with which I was occupied when you honored me with your visit last Sunday, I have become more and more convinced that the view held by some Americans and Englishmen concerning the authorship of the works bearing Shakespeare's name, is correct. Francis Bacon, he and he alone could have been the author of these master works; for it is one and the same fiery spirit which confronts us in the dramas on one hand and in the "moral essays" and the rest of Bacon's works on the other.

III. Relativity

> Whatever may be one's attitude in detail towards these arguments, this much seems fairly certain: new elements which are foreign to the continuum concept of the field will have to be added to the basic structure of the theories developed so far, before one can arrive at a satisfactory solution of the problem of matter.
>
> Wolfgang Pauli,
> *Theory of Relativity.*

The principal datum any competent theory of the physical universe in its entirety must account for or, minimally, be consistent with is the existence of human beings in it, *including the creative capacity of the human mind*. This is a modest enough requirement; however, I can say without hesitation that there exists at his point not a single general physical theory that satisfies it, or, for that matter, appears to have been designed to do so.

Given the, at best, cynically indifferent and, in many cases, thoroughly reactionary and anti-human social and political ambiance and practice of contemporary mathematicians and physicists, this is hardly surprising. Their intellectual environment is dominated by the most wretched behaviorist and positivist accounts of human existence, and there is nothing in their individual lives which would provide them with any kind of insight into the essential characteristics of the process of expanded social reproduction or demonstrate to them the necessity of fighting for a concept of the cohering process of the creative activity of their own mind. But how, in the absence of even the rudiments of a humanist outlook defined by the necessity of progress through the creative intervention of the individual, could the problem of identifying a structure of the physical universe abiding by the same kind of necessity even arise? Can anyone imagine a Massachusetts Institute of Technology physicist saying "here I am, molding human beings in my image," to be creative, to recognize no "boundaries for mankind," and this ability is the only thing worth explaining and it cannot be explained unless the whole world is molded in my image? But this is precisely what is required. Nothing short of it will succeed in wrenching natural science out of its present state of misery and abysmal mediocrity. Comparison of today's scientists' credo of either propitiation or anarchist reclusion with the outlook of the early 17th century founders of modern science — Giordano Bruno, Johannes Kepler, Rene Descartes, and even of the somewhat dubious Galileo — further defines the point. While their life-circumstances in the period of the Counter-Reformation and the devastation of the Thirty Years War were hardly preferable to ours, they pursued their scientific work with outstanding moral courage (— compare the dismal capitulationism of the McCarthy period! —), grounded firmly in the humanist principles of the primacy of conscious human existence, the perfectibility of the human mind and the unity of the universe in which these qualities must find their organic expression. No lesser standards of moral fortitude and cohering scientific rigor are acceptable today.

In light of this, I will now evaluate Einstein's relativity, specifically its cosmological implications. That amounts to the task of looking at General Relativity on the basis of Riemannian geometry as amended by Cantor's theory of the transfinite rather than in terms of the affine Levi-Civitá-Weyl interpretation. The connection between Riemann-Cantor and relativity is most easily established by way of Felix Klein's theory of invariants which he developed in his 1872 "*Erlanger Program*"; in particular, I will make a tentative identification between Klein's invariants with respect to a given group of transformations and Cantor's transfinite numbers, and demonstrate that the chief defect of General Relativity lies in limiting the notion of the physical continuum to that of a linear continuum governed by just *one* order of the transfinite.

Felix Klein: The "Erlanger Programm."

If a three-dimensional object such as a cube, a cone, or a pyramid is moved from one place in three-dimensional space to another, we assume that its size, its angles, etc. remain unchanged by this process. Now let a cube be given by a definite set of spatial coordinates x,y,z and instead of moving the cube, assume that the entire space surrounding it (and represented by a definite coordinate system) is displaced by a certain amount, rotated about the origin of the coordinate system, and so forth. Such motions of the total space can be expressed as *transformations* of the coordinate system originally given, and the geometrical properties of a rigid body are then definable as those properties of the body which remain *invariant* with respect to specified kinds of coordinate transformations.

Based on such considerations, Felix Klein in his 1872 inaugural address at the University of Erlangen programmatically defined all of geometry as the study of the following general type of problem:

> Given a manifold and a group of transformations defined in it; the objects belonging to the manifold are to be investigated with respect to such properties as remain unchanged through transformations of the group.

Or equivalently:

> Given a manifold and a transformation group in it, develop the theory of invariants with respect to the group.

Klein's concise and comprehensive formulation marks the culmination point of successive applications of the methods of *projective geometry* to ever wider areas of geometrical research — a success story which began only 50 years earlier with the 1822 pulication of Poncelet's *Traité des Propriétés Projectives des Figures. (13)* Poncelet starts from the notion of *central projection* (or *perspective*) and proposes to determine those fundamental internal relations of objects that remain unchanged under arbitrary projections of that kind.

Analytically, projections in three-space are given by fractional linear transformations of the coordinates x,y,z which have the same denominator:

$$x' = \frac{a_1 x + b_1 y + c_1 z + d_1}{a_y x + b_y y + c_y z + d_y}$$

$$y' = \frac{a_2 x + b_2 y + c_2 z + d_2}{a_y x + b_y y + c_y z + d_y}$$

$$z' = \frac{a_3 x + b_3 y + c_3 z + d_3}{a_y x + b_y y + c_y z + d_y}$$

Affine geometry can immediately be seen to be part of projective geometry by noting that affine transformations are simply projective transformations with the common denominator one. The transition to metric geometry, however, requires a somewhat different approach, and for a long time it was believed that metric geometry and the "geometry of position" (projective geometry) were two irreducibly different branches of the subject. The bifurcation was overcome with the 1859 publication of Arthur Cayley's *A Sixth Memoir on Quantics*, where the concept of general projective measure determination is defined with the result that

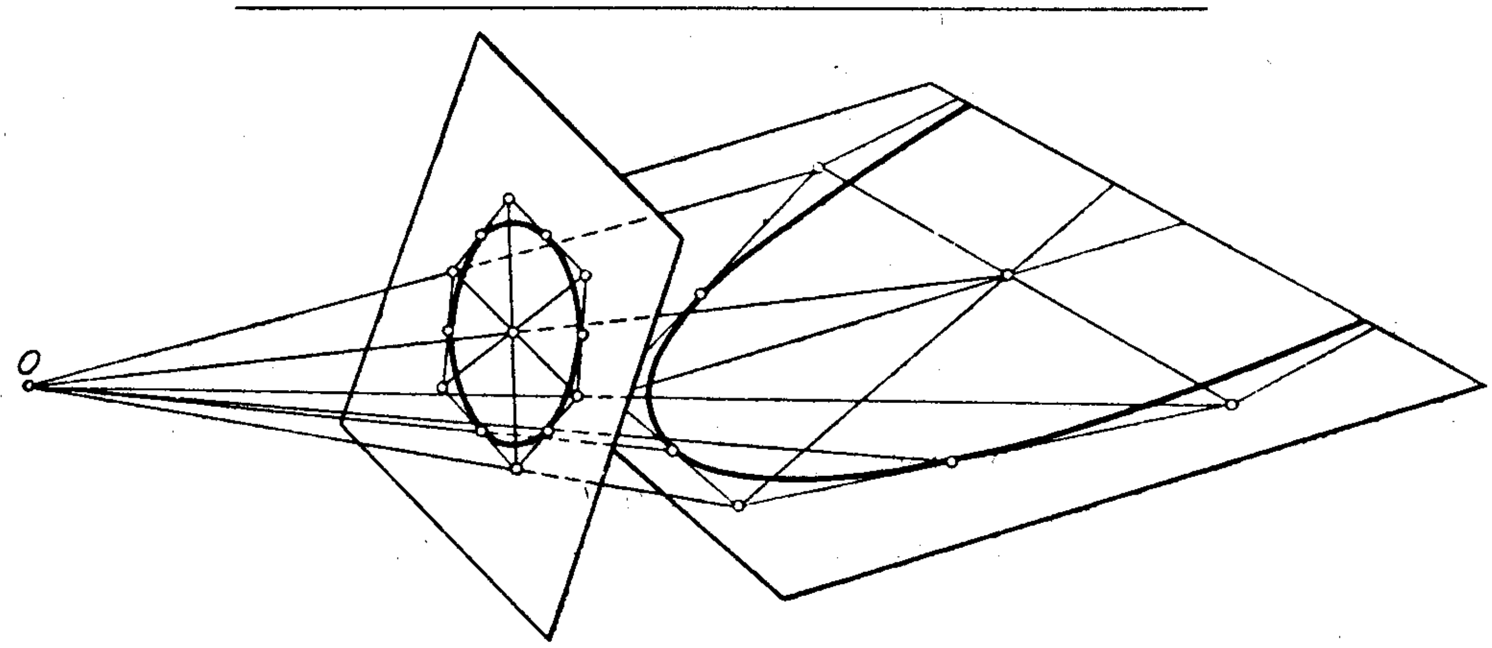
> metrical geometry is a part of descriptive (projective) geometry and descriptive geometry is all geometry and reciprocally (Cayley).

Cayley observed that the basic concepts of metric geometry (angles, distances) are covariants of the imaginary spherical circle, that is, remain unchanged by all linear functions which transform $x^2 + y^2 + z^2$ into itself. Analogous to projective and affine geometry, metrical geometry can thus be defined as the theory of invariants of that subgroup of the group of projective transformations which leave the spherical circle fixed.

Soon after first encountering Cayley's ideas in 1869, Felix Klein proposed their natural generalization: Through *adjoining* different invariant conic sections (or, more generally, quadratic forms

$$\left.\sum a_{ik} x_i x_k\right)$$

to a given system of objects, the projective group gets narrowed down to a variety of different subgroups, all representing generalized metrics of one sort or



Projective transformations are the most general kind of *linear* coordinate transformations. Like *affine* transformations, they map straight lines into straight lines (straight lines are projective invariants). However, projective transformations transform certain finite points to infinity, and consequently they do not preserve the parallelism of lines and more generally do not respect the differences between different conic sections, such as ellipses, parabolas, and hyperbolas. Expressed positively, any conic section can be continuously transformed into any other one by appropriate projections and projective geometry recognizes no inner differences between them.

another — including the non-Euclidean ones. Nor is there any need to limit attention to the projective group, itself a subgroup of a still larger group — the group of all continuous coordinate transformations. According to the fundamental theorem of projective geometry, which states that the only continuous transformations which transform straight lines into straight lines are the projective ones, the projective group can be singled out from the larger group through adjoining the manifold of straight lines (or, equivalently, planes). The group of all continuous transformations — if we require them to be one-to-one — is itself of great significance: properties of geometrical objects which remain invariant with respect to all such transformations are called *topological* properties, and topology therefore appears simply as a branch of geometry in Klein's sense.

Soon after the 1904-05 publication of Lorentz', Poincaré's and Einstein's fundamental results on the electrodynamics of moving bodies ("Special Relativity"), Klein noted that his generalized conception of geometry as the study of invariants with respect to specified groups of transformations could be used to cast the new physical theories into coherent mathematical form and to remove the air of paradox that accompanied their original formulation. The formal details are presented in a 1910 paper "On the Geometrical Foundations of the Lorentz-Group." In the introduction, Klein writes:

> What contemporary physicists call *relativity theory* is the theory of invariants of the four-dimensional space-time region x,y,z,t (the Minkowski "world") with respect to a specified group of collineations (projective transformations), viz. the "Lorentz-group"; — or more generally...: one could very well replace the name "theory of invariants relative to a group of transformations" by the word "relativity theory with respect to a group."

I will now give brief expositions of Special and General Relativity from Klein's standpoint.

Relativity Theories

The Michelson-Morley experiments (see illustration) are usually taken to have disproved the existence of the absolutely stationary "luminiferous aether" assumed by the Maxwell-Lorentz theory of electrodynamics as the medium for the propagation of electromagnetic waves (light, etc.). Such views at best represent a mistaken emphasis and will have to answer to the question of what might be meant by the "propagation of waves in empty space." What the experiments did suggest — and this is Einstein's starting point in his 1905 paper "On the Electrodynamics of Moving Bodies" — is

> that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest.

Unfortunately, there exists an extremely well-established electrodynamical phenomenon — the constancy of the speed of light and its independence of the velocity of the emitting source — which implies a direct contradiction with this hypothesis. It is the principle merit of the Michelson-Morley experiments to have brought this inconsistency between Newtonian mechanics and Maxwell's electrodynamics out into the open. The contradiction can be demonstrated explicitly as follows:

1. Galilei's Law of Inertia states that a body at rest or in uniform rectilinear motion remains in that state as long as no forces act upon it. This law is the root of the relativity principle of classical mechanics, that, for any two coordinate systems which move uniformly and rectilinearly with respect to each other ("inertial systems"), the laws of mechanics hold in the same simple form. Expressed in the language of the *"Erlanger Programm,"* the laws of Newtonian mechanics are invariant relative to the group of "Galilei transformations"

$$x' = x - vt$$
$$y' = y$$
$$z' = z$$
$$t' = t\,.$$

(The system of equations above is a simple substitution of the general Galilei-Newton group of Chapter II, section 1.)

The Michelson-Morley experiments, etc. suggest that the laws of electrodynamics are invariants of the Galilei group as well.

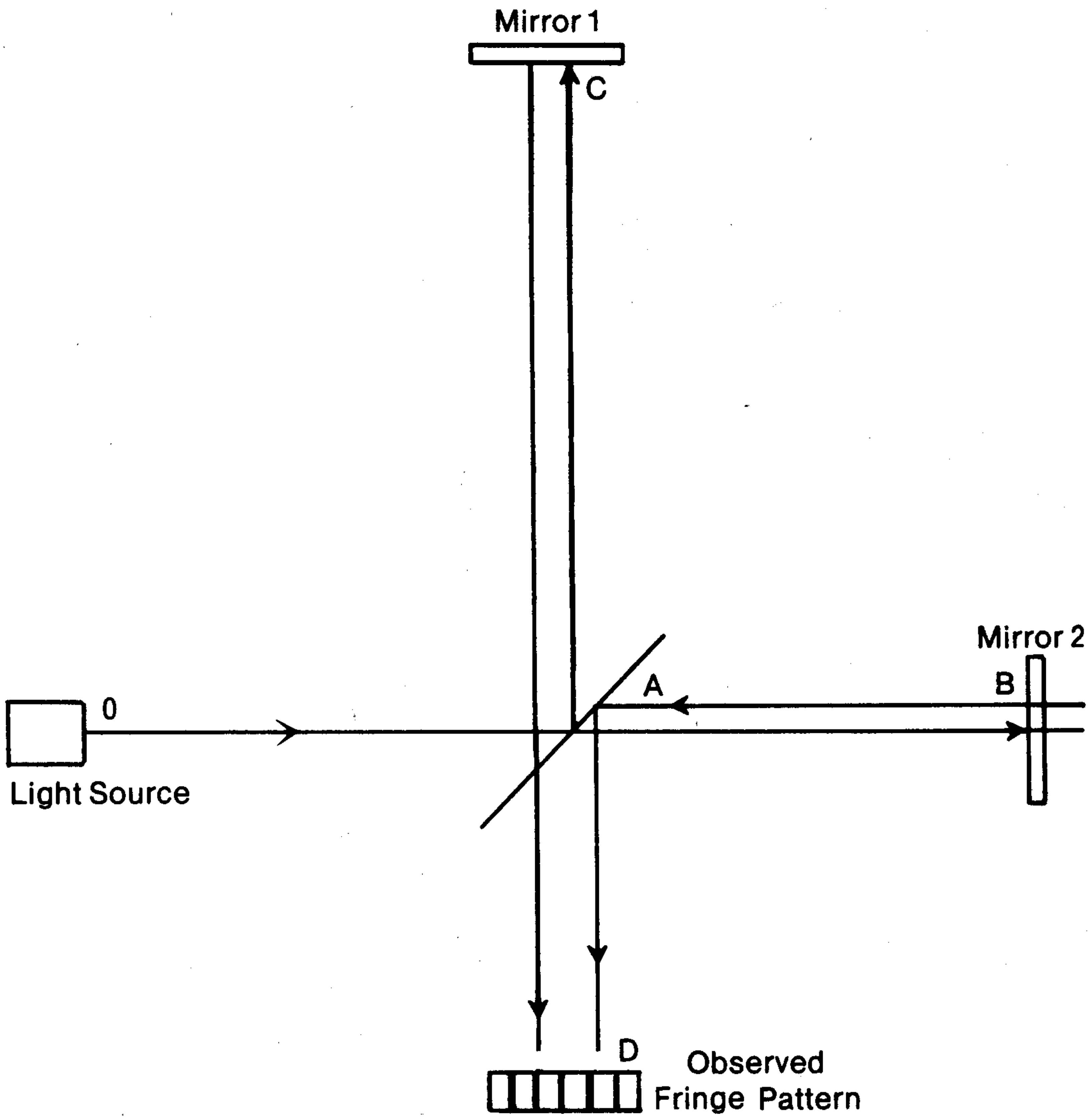
2. Let c be the velocity of a light ray with respect to an inertial system i ; let j be an inertial system which moves with constant velocity v in the direction of the x-axis of i. Then the velocity of the light ray with respect to j is c−v. Thus, contrary to hypothesis, a crucial law of electrodynamics, the constancy of the speed of light, is not an invariant of the Galilei group.

Rather than resorting to some ad hoc construction (aether convection, etc.) to deal with the difficulty posed by the inconsistency of the relativity principle with the constancy of the velocity of light, Einstein took it as the occasion for a thorough re-examination of the conceptual bases of the theories involved. Though not aware at the time of Riemann's investigation in the theory of manifolds and the radical relativity principle advanced in the *Hypothesen* that all measure-relations are to be determined relative to

The Michelson-Morley Experiment

As Maxwell first remarked, the time required by a ray of light to travel from a point A to a point B and back to A must vary — though only by a magnitude of second order — when the two points together undergo a displacement with respect to the stationary light-carrying aether.

In experiments carried out by Michelson in 1881 and again by Michelson and Morley in 1887 no such variation was discovered.



Details of the Experiment

A ray of light originating at 0 is aimed toward point A on a half-silvered mirror. Half the light is reflected toward mirror one at C and half of it passes through to mirror two at B. The ray at C is reflected back toward A and half of it is transmitted toward D; the ray at B is also reflected back toward A, with half of it reflected toward D. The two rays have paths in common between 0 and A, and between A and D. The parts of the rays that do not have paths in common make round trips in perpendicular directions. The two rays produce interference fringes at D, and it is this fringe pattern that is observed. Since it is a very sensitive measure of wave length and since wave length alters proportionately with the speed of light, a change in the interference pattern was expected as the entire experimental set-up (mounted on a massive stone disc floating on mercury) was rotated about a vertical axis. However, while a large number of experiments with the apparatus occupying many different orientations with respect to the fixed stars was carried out, the displacement of the interference fringes always remained well within the errors of observation. The speed of light is not influenced by the motion of the earth even to the extent involving second order quantities.

actual empirical processes, Einstein's conclusions tended precisely in that direction.

The formulation of the relativity principle of classical mechanics and the transition from one inertial system to another by means of transformations of the Galilei group are based on two tacit assumptions: the absoluteness (independence of the choice of the inertial system) of time and of length, or, more accurately, the existence of absolute standards of measurement of time and of length equally employable in all inertial frames independent of their state of motion. Otherwise how could we take the velocity v (as a function of time and length) of a body with respect to system i and compare it to the velocity v′ it has with respect to system i′? Einstein showed — and this is the essential contents of his "Special Relativity" Theory — how such comparisons can be made without invoking absoluteness assumptions, and that it is precisely by defining time relative to the actual physical process of the propagation of light in a vacuum that the stated contradiction between mechanical and electromagnetic phenomena is eliminated.

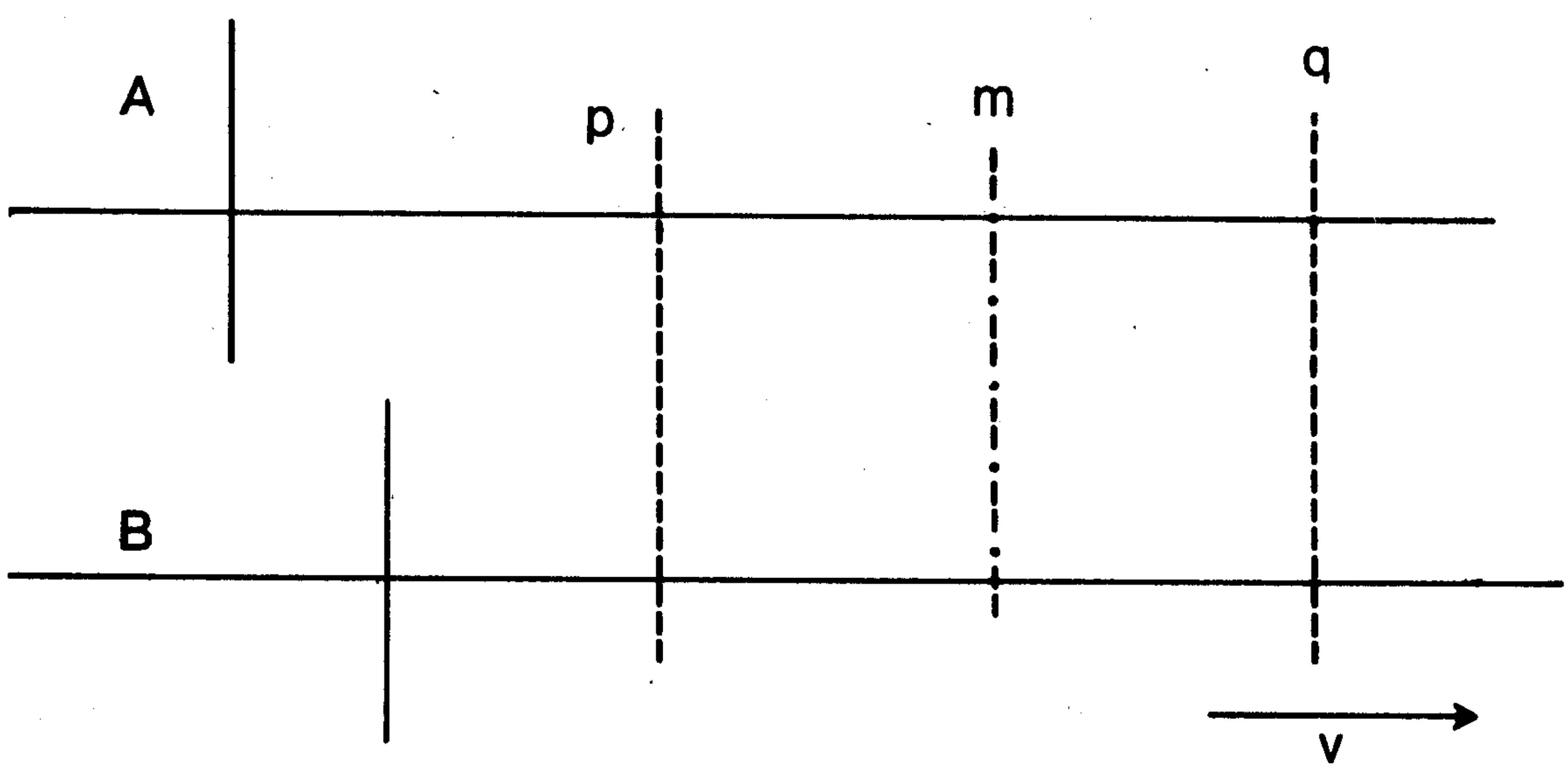
The epistemologically crucial point in the "empirical definition" of time is the relativity of simultaneity. In pre-relativity physics, the notion of simultaneity had posed no problem. Just as all physical events were assumed to take place and be measured with respect to stage-like "absolute space," so the simultaneous occurrence of two events even at great distances from one another was, at least in principle, supposed to be establishable by means of one big "absolute world-clock," with clocks used in actual measurement synchronized with respect to it. The problem arises when we ask ourselves how, metaphysical assumptions aside, such synchronization is actually to be carried out.

Einstein proposed the following: Let p and q be points at rest with respect to each other, and assume a clock has been positioned at each point. Assume further that at time t_p a light ray is emitted from point p, at time t_q the ray is reflected at point q, and at t'_p it returns to p. Then the clocks at p and q are said to be synchronized if

$$t_q = (t_p + t'_p) / 2 .$$

As a consequence of this notion of synchronization by means of an, in principle, arbitrary physical process connecting different places, simultaneity looses its absolute character. Events simultaneous relative to system $i = (p, q)$ will not be simultaneous in system $j = (r, s)$, if j moves with velocity v relative to i.

Relativity of Simultaneity



Let systems A and B be in relative motion (of velocity v) with respect to each other and let events e_1 and e_2 occur at points p and q respectively. We say that e_1 and e_2 occurred *simultaneously*, if light rays sent out from p and q at the occurrence of e_1 and e_2 meet exactly at the half-way point m.

Now assume simultaneity of e_1 and e_2 with respect to system A. Since system B moves with velocity v toward point q, the light rays sent out from p and q at the occurrence of e_1 and e_2 will *not* meet at the midpoint between p′ and q′ (the points corresponding to p and q in system B), but at a point closer to q′ ; e_1 and e_2 are *not simultaneous* with respect to system B.

The most significant implication of this relativity of simultaneity (and hence of time generally) is that the above contradiction between classical relativity and the constancy of the speed of light is now no longer derivable. Incorporating the law of the independence of the propagation of light from the motion of the source into classical relativity — given the newly found relativity of time — becomes an exercise in simple algebra. Transition from a coordinate system (x,y,z,t) to a system (x′,y′,z′,t′) is to be effected by means of the transformation of the so-called *Lorentz group*

$$x' = \frac{x - vt}{1 - \frac{v^2}{c^2}}, \quad y' = y, \quad z' = z,$$

$$t' = \frac{t - \frac{v}{c^2}x}{1 - \frac{v^2}{c^2}}$$

In mathematical terms, therefore, Special Relativity is just the invariance theory of this transformation group, which for $c \to \infty$ becomes identical to the Galilei group, and whose characteristic invariant is given by the expression

$$x^2 + y^2 + z^2 - c^2t^2$$

Close inspection of the formal properties of this invariant allowed Hermann Minkowski, who at one time had been Einstein's mathematics teacher at the Federal Institute of Technology in Zürich, to develop a formulation of Einstein's Special Theory, which brought out the fullest epistemological implications of the discovery of the relativity of time. A 1908 Cologne address Minkowski delivered to the Assembly of German Natural Scientists and Physicians starts out with the words:

> The views of space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. They are radical. Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality.

Minkowski observed that if in place of the time t the imaginary quantity $u = ict$ ($i = \sqrt{-1}$) is introduced, then the quadratic differential form

$$x^2 + y^2 + z^2 - c^2t^2$$

transforms into

$$x^2 + y^2 + z^2 + u^2$$

and thus becomes completely symmetrical in the space and time coordinates, a symmetry which is communicated to all physical laws which are invariant with respect to the Lorentz group. The four coordinates x,y,z,t (or x,y,z,u) can, therefore,from the outset be regarded as the coordinates of *world points* of a four-dimensional space-time manifold, the *world*. In that case, the expression

$$x^2 + y^2 + z^2 + u^2$$

which is Lorentz invariant, is most naturally viewed as the *square of the distance* of two world points, providing the Minkowski world with an almost-Euclidean metric structure. If this world is regarded as a substantial totality with no empty space allowed, then a substantial point at any world point x,y,z,t may be recognizable at another time, with the variations of its spatial coordinates dx,dy,dz corresponding to a time variation dt. For variations of the parameter t, the substantial point will describe a curve in the four-dimensional manifold, its *world line*, and all physical laws will find a simple expression as invariant reciprocal relations between these world lines.

Equipped with the economical concept of the four-dimensional Minkowski world, it is easy to apply the Riemannian relativity concept to Einstein's Special Theory and recognize an obvious asymmetry. Riemann's point of view demands that all measure-relations be empirically determined. In Special Relativity, this is satisfied for the temporal, but not for the spatial components of the world-manifold. Thus, after resolving the apparent contradiction between classical mechanics and electrodynamics, the proper question for Einstein (from a Riemannian perspective) would have been: what are the implications for physical theory and what would be an appropriate mathematical formalism for extending the relativity of measurement to *all* the dimensions of the four-dimensional world?

The most general answer is suggested by applying Riemann's relativity principle to the expression

$$E = \frac{mc^2}{\sqrt{1 - \frac{v^2}{c^2}}}$$

for the energy of a material particle implied by the Special Theory. It establishes a definite quantitative relationship between the inertial mass and the energy of a material particle, allows the measurement of the one through the other, and removes the mass-energy dualism of pre-relativity physics. Consequently, a Riemann-type generalization of Einstein's Special Relativity would define the measure-relations of the world-manifold relative to the in-general inhomogenous local distribution of energy and the energy-equivalents of local masses.

Such a theory, characterized by the dependency of the metric field of the manifold upon the overall energy-distribution field (i.e., the total material = energetic contents of the world) would contain Einstein's own General Relativity Theory, which he developed by way of a ten year detour via the principle of the equivalence of inertial and gravitational mass, as a special case. In it, the gravitational field and the inertial guidance field, rather than representing independent forms of energy, would be regarded as specialized "projections" of the total energy field and their otherwise extraordinary equivalence would come to be simply one more empirical fact about the world-manifold among others. This is the more significant, since only a "general" relativity theory, derived with the inherently limited objective of providing a theoretical justification for the equivalence principle, makes plausible the adoption of the restrictive assumption of the Euclidean-ness of the manifold in the small. From the standpoint of general Riemannian geometry, the empirically demonstrated identity of inertial and gravitational mass (or worse yet, *"Gedankenexperimente"* concerning imaginary elevators, etc.) appears as a mere accidental point of departure for the development of the relativity theory of the world-manifold; so, of course, does the implied exclusive choice (criticized in the Weyl section above) of a quadratic differential form as the world-metric. Einstein's derivation of the energy-equivalent of mass, on the other hand, represents the most important line of his investigation, and the suggested Riemann-generalization of Special Relativity based upon it would have raised directly the problem that still remains open today: the determination of the energy of the interior of charged particles. Consideration of this problem, in turn, would have indicated the choice of a world-geometry opposite to that which ultimately vitiates Einstein's entire approach: not linearity, but increasing complexity of the geometry of the manifold in the small would have been the obvious direction in which to proceed, opening up the way toward consideration not only of static geometrical configurations, but of the geometry of dynamical interactions between field and particles which are now beginning to be explored in plasma physics. (*14*)

Singularities

This world-order did none of gods or men make, but it always was and is and shall be: an everliving fire, kindling in measures and going out in measures.
Heraclitus, fragment 30.

The mathematical formulation of Einstein's General Relativity Theory in a sense is simpler than that of the Special Theory: the laws of nature are required to be invariants not just with respect to a certain group of orthogonal linear transformations (Lorentz group), but with respect to *all* continuous point transformations with the adjoint differential form

$$ds^2 = g_{ik}\,dx^i dx^k \quad (g_{ik} = g_{ki}).$$

This formulation, while it eliminates some of the asymmetries of the Special Theory, remains subject to the objections I raised above against Hermann Weyl's proposed solution of the *"Raumproblem"*: the exclusive adoption of the differential form (I) introduces the kind of linearity (Euclidean-ness in the small) into the world-manifold, which makes a theory of measurement based upon it inapplicable *in principle* to the problem of matter. Uneliminable singularities arise which have exactly the same epistemological status as Kantian "things-in-themselves": a priori unknowable, their existence engendered by the assumption of a priori categories of measurement.

Einstein was acutely aware of this problem of singularities and on several occasions expressed the hope of being able to handle it by extending Maxwell's electromagnetic or his own gravitational field theory into a singularity-free theory of the *unified* physical field. Presumably — cf., the 1919 paper "Do Gravitational Fields Play an Essential Part in the Structure of the Elementary Particles of Matter?" and also the 1917 "Cosmological Considerations on the General Theory of Relativity" — this was to be done by utilizing gravitational forces to counterbalance repulsive Coulomb forces and repulsive electromagnetic pressures to prevent gravitational collapse, thus avoiding both microscopic and macroscopic (cosmological) singularities. However, all attempts in this direction failed — as well they should have: there is only *one* unified physical field to begin with, rather than qualitatively different and separate gravitational, electrical, magnetic, etc. fields which are later glued together into one unified one. There exists, of course, qualitatively distinguishable components of the unified field, but they cannot conceivably bear the kind of linear relations to each other that Einstein assumes. And all along it is overlooked that it is actually the implicit linearity of the differential form (I), which is principally responsible for inducing singularities in the first place and will continue to do so if it is carried over into a unified field context, much as it did in the physically more impoverished environment of the gravitational field.

Aside from its inapplicability *in principle* to the problem of the structure of matter, the difficulties with Einstein's General Theory are best demonstrated through an examination of its cosmological consequences. Again we meet with the conjuncture of linearity and singularity: given certain reasonable assumptions, the Einsteinian universe has a "beginning" and an "end," and in between undergoes an essentially linear process of expansion and recontraction.

This was not at first recognized. In fact, assuming that the hypothesis of a non-static universe would lead to "boundless speculation," Einstein in the 1917 "Cosmological Considerations" had artificially introduced the "cosmological λ-term" into his original field equations as a force of repulsion which would keep a static model of the universe in equilibrium and prevent gravitational collapse. Then, in 1922, the Soviet mathematician A. Friedmann found a new set of solutions of the original field equations leading to world-models with a time-dependent metric, i.e., models in which the "world-radius" (or the typical distance between arbitrary galaxies) changes with time. When shortly thereafter the astronomer Hubble discovered a red shift of the spectral lines of galaxies proportional to their distance, which could only be interpreted as a Doppler shift due to a velocity of recession from the observer, the notion of an expansion of the universe in its entirety implied by the Friedmann solution to the Einstein equations rapidly became an accepted empirical fact.

However, there are further and much less satisfactory implications. If, as Einstein (I think correctly) argues, the universe is closed (i.e., finite, bounded in space) and spherically symmetric, then there are excellent reasons to assume that the metric of every such universe after the lapse of a finite proper time (either into the future or into the past) develops a singularity. (Cf., Einstein, *Meaning of Relativity*; J. A. Wheeler, *Geometrodynamics*).

So, some 10 or 12 billion years ago was there actually a "big bang," "creation," "the beginning of the world"? And after another 20, 30, or 50 billion years will "the world come to an end"? The notion is absurd. All that Einstein's equations imply is that the "laws of nature" or that fraction of the laws of nature which they express, cannot be extended beyond a finite period of time, and — as I have argued at several points — this is precisely the way things should be. There is *no one* set of invariants which governs the process of the evolution of universal substance once and for all. Hence, we cannot substantially improve our knowledge of that process by proceeding "in depth" from the limited set of laws of Einstein's General Relativity as a theory of the gravitational field to the more complete set of laws of the unified field. If, *per impossibile*, a unified field theory in Einstein's sense had been constructed, if we were in possession of a complete set of invariants for this epoch of universal evolution, it would still not be sufficiently complete to converge upon the set of invariants that will govern the next epoch. But such convergence, a kind of time-extended "principle of correspondence," a notion that not just "in depth," but also in time the laws of nature must be continuous with "previous" laws or contain them as a limiting case, ultimately defines the linearity of Einstein's approach, the Heraclitean character of his universe.

There is no such continuity of process governed by unchanging laws; no such process could have produced human existence, much less be consistent with our potential for future alterations of the laws of nature. The structure of the physical universe extended in time is necessarily that of a non-linear Cantorean continuum, characterized by variability of invariants, discontinuity of the process of nature from the standpoint of any given set of laws. For now this must remain in the form of negative assertion. The immediate challenge in the natural sciences consists in converting it into positive contents through jointly conceptualizing the necessary singularities of continuum-field theories and the discontinuities imprinted upon microscopic processes by the quantum of action.

Footnotes

(1) Gottlob Frege, (1848 - 1925) German mathematician; author of *The Foundations of Arithmetic* (1884); first to espouse the view that all mathematics is reducible to logic.

(2) Cf., Lyn Marcus, *Dialectical Economics*, D.C. Heath, Lexington, Mass., 1975: Free energy ratio, pp. 46-53; Simple vs. Extended Reproduction, pp. 199-228; Economy as Process, pp. 384-386.

(3) The unity principle is the explicit antithesis of any pluralistic conception and therefore, by implication, of any world outlook based upon a pluralistic social practice. In this context, it might briefly be mentioned that the historically, radically pluralistic social practice in the United States, which, from at least the 1880s on, was deliberately reinforced by a variety of counterinsurgency efforts by ruling circles, engendered a world outlook which was thoroughly antithetical to positive development of theoretical science and must be regarded as the cause for the otherwise astonishing fact that as late as the 1930s the United States, which by that time was without a doubt the world's most developed industrial nation, had not yet produced a single significant original thinker in any of the natural sciences.

(4) *De Magnete*, W. Gilbert, Dover, New York, 1958, p. 23: "It is to be understood, however, that not from a mathematical point does the force of the stone emanate, but from the parts themselves;..."

(5) Letter to Herwart von Hohenburg, Jan. 2, 1607: "...*nata mihi est haec speculatio: a Luna maria sic attrahi ut gravia omnia ipsague maria attrahuntur a Terra.*"

(6) Marsilio Ficino (1433 - 1499), Italian Renaissance philosopher, translator of Plato and Plotinus into Latin; the paradox is developed in "Five Questions Concerning the Mind of God."

(7) Newton, *Principia*: "Absolute space, in its own nature, without relation to anything external, remains always similar and immovable."

Cf. also Newton's hypothesis (!) that the universe has a center which is at rest.

(8) In this context, it is interesting to note that two of the most important German geometers of the first half of the 19th century — August Ferdinand Moebius, who authored and developed the concept of *Barycentric Calculus* in 1827, and Christian von Staudt, who published his thesis on *The Geometry of Position* in 1847 — were Gauss' students in astronomy for a number of years. Staudt, in particular, through his *Geometry of Position* along with the French Polytechnicien Poncelet was one of the co-founders of modern projective geometry.

(9) "An electron is a charge of total amount ϵ spread through a very small volume." — Sir James Jeans

(10) The existence of a set of individuals as a member of a sequence of those individuals implies the possibility of the existence of a "set of all sets" — a contradictory entity.

(11) When Einstein's Relativity Theory first became known, (and notorious) in the U.S., Rabbi Herbert S. Goldstein of New York cabled Einstein: "Do you believe in God?" Einstein cabled back: "I believe in Spinoza's God..." This is relevant in this context, as Cantor has pointed out: Spinoza's finite existences are hard put to maintain their distinctive existence in the face of his actual infinite (God).

(12) David Hilbert, 19th century German mathematician, since 1895 in Göttingen; made significant contributions to virtually all areas of mathematical research; in 1900 delivered a famous address to an international mathematicians' congress, formulating 20 outstanding problems which have largely determined mathematical research in the 20th century; founder of the formalist school of the foundations of mathematics and of the algebraic theories ("Hilbert spaces") employed in theoretical formulations of quantum theory.

(13) We note here that Poncelet was a student at the *Ecole* from 1808 to 1810. In 1812, he was assigned to the *grande armée* and participated in the Russian winter campaign — only to be taken prisoner of war. Much of the 1822 *Traité* was written during the following two years of imprisonment in Russia on the basis of intensive discussions with a small circle of other imprisoned *polytechniciens*.

(14) Fruitful formulations, are provided by the work of research groups led in the U.S. by physicists Dan R. Wells and Winston H. Bostick, and in the Soviet Union by scientists V.N. Tsytovich, V.E. Zakharov, and L.I. Rudakov.

W. H. Bostick of Stevens Institute developed the theoretical and experimental basis for "force-free" plasma structures, otherwise known as plasma filaments or, simply, plasmoids: W. H. Bostick and D. R. Wells, *Physics of Fluids*, 6, (1963), p. 1325; W. H. Bostick, et al., *Physics of Fluids*, 9 (1966), p. 2078. Besides demonstrating dynamic "self-subsistence," or dynamic stability, these structures are characterized by the fact that the plasma electric and magnetic fields and-or the plasma mass-fluid motion contains more energy than that of the thermal, or internal, energy of the plasma gas.

D. R. Wells developed a more comprehensive theoretical framework which led to a theory of general dynamic stability: D. R. Wells and J. Norwood, Jr. , *Journal of Plasma Physics*, 3 (1969), p. 21; D. R. Wells, *Journal of Plasma Physics*, 4 (1970), p. 645.

The complementary investigations of the Soviet groups is reviewed in a recent Lebedev Institute pre-print, "Electron Beams with Gas and Plasma," translated by the U.S. Department of Defense, and authored by V. N. Tsytovich. The paper describes theoretical and experimental work with "strongly turbulent" plasmas generated by the interaction of intense electron beams with a plasma. This "new type of turbulence" is characterized by the fact that, again, the energy of the plasma mass motion and electric fields is greater than the internal energy. The formation of new types of structures, described as "a new state of matter," are observed. These well-ordered structures, variously termed cavitons, spikons, and relatons, are in theoretical terms quite similar to what could be described as a Bostick-Wells type system of plasmoids or plasma filaments. While these fundamental investigations are only at their initial phase, as V. N. Tsytovich notes, the recent experimental achievement of the group headed by L. I. Rudakov, in the generation of electron beam pellet inertial thermonuclear fusion, indicates the fruitfulness of these more general theoretical-experimental investigations.

Bibliographical Notes

Books are listed in the order of occurrence of the relevant subject matter in the text of the "Introduction." Classical pre-20th century treatises are not included. All the ones referred to in the "Introduction" are readily available in contemporary editions and should be consulted as the principal collateral reading to the "Introduction." They are almost always more readable and convey the relevant concepts more directly than contemporary textbook-type treatments of the same subjects.

Whitehead and Russell, *Principia Mathematica*, Cambridge University Press, 1964.
The introduction presents as good an overview as any of the field of mathematical logic and includes a detailed analysis of the character of the paradoxes of set theory.

Benaceraff and Putnam, *Philosophy of Mathematics*, Prentice-Hall, Inc., 1964.
This is a book of readings in the philosophy of mathematics and contains at least three worthwhile essays on the subject: 1. Selections from Russell's "Introduction to Mathematical Philosophy," 2. Kurt Gödel's "Russell's Mathematical Logic," 3. "What is Cantor's Continuum Problem?" also by Gödel. Do yourself the favor and ignore the rest.

Max Caspar, *Kepler*, Abelard-Schuman, 1959.
An excellent biography of the astronomer.

Wilhelm Windelband, *Lehrbuch der Geschichte der Philosophie* (History of Philosophy), J.C.B. Mohr, 1950.
Aside from Hegel's *Lectures on the History of Philosophy*, Windelband's *History* is probably the best systematic treatment of the history of philosophy available. Written from a neo-Kantian perspective, it contains particularly strong sections on Ancient and Renaissance philosophy, including a good joint account of the post-Reformation, late-Renaissance philosophies of nature of Giordano Bruno and Jakob Böhme.

Wolfgang Pauli, *Theory of Relativity*, Pergamon Press, 1967.
This excellent, though somewhat technical, treatment of the Special and the General Theory was originally published in 1921 as part of the *Encyclopedia of the Mathematical Sciences*. Its most valuable chapter today is the last one entitled, "Theories on the Nature of Charged Elementary Particles." It provides a comprehensive overview (updated in the "Supplementary Notes") of the most important theories that take a strict continuum approach to the

problem of matter (so-called unified field theories).

Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrundert*, Julius Springer, 1926.
Klein's *Lectures* come in two volumes and represent the best history of mathematics I know. I have used them as a principal source in all parts of the paper. Along with the *Lectures* should be mentioned Klein's three volumes of *Elementarmathematik vom Höheren Standpunkte aus* (Elementary Mathematics from an Advanced Standpoint).

Hermann Weyl, *Space-Time-Matter*, Dover Publications, Inc.
Weyl's book contains the best treatment of Einstein's Relativity Theory from a rigorous geometrical standpoint. Chapter II develops the notion of an "affinely connected manifold" and sets out to show that every "reasonable" manifold must possess the property of "Euclideaness in the small" (Weyl's attempted solution to the "*Raumproblem*"). The "*Raumproblem*" is treated in greater detail in Weyl's *"Mathematische Analyse der Raumproblems"* in *Das Kontinuum und andere Monographien*, Chelsea Publishing Company. The latter volume also contains Weyl's edition with appendix of Riemann's *Hypothesen*. English translations of the *Hypothesen* can be found in D.E. Smith's *Source Book in Mathematics* (Dover) and M. Spivak's *Differential Geometry*, Vol. II.

Hermann Weyl, *Philosophy of Mathematics and Natural Science*, Princeton University Press, 1949.
Much less technical than *Space-Time-Matter* but, unfortunately, also much less interesting. Part I, ch. III and bits and pieces of Part II are useful.

Felix Hausdorff, *Grundzüge der Mengenlehre*, Liepzig, 1914.
I mention the 1914 edition, because it best represents the full scope at least of the mathematical side of Cantor's project. Unfortunately, the English edition of Hausdorff's *Set Theory* is a translation of an abbreviated later edition of Hansdorff's 1914 original. Still it is infinitely preferable to all the contemporary junk.

Albert Einstein, *The Meaning of Relativity*, Princeton Paperback, 1972.
Einstein's most coherent statement of his theories — with an appendix on the "cosmological problem." Einstein's original papers (plus others by Lorentz, Minkowski, and Weyl) are contained in *The Principle of Relativity*, Dover Publications, Inc. Work your way through these papers rather than utilizing one of the mainly positivistic popularized accounts (including Einstein's own!) of the Special and General Theory. Einstein's autobiography can be found in Vol. I of *Albert Einstein: Philosopher-Scientist*, Library of Living Philosophers, edited by Paul A. Schilpp, Open Court Publishing Co., 1969.

William Bonnor, *The Mystery of the Expanding Universe*, MacMillan, 1966.
The preface says: "This book is intended for the intelligent person who wants to know what modern cosmology is about. It assumes no previous knowledge of the subject and no mathematics ...." There are probably a dozen such books written in the last decade — all equally mediocre. Unfortunately they accurately reflect the present state of the art.

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Foundations of a General Theory of Manifolds

(Grundlagen einer Allgemeinen Mannigfaltigkeitslehre)

A Mathematical-Philosophical Study in the Theory of the Infinite

by Dr. Georg Cantor
Leipzig, 1883

Translator's Note

The translation follows the text of Cantor's *Grundlagen* as reprinted from the *Mathematische Annalen* in *Georg Cantor, Gesammelte Abhandlungen* (Collected Treatises), edited by Ernst Zermelo, Berlin, 1932, pp. 165-208. The preface is taken from a separate publication of the *Grundlagen*, which Cantor prepared because, in his own words, "it carries the subject much further in many respects and thus is, for the most part, independent of the earlier essays" — meaning essays one through four of "On Infinite, Linear Point-Manifolds," of which the *Grundlagen* is Number five.

All footnotes and notes are taken from Cantor's original treatise.

Parentheses contain original German terms or modern mathematical terms.

—Uwe Parpart

Preface

The treatise presented here will shortly appear in the *Mathematische Annalen* as Number Five of an essay entitled "On Infinite, Linear Point-Manifolds," the first four numbers of which are contained in Volumes XV, XVII, XX, and XXI of the same journal. All of these pieces stand in connection with two essays which I published in volumes LXXVII and LXXXIV of *Crelle's Journal,* which already contain in germinal form the main points of view that guide me in the theory of manifolds. Since, however, the present essay develops the subject much further in certain respects and thus is in the main independent of the earlier essays, I have decided to bring it out as a separate pamphlet and to provide it with a title appropriate to its contents.

In presenting these pages to the public, I will not leave unmentioned that I wrote them principally for two circles of readers: for philosophers who have followed the development of mathematics up to the most recent period, and for mathematicians who are familiar with the most important older writings and more recent works in philosophy.

I know very well that the theme I am dealing with has at all times met with the most varied opinions and interpretations, and that neither mathematicians nor philosophers have come to universal agreement with respect to this theme. I am, therefore, very far from believing that I will be in a position to have the last word in such a difficult, involved and comprehensive matter as is presented by the infinite; since, however, I have arrived at definite convictions on this subject through long years of research, and (since) these convictions have not become shaky but only more firm in the further course of my studies, I thus thought that I had a certain obligation to organize them and make them public.

I only hope that in so doing I have succeeded in finding and expressing the objective truth for which I have struggled.

The Author
Halle, Christmas 1882

Section 1

The presentation so far of my investigations in the theory of manifolds *(1)* has reached a point where its continuation becomes dependent upon an extension of the concept of a real whole number beyond the present boundaries; in particular, this extension goes in a direction in which, to my knowledge, no one has so far looked for it.

I find myself dependent to such an extent upon this extension of the concept of number that without it I would hardly be able to make without constraint, the smallest further step forward in the theory of aggregates (**sets**). May this circumstance serve as a justification, or if necessary an excuse, for the fact that I am introducing seemingly foreign ideas into my reflections. For what is at issue is an extension, or actually a continuation, of the sequence of real whole numbers beyond the infinite; as daring as this may seem, I can nonetheless express not only the hope, but also the firm conviction that in due time this extension will come to be regarded as a thoroughly simple, appropriate, and natural one. At the same time I am not at all unaware that with this undertaking I am placing myself in some contradiction with widespread notions on the mathematical infinite and views held all too frequently on the nature of numerical magnitude.

As far as the mathematical infinite is concerned: to the extent that it has found justifiable use in science so far and made a useful contribution, the mathematical infinite has principally occurred in the meaning of a variable magnitude, either growing beyond all limits or diminishing to an arbitrary smallness, always, however remaining *finite*. I call this infinite the *non-genuine-infinite (das Uneigentlich-unendliche)*.

Aside from this, in the modern and the contemporary periods, both in geometry and in particular also in function theory, a different but equally justifiable kind of infinity-concept has emerged. According to this concept, in the investigation of an analytic function of a complex variable, for example, it has become necessary and in fact common practice to imagine in the plane representing the complex variable a single point at infinity, i.e., an infinitely distant but determinate point, and to investigate the behavior of the function in the neighborhood of this point in the same way as in the neighborhood of any other point. In this case it turns out that the behavior of the function in the neighborhood of the infinitely distant point exhibits exactly the same behavior as at any other point lying in the finite, so that from this we can derive that we are fully justified in imagining the infinite in this case to be located at a wholly determinate point.

When the infinite occurs in such a determinate form, I call it *genuine-infinite (Eigentlich-Unendliches)*.

Both these manifestations of the mathematical infinite — in both of which it has effected the greatest progress in geometry, in analysis, and in mathematical physics — will be kept quite distinct to facilitate the understanding of what follows.

In the first form, as the non-genuine-infinite, it presents' itself as a *variable finitude*; in the other form, what I call the genuine-infinite, it occurs as an utterly determinate infinite. The infinite real whole numbers — which I will define in the following, and to which I was led already many years ago without it entering distinctly into my consciousness that in them

I possessed concrete numbers having a real meaning — have absolutely nothing in common with the first of these two forms, the non-genuine-infinite. On the contrary, they have the same character of determinateness which we met with in the case of the infinitely distant point in analytic function theory; they therefore belong to the forms and affects of the genuine-infinite.

While, however, the point at infinity in the complex number plane stands isolated vis-a-vis all finite points, we obtain not only a single infinite whole number but rather an infinite sequence of such numbers, which are quite distinct from one another and stand in lawful number-theoretic relationships both to one another and to the finite whole numbers. These relationships are not even in principle reducible to relationships among finite numbers; the latter phenomenon does in fact occur frequently with respect to the different intensities and forms of the non-genuine-infinite — for example, with respect to functions of a variable x becoming infinitely small or infinitely large, and where they possess determinate finite orders (ordinal numbers) of becoming-infinite (*des Unendlichwerdens*). Such relationships can indeed be regarded only as veiled ratios of the finite, or at any rate, as immediately reducible to the latter. The laws governing the genuinely-infinite whole numbers to be defined are, on the contrary, entirely different from the dependency relationships which obtain in the finite realm; this does not, however, preclude that the finite real numbers themselves may undergo certain new determinations with the help of the determinately-infinite numbers.

The *two principles of generation (Erzeugungsprinzipe*) with whose aid, as it will turn out, the new determinate infinite numbers are defined, are such that through their unified effect every barrier respecting the process of concept formation for real whole numbers can be broken through. Happily, however, they are opposed, as we shall see, by a *third* **principle**, which I call the *inhibiting* or *limiting* ***principle***, by means of which certain limits will be successively imposed upon the actually endless formation process, so that we obtain natural segments in the absolutely infinite sequence of the real whole numbers, segments which I call *number-classes.*

The *first* number-class (I) is the aggregate of the finite whole numbers

$$1, 2, 3, \ldots \nu, \ldots,$$

which is followed by the *second* number-class (II) consisting of certain infinite whole numbers following each other in determinate succession. Only after the second number-class has been defined do we arrive at the third, then the fourth, and so on.

First of all, the introduction of the new whole numbers appears to me to be of the greatest significance for the development and sharpening of the *power concept (Mächtigkeitsbegriff)* introduced in my papers (*Crelle's Journal,* Vol. 77, p. 257; Vol. 84, p. 242) and frequently used in the earlier numbers of this essay. In accordance with this, there corresponds to each well-defined aggregate a determinate power, so that the same power will be ascribed to two aggregates if they can be coordinated to one another reciprocally, univocally, element for element (**one-to-one mapping**).

In the case of finite aggregates the power coincides with the *number (Anzahl)* of elements since in any arrangement such aggregates have, as is well known, the same number of elements.

In the case of infinite aggregates, on the other hand, absolutely nothing has so far been said, either in my own papers or elsewhere, concerning a precisely defined *number* of their elements ; however, it was possible to ascribe even to infinite aggregates a determinate *power* completely independent of their arrangement.

The *smallest* power of infinite aggregates had to be ascribed, as was easily justified, to those aggregates which can be reciprocally, univocally coordinated to the *first* number-class, and thus have the same power as the latter. On the other hand, an equally simple, natural definition of the *higher* powers was lacking.

Our above-mentioned number-classes of the determinately infinite real whole numbers now prove themselves to be the natural uniform representatives of a lawful sequence of ascending powers of well-defined aggregates. I will demonstrate definitively that the power of the second number-class (II) is not only different from the power of the first number-class, but that it is in fact the *next higher* power; we can thus call it the *second* power, or the power of the *second class*. In the same fashion the third number-class yields the definition of the third power, or the power of the third class, and so on.

Section 2

Another great gain attributable to the new numbers, to my mind, is a *new* concept not previously in existence, the concept of the *number* of elements of a *well-ordered* infinite manifold. Since this concept is always expressed by an entirely determinate number of our expanded number-field, assuming only that the order of the elements of the aggregate — to be defined more closely below — is determined; and since, on the other hand, the concept of the number of elements *(Anzahlbegriff)* has an immediate objective representation in our inner intuition; therefore through this connection between number of elements *(Anzahl)* and number *(Zahl),* the reality of the latter, which I have emphasized, is proven even in cases where it is determinately infinite.

By a *well-ordered* aggregate we understand any well-defined aggregate, the elements of which are bound together by a specifically pre-assigned law of succession, according to which there exists both a *first* element of the aggregate and there follows after every single element (which is not the last in the sequence) another specific element, much as to any arbitrary finite or infinite aggregate of elements there corresponds a specific element which, respecting all of them, is the *next-following* element in the sequence (except if no element exists which follows all of them in the sequence). Two "well-ordered" aggregates are now said to be of the same number (with respect to their pre-assigned laws of succession) if it is possible to put them into a reciprocally univocal correspondence with each other, so that if E and F are any two elements of the one, E_1 and F_1 the corresponding elements of the other, the position of E and F in the succession of elements in the first aggregate is always in correspondence with the position of E_1 and F_1 in the succession of elements in the second aggregate. Thus if E precedes F in the succession of elements of the first aggregate, then E_1 also precedes F_1 in the succession of elements of the second aggregate. It is easily seen that this correspondence, if possible at all, is always an entirely determinate one, and since in the extended number sequence there is always found one and only one nu.nber α so that the same number *(Anzahl)* of numbers *(Zahlen)* (from 1 on) *precedes* it in the natural succession, it is necessary to set the "number" of these two "well-ordered" aggregates directly equal to α, if α is an infinitely large number, and equal to the number directly preceding α (or α-1), if α is a finite whole number.

The essential difference between finite and infinite aggregates is now shown: a finite aggregate exhibits the *same* number of elements for *every* order of succession that can be given to its elements; on the other hand, *different* numbers of elements will in general have to be attributed to aggregates consisting of infinitely many elements, depending upon the order of succession given to the elements. The *power* of an aggregate is, as we have seen, an attribute independent of the order of the elements. The *number of elements* of an aggregate, however, shows itself to be a factor generally dependent upon a given order of succession of the elements as soon as we are dealing with infinite aggregates. There nonetheless exists, even in the case of infinite aggregates, a certain connection between the *power* of the aggregate and the *number* of its elements determined by a given order of succession.

If, to begin with, we take an aggregate which is of the power of the first class, and assign to its elements an *arbitrary* but fixed order of succrssion so that it becomes a "well-ordered" aggregate, then the number of its elements will always be a specific number of the *second* number-class, and can never be determined by a number other than one belonging to the second number-class. On the other hand, any aggregate of the first power can be ordered in such a way that the number of its elements with respect to this order becomes equal to an arbitrarily pre-assigned number of the second number-class. We can express these propositions as follows: Every aggregate of the power of the *first* class is *countable by* numbers of the *second* number-class, and only by means of such numbers. In particular, we can always assign an order of succession to the elements of the aggregate so that with respect to this order it is counted by an arbitrarily pre-assigned number of the second number-class, which number gives us the *number* of elements of the aggregate with respct to that succession.

The analogous laws hold for the aggregates of higher powers. Thus every well-defined aggregate of the power of the *second* class is countable *by* numbers of the *third* number-class, and only by those; in particular we can always assign an order of succession to the elements of the aggregate so that in this order of succession the aggregate is counted* by an *arbitrarily pre-assigned* number of the *third* number-class, which number determines the number of elements of the aggregate with respect to that order of succession.

Section 3

The concept of the *well-ordered aggregate* proves fundamental for the theory of manifolds as a whole. To the law that it is always possible to put every *well-defined* aggregate into the *form* of a *well-ordered* aggregate — a law of thought which seems to me to be basic and consequential and, because of its general validity, especially remarkable — I will return in a later treatise. Here I limit myself to the proof of how the concept of the *well-ordered* aggregate yields in the simplest fashion the basic operations for the whole numbers, be they finite or determinately infinite, and how the laws for these operations are adduced from immediate inner intuition with apodictic certainty. If, to start with, we are given two *well-ordered* aggregates M and M_1, to which the numbers α and β correspond as numbers of their elements, then

$$M + M_1$$

*
What so far in the earlier numbers of this essay I have called "countable" is, according to the now introduced and at the same time sharpened and generalized definition, nothing but countability *through* (by means of) numbers of the first class (finite aggregates) or *through* (by means of) numbers of the second class (aggregates of the first power).

is again a well-ordered aggregate, which results when we posit the aggregate M and, following it, the aggregate M_1, uniting M_1 with M. Thus there also corresponds to the aggregate

$$M + M_1$$

with respect to the resulting order of succession of its elements, a specific number as the number of its elements; this number is called the *sum* of α and β and written as

$$\alpha + \beta \ .$$

Here it becomes immediately apparent that if α and β are not both finite, then in general

$$\alpha + \beta$$

is different from

$$\beta + \alpha$$

Thus the *commutative* law already fails to be generally valid in the case of addition. It is now sufficiently simple to form the concept of the sum of *several* summands given in specific sequence, where this sequence itself may be determinately infinite, so that I need not go into this more specifically here. Thus I merely remark that the *associative* law proves generally valid. In particular we have

$$\alpha + (\beta + \gamma) = (\alpha + \beta) + \gamma.$$

If we take an ordered sequence determined by a number of equal and equally ordered aggregates, where in each case the number of elements is equal to α, then we obtain a new well-ordered aggregate whose corresponding number of elements yields the definition for the product $\beta\alpha$ where β is the multiplier, α the multiplicand. Here again we find that in general $\beta\alpha$ is different from $\alpha\beta$, so that the commutative law is also in general *invalid* for the multiplication of numbers. The associative law, on the other hand, is generally found to be valid in the case of multiplication as well, so that we have

$$\alpha(\beta\gamma) = (\alpha\beta)\gamma.$$

Certain of the new numbers stand out from the others by the fact that they have the property of prime numbers; this property, however, must here be characterized in a somewhat more determinate manner, by understanding a prime number to be a number α for which the decomposition

$$\alpha = \beta\gamma,$$

where β is the multiplier, is only possible if

$$\beta = 1$$

or

$$\beta = \alpha.$$

On the other hand, the multiplicand will, in general, even in the case of prime numbers α, have a certain range of uncertainty which because of the nature of the case is unavoidable. In a later treatise we will nonetheless show that the decomposition of a number into its prime factors can always be achieved in an essentially *unique* manner which is *determinate* even with regard to the sequence of factors (so far as these are not finite prime numbers occurring next to each other in the product). In this way two different kinds of determinately infinite prime numbers come to the fore, the first of which more closely resembles the finite prime numbers, whereas the prime numbers of the second kind have a completely different character.

Furthermore, with the aid of these new insights it will now be possible for me to present in the near future a rigorous substantiation of the theorem on the so-called linear infinite manifolds, presented at the end of the treatise, "A Contribution to the Theory of Manifolds" (*Crelle's Journal*, Vol. 84, p. 257).

In the previous number (4) of this essay I adduced for point-aggregates P which are contained in an n-dimensional continuous region a theorem which, making use of the new earlier-defined mode of expression can be stated as follows: "If P is a point-aggregate whose derivitive $P^{(\alpha)}$ vanishes identically, where α is an arbitrary whole number of the *first* or *second* number-class, then the first derivative $P^{(1)}$, and thus also P itself is a point-aggregate of the power of the *first* class." I find it highly remarkable that this theorem can be inverted as follows: "If P is a point-aggregate whose first derivative $P^{(1)}$ has the power of the *first* class, then there are whole numbers α which belong to the first or the second number-class, for which P^{α} vanishes identically and so that, of the numbers α for which this phenomenon occurs, one of them is the smallest one."

In response to the friendly invitation of my highly esteemed friend, Professor Mittag-Leffler in Stockholm, I will in the near future publish the proof of this theorem in the first volume of the new mathematical journal which he is now editing. As a sequel, Mr. Mittag-Leffler will publish an essay in which he will show how, on the basis of this theorem, his and Professor Weierstrass' investigation on the existence of univocal analytic functions with given singularities may receive considerable generalization.

Section 4

The extended sequence of whole numbers can, if need be, be completed without difficulty to a continuous number aggregate by adjoining to every whole number α all real numbers x that are greater than zero and smaller than one.

Perhaps this will now raise the question of whether, since a certain extension of the real number field to the infinitely large has been achieved in this manner, we might not with equal success define certain infinitely small numbers or, what would amount to the same thing, finite numbers which do not coincide with the rational and irrational numbers (which appear as limits of sequences of rational numbers), but which might be inserted into supposed gaps between the real numbers, much as the irrational numbers are inserted into the chain of the rational numbers, or as the transcendental numbers are inserted into the structures of the algebraic numbers.

The question of the establishment of such interpolations, to which some authors have devoted a great deal of effort, can in my opinion (and as I shall show) first be answered clearly and distinctly with the aid of our new numbers, and in particular on the basis of the general number-of-elements-concept of well-ordered aggregates. Previous efforts, it seems to me, are grounded in part on an erroneous confusion of the non-genuine-infinite with the genuine-infinite, and in part were carried out on a totally insecure and shaky foundation.

Modern philosophers have often called the non-genuine-infinite a "bad" infinite, in my opinion, unjustifiably so, since it has proved to be a very good, highly useful instrument in mathematics and the natural sciences. The infinitely small magnitudes, to my knowledge, have so far been worked out for useful purposes *only* in the form of the non-genuine-infinite, and as such are capable of all those variations, modifications, and relationships which find application and expression in infinitesimal analysis as well as in function theory, in order to there form the basis of a rich abundance of analytical truths. On the other hand, all attempts to transform the infinitely small by force into a *genuinely* infinitely small magnitude should finally be abandoned as purposeless. If genuinely infinitely small magnitudes exist in any other form at all, i.e., are definable, still they surely do not have any immediate connection with the ordinary magnitudes that *become* infinitely small.

In opposition to the above-mentioned experiments with the infinitely small, and to the confusion of the two forms of appearance of the infinite, an opinion is frequently presented concerning the essence and meaning of numerical magnitudes, according to which no numbers can be assumed as really existing other than the *finite real whole* numbers of our number-class (I).

At most the *rational* numbers which immediately spring from them are granted a certain reality. However, the irrational numbers supposedly have a merely *formal* significance in pure mathematics, in that they, so to speak, serve only as computing devices to fix the attributes of groups of whole numbers, and to describe them in a simple and unified way. The true material of analysis, according to this view, consists exclusively of the finite real whole numbers, and all truths found in arithmetic and analysis, or still begging discovery, are presumably to be regarded as relationships among finite whole numbers; infinitesimal analysis and function theory are regarded as legitimate only in so far as their theorems are provably interpretable as laws governing finite whole numbers. This view of pure mathematics, even though I cannot agree with it, undoubtedly has certain advantages which I would like to stress here. Not the least among the circumstances that speak for its significance is the fact that among its proponents are found some of the most meritorious contemporary mathematicians.

If, as is assumed here, only the finite whole numbers are real, while all of the others are nothing but relational forms, then it can be demanded that the proofs of analytical theorems be checked for their "number-theoretic content," and that every gap which appears in them be filled in, in accordance with the principles of arithmetic; in the feasibility of such augmentation is seen the true touchstone for the genuineness and the perfect rigour of the proofs. It cannot be denied that in this way the formal proof of many theorems has been perfected, and also that certain other improvements in the different parts of analysis can be effected. Following the principles flowing from this view can also be seen as a way of safeguarding against all kinds of absurdities and errors.

In this way a certain principle, even if a rather dry and obvious one, has been set, which is recommended as a guideline to all. It is intended to contain the flight of mathematical speculation and conceptualization within its true limits, where it runs no danger of falling into the abyss of the "transcendent," the place where supposedly, as is said in order to inspire fear and wholesome terror, "all is possible." Be that as it may, who knows whether it was not precisely the viewpoint of utility alone which determined the originators of this view to recommend it, as an effective regulative principle for protection against all error, to the soaring powers which so easily endanger themselves through arrogance and lack of moderation, even though a *fruitful* principle cannot be found therein.

For I cannot accept the assumption that the originators of this view themselves, in the discovery of new truths, started from these principles; no matter how many good things I may cull from these maxims, I must strictly speaking regard them as *erroneous* ; no real progress stems from them, and if things had really happened precisely in accordance with them, then science would have been retarded or, at any rate, confined within the narrowest of boundaries. Happily, things are not really that bad, and the praise for as well as the adherence to these (under circumstances and assumptions) useful rules has never been taken all that literally. Also, conspicuously enough, there has not until now been anybody, so far as I know, who would have undertaken to formulate them better and more completely than I have attempted to here.

If we look at it historically, we find that similar views have frequently been held, and already occur with Aristotle. As is well known, we find that throughout the Middle Ages among all of the scholastics, *"infinitum actu non datur"* ("the actual infinite does not exist") presents itself as an incontestable proposition taken over from Aristotle. However, if we look at the reasons which Aristotle *(2)* advances against the real existence of the infinite (cf., e.g., his *Metaphysics* , Book XI, chapter 10), we find that in the main, they can be traced to a premise that involves a *petitio principii*, viz. the premise that only *finite* numbers exist, which he arrived at on the basis that only counting procedures with respect to finite aggregates were known to him. I believe, however, that I have proved above that similarly definite counting procedures as are carried out in the case of finite aggregates can also be carried out respecting infinite aggregates, assuming we provide a definite law for these aggregates in accordance with which they are transformed into *well-ordered* aggregates. This will become still clearer in the course of this treatise.

That without such a lawful succession of the elements of an aggregate no counting can be carried out within it — this lies in the nature of the concept of *counting*. Even in the case of finite aggregates, counting can be carried out only if there exists a determinate sequence of the counted elements. However, it appears here as a special property of *finite* aggregates that the result of the counting — *the number of elements* — is *independent* of the respective orderings; while in the case of infinite aggregates, as we have seen, such independence does *not*, in general, obtain. Rather the number of elements of an infinite aggregate is an infinite whole number *co-determined* by the law of counting; in this and in this alone lies the essential distinction between the finite and the infinite, which has its basis in nature itself, and thus can never be removed. Nevermore, however, will it be possible to deny the existence of the infinite on account of this difference, while on the other hand maintaining that of the finite; if we drop the one then we must also get rid of the other. Where then would we come to on this road?

Another argument used by Aristotle against the reality of the infinite consists in the assertion that the finite would be dissolved into the infinite and destroyed, if the latter existed, since the finite number allegedly is annulled by an infinite number. The truth of the matter, as we shall clearly see in the following, is that we can *very well* adjoin a finite number to an infinite number (if the latter is thought of as determinate and complete) and unite the finite number with the infinite number *without* bringing about the annulment of the former. (On the contrary, the infinite number will be modified through such an adjoinment of a finite number to it.) Only the *reverse* process, the adjoinment of an infinite number to a finite one, when the latter is posited first, brings about the annulment of the finite number without a modification of the infinite number occurring.

This actual state of affairs concerning the finite and the infinite, which Aristotle wholly misunderstood, should produce new impulses not only in analysis, but also in the other sciences, in particular the natural sciences.

The idea of considering the infinitely large not only in the form of the unlimitedly increasing magnitude and in the closely related form of convergent infinite series — first introduced in the 17th century — but to also fix it mathematically by numbers in the definite form of the completed infinite, was logically forced upon me, almost against my will since it was contrary to traditions which I had come to cherish in the course of many years of scientific effort and investigations. Thus I do not believe that reasons could be advanced against it which I would not know how to counter.

Section 5

When just now I spoke of traditions, I understood these to be not merely traditions in the narrower sense of something one has lived through; rather, I trace them back to the founders of modern philosophy and natural science. For the evaluation of the question at issue here, I list just some of the most important sources. Compare:

Locke, *Essay on Human Understanding*, lib. II, ch. XVI and XVII.

Descartes, *Letters and Explanations on his Meditations* ; also *Principia* I, 26.

Spinoza, Letter XXIX; *Cogitata Metaphysica* , parts I and II.

Leibniz, Erdmann edition, pp. 138, 244, 436, 744; Pertz edition II, 1 p. 209; III, 4 p. 218; III, 5 pp. 307, 322, 389; III, 7 p. 273.

(Also noteworthy are: Hobbes, *De Corpore* , Chapter

VII, II; Berkeley, *Treatise on the Principles of Human Knowledge*, 128-131.)

Stronger reasons against the introduction of infinite whole numbers than are found in these sources, taken together, can hardly be devised even today; thus they should be examined and compared with my arguments. I postpone for another occasion a detailed and thorough discussion of these passages and, in particular, of the extremely significant and weighty letter by Spinoza to L. Meyer, limiting myself here to the following.

As different as the doctrines of these writers may be, in the cited passages their judgment concerning the finite and the infinite essentially agrees in this that to the concept of a number belongs its finitude and that, on the other hand, the true infinite or absolute, which is in God, admits of no kind of determination. As far as the latter point is concerned, I entirely agree (how could it be different?), for to my mind the proposition, *"omnis determinatio est negatio"* (all determination is negation") is unquestionably true. However, the former point as I already said above in the discussion of the Aristotelian reasons against the *"infinitum actu,"* I see as involving a *petito principii*, which explains several contradictions which occur in all of these authors, notably Spinoza and Leibniz. The assumption that apart from the absolute — unreachable by any determination — and the finite, no modifications can exist which, though not finite, are nonetheless determinable by numbers and consequently are what I call the genuine-infinite — this assumption I do not find justified by anything; indeed, in my opinion, it contradicts certain propositions advanced by the latter two philosophers. What I maintain and believe I have proved in this paper as well as in my earlier endeavors is that after the finite there is a *transfinitum* (which also could be called *suprafinitum*), i.e., an unlimited gradation of determinate modes which in their nature are not finite but infinite, yet which, much as the finite, can be determined by determinate, well-defined and distinguishable *numbers*. I am convinced, therefore, that the domain of definable magnitudes is *not* limited to the finite magnitudes; accordingly, the limits of our cognition may be extended further without it being necessary to do any kind of violence to our nature. In place of the Aristotelian-scholastic proposition discussed in Section 4 I thus pose the different one:

"Omnia seu finita seu infinita ***definita*** *sunt et excepto Deo ab intellectu determinari possunt."* ("All things, whether finite or infinite, are *definite* and, with the exception of God, can be determined by the intellect.") *(3)*

Quite often the finitude of the human *understanding* is adduced as a reason why only finite numbers are thinkable; however, I again see in this assertion the above-mentioned circular inference. For by "finitude of the understanding" is tacitly meant that the capacity of the understanding in respect of the formation of numbers is limited to finite numbers. If it should turn out, however, that the understanding in a certain sense is also able to define infinite, i.e., *transfinite (überendliche)* numbers and distinguish them from one another, then either the words "finite understanding" must be given an extended meaning, after which that inference can then no longer be drawn from them; or else the human understanding must also be granted the predicate "infinite" in certain respects, which, in my considered opinion, is the only correct thing to do. The words "finite understanding" which one hears on so many occasions are, as I believe, in no way on the mark. As limited as human nature may in fact be, *much* of the infinite nonetheless adheres to it, and I even think that if it were not in many respects infinite itself, the strong confidence and certainty regarding the existence *(des Seins)* of the absolute, about which we are all in agreement, could not be explained. And in particular, it is my view that human understanding has an unlimited, inherent capacity for the step-wise formation of whole number-classes which stand in a definite relationship to the infinite modes and whose *powers* are of ascending strength.

The main difficulties in the outwardly different but inwardly nonetheless closely related systems of the two last-named thinkers can, I believe, be brought closer to solution in the way I have chosen, and a number of them can even now be satisfactorily solved and cleared up. These are difficulties which in part gave rise to the later doctrine of criticism *(Kritizismus)*, which with all its advantages does not appear to me to offer a sufficient substitute for even the inhibited development of the doctrines of Spinoza and Leibniz. For side by side with, or in place of, the mechanistic explanation of nature, which within its sphere has all the tools and advantages of mathematical analysis available to it, but whose one-sidedness and insufficiency have so pointedly been laid bare by Kant, not even the beginning of an *organic* explanation of nature, equipped with the same mathematical rigor but transcending the mechanistic one, has been developed. Such an organic explanation can, I believe, be approached only through a resumption and furthering of Spinoza's and Leibniz's works and endeavors.

One especially difficult point in the system of Spinoza is the relationship of the finite modes to the infinite modes; it remains unresolved why and under what circumstances the finite as against the infinite, or the infinite as against the more strongly infinite, can maintain its independent existence *(Selbständigkeit)*. The example, already touched upon in Section 4, seems to me in its unassuming symbolism to mark the way in which we can perhaps get closer to the solution of this question.

If ω is the first number of the second number-class, then we have

$$1 + \omega = \omega,$$

on the other hand,

$$\omega + 1 = (\omega + 1),$$

where

$$(\omega + 1)$$

is an entirely different number than ω. Thus, as one can clearly see here, everything depends upon the *position* of the finite vis-à-vis the infinite. If the former comes first, then it merges with the infinite and disappears in it; if, however, it *contents* itself and takes its place behind the infinite, then it is preserved and joins with the former to become a new, because modified, infinite.

Section 6

If it should cause difficulties to comprehend *infinitely large*, *closed* whole numbers, comparable among each other and with the finite numbers, linked to each other and to the finite numbers by fixed laws; then these difficulties will be associated with the perception that, while the new numbers in many ways have the character of the earlier ones, in many more respects, however, they have a specific nature altogether their own — which often even causes it to happen that different characteristics are found joined together in one and the same number, characteristics which are disparate in the case of finite numbers and never occur together. Thus in one of the passages cited in the previous section we find the considerations that an infinite whole number, if it existed, would have to be *both* an even *and* an odd number, and since these two characteristics cannot occur jointly, therefore *no* such number exists.

It is apparent that the tacit assumption made here is that characteristics which are disjointed in the case of conventional numbers must also have this relationship to each other in the case of the new numbers, and from this the impossibility of the infinite numbers is deduced. Who would not be struck by the paralogism here? Is not every generalization or extension of concepts associated with the giving up of particular determinations, and in fact unthinkable without it? Is it not only in recent times that the idea has been grasped which is so important for the development of analysis and leads to the greatest advances — the introduction of complex magnitudes, without regarding as a barrier the fact that they can be called neither positive nor negative? And it is only a similar step which I dare take here; perhaps it will even be considerably easier for the general consciousness to follow me than it was possible to make the transition from the real to the complex numbers. For the new whole numbers, even if they stand out above the conventional numbers by a more intensive, substantive determinateness, nonetheless as "number-of-elements" (*"Anzahlen"*) share with these the same kind of reality; whereas difficulties stood in the way of the introduction of complex magnitudes until, after great efforts, their geometric representation by points or line segments in a plane had been found.

To come back briefly to that train of thought about evenness and unevenness, we again take a look at the number ω in order to demonstrate, by means of this number, how those characteristics which in the case of finite numbers are incompatible can here occur jointly, without any contradiction. In Section 3 the general definitions for addition and multiplication are put forth, and I have stressed that in the case of these operations the commutative law is in general *not* valid; in this I see an essential distinction between the infinite and the finite numbers. It should also be observed that in a product $\beta\alpha$ I regard β as the multiplier, α as the multiplicand. The following two forms for ω are then immediate:

$$\omega = \omega \cdot 2$$

and

$$\omega = 1 + \omega \cdot 2.$$

Accordingly, ω can thus be regarded both as an even and as an odd number. From a different point of view (viz., if 2 is taken as multiplier) it could also be said, however, that ω is neither an even nor an odd number since, as is easily proved, ω is not representable either in the form 2α or in the form

$$2\alpha + 1$$

Thus the number ω has indeed, in comparison to the conventional numbers, a very specific nature of its own, since it combines all these characteristics and qualities. More peculiar still are the remaining numbers of the second number-class, as I shall demonstrate later on.

Section 7

Even though in Section 5 I presented many quotes from Leibniz's works in which he expresses himself in opposition to the infinite numbers, saying among other things: "There is no such thing as an infinite number, nor line or other infinite quality, if we take them as authentic wholes": "The true infinite is not a modification, it is the absolute; on the contrary, as soon as we modify or limit ourselves, we give shape to a finite" (where in the case of the latter passage I agree with him, on the first statement but not on the second). On the other hand, I am in the fortunate position to be able to point out statements by the same author in which, in a certain sense in contradiction with himself, he expresses himself in the most unambiguous fashion *in favor* of the genuine-infinite (which is different from the absolute). Thus in Erdmann, p. 118, he says:

> I am so much for an actual infinity, that instead of admitting that nature abhors it, as it is vulgarly claimed, I hold that it is everywhere disposed towards it in order to better bring out the perfections of its Author. Therefore, I do not think that there is any part of matter which is not, I do not say divisible but actually divided, and consequently the slightest particle must be seen as a world full of an infinity of different creatures.

However, the genuine-infinite as we encounter it, for example in the case of well-defined point aggregates, or in the constitution of bodies out of point-like atoms (I do not mean here the chemical-physical, or Democritean, atoms, because I cannot regard them as existent either in concept or in reality no matter how many useful things have up to a certain limit been accomplished by means of this fiction), has found its most decisive defender in a philosopher and mathematician of our century with a most acute mind, Bernhard Bolzano, who has developed his views relevant to the subject especially in the beautiful and substantial essay, *(Paradoxes of the Infinite).* (Leipzig, 1851). It is the purpose of this essay to demonstrate that the contradictions which skeptics and peripatetics of *all times* have tried to find in the infinite do not exist at all, if only one takes the trouble (which, of course, is not always altogether inconsiderable) to internalize the concepts of infinity in all seriousness and in accordance with their true content. We therefore also find in this essay a discourse which in many respects is right on the mark on the subject of the mathematical non-genuine-infinite as it occurs in the form of differentials of the first and higher order, or in the infinite sums of series or in other limiting processes. This kind of infinite (called "syncategorematic infinite" by some scholastics) is a mere auxiliary and relational concept *(Beziehungsbegriff)* of our thinking. According to its definition it includes the notion of variability and thus the *"datur"* ("it is given") can never be said of it in the true sense.

It is quite remarkable that with regard to *this* kind of infinite there exists absolutely no essential difference of opinion even among contemporary philosophers, if I may be permitted to ignore the fact that certain modern schools of so-called positivists or realists *(4)* or materialists believe that in this *syncategorematic* infinite, which they themselves must admit has no *genuine* being, they see the ***highest concept***.

However, in Leibniz we already find the essentially correct state of affairs stated in many places; the following passage, for example (Erdmann, p. 436), refers to this non-genuine-infinite:

> When I speak philosophically, I no more establish magnitudes infinitely small than infinitely large; no more infinitesimal than infinitudinal. But, as expeditious modes of speaking, I consider both notions to be mental fictions appropriate for calculation, as are the imaginary roots in algebra as well. At the same time, I have shown that these expressions have great use as short cuts in the thought process as well as for invention and cannot be a source of error, inasmuch as it is permissible to substitute for the infinitely small as small a magnitude as one wants such that error would be less than a given quantity, whence it follows that error cannot be introduced.

Bolzano is perhaps the only one who, to a certain extent, assigns the genuine-infinite numbers their rightful place; they are frequently spoken of at any rate. However, the actual way in which he deals with them, without being able to advance any kind of real definition of them, is something about which I am *not* at all in agreement with him, and I regard for example Sections 29-33 of that book as unfounded and erroneous. The author lacks both the general *concept of power* and the precise concept of *number-of-elements* for a real conceptual grasp of determinate-infinite numbers. Both occur with him in germinal fashion in a number of places, in the form of specialities; it seems to me, however, that he does *not* work through towards full clarity and distinctness, and this explains many *non sequiturs* and even several errors contained in this valuable essay.

I am convinced that *without* the two concepts mentioned, there will be *no* progress in the theory of manifolds, and the same is true, I believe, of those fields which depend upon the theory of manifolds or are most intimately in touch with it, for example modern function theory on one hand and logic and the theory of knowledge on the other. When I conceive of the infinite as I have done here and in my earlier attempts, I derive true pleasure (to which I gratefully yield) from seeing how the whole concept of number, which in the finite only has the background of *number-of-elements*, in a certain sense *splits up* into *two* concepts when we ascend to the infinite: that of *power*, which is independent of the order given to an aggregate, and that of *number-of-elements*, which is necessarily tied to a lawful ordering of the aggregate by means of which the latter becomes a *well-ordered aggregate.* And when I decend again from the infinite to the finite, I see just as clearly and beautifully how the two concepts again become one and *flow together* to form the concept of the finite whole number.

Section 8

We can speak of the reality or the existence of the whole numbers, both the finite and the infinite ones, in *two* senses; however, these are the same two ways, to be sure, in which any concepts or ideas can be considered. On the one hand we may regard the whole numbers as real insofar as they take up a very definite place in our mind (*Verstand*) on the basis of definitions, become clearly differentiated from all the other components of our thinking, stand in definite relations to them and thus modify the substance of our mind (*Geist*) in a definite way. Let me call this type of reality of our numbers their *intrasubjective* or *immanent reality (5).* Then again we can ascribe reality to numbers insofar as they must be regarded as an expression or image of occurrences and relationships in the external world confronting the intellect, further, insofar as the different number-classes (I), (II), (III), and so on represent powers, which in fact occur in corporeal and mental nature. This second type of reality I call the *transsubjective* or *transient reality* of the whole numbers.

Given the thoroughly realist — simultaneously, however, no less idealist — foundation of my investigations, there is no doubt in my mind that these two types of reality will always be found together, in the sense that a concept to be regarded as existent in the first respect will always in certain, even in infinitely many ways, possess a transient reality as well *(6).* Admittedly, the determination of this reality generally is among the most troublesome and difficult tasks of metaphysics and frequently it must be left to a time when the natural development of another science reveals the transient significance of the concept in question.

This coherence of the two realities has its true foundation in the *unity* of the *all, to which we ourselves belong as well.*

This coherence is referred to here in order to deduce from it what appears to me to be a most important consequence for mathematics, namely that mathematics in the shaping of its conceptual material need take into account *solely and uniquely* the *immanent* reality of its concepts and thus is under *no* obligation *whatsoever* to also test these concepts with respect to their *transient* reality. Because of this distinguished position, which differentiates mathematics from all other sciences and offers an explanation for the relatively easy and unconstrained manner of pursuing it, it quite specifically deserves the name of *free mathematics,* a designation to which, if I had the choice, I would give preference over the, now customary "pure" mathematics.

Mathematics is entirely free in its development, bound only by the self-evident concern that its concepts be both internally without contradiction and stand in definite relations, organized by means of definitions, to previously formed, already existing and proven concepts. *(7)* In particular, in introducing new numbers mathematics is obliged only to give such definitions of them as will lend them the kind of determiniteness and, under certain circumstances, ther kind of relationship to the older numbers, which in a given case will definitely permit them to be distinguished from one another. As soon as a number satisfies all these conditions, mathematics can and must regard it as existant and real. Here I see the reason, suggested in Section 4 why the rational, irrational and complex numbers should be regarded just as much as existent as the finite positive whole numbers.

I believe that it is not necessary to fear, as many do, that these principles contain any danger to science. On one hand the designated conditions under which the freedom of the formation of numbers can alone be exercised, are such that they leave extremely little room for arbitrariness. And then every mathematical concept also carries within itself the necessary corrective; if it is unfruitful and inapt this is soon demonstrated by its uselessness, and it will then be dropped because of its lack of success. Any superfluous confinement of mathematical research work, on the other hand, seems to me to carry with it a much greater danger, a danger that is so much the greater as there is really no justification for it that could be deduced from the essence of the science, for the *essence* of *mathematics* lies precisely in its *freedom.*

If this quality of mathematics had not presented itself to me for the reasons mentioned, still the whole development of the science itself, as we perceive it in our century, would necessarily lead me to exactly the same views.

If Gauss, Cauchy, Abel, Jacobi, Dirichlet, Weierstrass, Hermite, and Riemann had been bound to constantly subject their new ideas to metaphysical control, then we would not be able to rejoice in the magnificent structure of modern function theory, which, while designed and erected entirely freely and without transient purposes, nonetheless has already revealed its transient significance in applications to mechanics, astronomy, and mathematical physics, as was to be expected. We would not have seen Fuchs, Poincaré and many others bring about the great forward thrust in the theory of differential equations if these excellent intellects had been hemmed in and constricted by extraneous influences; and if Kummer had not taken the liberty, rich in consequences, of introducing the so-called "ideal" numbers into number theory, we would not today be in the position to admire the very important and excellent algebraic and arithmetical works of Kronecker and Dedekind.

As justified, therefore, as mathematics is to move entirely free from all metaphysical fetters, I do not find it possible on the other hand to grant the same right to "applied" mathematics, for example analytical mechanics or mathematical physics. These disciplines, in my opinion, are *metaphysical* both in their foundations and in their goals; if they try to free themselves from this, as has been proposed of late by a famous physicist, they degenerate into a "description of nature" which must lack both the fresh breeze of free mathematical thought and the power of the *explanation* and *exploration (Erklärung und Ergründung*) of natural phenomena.

Section 9

Given the great significance which attaches to the so-called real, rational, and irrational numbers in the theory of manifolds, I would not want to neglect to say here what is most important concerning their definition. I will not go into the introduction of the rational numbers more closely, since rigorously arithmetical presentations of this have frequently been formulated. Among the ones close to my own view I call special attention to those of H. Grassmann, *Lehrbuch der Arithmetik* (Berlin 1861) and J.H.T. Mueller, *Lehrbuch der Allgemeinen Arithmetik* (Halle 1855). Yet I want to briefly discuss in more detail the three forms known to me, probably essentially the only major forms, of the rigorously arithmetical introduction of the general real numbers. These are *first*, the mode of introduction which has been followed for many years by Prof. Weierstrass in his lectures on analytic functions, of which a few hints can be found in Herr E. Kossak's programmatic treatise *Die Elemente der Arithmetik* (Berlin 1872). *Second*, Herr. R. Dedekind, in his essay *Stetigkeit und Irrationale Zahlen* (Braunschweig 1872), has published a peculiar form of definition, and *third*, I put forth a form of definition in the year 1871 (*Mathematische Annalen*, Vol. 5, p. 123) which externally bears a certain resemblance to the Weierstrass definition, so that it was possible for Herr H. Weber (*Zeitschrift für Mathematik und Physik*, 27th year, Historical Literature Division, p. 163) to confuse it with the latter. In my opinion, however, this *third* form of definition, which later was also developed by Herr Lipschitz, (*Grundlagen der Analysis*, Bonn 1877), is the simplest and most natural of all, and has the advantage that it is most immediately adapted to the analytic calculus.

Part of the definition of an irrational real number is always a well-defined infinite aggregate of the first power of rational numbers; this is the common characteristic of all forms of definition. Their difference lies in the generative moment (*Erzeugungsmoment*) through which the aggregate is tied to the number it defines, and in the conditions which the aggregate must satisfy in order to be a suitable basis for the number definition in question.

In the case of the *first* definition, an aggregate of positive rational numbers a_ν, denoted by (a_ν), is taken as a basis, which satisfies the condition that no matter how many or which of a finite number of a_ν are summed up, this sum always remains below a specifiable bound. If now we have two such aggregates (a_ν) and (a_ν'), then it is rigorously shown that they can present three cases: either every part $\frac{1}{n}$ of unity is always contained equally often in both aggregates so long as a sufficient, augmentable, finite number of their elements are summed up; or, from a certain n on, $\frac{1}{n}$ is always contained more frequently in the first aggregate than in the second; or thirdly, from a certain n on, $\frac{1}{n}$ is always contained more frequently in the second than in the first. In accordance with these occurrences, if b and b' are the numbers to be defined by means of the two aggregates (a_ν) and (a_ν'), then in the first case we set

$$b = b',$$

in the second

$$b > b',$$

in the third

$$b < b'.$$

If the two aggregates are joined together in a new one

$$(a_\nu,\ a_\nu'),$$

then this provides the basis for the definition of

$$b + b';$$

if, however, we form of the two aggregates (a_ν) and (a_ν') the new one

$$(a_\nu \cdot a_\nu'),$$

the elements of which are the products of all the a_ν, and all the a_ν'

then this new aggregate is taken as a basis for the definition of the product bb'.

We see that here the generative moment which ties the aggregate to the number to be defined by it, lies in the *formation of sums*; however, it has to be stressed as *essential* that only the summation of an always finite number of rational elements is utilized so that the number b to be defined is *not* already posited from the outset as the sum

$$\Sigma a_\nu$$

of the infinite series (a_ν); this would embody a *logical mistake* since, on the contrary, the definition of the sum

$$\Sigma a_\nu$$

is attained only by setting it equal to the *completed* number b which must of necessity already have been defined in advance. I believe that this logical mistake, which was first avoided by Herr Weierstrass, was committed almost universally in previous times, and not noticed because it belongs among those rare cases in which actual mistakes cannot cause any significant damage to the calculus.

I am nonetheless convinced that all the difficulties which have been found in the concept of the irrational are linked to this mistake, whereas when this mistake is avoided, the irrational number will implant itself in our mind with the same determinateness, distinctness, and clarity as the rational number.

Herr Dedekind's form of definition takes as its basis the *entirety of all* rational numbers, partitioned into two groups in such a way that, if the numbers of the first group are denoted by A_ν, those of the second group by B_μ, then always

$$A_\nu < B_\mu .$$

Herr Dedekind calls such a partition of the rational number aggregate a "cut" of the latter, denotes it by

$$(A_\nu \mid B_\mu),$$

and associates a number b with it. If we compare two such cuts

$$(A_\nu \mid B_\mu)$$

and

$$(A'_\nu \mid B'_\mu),$$

we find that just as in the *first* form of definition there exist altogether *three* possibilities in accordance with which the numbers b and b' represented by the two cuts are posited as equal to each other, or as

$$b > b',$$

or as

$$b < b'$$

The first case, apart from certain easily adjustable exceptions which occur in the case of the being-rational (*Rationalsein*) of the numbers to be defined, takes place only in the case of the total identity of the two cuts, and in this the undeniable, decisive preferability of this form of definition over the two others comes to the fore, that to each number b there corresponds a unique cut. This, however, is counterbalanced by the great disadvantage, that in analysis numbers *never* present themselves in the form of "cuts," into which form they must first be brought with great skill and trouble.

Here, as well, the definitions for the sum

$$b + b'$$

and the product

$$bb'$$

follow on the basis of new cuts arising from the two given ones.

The disadvantage attaching to the *first* and the *third* form of definition — that here the same, i.e. equal, numbers present themselves infinitely often so that an unambiguous overview over the entirety of the real numbers is not immediately obtained — can be removed with the greatest of ease through specification of the base aggregates (a_ν), by drawing on any one of the well-known unique system formations such as the decimal system or simple continued-fraction expansions (*Kettenbruchentwickelung*).

I come now to the *third* form of the definition of real numbers. Here again an infinite aggregate of rational numbers (a_ν) of the first power is taken as a basis; however, a different character is demanded of it than in the Weierstrass form of definition. I postulate that after the choice of an arbitrarily small rational number ϵ a finite number of members of the aggregate can be separated off, so that those remaining have pairwise a difference which in absolute terms is smaller than ϵ. Every such aggregate (a_ν) which can also be characterized by the postulate

$$\operatorname*{Lim}_{\nu=\infty}(a_{\nu+\mu} - a_\nu) = 0$$

(for arbitrary μ)

I call a *fundamental series* (*Fundamentalreihe*), and associate with it a number b to be defined by it and for which the sign (a_ν) itself could even be fittingly used, as was proposed by Herr Heine, who after numerous discussions had come to agree with me on these questions. (Cf. *Crelle's Journal*, vol. 74, p. 172). Such a *fundamental series*, as can rigorously be deduced from the concept, presents three cases: either its members a_ν are, for sufficiently large values of ν, smaller in absolute terms than an arbitrarily pre-assigned number; or from a certain ν on, the latter are larger than a definitely determinable positive rational number ϱ; or from a certain ν on, they are smaller than a definitely determinable negative rational magnitude $-\varrho$. In the first case I say that b is equal to zero, in the second that b is greater than zero or positive, in the third that b is smaller than zero or negative.

Now come the elementary operations. If (a_ν) and (a_ν') are two *fundamental series* by means of which the numbers b and b' are determined, it then turns out that

$$(a_\nu \pm a'_\nu)$$

and

$$(a_\nu \cdot a_\nu')$$

are also *fundamental series*, which thus determine three new numbers. These serve as definitions for the sum and difference

$$b \pm b'$$

and the product

$$b \cdot b'.$$

If in addition b is different from zero, the definition of which has been given above, then it can be proved that

$$\left(\frac{a'_\nu}{a_\nu}\right)$$

is also a *fundamental series* whose associated number provides the definition for the quotient

$$\frac{b'}{b}.$$

The elementary operations between a number b given by a fundamental series (a_ν) and a directly given rational number a are included in the operations just established, by letting

$$a_\nu' = a,\ b' = a$$

Only now come the definitions of the equality, the being-smaller, and the being-greater of two numbers b and b' (of which b' can also equal a). In particular we say that

$$b = b'$$

or

$$b > b'$$

or

$$b < b',$$

according to whether

$$b - b'$$

is equal to zero or greater than or smaller than zero.

After all these preparations we get as the first *rigorously provable* theorem that if b is the number determined by a fundamental series (a_ν), then, with increasing ν

$$b - a_\nu$$

will become smaller in absolute terms than any conceivable rational number, or, what is the same, that

$$\operatorname*{Lim}_{\nu=\infty} a_\nu = b.$$

It would be well to observe this cardinal point, whose significance could easily be overlooked: in the case of the *third* form of definition it is not at all true that the number b is defined as the "limit" of the members a_ν of a fundamental series (a_ν). This would be a logical mistake similar to that pointed out in the discussion of the *first* form of definition, for the reason that then the *existence* of the limit

$$\operatorname*{Lim}_{\nu=\infty} a_\nu$$

would be presumed. Rather, the opposite is the case, so that by means of our preceding definitions the concept b has been furnished with properties and with relations to the rational numbers such that it can be concluded with logical certainty:

$$\operatorname*{Lim}_{\nu=\infty} a_\nu$$

exists and is equal to b. May I be forgiven my thoroughness, which I motivate with the perception that most people pass by this unpretentious detail and then easily get entangled in doubts and contradictions with respect to the irrational which, by observing the particulars emphasized here, they could have been spared entirely, for they would then recognize clearly that the irrational number, by virtue of the *characteristics given to it by the definitions*, is just as definite a reality in our mind as the rational number, even as the whole rational number, and that one need not first *obtain* it by a limiting process but on the contrary — through its *possession* one is convinced of the feasibility and evident admissibility of the limiting processes. *(8)* For now the just-adduced theorem is easily extended to yield the following: If (b_ν) is any aggregate of rational or irrational numbers such that

$$\lim_{\nu=\infty} (b_{\nu+\mu} - b_\nu) = 0,$$

(whatever μ may be),

then there is a number b determined by a fundamental series (a_ν) such that

$$\lim_{\nu=\infty} b_\nu = b\,.$$

Thus it turns out that the same numbers b, which on the basis of fundamental series (a_ν) (I call these fundamental series of the *first* order) are defined in such a way that they prove to be limits of a_ν, are also in manifold ways representable as limits of series (b_ν), where each b_ν is defined by a fundamental series of the first order

$$(a_\mu^{(\nu)}) \text{ (with fixed } \nu).$$

I therefore call such an aggregate (b_ν), if it has the property that

$$\lim_{\nu=\infty} (b_{\nu+\mu} - b_\nu) = 0 \text{ (for arbitrary } \mu)$$

a fundamental series of the *second* order.

Similarly, fundamental series of the *third, fourth,...nth order, and fundamental series of the α th* order can also be formed, where α is an arbitrary number of the second number-class.

All these fundamental series accomplish exactly the same thing for the determination of a real number b as the fundamental series of the *first* order, the only difference consisting of the more complicated and broader form in which they are given. It nonetheless seems to me highly appropriate, provided that one wants to assume the standpoint of the third form of definition at all, to fix this difference in the form noted, as I have done in similar fashion in the cited works (*Mathematische Annalen*, Vol. V, p. 123). Therefore, I now use the following mode of expression: the numerical magnitude b is given by a fundamental series of the n th or, respectively, the α th order. If we decide upon this, we achieve an extraordinarily free-flowing and simultaneously comprehensible language, enabling us to describe the richness of the multiform and often so complicated webs of analysis in the most simple and distinctive manner, through which, in my opinion, a gain in clarity and transparency is attained which should not be underestimated. In this I oppose the misgivings which Herr Dedekind voiced in the preface to his essay *Continuity and Irrational Numbers (Stetigkeit und Irrationale Zahlen)* concerning these distinctions. It was the farthest thing from my mind to introduce through the fundamental series of the second, the third order, etc., *new* numbers which are not already determinable through fundamental series of the first order; rather, I was merely focusing on the conceptually distinct forms of the being-given (*des Gegebenseins*) of the numbers. This clearly flows from particular parts of my paper itself.

In regard to this I would like to call attention to a remarkable circumstance. These orders of fundamental series, distinguished by me through numbers of the first and second number-classes, exhaust any and all conceivable forms of the usual series-character — whether analysis has already discovered them or not — in the sense that fundamental series, the number of whose order might be denoted by a number of the third number-class, actually do not exist, as I shall rigorously prove on a different occasion.

Now I will attempt to explain in brief the appropriateness of the *third* form of definition.

To denote the fact that a number b is given on the basis of a fundamental series (c_ν) of any order n or α, I will use the formulas

$$b \sim (c_\nu)$$

or

$$(c_\nu) \sim b.$$

If, for example, a convergent series with the general member c_ν is given, then the necessary and sufficient condition for convergence (as is well-known) is:

$$\lim_{\nu=\infty} (c_{\nu+1} + \cdots + c_{\nu+\mu}) = 0$$

(for arbitrary μ).

Thus the sum of the series is defined through the formula

$$\sum_{n=0}^{\infty} c_n \sim \left(\sum_{n=0}^{\nu} c_n\right).$$

If, for example, all c_n are defined on the basis of fundamental series of the k th order, then the same is true for

$$\sum_{n=0}^{\nu} c_n$$

and we meet here with the sum

$$\sum_{n=0}^{\infty} c_n$$

as defined by a fundamental series of the (k+1)th order.

If, for example, the thought-content of the proposition

$$\sin\left(\frac{\pi}{2}\right) = 1$$

is to be described, we could think of $\frac{\pi}{2}$ and its powers as given by the formulas:

$$\frac{\pi}{2} \sim (a_\nu), \quad \left(\frac{\pi}{2}\right)^{2m+1} \sim (a_\nu^{2m+1}),$$

where for purposes of abbreviation we put

$$2 \sum_{n=0}^{\nu} \frac{(-1)^n}{2n+1} = a_\nu.$$

Furthermore, we have

$$\sin\left(\frac{\pi}{2}\right) \sim \left(\sum_{m=0}^{\mu} (-1)^m \frac{\left(\frac{\pi}{2}\right)^{2m+1}}{(2m+1)!}\right),$$

i.e.,

$$\sin\left(\frac{\pi}{2}\right)$$

is defined on the basis of a fundamental series of the second order, and by means of that proposition is expressed, therefore, the equality of the rational number 1 and of a number

$$\sin\left(\frac{\pi}{2}\right)$$

given on the basis of a fundamental series of the second order.

In similar fashion the thought-content of more complicated formulas — as, for example, those of the theory of theta-functions — can be described with precision and relative ease, whereas the reduction of infinite series to series formed solely of rational members, and in particular of members with the same sign throughout, and which converge absolutely, is generally extremely involved. Here in the case of the *third* form of definition, in contrast to the *first*, this matter is entirely avoided, so long as we are dealing not with numerical approximations of sums of series through rational numbers, but only with absolutely sharp *definitions* of the latter. To me, the *first* form of definition indeed appears not to be as easily usable, if what is at issue is the precise definition of the sums of series, which do not converge absolutely; for which, on the contrary, the arrangement of both its positive and its negative numbers is definitely prescribed. However, even for series with absolute convergence, it will be possible to actually establish the sum (even though the latter is independent of the arrangement) only if a definite arrangement is given. Therefore even in such cases it is tempting to give preference to the *third* form of definition over the first. Finally, it seems to me that its capacity for generalization to the *case of transfinite* numbers speaks for the *third* form of definition, while such an extension of the *first* form of definition is entirely *impossible*. The difference lies simply in this, that for transfinite numbers the commutative law is in general already invalid for addition; the first form of definition, however, is *inseparably bound up* with this law — it stands and falls with it. However, for all types of numbers where the commutative law of addition is valid, the *first* form of definition (with the exception of the points referred to) proves to be quite outstanding.

Section 10

The concept of the "continuum" has not only played a significant role in every aspect of the development of science, but also has always called forth the greatest differences of opinion and even vehement controversies. This may be due to the fact that the idea upon which this concept is based has taken on a different content in its appearance for the dissenting, for the reason that the exact and complete definition of the concept was not transmitted to them. Perhaps, however — and this is to me the most probable reason — the Greeks who may have first grasped the idea of the continuum had already not conceived of it with the clarity and completeness which would have been required to preclude the possibility of different interpretations by those that followed them. So we see that Leucippus, Democritus, and Aristotle view the continuum as a compound which consists *ex partibus sine fine divisibilibus* (of parts divisible without limit), whereas Epicurus and Lucretius compose (*zusammensetzen*) the continuum out of their atoms considered as finite things. From this there subsequently grew a great dispute among the philosophers, some of whom followed Aristotle, others Epicurus. Still others, in order to stay away from this dispute, decreed along with Thomas Aquinas (9) that the continuum consisted neither of infinitely many nor of a finite number of parts, but of *no parts at all*. This latter opinion seems to me to contain less of an explication than a tacit confession that one has not gotten to the bottom of the matter and prefers to genteelly get out of its way. Here we see the *medieval-scholastic origin* of a view which still finds advocates today, according to whom the continuum is an indivisible concept or else, as others express it, a pure *a priori* intuition (*Anschauung*) which is hardly accessible to determination through concepts. Every attempt at arithmetical determination of this *mysterium* is viewed as an impermissible intervention and rebuked with proper vehemence; thereby timid souls get the impression that we are not dealing with a *mathematical-logical* concept in the case of the "continuum," but rather with a *religious dogma*.

Far be it from me to conjure up these disputes again; also, there would be no room for a more exact discussion of them in this narrow framework. I see myself obliged only to develop the concept of the continuum here as briefly as possible, in as logically sober a fashion as I must grasp it and as I need it in the theory of manifolds, and, also, only in respect to the *mathematical* theory of aggregates. This treatment was not so easy for me, for among mathematicians whose authority I like to call upon, not a single one has dealt closely with the continuum in the sense that I am in need of here.

Indeed, taking one or several real or complex continuous magnitudes (or, what I take to be the more correct expression, continuous sets of magnitudes) as a basis, the concept of a continuum depending on them either univocally or multivocally — i.e., the concept of a continuous function — has been shaped out in the best possible way and in the most varied directions. In this way the theory of the so-called *analytic* functions, as well as of the more general functions with their highly remarkable characteristics (such as non-differentiability and similar things), has come into being. However, the *independent* continuum itself has merely been presupposed by the mathematical authors in that most simple manifestation and has not been subjected to any more thorough consideration.

First of all I must explain that, in my opinion, the enlistment of the *concept of time* or of the *intuition of time* in the discussion of the much more fundamental and more general concept of the continuum is *not* in order. It is my judgment that *time* is a notion (*Vorstellung*) which for its clear explication presupposes the concept of continuity, which is independent of it, and even with the aid of this cannot be grasped, either objectively as a substance or subjectively as a necessary a priori form of intuition. Rather time is nothing but an *auxiliary and relational concept* by means of which the relation between different motions occurring in nature and perceived by us is determined. Such a thing as *objective* or *absolute time* exists nowhere in nature, and therefore *time* cannot be regarded as a measure of *motion*. Rather, in fact, could the latter be regarded as a measure of *time*, if that were not objectionable on the grounds that *time*, even in the unassuming role of a *subjectively necessary a priori* form of intuition, has not exactly experienced prosperous and undisputed success, even though since Kant it would not have lacked the time to do so.

Similarly, I am convinced that absolutely nothing can be made of the so-called *form of intuition* of *space* to gain insight into the continuum, since with *space*, too, much as with the objects thought of as contained in it, it is only with the help of a conceptually already *completed* continuum that the kind of contents can be achieved through which they can become the object not of mere aesthetic contemplation, or of philosophical sharp-wittedness, or of inaccurate comparisons, but of sober and exact mathematical investigations.

Thus I am left with no choice but to attempt, with the aid of the real number concepts defined in Section 9, as general as possible a definition of a purely arithmetical concept of a point-continuum. As a basis for this I choose — what else? — the n-dimensional plane of *arithmetical* space G_n, i.e., the embodiment (*Inbegriff*) of all value-systems

$$(x_1 \mid x_2 \mid \cdots \mid x_n),$$

in which every x, independently of the others, can take on *all real* number-values from $-\infty$ to $+\infty$. Every particular such value system I call an *arithmetical* point of G_n. The distance of two such points is defined by the expression

$$+\sqrt{(x'_1 - x_1)^2 + (x'_2 - x_2)^2 + \cdots + (x'_n - x_n)^2}$$

and by an *arithmetical* point aggregate P contained in G_n, we understand any lawfully given embodiment of points of the space G_n. The investigation thus aims to devise a sharp and at the same time as general as possible a definition of *when P is to be called a continuum*.

I have proved in *Crelle's Journal* (Vol. 84, p. 242) that all spaces G_n, no matter how large the so-called number of dimensions n may be, have the *same* power and consequently have the *same power* as the linear continuum, thus for example as the totality of all real numbers of the interval $(0 \ldots 1)$. The investigation and determination of the power of G_n therefore reduces to the same question for the special case of the interval (0...1), and I hope that already quite soon I will be able to answer this question, by means of a rigorous proof, to the effect that the sought-after power is none other than that of our *second number-class* (II). From this it will follow that all infinite point aggregates P either have the power of the first number-class (I) or the power of the second number-class (II). It will also be possible to draw from this the further consequence, that the totality of all functions of one or several variables which are represented by a preassigned infinite series-form, no matter which, *also only* possesses the power of the second number-class and therefore is *countable by means of* numbers of the third number-class (III). *(10)* This theorem will therefore be applicable to, for example, the totality of all "analytic" functions, i.e. functions of one or several variables generated through continuation of convergent power series, or the set of all functions of one or several real variables which are representable by trigonometric series.

In order to close in now on the general concept of a continuum contained in G_n, let us recall the concept of the derivative (*Ableitung*) $P^{(1)}$ of an arbitrarily given point aggregate P, first found in the paper in *Mathematische Annalen*, Vol. 5, and here developed further and expanded into the concept of a derivative $P^{(\gamma)}$, where γ can be any whole number of one of the number-classes (I), (II), (III), etc.

It is also possible now to divide the point aggregate P into two classes according to the power of their first derivative $P^{(1)}$. If $P^{(1)}$ has the power of (I), then it turns out (as I have already said in Section 3 of this paper), that there exists a first whole number α of the *first* or *second* number-class (II) for which $P^{(\alpha)}$ vanishes. If, however, $P^{(1)}$ has the power of the second number-class (II), then it is always possible, in one and only one fashion, to partition $P^{(1)}$ into two aggregates R and S such that

$$P^{(1)} = R + S,$$

where R and S have an extremely different character:

R is such that through the repeated derivation process it is capable of continued reduction up to the point of annihilation, so that there always exists a first whole number γ of the number-classes (I) or (II) for which

$$R^{(\gamma)} = 0;$$

I call such point aggregates *reducible.*

S, on the other hand, is such that in the case of this point aggregate the process of derivation produces absolutely no change, in that

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

and consequently also

$$S \equiv S^{(\gamma)} \quad ;$$

this kind of aggregate S I call *perfect* point aggregates. Thus we can say: if $P^{(1)}$ is of the power of the second number-class (II), then $P^{(1)}$ decomposes into a definite *reducible* and a definite *perfect* point aggregate.

Even though these two predicates, "reducible" and "perfect," cannot occur jointly in one and the same point aggregate, it is still, however, not the case that "irreducible" is the same as "perfect," just as "imperfect" is not exactly the same as "reducible," as a certain amount of attention will easily show.

The *perfect* point aggregates S are by no means always in their interior what I have called in my above-mentioned papers "everywhere dense." Therefore they are not yet by themselves suitable for a complete definition of the point continuum, even if it must be admitted immediately that the latter must always be a *perfect* aggregate.

Rather, an additional concept is required in order to define the continuum jointly with the previous one, viz. the concept of a *connected* (*zusammenhängend*) point aggregate T.

We call T a *connected* point aggregate if for any two points t and t' of the latter and for a pre-assigned arbitrarily small number ε, there always exists a *finite* number of points

$$t_1, \ t_2, \ \cdots \ t_\nu$$

of T in multiple ways, so that the distances

$$tt_1, \ t_1t_2, \ t_2t_3, \ \cdots \ t_\nu t'$$

are all smaller than ε.

All known geometrical point continua now also fall under this concept of the *connected* point aggregate, as is easily seen. I also believe, however, that with these *two* predicates, "perfect" and "connected," I have come across the necessary and *sufficient* characteristics of a point continuum, and I therefore define a point continuum within G_n as a *perfectly-connected aggregate*. *(12)* Here "perfect" and "connected" are not mere words, but entirely general predicates of the *continuum*, conceptually characterized in the sharpest possible manner by the preceding definitions.

The Bolzano definition of the continuum (*Paradoxes*, Section 38) is certainly incorrect; it one-sidedly expresses only *one* property of the continuum, which is however also satisfied by aggregates which result from G_n if one imagines any "isolated" point aggregate (cf. *Mathematische Annalen*, Vol. 21, p. 51) as removed from G_n. Similarly, it is satisfied in the case of aggregates which consist of several separate continua; clearly in such cases no continuum is present, even though according to Bolzano this would be the case. Thus we see here a violation of the maxim: "To the essence of any thing belongs that which when given necessarily posits the thing and when subtracted necessarily annihilates it; or that without which the thing — or vice versa, that which without the thing — can neither be nor be conceived."

Similarly, it also appears to me that in the essay of Herr Dedekind (*Continuity and Irrational Numbers*) only one *different* property of the continuum has been stressed one-sidedly, viz. the property which it shares with *all* "perfect" aggregates.

Section 11

It shall now be demonstrated how one is led to the definitions of the new numbers, and in what fashion the natural segments in the absolutely infinite real whole number-sequence, which I call *number-classes,* come about. To this discussion I will then add only the principal theorems about the *second* number-class and its relationship to the first. The sequence (I) of the positive real whole numbers

$$1, 2, 3, \cdots, \nu, \cdots$$

has the basis of its generation in the repeated positing and uniting of basic units which are regarded as equal; the number ν is the expression both for a definite finite number of such consecutive positings, as well as for the unification of the posited units into a whole. The formation of the finite whole real numbers thus rests on the principle of the addition of a unit to an existing, already formed number. I call this moment, which, as we shall see presently, also plays an essential role in the generation of the higher whole numbers, the *first principle of generation*. The number (*Anzahl*) of the numbers ν of class (I) to be formed in this way is infinite, and there is no greatest one among them. As contradictory as it would be, therefore, to speak of a greatest number of class (I), there is, on the other hand, nothing objectionable in conceiving of a *new* number — we shall call it ω — which is intended to be the expression for the fact that the totality (I) as a whole be given in its natural and lawful succession (similar to the way in which ν is an expression for the fact that a certain finite number of units is unified into a whole). It is even permissable to think of the newly created number ω as a *limit* toward which the numbers ν tend, if by that nothing else is understood than that ω is to be the *first* whole number which follows all the other numbers ν, i.e., is to be called greater than every one of the numbers ν. By letting the positing of the number ω be followed by further positings of unity we obtain, with the help of the *first* principle of generation, the further numbers

$$\omega + 1, \omega + 2, \cdots \omega + \nu, \cdots;$$

since here again no greatest number is reached, we conceive of a new one, which can be called 2ω and which is to be the first number following all previous numbers ν and $\omega + \nu$. If the *first* principle of generation is applied repeatedly to the number 2ω, then we arrive at the continuation

$$2\omega + 1, 2\omega + 2, \cdots 2\omega + \nu, \cdots$$

of the previous numbers.

The logical function which has given us the two numbers ω and 2ω is obviously different from the *first* principle of generation; I call it the *second principle of generation* of whole real numbers, and define it more closely to the effect that if any definite succession of defined whole real numbers is given of which there is no greatest, then on the basis of this second principle of generation a new number is created, which can be thought of as a *limit* of those numbers, i.e. can be defined as the next greater number to all of them.

By combined application of the two principles of generation one thus successively obtains the following continuations of the numbers attained by us so far:

$$\begin{array}{l} 3\omega, 3\omega + 1, \cdots, 3\omega + \nu, \cdots \\ \cdots\cdots\cdots\cdots\cdots\cdots \\ \mu\omega, \mu\omega + 1, \cdots, \mu\omega + \nu, \cdots \\ \cdots\cdots\cdots\cdots\cdots\cdots \end{array}$$

However, even this does not bring the matter to a close, since of the numbers

$$\mu\omega + \nu$$

likewise none is the greatest.

The second principle of generation therefore induces us to introduce a next-following number to all the numbers

$$\mu\omega + \nu$$

which can be called ω^2. This is followed in definite succession by numbers

$$\lambda\omega^2 + \mu\omega + \nu,$$

and then, by adhering to the two principles of generation, we arrive at numbers of the following form:

$$\nu_0 \omega^\mu + \nu_1 \omega^{\mu-1} \cdots + \nu_{\mu-1}\omega + \nu_\mu;$$

but then immediately the second principle of generation drives us to the positing of a new number which is to be the next greater to all of these numbers and is appropriately denoted by

$$\omega^\omega$$

As can be seen, there is no end to the formation of new numbers; in adhering to the *two* principles of generation we again and again obtain new numbers and number series of an entirely determinate succession.

Therefore it at first *appears* as if in this mode of formation of new, whole, determinately infinite numbers we would have to lose ourselves in the *unlimited*, and that we would be incapable of bringing this endless process to a *certain preliminary* close, through which to gain a similar limitation to that which, with respect to the older number-class (I), in a certain sense actually existed. There we only made use of the *first* principle of generation and consequently stepping out of the series (I) was impossible. The *second* principle of generation, however, not only had to lead beyond the number field given up to now, but indeed proves itself to be a means which, in conjunction with the *first* principle of generation, provides the capacity to *break through every boundary* in the concept formation of the real whole numbers.

If we now recognize, however, that all numbers obtained so far and the ones initially following them satisfy a certain condition, then this condition, when put forth as a demand to be met by all numbers to be formed initially, proves to be a new *third* principle, joining the other two, which I call the *inhibiting* or *limiting principle* and which, as I shall show, has the effect that the second number-class (II), in whose definition it is utilized, not only gets a higher power than (I), but in fact exactly the *next higher*, i.e., *second power*.

The mentioned condition, which, one immediately convinces oneself, is satisfied by all of the infinite numbers α defined so far, is — that the aggregate of numbers preceding this number in the number sequence is of the *power of the first number-class (I)*. If, for example, we take the number

$$\omega^\omega,$$

then the numbers preceding it are contained in the formula:

$$\nu_0 \omega^\mu + \nu_1 \omega^{\mu-1} + \cdots + \nu_{\mu-1}\omega + \nu_\mu,$$

where

$$\mu, \nu_0, \nu_1, \cdots \nu_\mu$$

have to take on all finite, positive, whole number-values including zero and excluding the combination:

$$\nu_0 = \nu_1 = \cdots = \nu_\mu = 0.$$

As is well known, this aggregate can be put into the form of a simply infinite series and thus has the power of (I).

Since furthermore every sequence of aggregates, of which each is of the *first* power, always yields another aggregate which has the power of (I), it is clear that in the continuation of our number sequence we *actually initially again and again get only such numbers* for which that condition is *in fact* satisfied.

We thus define the second number-class (II) as the *totality of all numbers α capable of being formed with the aid of the two principles of generation and progressing in definite succession:*

$$\omega, \omega + 1, \cdots, \nu_0\omega^\mu + \nu_1\omega^{\mu-1} + \cdots + \nu_{\mu-1}\omega + \nu_\mu, \cdots, \omega^\omega, \cdots, \alpha \cdots$$

which are subject to the condition that all numbers preceding α, from 1 on, form an aggregate of the power of the first number-class (I).

Section 12

The first thing we now have to demonstrate is the theorem *that the new number-class (II) has a power which is different from that of the first number-class (I).*

This theorem results from the following theorem: "If

$$\alpha_1, \alpha_2, \cdots, \alpha_\nu, \cdots$$

is any aggregate of the first power of different numbers of the *second* number-class, so that we are justified in taking it as given in the simple series form (α_ν), then either one of these numbers, say γ, is the greatest. Or if this is not the case, then there exists a definite number β of the second number-class (II) not occurring among the numbers α_ν so that β is greater than all α_ν while on the other hand every whole number $\beta' < \beta$ is exceeded in size by certain numbers of the series (α_ν). The numbers γ or β respectively can properly be called the upper limit of the aggregate (α_ν)."

The proof of this theorem is simply the following: Let $\alpha_{\varkappa_2}$ be the first number occurring in the series (α_ν) which is greater than α_1, $\alpha_{\varkappa_3}$ the first occurring

number greater than $\alpha_{\varkappa_2}$, and so on.
We then have

$$1 < \varkappa_2 < \varkappa_3 < \varkappa_4 < \cdots$$
$$\alpha_1 < \alpha_{\varkappa_2} < \alpha_{\varkappa_3} < \alpha_{\varkappa_4} < \cdots$$

and

$$\alpha_\nu < \alpha_{\varkappa_\lambda},$$

as long as

$$\nu < \varkappa_\lambda.$$

Now here it can happen that from a certain number $\alpha_{\varkappa_\varrho}$ on, all following numbers in the series (α_ν) are smaller. Then obviously this number is the greatest one of all, and we have: $\gamma = \alpha_{\varkappa_\varrho}$. Alternatively, think of the aggregate of all whole numbers from 1 on which are smaller than α_1, add to this aggregate first the aggregate of all whole numbers $\geqq \alpha_1$ and $< \alpha_{\varkappa_2}$, then the aggregate of all numbers which

$$\geqq \alpha_{\varkappa_2}$$

and

$$< \alpha_{\varkappa_3}$$

and so on. In that way is obtained a specific section of successive numbers of our first two number-classes, and, in particular, this number aggregate is obviously of the *first* power. Thus there exists (according to the definition of (II)) a definite number β of the totality (II), which is the next greater one for those numbers. Hence

$$\beta > \alpha_{\varkappa_\lambda}$$

and therefore also

$$\beta > \alpha_\nu,$$

since

$$\varkappa_\lambda$$

can always be taken great enough so that it becomes greater than a pre-assigned ν and since at that point

$$\alpha_\nu < \alpha_{\varkappa_\lambda}.$$

On the other hand it can easily be seen that every number

$$\beta' < \beta$$

is exceeded in size by certain numbers

$$\alpha_{k_\nu},$$

and with this all parts of the theorem have now been proved.

From this now follows the theorem that the entirety of all numbers of the second number-class (II) does *not* have the power of (I). Otherwise we could think of the whole totality (II) in the form of a simple series

$$\alpha_1, \alpha_2, \cdots, \alpha_\nu, \cdots$$

which according to the just-proved theorem would either have a greatest number γ or else, respecting the size of all its members

$$\alpha_\nu$$

would be exceeded by a certain number β of (II). In the first case the number

$$\gamma + 1,$$

would belong to class (II); in the second case the number β would on the one hand belong to class (II) and, on the other hand, would not occur in the series

$$(\alpha_\nu)$$

which, given the presupposed identity of the aggregate (II) and

$$(\alpha_\nu)$$

is a contradiction. Consequently the number-class (II) has a *different* power than the number-class (I).

That of the two powers of the number classes (I) and (II) the second is really the *next following* one to the first, i.e., that between the two powers there exist no others, follows with certainty from a theorem which I shall state and prove presently.

However, if we first take a look backwards and recall the means which led both to an extension of the real whole number concept and also to a new power, different from the first, of well-defined aggregates, there were three logical moments, standing out and distinguishable from one another, which were operative. They are the *two principles of generation* defined above and in addition an *inhibiting* or *limiting principle*, which consists of the demand to undertake the creation of a new whole number with the help of one of the other two principles *only when* the entirety of all preceding numbers has the power of a defined number-class already *existing* to its full extent.

Thus by observing these three principles, one can most reliably and conclusively arrive at ever new number-classes and, along with them, at all the different, successively ascending powers existing in corporeal and mental nature, and the new numbers so obtained will then always be of utterly the same concrete determinateness and objective reality as the earlier ones. I therefore truly do not know what should keep us from this activity of forming new numbers, as soon as it is demonstrated that introducing a new one of these innumerable number-classes into consideration has become desirable or even indispensable for the progress of science.

Section 13

I now come to the promised proof that the powers of (I) and (II) immediately follow each other so that no other powers lie in between.

If from the totality (II) one selects according to an arbitrary law an aggregate (α') of different numbers α', i.e., conceives of an arbitrary aggregate (α') contained in (II), then such an aggregate always has characteristics which may be expressed in the following theorems:

"Among the numbers of the aggregate (α') there is always a *smallest* one."

"If in particular we have a sequence of numbers of the totality (II):

$$\alpha_1, \alpha_2, \cdots, \alpha_\beta, \cdots,$$

which constantly decrease in size (so that

$$\alpha_\beta > \alpha_{\beta'}$$

when

$$\beta' > \beta),$$

then this series necessarily breaks off at a finite number of members and closes with the smallest of the numbers; the series can not be an infinite one."

It is remarkable that this theorem — which is immediately clear if the numbers are finite whole numbers — can also be proved for infinite numbers α_β . Indeed, according to the previous theorem, which easily follows from the definition of the number-series (II), there exists a smallest number among the numbers α_ν , if one takes into account only those numbers for which the index ν is finite. If this, say,

$$= \alpha_\varrho,$$

then it is plausible that, because

$$\alpha_\nu > \alpha_{\nu+1},$$

the series α_ν and consequently also the whole series α_β must consist of exactly ϱ members, and therefore is a finite series.

One now obtains the fundamental theorem:

"If (α') is any number-aggregate contained in the totality (II), then only the following three cases can occur: either (α') is a finite totality, i.e. consists of a finite number (*Anzahl*) of numbers (*Zahlen*); or (α') has the power of the first class; or thirdly (α') has the power of (II); *Quartum non datur* (there is no fourth case)."

The proof can be carried out easily as follows: Let Ω be the first number of the *third* number-class (III); then all numbers α' of the aggregate (α') are smaller than Ω, since the latter is contained in (II).

Let us now imagine the numbers α' ordered according to their size. Letting α_ω be the smallest one among them,

$$\alpha_{\omega+1}$$

the next greater, and so on, one obtains the aggregate (α') in the form of a "well-ordered" aggregate α_β, where β runs through numbers of our natural extended number-series from ω on. Obviously here β always remains smaller than or equal to α_β and since

$$\alpha_\beta < \Omega,$$

therefore also

$$\beta < \Omega.$$

The number β thus cannot go beyond the number-class (II), but remains within its domain. Therefore only three cases can occur: either β remains below a specifiable number of the series

$$\omega + \nu,$$

and so (α') is a finite aggregate; or β takes on all values of the series

$$\omega + \nu$$

but remains below a specifiable number of the series (II), and so (α') is obviously an aggregate of the *first* power; or thirdly β also takes on arbitrarily large values in (II), and then β runs through *all* numbers of (II); in this case the totality (α_β), i.e. the aggregate (α'), obviously has the power of (II); q.e.d.

As the immediate result of the theorem just-proved we now have the following:

"Given any well-defined aggregate M of the power of the number-class (II) and any infinite partial aggregate M' of M, then either the totality M' can be thought of in the form of a simply infinite series, or it is possible to map the two aggregates M' and M onto each other in reciprocally univocal fashion."

"Given any well-defined aggregate M of the second power, a partial aggregate M' of M and a partial aggregate M'' of M', and knowing that the latter M'' can be mapped reciprocally univocally onto the first M, then the second M' can also always be mapped reciprocally univocally onto the first and thus also onto the third."

Because of its connection with the preceding theorems I state this last theorem here as based on the assumption that M has the power of (II). Obviously it is also correct when M has the power of (I). However, it seems to me highly remarkable and I therefore expressly stress the fact that this theorm has *general* validity no matter what the power of the aggregate M may be. I want to take this up more closely in a later treatise and there identify the peculiar interest which attaches to this general theorem.

Section 14

In conclusion I now want to consider the numbers of the second number-class (II) and the operations which can be carried out with them. On this occasion, however, I want to limit myself to the issues nearest at hand, while reserving the publication of more thorough investigations of the subject for a later date.

The operations of adding and multiplying I defined generally in Section 1, and I have shown that for the infinite whole numbers they are in general *not* subject to the commutative but are subject to the associative law. Therefore this is also true particularly for the numbers of the second number-class. As to the distributive law, it has general validity only in the following form:

$$(\alpha + \beta)\gamma = \alpha\gamma + \beta\gamma$$

(where

$$\alpha + \beta,\ \alpha,\ \beta$$

appear as multipliers), as is immediately recognized on the basis of inner intuition.

Subtraction can be considered from two points of view. If α and β are any two whole numbers,

$$\alpha < \beta\,,$$

then it can easily be seen that the equation

$$\alpha + \xi = \beta$$

always admits of one and only one solution for ξ, such that ξ will be a number from (I) or (II) if α and β are numbers from (II). Let this number be equal to

$$\beta - \alpha\,.$$

If, however, one considers the equation:

$$\xi + \alpha = \beta$$

then it turns out that frequently this equation can in no way be solved for ξ. Such is the case, for example, for the following equation:

$$\xi + \omega = \omega + 1.$$

However, even in those cases where the equation

$$\xi + \alpha = \beta,$$

can be solved for ξ it is often found that it is satisfied by infinitely many number-values of ξ. Of these different solutions, however, one will always be the smallest one.

For this smallest root of the equation

$$\xi + \alpha = \beta\,,$$

if the latter is soluble at all, we choose the designation

$$\beta_{-\alpha}$$

which thus is different in general from

$$\beta - \alpha,$$

a number which always exists if only

$$\alpha < \beta\,.$$

If furthermore between the whole numbers

$$\beta,\ \alpha,\ \gamma$$

there exists the equation

$$\beta = \gamma\alpha,$$

(where γ is the multiplier), then it is easily seen that the equation

$$\beta = \xi\alpha$$

has no solution for ξ other than

$$\xi = \gamma,$$

and in this case γ is denoted by $\frac{\beta}{\alpha}$

On the other hand, the equation

$$\beta = \alpha \xi$$

(where ξ is the multiplicand), if it can be solved for ξ at all, often has several and even infinitely many roots, of which, however, one is always the smallest one. Let this smallest root satisfying the equation

$$\beta = \alpha \xi,$$

if the latter is solvable at all, be denoted by $\frac{\beta}{\alpha}$

The numbers α of the second number-class are of two kinds: 1) such α for which there is a next-preceding member in the series, which then is

$$\alpha_{-1}$$

these I call numbers of the *first* kind; 2) such α for which there does not exist a next-preceding member in the series, for which, therefore α_{-1}

does not exist; these I call numbers of the *second* kind. The numbers

$$\omega,\ 2\omega,\ \omega^{\nu} + \omega,\ \omega^{\omega}$$

for example, are of the second kind, whereas

$$\omega + 1,\ \omega^2 + \omega + 2,\ \omega^{\omega} + 3$$

are of the first kind.

In accordance with this the *prime numbers* of the second number-class, which I defined in general in Section 1, are also divided into prime numbers of the second and of the first kind.

Prime numbers of the second kind, in the order of their occurrence in the number-class (II), are the following:

$$\omega,\ \omega^{\omega},\ \omega^{\omega^2},\ \omega^{\omega^3},\ \cdots,$$

so that among all numbers of the form

$$\varphi = \nu_0 \omega^{\mu} + \nu_1 \omega^{\mu-1} + \cdots + \nu_{\mu-1}\omega + \nu_{\mu}$$

there exists only the *one* prime number of the *second* kind. It should not, however, be concluded from this relatively sparse distribution of the prime numbers of the second kind that the totality of all of them has a power less than the number-class (II) itself. It is found that this totality has the same power as (II).

The prime numbers of the first kind are first of all

$$\omega + 1,\ \omega^2 + 1,\ \cdots,\ \omega^{\mu} + 1,\ \cdots.$$

These are the only prime numbers of the first kind which occur among the number just designated by φ; the entirety of all prime numbers of the first kind in (II) also has the power of (II).

The prime numbers of the second kind have a quality which gives them a rather uncommon character. If η is such a prime number (of the second kind), then always

$$\eta\, \alpha = \eta$$

when α is any number smaller than η. From this it follows that if α and β are any two numbers both of which are smaller than η, then the product $\alpha\beta$ is always smaller than η.

Limiting ourselves at first to the numbers of the second number-class which are of the form φ, we find for these numbers the following rules of addition and multiplication. Let

$$\varphi = \nu_0 \omega^{\mu} + \nu_1 \omega^{\mu-1} + \cdots + \nu_{\mu},$$
$$\psi = \varrho_0 \omega^{\lambda} + \varrho_1 \omega^{\lambda-1} + \cdots + \varrho_{\lambda},$$

where we assume ν_0 and ϱ_0 to be different from zero.

Addition.

1) If

$$\mu < \lambda,$$

then we have

$$\varphi + \psi = \psi.$$

2) If

$$\mu > \lambda,$$

then we have

$$\varphi + \psi = \nu_0 \omega^{\mu} + \cdots + \nu_{\mu-\lambda-1}\omega^{\lambda+1} + (\nu_{\mu-\lambda} + \varrho_0)\,\omega^{\lambda} + \varrho_1 \omega^{\lambda-1} + \varrho_2 \omega^{\lambda-2} + \cdots + \varrho_{\lambda}.$$

3) For $\mu = \lambda$:

$$\varphi + \psi = (\nu_0 + \varrho_0)\,\omega^{\lambda} + \varrho_1 \omega^{\lambda-1} + \cdots + \varrho_{\lambda}.$$

Multiplication.

1) If ν_{μ} is different from zero, then we have

$$\varphi\psi = \nu_0\omega^{\mu+\lambda} + \nu_1\omega^{\mu+\lambda-1} + \cdots + \nu_{\mu-1}\omega^{\lambda+1} + \nu_\mu\varrho_0\omega^\lambda + \varrho_1\omega^{\lambda-1} + \cdots + \varrho_\lambda.$$

If

$$\lambda = 0,$$

then the last member on the right is

$$\nu_\mu\varrho_0.$$

2) If

$$\nu_\mu = 0,$$

then we have

$$\varphi\psi = \nu_0\omega^{\mu+\lambda} + \nu_1\omega^{\mu+\lambda-1} + \cdots + \nu_{\mu-1}\omega^{\lambda+1} = \varphi\omega^\lambda.$$

The decomposition of a number φ into its prime factors is the following. If we have

$$\varphi = c_0\omega^\mu + c_1\omega^{\mu_1} + c_2\omega^{\mu_2} + \cdots + c_\sigma\omega^{\mu_\sigma},$$

where

$$\mu > \mu_1 > \mu_2 > \cdots > \mu_\sigma$$

and

$$c_0, c_1, \cdots c_\sigma$$

are positive, finite numbers different from zero, then

$$\varphi = c_0(\omega^{\mu-\mu_1} + 1)c_1(\omega^{\mu_1-\mu_2} + 1)c_2 \cdots c_{\sigma-1}(\omega^{\mu_{\sigma-1}-\mu_\sigma} + 1)c_\sigma\omega^{\mu_\sigma};$$

if we further imagine

$$c_0, c_1, \cdots c_{\sigma-1}\, c_\sigma$$

to be decomposed into prime factors according to the rules of the first number-class, then we have at that point the decomposition of φ into prime factors; for the factors

$$\omega^\varkappa + 1$$

and ω are, as remarked above, themselves prime factors. This decomposition of numbers of the form φ is uniquely determined, even with respect to the order of succession of the factors, if one abstracts from the commutability of the prime factors of the individual numbers c and if it is decreed that the last factor is to be a power of ω or equal to one and that ω may be a factor only in the last place. I will write about the generalization of this decomposition into prime factors to arbitrary numbers α of the second number-class (II) on a later occasion.

Author's Notes

On Section 1

(1) *Theory of manifolds.* With this word I designate a doctrinal concept (*Lehrbegriff*) which encompasses a great deal and which so far I have attempted to develop only in the specific form of an arithmetical or geometrical theory of aggregates. By a "manifold" or "aggregate" I generally understand every multiplicity which can be thought of as one, i.e., any totality of definite elements which by means of a law can be bound up into a whole, and I believe that in this I am defining something which is related to the Platonic **εἶδος** (*eidos*) or **ἰδέα,** (*idea*), as well as to that which Plato in his dialogue "Philebus or the Highest Good" calls **μικτόν** (*mikton*). He counterposes this to the **ἄπειρον,** (*apeiron*), i.e., the unlimited, indeterminate, which I call the non-genuine-infinite, as well as to the **πέρας** (*peras*), i.e., the limit, and explains it as an ordered "mixture" of the two latter. Plato himself indicates that these concepts are of Pythagorean origin; cf. A. Boeckh, *Philolaos des Pythagoreers Lehren (The Teachings of Philolaos, the Pythagorean)*, Berlin 1819.

On Section 4

(2) *Aristotle*. Compare Zeller's presentation in his great work, *Die Philosophie der Griechen (The Philosophy of the Greeks)*, third edition, part II, second section, pp. 393-403. Plato's conception of the infinite is an entirely different one than that of Aristotle; compare Zeller, part II, first section, pp. 628-646. Similarly I find points of contact for my conceptions in the philosophy of Nicolaus Cusanus. Cf. R. Zimmerman, *Der Cardinal Nicolaus von Cusa als Vorganger Leibnizens (The Cardinal Nicolaus of Cusa as a Precursor of Leibniz)*, Sitzungsberichte d. Wiener Akademie d. Wiss. Jahrg. 1852. I note the same thing with respect to Giordano Bruno, the successor of the man from Cusa. Cf. Brunnhofer, *Giordano Brunos Weltanschauung und Verhängnis (Giordano Bruno's World View and Tragic Fate)*, Leipzig 1882.

An essential difference, however, is that I conceptually fix the different gradations of the genuine infinite once and for all by means of number-classes (I), (II), (III), and so on, and only now consider the task not only to investigate the relations of the transfinite numbers mathematically, but also to identify and pursue them wherever they might occur in nature. There is no doubt in my mind that in this way we will get farther and farther ahead, never reaching an unsurmountable limit, but also attaining not even an approximate grasp of the absolute. The absolute can only be acknowledged, but never known, not even approximately. For just as within the first number-class (I) for every finite number, no matter how great, we are always confronted by the same power of the finite numbers greater than it, in the same way every transfinite number, no matter how large, of any of the higher number-classes (II) or (III) and so on is followed by a totality of numbers and number-classes which has not suffered the least in power in comparison to the whole of the absolutely infinite totality of numbers starting at 1. This is a situation similar to what Albrecht von Haller says of eternity: "I subtract it (the enormous number) and you (eternity) still lie in front of me in your entirety."

The absolutely infinite number-sequence therefore appears to me in a certain sense as an appropriate symbol of the absolute; whereas the infinity of the first number-class (I), which up until now has alone served this purpose, seems to me in comparison like an entirely insignificant nothing, not in the least, because I regard it as a comprehensible idea (not notion) (*Vorstellung*). I also regard it as noteworthy that each of the number-classes, and hence each of the powers, is associated with an entirely determinate number of the absolutely infinite totality of numbers, and in particular, in such a way that for every transfinite number γ there exists a power which is to be called the γth one. Thus the different powers, too, form an absolutely infinite sequence. This is even more striking, because the number γ, which indicates the order of a power (provided that for the number γ there exists an immediately preceding one), has a magnitude relative to the numbers of the number-class which have this power, whose smallness defies all description — and this the more so the greater γ is assumed to be.

On Section 5

(3) *determinari possunt*. I cannot ascribe any existence (*Sein*) to the indefinite, the variable, the non-genuine-infinite, in whatever form they may appear, for they are nothing but relational concepts or purely subjective notions (*Vorstellungen*) or intuitions (*Anschauungen*) (imaginationes), in no case adequate ideas. If therefore only the non-genuine-infinite was referred to in the proposition "*infinitum actu non datur*," then I could underwrite it, but the proposition would then be a purely tautological one. However, it seems to me that the meaning of this proposition in the cited sources is rather that it is intended to express the impossibility of the conceptual positing of a determinate infinity, and in this meaning I regard it as false.

On Section 7

(4) *Realists*. The positivist and realist standpoint of the infinite are discussed, for example, in Dühring, *Natürliche Dialektik* (*Natural Dialectics*) Berlin 1865, pp. 109-135, and in v. Kirchmann, *Katechismus der Philosophie* (*Catechism of Philosophy*), pp. 124-130. Compare also Ueberweg's remarks on Berkeley's "Treatise Concerning the Principles of Human Knowledge" in v. Kirchmann's *Philosophical Library*. I can only repeat that I essentially agree with all these authors on the evaluation of the non-genuine infinite; the point of difference lies only in this: that they regard this syncategorematic infinite as the *only* infinite which can be comprehended by means of "turns of expression" or concepts, and here even by mere relational concepts. Dühring's proofs against the genuine-infinite could be carried out with much fewer words and either amount to this, that the determinate finite number, no matter how great it is imagined to be, can never be an infinite one, which immediately follows from its concept; or to this, that the variable unlimitedly great finite number cannot be thought of with the predicate of determinateness nor thus with the predicate of existence (*Sein*), which again immediately follows from the essence of variability. There is no doubt in my mind that this does not make the slightest case against the conceivability of determinate transfinite numbers. Still, those proofs are regarded as proofs against the reality of transfinite numbers. This mode of argument seems to me similar to an attempt to prove that there is no red from the fact that there are infinitely many intensities of green. It is indeed remarkable, however, that Dühring on p. 126 of his text himself admits that there must be a reason for the explanation of the "possibility of unlimited synthesis," which he calls "understandably unknown." It seems to me that a contradiction lies in this.

Similarly, however, we also find that thinkers close to idealism or even fully subscribing to it deny any justification to the determinate-infinite numbers.

Chr. Sigwart in his excellent work, *Logik*, Vol. II, *Die Methodenlehre*, (The Doctrine of Method), Tübingen 1878, argues in a fashion quite similar to Dühring's and says on p. 47, "an infinite number is a contradiction *in adjecto.*"

Similar things are found in Kant and J.F. Fries; cf. the *System der Metaphysik* (*System of Metaphysics*), Heidelberg 1824, of the latter, in section 51 and section 52. Also the philosophers of the Hegelian School do not admit the genuine-infinite numbers; I would only mention the deserving work of K. Fischer, his *System der Logik und Metaphysik oder Wissenschaftslehre* (System of Logic and Metaphysics or Theory of Science), 2nd edition, Heidelberg 1865, p. 275.

On Section 8

(5) What I call here the "intrasubjective" or "immanent" reality of concepts or ideas will probably agree with the determination "adequate" in the sense in which this word is used by Spinoza when he says in *Ethics*, part II, def. IV: "*Per ideam adaequatam intelligo ideam, quae, quatenus in se sine relatione ad objectum consideratur, omnes, verae ideae proprietates sive denominationes intrinsecas habet.*" ("By an adequate idea I understand an idea which, insofar as it is considered in itself without relation to an object, has all the properties or inner characteristics of a true idea.")

(6) This conviction concurs essentially both with the principles of the Platonic system and also with an essential feature of Spinoza's system. In relation to the former I reference Zeller, *Philosophie der Griechen*, (Philosophy of the Greeks), 3rd edition, 2nd part, 1st section pp. 541-602. It says there, right at the beginning of the section, "Only conceptual knowledge (according to Plato) will provide true insight. To the extent, however, that our notions (*Vorstellungen*) are true — this premise Plato shares with others (Parmenides) — to the same extent their objects must be real, and vice versa. What can be known is, what cannot be known, is not, and to the same extent that something is, to that extent it is also knowable."

With respect ot Spinoza I need only call attention to his proposition in Ethics, pars II, prop. VII: "*ordo et connexio idearum idem est ac ordo et connexio rerum* (The ordering and connection of ideas is the same as the ordering and connection of things)."

The same epistemological principle can also be demonstrated in the philosophy of Leibniz. Only since modern Empiricism, Sensualism, and Skepticism, and the Kantian Criticism which arose from them, is it believed that the source of knowledge and certainty must be relocated into the senses or, at any rate, into the so-called pure forms of intuition of the world of the imagination (*Vorstellungswelt*) and must be limited to these forms. I am convinced that these elements by no means provide certain knowledge, since the latter can be obtained only by means of concepts and ideas which are at most stimulated by outer experience, but mainly are formed through inner induction and deduction as something which in a certain sense lay in us already and merely had to be awakened and brought to consciousness.

On Section 8 and Section 9

(7), (8) The procedure in the correct formation of concepts is, in my opinion, everywhere the same: a thing without qualities is posited, which at first is nothing but a name or a sign A, and to it are assigned different, even infinitely many understandable predicates in orderly fashion, predicates whose meaning as attaching to already existing ideas is known, and which must not contradict each other; in this way the relationships of A to the already existing concepts, and in particular to the cognate ones, are determined. Once this process is completely finished, then all the conditions for the awakening of the concept A, which has slumbered within us, exist and the concept comes into being ready-made, equipped with the intrasubjective reality which is the only thing that can be demanded of concepts anywhere. To establish its transient significance is then the task of metaphysics.

On Section 10

(9) Thomas Aquinas, *Opuscula*, XLII *de natura generis*, cap. 19 et 20; LII *de natura loci*; XXXII *de natura materiae et de dimensionibus interminatis*. Cf.: C. Jourdin, *La Philosophie d'Aquin*, pp. 303-329; K. Werner, *Der Heilige Thomas von Aquino*, Regensberg 1859, 2nd vol. pp. 177—201.

(10) Even the totality of all continuous but also of all integrable functions of one or several variables probably only has the power of the second number-class (II). However, if all restrictions are dropped and the totality of all continuous and discontinuous functions of one or several variables is considered, then this aggregate has the power of the third number-class (III).

(11) For perfect aggregates we can prove the theorem that they never have the power of (I).

As an example of a perfect point-aggregate which is not everywhere dense in any interval, no matter how small, I cite the totality of all real numbers contained in the formula

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

where the coefficients c_ν are to arbitrarily take on the two values 0 and 2 and where the series may consist of a finite as well as of an infinite number of numbers.

(12) Observe that this definition of the continuum is free from any reference to what is called the *dimension* of a continuous structure, for the definition also encompasses such continua as consist of connected pieces of different dimensions such as lines, planes, solids and so on. On a later occasion I want to show how one gets in orderly fashion from this general continuum to the more specialized continua of a definite dimension. I know very well that the word "continuum" so far has *not* taken on a *fixed* meaning in mathematics. My definition of it will therefore be judged by some as too *narrow*, by others as too *broad*; hopefully I have succeeded in this in finding the *correct mean*.

According to my conception, by a *continuum* one can *only* understand *perfect* and *connected* structures. Accordingly, for example, a straight line segment, which is lacking one or both endpoints, similarly a circular plane (disc) which does not include its boundary, are not *complete* continua; I call such point aggregates *semi-continua*.

In general I understand by a *semi-continuum* an *imperfect connected* point aggregate belonging to the *second class*, such that any two points of it can be connected by a *complete* continuum which is a part of the point-aggregate. Thus, for example, the space denoted by me by the letter $\mathfrak{A}$ in Math. Ann. Vol. 20, P. 119, which results from G_n by removal of any point-aggregate of the *first* power, is a *semi-continuum*.

The *derivative* of a connected point-aggregate is *always* a *continuum*, where it makes no difference whether the connected point-aggregate is of the *first* or *second* power.

If a connected point-aggregate is of the *first* power, I can call it *neither* a continuum *nor* a semi-continuum.

By means of the concepts which I put at the head of the theory of manifolds, I propose to investigate all the structures of algebraic as well as of transcendent geometry according to all their possibilities; and I do not expect the generality and acuity of the results to be exceeded by any other method.

Appendix

Excerpts from Riemann, "Fragments of a Philosophical Contents" in *The Collected Works of Bernhard Riemann*, edited by Heinrich Weber with the assistance of Richard Dedekind, Dover Publications, Inc., New York, N.Y., 1953. Translations by James Cleary and Uwe Parpart.

I. The work which I am primarily occupied with now is:

1. To introduce the imaginary into the theory of other transcendent functions in a manner similar to the way this has already been done with such great success for algebraic functions, exponential and cyclical functions, elliptical and Abelian functions; for this I have supplied the minimally necessary general preliminary studies in my inaugural dissertation.

2. Connected to this are new methods for the integration of partial differential equations which I have already successfully applied to several subjects in physics.

3. My main work concerns a new conception of the known laws of nature — an expression of these by means of different fundamental concepts — through which the utilization of experimental data about the mutual interaction between heat, light, magnetism and electricity for the purpose of investigating their relationship became possible. I was led to this mainly through the study of the works of *Newton, Euler,* and — on the other hand — *Herbart*. Concerning the latter, I was in almost complete agreement with Herbart's earliest investigations, the results of which are expressed in his doctoral and habilitation theses, (Oct. 22 and 23, 1802). However, I had to depart from the later course of his speculation on an essential point. Because of this, a difference is stipulated regarding his natural philosophy and those propositions of psychology which concern its connection with natural philosophy.

II. Antinomies

Thesis

Finite things, imaginable things.

1.

Finite time and space elements.

2.

Freedom, i.e., not the ability to initiate things in an absolute sense but to decide between two or more given possibilities.

In order that decision through free will be possible, in spite of the totally determinate laws of the action of the conceptions, one must assume that the psychic mechanism itself has or at least, in the course of its development, assumes the characteristic quality of bringing about the necessity of the latter.

3.

A God active in time
(world government)

4.

Immortality

Freedom is entirely compatible with the strict lawfulness of the course of nature. But the concept of a timeless God is not tenable alongside of it. Rather, the restriction which omnipotence and omniscience suffer through the freedom of the creatures in the sense presented above, must be removed through the acceptance of a God active in time, of a guide for the hearts and fates of men. The concept of providence must be expanded, and be partially replaced by the concept of world government.

Antithesis

Infinite things, conceptual systems which lie one the borders of the imaginable.

Continuous things.

Determinism.

No one engaged in action can give up the conviction that future will be co-determined through his actions.

A timeless, personal, all-knowing, all-powerful, all-good God (providence).

A thing in itself which is the basis of our transient phenomenal existence and is equipped with transcendental freedom, radical evilness and an intellectual character.

The General Relationship of the Conceptual Systems of the Thesis and Antithesis

The method, which Newton used for the founding of the infinitesimal calculus, and which has been recognized, since the beginning of this century, by the best mathematicians as the only one which supplies reliable results, is the method of limits. Instead of consisting of a continuous transition from one value of a magnitude to another one, or from one position to another one, or in general from one method of determination of a concept to another method, this method examines, first of all, a transition by way of a finite number of intermediate stages, and then it allows the number of these intermediate stages to increase in such a fashion that the intervals between two successive intermediate stages all decrease to the infinite.

The conceptual systems of the antithesis are conceptions that are firmly determined through negative predicates, however, they are not representable in a positive manner.

For the very reason that an exact and complete representation of these conceptual systems is impossible, they are inaccessible to direct investigation and molding by our reflection and deliberation. But they can be considered to be lying on the border of the representable, i.e., one can form a conceptual system which lies within the boundaries of the representable, and which through a mere change in the order of magnitude is transformed into the given conceptual system. Apart from the order of magnitude, the conceptual system remains unchanged in the transition to the limit. But in the limiting case itself, some of the correlative conceptions of the system lose their representability, and in particular, they are those which mediate the relationship between other concepts.

III. New Mathematical Principles of Natural Philosophy

Found on March 1, 1853

Although the title of this essay will hardly arouse a favorable predisposition in most readers, it nonetheless seemed to me to best express the overall tendency of the essay. Its purpose is to drive forward beyond the foundations laid by Galilei and Newton of astronomy and physics into the interior (essence) of nature. For astronomy, actually, this speculation cannot immediately have any practical use, but I hope this circumstance even in the eyes of the readers of this publication will cause no diminution in interest....

The basis for the general laws of motion for ponderables, which are found assembled at the beginning of Newton's *Principles*, lies in the inner state of the latter. Let us try to deduce it by way of analogy from our own inner (mode of) perception. New imagination-masses constantly arise in us and very rapidly disappear again from our consciousness. We observe a continuous activity of our soul. Every act of the latter is based upon something permanent, which on special occasions (through memory) gives note of itself as such, without exerting a lasting influence upon the appearances. Thus, constantly (with every thought-act) something permanent enters our soul, which, however, does not exert a lasting influence upon the world of appearances. Thus, every act of our soul is based upon something permanent, which, with this act, enters our soul, but at the same moment completely disappears from the world of appearances.

Guided by this fact, I make the hypothesis that the world-space is filled with a substance (*Stoff*), which constantly streams into the ponderable atoms and there disappears from the world of appearances (the world of bodies).

Both hypotheses can be replaced by the one that in all ponderable atoms, substance of the world of bodies constantly enters into the world of spirit (mind). The reason why the substance disappears there is to be sought in the spiritual (mind-) substance formed there immediately before (then), and hence the ponderable bodies are the place (point) where the world of spirit (mind) intervenes into the world of bodies.*

The effect of universal gravitation, the first thing which shall be explained from this hypothesis, is — as is well known — entirely determined for every part of space, if the potential function P of all ponderable masses for this part of space is given, or, which is the same, a place-function P which is such that the ponderable masses contained in the interior of a closed surface S are

$$\frac{1}{4\pi}\int \frac{\partial P}{\partial p}\, dS.$$

If now it is assumed that the space-filling substance is an incompressible homogeneous fluid without inertia, and always in equal times equal amounts, proportional to its mass, will stream into every ponderable atom, then obviously the pressure experienced by the ponderable atom [will be proportional to the velocity of the motion of the substance at the place of the atom(?)].

Thus, the effect of universal gravitation upon a ponderable atom can be expressed through and thought of as dependent upon the pressure of the space-filling substance in the immediate neighborhood of the atom.

From our hypothesis it follows necessarily that the space-filling substance must propagate the vibrations which we perceive as light and heat.

If we consider a simply polarized ray, call **x** the distance of an arbitrary point of the ray from a fixed starting point and y its elongation at time t, then, since the propagation speed of vibrations in a space free from ponderables is under all circumstances very near constant (equal to α), the equation:

$$y = f(x + \alpha t) + \phi(x - \alpha t)$$

must at least be very near being satisfied.

If it were strictly satisfied, we would have to have:

$$\frac{\partial y}{\partial t} = \alpha\,\alpha \int^{t} \frac{\partial^2 y}{\partial x^2}\, d\tau;$$

apparently, however, our experience can also be satisfied by the equation:

$$\frac{\partial y}{\partial t} = \alpha\,\alpha \int^{t} \frac{\partial^2 y}{\partial x^2}\, \phi\,(t - \tau)\, d\tau,$$

even if ϕ (t- τ) is not equal to 1 for all positive values of t- τ (with increasing t- τ decreases to infinity), as long as for a sufficiently large amount of time it remains very close to 1...

Let the position of substance-points at a definite time t be expressed by a rectilinear coordinate system, and let **x,y,z** be the coordinates of an arbitrary point *O*. Similarly, also with respect to a rectilinear coordinate system, let x',y',z' be the coordinates of the point O'. Then x',y',z' are functions of **x,y,z** and

$$ds'^2 = dx'^2 + dy'^2 + dz'^2$$

will be equal to a homogeneous second degree expression of **dx,dy,dz**. According to a well-known theorem, the linear expressions of **dx, dy, dz**

$$\alpha_1\, dx + \beta_1\, dy + \gamma_1\, dz = ds_1$$

$$\alpha_2\, dx + \beta_2\, dy + \gamma_2\, dz = ds_2$$

$$\alpha_3\, dx + \beta_3\, dy + \gamma_3\, dz = ds_3$$

---

*

In every instant, a fixed quantity of substance, proportional to the gravitational force, enters into every ponderable atom and vanishes there.

It is the consequence of the psychology established on Herbartian foundations that substantiality accrues not to the soul but to every individual conception *(Vorstellung)* formed inside of us.

now can always and in only one way be determined in such a way that

$$dx'^2 + dy'^2 + dz'^2 = G_1{}^2 ds_1{}^2 + G_2{}^2 ds_2{}^2 + G_3{}^2 ds_3{}^2,$$

while

$$ds^2 = dx^2 + dy^2 + dz^2 = ds_1{}^2 + ds_2{}^2 + ds_3{}^2.$$

The magnitudes G_1-1, G_2-1, G_3-1 are then called the main dilatations of the substance-particle at O in the transition from the former form to the latter; I label them $\lambda_1, \lambda_2, \lambda_3$.

I now assume that from the difference of the earlier form of the substance-particle from its form at time t there results a force which attempts to change this one (this form at t), and that the influence of an earlier form (*caeteris paribus*) will become the less the longer it occurred before t, and, in particular, such that from a certain limit on, all the earlier ones can be neglected. I further assume that those states which still exercise a detectable influence differ so little from that at time t that the dilatations can be regarded as infinitely small. The force which attempts to decrease $\lambda_1, \lambda_2, \lambda_3$ can then be regarded as linear functions of $\lambda_1, \lambda_2, \lambda_3$; in particular, because of the homogeneity of the aether, we obtain for the total moment of these forces (the force which attempts to decrease λ_1, must be a function of $\lambda_1, \lambda_2, \lambda_3$ which remains unchanged, when we exchange λ_2 for λ_3, and the remaining forces must follow from it, when λ_2 is exchanged for λ_1, λ_3 for λ_1) the following expression:

$$\partial\lambda_1 (a\lambda_1 + b\lambda_2 + b\lambda_3) + \partial\lambda_2 (b\lambda_1 + a\lambda_2 + b\lambda_3) + \partial\lambda_3 (b\lambda_1 + b\lambda_2 + a\lambda_3),$$

or, with a slight change in the meaning of the constants,

$$\begin{aligned} &\partial\lambda_1 (a(\lambda_1 + \lambda_2 + \lambda_3) + b\lambda_1) \\ &+ \partial\lambda_2 (a(\lambda_1 + \lambda_2 + \lambda_3) + b\lambda_2) \\ &+ \partial\lambda_3 (a(\lambda_1 + \lambda_2 + \lambda_3) + b\lambda_3) \\ &= \tfrac{1}{2}\, \partial\, (a(\lambda_1 + \lambda_2 + \lambda_3)^2 + b(\lambda_1{}^2 + \lambda_2{}^2 + \lambda_3{}^2)). \end{aligned}$$

Now the moment of the force, which attempts to change the form of the infinitely small substance-particle at O, can be regarded as resulting from forces, which attempt to change the length of the line elements ending at O. We then arrive at the following law of action:

If dV is the volume of an infinitely small substance-particle at point O and time t, dV′ the volume of the same substance-particle at time t′, then the force, resulting from the difference in the two states of the substance, which attempts to elongate ds is expressed by

$$a \cdot \frac{dV - dV'}{dV} + b \cdot \frac{ds - ds'}{ds}$$

The first part of this expression stems from the force with which a substance-particle resists a change in volume without a change in form, the second from the force with which a physical line element resists a change in length.

Now there is no reason to assume that the effects of both causes change with time in accordance with the same laws; thus adding up the effects of all the earlier forms of a substance-particle upon the change of the line element ds at time t, then the value of $\frac{\partial ds}{dt}$, which they attempt to bring about, becomes

$$= \int_{-\infty}^{t} \frac{dV' - dV}{dV}\, \psi(t - t')\, \partial t' + \int_{-\infty}^{t} \frac{ds' - ds}{ds}\, \phi(t - t')\, \partial t'.$$

How now must the functions ψ and ϕ be constituted so that gravitation, light and radiating heat can be propagated by the spatial medium?

The effects of ponderable matter upon ponderable matter are:

1. Attractive and repulsive forces inversely proportional to the square of the distance.
2. Light and radiating heat.

Both classes of phenomena can be explained if one assumes that the entirety of infinite space is filled by a homogeneous substance, and each substance-particle acts directly upon its immediate neighborhood.

The mathematical law in accordance with which this occurs can be thought of as split up into

1. the resistance which a substance-particle puts up to a change in volume, and
2. the resistance which a physical line element puts up to a change in length.

Upon the first part rest gravitation and electrostatic attraction and repulsion, upon the second, the propagation of light and heat and electrodynamical and magnetic attraction and repulsion.

IV. References to Attempts at Creating a Unified Physical Theory

November, 1850

Thus, for example, a mathematical theory, entirely complete in itself, can be assembled, which proceeds from the elementary laws valid for single points to events in the actually given, continuously filled space, without separating as to whether we are dealing with gravitation, or electricity, or magnetism, or the equilibrium of heat.

December, 1853

My other investigation on the connection between electricity, galvanism, light, and gravity I had resumed immediately after the completion of my *"Habilitationsschrift,"* and I have gotten far enough with it, so that without second thoughts I can publish it at this time. At the same time I have become more and more certain that Gauss has also been working on this for several years, and has communicated the matter under the seal of silence to a number of friends, Weber among others....

February, 1858

I have handed over my discovery on the connection between electricity and light to the Royal Society here. From a number of remarks I have heard about this, I am forced to conclude that Gauss has established and communicated to his closest acquaintances a theory of this connection different from my own. However, I am completely convinced that my theory is the right one and in a few years will generally be accepted as such.

V. A Contribution to Electrodynamics

February, 1858

I am taking the liberty to communicate to the Royal Society a remark which brings the theory of electricity and magnetism into close connection with that of light and radiating heat. I have found that the electrodynamic effects of galvanic currents can be explained if one assumes that the action of one electrical mass upon the others does not occur instantaneously, but is propagated to them with a constant velocity (within the errors of observation equal to the speed of light). The differential equations for the propagation of electrical force under this assumption becomes the same as that for the propagation of light and of radiating heat....

....According to present assumptions concerning electrostatic action, the potential function U of arbitrarily distributed electrical masses, if ρ is their density at point (x,y,z), is determined by the condition

$$\frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} + \frac{\partial^2 U}{\partial z^2} - 4\pi\rho = 0$$

and the condition that U is continuous and constant at an infinite distance of acting masses. A particular integral of the equation

$$\frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} + \frac{\partial^2 U}{\partial z^2} = 0,$$

which is continuous everywhere except for the point (x',y',z'), is given by

$$\frac{f(t)}{r}$$

and this function constitutes the potential function generated at the point (x',y',z'), if at time t the mass -f(t) is present at that point.

Instead of this I assume that the potential function U is determined by the condition

$$\frac{\partial^2 U}{\partial t^2} - aa\left(\frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} + \frac{\partial^2 U}{\partial z^2}\right) + aa\,4\pi\rho = 0$$

so that the potential function generated at the point (x',y',z'), if at time t the mass -f(t) is present there, is given by

$$= \frac{f(t - \frac{r}{a})}{r}$$

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