REVISITING THE FOUNDATIONS OF RELATIVISTIC PHYSICS

Festschrift in Honor of John Stachel

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CATHERINE GOLDSTEIN AND JIM RITTER

THE VARIETIES OF UNITY: SOUNDING UNIFIED THEORIES 1920–1930*

> *Die Voraussetzungen, mit denen wir beginnen, sind keine willkürlichen, keine Dogmen, es sind wirkliche Voraussetzungen, von denen man nur in der Einbildung abstrahieren kann. Es sind die wirklichen Individuen, ihre Aktion und ihre materiellen Lebensbedingungen, sowohl die vorgefundenen wie die durch ihre eigne Aktion erzeugten. Die Voraussetzungen sind also auf rein empirischem Wege konstatierbar.*
>
> Karl Marx and Friedrich Engels
> *Deutsche Ideologie*

The "goal of the ultimate"[1] — that is, the unification of all fundamental physical phenomena in a single explanatory scheme — had perhaps never seemed so close at hand for many physicists and mathematicians as in the third decade of the twentieth century. And if there were those who saw a great promise in this, there were equally those who opposed it.[2] Even among its partisans, just what such an 'ultimate' might resemble was not clear, its scope and its formulation seemed infinitely extendible, varying by author and even in the same author, by period. Hermann Weyl's trajectory provides an object lesson on this theme. In 1919, the preface to the third edition of his celebrated *Raum, Zeit, Materie*, devoted to an exposition of Einstein's general theory of relativity, hopefully announced:

> A new theory by the author has been added, which ... represents an attempt to derive from world-geometry not only gravitational but also electromagnetic phenomena. Even if this theory is still only in its infant stage, I feel convinced that it contains no less truth than Einstein's Theory of Gravitation[3] (Weyl 1919, vi).

Ten years later, the unity in sight at the beginning of the decade had been blurred for Weyl in the failure of his own and many similar attempts; moreover, his idea of what a unified theory ought to take into account, and how, had changed. To an American journalist at the Science Service in Washington D. C., who had publicized a recent unified theory of Einstein, much in the classical mold of Weyl's first, the latter wrote:

> Einstein's work is a new contribution to a search which he first undertook some years ago — one among many, many others which have been tried in the last ten years. ... I believe that the development of quantum theory in recent years has so displaced the status of the problem that we cannot expect to find the sought-for unity without involving matter waves, by which wave mechanics replaces moving material particles, in the framework.[4]

A. Ashtekar et al. (eds.), Revisiting the Foundations of Relativistic Physics, 93–149.

When, in 1950, he was asked for a new preface to the first American printing of *Raum, Zeit, Materie*, his judgement was final:

> My book describes an attempt to attain this goal [of unification]. This attempt has failed. ... Quite a number of unified field theories have sprung up in the meantime. ... None has had a conspicuous success (Weyl 1952, v–vi).

This story has been told a number of times in recent years, sometimes stressing the growing isolation of Einstein, shackled to a dying program, sometimes underlining the later revivals of some of the theories and their impact on the development of contemporary differential geometry and physics.[5] The appeal for us, however, lies essentially elsewhere, in just the absence of an obvious winner or — *pace* Weyl — of an obvious loser, and the puzzling historiographical issues this raises.

Traditional history of science has aimed at retracing the path, however tortuous, leading to the establishment of new truths. Since the sixties, at least, this aim, indeed the very notion of truth and its connection to the scientific enterprise, has been much contested; debates among scientists, their choices of values or paradigms, their more or less efficient uses of arguments and rallying of allies from different spheres of activities, political as well as technical, have become the focus of the historian's attention. Even if the work of a loser is handled with the same historical tools and the same respect as that of a winner, the difference between them still often provides the incentive of the narrative. How then to deal with the numerous scientific situations where no theory has lost or won, no general agreement, at any level, has been reached?

Most of the unified theories proposed in the twenties have been rediscovered and buried again; in a few cases, repetitively. They have been commented upon, expanded, compared, tested. In short, they are part and parcel of professional scientific activities. But which part? Did these theories represent marginal forays? Or the speculative forefront of some trends in physics? Or a recognized branch of research ordinarily practiced by competing groups? How to understand the dynamics of such a topic, and its role? How did it relate and interact with the most famous innovations of the early twentieth century, relativity theory and quantum theory? Indeed to what extent did such attempts towards unification ever become a recognized scientific discipline at all?

These questions deal with *collective* processes. The investigation of a few landmarks, however detailed it may be, will not be appropriate to answer them. Our ultimate goal is to understand how the whole body of work devoted to unified theories is organized, to analyze the possible alternatives, not only concerning the path to follow, but even as to what such a path might look like, and to trace the links (or lack thereof) between the different approaches and debates.

Our point of departure has been as concrete as possible — "real individuals, their actions and the material conditions of their existence." The material life of scientists in our century is punctuated by the writing and the publication of papers and books; these are, for us, the marks of production as a collective process and so have constituted the basis of our study. Since we wish to examine *professional* responses, we need to remain within the limits of professional acceptability; we have thus chosen to construct our corpus among the articles summarized in the main professional review journals of the twenties, *Physikalische Berichte*, widely read by physicists, and *Jahrbuch über die Fortschritte der Mathematik*, which played the same role for mathematicians. And here a first difficulty presents itself; in this period, there was no specific section devoted to unified theories (in itself, this is of course significant). Most attempts in 1920 deal with Einstein's general relativity, either to integrate it or to replace it, and are reviewed in the sections devoted to relativity theory and gravitation. These sections are also the unique obvious choice which is common to the two review journals during the twenties. Thus, while there are of course relevant papers in other sections, we have provisionally here chosen to concentrate on the papers reviewed in the section "relativity theory and gravitation." Within them, we have selected every article in which the author expresses an ambition to relate classes of natural phenomena seen as distinct. As we shall have more than one occasion to repeat in what follows, however, the conceptions of unification thus expressed vary widely.

Unification can mean the more or less complete merging of two fields into a single object, a metric for instance, or a unique action functional. It can mean the creation of a single englobing framework where different, 'natural', components (like the two fundamental forms of a hypersurface in a general space) take charge of the various phenomena or where the same mathematical object houses in turn each of them, according to the need or the interest of the physicist. We shall also find reductionist programs, in which one class of phenomena is shown to be an apparent instance of another, as well as schemes that coordinate different theories by having one replace the phenomenological aspects of the other. As this (non-exhaustive) list suggests, a deep ontological commitment is not necessarily considered as essential to a unification project, and indeed is totally lacking in some of our papers.

In a preliminary section, we shall first use the data generated by our selection to display some general trends of evolution during the 1920s, for relativity and for quantum theory in general, as well as for unified theories compared to those of relativity.

Such a rough-grained quantitative analysis, however, cannot give us access to the collective *practice* of unified theories. To do this we must study the papers themselves, locate and trace elements which relate them to others; either positively, by integrating these elements into the work itself, or negatively, by airing criticisms or stating alternatives. In contemporary scientific texts, *references* provide precisely such a means of capturing linking elements and we shall use them as our main guidelines within the limited space of this paper. Citation analysis has been, of course, a standard routine for some time in bibliometric studies and 'network analysis'.[6] But our technique is different; in particular, it is not derived from counting or automatic indexation, and we shall understand the word "references" in a larger sense than that of explicit bibliographical citations. We shall, of course, examine explicit footnotes and in-text citations, but we shall also take into account vaguer allusions to an idea or a rallying cry, as well as the use of a specific mathematical technique, in so far as such indicators appear to signify a collective practice.

Since, again, our emphasis is not on the communication of knowledge, but on its (collective) production, we shall need to take into account how the citations are used and precisely what kind of relation each reveals. Some of the links we shall examine have an obvious role in the papers they relate and the configurations[7] that they delineate are clearly-cut, for instance, the set of articles linked by the fact that they develop one particular affine theory. Others, however, are more subtle — for instance, the fact of taking for granted Einstein's general relativity as the correct theory of gravitation — and their concrete implementation in each paper authorizes a larger range of possibilities, which can be illustrated only through a more detailed analysis of the texts. In short, a close reading of the papers will here often ground a social analysis — and vice versa.

To fit all this into a finite space, we shall present here only a *sondage*,[8] the analysis of the articles dealing with unified theories and reviewed in one of our abstracting journals for three years only: 1920, 1925, 1930. These dates are not *anni mirabiles*.[9] We shall try in each case to outline most of the papers in sufficient detail to give a flavor of their variety. But, like its archeological counterpart, the result of our *sondage* will mainly consist of snapshots of the global organization of our topic at these different dates, allowing us to locate the main axes of its production and some of its characters. Their comparison, then, will give access to a more precise sense of the normality of this, a priori, very abnormal subject, and of its transformation during our decade.

1. UNIFIED THEORIES: SOME QUANTITATIVE DATA

Let us first present briefly the two abstracting journals on which we have relied for the initial selection of our corpus. The *Physikalische Berichte* was founded in 1920 as the amalgamation, under the auspices of the Deutsche Physikalische Gesellschaft and the Deutsche Gesellschaft für technische Physik, of a number of pre-war German physics abstracting journals: *Fortschritte der Physik*, *Halbmonatliches Literaturverzeichnis*, and *Beiblätter zu den Annalen der Physik.* The rate of publication was biweekly, but, fortunately for us, the review was indexed thematically at the end of each year. That part of *Physikalische Berichte* which interests us here is the second, "Allgemeine Grundlagen der Physik" [General foundations of physics], including several sections: "Prinzipien der älteren Physik" [Principles of the older physics], "Relativitätsprinzip" [Relativity principle], "Quantenlehre" [Quantum theory],"Wahrscheinlichkeit und Statistik" [Probability and statistics], "Erkenntnistheorie" [Epistemology].[10] The last year of our study, 1930, saw a change in the name of the second section, which hereafter became "Relativitätstheorie."

In 1920, on the other hand, the *Jahrbuch über die Fortschritte der Mathematik* was already a long-established mathematical review organ (it had been founded in 1869). The rhythm of publication, however, was not stable — biannual for the years 1919–1922 and annual thereafter — and the publication date was quite irregular, delayed generally three or four years after the nominal date.[11] Starting precisely with volume 47 ('1919–1920'; published in 1924–1926), a new Section VII was added, intercalated between "Mechanik" [Mechanics] and "Astronomie, Geodäsie und Geophysik" [Astronomy, geodesy and geophysics]; it was entitled "Relativitätstheorie und Theorie der Gravitation" [Relativity theory and theory of gravitation]. From the reorganisation of the '1925' volume on,[12] Section VII becomes "Mathematische Physik" with a subsection "2. Relativitätstheorie" and a new addition, "3. Quantentheorie."

Our first idea had been to capture how mathematicians and physicists received and reviewed different unified theories through a comparison of the titles present in one or the other of the two journals, in their unique common section, relativity. We discovered that, in fact, most of the articles were reviewed in both journals, although not always in the same year. We have thus simply aggregated their information in the selection of our corpus. The following quantitative analysis will, however, be based only on *Physikalische Berichte*, because it will allow us to draw some comparisons with quantum theory during the whole decade.[13]

Fig. 1 below displays the number of publications reviewed per year in the section on relativity and in the section on quantum theory. We note immediately that the number of relativity articles oscillates, with peaks around 1921–1923 and 1927, and that relativity dominates quantum theory up to 1925, after which it lags far behind.

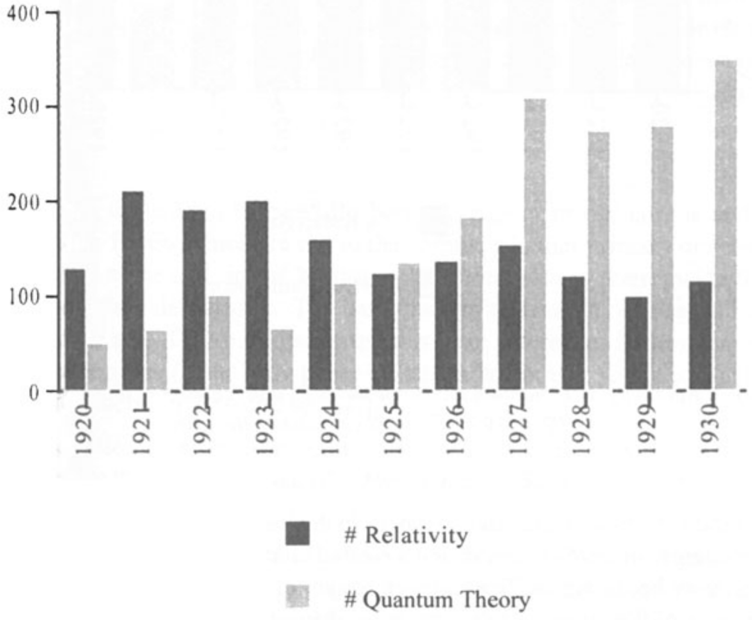

Figure 1. Number of relativity and quantum physics articles reviewed between 1920 and 1930. From Physikalische Berichte.

To gain more perspective, it is interesting to look at these publications as a percentage of *all* the physics articles and books published in the corresponding years and reviewed in *Physikalische Berichte* (fig. 2). Note that the combined production of the two sections, relativity and quantum theory (theoretical and experimental articles together), make up a nearly fixed percentage of total output, which rarely exceeds 5% and is never as much as 6%. The usual historiography of this 'golden age of physics' thus concentrates on a minute part of the activity in physics. Within this, relativity theory declines rather smoothly from 1922 on, while quantum theory takes up the slack.

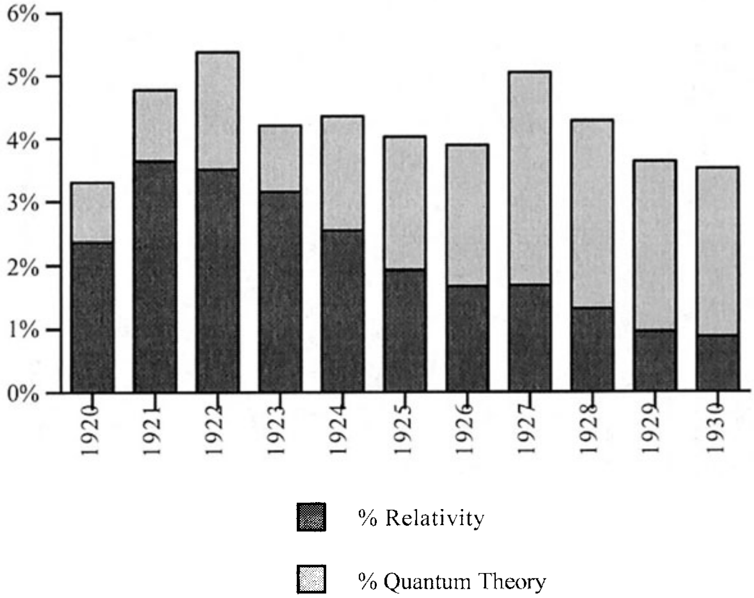

Figure 2. Relativity and quantum physics articles as a percentage of all physics articles: 1920–1930. From Physikalische Berichte.

Thus the view that spectacular advances in the 'new' quantum theory of Heisenberg and Schrödinger in 1925–1926 created a rival to relativity that drew interest away from the latter, may be, as we see, correct as a rough, global, picture, but does not capture the dynamics of the process:[14] the decline in the number of relativity publications had begun in 1924 in absolute numbers, but already in 1923 from a relative point of view, i.e., *before* the introduction of the 'new' quantum theory in the years immediately following. Indeed the real increase in quantum articles occurred only in 1927.

How to locate the unified field theories within this corpus? For neither of the two journals did they constitute a subcategory at this time. As mentioned before, we have restricted ourselves here to the papers in the section on "Relativity theory" (or "principle") in both journals.[15] Among them, we have selected, through the reading of both the reviews and the individual papers, those which attempt to unify two or more phenomena seen as fundamental and distinct: this means, for most cases, gravitation, electromagnetism or matter.[16] The type of integration, as we have discussed in the introduction, can vary widely, and we have imposed no restrictions on it in selecting our corpus.

Some 200 articles met these criteria.[17] Their number per year increases during our decade, which, coupled with the slow diminution in the total number of relativity papers, implies that articles dealing with unified theories constitute a non-negligible part of these last; to fix our ideas, they make up some 11% in 1920, 17% in 1925, and 36% in 1930, for articles reviewed in *Physikalische Berichte*.[18] Furthermore, between 15 and 25 authors each year among the eighty or so relativists devote one or several papers to unification.

These figures are large enough to suggest that working on unified theories was far from marginal in the twenties — at least inside the quite marginal area, in terms of production, that was relativity theory. They are also small enough to hold out the hope that a more systematic analysis is within reach. We shall begin it here, as announced above, through a detailed survey of the articles reviewed during three years: 1920, 1925, 1930, considered in turn. But we will take into account the two abstracting journals, which, in view of the relative shift in their dates of publication and in their handling of the material, means a covering of greater time intervals; in fact, the landscape we shall sketch includes almost half of the articles reviewed in the decade.

Note:

1. We have tried to distinguish carefully between papers in our corpus and those which fall outside. Papers which are not in the corpus, whether primary or secondary sources, are listed at the end, in the bibliography; references to them inside the text are given in the author–date system. The list of papers selected in our corpus for each year is given at the end of the part discussing this year; references to these papers are indicated by name alone (if the only paper by the author in that year's corpus) or by author–Roman numeral (if there are several).

2. In the list of titles for each year's corpus, the reference in brackets at the end of each entry refers to the review journal (P = *Physikalische Berichte*, J = *Jahrbuch*) and page number of the review.

3. The 69 authors selected use very different symbolic conventions — a given author will often even change them from one article to another. The choice of Greek or Latin letters for indices is non-systematic, the same Christoffel symbol is sometimes denoted $\left\{ {j \atop ik} \right\}$ and sometimes $\left\{ {ik \atop j} \right\}$, etc. For the sake of space and to facilitate reference to the original papers, we have nonetheless retained their notation unless otherwise indicated.

4. Foreign, especially Slavic, proper names have been generally transcribed following the international linguistic system except when used in a reference, where the printed version, often using another transcription system, has been retained.

2. 1920

It is in this first year[19] of our *sondage* that the question of the definition of a unified theory is posed in its most acute form. Applying the criteria discussed above, we have retained 34 papers, written by 18 authors.[20] Among them, 26 are original research papers, 5 summarize other articles in our corpus, and 3 are general, non-technical, discussions of the state of the art.[21]

The most striking feature of the 1920 unification theories is their heterogeneity; a heterogeneity which concerns equally the nature of the unification, the choice of phenomena to unify, and the technical means mobilized to this end. Indeed, their only common features are an awareness of Einstein's general relativity and the presence of electromagnetism among the fundamental phenomena — electromagnetism, not gravitation, despite our restriction to the sections on relativity.

To illustrate how large is the spectrum of unified theories in 1920, consider the case of the well-known mathematician, Harry Bateman: in a letter to the editor of the *Philosophical Magazine* he draws aggrieved attention to his own priority in the creation of a theory of general relativity. This priority is based on an article of his, published in 1910, now generally described as giving a proof of the conformal invariance of the Maxwell equations, but interpreted by him as part of a unification program.

> My work on the subject of General Relativity was published before that of Einstein and Kottler, and appears to have been overlooked by recent writers. In 1909 I proposed a scheme of electromagnetic equations... which are covariant for all transformations of co-ordinates which are biuniform in the domain we are interested in. These equations were similar to Maxwell's equations, except that the familiar relations $B = \mu H, D = kE$ of Maxwell's theory were replaced by more general equations, which implied that two fundamental integral forms were reciprocals with regard to a quadratic differential form
>
> $$\sum \sum g_{m,n} \, dx_m \, dx_n ,$$
>
> which was assumed to be invariant for all transformations of co-ordinates (pp. 219–220).
>
> The idea that the coefficients of the quadratic form might be considered as characteristics of the mind interpreting the phenomena was also entertained,... and it was suggested that a correspondence or transformation of co-ordinates might be employed as a crude mathematical symbol for a mind. ... If we assume that the nature of an electromagnetic field depends on the type of fundamental quadratic form, which determine the constitutive relations, and thus depends indirectly on a transformation which alters the coefficients of this quadratic form, this dependence may be a symbol for the relation between physical and mental phenomena instead of giving the influence of gravitation on light as in Einstein's theory.
>
> Einstein and the others have attempted to formulate a set of equations of motion which will cover all physical phenomena; but... the true equations of motion should be capable of accounting for the phenomena of life (pp. 220–221).

In this limiting case we have a theory where electromagnetism and mental phenomena, seen as basic forces, are linked together through a quadratic form, the mind operating through coordinate transformations; moreover, Einstein's theory itself is reinterpreted not as theory of gravitation alone, but as a unified theory, encapsulating light and gravitation.

Like Bateman, 11 of the 18 authors present autonomous theories and refer mainly to their own previous work. In some cases these approaches appear to be recent, launched within the previous few years, like that of Hermann Weyl alluded to in the introduction, or the theories proposed by Théophile De Donder and Henri Vanderlinden, or by Ernst Reichenbächer; in others, it consists of an attempt to reactivate older material, produced in another period, and refurbished in new, primarily geometrical, clothing. Such a case is Joseph Larmor, for whom his currently proposed five-dimensional unified field theory is an elaboration of ideas contained in his classic book of 1900, *Æther and Matter*. Again, Emil Wiechert's major references are to his own work of the late 1890s. One obvious reason for this rejuvenation is the 1919 eclipse report on the deviation of light in the Sun's gravitational field, which confirmed Einstein's previsions and thus motivated more than a few sceptics of general relativity and defenders of the old electromagnetic world-view[22] (Larmor and Wiechert, in particular) to readjust their own projects, and integrate these phenomena within them; for, in Wiechert's words, "A discovery has been made!"[23] (Wiechert I, 301) and this had to be accounted for.

In most cases, then, references serving to designate the approach used are of an idiosyncratic nature. The two main exceptions are De Donder's work, which is discussed and developed by three authors, and, above all, Weyl's theory, which succeeds in gathering around it a small group of papers by several authors. Besides this type of quotation, and the references to general relativity which we will explore more closely later, there are also some mutual citations of a vaguer nature, mention, for instance, of work considered as related in some way.

These allusions rather neatly divide our corpus into two main sets, which essentially correspond to the language(s) (and place) of publication: German on the one hand and French or English on the other. German-language papers largely dominate, with 20 articles by 10 authors, among which 10 (by 5 authors) appear in *Annalen der Physik* and 3 (by 3 authors) in *Physikalische Zeitschrift*;[24] on the other side, we find 5 articles in English (by 3 authors) and 7 in French (by 4 authors). A single paper in Dutch completes the scene.

The meaning of such a dichotomy needs to be elucidated in detail.[25] It is, at least partly, a consequence of the First World War and we will see the situation change in the later years of our *sondage*. But how does it operate? What divides the two sets is not a question of approach; the variety mentioned above runs through both groups. Electromagnetic programs are common to Wiechert and Lodge, variational principles are important for Weyl and De Donder, but not for Reichenbächer. The division mainly indicates the limits of *reciprocal visibility*; the authors on one side seem almost not to see those on the other; not only do they not explore their proposals, they do not even engage in discussions or debates on the same issues. First established by the examination of mutual references, this observation can be reinforced by a number of further details, within or outside our corpus. When, for example, Einstein corresponds with De Donder, or mentions the latter or Bateman in his correspondence with third parties, he is either vague or critical.[26] And while Larmor publishes in 1921 a eulogistic review of De Donder's first book (De Donder 1921) in the London *Times* (Larmor 1921), he makes no mention of Wiechert or Weyl. Similarly, in the later editions of his *Raum-Zeit-Materie*, Weyl, in a note concerning projects analogous to his own, refers to Wiechert and even to the evidently quite marginal Reichenbächer[27] — but never to De Donder, who might be considered to be scientifically closer. Leading to the same grouping are the brief historical introductions to some of the papers; the Mie, Hilbert, Einstein trilogy (in varying order) of founding fathers, which appears in German-speaking authors like Reichenbächer, Humm, or Einstein himself, is never mentioned in articles written in other languages.

An obvious counterargument seems to be offered by Einstein himself; his name is cited by every author. Furthermore, in a 1920 article in the *Berliner Tageblatt* (Einstein 1920), he himself writes: "The greatest names in theoretical physics, H. A. Lorentz, M. Planck, Sommerfeld, Laue, Born, Larmor, Eddington, Debye, Langevin, Levi-Civita, based their work on the theory [of general relativity] and have, in general, made valuable contributions to it," thus drawing the implicit contour of an international community of one heart and mind. However, Einstein's visibility concerns his theory of general relativity, not his 1919 paper: that is, Einstein is no exception *as an author in our corpus*. His 1920 article in the *Berliner Tageblatt* was written in the context of the conflict with the "Anti-Relativity Inc." group inside Germany; even if Einstein's sincerity about his internationalism is not in doubt, the fact is that he only very rarely uses or cites at this date non-Germanophone contributions (other than Eddington) to his theory; moreover the (essentially critical) views of general relativity and its extensions of someone like Larmor, for instance, makes his inclusion seem more a move to extend the list than a reflection of serious scientific interaction.[28]

Let us then examine first the German-speaking landscape. The circle of authors, it should be pointed out, is surprisingly narrow. All have some close connection with Göttingen, with the exception of Reichenbächer — and, to some extent, of Mie and Einstein, whose works however were adopted and promoted by the Göttingen mathematicians (Corry 1999). Wiechert, of course, was professor of geophysics there and co-directed the Göttingen electron seminar of 1905 with Hilbert and Minkowski (Pyenson 1979), Weyl and Humm were students of Hilbert, Dällenbach in turn was a student of Weyl's in Zürich, Arthur Haas had studied in Göttingen in the first decade of the century. Wiechert, Roland Weitzenböck and Wolfgang Pauli collaborated in the *Encyclopädie der mathematischen Wissenschaften*, an enterprise tightly connected with Göttingen mathematician Felix Klein's perspective (Tobies 1994). Pauli, furthermore, was directly linked to Weyl through Sommerfeld and Einstein — a connection which permitted Weyl (I) and Pauli to make reference to each other, their papers having been exchanged as preprints. We have further testimony of direct, professional or personal encounters between some of our authors; for instance, the discussion of Weyl's communication at the Bad Nauheim conference on relativity (Weyl II), showcases Einstein, Pauli, Reichenbächer and Mie.

A closer look at the articles suggests however a finer delineation. A first set of papers, to which we have already alluded, cluster around the exploration of Weyl's theory. It includes in particular 2 articles by Weyl himself, 2 articles by Weitzenböck, 1 by Pauli, as well as the 2 expository papers of Haas; to these may be added 2 of the 5 papers by Reichenbächer (IV–V), which compare Weyl's theory to the author's own. Weyl's theory, the usual prototype for a unified field theory, has been well studied.[29] Let us here only recall that Weyl, in an attempt to develop a true "geometry of proximity," that would be adapted to a physics excluding action at a distance, extends the (pseudo-)Riemanian geometry of general relativity; in Weyl's geometry, not only can the direction of vectors not be transferred from one point to another independently of the path taken, but neither can their lengths. On the other hand, Weyl retains an extra condition of "gauge"-invariance for the laws of nature, besides the general covariance of Einstein's theory: the line-element $ds^2 = g_{ij}dx^i dx^j$ at a point $P(x^i)$ becomes $ds^2(1 + d\phi)$ at $P(x^i + dx^i)$, with $d\phi = \phi_i dx^i$. While the g_{ij}, as in Einstein's theory, are to be associated with the gravitational field, Weyl proposes identifying the linear metric element ϕ_i with the electromagnetic four-potential.

> Then not only gravitational but also electromagnetic forces would spring out of the world metric; and since no other truly fundamental forces other than these two are known to us, through the resulting theory, in a strange, unforeseen way, Descartes' dream of a purely geometrical physics would be fulfilled. In this it appears that physics, with its conceptual content, does not at all extend beyond geometry; *in matter and natural forces only the metric field reveals itself.* Gravitation and electricity would thus be accounted for in terms of a unified source[30] (Weyl I, 112).

Weyl presented his theory in 1918, but criticisms on physical grounds by Einstein, among others, moved him to modify this initial version in two ways, both explained in his 1919 articles. The first is to separate the actual procedure of measurement by rods and clocks from the ideal procedure associated with parallel transport and basic to his geometry.[31] The other consists of exploring more precisely the field equations; Weyl derives them from a variational principle applied to the invariant integral $\int \mathfrak{W}\,dx$ for a specific Lagrangian $\mathfrak{W}$ ("the action function"), selected on formal and philosophical grounds, and he obtains both Maxwell's equations and new gravitational equations, different from Einstein's, and allowing a closed world without recourse to a cosmological term.

Both Pauli and Weitzenböck, as well as Weyl, explore in 1919 specific choices of the action function leading to the field equations. But they do so in a fashion which illustrates perfectly their differences in perspective. Weitzenböck's work is a search for the *complete* list of invariants of the theory.[32] Pauli, on the contrary, makes his choice among the possible action functions by examining their *physical* consequences, in particular, in the static case, stating independently some of Weyl's 1919 results. He shows that the field equations are symmetric with respect to the two kinds of electricity, positive and negative — a circumstance he sees at the time as an important drawback to the theory since no positively charged particle of electronic mass was then known — and that Weyl's equations can lead to a correct value for the advance of Mercury's perihelion. He also studies the problem of the electron, that is the possibility of static, spherically symmetric solutions.

On a more popular level, the two essays by Haas plead, from the tribune offered by Weyl's theory, for the reduction of physics to geometry. Haas acts as an unofficial spokesman for Hilbert's approach, including the advocacy of an axiomatic program for physics; this, he claims, is the essence of the Einsteinian revolution.[33] Haas concludes his interventions with the comment that now that gravitation and electromagnetism have already been taken care of in this approach, the next step should be the introduction of a discontinuous geometry in order to integrate matter.

> Thus all physical laws are reduced to the single problem of the metric of a four-dimensional space-time manifold. ... One of the most important tasks for the future in this respect ... is certainly the introduction of quantum theory into general relativity.

> To handle this problem, physical axiomatics must clearly enter into a thought that … Riemann… had expressed: that the object of geometry could also be a *discontinuous manifold*. … But if the … manifold itself were taken as discontinuous, then it would be understandable why the quantity of action that appears in given physical processes necessarily must be an integer multiple of an *elementary quantum of action*[34] (Haas I, 749).

Haas is not isolated in his concern with quanta. Indeed, Weyl himself, in the preface to *Raum, Zeit, Materie*, evokes quantum physics as a possible boundary for his theory. At Bad Nauheim, in a question following Weyl's talk (Pauli *apud* Weyl II, 651), but aimed at Einstein as well, Pauli begins to challenge the capacity of a purely classical field approach (i.e., a continuous theory) to deal with the situation inside the electron, and as a consequence, to doubt the validity of Einstein's and Weyl's unification programs; from this time on, Pauli, as is well known, turned his efforts in quite another direction.[35]

Besides these explorations of a single theory, we find, within the German-speaking group in our corpus, several papers which share among themselves, and in common with those just discussed, one specific feature: they refer to Einstein's theory of gravitation in a positive way, integrating it as a part of their program or at least as an horizon bounding it. They are distinguished, however, by quite different approaches to matter: its nature, its physical properties and representations, and the way it interacts with gravitation.

The nearest to Weyl's program, in terms of objectives and of the role played in a communication network, is that of Einstein himself, in his first published attempt at tying gravitation and electromagnetism together more efficiently than in his 1915 theory, by proposing the second modification of his field equations in three years. Two problems have led him to doubt his original field equations. The cosmological question had obliged him (Einstein 1917) to add an extra cosmological term in order to have a closed, static model of the universe; now the problem of matter, the need to derive a solution corresponding to an electron, motivates his new approach. Leaving the (pseudo-)Riemannian geometry without change, he puts forward as his new equations for the case of gravitational and electromagnetic fields,

$$R_{ik} - \frac{1}{4}\, g_{ik}\, R = -\kappa\, T_{ik}^{\mathrm{EM}},$$

R_{ik} being the Ricci tensor, R the scalar curvature and T_{ik}^{EM} the Maxwell energy tensor of the electromagnetic field. The new coefficient 1/4 is intended to kill two birds with one stone: to provide the cosmological term more naturally, as a constant of integration, and to yield a regular, static, spherically symmetric solution that will represent the electron. The indeterminacy of the solution was to push Einstein to explore other theories in the following years (Ritter 1993), though keeping always the same central ambition; the recuperation of matter from the interlinkage between gravitation and electromagnetism.

A quite different approach to the problem of matter is to be seen in the papers which constitute Dällenbach's thesis (Dällenbach I and II). He operates within the framework of a flat Minkowski space, but, he incorrectly believes, only as a technical simplification which "easily" generalizes to a generally covariant theory. Dällenbach essentially recasts Lorentz's electron-theoretic approach to electromagnetism (Lorentz 1904) in an Einsteinian mold, which allows him to derive the (phenomenological) constitutive equations of Maxwell as spatial averages integrated over properties of the electronic constituents of matter.

Gustav Mie, too, explores the interface between Einstein's theory of gravitation and classical electromagnetic questions.[36] He first finds the key to the "wonderful and consummately beautiful mathematical structure"[37] (Mie, 653) of Einstein's theory, and the clarification of its role in any future unified theory, in what he calls the "generalized principle of the relativity of gravitational action." This he understands as the possibility of transforming, through an appropriate choice of coordinates, the study of a moving test body in a gravitational field into one of a body at rest in a space with a non-Minkowskian geometry. Mie uses this principle to study the following paradox: Einstein's theory predicts that there can be no electromagnetic radiation from a charged particle moving in a gravitational orbit in empty space, in agreement with what is expected from Bohr's quantum theory; Maxwell's equations assure us of the contrary. Using the Schwarzschild metric to provide the geometry of space in which the transformed particle is at rest, a generalized Laplace electrostatic equation for the resulting Coulomb potential and a particular cylindrical coordinate system borrowed from the work of Reichenbächer (1917 and I–III) to transform the result back into the physical rotating situation, Mie exhibits a solution in Einstein's framework which indeed does not radiate. He is then able to identify it, in the far field, with a particular Maxwell equation solution, but one which represents a moving particle surrounded by a spherical standing wave, such as would be provided by a purely reflecting shell. Conversely, a radiating solution can be created for empty space in the Einstein theory, but only at the price of adding to the original potential one representing a sourceless electric rotation field; happily this turns out to be necessary in any case to provide conservation of energy.

Finally, two of our German-speaking authors — Reichenbächer and Wiechert — do not accept general relativity. They prefer to develop their own approach, in each case an electromagnetic reductionism, to take into account the new results brought in by Einstein's theory.[38] Their similarities stop here; there is no unified front in these alternative attempts, and their conception of matter, as well as the techniques they use, link them more directly to other authors previously discussed than to each other.

Ernst Reichenbächer situates his work within the Einsteinian geometric tradition, but seeks to base it on a direct expression of electronic properties, thus avoiding the phenomenological features he reads in both Einstein's and Weyl's proposals for the determination of the metric:

> Contrary to ordinary intuition, the mass density appears [in general relativity] not as a scalar, but as the 44-component of a sixteen-component tensor... . This and the fact that, because of their dependence on the choice of coordinate system, the $g_{\mu\nu}$ are nevertheless subjected to a restricted arbitrariness, did not please me in Einstein's theory. Therefore, in my [1917] article "Characteristics of a Theory of Electricity and Gravitation," I attempted to set up the theory of a scalar gravitational potential, which I identified with the speed of light and where I introduced certain conditions on the gravitational perturbation by electrons — positive and negative — which I saw as the only kind of matter. I completed

> the simplest case ... of a single electron and set up the general equation
>
> $$\mathfrak{K} = 2\,\mathrm{Div\,Grad\,lg}\,l,$$
>
> following the analogy of this case[39] (Reichenbächer I, 1).

More precisely, for the case of one electron, Reichenbächer builds his metric by gluing: far from the electron, the metric is supposed to be Minkowskian; inside the electron, the deviation of the metric from its Minkowskian values is interpreted as a rotation, of which the angles, associated with the 6 pairs of coordinate lines, are given by the components of the electromagnetic 6-vector, the first set of Maxwell equations providing exactly the required compatibility conditions. The second set of Maxwell equations is used to derive the fundamental equation quoted above. The gravitational force is then associated with the variable time-component of the metric, and appears as a (variable) velocity of light.[40] In his 1919 and 1920 papers, Reichenbächer generalizes his construction by recurrence for a finite number of electrons and derives from his theory an advance for the perihelion of Mercury and a deflection of light in a gravitational field, both one-half the general relativistic value. He also adapts his theory to the Weyl approach, in order to obtain covariant and gauge-invariant laws of gravitation and electromagnetism:

> It is then possible ... to arrive at a ... solution of the world problem ... by taking a realistic point of view instead of the more phenomenological one of the relativist[41] (Reichenbächer IV, 113).

Another dissident is Emil Wiechert, for whom, as for Reichenbächer, matter is of electromagnetic origin, here comprised of ether and electrons. Wiechert, with titles like "Gravitation as an Electrodynamic Phenomenon" (I) and "Remarks on an Electrodynamic Theory of Gravitation" (II) clearly announcing his program, is quite explicit:

> The foundation of the theory should be the acceptance that molecular matter is built up out of electrical particles. This then explains electrification as a basic property of all the building blocks of matter. The assumption appears as the natural consequence of the results of the molecular-physical research of the last three decades. Once having recognized through electrodynamics that electrification is an essential cause of *inertia*, the proof should now be sought that electrification is also an essential cause of *gravitation*[42] (Wiechert I, 331).

In contrast to Reichenbächer, his theory does not use any metric; he had already, in 1916, suggested an alternative Lagrangian to that of general relativity in order to compute the advance of the perihelion of Mercury, interpreting it also as a variable speed of light. The purpose of his 1919 papers is to obtain an electromagnetic theory of gravitation which could explain all the experimental results obtained by the Einstein theory. Starting from the Lagrangian $L = -2/3\,(e^2/a)\sqrt{1 - v^2/c^2}$, an idea borrowed from Abraham (1902), he derives new field equations with two free parameters, which he then tries to evaluate on the basis of experimental data. He points out that his hope is to ultimately derive these values *ab initio* from a theory of the structure of ether, though this he has not yet been able to obtain.

If we now turn to the non-German language articles, further groupings are recognizable, though of a very different nature. The Cambridge trio, Lodge, Larmor and

Bateman, whom we have already met at the beginning of this section, share not only an interpretation of general relativity as a unified theory of gravitation and light, but also a number of mathematical techniques, in particular a stress on changes of coordinate systems, coming out of long-established ways of working in physics. But they do not share a well-defined project nor do they cite each other on the issue of unification.[43] On the contrary, De Donder, Vanderlinden and the Toulousan mathematician Adolphe Buhl, among the authors of papers in French, do participate in such a project. All explore De Donder's theory, putting the emphasis on analytical, not geometrical, techniques; they are convinced these will provide a common formal machinery which could uniformly accommodate different natural phenomena. For the sake of space, we shall briefly present only one example of each group.

A geometry with five dimensions is the framework suggested by Larmor (I) to englobe electromagnetism and gravitation.[44] Commenting on the work of a young mathematician who had rediscovered Clifford algebra, Larmor shows, in an article submitted in the summer of 1919, how such a mathematical framework might be used to give a new geometrical interpretation to the special theory of relativity, in which the electromagnetic field would be a four-dimensional flat surface embedded in a five-dimensional space. And then the 6 November news of the results of the eclipse expedition, and its verification of the general relativistic deflection of light, arrives prior to publication. Two weeks later, Larmor has written a generalization which allows gravitation an entry into this picture:

> *Note added 20 November 1919* — … The phenomena of gravitation have been included by Einstein in this Minkowski scheme by altering slightly the expression for $\delta\sigma^2$. … This generalisation can be still be brought within the range of the elements of the Clifford geometry … by introducing into the analysis a new dimension (ξ), preferably of space; so that
>
> $$\delta\sigma^2 = \delta x^2 + \delta y^2 + \delta z^2 + \delta\xi^2 + \delta w^2, \qquad w = ict.$$
>
> … Now any continuum of four dimensions, having a quadratic line-element, however complex, is expressible as a hypersurface in this homaloid [flat] continuum of five dimensions. If these considerations are correct, the Einstein generalization, made with a view to include gravitation within his four dimensions, must be interpretable as the geometry of some type of hypersurface constructed in this extended homaloid of five dimensions.
>
> … Thus we postulate a fivefold electrodynamic potential … in the Euclidean auxiliary space (x, y, z, ξ, ict). Then any section of this space and its vector-system is a hypersurface of four dimensions … and represents a possible electrodynamic world process; including implicitly its gravitation, which would become apparent only when the hypersurface, actually already nearly flat, is forced into representation on a hyperplane (Larmor I, 353–354, 362).

In other words, electromagnetism was to provide the metric — the first fundamental form — of the four-dimensional embedded surface, while gravitation was to describe this embedding, i.e., determine the second fundamental form.

Théophile De Donder had, in 1914, proposed a theory analogous, as he claimed on various occasions, to Einstein's general relativity. In the following years he will devote numerous books and papers to the presentation and development of a "théorie de la gravifique," either his own, Einstein's or, later, Weyl's. He defines this theory as the study of relationships between a "twisted space-time" (which for some applications can be a flat Minkowski space) and different fields, either electromagnetic or “material”; this last category including, in opposition to Einstein, gravitational properties of matter. In particular, the geometrical setting, that is, the very definition of the space-time, is a given, to be exhibited at the beginning of each paper, the work itself bearing on essentially analytical aspects. Here, De Donder, with the help of Vanderlinden, lays special emphasis on a modified variational principle formalism, used to obtain Einstein’s field equations by means of ordinary calculus without recourse to tensor analysis; the aim is to develop this principle with as much generality as possible, and then specialize it for the treatment of various phenomena, one example being the inclusion of Poincaré stresses in the construction of atoms.[45]

1920 Corpus

Bateman, Harry
“On General Relativity” [Letter to the editor, 10 August 1918]. *Phil. Mag.* (6) 37 (1919) 219–223 [P70; J812]
Bloch, Léon
“Remarque sur la théorie de Lorentz comparée à celle de Mie.” *CRASP* 171 (1920) 1379–1380 [J805]
Buhl, Adolf
I “Sur les symétries du champ électromagnétique et gravifique.” *CRASP* 171 (1920) 345–348 [J804]
II “Sur la formule de Stokes dans l’espace-temps.” *CRASP* 171 (1920) 547–549 [J804]
III “Sur les symétries du champ gravifique et l’extension lorentzienne du principe d’Hamilton.” *CRASP* 171 (1920) 786–788 [J804]
Dällenbach, Walter
I “Die allgemein kovarianten Grundgleichungen des elektromagnetischen Feldes im Innern ponderabler Materie vom Standpunkt der Elektronentheorie.” *Ann. Phys.* (4) 58 (1919) 523–548 [P126; J792]
II “Hamiltonsches Prinzip der elektromagnetischen Grundgleichungen im Innern ponderabler Materie.” *Ann. Phys.* (4) 59 (1919) 28–32 [P127; J793]
De Donder, Théophile
“Le Tenseur gravifique.” *Versl. Kon. Akad. A’dam* 27 (1918/19) 432–440 [J803]
De Donder, Théophile and Henri Vanderlinden
I “Théorie nouvelle de la gravifique.” *Bull. Acad. Roy. Belgique* (5) 6 (1920) 232–245 [J803]
II “Les Nouvelles Équations fondamentales de la gravifique.” *CRASP* 170 (1920) 1107–1109 [P1178; J804]
Einstein, Albert
“Spielen Gravitationsfelder im Aufbau der materiellen Elementarteilchen eine wesentliche Rolle?” *SPAW* (1919) 349–356 [P193]
Haas, Arthur
I “Die Axiomatik der modernen Physik.” *Naturwiss.* 7 (1919) 744–750 [P520]
II “Die Physik als geometrische Notwendigkeit.” *Naturwiss.* 8 (1920) 121–127 [P518]

Humm, Rudolf Jakob

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Larmor, Joseph

I "On Generalized Relativity in Connection with Mr. W. J. Johnston's Symbolic Calculus." *Proc. Roy. Soc. London* 96 (1919) 334–363 [P123]

II [Report on Meeting of the Royal Society, 20 November 1919.] *Nature* 104 (1919) 365 [P456]

Lodge, Oliver

I "Connexion between Light and Gravitation." *Phil. Mag.* (6) 38 (1919) 737 [P791; J811]

II "Gravitation and Light." [Letter to the editor, 30 November 1919]. *Nature* 104 (1919) 354 [J812]

Mie, Gustav

"Das elektrische Feld eines um ein Gravitationszentrum rotierenden geladenen Partikelchens." *Phys. Z.* 21 (1920) 651–659 [J786]

Nordström, Gunnar

"Opmerking over het niet Uitstralen van een overeenkomstig kwantenvoorwaarden bewegende elektrische Lading." *Versl. Kon. Akad. A'dam* 28 (1919/20) 67–72 [J806]

Pauli, Wolfgang, jr.

"Zur Theorie der Gravitation und der Elektrizität von Hermann WEYL." *Phys. Z.* 20 (1919) 457–467 [J791]

Reichenbächer, Ernst

I "Das skalare Gravitationspotential." *Ann. Phys.* (4) 61 (1920) 1–20 [P457; J794]

II "Die Krummung des Lichtstrahls infolge der Gravitation." *Ann. Phys.* (4) 61 (1920) 21–24 [P457; J794]

III "Die Punktbewegung im allgemeinen Gravitationsfelde." *Ann. Phys.*(4) 61 (1920) 25–31 [P457; J794]

IV "Über die Nichtintegrabilität der Streckenübertragung und die Weltfunktion in der Weylschen verallgemeinerten Relativitätstheorie." *Ann. Phys.* (4) 63 (1920) 93–114 [J794]

V "Die Feldgleichungen der Gravitation und der Elektrizität innerhalb der Materie." *Ann. Phys.* (4) 63 (1920) 115–144 [J795]

Weitzenböck, Roland

I "Über die Wirkungsfunktion in der Weyl'schen Physik. I." *SAW Wien* (2) 129 (1920) 683–696 [J784]

II "Über die Wirkungsfunktion in der Weyl'schen Physik. II." *SAW Wien* (2) 129 (1920)697–708 [J784]

Weyl, Hermann

I "Eine neue Erweiterung der Relativitätstheorie." *Ann. Phys.* (4) 59 (1919) 101–133 [P257; J782]

II "Elektrizität und Gravitation." *Phys. Z.* 21 (1920) 649–651 [J784]

Wiechert, J. Emil

I "Die Gravitation als elektrodynamische Erscheinung." *Ann. Phys.* (4) 63 (1920) 301–381 [J789]

II "Die Gravitation als elektrodynamische Erscheinung." *Nachr. Göttingen* (1920) 101–108 [J799]

III "Bemerkungen zu einer elektrodynamischen Theorie der Gravitation." *Astronomische Nachrichten* 211 (1920) 275–284, 287–288 [J790]

3. 1925

The selection for this second year of our *sondage*, 1925, consists of 34 articles, written by 21 authors. The corpus, thus, is quite similar quantitatively to that obtained for 1920, but this apparent resemblance only underlines the limits of a purely quantitative approach; the situations in 1920 and in 1925 are very different indeed.

The first striking difference is the internationalization of the topic. Besides the languages and countries represented in 1920, we now find authors publishing in Japanese, American, Hungarian and Italian journals; moreover, the impressive domination in 1920 of a single outlet, the *Annalen der Physik*, has disappeared. Indeed, not a single paper from this journal appears in our list for 1925. In its place we find in particular 4 papers (2 authors in each case) in the *Physikalische Zeitschrift* and the *Notes aux Comptes rendus de l'Académie des sciences de Paris*, 3 (by 3 authors) in *Nature*, *Zeitschrift für Physik* and *Physical Review*, this last in the context of abstracts from meetings of the American Physical Society. A further element of this internationalization is the translation into a number of languages of two reference books, Weyl's *Raum-Zeit-Materie* and Eddington's *The Mathematical Theory of Relativity* of 1923; a number of our papers refer to them for their basic notation and an introduction to their main tools. This new distribution, however, does not mean a homogenous visibility and the place of publication is still a good marker for the readership and use of an article.[46] Yet some of the newcomers have a broad perspective on the various trends; significant in this respect, even if on quite an exceptional scale, is the paper of Manuel Sandoval Vallarta, a Mexican physicist working at the time at MIT, who quotes, with equal ease, De Donder and Vanderlinden as well as Weyl and Einstein, Bateman as well as the German quantum theorists Max Born, Werner Heisenberg and Pauli.

A second crucial difference with 1920 is that there are fewer authors (6) claiming that they pursue their own, personal theory. Most of the authors place themselves quite consciously in one of a few major traditions (or some combination of them). The principal one is now standardly referred to as the (Einstein-)Weyl-Eddington theory, in one of its variants; in 1925 we find, for example, papers following Weyl's theory (Vallarta, Eyraud I and Reichenbächer), Eddington's (Rice I, II), or Jan Schouten's and Élie Cartan's work (Eyraud II, III).[47] Even De Donder now presents his work as an extension of the Weyl-Eddington-Einstein trend, while it also constitutes an extension of his own work of 1920. What renders this difference with the situation in 1920 ambiguous is that there are almost as many traditions as papers; the explanation is that the kernel of papers is not so closed in 1925 as in 1920. The 1925 articles more frequently refer either to earlier (1921–1924) authors — who thus do not appear directly in our *sondage* — or to works which were reviewed in other sections of *Physikalische Berichte* and *Jahrbuch* ("differential geometry" for Cartan's papers or "quantum theory" for Bateman's, for example). The regrouping of the intellectual traditions at work, and the more elaborately structured organization of the programs, are thus counterbalanced by an enlargement of the possible sources for the theories that are to be adapted, mixed, or developed.

Two main technical innovations, to which we have already alluded, appear in these papers. The first concerns Eddington's generalization of Weyl's ideas (Eddington 1921), to initially posit a connection rather than a metric as the fundamental element of the theory. The connection Γ^i_{jk}, a concept borrowed by Weyl and then by Eddington from Levi-Civita's work (1917),[48] describes the parallel displacement of a contravariant vector A^i along a curve x^s, that is $dA^i = -\Gamma^i_{rs} A^r \, dx^s$. The Riemann and Ricci tensors could then be defined directly in terms of it. In the classical case of general relativity, the connection is given by the (symmetric) Christoffel symbol, defined by the metric and its derivatives; the Ricci tensor is then symmetric. Starting with a general (though still symmetric) connection allowed Eddington — and Einstein following him in 1923 (see Einstein 1923a) — to obtain a non-symmetric Ricci tensor; its antisymmetric part could then be taken as a representation of the (antisymmetric) electromagnetic field tensor.

This work led to the idea of relaxing in turn all the constraints on symmetry and on the relationship between the connection and the metric in order to find room to house electromagnetism; some of these further possibilities are explored in the 1925 corpus. For instance, the young Japanese physicist Bunsaku Arakatsu considers two symmetric connections, one of which he associates with the metric, and thus with the "geometry" of the space, the other with the "physics" of the space.[49] He then sets the Riemann curvature tensor built from the second connection to zero, interpreting this as saying that the space-time is "physically flat." This, in turn, implies that the curl of a certain vector, linked to the difference between the two connections, is also zero and thus that the vector can be chosen to encapsulate the electromagnetic field. Henri Eyraud, to whose work we shall return, following Schouten 1923, uses an asymmetric connection, whose antisymmetric part incarnates the electromagnetic tensor. Einstein introduces, in I, both a general metric and an independent general connection, the antisymmetric part of the metric serving to represent electromagnetism; his field equations are then obtained by *separate* applications of a variational principle to the metric and the connection.

The second new feature is the appearance of quantum theory as an integral part of some unification schemes. Though quantum matter had already appeared as a programmatic goal for a few authors (Haas, Mie, De Donder) at the beginning of the decade, there are in 1925 — and have been since 1922— more technical attempts to combine gravitation with the 'old' quantum theory of Bohr (1913) and Sommerfeld (see Sommerfeld 1919), specifically using their rules for determining the orbital parameters of electrons in hydrogenic atoms. While 8 of our papers take quantum theory as a main component of the unification, the compatibility with quantum effects, or the possibility of including quanta in a theory unifying gravitation and electromagnetic phenomena, is alluded to in more than a dozen others.

Let us now return to the relationships between these articles. As already indicated, a first kind of reference, that used to situate the article's immediate antecedents, structures our corpus into a number of short-term traditions; their choice can in some cases be linked to a direct, personal or institutional, relation, but there is no systematicity in the process. Another kind of citation is used, for instance, in dealing with a technical point, but, unlike the 1920 corpus, the 1925 papers do not permit an evaluation of their importance for the formation of social configurations. Finally, a third type of reference, more discursive and prominent in the review articles and letters to the editor of journals (6 such in 1925), as well as in the introductions to some of the other articles, allows us to scrutinize how the authors themselves represent the organization of the topic, how they envision the main choices offered to them. From this last, we obtain not one classification, but two: one based on the role that matter plays and the nature of its representation, the other on the degree of geometrization.

Structuring unified theories according to this double criteria may seem familiar; but we would like to clarify its specific historicity. First of all, while such classifications, of course, could be applied retrospectively, for example to 1920, our point is that they were put forward by only a minority of authors at this moment and that they were not operational in configuring the links among the articles. For instance, in 1920, the opponents of relativity theory were not necessarily explicit opponents of a geometrical approach, nor the contrary.[50]

Secondly, these two classifications are today often rigidly articulated: geometry associated with a preference for a field approach and a continuous conception of matter, quantum program seen basically as non-geometric.[51] If this identification may be relevant for later periods and will be explicitly promoted by both partisans of quantum theory and of general relativity, we would like to stress emphatically that it does not fit our 1925 corpus. We find, on the contrary, all combinations: the most analytical, non-geometric aspect of general relativity associated either with quantum theory (Kudar I, II,III) or with a concern for purely classical matter (Reißner I, II); geometrization of gravitation and electromagnetism without any reference to matter (Arakatsu) or linked to an ambitious quantization program (Vallarta). We will thus discuss and illustrate separately both axes.

The first alternative, then, is articulated around the opposition between those programs that include quanta as an integral part of the unification scheme and those based on a unification of gravitational and electromagnetic fields from which one hopes to derive a theory of matter. The two positions are well described at the beginning of a paper by Hans Reißner:

> Either one holds that time has yet not come and defers the solution [of the problem of electron and nucleus] until the perhaps identical sources of the still more mysterious quantum laws attached to the names of Planck, Einstein and Bohr are uncovered.
> Or one holds that a solution created on the foundations of Maxwell, Lorentz, Mie, Einstein, Hilbert, Weyl, etc. is possible[52] (Reißner I, 925).

Reißner, who situates himself in the second camp, tries to derive some essential properties of the electron and proton within the Einstein-Weyl framework by introducing into the electromagnetic source term an auxiliary non-Maxwellian tensor. Another attempt that had been made by Einstein in 1923 (Einstein 1923a) is reported on in an expository paper by Rolin Wavre. As Wavre explains it, Einstein had tried to derive the behavior of matter from the field, by providing extra partial differential equations in order to overdetermine the initial state, that is to be able to "define quantum physics inside relativistic physics like a species in a genus by the adjunction of a specific character" (Wavre, 300).[53] Einstein's paper in our corpus (Einstein I), that we have mentioned above — as well as Einstein's 1919 attempt discussed in the preceding section — belongs to the same trend.[54] After his derivation of the field equations for the metric and the connection, he presents, as a future question to be settled, the possible appearance in this theory of an electrically charged mass with spherical symmetry and without singularity, a solution that would represent an electron.[55]

An example of the opposite position — that of treating quantum theory as a domain entering into the unification from the outset — is given by the series of three articles by Johann Kudar. Their aim is to show how the Sommerfeld quantum relations for conditionally periodic systems and the Bohr frequency relation can be used in the context of general relativity to derive the gravitational redshift law from first principles, without any use of the Einstein assumption that the proper frequency rate of atomic clocks is independent of the gravitational field. The result is thus established for those areas in which the "old" quantum theory gives reasonable results — hydrogenlike atomic spectral series and Deslandres-Schwarzschild band spectra — and Kudar clearly feels the way is open towards a program of replacing the phenomenological aspects of relativity theory with exact quantum mechanical principles.

An even more explicit promoter of this position is Vallarta, who announces a program to integrate Einstein's theory of gravitation and Bohr's approach. Vallarta's aim is to counter the objection that Sommerfeld's treatment of the fine structure of matter is not derivable in a unique way from special relativity by offering a well-defined approach from the standpoint of general relativity, which reduces to Sommerfeld's results in a weak gravitational field. He uses Nordström's solution for a static charged particle and the Weyl-Eddington equation of electronic motion to show that the curvature of the material field associated with the nucleus almost vanishes and that its field is nearly static, allowing him to treat an atom as a relativistic one-body problem. The paper, presented at the Annual Meeting of the American Physical Society, ends with a promise of a future, more thorough unification of quantum theory and general relativity, one which would resolve in particular the then pressing quantum problem of "unmechanical orbits" in atoms of more than one electron.[56]

We come now to the second alternative, that which concerns the relationship between geometry (and, more widely, mathematics) and physics. Though we have met this question first in Haas' 1920 paper and in Freundlich's answers, it seems no longer to be an important point of controversy for German-speaking authors in 1925. On the other hand, the issue is now quite acute among the English specialists, the two main camps being incarnated, to caricature only slightly, by Eddington on one side and Lodge on the other.[57] For example, a letter from Lodge to the editor of *Nature* comments, with his typical irony, on a 'pro-geometric' and 'anti-ether' lecture by James Jeans on the "present position in physics."

> Dr Jeans makes it clear that in his view the terms ether and force are unnecessary since all that they connote can be represented equally well by pure geometry ... It is marvellous

> what hyper-geometry can be made to express, and what high reasoning about reality can be thus carried on (Lodge, 419).

This distrust of geometry is also expressed by a mathematician, Alfred North Whitehead. He comments favorably on George Temple's lecture at the Physical Society of London, in these terms:

> In investigating the laws of nature what really concerns us is our own experiences and the uniformities which they exhibit, and the extreme generalizations of the Einstein method are only of value in so far as they suggest lines along which these experiences may be investigated. There is a danger in taking such generalizations as our essential realities, and in particular the metaphorical "warp" in space-time is liable to cramp the imagination of the physicist, by turning physics into geometry (Whitehead *apud* Temple, 193).

An opposing position is defended by James Rice, whose paper (I) begins with a resume of some of Eddington's theses, in particular his principle of identification:

> If any further advance be made in physical science, conforming presumably to the Principle of Relativity and therefore involving the introduction of fresh tensors in its mathematical formulation, it may be possible to discover geometrical tensors possessing by virtues of identities just the same properties as the newly introduced physical tensors possess by reason of experimental facts (Rice I, 457).

This principle suggests then to Rice a system of natural units,[58] about which he states in his introduction that

> such an application finds but little favour in certain quarters where it is described as a geometrization of physics. ... The radius of curvature of [the Einstein-Eddington world] should be the natural unit of length on which the units employed in the geometrical tensors should be based. But if there be an underlying connexion between geometrical and physical tensors, such a fundamental unit of length might conceivably lead to the discovery of natural units in which to measure physical quantities (Rice I, 457–458).

To accord the preeminent place to geometry — or, inversely, to physics — is not only a theme for philosophical debate, it is put into action in the scientific work itself. But the ways in which this comes about vary and it is difficult to delineate possible corresponding solidarities; both physicists and mathematicians appear on each side, neither position seems to be linked to a specific group or place. Still, we may detect subtler links between the role accorded to geometry and the task of unification itself. To illustrate this, we shall present four cases, two in which the privileged place falls to geometry and two to physics.[59] We have chosen them to be as close as possible to each other in order to bring out the exact point at which this hierarchizing intervenes;[60] all four use the basic principles of Riemannian geometry, set themselves the task of integrating gravitational and electromagnetical fields and ignore the problem of the constitution of matter.

An extreme position is held by the mathematician George Rainich (I–III), who presents a new departure for unified field theory with his claim that general relativity already contains a unification of gravitation and electromagnetism.[61] After having underlined that, in standard general relativity,

> gravitation may be said to have been 'geometricized' — when the space is given, all the gravitational features are determined; on the contrary it seemed that the electromagnetic

> tensor is superposed on the space, that it is something external with respect to the space, that after space is given the electromagnetic tensor can be given in different ways (Rainich III, 106).

and pointed out that the attempts by Weyl and others to remedy this situation have failed as physical theories, Rainich remarks that the tensors of gravitation (the Einstein tensor G^i_j) and electromagnetism (the electromagnetic tensor F^i_j and its dual ${}^*F^i_j$) are connected through the "energy relation,"

$$G^i_j = F^i_s F^s_j - {}^*F^i_s {}^*F^s_j$$

and proceeds to study this relation more closely in the framework of classical Riemannian geometry, making essential use of the algebraic classification of tensors.

> The result of this study is quite unexpected; it is that under certain assumptions, the electromagnetic field is entirely determined by the curvature of space-time; … without any modifications it takes care of the electromagnetic field, as far as "classical electrodynamics" is concerned (Rainich III, 107).

Our second example, Eyraud (II, III), is drawn from within the Weyl-Eddington affine tradition and shows how a maximal form of geometrization can be displayed by means of term-by-term identification between geometrical and physical magnitudes — that is, through the Eddington principle.[62] Eyraud's point of departure, as we have pointed out earlier, is an asymmetric connection, Γ^j_{ik}, whose antisymmetric part, Λ^j_{ik}, is the torsion, in Cartan's sense, of the four-dimensional space-time; R_{ik} as usual denotes the Ricci tensor. Eyraud *defines* the electromagnetic field as

$$E_{ik} = \frac{1}{2}\left(\frac{\Gamma^s_{is}}{x^k} - \frac{\Gamma^s_{ks}}{x^i}\right)$$

and the gravitational field as

$$K_{ik} = \frac{1}{2}(R_{ik} + R_{ki}).$$

Then the field equations are obtained from a variational principle applied to a *unique* Lagrangian, constructed as a function of E_{ik} and K_{ik}. Eyraud shows in particular that there exists a covariant vector Λ_k, such that the torsion can be written

$$\Lambda^j_{ik} = -\frac{1}{3}\left(\delta^j_i \Lambda_k - \delta^j_k \Lambda_i\right),$$

and that the symmetric part of the connection is given by

$$\frac{1}{2}\left(\Gamma^j_{ik} + \Gamma^j_{ki}\right) = \left\{ {j \atop ik} \right\} + \frac{1}{6}\left(\delta^j_i \Lambda_k + \delta^j_k \Lambda_i\right).$$

In other terms, he proves that the connection is semisymmetric (an *hypothesis* in Schouten 1923) and also that the space-time has the same geodesics as a Riemannian space. Eyraud also extracts from his results some complementary geometrical interpretations of physical terms, for instance that the "potential vector finds its geometrical expression in the torsion"[63] (II, 129). Here too the geometrization of the physics is complete.

What do the cases where physics takes the lead look like? George Temple, our first example, follows a path opened up by Ludwig Silberstein (1918) and Alfred North Whitehead (1922), who both rejected the dominant role accorded to gravitation in Einstein's theory and its variants, a role based on its privileged relation to the space-time metric.[64] Temple distinguishes between two geometries: one, the "true" geometry, corresponds to the geometry of the real world, assumed to be a space with a metric dG^2 of constant curvature;[65] the other, the "fictitious" geometry, is a convenient tool which allows one to deal with physical dynamics. This dynamical manifold is represented, again strictly following Whitehead (Whitehead 1922, 79–82), by the "potential mass impetus," dJ, and the electromagnetic potential dF created by a particle of mass M and charge E, both taking the form of a metric,

$$\begin{aligned} dJ^2 &= dG_M^2 - \frac{2}{c^2} \sum \psi_m \cdot dG_m^2, \\ dF &= \sum_\mu F_\mu \, dx_\mu, \qquad \mu = 1, 2, 3, 4. \end{aligned}$$

The dG_M^2 and dG_m^2 are the line elements in the *true* manifold along the path described by the corresponding particles of mass M or m, the sum on m being extended to all particles in the causal future of the particle of mass M; ψ_m is a retarded potential with a moving singularity due to the particle of mass m, which is intended to express the "law of the diminishing intensity of the perturbing influence of other particles" (Temple, 177) on the particle of mass M. Temple then exhibits an explicit expression for the potential ψ_m, by solving a differential equation which plays the role of a field equation; this equation is an empirical compound, borrowed from Silberstein.

The electromagnetic tensor $F_{\mu\nu}$, on the other hand, defined as the curl of the electromagnetic potential F_ν, satisfies the (non-covariant) Maxwell-Lorentz equations:

$$\frac{F_{\mu\nu}}{x_\lambda} + \frac{F_{\nu\lambda}}{x_\mu} + \frac{F_{\lambda\mu}}{x_\nu} = 0 \qquad \lambda, \mu, \nu \quad \text{different}$$

$$F_{(\nu)}^{\mu\nu} =: \frac{F^{\mu\nu}}{x_\nu} + \left\{ \begin{matrix} \nu\alpha \\ \mu \end{matrix} \right\} \cdot F^{\alpha\nu} + \left\{ \begin{matrix} \nu\alpha \\ \nu \end{matrix} \right\} \cdot F^{\mu\alpha} = \frac{4\pi\rho}{c} \frac{dx_\mu}{dx_4},$$

where ρ is the electric charge density, the x_μ are coordinates in the true manifold and indices are raised and lowered using the metric associated with dJ^2.

The path of a particle is then assumed to minimize the integral $\int (MdJ + c^{-1}E \cdot dF)$ along the path. Explicit calculations are made among others for planetary motions, leading to an expression for the perihelion advance equal to the general relativistic value plus a correction term involving the radius of curvature of space, R; supposing that this adds less than 1% to the Einstein value for Mercury gives a lower bound for R ($R > 2.5 \times 10^{16}$ km). Using this value to evaluate the corresponding correction to the Einsteinian value for the bending of light rays grazing the sun's surface gives a modification of 2×10^{-8}, "therefore wholly inappreciable" (Temple, 191).

The geometrical setting appears here as a mere framework, whose shape helps to treat the physical material, but not to produce it. Physics plays the central role: equations defining the metric are chosen a priori, as encapsulating experimental observations; the mathematics is then used to transform these initial data into explicit computations, with which in turn to confront experience. The dissociation of the dynamical space from the true space-time robs the two forces of any privileged status among possible phenomena and their coordination takes its legitimacy from observation, not geometry. As Whitehead puts it:

> A further advantage of distinguishing between space-time relations as universally valid and physical relations as contingent is that a wider choice of possible laws of nature (e.g., of gravity) thereby becomes available, and while the one actual law of gravity must ultimately be selected from these by experiment, it is advantageous to choose that outlook on Nature which gives the greater freedom to experimental enquiry (Whitehead *apud* Temple, 193).

In view of the representation of Einstein's work, both by his contemporaries and by some historians, our final example may appear strange: it is Einstein's own second article. Though of course Einstein I, as we have said, is a typical example of a theory in the Weyl-Eddington tradition, a pair of paragraphs at the end raises a point of another order: they purport to settle the question of the physical identification of parts of the electromagnetic tensor by considering the behavior of a solution under time reflection. In the sequel, II, however, Einstein reveals that the real question prompting the examination of this behavior is the difficulty that every negatively-charged solution of a given mass admits an equal-mass, positively-charged solution under the action of time reversal — and in 1925 the very unequally massive electron and proton are the only known charged elementary particles. He meanwhile has obtained an elementary proof that, in any set of covariant equations, no relabeling of the electromagnetic tensor will suffice to resolve the problem since the addition of a space-reflection reinstates the result. Even the requirement of a positive determinant for the transformation cannot help since a combined space- and time-reflection reproduces the difficulty.[66]

Finally, a note added in proof raises the point that, since the electric density ρ is equal to a square root, $\sqrt{g_{ik} \cdot F^{ir}/x^r \cdot F^{ks}/x^s}$, with its inherent ambiguity of sign, such unwanted symmetry is inescapable — unless the sign of ρ can be fixed within the field tensor. And this is possible if electromagnetism can be constructed with an attached "arrow of time." But this cannot be done in the gravitational case and Einstein surprisingly ends with the following:

> The conclusion seems to me to be essentially that an explanation of the disparity of the two electricities is only possible if a directional arrow is ascribed to time and this is then used in the definition of the principal physical quantities. In this respect, electromagnetism is fundamentally different from gravitation; thus it appears to me that the attempt to fuse electrodynamics and the laws of gravitation into a unity is no longer justified[67] (Einstein II, 334).

Confronted by a contradiction between geometry — for Einstein, as often, incarnated by tensors and symmetry conditions — and physics, Einstein does not consider new experiments to detect the particles predicted by the theory; on the contrary, he abandons (for a short moment as it turns out) the search for a unified field theory. In this classical setting, the retreat of geometry before physical considerations leads to a disruption of the unification program.

1925 Corpus

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4. 1930

The last year of our *sondage* shows a massive increase in the number of unified theory publications: 57 articles written by 30 authors (or couples of authors).[68] Among these articles, 15 (by 9 authors) appear in *Zeitschrift für Physik*, 10 (5 authors) in *Proceedings of the National Academy of Science*, 5 (4 authors) in *Physical Review* and 5 (5 authors) in *Physikalische Zeitschrift*; other journals are represented by one or two authors each. This distribution manifests the new importance of the US scene for this subject. We would also point out that all the articles published in *Physikalische Zeitschrift* are summaries, written in German, of contributions to a conference held at Kharkov, in the Soviet Union, during the week from 19 to 25 May 1929.[69] Besides the participants in this conference, two other Soviet physicists make contributions to this year's corpus.[70] The other authors work in various European countries (France, Germany, Great Britain, Italy, Romania, Bulgaria, Hungary), as well as in China and India. But, as we shall see — and in opposition to the situation in 1920 — geographical location, either of publication or of residence, though in 1930 still significant insofar as they facilitate teamwork, are far less so for questions of visibility or citation of the articles of others.

Though the mean percentage of papers per author seems to have remained stable for the three years of our *sondage*, in this year we find more frequent traces of long series of papers and notes: 5 in our corpus authored by Einstein alone or with his assistant of this period, Walther Mayer, following 4 others by him directly connected to the same theme in the preceding months; 5 by Igor Tamm and his coauthors; 4 by Manuel Sandoval Vallarta and the MIT group, out of a series of 7; 6 by Tracy Yerkes Thomas; 5 by Gavrilov Raško Zaikov, the earliest in our corpus being, in fact, the last in a series of 6. This quick succession of articles is, in part, due to the reactions of readers to the first papers of the series and to the objections that are raised. Interactions, at least in an important subgroup in our corpus, are in 1930 effective and prompt, even sometimes hasty.

Moreover, in clear opposition to the two other years we have studied, a single theory dominates the scene. In 1928 Einstein had launched the unified field theory approach that was to attract the widest attention of any he was to put forward until his final attempt of 1945–1955, the theory of distant parallelism (*Fernparallelismus*). Newspapers as well as scientific journals welcomed articles on the question; the *New York Times* of 3 February 1929 put Einstein's photograph, together with a long article on "his new discoveries," on the first page of their Sunday supplement; it was followed by the London *Times* of 4 and 5 February and numerous other newspapers.[71] The theory also triggered a large number of more technical articles; witness the fact that 42 of our 57 papers — 21 out of our 30 authors — explain, comment, modify, combine, or complete the theory of distant parallelism.

In this theory, Einstein uses a kind of metric space in which a notion of parallelism between vectors (in the tangent spaces) at two distant points can be defined. He also introduces a new formalism, that of the *Vierbein* h^{ν}_{a}, which represents a local orthonormal coordinate system (Latin letters index the different vectors of the system, Greek letters the components of each).[72] Parallel transport is defined by the exact differential

$$dA^{\nu} = -h^{\nu}_{a} \frac{h_{\mu a}}{x^{\sigma}} A^{\mu} dx^{\sigma},$$

with $h_{\mu a}$ such that $h_{\mu a} h^{\nu}_{a} = \delta^{\nu}_{\mu}$.[73] The torsion $\Lambda^{\nu}_{\alpha\beta}$, the antisymmetric part of the associated connection, is non-zero, while the Riemann curvature tensor is shown to vanish identically. The metric is given by $g_{\mu\nu} = h_{\mu a}\, h_{\nu a}$, and thus it is determined by the *Vierbein*, though the converse is not true. In particular, the 16 components of the *Vierbein*, compared to the 10 of the metric, offer a latitude which nourishes Einstein's hopes to fit electromagnetic phenomena into the theory; e.g., in his early papers on the subject, by viewing the trace of the torsion, $\Lambda^{\nu}_{\alpha\nu}$, as the electromagnetic potential vector.

Such a proposal, of course, seems quite close to some attempts already mentioned in the Weyl-Eddington lineage, and enters naturally into the general framework of affine spaces proposed by Élie Cartan as early as 1922 (Cartan 1922, 1923–25).[74] It seems at first sight only one of many possibilities to explore. Why, then, its particular attraction?

First of all, the classification of affine spaces is not considered to be obvious, nor is it common knowledge; Hans Reichenbach devotes the first half of his epistemologically oriented paper in our corpus to explain the relationship between Riemannian geometry and the geometry used in *Fernparallelismus*. Again, the introductory paper to the Kharkov conference by Vsevelod Frederiks and A. Isakson is precisely devoted to such a classification of metric spaces, partly inspired by Reichenbach's paper, partly by the classification given by Schouten in his 1924 *Der Ricci-Kalkül*. Einstein himself, in his third paper of our corpus, presents his theory, not as a kind of affine space, but as an *intermediate* case, situated between Riemannian and Euclidean geometry. Whereas Weyl's geometry provides no possibility of comparing either lengths or directions of vectors at a finite distance, and Riemann's only permits a comparison of lengths, the new geometry, like the Euclidean case, allows both, and Einstein proudly announces that he has found "a metric structure for the continuum which lies between the Riemannian and the Euclidean."[75] (Einstein 1929a, 130).

But there is more than a question of geometry in play. The field equations proposed by Einstein yield classical equations of gravitation and of electromagnetism only to first order.[76] For some indeed, this novelty constitutes a drawback of the theory, the experimental confirmations of general relativity being apparently lost with no compensatory gain.[77] But others express the opposite opinion. In Reichenbach's view, for instance, *Fernparallelismus* appears not only as a formally satisfying unification, but as a real cognitive advance over previous attempts, precisely because it is not reducible to Einstein's earlier theory of gravitation. Many physicists and mathematicians are in agreement; the theory is not seen simply as one of many, but as a creation of Einstein on a par with his earlier work, the next step beyond the special and the general relativity theories. In more than one instance, it is alluded to by others as "the" unified field theory, with at most a vague mention of a few analogous projects in the past — hence Weyl's annoyance mentioned in the introduction. Even those well-versed in some of the preceding attempts, like Zaikov, distinguish it from the others:

> But a return to the old theory of relativity (four-dimensional ... as well as five-dimensional ...) appears to be excluded once and for all. With the idea of *Fernparallelismus* a real step forward in our knowledge has been taken![78] (Zaikov II, 835).

Paradoxical as it might seem, a last feature favoring this overwhelming interest lies in the new Dirac quantum theory. Paul A. M. Dirac's (special-) relativistic theory of the electron was published in 1928 (Dirac 1928) and changed the topography of unified theories in important ways. Physically, it showed how one could couple a quantum charged particle to the electromagnetic field. Mathematically, it introduced *spinors* (*semi-vectors* for most of our authors at this time) as the mathematico-physical objects necessary to do this, in the context of a linear first-order differential equation. Different lines of inquiry were then explored to integrate a theory of gravitation with it.[79] From the start, *Fernparallelismus* appeared as a very promising candidate; this combination is indeed the very theme of the Kharkov conference and, among the 42 papers of our corpus devoted to distant parallelism, 9 (involving 3 authors) concern just such a combination. The hope is perfectly expressed by Norbert Wiener and Vallarta in a letter to the editor of *Nature* dated 7 February 1929.[80]

> May we be permitted to direct attention to a certain aspect of Einstein's three recent papers ... on distant parallelism which came to light in a discussion with Prof. D. J. Struik? The avowed aim of these papers is to develop an improved unified field theory of electricity and gravitation. A much more pressing need of general relativity theory is a harmonisation with quantum theory, particularly with Dirac's theory of the spinning electron. On the basis of Levi-Civita's parallelism the task seems hopeless, inasmuch as we have no adequate means of comparing spins at different points. On the other hand, the notion of a parallelism valid for the whole of space and of Einstein's n-uples enables us to carry over the Dirac theory into general relativity almost without alteration ... .
>
> ... The quantities ${}^s h_\lambda$ of Einstein['s *Fernparallelismus* theory] seem to have one foot in the macro-mechanical world formally described by Einstein's gravitational potentials and characterised by the index λ, and the other foot in a Minkowskian world of micro-mechanics characterised by the index s. ... This seems to us the most important aspect of Einstein's recent work, and by far the most hopeful portent for a unification of the divergent theories of quanta and gravitational relativity (Wiener and Vallarta 1929b).

This obvious center of interest, corroborated by the references cited in the papers, imposes a treatment of the papers related to *Fernparallelismus* as our first group.[81] A second smaller group of papers centers around five-dimensional theories, though even here showing important links with the articles on distant parallelism. The remaining papers are much more isolated, in some cases completely marginal, attempts to develop a unified theory, and for the sake of space we shall restrict ourselves to a few comments on them at the end.

What happens in the 42 papers devoted to distant parallelism? The difficulty in describing them is due to their organization; the resulting configuration is roughly star-shaped, with the core constituted by Einstein's series of papers. But a detailed chronology is necessary to follow the quick responses of some authors to Einstein's papers, and to understand the unfolding of events.[82] We will thus begin with the hero of the year, then examine some characteristic answers by others and finally discuss more globally the links between the papers.

Chronologically, the first of Einstein's papers in our corpus is dated 19 August 1929 and published in *Mathematischen Annalen* (Einstein I); his purpose is to establish the mathematical foundations of his *Fernparallelismus* theory and to present it in a way which makes it accessible to specialists of general relativity. While acknowledging that the manifolds used are not new, Einstein underlines the importance and originality of his work, "the discovery of the simplest field equations to which a Riemannian manifold with *Fernparallelismus* can be subjected"[83] (Einstein I, 685). Einstein posits directly his 22 field equations, without using a variational principle:

$$G^{\mu\alpha} = 0, \qquad F^{\mu\alpha} = 0,$$

where

$$G^{\mu\alpha} = (\Lambda^{\alpha}_{\beta\gamma} g^{\mu\beta} g^{\nu\gamma})_{;\nu} - (\Lambda^{\sigma}_{\beta\gamma} g^{\mu\beta} g^{\tau\gamma}) \Lambda^{\alpha}_{\sigma\tau}$$

and

$$F^{\mu\nu} = (\Lambda^{\alpha}_{\beta\gamma} g^{\mu\beta} g^{\nu\gamma})_{;\alpha}.$$

Here, as before, $\Lambda^{\alpha}_{\beta\gamma}$ is the antisymmetric part of the connection and the semicolon notation indicates a covariant derivative with respect to the affine connection associated with the *Vierbein*. In this case, the field equations, in first approximation, reduce to the classical Poisson (sourceless) gravitational equation and to the vacuum Maxwell equations. In his talk given at the Institut Henri Poincaré in Paris in November 1929, transcribed by Alexandre Proca (Einstein II), Einstein describes at length a heuristic path to obtain these equations, by first annihilating the various divergences built from the covariant derivatives of the torsion, then correcting the equations thus obtained by a careful examination of identities among the $\Lambda^{\alpha}_{\beta\gamma}$; in particular, he succeeds in giving a sufficient number of relations between these equations in 12 variables to ensure compatibility. Indeed, the overdetermination of the field equations appeals to him as much as it did in 1923: "The great charm of the theory for me lies in its unitarity and in the high (authorized) degree of its overdetermination"[84] (Einstein I, 697).

The determination of the relations among the field equations becomes the focus of Einstein's interest during the following months, and the core of his correspondence with Cartan, using the mathematician's suggestions to simplify and correct his previous publications (Debever 1979, Biezunski 1989). In the first of two notes to the Berlin Academy of Science, in January and July 1930 (Einstein III, V), he stresses once more the interest of overdetermination: "The higher the number of equations (and consequently also of the identities among them), the more the theory makes definite assertions beyond the requirement of mere determinism; and thus the more valuable the theory is, provided it is compatible with experimental facts"[85] (Einstein III, 21).

Once the field equations are obtained, the path of investigation is clearly drawn, as is explained repetitively by Einstein and his followers in their research papers and in the more popular expositions of the theory: first, to discover suitable solutions of the field equations that will represent elementary particles; secondly, to determine the laws of motion in these spaces; finally, to test these last through experiment.

The first problem is studied by Einstein in his joint paper with Mayer in February 1930 — and as we shall see, in the work of quite a number of other authors as well — where exact solutions are found in two specific cases: that of spherical and mirror symmetry, and that of a static, purely gravitational, field. In the first, which corresponds to the physical situation of the external field of a charged massive sphere, there appear exactly two constants, prompting, in a satisfying manner, a natural interpretation of them as mass and charge. The second case leads to a solution which is, at first sight, rather discouraging; any arbitrary distribution of non-charged masses will remain at rest! The authors, undismayed, point out that since the laws of motion of singularities in this version of the theory cannot be derived from the field equations, such a solution provides no argument against its applicability: "One should surely recognize that, in the new theory, solutions which will represent the elementary particules of matter must be required to be free from singularities"[86] (Einstein and Mayer, 120).

But the laws of motion remain elusive, and, with them, questions of experimental verification. On 11 April 1930, William Francis Gray Swann reports in *Science*:

> It now appears that Einstein has succeeded in working out the consequences of his general law of gravity and electromagnetism for two special cases .... It is hoped that the present solutions obtained by Einstein, or if not these, then others which may later evolve, will suggest some experiments by which the theory may be tested (Swann, 390).

We are still awaiting them.

But even viewed from 1930, the quasi-success story provided by Einstein's rational reconstruction of his own work and reinforced by the nature of our selection process seems already overtidy; between 7 June 1928, the date of the Berlin Academy of Science gathering which includes Einstein's first article on *Fernparallelismus* (Einstein 1928b), and 19 August 1929, the date of the first paper of his in our corpus, Einstein proposes in fact three different *Fernparallelismus* theories, with varying sets of field equations (Einstein 1928a, 1929b, c and I).[87] And despite his pride and pleasure, cited above, in the August 1929 equations, Einstein does not stop there: in June 1931, after a long silence, he writes to Cartan: "Meanwhile I have been working a great deal with Dr. Mayer on the subject and I have abandoned those field equations."[88] (Einstein 1931). This comedy of errors has its impact on the work of other authors: some try to work by sticking close to Einstein's heels, following his change of equations as soon as they are produced; some study specific problems associated with one set of field equations; some, finally, try to reformulate the theory and obtain their own field equations.

A first example of such a reframing of *Fernparallelismus* theory is presented at the Kharkov conference by the Leningrad physicist Heinrich Mandel', and then developed in a longer article in June 1929 (Mandel' I, II). He treats the relationship between distant parallelism and his own theory of 1927, a five-dimensional theory of Kaluza-Klein type with a cylindricality condition:

> According to the fundamental idea of the five-dimensional theory, the set of the ∞^4 world-points is not to be considered as the set of ∞^4 points of a hypersurface in [the five-dimensional space] R_5, but as the set of the ∞^4 lines (of the congruence X_5^i) in the cylindrical R_5[89] (Mandel' II, 240).

As at every point, the local *Vierbein*, $X_1^i, X_2^i, X_3^i, X_4^i$, may be oriented orthogonally to X_5^i, it is possible to choose as the fundamental tensor $g_{ik} = \sum_{\rho=1}^{4} X_\rho^i X_\rho^k$. The geometry of the four-dimensional space-time is thus to be understood as the geometry of the projection of the *Vierbein* on the axes $X_1, \ldots, X_4$. Mandel' describes, for a special case, parallel displacement in such a geometry. He then expresses the fact that the Riemann curvature tensor is zero in terms of the five-dimensional curvature tensor and derives field equations, easily interpretable as gravitational equations. Morever, Mandel' suggests a geometrical interpretation, not of the potential vector, as Einstein has proposed up to this point, but of the electromagnetic field itself. Defining $F_{\mu k}^i$ as the difference between the connection of the space with distant parallelism and the classical Christoffel symbols, Mandel' shows that the equation of the geodesics (here the straightest lines), if interpreted as equations of motion of a charged body of charge e and mass m, leads to $F_{\mu k}^i \dot{x}^\mu = 2(e/m)M_k^i$, with $2M_k^i$ the tensor of the electromagnetic field.

Another interesting example is the series of articles (I–VI) by the mathematician Tracy Y. Thomas, communicated to the *Proceedings of the National Academy of Sciences* between 30 September 1930 and 2 April 1931. Thomas, a former student of Oswald Veblen, is a member of the Princeton group of differential geometers. Their trademark has been, for the previous decade, the development of a differential geometry starting from *paths*, i.e., autoparallel curves, rather than the more usual connection, as the fundamental geometric objects.[90] In his first note, Thomas axiomatically reformulates the fundamentals of the theory of spaces with distant parallelism. He requires the existence at each point of a local system of coordinates (z^i) $(i = 1, \ldots, 4)$ such that the coordinate axes z^i are tangent to the vectors h_i^α of the *Vierbein*; the local metric is Minkowskian, with z^1 the time-coordinate, the paths of the space are straight lines in the local coordinates. He then expresses the covariant derivative in terms of the local system of coordinates, thus privileging another choice for the differentiation of tensors than that of Einstein. Thomas then *postulates* a system of 16 field equations analogous in local coordinates to a 4-dimensional wave equation:[91]

$$h_{j,11}^i - h_{j,22}^i - h_{j,33}^i - h_{j,44}^i = 0,$$

where the covariant derivatives $h_{j,kl}^i$ are given by

$$h_{j,kl}^i = \left(\frac{\partial^2 \left(h_\alpha^i \frac{\partial x^\alpha}{\partial z^j} \right)}{\partial z^k \partial z^l} \right)_{z=0}.$$

Thomas then suggests possible interpretations for the electromagnetic potentials in terms of the covariant components h_α^i of the *Vierbein* and for the gravitational potentials in terms of the coefficients $g_{\alpha\beta}$ of the metric. As Thomas comments in an endnote:

> There is a certain psychological influence exerted by the method itself upon the investigator … . So, for example, the field equations proposed by Einstein have a very simple analytical form in terms of the covariant derivative used by him … also the simple form of the field equations assumed in the above investigation is peculiar to the method of absolute differentiation which I have adopted (Thomas I, 776).

In his second note, however, Thomas himself modifies his field equations, displacing his emphasis from the obtaining of exact Maxwellian equations in the local system to that of a law of conservation. More precisely, he now sets to zero the divergence — a notion that Thomas has to redefine in his framework — of what corresponds to the electromagnetic forces, $\nabla_k h^i_{j,k} = 0$, introducing changes of second order in the $h^i_{j,k}$ in his former field equations. The following notes are then devoted to a standard, analytic, study of these new field equations. Thomas establishes, in particular, a general existence theorem of the Cauchy-Kovalevskaya type; examines, in the Hadamard tradition, the characteristic surfaces, "which appear to an observer in the local system as a spherical wave propagated with unit velocity." (Thomas IV, 112); and, finally, shows that the null geodesics are the light paths in this unified field theory, as in general relativity.

The range of activities around one set of field equations is examplified by R. N. Sen and George McVittie. The first takes up some remarks made by Edmund Whittaker during his presidential adress to the London Mathematical Society on 14 November 1929 (Whittaker 1930), according to which Clifford parallelism in a three-dimensional space of constant curvature is a distant parallelism in Einstein's sense. Sen writes down the January 1929 Einstein equations (Einstein 1929b) in the static case, for the metric $ds^2 = V^2\,dx_0^2 - \sum_{p,q=1}^{3} g_{pq}\,dx_p dx_q$, with the constant V representing the velocity of light; he computes the *Vierbein* when the three-metric represents a space of constant curvature and the distant parallelism is Clifford parallelism, proving that this case is exactly that of Minkowski space. As for McVittie, his aim is to compare, for the special case of the gravitational field of a uniform electrostatic field, Einstein's approach (with the January 1929 equations) to the alternative proposed by Levi-Civita in March 1929 (Levi-Civita 1929), using Ricci's tool of orthogonal congruences of lines in an (ordinary) Riemannian space, instead of Einstein's *Vierbein*. McVittie computes a solution in the Levi-Civita framework, showing it to be in agreement with the solution he has already found with the Einstein approach (McVittie 1929) and deduces, for this particular case, a geometrical interpretation of the electromagnetic potential vector in Levi-Civita's theory.

Finally, to grasp what it concretely means to match Einstein's pace, let us follow a group of young MIT mathematicians and physicists: Vallarta, Wiener, Dirk Struik, Nathan Rosen.[92] We have already cited the program of unification between Dirac theory and *Fernparallelismus* presented by Vallarta and Wiener in February 1929 (Wiener and Vallarta 1929b). (In fact, of this program they will publish only the part concerned with solutions of the classical *Fernparallelismus* theory). On 1 March 1929, they send to the *Proceedings of the National Academy of Sciences* a joint paper (Wiener and Vallarta 1929a) on the (non-) existence of a spherically-symmetric static field solution to Einstein's first set of field equations (Einstein 1928a).[93] Supposing the components of the *Vierbein* (in spherical coordinates) to be functions of the radius r alone, and assuming a time symmetry of past and future,[94] they show that both the electromagnetic and the gravitational field vanish. On 26 June 1929, Vallarta, this time alone (Vallarta I), explores the same problem for the second set of Einstein's field equations, those of January 1929; the result for the electrostatic field is the same. Indeed: "[its vanishing is] a consequence of the definition of the electromagnetic potential and is independent of the particular choice of field equations." (Vallarta I, 787). Moreover no Schwarzschild-like solution can be found for the gravitational part.

A blow falls at the New Year; a letter to the editor of *Physical Review* by Meyer Salkover, of the University of Cincinatti, points out an error in Vallarta's paper and exhibits a Schwarzschild solution (Salkover I), a second letter (11 January) completes the study of the solutions (Salkover II). On 3 February, Vallarta acknowledges his mistake (Vallarta II), pointing out, however, that his conclusion for the electrostatic field remains valid. In the meantime, according to Vallarta, Wiener has checked the validity of the usual Schwarzschild solution in the context of Einstein's March 1929 paper (Einstein 1929b) and of Levi-Civita's variant (Wiener's results were apparently never published, perhaps because, as we shall see, he was forestalled by a quicker team).[95]

The game ends on 15 May 1930; a last paper of Vallarta, this time in collaboration with Nathan Rosen, takes over a last set of Einstein field equations, those of August 1929 (Einstein I), both with and without the assumption of time-symmetry. In the second case, they find, up to a change of variables, the Einstein-Mayer solution; they interpret the inherent nonseparability of electric and gravitational fields in the immediate neighborhood of a charged mass in this theory as a possible explanation of nuclear and electronic stability. In the time-symmetric case, they obtain, as in their earlier papers, a pure gravitational solution, though none with charge.

> Thus the existence of an electrostatic field in the unified theory depends on the asymmetry of past and future. We believe that this is the first instance that this asymmetry has been found to have any physical significance in connection with a field theory. The existence of the gravitational field, on the other hand, is apparently not connected with this asymmetry. We may perhaps have found here the fundamental difference, superficial similarity notwithstanding, between the gravitational and the electric field of a charged mass particle.
>
> ... In the absence of a law of motion, not yet discovered, the path of an exploring particle in the unified field cannot be calculated. ... The shift of spectral lines towards the red, on the other hand, does not depend on the law of motion of an exploring particle, but only on the component g_{44} of the Riemann metric. ... The red shift obtained on the basis of the present theory is the same to a first approximation as that predicted on the basis of the 1916 theory (Rosen and Vallarta, 119–120).

The quicker team alluded to above is that composed of the Moscow physicists Igor Tamm and Mikhail Leontovič. Like the MIT group, Tamm also has a program to unify quanta and distant parallelism: first find a generalization of the Dirac equation in spaces with distant parallelism which will serve as an equation of motion (Tamm I and II);[96] then determine solutions of the *Fernparallelismus* field equations to act as a source in the modified Dirac equation (Tamm and Leontovič I and II).

Tamm's point of departure is the usual Dirac wave equation in the absence of an electromagnetic field, $({}^{s}\alpha\, p_s + imc)\psi = 0$ — the Pauli-matrices ${}^{s}\alpha$ are, as usual, the components of a constant q-vector and the p_s are the momentum operators. In the presence of an external electromagnetic field, these operators are transformed in the usual Dirac theory by the addition of an interaction term coupling the wave function to the electromagnetic potential. Tamm proposes to treat the problem in spaces with distant parallelism by means of two hypotheses: the components of the q-vector *relative to the Vierbein* will be taken to be constant, and the usual form of the *free* Dirac equation will be assumed to hold in general — in other words, the geometry of the space will automatically take care of the fields. He thus obtains the equation (Tamm II, 653):

$$\left[\alpha^{\nu}\left(p_{\nu} + (1 - in)iK\Lambda^{\lambda}_{\nu\lambda}\right) + imc\right]\psi = 0,$$

where n is a real number to be determined, α^{ν} is ${}_{s}h^{\nu} \cdot {}^{s}\alpha$, $K = h/2\pi$ is the reduced Planck constant, and the operators p_s are no longer ordinary but rather covariant differential operators.[97] If a proportionality between the trace of the torsion and the electromagnetic potential, $\Lambda^{\lambda}_{\nu\lambda} = a\Phi_{\nu}$, is posited, this becomes a Dirac equation with an interaction term; for a specific value of the product na, Tamm recovers the Schrödinger equation up to second order terms.

> The fact that the procedure sketched out really leads to a reasonable wave equation is all the more interesting because the "classical" formulation analogous to the mentioned wave-mechanical hypothesis — that the motion of the electron relative to the *Vierbein* is always uniform — leads to no useful equations of motion. *Thus the wave-mechanical principle appears, in Einstein's theory, to have priority over the principle of the shortest path of geometrical optics* [98] (Tamm I, 290).

That is, Tamm sees general relativity — and its geodesic equation of motion — as the geometrical-optics limit of an intrinsically wave-mechanical *Fernparallelismus* theory.

The next step then is to obtain a particular solution for the *Vierbein*. For this, as we said, Tamm works in collaboration with Leontovič; they present their joint work at the Kharkov conference (I) and extend it in a longer article a month later (II). Here again, they find a static, spherically-symmetric exact Schwarzschild solution of the Einstein field equations of March 1929, which they now interpret as the ground state of the (neutral) hydrogen atom,[99] but none corresponding to a charged particle. Their interpretation of these results is quite optimistic; the non-existence of a charged solution with spherical symmetry, were it to be coupled with the existence of an axially-symmetric solution, would be a reflection of the fact that the electron possesses spin.[100]

> One of us has recently attempted elsewhere [Tamm I] to show how naturally the electron wave equation arises in the new Einstein theory, and has, in addition, put forward the conjecture that, in this theory, the wave-mechanical principle has priority over the principle of the shortest path, so that the equations of motion of a (charged) particle are to be derived from the wave equation by a limiting process. If this conjecture, as well as the conjecture that the solution of the Einstein field equations corresponding to a charged particle accounts for the spin of the elementary charge, should really be confirmed, then the mi-

> croscopic interpretation of the Einstein theory would be considerably strengthened[101] (Tamm and Leontowitsch II, 356).

There is no lack of criticism in Kharkov when Tamm and Leontovič present their results, in particular from the Leningrad team of Vladimir Fok and Dimitri Ivanenko, who advocate quite another path towards the unification of Dirac theory and general relativity: to develop a geometry of operators and integrate Dirac matrices as a correcting linear term in the metric.[102]

Several other proposals for the reconciliation between quantum and classical theories are put forward in this year. A few months after Kharkov, in September 1929, Gleb Wataghin underlines that all previous attempts to extend Dirac theory to the framework of general relativity rely on the union of Dirac matrices and Einstein *Vierbein*. But while many authors, as we have seen above, have judged a major advantage of Einstein's new equations precisely the fact that they reduce to the old general relativity equations only to first approximation, Wataghin regards them with disfavor for this very reason, at least so long as no experimental evidence will have come to disconfirm the latter. He himself adopts the theory of distant parallelism in the Levi-Civita form and exhibits a Lagrangian as a sum of three terms, but such that a single variational principle allows him to derive the 4 Dirac, 8 Maxwell and 10 general relativity equations. Thus, *Fernparallelismus* appears here mainly as a convenient technical framework, encapsulating physical equations coming from other theories. But, through the interpretation of the various variables, the computations cast light on an interdependence of the three classes of phenomena; Wataghin concludes, in particular, that the gravitational potentials have an essentially statistical significance.

A last example of the combination of *Fernparallelismus* and quantum physics, Zaikov's work, also witnesses the important effort of assimilation made by the newcomers. Like the previous groups, Zaikov has attempted to follow Einstein's exploration of the theory of distant parallelism, as well as its compatibility with the Dirac wave equation. During the autumn of 1929, however, Zaikov proposes a new path (Zaycoff II, III): extend the theory of distant parallelism with one supplementary dimension and operate directly with the ψ-functions. More precisely, his cylindrical five-dimensional geometry is defined with its fundamental covariant components, $H_{\alpha m} = h_{\alpha m}, \;\; H_{\alpha 0} = -f_\alpha, \;\; H_{0m} = 0, \;\; H_{00} = 1$, where the $h_{\alpha m}$ are as usual defined out of the *Vierbein* and the f_α are proportional to the electromagnetic potentials, such that the $h_{\alpha m}$ and the f_α are independent of the fifth coordinate x^0. Zaikov then introduces the ψ-functions (and their conjugates), also independent of x^0, and proposes a Lagrangian such that the variation of the $h_{\alpha m}$, f_α, ψ and its conjugate, $\bar{\psi}$, produces 28 field equations: 16 of second order in the $h_{\alpha m}$ and first order in the f_α and ψ, describing gravitational and spin phenomena, 4 of the second order in the f_α and of the first in the $h_{\alpha m}$ describing electromagnetism. The 8 complementary equations are of the first order in the $h_{\alpha m}$ and the ψ. In October 1930 (Zaycoff V), however, he switches to the new Einsteinian field equations (Einstein I) and is able to derive from his preceding work an equation of the type $R_{\alpha\beta} - 1/2 g_{\alpha\beta} R + T_{\alpha\beta} = 0$, with the quantities R and T suitably defined (in particular, T depends on the component of the *Vierbein* and on the function ψ), thus mimicking those of general relativity. Un-fortunately, the properties of T are very different from those of an energy-momentum tensor, and the conclusion of Zaikov's paper is to call for new concepts to be developed.

The emerging picture of the work centering on Einstein's new theory is thus twofold: on one side, global acute awareness of Einstein's work in progress, but on the other, a constellation of more local debates, joint work and solidarities. There exists no important, very tight, general network of communications among the various protagonists. It is true that the Kharkov proceedings, and the extended versions of the results presented there, are commented and discussed, and the projects of the Soviet groups are faithfully followed by most of the contributors, but no other reciprocal impact is to be seen. We have located a few teams and competitors, in Boston and Princeton, Moscow and Leningrad. Besides the leadership provided by Einstein himself we find more elusive traces of activities inspired by various local leaders, like Whittaker (Sen) or Eddington (McVittie). But we have no evidence from the mutual references in our corpus of, say, direct scientific links between the MIT group and the Princeton geometers, nor is there a specific relationship between the various physicists and mathematicians trying to combine quantum theory and *Fernparallelismus*; neither nationality nor technical orientation are a warrant for effective relationships. A small exception, for the second case, is the constellation around five-dimensional theories, Zaikov referring to the work of Mandel' for instance.[103]

As indicated at the beginning of this section on 1930, the other 18 articles are much more isolated, both scientifically and socially. They compose a digest of most of the programs we have previously met — except that quantum phenomena are, at least as an horizon, a part of more than half these remaining articles. We find among them an attempt to combine various geometries with variable mass (Manev), a general exploration of minimal assumptions for a unified field theory (Whyte), a non-tensorial calculus to integrate electromagnetism, light phenomena and gravitation and reproduce quantum effects (Sevin), a rewriting of quantum theory to fit with Riemannian geometry (Reichenbächer), a criticism of geometrization as anything more than a tool in the construction of unified theories (Band), a multidimensional theory with a strong (Kantian) epistemological component (Rumer) and of course several proposals of one form or another of the affine theories (Novobátzky, Lagunov). It is in this respect quite interesting to remark that the last two articles seem much less connected with the main stream of papers dealing with *Fernparallelismus* than, say, Rumer's papers on multi-dimensional theories.[104] The papers dealing with affine theories refer mainly to the now ancient articles of 1923 by Eddington and Einstein. The status of these "other affine theories" is thus completely different in 1930 from the status of distant parallellism, and even that of five-dimensional theories. It is only fitting that these last two types of theories will be just those on which Einstein will work in the years immediately following.

1930 Corpus

Band, William

I “A New Relativity Theory of the Unified Physical Field.” [Letter to the editor, 26 November 1929]. *Nature* 125 (1930) 130 [P588; J1288] (= *Phys. Rev.* 35 (1930) 115–116 [P816])

II “A New Unified Field Theory and Wave Mechanics.” [Letter to the editor, 28 February 1930]. *Phys. Rev.* 35 (1930) 1015–1016 [P1821]

Einstein, Albert

I “Auf die Riemann-Metrik und den Fern-Parallelismus gegründete einheitliche Feldtheorie.” *Math. Ann.* 102 (1930) 685–697 [J734]

II “Théorie unitaire du champ physique.” [Lectures IHP, November 1929, “rédigées par Al. Proca”]. *Ann. Inst. Poincaré* 1 (1930) 1–24 [P2307]

III “Die Kompatibilität der Feldgleichungen in der einheitlichen Feldtheorie.” *SPAW* (1930) 18–23 [P1821; J735]

IV “Professor Einstein’s Address at the University of Nottingham.” [CR of lecture, 7 June 1930, by I. H. Brose]. *Science* 71 (1930) 608–610 [P1916]

V “Zur Theorie der Räume mit Riemann-Metrik und Fernparallelismus.” *SPAW* (1930) 401–402 [P2671, J738]

Einstein, Albert and Walther Mayer

“Zwei strenge statische Lösungen der Feldgleichungen der einheitlichen Feldtheorie.” *SPAW* (1930) 110–120 [P1821; J736]

Fréedericksz [Frederiks], K. Vsevelod and A. Isakson

“Einige Bemerkungen über die Feldgeometrie.” *Phys. Z.* 30 (1929) 645 [P82]

Grommer, Jakob

“Eine kleine Bemerkung zur neuen Einsteinschen Feldtheorie.” *Phys. Z.* 30 (1929) 645 [P88]

Kunz, Jakob

“Bewegung von Licht und Materie im Gravitationsfeld.” *Phys. Z.* 31 (1930) 83–87 [P1829; J1286]

Lagunoff [Lagunov], B.

“Über eine Erweiterung der Gleichungen des elektromagnetischen Feldes.” *Z. Phys.* 64 (1930) 425–430 [J744]

McVittie, George C.

“On Levi-Civita’s Modification of Einstein’s Unified Field Theory.” *Phil. Mag.* 8 (1929) 1033–1040 [P413]

Mandel’, Heinrich

I “Über den Zusammenhang zwischen der Einsteinschen Theorie des Fernparallelismus und der fünfdimensionalen Feldtheorie.” *Phys. Z.* 30 (1929) 646–648 [P196]

II “Über den Zusammenhang zwischen der Einsteinschen Theorie des Fernparallelismus und der fünfdimensionalen Feldtheorie.” *Z. Phys.* 56 (1929) 838–844 [P196]

Maneff [Manev], Georgi Ivanovič

I “Le Principe de la moindre action et la gravitation.” *CRASP* 190 (1930) 963–965 [P2307; J742]

II “L’Énergie électromagnétique dans le champ de gravitation.” *CRASP* 190 (1930) 1180–1182 [J742]

III “La Gravitation et l’énergie au zéro.” *CRASP* 190 (1930) 1374–1377 [J742]

Northrop, Filmer S. C.

“Two Contradictions in Current Physical Theory and their Resolution.” *Proc. Nat. Acad. Sci.* 16 (1930) 55–68 [P820]

Novobátzky, Karl

“Schema einer Feldtheorie.” *Z. Phys.* 58 (1929) 556–561 [P412]

Proca, Alexandre

“La Nouvelle Théorie d’Einstein.” *Bull. Math. Phys. Bucarest* 1 (1929) 170–176, 2 (1930/31) 15–22 (2 articles) [J738]

Rainich, George Yuri

“Radiation and Relativity. II.” *Proc. Nat. Acad. Sci.* 14 (1928) 654–657 [P1115]

Reichenbach, Hans

“Zur Einordnung des neuen Einsteinschen Ansatzes über Gravitation und Elektrizität.” *Z. Phys.* 53 (1929) 683–689 [P3]

Reichenbächer, Ernst

I “Ist Diracs Theorie mit nur zwei Komponenten durchführbar?” [Letter to the editor, 11 September 1929]. *Naturwissen.* 17 (1929) 805 [P727]

II “Eine wellenmechanische Zweikomponententheorie. I.” *Z. Phys.* 58 (1929) 402–424 [P89]

III “Eine wellenmechanische Zweikomponententheorie. II.” *Z. Phys.* 61 (1930) 490–510 [P1395]

IV “Die Weltfunktion in dem vereinigten Wirkungsintegral der Gravitation, Elektrizität und Materie. I.” *Z. Phys.* 65 (1930) 564–570 [J1282]

Rosen, Nathan and Manuel Sandoval Vallarta

“The Spherically Symmetrical Field in the Unified Theory.” *Phys. Rev.* (2) 36 (1930) 110–120 [P2671; J740]

Rumer, Yuri Borisovič

I “Über eine Erweiterung der allgemeinen Relativitätstheorie.” *Nachr. Göttingen* (1929) 92–99 [P413 and 1820]

II “Form und Substanz.” *Z. Phys.* 58 (1929) 273–279 [P413]

Salkover, Meyer

I “The Unified Field-Theory and Schwarzschild’s Solution.” [Letter to the editor, 31 December 1929]. *Phys. Rev.* (2) 35 (1930) 209 [P727]

II “The Unified Field Equations and Schwarzschild’s Solution. II.” [Letter to the editor, 11 January 1930]. *Phys. Rev.* (2) 35 (1930) 214 [P727]

Sen, R. N.

“On the New Field Theory.” *Ind. Phys.-Math. Jour.* 1 (1930) 28–31 [J741]

Sevin, Émile

“Introduction d’un vecteur charge électrique. Application à la synthèse des théories de l’électromagnétisme, de la lumière et de la gravitation.” *CRASP* 188 (1929) 1603–1604 [P2506]

Swann, William Francis Gray

“Statement in Regard to Professor Einstein’s Publications.” *Science* 71 (1930) 390–391 [P1821]

Tamm, Igor Evegen’evič

I “Über den Zusammenhang der Einsteinschen einheitlichen Feldtheorie mit der Quantentheorie.” *J. Appl. Phys.* 6 (1929) 130–133 [P1823] (= *Proc. Roy. Acad. A’dam* 32 (1929) 288–291)

II “Die Einsteinsche einheitliche Feldtheorie und die Quantentheorie.” *Phys. Z.* 30 (1929) 652–654 [P89]

Tamm, Igor Evgen’evič and Mikhail Aleksandrovič Leontowitsch [Leontovič]

I “Über die Lösung einiger Probleme in der neuen Feldtheorie.” *Phys. Z.* 30 (1929) 648 [P82]

II “Bermerkungen zur Einsteinschen einheitlichen Feldtheorie.” *Z. Phys.* 57 (1929) 354–366 [P412]

Thomas, Tracy Yerkes

I–VI “On the Unified Field Theory.” *Proc. Nat. Acad. Sci.* 16 (1930) 761–776, 830–835; 17 (1931) 48–56, 111–119, 199–210, 325–329 (six articles) [J738]

Vallarta, Manuel Sandoval

I “On Einstein’s Unified Field Equations and the Schwarzschild Solution.” *Proc. Nat. Acad. Sci.* 15 (1929) 784–788 [P413]

II “The Unified Field Theory and Schwarzschild’s Solution: A Reply.” [Letter to the editor, 3 February 1930]. *Phys. Rev.* (2) 35 (1930) 435 [P1203]

Wataghin, Gleb

I “Sopra un’applicazione della relatività alla meccanica quantica.” *Atti Lincei* 10 (1929) 423–429 [P816]

II “Relatività e meccanica ondulatoria.” [Abstract, SIF Meeting, Florence, 18–24 September 1929]. *Nuovo Cim.* (NS) 6 (1929) CLVI–CLVII [P1627]

Whyte, Lancelot Law

“Über die Eigenschaften einer einheitlichen physikalischen Theorie. II. Maßstäbe Uhren und eine mögliche Alternative zur Vierkoordinatenbeschreibung.” *Z. Phys.* 61 (1930) 274–289 [P1202; J732]

Wiener, Norbert and Manuel Sandoval Vallarta

“On the Spherically Symmetrical Statical Field in Einstein’s Unified Theory: A Correction.” *Proc. Nat. Acad. Sci.* 15 (1929) 802–804 [P413]

Zaycoff [Zaikov], Gavrilov Raško

I “Zur einheitlichen Feldtheorie.” *Z. Phys.* 58 (1929) 280–290 [P88]

II “Fernparallelismus und Wellenmechanik. I.” *Z. Phys.* 58 (1929) 833–840 [P292]

III “Fernparallelismus und Wellenmechanik. II.” *Z. Phys.* 59 (1929) 110–113 [P292]

IV “Das relativistische Elektron.” *Z. Phys.* 61 (1930) 395–410 [P1724]

V “Über die Einsteinsche Theorie des Fernparallelismus.” *Z. Phys.* 66 (1930) 572–576 [J1282]

5. CONCLUSION

Before discussing the collective aspects of the production of unified theories, we would like to pause for a moment to revisit two classic questions: the first touches on the role of Einstein, the second on that of quantum theory.

Einstein, with general relativity theory and, to an even greater degree, with his various attempts at unification during our decade and afterwards, is often considered today as the major promoter of the *geometrization* of physics. The articles we have studied here suggest the need to redraw this picture. The attachment of physics to geometry was indeed a controversial topic during this decade, and it was Einstein's name that was frequently put forward as the main target of the project's adversaries and as a rallying banner for its defenders. Moreover, in his more popular works, Einstein often focused on the presentation of a geometrical space-time and its properties — as did the celebrated introductory texts by Weyl and Eddington as well as others in articles addressed to a general audience or devoted to epistemological questions. But if one concentrates on Einstein's technical production, the emphasis is globally different; his interest — and the key point in his interaction with other scientists — was not so much in the geometrical shape of the world per se, as in the choice of field equations.[105] There, in their properties, in the conditions to which they are submitted — in particular their degree of overdeterminacy — is to be found the core of Einstein's *work*. His readiness to abandon, when necessary, the variational principles dear to the Göttingen circle or to leave unsettled problems of the identification between physical quantities and geometrical magnitudes sets him apart from an Eyraud or an Eddington for instance. Even Einstein's increasing eagerness to reach mathematicians seems more indicative of a seeking after complementarity than of a deeply-felt solidarity in *Weltanschauung*.

A second point concerns more directly Einstein's persona. As we have pointed out, Einstein, after 1919 at least, benefited from a universal visibility; his name was known and cited by every other author in our corpus. But the nature of these citations changed with the decade, and in a sense which ran contrary to Einstein's explicit perception of his position. Before 1926, Einstein saw the interest in unified theories as being largely shared by many in the general relativity community:

> The conviction of the essential unity of the gravitational field and the electromagnetic field is firmly established today among the theoretical physicists who work in the field of general relativity theory[106] (1925 Corpus, Einstein I, 414).

But he felt the ground to have largely shifted by the end of the decade:

> As to the way in which the problem [of a unified field theory] may be solved Professor Einstein says that it is a very difficult question to answer, and it has not yet been finished. His colleagues regard his view as a particular craze and do not support it (1930 Corpus, Einstein IV, 610).

Most references during the first half of the decade to Einstein's work in unified field theory, however, occurred as part of a generic name: the "Mie-Hilbert-Weyl-Einstein" approach or, later, the "Weyl-Eddington-Einstein" program. With rare exceptions, articles by Einstein himself in this period were neither developed nor commented on by others. Paradoxically, it was in 1929–1930, when Einstein complained most bitterly of his isolated position, that he, with his unified theory of that time, became the uncontested leader of the domain. As we have shown, almost three-quarters of all the articles of the 1930 corpus relate to Einstein's (and Mayer's) work on *Fernparallelismus*. Both our global data and our more detailed *sondage* thus contradict Einstein's feeling of increasing isolation. That the self-description of a scientist should not be taken at its face-value is a well-admitted historical rule: but at which value then should it be taken? Our study suggest two principal paths to trace this dissonance more accurately and integrate it into a more comprehensive view of Einstein's identity: the first leads to the identification of who counts for him as a 'significant' colleague;[107] the second to a more precise characterization of his program, the emphasis no longer being on a mere geometrical unification of gravitation and electromagnetism, but on the much stronger requirement that matter appear as a consequence of the field theory, with the consequent gradual distancing of Einstein from the main trends in the theoretical physics of the time.

This last suggestion offers a smooth transition to the question of quantum matter. In large part because of Einstein's increasing opposition, quantum theory has been often presented as the alternative to geometrical unified field theories, and its successes a progressive trespass on their territory. What we have seen is different and the demarcation lines are not so clear. It appears that there were never two hermetic programs vying for hegemony, classical and geometrical on one side and quantum on the other. From the beginning of our decade, quantum theory appears, at least as an horizon, for even the most avid promoter of the geometrical approach at the time. The complete Weyl quotation, of which a part begins this article, looks like this:

> A new theory by the author has been added, which ... represents an attempt to derive from world-geometry not only gravitational but also electromagnetic phenomena. Even if this theory is still only in its infant stage, I feel convinced that it contains no less truth that Einstein's Theory of Gravitation — whether this amount of truth is unlimited or, what is more probable, is bounded by the Quantum Theory[108] (Weyl 1919, vi).

And very soon quantum elements occur as an effective component in a number of proposals, as we have seen for 1925 in — the very different — Kudar I–III and Vallarta. In fact such attempts occur as early as 1922, in unified theories which are combinations of a geometrical approach and the older quantum theory reworked in various patterns (e.g., Schrödinger 1922; Wilson 1922; Wereide 1923). In this sense, the success of the quantum program, as witnessed in the attempts to integrate it with a theory of gravitation, is at once earlier than usually placed, but less devastating in its impact for classical unification theories.

Moreover, we have seen not one quantum theory, but a variety of quantum approaches: the Bohr-Sommerfeld quantum rules, the Schrödinger-Dirac wave mechanics (though only occasional allusions are made to the matrix mechanics approach); at the end of our decade, it is the budding quantum electrodynamics that is seen as a true alternative to the flagging Dirac-*Fernparallelismus* agenda:

> Until recently there seemed to be little doubt that the connecting link between the unified theory and the quantum theory would be found through some generalization of the Dirac

> equations, as suggested by Wigner, Wiener and Vallarta, Tamm, Fock, Weyl and others. None of these attempts has proved satisfactory and some of them have been shown to be definitely erroneous. An entirely new method of attack, however, has been opened by the quantum electrodynamics of Heisenberg, Pauli, Jordan, and Fermi . . . (1930 Corpus, Rosen and Vallarta, 119–120).

Indeed, if quantum theory replaces anything — at least in those articles reviewed in sections devoted to gravitation — it is one form or another of the older theories of matter, and, in particular for our decade, the (classical) theory of the electron. This should be taken not merely in the obvious sense that such quantum theories become the new foundations for matter, but in the sense that quantum approaches take over exactly the various functional roles occupied by the older theories in the unification programs. From 1925 on, they are used, sometimes on an equal footing with gravitation, sometimes as a means of replacing the phenomenological aspects of gravitation or electromagnetic theory by a first-principle theory, sometimes as a source of reduction of one class of phenomena to another.

Let us now return to the questions we raised in the introduction, in particular that of unification theories as collective production. In this respect we have found important modifications during the twenties, modifications in the content and the techniques, of course, but also in the organization of work, for instance in the rhythm and type of publications.

In 1920, the German-language scene was dominant and, on the whole, little disposed to look beyond its borders. A clear epicenter was located around the Hilbert-Weyl program, though there existed a wide variety of alternative proposals. The various directions of research, however, all bore the imprint of the still recent success of general relativity, either in a positive or negative sense.

In 1925, textbooks on general relativity have widely diffused a common set of basic tensorial and Riemannian techniques; there existed a more international, but more scattered, scene — though still largely European — with proposals concerned with the exploration and completion of a relatively limited number of specific theories. In particular, the idea of unification as a geometrical combination, on an equal footing, of Einstein's 1916 theory of gravitation and Maxwell's electromagnetism is well-established, even if neither universally accepted nor necessarily coupled with a reification of geometry. Indeed, especially in Great Britain, there are lively debates on the respective role of geometry and physics, having concrete resonance in scientific work and engaging major figures on both sides. Moreover, quantified matter has entered the picture, as a technical part of several unification programs, and as an alternative to both continuous and classical particulate theories of matter.

In 1930, finally, an overwhelming interest is expressed for a single theory, explored, however, in a variety of directions and at a rapid pace — with a residual interest for a second approach and a collection of isolated projects. The scene is world-wide, the massive arrival of US and Soviet scientists being marked by an emphasis on institutionally centered group work with a strong division of labor. The newcomers pursue however two very different publication policies: the first national, with *Physical Review* and *Proceedings of the National Academy of Science*, the other oriented towards publication in foreign, particularly German and, for short notes, French journals. Quantum theory is widely recognized as an inevitable component of every future unification, even if its nature, its role and its interplay with other phenomena remain variable.

Some of these aspects require a comparative perspective — and thus complementary studies — to be properly appreciated. We can at least underline that taking into account a larger corpus than is usually done allows us to restore the concrete texture of the debates at this early period and to make precise the periodization of the various proposals. Thus, no historiography which selects only the most famous German-language authors (plus Eddington as honorary member) can hope to capture the global dynamics, which requires a knowlege of the standpoints of other groups.

Indeed, a crucial problem in the understanding of historical dynamics lies in the great sensitivity of its models to selection effects. For instance, it would be easy, by picking out appropriate elements, to mimic here a Kuhnian dynamics for the genesis of a new discipline: an initial dispersion of interests, a preliminary coagulation in a range of systematically explored possibilities, and the final emergence of a paradigm, here *Fernparallelismus*. But we know of course that this conclusion does not hold: the flock of sparrows on the Einsteinian *Fern*cake, including Einstein himself, scatters almost immediately. The brief fame of *Fernparallelismus* does not result in a victory in 1930 of a 1925 competition between rival affine theories: there was no such competition and morevoer, Einstein's theory with distant parallelism was not even perceived, at the beginning, as a direct successor of affine ones.

The lack of continuity is apparent at other levels as well. The range of combinations seems molded for most authors far more by the concrete possibilities of available techniques than by a more global conviction concerning the constitution of the world. The goal of unity might be an ideal, its embodied shape is often the consequence of very technical constraints. Links between the various constitutive elements of the proposals then are ephemeral and local; geometry and variational principles, for example, are much less associated in later versions of *Fernparallelismus* than they were in Eyraud's work. To paraphrase Marx as well as Einstein, we lack sufficient overdetermination to suggest a satisfying dynamics at this level.

Moreover, the period of production of many of the scientists engaged in unification programs is short. As a look at the scientific biographies of our authors testifies, most do not remain long in the field.[109] There was no specialist of unification, no "unitarist," as one might have been, in the same period, differential geometer or relativist.

But, while the short active lifetime of a scientist in unification work shows that unification did not constitute a discipline, the very variety of these scientists indicates, strangely enough, a standard, 'normal', research activity.[110] We do not find only a few geniuses and cranks, but all sorts of scientists, at various stages in their careers; some of exceptional rank, a good number more or less well-known, and, in general, regularly productive, most of them in full-time positions as physicists or mathematicians (with the usual exceptions of a few teachers, engineers and unemployed). Nor is unification merely an activity for the elderly; each year, we find contributions originally developed as theses. And the references and other information bear witness to quite a regular flow of exchange and communication.

We have then not the constitution of a discipline,[111] but activity in a respectable area of research. To grasp the nature of this topic during our decade and its evolution, we have to take into account the concrete tensions which structure the configurations of articles we have detected and look at the elements which have been stabilized during this period. Two features, already evoked, would require a larger perspective to be articulated, because of the shift in time of their impact. One, the effect of which is increasingly perceptible in the second half of the decade, concerns the very conception of matter and of its role: quanta evacuate, in *this area of physics*, most other representations or theories. The second feature, appearing only at the very end, is the overall transformation of the research activities and publications, the transformation from the cottage industry of 1920 to the industrial enterprise of the thirties and later.

But the major component has to do with general relativity as the dominant theory of gravitation: although no new (non-cosmological) experimental evidence was found during our decade, our study clearly shows an acceptance of Einstein's 1916 theory — for some, in 1930, even *contra* Einstein. The alternative theories (Whitehead's or Wiechert's for example) we have seen during the first half of the decade have disappeared at its end or have been marginalized. As is confirmed by the place of unified theories in the reviews on gravitation, unification is one important, current way of learning and working in general relativity, during a period the last half of which marks the beginning of the *étiage*, to use Jean Eisenstaedt's term (Eisenstaedt 1986). Paradoxically enough, it is sometimes through the most exotic efforts to go beyond it that a scientific theory consolidates its status.

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NOTES

* To John Stachel, in affection and comradeship.

1. The expression is Robert D. Carmichael's in a 1926 debate on the theory of relativity (Carmichael 1927, 12).
2. For an interdisciplinary discussion of the role of unification in various domains and, in particular, of the political issues associated with them, see (Galison and Stump 1996).
3. "Dann aber ist eine neue, vom Verfasser herrührende Theorie hinzugefügt worden, welche ... aus der Weltgeometrie nicht nur die Gravitations-, sondern auch die elektromagnetischen Erscheinungen abzuleiten. Steckt diese Theorie auch gegenwärtig noch in den Kinderschuhen, so bin ich doch überzeugt, daß ihr der gleiche Wahrheitswert zukommt wie der Einsteinschen Gravitationstheorie ... " English translation by H. L. Brose in (Weyl 1922, xi).

4. "Einstein's Arbeit ist ein neuer Beitrag zu einem Versuch, den er vor etwa Jahresfrist unternahm — eines neben vielen, vielen anderen, die in den letzten zehn Jahren unternommen wurden.... Ich glaube, daß durch die Entwicklung der Quantentheorie in den letzten Jahren die Problemlage so verschoben ist, daß man nicht erwarten kann, die gesuchte Einheit zu finden, ohne die materiellen Wellen in das Schema mit einzubeziehen, durch welche die Wellenmechanik die sich bewegenden Materieteilchen ersetzte" (Letter from Hermann Weyl to James Stokley, 3 February 1929).
5. On the history of unified theories, see the pioneering books of Marie-Antoinette Tonnelat (Tonnelat 1965, 1971) and the recent (1985) excellent synthesis by Vladimir Vizgin (quoted here from the English translation: Vizgin 1994), as well as the articles (Goenner 1984; Bergia 1993).
6. See for instance (Garfield 1964; Price 1965; Callon et al. 1986).
7. We prefer this term, as used by Norbert Elias in, for example, *Engagement und Distanzierung*, to the term 'community', which Thomas Kuhn's *Structure of Scientific Revolutions* has made familiar to historians of science, because, as we shall see, even tight relationships do not necessarily imply the emotional commitment or sense of sharing involving human beings as a whole and implicitly conveyed by the latter term.
8. We use the traditional archeological term "sondage," in the usual English sense of trial-trenching as a preliminary to full excavation, see, for example, Mortimer Wheeler's *Archaeologyfrom the Earth*.
9. Quite the contrary: we shall miss out 1921, Vizgin's "pivotal year" for affine theories; 1923, and Cartan's classification of affine spaces; 1926/1927, and the birth of a new and successful quantum program. Nor shall we see most contributions to Kaluza-Klein theory (although other five- and higher-dimensional theories will make their appearance). But as will be seen, we shall be able to see their effects (or lack thereof) in our decade.
10. Within each section, the articles are grouped under specific headings, but these subsections changed continuously, including for instance a heading "Light" or "Quanta" in the section on relativity for certain years.
11. The problems of the *Jahrbuch* during this period have been closely studied by Reinhard Siegmund-Schultze (1993).
12. Quite typically, its publication date is 1932; in fact the change is made first in 1931, with the '1927' volume.
13. The years mentioned correspond to the dates of the reviews and not to the publication dates of the articles. For *Physikalische Berichte*, the volume year corresponds roughly to publication dates in the last half of the previous year and the first half of the nominal year.
14. The traditional view already appeared dubious to Hubert Goenner in his study of German books on relativity (Goenner 1992; see also Eisenstaedt 1986).
15. We are comforted in this choice by the fact that those responsible for classifying the articles have also put into this section, at least at the beginning of our period, attempts to unify various phenomena other than gravitation, and which contest (part of) relativity theory.
16. Although a case might be made for it, we shall not consider general relativistic thermodynamics as a unified theory.
17. It is interesting to note a certain degree of specialization in the reviewers themselves, such as Philipp Frank, who signed the review of almost all these papers at the beginning of the period in the *Jahrbuch*, or Cornelius Lanczos who was in the same privileged situation for the *Berichte* from 1925 on.
18. The year 1930 marks the peak in their production: in 1935, 22% of the relativity papers are devoted to unification and in 1940, 15%, but this proportion should be appreciated against a background of a drastic reduction in absolute numbers; between 1920 and 1940, the number of articles in relativity falls by one-half.
19. As has been explained, by "year," we mean the volume year of *Jahrbuch über die Fortschritte der Mathematik* and *Physikalische Berichte*; their combined coverage corresponds to roughly one and a half years of actual publications.
20. In general, we insist on the distinction between articles and authors. Someone like Einstein, who tried almost every approach to unified (field) theories in turn, and sometimes several in one year, makes the point. In 1920, however, papers by the same author are continuations of each other and, save explicit mention, this distinction will be relaxed here for simplicity.

21. As in each of our years, there are one or two doubtful cases; for instance, we have retained Walter Dällenbach's articles, although they appear to be only a special relativistic extension of Lorentz's 1904 theory, and this on two grounds. Dällenbach announces — too optimistically — the possibility of an "obvious" extension to general relativity and Lorentz's theory itself can be seen as a step towards a global interpretation of natural phenomena, see (Vizgin 1994; Miller 1981). In any case, these borderline cases do not change the general picture.
22. For a start on the rich literature on this subject, see (McCormmach 1970; Miller 1981; Hunt 1991; Darrigol 1996).
23. "Eine Entdeckung ist gemacht worden!"
24. These last represent some of the communications made at the famous meeting in Bad Nauheim, where Einstein had to confront the hostility of Philip Lenard and other tenants of anti-relativistic "German physics," see (Goenner 1993a, 1993b).
25. All the more so because the moment we are looking at offers all too many temptations towards attempting a description in terms of 'national' styles or schools. For a clear vision of the pitfalls in such an approach see the counterexamples in (Warwick 1992–93).
26. See Docs. 230, 232, 236, 240, 413, 438 (De Donder) and 328, 408 (Bateman) in (Schulman et al. 1998). De Donder seemed to see himself as the Lagrange of an Einsteinian Newton, though Einstein was quite critical of De Donder's approach which appeared to him to be an elimination of physics from relativity theory.
27. Chapter IV, note 30 of the third edition (Weyl 1919) mentions Reichenbächer alone, to which chapter IV, note 32 of the fourth edition (Weyl 1921) adds Abraham, Nordström and Wiechert.
28. See (Sánchez-Ron 1999) on Larmor and relativity. Even Eddington's views should not be confused with Einstein's, see (Stachel 1986).
29. See, for different aspects, (Sigurdsson 1991, 1994; Scholz 1995, 1999) and (Vizgin 1994, ch. 3), which also discusses Einstein's and Pauli's reactions.
30. "Dann würden nicht nur die Gravitationskräfte, sondern auch die elektromagnetischen aus der Weltmetrik entspringen; und da uns andere wahrhaft ursprüngliche Kraftwirkungen außer diesen beiden überhaupt nicht bekannt sind, würde durch die so hervorgehende Theorie der Traum des Descartes von einer rein geometrischen Physik in merkwürdiger, von ihm selbst freilich gar nicht vorauszusehender Weise in Erfüllung gehen, indem sich zeigte: die Physik ragt mit ihrem Begriffsgehalt überhaupt nicht über die Geometrie hinaus, *in der Materie und den Naturkräften äußert sich lediglich das metrische Feld.* Gravitation und Elektrizität wären damit aus einer einheitlichen Quelle erklärt."
31. Einstein's commented on this move at Bad Nauheim in these terms: "Since Weyl's theory abandons this empirically grounded category, it deprives the theory of one of its most solid empirical supports and test possibilities." ("Indem die Weylsche Theorie auf diese empirisch begründete Zuordnung verzichtet, beraubt sie die Theorie einer ihrer solidesten empirischen Stützen und Prüfungsmöglichkeiten.") Einstein *apud* Weyl II: 651.
32. Invariant theory was, of course, Weitzenböck's specialty; in the same year, for example, he published an article doing a similar job for the "Galilei-Newton" group (Weitzenböck 1919/20).
33. His communication was to be criticized the following year by Erwin Freundlich (1920), acting as a defender of Einstein's position, for its neglect of physical contents; relativity, Freundlich would explain, is not a mathematical but a physical theory. In many respects this controversy recalls that between Einstein and the mathematicians Hilbert and Weyl. For the opinions of Einstein, see (Vizgin 1989, 1994, 98–104).
34. "So sind alle physikalischen Gesetze schließlich zurückgeführt auf das einzige Problem der Metrik der … vier-dimensionalen räumlich-zeitlichen Mannigfaltigkeit. … Eine der wichtigsten Zukunftsaufgaben, die in dieser Hinsicht … gestellt ist, ist wohl die Einfügung der *Quantentheorie* in das System der allgemeinen Relativitätstheorie.

    Bei der Inangriffnahme dieser Aufgabe müßte die physikalische Axiomatik offenbar an einen Gedanken anknüpfen, den schon … Riemann … geäußert hat: daß nämlich das Objekt der Geometrie auch eine *diskontinuierliche Mannigfaltigkeit* sein könnte. … Wäre aber die … Mannigfaltigkeit selbst diskontinuierlich aufzufassen, dann würde es begreiflich sein, warum die bei bestimmten physikalischen Prozessen auftretende Menge an Wirkung notwendigerweise ein ganzzahliges Vielfaches eines *elementaren Wirkungsquantums* sein müßte."

35. The case of Weitzenböck and Pauli vindicates our reticence to speak of "community": while their articles, as well as Weyl's, were certainly involved in a tight network of exchanges favoring quick responses, there was nonetheless no question of personal commitment of the authors to the theory itself.
36. Vizgin mentions Mie in his list of opponents to Einstein, in particular at Bad Nauheim. We have not found any evidence that points in this direction; on the contrary, at this time, Mie seems quite enthusiastic about Einstein's achievements, see (Illy 1992). His critical comments seem mainly directed against calling Einstein's theory of gravitation a theory of 'general relativity'. Moreover, his name does not appear on the lists provided by the "Hundred Authors against Einstein," see (Goenner 1993b).
37. "wunderbare, vollendet schöne mathematische Struktur"
38. Note that these two form precisely the intersection of our authors with the list of anti-relativists published in (Goenner 1993b). However, their papers show clearly that there was not, in their case at least, any question of negating the importance of Einstein's work.
39. "Hierbei ergibt sich die Massendichte im Gegensatz zu der gewöhnlichen Anschauung nicht als Skalar, sondern als 44-Komponente eines 16gliedrigen Tensors … . Dies und die Tatsache, daß die $g_{\mu\nu}$ wegen ihrer Abhängigkeit von der Wahl des Koordinatensystems einer freilich eingeschränkten Willkür unterworfen sind, hat mich in der Einsteinschen Theorie nicht befriedigt, und ich habe deshalb in meiner Arbeit: "Grundzüge zu einer Theorie der Elektrizität und der Gravitation" versucht, die Theorie eines skalaren Gravitationspotentials, das ich mit der Lichtgeschwindigkeit identifizierte, aufzustellen, wobei ich an bestimmte Voraussetzungen über die Gravitationserregung durch die Elektronen anknüpfte, die ich — positive und negative — als das einzig Materielle ansah. Die einfachste Fall … eines einzigen Elektrons hatte ich dabei erledigt und nach Analogie dieses Falles allgemein die Gleichung … aufgestellt."
40. A scalar theory had, of course, already been proposed by Einstein some years earlier (Einstein 1912a, 1912b) in the context of a theory of the static gravitational field.
41. "Es ist demnach möglich, zu einer … Lösung des Weltproblems … zu gelangen, wenn man … sich damit auf einen realistischen Standpunkt gegenüber dem mehr phänomenalistischen der Relativitätstheoretiker stellt."
42. "Das Fundament der Theorie soll die Annahme sein, daß die molekulare Materie aus elektrischen Teilchen aufgebaut ist. Es wird damit die Elektrisierung als eine Grundeigenschaft aller Bausteine der Materie erklärt. Die Annahme erscheint als die natürliche Folgerung aus den Ergebnissen der molekularphysikalischen Forschung der letzten drei Dezennien. Einst lehrte die Elektrodynamik in der Elektrisierung eine wesentliche Ursache der *Trägheit* kennen, nun soll der Nachweis versucht werden, daß die Elektrisierung auch eine wesentliche Ursache der *Gravitation* ist."
43. The reception of relativity theory at Cambridge has been thoroughly explored by Andrew Warwick (1988; 1992–93), who has also stressed the differences, as well as the links, among these people. Note that, though Bateman was at this time at CalTech (then known as Throop's College) working on hydrodynamics, his earlier career was a typical, though brilliant, Cambridge one, and, in this field at least, he published in a British journal, the *Philosophical Magazine.*
44. For a *mise en contexte* of this publication, see (Sánchez-Ron 1999).
45. It may be revealing to note that, in the journal where they appear, De Donder's and Vanderlinden's articles are classified in the "mathematical physics" rather than the "theoretical physics" section.
46. An instance is Ludwik Silberstein, a Polish physicist whose path crosses a number of countries; at this time he publishes principally in British journals and his work is quite abundantly, though not exclusively, discussed by British and American authors. On Silberstein see (Sánchez-Ron 1992) and, for his later debate with Einstein, (Havas 1993).
47. We will come back soon to these theories. Note that this alignment with particular traditions does not mean that the same author is always restricted to just one. Henri Eyraud, for instance, devotes one note to exploring the framework of Weyl's geometry in 1924 (I) but then turns more systematically to the consequences of Schouten's point of view (II, III).
48. Similar concepts were independently invented by a number of mathematicians. For a history of this topic, see (Reich 1992).
49. Note that Arakatsu defines the covariant derivative of a covariant vector for this second connection (equation (2.7) in his paper) in a way which would imply that the two connections are in fact the same. His definition can be corrected, however, without harm to the conclusions of the article.

50. In fact, one of the most articulate speakers in 1920 against a geometry having no regard for experiment is Einstein himself.
51. It is this dichotomy, for instance, which helps Vizgin to define two distinct research programs in the Lakatosian sense, see (Vizgin 1994, 129).
52. "Entweder man hält die Zeit für noch nicht gekommen und verschiebt seine Lösung, bis die vielleicht gleichen Quellen der noch rätselhafteren, an die Namen von Planck, Einstein und Bohr geknüpften Quantengesetze freigelegt sind.
    Oder man hält die Lösung auf den von Maxwell, Lorentz, Mie, Einstein, Hilbert, Weyl, u. a. geschaffenen Grundlagen für möglich."
53. "Définir la physique quantique au sein de la physique relativiste comme une espèce dans un genre par l'adjonction d'un caractère spécifique." We might mention that Wavre is rather sceptical of the chances of success for this approach that he sees as a last attempt to avoid the discretization called for by quantum theory.
54. Note that the title of the paper, "Einheitliche Feldtheorie … " marks Einstein's first public use of the term 'unified field theory' in connection with this topic.
55. Einstein was still at that time quite concerned with quantum theory. Explaining this new project in a letter to his friend Michele Besso, he writes: "This is then a magnificent opportunity, which should probably correspond to reality. There now arises the question whether this field theory is compatible with the existence of atoms and quanta." ("Dies ist doch eine prachtvolle Möglichkeit, die wohl der Realität entsprechen dürfte. Nun ist die Frage, ob diese Feldtheorie nicht der Existenz der Atome und Quanten vereinbar ist.") (Einstein 1925). On the complex relationship between Einstein and quanta, see (Stachel 1993).
56. This paper seems never to have been published.
57. On Eddington's epistemology, see (Merleau-Ponty 1965; Kilmister 1994a). On the hostile reactions to Eddington and especially Jeans, on these issues, see (Sigurdsson 1996). For Lodge's views at this time, see (Rowlands 1990, 270–290) as well as (Sánchez-Ron 1999).
58. This early example of "numerology" had to be retracted a month later (Rice II) in a letter to the editor of the same review; Rice had misread the length units in which the radius of the universe he used were expressed. It is ironical that the current values of R are just what Rice needed!
59. There are of course several intermediate cases, as illustrated by Arakatsu's article, already discussed.
60. We do not consider here in detail the question of the ontological commitment of these authors, nor the meaning of the word "geometry" (or "physics"), and their relations to the question of matter, for them; we propose to examine these issues elsewhere.
61. This approach, which passed practically unnoticed at the time, except by Einstein himself (Ritter 1993, 142), was rediscovered and prominently featured by John Wheeler and his school in the 1950s and 1960s under the name of "geometrodynamics" (Wheeler 1962); see (Stachel 1974). Note that virtually all of Rainich's publications on the question were in American mathematical journals, which may account for their lack of impact on the scientists we study here.
62. Eyraud, in fact, learned general relativity in Weyl's 1917 course at the ETH in Zurich, where he had been placed by the Red Cross, under Swiss control, as an ex-prisoner of war. We would like to thank M. Gustave Malecot for this information (private communication).
63. "Le potentiel vecteur trouve son expression géométrique dans la torsion."
64. On Temple's style of work and his relation to Whitehead, see (Kilmister 1994b), in particular p. 386. We are grateful to the author and to Felix Pirani for this reference.
65. This manifold is the one associated with measurement, the coordinates being evaluated by means of clocks and rigid rods. Whitehead (1922) had originally considered "true" space to be flat.
66. Such considerations have become, of course, more familiar in a quantum field-theoretic context, under the name of "*CPT* invariance;" see for example (Pais 1986, 525–529).
67. "Wesentlich scheint mir die Erkenntnis zu sein, daß eine Erklärung der Ungleichartigkeit der beiden Elektrizitäten nur möglich ist, wenn man der Zeit eine Ablaufsrichtung zuschreibt und diese bei der Definition der maßgebenden physikalischen Größen heranzieht. Hierin unterscheidet sich die Elektromagnetik grundsätzlich von der Gravitation: deshalb erscheint mir auch das Bestreben, die Elektrodynamik mit dem Gravitationsgesetz zu einer Einheit zu verschmelzen, nicht mehr gerechtfertigt."

68. In addition, a number of papers mixing gravitation and quantum theory are now classified in the section "Quantenlehre" of the reviewing journals and thus do not enter into our corpus.
69. And not during the "summer of 1929," as sometimes suggested in the current literature. As we shall see, the precise dates play a role in the interpretation of the event.
70. The importance of Soviet work around general relativity, quantum physics and unified theories may surprise those who have read of the critical manner in which these theories were supposed to be viewed by orthodox Marxist-Leninist philosophers and politicians in the Soviet Union as early as the late 1920s. While the Russian physicist Yuri Rumer, working in Born's laboratory in Göttingen, felt that conditions were indeed difficult for those working in these fields (Born 1929), the holding of the Kharkov conference and the number of Soviet physicists working in these areas seems to raise some serious doubts. See (Graham 1966).
71. Lively examples of the appeal of this theory for a general public are given in (Pais 1982, 346). However the stir was not limited, as Pais implies, to popular journalists and their readers.
72. The problem of notation, already mentioned, is specially interesting in the case of the *Vierbein*. Einstein himself changed his notations several times, adopting Weizenböck's when this author pointed out to him previous work on similar spaces (Weizenböck 1928), changing again when Cartan's priority was established (Cartan 1930). However, to follow these changes would have been intractable in an article and, on this point, we have chosen to uniformize the notation.
73. The corresponding lines in (Einstein 1928, 219) are incorrect: the reference in the line leading up to eq. 7a should be to eq. 4 and not eq. 5, and the expression for dA^{ν} includes a mysterious $h^{\nu a}$ instead of h^{ν}_{a}. Such typographical errors are not uncommon in Einstein's publications in the *Sitzungsberichte*.
74. Cartan has already been mentioned in the 1925 *sondage* a propos of Eyraud's article, which used an affine space with non-zero torsion and curvature. Indeed, Einstein acknowledged the lack of novelty of his theory in this respect, after this had been pointed out to him by several authors. Cartan himself, in a letter of 8 May 1929, reminded Einstein that he had spoken to him of this very possibility as early as 1922, during Einstein's visit to Paris (Cartan 1929). This letter is the origin of the historical survey article published by Cartan, at Einstein's request, in the 1930 *Mathematischen Annalen* (Cartan 1930), and of an important exchange on the mathematical and physical possibilities of the theory between the two scientists, published in (Debever 1979).
75. " … eine metrische Kontinuumsstruktur, welche zwischen der Riemannschen und der Euklidischen liegt." Despite the corrections published by several mathematicians, the same point of view is maintained in Einstein's adress at Nottingham, as late as June 1930, transcribed by I. H. Brose in *Science* (Einstein IV).
76. Precisely which laws they yield depends in fact on the version of the theory under consideration; for reasons of space, we shall not enter into this question here.
77. See the reactions of Weyl and Pauli mentioned in (Pais 1982, 347).
78. "Ein Rückgang zu den alten Relativitätstheorien, [vierdimensionalen… wie auch fünfdimensionalen…] scheint jedoch ein für allemal ausgeschlossenen zu sein. Man hat mit dem Gedanken an einen Fernparallelismus wirklich einen Erkenntnisschritt gemacht!"
79. For a physicist's survey of the later developments of this topic, see (Kichenassamy 1992).
80. The note is slightly too early to be included in our corpus for 1930, but the continuation of the program is included. See also (Vizgin 1994, 246).
81. For a number of them, in particular Einstein's articles, cf. also the discussions in (Vizgin 1992, 234–255).
82. In particular, it is sometimes useful to distinguish between the date of submission to a journal, or the date of presentation to a conference, and the date of publication.
83. "… die Auffindung der einfachsten Feldgesetze, welchen eine Riemannsche Mannigfaltigkeit mit Fern-Parallelismus unterworfen werden kann."
84. "Der große Reiz der hier dargelegten Theorie liegt für mich in ihrer Einheitlichkeit und in der hochgradigen (erlaubten) Übereinstimmung der Feldvariablen."

85. "Je höher die Zahl der Gleichungen ist (und folglich auch der zwischen ihnen bestehenden Identitäten), desto bestimmtere, über die Forderung des bloßen Determinismus hinausgehende Aussagen macht die Theorie: desto wertvoller ist also die Theorie, falls sie mit den Erfahrungstatsachen verträglich ist." Not only does Einstein hope to constrain the initial conditions as far as possible, but he equally wants to account for the specific conditions put forward by quantum theory.
86. "Wohl aber erkennt man, daß in der neuen Theorie die Singularitätsfreiheit derjenigen Lösungen verlangt werden muß, die die Elementarpartikeln der Materie darstellen sollen."
87. Among the reasons given in the individual articles to dismiss the previously announced equations, we find objections stemming from Einstein himself as well as criticisms by others, in particular Lanczos and H. Müntz. None of this is mentioned in the IHP lecture, but has to be taken into account in order to understand Einstein's variable mood during this period. Moreover, the identification of the physical quantities with the mathematical elements of the theory is also relaxed.
88. "In der Zwischenzeit habe ich zusammen mit Dr. Mayer viel über den Gegenstand gearbeitet und bin von den damaligen Feldgleichungen abgekommen."
89. "Nach dem Grundgedanken der fünfdimensionalen Theorie ist die Gesamtheit der ∞^4 Weltpunkte nicht als Gesamtheit der ∞^4 Punkte einer Hyperfläche im R_5, sondern als Gesamtheit der ∞^4 Linien (der Kongruenz X^i_5) im zylindrischen R_5 aufzufassen."
90. For a presentation of the Princeton school and their program of a new differential geometry based on paths, see (Eisenhart 1927).
91. Here h^i_α is $h_{i\alpha}$ in Einstein's previous notation.
92. A lively recollection of the group is to be found in (Struik 1989), in particular, p. 172.
93. Their paper in our corpus is a corrected version of this, sent to the same journal on 23 May.
94. These conditions are taken from Eddington's 1923 book on relativity, in his presentation of the classical Schwarzschild solution.
95. In these cases, as well as in the essentially analogous theory in Einstein's January 1929 article, the result is to be expected because the equations lead, in first approximation, to those of general relativity. It should be noted that in December 1929 (Einstein III, 18) Einstein acknowledges an error in his March 1929 paper.
96. See also his note to the French Academy of Sciences of 15 April 1929 (Tamm 1929).
97. This idea was also advocated by, among others, Wiener and Vallarta in their work discussed above.
98. "Die Tatsache, daß das skizzierte Verfahren wirklich zu einer vernünftigen Wellengleichung führt, ist deshalb besonders interessant, weil der zu der erwähnten wellenmechanischen Annahme analoge "klassische" Ansatz: die auf die 4-Beine bezogene Bewegung des Elektrons sei immer gleichförmig, zu keinen brauchbaren Bewegungsgleichungen führt. *Somit erscheint in der Einsteinschen Theorie das wellenmechanische Prinzip dem Prinzipe des kürzesten Weges der geometrischen Optik übergeordnet.*"
99. Lanczos, who reviews the paper in the *Physikalische Berichte*, points out that this result might have been foreseen directly from the field equations, without the need of further computation, see note 95.
100. Besides the interpretation discussed here, another possibility suggested is that these results only point to the necessity of new field equations. We do not have however any evidence that the new field equations derived by Einstein one month later, or indeed any others, renewed their interest in these questions. The search for axial symmetry however was taken up by others, e.g., (McVittie 1930/31), with no satisfactory result.
101. "Einer von uns hat kürzlich zu zeigen versucht, wie ungezwungen die Wellengleichung des Elektrons sich in der neuen Einsteinschen Theorie ergibt, und dabei die Vermutung ausgesprochen, daß in dieser Theorie das wellenmechanische Prinzip dem Prinzip des kürzesten Weges übergeordnet ist, so daß die Bewegungsgleichungen einer (geladenen) Korpuskel durch einen Limesübergang aus der Wellengleichung abzuleiten sind. Wenn diese Vermutung und auch die Vermutung, daß die einer geladenen Partikel entsprechende Lösung der Einsteinschen Feldgleichungen von dem Spin der Elementarladungen Rechenschaft gibt, sich wirklich bestätigen sollte, so wird damit die mikroskopische Deutung der Einsteinschen Theorie weitgehend gestützt sein."

102. These papers, (Fock and Iwanenko 1929a, 1929b) — as well as Fok's own search for a union of Dirac theory and general relativity (Fock 1929a–c) — are missing from our corpus since they were reviewed in the quantum physics section of *Physikalische Berichte*; the same is true of other, similar approaches (Wigner 1929; Weyl 1929a, 1929b). We shall therefore not discuss them here, see (Vizgin 1994) for some of them.

103. In this respect, our study helps to understand the chronology of the reception of Kaluza-Klein theories in the 1920s. What little impact Kaluza's original article (Kaluza 1921) had on the physics community had completely dissipated by 1925, while the echoes of the more influential reworking by Klein (1926) had been only partly drowned out by the tidal wave of distant parallelism after 1929.

104. This difference could be traced to a question of personal relationships. Whereas Zaikov had studied in Göttingen and Berlin, Rumer works at Göttingen at this time in Born's laboratory, Mandel', as we have pointed out, was an active participant at the Kharkov conference and in personal contact with Einstein, Lagunov, on the contrary, is not listed as a participant at Kharkov.

105. Such an emphasis is already noticeable in his development of general relativity, see (Renn and Sauer 1999). Field equations, and their degree of overdetermination, were also Einstein's main interest in his correspondence with Cartan in the thirties (Debever 1979).

106. "Die Überzeugung von der Wesenseinheit des Gravitationsfeldes und des elektromagnetischen Feldes dürfte heute bei den theoretischen Physikern, die auf dem Gebiete der allgemeinen Relativitätstheorie arbeiten, feststehen."

107. In this respect, it is interesting to analyze the responses of his contemporary correspondents, see (Pais 1982, 347). Also telling is Vallarta's commentary, in Norbert Wiener's *Collected Works*, on their joint work on *Fernparallelismus* (Vallarta 1982); according to this account, Einstein's reformulation of his theory was a consequence of his reception of Vallarta's and Wiener's results concerning the first set of field equations, though Einstein never mentions this in his later publications.

108. "Dann aber ist eine neue, vom Verfasser herrührende Theorie hinzugefügt worden, welche ... aus der Weltgeometrie nicht nur die Gravitations-, sondern auch die elektromagnetischen Erscheinungen abzuleiten. Steckt diese Theorie auch gegenwärtig noch in den Kinderschuhen, so bin ich doch überzeugt, daß ihr der gleiche Wahrheitswert zukommt wie der Einsteinschen Gravitationstheorie — mag nun dieser Wahrheitswert ein unbegrenzter sein oder, wie es wohl wahrscheinlicher ist, begrenzt werden müssen durch die Quantentheorie." English translation by H. L. Brose in (Weyl 1922, vii).

109. It would be all the more interesting to look closely at the rare exceptions (besides the much studied Einstein, Reichenbächer, and De Donder), in the perspective of the constitution of individual trajectories and collective scientific production, see (Goldstein 1994) for examples in number theory.

110. We of course lack analogous studies for other contemporary topics in order to appreciate more precisely how 'normal' it was. Certainly, that people generally did not remain in a given area does not seem to us to be a regular feature in physics at the time.

111. It is remarkable in this respect that the varieties of unification are not reduced; in 1930 as in 1920, we have found reductionist projects, attempts to integrate different fundamental phenomena on an equal footing, replacement by one phenomenon of a specific aspect in the theory of another, etc.

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