Karl-Heinz Schlote, Martina Schneider (eds.)

Mathematics meets physics

A contribution to their interaction in the 19th and the first half of the 20th century

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Geometry as Physics: Oswald Veblen and the Princeton School

Jim Ritter

Einstein's General Theory of Relativity has often been taken as the quintessential modern example of what can happen when a mathematical field comes into contact with a physical discipline. Indeed the story of the meeting between differential geometry and general relativity is often summed up in the literature by the expression "the geometrization of physics".

We shall be telling a different story here, one in which a group of mathematicians, in alliance with some physicists, consciously organized themselves into a school, organized on a large scale with a definite program. Their aim: to produce a new joint mathematics and physics project which would empower a great scientific advance in the domain of mathematics/theoretical physics, in turn raising the United States in general – and Princeton University in particular – to the position of a world-class scientific power. The primary creator and principal theorist of this project was Oswald Veblen and we shall be following the story from his point of view, as well that of his colleague and co-organizer Luther Pfahler Eisenhart. Having introduced the actors we now turn to the scenography.

The physical theory which the Princeton School saw as the target of their own mathematical work was that of general relativity and its extensions. What should be understood here by "general relativity" is that viewpoint, shared by many physicists in the nineteen-twenties, that general relativity, as a theory of gravitation, was just one step along the road that would lead to an ultimate theory of all physical interactions. As Einstein himself expressed it in 1925:

> "The conviction of the essential unity of the gravitational and the electromagnetic fields is firmly established today among the theoretical physicists working in the field of general relativity theory."[1]

To some, like Einstein, the constituent theories to be combined were limited to general-relativistic gravitation and Maxwellian electrodynam-

[1] «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.» (Einstein 1925, 414)

ics. For others, however, a third domain, that of the atomic structure of matter, had to be integrated on an equal footing.[2]

But one need not take Einstein's word for the interest in unification. An examination of the published literature for this decade yields a confirmation of this attitude.

Comparing the number of articles published in the field of relativity and in that of unified theories (defined as those attempting a union of at least two of the fields of gravitation, electromagnetism and theories of matter) in the years 1920, 1925 and 1930 yields the result shown in Figure 1.

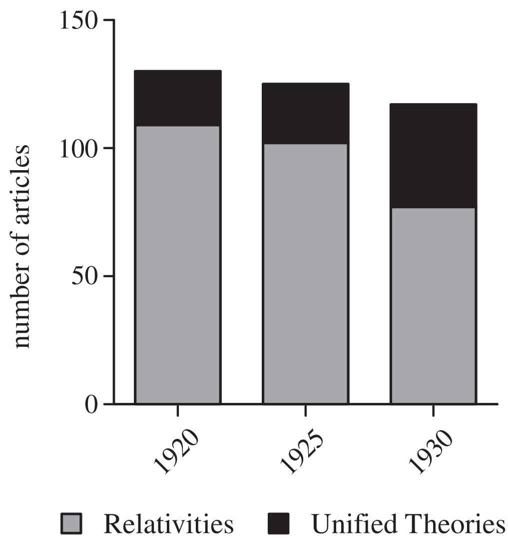

Figure 1
Number of publications in relativity (special + general) and unified theories 1920 – 1925 – 1930

The total number of articles show only a slight secular decrease over the decade but unified theories take a progressively larger proportion of this total, fueled at first by a decrease in the number of pure relativity articles and then, additionally, in 1930, by a major absolute increase in the number of unified-theoretical publications. A closer analysis of the contents of the articles and their authors show that the initial high level of interest in general relativity undergoes an important transfer

[2] Einstein's well-known objection to quantum theory was not based on a refusal of its results but arose from a conviction that quantum theory – old or new – should arise as a *consequence* of a correct unification of gravitational and electromagnetic interactions. This and the discussion which follows here are drawn from Goldstein & Ritter 2003.

to the new quantum theory of 1925/26, not immediately, but a bit later, following the publication of Dirac's electron theory (Dirac 1928). It was this latter which was seen at the time as offering a new alternative route to the unification of gravitation, electromagnetism and matter theory and, as such, inspired the surge in unified-theory publications at the end of the decade.

This then is the backdrop to a story which involves a group of bright and ambitious young American mathematicians in a small teaching university just after the First World War, with expertise in axiomatic and differential geometry facing new and exciting developments in the field of theoretical physics which seem to have a special relationship with their own or related fields. Within less than a decade Princeton University will be one of the foremost mathematical research institutions in America and the disciplinary program put in place will attract not only some of the brightest American mathematicians but quite a number of prestigious European scientists as well.

1 Oswald Veblen

When Oswald Veblen left the University of Chicago in 1905, two years after obtaining his doctorate, to take up his position at Princeton University as one of University president Woodrow Wilson's new "preceptors", he was already recognized as one of America's brightest young geometers. In particular, he had become, with Edward Vermilye Huntington at Harvard, the leading exponent of "American postulate theory",[3] an approach initiated by his Chicago thesis advisor, Eliakim Hastings Moore.

In the wake of the interest generated by the publication of David Hilbert's *Grundlagen der Geometrie* in 1899, Moore had organized a seminar at the University of Chicago on the foundations of geometry and analysis during the Fall quarter of 1901, with Veblen its most enthusiastic participant. (Parshall & Rowe 1994, 383 – 384). It was his experience in this seminar which set Veblen's interest and directly inspired his

[3] The term is due to Corcoran 1980. No particular name was used by the protagonists, who saw themselves simply as participants in the general and international discussion of the logical foundations of mathematics.

doctoral thesis on an alternate rigorous axiomatization of Euclidean geometry, based on "point" and "order" rather than Hilbert's "point", "line", "plane" and "between" as its founding undefined terms. But the most significant advance in this formulation was its emphasis on the question of the unique specification by the axioms of their object of study. As Veblen expressed it in the published version of his thesis:

> "Inasmuch as the terms *point* and *order* are undefined one has a right, in thinking of the propositions, to apply the terms in connection with any class of objects of which the axioms are valid propositions. It is part of our purpose however to show that there is *essentially only one* class of which the twelve axioms are valid. (...) Consequently any proposition which can be made in terms of points and order either is in contradiction with our axioms or is equally true of all classes that verify our axioms. The validity of any possible statement in these terms is therefore completely determined by the axioms; and so any further axiom would have to be considered redundant. Thus, if our axioms are valid geometrical propositions, they are sufficient for the complete determination of euclidean geometry."[4]

This question of "complete determination" – or *categoricity*,[5] the term introduced in this publication, borrowed by Veblen from fellow Chicagoan John Dewey[6] – was at first aimed at establishing a criterion for the determination of the possible redundancy of a set of axioms which, following the Italian school (Peano, Padoa), constituted the main thrust of Moore's own interest.[7]

But Veblen was quick to see a deeper significance to this question. For him and the other postulationists the real interest lay in the degree of precision and uniqueness which a set of axioms could achieve in designating the object(s) which they were created to formalize. By building up blocks of axioms, one could progressively sharpen the focus

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[4] Veblen 1904, 346

[5] Actually, the current term "categoricity" is of recent vintage. Veblen either uses the adjectival form "categorical" or, later and more rarely, the substantive "categoricalness", e. g., Veblen 1911, 49.

[6] Veblen 1904, 346, note '‡'; see the discussion in Grattan-Guinness 2000, 211.

[7] See for example Moore's own publication arising from this seminar, (Moore 1902), and the first part of his Presidential farewell address to the American Mathematical Society in December 1902, (Moore 1903).

and eliminate unwanted objects from the class of those defined by the axioms up to that point. As Veblen expressed it some twenty years later with reference to "elementary geometry," i. e., that class of theorems extending from topology through projective and affine to Euclidean geometry:

> "Each one of these groups of theorems is logically distinguished from its predecessor by the appearance of new relations which are brought in either by means of new axioms and undefined terms or by means of definitions which limit attention to a restricted class among the totality of possible geometric objects. At each stage the freedom of physical interpretation is restricted until, at the final step, it is necessary to specify the physical significance of a measuring stick and of a rectangular cartesian coordinate system."[8]

Veblen's further pre-War work on the axiomatic foundations of projective geometry and of topology (Veblen always preferred the older term "analysis situs") will not detain us here.[9]

For the rest of Veblen's story there are two significant influences at work in the period leading up to the 1920s: Veblen's research experience during the First World War at the Aberdeen Proving Ground, and his contact and collaboration with his fellow Princeton preceptor, then professor, the differential geometer Luther Pfahler Eisenhart.

The combination of the two led to Veblen's "differential-geometric turn" and his post-War program, jointly with Eisenhart, to apply the postulationist approach to that new field straddling differential geometry and physics that was general relativity. In fact, the program was more ambitious still: to refound both geometry and physics in a new synthesis that would replace the old – Euclidean geometry and Newtonian mechanics – in a way that would include, and transcend, all contemporary work in topology, projective, affine and Riemannian geometry, general relativity, and the new unified field theories of Hermann Weyl and Arthur S. Eddington.

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[8] Veblen 1923, 131 – 132

[9] For information on Veblen's work in these areas, as well as his later influence on Princeton logicians, see Aspray 1991.

2 The lessons of war

In a synthesis of the role of physics in American scientific war work, given at the end of 1919 by the physicist Gordon Hull before the American Association for the Advancement of Science, Veblen was singled out for particular praise:

> "A number of experiments [in ballistics] were carried on at Aberdeen [Proving Ground], chiefly by Major Veblen and Lieutenant Alger … It is seen that these experiments added greatly to the effectiveness and therefore to the value of the guns in question. The work belongs to physics, notwithstanding the fact that one of these civilian officers was and is a professor of mathematics of the purest quality. That he was able to bring himself temporarily to neglect the fundamental concepts of geometry, in which realm he is one of our foremost thinkers, to enter into the problems of the war with an eagerness for close observation of actualities and a readiness to try out new methods, is very greatly to his credit. He is evidently a physicist by intuition and a mathematician by profession."[10]

From this war experience and his recognized success in organizing both the personnel and the scientific-technical ballistics program of the brand-new Aberdeen Proving Ground, Veblen came away with two major ideas: 1° that a new style of organization, centering on large groups, working collectively on a common research program, would be necessary for America to gain world status in science and 2° that the subject of such a scientific project – for America at least – would lay in the close integration of mathematics with physics to tackle the principal question of contemporary science.[11]

2.1 The Princeton School

Yet another aspect of Veblen's war work to be integrated into the project concerned the organizational aspects of the geometry/physics program. The War – and Veblen's own experience in setting up a fruitful and practical ballistics group in a new testing facility – convinced him that the old ways of individual researchers were now outmoded; real advances

[10] Hull 1919, 228 – 229

[11] For details on Veblen's war experiences, see Grier 2001.

were to be expected only through teamwork and the full mobilization of talents. To this end the Princeton Mathematics Department, faculty and doctoral students alike, became part of one of the first experiments in organized research in the US academic world. Concentration on the complementation of individual research projects, a structured and focused curriculum, and money and space for creating an efficient infrastructure were all ingredients.

As to the subject of such a collective project, the enormous success of Einstein's new theory of relativity following the eclipse results announced in November 1919 gave an obvious indication of where a focus might be found to really advance the position and role of American mathematics in the new post-war world. Moreover, there would be little competition from Europe in the field of fundamental applied mathematics. As he expressed it a few years later in seeking to organize an independent research center based at Princeton: "This programme [at Princeton] embraces studies in the geometry of paths and analysis situs which are becoming more and more clearly the foundations of [general relativistic] dynamics and the quantum theory."[12]

The difficulty was that Veblen himself had little experience with either the new physics or the particular mathematical tools – differential geometry and tensor analysis on manifolds – that were used by general relativity and many of the proposed unified theories. Luckily for him there was in the Mathematics Department someone who did know these things: Luther Pfahler Eisenhart. It was he who had been invited to introduce the mathematical aspects of relativity to the American mathematical community (with Leigh Page of Yale for the physics) at the special session on relativity at the annual meeting of the American Mathematical Society at Columbia on 24 April 1920 (published as Eisenhart 1920a). And it was Eisenhart who defended the new Einstein theory against critical attack by anti-relativists like Philipp Lenard and the idiosyncratic American astronomer T. J. J. See (Eisenhart 1923b).[13] Finally it was Eisenhart who wrote to Einstein to invite him to Princeton

[12] "Institute for Mathematical Research at Princeton": the manuscript is undated and unsigned but certainly by Veblen, circa 1925. Cited in Aspray 1988, 352.

[13] For the relativity debate in the American astronomical community see Crelinsten 2006.

to lecture on his new theory.[14] The original invitation was an offer of a semester's visiting lectureship, which Einstein refused. When he was later convinced of the interest of a visit to the United States, he agreed to give the Stafford Little Lectures at Princeton in May 1921.[15] These became the classic Einsteinian introduction to general relativity in the English-speaking world and served as an implicit declaration by Princeton University of its claim to be the center of relativity research in America.

Moreover, in the decade following 1923, the doctoral students of Veblen and Eisenhart (and sometimes of both jointly): Tracy Y. Thomas, Harry Levy, Morris S. Knebelman, John H. C. Whitehead, all did their theses in the domain of the geometry of paths.[16] Even the logician Alonzo Church, who had stayed on at Princeton to write a thesis on mathematical logic under Veblen, was persuaded to publish two articles as a doctoral student in the geometry of paths program (Church 1924; Church 1927).

In addition to the more or less permanent members the Princeton Mathematics Department hosted a number of Visiting Fellows during this period who worked on the geometry of paths program or closely related issues, their financing provided by the new National Research Council fellowship program in mathematics instituted in large part through Veblen's urging.[17]

The recruitment of permanent staff was a particularly important desideratum for the Department. Tracy Thomas was perhaps the most enthusiastic younger member of the School; after having obtained his doctorate with Veblen in 1923, he then spent two years as a National Research Fellow, the first year at the University of Chicago and the second with Hermann Weyl at the ETH in Zurich. He returned to Princeton as a lecturer for the Fall Term of 1926 where he remained

[14] Eisenhart to Einstein, 20 October 1920 (Eisenhart 1920b).

[15] These five lectures, delivered in German, were published simultaneously in English translation in the U. S. and Great Britain (Einstein 1922a) and, in the same year, in German (Einstein 1922b).

[16] John L. Vanderslice was a late addition, finishing his thesis under Veblen in 1934, after the latter had left the University in 1932 to become the first director of the mathematical section of the Institute for Advanced Study.

[17] For Veblen's role in structuring post-World War I mathematics financing see Feffer 1999.

Table 1
The Princeton School 1922–1934.
(**Boldface** = permanent staff; *Italics* = Visiting Fellow; Roman = doctoral student with date of Ph. D.).

Luther P. Eisenhart 1900–1945		**Oswald Veblen** 1905–1932
	⟵Tracy Y. Thomas 1923⟶	
Harry Levy 1924		
		(Alonzo Church 1927)
	⟵Morris S. Knebelman 1928⟶	
		John H. C. Whitehead 1930 Banesh Hoffman 1932 John L. Vanderslice 1934
Mathematics	*Joseph M. Thomas* 1925–1926 **Tracy Y. Thomas** 1926–1938 *Jesse Douglas* 1927–1928 **Hermann Weyl** 1928–1929	
Physics	*Arthur H. Bramley* 1922–1925 **Howard P. Robertson** 1929–1947	

until his departure for UCLA in 1938. Howard Robertson, a brilliant young physicist from Cal Tech, was recruited in 1929 with a joint chair in Physics and Mathematics. At home in general relativity, quantum physics and differential geometry, he worked at first with Hermann Weyl. The latter had arrived as a Visiting Professor in his eyes but a permanent staff member for the Princeton group and his departure for Göttingen after only a year was a great disappointment to the Group at the time, though he was to Princeton (at the newly-founded Institute for Advanced Study) a few years later.

The course structure, both upper-class undergraduate and graduate, was revamped to provide more differential geometry and relativity courses, as well as joint seminars with the Physics Department. This revamping lasted much longer at Princeton than the many other similar attempts at other American universities. Indeed to the demographer George E. Immerwahr, recalling his undergraduate days at Princeton in 1926 – 1930, it seemed that "almost all the upper-class math was related somehow to relativity, which was a big subject at that time."[18]

2.2 The geometry of paths program

If the restructuring of the curriculum was a crucial element in the program, the choice of a research project was at the heart of the matter. The basic idea had arisen during Veblen's and Eisenhart's joint mathematical seminar on "The Theory of Relativity" during the 1921 – 1922 academic year.[19] It was in this context that a very ambitious program not only for mathematics but also for the new physics of the twentieth century was laid out. It was named the *geometry of paths* program and, though modified over the coming years, it remained the hallmark of the greater part of research in mathematics carried out for a decade at Princeton.

The actual work in the program had begun in 1922 with the publication of a series of notes in the *Proceedings of the National Academy of Sciences* (and not for example, as was standard for mathematicians, in the *Bulletin of the American Mathematical Society*), a sign that, as in that year's American Association for the Advancement of Science address, the Princeton

[18] Immerwahr 2003.
[19] Princeton University 1922, 260.

group were appealing to a wider audience, beyond the boundaries of pure mathematicians. However the narrower mathematical public was not ignored and in the following years, publications were sent to the *Bulletin* and *Transactions* of the American Mathematical Society (an organization of which Veblen was President in 1923 – 1924 and Eisenhart in 1931 – 1932), as well as to the *Annals of Mathematics* (of which both Eisenhart and Veblen were editors). These publications continued without respite during the nineteen-twenties; in the decade following 1922 and ending with Veblen's 'departure' for the Institute for Advanced Study in 1932 there were some one hundred articles and books published by the Princeton group relating to the geometry of paths program.

The mathematical basis of the program was introduced in the group's very first publication:

> "1. One of the simplest ways of generalizing Euclidean Geometry is to start by assuming (1) that the space to be considered is an n-dimensional manifold in the sense of Analysis Situs, and (2) that in this space there exists a system of curves called *paths* which, like the straight lines in a euclidean space, serve as a means of finding one's way about.
>
> These paths are defined as the solutions of a system of differential equations,
>
> $$\frac{\mathrm{d}^2x^i}{\mathrm{d}s^2} + \Gamma^i_{jk}\frac{\mathrm{d}x^j}{\mathrm{d}s}\frac{\mathrm{d}x^k}{\mathrm{d}s} = 0, \qquad (1.1)$$
>
> in which the Γ^i_{jk}'s are analytic functions of $(x^1, x^2, \ldots, x^n)$ and the indices i, j, k run from 1 to n. (...)
>
> This definition of the *paths* is immediately suggested by the fact that the differential equations of the straight lines in a euclidean space which are
>
> $$\frac{d^2x^i}{ds^2} = 0 \qquad (1.3)$$
>
> in cartesian coördinates, take the form (1.1) in general coördinates, the Γ's now being such that there shall exist an analytic transformation of $(x_1, x_2, \ldots, x_n)$ converting (1.1) into (1.3)."[20]

[20] Eisenhart & Veblen 1922, 19 – 20

However paths are not necessarily geodesics since no minimizing or maximizing criteria are to be demanded of them. They are defined simply as the solutions to the set of n^3 differential equations given by (1.1).

Now since in general relativity these solutions with Γ^i_{jk} the Levi-Civita connection (Christoffel symbols) are precisely the trajectories of physical particles, with mass (matter) or without (light), such an approach is not only a natural way to explore the underlying levels of affine and metric geometries, but gives an immediate and physically intuitive way into the higher-level kinematics and dynamics of the new physics.

Elected president of the American Mathematical Society for 1923 – 1924, Veblen devoted his presidential address to the question of a survey of recent foundational advances in geometry. In the opening section he raised the question of the relations between physics and mathematics:

> "The foundations of geometry must be studied both as a branch of physics and as a branch of mathematics. From the point of view of physics we ask what information is given by experience and observation as to the nature of space and time. From the point of view of mathematics, we ask how this information can be formulated and what logical conclusions can be drawn from it.
>
> It is from the side of physics that has come the most important contribution in the last two decades."[21]

That geometry and physics formed indeed part of the same discipline had been first put forward in an explicit fashion by Veblen in his retirement address as Vice-President of the Mathematics Section of the American Association for the Advancement of Science in the closing days of 1922:

> "[Geometry] consists of a sequence of statements arranged in a certain logical order but void of all physical meaning. In order to apply them to nature we identify the undefined terms (points, lines, etc.) as names of recognizable objects. The unproved propositions (axioms) are then given a meaning, and we can ask whether they are true statements. If they are true, then we expect that the theorems which are their logical consequences are also

[21] Veblen 1925, 121

> true and that the abstract geometry will take its place as a useful branch of physics."[22]

Moreover it was at this same Boston AAAS annual meeting that Veblen first publicly presented the outline of a specific and ambitious program to carry out for the new relativistic physics what had historically been done for the old Newtonian theory; discussing in some detail the manner in which the components of the classical Euclidean geometry–Newtonian physics complex could be seen as built up in Postulationist style out of incremental additions of blocks of undefined terms and the axioms and definitions concerning them. His presentation can be summed up by a diagram (Figure 2) in which each level is denoted by a circle which subsumes the axioms of those sub-domains (circles) within it as well as adding other postulates to capture the introduction of new higher-level concepts.[23]

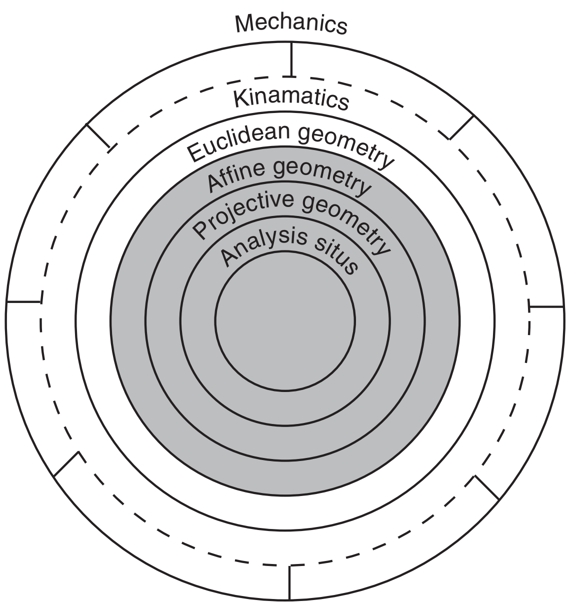

Figure 2
Veblen's view of the build-up of classical geometry and physics in terms of englobing axiom blocks (Based on Veblen 1923)

[22] Veblen 1923, 130. One might ask if, in Veblen's view, geometry is a part of physics why had it been miscatalogued as mathematics for so long? The answer is that "the branch of physics which is called Elementary Geometry was long ago delivered into the hands of mathematicians for the purposes of instruction" (Veblen 1923, 130).

[23] This diagram is my own résumé of the discussion in Veblen 1923.

> "Each one of these groups of theorems [defining a sub-discipline] is logically distinguished from its predecessor by the appearance of new relations which are brought in either by means of new axioms and undefined terms or by means of definitions which limit attention to a restricted class among the totality of possible geometric objects."[24]

The shaded circles represent those areas traditionally called "geometry". Further additions of axioms continue the construction into the domain traditionally called "mechanics" (non-shaded circles) though of course for Veblen there is no intrinsic difference in the nature of the enterprise.

As for the specific content of these various levels, Veblen goes into some detail for classical geometry/mechanics. One might summarize his discussion in the talk of the various sub-domains of this field by the first two columns of Table 2.

Thus starting from the fundamental level of analysis situs which associates triples of reals with points of a space, making analytic geometry possible, consecutively adds axioms concerning "straightness" (projective geometry), "parallels" (affine geometry) and "distance" (Euclidean metric geometry). With this last the domain of traditional domain called geometry is complete:

> "At each stage the freedom of physical interpretation is restricted until, at the final step, it is necessary to specify the physical significance of a measuring stick and of a rectangular cartesian coordinate system."[25]

Equipped with this measuring stick and cartesian coordinate system, one can build up the hierarchy of kinematics with the introduction of the postulates of time and substance (kinematics) and then of mass (mechanics in general).

The completion of the final stage, to produce specific dynamical systems is more complex. Newtonian mechanics, beyond the common "mechanics in general" is divided into a series of segmented higher-level theories because, while all the low-lying sub-disciplines are categorical (in the Postulationist sense),

[24] Veblen 1923, 131
[25] Veblen 1923, 131 – 132

> "the postulates for [classical] mechanics do not form a categorical set and cannot … until the [specific] substance and the forces are specified. … Mechanics is not a mathematical science, but is the group of theorems common to a collection of sciences. Each particular problem involves certain axioms in addition to those of mechanics in general."[26]

Table 2
Classical and modern constructions of geometry/physics

Domain of physics	**Classical physics**	**Modern physics**
Analysis situs	points $\longleftrightarrow (x, y, z)$ (analytic geometry)	continuous or discontinuous manifold
Projective geometry	theory of straightness	properties independent of infinitesimal parallelism
Affine geometry	theory of parallels	properties involving infinitesimal parallelism
Metric geometry	Euclidean geometry	Riemannian geometry
Kinematics	definitions of time and substance	special relativity
Mechanics	definitions of mass + specific axioms for each problem	general relativity and unified theories

So much for the past. Now "a series of brilliant discoveries in physics has been making the abstract [Postulationist] point of view a vital issue in that science also." (Veblen 1923, 129) Here Veblen has general relativity and its unified theory extensions in mind; their geometric base in Riemannian geometry however forces infinitesimal rather than finite structures in the construction of the geometry/physics discipline. Hence the necessary shift in the axioms and interpretation of the various levels of the hierarchy indicated in Table 2. It is this project that Veblen and Eisenhart had set into motion some eleven months earlier under the name of "the geometry of paths":

> "The geometry of paths can be considered as a generalization both of the earliest part of elementary geometry and of some of the most

[26] Veblen 1923, 134

> refined of physical theories. The study of the projective, affine and the metric geometry of paths ought to result in a comprehensive idea of what types of physical theory it is possible to construct along the lines which have been successful in the past."[27]

Others had, it is true, already begun the study of these geometries:

> "This generalized geometry has been studied by H[ermann] Weyl ... and ... by A[rthur] S[tanley] Eddington .... Both these authors define it in terms of a generalization of Levi-Civita's concept of infinitesimal parallelism rather than by the more natural idea of a system of paths."[28]

Not only did the Princeton School see their approach as different from one tied to a specific geometric interpretation of the connection and its (affine) generalization,[29] but they were to differentiate it even more sharply a bit later from the revived Erlangen Program championed in France by Élie Cartan, who had just begun to publish on this question in the same year.

The mention of Hermann Weyl in the filiation paragraph just cited was certainly the most important one for the Princeton School. They saw Weyl as the intellectual father of the approach which they were undertaking, particularly his gauge unified theory of 1919 – 1921; thus the importance of his acceptance of a position at Princeton University. I will not have the time here to go into detail concerning Weyl's relationship with the Princeton Group.[30] For what follows it is necessary only to point out that a unification of gravitation and electromagnetism was seen by Weyl to arise from the addition of a scalar field ϕ_i to the gravitational metric tensor g_{ij} such that a single variational principle will yield both the Einstein and the Maxwell equations, with the metric tensor as the gravitational potential and the "gauge field" ϕ as the electromagnetic potential. Both quantities are invariant under change of coordinates:

[27] Veblen 1923, 137

[28] Eisenhart & Veblen 1922, 20

[29] Aside from this reference to the affine generalization of the connection; the approach through a generalization of the idea of connection was adopted by the group around Jan Schouten at Delft.

[30] For this the reader is warmly recommended to Scholz 2001 and his chapter in this book.

"The linear and quadratic fundamental forms

$$d\phi = \phi_i \, dx^i \quad \text{and} \quad ds^2 = g_{ik} dx^i dx^k$$

describe the metric of the manifold relative to a reference frame (=*coordinate system* + *gauge*); they are invariant under coordinate transformations: under a change of gauge the second form gains a factor α which is a positive continuous function of position (the 'gauge factor'), and the first form is diminished by a total differential $d \lg \alpha$."[31]

Given the role played by Weyl in the inception of the path-theoretical approach it was thus appropriate that it was with his theory that the Princeton Group made its first intervention in physics.

3 Physics and the Princeton School

3.1 Responding to Weyl

Eisenhart, who had already published a series of articles on the groups of motion of static solutions of the Einstein field equations (Eisenhart 1921) became after 1922 the group's spokesperson for the application of the paths program to relativity theory and its extensions, with Veblen and other early members of the School, such as Tracy Y. Thomas and Harry Levy, concentrating at first on the geometric core.[32]

Though the program had been based from the beginning on paths as the fundamental object precisely in order to establish a direct connection between geometric objects and the trajectory of particles in the new physics, this point was first made explicit in the introduction to the first

[31] «Die lineare und die quadratische Fundamentalform

$$d\phi = \phi_i \, dx^i \quad \text{und} \quad ds^2 = g_{ik} \, dx^i \, dx^k$$

beschreiben die Metrik der Mannigfaltigkeit relativ zu einem Bezugssystem (= Koordinatensystem + Eichung); sie bleiben bei Koordinatentransformationen invariant, bei Abänderung der Eichung nimmt die zweite einen Faktor α an, der eine positive stetige Ortsfunktion ist (das ‹Eichverhältnis›), die erste vermindert sich um das totale Differential $d \lg \alpha$.» (Weyl 1919, 105)

[32] Eisenhart also of course participated in work on the geometric aspects of the theory and presented the group's general treatment of the geometry of paths approach in a series of five public lectures at the 1925 American Mathematical Society Colloquium, published as *Non-Riemannian Geometry* (Eisenhart 1927).

post-1922 Eisenhart physics article, concerning the Weyl unified gauge theory.

> "In the geometry of paths as developed by Professor Veblen and myself in a number of papers...the idea is that the paths are a generalization of straight lines in euclidean space.... Now I make the assumption that *physical phenomena manifest themselves in paths in a space-time continuum of four dimensions and that the functions* Γ^i_{jk} *are determined by the character of the phenomena*. In this note I apply this idea to the case of electro-magnetic phenomena as developed in the general theory of relativity, and the results raise the question whether Weyl, and later Eddington, are justified in the assumption that the fundamental vector introduced by Weyl in his gauging system is the electro-magnetic potential of the field."[33]

Now, directly from the condition that the class of paths be invariant (up to reparameterization) under a transformation of coordinates, Eisenhart obtains the class of physically equivalent paths (Γs); they are of the form:

$$\overline{\Gamma}^i_{\alpha\beta} = \left\{ {i \atop \alpha\beta} \right\} + \delta^i_\alpha \phi_\beta + \delta^i_\beta \phi_\alpha - g_{\alpha\beta}\phi^i$$

with $\{ {i \atop \alpha\beta} \}$ the Levi-Civita connection and ϕ an arbitrary vector. But this, with ϕ interpreted as the electromagnetic potential, is just the Weyl affine connection. This result had already been mentioned in passing in the 1922 articles on the geometry of paths program, e. g., (Eisenhart & Veblen 1922, 23). In spite of its casual treatment in these early notes, it seems clear that this result was central in the motivation of the Princeton group to pursue the geometry of paths approach.

Turning to Weyl's theory Eisenhart shows how the ϕ actually used is really

$$\phi^i = -\frac{1}{\mu}\phi^i_\nu \nabla_\rho \phi^{\nu\rho}$$

where $\phi^{\mu\nu}$ is the electromagnetic tensor (the 'curl' of the potential) and μ is the mass density of matter. Note that ϕ^i here is *not* the electromagnetic vector potential but a vector dependent on the electromagnetic *and* gravitational field (through the covariant derivative ∇ and the metric tensor used to raise and lower indices).

[33] Eisenhart 1923a, 175; emphasis in the original

Now enters the geometry of paths. Eisenhart had already established, as we saw earlier, that physically equivalent paths are only defined up to an arbitrary vector field. Choosing this ϕ for such a field defines then an affine connection, precisely that chosen by Weyl. But, Eisenhart points out, the ϕ here employed is *not* the vector potential as was assumed by Weyl. Furthermore, argues Eisenhart, if the conformal weight of the ordinary vector potential is normalized to +1, then ϕ's conformal weight is -1, underlying the difference between them. Weyl and Eddington are wrong, concludes Eisenhart, in assuming that the gauge vector is the electromagnetic potential; it is ϕ that plays this role.

Thus the Weyl theory as it stands is apparently not a unified theory of the gravitational and electromagnetic field but needs to be modified if the electromagnetic potential is to figure directly as a variable.

Eisenhart ends his paper with a mention of applications of the program to Einstein's field equations as well as those introduced by the latter in 1919 as his first proposal for a unified field theory[34]:

> "Mr. A. Bramley of the Department of Physics of Princeton University has shown, in a paper to be offered to the Philosophical Magazine, that [the Einstein 1915 and the Einstein 1919] equations … are consistent, if the weight of [the electromagnetic potential] is taken to be one, and if [this potential] is not supposed to be the fundamental vector ϕ_α of the gauging system, but functionally related to it in such a way that ϕ_α is of weight zero."[35]

3.2 Widening the circle

The article referred to by Eisenhart was published under the rather unexpected title "Electronic conduction in metals" (Bramley 1923) by a young doctoral student at Princeton, Arthur Bramley. It was in fact a follow-up to a note proposing a derivation of value of the Planck constant *h* from the Maxwell equations and a model of electron radiation that had been published in a preceding *Philosophical Magazine* article (Bramley 1922) when Bramley was still an undergraduate at the University of Oregon. Coming to Princeton in 1923 to do experimental physics and write a thesis on the refractive index of helium, he discovered exciting new mathematical tools, probably first through Edwin P. Adams of the

[34] Einstein 1919. We shall return to this theory in a later section.

[35] Eisenhart 1923a, 178

Physics Department, the author of the first standard American textbook on (the old) quantum theory and the translator of Einstein's Princeton Lectures. The new article, written at Princeton, deals with an extension of his original model, now using new geometrical ideas explicitly drawn from the geometry of paths to explain J. J. Thomson's then popular "doublet theory" of metallic conduction.

Arthur Bramley was the first instance of the Princeton group's attempt to widen the disciplinary attraction of their program. After a purely mathematical collaboration with Eisenhart's doctoral student Harry Levy (Levy & Bramley 1923 – 24), published in the *Annals of Mathematics*, Bramley concentrated on the application of geometrical methods – specifically the geometry of paths – to problems arising in atomic physics, publishing in both mathematics and physics journals. He served as the group's principle source of information on developments in quantum theory until he left to become the first Bartol Fellow at the new Bartol Research Foundation in 1925.

This first collaboration with the Physics Department was to be followed by others, most notably with Harry Robertson at the end of the decade. Furthermore Veblen insisted on close physical proximity between the two Departments. Veblen's original office was inside the Physics Department's Palmer Physical Laboratory and when he has the occasion to help design the first Mathematics Department building in 1929, he added a hallway to connect the new Fine Hall and Palmer Laboratory.[36]

3.3 Responding to Einstein

Einstein had spent the early nineteen-twenties in modifying the unified theories of others but in 1925 he had his own non-symmetric affine theory to propose.[37] Starting from an asymmetric affine connection (from

[36] Until the completion of Fine Hall in 1931, the Princeton Mathematics Department possessed no specific building. The few mathematicians with office space were scattered about the campus, Veblen in Palmer and Eisenhart and Department chairman Henry Fine in the main administration building, Nassau Hall (Aspray 1988).

[37] A discussion of Einstein's various unified-theory proposals, along with French translations of some of the articles, is to be found in Ritter 1993. See also Goenner 2004.

which he constructs an asymmetric Ricci tensor R_{ab}) and an independent asymmetric tensor density $\mathcal{G}^{ab}$, Einstein varies the Lagrangian

$$\delta \int \mathcal{G}^{ab} R_{ab} d\mathbf{x} = 0$$

independently with respect to the two quantities $\mathcal{G}$ and Γ^a_{bc}, which yields the following field equations:

$$R_{ab} = 0$$
$$\frac{\partial \mathcal{G}^{ab}}{\partial x^c} - \frac{\partial \mathcal{G}^{ba}}{\partial x^c} = 0$$
$$-\frac{\partial g_{ab}}{\partial x^c} + g_{rb}\Gamma^r_{ac} + g_{ar}\Gamma^r_{cb} + g_{ab}\phi_c + g_{ac}\phi_b = 0$$

with ϕ_a an arbitrary covariant vector that Einstein will want to interpret as a electromagnetic potential. The first set of field equations arising from variation of the $\mathcal{G}^{ab}$ represent the generalization of general relativity while the last two, coming from variation of the Γ^a_{bc}, are the equivalents of Maxwell's equations in this context.

Three years after the critique of Weyl's unification scheme Eisenhart felt ready to tackle this new Einstein theory (Eisenhart 1926). In the introduction to the article he states his aim:

> "In proposing his recent theory of gravitation and electricity Einstein has derived his equations by expressing that a certain integral is stationary for the variations of a … tensor density of the second order and the coefficient of an asymmetrical connection. In this note we show more particularly what kind of a linear connection Einstein has employed and obtain in tensor form the equations which in this theory should replace Maxwell's equations."[38]

As in his response to Weyl, Eisenhart points out in his final section that the identification of the arbitrary ϕ_a as the electromagnetic 4-potential is not as straightforward as the author thinks. Taking the linear approximation of the last set of the new Einstein field equations, he shows that they do *not* reduce to the linearized version of the corresponding Maxwell equations and that ϕ_a is not simply the expected potential.

[38] Eisenhart 1926, 125

But now, even though Bramley was gone, Eisenhart was no longer alone among the Princeton School members to venture onto the terrain of contemporary physics. Joseph M. Thomas, a National Research Fellow for 1925 – 1926 recruited from the University of Pennsylvania, participated too in the Princeton School's response to the new unified theory. In a paper published in the same number of the *Proceedings of the National Academy of Science* as Eisenhart's, he proposed an alternate derivation of Einstein's field equations via a certain generalization of the ordinary general relativistic equations:

> "Recently Einstein has deduced a unified theory of electric and gravitational fields. ... I show in the present paper that his equations can be obtained by direct generalization of the [empty space GR] equations. The process of generalization consists in abandoning assumptions of symmetry and in adopting a definition of covariant differentiation which is not the usual one, but which reduces to the usual one in case the connection is symmetric."[39]

Thus far, the attitude is not far from that of Eisenhart. The role of the mathematician is to make rigorous the mathematics used in an intuitive fashion by the physicist, pointing out how other options are available for the latter to do or to do more clearly what he has already done. But unlike Eisenhart, J. M. Thomas was prepared to go further. The method of generalization he develops in the article can be used not only to redo the Einstein route but can reproduce other physicists' work. Thomas devotes the last section of his paper to

> "show that the adoption of the ordinary definition of covariant differentiation leads to a geometry which is a special case of Weyl as a basis for the electric theory; further, that the asymmetric connection for this special case is of the type adopted by Schouten for the geometry at the basis of his electric theory."[40]

The references to Weyl and Schouten refer to the Weylian gauge theory discussed above and Jan Schouten's 1923 unified theory (Schouten 1923) as a generalization of Weyl's. The grounds of the intervention of J. M. Thomas in the debate among physicists is thus different than the more traditional attitude represented by Eisenhart. A mathematician can

[39] J. M. Thomas 1926, 187
[40] J. M. Thomas 1926, 187

not only improve the understanding of a particular theory, he can use sophisticated methods to restructure the domain by relating apparently disparate theories to each other.

This work sprang out of the Princeton Group's turn to the projective-geometric part of their project and, in this context, to a more analytic approach, centering on differential invariants as the privileged point of departure for further research.[41] When, the year before, Eisenhart had given one of the two annual American Mathematical Society Colloquium lecture series on the subject of "The New Differential Geometry", which summed up the position of the program as of that date, no particular mention of projective spaces had been made. When he published the lectures in 1927 under the title *Non-Riemannian Geometry* (Eisenhart 1927), he was obliged to add a whole section, over one-quarter of the book, to cover this subject.

3.4 Differential invariants

When Veblen was invited to give one of the plenary lectures at the September 1928 International Congress of Mathematicians in Bologna, Italy, he chose as the subject to introduce the Princeton Group project: "Differential Invariants and Geometry." In a very real sense this lecture was a response to one given at the previous Congress in Toronto four years earlier by Élie Cartan on "La théorie des groupes et les recherches récentes de géométrie différentielle" in which the French mathematician had expounded his program to refound differential geometry on the basis of a revitalized Erlangen Program, one based on Lie groups rather than the classical groups that Klein had originally proposed as the basis for geometry. At the end of his talk he had mentioned in passing the Princeton program.[42] Veblen now took this opportunity to reply to Cartan:

> "The Klein theory of geometry seems to be showing the same symptoms as a physical theory whose heyday is past. More and more complicated devices have to be introduced in order to fit it

[41] The differential-invariant approach had been summed up by Veblen two years earlier in his Cambridge Tract volume *Invariants of Quadratic Differential Forms* (Veblen 1927).

[42] Cartan 1928. For a detailed analysis of the Princeton and Paris programs see Ritter 2011.

to the facts of nature. Its fate I should expect will be the same as that of a physical theory – it becomes classical and its limitations as well as its merits are recognized. (...) We are on the way to recognize that the space may be characterized in many other ways than by means of a group. For example, there is the fundamental class of spaces of paths ... which are characterized by the presence of a system of curves such that each pair of points is joined by one and only one curve of the system...

If we give up the idea of making one concept – such as the group concept – dominant in geometry, we naturally return to something like the starting point of Riemann's discussion. ... We prescribe only the continuous nature of the manifold to be considered and the analytic character of the operations. There has indeed been an uninterrupted development of the Riemannian geometries along these... unprejudiced lines. I mean Lipschitz, Christoffel, Ricci and, more recently, the mathematical physicists. This work seemed to most mathematicians to be extremely formal and narrow in outlook. But it was continually developing the ideas of differential invariant theory.... The theory of one or more such invariants is what we call a geometry."[43]

Veblen had chosen to take a sabbatical first semester that year, not only to attend the Congress but also, invited by his friend G. H. Hardy, to teach a term at Oxford. In an invited lecture delivered to the London Mathematical Society on 14 February 1929, Veblen outlined once again the recast program for the path-geometrical project:

"In recent years geometry has passed definitely beyond the boundaries set for it by the Erlanger *Programm*. According to the Erlanger *Programm* a geometry is the invariant theory of a group. According to the new conception a geometry is the theory of an invariant. This invariant may or may not have a non-identical group of automorphisms. If it has such a group, the geometry will be one of the classical type characterized by Klein. If not, it is of a generalized type. Thus the group of a space is regarded as one of its important properties, but not as its all-sufficient characteristic one.

I do not propose to discuss this question in general terms ... Instead, I shall attempt an introductory account of a particular

[43] Veblen 1929a, 182 – 183

> class of geometries which arise by generalization from the classical projective geometry. The first mathematician to recognize the possibility of a generalized projective geometry was, I think, Weyl, who showed in 1921 how it is possible to vary an affine connection in such a way as to keep projective properties unaltered, and to obtain a tensor, analogous to the curvature tensor, which is unaltered by these changes.
>
> This discovery was soon followed by studies of infinitesimal projective displacements – analogous to the infinitesimal parallelism of Levi-Civita – by Schouten and Cartan, from which there emerges a theory of what we may call (after Cartan) the non-holonomic projective spaces.
>
> At the same time some of my colleagues, especially Eisenhart and T. Y. Thomas and J. M. Thomas, in looking for the theorems of a geometry of paths which would be independent of any particular representation of the system of paths by means of an affine connection, were finding further projective invariants and getting at their geometric significance.
>
> Also the mathematical physicists, particularly O. Klein, were developing the so-called five-dimensional relativity which was first put forward in 1921 by Kaluza as a method of giving a unified theory of gravitation and electromagnetism.
>
> What they were doing is, however, as I hope to show elsewhere, better understood as a projective theory in which the supposed five-dimensional feature is a device covering the use of homogeneous coordinates in a space of four dimensions."[44]

Armed with these new tools and with, as we shall see, a new recruit to the Group, Veblen was ready to carry the fight onto the physicists' own ground. Veblen had moved into the outer, physical circles of his geometrico-physical concentric rings.

3.5 Talking to physicists

The last two paragraphs of his London talk cited above have no counterpart in the Congress address of five months previously. They are the result of Veblen's contact with one of the students at Oxford that term, a young physicist named Banesh Hoffmann, who was very

[44] Veblen 1929b, 140 – 141

impressed by both the Princeton project and social and intellectual style of Veblen (Hoffmann 1984). Once again the American mathematician had found someone who could provide the Princeton mathematicians with expertise on the physics side, both in general relativity and in modern quantum mechanics. Hoffmann came to Princeton and, together with Veblen, published an article on a projective reformulation of the Kaluza-Klein unified theory to which they gave the name of "projective relativity". Drawing on the geometry of paths but resolutely physical in content, it was sent to *Physical Review* and appeared in the 1 September 1930 issue of that journal.

> "In this paper we show that the formalism of O. Klein's version of the five-dimensional relativity can be interpreted as a four-dimensional theory based on projective instead of affine geometry. The most natural field equations for the empty spacetime case are a combination into a single invariant set of the gravitational and electromagnetic field equations of the classical relativity without modification. This seems to be the simplest possible solution of the unification problem.
>
> When we drop a restriction on the fundamental projective tensor which was imposed in order to reduce our theory to that of Klein a new set of field equations is obtained which includes a wave equation of the type already studied by various authors. The use of projective tensors and projective geometry in relativity theory therefore seems to make it possible to bring wave mechanics into the relativity scheme."[45]

Coauthoring the article with Hoffmann was, for Veblen, a way into contemporary physics. For the first time since the inception of the program in 1922 he was publishing an article proposing a specific physical theory. On his trip through Europe in 1932 he gave lectures on the new theory; at Göttingen, Vienna and Hamburg he lectured and propagandized on the subject. The lectures themselves were published by Veblen – in German – as the second volume of the newly-founded and prestigious Springer series, *Ergebnisse der Mathematik und ihrer Grenzgebiete*.[46]

[45] Veblen & Hoffmann 1930
[46] Veblen 1933

But when Veblen made this trip he was no longer – at least from an administrative point of view – part of Princeton University. Indeed, the main purpose of that trip was to find and recruit mathematicians for the Institute for Advanced Study of which he was the first head of the Mathematics School.[47] In many respects the just-created Institute was a realization of the earlier plans by Veblen for an American research center in mathematics and physics. Though initiated by people and institutions without much knowledge of Veblen's original ideas, the final selection of the Princeton mathematician to be scientific head of the new organization (instead of the original choice, Harvard's George David Birkhoff) meant that the program to be instituted bore a very close resemblance to what Veblen had thought of as the ideal infrastructure for the carrying-out of the plan to combine mathematics and physics. Of the five original members of the Mathematics School: James Alexander, Albert Einstein, Walther Mayer, John von Neumann, and Hermann Weyl, one (Einstein) was not a mathematician but a physicist and two had published recently on physics as well as on mathematics (Weyl on relativity and unified theories and Von Neumann on quantum mechanics).[48] Only Alexander was a pure mathematician, a topologist; he had however passed his entire career up to that point at Princeton where he had been Veblen's student[49] then colleague. And the fact that the institute remained physically at Fine Hall until the completion of its own building, Fuld Hall, in 1939, meant that a constant contact was maintained between the two institutions. Still with no classes to teach or students to advise, the old dynamic could no longer be maintained.

[47] For a detailed discussion of the early years of the Institute see now Batterson 2006.

[48] Einstein and Weyl of course were viewed by the Princeton School, as a number of citations above witness, as the precursors and founders in a sense of the Princeton program. Walther Mayer, a mathematician, was Einstein's collaborator and had been given his position at the Institute on the demand of the latter with the understanding that Mayer would continue to collaborate with Einstein on unified theories.

[49] As such, of course, he did not escape a temporary participation in the geometry of paths program and, like Alonzo Church, had published two articles on the subject: Alexander 1925–26 and Alexander 1926–27. The question of the extent to which Alexander saw his topological work of the nineteen-twenties as part of the path-geometrical program remains an open question.

3.6 Winding down

With Veblen involved with setting up the Institute for Advanced Study and Eisenhart, as Dean of the College, very occupied with administrative duties, the Princeton School no longer had a functioning center. But the University now had a reputation in the field of mathematics such that the activities centered around other faculty members such as Solomon Lefschetz, Eugene Wigner and Alonzo Church would constitute new centers. Moreover the recruitment of talent on both the national and international levels no longer posed any problem. The visitors that filed through Princeton included Paul Alexandroff and Heinz Hopf (1927 – 28); G. H. Hardy (1928 – 29); John von Neumann (1929 – 30 and later); John H. Roberts and J. H. van Vleck (1937 – 38); and Claude Chevalley (1939 – 40). But their agendas were no longer those of the Princeton School and the latter's part in the story of the rise of Princeton University as a center for research in differential geometry and in theoretical physics was largely forgotten.

But not quite completely. There had been three major centers of research in the differential geometry of generalized spaces and their connection with general relativity and unified theories during the first decade and a half following the Great War: Princeton, Paris with Élie Cartan and Delft, Holland with Jan Schouten and his school.[50] Often in conflict during the earlier period, the nineteen-thirties saw a convergence. Schouten and David van Dantzig had developed a projective unified theory quite similar to Veblen and Hoffmann's at the same time[51] using a very different approach, directly generalizing the connection. They explicitly pushed the similarities between their results and those of both the Princeton School and Cartan's work on projective spaces. Although Cartan himself never attempted a direct foray into physics, Shing-Shen Chern, back in Beijing, China after his studies with Wilhelm Blaschke in Hamburg and Cartan in Paris, produced his first major paper: "On Projective Normal Coordinates" in 1937, which he published in the Princeton-based *Annals of Mathematics*, and in which he showed the conditions for reconciling the Cartan and Princeton School definitions

[50] Details of these three centers will be discussed in Ritter 2011.

[51] See the review article they produced for the physics community, Schouten & van Dantzig 1932, and the literature cited therein.

of projective normal coordinates.[52] And in Princeton itself, Veblen's last student, John L. Vanderslice, who had followed him to the Institute in 1932, published a thesis in which he showed how the "non-holonomic" (generalized) spaces of Cartan could be viewed from the standpoint of the geometry of paths – using moreover a postulationist axiomatic approach. But this is not an abandonment of the Princeton School's program, as he points out in the introduction to his thesis:

> "We do not take the position that the non-holonomic geometries defined by our postulates represent the only significant generalized geometries, nor do we wish to minimize the importance of other points of view toward these same geometries. Our treatment is not in conflict with the conception of a geometry as the theory of a geometric object;[53] the analytical development soon gives rise to a "geometric object"... upon which the subsequent discussion is based. Rather does our theory furnish one method among many of discovering geometrical objects which are of significance."[54]

Moreover Eisenhart continued to publish his very influential books on differential geometry; significantly his next, after the programatic *Non-Riemannian Geometry* of 1927, was called *Continuous Groups of Transformations* which appeared in 1933, with generous references to Cartan and Schouten as well as to the Princeton School.[55]

The nineteen-thirties thus saw a series of attempts from all sides to show the equivalence of the three major approaches to the question of the generalization of traditional differential geometry. The final result was a certain consensus which, generalized again in a complicated development, starting in the late nineteen-thirties but extending well into the nineteen-fifties, led to a modern synthesis, essentially under the name of Cartan. Without denying the central role played by the French mathematician, the result historically is in part a cause and in part an effect of the post-World War II split between mathematics and physics in this domain, together with the marginalization of general relativity,

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[52] Chern 1938. Furthermore Chern always referred to modern differential geometry as a joint production of the Princeton School and of Cartan. See, for example, Chern 1979.

[53] The reference here is to Veblen 1929b, see the quotation above, section 3.4.

[54] Vanderslice 1934, 154

[55] Eisenhart 1927; Eisenhart 1933.

both classical and in the extended sense of unified field theories. The unity between mathematics and physics worked for by the Princeton School and to a large extent also by the group around Schouten was lost, ironically perhaps, in large part due to the work of Princeton geometers of the post-Veblen period.

Thus ended one of the first attempts to create a modern research organization in American mathematics and physics. The kind of unity across mathematics and physics that Veblen foresaw was one that demanded not a simple application of mathematics to already existing physics or even a modification of mathematics through its contact with contemporary physical problems, but one that would remodel both mathematics and physics by showing their essential unity and drawing on the lessons of the past – Euclidean geometry and Newtonian physics – to inspire new geometries and new physics. Not that this demanded a uniform means of putting this into practice: we have seen the difference among the approaches of Eisenhart, the traditionalist, offering rigor and completeness as a mathematician to existing theories; J. M. Thomas proposing to go further and modify existing theories on the basis of mathematical demands; and Veblen and Hoffmann, engaging fully in terms of their own physical theories. Moreover the idea of having mathematicians and physicists working together through joint professorships for mature researchers and doctoral mentoring for younger ones, and joint seminars and publications, remind us to what extent the mathematics-physics frontier is not about the interface between some hypostatized intellectual domains but rather one of real mathematicians and physicists meeting to solve real problems.

That they did not in the end solve these problems is hardly surprising; they are with us still today. Nor is the model they created necessarily the best-suited to achieve that aim. But the experiment that started in post-War Princeton was in many respects a forerunner of our modernity, both in terms of infrastructure and of intellectual approach. The fact that mathematics and physics are now in one of their episodic *rapprochements* renders it profitable to look back at the last time the two fields nearly met, in a truly usable past.

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