"W. R. Hamiltons Bedeutung für die geometrische Optik," Jahresber. d. deutschen Math.-Verein. **30** (1921), 69-82.

W. R. Hamilton's significance to geometrical optics

Inaugural speech delivered on 26 February 1921

By G. PRANGE in Halle a. d. S.

Translated by D. H. Delphenich

The history of science gives many examples of valuable insights that were unnoticed to the detriment of its development. However, it is extremely rare that great scientific achievements are indeed discussed by all, but their intrinsic essence remains completely unknown, which was the fate of one part of the lifework of the Irish mathematician W. R. Hamilton. Indeed, one finds extensive chapters on Hamilton-Jacobi theory in the textbooks on *analytical mechanics*, one hears of the *Hamiltonian formula* in the *differential geometry* of line congruences, and Hamilton's discovery of *conical refraction* is celebrated as one of the great works of the human spirit in any physics book. However, it is less known that all of those things belong to a unified conceptual picture that arose from advances in the domain of geometrical optics that first appeared almost one hundred years ago when the aforementioned researcher, who was just nineteen years old at the time, presented a treatise with the title "On Caustics" to the Irish Academy ([1]). That treatise was not printed, but the seeds that were planted in it were fortunately developed in four great treatises on the theory of ray systems ([2]), which is, unfortunately, even less well-known. Only a rather small part of Hamilton's wealth of new ideas have been recovered in two better-known treatises in the Philosophical Transactions ([3]), in which **Hamilton** adapted the methods that he had developed in *optics* to *mechanics*, and in particular to the treatment of *perturbation problems* in astronomy. The textbooks on mechanics do not even present that small amount in true Hamiltonian form, but in a *recasting* of it that it experienced due to **Jacobi** and his school. For example, in the *calculus of variations*, **Hamilton** had employed many of the arguments that were recently posed in the construction of the theory of extremal fields as essentially its starting point. The *geometry of ray systems* has suffered less from that. Although one can also regret the fact that **Hamilton**'s exceedingly intuitive method of operation has not been perpetuated, nonetheless, the theory has developed up to now, and it soon vastly outgrew **Hamilton**'s results in its systematic formulation

([1]) Remarkably, a reference to it in a lecture by **F. Klein** to the Naturforscherversammlung in Halle (1890) attracted very little attention. Cf., Jahresbericht der Deutschen Mathematiker-Vereinigung **1** (1890/91), pp. 35.

([2]) **W. R. Hamilton**, "Essay on the theory of systems of rays," Trans. R. Irish Acad. **15** (1828), 69-174. Three supplements to it in *loc. cit.*, **16** (1830), 3-62, 93-126, as well as **17** (1837), 1-144. The extensive third supplement is especially important.

([3]) **W. R. Hamilton**, "On a general method in dynamics," Phil. Trans. (1834), 247-308 and "Second essay on a general method in dynamics," Phil. Trans. (1835), 95-144.

as line geometry. However, severe damage has been done to *geometrical optics* by the *widespread ignorance* of Hamilton's work, and indeed not only in a theoretical context, but also in precisely its practical application to the *construction of optical instruments.*

If one surveys the history of geometrical optics over the last one hundred years then it will seem clear that is was just the demands of improving the construction of optical instruments that were driving the advances in the general properties of optical instruments, rather than restricting oneself to investigating one instrument of a particular design. One repeatedly finds first attempts at achieving that great objective in one way or another. None of those researchers suspected that they might find answer to their questions in the works of **Hamilton**, which already had much more to offer than one might even dare to ask. Please permit me to attempt to acknowledge the true significance of Hamilton's works. Corresponding to modern thinking, I would then like to direct one's attention to the question of applying theoretical knowledge to the practice of constructing instruments, in particular.

One cannot establish what induced **Hamilton** to investigate geometrical optics, since his first treatise "On Caustics" was unpublished. From its title, one might infer that he believed that geometrical optics was connected with the investigation of planar caustics, the theory of which was revitalized in the first decade of the previous century. However, the true source of his ideas is not revealed by that. Rather, they arose from the results of the conflict between the wave theory of light and the emission theory, by which the *light wave* of *undulatory optics* and the *light ray* of the *emissive optics* were intrinsically related ([1]). In ordinary optical media, the *rays* that emanate from a luminous point and go through an optical instrument are the *normals* to the wave surfaces. **Hamilton**'s starting point for that situation is the knowledge that one can also find the wave surfaces in the congruence of light rays independently of the wave theory of light. Their existence is based upon the fact that the propagation of light along the rays is governed by a variational principle, viz., the so-called *principle of shortest light path:*

$$\int n\,ds = \text{extremum},$$

such that the wave theory and the emission theory seem to be formally unified at a higher level since that principle has deep significance for both theories. Each point P of the rays that emanate from a luminous point (viz., the extremals of the variational problem) is associated with a certain value of the light path:

$$V(x, y, z) = \int_0^P n\,ds,$$

such that we will get a covering of space by the function V (the light path length), and the level surfaces of that covering are the surfaces that the undulatory theory of light refers to as wave surfaces, since the light path means the time that it takes for light to propagate in the wave theory. The level surfaces V = const. cut the light rays perpendicularly, because from the boundary formula of the calculus of variations, the partial derivatives are:

([1]) On that, cf., **F. Klein**, *loc. cit.*

$$\frac{\partial V}{\partial x} = n\,\alpha\,, \quad \frac{\partial V}{\partial y} = n\,\beta\,, \quad \frac{\partial V}{\partial z} = n\,\gamma\,,$$

when α, β, γ are the direction cosines of the rays. To some extent, those surfaces then serve to fix the system of light rays as a totality, as they characterize the nature of the ray system. Correspondingly, **Hamilton** referred to the function V as the *characteristic function* of the ray system. As one sees immediately, it satisfies the partial differential equation:

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

which says that the gradient of the covering is constant.

When one translates those steps into the language of the modern calculus of variations, they will obviously say that one is dealing with the extremals of a variational problem with its transversal surfaces, and the essential properties of such a thing are already known. I expressly emphasize the fact that the integral, which one calls the **Hilbert** integral nowadays, must be independent of the integration path.

In the textbooks, the *Hamilton-Jacobi theory* is presented *as a method of integration* for the differential equations of mechanics, or more generally, the Euler-Lagrange equations of a variational problem. The fact that such a determination of the extremals *was not Hamilton's starting point* is closely related to the fact that in ordinary optics, where light traverses homogeneous media, the extremals – i.e., the light rays – are simply straight lines, and also the fact that light rays can be kinked immediately at the separation surface between two media under refractions and reflections. If **Hamilton** (and I shall return to this) thought of determining the characteristic function V from the partial differential equation then his goal was just that of establishing the ray system as a whole.

Naturally, that should not be understood to mean that **Hamilton** only had the general case of rectilinear light rays in mind and did not recognize the possibility of directly generalizing things to all variational problems. On the contrary, he thought that the relationship between light rays and wave surfaces must indeed remain meaningful when the index of refraction n is more generally a function of position and direction in the medium:

$$n = n(\dot{x}, \dot{y}, \dot{z}\,; x, y, z).$$

In fact, the two treatises on mechanics also show that **Hamilton** really did recognize the generality of his method. Above all, it emerged from a note to the British Association Reports ([1]), which remains completely unknown, in which he applied it to an entirely general variational problem that one probably cares to associate with the name of **A. Mayer** nowadays.

([1]) **W. R. Hamilton**, "Calculus of principal relations," Reports of the British Association for the Advancement of Science **5** (1836, pt. 2), 41-44.

The "field," with its characteristic function $V(x, y, z)$ is always placed at the center of attention. The *coordinates of the luminous point* then appear in the function $V = V(x, y, z\,;\, x_0, y_0, z_0)$ *as parameters*. However, from the standpoint of the variational problem that was just posed, it is also possible to look at things a different way, namely, to consider those coordinates to be *equivalent to the coordinates of the running point*, namely, when one fixes a certain light path between two prescribed points. The light path length along the ray:

$$V(x, y, z; x_0, y_0, z_0) = \int_{x_0, y_0, z_0}^{x, y, z} n\, ds$$

will then be a pair of limiting points – a *point-pair function*, one might say. Each of the two points of the pair of limiting points is associated with a plane by the relations:

$$\frac{\partial V}{\partial x} = n_x\,, \quad \frac{\partial V}{\partial y} = n_y\,, \quad \frac{\partial V}{\partial z} = n_z\,; \quad \frac{\partial V}{\partial x_0} = -n_{x_0}^{(0)}\,, \quad \frac{\partial V}{\partial y_0} = -n_{y_0}^{(0)}\,, \quad \frac{\partial V}{\partial z_0} = -n_{z_0}^{(0)}\,,$$

such that a transformation of the "element" that consists of the point and plane at the one limiting point into the "element" at the other limiting point seems to be exhibited in the variational problem. If one regards the value of V as an independent variable, as **Hamilton** did expressly ([1]), then one will have a one-parameter group of transformations of elements, and the characteristic property of variational problems means that any union of elements will be taken to another union of elements. One sees from that suggestion how clearly he was rooted in the concept of the group of contact transformations, which arises from the variational problem. That conception of the last formulas is his most deeply-rooted idea. However, by no means did he initially have any intention of constructing a systematic theory of integration for those differential equations from them, which he naturally also recognized as the first and second integral of the differential equations of the light ray. Under no circumstances did he wish to obtain the function V from the two partial differential equations:

$$\Omega\left(\frac{\partial V}{\partial x}, \frac{\partial V}{\partial y}, \frac{\partial V}{\partial y}, x, y, z\right) = 0, \quad \Omega_0\left(-\frac{\partial V}{\partial x_0}, -\frac{\partial V}{\partial y_0}, -\frac{\partial V}{\partial y_0}, x_0, y_0, z_0\right) = 0,$$

which arise by eliminating $\dot{x}, \dot{y}, \dot{z}$ ($\dot{x}_0, \dot{y}_0, \dot{z}_0$, resp.), in order to then solve the differential equations with their help.

In the two treatises on mechanics, where the optical variational problem was replaced with a mechanical one, the state of affairs above no longer emerges clearly. The partial differential equations and the representation of the integrals of the equations of motion with the help of partial derivatives of V took the foreground then, while the wealth of other ideas that the optical considerations had given rise to were left in the background. However, the fact that one does not also imagine a systematic theory of integration now already emerges from the fact that he said one must derive an *intellectual pleasure* from the representation of the first and second integrals, even when no *practical facility* can be achieved from it. **F. Klein** had occasionally said of that remark

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([1]) Cf., also **E. Study**, "Über Hamiltons geometrische Optik und deren Beziehung zu den Berührungstransformationen," Jahresbericht der Deutschen Mat.-Verein. **14** (1905), 424-438.

that the two partial differential equations in Hamilton's theory are not a "mere ornament." Naturally, Klein was only rejecting the idea with that, since **Hamilton** actually wanted to determine the characteristic function V from them. However, that should not diminish their significance, since it is precisely in mechanics that their application to the perturbation calculations of planetary systems that they define the gist of the argument.

I will quite briefly gloss over the *relationships* between **Jacobi** and **Hamilton**, and say that **Jacobi** learned of Hamilton's two treatises on *mechanics* only at precisely the time that they appeared, while he did not know of the foregoing treatises on optics at the time, and indeed it is doubtful whether he had studied them in detail at all.

His attention was focused most intently on the relationship between the two partial differential equations and the system of total differential equations. That is because he had been familiar with those relationships from his earliest work onward, in which he was concerned with the integration of a single partial differential equation, which was begun by **Lagrange** and **Pfaff**. With his own preconceived ideas, he had to regard Hamilton's thoughts in a totally one-sided way and recast them. I shall once more emphasize that the thought of a systematic integration that Jacobi nurtured was completely foreign to **Hamilton**, since the latter believed that it did not introduce anything new at all, but only that it led back to Hamilton's starting point in optics when one went from the point-pair function and the two partial differential equations to *one* partial differential equation and its "complete integral."

For the further treatment of the optical problem by **Hamilton**, I once more link it with the conversion of one union of elements into another union of elements, in which the luminous point must also be regarded as the carrier of a two-dimensional union of elements, in particular. In the normal case of rectilinear light rays, the planes will remain parallel to each other under propagation along a ray. In that case, it is preferable to introduce another function in place of the characteristic function V that arises from V applying the so-called *Legendre transformation*. **Hamilton** called it T:

$$\frac{\partial V}{\partial x} = n_{\dot{x}} = \sigma, \quad \frac{\partial V}{\partial y} = n_{\dot{y}} = \tau, \quad \frac{\partial V}{\partial z} = n_{\dot{z}} = v,$$
$$\frac{\partial V}{\partial x_0} = -n_{\dot{x}_0}^{(0)} = -\sigma_0, \quad \frac{\partial V}{\partial y_0} = -n_{\dot{y}_0}^{(0)} = -\tau_0, \quad \frac{\partial V}{\partial z_0} = -n_{\dot{z}_0}^{(0)} = -v_0,$$

$$T(\sigma, \tau, v; \sigma_0, \tau_0, v_0) = \sum (\sigma x - \sigma_0 x_0) - V.$$

One immediately attaches the first derivatives of T to the equations of a light ray with a given direction.

For rectilinear light rays, the function T has the advantage over the function V that it is not coupled with a partial differential equation. That is because when one transforms the partial differential equations for V, they will each degenerate into a finite equation:

$$\sigma^2 + \tau^2 + v^2 = n^2,$$
$$\sigma_0^2 + \tau_0^2 + v_0^2 = n_0^2$$

that are also still free of T itself.

If one restricts oneself to considering a single congruence of rays with a fixed center, as one does in optics when investigating the mapping properties of optical instruments, then one would have to subject only one sequence of variables x, y, z in V to the Legendre transformation and thus arrive at a function W that exists between V and T. W is not restricted by a partial differential equation either, when it is regarded as a function of the direction of the ray. I shall note in passing that one can conversely fix that function W as a function of the direction cosines arbitrarily and then go to the function V that satisfies the partial differential equation by performing the Legendre transformation backwards, such that one will find a solution of the partial differential equation for V. That argument, which **Hamilton** has thought through more carefully ([1]), is obviously an example of what **Lie** ([2]) systematically carried out for the corresponding integration of partial differential equations.

If I now further go a little way into the applications of those arguments in optical practice then I will first encounter their connection with the *differential geometry* of ray systems. For the questions of optical maps, **Hamilton** indeed had to concern himself with a section of neighboring rays in a congruence, so with the simplest case when wave surfaces and light rays were perpendicular to each other, namely, the section of neighboring surface normals. Indeed, the connection to the theory of surface curvature that it then implied broke down when the wave and the light ray were no longer normal to each other. However, it provides the possibility of a generalization insofar as it regards the contacting spheres of the elementary theory of surface curvature as wave surfaces of light propagation that result from their centers. When one replaces the contacting spheres in the general case with the "ray surfaces of optics," i.e., the waves of a homocentric light propagation in one and the same medium, one will then suitably adapt the considerations in the curvature theory of surfaces to the concerns of optics.

For optics, the investigations into the formation of the focal surface and the arrangement and distribution of the rays in the vicinity of the focal surface are also still meaningful to this day. However, it is a problem in practical optics to give that focal surface a singularity that is as high as possible in order to achieve an at least approximately homocentric reunion of the rays of the congruence. Whereas one might expect from this remark that investigations of that kind would be followed up on zealously, it is somewhat shameful for mathematics that it has not confronted that problem spontaneously, and that it was the Swedish eye doctor **Gullstrand**, the discoverer of the well-known point lenses (*Punktalbrillengläser*), who had addressed a systematic investigation of the form of the focal surface ([3]).

For the construction of optical instruments, the axially-symmetric instruments have a preeminent significance in practice. It is in precisely that case that the fecundity of Hamilton's method is illuminated most brilliantly, and that made it possible for him to promote the theory of those instruments so far that one cannot even grasp the significance of his results, especially since he himself published only a brief note ([4]) with a concise summary of the final results.

([1]) Cf., the second supplement to the essay.

([2]) Cf., e.g., **S. Lie**, *Geometrie der Berührungstransformationen*, Leipzig, 1896), pp. 531.

([3]) **A. Gullstrand**, "Zur Kenntnis der Kreispunkte," Acta math. **29** (1905), 59-100. "Über Astigmatismus, Koma und Aberration," Ann. Phys. (Leipzig) (4) **18** (1905), 941-973. "Die reelle optische Abbildung," Svenska Vetenskapsakad. Handlinger **41** (1905), no. 3, 119 pages.

([4]) **W. R. Hamilton**, "On some results of the view of a characteristic function in optics," Reports of the British Association for the Advancement of Science **3** (1833), 360-370.

Due to axial symmetry, the characteristic functions depend upon only certain combinations of coordinates. For a theory of maps that always take a ray to another ray, the function $T(\alpha, \beta, \gamma; \alpha_0, \beta_0, \gamma_0)$, which depends upon the directions of the rays, seems to be most suitable. Due to axial symmetry, the variables appear in it only in the combinations:

$$\alpha_0^2 + \beta_0^2, \quad \alpha_0 \alpha + \beta_0 \beta, \quad \alpha^2 + \beta^2.$$

Now, if one develops the function T in a series in a neighborhood of the symmetry axis:

$$T = T_0 + T_2 + T_4 + \ldots,$$

and one truncates the series at the second-order term then one will get, as a first approximation, precisely the relation between object space and image space, which depends upon three constants, that **Gauss** ([1]) had worked out fully (somewhat later) in his dioptric investigations. If one takes one more term then one will get the so-called third-order *aberration* in the map.

Since one would always like to map only a single plane in the object space, one would do better to employ the function $W(\alpha, \beta, \gamma; \alpha_0, \beta_0, \gamma_0)$, instead of the function $T(\alpha, \beta, \gamma; \alpha_0, \beta_0, \gamma_0)$. Now, it is possible (and **Hamilton** still did not generally work though this in detail) to implement this inclusion of further terms in the theory of series developments in a way that is completely analogous to the method in celestial mechanics by which one places the Gaussian dioptrics in a sense "parallel" to the intermediary Kepler ellipse and the considers the deviations that result from the introduction of perturbation terms. In that way, as **Schwarzschild** showed ([2]) (but generally in a different way), one will succeed in giving the practical calculations for optical instruments a very transparent form, and that will clear the path to a theory of fifth-order aberrations. **Hamilton** could address the problem of *achromatism*, viz., the elimination of chromatic aberration, which is even more essential than the union of the rays for the quality of the image, in his theory by establishing the dependency of the color on one parameter in the index of refraction, and further in the characteristic function.

Hamilton himself gave a brief summary of the results of his theory of third-order aberration in the 1833 note. Starting from **Seidel**'s arguments ([3]), **Finsterwalder** ([4]) examined the same problem in the year 1892. It is quite surprising to see how the **Finsterwalder**'s results agree precisely with what **Hamilton** had summarized sixty years before as a *resumé* of his investigations.

Naturally, optics would prefer to eliminate those "aberrations" completely and impose the demand upon the *ideal instrument* that every congruence of rays from a luminous point will again be united into precisely a centric congruence. **Hamilton** had treated the preliminary question of defining such an instrument and showed that all light paths would have to possess the same length

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([1]) Cf., **Fr. Gauss**, "Dioptrische Untersuchungen," Abhandl. der Kgl. Gesellschaft der Wissenschaften zu Göttingen **1** (1840), 1-34. Cf., also *Ges. Werke*, Bd. 5, pp. 243-276.

([2]) **K. Schwarzschild**, "Untersuchungen zur geometrischen Optik I," Abhandl. der Kgl. Gesellschaft der Wissenschaften zu Göttingen (2) **4** (1905), 1-31.

([3]) **L. Seidel**, "Zur Dioptrik," Astron. Nachrichten **43** (1856), 289-332.

([4]) **S. Finsterwalder**, "Die von optischen Systemen größer Öffnung und größer Gesichtsfeldes erzeugten Bilder," Abhandl. der Kgl. Bayerischen Akademie der Wissenschaften, Math.-phys. Klasse **17** (1892), 519-587.

then, which would immediately come to mind if one considered the wave surfaces. Naturally, the same thing would also be true when the congruence was not really united, strictly speaking, but any two neighboring light rays intersected. We note in passing that this led him to the connection between the sections of neighboring extremals and the vanishing of the second variation, as it appeared in the so-called "Jacobi criterion" in the calculus of variations.

Meanwhile, the question of the so-called anastigmatic map of a finite region in space or a surface patch is nowhere to be found in **Hamilton**. The first property can, in fact, only pertain to entirely trivial cases, as it is currently assessed, while the question of anastigmatic surface patches still awaits further study, and up to now only **Abbé** ([1]) has posed his sine condition for the approximate anastigmatism of two surface elements.

The sine condition is very closely connected with certain reciprocity relations between object and image that were noticed very early on (although I would not like to go further into that at the moment). **Newton**'s contemporary, **Roger Cotes** ([2]) had already pursued the relationship between the mapped object and the cone of light rays for an axially-symmetric system of lenses that links the center of the pupil of an eye to the individual object points. In that way, one does not deal with the optical map of the object point to the image point then, but with the so-called optical projection. One selects a principal ray through the center of the pupil from each mapped congruence of rays. However, the so-called "congruence of principal rays" that arises in that way naturally possesses all of the properties of a congruence of rays that emanates from a luminous point.

Cotes intersected the rays of the cone that entered into the eye with an axis-perpendicular plane such that a figure that was congruent to the object arose in that plane and referred to the distance from that plane to the eye as the *apparent distance*. The formula that he derived for the apparent distance in an axially-symmetric instrument of thin lenses showed that the particular symmetry remained unchanged when one switched the places of the object and image along the axis. That law is not (as it might perhaps seem at the moment) an interesting curiosity by itself, but it still possesses great optical significance, because an important consequence concerning the ratio of the surface luminosities of the object and image can be derived from it. Moreover, that consequence defined the starting point for **Helmholtz** ([3]), who discovered the sine law at the same time as **Abbé**.

The reciprocity of the apparent distance is self-explanatory when considered from **Hamilton**'s standpoint. Namely, if one defines the characteristic function V for the light path that is bounded by the object point and the pupil of the eye then that apparent distance will be nothing at all but one of its second derivatives of the form $\frac{\partial^2 V}{\partial y_1 \partial y_2}$, which is differentiated with respect to the coordinates of the eye 1 and the object 2. Exchanging the object and the eye will not change that second derivative.

Extending from the special case considered, **Hamilton** had made a general study of the relationship between the object and the cone of projected rays through the center of the pupil of the eye for an entirely arbitrary optical device. The result that could be achieved from the symmetry

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([1]) **E. Abbé**, "Beiträge zur Theorie des Mikroskops usw.," Schultzes Archiv für mikroskop. Anat. **9** (1873), 413-468. See also *Ges. Abhandlungen*, Bd. I, pp. 45-100.

([2]) Cf., the presentation in **R. Smith**, *A compleat system of optics*, Cambridge, 1738.

([3]) **H. von Helmholtz**, "Die theoretische Grenze für die Leistungsfähigkeit der Mikroskope," Ann. Phys. (Leipzig) Jubelband (1874), 557-584. See also *Wissenschaftliche Abhandlungen*, Bd. II, pp. 185-212.

of the second derivatives is sometimes summarized briefly as: *Two eyes with congruent circular pupils that regard each other through an instrument will see the pupils as congruent ellipses* ([1]).

Naturally, those reciprocity relations depend upon only the existence of the characteristic function, so they can be found once more everywhere that one is dealing with variational problems. It is well-known that **Helmholtz** ([2]), in particular, had later deduced far-reaching physical consequences from such laws.

In the latter comments, I have already touched upon the developments in the last hundred years several times. In particular, how one looks for general properties of optical maps came into view in the example of the sine law. We should not wonder how the idea of introducing a characteristic function popped up repeatedly in those studies. However, not once had anyone recognized the relationship between such a function and analytical mechanics, and not once was the full richness of **Hamilton**'s ideas realized, even approximately, in the domain of optics. I mentioned only **Helmholtz** ([3]), who probably knew of the characteristic function from England and had employed it occasionally. He encouraged the physicist **Thiesen** ([4]) to seek to develop a theory of optical instruments with the light path length, but the latter remained stuck in the early stages of that project. On the other hand, the great work of **Bruns** ([5]) on the *eikonal* is well-known, in which he started from the idea of contact transformation and represented the ray-wise relationship between object space and image space by introducing a single concept that he knew of as the *eikonal*, but likewise without rediscovering rich content of Hamilton's ideas. It was **F. Klein** ([6]) who recognized the close kinship between Bruns's eikonal and Hamilton's characteristic function. In particular, **Bruns** was led to his investigations by the ambition to continue the work of **Abbé** ([7]), who had placed the general properties of Gaussian dioptrics that are independent of the special arrangement of the individual instruments at the forefront. **Abbé**, who united scientific research and its engineering exploitation in a singular way, referred to the fact that it is precisely the optical industry that requires a general theory like the one that had existed in **Hamilton**'s work for a long time. It would certainly be advantageous for its development if a reference to his results were to be introduced into it. The fact that this has generally not happened, even in England itself, should surprise no one that knows the arrogance with which English engineering looks down upon the so-called *scientific humbug.* By contrast, in Germany there is an ambition to organically incorporate the results of scientific research activities continually into the business of engineering production (we see this especially in the chemical industry, along with the optical industry). In order for that to happen, it is necessary that scientific research and engineering practice should not be foreign to each other. They must continually take care that they understand each other. I believe that I see in

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([1]) Cf., **W. Thomson** and **P. Tait**, *Treatise on natural philosophy*, v. I, Cambridge, 1879, pp. 358.

([2]) **H. von Helmholtz**, "Über die physikalische Bedeutung des Prinzips der kleinsten Wirkung," Crelles Journal für die Mathematik **100** (1887), 137-166 and 213-222. Cf., also *Wissenschaftliche Abhandlungen*, Bd. III, pp. 202-248.

([3]) **H. von Helmholtz**, *Handbuch der physiologischen Optik*, Leipzig, 1867 (2nd ed., 1896), esp. § **19**. Cf., also *Wissenschaftliche Abhandlungen*, Bd. II, pp. 147, *et seq*.

([4]) **M. Thiesen**, "Beiträge zur Dioptrik," Sitzungsberichte der Kgl. Preussischen Akademie der Wissenschaften zu Berlin (1890), 799-813.

([5]) **H. Bruns**, "Das Eikonal," Abhandlungen der Kgl. Sächsischen Gesellschaft der Wissenschaften, math.-physische Klasse **21** (1895), 325-435.

([6]) **F. Klein**, "Über das Brunssche Eikonal," Z. Math. Phys. **46** (1900), pp. 372.

([7]) For the work of **E. Abbé**, cf., **S. Czapski**, *Theorie der optischen Instrumente nach Abbé*, Breslau, 1893.

that fact one of the most important problems that is entrusted to applied mathematics. The problem of the "mathematical executive" shall not just be the graphical and numerical treatment of mathematical formulas up to their numerical evaluation, but beyond that lies the working through of the Ansätze and conceptual structures of engineering methodically from their mathematical content and to clarify and then work through them in order to ensure the production of mathematical tools and possibly create new ones. Our problem in optics is a model for those ideas. If one is to avoid the danger that lies in the fact that one seeks to work with inadequate Ansätze with overly-detailed mathematical tools and thus to once more revive the ominous clash between (bad) theory and practice, then one has every right to place optics next to mathematical physics as a younger sister. If the concept enters into the general consciousness (and I mean that the future teacher should also help with that agenda) then the skepticism with which the majority of non-specialists regard new results in mathematical research will wane.

H. Poincaré ([1]) had once countered such voices by saying that the engineer in the Twenty-Second Century would know how to assess the current research in the domain of mathematics. I believe that prominent men of contemporary engineering might reply: "Do not just think about the race of engineers in such distant times, but also think of us a little. We know what powerful tools can be created, but we also know how to forge them into a shape that is useful in our hands."

As I said, I would like to see one of the problems of applied mathematics in that general conception of the confrontation of the natural philosophy of science and the mastery of nature by engineering. The fact that it is often the case that one simply uses what is available is shown, not least of all, by the example that we have just treated. However, my latter comments probably make it clear that I do not mean it in the narrow historical sense if I would like to choose the following words to be the motto of this paper:

"What you inherited from your fathers,
acquire it in order to possess it!"

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([1]) **H. Poincaré**, "L'avenir des mathematiques," Rendiconti del circolo matematico di Palermo **26** (1908), 152-168.