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The principle of least action as the logical empiricist's *Shibboleth*

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Abstract

The present paper investigates why logical empiricists remained silent about one of the most philosophy-laden matters of theoretical physics of their day, the principle of least action (PLA). In the two decades around 1900, the PLA enjoyed a remarkable renaissance as a formal unification of mechanics, electrodynamics, thermodynamics, and relativity theory. Taking Ernst Mach's historico-critical stance, it could be liberated from much of its physico-theological dross. Variational calculus, the mathematical discipline on which the PLA was based, obtained a new rigorous basis. These three developments prompted Max Planck to consider the PLA as formal embodiment of his convergent realist methodology. Typically rejecting ontological reductionism, David Hilbert took the PLA as the key concept in his axiomatizations of physical theories. It served one of the main goals of the axiomatic method: "deepening the foundations." Although Moritz Schlick was a student of Planck's, and Hans Hahn and Philipp Frank enjoyed close ties to Göttingen, the PLA became a veritable *Shibboleth* to them. Rather than being worried by its historical connections with teleology and determinism, they erroneously identified Hilbert's axiomatic method *tout court* with Planck's metaphysical realsm. Logical empiricists' strict containment policy against metaphysics required so strict a separation between physics and mathematics to exclude even those features of the PLA and the axiomatic method not tainted with metaphysics.

Keywords: Principle of least action; Calculus of variations; Hilbert's axiomatic method in physics; Mach-Planck controversy; Logical empiricism; Moritz Schlick; Hans Hahn; Philipp Frank

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1. Introduction

Over the centuries, no other principle of classical physics has to a larger extent nourished exalted hopes of a universal theory, has constantly been plagued by mathematical counterexamples, and has ignited metaphysical controversies about causality and teleology than has the principle of least action (henceforth PLA).[1] After some decades of relative neglect, by the end of the nineteenth century the PLA and its kin enjoyed a remarkable renaissance on three levels.

Since the work of Hermann von Helmholtz, the PLA had become a very successful scheme applicable not only in mechanics, but also in electrodynamics, thermodynamics and relativity theory. Did this spectacular success indicate that physicists possessed—to cite Helmholtz—"a valuable heuristic principle and leitmotif in striving for a formulation of the laws of new classes of phenomena" (Helmholtz, 1886, p. 210), or were these principles—as Ernst Mach held—just useful rules that served the economy of thought in various domains of experience?

A second important reorientation took place in variational calculus, the mathematical discipline on which the PLA was based and which had accompanied it through more than two centuries of philosophical debates. Karl Weierstraß' critical investigations demonstrated that the precise relationship between the PLA and the differential equations resulting from it was extremely subtle, and that physicists' customary reasoning in solving important cases only obtained under supplementary conditions. The generations of Euler and Lagrange typically had identified the PLA and the differential equations resulting from it regardless of their metaphysical attitude towards the PLA and the quantity of action. In the nineteenth century, variational calculus was regarded as a very useful method in analysis the application of which however required considerable caution. Gauß, Jacobi, and many others obtained several important rigorous results, but only Weierstraß found the first sufficient condition for the variational integral to actually attain its minimum value. In three of his most influential twenty-three "Mathematical Problems," David Hilbert (1900) filed a plea to rigorously and systematically develop variational calculus in the direction shown by Weierstraß, and in the twenty-third problem he supplied new technical means to do so.

It was, thirdly, Mach's interpretation of the history of the PLA which permitted a fresh start on the philosophical level. All natural teleology associated with the

[1] There is a fairly decent historical literature on the PLA and variational calculus up to 1900; among them (Goldstine, 1980), (Pulte, 1989), and (Schramm, 1985). On Helmholtz's very influential work, see (Hecht, 1994). An overview of the physical applications is given in the classic of Lanczos (1986), and more recently (Lemons, 1997). Yourgrau and Mandelstam (1968) combines a detailed study of the physical applications of the PLA with an analysis of its significance in natural philosophy (Ch. 14). Their negative conclusion rests upon the presupposition that the PLA and the differential equations are completely equivalent. As almost all the physical and philosophical literature, they thus neglect the mathematical subtleties of variational calculus which played a substantial role in Planck's and, in particular, in Hilbert's appreciation of the PLA. As I shall also show, Hahn, Frank, and, arguably, Schlick were well aware these results. Unfortunately reasons of space make it impossible to enter into further mathematical detail here. But the coherence of the philosophical argument presented here requires only historical proof that Hahn and Frank were well-versed in variational calculus.

PLA—so he argued in *The Science of Mechanics* (first edition 1883)—was the product of an epoch that was theologically tempered. The main obstacle for empiricists to assess the PLA, the claim that it revealed a superior harmony or material teleology [*Zweckmäßigkeit*][2] inaccessible to empirical investigations, thus disappeared. Moreover, well-entrenched teleological explanations in biology could be embraced within the Machian notion of causality: functional dependencies between the determining elements—a notion that was intended to cut back the metaphysical concepts of cause and effect to their empirical basis of factual relations.

Logical empiricists saw themselves to a large extent in Mach's footsteps, but they rejected his empiricist philosophy of mathematics and sided with Boltzmann as to the indispensability of non-observational terms in scientific theories. Largely accepting Mach's empiricist notion of causality, they were not biased by a Kantian approach that would a priori give preference to differential equations and Newtonian local determinism over the PLA. While in Kant's *Critique of Pure Reason* (Newtonian) causality was ranked as a (synthetic a priori) category, the *Critique of Judgment* treated all teleological maxims only as regulative principles directing human judgement.

At first glance, these three changes could have permitted logical empiricists to reject the classical metaphysical claim that all natural laws boil down to a fixed set of minimality principles but cherish the PLA as a mathematical principle that was almost universally valid in physics—and thus presumably more fundamental than differential equations and the concept of causality built upon them. They could have further argued that this general principle receives its concrete physical content by supplying the Lagrangian characteristic for the particular theories of mechanics, relativity, etc.; likewise, Newton's axioms are specified by the force acting on the material bodies.

An interpretation of this kind never came to the fore; not even in a version mitigated by conventionalism according to which the PLA represented an empirically equivalent formulation that was simpler in important respects and permitted a unified approach. Browsing through the writings of logical empiricists, one finds instead almost no mention of one of the most-discussed and most philosophy-laden physical principles of the day. It is the aim of the present paper to explain why.

As a matter of fact, the silence was not a matter of ignorance. Moritz Schlick had been a student of Max Planck, one of the key advocates of the PLA. Philipp Frank wrote his dissertation on the PLA from the modern mathematical perspective, and Hans Hahn was a leading researcher in variational calculus. Moreover, both Hahn and Frank spent one semester at Göttingen where they studied under another main

[2] When it comes to philosophy, the German *Zweckmäßigkeit* is notoriously difficult to translate. *Teleology*, *finality*, and *purposiveness* capture only part of it, and since Kant's *Critique of Judgment, Zweckmäßigkeit* also denotes the systemic structure found in organisms. For reasons of historical continuity with the philosophical tradition around the PLA, I will stick to the term "teleology" even though one of the protagonists of this paper, Moritz Schlick (1925), explicitly distinguishes biological *Zweckmäßigkeit* and metaphysical *Teleologie*.

advocate of the PLA, David Hilbert. To be sure, there were major conceptual differences between the approaches of Planck and Hilbert. But these differences would have been a topic worthy of attention for philosophers of science, not the least because they involved two founding fathers of that modern science for which Logical Empiricism sought to be the most natural philosophy. Why, then, did the PLA rather become a philosophical *Shibboleth* to Schlick, Hahn, and Frank?

First of all, one might be inclined to cite its teleological connotations. Yourgrau and Mandelstam, in this vein, hold that "[t]he belief in a purposive power functioning throughout the universe, antiquated and naive as this faith may appear, is the inevitable consequence of the opinion that minimum principles with their distinctive properties are signposts towards a deeper understanding of nature and not simply alternative formulations of differential equations in mechanics." (1968, p. 174) In reality, to their mind, "variational principles evince greater propinquity to derived mathematico-physical theorems than to fundamental laws," (1968, p. 178f.) such that all teleology ascribed to them "presupposes that the variational principles themselves have mathematical characteristics which they de facto do not possess." (1968, p. 175) Here I disagree. There are subtle differences between the local differential equations and the integral PLA, even though the quantity of action has no fundamental physical importance. To be sure, these mathematical intricacies cannot back metaphysical claims about a general teleology in the style of Maupertuis. However, insisting that the PLA possesses particular mathematical characteristics which support a merely formal unification of physical theories per se does not require a metaphysical stand at all.

Both its staunchest advocates and those remaining silent about the PLA shared the conviction that final causation, material or organismic teleology, and analogies with human behavior had to be kept out of physics. The only exceptions were some passages of the late Planck written in the context of the relation of science and religion. Moreover, none of the protagonists of the debate under investigation considered the PLA as an instance of backward causation. The history of physical teleology might alternatively suggest a relationship between the PLA and the problem of determinism. This reached back to the classical criticism which Richard Bentley had leveled against the explanatory completeness of Newton's celestial mechanics.[3] Although to some protagonists of the present story, Ludwig Boltzmann's statistical mechanics had made it a viable option that the basic processes of nature were indeterministic, neither PLA-advocates nor logical empiricists contemplated any relation between the PLA and the second law of thermodynamics.[4] Rather, they explicitly restricted the validity of the PLA to reversible phenomena regardless of their views on causality.

[3] See (Schramm, 1985, Chap. 2).

[4] The only exception is, interestingly, Boltzmann (1866) himself who attempted to relate Clausius' second law of thermodynamics to the PLA. But he succeeded only for strictly periodic systems—not quite a generic case in thermodynamics. Although he would subsequently assign ever increasing importance to statistical concepts in understanding the Second Law, as late as 1899 he returned to his old idea when closing his lectures at Clark University: "It turns out that the analogies with the Second Law are neither

Thus the present paper has to seek an answer by a different route. (i) To logical empiricists, the mathematical universality claimed for the PLA represented an illegitimate border crossing between physics and mathematics because, on their account, there was no way that mathematics could contribute to the factual content of a scientific theory. In their perspective, the PLA was nothing but an equivalent mode of mathematical description. (ii) logical empiricists widely held that the price to be paid to reconcile Mach's empiricism with modern mathematics was to consider mathematics as a science of tautologous transformations. This did not permit them to attribute any other advantage to the PLA than calculatory economy. (iii) The same containment strategy against metaphysics also prevented a due appreciation of Hilbert's axiomatic method in the empirical sciences. In the end, both Hilbert and Planck were—at least implicitly—charged with realist metaphysics. This neglected the two levels present in the PLA and in Hilbert's axiomatizations. There were *general* mathematical arguments—such as coordinate invariance or the non-trivial fact that a variational principle could be set up—and there were *particular* physical axioms or the specific Lagrangians. To logical empiricists, all that was just a homogeneous set of logical relations coordinated to observations.

Reconstructing the debates and the silences, I shall investigate Mach's stand first. At surface value, the PLA represented merely an economical reformulation of the differential equations of motion. But Mach also adopted a principle of unique determination that had become quite popular among energeticists and his Berlin ally Joseph Petzoldt, a principle that in their hands even resounded classical Leibnizian ideas. Second, I discuss Planck's and Hilbert's pleas for the PLA. Although Planck was well aware that the PLA represented a universal scheme rather than a world formula, he considered it as an important step towards the ideal aim of attaining knowledge about the real world. Hilbert, as a matter of fact, repeatedly alluded to a (non-Leibnizian) pre-established harmony between nature and thought, but his mathematical reductionism expressed in the slogan "deepening the foundations" (*Tieferlegung*) was rather methodologically oriented. It was grounded in his joint beliefs in the unity of mathematics and that all mathematical problems ultimately receive a definitive answer in a suitable sense.

If "deepening the foundations" were to suggest that empirical content could be anchored in mathematics proper, logical empiricists had to vigorously object and deem Hilbert's reverence for Leibniz as a sure sign of out-dated metaphysics. In the remaining sections I shall show how indeed Schlick, Hahn and Frank each argued on this line. While Hahn advocated basically the pure form of my above-stated thesis, in Schlick and Frank there exists also a link between the notion of causality and the PLA. Schlick's early esteem for the PLA was influenced by the fact that simplicity represented a constitutive feature of causal laws, a view he was to abandon in 1931. Frank's concept of causality was more liberal than Schlick's and intended to embrace all allegedly teleological explanations in the life sciences. But Frank's

(*footnote continued*)
simply identical to the Principle of Least Action, nor to Hamilton's Principle, but that they are closely related to each of them." (1905, p. 306)

containment strategy did not halt at biological teleology, and he strove against the slogan that "the new physics was mathematical."[5] This very general criticism of Frank is at odds with the fact that *The Law of Causality and its Limits* (1932, Chap. III, p. 22) brings up a rather intricate example in which the PLA fails to recover the equations of motion.[6] Silence in Frank's case thus means not to bridge this gap and ignore virtually all the sophisticated philosophical problems raised by Helmholtz, Planck and Hilbert.

2. The PLA in Mach's Mechanics

In his influential *Science of Mechanics*, Mach considered the PLA and its kin as "theorems"—not as "principles." He reserved the word "principle" for facts that can be directly intuited, among them the principle of the lever and the principle of virtual displacements.[7]

> [A]fter we have deduced from the expression for the most elementary facts (the principles) the expression for more common and more complex facts (the theorems) and have intuited [*erschaut*] the same elements in all phenomena … [t]he deductive development of science is followed by its *formal* development. Here it is sought to put in an order easy to survey, or a *system*, the facts to be reproduced, such that each can be found and reproduced with the *least intellectual eort.* (1988, pp. 444/516)[8]

The PLA and its kin belonged to the second and third stage of development. Still, the factual physical content of the PLA could always be intuited at an equilibrium of strings. On Mach's account, not only simple facts but also "grand facts" like the PLA could be grasped by intuiting their determining circumstances and the functional dependencies between them.

Mach emphasized that the core of the PLA is the variation of the determining circumstances. It roots in the general principle of continuity that guides scientific research. The feature of minimality present, on the other hand, only stems from the PLA's historical origin. "The important thing, therefore, is not the maximum or minimum, but the removal of *work* from this state; work being the factor

[5] One might wonder why Hans Reichenbach's name is missing here. Admittedly, his views on relativity theory are today more influential in philosophy than Schlick's. But—or so I conjecture—his own axiomatization of relativity theory was guided by the mentioned philosophical views about the relationship between mathematics and physics which made the PLA a *Shibboleth*. In particular, Reichenbach's axioms intend a simple coordination of the mathematical concepts to basic experiences rather than a simple unifying principle. Accordingly, there was not much use for the PLA within Reichenbach's formal approach from the very beginning.

[6] In Section 7 below I shall deal in more detail with this example involving sufficient conditions for the PLA to yield a minimum.

[7] The authorized English translation by McCormack unfortunately uses the word "principle" in all cases.

[8] I normally quote according to German originals. But for readers' convenience I have added references to English translations. All other translations are mine.

determinative of the alteration." (1988, pp. 476/555) Thus Mach concluded that "the principle of *vis viva* … is the real foundation of the theorem of least action." (1988, pp. 409/474) But the dependence of the determining circumstances contains yet an aspect more general than energeticism.

> Notice that the Principle of Least Action … do[es] not express other than that in the instances in question precisely *so much* happens as possibly *can happen* under the conditions, or as is *determined*, viz., *uniquely* determined by them … [T]he principle of *unique determination* has been better and more *perspicuously* elucidated than in my case by J. Petzoldt …: "In the case of all motions, the paths *actually* traversed can be interpreted as *distinguished* instances chosen from an *infinite number of conceivable instances* …"… I am in entire accord with Petzoldt when he says: "The theorems of Euler and Hamilton … are thus nothing more than analytic expressions for the fact of experience that the phenomena of nature are uniquely determined." The "*uniqueness*" of the minimum is decisive. (1988, pp. 404f./470f.)

In the cited article, Petzoldt argued "that the variation of an integral vanishes only for those values [of the actual motion] which have a distinguished position insofar they occur *singularly, uniquely*. The values in the immediate neighborhood of the minimum, maximum or [saddle point]… appear at least pairwise."(Petzoldt, 1890, p. 209f.) Petzoldt even viewed these principles "as *analytical expressions for the principle of sucient reason*."(1890, p. 216) Analogously, Wilhelm Ostwald, the founder of energeticism, regarded his "principle of the distinguished case" as generalization of all minimum principles. "*If there is present an infinite number of possibilities for a process, then what actually happens is distinguished among the possible cases*." (Ostwald, 1893, p. 600) Ostwald admitted the difficulty of specifying in each case the characteristic quantity for which the variation vanishes. Nevertheless, a later paper of Petzoldt's even elevated uniqueness to "the *supreme law of nature*" (Petzoldt, 1895, p. 203), at least in a regulative sense. Interestingly, Petzoldt here revived an argument from Leibniz's *Tentamen Anagonicum* (Leibniz, 1696) that had been devised to circumvent the notorious issue of minimality by emphasizing that there are cases in which "the most simple and the most determined" realize the demands of the principle of sufficient reasons.[9]

While energeticists conceived in their principle a substantialist reduction of all physical quantities to energy, Mach was at pains to insist that unification has an economical advantage, but is ontologically neutral. Although Mach approved Petzoldt's uniqueness argument, he rejected its employment as a condition on possible worlds. There "is no choice between the *actual* happening and *another*."(1919, pp. 393/360)

> [I]t is possible to discover analogies for the Principle of Least Action in the various departments without reaching them through the circuitous course of

[9] Leibniz's essay remained unpublished until 1890, the same year when Petzoldt wrote his first paper; cf. (Stöltzner, 2000).

> mechanics. I look upon mechanics not as the ultimate explanatory foundation of all the other provinces, but rather, owing to its superior formal development, as an admirable prototype of such an explanation. (1988, pp. 406/471)

Neither did Mach subscribe to any form of theory reduction. General properties of systems of mass points, e.g., conservation of the center of mass or of energy, constitute rules by which problems can be treated "by routine forms" (1988, pp. 325/376). Even the more abstract principles, such as the PLA, do not convey any physical understanding. They are "new only in *form* and not in *matter*"(1988, pp. 389/452). Thus for Mach, after Lagrange's *Méchanique analytique*, only mathematical problems remained. And in mathematics, too, there was nothing but economy. Still, in the *Theory of Heat* he wrote: "I long ago characterized mathematics as *economically ordered experience of counting, made ready for immediate use*, the purpose of which is to replace direct counting … by operations previously performed. (1919, pp. 68/70) That Mach's philosophy of mathematics did not provide a basis for assessing the formal virtues of the PLA can also be seen, more specifically, by his judging Euler's well-based precaution in "perfecting" the analogy between variations and differentials as "singularly timid" (1988, pp. 457/532). But precisely this identification had been a principal weakness of energeticism because, as Boltzmann repeatedly stressed, the PLA yielded all equations of motion while energeticists had to add further ad hoc conditions to obtain them, such as *independent* energy conservation for *each* direction in space.

3. Planck—the PLA and the unity of nature

In 1915, Max Planck wrote an entry on the PLA for the encyclopedia *Die Kultur der Gegenwart*. It opened:

> As long as there exists physical science, its highest desirable goal had been the solution of the problem to integrate all natural phenomena observed and still to be observed into a single simple principle which permits us to calculate all past and, in particular, all future processes from the present ones. It is natural that this goal has not been reached to date, nor ever will it be reached entirely. It is quite possible, however, to approach it closer and closer, and the history of theoretical physics demonstrates that in this way a rich number of important successes could already be gained; which clearly indicates that this ideal problem is not merely utopian, but eminently fertile. … Among the more or less general laws which manifest the achievements of physical science in the course of recent centuries, the Principle of Least Action is probably the one which, as regards form and content, may claim to come nearest to that final ideal goal of theoretical research. (1915, p. 68)

Reading these emphatic lines one might safely consider the PLA as the embodiment of Planck's scientific methodology. Planck's philosophical activities started with his 1908 Leyden lecture on "The Unity of the Physical World View."

There, he initially distinguished two mutually enhancing and correcting methods in science. Careful description in the sense of Kirchhoff and Mach, on the one hand, was confined to observations as the only legitimate basis of physics. Theoretical research, on the other hand, boldly generalized particular results and sought a conceptual unity in the manifold of experiences. The development of theoretical physics had been characterized "by the unification of its system which was reached by a certain emancipation from the anthropomorphic elements, in particular from specific sense impressions" (1908, p. 4). He considered Boltzmann's life work to have achieved this emancipation for the Second Law of Thermodynamics, while Mach's epistemology was judged as a relapse into an outdated anthropomorphism. Planck's subsequent vigorous polemic against Mach mainly targeted two points (Mach's anti-realism and the principle of economy) which were both deemed fruitless maxims for scientific research. "By their *fruits* shall ye know them!" (1908, pp. 24/132)—a biblical allusion which more than anything else was to provoke Mach.

In his criticism, Planck distorted Mach's anti-substantialist ontology of neutral monism into sensualism, holding "that there are no other realities than one's own sensations and that all natural science in the last analysis is only an economic adaptation [*Anpassung*] of our thoughts to our sensations by which we are driven by the struggle for existence… The essential and only elements of the world are sensations."(Planck, 1908, pp. 20/129) In *Leading Thoughts,* Mach countered with his famous slogan about the task of science: "*Adaptation of thoughts to facts and adaptation of facts to each other*." (Mach, 1919, pp. 226/133f.)[10] Contrary to Planck's belief, Machian facts were not isolated sensations but are constituted by relatively stable functional dependencies between these elements. "One recognizes the *relations* between condition and *conditioned*, the equations which cover greater or less domains, as the *inherent permanency, substantiality*, as that whose ascertainment makes possible a *stable world picture*." (Mach, 1910, pp. 431/390) While Mach's relational ontology avoided any absolutist commitments, by Planck's lights, an increased constancy of the world picture warranted stronger ontological conclusions. "This constancy which is independent of every human—especially every intellectual—individuality, is that which we now call the real [*das Reale*]." (Planck, 1908, pp. 22/131).

Planck's rejoinder to *Leading Thoughts* confined Mach's principle of economy to the practical sphere only. By Mach's "generalizing it without further ado, the concept of economy … is transformed into a metaphysical one." (Planck, 1910a, pp. 672/142) Moreover, as this notion is in retrospect adaptable to any scientific progress, "the physicist, if he wants to promote science, has to be a realist, not an economist." (1910a, pp. 678/146) Here Planck misunderstood the descriptive-normative nature of the principle. It is, indeed, a biological-economical principle that factually governs the development of science from instinctive experiences onward. At

[10] I am indebted to Veronika Hofer for explaining to me the biological aspects of Mach's thoughts. This has led me to translate *Anpassung* in a biological manner rather than by the physicist's curve "fitting" chosen by Blackmore. Notice that while Mach intended a unified theory of biology and physics, Planck limited himself to physics.

later stages of the evolution of science, however, its application is mainly regulative. "If economy of thought be conceived merely as a teleological and provisional leitmotif, such a conception does not exclude its reduction to deeper foundations, but even demands it." (Mach, 1988, pp. 508/594) Embedding economy of thought into the tradition of teleology and regarding the latter as provisional or regulative only, such that no contradiction with the ideal of causal explanation emerges, appears quite close to the conceptual framework of the *Critique of Judgment* in which Kant had stressed the systemizing function of teleological maxims, such as the *lex parsimoniae*. Given that at the beginning of the rejoinder, Planck had contently noted the assent of transcendental philosophers to his earlier polemic, one wonders how this regulative employment of ideas could have escaped his attention. One reason is that Planck was at pains to avoid any trace of teleology however provisional or heuristic within his cherished PLA.

> Whoever sticks to the principle of causality alone will demand that causes and properties of a motion can be made comprehensible and deducible from earlier states regardless of what will happen later on. This appears not only feasible, but also a direct requirement of the economy of thought. [sic!] Whoever instead seeks for higher connections within the system of natural laws which are most easy to survey, in the interest of the aspired harmony will, from the outset, also admit those means, such as reference to the events at later instances of time, which are not utterly necessary for the complete description of natural processes, but which are easy to handle and can be interpreted intuitively. (1915, pp. 71–72)

In mathematical physics, for instance, one keeps redundant variables in order to maintain the symmetry of the equations. Similarly for the PLA and its kin, "[t]he question of their legitimacy has nothing to do with teleology, but it is merely a practical one." (1915, p. 72) Planck even provided some examples of how these principles could lead one astray if interpreted as instances of a universal teleology. And he rightly emphasized that only after a precise mathematical specification of the Lagrangian and of the conditions for the virtual displacements, the PLA ceases to be "an empty form" (1915, p. 70) and becomes at all meaningful.

In the 1930s, Planck's attitude had shifted significantly towards metaphysics. In his frequently repeated talk "Religion and Natural Science," the PLA was presented as a most comprehensive and strictly valid law—even within the newest physics—"the proper formulation of which in every unbiased observer arouses the impression as if nature be governed by a rational and purposeful [*zweckbewußt*] will … Photons [deflected by the gravitational field of a star] act like rational beings. They select among all possible curves offered to them always that one by which they reach the goal most quickly." (1937, p. 302) The PLA thus introduced the *causa finalis* into physics, but both the teleological and the causal approach represented only different mathematical forms for the same fact. Nonetheless, in a religious perspective it is important that there existed a regularity independent of man "which admits a formulation that corresponds to purposive action. This represents a rational order of the world, to which both nature and man are subjected."(1937, p. 303) Putting these passages alongside Planck's earlier statements on the PLA, one might conclude that "his advocacy of a teleological interpretation of this law is characterized by a certain measure of contradiction," (Yourgrau & Mandelstam, 1968, p. 164) and "that Planck, despite his admonition that all anthropomorphism should be eliminated from exact science, himself succumbed to the very errors he denounced." (1968, p. 180) But one must be careful to show how Planck related theological or moral and scientific matters. "[B]oth ways [of stepwise perfection of knowledge] do not diverge, but run parallel to one another and they meet in the distant infinity of the respective goal." (Planck, 1937, p. 306) Only in this unreachable limit is it possible "to identify the world order of natural science and the god of the religions." (1937, p. 304) In earlier works, Planck (1923) had insisted on a clear separation of the scientific issue of determinism and the ethical problem of the freedom of the will, a position which he emphatically maintained in the view of quantum mechanics (Planck, 1936). It is sufficient in the present context to note that he found logical empiricists on his side (cf. Frank, 1932, Chap. X, 9 and 1936, Sec. iv). Of course, both parties strongly disagreed as to whether the metaphysical questions concerning absolute reality and the freedom of the will were at all meaningful.

Planck's reverence for the PLA had a more specific side, too. "The fundamental importance of the Principle of Least Action became generally recognized only when it proved its applicability to such systems whose mechanism is either completely unknown or it is too complex to think of a reduction to ordinary coordinates." (Planck, 1915, p. 76) In contrast to the differential equations of motion, the PLA as an integral principle is independent of any choice of coordinates and a fortiori invariant under coordinate transformations. As had Boltzmann, Planck emphasized that the PLA is stronger than the principle of energy conservation, but full clarity is obtained only in relativity theory where the PLA "contains all four world coordinates in fully symmetrical order" (Planck, 1910b, p. 38) and is invariant under Lorentz-transformation while energy and momentum are not. Thus, the PLA unites the energeticist view of nature based on energy conservation and the mechanical view of nature based on the conservation of momentum.

In the same year, Planck took a similar stance in another field. He admitted that his law of black-body radiation required a fundamental break with classical electrodynamics in favor of an elementary discontinuity in nature because classical physics unavoidably yielded Jeans's law, in blatant contradiction even to everyday experience.

> In my opinion, one will not for this purpose have to give up the Principle of Least Action, which has so strongly attested its universal significance, but the universal validity of the Hamiltonian differential equations; for those are derived from the Principle of Least Action under the assumption that all physical processes can be reduced to changes occurring continuously in time. Once radiation processes no longer obey the Hamiltonian differential equations, the ground is cut from under Jeans's theory. (Planck, 1910c, p. 239)

Apparently, Planck considered the applicability of the PLA to discontinuous functions as a major virtue. Such functions had indeed become an important source of progress in the genuinely mathematical development of variational calculus since

Weierstraß, but—as will be reflected in Frank's criticism of the PLA below—the mathematical results often did not meet physicists' intuitions.

That universality and invariance were the main tenets of Planck's ideal of physics can also be seen at the other pillar of his world view, the constants of nature, in particular the quantum of action. Planck's and Boltzmann's constants characterizing thermal radiation plus the gravitational constant provided a universal system of units that did not depend on convention. In later years Planck would consider these fundamental constants as an important step towards the ideal aim of absolute knowledge.

Although Planck emphasized the value of mathematical precision regarding the PLA, within his epistemology there was surprisingly little reflection about the role of mathematics in physics, and he was not really consistent. In 1914, he considered mathematics, at least partially, as "an empirical science about intellectual culture" (Planck, 1944, p. 55). While such a formulation appears in accord with an empiricist foundation of mathematics in the style of Mach and Boltzmann, in the following year, Planck insisted on a principal difference between mathematics and physics. Unlike physical theories, mathematical theories could not contradict one another, "such that in mathematics one cannot speak of an opposition of theories, but only of an opposition of methods." (1944, p. 79) Thus the history of mathematics is not driven by the mutual modification of competing theories which are typical for the history of physics. And concerning general relativity, Planck remarked in 1926 "that a theory the complete content of which can be expressed in a single mathematical formula can contradict itself as little as can two different inferences drawn from the same equation." (1944, p. 172) Thus, on Planck's account, consistency was a worthy goal of physics, at least at a mature stage of a theory's formal development. This sounds close to one of the cornerstones of Hilbert's program of the axiomatization of the sciences.

4. Hilbert—the axiomatic method and the PLA

Historians of science have broadly discussed to what extent general relativity emerged within the context of Mach's anti-absolutist epistemology. At any rate, Planck's understanding of general relativity went directly against how a receptive Machian would put it. Relativization of the allegedly absolute Newtonian space and time brought us closer to the ideal aim of genuinely absolute knowledge by recognizing the metric of the four-dimensional space–time manifold as a deeper absolute concept or, in Planck's (somewhat misleading) words by "moving the absolute more backward … [by] welding space and time together by means of the velocity of light into a uniform continuum." (Planck, 1925, p. 154)

In the same year Planck completed his encyclopedia entry on the PLA, David Hilbert gave an independent derivation of the field equations of general relativity by means of a single action principle. Corry, Renn, and Stachel (1997) have recently uncovered that the galley proofs of Hilbert's *Die Grundlagen der Physik* (1916) did not contain the explicit form of the field equations. The printed version, however, appeared only after Einstein's (1916a) seminal paper that used a more pedestrian but physically more transparent derivation. For PLA advocates the question of priority might seem crucial because it could demonstrate the principle's superior heuristic power. However, such an understanding would be rather short-sighted because, in effect, it would severely limit the import of Hilbert's ambitious program of the axiomatization of physics to a matter of finishing order. As I shall argue, the PLA represents a core instance of this program which laid the foundations of modern mathematical physics.

Moreover, both "Hilbert and Einstein saw their achievements of November 1915 as the culmination of year-long efforts of scientific research along their respective research programs" (Sauer, 1999, p. 566). These were by no means identical and followed different heuristics. David Rowe has even diagnosed a basic difference in their physical agendas: "It was microphysics and not gravitation that Hilbert saw as the central problem area. His eyes had been set all along on the possibility of linking Einstein's theory of gravitation with Mie's theory of matter by exploiting the formalisms of invariant theory and variational methods" (Rowe, 2001, p. 404). As Rowe's paper shows in detail, Einstein pursued a zigzag course concerning the PLA. Originally he did not dissent from the Machian outlook, according to which the PLA was a mere mathematical reformulation of the respective differential equations. He used it where appropriate, in particular when discussing Planck's reformulation of the special theory.[11] But already the *Entwurf* theory published in the following year Einstein & Grossmann (1913) and the subsequent paper on "The formal basis of general relativity" (Einstein, 1914) put the PLA in a much more prominent position, perhaps thus contributing to Hilbert's interest in the topic. But the approach involved "some messy considerations about the use of certain variational principles" (Rowe, 2001, p. 394) which had to yield to the detailed criticism of Tullio Levi-Civita. Yet in Einstein's definitive theory (1916a), the PLA appeared only as a calculatory device and without being generally covariant. Most interestingly, there exists a manuscript presumably intended as an appendix to this paper which contains an argument close to Hilbert's derivation (Einstein, 1996). It found entrance only into a later paper (Einstein, 1916b) after the derivation had become standard. Rowe concludes that "in 1916 Einstein had rather less interest in such formal niceties: what he wanted most of all was a theory that would enable him to attack the physical phenomena he had long ago predicted on the basis of his early attempts to extend the principle of relativity to non-inertial frames" (2001, p. 410).

Einstein's zigzag path clearly shows that the PLA never became a core element of his methodology. This certainly had some influence on the attitudes of Moritz Schlick, who would become the leading philosopher of relativity and a close friend of Einstein, and of Philipp Frank, who by 1916 was already an active contributor in relativistic physics. But a similar attitude seems to have been widespread within the physics community of the day, and is was mainly due to Felix Klein's insistence that Hilbert's paper made it into Wolfgang Pauli's (1921) report.[12]

[11] "Note added in proof" to (Einstein, 1912). I owe thanks to Tilman Sauer for this hint.

[12] See the footnote to the letter of 8 March 1921 (Pauli, 1979, p. 27).

In Hilbert's case, the PLA was part and parcel of the axiomatic method. Before 1915, he had built his axiomatizations of mechanics and continuum theory (cf. Corry, 1997) upon this simple unifying principle in the vein which Planck demanded. But the *Grundlagen* eventually prompted Klein's pithy remark to Pauli about Hilbert's "fanatical belief in variational principles, the opinion that the essence of nature could be explained by mere mathematical reflection" (Pauli, 1979, p. 31). One should, however, not extend Klein's negative judgment about the PLA to Hilbert's axiomatic method *tout court*. First, to a writer so precise as Klein, the hastily written *Grundlagen* (1916) suggested appallingly messy mathematics. Second, there is an important philosophical distinction concerning the "essence of nature." While the physicist Planck considered the steps towards a unified world-view as ontological reductions, the mathematician Hilbert was after unifications in a methodological sense that were based on the central role of mathematics among all sciences. This permitted Hilbert to simultaneously apply his axiomatic method to phenomenological or even preliminary theories and to succumb to exalted hopes in a final unified field theory at other times, in particular in his work on relativity theory. Either way, the PLA could serve as a guide. But only the first strategy was viable to logical empiricists, and they failed to embrace this important distinction because of their philosophy of mathematics. I shall develop this argument starting from a closer look at the *Grundlagen* and then outline Hilbert's axiomatization program in general and its relation to the calculus of variations.

In the first published version Hilbert posited two axioms. (I) Mie's axiom of the world function H demands that the variation of $\int H\sqrt{g}\,d\omega$ vanishes for each gravitational potential $g_{\mu\nu}$ and each electromagnetic potential q_s, where g is the determinant of $g_{\mu\nu}$ and $d\omega = d\omega_1\, d\omega_2\, d\omega_3\, d\omega_4$ is the differential of the world parameters ω_k uniquely fixing the world points. H contains gravitational arguments, the $g_{\mu\nu}$ and their first and second partial derivatives with respect to the ω_k, and electromagnetic arguments, the q_s and their first partial derivatives with respect to the ω_k. Axiom (II) states that H is invariant with respect to an arbitrary transformation of the world parameters ω_k. Hilbert considered this axiom as "the simplest mathematical expression of the requirement that the coordinates in themselves do not possess any physical significance, but only represent an enumeration of the world points" (Hilbert, 1924, p. 50). In a footnote Hilbert connected this axiom to Einstein's idea of general invariance (today: "covariance") and remarked that "Hamilton's principle, however, only plays a minor part in Einstein whose functions H are not at all general invariants" (1916, p. 396, 1924, p. 50). As shown above, general invariance also represented a main virtue of the PLA for Planck that was on a par with the (absolute) fundamental constants and other universal characteristics, such as the black-body spectrum.

Hilbert next formulated a theorem that he called the "leitmotif for the construction of his theory" (1916, p. 396), but he does not provide a proof. In the 1924 reprint of the *Grundlagen*, a weakened version was proven as theorem II.[13]

[13] On the import and history of these early "folk theorems," see (Rowe, 2001). There are important unmarked differences between both printed versions of Hilbert's paper, a fact which Rowe judges "a

A footnote acknowledged that Emmy Noether had provided the proof of a more general result which today is called Noether's second theorem.[14] In 1924, he tacitly skipped an unwarranted claim which had presumably been the physical motivation for calling the theorem a "leitmotif" and for initially contemplating the title *Die Grundgleichungen der Physik* (as it appears in the galley proofs), to wit, that "*the electrodynamic phenomena are an eect of gravitation*" (1916, p. 397).[15] Accordingly, the introduction of 1924 was rather careful as to what extent the field-theoretic ideal of unity would prove to be a definitive one in the future development of physics.

Although sufficient for a derivation of Noether's theorem and the Bianchi identities, axioms (I) and (II) did not fix H uniquely, so Hilbert introduced two further axioms of a more physical kind. Axiom (III) demanded the additivity of pure gravity and electromagnetism $H = R + L$, with R being the usual Riemann scalar curvature and L not containing second derivatives of the $g_{\mu\nu}$. It guarantees that no higher than second order derivatives of the $g_{\mu\nu}$ appear in the field equations, such that one obtains a reasonable dynamics. Axiom (IV) "further elucidates the connection of the theory with experience" (1924, p. 57) by specifying the signature of the metric in order to obtain the required $3+1$ pseudo-geometry for space–time.

In addition, there are two supplementary conditions concerning causality and regularity. A restriction on g guarantees that the time coordinate respects causal order, i.e., that cause and effect are not transformed to equal times. This condition, however, is in general not compatible with the relations between pure gravity and Mie's electrodynamics. Hilbert tried a way out by postulating a meaning criterion: "From knowing the state variables at present, all future statements about them follow necessarily and uniquely, *provided they are physically meaningful*." (1924, p. 64) He believed that

> only a sharper comprehension of the idea basic to the principle of general relativity is needed in order to maintain the principle of causality also within the new physics. That is, in accordance with the essence of the new principle of relativity we have to require not only the invariance of the general laws of physics, but also attribute an invariant character to each individual statement in physics, if it is to be physically meaningful—consonant with that, after all, every physical fact must be established by light clocks, i.e., by instruments of an *invariant* character. (Hilbert, 1917, p. 61; 1924, p. 63)

In a footnote of (1924), Hilbert discussed a simple example of an invariant electromagnetic Lagrangian fulfilling this causality condition from which he concluded that the condition typically corresponded to a restriction on the initial conditions. Since Gödel's rotating universe of 1949, however, we know that even for

(*footnote continued*)
blatant abuse of power" (p. 418) on Hilbert's part in his capacity as chief editor of the *Mathematische Annalen*. Thus, I always give the page numbers of both versions in cases where they coincide.

[14] This should not be confused with Noether's first theorem that relates one-parameter group symmetries and conserved quantities, which today plays the more prominent role in mathematical physics.

[15] For more details on this point, see (Sauer, 1999) who provides a detailed analysis of the theorem. For Hilbert's use of Mie's theory, see (Corry, 1999a).

pure gravity, there exist solutions which globally violate causal chronology. Even today no simple condition is in sight that would correspond to Hilbert's intuition.

The boom of research into singularities since the 1960s has also shown that Hilbert's insistence on the regularity of all physical solutions[16] was too restrictive. Nevertheless, "solutions with non-regular points are an important mathematical means for approximating characteristic regular solutions" (Hilbert, 1916, p. 70, 1924, p. 73). Hilbert's belief in the regularity of physical solutions also reflected a characteristic trait of variational calculus which is the core of the nineteenth problem, to wit, that solutions of a variational problem are typically nicer[17] than had been initially required from the candidates. In the twentieth problem Hilbert was very optimistic about the whole field: "Has not every regular variation problem a solution, provided certain assumptions regarding the given boundary conditions are satisfied..., and provided also if need be that the notion of a solution shall be suitably extended?" (1900, p. 289/470). Thiele (1997) rightly stresses that for each definition of "solution" one solves, in effect, a different problem. Hilbert was strongly convinced that such existence proofs for the variational problem could be achieved more easily than for the related (differential) boundary value problem. Rowe (2001, p. 415) surmises that this was also a principal motive in how Hilbert approached general relativity.

In variational calculus there is thus no *ignorabimus*. In virtue of their key role in science, variational problems involve yet another philosophical principle above and beyond Hilbert's deep-seated mathematical optimism. In both the "Problems" (1900, pp. 257/440) and his Königsberg address (1930, p. 960), Hilbert professed faith in a non-Leibnizian pre-established harmony between mathematics and experience. Did Hilbert also endorse with a teleological interpretation of the PLA? For Leibniz, "[t]here is evidently in all things a principle of determination which is derived from a maximum or a minimum." (Leibniz, 1697, p. 487) Moreover, Hilbert's belief (substantially mitigated in 1924) in the ideal of unified field physics which supplanted his earlier mechanical reductionism (cf. Corry, 1999b) suggests that he shared Planck's belief in ontological reductionism.

But viewed in this way, a substantial part of Hilbert's activities in mathematical physics would be incomprehensible, for instance his axiomatic treatment of Kirchhoff's law of radiation that was a (merely descriptive) phenomenological theory in Mach's sense. Recalling an ensuing polemic between Hilbert and the experimentalist Ernst Pringsheim, Max Born—who had been an assistant to Hilbert—concluded that[18]

[16] Earman (1995) stresses that Hilbert's definition of regularity is "defective in failing to capture the distinction between genuine singularities ... and mere coordinate singularities" (1995, p. 6), such as the horizon of a Schwarzschild black hole. Earman's book also provides most competent information about causality-violating spacetimes.

[17] Variational problems enjoying this property are also called "regular," but in a sense different from being just non-singular. See (Gray, 2000, pp. 117–133) for a history of Hilbert's three problems on variational calculus.

[18] For the wider context and the subsequent changes of Born's perspective, see (Schirrmacher, 2001).

> …in *Hilbert's* terms the axiomatic treatment of a discipline does not signify the final assertion of certain axioms as eternal truths, but the methodological requirement: state your assumptions at the beginning of your considerations, stick to them and investigate whether these assumptions are not partially superfluous or even mutually inconsistent. (Born, 1922, p. 90f.)

According to Ulrich Majer, Hilbert's attitude in gas theory was similar. "From an axiomatic point of view … the macroscopic-phenomenological approach is as suited for an axiomatic investigation as the microscopic-molecular one" (2001, p. 20). Here is a passage from the introduction to Hilbert's second lecture course on the mechanics of continua in the winter of 1906/7, the class which Frank attended in the semester before.

> As goal of *mathematical physics* we can perhaps describe, to treat also all not purely mechanical phenomena according to the model of point mechanics; hence … on the basis of Hamilton's principle, perhaps after appropriately generalizing it. Physics has … already gained brilliant successes in this direction …
>
> Even if the keen hypotheses, which have been made in the realm of molecular physics, sometimes certainly come close to the truth because the predictions are often confirmed in a surprising manner, one has to characterize the achievements still as small and often as rather insecure, because the hypotheses are in many cases still in need of supplementation and they sometimes fail completely. … Such considerations recommend it as advisable to take *meanwhile* a completely different, yet a *directly opposite* path in the treatment of physics—as it indeed has happened. Namely, one tries from the start to produce as little detailed ideas as possible of the physical process, but fixes instead only its general parameters, which determine its external development; then one can by *axiomatic physical assumptions* determine the form of the Lagrangian function L as function of the parameters and their differential quotients. If the development is given by the minimal principle $\int_{t_1}^{t_2} L\,\mathrm{d}t = \text{Min.}$, then we can infer general properties of the state of motion solely from the assumptions with respect to the form of L, without any closer knowledge of the processes. … The presentation of physics just indicated, … which permits the deduction of essential statements from formal assumptions about L, shall be the core of my lecture. (quoted acc. to Majer, 2001, p. 18)

How could the foundational and the phenomenological perspectives on the axiomatic method in physics be reconciled? The end of Hilbert's programmatic article "Axiomatic Thought" gave a clue.

> Once it has become sufficiently mature for the formation of a theory, anything which can at all be the object of scientific thinking succumbs to the axiomatic method and consequentially to mathematics. By penetrating into deeper levels of axioms … we also gain deeper insight into the essence of scientific thinking and become more and more conscious of the unity of our knowledge. Under the banner of the axiomatic method, mathematics appears to be destined to a leading role in all science. (1918, pp. 415/1115)

Thus, the pre-established harmony was based in the unity of mathematics which Hilbert called "an indivisible whole, an organism whose vitality is conditioned upon the connection of its parts" (1900, pp. 297/478). Concerning the *raison d'être* of this organism, he took a Kantian tack and emphasized that the finitary attitude in metamathematics constitutes the only legitimate heir of the a priori. Of course, Hilbert rejected Kant's categorial a priori presupposition of Euclidean geometry. General relativity "at one stroke yields the geometrical and physical laws by one and the same Hamilton principle … [Thus] geometry and physics are of equal character and rest upon a common basis as one *single* science" (1917, p. 63f., 1924, p. 68) which was empirical. Hilbert was initially even convinced "that a reduction of all physical constants to mathematical constants should be possible." (1916, p. 407) Since he had to drop the claim that electromagnetism is an effect of gravitation, the goal of reducing the constants of matter to purely geometrical invariants of the PLA became illusory as well.

The ideal of geometrization looks more reductionist than it actually was because it did not embrace the full physical content of the theory. In Hilbert's axiomatization of general relativity there were two types of axioms, the purely geometrical ones (I, II), the physical specifications (III, IV), and two supplementary conditions. This distinction between different level of axioms was a general feature throughout Hilbert's axiomatizations. It already appeared in the Sixth Problem where Hilbert listed the further elements of his program.

> [W]e shall try first by a small number of axioms to include as large a class as possible of physical phenomena, and then by adjoining new axioms to arrive gradually at the more special theories … The mathematician will have also to take account not only of those theories coming near to reality, but also, as in geometry, of all logically possible theories.
>
> Further, the mathematician has the duty to test exactly in each instance whether the new axioms are compatible with the previous ones. The physicist, *as his theories develop*, often finds himself forced by the results of his experiments to make new hypotheses, while he depends, with respect to the compatibility of the new hypotheses with the old axioms, solely upon these experiments or upon a certain physical intuition. (1900, p. 272f./454f.)

Apart from the completeness of the axioms, i.e. that they permit the derivation of all laws of the respective field, there is the theory's internal and external consistency. Hilbert emphasized that for the internal consistency of the theory based on Fourier's heat equation—one of Mach's examples for a phenomenological description—"it is necessary to prove that the familiar boundary-value problem of potential theory is always solvable; for only the solution of this boundary-value problem shows that a temperature distribution satisfying the equation of heat conduction is at all possible" (Hilbert, 1918, pp. 410/1111). Thus internal consistency of a physical theory boils down to a purely mathematical problem about the respective axiom system, which Hilbert traced back to the consistency of arithmetic. It is this last step, only, which proved unfeasible after Gödel's Incompleteness Theorems.

Hilbert's examples for external consistency between theories made the concept not fully clear. Kinetic theory is consistent with thermodynamics, and Einsteinian gravity possesses a well-defined Newtonian limit, while quantum theory contradicts Maxwell's equations, such that a new foundation of electrodynamics is called for. Did Hilbert, accordingly, subscribe to physical reductionism or ontological unification which would make the external consistency an internal one? This seems, first, to run against his proposal to modify each axiom in order to check whether it is really independent and whether one could formulate other consistent theories.[19] Hilbert, however, did not mention the conventionalist's contention that even two entirely different theories may describe the same factual domain. Second, such an understanding misses the mathematical nature of the notion of "deepening the foundations" which starts from the analysis of the independence of the axioms. It is an heir of the ancient attempts, e.g. of Archimedes, to prove the fundamental presuppositions of science themselves. These reductions

> …are not in themselves proofs, but basically only make it possible to trace things back to certain deeper propositions, which in turn are now to be regarded as new axioms …. The actual *axioms* of geometry, arithmetic, statics, mechanics, radiation theory, or thermodynamics arose in this way …. The procedure of the axiomatic method, as is expressed here, amounts to *deepening the foundations* of the individual domains of knowledge—a deepening that is necessary for every edifice that one wishes to expand and to build higher while preserving its stability. (1918, pp. 407/1109)

One can distinguish at least eight types of "deepenings" of different strength,[20] the most simple one being just to drop a dependent axiom. Of interest here are the following. (i) Hilbert lauded both Boltzmann and Hertz for having deepened the foundations of Lagrange's mechanics containing arbitrary forces and constraints to either forces without constraints or constraints without forces.[21] The fact that there are two conceptually inequivalent deepenings demonstrates that Hilbert's 'deepenings" did not necessarily aspire at ontological reductions where one would expect unique basic entities. (ii) Hilbert's deepenings could also arrive at a physically non-standard formulation.

> The axioms of classical mechanics can be deepened if, using the axiom of continuity [which is a very deep mathematical concept], one imagines continuous motions to be decomposed into small uniform rectilinear motions caused by discrete impulses and following one another in rapid succession. One then applies

[19] This is, to my mind, the main reason why Hilbert listed Mach in the sixth problem because apart from Mach's historical-critical inquiries that sketch alternative histories, there is hardly anything attractive to Hilbert on the methodological level.

[20] See (Stöltzner, 2002) where the concept of "elimination" appearing in (Hilbert, 1930) is interpreted as a descendant of "deepening the foundations."

[21] In view of Boltzmann's (1905, pp. 269/113) negative judgment, it is quite surprising that Hilbert remained silent about the great difficulties in finding those supplementary axioms which make Hertz's theory at all applicable.

> Bertrand's maximum principle as the essential axiom of mechanics, according to which the motion that actually occurs after each impulse is that which always maximizes the kinetic energy of the system with respect to all motions that are compatible with the law of the conservation of energy. (1918, pp. 409/1111)

(iii) Hilbert's formulation of general relativity amounted to the strongest type of deepening, in particular by the failed attempt to reduce all physical constants to geometrical ones.

To recap, in the concept of "deepening the foundations" we obtain, to my mind, a more precise form of Hilbert's belief in a pre-established harmony between mathematics and the sciences. Typically, the PLA led to such deepenings. But the deepening concept remained nebulous in Hilbert's published writings, and it united both claims of methodological reduction and reductions which deliberately crossed the border between physics and mathematics. It is also important to note the multi-layered structure of Hilbert's deepenings which was most clearly visible in the three groups of axioms and conditions of the "Grundlagen." Moreover, this layering was not necessarily unique as shown in case (i) above. Both features are generic for the PLA which embraces simultaneously the general fact that a domain of the physical be expressed by an integral principle and the specific Lagrangian of the problem which need not be unique. Let me now turn to how logical empiricists positioned themselves within this setting.

5. Schlick and the criterion of simplicity

In Schlick's 1925 textbook entry on the "Philosophy of Nature" one finds a passage that initially rehearsed Planck's praise for the PLA in a slightly conventionalist fashion.

> Physics often finds it convenient … to state the laws of nature in such a way that it assumes the beginning and end of a natural process to be given, and derives from thence the intervening course of the process; thus it treats this course as if it were something dependent on both past and future at once. A law of this form is Hamilton's principle, or "the principle of least action," and it is of great significance for the formal construction of physics that this very principle is capable of the most universal application. In all the advances of physics it has turned out that, in contrast to many another laws, the action principle preserves its validity unshaken; all newly discovered laws of nature, including those of relativity theory, can be regarded as consequences of a principle of least action, which thereby appears to assume the highest rank of formal generality. It is obviously capable of this, because its formulation involves the fewest assumptions about the particular type of reciprocal dependency among natural processes. In regard to these dependencies there is actually a considerable arbitrariness in our choice of views: the one is as legitimate as the other, so long as it does but conform to the idea of a thoroughgoing perfect determinacy of the whole. (1925, pp. 433f./35)

In his 1920 "Reflections of the Causal Principle," Schlick had continued a similar argument:

> This [arbitrariness of description] should be borne in mind, above all, when examining the difference, and the legitimacy, of causal and finalistic or teleological viewpoints; many erroneous questions in this area have arisen from lack of clarity in regard to the simple relationships we have discussed. (1920, pp. 462/298)

This marked a significant departure from Planck's position. Although in his early days Planck abhorred any teleological association of the PLA, he stressed that it stood on a higher level than the differential equations.

"Thoroughgoing determinacy" of the processes by natural law constituted, to Schlick's mind, the essence of the principle of causality. But he did not understand unique determinacy in a Machian sense and rejected Mach's famous pithy phrase "nature happens only *once*" as an argument against the possibility of a recurrence of identical events.[22] For Schlick, this uniformity—at least in an approximative sense—was a precondition for the principle of causality because otherwise there could be individual laws of nature which explicitly depend upon position and time. Such laws, in which space and time attain an absolute meaning, however, could not be empirically distinguished from a completely lawless universe. Thus any natural law must be sufficiently general and homogeneous in space and time.[23] This relative character of space and time received further support from relativity theory.

Schlick distinguished differential micro-laws and integral macro-laws which resulted as their integrals, but without mentioning the PLA.

> Only the latter fall within experience, for the infinitely small is not observable. The differential laws prevailing in nature can therefore be conjectured only from the integral laws, and these inferences are never, strictly speaking, univocal, since one can always account for the observed macro-laws by various hypotheses about the underlying micro-laws. Among the various possibilities we naturally choose that marked by the greatest simplicity. It is the final aim of exact science to reduce all events to the fewest and simplest possible differential laws. (1920, pp. 462/297)

Thus simplicity is a constitutive trait of the principle of causality. The last sentence of the quoted passage could well have been written by Planck. Unifications such as the PLA, accordingly, were not a matter of mere economy. Thus together with his teacher Planck, the early Schlick could be found guilty of "a traditional misapprehension to construe an accord between the postulate of simplicity and 'least' principles in nature" (Yourgrau & Mandelstam, 1968, p. 173). But such an allegation misses the point because neither Schlick nor Planck attributed any

[22] In (Schlick, 1920, pp. 462/299). Schlick also had a low opinion of Mach's role in bringing about relativity theory; cf. (1920, p. 471/313). And in the *Erkenntnislehre,* he largely took Planck's side in the polemics with Mach, cf. (Schlick, 1974, p. 99).

[23] Typically, such a homogeneity can be expressed by the invariance of the law under an appropriate class of coordinate transformations.

fundamental significance to the physical quantity "action." Simplicity was a formal criterion of the laws of nature. Siding more and more with conventionalism, Schlick (1932) emphatically rejected Planck's metaphysical belief that simplicity is an indication of our approaching absolute knowledge about physical phenomena. It always remained possible to choose another formulation, as long as unique determinacy holds true. For—in contrast to Frank's account discussed below—the principle of causality was not empirical, "but rather a general expression *that* all events in nature are subject without exception to natural laws." (1920, pp. 461/295)

Schlick's first theory of causality was modeled after relativity theory; there was almost no mention of statistical mechanics or atomic physics. Because of this reliance on determinism, the new quantum mechanics forced him into a major change of position. Although not driven by the notoriously vague nature of the concept of simplicity, it a fortiori eliminated its privileged status, which had been a major point in favor of the PLA. A decade later, he revoked his earlier attempts to characterize lawlikeness by explicit conditions on natural laws.

> Our mistake hitherto has been a failure to adhere with sufficient exactness to the actual procedure whereby we actually test, in science, whether … a law, a causal sequence, is or is not present.… It is quite generally the case that the meaning of a proposition is always revealed to us only through the manner of its verification. (1931, pp. 149/185)

The one and only criterion for causality was thus the fulfillment of predictions. On this line, of course, no law of nature can ever be finally verified. But, "at bottom a law of nature does not even have the logical status of an "assertion," but represents, rather, a "prescription for the making of assertions"' (Schlick, 1931, pp. 151/188)—adopting Wittgensteinian terms. Quantum mechanics teaches us "that a limit of principle is set to the exactness of prediction by the laws of nature themselves" (1931, pp. 153/191) which puts a limit to the usefulness of this prescription. Schlick's new concept of causality was less restrictive than the former one, and accordingly it was able to encompass all empirically founded explanations in biology. This brought his theory closer to Frank's, still without sharing the aim that the principle of causality be empirical as well.

It is surprising that Schlick, the leading philosopher of relativity theory in the 1920s, never mentioned Hilbert's contributions; even more so because Hilbert had considered his work an implementation of the axiomatization program launched in the *Foundations of Geometry*. Schlick did make prominent reference to them in his theory of implicit definitions, according to which the basic concepts of a consistent formal system are defined just by the fact that they satisfy the axioms.

> David Hilbert undertook the construction of geometry on a foundation whose absolute certainty would not be placed in jeopardy at any point by an appeal to [a Kantian a priori] intuition. (1974, p. 33)
>
> It is therefore all the more important that in *implicit definition* we have found an instrument that enables us to determine concepts completely. … To this end,

> however, we have had to effect a radical separation between concept and intuition, between thought and reality. (1974, p. 38)

According to Majer (2002), this separation was quite far from Hilbert, who took geometry as the simplest and most perfect science. When Schlick wrote that "the construction of a strict deductive science has only the significance of a game with symbols"(Schlick, 1974, p. 37), he was even further from Hilbert's understanding of the axiomatic method as conceptual criticism.

6. Hans Hahn's response to a former teacher

In Hahn's case, the silence about the PLA is most surprising. When Hahn came to Göttingen for the winter semester of 1903/4, he brought with him a new approach to the problem of the Second Variation, that is, about sufficient conditions for a variational problem, developed by his teacher Gustav von Escherich. (cf. W. Frank, 1993) In 1904, Hahn wrote an entry on the recent developments in variational calculus for the *Enzyklopädie der Mathematischen Wissenschaften* coordinated by Felix Klein. His co-author was Planck's former student and assistant Ernst Zermelo (Hahn & Zermelo, 1904). During his Göttingen semester, Hahn also attended the classes of Hilbert and Minkowski,[24] and thus one can safely assume that he was familiar with the Göttingen approach to mathematical physics. In later years, Hahn would become an eminent figure in variational calculus whose publications "often constituted important steps in the development and the simplification of the methods of the calculus of variations." (W. Frank, 1996, p. 1)

In 1933, a small booklet entitled *Logik, Mathematik und Naturerkennen* by Hahn appeared as the second volume of the series *Einheitswissenschaft* (Unified Science) edited by the Vienna Circle. Although Hilbert's name did not appear in the booklet, its title openly alluded to his widely read Königsberg lecture *Naturerkennen und Logik* (Hilbert, 1930). And Hahn directly addressed Hilbert's main topic, the relationship between thought and reality.

> The usual view can then be described like this: from experience we gather certain facts and formulate them as "laws of nature"; but since by thought we apprehend the most general lawlike connections in reality (of a logical and mathematical nature), our mastery over nature on the basis of facts we have gathered by observation extends much further than our actual observations; for we also know that everything that can be inferred from what we have observed by applying logic and mathematics must be real. ... This view seems to find a powerful support in the numerous discoveries made in a theoretical manner... .
>
> But we are nevertheless of the opinion that this view is completely untenable. For upon closer reflection it appears that the role of thought is incomparably

[24] In a Curriculum Vitae written for his habilitation (*Personalakt* at the Archive of the University of Vienna), Hahn listed the lectures of Hilbert and Minkowski and seminars of Hilbert, Klein, and Minkowski.

> more modest than the role ascribed to it on this view. ... Why should what is compelling to our thought also be compelling to the course of the world? Our only recourse would be to believe in a miraculous pre-established harmony between the course of our thought and the course of the world, an idea which is deeply mystical and ultimately theological. (Hahn, 1933, pp. 64–66/27–28.)

This was the main charge against Hilbert: pre-established harmony however mitigated by mathematics amounts to mysticism. To be sure, Hilbert had emphasized that our knowledge of natural laws is of empirical origin, but this could have been easily assented to by a Kantian. Hahn required a firmer stand.

> There seems to be no other way out of this situation than a return to a pure *empiricist* position, a return to the view that observation is the only source of our knowledge of facts: there is no factual knowledge a priori, *no ''material''* a priori. Only we must avoid the mistake of earlier empiricists who would see nothing but empirical facts in the propositions of logic and mathematics; we must look around for a different view of logic and mathematics. (1933, pp. 66/28)

To Hahn's mind, the only way to reconcile a consistent empiricism in Mach's tradition with modern mathematics was to deny any reality at all to the concepts of logic and mathematics, and regard them—as did Wittgenstein's *Tractatus*—as mere conventions about the use of the symbols of a formal language. Mach (1988, Chap. I,1) had criticized Archimedes' derivation of the law of the lever by means of Euclidean geometry because it implicitly presupposed factual knowledge that could only be attained by previous experiences. Such illegitimate border crossings had to be banned from the application of the new axiomatic methods in the sciences. And, on the logical empiricist's account, Hilbert's concept of ''deepening,'' the heir of Archimedes' faulty proof, did not fully sever the bond with the antique prototype. Similar to Schlick, Hahn believed that after a rigorous separation between tautologous mathematics and empirical facts the axiomatization of the sciences could fully thrive because all theorems become tautological implications of freely chosen assumptions.

> Some chapters of physics have already been axiomatized in the same sense as geometry and turned thereby into special chapters of the theory of relations. Yet they remain chapters of physics and hence of an empirical and factual science because the basic concepts that occur in them are constituted out of the given.
>
> In doing this we may have the following goal in mind: to set up an axiomatic system by which the whole of physics is logicized and incorporated into the theory of relations. If we do this, it may well turn out that, as the axiomatic systems become more comprehensive, as they encompass more of the whole field of physics, their basic concepts become increasingly remote from reality and are connected with the given by increasingly longer, increasingly more complicated constitutive chains. All we can do is state this fact, as a peculiarity of the given; but there is no bridge that leads from here to the assertion that behind the sensible world there lies a second, ''real'' world enjoying an independent being and

differing in kind from the world of our senses, a world which we can never directly perceive. (1930, pp. 44–45/27–28)

Here another anathema of Logical Empiricism rose suddenly within the axiomatic method: metaphysical realism. Thus at bottom, Hahn identified the different philosophical contexts of the PLA in Planck's and Hilbert's thinking. This shows that it was logical empiricists' strict containment strategy against metaphysics which both prevented a due appreciation of Hilbert's axiomatic method and rendered the PLA their *Shibboleth*.

In a Hilbertian perspective, Hahn's worries originated, firstly, from falsely equating the establishment of a single axiom system and its subsequent critical analysis with ontological reduction and unification, as Planck did. But Hilbert had rather insisted that "deepening the foundations" was a methodological reduction to mathematically more basic entities, such as the PLA. This methodological aspect which included phenomenological theories must be clearly distinguished from claims which Hilbert deliberately made at places that we had actually reached a unified theory and some traits of reality in Planck's sense. Admittedly, some "deepenings" of Hilbert crossed the border set by the empiricist criterion of meaning. (cf. Stöltzner, 2002).

Secondly, Hahn's ideas about axiomatization treated all basic concepts on a par within a single network of logical relations. What Hilbert considered as "deepening the foundations," on Hahn's account, was either metaphysical or just an economical convention. This yielded a highly static picture of the axiomatization of science that was oriented at justification rather than providing, as did Hilbert's axiomatic method, a critical instance for theory dynamics. Thirdly, by considering any axiom system exclusively as a system of logical relations plus constitutive definitions of the basic concepts, Hahn made the axiomatic method much more dependent on the success of a foundational program for mathematics than Hilbert ever did. From a Planckian perspective as well, Hahn's identification of any physical excess content of the PLA with metaphysics was not quite persuasive, because Planck regarded fundamental constants of nature—not the PLA—as the best candidate for "absolutely real" entities.

7. Frank—mathematics and antimetaphysics

Apart from Schlick, Philipp Frank was the key person on physical matters within the Vienna Circle. He was one of the first to work on the theory of relativity which would earn him Einstein's former Prague professorship in 1912, but his earlier activities were devoted to the PLA and variational calculus. Most interestingly, Frank's 1906 dissertation and related papers were written mainly from a mathematician's perspective, and they concerned a topic that was—as the author repeatedly stressed—typically absent from the treatises of mechanics including the one by his late teacher Boltzmann: sufficient conditions for a minimum of the action functional or the theory of the second variation. Both were research fields of Hahn and von Escherich. Frank constantly attended their courses,[25] in particular Hahn's maiden lectures on variational calculus in the summer term of 1905. Thus, he was well prepared to broaden his mathematical knowledge in Göttingen during the summer semester of 1906 where he studied with Hilbert, Klein and Zermelo, among others.[26] From the university catalogue,[27] one can infer that Frank went to Hilbert's lecture on continuum mechanics, and thus was quite familiar with Hilbert's use of the axiomatic method in physics including its provisional application to phenomenological theories.

Although after Boltzmann's death the experimentalists Franz Serafin Exner and Viktor von Lang signed the opinion for Frank's thesis, its topic evidently resulted from his interactions with the Göttingen and Vienna mathematicians, above all with Hahn who—so Frank reports in his recollections (Frank, 1961)—would join him and Otto Neurath from 1907–1912 in weekly coffee house discussions on problems of science and philosophy upon which are sometimes bestowed the name "First Vienna Circle." (cf. Uebel, 2000).

In contrast to the publication resulting from it (Frank, 1909), Frank's handwritten dissertation leaves little doubt about the philosophical background of the project. It began:

> In mechanics it is proven that in virtue of the equations of motion of a material point which moves in the plane with energy h and the force function V [today: potential energy] the first variation of the integral $J = \int_a^b \sqrt{h - V}\,\mathrm{d}s$ between two points of the orbit vanishes; i.e. the orbital curves satisfy Lagrange's necessary conditions for J becoming minimal by them. In former times this theorem which was precipitately designated as the Principle of Least Action was directly stated as such: The orbital curves minimize the integral J. Already Jakobi [sic!] observed that the orbital curves only minimize J between points a and b sufficiently close to one another; incidentally he believes that the question of the extent to which the orbital curves yield a factual minimum is "of no importance for mechanics in the narrower sense" (Jakobi, *Vorlesungen über Dynamik*, p. 48) [1866]. This sentence expresses opposition to [*eine Pointe gegen*] the view then not yet generally overcome that precisely the property of the orbital curves to be curves of minimal action was their characteristic property and that in this the wisdom of the Creator of the lex parsimoniae naturae manifest itself. That this tendency prevailed in Jakobi can be seen in a phrase on p. 45 of the same work. After Jakobi has stated the principle in a form more precise than ever done before him, he says:
>
> "It is difficult to find a metaphysical cause for the Principle of Least Action, if it is expressed in this true form, as is necessary. There exist minima of an entirely

[25] See his *Nationale* (a list of the courses a student enrolled and paid for) at the Archive of the University of Vienna.

[26] See his Curriculum Vitae in the *Habilitationsakt* at the Archive of the University of Vienna.

[27] For Hilbert, the catalogue listed a "Seminar on the theory of functions"' (together with Klein and Minkowski) a lecture on "Continuum Mechanics," and an introductory class "Differential and Integral Calculus I" (together with Carathéodory) which was certainly not on the agenda of a visiting scientist. I thank Ulrich Majer for this information.

different type, from which one can also derive the differential equations of the motion, which in this respect are much more appealing.''

One can give the theorem an even more ametaphysical form than Jakobi's by saying: A material point moves according to the Lagrange equations appertaining to the variational problem $J = \int_a^b \sqrt{h - V}\,\mathrm{d}s$. This casts off the last remnant of minimum-romance.

And for the time being, in this way nothing seems to be lost for mechanics. For by this formulation of the theorem one reaches the advantage of higher precision because it also embraces the following two circumstances:

(1) The orbital curves stay orbital curves even where they cease to be minimal curves of J.

(2) As I have proven in connection with a remark of Routh (Dynamik, Vol. II, §455) in the *Mathematische Annalen* [1906b], there exists a curve minimizing J without being an orbital curve...

Thus for the time being it seems indeed the best for mechanics to give up any reference to a maximum or a minimum as precipitately metaphysical. ... And in fact, in the textbooks of mechanics one nowhere finds a proof that the orbital curves yield a minimum though only between sufficiently close points. But this proof can be conducted very easily by the means of modern variational calculus. (Frank, 1906a, pp. 1–3)

The remainder of the thesis contained a physical interpretation of what ''sufficiently close'' means. But let me first explain Frank's result 2) in the example of the ballistic trajectory from A to B (Fig. 1). One can prove that J attains a smaller value for the piecewise continuous curve $AA'B'B$ than for the parabola AB which is the actual orbital curve. The main reason is that the horizontal line $A'B'$ is the limit curve along which the projectile has zero velocity, such that $h = V$ and the integral vanishes. As Hilbert's Twentieth Problem had stated, the important point was to find the appropriate class of solutions for the PLA. This was also pivotal for the

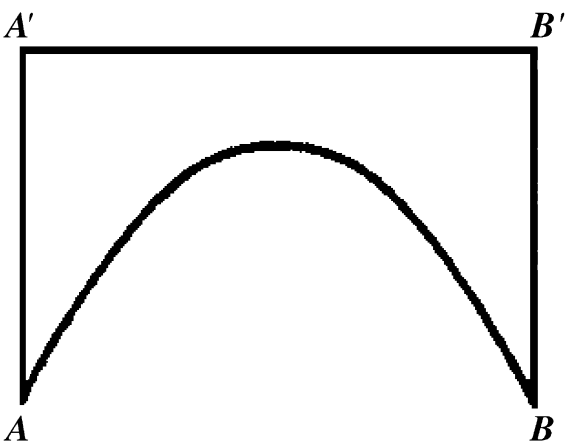

Fig. 1. An absolute minimum which is no orbital curve (from Frank, 1932, pp. 83/91).

axiomatic method in physics because unintended models such as the trajectory $AA'B'B$ have to be excluded by a reasonable physicality condition, as Hilbert attempted to do in 1915. Emphatic advocates of a universal application of the PLA, such as Planck, must assume a priori that such a criterion can always be found.

Frank now searched for a precise physical characterization of the sufficient conditions which is, however, not found in the minimality itself because for sufficiently close points a and b this can always be achieved. Instead he obtained a geometric condition for the orbital curve as a whole which expressed a global property of the set of possible dynamics and physically signified stability against perturbation. If there are Jacobian *conjugate points*, i.e., two points through which all varied curves pass like through foci, within the interval $[a,b]$ then the Lagrange equations yield no absolute minimum of J and the curve is *oscillatory stable,* i.e. small perturbations will repeatedly intersect the orbital curve and oscillate around it. If conjugate points exist only at infinity, J attains its absolute minimum and there is no oscillatory stability. Frank emphasized that this classification could be achieved without integrating the equations of motion, and he showed which force laws permit stable rotations around a central mass.

In Frank's philosophical works, however, the PLA was almost absent. In his seminal book *The Law of Causality and Its Limits* one finds only two short sections in the chapter "Currents of thought hostile to causality." Frank took the PLA as example of "another widely spread manner of treating natural phenomena by analogy to human emotional life" (1932, p. 82/90). As in the dissertation he criticized the German (and a fortiori also the English) translation of Maupertuis' "principe de la moindre action" but without mentioning that Jacobi had also done so because he, as would Mach, conceived the true meaning of the PLA in the least expenditure of work (cf. Jacobi, 1866, pp. 1 and 44). Subsequently Frank repeated the example of the ballistic trajectory from his dissertation (Frank, 1906b) and concluded:

> It is not at all characteristic for the orbit a point-mass follows that along that orbit any magnitude assumes its smallest value. If the orbital curves satisfied another law … there would always be a magnitude that depends on the velocity (or acceleration) and which is smaller for the orbital curves than for any other curve. Just this magnitude would then be regarded as a measure of the action of nature. We should therefore be able to prove why a definite magnitude signifies the action of nature… This would mean a return to pure anthropomorphism, to the animistic world-conception of the pre-scientific age. (1932, pp. 84/91f.)

In contrast to the dissertation, Frank subsequently did not enter into the area of sufficient conditions. Nor did he comment upon the successful applications the PLA had found in other domains of physics since. Instead he turned to a conclusion that even in comparison to Mach minimized the import of the PLA.

> Only a certain mathematical simplification is hidden in the minimal principles of mechanics. With its help the laws of the orbital curves can be expressed in fewer variables … From the one concept "length," the whole law of the formation of straight lines can be deduced [when setting up a PLA for geodetic motion].

> Something similar is the case with all orbital curves of mechanics. Complicated equations are replaced by the somewhat less complicated concept of "action" or "effect." This has, however, nothing to do with economical measures of nature, since such an expression exists for any group of curves, if only they obey differential equations.
>
> For mechanics and physics, this is all actually obvious and will hardly be disputed by anybody. I have discussed it in so much detail only in order to show that in biology matters are in no way different. (1932, pp. 84f./92)

Frank's metaphysical worries were more specific than just the specter of realism and the material a priori which beleaguered Hahn. First of all, by granting independent significance to the PLA, Frank feared opening the door for a return of anthropomorphic design arguments within biology and beyond. Throughout the book he was at pains to show that any teleological argument is either theological by positing a higher intelligence or tautological because it contains nothing that cannot be phrased in causal terms.[28] A major target of his criticism was Driesch's entelechy which erroneously purported to provide an objective measure of life. Through Frank's exclusive focus on the physical quantity of action and his near-to complete silence about those formal and mathematical virtues which stood behind Planck's and Hilbert's high esteem for the PLA, he eventually put the principle on a par with the tautologous notion of entelechy. Second, where Planck saw an association with teleology, Frank just stated that "the difference is in fact not between a specification of the initial state alone or of the initial and final states, but there is always a specification through several points, and the question is only whether the points A and B are close to each other or distant" (1932, pp. 96/101). For instance, any determination of an initial velocity requires two distant points. Such a view, of course, requires a notion of causality at least as wide as Mach's functional dependencies—even one that comes close to tautology, as the author frankly admitted.

But Frank's worries were not limited to biology. In another booklet of the series *Einheitswissenschaft*, Frank pursued a containment strategy similar to his anti-vitalism against the slogan that "the new physics is not mechanistic but mathematical." (Frank, 1935, p. 169/111) His targets were General Smuts and James Jeans who held that the fall of the mechanical world view and the rise of abstract mathematical entities led to a return of spiritual elements within modern science, so that "the universe is now more like a great [organic] idea than a great machine." (1935, pp. 171/112) But, Frank contended, geodesics in space–time and quantum mechanical probabilities are by no means different from Newtonian gravitational forces. Whoever desires to find spiritual analogies will succeed both in classical and in modern physics. Hence, the "assertion that the new physics is not 'mechanical' but 'mathematical' only means that the formulae of relativity and

[28] Most instructive here is Frank's criticism of Ludwig von Bertalanffy's "attempts to formulate vitalism 'positivistically'"—so the title of the respective section IV.19. For details, see (Hofer, 2002).

quantum mechanics *contradict* those of the old mechanics or to put it more precisely, agree with them only for small velocities and large masses.''(1935, pp. 172/113) Moreover,

> [t]he laws of physics consist of mathematical relations between quantities, as well as of directions on how these quantities can be related to actual observations, and in this respect nothing has changed even in the twentieth century. The equations have changed, the quantities are different, and the directions, too, are therefore no longer the same; but the general scheme according to which a physical theory is constructed still has the same fundamental character today as it had in Newton's time. (1935, pp. 198/128f.)

This was, of course, hardly a basis to assess the unifying force attributed to the PLA and Hilbert's program of the axiomatization of the sciences. In this perspective, the only passage of his biography of Einstein (Frank, 1947) that referred to Hilbert is no longer astonishing, though with a little wink as to the motivations of the mathematician's work.

> Hilbert once said: ''Every boy in the streets of our mathematical Göttingen understands more about four-dimensional geometry than Einstein. Yet, despite that, Einstein did the work and not the mathematicians.'' And he once asked a gathering of mathematicians: ''Do you know why Einstein said the most original and profound things about space and time that have been said in our generation? Because he had learnt nothing about all the philosophy and mathematics of time and space.'' (1947, p. 249f.)

Let me conclude with a short guide to placing this paper within the emerging field of the history of philosophy of science. The intense research conducted during the last two decades has taught us that Logical Empiricism was not a homogeneous movement, and that in various members starkly different philosophical backgrounds came to bear. Yet undoubtedly there were important cohesive elements originating from the sciences, such as modern logic and relativity theory. Rather than insisting exclusively on a particular interpretation of these theories, the identification with scientific modernism as a ''world conception'' played its important historical role. This also involved positioning oneself within various traditions in the history of science and philosophy, among them the Principle of Least Action.

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