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CHRISTIAN THIEL

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Beth and Lorenzen on the History of Science

Christian Thiel
Universität Erlangen-Nürnberg

Abstract. Evert Willem Beth (1908–1964) and Paul Lorenzen (1915–1994) are well-known for their contributions to philosophy of mathematics and to formal logic (e.g., semantic tableaux and the semantics of dialogue schemata, respectively). Less known are their "excursions" into the historiography of science, represented by several pertinent papers and a small *Geschiedenis der logica* (1944, ^{2}1948) by Beth, and by Lorenzen's *Die Entstehung der exakten Wissenschaften* (1960). The paper paradigmatically presents Beth's reconstruction of Aristotle's definition of a deductive science, as well as his formulation of "Aristotle's Principle (of the Absolute)" and of "Plato's Principle (of the Idea)". A survey of the contents of Lorenzen's monograph is followed by an outline and discussion of the criticism put forward by three leading historians of mathematics against some of Lorenzen's theses. Beth's and Lorenzen's concerns in their approaches to the history of logic and of science are expounded and scrutinized, and their merits for contemporary and future work in this field are highlighted.

Résumé. Evert Willem Beth (1908–1964) et Paul Lorenzen (1915–1994) sont connus à juste titre pour leurs contributions à la philosophie des mathématiques et à la logique formelle (e.g. , les tableaux sémantiques et la sémantique des schèmes dialogiques respectivement). Leurs « excursions » dans l'historiographie des sciences sont moins connues bien que la bibliographie de Beth comporte plusieurs articles à ce sujet ainsi qu'un petit *Geschiedenis der logica* (1944, ^{2}1948) et celle de Lorenzen le livre *Die Entstehung der exakten Wissenschaften* (1960). Cet article propose la reconstruction de Beth des définitions par Aristote et Platon : celle de la science déductive et de la formulation du « principe Aristotélicien (de l'Absolu) » , ainsi que celle du « principe Platonicien (de

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l'Idée) ». Une vue d'ensemble du contenu de la monographie de Lorenzen est suivie d'un résumé et d'une discussion des critiques apportées aux thèses de Lorenzen par trois historiens des mathématiques influents. Les intérêts de Beth et Lorenzen dans leurs approches de l'histoire de la logique et de la science en général sont exposés et étudiés, et leur mérites pour les travaux contemporains et futurs dans ce domaine sont mis en lumière.

It is certainly no far-fetched idea to talk about Beth and Lorenzen jointly, considering that they have often been dealt with as leading philosophers of mathematics and as contributors to the foundation of intuitonistic logic and mathematics together, e.g. in Mostowski's *Thirty Years of Foundational Studies* [Mostowski 1966, 93]. Their personal contacts were not close, however, and it seems that they did not meet personally before the 1959 Symposium on Foundations in Warsaw. But there is an exchange of letters beginning some time before that meeting, and lasting until at least 1962.[1] The main topic of this correspondence is the connection between Beth's method of semantic tableaux and Lorenzen's method of dialogue games, that "alternative approach to the method of semantic tableaux", as Beth called it [Beth 1964, XXII], while Lorenzen praised Beth's device as "that wonderful method of reading the logical calculuses upside-down, so that the logical rules become susceptible to another interpretation" [Lorenzen 1987, 128].

There is also another interest shared by Beth and Lorenzen, an interest that shows up, in an incidental manner, even in their correspondence. Lorenzen did not only receive from Beth a copy of his *Foundations of Mathematics* (of which he wrote a review to which I will come back later), but also a copy of a much earlier book on the philosophy of mathematics.[2] In his letter to Beth of September 27, 1959, Lorenzen writes:[3]

> Vor einigen Tagen erhielt ich – ganz überraschend – Ihr historisches Buch "De Wijsbegeerte der Wiskunde". Das finde ich aber sehr nett

[1] Cf. the English translation of part of Lorenzen's letter of August 17, 1959, in [Barth 1990, p. 5].

[2] I assume that the book sent was a copy of [Beth 1944a] since its main title, displayed in big letters on the front cover of the book, matches Lorenzen's description of it as "historisches Buch" as much as its content.

[3] The letter is in the Rijksarchief in Noord-Holland, Papers of Evert Willem Beth (1908–1964), philosopher, logician and mathematician, 1920–1964 (c. 1980), inv.nr. 383/397.

Exactly five months later, on February 27, 1960, Beth writes to Lorenzen:[4]

> Haben Sie herzlichsten Dank für die freundliche Zusendung Ihres reizenden Buches über *Die Entstehung der exakten Wissenschaften*, das soeben eintraf. Es kommt mir vor, dass es Ihnen Erfolg und Freude bereiten wird, denn es ist wirklich, wie zu erwarten, verständlich, exakt und wissenschaftlich.

The booklet did indeed receive at least 33 reviews, most of them, however, very short and non-committal ones, among them a note of seven lines by Beth in a collective review of eight publications in the *Algemeen Nederlands Tijdschrift voor Wijsbegeerte en Psychologie* [53, 1960–61, 263–265]. It remained Lorenzen's only monographical contribution to the history of science, comparable in some way to Beth's booklet *Geschiedenis der Logica*, which was first published in 1944 [Beth 1944b] and saw a second, slightly revised edition in 1948. Both shared even the reviewers' reactions: Lorenzen's booklet had less than 170 pages, Beth's less than 100, and in both cases their reviewers found fault with the scanty information squeezed into that space, and also with a certain superficiality which was presumably likewise due to this restraint. But what about the content that both authors managed to pack into the few pages nevertheless, and what about the approaches to their subject ?

I think there is quite a lot to be observed in this respect, and I will select some of these topics. In doing so I disregard the fact that Lorenzen's book deals with the history of the *exact sciences*, whereas Beth's book is about the history of *logic*, with only a few methodological excursions. I will compensate for this by leaving out any historical statements of Lorenzen's made in other publications, and by including some important observations of Beth's made elsewhere. As is well known, Beth's main interest was in the history of (so-called) foundations, which in this case means: in the origins and development of the different *philosophical* backgrounds of new ideas and methods in logic and mathematics. As a result, Beth often discusses matters that are normally the subject of historians of philosophy, thereby broadening his approach in a way that we will have to assess later.

A difficulty for my exposition is that Beth, like Lorenzen, used to publish his ideas in several places at a time and to (re-)publish improved versions of passages from earlier writings, so that we often encounter large textual overlaps. This is the case, e.g., with [Beth 1946-47], [Beth 1950], and [Beth 1952-53]. As most of you will know, nearly everything expounded in

[4] RANH, Evert Willem Beth Papers (see preceding footnote).

these papers was incorporated into Part I of *The Foundations of Science*, which consists of the first three chapters. Chapter 1 even bears the title of [Beth 1953], Chapter 2 that of one of the two paragraphs of [Beth 1947] that were not taken over into [Beth 1953], and chapter 3 contains much of the material of [Beth 1947-48]. In the face of this situation, I will generally refer to the revised edition of the *Foundations*, quoting from the earlier papers only where they contain a noteworthy alternative formulation. I will begin with a selection of topics from Beth, continue with some glimpses into Lorenzen's monograph, and end with a comparison and a conclusion.

Beth was deeply interested in the origin, development and status of logic and methodology in classical antiquity, in themselves and as a prehistory of current knowledge and research; he even contributed a paper entitled "*Deux études de philosophie grecque*" to the 1948 Congress of Philosophy at Amsterdam [Beth 1948]. Quite naturally, Aristotle's theory of science comes into focus, and Beth does not hesitate to condense its essentials into the following definition of a deductive science, or, as Aristotle calls it, an "apodeictic" science ([Beth 1964, 31 f.], but already – presumably first – in [Beth 1943, 46 f.], and repeated in [Beth 1944a], 63 f.):

> A *deductive science* is a system S of sentences, which satisfies the following postulates:

(I) Any sentence belonging to S must refer to a specific domain of real entities;

(II) Any sentence belonging to S must be true;

(III) If certain sentences belong to S, any logical consequence of these sentences must belong to S;

(IV) There are in S a (finite) number of terms, such that

(a) the meaning of these terms is so obvious as to require no further explanation;

(b) any other term occurring in S is definable by means of these terms;

(V) There are in S a (finite) number of sentences, such that

(a) the truth of these sentences is so obvious as to require no further proof;

(b) the truth of any other sentence belonging to S may be established by logical inference starting from these sentences.

In [Beth 1947, 254], postulates (IVa) and (Va) read as follows:

> 4a) these terms are so clear as to require no further explanation; [...]
>
> 5a) these statements are so evident as to require no further proof.

In both versions, postulates IV and V constitute the *evidence postulate*, and the fundamental terms and sentences referred to therein are called the *principles* of the science under consideration. The postulates I, II, and III are called the *reality postulate*, the *truth postulate* and the *deductivity postulate*, respectively.

Aristotle's doctrine "met with a fierce and systematic opposition from the School of Megara" founded by Euclides of Megara, but was later unanimously accepted by philosophers (like Descartes, see the preface of his *Principes de la philosophie*, and Kant, for whom Beth refers us to the *Kritik der reinen Vernunft*, B 4). It became the starting-point and basis of traditional metaphysics and gnoseology, because, on the one hand [Beth 1964, 32 f.],

> Aristotle's theory of science of necessity demands a *metaphysics* as a science of the principles. The specialised sciences turn out to be incapable of giving a foundation for their own specific principles without either lapsing into a vicious circle – in definition or in proof – or an infinite regress, or intruding into the domain of another science by borrowing its principles. Nevertheless, it appears to be desirable to subject these principles, which the particular sciences have to admit without definition or proof, to a close inspection.
>
> This now will be the specific task of metaphysics, which may therefore, claim to provide knowledge of the highest order.

Consequently, Aristotle's metaphysics is nothing else but what is now commonly called *research on foundations* or *foundational research*, and (considering that Aristotle denotes as "philosophy", in a wider sense, any domain of pure science) the designation of metaphysics as the "first philosophy" seems apposite indeed. I will only mention, without comment, Beth's claim that Aristotle, in his

foundational research, deals especially with the principles of physics, and Beth's belief that this accounts for its traditional designation as "metaphysics".

On the other hand, Aristotle's theory of science yields a basis for the theory of knowledge as well, since, "if all scientific knowledge is acquired by means of logical inference starting from a certain number of immediate, irreducible principles" [*ibid.*, 34], the central question will obviously be this: "whence do we, as human beings, obtain these principles, and in what manner may we account for our possession and our use of them?" [*ibid.*].

All this would belong to the history of metaphysics and epistemology rather than to the history of science, were it not for the fact that [Beth 1964, 37]

> mathematics constitutes the classical example – practically the only one which is generally accepted as such – of a deductive science in the sense of Aristotle's theory. The Stagirite himself takes his illustrations mainly from the mathematical sciences.

For this fact Beth has a simple explanation: The foundations of mathematics seemed to Aristotle to have been clarified and consolidated by Eudoxos and Theaetetus, whose work Aristotle knew quite well; here nothing remained to be done, apart from using mathematics as a typical example of a deductive science. In contradistinction, the foundations of physics appeared as highly dubious, and when Aristotle made them the subject of his own work in foundations, he deliberately took the mathematics of Eudoxos and Theaetetus as a model or a standard.

Aristotle's theory of science requires any science to be deductive, to start from obvious principles, and to have an empirical foundation. It was only at the beginning of the *Neuzeit*, i.e. about 1600, that non- and anti-Aristotelian conceptions could successfully compete with the peripatetic paradigm and make it obvious, by referring to scientific practice, that sciences could hardly fulfil these three postulates at once. So that Beth can conclude [Beth 1964, 38]:

> From then on, it became customary to recognise two different types of science, one of which conforms to the postulates of deductivity and evidence, whereas the other answers to the requirement of an empirical foundation.

In this way, rationalism and empiricism were established as antagonistic conceptions, which nevertheless were rooted in the same historical situation and shared many important features, so that – as Beth seems to have been the first to point out – a scholar like Bernard Nieuwentyt (1654–1718) could succeed in

reconciling the main theses of rationalism and empiricism, and modern scientific thought could profit for its expansion from the interaction and the combined influence of both schools. For lack of time I must skip Beth's interesting assessment of Kant's pre-critical philosophy of science and his later attempt at a real unification of rational and empirical sciences, an attempt that aimed at restoring the Aristotelian theory of science, but was doomed to failure from the very beginning (if we may believe Beth).

Let me sketch a more daring, but perhaps even more typical example of Beth's search for new pathways in the history of science. In the 1947 and 1953 papers and in chapter 1 of the *Foundations*, after expounding and interpreting the well-known paradoxes from the Pre-Socratics to Aristotle and beyond, Beth comes forth with two principles underlying most of the basic notions employed in this debate. "A considerable number of arguments in speculative philosophy", he says [Beth 1964, 9], "are based on a certain principle, which is, in most cases, tacitly assumed". As it was used by Aristotle "with remarkable virtuosity", Beth calls it "Aristotle's Principle", or "the Principle of the Absolute", and states it as follows:

> Suppose we have entities u and v, and let u have to v the relation F. Then there is an entity f with the following property: for any entity x distinct from f, x has the relation F to f, but f has not the relation F to x.

A look at Beth's examples will make this clearer. If we take, for F(x,y), the relation "x takes its origin from y", then f will be the αρχη, the principle in the sense of pre-Socratic philosophy. If we take as F(x,y) the phrase "the truth (or the notion) of x presupposes the truth (or the notion) of y", then f will be the principle in the sense of Aristotle's theory of science. If we let F(x,y) be "x is moved by y", f will be the Prime Mover in the sense of Aristotle. If we take F(x,y) to be "x is in a certain state of movement with regard to y", f will be Newton's absolute space. If for F(x,y) we choose "x is desired for the sake of y", f will be the *summum bonum*, etc. etc.

Aristotle's Principle is *not* a logical identity (since some interpretations yield counterexamples, e.g. if as F(x,y) we take "the segment x is larger than the segment y"). Therefore, "its unrestricted application must sooner or later lead to incorrect conclusions" [Beth 1964, 11], but it may be used as a heuristic principle, and by the historian as a guideline for understanding *prima facie* unintelligible speculative notions and arguments. Beth himself has given an example of this by deriving from Aristotle's Principle another one which he calls "Plato's Principle" or the "Principle of the Idea". We obtain it by taking for F(x,y) the phrase "the fact that the entity x has the property A presupposes

the fact that the entity y has the property A". If *a* is the absolute entity corresponding to F, then it is an *absolute entity* corresponding to the property A. It is manifest for Beth that this entity is nothing else but Plato's ειδος or ιδεα. I must admit that many trains of thought and arguments in the tradition of Platonism gain in perspicuity by this stratagem of Beth's, precarious as it appears, but I cannot go into more detail here.

Let me also save some remarks on Beth's treatment of the history of logic for the final evaluative part of this paper, and let me turn now to Lorenzen's way of approaching history, the way of a man who was totally disinclined to invent heuristic devices for helping speculative notions and lines of thought. Lorenzen's monograph *Die Entstehung der exakten Wissenschaften* [Lorenzen 1960] is a short survey of the history of science, published as volume 72 of the semi-popular Springer series *Verständliche Wissenschaft*. In many reviews it was highly praised for its intelligibility and didactical skill, but certain details were severely criticised by leading historians of science (I will come to this in a moment). The reason and motivation for writing this book seems to have been Lorenzen's appointment to *außerplanmäßiger Professor* of Mathematics and the History of Mathematics at the University of Bonn in 1952; the booklet presumably grew out of his first lecture courses on the history of mathematics and the exact sciences. This may also explain the fact that the book begins with two paragraphs containing "preparatory considerations" („Vorbereitende Betrachtungen") on the sense and purpose of the history of science, and on the meaning of the term "exact sciences".

Turning to the historical main part of his book, we find that Lorenzen guides his readers from early oriental science through the exact sciences in classical, pre-Attic, Attic and hellenistic antiquity, and later in the Middle Ages, into the period of the rise of modern science. The vivacity of Lorenzen's style and the alternation between informal expositions, graphic illustrations and short calculations or formal derivations make this an almost entertaining journey through history for anyone not determined to pass out when confronted with a formula. There are many helpful remarks, e.g. concerning the respectability of the methods of approximation in Babylonian astronomy – we learn that our approximations are more precise, but as they would not have improved the observational results of the Babylonian astronomer, he did exactly what a positivistic theory of knowledge would expect him to do. The arithmetic of ps phoi (calculating pebbles) is explained and visualised, and the definitions and methods for calculating the various kinds of "means" in early Greek arithmetic (arithmetical, geometrical and harmonic means) acquire practical significance in a lucid chapter on the Greek theory of music. Zeno's paradoxes are described as giving rise to the "dialectical game of confuting one

another" and preparing the step to a rule-governed technique of formal logic, the development of which from the times of the Sophists through Aristotle, the Stoics and the Megarians well into the Middle Ages is sketched to the extent possible in one small chapter.

Lorenzen does not shy away from well-founded conjectures, e.g. when he declares it impossible that Diophantus and Heron of Alexandria did not have any precursors in arithmetic, and concludes that an "underground" tradition, perhaps from Babylonian times, must have existed. A balanced treatment of ancient and mediaeval mechanics prepares the passage to the era of modern science. Here the reader is warned of the fallacious assumption that a scientist of the 17th century did not yet *know* as much as we do, but was working towards exactly the same *ends* – a totally wrong assumption, as Lorenzen promises to show. And the reader is also told to be cautious with slogans like the "Copernican revolution", since although it is true that the Copernican system has become a decisive part of modern physics, it is equally true that Copernicus himself was not even aiming at something like modern physics.

The few chapters devoted to more recent developments are demanding and perhaps not fully intelligible to a reader without some training in mathematics and the elements of physics. It deserves mention, however, that the importance of Lagrange's *Mécanique analytique* of 1788 as a paradigm of an entirely new type of science is emphasized, and that the book closes with a short look at the open situation today, where signs of a complementation of the classical "synthetic" type of science and the modern "analytic" type of science by a new "calculatory" type of science are already visible – i.e., were visible almost forty years ago.

I mentioned earlier that Lorenzen's booklet received some very detailed criticism from historians of mathematics, which shows that it was at least taken seriously. Oskar Becker in his review in the journal *Gnomon* is not chary of attributes like "profound", "ingenious", "solid" etc., but he lists six points of disagreement with Lorenzen's exposition, ranging from the Babylonian treatment of cubic equations and the alleged geometrization of Babylonian algebra by the Greeks, to the discovery of the incommensurability of side and diagonal of a square, to Lorenzen's claim that the Greeks did not have the method of mathematical induction, and his statement that "Aristotle of course completely lacks the notion of uniformly accelerated motion" [Becker 1961, 116]. But there is no doubt that Becker's review is a very positive one.

Of quite another kind is the review by Joseph Ehrenfried Hofmann in *Natur und Kultur* [Hofmann 1960]. He thinks that Lorenzen "outlines a very subjective picture of the rise of the exact sciences" and does "not fully succeed

in giving a survey of the principal thoughts of the exact sciences without going into detail"; he also regrets that the externally prescribed shortness of the exposition has led to a colourless picture of the multiply interwoven historical connections. In detail, Hofmann points to inaccuracies and plain mistakes as e.g. concerning the relation between algebra and limit processes in Descartes, the ascription of the fundamental theorem of the calculus to Barrow instead of Torricelli, and the passing over of James Gregory. Hofmann calls this "embarrassing" and comes to the conclusion that the book, in spite of its meritorious tendency, "should be used only with caution and not without reference to the literature which is listed quite well in the book".

The third critic is Kurt Reidemeister, who wrote a review for *Die Naturwissenschaften* [Reidemeister 1961]. His attitude towards the book is not quite clear. After drawing attention to the fact that three quarters of the book are devoted to mathematics and the mathematically oriented natural philosophy of the Ancients, he gives a rather extensive survey of its contents. Then, quoting from a famous article of Kurt von Fritz [von Fritz 1959], he complains that the extreme scarcity of our knowledge about the origins of Greek mathematics and the ensuing shifty situation of the modern historian of that time do not become visible in Lorenzen's treatment. Moreover, he says, neither the philosophical relevance of the rise of exact thinking, nor that of the development of mathematical theories, is apparent in the book. Lorenzen's discussion of the incommensurability problem is insufficient, and a reference to the philosophically important controversy between Plato and Aristotle on the paradoxes is missing. On the whole, Reidemeister diagnoses a lamentable shift of weights, partly due to the choice of topics by the author, partly by his treatment of and commentary on the historical development. In short, this review is certainly not very positive either.

It is conspicuous that the main objections mentioned concern details from *ancient* mathematics, and that they were put forward by experts for that epoch. Some of them may simply have to be accepted, but others, if I am not mistaken, are by no means more decisive than most criticism bandied between historians of Greek mathematics themselves. What must be taken seriously is the critique directed against the overall approach, including the choice of topics and the assessment of particular achievements as important or not. It seems to me that similar objections could have been made against many of Beth's historical statements in the *Foundations* and elsewhere, but unfortunately I do not know of any reviews containing criticism in this respect. Even Lorenzen's review of the first edition of *Foundations*, already published in 1959 [Lorenzen 1959], rests content with stating that according to Beth the Aristotelian conception of science as a system of evident axioms with empirical foundation

dominated the historical development until its overthrow by the abandonment of the postulate of empirical foundation (when empirical sciences became independent) and of the postulate of evidence (when non-Euclidean geometries were shown to be logically consistent). Released from the fetters of evidence and empirical foundations, the investigation of arbitrary axiomatic theories became the first and foremost task of mathematics — and Lorenzen, having stated this fact, turns to the weighty remainder of Beth's book.

I realize that it is equally inadequate if I take notice of Beth's *Geschiedenis der Logica* by only a very short glimpse. The book covers the history of logic in four chapters, the first on Greek and hellenistic logic, the second on mediaeval logic, the third on the "newer logic" from the later Renaissance to Kant, and the fourth on modern logic up to the late Thirties (the "most recent investigations" touched upon in the five pages of the last chapter are not investigations in logic but in the history of logic). Albert A. Bennett's review of the 1944 edition ([Bennett 1946] ; cf. also [Freudenthal 1946] and [Martin 1952]) is benevolent in tone, but critical with regard to the subject matter. "The treatment throughout is simple, clear, and within any one part shows a pleasing sense of proportion". But space is too short to do an altogether satisfactory job [Bennett 1946, 96]:

> In a total of less than twenty-seven hundred lines of text the logical views of no less than one hundred thirty-two writers are discussed. The author's remarks, although informative, generous, and individually appropriate enough, are necessarily brief and non-technical. There seems never a chance for the reader fully to appreciate any important problems, still less, to weigh for himself the significance of any single contribution. One can but hope that the non-technical reader will not gain the unflattering impression of innumerable workers all indulging in rather trite observations on trivial topics. The author has succeeded in fitting many names into a single progressive story and in a manner possible only to one intimately familiar with logic in all its aspects. This is perhaps all that was intended.

To be fair to Bennett as well as to Beth, the immediately preceding passage should also be quoted (loc. cit.):

> Formal logic would seem to be a most appropriate field for applying the technique of an abstract deductive science, international in its symbolism and free from accidents of race, date, place, and personal temperament. The author, who is well-known for his many contributions

> to the exposition of formal logic, has here chosen to focus attention upon persons, dates, and movements.

The formulation leaves open the reviewer's opinion on this approach, but to me it sounds less appreciative than critical of Beth's decision. From the more recent perspective of a "contextual" historiography of science, Beth's choice would appear as far-sighted and truly progressive. Where else in shorter histories of logic do we find a reference to the origins of the logic of modalities in the Greek attempts at a logic of change, and its suitability for handling predicates like "obligatory", "permitted", "forbidden", "necessary", "possible" and "impossible" as required in ethics, theology, and law? Where else is this feature connected to the difference of asking for necessary conditions in contrast to sufficient conditions, and to the importance of this difference for the Humanities with their need to describe and to *understand* human action instead of *explaining* them in a manner mimicking explanations in the natural sciences?

For Beth, mentioning this situation seems a matter of course, and the same holds of an aside concerning the importance of the universal language movement for Renaissance and Baroque logic, and the re-emergence of the difference of *ars iudicandi* and *ars inveniendi* in the passage from pre-Schröderian algebra of logic to its post-Schröderian form where the interest shifts from an imitation of calculatory processes proper by formal logic to an adequate reflection of processes of correct thinking in a formal language with sufficient power of differentiation. While admitting that Beth's manner and style of approaching these questions is typical of the Thirties and Forties of our century, his insistence on their importance and the necessity of their inclusion in a comprehensive history of logic seems to me a very modern trait.

What is the upshot of this medley of summaries and reflections? As was to be expected, there are differences between as well as common features to Beth's and Lorenzen's approaches to the history of science. A notable common feature is their quest for grasping the aims and ends of historical developments and achievements. What caused the early emergence of the fundamental ideas of symbolic logic, and what prevented an earlier realization of these fundamental ideas — these are Beth's initial questions in his inquiry into "The Origin and Growth of Symbolic Logic" quoted above [Beth 1947–48]. And he does not begin an exposition of the calculi of modal logic without emphasizing that in classical antiquity "the purpose of modal logic is to provide a means for a deductive treatment of *change*" ([Beth 1950], 255; cf. also [Beth 1964], 18).

Generally, Beth seems concerned about current divergences and controversies between mathematicians, scientists and philosophers, and

between different schools in the philosophy of logic and mathematics. He is convinced that the reasons for these debates can be understood only by tracing their historical roots. He is aware of the fact that this entails a sort of dialectical procedure insofar as the contemporary historian can make use of methods developed only recently, and will compare recent findings with hitherto unexplained phenomena of the past. Although this need not amount to sliding into a methodical circle, I was surprised to find that Beth does not seem to be particularly concerned about the problem and simply refers to successful feats of this kind performed by Bocheński, Scholz, Łukasiewicz and others.

Lorenzen, on the other hand, is obviously concerned about the justification of present scientific practice, and seeks to understand what he considers to be unjustified, unreflected or unintelligible developments or parts of practice today. For him, the study of history may disclose to us wrong tracks, and motivate and support our attempts at correction. His attitude towards the current *Wissenschaftsbetrieb* is mainly critical and sceptical, borne by the conviction that in many cases not everything, but something has gone wrong in the past. So, despite occasional outbursts of enthusiasm about great ideas in logic or mathematics, Lorenzen's attitude towards history is rather guarded, and his message seems to be that we can often learn from history, but mainly about things that have gone wrong. We learn how something should have been, although it did not come to be so. From a historian's point of view, Lorenzen's interest in history, even if it be the history of science, is not "intrinsic".

Sociologically, if I may say so, Beth and Lorenzen were motivated by a rather similar diagnosis of the situation in which they carried on their work as philosophers and mathematicians. Beth was convinced that [Beth 1950, 27]

> recent discussions on the foundations of mathematics and physical science cannot be fully understood without reference to their historical and philosophical background. These discussions for the greater part originate not merely from the results of contemporary scientific research in themselves, but rather from the incompatibility of these results with certain preconceived philosophical doctrines. [...] we must go back [not only to Kant, but] at least to Aristotle if we want to grasp the roots of the doctrinal divergences to which the results of modern research into the foundations of mathematics and physical science have given rise.

Lorenzen, on the other hand, emphatically approves of Collingwood's statement (contained in an extensive quotation at the beginning of *Die Entstehung der exakten Wissenschaften*) that "we study history in order to see more clearly into the situation in which we are called upon to act"

[Collingwood 1951, 114]. The same, Lorenzen says, holds for the history of science, since the consequences of abandoning old scientifc traditions out of pure ignorance may – as in every domain – be disastrous [Lorenzen 1960, 9].

From the writings I have seen, it appears to me that Beth and Lorenzen both saw themselves in a tradition. For Beth this was the tradition of an enlightened scientific philosophy, inspired by Mannoury's significs, by logical intuitionism and by the Vienna Circle (as Beth himself described his intellectual development in a letter to Lorenzen of October 4, 1959). Lorenzen saw himself rather in the tradition of critics of the main trend, like Brouwer, Weyl and perhaps Dingler, although I know from many conversations that he also felt that he was a (critical) member of the Göttingen school as represented by Hilbert and Gentzen.

However, *both* Beth and Lorenzen sometimes exhibit a strong tendency towards unorthodox views and interpretations of data and lines of development in the history of science and philosophy – I am thinking of Beth on some topics in Aristotle, of his view of Kant's philosophy of mathematics, and of Lorenzen on relativity, on the foundations of geometry, or on the importance of Gödel's incompleteness results. In both cases, I would surmise that their commitment to, and their creativity in mathematics, logic and methodology went along with a certain lightness concerning sources and their evaluation in the current historiography of science. This did not inhibit their concentration on particular problems in the history of science which fascinated them and for which they were prepared to go into much detail and to propose solutions of their own.

To summarize, it seems to me a stroke of luck for the history of science that two scholars of such format and standing as Beth and Lorenzen, two scholars who had distinguished themselves by highly original systematic contributions to their field, developed a serious interest in the history of science and philosophy, and devoted so much time to open questions in this problem-laden area, and even ventured to write semi-popular expositions of the history of their fields of research. They have directed our attention to many topics and difficulties and drawn into contemporary discussions many subjects that would very likely have escaped the notice of "pure" historians of science or even have been avoided by them. Not only by sharpening our consciousness for the justification of the historian's activity, but also by unconventional proposals for the solution of open problems in the history of science have Beth and Lorenzen contributed substantially to this field and its progress. We should be grateful for having been able to learn from them.

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