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Bair, J.; Baszczyk, P.; Ely, R.; Henry, V.; Kanovei, V.; Katz, K. U.; Katz, M. G.; Kudryk, T.; Kutateladze, S. S.; McGaffey, T.; Mormann, T.; Schaps, D. M.; Sherry, D. Cauchy, infinitesimals and ghosts of departed quantifiers. (English)

Dealing with dissent

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Published in:
Zentralblatt MATH (Online)

Publication date:
2022

Document Version
Publisher's PDF, also known as Version of record

Citation for published version (APA):
Booss-Bavnbek, B. (2022). Bair, J.; Baszczyk, P.; Ely, R.; Henry, V.; Kanovei, V.; Katz, K. U.; Katz, M. G.; Kudryk, T.; Kutateladze, S. S.; McGaffey, T.; Mormann, T.; Schaps, D. M.; Sherry, D. Cauchy, infinitesimals and ghosts of departed quantifiers. (English): Dealing with dissent. *Zentralblatt MATH (Online)*, *2022*, [070537961].

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Download date: 05. May. 2022

Bair, J.; **Błaszczyk, P.**; **Ely, R.**; **Henry, V.**; **Kanovei, V.**; **Katz, K. U.**; **Katz, M. G.**; **Kudryk, T.**; **Kutateladze, S. S.**; **McGaffey, T.**; **Mormann, T.**; **Schaps, D. M.**; **Sherry, D.**
Cauchy, infinitesimals and ghosts of departed quantifiers. (English) Zbl 07053796
Mat. Stud. 47, No. 2, 115-144 (2017)

Dealing with dissent.

This paper is about different readings of Cauchy's work in analysis, particularly of his concept of convergence, continuity, and derivatives. More precisely, the paper is about a controversy whether Cauchy's thinking about these topics may be considered, as the authors argue for, exclusively as a precursor of (A) non-standard analysis where, roughly speaking, the reals are expanded in field extension by infinitesimals with their own rules, or whether Cauchy's work shows traits of arguing in a way that can be read as precursor of nowadays (B) standard analysis of limits, that is based on quantifiers. To the dismay of the authors, Jesper Lützen, the highly respected historian of mathematics and physical sciences, had argued for the second view. As a service for the reader the authors present a short but insightful analysis of the dichotomy between practice and procedure versus ontology.

That is not an esoteric topic which can be of interest for historians of mathematics alone: In undergraduate teaching, mathematicians must bridge a student's intuitive understanding, based on a vague concept of infinitely small changes (that, admittedly, can be made rigorous), with a, for some students often alienating, rigorous presentation of the abstract mathematical mainstream definitions based on quantifiers. Such bridges are useful, as well, when one works with a physicist who is trained to master even the most elaborate chain rule and partial integration situations – without blinking, in an admirable way, and mostly correct and mostly without quantifiers. The late Richard Kadison, himself a genius in operator theory, a branch of modern analysis, used to tell the following story of a dissent between two geniuses, Marc Kac and Richard Feynman when they both were at Cornell: Kac was lecturing on his new ideas connecting partial differential equations and stochastic processes, a hot topic in statistical mechanics. Feynman was sitting visible with his hight stature in the first row – and apparently increasingly disquiet, until he raised, interrupted Kac, went to the blackboard, took the chalk, and explained with only a few substitutions and cancellations "here this, Marc, and there that" why Kac's claim was obvious and didn't need further elaboration. Then he returned to his seat. Kac smiled and said with his melodic Polish-American accent: "You are right, Dick. But look, I explain the audience how to argue when you are not around."

Since the rigorous foundation of non-standard analysis in the late 1950s, by Detlef Laugwitz and Curt Schmieden, and independently by Abraham Robinson, there is a consensus that approaches (A) and (B) are both valid and that preferences are a matter of taste and situation. This is perhaps best expressed by the inclusion of a separate chapter on nonstandard analysis in the representative Springer monograph "Zahlen" of 1983, also available in English translation. Then what is the controversy of the present paper about?

There is a widespread perception of the community of mathematicians as a brotherhood where conflicts cannot endure due to the shared acceptance of the rules of logic, resulting in the final rejection and extinction of erroneous claims. However, as human artefacts, mathematical publications can arouse adversary feelings. Within the community, we cultivate adversary feelings in the elaborate system of peer reviewing. We distinguish between four types of objections, namely regarding I) meaning, II) clarity, III) correctness, and IV) priority. This distinction goes back to Gauss who is credited – and ironized – for the, logically somewhat inconsistent, constant grumbling: "This is meaningless"; "out of all reason, opaque, incomprehensible"; "utterly erroneous"; and "I did it before".

To me, the gist of the matter is the objection IV, the question of priority. Why so, is a short story that I shall tell further below.

Objection III) doesn't seem relevant here: Lützen's claim, on one side, doesn't call the authors' enthusiasm for nonstandard analysis in question and for their various suggestions for precursors of their ideas. Neither, on the other side, do the authors cast doubt on Lützen's correct quotes. The description of the past by an interpreter who uses the full knowledge of developments of subsequent importance is a typical case of so-called 'present-centred history (PCH)'. Recognizing the descriptive value of PCH, Philip J. Davis and I once raised the question: "But can the interpreter do better than that and really get into the minds of the past creators?" – "Only imperfectly." That was the best answer we found by Eleanor Robson, noted

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for her studies of ancient Babylonian mathematics.

Let's turn now to objections of type I) and II). Each period of the history of mathematics had its own controversies of these types. I shall give three examples.

(1) The irrationality of the length of a unit square's diagonal was easily established in the sense, that $\sqrt{2}$ cannot be written as the quotient of two natural numbers. The lack of commensurability between the lengths of the sides and the diagonal of a square, however, invites to logical quarrels: Kronecker vehemently rejected the concept of irrational numbers as opaque long before Gödel's Incompleteness Theorem. He demonstrated that in number theory deep results can be obtained by working solely with a working language of ideals in polynomial rings and of equivalence classes of recursive formulae to determine a wanted quantity with arbitrary precision, without appealing to an ontological concept of, e.g., algebraic or transcendental irrational numbers. Though it is known that Kronecker acknowledged the beauty and ingenuity of Lindemann's proof of the transcendency of π, "at least if one could assert a meaning to the opaque concept of irrationality".

(2) Similar controversies arouse about the stunning usefulness of calculating with imaginary units in the absence of a definite clarification of the concept of complex numbers. The controversies began with the study of the cubic and quartic equations in the 16th century and culminated with d'Alembert's proof of the Fundamental Theorem of Algebra and Gauss' criticism of it.

(3) More recent are controversies about the prehistory of the theory of distributions, illuminated by Lützen in a widely read monograph. Long before the rigorous introduction of distributions as functionals, physicists demonstrated that, working with 'functions' that are constant zero with an infinity at one point and having finite nonvanishing integral helps finding 'true' solutions of important partial differential equations which otherwise were hard to win. Clearly, lifting that working knowledge to a rigorous concept of distributions was a breakthrough in the theory of systems of linear partial differential equations, e.g., when guaranteeing distributional ('weak') solutions by simple inversion formulae, leaving the hard work to the researcher, to establish whether these weak solutions can be written as hard solutions, i.e., as conventional functions, or proving a intricate result by approximating distributions by sequences of conventional step functions and other piecewise linear functions.

In light of these three examples, we must agree with the authors that one has to distinguish between the working practice and the development of what they call ontological concepts. Rightly they emphasize that aspects of Cauchy's working practice do not injure or devaluate the common perception that elements of modern non-standard analysis are present in Cauchy's writing and thoughts.

Personally, I'm grateful to the authors that in their criticism of Lützen's observations and claims they have clarified the relation – and difference between working practice and carrying ideas.

For a general mathematical reader, however, the emotional atmosphere of the present article is disconcerting. What they discover and emphasize is the difference between working practice and mature concepts. That is an old hat for most mathematicians. Therefore, I stick to an objection of type IV. The priority of their alleged new discovery belongs to 2500 years of history of mathematics and physical sciences.

Reviewer: Bernhelm Booß-Bavnbek (Roskilde)

MSC:

01A45 History of mathematics in the 17th century
01A55 History of mathematics in the 19th century
01A85 Historiography

Cited in **3** Documents

Keywords:

historiography; infinitesimal analysis; Latin model; butterfly model; continuity; convergence; derivatives

Biographic references:

Cauchy, Augustin-Louis

Full Text: DOI

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