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Sergeyev, Yaroslav D. (I-CLBR-IME)
Numerical infinities and infinitesimals: methodology, applications, and repercussions on two Hilbert problems. (English summary)
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On 14 December 2017, a month after the online publication of the paper under review, the editorial board of EMS Surveys in Mathematical Sciences issued the following statement, made available online at the journal's site:

"We deeply regret that the article

> Yaroslav D. Sergeyev, *Numerical infinities and infinitesimals: Methodology, applications, and repercussions on two Hilbert problems*

appears in this issue of the EMS Surveys in Mathematical Sciences.

"It was a serious mistake to accept it for publication. Owing to an unfortunate error, the entire processing of the paper, including the decision to accept it, took place without the editorial board being aware of what was happening. The editorial board unanimously dissociates itself from this decision. It is not representative of the very high level that we expect to see in our journal, which can be assessed from all other papers that we have published.

"Both editors-in-chief have assumed responsibility for these mistakes and resigned from their position. Having said that, we add that this journal would not exist without their dedication and years of hard work, and we wish to register our thanks to them."

The incident was reported at Retraction Watch (RW) on 19 December 2017. RW reports that Pavel Exner, president of the European Mathematical Society (EMS), sent a letter to the author containing a request to retract the paper. The request was denied.

What exactly does the author seek to accomplish by means of his grossone production? In paragraph [0008] of the European patent EP 1 728 149 B1, the proposer (the author) writes: "In this invention we describe a new type of computer—infinity computer—that is able to operate with infinite, infinitesimal, and finite numbers in such a way that it becomes possible to execute the usual arithmetical operations with all of them. For the new computer it is shown how the memory for storage of these numbers is organized and how the new arithmetic logic unit (NALU) executing arithmetical operations with them works."

Here the author proposes to deploy his grossone in ALUs (arithmetic logic units) in custom CPUs. The proposal to implement special grossone instructions (and the related patents) seems analogous to other efforts to make special instructions on CPUs, e.g., those actually implemented for AES (a commonly used encryption scheme).

The proposal to deploy the grossone in ALUs appears to be what the author refers to when he speaks of "numerical computation with infinity" in his articles. Namely, numerical computation (which the author repeatedly contrasts with merely *symbolic* computation) with infinity appears to refer to computation with a computer equipped with a new type of grossone-enriched ALUs, or with the equivalent software implementation of a new numeric data type. Whatever the *technological* merits or otherwise of numerical infinities in this sense, they do not constitute a new *mathematical* theory of the infinite unless solutions to certain algorithmic problems are implemented. Computer implementation of infinity inevitably leads to algorithmic problems that have not been addressed properly by the author. This objection has been presented in detail in

A. E. Gutman and S. S. Kutateladze, Sibirsk. Mat. Zh. **49** (2008), no. 5, 1054–1063; MR2469053.

A. E. Gutman et al., Found. Sci. **22** (2017), no. 3, 539–555; MR3696393.

A calculator manipulating infinitesimals and infinite numbers based on Levi-Civita fields was developed by Ben Crowell and Mustafa Khafateh and is freely available at lightandmatter.com without media fanfare. A theoretical justification can be found, e.g., in the following articles:

K. Shamseddine and M. Berz, in *Computational differentiation (Santa Fe, NM, 1996)*, 37–51, SIAM, Philadelphia, PA, 1996; MR1431040.

K. Shamseddine and M. Berz, in *Advances in p-adic and non-Archimedean analysis*, 215–237, Contemp. Math., 508, Amer. Math. Soc., Providence, RI, 2010; MR2597696.

Popularisation of infinite numbers and infinitesimals is a major part of the author's production, and it is also an interest of the reviewer's. One example the author often gives is that of the limited arithmetic of the Pirahã people. This may be a useful example to motivate extending number systems beyond those currently available. A colleague of the reviewer's has used this example with high school audiences so as to motivate the introduction of distinct levels of infinity.

However, when such popularisation material is presented to an audience of mathematicians in the form of a research article, one expects the popularisation to be accompanied by new mathematical insight. There is little in the author's production that actually contributes to the mathematical implementation of infinitesimals. The explanatory metaphors used in his production are largely derived from the work of V. Benci and M. Di Nasso:

M. Di Nasso, in *Reuniting the antipodes—constructive and nonstandard views of the continuum (Venice, 1999)*, 63–73, Synthese Lib., 306, Kluwer Acad. Publ., Dordrecht, 2001; MR1895383.

V. Benci and M. Di Nasso, Adv. Math. **173** (2003), no. 1, 50–67; MR1954455.

V. Benci and M. Di Nasso, Expo. Math. **21** (2003), no. 4, 355–386; MR2022004.

But in Benci and Di Nasso's fine work, whatever metaphors can be found accompany actual mathematics. Meanwhile, in the author's production, what one finds in abundance are grandstanding claims concerning purported "repercussions on" problems from David Hilbert's famous list, echoing his earlier attempt in this direction: [Ya. D. Sergeyev, Nonlinear Anal. **72** (2010), no. 3-4, 1701–1708; MR2577570].

Next, the author takes aim at the "outdated" mathematical ideal of precision:

"In contrast to the modern mathematical fashion that tries to make all axiomatic systems more and more *precise* (decreasing so degrees of freedom of the studied part of Mathematics), we just define a set of general rules describing how practical computations should be executed leaving as much space as possible for further changes and developments dictated by practice of the introduced mathematical language." (page 229; emphasis added)

Lack of precision with regard to definitions is elevated to a methodological principle:

"**Methodological Postulate 2.** We shall not tell *what are* the mathematical objects we deal with; we just shall construct more powerful tools that will allow us to improve our capacities to observe and to describe properties of mathematical objects." (page 230).

The art of dressing up a bug to look like a feature is not unknown in Calabria. In the context of his Postulate 2, the author comments that

"... [the Sapir-Whorf linguistic relativity thesis] does not accept the idea of the universality of language and postulates that the nature of a particular language influences the thought of its speakers." (page 232)

The kind of relation the author sees between Sapir-Whorf (S-W) and his own Postulate 2 is stated even more clearly in his article [Ya. D. Sergeyev and A. Garro, Informatica (Vilnius) **21** (2010), no. 3, 425–454; MR2742193]. Here the author claims the following:

"… any process itself, considered independently on [sic] the researcher, is not subdivided in iterations, intermediate results, moments of observations, etc. This is a direct consequence of *Postulate 2*, the consequence that is also in line with the *Sapir-Whorf thesis*. …" (pages 443–444 of [Ya. D. Sergeyev and A. Garro, op. cit.]; emphasis added)

However, contrary to the author's claims, S-W tends to undermine rather than support his Postulate 2. Apart from the issue of the applicability of S-W to mathematical languages, S-W warns us about the unreliability of language, and how treacherous language can be in representing "the thing out there". Hence, the lesson of S-W is that we should be careful to explain what we mean, and in particular to try to present precise definitions rather than imprecise ones. The author's tirade against looking for ultimate axiomatic foundations misses the point and is a straw man criticism, since what is at stake is precision in mathematical *procedures*, rather than any ultimate axiomatic account of mathematical *ontology*; see the article [P. Błaszczyk et al., Found. Sci. **22** (2017), no. 4, 763–783; MR3720415] for a more detailed discussion.

According to the author, physicists are apparently better at imprecision than mathematicians:

"… physicists do not give absolute results of their observations. Together with the result of the observation they always supply the *accuracy of the instrument* used for this observation." (page 221)

A. Robinson seems to be faulted for being too precise:

"Even the brilliant efforts of the creator of the non-standard analysis Robinson that were made in the middle of the XXth century have been also directed to a reformulation of the classical analysis (i.e. analysis created two hundred years before Robinson) in terms of infinitesimals and not to the creation of a new kind of analysis that would incorporate new achievements of Physics." (page 221, note 1)

Giving a precise definition of infinitesimals appears to be no more desirable than an absolute definition of a hammer:

"… the methodological postulates we are to introduce will follow Physics and state that an 'absolute' or 'final' definition of a hammer cannot be given." (page 229)

How are we to evaluate the author's provocative statements? Failure to give precise definitions would not impress a freshman calculus exam grader.

In the spirit of imprecision codified in his Postulate 2, the author never gives an explicit definition of the concept of derivative using his grossone. However, it can be deduced from the example of calculating the derivative he gives in his 2011 article [Optim. Lett. **5** (2011), no. 4, 575–585; MR2836038]. It turns out that his calculation of the derivative of a special type of function in 2017 is inconsistent with his implied definition of the derivative in 2011. Namely, Example 1 on page 582 of the 2011 article computes the derivative of $f(y) = y^3$ at the point $y = 5$. Let us denote an "infinite integer" by H to simplify notation. Sergeyev's formula (14) on page 582 then reads as follows:

$$f(5 + H^{-1}) = 125H^0 + 75H^{-1} + 15H^{-2} + 1H^{-3}.$$

Sergeyev goes on to declare that $f'(5) = 75$, which is the coefficient of the term H^{-1}, namely the leading term in the radix-H development of the expression

$$H\left(f(y + H^{-1}) - f(y)\right)$$

at $y = 5$.

Now we examine an example he gives in 2017 on page 283. Here the author sets $f(x) = H^{-1}x^2 + Hx$. He goes on to claim that $f'(x) = 2H^{-1}x + H$ and to evaluate his "derivative" at H, with a claimed value of $f'(H) = 2 + H$. However, these calculations are inconsistent with his 2011 implied definition of the derivative by means of taking the leading term in the radix-H development of $H(f(x + H^{-1}) - f(x))$. Indeed, for $f(x) = H^{-1}x^2 + Hx$ one finds

$$\begin{aligned} &H\left(f(x + H^{-1}) - f(x)\right) = \\ &\quad = H\left(H^{-1}(x + H^{-1})^2 + H(x + H^{-1}) - H^{-1}x^2 - Hx\right) \\ &\quad = H + 2xH^{-1} + H^{-2}. \end{aligned}$$

Evaluating at $x = H$, we obtain $H + 2 + H^{-2}$ with leading term H. The leading term is posited as the derivative in 2011, but it is inconsistent with the author's 2017 claimed value of $2 + H$ for the derivative. The discrepancy results not from a computational error but rather from the author's having failed to digest properly a conceptual point about dealing with infinitesimals and infinite numerals. Or, perhaps, from a simple lack of proper definitions.

Concerning the natural numbers, the author comments as follows:

"... the set of natural numbers will be written as $\mathbb{N} = \{1, 2, 3, \dots\}$ instead of $\mathbb{N} = \{1, 2, 3, \dots, ① - 1, ①\}$. We emphasize that in both cases we deal with the same mathematical object—the set of natural numbers—that is observed through two different instruments. In the first case traditional numeral systems do not allow us to express infinite numbers whereas the numeral system with grossone offers this possibility. Similarly, Pirahã are not able to see finite natural numbers greater than 2 but these numbers (e.g. 3 and 4) belong to $\mathbb{N}$ and are visible if one uses a more powerful numeral system." (page 236)

The author's comment is perhaps not wrong, but applying the more powerful instrument of the Logic of Common Sense (LOCS) would highlight the merit of trying to avoid denoting two distinct entities by the same symbol, in this case $\mathbb{N}$. By the higher accuracy of LOCS, the author's comments are not wrong but just *inaccurate*. The author's inspirational comments about instruments and Pirahãs don't help much, especially since an *element* of $\mathbb{N}$ denoted ① is also claimed to be the *size* of $\mathbb{N}$ by the author, leading to a circularity.

Many mathematicians' reactions to the author's production appear in the informal discussion for question 226277 at mathoverflow.net. The author's threats of legal action were documented at the Russian news outlet lenta.ru on 29 November 2017. On that occasion, the author repeatedly insisted on having published an article in EMS Surveys in Mathematical Sciences as evidence in favor of respectability (the interview took place prior to the statement by the Editorial Board reproduced above).

The article features an occasional pearl, such as the following comment on the Pirahã arithmetic of *one, two, many* (where the operations $2 + 2$ and $2 + 1$ give the same result, i.e., "many"):

"It is worthy of mention that the result 'many' is not wrong. It is just *inaccurate*." (page 226)

Thus, Pirahã arithmetic is not wrong. It is correct but just inaccurate. Similarly,

"... the choice of a mathematical language depends on the practical problem that one intends to solve and on the accuracy required for such a solution. Such results as

$$\aleph_0 + 1 = \aleph_0, \quad \mathfrak{c} + \aleph_0 = \mathfrak{c}, \quad \text{'many'} + 1 = \text{'many'}$$

are correct. *If* one is satisfied with their accuracy, these answers can be successfully used in practice (even 'many' is used by Pirahã nowadays)." (page 293; emphasis added)

Apparently Cantorian formulas are similarly correct, but just inaccurate, in this view.

The author goes on to redefine cardinality as the number of possibilities available with the numerical system enriched with ①. We will refer to this modified notion as ①-cardinality, or *grossone cardinality* for short. In section 7, occupying pages 284 through 294, the author redefines $\aleph_0$ as the ①-cardinality of the natural numbers and $\mathfrak{c}$ as the ①-cardinality of the real numbers. He then claims to resolve the continuum hypothesis (CH) in the negative. This, however, is immediate from his

"**Methodological Postulate 3.** We adopt the principle 'The part is less than the whole' to all numbers (finite, infinite, and infinitesimal) and to all sets and processes (finite and infinite)." (page 233)

Namely, we remark that the set $\mathbb{R} \setminus \{0\}$ is only *part* of the *whole* set $\mathbb{R}$ and therefore has strictly smaller grossone cardinality which is therefore strictly intermediate between the grossone cardinalities $\aleph_0$ and $\mathfrak{c}$. The 10-page section 7 can therefore be replaced by this remark.

The author's title promises "repercussions on two Hilbert problems", namely CH and the Riemann Hypothesis. His section 8 on the zeta function $\zeta(s)$ concludes with the remark that for the partial sums defining $\zeta(s)$, the zeros don't necessarily lie on the critical line, with references to the following three papers:

> M. Balazard and O. Velásquez Castañón, C. R. Math. Acad. Sci. Paris **347** (2009), no. 7-8, 343–346; MR2537227.
>
> P. B. Borwein et al., Experiment. Math. **16** (2007), no. 1, 21–39; MR2312975.
>
> R. S. Spira, Math. Comp. **20** (1966), 542–550; MR0203910.

None of the material in section 8 has any bearing on the location of the zeros in the critical strip except for the three references given, which study the location of the zeros for the partial sums. Thus, the 12-page section 8 can be profitably replaced by this remark. The author claims that:

"The analysis performed in this study shows that the traditional mathematical language using symbols ∞, ω, $\aleph_0$, $\aleph_1$, etc. very often does not possess a sufficiently high accuracy when one deals with problems having their interesting properties at infinity. This lack of accuracy can lead to paradoxes and problems that are considered to be very hard in traditional Mathematics... Numerous theoretical and applied problems considered here show that the ①-based numeral system can help avoid difficulties and paradoxes on several occasions...." (page 313)

In what sense does ① outperform ∞, ω, $\aleph_0$, $\aleph_1$ as the author claims here? As is clear from the discussion above, he repeatedly employs the gimmick of replacing traditional mathematical questions by their ①-versions and answers the ①-questions rather than the original ones. The author claims to develop a "new approach" to mathematics and "a new point of view on infinity" but then purports to provide "repercussions" on questions from traditional mathematical frameworks. Without an account of how his purported system fits in relation to traditional frameworks (an unlikely possibility given the inconsistencies of his system), such a gimmick amounts to moving the goalposts in order to score a point.

In addition to elaborating (trivial) ①-versions of Hilbert's problems, the author develops a ①-version of the concept of *theorem* itself. Thus, ①-Theorem 5.1 on pages 251–252 (copied verbatim from Theorem 1 on page 580 of the 2011 article mentioned above without any acknowledgment of duplication) contains an assertion that, under certain hypotheses, an entity entitled *Infinity Computer* produces certain outputs (namely, a Taylor polynomial). The assumption seems to be that an entity called *Infinity Computer* is a sufficiently well-defined mathematical entity that grossone theorems can be formulated about it.

The reviewer wrote to the author as early as 2011 to point out that Abraham Robinson's work [*Non-standard analysis*, North-Holland, Amsterdam, 1966; MR0205854] needs to be acknowledged properly. Instead, the author consistently couches his production in ambiguities so as to suggest that he has developed a new mathematical theory of interest (rather than providing a—rather poor—popularisation of existing mathematical theories such as the hyperreals or the Levi-Civita fields). We agree with the author's sentiment expressed in ["Independence of the ①-based infinity methodology from non-standard analysis and comments upon logical fallacies in some texts asserting the opposite", preprint, arXiv:1802.01408] to the effect that

"Mathematics is not about gossip columns, thus, an impassioned 'Sergeyev's theory is nothing! All experts agree, and those who do not agree are ignoramuses!' is not the way to go." (page 6)

The author's massive grossone production is not even wrong. It is just *inaccurate*.

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Note: This list reflects references listed in the original paper as accurately as possible with no attempt to correct errors.