Chapter 14
Analysis with or Without Paradoxes?

In this chapter, we shall introduce attempts to base the calculus on a different concept from that of Weierstraß.

Built on Very Thin Ice: Cantor's Diagonal Argument

The Presentation of Evidence

It is quite astonishing to discover which precarious arguments are sometimes accepted as being mathematically sound.

An outstanding example thereof is the (time and time again, by experts and non-experts alike) celebrated "proof for the existence of the uncountable infinity". This bizarre "proof" runs as follows (there should be added some technical details in case the number ends with infinitely many nines, but this is just technical and does not touch the essential reasoning; therefore, it can be omitted):

> *Suppose* we could enumerate the numbers between 0 and 1, written as decimals. If this is done, we arrive at a list
>
> $$\begin{aligned} b_1 &= 0.a_{11}a_{12}a_{13}a_{14}\ldots \\ b_2 &= 0.a_{21}a_{22}a_{23}a_{24}\ldots \\ b_3 &= 0.a_{31}a_{32}a_{33}a_{34}\ldots \\ &\vdots \end{aligned}$$
>
> The a_{kn} are digits from 0 to 9.—Possibly, we have
>
> $$b_{59284} = 0.5000\ldots$$
>
> According to our assumption, the list includes each number between 0 and 1.

D. D. Spalt, *A Brief History of Analysis*,
https://doi.org/10.1007/978-3-031-00650-0_14

The surprising "argument" that should supply the contradiction to our supposition is as follows:

- Take the number $c = 0.c_1c_2c_3c_4\ldots$, defined by
  c_1 = any number, *but not* a_{11};
  c_2 = any number, *but not* a_{22};
  c_3 = any number, *but not* a_{33};
  etc.
- This number c differs obviously from all b_m. (This c deviates in the m-th digit from b_m, as this digit of b_m is $= a_{mm} \neq c_m$.)
- *Consequently,* the list of the b_m does not contain c.
- But this contradicts the initial *assumption* that we *had* a list b_m of *all* numbers between 0 and 1.
- Contradiction. *Presumed* end of proof.

Every modern book on the mathematical infinite presents this "proof", including the book *Proofs from THE BOOK* from Martin Aigner and Günter M. Ziegler from 1998 as well (pp. 92f). And certainly the authors are excited: star writer David Foster Wallace (1962–2008) e.g. called this proof "both ingenious and beautiful—a total confirmation of art's compresence in pure math" (p. 255). An appraisal such as "ingenious" calls for attention, no doubt about that!

The Impotence of This Reasoning

Let us pause for thought and ask: does this actually convince anyone?

Let us work out the details. At once we realise:

The above "proof" is not necessarily valid.

(This is no news. I only repeat a well-known reasoning.)—Therefore, I ask two questions.

(1) *How many decimal numbers with n digits in between* 0 *and* 1 *do exist?*

The answer is straightforward:

1. If $n = 1$, we have exactly 10, to wit: 0.0; 0.1; 0.2; … 0.9.
2. If $n = 2$, we have exactly $100 = 10^2$, to wit: 0.00; 0.01; 0.02; … 0.99.
   etc.

n. Consequently, in the general case n, we have 10^n numbers.

And now my second question:

(2) *How many digits of c are safely determined by this so-called proof?*

The answer to this question is evident: exactly n—one for each digit under consideration. But what is the consequence? Plainly this: the number c defined by this professed proof certainly differs from the first n numbers in the list—but *this does not demonstrate* CONCLUSIVELY that it is not included in the list at all; instead, it *only* proves: $c = b_{m'}$ as well as $m' > n$, nothing else.

To be practical, let $n = 2$. We write down the list of the $10^2 = 100$ numbers in between 0 and 1 with two digits after the decimal point:

$b_1 = 0.00\,,$	$b_2 = 0.01$	$b_{11} = 0.10$	$\ldots\,,$	$b_{100} = 0.99\,.$
	$b_3 = 0.02$	$b_{12} = 0.11$		
	$\ldots\,,$	$\ldots\,,$		

Let us take $c = 0.12$. Then we know $c \neq b_1$ and $c \neq b_2$; *but* we have $c = b_{13}$. c is not to be found among the first $n = 2$ numbers but later on it *is* included in the list.

This reasoning imputes a (MOST) EXTREMELY STRANGE *hidden assumption* to this "both ingenious and beautiful" "proof". It is this one:

$$\mathbf{10^{\infty} = \infty\,.}$$

For in fact it is proclaimed: The number c is *not* contained in the list of the b_m. What is true, however, is just this: The number c is *not among the first infinitely many* ("∞") *numbers of the list,* but only subsequently.

However, who is demanding to calculate with infinity in this way? It is true, e.g. **Weierstraß** authorized this—i.e. $10^{\infty} = \infty$—but at the same time he demanded to exclude infinity ("∞") from analysis (p. 177).

But **Euler** did use infinity in his calculations. He even assumed $i > i - 1$ (p. 77) and moreover

$$\mathbf{10^{i} > i}$$

for infinitely large i.

There is no obligation to think like Euler and also none to think like Weierstraß.

In mathematics we do not deal with *dogmas* but with *arguments.*

From Chap. 5 we know:

There exists no conclusive or ultimate reasoning for dealing with the actual infinite in analysis.

The Origin of the "Diagonal Argument"

The "proof", which is described above, was invented by Georg Cantor. Cantor demonstrated thus in 1891 for the third time:

> Theorem. *There exist infinite sets which cannot be related one-to-one to the set of natural numbers.*

Cantor designed his proof slightly differently. (He took only *two* different digits instead of *ten* as we did.—Strikingly, this proof, given for binary numbers instead of decimal numbers, is not only *not conclusive* but also *wrong*—why exactly? And: is it permissible to make a proof about the *real numbers* dependent on *the mode of their representation?*)

The **True** *Understanding of the "Diagonal Argument"*

It was Bertrand Russell who unearthed the rational essence of Cantor's proof.

> The *true* theorem of Cantor's proof is this:
> Theorem. *The number of subsets of a set is larger than the number of the elements of this set.*

Let us take an example. The given set may contain two elements, a and b:

$$M = \{ a , b \} .$$

Then M has the following $2^2 = 4$ subsets, first of all the empty set $\{\ \} = \emptyset$:

$$\emptyset , \quad \{ a \} , \quad \{ b \} , \quad \{ a , b \} .$$

Hence, there are more subsets than elements: $4 > 2$.

Now we faithfully present Russell's general proof of his theorem (changed, however, are the names of the sets—which Russell calls "classes"). Like Cantor, Russell starts with a *list* of subsets signified by the elements. Russell calls this list a "one-one correlation R".

> When a one-one correlation R is established between all the members of M and some of its sub-classes, it may happen that a given member x is correlated with a sub-class of which it is a member; or, again, it may happen that x is correlated with a sub-class of which it is not a member.

We interrupt Russell and illustrate his set-theoretical construction with the help of our example, and that twice.

1. Illustration. The list of the correspondences of the elements and the subsets of $M = \{a\,, b\}$ might be

$$a \mapsto \{\, a \,\}\,, \qquad b \mapsto \{\, a\,, b \,\}\,.$$

Then we have for *both* elements a and b that they correspond to a subset that *contains them as an element.*

2. Illustration. A second list might be:

$$a \mapsto \emptyset\,, \qquad b \mapsto \{\, b \,\}\,.$$

In this case, only element b corresponds to a subset, which contains itself as an element, but element a does not.

Now we continue with Russell's proof.

> Let us form the whole class, N say, of those members x which are correlated with sub-classes of which they are not a member.

In our first illustration, we have $N = \emptyset$; in the second, it is $N = \{a\}$. We continue with the proof. Now follows the decisive sentence (I give it *in italics*):

> *This is a sub-class of M, and it is not correlated with any member of M.*
>
> For taking first the members of N, each of them is (by the definition of N) correlated with some sub-class of which it is not a member, and is therefore not correlated with N.
>
> Taking next the terms that are not members of N, each of them (by the definition of N) is correlated with some sub-class of which it is a member, and therefore again is not correlated with N.
>
> Thus no member of M is correlated with N.
>
> Since R was *any* one-one correlation of all members with some sub-classes, it follows that there is no correlation of all members with *all* sub-classes.

It is extremely impressive how a few words can express a really entangled reasoning. Those who are able to understand this demonstration from Russell may be pleased with their capability of abstract thought.

If you love formalism, you may write down this proof in the following manner (usually "2^M" denotes the set of all subsets of M; "$M \setminus N$" indicates the difference of the sets M and N):

Let $\quad R : M \longrightarrow 2^M$; therefore $\quad M \ni x \overset{R}{\longmapsto} Rx \in 2^M$.

Let $\quad N = \{\, x \mid x \notin Rx \,\} \subseteqq M$. Then we have for all $x \in M$: $\quad Rx \neq N$.

Proof :

1. Let $x \in N$. Then $\quad x \notin Rx$, and consequently, $\quad N \neq Rx \quad$ (for $N \ni x \notin Rx$) .
2. Let $x \in M \setminus N$. Then we have $\quad x \notin N$; therefore, $\quad x \in Rx$, and again $\quad Rx \neq N$.

(In both cases, an element of one set is shown,

which is missing in the other set.)

1 & 2: $\quad x \in M \Longrightarrow Rx \neq N$, as was claimed.

In this way, Russell established:

> Theorem. 2^n is always larger than n—even if n is infinite.
> This implies that the infinite cardinal numbers do not have a maximum.

The Significance of the "Diagonal Argument"

That is why the so-called Diagonal Argument from Cantor is a *method of proof* in set-theory. It gives the means of proving e.g. that there always exist larger "cardinalities", even in the infinite.

The "numbers" in set-theory are "cardinal" numbers. In analysis they do not feature (at least not in traditional analysis, in calculus). Calculus needs "numbers for calculations".

Therefore, Russell's proof that $2^n > n$ (which holds *in set-theory*) does not contradict Weierstraß' principle that $10^\infty = \infty$. Quite the contrary: if one accepts Cantor's Diagonal Argument as permissible reasoning *in analysis,* you *must* also accept Weierstraß' dictum that $10^\infty = \infty$, for otherwise the Diagonal Argument fails! This is explained above.

> So, if you think analysis (in this sense) and set-theory as one thing, you will be convinced that $10^\infty = \infty$ (in analysis) *and* that $2^n > n$ for all n, including infinite numbers (in set-theory).

Some will like this ... But in some vague sense, this does not look altogether *consistent.* Luckily, one is not *forced* to think in this style.

For the record: on the case of *numbers used for calculating,* i.e. in analysis, you *may* accept the Diagonal Argument. Or you may not. In any case, it is *not conclusive* there. And if you want to relate it to the facts connected to the decimal numbers (*for which it* CLAIMS *validity*), it is even *less conclusive.* For we have seen:

> The Diagonal Argument demands the acceptance of an argument regarding *infinity,* which *is invalid in any finite case.*

As we know, Leibniz once argued in the same way, thereby surprising Johann Bernoulli (p. 55).

It might cross one's mind to interpret this argument as a *proof by induction:*

- Start: choose $c_1 \neq a_{11}$.
- Inductive step: Let $c_n \neq a_{nn}$. Then choose $c_{n+1} \neq a_{n+1,n+1}$.
  —Finished.

An *objection* to this kind of reasoning is obvious: it is *presupposed* that the first part of each number $0.c_1c_2 \ldots c_n$ formed in this way is contained in the list of the b_m: $0.c_1c_2 \ldots c_n = b_{m'}$; of course, it is $m' > n$. So what? If *any* first part of c is contained in the list of the b_m—why should this hold no longer for c itself?

If you do not accept the Diagonal Argument in analysis (to repeat: this is as legitimate as is the opposite), you are accepting the validity of

$$10^i > i$$

for infinite numbers used in calculations. Finally, we shall demonstrate where this kind of thinking will lead to.

Paradox I: Conditionally Convergent Series

Let us finish our detour into set-theory and return to analysis. There are certain curiosities that have arisen during the last three and a half centuries that caught the attention of some analysts.

$$1 - \tfrac{1}{2} + \tfrac{1}{3} - \tfrac{1}{4} + \tfrac{1}{5} - + \ldots = \ln 2\,.$$

Let us now tackle some of these curiosities in more detail, using the title “paradoxes”. The creator of this name will be uncovered later.

In order to prepare for this, we now return to Riemann. We know: Riemann is always one for offering a surprising perspective.

A Mathematical Monstrosity: The Riemann Theorem on Rearrangements of 1854

Riemann proved the following oddity, nowadays called the “Riemann Theorem on Rearrangements”, in his habilitation thesis of 1854:

> Theorem. If a convergent series is no longer convergent when its terms are made positive, the terms can be rearranged in such a way that the series converges on any arbitrarily chosen value.

The proof seems to be straightforward. One assumes a *convergent* series of numbers:

$$\sum_{k=1}^{\infty} a_k = s\,.$$

Infinitely many terms have the sign $+$, and infinitely many have the sign $-$. We call the positive terms of the series b, the negative ones c:

$$\sum_{i=1}^{\infty} b_i = \sum_{\substack{a_k>0 \text{ and} \\ k=1}}^{k=\infty} a_k \qquad \text{as well as} \qquad \sum_{i=1}^{\infty} c_i = \sum_{\substack{a_k<0 \text{ and} \\ k=1}}^{k=\infty} a_k\,.$$

So we have $b_i > 0$ as well as $c_i < 0$. Neither of those series converges:

$$\sum_{i=1}^{\infty} b_i = \sum_{\substack{a_k>0 \text{ and} \\ k=1}}^{k=\infty} a_k = \infty \qquad \text{as well as} \qquad \sum_{i=1}^{\infty} c_i = \sum_{\substack{a_k<0 \text{ and} \\ k=1}}^{k=\infty} a_k = -\infty\,.$$

If *both* series had finite sums, then the series $\sum |a_k|$ would also converge, which contradicts our assumption. If only one of these two series converges, the initial series could not converge. That is why none of our partial series converges.

Now Riemann's reasoning: let D, say $D > 0$, be an arbitrarily given value. Then we approach D gradually—at first from below, then from above; and so on, alternating. That is to say, we choose the smallest number n_1 with

$$\sum_{k=1}^{n_1} b_k > D\,; \quad \text{thereafter, the smallest number } m_1 \text{ with} \quad \sum_{k=1}^{n_1} b_k + \sum_{k=1}^{m_1} c_k < D\,,$$

etc. The difference to D will never be more than the absolute value of the term that is the one before the last sign change. But $\sum a_k$ converges, and therefore, b_k as well as $|c_k|$ are decreasing with increasing k below any given quantity; and so does the difference to D—which seems to prove all.

This theorem is a monster. Weierstraß obviously did not like it, and he indeed mentions the notion of conditional convergence (see p. 184), but he ignores this concept in his own analysis.

Why is this theorem a monster? Because it assumes that a mathematician is able *to decide infinitely many times* AT WILL to change between the partial series of the b_i and c_i back and forth. "An infinite number of arbitrary choices is an impossibility" says Russell. Infinitely many *actual* decisions have nothing in common with reality.

Note: A proof may be easy, but the proven statement may be absurd.

Mitigation

A theory of infinitely many *single* choices in analysis certainly is an extreme. An essential mitigation is to think about a *regular* change of infinitely many terms of a series. We might have more chance of acceptance if we concentrate on *regular* mathematical constructions—although Weierstraß *might* still have taken a different view.

Let us take an **example.** We start with a series A, halve every term, add both series term-by-term, and regain the initial series, although slightly rearranged.

We have:

$$A = \quad \ln 2 = 1 - \tfrac{1}{2} + \tfrac{1}{3} - \tfrac{1}{4} + \tfrac{1}{5} - \tfrac{1}{6} + \tfrac{1}{7} - \tfrac{1}{8} + - \ldots$$

Therefore, $\quad \frac{1}{2}\ \ln 2 = \quad \frac{1}{2} \quad - \frac{1}{4} \quad + \frac{1}{6} \quad - \frac{1}{8} + - \ldots ,$

and consequently $\quad B = \frac{3}{2}\ \ln 2 = 1 \quad + \frac{1}{3} - \frac{1}{2} + \frac{1}{5} \quad + \frac{1}{7} - \frac{1}{4} + + - \ldots$

$= \quad$ [rearrangement of] $\quad \ln 2.$

Do we have $A = B$, i.e. $\ln 2 = \frac{3}{2}\ \ln 2$?

According to Weierstraß, this conclusion is a "fallacy" (p. 177). Why?—Is the Riemann Theorem on Rearrangements an answer?—Is this answer satisfactory?

Paradox II: Methods of Summation

For divergent series like

$$A = 1 - 1 + 1 - 1 + 1 - 1 + - \ldots$$

sometimes other summation methods are in use, e.g. the so-called method of C-1 summation:

$$S^{C1} = \lim_{n \to \infty} \frac{s_1 + s_2 + s_3 + \ldots + s_n}{n} .$$

In case of series A, the partial sums are $s_1 = 1$; $s_2 = 1 - 1$; $s_3 = 1 - 1 + 1$; $s_4 = 1 - 1 + 1 - 1$ etc., that is:

$$s_1 = s_3 = s_5 = \ldots = 1 ,$$

$$s_2 = s_4 = s_6 = \ldots = 0 ,$$

and consequently, the terms of $S^{C\ 1}$ are

$$p_1 = \tfrac{s_1}{1} = 1 ,$$

$$p_2 = \tfrac{s_1+s_2}{2} = \tfrac{1}{2} ,$$

$$p_3 = \tfrac{s_1+s_2+s_3}{3} = \tfrac{2}{3} ,$$

$$p_4 = \tfrac{s_1+s_2+s_3+s_4}{4} = \tfrac{2}{4} = \tfrac{1}{2} ,$$

$$p_5 = \tfrac{s_1+s_2+s_3+s_4+s_5}{5} = \tfrac{3}{5} ,$$

$$\ldots$$

i.e. $\quad p_{2n-1} = \frac{n}{2n-1} ,$

$$p_{2n} = \tfrac{n}{2n} = \tfrac{1}{2} ,$$

all together: $\quad S^{C\ 1} = \lim\limits_{n \to \infty} p_n = \frac{1}{2} .$

With this method, we do get:

$$A = 1 - 1 + 1 - 1 + 1 - 1 + - \ldots = \tfrac{1}{2} \ !?$$

Paradox III: The Convergence of Function Series

Let us take the geometric series

$$s(x) = 1 - x + x^2 - x^3 + x^4 - + \ldots$$

The well-known way to calculate the sum $s(x)$ is to add $s_n(x)$ and $x \cdot s_n(x)$:

$$\begin{array}{rl} s_n(x) = & 1 - x + x^2 - x^3 + x^4 - x^5 + - \ldots + (-1)^{n-1}x^{n-1} \\ x \cdot s_n(x) = & \quad\; x - x^2 + x^3 - x^4 + x^5 - + \ldots + (-1)^{n-2}x^{n-1} + (-1)^{n-1}x^n \\ \hline (1+x) \cdot s_n(x) = & 1 + (-1)^{n-1}x^n \end{array}$$

and $\boxed{s_n(x) = \frac{1+(-1)^{n-1}x^n}{1+x} \qquad \text{if} \quad x \neq -1.}$

The consequence is

If $|x| < 1$ we have $s(x) = \lim\limits_{n\to\infty} s_n(x) = \frac{1}{1+x}$,

if $x = 1$ we have $s(1) = \frac{1}{2}(1 - 1 + 1 - 1 + - \ldots)$, which does not exist.

Yet, “does not exist” is not a beautiful result of a calculation.

Paradox IV: The Term-by-Term Integration of Series

Let us take the series of functions

$$f_n(x) = \frac{nx}{1 + n^2x^4} \qquad \text{with} \quad 0 \leqq x \leqq 1\,.$$

As $\lim\limits_{n\to\infty} \frac{nx}{1+n^2x^4} = 0$ if $0 \leqq x \leqq 1$, we usually conclude for the limit $n \to \infty$:

$$f(x) = \lim_{n\to\infty} f_n(x) = \lim_{n\to\infty} \frac{nx}{1 + n^2x^4} = 0\,. \qquad (‡‡)$$

But if $x_n = \frac{1}{\sqrt{n}}$ (and consequently $\lim\limits_{n\to\infty} x_n = \lim\limits_{n\to\infty} \frac{1}{\sqrt{n}} = 0$), the value of $f(x)$ at the value $0 = \lim\limits_{n\to\infty} \frac{1}{\sqrt{n}}$ is

$$\lim_{n\to\infty} f_n\left(\frac{1}{\sqrt{n}}\right) = \lim_{n\to\infty} \frac{\sqrt{n}}{1+\frac{n^2}{n^2}} = \lim_{n\to\infty} \frac{\sqrt{n}}{2} = \infty\ !$$

We remember: in these cases Cauchy concludes ∞ to be a value of the function $f(x)$ at the value $x = 0$ (p. 124). However, present-day analysis handles things differently and demands:

$$f(0) = \lim_{n\to\infty} f_n(0) = \lim_{n\to\infty} \frac{n\cdot 0}{1+n^2\cdot 0} = \frac{0}{1} = 0\,,$$

unambiguous and crystal-clear.

If we have, following ‡‡, $f(x) = 0$ for all $0 \leqq x \leqq 1$, integration is easy:

$$\int_0^1 f(x)\,dx = \int_0^1 0\cdot\, dx = 0\,.$$

But let us integrate term-by-term:

$$\int_0^1 f_n(x)\,dx = \int_0^1 \frac{nx}{1+n^2x^4}\,dx\ .$$

Substituting $z = nx^2$ gives $dz = 2nx\,dx$ and we arrive at

$$= \tfrac{1}{2}\cdot \int_0^n \frac{dz}{1+z^2} = \tfrac{1}{2}\cdot \arctan z\,\Big|_{z=0}^{z=n} = \tfrac{1}{2}\cdot \arctan n\,.$$

With the help of $f(x) = \lim f_n(x)$, we conclude

$$\int_0^1 f(x)\,dx = \lim_{n\to\infty} \int_0^1 f_n(x)\,dx = \lim_{n\to\infty} \tfrac{1}{2}\cdot \arctan n = \tfrac{\pi}{4}\,.$$

This contradicts the previous result. Consequently, the first equality in the last line is wrong—the others being indubitable. So, for this series, it is *not* possible to interchange integration and limit.

Conclusion: We need a *theorem* to tell us, *in which cases* the interchange of integration and limit is allowed. Naturally, it would be more attractive if *the calculation itself* would make the problem *visible.* Some analysts enjoy calculating more than hunting for theorems that allow them to calculate.

Is an Analysis Without Those Paradoxes Possible?

A Source from the Years 1948–53

Curt Schmieden (1905–91) was an eminent mathematician. In 1934, he became a professor at the Technical University of Darmstadt, where he was awarded a chair in mathematics in 1937, and in 1957/58 he was the rector of this university. In a manuscript dating from 1948–53, Schmieden commented on examples of this kind (ALL *emphases* added):

> Such examples could be given endlessly; the deeper one penetrates into mathematics, the more such paradoxes emerge—even if one completely disregards set-theory.
>
> The most thrilling aspect of this kind of mathematics is that in spite of those paradoxes one always arrives at a "true" result for each concrete problem. Consequently, it does not surprise that some mathematicians decide at some point that *the naive intuition about infinity does no longer suffice in some way or other,* whereas working along definite rules in regard to infinity makes it possible to tame those quantities.
>
> *Nevertheless, there remains a feeling of discomfort. Also the question remains,* WHETHER IT IS NOT POSSIBLE AFTER ALL TO CONSTRUCT ANALYSIS IN SUCH A WAY THAT IT WORKS ALONGSIDE FIXED RULES AS IT HAS BEEN DOING SO FAR, AND THIS IN SUCH A WAY THAT *the kinds of aforementioned* PARADOXES DO NOT EMERGE. *Thus our naive intuition* which according to Goethe, more often than not wishes "to represent the infinite with the help of the finite" [1], *would be granted.*

Schmieden, being "fully aware of the imperfections of this attempt" stated some foundational principles along those lines. Among them was the following that is obviously inspired by Johann Bernoulli and Euler but which also surpasses both conceptually:

> Reason forcibly demands that there *must* exist infinitely large numbers, which can *only* be meaningfully defined as follows:
>
> An infinitely large natural number is a natural number, which *cannot* be grasped by an unlimited continuation of the process of counting, for counting is necessarily bound to the degree of finiteness.
>
> Consequently, to arrive at such a "number", it requires, figuratively speaking, a jump, or as a metaphor: infinitely large numbers form the "horizon of finiteness".

The idea is:

> If we conceive an infinitely large number called Ω (capital omega), reached by such a jump, obviously nothing speaks against but everything for it that we are allowed to calculate with such a "number" in just the same way as with a usual number.

Basically, Johann Bernoulli and Euler had shared this opinion. A little later Schmieden gives the following definition:

> $\omega = \frac{1}{\Omega}$ (small omega) is an infinitely small number which is smaller than each positive rational number and which is defined by the level of finite numbers. (ω is a "zero number".)

[1] This is the exact translation of Goethe's words.

> In the same way as for any infinitely large number, there exists one of a larger *order*[2], and there exists for any zero number one of a smaller *order*[2].
>
> Therefore, our ordinary zero known from conventional analysis is only allowed to be used as another notation of the identity $a \equiv a$.

The latter would have pleased Frege (p. 195), and Weierstraß, too, would have given his approval in regard to such precision in relation to equality.

Finally, there is Schmieden's foundational equation:

> We define
>
> $$\lim_{n \to \infty} n = \Omega \;\; ;$$
>
> each other limit has to be related to this expression.

(The sign "∞" is not a good choice here and should have been avoided altogether.)

It is fairly evident that it is possible to construct from infinitely large natural numbers suitable rational numbers, e.g. $\frac{1}{\Omega}$ or $1 + \frac{1}{\Omega}$ etc. The essential question is: *Is this enough for analysis?* After all, we now have "infinitely near" numbers, and this also in the neighbourhood of each rational number. In Schmieden's words:

> Around each ordinary rational number there exists an ω-sphere which does not include any other ordinary rational number, but which already contains infinitely many rational numbers of the second class [i.e. level] in the lowest ω-level.
>
> With the help of the enlarged notion of rational numbers, which inevitably follows from the introduction of Ω, we reach the seemingly paradoxical result that analysis, if founded on Ω-numbers, gets along with these [Ω-]rational numbers, and even more—that no other numbers can occur, because each number defined via the usual limit arises in our system as an Ω-rational number.

Here we have the essence of Schmieden's idea: instead of the real numbers (following Müller or Bertrand or Cantor or Dedekind), we take "Ω-rational" numbers, e.g. $q \pm \frac{1}{\Omega}$ or $q \pm \frac{5\Omega^2 - 7\Omega + 3}{\Omega^3 - \frac{3}{4}}$, etc. These can be used in calculations in the usual manner. The "Ω-rational" numbers should be enough because—as we know since Euler—analysis consists essentially of calculations, especially of calculations with the infinite.

Schmieden Dissolves the Paradoxes

It was the aim of Schmieden to dissolve such "paradoxes" as shown above with the help of more precise calculations—i.e. with his Ω-numbers. Schmieden states:

> We especially inspect those instances where analysis puts up signs with the warning: here it is forbidden to calculate as in the finite case!

In what follows, representative examples of his method are shown.

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[2] emphases added

Resolving paradox I (p. 227).

Take the series

$$\ln 2 = 1 - \tfrac{1}{2} + \tfrac{1}{3} - \tfrac{1}{4} + \tfrac{1}{5} - \tfrac{1}{6} + \tfrac{1}{7} - \tfrac{1}{8} + - \dots$$

Schmieden now takes care in regard to the infinite ("$+ - \dots$") and calculates precisely:

$$\ln 2 = \sum_{n=1}^{\Omega} \tfrac{(-1)^{n-1}}{n} = 1 - \tfrac{1}{2} + \tfrac{1}{3} - \tfrac{1}{4} + \tfrac{1}{5} - \tfrac{1}{6} + \tfrac{1}{7} - \tfrac{1}{8} + - \dots + \tfrac{1}{\Omega-1} - \tfrac{1}{\Omega} =: A\,.$$

Here Ω is assumed to be even. But careful: "$+ - \dots$" has *different* meanings in the last two lines of formulae. (a) In the first line, "$+-\dots$" indicates "and so on" without last term. (b) In the second line, "$+-\dots$" marks a gap, like in $1 + \frac{1}{2} + \frac{1}{4} + \dots + \frac{1}{2^n}$.

Now Schmieden rearranges the series A and supposes that Ω divided by 4 leaves no remainder. (Why not? If it is useful! At a pinch, make $\Omega' = 4\Omega$.) He then realizes the sum formula of the rearranged series:

$$\sum_{n=1}^{\Omega/4} \left(\tfrac{1}{4n-3} + \tfrac{1}{4n-1} - \tfrac{2}{4n} \right) = 1 + \tfrac{1}{3} - \tfrac{1}{2} + \tfrac{1}{5} + \tfrac{1}{7} - \tfrac{1}{4} + + - \dots + \tfrac{1}{\Omega-3} + \tfrac{1}{\Omega-1} - \tfrac{1}{\Omega/2} =: B\,.$$

We *see:*

It is true, A and B have the same positive terms (all odd denominators, including $\Omega - 1$), but the second half of the negative terms of A (even denominators) are missing in B, up to Ω:

$$-\frac{1}{\Omega/2+2} - \frac{1}{\Omega/2+4} - \frac{1}{\Omega/2+6} - \dots - \frac{1}{\Omega} = -\sum_{n=\frac{\Omega}{4}+1}^{\Omega/2} \tfrac{1}{2n} =: -C\,,$$

and we have

$$A = B - C\,.$$

There you are! *That is why* we have $B > A$!

B is not at all a rearrangement of A, for it has infinitely *fewer* (negative) terms.

Schmieden even calculates the value of this difference C. This he does by integration—using the fact that in case of a continuous function and an infinitely small interval the estimated value of the area under this function only differs from the integral by an infinitely small amount. We denote this just like we did in the case of Johann Bernoulli (p. 62) with $\approx$ and observe that $\omega = \frac{1}{\Omega}$ is infinitely small. We are going to calculate

$$C = \sum_{n=\frac{\Omega}{4}+1}^{\Omega/2} \frac{1}{2n} \cdot \frac{\omega}{\omega}\,.$$

Schmieden defines $z = 2n\omega$, consequently $dz = 2\omega$ (as n always grows by 1), and therefore $\frac{\omega}{2n\omega} = \frac{1}{2}\frac{dz}{z}$. But $\frac{1}{2n\omega}$ is continuous (in the sense of ε-δ). Lower limit: $z = 2n\omega = 2\left(\frac{\Omega}{4}+1\right)\cdot\omega = \frac{1}{2} + 2\omega$, upper limit: $z = 2n\omega = 2\left(\frac{\Omega}{2}\right)\cdot\omega = 1$, and therefore:

$$C \approx \frac{1}{2}\cdot \int_{z=\frac{1}{2}+2\omega}^{1} \frac{dz}{z} \approx \frac{1}{2}\cdot(\ln 1 - \ln\tfrac{1}{2}) = \tfrac{1}{2}(\ln 1 - \ln 1 + \ln 2) = \tfrac{1}{2}\ln 2\,.$$

So we get in fact

$$B - C \approx \tfrac{3}{2}\ln 2 - \tfrac{1}{2}\ln 2 = \ln 2 = A$$

$$\textit{resp.} \qquad A - B = -C \approx -\tfrac{1}{2}\ln 2\,,$$

as we expected. Nothing of the kind $\frac{3}{2}\ln 2 = \ln 2$!

We record:

> The investigation of *regular* "rearrangements" of conditionally convergent series leads to comparable results: *seemingly* paradoxical equations of conventional analysis are explained in calculations with Ω-numbers through the omission of infinitely many terms with a finite sum.

Besides, the concept of "conditional" (i.e. not "absolute") convergence is dispensable. Instead, all series are treated equally, like finite sums. (This would have pleased Weierstraß.)

Interestingly, in their publication from 1958, Schmieden and Laugwitz hint at the fact that "nevertheless, the difference between absolute and conditional convergence remains important for technical calculations, insofar as absolutely convergent series do allow arbitrary rearrangements without further considerations". Obviously, this sentence is a concession by the authors and is probably due to the pressure exerted by the editors of the journal—because essentially it contradicts the foundational view of the authors: in their way of *precisely* calculating, all series are treated equally.

Resolving paradox II (p. 223).

Schmieden considers the C-1 Summation of the series

$$A = 1 - 1 + 1 - 1 + 1 - 1 + - \ldots$$

from p. 223. There we examined the expression

$$S^{C\,1} = \lim_{n\to\infty} \frac{s_1 + s_2 + s_3 + \ldots + s_n}{n} .$$

Schmieden presents this series more conveniently via Ω instead of lim and ∞:

$$A^{C\,1} = \frac{s_1 + s_2 + s_3 + \ldots + s_\Omega}{\Omega} .$$

This fraction he writes down in more detail. Thereby he pays attention to the following: (i) The first 1 of series A is included in *all* partial sums s_k; this amounts to the first term being $\frac{\Omega}{\Omega}$. (ii) The second 1 of series A is only included in $\Omega - 1$ of the partial sums s_k (i.e. not in s_1); this gives the second term as $\frac{\Omega-1}{\Omega}$; etc.

$$\begin{aligned} A^{C\,1} &= \tfrac{\Omega}{\Omega} - \tfrac{\Omega-1}{\Omega} + \tfrac{\Omega-2}{\Omega} - + \ldots + (-1)^{\Omega}\, \tfrac{\Omega-(\Omega-2)}{\Omega} + \tfrac{(-1)^{\Omega+1}}{\Omega} \\ &= 1 - (1-\omega) + (1-2\omega) - + \ldots + (-1)^{\Omega} \cdot 2\omega \quad + (-1)^{\Omega+1} \cdot 1\omega \end{aligned}$$

He then considers the difference of $A - A^{C\,1}$ step-by-step, going from term to term, as follows:

$$A - A^{C\,1} = \underbrace{\underbrace{\underbrace{\underbrace{\underbrace{\underbrace{\underbrace{0}_{0}\ -\omega}_{-\omega}\ +2\omega}_{\omega}\ -3\omega}_{-2\omega}\ +4\omega}_{2\omega}\ \ldots}_{\ldots}\ +(-1)^{\Omega+1}(-\omega)}$$

$$= \begin{cases} -\frac{\Omega}{2}\omega = -\frac{1}{2} & \text{if} \quad \Omega \quad \text{even, then} \quad A = 0 \\ +\frac{\Omega-1}{2}\omega \approx +\frac{1}{2} & \text{if} \quad \Omega \quad \text{odd, then} \quad A = 1 . \end{cases}$$

All this is very accurate. *Note the sum A has the value* $\frac{1}{2}$, but we have $A^{C\,1} = -\frac{1}{2}$ if Ω is even, if not we have $A^{C\,1} \approx \frac{1}{2}$. *The sum A* ALWAYS *has one of the values* 1 *or* 0.—No paradox whatsoever.

The following question presents itself:

What constitutes the sum $A^{C\,1}$ really?

Schmieden just calculates. For the general

$$A = a_0 + a_1 + a_2 + \ldots + a_{\Omega-1}$$

he takes, as we have already seen,

$$A^{C\ 1} = \tfrac{1}{\Omega} \sum_{n=0}^{\Omega-1} s_n = \tfrac{1}{\Omega} \left(\Omega a_0 + (\Omega - 1)a_1 + (\Omega - 2)a_2 + \ldots + (\Omega - (\Omega - 1))a_{\Omega-1}\right) .$$

Especially, if

$$A(x) = 1 + x + x^2 + \ldots + x^{\Omega-1},$$

he substitutes $a_n = x^n$ and gets

$$\begin{aligned} A^{C\ 1}(x) = \tfrac{1}{\Omega} \sum_{n=0}^{\Omega-1} s_n &= \tfrac{1}{\Omega} \left(\Omega \cdot 1 + (\Omega - 1)x + (\Omega - 2)x^2 + \ldots + (\Omega - (\Omega - 1))x^{\Omega-1}\right) \\ &= 1 - x(1 - \omega) + x^2(1 - 2\omega) - + \ldots + (-1)^{\Omega-1}x^{\Omega-1}(1 - (1 - \Omega)\omega) \\ &= \sum_{n=0}^{\Omega-1} (-1)^n x^n \quad + \quad \omega \sum_{n=0}^{\Omega-1} (-1)^{n+1} n x^n \\ &= \quad s_\Omega(x) \quad + \quad \omega \sum_{n=0}^{\Omega-1} (-1)^{n+1} n x^n . \end{aligned}$$

Therefore, we have

$$s_\Omega(x) - A^{C\ 1}(x) = \omega \sum_{n=0}^{\Omega-1} (-1)^n n x^n = K .$$

K generates the convergence of $s_\Omega(x)$ to $A^{C\ 1}(x)$, but only in the infinite: As the factor ω in K shows, a finite number of terms of K will have sum ≈ 0.

Resolving paradox III (p. 224).

From the formula

$$s_n(x) = \frac{1 + (-1)^{n-1}x^n}{1 + x} \qquad \text{if} \qquad x \neq -1 ,$$

Schmieden directly concludes

$$s_\Omega(x) = \tfrac{1}{1+x} \left(1 + (-1)^{\Omega-1}x^\Omega\right) . \tag{§§}$$

1. If $|x| < 1$ and $|x| \not\approx 1$, we get $x^\Omega \approx 0$, and consequently,

$$s_\Omega(x) \approx \tfrac{1}{1+x} .$$

2. If $x = 1$, we get for an even or odd number Ω of terms:

$$s_\Omega(1) = \tfrac{1}{2}\left(1 + (-1)^{\Omega-1}\right) = \begin{cases} 1 & \text{if } \Omega \text{ is odd} \\ 0 & \text{if } \Omega \text{ is even.} \end{cases}$$

3. If $x < 1$ and $x \approx 1$, the tricky x^Ω is calculated by means of a trick. Like Euler, Schmieden takes for $\nu > 0$:

$$x := 1 - \xi\omega^\nu$$

where ξ is finite and positive. He gets (remember from p. 77: instead of $\lim_{n\to\infty}\left(1 + \frac{x}{n}\right)^n = e^x$, Schmieden has of course $(1 + \omega x)^\Omega \approx e^x$) with the help of the following subsidiary calculation:

$$x^\Omega = \left(1 - \xi\omega^\nu\right)^\Omega = \left(1 - \omega\cdot\xi\omega^{\nu-1}\right)^\Omega \approx e^{-\xi\omega^{\nu-1}} \begin{cases} \approx 1 & \text{if } \nu > 1\,, \\ = e^{-\xi} & \text{if } \nu = 1\,, \\ \approx 0 & \text{if } \nu < 1\,, \end{cases}$$

and finally for §§ (as $1 + x \approx 2$), the result

$$s_\Omega(x) \approx \begin{cases} \frac{1}{2}\left(1 + (-1)^{\Omega-1}\right) & \text{if } \nu > 1\,, \\ \frac{1}{2}\left(1 + (-1)^{\Omega-1}e^{-\xi}\right) & \text{if } \nu = 1\,, \\ \frac{1}{2} & \text{if } 0 < \nu < 1\,. \end{cases}$$

The case $|x| > 1$ is judged by Schmieden as being "nonsensical" (Fig. 14.1).

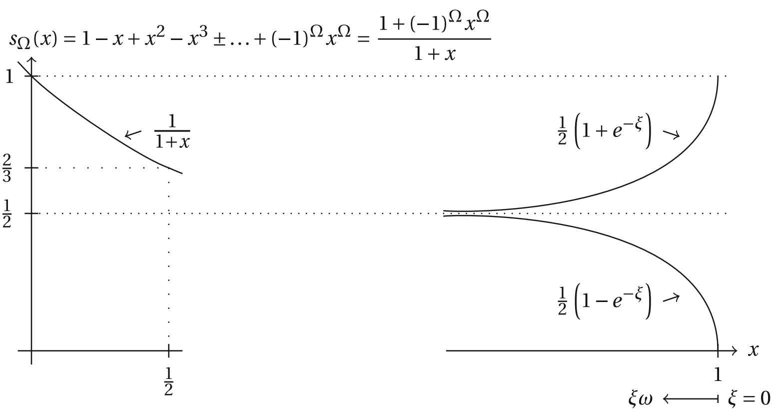

Fig. 14.1 The function $s_\Omega(x) = 1 - x + x^2 - + \ldots$ in [0, 1], at right for $x \lessapprox 1$

Everything is exactly explained: if $\nu = 1$ and:

(i) Ω is odd, the function $s_\Omega(x)$ increases from the value $\frac{1}{2}$ near $x = 1$ as e-function $\frac{1}{2}\left(1 + e^{-\xi}\right)$ up to the value 1 for $x = 1$ (i.e. up to $\xi = 0$).

(ii) Ω is even, the function decreases from the value $\frac{1}{2}$ near $x = 1$ as e-function $\frac{1}{2}\left(1 - e^{-\xi}\right)$ up to the value 0 for $x = 1$.

In other words: depending on whether Ω is odd or even, the value of the function *near* $x = 1$ decreases from $\frac{1}{2}$ to 0 or increases from $\frac{1}{2}$ to 1. *This is by no means indeterminate!* Of course, some calculation is needed—in mathematics, you do not get anything without work. The distinction between Ω being odd or even shows the different *possibilities* of the results of your calculation.

Resolving paradox IV (p. 224).

Only Eq. ‡‡ is thorny. Schmieden considers yet again the details and studies

$$f_\Omega(x) = \frac{\Omega x}{1 + \Omega^2 x^4}\,.$$

As before, he takes $x = \xi\omega^\nu$ and gets

$$f_\Omega\left(\xi\omega^\nu\right) = \frac{\xi\omega^{\nu-1}}{1 + \xi^4\omega^{4\nu-2}} = \frac{\xi\Omega^{1-\nu}}{1 + \xi^4\Omega^{2-4\nu}} \approx \begin{cases} 0 & \text{if } \nu > 1\,, \\ \xi & \text{if } \nu = 1\,, \\ \xi^{-3}\Omega^\alpha\,,\ (0 < \alpha < 2)\,, & \text{if } \frac{1}{3} < \nu < 1, \\ \quad \text{as } \frac{\xi\Omega^{1-\nu}}{1+\xi^4\Omega^{2-4\nu}} \sim \Omega^{1-\nu-2+4\nu} & \\ \quad = \Omega^{3\nu-1} & \\ \xi^{-3} & \text{if } \nu = \frac{1}{3}\,, \\ 0 & \text{if } 0 < \nu < \frac{1}{3}\,. \end{cases}$$

Therefore near 0 in the case $\frac{1}{3} \leqq \nu \leqq 1$, we unambiguously obtain

$$f_\Omega \neq f\ !$$

Schmieden also deals with delta-functions (indeed, if you know Ω-numbers, you have *true* delta-*functions!*). This would lead us astray, but one example of an elementary delta-function will nevertheless be shown below.

The First Formal Version of a Nonstandard-Analysis in the Year 1958

In 1954, the then young Detlef Laugwitz (1932–2000) was handed a manuscript by Carl Friedrich von Weizsäcker (1912–2007) to report on it in his seminar in

Göttingen. After Laugwitz' affirmative reaction to it, Weizsäcker brought him into contact with the author Curt Schmieden. This was the beginning of a very fruitful cooperation, and in 1962, at the age of 29, Laugwitz took up a position as chair in mathematics at the University of Darmstadt.

Four years earlier, in 1958, and after some quarrels behind the scenes, the renowned journal *Mathematische Zeitschrift* had published an article by Schmieden and Laugwitz in its volume 69. Later it turned out to be the first publication in a field nowadays called "nonstandard-analysis".

In it, the two authors expressively emphasize that:

> It should be noticed that not only a rebuilding or a mere modification of the conventional analysis emerges here, but that a true enlargement is also produced.

Right at the start, they showed that their new analysis incorporates the species of Dirac's delta-functions as true "functions", without the necessity of modifying the notion of a function. As an example of a delta-function, they presented

$$\delta(x) = \frac{1}{\pi} \cdot \frac{\Omega}{1 + \Omega^2 x^2} .$$

The typical features of a delta-function are easily confirmed: $\delta(x) \approx 0$, except when $x \approx 0$—where $\delta(x)$ is infinitely large, and $\int\limits_{-\infty}^{+\infty} \delta(x)\, dx = 1$.

The Foundation in the Year 1958

Laugwitz formulated some of Schmieden's principal ideas in the language of the then fashionable algebra. He referred to the definitions of Cantor. Cantor had defined *convergent sequences of rational numbers* as novel numbers, and he had named these sequences "limits" (p. 191). The duo Schmieden/Laugwitz proposed to take *the totality* of sequences of rational numbers and to define EACH *of these sequences as an individual "number"*—independent of their convergence, just: *all* these sequences! As a name for the sequences, they chose "Limit" (as Cantor did, but with a capital "L" as a distinction) as well as "Ω-number".

$$(a_n)_n = a_\Omega = \operatorname*{Lim}_{n=\Omega} a_n ,$$

$$\text{especially} \qquad (n)_n = \Omega , \quad (\tfrac{1}{n})_n = \omega \quad \text{and also} \quad ((-1)^n)_n = (-1)^\Omega .$$

That is to say, Schmieden's "jump" from the process of counting 1, 2, 3, ... to Ω (see p. 226) now obtains as its final destination the *complete sequence* $\Omega = (n)_n$! Even this divergent sequence defines an Ω-number, of course an infinitely large one.

The preceding number to Ω, i.e. $\Omega - 1$, is the sequence $(n-1)_n = (0, 1, 2, 3, \ldots)$; its successor, i.e. $\Omega + 1$, is the sequence $(2, 3, 4, \ldots)$, etc.

The discombobulating facts in favour of conventional analysis (that the new numbers comprise zero divisors and therefore do not constitute a field—and consequently division is not always possible—as well as the missing of their "total order") were clearly articulated in this chapter, but not a single word of valuation was added.

For example, we have

$$(0, 1, 0, 1, 0, 1, \ldots) \cdot (1, 0, 1, 0, 1, 0, \ldots) = 0,$$

although neither factor being $0 = (0, 0, 0, \ldots)$. And, moreover, we are not able to call one of these two factors greater than the other—although obviously they are different.

Further Peculiarities of the New Analysis in the Year 1958

The authors also clearly identified those facts that appear in this new "enlargement" of analysis *differently* from in conventional analysis:

- There are three different equivalence relations as orders of magnitude: 1. "finitely equal", 2. "having the same magnitude" and 3. "having the same order of magnitude".
- **Some theorems are stated and proved, which are *wrong* in conventional analysis but *true* in the new enlargement.**
  1. *Every limit exists.* This is just the new definition of "number".
     In detail (in the case of sequences of Ω-numbers, we write their "index" *above*): The limit of a sequence of Ω-numbers $(a_\Omega^{(p)})_p$ is defined for each "positive integer" as well as for each "infinitely large" Ω-number g_Ω, i.e. $g_\Omega = (g_n)_n$ where $g_n > g_k > 0$ if $n > k$. The limit

     $$\operatorname*{Lim}_{p=g_\Omega} a_\Omega^{(p)} = a_\Omega^{(g_\Omega)} = b_\Omega$$

     is defined as the sequence of components

     $$b_n = a_n^{(g_n)},$$

     i.e. by the "diagonal sequence" defined from the sequence $(a_\Omega^{(p)})_p$ of Ω-numbers $a_\Omega^{(p)}$.—Examples: (a) In case of the constant sequence $a_\Omega = \left(\frac{1}{n}\right)_n = \omega$, we have $\lim\limits_{p=\Omega} a_\Omega^{(p)} = \left(\frac{1}{n}\right)_n = a_\Omega = \omega$, as it should be; and $\lim\limits_{p=2\Omega} a_\Omega^{(p)} = \left(\frac{1}{2n}\right)_n = \frac{1}{2}\omega$. (b) The sequence $b_\Omega^{(p)}$ with the pth term

$b_\Omega^{(p)} = \left(\frac{1}{p \cdot n}\right)_n$ is $\lim\limits_{p=\Omega} b_\Omega^{(p)} = \left(\frac{1}{n \cdot n}\right)_n = \frac{1}{\Omega \cdot \Omega} = \omega^2$ and $\lim\limits_{p=2\Omega} b_\Omega^{(p)} = \left(\frac{1}{2n \cdot n}\right)_n = \frac{1}{2\Omega \cdot \Omega} = \frac{1}{2}\omega^2$—right?

2. *Any two limits are interchangeable.* The essential reason for this is that limits are only indicated but not calculated.
   In detail: Given a double sequence $(a_\Omega^{p,q})_{p,q}$ of Ω-numbers $a_\Omega^{g_n,h_n}$, the limit is given by the sequence of the components $a_n^{g_n,h_n}$.
   Example:

$$a^{p,q} = \frac{1}{1 + \frac{p}{q}} \,.$$

   (The given Ω-numbers are constant sequences of rational numbers with the constant components $\frac{1}{1+p/q}$.)
   Conventionally, we have

$$\lim_{q\to\infty} \lim_{p\to\infty} a^{p,q} = 0\,, \qquad \text{but} \qquad \lim_{p\to\infty} \lim_{q\to\infty} a^{p,q} = 1\,.$$

   However, for Ω-*numbers,* independently from the order, we have

$$\operatorname*{Lim}_{\substack{p=g_\Omega \\ q=h_\Omega}} a^{p,q} = \frac{1}{1 + \frac{g_\Omega}{h_\Omega}} \,.$$

   If you choose $p = g_\Omega$ (conventionally: $p \to \infty$) for taking the first limit, you obtain

$$\frac{1}{1+\frac{g_\Omega}{q}} \,,$$

   i.e. an infinitely small number for each finite $q = 1, 2, 3, \ldots$ Yet, if you then choose for the other limit $h_\Omega = g_\Omega$, you nevertheless get the finite value

$$\frac{1}{1+\frac{g_\Omega}{h_\Omega}} = \frac{1}{1+\frac{g_\Omega}{g_\Omega}} = \frac{1}{1+1} = \frac{1}{2} \,,$$

   which says that the infinitely many small numbers $\frac{1}{1+\frac{g_\Omega}{q}}$ nevertheless have a finite limit of the value $\frac{1}{2}$.

3. *Divergent series are of equal rank to the others,* as in the new version of analysis the calculations are the same: in the finite and in the infinite.
   As an example, the famous series:

$$\sum_{p=1}^{\Omega} \frac{1}{p(p+1)} = \sum_{p=1}^{\Omega} \left(\frac{1}{p} - \frac{1}{p+1}\right) = \sum_{p=1}^{\Omega} \frac{1}{p} - \sum_{p=2}^{\Omega+1} \frac{1}{p} = 1 - \frac{1}{\Omega+1} \approx 1\,.$$

A convergent series on the left is split into the difference of two divergent series, and then this difference is calculated to be a finite value. Conventionally, an absolute no-go!

4. *The limit function of a sequence of continuous functions is continuous.* The authors' comment: "This theorem has no simple analogy in conventional analysis". There is no reference made to the "Cauchy Sum Theorem" (p. 138). As an example, the sequence of functions x^n, for the closed interval $0 \leqq x \leqq 1$, is given (already treated above on p. 140).

- The *Weierstraß Approximation Theorem* was also obtained for the new version of analysis.
- *Differentiation* and *integration* were briefly mentioned. However, a "differentiable" function does *not* need to have a derivative *for each value!* ("The 'derivative' might be 'irrational'".)
- The *Mean Value Theorem* is proved (for "normal" functions).

Naturally, the notion of "function" is precarious. *Of course,* complete freedom in the sense of Bolzano (p. 111) could not be permitted. After all the starting point of Schmieden/Laugwitz is conventional analysis (which they call "usual")—and not a completely new, extravagant shape of analysis. That is why they restrict themselves to such functions that are "already completely defined, if their values are given for the rational numbers of the domain". Sensibly, the description of the values of the function has to be given in a standardized mathematical language.

Finale

Curt Schmieden was a practitioner of calculating. As a consequence, he had the idea of practicing analysis *by calculating:* no complicated general theorems that state the rules of calculation or rule out certain techniques of calculation—just *calculate* and, besides, treat the infinite (i.e. the *specifically* analytical) in just the same way as the finite. (He expressed it this way: "*calculating* analysis".) The result of the calculation has to be assessed according to the requirements of the problem at the end of that calculation.

That this is *permissible* was shown to him by his calculations ("exactly as in the finite"—what should be wrong with this?) as well as by his results. To anchor his technique of "Omega Analysis"—an *enlarged* version of "Value Analysis"—reliably within the current foundational principles of mathematics, he left to others.

His enthusiastic colleague and co-worker Detlef Laugwitz gained credit for his precise algebraic foundation of Schmieden's ideas. This was first published in the most general version in an article from 1958, which was signed by both of them.

In 1978, Laugwitz published a stronger algebraic construction, developed from Schmieden's essential ideas, and in 1986, he presented a new version, which incorporates some features of formal logic.

Laugwitz was always aware that Schmieden's Ω-numbers are not a field because one cannot divide by all non-zero numbers. This seemed to be a problem, especially at a time, when all mathematics was dominated by Nicolas Bourbaki's idea of structure. Subsequently, in 1978, Laugwitz constructed, following others, a field $^*K$ by algebraic means. Later, in 1986, Laugwitz *pretended to, but did not clearly define* a set $^{\Omega}K$ with Ω-numbers. Instead of defining this, he changed to a formal language and developed a theory where "the rules of an ordered field" are valid.

These attempts to represent Schmieden's ideas within the actual methodology of calculus could not do justice to Schmieden's fundamental concerns. In a *field* that includes Ω-numbers, it *must* be settled, whether $\Omega/2$ is a natural number or not. The mathematician might lack the means to *decide* which it is—but it will definitely *be* one of the two. But this does not fit to Schmieden's essential idea. Schmieden really *left open* both possibilities—and if it were necessary, he distinguished both cases. Methodical stipulations of this kind—even more if they were bound to be unknown in every detail—were surely not his aim.

Foundational Problems

This first publication of a nonstandard analysis by these two men was soon followed by versions from other authors.

The first was by Abraham Robinson (1918–74), a model theorist who also coined the name "nonstandard-analysis". This approach to nonstandard analysis requires the knowledge of logics as a precondition to deal with continuous functions.

Willem A. J. Luxemburg (1929–2018) confined himself to algebraic constructions, essentially relying on ultra-filters for his definition of a field with "infinite" numbers.

In 1977, Edward Nelson (1932–2014) published a first axiom system for nonstandard analysis. Other versions followed.

This means that another mathematical theory (logics, universal algebra) is always needed to provide nonstandard analysis with suitable "numbers". This is not satisfactory and is also a severe obstacle for an easy acceptance of this novel approach to analysis, unless you simply confine yourself to an axiom system.

Axiomatics

When inspecting the actual textbooks of analysis, it appears that there is no adequate consideration paid to the foundations. Generally, the real numbers are not introduced constructively, as was taught by Müller, Bertrand and Dedekind or by Cantor and Heine, but instead, following Hilbert, axiomatically. This helps to save time.

Well, possibly, this fits excellently to the new role of mathematics as a security police for all sciences, which was promulgated by Hilbert in 1917 (p. 209) in order to teach the unconditional (at least provisional) acceptance of *arbitrary* axiom systems detached from any informal substantial justifications.

If it is so, there is then no argument to be put forward against the idea of taking an axiom system for nonstandard numbers (today usually called "hyper-real numbers")—possibly apart from the fact that the elaboration of a nonstandard analysis *differs* in some respects from standard analysis. (This has been shown with the help of some "paradoxes" as well as generalized: see from p. 235.) This challenges one's independent thought as well as impedes the usage of textbooks: you always have to check *which* version of analysis the chosen author likes. Standardization of the curriculum facilitates lecturing, especially if many students have to be educated. In small circles of specialists, subtle discussions will be eased.

A Path to Independency

Schmieden's intention had been to rescue conventional analysis from its "paradoxes" as well as to give it a better form: fewer theorems, more computations.

An opposite development started in the 1980s. It continued for some decades in small circles. Under the (for the laymen astonishing) title "constructive nonstandard-analysis", the theory was developed further as a special field in its own right in which more advanced methods (like sheafs and topoi) were implemented. This leads to a departure from the first origins of nonstandard analysis as well as from classical calculus, but it undoubtedly created some marvellous mathematics.

After the discovery of Weierstraß' construction of the real numbers in 2016, it may well be possible to develop a new kind of approach to nonstandard analysis.

Nonstandard-Analysis and the History of Analysis

Not Schmieden and Laugwitz, but Robinson was the one who immediately started to relate the new theory to historical texts. The theorem nowadays called the "Cauchy Sum Theorem" (p. 138) came up fairly early for discussion. In his book from 1963, Robinson gave an interpretation of Cauchy's writings. His result was: Cauchy's theorem is correct if we add one of the two additional assumptions: (a) the series is uniformly convergent or (b) the family $(s_n(x))_n$ of partial sums is equicontinuous in the interval.

Robinson's idea of translating historical mathematical texts into the language of Nonstandard-analysis impressed the philosopher of science Imre Lakatos (1922–74). In a lecture, which appeared in print only after his death, he claimed that "Cauchy's theorem was true and his proof as correct as an informal proof can be". According to Lakatos, the Cauchy Sum Theorem does therefore *not* need any additional assumptions to gain validity. Thus Lakatos contradicted Robinson.—The further development (i.e. that Lakatos was right in *quite another* sense from what he thought) was traced on pp. 130f.

A Satisfying Finish

It was the aim of my dissertation of 1981 to substantiate Lakatos' thesis. In contrast to him, I based my arguments on the Darmstadt version of Nonstandard-analysis. Today I am aware that my approach (see p. 132) was mistaken.

My doctor father Laugwitz was intrigued by my work, and it inspired him to undertake his own detailed studies of former analysts, *esp*. of Cauchy's works, thereby producing quite a few articles on the topic.

My studies of Bolzano's mathematics, which started in 1986, showed me the inadequacy of Lakatos' methodology in regard to the understanding of historical mathematical writings. As a consequence, I had to change my approach. In winter 1990, I turned again to the study of Cauchy's analysis, this time from a new perspective. Prof Dr Laugwitz tried to hinder my work with a maximum of vigour. After he failed, I lost his favour and consequently all local academic support (and sadly, more than that). The Darmstadt University showed itself incapable of an unbiased clarification of this issue until today.

If Prof Dr Laugwitz had been factually successful at his time (not only institutionally), this book could not have been written.

Literature

Aigner, M. & Ziegler, G. M. (1998). *Proofs from the book.* Berlin: Springer.

Lakatos, I. (1980). Philosophical papers (2 vols). In *Mathematics, science and epistemology* (Vol. 1). Cambridge: Cambridge University Press 1980.

Laugwitz, D. (1986). *Zahlen und Kontinuum. Eine Einführung in die Infinitesimalmathematik.* Mannheim: Bibliographisches Institut.

Luxemburg, W. A. J. (1962). Non-Standard Analysis. *In Lectures on A. Robinson's Theory of Infinitesimals and Infinitely Large Numbers. Lecture notes*. Pasadena, California: Mathematics Department, California Institute of Technology.

Luxemburg, W. A. J., & Körner, S. (Eds.) (1979). *Selected papers of Abraham Robinson* (Vol. 2). Amsterdam, New York, Oxford: North Holland Publishing Company.

Nelson, E. (1977). Internal set theory: A new approach to nonstandard analysis. *Bulletin of the American Mathematical Society, 83*(6), 1165–1198.

Palmgren, E. (1995). A constructive approach to nonstandard analysis. *Annals of Pure and Applied Logic, 73*, 297–325.

Palmgren, E. (1996). Constructive nonstandard analysis. *Cahiers du Centre de logique, 9*, 69–97.

Palmgren, E. (1997). A sheaf-theoretic approach for nonstandard analysis. *Annals of Pure and Applied Logic, 85*, 69–86.

Palmgren, E. (1998). Developments in constructive nonstandard analysis. *The Bulletin of Symbolic Logic, 4*(3), 233–272.

Palmgren, E. (2001). Unifying constructive and nonstandard analysis. In P. Schuster, U. Berger, & H. Osswald (Eds.), *Reuniting the antipodes—constructive and nonstandard views of the continuum 2001* (pp. 167–183).
Richman, F. (1998). Generalized real numbers in constructive mathematics. *Indagationes Mathematicae, N. S., 9*(4), 595–606.
Riemann, B. (1854). *Ueber die Darstellbarkeit einer Function durch eine trigonometrische Reihe*. cited from Weber and Dedekind 1953, pp. 227–271.
Robinson, A. (1961). Non-Standard analysis. *Indagationes Mathematicae, 23*, 432–440. cited from Luxemburg and Körner 1979, pp. 3–11.
Robinson, A. (1963). *Introduction to model theory and to the metamathematics of algebra.* Amsterdam: North-Holland Publishing Company.
Russell, B. (1919). *Introduction to mathematical philosophy*. In Georg Allen & Unwin, London (New York: The MacMillan Co.). www.gutenberg.org/ebooks/41654.
Schmieden, C. (1948–53). Vom Unendlichen und der Null. Versuch einer Neubegründung der Analysis. manuscript, unpublished.
Schmieden, C. & Laugwitz, D. (1958). Eine Erweiterung der Infinitesimalrechnung. *Mathematische Zeitschrift, 69*, 1–39.
Schuster, P., Berger, U., & Osswald, H. (2001). *Reuniting the antipodes: Constructive and nonstandard views of the continuum.* Dordrecht: Kluwer.
Schuster, P. M. (2000). A constructive look at generalized Cauchy reals. *Mathematical Logic Quarterly, 46*, 125–134.
Spalt, D. D. (1981). *Vom Mythos der Mathematischen Vernunft.* Darmstadt: Wissenschaftliche Buchgesellschaft. ^{2}1987.
Spalt, D. D. (1966). *Die Vernunft im Cauchy-Mythos.* Thun und Frankfurt am Main: Harri Deutsch.
Spalt, D. D. (2022). *Die Grundlegung der Analysis durch Karl Weierstraß—eine bislang unbekannte Konstruktion der natürlichen und der reellen Zahlen.* Berlin: Springer.
Strauß, E. (1880/81). *Weierstrass, Einleitung in die Theorie der Analytischen Functionen*. Universität Frankfurt am Main: Archiv. Sign 2.11.01; 170348.
Wallace, D. F. (2003). *Everything and more: A compact history of infinity.* New York, London: W. W. Norton (2010)
Weber, H. & Dedekind, R. (Eds.) (1953). *Bernhard Riemann, Gesammelte mathematische Werke.* reprint of the second edition 1892. New York: Dover Publications.