FOUNDATIONS OF MATHEMATICS. A NEOCLASSICAL APPROACH TO INFINITY

Foundations of Mathematics. A Neoclassical Approach to Infinity

Pro Occam Contra Cantor.
Proper Constructivism with Abstraction.
A condition by Paul of Venice (1369-1429) solves Russell's paradox, blocks Cantor's diagonal argument, and provides a challenge to ZFC.
Two results on ZFC: (1) If ZFC is consistent then it is deductively incomplete, (2) ZFC is inconsistent.

Companion to

A Logic of Exceptions (1981, 2007, 2011)

and

Elegance with Substance (2009, 2015)

Thomas Colignatus

Samuel van Houten Genootschap

Colignatus is the science name of Thomas Cool (1954), econometrician (Groningen 1982) and teacher of mathematics (Leiden 2008). He worked at the Dutch Central Planning Bureau (CPB) in 1982-1991. His analysis on unemployment met with censorship by the CPB Directorate and he was dismissed with an abuse of power. He advises to a boycott of Holland till this censorship of science is resolved. He is candidate for President of the European Union for the Dutch Sociaal Liberaal Forum.

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NUR
918 Wiskunde algemeen
921 Fundamentele wiskunde
846 Didactiek

MSC2010
In bold the three major ones if required to select
General
00A30 Philosophy of mathematics
Mathematics education research
00A35 Methodology of mathematics, didactics
97D20 Mathematics Education - Philosophical and theoretical contributions (maths didactics)
97E60 Mathematics Education - Foundations: Sets, relations, set theory
97C70 Mathematics Education - Research: Teaching-learning processes
97B10 Mathematics Education - Educational research and planning
Mathematics itself
03B50 Many-valued logic
03E30 Axiomatics of classical set theory and its fragments
03E35 Consistency and independence results
03E70 Nonclassical and second-order set theories
03F65 Other constructive mathematics

Preface

The target readership are (1) students with an interest in methodology of science and the foundations of mathematics – for example students in physics, engineering, economics, psychology, thus a broad group that uses mathematics and not only those majoring in mathematics – and (2) fellow teachers of mathematics who are sympathetic to the idea of bringing set theory and number theory into general mathematics education – while avoiding the *New Math* disaster in the 1960s in highschool. [1]

Readers would be interested in:

- (A) Constructivism with Abstraction, as a scientific methodology
- (B) Particulars about infinity and number theory, within foundations and set theory
- (C) Correction of errors within mathematics on (B) caused by neglect of (A).

Other readers are (3) research mathematicians, but while they would benefit from the last correction in (C), they must mend for that they are not in the prime target groups. They would start with pages 61-72 below (Colignatus (2015g)) and then restart here again.

Set theory and number theory would be crucial for a better educational programme:

- (i) They greatly enhance competence and confidence
- (ii) They open up the mind to logical structure and calculation also in other subjects
- (iii) They are fundamental for learning and teaching themselves.

The world can be amazed that (A) and (B) are not taught systematically in current school and first year higher education. There are two explanations. One is the mentioned *New Math* disaster in the 1960s. Another more hidden cause are the *transfinites* created by Georg Cantor (1845-1918). When a mathematics teacher starts on the topics of numbers and set theory, and then infinity, then he or she feels obliged to discuss these transfinites. However, her or she also feels doubt whether these should be taught. For highschool and first year students they might be too complex and paradoxical. People in real life have no application for these transfinites and it makes little sense to have transfinites in the highschool diploma. They are relevant purely for mathematicians – and for a particular branch of mathematics as well. Thus, mathematics teaching is stuck. A mathematical *curl* causes so much complexity and irrelevance *that the wonderful basics are not taught.* This book proposes to cut the knot. It adds the bitter irony that Cantor's analysis appears to be misguided. Neglect of (A) made generations of mathematicians blind to some crucial errors.

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As a student in 1980 I greatly benefitted from the admirable book by Howard DeLong (1971) *A profile of mathematical logic.* His book provides the mixture of history, philosophy and mathematics that I still find the best approach. I did not agree with some deductions though, and my response was *A Logic of Exceptions* (ALOE) (1981 unpublished, 2007, 2011). Originally I focused on the Liar paradox and accepted the transfinites. In 2007 I however saw how Cantor's proof on the power set linked up to Russell's paradox. In 2009 I collected my observations on mathematics education in *Elegance with Substance* (EWS). Combining both issues caused two longer papers – in the next paragraph – that comprise this book. An advice to readers is to indeed look at ALOE, EWS and DeLong (1971) too.

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This book contains some original mathematics. This only serves the main purpose of this book. The original papers have abbreviations CCPO-PCWA and PV-RP-CDA-ZFC. Their arguments have been polished up so that this book replaces them. Discussions have been cut up, re-edited and dispersed over chapters, so that they actually create this book. The papers have a *Contra Cantor* (CC) flavour but the book is constructively *Pro Occam* (PO).

[1] https://en.wikipedia.org/wiki/New_Math

Abstract

Contra Cantor Pro Occam - Proper Constructivism with Abstraction

> Context • In the philosophy of mathematics there is the distinction between *platonism* (realism), *formalism*, and *constructivism*. There seems to be no distinguishing or decisive experiment to determine which approach is best according to non-trivial and self-evident criteria. As an alternative approach it is suggested here that philosophy finds a sounding board in the *didactics of mathematics* rather than mathematics itself. Philosophers can go astray when they don't realise the distinction between mathematics (possibly pure modeling) and the didactics of mathematics (an empirical science). The approach also requires that the didactics of mathematics is cleansed of its current errors. Mathematicians are trained for abstract thought but in class they meet with real world students. Traditional mathematicians resolve their cognitive dissonance by relying on tradition. That tradition however is not targetted at didactic clarity and empirical relevance with respect to psychology. The mathematical curriculum is a mess. Mathematical education requires a (constructivist) re-engineering. Better mathematical concepts will also be crucial in other areas, such as e.g. brain research. **> Problem** • Aristotle distinguished between potential and actual infinite, Cantor proposed the transfinites, and Occam would want to reject those transfinites if they aren't really necessary. My book *"A Logic of Exceptions"* already refuted 'the' general proof of Cantor's Conjecture on the power set, so that the latter holds only for finite sets but not for 'any' set. There still remains Cantor's diagonal argument on the real numbers. **> Results** • There is a *bijection by abstraction* between $\mathbb{N}$ and $\mathbb{R}$. Potential and actual infinity are two faces of the same coin. Potential infinity associates with counting, actual infinity with the continuum, but they would be 'equally large'. The notion of a limit in $\mathbb{R}$ cannot be defined independently from the construction of $\mathbb{R}$ itself. Occam's razor eliminates Cantor's transfinites. **> Constructivist content** • Constructive steps S_1, ..., S_5 are identified while S_6 gives non-constructivism (possibly the transfinites). Here S_3 gives potential infinity and S_4 actual infinity. The latter is taken as 'proper constructivism with abstraction'. The confusions about S_6 derive rather from logic than from infinity.

ZFC is inconsistent. A condition by Paul of Venice (1369-1429) solves Russell's paradox, blocks Cantor's diagonal argument, and provides a challenge to ZFC

Paul of Venice (1369-1429) provides a consistency condition that resolves Russell's Paradox in naive set theory without using a Theory of Types. It allows a set of all sets. It also blocks the (diagonal) general proof of Cantor's Conjecture (in Russell's form, for the power set). The Zermelo-Fraenkel-Axiom-of-Choice (ZFC) axioms for set theory appear to be inconsistent. They are still too lax on the notion of a well-defined set. The transfinites of ZFC may be a mirage, and a consequence of still imperfect axiomatics in ZFC for the foundations of set theory. For amendment of ZFC two alternatives are mentioned: ZFC-PV (amendment of de Axiom of Separation) or BST (Basic Set Theory).

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Contents

List of theorems and conjectures

List of main symbols and abbreviations

{*a, ..., b*}	the set with elements *a* to *b*
Ø	the empty set, the set with no members, p 61
{*x* | *p*}	the set of elements *x* for which property *p* = *p*[*x*] applies
{*x* | *p* ↑ *q*}	{*x*| *p*[*x*] unless (*p*[*x*] ∧ *q*[*x*]) is contradictory (also formally)} , p 80
ℕ[*n*]	{0, 1, 2, ..., *n*}, a list of some natural numbers (cardinal), potential infinity
ℕ	{0, 1, 2, ...} or the natural numbers (cardinal), actual infinity, p 22
@	a step of abstraction, e.g. ℕ[*n*] @ ℕ, p 23
𝕆	the ordinal numbers: {1^{st}, 2^{nd}, 3^{rd}, ...}, p 33
𝕤	{1, 2, ...} or a pure sequence created by the successor function, p 33
𝕊	set of all sets, p 55
ℝ[*d*]	numbers in [0, 1] with *d* the number of decimal digits, potential infinity
ℝ	the real numbers, actual infinity, p 42
x^H	1 / *x*, with *H* = -1 the Harremoës operator, pronounced as "eta", p 132
S_1, ..., S_6	steps (degrees) in constructivism, p 39
$x \in A$	*x* is an element of set *A*
$x \notin A$	*x* is not an element of set *A*
∀ *x*	for all *x*
∃ *x*	there is a *x* , or, for some *x*
A ∪ *B*	union of set *A* with set *B*
A \ *B*	the difference: set *A* excluding the elements of *B*
$A \subseteq B$	*A* is a subset of *B,* possibly equal to *B*
P[*A*]	the power set of set *A,* i.e. the set of all subsets of *A,* see p 61
A ~ *B*	two sets *A* and *B* are equally large, with a one-to-one relation, p 32
f: D → *R*	function *f* from domain *D* to range *R*: Any *x* ∈ *D* gives at most one *f*[*x*] ∈ *R*
Б	bijection by abstraction between ℕ and ℝ, so that ℕ ~ ℝ, p 43
¬ *p*	not-*p* (for proposition *p*)
† *p*	*p* is nonsensical, or, *not-at-all p,* see p 15 (dagger)
p ⇒ *q*	*p* implies *q,* or, if *p* then *q*
p ⇔ *q*	*p* is equivalent to *q,* or, *p* if and only if *q,* or, *p* iff *q*
p ∨ *q*	*p* or q
p ∧ *q*	*p* and *q*
p & *q*	*p* and *q* (another notation)
∞	infinity, ambiguously potential or actual, also "undefined"
[*a*, *b*]	closed interval *a* ≤ *x* ≤ *b*
(*a, b*)	open interval *a* < *x* < *b*
ZFC	Zermelo-Fraenkel-Axiom-of-Choice system of axioms for set theory, p 61
ZFC-PV	ZFC adjusted for the consistency condition inspired by Paul of Venice, p 89
BST	Basic Set Theory, closer to naive set theory but with a PV condition, p 89

Part 1. Introduction

A neoclassical approach in mathematics

Two key rules for paradoxical self-reference

Our subject originally is set theory and number theory. Immediately relevant are Cantor's conjectures on the infinite. Subsequently, infinity becomes our main topic, and the earlier subjects are subsidiary. Cantor's conjectures rely on self-reference, which is a logical rather than a mathematical issue. Cases of self-reference that cause a contradiction are notoriously confusing. Two rules appear to be key in tackling such cases:

(1) We can maintain clarity by holding on to the notion of freedom of definition. When a restriction on this freedom generates a consistent framework, while release of the restriction generates confusion, then the restriction is to be preferred.

(2) There is a remarkable distinction between *not-well-defined* and *non-existent.* We can meaningfully discuss the existence or non-existence of something when we know what we are speaking about. When a rhinoceros exists, we can say whether it is in the room or not. For well-defined topics we can accept $p \lor \neg p$, known as *Tertium non datur* (TND). (The term *Law of the Excluded Middle* (LEM) needlessly imposes order.) But it may be that we are dealing with nonsense, $\dagger p$, so that in general only $p \lor \neg p \lor \dagger p$. For nonsense we may say *that it doesn't exist* but we actually mean to say *that the notion isn't well-defined*. Thus:

> In a dilemma $p \lor \neg p$ the non-existence of the one horn implies the other, but when there is nonsense or $p \lor \neg p \lor \dagger p$ then the non-existence of one horn cannot be turned into positive evidence for the other horn.

Constructivism

Hodges (1998) discusses submissions to the *Bulletin of Symbolic Logic* that claimed to refute Cantor but that failed on basic academic standards. This is indeed an area where intuition meets hard proof. Hodges sent me an email (August 10 2012) that he allows me to quote from:

> “You are coming at Cantor’s proof from a constructivist point of view. That’s something that I didn’t consider in my paper, because all of the critics that I was reviewing there seemed to be attacking Cantor from the point of view of classical mathematics; I don’t think they knew about constructivist approaches. Since then some other people have written to me with constructivist criticisms of Cantor. There is not much I can say in general about this kind of approach, because constructivist mathematicians don’t always agree with each other about what is constructivist and what isn’t.”

The core of this book is the new definition of *bijection by abstraction.* [2] This new definition should appeal to all those who have had intuitive misgivings about Cantor’s proof. The definition includes an aspect of *completion* that some readers may consider rather classical and non-constructivist. This book also discusses where Cantor’s proof goes wrong. I suppose that there will be discussion about this but consider this of secondary value. It is more important to improve the didactics in highschool and matricola.

What is a neoclassical approach in mathematics ?

This book distinguishes:

- classical mathematics, from perhaps Pythagoras to Georg Cantor (1845-1918)
- traditional mathematics, from Cantor to *hopefully soon to end*
- neoclassical mathematics, as explained in this book.

[2] The notion of a bijection or a *one-to-one relationship* is defined on page 32.

The neoclassical approach in mathematics claims to maintain a better balance between *abstraction* and *empirics* – even though Cantor used the notion of abstraction himself. See Table 1 on page 28 for an overview of the differences. Below we will define abstraction, while we will assume that empirics are considered in the empirical sciences.

The core business of mathematics is abstraction, but abstraction by itself can lead people astray. Empirical researchers like engineers have more balance in their results by testing their ideas on nature. Mathematics lacks this counterveiling force of nature. The suggestion is to take the *education in mathematics* as the empirical area of relevance for mathematics. This suggestion also holds for philosophy in general, that also is in danger of getting lost in abstraction.

This book has a constructive and destructive component:

- It is constructive in two ways. It intends to help build up a better balance in doing and teaching mathematics. It also follows the philosophies of *nominalism* and *constructivism* as opposed to *realism* and *platonism* – see page 31. A key point is the *combination of constructivism with abstraction.* Abstraction might cause methods that some people may not regard as constructivist.
- The book is somewhat destructive is exposing the errors of traditional mathematics. It is a good question whether this destructive component is really so useful. Why not present the new approach and simply forget about old ways ? The main reason is that we are still too close to tradition, so that it is hard to let go. This book spends a major part of its attention to the errors of the traditional ways, to explain that these are errors, that there is a need for change, in particular w.r.t. the fundamental attitude that causes those errors.

While this book proposes a change in the way of doing mathematics, this is actually rather presumptuous since the author has little or any experience in research mathematics (RM). **Appendix D** contains a background. As an econometrician and teacher of mathematics – trying to reform school mathematics (SM) and matricola – my experience is that I meet with too many errors coming from research mathematicians: hence there must be something wrong at that source.

This introduction perhaps should also provide a definition of infinity, but we will do so later on. Before we proceed, it is useful to consider some paradoxes that can arise when you lose your sense of reality. Such paradoxes have led traditional mathematicians seriously astray on their notion of infinity.

Some paradoxes

(1) Consider the logic: I fit in my coat. My coat fits in my bag. Thus I fit in my bag.

A mathematician may be perfectly happy with this since the propositions are abstract and need not concern a real world and might only concern some topology. For an engineer, interested in an application to reality, the reasoning gives a problem. The assumptions seem true, the reasoning is sound, the conclusion is false, hence something is amiss.

The correction is straightforward: If I wear it, I fit in my coat. If nobody wears it, the coat fits in my bag. Conclusion: If I want to put the coat into the bag then I have to take it off.

(2) Axiomatics may create (seemingly) consistent systems that don't fit an intended interpretation. Van Bendegem (2012:143) gives the example that (a) 1 is small, (b) for each n, if n is small then n+1 is small, (c) hence all n are small. The quick fix is to hold that "small" can be nonsensical when taken absolutely, and that (a') 1 is less than 100, (b') for each n, if n is less than 100 n, then n+1 is less than 100 (n+1), (c') hence for all n, n is less than 100 n. The conclusion is that not all concepts or axiomatic developments are sensical in terms of the intended interpretation even though they may seem so.

(3) A *reductio ad absurdum* format of proof is as follows: one assumes hypotheses, deduces a contradiction, and concludes to the falsity of at least one of the hypotheses.

This format of proof seems to be a convenient way for the human mind to reason. This convenience may derive from cultural convention: there further doesn't seem to be anything special about it. The use of a contradiction may enhance confusion by nonsense though.

For example, define a *squircle* as a shape in Euclidean space that is both square and circular. A theorem is that it cannot exist. If it is square then the distance to the center will differ for corners and other points, and this contradicts the property of being circular. If it is circular, then it cannot have right angles, and this contradicts the property of being square. Hence a squircle does not exist in Euclidean space. QED. This is a fine proof.

Now suppose that this proof is not known. Consider the theorem that squares cannot exist in Euclidean space. We use the definition of squircles. There is a lemma that any square associates with a squircle, e.g. the squircle with a circle with the same area as the square. The proof then is: Take a square, find its associated squircle, and deduce a contradiction as done above. Square implies falsehood. Ergo, squares don't exist. QED.

We know that squares exist in Euclidean space, so something must be wrong. To pinpoint where it goes wrong may be less clear. After careful study we may conclude that the proof uses the *existence* of squircles as a hidden assumption. The lemma is false. Once this is spelled out, it is rather clear for this example.

We will see that it appears to be a bit more complex for Cantor's conjectures.

The difference between two-valued and three-valued logic is relevant here.

Consider the proof that squares don't exist. Let p = "Squares exist" and q = "Squircles exist." We had the lemma that $p \Rightarrow q$. Subsequently we find $p \Rightarrow \neg p$. Trivially $\neg p \Rightarrow \neg p$. The TND is that $p \vee \neg p$. Hence in all cases $\neg p$, or that squares do not exist.

With three-valued logic we must allow that there can be nonsense. Thus $p \vee \neg p \vee \dagger p$. What about the path $\dagger p \Rightarrow \neg p$? If we want squares to truly exist, the implication $\dagger p \Rightarrow \neg p$ must be false, and then we couldn't use $p \vee \neg p \vee \dagger p$ to conclude that $\neg p$. According to the truth-table (ALOE:183) an implication from *nonsense* is only true if the consequence is *true* or again *nonsense*. It is false when the consequence is *false*. Thus the path $\dagger p \Rightarrow \neg p$ is blocked when $\neg p$ is false. However, since an implication is false if the antecedens is true and the consequence is false, we must allow that $\dagger p$ is true, or that squares are a nonsensical idea. When we compare $\dagger p$ and $\dagger q$, which makes most sense to us ? A problem is: the definition of squircles and the lemma $p \Rightarrow q$ start to make the notion of a square nonsensical itself too. This however indicates that it is more likely that $\dagger q$ than $\dagger p$. In this simple case it is clear that the lemma $p \Rightarrow q$ *is* false. We rather look for a proof that $q \Rightarrow \neg q$. While three-valued logic is more complex than two-valued logic, it has the advantage stated in the rule on page 15, that the rejection of one horn is no proof for the other horn.

Structure of this book

This book has two main papers as its core. Some other short papers have been included that give relevant support. The development of the idea of some *neoclassical approach in mathematics* has been gradual. At some point there was the need to take stock, and to wonder what it all amounted to. Thus, this author did not sit down and decide to develop a new paradigm by deliberation. By consequence, the structure of this book may be somewhat less organised than one might expect from design. The book uses the material that is available, and reorganises it. The short papers could be included as they are, with a bit editing. The two main papers had such a complex argument that they were cut up and dispersed over the various parts and chapters, i.e. Colignatus (2012, 2013) (CCPO-PCWA) and (2014b, 2015) (PV-RP-CDA-ZFC).

The book has been divided into parts. Parts 1 and 2 are within naive set theory. Parts 3 and 4 are within formal ZFC (see below). Parts 5 and 6 contain discussions and more observations on constructivism. Some readers might prefer to look first at Part 6 on the philosophical aspects. For most readers it will be useful to begin by defining abstraction.

An explanation for Wigner's "Unreasonable effectiveness of mathematics in the natural sciences"

A definition of abstraction.

January 9 2015 [3]

Scientists can be fans of magic but they would decline it within their professional work. Yet there is this curious proposition by Eugene Wigner (1902-1995) of some *"Unreasonable effectiveness of mathematics in the natural sciences*" that resorts to such magic – and which proposition finds mention without outright rejection by other writers, notably by Davis & Hersh (1983) on the mathematical experience, and recently in the book review by Burgess (2014) on a book by Hacking. The latter triggers this response.

Wigner (1960) states: "The first point is that the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and that there is no rational explanation for it." I disagree. The world is a wonder since we know so little about it but that is no reason to call it irrational. Yes, be amazed, but please do not turn this into magic. There is a very good explanation for the phenomenon indicated by Wigner.

My suggestion is that there is nothing "unreasonable" about the effectiveness of mathematics. When we regard mathematics as abstracting from the world, then the root lies in the world, and then it should not be surprising that the result may apply to the world. There is neither need for some Platonic view in which concepts 'exist' as ideas in some magical realm outside of physics, for we are merely speaking about abstraction.

Just to be sure: abstraction is defined as *leaving out other aspects.* Abstraction is nothing special but the mere ability of the brain to select some aspects of some mental model and drop (most) other aspects of it. That mental model will relate to empirical phenomena or sensations that the brain experiences.

I am neither mathematician nor physicist, but as econometrician I have some experience in the empirical sciences since 1982 and I got another degree in Leiden 2008 as teacher of mathematics. Let me point to the theory by Pierre van Hiele (1986) about the levels of insight in understanding mathematics. It appears to be commonly thought that Van Hiele would see those levels only applicable to geometry but he presented them as a general theory of knowledge, see page 100 and Colignatus (2014c). The Van Hiele theory explains that students operating at one level cannot imagine how things are at the other level. Students at the highest level can no longer imagine what they were struggling with in the past. This theory also partly explains why teaching math is rather incomprehensible to research mathematicians. The Van Hiele theory appears to be very relevant for the re-engineering of mathematics education, see COTP. [4]

While insight is relevant for education, for the present discussion it is more enlightening to regard those levels of insight as levels of abstraction. Application to Wigner's view generates "levels of unreasonableness" (in reversed order): which conveys the message that one should look at the issue in the proper perspective (bottom-up rather than top-down). When Wigner states "The complex numbers provide a particularly striking example for the foregoing. Certainly, nothing in our experience suggests the introduction of these quantities." then he presumes a paradise of simplicity of only one level, and he neglects the process in teaching, starting from the perception of a two dimensional plane and only *concluding* with the formal development.

[3] http://thomascool.eu/Papers/Math/2015-01-09-Explanation-Wigner.pdf
[4] Colignatus (2011c)

I also suppose that the evaluation of the relation between mathematics and physics should not be confused by interference by other topics of discussion. A supporter of Wigner might hold that this merely shifts the frontier of the "miracle", but my suggestion is that the answer has been given by abstraction, and that the following are really different topics.

(1) W.r.t. empirical modeling also involving human agents, we have different approaches: determinism, chance, volition. There is no experiment that will allow us to determine what is the right approach. We are forced to be pragmatic from the perspective of the objectives of the research, see ALOE:179. [5]

Human views may be confused by chance events with potentially hidden determinism. That someone wins the lottery twice might seem absurd but given the number of lotteries held around the world it becomes more understandable. Wigner's description of physicists struggling with their subject and mathematics leaves out all failures and potentially hidden determinism.

(2) We are in need for a biological theory of the mind - with good definitions for the mind in relation to the empirical brain. Generally mathematics is seen as mental activity, with the formula's on the blackboard only as a record for communication. A suggestion is that the mind may be defined as working with abstractions in general, created by processes in the brain, see page 109 (Colignatus (2011e)). Mathematical abstractions (deserving that name) are merely those perfected by tradition and professional development. A re-engineering of mathematics may be required if we want that studies on the brain are to be useful for math education. For example 2½ is supposed to be two-and-a-half but reads as two-times-a-half and thus better be coded as 2 + ½. See COTP again. An even better notation is $2 + 2^H$, with $x^H = 1 / x$, see **Appendix E** on fractions.

I am no physicist and thus cannot experience the wonder that Wigner apparently experiences in his examples. I only have a highschool understanding of $E = mc^2$, and e.g. when the abstract notion of space is already defined as Euclidean space then I cannot phantom why physicists think that they are free to redefine space (or what this would mean) merely to get rid of measurement errors, see COTP. Yet I presume that some aspects are the same in all empirical sciences and naturally in mathematics. Supporters of Wigner will agree anyhow that "unreasonable" likely isn't a physical concept. Thus there should be scope for agreement.

PM 1. Wigner (1960) refers to Galileo (1564-1642) doing an experiment on gravity with two objects dropped from the tower of Pisa. Viviani locates this event in 1589. The experiment was done before by Simon Stevin (1548-1620) on a tower in Delft around 1586. But is seems not to be in doubt that Galileo developed his gravity laws (*De Motu*), see Lendering (2014).

PM 2. It so happens that FQXi had an essay contest on Wigner's topic in Spring 2015. I did not know about it until the winner Sylvia Wenmackers was announced and made some headlines. I recommend Lee Smolin (2015) who presents an important new angle on the abused distinction between *discovery* and *invention.* [6]

[5] Colignatus (1981, 2007, 2011)

[6] http://fqxi.org/community/forum/category/31424: *"Trick or Truth: the Mysterious Connection Between Physics and Mathematics"*

Abstraction vs Eugene Wigner & Edward Frenkel

May 23 2015 [7]

Thinking depends upon abstraction. Let Isaac Newton observe an apple falling from a tree. The apple and the tree are concrete objects. The observation consists of processes in Newton's mind. The processes differ from the concrete objects and leave out a wide range of aspects. *This is the definition of abstraction: to leave out aspects.* Perhaps nature "thinks" by means of the concrete objects, but a mind necessarily must omit details and can only deal with such abstractions. For example, when Newton suddenly is hit by the idea of the universal law of gravity, then this still is an idea in his mind, and not the real gravity that the apple – and he himself – are subjected to.

Edward Frenkel's reference to Eugene Wigner

There is this quote by Edward Frenkel, "Love & Math", 2013, p 202, my emphasis:

> "The concepts that Yang and Mills used to describe forces of nature appeared in mathematics earlier because they were natural also within the paradigm of geometry that mathematicians were developing following the inner logic of the subject. This is a great example of what another Nobel Prize-winner, physicist Eugene Wigner, called the *"unreasonable effectiveness of mathematics in the natural sciences."* [ref] Though scientists have been exploiting this "effectiveness" for centuries, its roots are still poorly understood. ***Mathematical truths seem to exist objectively and independently of both the physical world and the human brain.*** There is no doubt that the links between the world of mathematical ideas, physical reality, and consciousness are profound and need to be further explored. (We will talk more about this in Chapter 18.)"

Hopefully you spot the confusion. Frenkel is an abstract thinking mathematician with some experience in science – e.g. with a patent – but apparently without having understood the philosophy of science. My weblog has already discussed some of his views, [8] especially his confusion about mathematics education while he hasn't studied the empirical science of didactics. It is a chilling horror to hear him lecture about how math should be taught and then see the audience listening in rapture because they think that his mathematical brilliance will certainly also generate truth in this domain.

Eugene Wigner's error is to forget that abstraction still is based upon reality. When reality consists of {*A, B, C, …, Z*} and you abstract from this reality by looking only at *A* and leaving out {*B, C, …., Z*} then it should not surprise you that *A* still applies to reality since it has been taken from there.

Mathematical ideas have a perfection that doesn't seem to exist in concrete form in reality. A circle is perfectly round in a manner that a machine likely cannot reproduce – and how would we check ? If the universe has limited size then it cannot contain a line, which is infinite in both directions. Both examples however are, or depend upon, abstractions from reality.

Since mathematics consists of abstractions, we should not be surprised when its concepts don't fully apply to reality, and neither should we be surprised when some applications do. That is, there is no surprise in terms of philosophy. In practice we can be

[7] https://boycottholland.wordpress.com/2015/05/23/abstraction-vs-eugene-wigner-edward-frenkel/
[8] https://boycottholland.wordpress.com/?s=Frenkel

surprised, but this is only because we are merely human. (Being merely human explains a lot.)

Note on abstraction

This issue on the definition and role of abstraction is developed with a bit more specification in this note, also in its relevance for mathematics education and our study of mind and brain: *An explanation for Wigner's "Unreasonable effectiveness of mathematics in the natural sciences", January* 9 2015. [9] (See page 18 above.)

A reader commented:

> "It seems to me that the question that Wigner is asking is "Why is mathematics so much more effective in *physics* (which is what he means by 'natural sciences') than in most other studies?" Physics textbooks are full of formulas; these comprise a large fraction of what the field is, and have great predictive power. Textbooks on invertebrate biology have few mathematical formulas, and they comprise only a small part of the field. Textbooks on comparative literature mostly have no formulas. So an answer to Wigner's question would have to say something about what it is about *physics specifically* that lends itself to mathematimization; merely appealing to the human desire for abstraction doesn't explain why physics is different from these other fields.
> I have no idea what an answer to Wigner's question could possibly look like. My feeling is that it is better viewed as an expression of wonderment than as an actual question that expects an answer." (Comment made anonymous, January 9 2015)

I don't agree with this comment. In my reading, Wigner really poses the fundamental philosophical question, and not a question about a difference in degree between physics and literature. The philosophical question is about the relation between abstraction and reality. And that question is answered by reminding about the definition of abstraction.

I can agree that physics seems to be more mathematical in degree than literature, i.e. when we adopt the common notions about mathematics. This obviously has to do with measurement. Use a lower arm's length, call this an "ell", [10] and proceed from there. Physics only has taken the lead – and thus has also the drawbacks of having a lead (Jan Romein's law [11]). Literature however also exists in the mind, and thus also depends upon abstractions. Over time these abstractions might be used for a new area of mathematics. Mathematics is the study of patterns. Patterns in literature would only be more complex than those in physics – and still so inaccessible that we call them 'subjective'.

PM. The above assumes that *A, ...,* Z exist without problem. The true invention might be to create those, in order to subsequently abstract from those to *A.* But this might also be a play with words, when the mind does not know what reality *really* is, and only creates a model anyhow – see page 109.

[9] http://thomascool.eu/Papers/Math/2015-01-09-Explanation-Wigner.pdf
[10] http://en.wikipedia.org/wiki/Ell
[11] http://en.wikipedia.org/wiki/Law_of_the_handicap_of_a_head_start

Abstraction & numerical succession versus 'mathematical induction'

May 26 2015 [12]

Abstraction has been defined in the preceding discussion. A convenient sequel concerns what is commonly called *'mathematical induction'.* This is an instance of abstraction.

Mathematical induction has a wrong name

Mathematical induction has a wrong name. It is a boy called Sue. It is czar Putin called president. There is no induction in 'mathematical induction'. The term is used to indicate that each natural number n has a next one, n+1. Thus for number 665 the mathematician induces 666: big surprise. And then 667 again, even a bigger surprise after 666 should be the end of the world. The second confusion is that the full name of *'proof by mathematical induction'* is often shortened to only 'mathematical induction': which obscures the distinction between *definition* and *method of proof*.

This method applies to the natural numbers. It actually is a *deduction* based upon the definition of the natural numbers. Since the natural numbers are created by numerical succession, a proper name for the method is ***proof by numerical succession.***

Let us define the natural numbers and then establish this particular method of proof. It is assumed that you are familiar with the decimal system so that we don't have to develop such definitions. It is also assumed that zero is a cardinal number. [13] (This book page 33.)

Definition of the natural numbers

A finite sequence of natural numbers is $\mathbb{N}[5] = \{0, 1, 2, 3, 4, 5\}$. Since we can imagine such sequences for any number, there arises the following distinction given by Aristotle. He called it the difference between *potential* and *actual infinity.*

(1) **Potential infinity**: $\mathbb{N}[n] = \{0, 1, 2, 3, …., n\}$. This reflects the human ability to count.

(1a) It uses the successor function ("+1″): $s[n] = n + 1$. For each n there is a n+1. The successor function is a primitive notion that cannot be defined. You get it or you don't get it. As a formula we can 'define' it by writing 'For each n there is a n+1′, but this is not really a definition but rather the establishment of a convention how to denote it.

(1b) Numerical succession might actually be limited to a finite number, say for a window of a small calculator that allows for 6 digits: $0 \leq n \leq 999{,}999$. The crux of $\mathbb{N}[n]$ however is that n can be chosen and re-chosen at will. For each $\mathbb{N}[n]$ we can choose a $\mathbb{N}[n+1]$.

(2) **Actual infinity**: $\mathbb{N} = \{0, 1, 2, 3, …\}$. This reflects the human ability to give a name to some totality. Here the name is *'the natural numbers'*.

Another formulation uses recursion: $\mathbb{N} = \{n \mid n = 0, \text{ or } (n - 1) \in \mathbb{N}\}$. Thus $1 \in \mathbb{N}$ because 0 is; $2 \in \mathbb{N}$ because 1 is; and so on. Thus, we now have defined *the natural numbers.*

The *potential* infinite deals with finite lists. Each list has a finite length. The distinctive property of these lists is that for each such number one can find a longer list. But they are all finite. It is an entirely different situation to shift to the *actual* infinite, in which there is a single list that contains *all* natural numbers.

There need be no doubt about the 'existence' of the natural numbers. The notion in our minds suffices. However, our mental image may also be a model for reality. If the universe

[12] https://boycottholland.wordpress.com/2015/05/26/abstraction-numerical-succession-versus-mathematical-induction/

[13] https://boycottholland.wordpress.com/2014/08/01/is-zero-an-ordinal-or-cardinal-number-q/

is finite, then it will not contain an infinite line, and there cannot be a calculator with a window of infinite length. But, on every yardstick in the range [0, 1] we have *all* 1^H, 2^H, 3^H, ... PM. We denote $n^H = 1 / n$, pronounced as *per-n,* see **Appendix E**, page 132. [14]

The relation between potential and actual infinity

The shift from ℕ[*n*] to ℕ is an instance of *abstraction*. ℕ[*n*] is a completed whole but with a need to build it, with a process of repetition. ℕ 'leaves out' that one is caught in some process of repetition, while there still is a completed whole. Let us use a separate symbol '@' for the particular kind or instance of abstraction that occurs in the shift from (1) to (2).

(3) **Relation** between potential and actual infinity: ℕ[*n*] @ ℕ. This *records* that (1) and (2) are related in their concepts and notations. In the potential form for each *n* there is a *n*+1. In the actual form there is a conceptual switch to some totality, caught in the label ℕ.

Since we already defined (1) and (2) to our satisfaction, (3) is entirely inferential and does not require an additional definition. It merely puts (1) and (2) next to each other, while the symbol '@' indicates the change in perspective from the potential to the actual infinite.

(There might be a link to the notion of 'taking a limit' but it is better to leave the word 'limit' to its well-defined uses and take '@' as capturing above instance of abstraction.)

Proof by numerical succession

The proof by numerical succession follows the definition of the natural numbers.

Definition: Let there be a property *P*[*n*] that depends upon natural number *n.* The property can be established – or become a theorem – for *all* natural numbers *n* ≥ *m*, by the following method of proof, called the *method by numerical succession*:

(i) show that *P*[*m*] holds,

(ii) show that *P*[*n*–1] ⇒ *P*[*n*].

(The proof is only valid when these two steps have been taken well of course.)

When *m* = 0 then the property holds for all natural numbers (if proven). The second step copies the definition of ℕ: If *n*–1 ∈ ℕ then *n* ∈ ℕ. If P[*n*–1] then it must be shown that *P*[*n*], if it is to hold that *P*[*n*] for all *n* ∈ ℕ.

An example below (under PM 1) uses the conventional step of going from *n* to *n*+1.

The definition of the method of proof doesn't state this explictly: In the background there always is (ℕ[*n*] @ ℕ) w.r.t. the fundamental distinction between the finite ℕ[*n*] and the infinite ℕ. Conceivably we could formulate a method for ℕ[*n*] separately which emphasizes the finitary view but there is no need for this here.

Aristotle

Judith V. Grabiner states: "Aristotle, arguing for the potentially infinite divisibility of the continuum, had explicitly ruled out both indivisibles and the actual infinite." [15]

I doubt this. Aristotle, Physics III, Part 6 gives: [16]

> "Further, a thing is infinite either by addition or by division. Now, as we have seen, magnitude is not actually infinite. But by division it is infinite. (There is no difficulty in

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[14] https://boycottholland.wordpress.com/2014/09/04/with-your-undivided-attention/

[15] http://www.ww.amc8.org/publications/maa-reviews/infinitesimal-how-a-dangerous-mathematical-theory-shaped-the-modern-world

[16] http://classics.mit.edu/Aristotle/physics.3.iii.html

refuting the theory of indivisible lines.) The alternative then remains that the infinite has a potential existence." and "Our definition then is as follows: A quantity is infinite if it is such that we can always take a part outside what has been already taken. On the other hand, what has nothing outside it is complete and whole."

The latter definition chooses the potential infinite, and this fits Grabiner's observation. However we can also observe the idea that the continuum is an actual infinite, as [0, 1] is a whole. My view is that Aristotle gave the distinction between potential and actual infinity.

Conclusions

(1) A prime instance of abstraction is the relation $\mathbb{N}[n]$ @ $\mathbb{N}$, i.e. the shift from the potential to the actual infinity of natural numbers.
(2) The method of *'proof by numerical succession'* is a deductive method based upon the definition of the natural numbers.
(3) *'Proof by numerical succession'* is a proper name, for what confusingly is called 'proof by mathematical induction'.
(4) Without further discussion: There is no *'unreasonable effectiveness'* in the creation of the infinity of the natural numbers and the method of proof by numerical succession, and thus neither in the application to the natural sciences, even when the natural sciences would only know about a finite number (say number of atoms in the universe).

PM 1. An example of a proof by numerical succession

We denote $n^H = 1 / n$, see the discussion on n^H in **Appendix E**.

Theorem: For all $n \in \mathbb{N}$: $1 + 2 + 3 + \ldots + n = n\,(n + 1)\,2^H$
Proof. By numerical succession:
(i) It is trivially true for $n = 0$. For $n = 1$: $1 = 1 * (1 + 1)\,2^H$. Use that $2\,2^H = 1$.
(ii) Assume that it is true for *n.* In this case the expression above holds, and we must prove that it holds for n+1. Substitution gives what must be proven:

$$1 + 2 + 3 + \ldots + n + (n + 1) \overset{?}{=} (n + 1)(n + 2)\,2^H$$

On the LHS we use the assumption that the theorem holds for *n* and we substitute:

$$n\,(n + 1)\,2^H + (n + 1) \overset{?}{=} (n + 1)(n + 2)\,2^H$$

Multiply by 2:

$$n\,(n + 1) + 2\,(n + 1) \overset{?}{=} (n + 1)(n + 2)$$

The latter equality can be established by either doing all multiplications or by collecting (n+1) on the left. Q.E.D.

PM 2. Background theory

Below we will say more about @.

PM 3. Rejection of alternative names

The name *'mathematical succession'* can be rejected since we are dealing with numbers while mathematics is wider. The name *'natural succession'* can be rejected since it doesn't refer to mathematics – consider for example the natural succession to Putin. The name *'succession for the natural numbers'* might also be considered but *'numerical succession'* is shorter and on the mark too.

Neoclassical mathematics for the schools

Abstract, September 6 & December 20 2011 [17] *- edited 2015*

National Parliaments around the world are advised to each have their own national parliamentary enquiry into the education in mathematics and into what is called 'mathematics'. Current mathematics education namely fails and causes extreme social costs. The failure can be traced to a deep rooted tradition and culture in mathematics itself. Mathematicians are trained for abstract theory but when they teach then they meet with real life pupils and students. Didactics requires a mindset that is sensitive to empirical observation which is not what mathematicians are basically trained for. The recent call by professor Wu to research mathematicians to start participating actively in the education enterprise (see the *AMS Notices* March 2011) calls for the wrong cavalry. We need engineers with an empirical set of mind rather than abstract academics. The mathematics required for schools likely can best be called *Neoclassical Mathematics* and is based upon the books *A Logic of Exceptions, Elegance with Substance* and *Conquest of the Plane* and now also this present book on the foundations of mathematics.

Introduction

If we want to improve the education in mathematics then we must consider the content, the education of teachers and the tools. Below gives an outline redefinition of the content into Neoclassical mathematics (NM). For the (re-) education of teachers we need the involvement not quite of research mathematicians but rather of the empirical sciences, since education is an empirical issue. The tools follow from these.

This point of view differs from the distinction by Hung-Hsi Wu (2011ab) into *Research mathematics* (RM), *School mathematics* (SM) and *Textbook SM* (TSM). Wu estimates roughly that TSM may contain an error every two pages. Teachers get TSM as basic education and RM at higher education, and never really arrive at some ideal SM. Wu (2011a) is a call for action directed at research mathematicians to co-operate with the teaching community to actually create that SM and its (re-) education of teachers. Wu's call does not mean that only research mathematicians can help out, since also the education community has a stake. However, it is not a call to empirical science.

The distinction between these two views concerns empirics. The education community is insufficiently empirical and research mathematicians may help but might also do damage. Mathematicians are trained for abstract thought but pupils happen to occur in real life. Teachers try to resolve their cognitive dissonance by relying on tradition, but traditional mathematical content is a nightmare. The ideal SM that Wu paints still suffers from the very same blindness to reality. Creating more consistency into a nightmare does not remove the very nightmare itself. This will be illustrated below with a discussion on fractions. A longer exposition and many more cases can be found in *Elegance with Substance* (EWS) and *Conquest of the Plane* (COTP). I admire professor Wu for his insights in and contribution to the education of mathematics. That even professor Wu falls into the trap of underestimating empirical science shows how difficult the subject is.

We can only hope that the issue gets the best of our possible attention and therefor I advise each nation to have an enquiry by its national parliament. Mathematicians should be the first in line to ask Parliaments to help them to carry the burden assigned to them of caring for the education in mathematics. Parliaments can be motivated by the properties of mathematics education: the costly investments in manpower and computer programs and equipment, as well as the level of education itself and the economic consequences.

The name "neoclassical mathematics" derives from the foundations of mathematics. We are familiar with the distinction between *logicism, formalism* and *constructivism* as those arose around 1900. Classical mathematics (CM) came into two major problems:

[17] http://thomascool.eu/Papers/Math/2011-09-06-NeoclassicalMathematics.pdf or EWS (2015)

(1) The 'division by zero' of the derivative created historically the approaches of (a) exhaustion by Antiphon and Eudoxos, (b) infinitesimals by Archimede, Newton and Leibniz, (c) algebra by Euler and Lagrange, (d) limits by Cauchy and Weierstraß.

(2) With the Liar paradox of the ancient world there came the paradox by Russell and the theorems by Gödel.

These issues (1) and (2) however are resolved by *A Logic of Exceptions* (ALOE), and see the review in the Dutch journal of mathematics NAW, written by professor Gill (2008) of the Dutch Royal Academy of Sciences. Hence it is possible to teach mathematics again in quite classical perspective, using 2000 years of didactic advance as well of course. Research mathematics might continue with the neglect of ALOE but keeping this neglect in research only would not put a burden on education (though possibly on financial markets and such).

An offspring of ALOE is *Conquest of the Plane* (COTP) with a favourable review by Gamboa (2011), at the website of the European Mathematical Society. The book *Elegance with Substance* (EWS) lies in time and purposes between ALOE and COTP, and got a mixed review by Limpens (2010), who is critical of some aspects but in sum appreciates the critical look at mathematics itself. A favourable review of EWS and COTP is again by Gill (2012). [18]

My suggestion is that the reform could be for K-12 but also the first year of college or university, but when the discussion takes place in the context of professor Wu's paper then it suffices to use the term "schools".

We first consider the fractions and then give an outline of the neoclassical approach.

Fractions, but also division in general

(a) First consider 2½ for *"two and a half",* where the position next to each other means addition. Secondly consider 2*a* for *"two times a"* or 2√2 for *"two times the square root of two"*, where the position next to each other means multiplication. Comparing these, the adjoining positions thus are interpreted differently, and pupils must be trained to see the difference. This also causes that we must make sure that there is a space inbetween 2 ½ when we want it to reduce to 1. This tradition of different interpretations of positions is curious, but it might be acceptable when we use typesetting with fixed places. The tradition however is asking for problems in handwriting when a pupil may write 2½ as 2 ½ or conversely, and thus slip into error. The solution is abolish the notation 2½ and to keep 2 + ½ so that the "+" nicely reflects the "and" in *"two and a half"* and so that the "+" may also be an end-station. This is similar to the case that √2 can be an end-station and need not be expanded into decimals 1.414... It takes a huge amount of time to train pupils now to write 2 + ½ as 2½ (and not reduce this to 1), and later again to unlearn this positional approach for 2*a*. The only reason for this waste of time is tradition for tradition's sake.

Updating on 2011: see **Appendix E** with the suggestion how fractions as we know them can be abolished. Observe that the pronunciation of fractions also abuses rank order names: traditionally 1 / 5 is pronounced "a fifth", which means that fractions borrow from a different concept (ranking: 1^{st}, 2^{nd}, 3^{rd}, ...) and that it is better to pronounce 1 / 5 = 5^H as "per 5".) [19]

(b) EWS and COTP both present a *proportion space* and defend the point of view of Pierre van Hiele that kids at elementary school would be able to work with vectors and thus a vector space. Proportion and vector spaces need an integrated discussion otherwise there arises confusion.

(c) Another point is that division essentially links up with the algebraic approach to the derivative. Since Cauchy and Weierstraß we have been trained to focus on numerical aspects but Weierstraß already uses predicate logic and it appears that algebra and the logic of the manipulation of the domain create the derivative just as well. Even better, since this eliminates the paradox of 'division by zero' and it avoids the educational combi-load of

[18] For completeness, let me mention a slandering text that is no real review, see http://thomascool.eu/Papers/COTP/LOWI/Index.html

[19] https://boycottholland.wordpress.com/2014/08/25/confusing-math-in-elementary-school/

both limits and the derivative. Limits and infinitesimals are useful, e.g. for the understanding of real numbers and approximations, but not necessarily for the derivative of functions used in K-12 (and likely wider). See COTP and this book (based upon CCPO-PCWA). Hence, a good understanding of division is not only required to survive 3rd grade but also the derivative.

These insights (a), (b) and (c) are missing in Wu (2011c). It merely illustrates the importance of the empirical approach to education, and may cause the reader to look at the other cases mentioned in EWS and developed in COTP.

An outline

Neoclassical mathematics has no precise definition yet but uses ALOE, with an application to education in EWS. The latter is implemented again in COTP. Now there is this present book. Neoclassical mathematics gives a point of view that Aristotle and Euclid supposedly could live with, and that people might find rather natural to understand. Some points are:

(1) The Liar and Gödeliar statements are nonsensical, in a three-valued logic.

(2) Russell's set paradox and Cantor's Conjecture for infinite sets are nonsense too. We may use a *set of all sets*. There are no 'transfinites'.

For example, Russell's set is $R = \{x \mid x \notin x\}$. This definition can be diagnosed as self-contradictory, whence it is decided that the concept is nonsensical. Using a three-valued logic, the definition is still allowed, i.e. not excluded by a *Theory of Types* (that makes it non-sensical too). Statements using it receive a truthvalue *nonsense* or *Indeterminate*. An example of a set similar to Russell's set but without contradiction is $S = \{x \mid x \notin x \wedge x \in S\}$ which definition uses a small consistency condition, taken from Paul of Venice, see ALOE:127-129. (See page 80 below for a discussion on infinite regress.)

(3) Euclidean space is defined as our notion of space. Non-Euclidean space can only be imagined in Euclidean space.

(4) The natural numbers $\mathbb{N}$ = {0, 1, 2, ...} are denumerable, with a potential infinite while their 'total' is an actual infinite. The continuum $\mathbb{R}$ or the interval [0, 1] is also an actual infinite. There is a *bijection by abstraction* between $\mathbb{N}$ and $\mathbb{R}$ such that these are 'equally large'. See this book (edited from CCPO-PCWA).

(5) Probability and statistics in relation to the *sorites paradox*. (Not developed.)

(6) Mathematics (abstraction) and engineering (approximation to reality) are discussed in conjunction, to foster sensitivity to the translations. For example, measurement errors due to the constancy of the speed of light do not mean a distortion of space but remain measurement errors.

(7) An encyclopedia of mathematics, e.g. what might result if some assumptions are changed. For example fuzzy logic, the Brouwer-Heyting axioms, incompleteness, computability, transfinites, fractals, chaos theory ...

(8) Democracy is a key concept but generally misrepresented by mathematicians, see my book *Voting Theory for Democracy* (VTFD). Mathematician Kenneth Arrow claimed that reasonable and morally desirable properties caused an inconsistency, and hence that what we ideally expect from democracy would be impossible. This however appears to be unwarranted. See Colignatus (2011h) how some mathematicians are still locked in denial of reasonable analysis on democracy.

A table

Table 1 gives an overview of the differences between traditional and the proposed neoclassical mathematics. The table lists my books in which the points are discussed. This also identifies what this present book (FMNAI) emphasizes (i.e. infinity).

Table 1. Comparison of traditional and neoclassical mathematics

	*Traditional mathematics (*TM)	*Neoclassical mathematics* (NM)
1	Two-valued logic. What is nonsensical is excluded by restrictions on form	Three-valued logic. Closer to linguistic freedom. What is nonsensical is explicitly called nonsensical (ALOE)
2	Gödel's theorems on undecidability	Under some stronger properties of the proof predicate the Gödeliar sentence causes a contradiction so that it can be judged to be as nonsensical as the Liar statement. There remains a similar kind of philosophy: that mathematical activity by mankind has the fundamental uncertainty that some inconsistency may pop up (ALOE)
3	Zermelo Fraenkel axioms of set theory, also to deal with Russell's paradox	Self-reference is allowed, and nonsensical cases like Russell's paradox are recognised for what they are. ZFC is inconsistent (FMNAI)
4	Cantor's Conjecture on the power set	The conjecture holds for finite sets but not for infinite sets. The diagonal argument appears to be nonsense (FMNAI)
5	Difference between denumerable and non-denumerable infinity. There are transfinites	Potential infinity associates with counting, actual infinity associates with the continuum. There is a bijection by abstraction between natural and real numbers. There are no transfinites (FMNAI)
6	Weierstraß for the derivative of regular functions (i.e. used in highschool)	Algebraic definition of derivative and integral for such functions. Limits are useful but not for the derivative. (Possibly Weierstraß for other functions.) (ALOE, COTP)
7	What is 'space' depends upon axioms	Euclidean space is defined as our notion of space. Non-Euclidean space can only be imagined in Euclidean space (COTP)
8	Arrow's Theorem shows that ideal democracy is impossible	A key property of the ideal of democracy is that it should work. Hence one of Arrow's axioms has to be rejected. This appears to be the axiom of pairwise decision making (VTFD)
9	Mathematics education for highschool and first year of college requires training on traditional concepts	Mathematics education requires a fundamental re-engineering. Much of mathematical content will remain the same but there are key gains in consistency and didactics (EWS, COTP, FMNAI).

Conclusion

If neoclassical mathematics as indicated above is adopted as school mathematics then professor Wu probably still might be happy that there at least is a SM, and undoubtedly many kids would be happy too. The choices involved will be clear. When research mathematicians (RM) drop the nonsense and look more into engineering with sound standards, and when the empirical sciences look into the education in mathematics, then there will be more cause for hope for improvement. Since so much is at stake and since professionals entertain standards that currently cannot be met without sizeable investments, and since individuals should not try to do the impossible, it is advisable that the national Parliaments investigate the issue.

Part 2. Pro Occam Contra Cantor

William of Ockham (Occam)

William of Ockham (c. 1287-1347) [20] – henceforth *Occam* – is known for his philosophy of *nominalism*, that holds that *words are just words*, so that words need not refer to things that really exist. He opposed *realism*, that assumes that words refer to something. In mathematics, *platonism* is that mathematical notions exist in some realm, and are discovered rather than invented.

Mankind is a story-telling breed, and people may prefer a story – with a storyline, engaging persons, drama, enticement – more than a sober reckoning of reality. Perhaps science only became succesful when scientists discovered that they had to tell better stories than the sorcerers.

Occam's *razor* is a criterion to get rid of superfluous words, and confusing notions that they would cause. The razor requires *necessity.* When you don't really need something, why bother ?

Apparently it is historically unclear how Occam formulated his razor, but the common formulation also seems the strongest:

Entia non sunt multiplicanda sine necessitate.

Things are not to be multiplied without necessity.

Razors are to be used with caution. Dictators might well desire to get rid of words like *freedom* and *democracy,* since these words would be confusing too, and apparently are not necessary, since everything is fine as long as you do – and think – as your local dictator desires.

Thus it may well be that Occam's introduction of his razor didn't really solve anything. For us it may be a mere didactic tool: to help us focus on the points – or rather the sharp edges – that we should *really think about*.

We now think about numbers and infinity. As holds for evolutionary biology where we tend to forget what *'deep time'* is, we may forget for the natural numbers what infinity really means.

- The googol is 10^100.
- Let *g*[*n*] = *n*^...^*n* with *n* times ^ (dropping brackets). For example *g*[2] = 2^(2^2) = 16. My version of *Mathematica* already generates an overflow for *g*[3] = 3^(3^(3^3)).
- Try *g*[googol].
- Apply *g* a googol times to itself, as in *g*[... *g*[googol]...].

These are just small numbers compared to what is possible. While this concerns the natural numbers and their infinity, Georg Cantor (1845-1918) [21] argued that the real numbers had a different kind of infinity, much greater. Would it really be true that the natural numbers are not enough to take stock of the real numbers ? If it is necessary then we must accept that idea. Otherwise it would be called philosophy.

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[20] https://en.wikipedia.org/wiki/William_of_Ockham
[21] https://en.wikipedia.org/wiki/Georg_Cantor

Notation, injection, surjection, bijection

When two sets *X* and *Y* are equally large then we denote this as $X \sim Y$. Then we say that the two sets are equivalent.

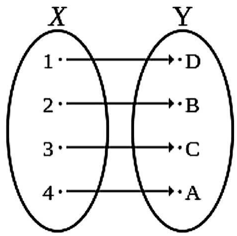
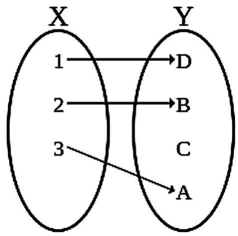
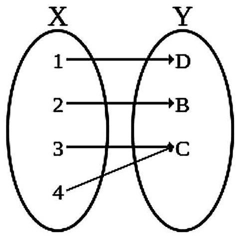
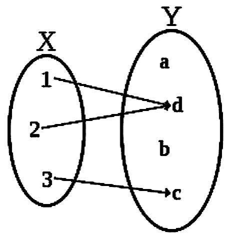
A *bijection* is a *one-to-one relationship* or map. If a merry-go-round has as many seats as children then it is possible to match each seat to a single kid and to match each kid to a single seat. The map explains where everyone sits. A bijection avoids empty seats (or one kid having more seats) and kids who cannot find a seat (or have to sit together).

A bijection thus provides for a proper definition for the notion of 'equally large' sets.

A bijection relies upon these underlying notions, see also Table 2:

- *X* is called the domain and *Y* is called the range.
- A function $f : X \to Y$ maps elements from the domain to elements in the range.
- For elements *x* in *X* and *y* in *Y* we can write $f[x] = y$ when the definition of *f* applies.
- We are using functions and not correspondences. For a function, if $f[x]$ has a value, then this is a unique single value. A correspondence $c[x]$ might have more values. An example of a correspondence is in plane geometry, where the vertical line at $x = a$ provides for multiple values of *y.*
- A function is *injective* iff for all *a* and *b* in *X*: $f[a] = f[b] \Rightarrow a = b$.
- A function is *surjective* iff for all *y* in *Y* there is a *x* in *X* such that $f[x] = y$.
- A function is *bijective* iff it is both injective and surjective.
- A bijection *f* has an inverse function, f^{-1}: $Y \to X$, that is also a bijection.

Table 2. A bijection iff both injective and surjective [22]

$f : X \to Y$	*Surjective*	*Non-surjective*
Injective		
Non-injective		

[22] The graphs and the idea of this table format are by Schapel in Wikimedia Commons, https://en.wikipedia.org/wiki/Bijection,_injection_and_surjection

Is zero an ordinal or cardinal number ?

August 1 2014 [23]

Introduction

Peter Harremoës (2011) [24] wondered whether zero is a natural number. He starts out by admitting that it is a matter of definition, but then proceeds with the issue of ordinal and cardinal numbers, so perhaps I should rephrase his true question as I did now in my own title above. A google on *"Is zero a natural number ?"* and *"Is 0 a natural number ?"* generates some 15,000 hits. A bit to my amazement there are more people pondering the question – though close to 0.0% of mankind in statistical approximation (in writing and not reading).

Harremoës approaches the issue from a set-theoretic point of view, though visits Kindergarten with some nice observations. My focus is didactics, and thus I will begin with Kindergarten.

Kids learn the sequence 𝕤 = {1, 2, 3, … } – the lower case 𝕤 refers to the successor function – and tally off their fingers. When they count the elements of a set *A* (apples), they bring the elements in *A* in a one-to-one relation with the elements in 𝕤, and also in that order {1, 2, 3, … }. The last element counted gives the total number of elements in *A.*

Ordinal and cardinal numbers

For terminology: an ordered sequence gives an *ordinal measure*, while the total gives the *cardinal measure* for the number of elements in a set.

The kids learn also the sequence 𝕆 = {1st, 2nd, 3rd, ….}. Now this is interesting ! It is rather this list 𝕆 that gives the ordinals ! Rather than saying *"This is apple one, this is apple two, this is …."* they may say *"This is the first apple, this is the second apple, this is …".*

When counting elements in a set *A* then it does not matter in which order the elements are put – and the cardinal number has the property that it has the same value in whatever order its elements are put. But, for 𝕆*,* the order must follow the order of the elements of the set *A* that is being considered.

Thus, kids first learn ordinals and cardinals in a mixed manner with 𝕤. Then the ordinals are created separately in 𝕆. Then 𝕤 becomes important for the cardinals. The introduction of 𝕆 requires a bit of *unlearning* alongside *learning.*

Set *A* is counted using ordered {1, 2, 3, …}	*Order in A is not relevant*	*Order in A is relevant*
Counting (process) *("Order some or all.")*	𝕤 = {1, 2, 3, ….} first training	𝕆 = {1st, 2nd, 3rd, …}
Cardinal (result) *("How many elements ?")*	$\mathbb{N}^+$ = {1, 2, 3, ….}	$\mathbb{N}^+$ = {1, 2, 3, ….}

With this established, I think that I must object to the use of the idea that *"ordinal numbers"* and *"cardinal numbers"* would be separate sorts of numbers. We only have the numbers 𝕤*.* The "cardinal number of a set *A*" is just idiom to identify the number of elements, but this does not suggest that "cardinal number" is a specific kind of number (like rational number or complex number). It is less common to speak about the "ordinal number of an element". While *"What number are you in line ?"* indicates such ordering, still you are not a number. "Ordinal number" is not a special number but merely 𝕤 applied to ordering.

[23] https://boycottholland.wordpress.com/2014/08/01/is-zero-an-ordinal-or-cardinal-number-q
[24] http://www.harremoes.dk/Peter

Seen from abstraction - update 2015

The latter may be expressed such that we have ℕ ~ 𝕆 ~ 𝕊 with the following relations:

- 𝕊 is a pure sequence. It abstracts from position and size. If element *a* is given then the list gives the instruction that *b* follows.
- 𝕆 has the notion of position (cumulation of position but not of size). Being the 6^{th} means that 5 have passed (or will come). (There are addition and subtraction.)
- ℕ has the notion of size (full cumulation). For example 6 is twice 3. (There are also multiplication and division.) (Group theory distinguishes *plus* and *times*.)

Answer

Thus *"Is zero an ordinal or cardinal number ?"* is a nonsensical question. Zero is just a natural number. Zero can be the value of the cardinal number of a set. Whether you start counting with 0 is an issue of convenience, and probably not practical in Kindergarten (but this needs testing). The true issue at hand is not quite arithmetic but actually part of the theory of measurement, with nominal, ordinal, interval and ratio scales (see below).

Some remaining comments for context

In etymology we can find a curious connection of counting with *speech* itself. To *give an account or reckoning* may consist of a story or a list of numbers: "Origin of tale: [25] (Webster) Middle English ; from Old English *talu,* speech, number, akin to German *zahl,* number, Dutch *taal,* speech ; from Indo-European base an unverified form *del-,* to aim, reckon, trick from source Classical Greek *dolos,* Classical Latin *dolus,* guile, artifice". In Dutch there is the subtle distinction between "tellen" (to count) and "vertellen" (to tell). Dutch has different words for number (*"getal",* as in the list of natural numbers, or the pure decimal system, old-English "tale") and cardinal number ("aantal", the number of elements, English "tally"). Teaching this in Dutch is a bit easier than in English.

Let us consider kids in Syria, Israel, Gaza or Ukraine who have ℕ[10] = {0, 1, 2, 3, …, 10} fingers left. Since the places of these fingers on their hands or the way how these fingers are ordered doesn't matter, the natural point of view is that the numbers are cardinal values ("how many left ?"). There also arises a natural order that one set is larger than another, (but 0 < 1 < 2 < 3 … < 10 might also be ordinal).

The Egyptians already had a symbol for "none". They didn't regard it as a number though. However once you set up a system of arithmetic then it becomes convenient to regard 0 as a number, so that 1 – 1 = 0. Slowly language adapts to that use. Given the naturalness of the question for kids to ask *"How many fingers do I have left ?",* thus with the focus on cardinality, and given that the answer may also be "zero", it is more reasonable to include 0 in the list of numbers that are regarded as natural, giving ℕ = {0, 1, 2, … }.

When you have a list of elements, it is not so practical to start the labeling with 0, since the rank numbers might become adjectives that differ from the proper ranks. For example, Pierre van Hiele (1909-2010), in his masterly exposition on didactics, [26] labeled his levels of understanding by starting at a base or zero level, counting on to level 4. In terms of rank, the first level would be level 0. However, the tendency would be to associated "level 3″ with "the third level", with "third" the adjective of "three". It appears difficult to suppress that tendency. Hence it is better to start lists with label 1. (In inverted manner, the calendar has no year 0 but it has a first year – and a zero year would seem to be useful.)

However, when you know that a set is non-empty, then you can use $\mathbb{N}^+$ = 𝕊 for the positive integers. Hopefully the little ones still have a thumb to suck on, to fall asleep, and be ready for another day of counting.

[25] http://www.yourdictionary.com/tale
[26] http://en.wikipedia.org/wiki/Van_Hiele_model

The distinction between counting (how many) and measuring (how much) and the supposed distinction between denumerable and non-denumerable infinity

Introduction

The following distinction can be accepted without problem:

- The natural numbers $\mathbb{N} = \{0, 1, 2, 3, \ldots\}$ reflect *counting* and a *denumerable* infinity.
- The real numbers $\mathbb{R} = \{x \mid x$ can be represented as a decimal number$\}$ reflect *measuring* and also give an infinity: a question is what this latter infinity is.

The *Key Question* is: Is that latter infinity also denumerable or is it non-denumerable ? This can be stated as the question whether $\mathbb{N} \sim \mathbb{R}$, or, in the definition given earlier, whether there exists a bijection between these sets.

Constructivism and abstraction

The distinction between *counting* (how many) and *measuring* (how much) is basic. Above short introduction defines it and should be clear. Counting takes discrete lumps. Measuring is for continuous phenomena. For length we may use a measuring rod and mark the appropriate length at any particular place.

It may still be the same infinity that only looks differently.

An analogy is this: take a cup of tea and a sugar cube, and call this Situation $\mathbb{N}$. Now put the cube in the tea, watch it dissolve, and call this Situation $\mathbb{R}$. There are still as many sugar molecules in the tea as were in the cube before, but we certainly lost track of where they are, except for: *in the tea*. If we call Situation $\mathbb{N}$ *denumerable* then Situation $\mathbb{R}$ is not the same, and might be called *non*-denumerable, but given that there are as many molecules as before, we many still presume the existence of some bijection. The only thing that happened is that we lost clarity of which is where.

There are at least two approaches to the Key Question:

- Constructivism tends to require the formulation of a bijection f such that for each n and r we can calculate $f[n] = r$ or the inverse value $n = f^{-1}[r]$.
- Constructivism *with abstraction* allows that we can determine by abstraction that such a bijection must exist, even though we cannot actually give a construction like required normally.

The *bijection by abstraction* contains the notion that the human mind applies abstraction to create such a bijection between $\mathbb{N}$ and $\mathbb{R}$. We can denote $\mathbb{N} \sim \mathbb{R}$ to express that the sets are 'equally large' (though ordered differently). Since one can hardly object to a definition, the prime goal of this approach succeeds by itself. The only possible objection to a definition is that it is vacuous and has no application. A strong version of this rejection is that the definition is inconsistent and has no application by necessity.

When one has a bijection then one has a multiplicity of bijections. The question arises which one to use. How to order the alphabet ? There might be no *natural* bijection between any two equivalent sets. Thus, the question about a possible bijection between $\mathbb{N}$ and $\mathbb{R}$ might well be academic, without much relevance for the practice of counting or measuring. Saying that $\Theta = 2\pi$ would be the 1001^{th} number would be irrelevant if someone else would hold it to be the 3rd number, and so on, while we already have a decimal expansion $\Theta = 2\pi = 6.28...$ that people tend to agree upon. The only reason why the Key Question might be interesting are the *transfinites,* see below. Before we consider those, let us first consider the levels of measurement.

Levels of measurement

Within the theory of measurement there is this distinction given by S.S. Stevens: [27]

- **Nominal** scale: names, classifications. For example the names of different languages. Statistics can give the frequencies of occurrence.
- **Ordinal** scale: a rank order, but the difference between the ranks has no meaning other than the difference in rank. The objects can be sorted in 1st, 2nd, 3rd, and so on. Statistics can meaningfully determine a *median* observation. This is $\mathbb{O}[n]$ for some *n,* see above.
- **Interval** scale: differences have meaning, but not the ratio of measured points. The difference between 10 °C and 20 °C is the same as the difference between 40 °C and 50 °C, but 20 °C is not "twice as hot" as 10 °C. An important model with an interval scale is the Georg Rasch model for competence in reading. [28] It was found independently by Arpad Elo for the Elo-rating in chess. [29] It now is generally called the Item-Response model. When students do a test, the test measures their competence, but, also, the students measure the difficulty of the test. If you are more competent than the test then you grow bored. If you are less competent then you grow frustrated. The ideal test gives a *flow*. [30]
- **Ratio** scale: ratios of measured points have meaning. "The measurement is the estimation of the ratio between a magnitude of a continuous quantity and a *unit magnitude* of the same kind" (quote from Mitchell in wikipedia). This is actually $\mathbb{N}$ for ***discrete*** data and $\mathbb{R}$ for ***continuous*** data.

In psychological constructivism one may observe that measurement tends to reduce to the use of a discrete grid since instruments or sense organs may never capture infinite accuracy. The point however is that repeated measurements can generate different values on the same phenomenon, so that the set of real numbers has been developed to capture that very notion of the infinite accuracy of the underlying model for reality. The ancient Greeks used a theory of proportions to deal with geometric lengths but in the subsequent two millennia mathematicians have developed the real numbers to better handle these phenomena.

For our purposes it suffices to mention that the discussion might be embedded in this structure of measurement. We will not actually try this embedding at this very moment. It suffices here that we consider $\mathbb{N}$ and $\mathbb{R}$. The reader might object that we could have said so before mentioning the theory of measurement. However, above identification of different types of scales really is useful, since our purpose is not abstract mathematics but an analysis that relates to scientific practice and understanding in the classroom.

George Cantor, the transfinites and Occam's razor

Georg Cantor (1845-1918) [31] developed the *transfinites,* that do not occur in above levels of measurement. They are entirely a product of mathematical imagination.

[27] https://en.wikipedia.org/wiki/Level_of_measurement
[28] https://en.wikipedia.org/wiki/Rasch_model
[29] https://en.wikipedia.org/wiki/Elo_rating_system
[30] https://en.wikipedia.org/wiki/Mihaly_Csikszentmihalyi
[31] https://en.wikipedia.org/wiki/Georg_Cantor

To this day, no-one has presented a model about some phenomenon in empirical reality that can be explained by the transfinites. There is a body of mathematical research after Cantor that has contributed to modern society, but those results do not depend upon Cantor's transfinites.

What are those transfinites ? Cantor suggested that $\mathbb{R} = 2^{\mathbb{N}}$, and that beyond $\mathbb{R}$ there are $2^{\mathbb{R}}$, and so on. If we accept that the natural numbers give the *infinite*, then there are various *transfinites* beyond that. Cantor's suggestion:

- The denumerable infinite of $\mathbb{N}$ is denoted as $\aleph_0$ (Hebrew *A,* "aleph null")
- The "non-denumerable" infinite of $\mathbb{R}$ is denoted as $\aleph_1$ ("aleph one")
- And so on.

One of the mathematical research questions is whether there are intermediate stages. The *continuum hypothesis* is that $\aleph_1 = 2^{\aleph_0}$, i.e. there is nothing inbetween. [32] (I regard this phrase as an abuse of the notion of the *continuum.*)

A fine introduction into such traditional logic is by Howard DeLong (1971), who also explains about the transfinites. There are numerous discussions on the internet, but frequently less comprehensible than DeLong's book. Popular science books are by Aczel (2000) and Wallace (2003) but be sure to read the review of the latter by Harris (2008). A critique is by Brady & Rush (2008) but I would have to think more on this.

Applying Occam's razor to Cantor's transfinites causes the question: are we really forced to accept those, or are they created by a mere act of volition ?

Cantor was deeply aware of this question. He had to convince himself of course, and subsequently also his sceptical mathematical colleagues. Thus he provided two proofs, one in 1874 and another one in 1890/91, see Hart (2015) but see also **Appendix B**. [33]

Around that time mathematical logic and set theory were still under development. Bertrand Russell (1872-1970) [34] produced his famous paradox, and subsequently the *Theory of Types* to block it. Russell also reformulated Cantor's proof of 1890/91 into what became known as *Cantor's Theorem on the Power Set*, which has become the bread and butter of every course in set theory. We will refer to it as *Cantor's Conjecture.*

The set theoretic conjecture is a generalisation, and stands by itself. Thus there are at least three proofs for Cantor's transfinites: 1874, 1890/91 and the set-theoretic version.

Taking stock

Collecting these points, also including some other information, we find:

- It is good practice in mathematics to maintain Occam's razor.
- There would be at least three proofs for Cantor's transfinites.
- It must be observed also that Russell's version of Cantor's conjecture clearly relates to Russell's paradox.
- There is a *bijection by abstraction* with $\mathbb{N} \sim \mathbb{R}$, that contradicts Cantor's transfinites.
- There is no empirical application for the transfinites (after more than 140 years).
- Traditional logic and set theory are still rather chaotic, and ALOE with its three-valued logic allows a consistent approach to nonsense, like the Liar paradox, the Gödeliar, Russell's paradox, and now also Cantor's conjecture.
- There is a breach in scientific integrity since 1980 on this, see Colignatus (2015e).

[32] http://mathworld.wolfram.com/ContinuumHypothesis.html
[33] Originally Colignatus (2015c)
[34] https://en.wikipedia.org/wiki/Bertrand_Russell

Overview of various Cantorian conjectures and refutations of their proofs

We will refer to various forms of *Cantor's Conjecture,* also known as the *diagonal argument*, see Table 3.

Table 3. Various Cantorian conjectures and their refutations

Author & date	*Conjectures, page numbers in this book*	*Refutation*
Cantor 1874	Reals are non-denumerable, via intervals, p 51	CCPO-PCWA
Cantor 1890/91	Diagonal argument, binary, bijection, p 47	CCPO 2007j
Russell 1907	Power set, using bijection ("common"), p 75	ALOE
Coplakova et al. 2011	Power set, using surjection ("standard"), p 138	PV-RP-CDA-ZFC
Hart 2012	Weakest form that underlies the argument, p 64	The latter too

PM. Wikipedia (a portal, no source) uses "Cantor's Theorem".

There is an issue on terminology w.r.t. the term *refutation* in the table. Observe that the table gives refutations of *proofs* and not of the *conjectures* themselves. There might be other proofs, unless a counterexample has been presented.

A theorem or conjecture is generally refuted by a counterexample. For the natural numbers and the reals we can find $\mathbb{N} \sim \mathbb{R}$, via the notion of *bijection by abstraction*, below. This uses constructive methods that some might not agree with, and it uses abstraction that some constructivists might not agree with.

Perhaps a better term for the RHS in the table might be *deproven*, in the sense that the theorem is stripped from its proof and no longer can count as a theorem and only survives as a conjecture. It may be that it would still hold, but via a different proof.

The refutation of Cantor's Conjecture or the diagonal argument is done by showing that the proof relies on logically improper constructs so that the proof can be rejected as invalid.

Saying that *'the proof is rejected'* would be too simple because in this realm of discussion – axiomatics – this might suggest that it is a mere act of volition to reject one of the axioms.

One might say that the proof is 'invalidated' but this seems uncommon. A proper phrase is that *'it is refuted that the 'proof' would be valid'.* The latter becomes short: *'the proof is refuted'.*

Below we will look at ZFC, as the set-theoretic axiomatic system that *Cantor's Conjecture on the Power Set* relies upon. A major refutation is that ZFC is inconsistent.

The latter form of the conjecture was actually devised by Russell in 1907. Cantor himself published his own versions before the ZFC system came about in the 1920s. [35] Thus there is value in refuting Cantor's original proofs as well. When a new axiomatic system is developed, it would be advisable to prevent the re-occurrence of the transfinites.

Hart (2015) gives a review in Dutch of the various forms of Cantor's Conjecture. See **Appendix B** for a critical rejection of his presentation. Hart also provided the weakest form of the Conjecture. This form will play a key role in this book for its very useful properties. A major feature is that it obscures the internal contradiction – useful for us to show the confusion. Its refutation can be found in **Appendix C**. Since we can derive that ZFC is inconsistent, this refutation has lost pride of place and has been put in that appendix.

[35] https://en.wikipedia.org/wiki/Zermelo%E2%80%93Fraenkel_set_theory

Steps in constructivism and bijection by abstraction

This section will repeat some of what we discussed above (page 22) and reformulate this as *steps (degrees) in constructivism.* The label S_i will be used. A main advice is to avoid the term *level* since this is already put to use in perhaps already too many different meanings.

1. A place for abstraction in constructivism

The author thinks that abstraction is an activity of the human mind that can sometimes be seen as proper constructivist.

For example, the natural numbers are figments of abstraction and don't occur in empirical reality (in the standard sense, non-platonic). Brain research suggests that at least the first numbers are hard-wired in the brain, but are these proper representations of the notion of 'number' ? While our thoughts might be equated with abstraction itself perhaps those hard-wired 'numbers' are more computer-like. Results of abstract thought can be put into computer programs that might not fully copy that abstraction – since computers cannot think (yet) – but they reproduce it to good effect.

Abstraction is the elimination of other aspects.

An example of dealing with abstraction is the ability to give a name or label to the infinite set of natural numbers without actually counting all of them, $\mathbb{N} = \{0, 1, 2, ... \}$. This can be done in computer algebra systems and it is unclear how the mind does it though we can presume that it doesn't store all numbers. It is customary in math to use numerical succession but the latter is procedural and doesn't seem quite the same as human abstraction. With finite $\mathbb{N}[n] = \{0, 1, 2, ..., n\}$ then numerical succession is: that for each $\mathbb{N}[n]$ there is an $\mathbb{N}[n+1]$. The procedure uses n to create $n+1$ and subsequently the set. Introspection suggests that this procedure differs from the mental act to grasp the whole $\mathbb{N}$. Perhaps one type of computer program can only count in actual numbers (printed on a paper trail), with only different instances and a specific value per instance, but another type of computer algebra system might use the symbol $\mathbb{N}$ as a representation for all natural numbers (with the associated algebra to make it work). While the specification $\mathbb{N} = \{0, 1, 2, ... \}$ suggests that we are hard-pressed to understand what infinity could be as a completed whole, the list $\{1, 2^H, 3^H, 4^H, ... \}$ clarifies that we only need the interval [0, 1] (with 0 and 1 included) to grasp that completed whole.

Another example of dealing with abstraction is to work with the real numbers $\mathbb{R}$ also using a calculator even though the calculator represents such numbers only in finite form up to a certain depth of digits. For example, for $9^H = 0.111...$ the calculator screen may show only 8 digits, but on paper we can include the ellipsis (trailing dots) to indicate that the 1's continue, while a computer algebra system may formalise that and only display 0.111... but continue internally to work with 9^H till a final answer is required.

In my mind I might think of a circle and a computer might print the mere word "Circle[*r*]" (with radius *r*). Some authors might hold that thinking about a number or circle is platonic but others might agree that ontology is a different subject (since people may also dream about ghosts).

It seems that it suffices to hold that this kind of abstraction is precisely what we want to include in the constructivist view on mathematical activity. This concerns the use of symbols with properties that are described in procedures.

Let us design *steps* (or *degrees*) in constructivism S_1 to S_6. Agreement and disagreement on the following step-by-step approach will help to delineate what constructivism is, or what kind applies at a particular instance. Presumably a particular view is more efficient in terms of information processing in some cases than in others. (This holds *a fortiori* with respect to points of view on volition, determinism or randomness, where we also lack distinguishing experiments to decide what really is the case, but where we can develop models that result in different successes and failures.)

PM. Ludwig Wittgenstein (1889-1951) used the term 'language game' to indicate that individuals have their own understandings and negotiate meanings with one another. This approach might reduce language to a soup. Mathematics educator Pierre van Hiele (1909-2010) allowed for levels in understanding or abstraction, which notion seems to have merit of itself and does not merely derive from the stratified language game in class. With words that have a different meaning depending upon the level of (mathematical) understanding, a language contains at least four sublanguages relating to these levels. Our reference to the ordinal and ratio scales implies a reference to such levels, going from a child that is trying to master arithmetic to research mathematicians doing axiomatics.

2. Abstraction on natural numbers

Aristotle's distinction between the potential and the actual infinite is a superb common sense observation on the workings of the human mind. Elements of $\mathbb{N}$ and the notion of repetition or recursion allow us to develop the potential infinite. The actual infinite is developed (a) via abstraction, with associated 'naming' (while recursion is less crucial than the step of abstraction), and (b) the notion of continuity of space (rather than time as Brouwer does).

While we use the symbol $\mathbb{N}$ to denote the natural numbers, this not merely means that we can give a program to construct integer values consecutively but at the same moment our mind leaps to the idea of the completed whole (represented by the symbol $\mathbb{N}$ or the phrase "natural numbers"), even though the latter seems as much a figment of the imagination as the idea of an infinite line.

The notion of continuity for say the interval [0, 1] would be a close encounter with the actual infinite. In the same way it is OK to use the mathematical construct that the decimal expansion of $\Theta = 2\pi$ has an infinity of digits, which is apparently the conclusion when we use such decimals.

Recall what we wrote on page 23. Some observations are:

- The abstraction $\mathbb{N}[n]$ @ $\mathbb{N}$ that is implicit in the difference between $\mathbb{N}[n]$ and $\mathbb{N}$ can be interpreted as the change from *counting* to *measuring*. We might keep the notion that $\mathbb{N} \sim \mathbb{R}$, or that both are 'equally large', [36] but they are only ordered differently.
- The step from (1) *potential infinity* to (2) *actual infinity* may be too large and we can try to find intermediate steps. At a lower level of abstraction you can be blind to the larger implications and higher levels. At a higher level of abstraction you might think that this might be logically be included in the lower level though you didn't see it. (This is one link between philosophy and didactics.) Introducing steps is also tricky since it might create what isn't there.
- One approach is to distinguish numbers 0, 1, 2, ... from lists of numbers {0}, {0, 1}, {0, 1, 2}, ... that display a whole. Making 'lists of numbers' generates the notion of a 'whole' which might be absent from the numbers themselves. But in the intended interpretation the numbers are supposed to *count* something and thus include some notion of 'whole' anyway – see page 34. Perhaps the successor function might be used without the notion of a 'whole', but when numbers are used for counting as generally understood then the notion of cumulation is present.

We proceed as follows. Developing these steps is one of the key contributions of this book.

[36] CCPO-WIP (2011, superseded) used the terms 'limit' and 'bijection in the limit'. The mathematical notion of a limit might perhaps be used to express the leap from the potential to the actual, though the use and precise definition of that notion of a limit also appears to depend upon context, e.g. with a distinction between '*up to but not including'* and '*up to and including'*. To avoid confusion we use the notion of abstraction, as is implicit in the conceptual difference between $\mathbb{N}[n]$ and $\mathbb{N}$.

3. Steps (degrees) in construction with abstraction

We can recognise these steps (degrees) in constructivism with abstraction:

(S_1) $\mathbb{N}[0]$, $\mathbb{N}[1]$, ... for concrete numbers only. (They just 'are'. This might be seen as the platonic case, where there is no invention but discovery. In strict finitism there might even be a biggest number.)

(S_2) $\mathbb{N}[n] \Rightarrow \mathbb{N}[n+1]$. This would be an algorithm that generates the numbers consecutively. Given some n, it has the ability to calculate n+1 and include it in a list. There is no recognition of a variable n yet however. (This cannot be Aristotle's potential infinite. Though Aristotle didn't explicitly use the modern notion of a variable his reasoning anticipated it.) (This may be the successor function but without a variable.)

(S_3) $\mathbb{N}[n] = \{0, 1, 2, ..., n\}$ as an abstraction of S_2. The variable n is identified explicitly. (The neoclassical form of Aristotle's potential infinite.)

Numerical succession (*NS*) is at the level of S_3 because of the abstract use of the variable *n.* Namely, for predicate P the application of $P[n] \Rightarrow P[n+1]$ is numerical succession only if there is explicit understanding of the necessary link via *n.* A computer program that for each $P[n]$ subsequently prints $P[n+1]$ merely shows the execution of a mathematical proposition (S_2), but does not provide a proof that something would hold for all n (S_3) *.* (A student might continue to work at level S_2 before it dawns that this could potentially continue *ad infinitum*, S_3.)

(S_4) $\mathbb{N} = \{n \mid n = 0 \lor (n - 1) \in \mathbb{N}\}$. This uses the recursive procedure written as up to *n,* but any n still transforms into all *n.* This leaves out from S_3 that there is some *n,* but it still generates a completed whole. (The neoclassical form of Aristotle's actual infinite.)

As Kronecker is reported to have said *"God made the integers"* the subsequent question is: *"Really all of them ? He didn't forget a single one ?"* The crux in S_4 lies in the symbol $\mathbb{N}$ that captures the "all", and a consistent Kronecker thus would accept S_4. (But it seems that he wanted to remain in S_3.) *NS* is often understood to be relevant for this level. In that case it might be useful to speak about 'basic' *NS* for S_3 and 'full' *NS* for S_4.

(S_5) $\mathbb{N} = \{0, 1, 2, ...\}$. The reformulation of S_4 in the format of S_3 with an ellipsis, to emphasize the shift from finite n to a completed whole. (Compared to S_1, the dots now are used within the notation and not at the meta level. Compared to S_4 it is merely a matter of notation.Some students may have a problem to shift from the procedural S_4 to the more abstract S_5, but it shows mathematical maturity to see that the forms are equivalent.)

(S_6) The next step would leave the realm of constructivism. The intellectual movement towards constructivism might become so popular that all want to join up, also users of non-constructive methods. But a line may be drawn and this line will actually define constructivism. An example of a non-constructive method is Cantor's manipulation of the diagonal element, see below, where he assumes some positional number C so that there is a digit $d_{C,C}$ on the diagonal, but this number remains undefined, so that actually $C \to \infty$. Drawing the line here, allows us to express that S_5 with its abstraction still belongs to constructivism.

The distinctions between these S_i would be crucial if we would deal with inflexible minds who cannot get used to some degree of constructivism. For those who can use all degrees, the distinctions may seem somewhat arbitrary, because they will wonder: don't the simpler degrees invite the abstractions to the higher degrees ?

My suggestion is that S_1 and S_2 aren't relevant for mathematics (except for the engineering of calculators), and that Aristotle was right that the interesting question concerns the distinction between S_3 and S_4 (or the step S_5) (which also could apply to the engineering of computer algebra languages).

4. Definition of the real numbers

Definition of $\mathbb{R}$

Above we had $\mathbb{R}$ = {*x* | *x* can be represented as a decimal number}.

This is commonly regarded as somewhat imprecise. Regard the following paradox:

(a) 3^H = 0.333333...
(b) 3 * 0.333333... = 0.999999...
(c) 3 * 3^H = 1 = 1.00000...

In general we better take 0.999999... = 1.00000... and we better also adopt a definition that includes this.

Let us define the real numbers in a variant of Gowers (2003), leaving out some of his algebra. It suffices to look at the points in [0, 1] (and others could be found by x^H etcetera). Thus $\mathbb{R}$ is the set of numbers from 0 to 1 inclusive. A number between 0 and 1 is an infinite sequence of digits not ending with only 9's; if it ends with only 0's we call it terminating. Rather than defining $\mathbb{R}$ independently it is better to create it simultaneously with a map (bijection) with $\mathbb{N}$ to account for the otherwise hidden dependence.

A map between $\mathbb{N}$ and $\mathbb{R}$

Let *d* be the number of digits. For each *d* we have $\mathbb{R}[d]$:

For *d* = 1, we have 0.0, 0.1, 0.2, ..., 0.9, 1.0.

For *d* = 2, we have 0.00, 0.01, 0.02, ..., 0.09, 0.10, 0.11, 0.12, ..., 0.98, 0.99, 1.00.

For *d* = 3, we have 0.000, 0.001, ..., 1.000

Etcetera.

Values in $\mathbb{N}$ can be assigned to these, using this algorithm:

For *d* = 1 we assign numbers 0, ... , 10.
For *d* = 2 we find that 0 = 0.0 = 0.00 and thus we assign 11 to 0.01, 12 to 0.02, etcetera, skipping 0.10, 0.20, 0.30, ... since those have already been assigned.
Thus the rule is that an assignment of 0 does not require a new number from $\mathbb{N}$.
Thus for numbers with a finite number of digits *d* in $\mathbb{R}$ we associate a finite list of 1 + 10^d numbers in $\mathbb{N}$, or $\mathbb{N}[10^d]$.
Subsequently, $\mathbb{N}[10^d]$ @ $\mathbb{N}$.
The latter creates both $\mathbb{R}$ and a map between that $\mathbb{R}$ and $\mathbb{N}$.
Let us look into the latter map.

PM 1) There has been much discussion whether a point has a size or not, but when we understand it as a co-ordinate, then it will have no size.
PM 2) Mathematicians apparently tend to prefer to construct the real numbers in other ways. Above construction however helps in deconstructing Cantor's conjectures in didactic manner.

5. Definition of *bijection by abstraction*

The creation of $\mathbb{R}$ above simply defines away Cantor's problem. Crucially:

The state of paradox is turned into a definition.

The intention is to only *capture* what we have been doing in mathematics for ages. It is not intended to present something horribly new. The procedure above only describes what we have been doing, but what has not been described in these terms before. It is a new photograph but at higher resolution, and it allows to see where Cantor was too quick.

The created map is better not called merely a *'bijection'* but rather *'bijection by abstraction'.* A common bijection should allow us to identify the index of say 3^H = 0.333.... while we don't have this ability in actual infinity S_4 – and didn't have it in potential infinity S_3.

In an overview, the procedure thus is:

(1) Potential infinity: $\mathbb{N}[n]$ = {0, 1, 2, ..., n}
(2) Actual infinity: $\mathbb{N}$ = {0, 1, 2, ...}
(3) For all $n \in \mathbb{N}$, including variables $n \in \mathbb{N}$: $\mathbb{N}[n]$ @ $\mathbb{N}$
(4) The definition of $\mathbb{R}[d]$, and then the creation of $\mathbb{R}$ via ($\mathbb{N}[10^d]$ @ $\mathbb{N}$) ⇒ ($\mathbb{R}[d]$ @ $\mathbb{R}$). Check that indeed $\mathbb{R}$ arises: no holes.
(5) We construct the bijection $b[d]$: $\mathbb{N}[10^d] \leftrightarrow \mathbb{R}[d]$ for d a finite depth of digits.
(6) **Definition** of what it means to have a **bijection by abstraction** between domain D and range $\mathbb{R}$: this applies when these three properties are satisfied:
    (a) There are a function $f[d]$: $\mathbb{N} \to \mathbb{N}$ and a bijection $b[d]$: $D[f[d]] \leftrightarrow \mathbb{R}[d]$
    (b) ($D[d]$ @ D) or ($D[f[d]]$ @ D)
    (c) $\mathbb{R}[d]$ @ $\mathbb{R}$

    Bijection by abstraction can be denoted Б: $D \leftrightarrow \mathbb{R}$ or $D \sim \mathbb{R}$. In that sense D and $\mathbb{R}$ are equally large. When (6a) - (6c) are satisfied then this is also accepted as sufficient proof that there is a bijection b, at constructive level S_4 even though that b no longer needs to be constructive in S_3.

    Note that the function f also allows the use of binaries (0 and 1 only) and other formats.
(7) Then we get the scheme: on the left we use $\mathbb{N}[10^d]$ @ $\mathbb{N}$ and on the right simultaneously $\mathbb{R}[d]$ @ $\mathbb{R}$:

$b[d]$:	$\mathbb{N}[10^d]$	$\leftrightarrow$	$\mathbb{R}[d]$	
↓	@	↓	@	
?:	$\mathbb{N}$	??	$\mathbb{R}$	

(8) Filling in the question marks: *given its definition*, there is a 'bijection by abstraction' between $\mathbb{R}$ and $\mathbb{N}$.

Our construction apparently is valid for the creation of $\mathbb{R}$. Since we have a map with $\mathbb{N}$ for each value of d, we find ourselves forced to the conclusion that with the creation of $\mathbb{R}$ there is *simultaneously* the creation of a map between $\mathbb{R}$ and $\mathbb{N}$.

PM. In (4) for $m = 10^d$, $\mathbb{N}[m]$ @ $\mathbb{N}$ has the same portent as $\mathbb{N}[n]$ @ $\mathbb{N}$ in (3), and this has the same portent as $\mathbb{N}[d]$ @ $\mathbb{N}$. Thus ($\mathbb{N}[10^d]$ @ $\mathbb{N}$) ⇔ ($\mathbb{N}[d]$ @ $\mathbb{N}$). Thus we can also use ($\mathbb{N}[d]$ @ $\mathbb{N}$) ⇒ ($\mathbb{R}[d]$ @ $\mathbb{R}$). Currently (6) uses the function f to stay closer to what actually is done, but also the latter might be used.

6. The interpretation of 'bijection by abstraction'

The definition *captures* and *emphasizes* the wonder of ℕ ~ ℝ rather than *hiding* it. ℕ and ℝ are abstract notions that may be understood by a mental act by a conscious brain. Nobody has ever seen a fully listed print of these numbers and it is physically inconceivable that this will ever happen. The above steps seem to properly capture what steps in abstractions are taken to handle these notions.

There are the two observations on the 'to' and 'from' relations:

(a) From ℕ to ℝ. Above scheme allows for each particular element in ℕ to determine what number in ℝ is associated with it (and it will have a finite number of digits: these form a subset of the rational numbers).

(b) From ℝ to ℕ. The abstraction ℕ[*d*] @ ℕ appears to be vague and insufficiently constructive (in terms of S_3) to the effect we cannot pinpoint a particular value in ℕ associated with say 3^H or a truly random sequence. It is paradoxical that we can decode a value in ℕ to a particular number in ℝ but that we cannot specify an algorithm to decode from 3^H to a particular value in ℕ. The construction with ℕ[*d*] @ ℕ apparently introduces vagueness, even though we can infer that such a map *must have been created* since also ℝ has been created. See the analog of tea and sugar on page 35. (Perhaps it is this very vagueness that causes that we have to distinguish between ℕ and ℝ, and make the distinction between *counting* and *measuring*. Likely though the latter is the basic empirical observation, and we have modeling ℕ and ℝ, to fit that observation.)

This might also be summarised in this manner. Though the name ℕ suggests an actual infinite, and though the collection is an actual infinite, the natural numbers remain associated with *counting* and counting associates more strongly with the potential infinite. Whence ℝ associates much better with the actual infinite given by the totality of ℕ, which is the continuum, which is *measuring*.

If you look for something in a filing cabinet or encyclopedia, you might start with A, and step through all values, but it is smarter ('measuring') to jump to the appropriate first letter, etcetera.

An unrepenting constructivist (S_3) might want to see a 'constructive' bijection between ℕ and ℝ and might reject the vagueness of the 'bijection by abstraction'.

An eclectic and unrepenting Aristotelian (S_4) might be happy that both sets have the same 'cardinal number', namely infinity, and that there is no necessity for 'transfinites'.

7. Some observations

A fallacy of composition

When we consider a real number with an infinite string of digits, like 3^H or a truly random sequence, we employ the notion of the actual infinite. At the same time, in above definition and construction of ℝ we employ the potential infinite. When we combine these notions then we are at risk of making a *fallacy of composition.* We should not mix these notions.

When 3^H is still in the process of being built up as an element in ℝ, then we can ask for a value in ℕ[10^d], but it is not guaranteed that we can ask for the value in ℕ for 3^H once it has been 'completed' (a notion that is as well-defined as ℕ).

By abstraction we get ℝ, including 3^H, but this apparently also means that we resign constructive specifics.

Stating ℕ[*d*] @ ℕ means a *conceptual leap* or a *shift of perspective* from the potential to the actual infinite. Rather than counting 1, 2, 3, we shift to the set of natural numbers, ℕ (and the name ‘the natural numbers’ refers to that actual infinite). When we use that symbol then this does not mean that we actually *have* a full list of all the natural numbers. We only have the name. The shift in perspective is not per se ‘constructive’ in the sense of S_3 but can be accepted as ‘constructive’ in the sense of S_4.

A misunderstanding due to ‘replacement’

One might see the step from finite *d* to infinity as a mere replacement of *d* by the symbol ∞ (now not read as “undefined” but as “infinity”). This could be a form of algebra. Such mere replacement might be relevant for how our actual brains work. It might be relevant for didactics, something to suggest to some students who have difficulty understanding what is happening. However, at this point in the discussion there is no developed algebra on such methods, and the proper interpretation still is, only, the switch from the potential infinite to the actual infinite: which is a *conceptual leap* (and no simple substitution).

Properties of @

The symbol @ has been introduced for $\mathbb{N}$ and $\mathbb{R}$ specifically, and without claim for generality. Perhaps we can work towards some rules on it, such that we can assume those rules and some weaker property to arrive at the same outcome. This however is a tricky area.

(i) One reader argued:

(1) $\mathbb{N}[d]$ @ $\mathbb{N}$ means that for every $n \in \mathbb{N}$ there is an *m* (say *n*+1) such that for $d > m$ we have $n \in \mathbb{N}[d]$.

(2) Then $\mathbb{R}[d]$ @ $\mathbb{R}$ means that for every $r \in \mathbb{R}$ there is an *m* such that for $d > m$ we have $r \in \mathbb{R}[d]$.

(3) The latter however is not true. Trivially, 3^H has no finite number of digits.

(4) Hence the meaning of *a*[*d*] @ *a* differs for $\mathbb{N}$ and $\mathbb{R}$ and "thus is not well-defined".

End of the argument.

In reply: Above, the symbol @ is *not* presented in a general format *a*[*d*] @ *a*. Only the expressions $\mathbb{N}[d]$ @ $\mathbb{N}$ and $\mathbb{R}[d]$ @ $\mathbb{R}$ are defined separately, where it thus matters whether we look at $\mathbb{N}$ or $\mathbb{R}$. The observation by the reader thus is partly accurate since there is indeed no general definition given for *a*[*d*] @ *a,* but it is inaccurate since it wants to impose such a definition while it hasn’t been given.

(ii) One reader wondered whether the expression $\Theta[d]$ @ Θ (for $\Theta = 2\pi$) would be meaningful. It is doubtful whether there is any value in looking into this kind of question. Perhaps $\Theta[d]$ might be defined as the number with the first *d* digits of Θ, and then what ? There is no meaningful way in how abstraction might cause one to get from such a value to the full value of Θ. This is discussed in **Appendix F**.

(iii) Some potential algebraic properties in relation to subsection 5 step (5) might be:

((*A* @ *B*) $\wedge$ (*A* ~ *C*)) $\Rightarrow$ (*C* @ *B*)
applied to ($\mathbb{R}[d]$ @ $\mathbb{R}$) $\wedge$ ($\mathbb{R}[d] \sim \mathbb{N}[m]$ for some $m = 10^d$) $\Rightarrow$ ($\mathbb{N}[m]$ @ $\mathbb{R}$)

((*A* @ *B*) $\wedge$ (*A* @ *C*)) $\Rightarrow$ (*B* ~ *C*) applied to ($\mathbb{N}[m]$ @ $\mathbb{N}$) $\wedge$ ($\mathbb{N}[m]$ @ $\mathbb{R}$) $\Rightarrow$ ($\mathbb{N} \sim \mathbb{R}$)

8. Conclusions

The interpretation is:

(1) The decimals in [0, 1] can be constructed via a loop on *d*, the depth of decimals, and then assuming this for *denumerable* infinity. This is not radically novel. The distinction between potential and actual infinity is given by Aristotle, and everyone has been aware of amazement and a sense of wonder.

(2) Due to Cantor people have started thinking that the loop would require 'higher' infinity. Cantor's arguments however collapse: see the deconstructions below.

(3) The concept of *bijection by abstraction* helps to get our feet on the ground again. The potential infinite can be associated with counting and the actual infinite can be associated with measuring (the continuum). Two faces of the same infinity. Clarity restored.

(4) The clarity actually arises by taking the wonder of the relation between the natural numbers and the continuum as the **definition** of bijection via abstraction. (The amazement is that for each *d* we have 10^d decimal numbers but for $\mathbb{N}[d]$ @ $\mathbb{N}$ we lose identification.)

(5) To avoid confusion in discussion:

- $\mathbb{N}$ is 'denumerable infinite' in all approaches, also via abstraction.
- $\mathbb{R}$ is 'non-denumerable infinite' in Cantor's view, i.e. with identification lost.
- $\mathbb{R}$ is 'denumerable infinite by abstraction' according to the present discussion.
- For $\mathbb{N}$ we might drop the "via abstraction" but for $\mathbb{R}$ we might include it for clarity. We may also say that $\mathbb{R}$ is 'Cantor-non-denumerable infinite' for clarity.

(6) The construction works for two reasons: (a) We construct $\mathbb{R}$ from the decimal format. Somehow mathematicians have been keener on using other ways to create $\mathbb{R}$, which may have caused that they missed this particular development. (b) We do not use a notion of *limit* of $d \to \infty$, but we use the notion of *abstraction* that is implicit in $\mathbb{N}[n]$ @ $\mathbb{N}$. This abstraction already has been given while the notion of a limit with this particular purpose would require some more development, while it is not guaranteed that it would work.

(7) This discussion of how $\mathbb{R}$ is constructed, and that $\mathbb{N} \sim \mathbb{R}$, can also be held in secondary school, where pupils have to develop a number sense, except for perhaps some philosophical technicalities and use of language. The point of view deserves to be included in courses on set theory, also for math majors, since students ought to have a chance to occulate themselves against the transfinites.

Cantor's diagonal argument for the real numbers, 1890/91

1. Occam's razor

Let us first consider Cantor's 2nd argument from 1890/91 that the real numbers would be non-denumerable. Cantor used a binary representation, with values *m* and *w*. When these values are flipped then *m* becomes *w* and *w* becomes *m*. Compared to this necessity in outcome, the changing of values in the decimal case causes a feeling of arbitrariness. However, students are more used to decimals, and thus decimals are more didactic. [37]

Cantor is known for his plea of freedom in mathematics. This freedom holds for the posing of axioms and for the pursuing of lines of questioning. It is quite another thing to derive 'transfinites'. I wholeheartedly agree with Cantor's plea for freedom but mathematics turns to philosophy indeed if there is no *necessary* reason to distinguish different cardinalities for $\mathbb{N}$ and $\mathbb{R}$. See also Edwards (1988) and (2008). If there is no necessity, then Occam's razor applies. Let us see whether there is necessity.

2. Restatement

Cantor's diagonal argument on the non-denumerability of the reals $\mathbb{R}$ is presented in DeLong (1971:75, 83) and Wallace (2003:254). Let us restate it.

Conjecture. (R), (Cantor's Conjecture on $\mathbb{R}$) The real numbers are not denumerable.

Proof. (Cantor, 1890/91) If $\mathbb{R}$ is denumerable, then its numbers can be put into a list. It suffices to assume a bijection between $\mathbb{N}$ and [0, 1] that uses digits $d_{i,j}$:

$1 \sim 0.d_{1,1}d_{1,2}...$

$2 \sim 0.d_{2,1}d_{2,2}...$

... etcetera.

Using suffix *D* as a label and not as an index, the diagonal number is $n_D = 0.d_{1,1}d_{2,2}....$ taken from that list. We can define a real number that would not be in the list. For example, with index *C* ("Cantor"): $n_C = 0.n_{C,1}n_{C,2}....$, where $n_{C,j} = 2$ iff $d_{j,j} = 1$, and $n_{C,j} = 1$ iff $d_{j,j} \neq 1$. If the position in the list would be *C* then $n_{C,C} = d_{C,C}$ by definition of the list and $n_{C,C} \neq d_{C,C}$ by definition of n_C, which is a contradiction. Nevertheless, n_C would be a true real number and thus should be in the list somewhere. Hence there is no such list, and the real numbers are not denumerable. Q.E.D. PM. We can create an infinity of such points.

Conjectured Corollary. (2015) The definition of *bijection by abstraction* is nonsensical. It should create a list but clearly cannot.

I've seen the diagonal argument on $\mathbb{R}$ in 1980 and considered it at some length, and have done so now again. In 1980-2010 I still accepted it. My only excuse is that I did not teach the subject. With some more maturity I can better appreciate some constructivist views. One may observe that neither DeLong (1971) nor Wallace (2003) mentions those constructivist considerations on this proof. It would be better if those would be mentioned in summary statements since they better clarify what is at issue. Curiously though I have not found a direct counterargument yet, neither in papers by others on Kronecker, while Hodges (1998) tends to give only outlines. Thus the following is my own. [38]

[37] Hart (2015) – see **Appendix B** for some critique – mentions that the decimal form was presented by Young & Young (1906), who referred to it as "Cantor's second proof".

[38] Hodges (1998) discusses some "hopeless papers", apparently with arguments that he rejected – but apparently with non-constructivist arguments, see his email page 15 above.

3. An aspect of self-reference

Cantor's argument would be *logical.* The suggestion is that we wouldn't have to calculate *C* since for every ranking and mutation rule there would be a different *C.*

Still, the diagonal argument attracts attention since there seems to be something fishy about taking an element $d_{C,C}$ and redefine it to have another value than it already has. This aspect of self-reference would be clearer if we could pinpoint a value for *C.*

The reader is invited to look again at the paradox with the squircle, page 17. If we use *X* for which we don't have a proof of existence, then we may be in problem. Particularly so, when this *X* shows that (1) it cannot exist and (2) we continue using it for a proof (think of the Cheshire cat that leaves its grin).

4. $C \rightarrow \infty$ or $C = \infty$?

There is no algorithm to find the specific number *C* for the diagonal digit $d_{C,C}$. The reasoning is non-constructive in the sense that the number cannot be calculated. This might be clarified by writing $C \rightarrow \infty$ or $C = \infty$ so that we are discussing $n_{\infty,\infty}$ which may be recognized as rather awkward since the symbol ∞ generally stands for “undefined” as well.

5. A formalisation of the rejection

Let us make the above a bit more formal.

Rejection of conjecture R. Conjecture R is fallacious.

Proof. Let *p* = “There is a (well-defined) diagonal element $d_{C,C}$”.

Cantor concludes that there is no list and thus no diagonal. Thus he agrees with $\neg p$. His argument can be rephrased as the following scheme: $p \Rightarrow \neg p$, ergo $\neg p$.

However, his true deduction is $\neg p \Rightarrow (p \Rightarrow \neg p)$, which is an instance of $\neg p \Rightarrow (p \Rightarrow q)$, which tautology is known as *‘ex falso sequitur quodlibet’* – EFSQ: *from falsehood follows whatever one wishes*, see Table 4. For $q = \neg p$ look at rows 2 and 3.

For position *C* we have $n_{C,C} = d_{C,C}$ by definition of the list and $n_{C,C} \neq d_{C,C}$ by definition of n_C, which is a contradiction. Hence *C* not-well-defined, hence neither $d_{C,C}$. Q.E.D.

Table 4. Truthtable of *Ex falso sequitur quodlibet (EFSQ)*

$\neg p$	$\Rightarrow$	$(p$	$\Rightarrow$	$q)$
0	1	1	1	1
0	1	1	0	0
1	1	0	1	1
1	1	0	1	0

Another way to put it: Cantor implicitly uses that the diagonal element $d_{C,C}$ does not exist (for he suggests to give it a value) to prove its nonexistence. Hence it is also *‘petitio principii’* or begging the question, $\neg p \Rightarrow \neg p$. (This fits with EFSQ with $q = \neg p$.)

6. Discussion: the reasons why *C* is undefined

6.1. Fallacy of composition

Above diagonal argument suffers from the fallacy of composition. The list of numbers in $\mathbb{R}$ is created in the manner of a potential infinite but the diagonal proof suggests that they can be accessed as actual infinites. This rather confuses the argumentation, since it requires more of the bijection than the bijection by abstraction is capable of.

(1) Above, for each $\mathbb{R}[d]$ in the list we might try to take a diagonal but the numbers are not long enough. For d = 2 we already get stuck at 0.01. The mutated number becomes 0.12 and when we move up the list we find it.

(1a) The 'mutation rule' on the 'diagonal' stated in n_C is rather a *waiting rule* than a number creation rule. The numbers are in the list at some point, and do not have to be created anew. We only have to go from one value of d to another value of d to let the mutated number appear (up to the required value of d). And given the approach of abstraction this apparently also holds for completed $\mathbb{N}$ and $\mathbb{R}$.

(1b) Supposedly though we could extend the numbers with a sufficient length of zero's. Creating a new number based upon such a diagonal number would not be proper however since we are already creating $\mathbb{R}$ in another fashion. Such diagonal number conflicts with the situation defined for that particular value of *d.*

(2) Cantor's argument has this structure: "Suppose that there is a list, then there is a diagonal, then a new number is created that cannot be on the list. Hence there is no such list, hence real numbers are not denumerable."

But the bijection by abstraction showed that there *must be* a list, that comes about alongside with the creation of $\mathbb{R}$ itself. The alternative argument is rather: *Given the list and if we allow that such a notion of a diagonal is implied and well-defined, we apparently cannot find a value for C, whence such rules of creation like* n_C *are nonsense.*

This book comes close to generating a particular diagonal, via the bijection $b[d]$ and the step of abstraction. But that final step loses both identification and a value for C that Cantor wishes to use. When something cannot be identified then we should be cautious to use it. This is rather not an issue on the infinite but rather a point of logic.

(3) The unrepenting constructivist (S_3) who rejects the usefulness of the 'bijection by abstraction' and who wants to see a constructive bijection such that we can calculate the proper number for 3^H, would also stick to a constructive approach for the diagonal, which however is not what Cantor offers.

In other words, S_3 regards it as a fallacy to suggest that there would be a 'diagonal' n_D = $0.d_{1,1}d_{2,2}$.... Cantor's proof assumes a diagonal but rather that diagonal should be created. (While it is constructed, at the same time the mapped value n_C is created, and then it appears that it could *not* be created, since it is inconsistent that $n_{C,C} = d_{C,C}$ by definition of the list but $n_{C,C} \neq d_{C,C}$ by definition of n_C.)

(4) Readers who allow Cantor this freedom to be non-constructive on diagonal digit $d_{C,C}$ should also allow for the 'non-constructive' aspect in the 'bijection by abstraction' – namely that an index for 3^H must exist but cannot be identified. Of course, allowing Cantor his freedom would generate the contradiction of a list that cannot be a list.

Conversely, however: who allows for the 'non-constructive' aspect in the bijection by abstraction (S_4 instead of S_3) does not necessarily have to allow for the non-constructive Cantorian handling of that diagonal digit $d_{C,C}$ (with $C \to \infty$).

In $\mathbb{R}[d]$ @ $\mathbb{R}$ we allow that $\mathbb{R}$ is completed; and some readers object to this, because they interprete this as if $\mathbb{R}$ were constructed from the rational numbers (quod non: for all numbers are there). On the other hand, we criticize that C has no completed value. The difference is that we have a construction for $\mathbb{R}$ while there is no way to find C.

6.2. Compare with the squircle

On page 17 we pointed to the fallacious lemma: Each square has an associated squircle. In the same way Cantor's argument uses a fallacious lemma: Each diagonal has an associated mutated number. The fallacy obviously lies in the $n_{C,C}$ construction.

(1) It is too simple to say that $d_{C,C}$ must be a digit from 0,..., 9, and that each digit allows a redefinition. The latter only allows a conclusion that *if* $n_{C,C}$ *exists* then it has a value, but it does not allow a calculation of *C,* and thus we still have no $d_{C,C}$.

Indeed, once we have $d_{C,C}$ then we can define $n_{C,C}$, but we need $n_{C,C}$ to find *C*, to find $d_{C,C}$. The argument that $n_{C,C}$ only gets a *new* value is fallacious: since that assumes that there is a value $d_{C,C}$ to start with.

(2) Consider $n_C = 0.n_{C,1}n_{C,2}......n_{C,C}....$ and the 10 adjacent numbers. Use $C \pm k$ for some value of *k* such that all digits 0, ..., 9 in column *C* are covered. For 0 there will be a k_0, for 1 there will be a k_1, etcetera. For convenience we write *k* for all. For purity we also write *m*, so that if $k = 0$ then the $m_{i,j} = n_{i,j}$ and otherwise $m_{i,j} = d_{i,j}$. At least, if this is consistent.

$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 0)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 1)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 2)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 3)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 4)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 5)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 6)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 7)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 8)....$
$m_{C\pm k} = 0.m_{C\pm k,1}m_{C\pm k,2}......(m_{C\pm k,C} = 9)....$

Consider the two possibilities for unknown *C*:

Possibility (a) has $n_{C,C} = d_{C,C}$ by definition of the list. Other values are off-diagonal:
(a1) For $k = 0$ there would be a value $0 \leq m_{C\pm k\ ,C} = d_{C,C} \leq 9$.
(a2) For $k \neq 0$ the values in the column are different, with $0 \leq d_{C\pm k\ ,C} \neq d_{C,C} \leq 9$.

Possibility (b) goes inversely from $n_{C,C}$ and tries to locate where it came from:
(b1) For $k = 0$ there would be a value $0 \leq m_{C\pm k\ ,C} = n_{C,C} \leq 9$. But still $n_{C,C} = d_{C,C}$ by definition of the list.
(b2) For $k \neq 0$ there must be an original value with $0 \leq d_{C\pm k\ ,C} \neq n_{C,C} \leq 9$.

Other ways are self-contradictory – which however is what Cantor proposes. We can redefine a value but it means shifting around the diagonal – and not only on it.

(3) In the creation of $\mathbb{N}$ and $\mathbb{R}$ by abstraction, the diagonal element $d_{C,C}$ is not well-defined since the value of *C* remains vague. That this diagonal element $d_{C,C}$ is not well-defined does not prove that $\mathbb{R}$ is non-denumerable. When something is not well-defined then it is tempting to conclude that it doesn't exist, and then Cantorian reasoning $p \Rightarrow \neg p$ takes off. It is better to hold on to the notion that it is not-well-defined what $d_{C,C}$ would be.

As said in the *Introduction (*page 15)*:* There is a distinction between not-being-well-defined and non-existence. We can sensibly discuss the existence or non-existence of something when we know what we are speaking about. For well-defined topics we can accept the TND $p \vee \neg p$. But it may be that we are dealing with nonsense, †*p,* so that in general only $p \vee \neg p \vee \dagger p$. When a rhinoceros exists, we can say whether it is in the room or not. For squircles we may say that they don't exist but we actually mean to say that the notion isn't well-defined. We may say that elements $d_{j,j}$ exist but we cannot say that $d_{C,C}$ sensically exists (though it has meaning): since, what *C* is being used ?

Cantor's original argument of 1874

The original argument of 1874 suffers the same fallacy of composition. The formulation of the conjecture assumes $\mathbb{R}$, which is built up in the manner of a potential infinite, but the proof uses that all elements are actual infinites. Instead, a constructive proof can only use numbers up to a certain digital depth *d,* and create what Cantor proposes to do only alongside the creation of $\mathbb{R}$.

The argument of 1874 uses the notion of an interval. The interval [*a, b*] contains all *x* with $a \leq x \leq b$. The interval (*a, b*) contains all *x* with $a < x < b$. The interior of [*a, b*] is (*a, b*).

Conjecture on the continuum. (C), (Cantor 1874, Hart (2011) in Dutch, wikipedia [39]):

> Wenn eine nach irgendeinerm Gesetze gegebenen unendliche Reihe von einander verschiedener reeller Zahlgrößen
>
> $$\omega_1, \omega_2, \ldots, \omega_\nu, \ldots \tag{4}$$
>
> vorliegt, so läßt sich in jedem vorgegebenen Intervalle $(\alpha \ldots \beta)$ eine Zahl η (und folglich unendlich viele solcher Zahlen) bestimmen, welche in der Reihe (4) nicht vorkommt; dies sol nun bewiesen werden. *G. Cantor* [1874]

Translation: "Given any sequence of real numbers x_1, x_2, x_3, … and any interval [*a, b*], one can determine numbers in [*a, b*] that are not contained in the given sequence."

Reproduction - Edited Quote (Hart (2011), Wikipedia March 6 2012):

Proof. Build two sequences of real numbers as follows: Find the first two numbers of the given sequence x_1, x_2, x_3, … that belong to the interior of the interval [*a, b*]. Designate the lesser of these two numbers by a_1, and the greater by b_1. This gives interval [a_1, b_1]. Similarly, find the first two numbers of the given sequence belonging to the interior of the interval [a_1, b_1]. Designate the lesser by a_2 and the greater by b_2. Continuing this procedure generates a sequence of intervals [a_1, b_1], [a_2, b_2], … such that each interval in the sequence contains all succeeding intervals. This implies the sequence a_1, a_2, a_3, … is increasing, the sequence b_1, b_2, b_3, … is decreasing, and every member of the first sequence is less than every member of the second sequence.

The number of intervals generated is either finite or infinite.

If *finite,* let [a_N, b_N] be the last interval. Since at most one x_n can belong to the interior of [a_N, b_N], any number η belonging to this interior besides x_n is not contained in the given sequence.

If *infinite*, let $a_\infty = \lim(n \to \infty)\, a_n$. (**Remark**.)

Let $b_\infty = \lim(n \to \infty)\, b_n$ and break the proof into two cases: $a_\infty = b_\infty$ and $a_\infty < b_\infty$.

In the first case, $\eta = a_\infty = b_\infty$ is not contained in the given sequence. In the second case, any real number η in [a_∞, b_∞] is not contained in the given sequence. Q.E.D.

Remark. At this point, Cantor could finish his proof by noting that a_∞ is not contained in the given sequence, since for every *n*: a_∞ belongs to the interior of [a_n, b_n], but x_n does not. (Wikipedia footnote)

End Quote.

Quote: "Cantor does not explicitly prove [the non-denumerability of the real numbers], which follows easily from [Conjecture C]. To prove it, use proof by contradiction. Assume

[39] Proof checked on Hart (2011); https://en.wikipedia.org/wiki/Cantor%27s_first_uncountability_proof

that the interval [a, b] can be put into one-to-one correspondence with the set of positive integers, or equivalently: The real numbers in [a, b] can be written as a sequence in which each real number appears only once. Applying [Conjecture C] to this sequence and [a, b] produces a real number in [a, b] that does not belong to the sequence. This contradicts our original assumption, and proves the uncountability theorem." (wikipedia)

This approach however breaks down when Conjecture C is invalid.

Counter-example to (C). The proof of Conjecture C is fallacious.

Proof. Let us redo the 'proof' using $\mathbb{R}[1]$, ... , $\mathbb{R}[d]$. These numbers are ranked up to 10^d. Take *the news* $D[d] = \mathbb{R}[d] \setminus \mathbb{R}[d\text{-}1]$, and rank the numbers as $X[d] = D[1] \cup D[2] \cup ... \cup D[d] = \{x_1, x_2, ..., x_{10^d}\}$, where the union maintains order. Taking the interval from [a, b] generates [a_d, b_d]. Starting on [0, 1] then [a_1, b_1] = [0.1, 0.2], then [a_2, b_2] = [0.11, 0.12], then [a_3, b_3] = [0.111, 0.112] and so on. There will be a limit in 9^H = 0.1111.....

We take $\mathbb{R}[d]$ @ $\mathbb{R}$. Subsequently also $X[d]$ @ X. Clearly X is only a permutation of $\mathbb{R}$, and all numbers are in the list. It is erroneous to think that some number is missing. Q.E.D.

Discussion:

(1) Consider the possibility of a final interval [α, β]. The idea of $\eta \in [\alpha, \beta]$ but $\eta \notin \mathbb{R}$ is erroneous since all elements of $\mathbb{R}$ are present in X.

(2) We build $\mathbb{R}$ up in the manner of a potential infinite and then take a final step of abstraction. A constructive proof can only use numbers up to a certain digital depth d, and create the interval only alongside the construction of $\mathbb{R}$ itself. This $\mathbb{R}[d]$ case has no finite number of steps for the final interval. Taking the interior of [a_d, b_d] is possible since the numbers are defined such that $a_d \neq b_d$. The notion of an 'interior' may become nonsensical when we take the step of abstraction: e.g. taking the interior of [α, β] is impossible if $\alpha = \beta$.

The 'proof' to Conjecture C assumes that all elements have an actual infinity of digits. The 'proof' assumes that one can define the various notions on limit and $\mathbb{R}$ independently, but they get only meaning in their mutual dependence, and then must be constructed in a dependent manner.

(3) A potential possibility is that $\alpha = \beta$ and that this number is in the list. However, the algorithm does not allow us to take only a single value x and create an interval [x, x]. The logical conclusion is that there is no other result than: the creation of ever smaller intervals.

Taking limits a_∞ and b_∞ need not be relevant for these intervals. In this counter-example we might regard $a_\infty = b_\infty = 9^H$ = 0.111... but this value will never be actually selected to create the interval [9^H, 9^H] since the algorithm is to take two values and not a single one. One cannot say that 9^H isn't in the list because all elements of $\mathbb{R}$ are.

(4) The statement "$\eta = a_\infty = b_\infty$ is not contained in the given sequence" depends upon this footnote in wikipedia: "Cantor states (without proof) that x_n does not belong to the interior of [a_n, b_n]. To prove this, we use an inductive proof to prove the stronger result: x_1, x_2, ..., x_{2n} do not belong to the interior of [a_n, b_n]." [40] The latter statement and proof by numerical succession are correct. However, observe the two different statements:

- (a) x_n does not belong to the interior of [a_n, b_n] (if it is, then it is made a border)
- (b) $\eta = a_\infty = b_\infty$ is not contained in the sequence x_1, x_2, x_3, …

One cannot hold that (a) implies (b). While (a) is true, (b) can be false: since $\eta = a_\infty = b_\infty = 9^H$ = 0.111... is in the list of $\mathbb{R}$. (It is tricky to show this. An effort at counterexample is using the found sequence b_1, b_2, b_3, …and consider [η, b_n]. However, if we set $x_1 = a_1 = \eta = a_\infty = b_\infty = 9^H$ and look at [a_n, b_n] then the algorithm would generate a different sequence ... It should suffice to hold on to the basic notion that $\mathbb{R}[d]$ @ $\mathbb{R}$ generates the full list.)

---

[40] https://en.wikipedia.org/wiki/Cantor%27s_first_uncountability_proof#cite_note-15

The context of education

This analysis has been written in the context of education. This appears to cause misunderstandings amongst some readers, so it is useful to spend some attention to that context. This analysis is not about education itself but about how to handle the infinite and the mathematics of the infinite, such that there is more scope for proper treatment in education. This present analysis does not develop a course on the infinite, but identifies essential issues, and creates scope for the development of such a course.

An essential feature in education is that we do not want to overburden students, even though they may think to the contrary. If a textbook chapter would end with the statement *"We didn't tell you all yet, for there are still things too complex for you",* then students might feel lost and cheated. They quite understand that there are many things that they do not understand yet, but, the closing statement of a chapter should be about what they have learned. Then a test to check this, then a new chapter.

The steps S_1, ..., S_6 allow an educational ladder, in which there is an increasing grasp of counting, measuring, and the infinite. There is a cesure between constructive S_5 and non-constructive S_6.

S_5 is constructive, uses three-valued logic to eliminate Cantorian nonsense, and uses the label 'bijection by abstraction' to capture the paradox that we can identify a bijection in potential infinity but lose identification when we create actual infinity by the mental act of abstraction (and mapping onto the continuum).

S_6 is the standard mathematical realm, is non-constructive, uses two-valued logic to support the Cantorian figments, and might use the loss of identification as an argument that there would be 'different kinds of infinity'.

Below we will see that the ZFC system for set theory is inconsistent, so that S_6 is a bit different than expected at the time in 2012 when the steps S_1, ..., S_6 were designed. It still is conceivable that some research mathematicians develop a system ZFC-X such that the transfinites survive.

For S_5, 'bijection by abstraction' is just a term, and in a sense it is also 'no-bijection by abstraction' namely given the loss of identification. The didactic situation still is:

(a) Cantor's proofs of 1874 and 1890/91 have evaporated,
(b) we want to *grasp* $\mathbb{N} \sim \mathbb{R}$: describe what we *know* and *don't know* about infinity,
(c) we want closure, without the unsettling *"This is too complex for you now."*

Having $\mathbb{N} \sim \mathbb{R}$, or that there is only one kind of infinity, allows for simplicity, and creates room for the learning of the other elements in the discussion:

(i) the construction of $\mathbb{R}$,
(ii) properties such as 1 = 0.999...,
(iii) the distinction between counting and measuring,
(iv) the notion of bijection and 'equality of sets'. (Yes, set theory in the schools.)

A mathematician with a firm root in S_6 may be offended by S_5. I take the liberty to quote from an email and keep this anonymous (March 2013):

> "Your proposal is anti-scientific and thus anti-Occam's-spirit. You want to obscure a distinction that actually is important. Occam says: why a complex explanation when there is a simple one ? You propose to no longer *speak* about a distinction, but this distinction explains all kinds of issues. You propose to close your eyes, so that you don't see some phenomena: yes, then you don't need an explanation ! (....) You change a definition in order to remove an imagined conflict with a metaphysical

stance which you **want** to hold about mathematics, but the pay-off for mathematics is zero. It's obscurantism. Newspeak. Big Brother. Abolish certain words from the dictionary and certain "problems" dissappear because they can't be stated in words any more. In fact they were not problems at all, they were challenges, and they've been surmounted, and this has borne enormous fruit. (....) My impression is strengthened that you are building an elaborate construction in order to be able to tell lies to children. You're a neo-Pythagorean: mathematical truth has to be bent to conform to your world-view (in this case, your view of the sociology of mathematicians). I just don't see the point. I don't see a problem. I do see a major misapprehension: you seem to think that modern non-constructivist mathematics is an abstract game. There you are wrong. It helps real people to effectively and constructively solve real problems, e.g. in modern statistics, in modern quantum engineering. (...) We don't solve real problems by redefining the concept "countable" so that the real numbers are no longer "uncountable". (...) But who wants to discuss $d(C, C)$ with C = infinity? Only you, as far as I know."

The latter statements may have some weak points:

(a) "modern non-constructive mathematics" would "constructively solve real problems": constructivists will challenge the "constructiveness" of non-constructive methods,
(b) there is a blindness on $d[C, C]$ with C = infinity, which I propose should be lifted,
(c) my 'sociology of mathematicians' is that they confuse their abstract thoughts for reality: but this is not an axiom of me but a result from empirical observation, see (i) Table 1, (ii) **Appendix D**, and (iii) EWS (2009, 2015).

The key point is that this analysis has been written in the context of education. If S_6 teachers want to continue with the nonsense of the transfinites while there is no necessity for it then they are still free to do so, but they ought to *inform* their students that there is another way to look at it too, namely S_5.

The argument of openness of mind cuts two ways. In an educational ladder, S_5 is followed by S_6, for historical reasons, since it is useful to know what the illusions of Cantor have been, and how most mathematicians followed him. If there would be a result for the real world that relies on the transfinites, I am interested to hear. Conversely, a teacher of non-constructivist denomination would be required to explain the approach of S_5. Students ought to have a chance to be inocculated against nonsense, instead of being lured into it by fallacies.

Conclusions

Apart from the more mundane conclusion that it indeed appears feasible to set up a highschool or first year higher education course on infinity without the need to refer to the transfinites, the following conclusions are possible.

1. A summary of the differences

Table 5 collects the differences in views. It is assumed that the column on the right is correct so that erroneous claims in the middle are put in quotation marks.

Table 5. Differences between Cantorian and Occamite positions

Topic	*Cantor*	*ALOE, EWS, FMNAI (Occam)*
Logic	Two-valued	Three-valued
Cantor's Conjecture	Accept	Reject, like Russell's paradox
Potential & actual infinity	Commits a fallacy of composition	Proper distinction
Diagonal	"Assuming a diagonal causes its rejection"	Is only *d* deep in potential infinity and is implied in actual infinity
Mutation rule on diagonal	"Creates a new number"	A waiting rule for $\mathbb{R}[d]$, and a logical error for $\mathbb{R}$
Bijection	"Impossible to create"	By abstraction
Cardinality	"$\mathbb{N} < \mathbb{R}$"	$\mathbb{N} \sim \mathbb{R}$

In the RHS view the following statements mean precisely the same:

(i) the shift in perspective from potential infinity to actual infinity (the continuum)
(ii) the imagination of the continuous interval of [0, 1]
(iii) regarding this imagination as a constructive act (for geometry)
(iv) accepting this to be what we mean by a ‘bijection by abstraction’ between $\mathbb{N}$ and $\mathbb{R}$
(v) the specification in the steps above for the definition of ‘bijection by abstraction’.

2. Conclusion on the continuum

The unrepenting constructivist (S_3) has a strong position and might actually be right. On the other hand, below will show that ZFC in S_6 is inconsistent. The creation of some ZFC-*X* might still contain Cantor's Conjecture, perhaps even as a separate axiom. There might be theoretical advantages in assuming a continuity with a higher cardinality than the set of natural numbers – but this hasn't been shown yet.

Currently, the main reason to accept the diagonal argument and thus different cardinal numbers for $\mathbb{N}$ and $\mathbb{R}$ is rather not ‘mathematical’ but ‘philosophical’. Instead of getting entangled in logical knots we might also use Occam’s razor and assume the same cardinality. Above considerations on ‘bijection by abstraction’ would support the latter.

3. Conclusion on the foundations

The diagonal argument on the real numbers can be rejected (a new finding in 2011, restated above). There is a *bijection by abstraction* between $\mathbb{N}$ and $\mathbb{R}$.

(Below in the discussion on Set Theory, we will restate the refutation by ALOE 2007 of 'the' general proof of Cantor's Conjecture on the power set (while there is truth for finite sets but not for infinite sets). If no contradiction turns up then it would become feasible to use the notion of a *set of all sets* $\mathbb{S}$, as it would no longer be considered a contradiction that the power set of $\mathbb{S}$ would be an element and subset of $\mathbb{S}$ itself. These terms are explained in the part on Set Theory below.)

4. Conclusion on constructivism

The specification of the construction steps (degrees) S_1, ..., S_6 worked well in identifying the various mathematical and philosophical aspects in the various arguments. S_3 would be the potential infinite and S_4 the actual infinite, and the latter would still be constructive but with some abstraction. The two concepts of infinity would be two faces of the same coin. The confusions within non-constructive S_6, the transfinites, derive rather from logic than from infinity.

Part 3. Set theory: ZFC and propositional logic

From Epimenides to Paul of Venice to Cantor to Russell to ZFC

The relevance of freedom of definition

This book deals with self-reference and derives contradictions. It may thus be difficult to follow. As stated in the *Introduction:* We can maintain clarity by holding on to the notion of freedom of definition. When a restriction on that freedom generates a consistent framework, while release of that restriction generates confusion, then the restriction is to be preferred above too much freedom. This notion is particularly important for these next Parts 3, 4 and 5 on set theory.

Paul of Venice (1369-1429)

There is an old way to deal with self-reference. Bochenski (1956, 1970:250) discusses the approach by Paul of Venice (1369-1429) (Paulus Venetus) [41] w.r.t. the Liar paradox. A proposition may be extended with *"and this proposition is true".*

The Liar: *"This proposition is false"*

becomes: *"This proposition is false and this proposition is true",*

whence it shows itself to be inconsistent. An idea is to apply such a condition to the paradoxes in set theory.

Logic, set theory, and Cantor's Conjecture on the Power Set

Aristotle gave the first formalisation of the notions of *all*, *some* and *none*, of which an origin can be found in the Greek language. This developed into modern set theory, in which the notion of a *set* provides for the *all*.

There is a parallel between constants in propositional logic and set theory: ***and*** giving *intersection*, ***or*** giving *union,* ***implication*** giving *subset.*

Different axioms give different systems. A common contrast is between the formal ZFC system for set theory (from Zermelo, Fraenkel and the Axiom of Choice) and *naive set theory.* The latter is not quite defined, but think perhaps of Frege's system, and it is not to be confused with Halmos's verbal description of ZFC. There is a plethora – perhaps an infinity – of models for properties of sets.

Frege gave the first system, and Russell's problem was a blow to it. Researchers spoke about a crisis in the foundations of logic and mathematics. *Logicomix* by Doxiadis and Papadimitriou (2009) is a nice graphic display. The idea of a crisis was eventually put to rest by the ZFC system. A consequence of ZFC is the impossibility of a *set of all sets.*

Cantor will be remembered at least for starting set-theory. He developed his original conjectures on infinity and the transfinites before there was an axiomatic development of set theory – we might say: he worked within the intended interpretation. Bertrand Russell in 1907 reformulated Cantor's 1890/91 proof into a form that uses sets. This proof was renamed into *Cantor's Theorem on the Power Set.* (See below, page 75.) Once the ZFC system was developed, this "theorem" became the corner stone in every introduction in set theory. A drawback is that it causes the transfinites.

We will deproof this "theorem" and for us it is wiser to speak about a conjecture.

Set theory belongs to logic because of the notions of *all, some* and *none*, and it belongs to mathematics once we start counting and measuring. Cantor's Conjecture on the Power Set somewhat blurs that distinction since the general proof uses logical methods while it would also apply to infinity – and the latter notion applies to the set of natural numbers $\mathbb{N}$ = {0, 1, 2, ... } and the set of reals $\mathbb{R}$ (the continuum, actual infinity, the interval [0, 1]).

[41] https://en.wikipedia.org/wiki/Paul_of_Venice

The intended interpretation

When we create a formal and even axiomatic system then the original environment from which we started becomes the *intended interpretation.* For this book, the intended interpretation of set theory remains an important notion. Mathematicians might tend to forget about this once they have developed the formal system, but we don't.

For example, a formal development of addition 1 + 1 = 2 has the intended interpretation that one apple plus one apple gives two apples. An intended interpretation of a set is that it collects items. For example there can be the collection of these two apples, and the cardinality of that set is 2.

A problem is that the transfinites are no part of any intended interpretation. They came about from Cantor's reasoning. We have shown above that this reasoning was misguided. But Russell in 1907 created that Power Set "Theorem", that later was set into stone in ZFC. The transfinites are now a product of ZFC, and looking for an interpretation. To my knowledge no one has given an example of what they help solving in practice.

Three strategies on the transfinites

When we want to eliminate the confusion of the transfinites then there are these options:

(1) The *philosophical* approach is to reject ZFC and present a new system that does not allow for transfinites. Mathematicians will not easily go along with this. ZFC is the working horse for mathematics, and few are willing to learn to ride a new horse. Mathematicians have grown fond of the complexities of the transfinites too: "those show the wonders that mathematics can create".

(2) The *traditional mathematical* approach is to prove that ZFC is inconsistent. In this case a new system must be chosen by necessity. It puts the key question into focus: are the transfinites really necessary, or are they only a figment of an inconsistent system ? Above, in considering Cantor 1874 and 1890/91 we saw that part of his reasoning relied on self-contradictory self-reference. Is it possible that such also got set into mathematical stone in set theory ?

(3) The *neoclassical mathematical* approach allows for a third option. This is to perform an analysis within the intended interpretation – i.e. outside of formal ZFC and not necessarily within philosophy – and show that Cantor's conjecture generates nonsense. This gives an anomaly, and then may branch into (1) or (2). It depends upon how much mathematicians still value the intended interpretation whether they start with (2).

Our discussion about set theory is mostly within the intended interpretation and doesn't rely on an axiomatic base. If we arrive at some coherent view then it will be up to others to see whether they can create an axiomatic system that fits it.

However, in handling ZFC, we will work within ZFC, and show that it is inconsistent.

Parts 3, 4 and 5 on set theory

Our handling of set theory is split into Parts:

(i) This Part 3 gives basic notions of ZFC and looks at the singleton, i.e. a set with one element. Element α gives set $A = \{ \alpha \}$. Propositional logic gives a condition, that is used to show that ZFC is inconsistent. It appears to be a Paul of Venice condition.

(ii) Part 4 will look at ZFC more generally, and suggest alternative systems. The condition by Paul of Venice is formalised, also to design alternative axioms.

(iii) Part 5 gives a framework for discussion of these results.

This structure will help to focus on the different angles. Part 3 looks at logic and inconsistency, Part 4 looks at the whole system that we want to use. Part 5 returns to what we discussed above about the three strategies on mathematics and philosophy.

We now must set up ZFC and identify the basic notions.

Definition of ZFC

1. A syllabus at Leiden and Delft

We take our definitions from a matricola course in set theory at Leiden and Delft.

Definition of ZFC. [42]

Axioms that are directly relevant for us are:

***Axiom** of the Power Set.* [43] For each set *A* there is a set of all subsets of *A.* (POW)

Definition of the power set. The latter set of subsets is called the *power set* and is denoted as *P*[*A*]. Another notation is 2^A, whence its name.
Remark: For example, if *A* = {*a, b*} then *P*[*A*] = {Ø, {*a*}, {*b*}, *A*} in which Ø is the empty set.

***Axiom** of Regularity.* No formula is needed here. (REG)
Remark. REG implies that a set cannot be a member of itself.
Remark. Sets that satisfy ZFC are called ZFC-sets, or often just sets. Sets that do not satisfy ZFC can be called *entities,* or *classes,* or non-ZFC-sets. For example, the universe $\mathbb{U}$ and the set of all sets $\mathbb{S}$ would not be ZFC-sets. They are blocked by REG.

***Axiom** of Extensionality*. [44] Two sets are identical when they have the same elements:

$$(A = B) \Leftrightarrow (\forall x)\ (x \in A \ \Leftrightarrow x \in B) \qquad \text{(EXT)}$$

***Axiom** of Separation*, [45] (inserting here a by-line on freedom): If *A* is a set and $\gamma[x]$ is a formula with variable *x,* while *B* is not free in $\gamma[x]$, then there exists a set *B* that consists of the elements of *A* that satisfy $\gamma[x]$:

$$(\forall A)\ (\exists B)\ (\forall x)\ (x \in B \Leftrightarrow ((x \in A) \wedge \gamma[x])) \qquad \text{(SEP)}$$

Remark. This is also called an axiom-schema since there is no quantifier on γ.

2. Structure of the discussion in this Part

The discussion in this Part below has this structure:

- Above system of axioms appears to be inconsistent. The axioms are still too lax on the notion of a *well-defined set*.
- A step in developing this proof is the observation that ZFC would be deductively incomplete if it were consistent. This means that there is a truth that cannot be derived.
- Both points are proven by means of the singleton.

To start, let us first look at Russell's paradox and how ZFC blocks it, and then at the paradoxical properties of the Cantorian sets.

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[42] Coplakova et al. (2011:144-145)
[43] Coplakova et al. (2011:18), I.4.7
[44] Coplakova et al. (2011:145)
[45] Coplakova et al. (2011:145). Some authors call it by its German name: *Aussonderung.* Weisstein (2015) of MathWorld calls it the *Axiom of Subsets,* see page 95 below.

Russell's paradox and how ZFC blocks it

Russell's paradox

Bertrand Russell devised his paradox after studying Cantor. In naive set theory, Russell's set is $R = \{x \mid x \notin x\}$. His own example are the teaspoons. The set of teaspoons is not a teaspoon itself. However, applying R to itself gives $(R \in R) \Leftrightarrow (R \notin R)$ and naive set theory collapses.

The definition of R can be diagnosed as self-contradictory, whence it is decided that the concept is nonsensical. Russell himself proposed a *Theory of Types* to eliminate the paradox. Set's shouldn't refer to themselves but only to sets of a lower type.

Observe: (1) a theory of types forbids the set of all sets $\mathbb{S}$ while it is a useful concept, (2) a theory of types has R in the category *'may not be formed'* and thus implies a third category next to truth and falsehood. It would be illogical to reject that third category. It is logical instead to generalize that third category into the general notion of 'nonsense'. Thus we do well to formally develop a three-valued logic that allows both the definition of R and also to determine that R is nonsense. (It has meaning, that allows us to see that it is nonsense.) It remains an issue that three-valued logic is not without its paradoxes, but ALOE holds that these can be solved too.

ZFC is a partial application of the Theory of Types. It blocks Russell's paradox but it doesn't block Cantor's transfinites. The *Axiom of Regularity* (REG) already forbids $R \in R$. When we want a set of all sets $\mathbb{S}$ then we would abolish REG. Then Russell's paradox would still be blocked by Separation. Let us look into this.

Truthtable

For ZFC, take the axiom of separation, substitute Russell's idea that the set of teaspoons is not a teaspoon, denote that result as $R = R[A]$, then test it.

$$(\forall A)\ (\exists B)\ (\forall x)\ (x \in B \Leftrightarrow ((x \in A) \wedge \gamma[x])) \qquad \text{(SEP)}$$

$$(\forall A)\ (\forall x)\ (x \in R \Leftrightarrow ((x \in A) \wedge (x \notin x)) \qquad \text{(RUS)}$$

$$(\forall A)\ \ (R \in R \Leftrightarrow ((R \in A) \wedge (R \notin R)) \qquad \text{(test } R\text{)}$$

The truthtable allows only for $R \notin A \wedge R \notin R$.

$R \in R$	$\Leftrightarrow$	$(R \in A$	$\wedge$	$R \notin R)$
1	0	1	0	0
1	0	0	0	0
0	0	1	1	1
0	1	0	0	1

Thus the definition of RUS itself is not blocked. RUS means that $R = R[A]$ can exist as a subset of A, i.e. $(R \subseteq A)$. Thus $R \in P[A]$.

Let us play with this a bit, and see whether $\rho = P[A]$ also has a Russell set.
The Russell set for ρ is different now. Applying Separation to it:

$$(\forall x)\ (x \in R[\rho] \Leftrightarrow ((x \in \rho) \wedge (x \notin x)) \qquad \text{(RUS on } \rho = P[A])$$

For $R = R[A]$: $R \in \rho$ and $R \notin R$, thus we find $R \in R[\rho]$ without contradiction.
When $R[\rho]$ is applied to itself again:

$$(R[\rho] \in R[\rho] \Leftrightarrow ((R[\rho] \in \rho) \wedge (R[\rho] \notin R[\rho])) \qquad (R[\rho] \text{ with } \rho = P[A])$$

The truthtable generates: $(R[\rho] \notin \rho) \wedge (R[\rho] \notin R[\rho])$. Thus $R[\rho] \subseteq P[A]$.

Stanford Encyclopedia of Philosophy

Irvine & Deutsch (2014) seem to take the Cantorian position in the reading of the Axiom of Separation (see Part 5, page 84). In these quotes their notation has been adapted to the present one. *Comments* are in square brackets:

> "Again, to avoid circularity [really ?], *B* cannot be free in γ. [Presumably the Cantorian reading ?] This demands that in order to gain entry into *B*, *x* must be a member of an existing set *A.* [This is a different property than the Cantorian reading.] As one might imagine, this requires a host of additional set-existence axioms, none of which would be required if [naive set theory] had held up.
> How does SEP avoid Russell's paradox? One might think at first that it doesn't. After all, if we let *A* be $\mathbb{S}$ – the whole universe of sets – and $\gamma[x]$ be $x \notin x$, a contradiction again appears to arise. [Indeed, for then $((R \in \mathbb{S}) \wedge (R \notin \mathbb{S}))$] But in this case, all the contradiction shows is that $\mathbb{S}$ is not a set. All the contradiction shows is that "$\mathbb{S}$" is an empty name (i.e., that it has no reference, that $\mathbb{S}$ does not exist), since the ontology of Zermelo's system consists solely of sets."
> [Observe that REG causes that $\mathbb{S}$ would not be a ZFC-set anyhow. It is a pity that the authors give the word "set" to ZFC, instead of speaking about ZFC-sets.]
> "This same point can be made in yet another way, involving a relativized form of Russell's argument. [Meaning: not $\mathbb{S}$ but any *A*.] Let *A* be any set. By SEP, the set $R[A] = \{x \in A \mid x \notin x\}$ exists, but it cannot be an element of *A*. For if it is an element of *A*, then we can ask whether or not it is an element of *R*[*A*]; and it is if and only if it is not. Thus something, namely *R*[*A*], is "missing" from each set *A*. So again, $\mathbb{S}$ is not a set, since nothing can be missing from $\mathbb{S}$. But notice the following subtlety: unlike the previous argument involving the direct application of *Aussonderungs* to $\mathbb{S}$, the present argument hints at the idea that, while $\mathbb{S}$ is not a set, "$\mathbb{S}$" is not an empty name. The next strategy for dealing with Russell's paradox capitalizes on this hint. [Referring to Von Neumann's approach that they discuss next]"

Wikipedia

The portal (no source) Wikipedia on Russell's paradox (retrieved May 30 2015) gives the traditional view. They sometimes clearly refer to ZFC-sets, but not always.

> "ZFC does not assume that, for every property, there is a set of all things satisfying that property. Rather, it asserts that given any set *X*, any subset of *X* definable using first-order logic exists. The object *R* discussed above [in naive set theory] cannot be constructed in this fashion, and is therefore not a ZFC set. In some extensions of ZFC, objects like *R* are called proper classes."
> "In ZFC, given a set *A,* it is possible to define a set *B* that consists of exactly the sets in *A* that are not members of themselves. *B* cannot be in *A* by the same reasoning in Russell's Paradox. This variation of Russell's paradox shows that no set contains everything."

The latter should rather be "no ZFC-set contains everything". The reasoning is a bit implicit. It is better to show it. Assume the possibility that there is $\mathbb{S}$*,* then $R[\mathbb{S}] \notin \mathbb{S}$ would generate a contradiction. (In ALOE it would be nonsense.)

Conjectures E and W on strictly Cantorian sets

Now that we have seen Russell's paradox, we may better appreciate the Cantorian sets that are so much like it. The following is streamlined and set within ZFC.

The following conjectures are "theorems of ZFC". Below we will show that ZFC is inconsistent, so that it proves everything. Thus these aren't "theorems" but *conjectures.*

Conjecture. (E), (*Existence of strictly Cantorian sets*). [46] Let A be a set, $P[A]$ its power set. For every function $f: A \to P[A]$ there is a set $\Psi = \{x \in A \mid x \notin f[x]\}$.

Proof. (a) $P[A]$ exists because of the Axiom of the Power Set. (b) f can be regarded as a subset of $A \times P[A]$, and f exists because of Axiom of Pairing. (c) Ψ exists because of the Axiom of Separation. Q.E.D.

Remark. Find $\Psi \subseteq A$, thus $\Psi \in P[A]$. Observe that Ψ depends upon f, i.e. $\Psi = \Psi[f]$.

When $x \in A$ then we can use $(x \in \Psi) \Leftrightarrow (x \notin f[x])$.

Definition of a Cantorian set. Above $\Psi = \{x \in A \mid x \notin f[x]\}$ is called a *strictly* Cantorian set. A generalised Cantorian set has $x \notin f[x]$ as part of its definition. The meaning of 'Cantorian set' without qualification depends upon the context.

Remark. The Cantorian set clearly has some kind of self-reference.

Conjecture. (W), (*Weakest conjecture on strictly Cantorian sets.*) [47] Let (domain) A be a set, and (range) $B \subseteq P[A]$. For every $f: A \to B$ there is a $\Psi \in P[A]$ such that for all $\alpha \in A$ it holds that $\Psi \neq f[\alpha]$.

Proof. Define $\Psi = \{x \in A \mid x \notin f[x]\}$. Take $\alpha \in A$. Check the two possibilities.
Case 1: $\alpha \in \Psi$. In that case $\alpha \notin f[\alpha]$. Thus $\Psi \neq f[\alpha]$. (We have $\alpha \in \Psi \setminus f[\alpha]$.)
Case 2: $\alpha \notin \Psi$. In that case $\alpha \in f[\alpha]$. Thus $\Psi \neq f[\alpha]$. (We have $\alpha \in f[\alpha] \setminus \Psi$.) Q.E.D.

*Remark.*This conjecture combines the definition of strictly Cantorian sets, the existence Conjecture E and an identification of their key property. It is essentially a rewrite of:

(i) $\forall \alpha \in A$: $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin f[\alpha])$ and application of extensionality (EXT),
(ii) which is deducible from $\forall \alpha$: $(\alpha \in \Psi) \Leftrightarrow ((\alpha \in A) \wedge (\alpha \notin f[\alpha]))$, i.e. the definition of Ψ.

For example: Let $K[n] = \{x \mid x$ is a prime number less than $n\}$.
Take $A = K[10] = \{2, 3, 5, 7\}$. Thus $P[A] = \{\varnothing, \{2\}, \{3\}, \{5\}, \{7\}, \{2, 3\}, (2, 5\}, ..., K[10]\}$.
We are interested in a bijection. Let us e.g. take $B = \{\{2\}, \{3\}, \{5\}, \{7\}\}$, with $f[x] = \{x\}$.
Regard $\Psi = \{x \in A \mid x \notin f[x]\}$. We find that all x are in their images, so that $\Psi[f] = \varnothing$.
Alternatively, let us take $B = \{\{2\}, \{2, 7\}, \{2, 5\}, \{2, 3\}\}$, with $g[x]$ specified by that order.
Now $\Psi[g] = \{3, 7\}$, since $3 \notin \{2, 7\}$ and $7 \notin \{2, 3\}$.
Alternatively, let us take $B = \{\{3, 5\}, \{2, 5\}, \{2, 7\}, \{2, 3\}\}$, with $h[x]$ specified by that order.
Now $\Psi[h] = A$.
These examples indicate that Ψ would be a sensible concept. But is it really ?

We can get logical clarity about these conjectures by looking at the singleton. There is a deproof of Conjecture W in **Appendix C** but the singleton is a much better stepping stone.

[46] Conjecture E is a reformulation of the addendum provided by B. Edixhoven, statement in Colignatus (2014a), its appendix D (there, not below).
[47] Conjecture W for $B = P[A]$ was given by K.P. Hart (TU Delft), 2012, in Colignatus (2015b). An advantage of this form is that it doesn't specify notions of injection, surjection and bijection.

The singleton with a nutshell link between Russell and Cantor

Let A be a set with a single element, $A = \{\alpha\}$. Thus $P[A] = \{\emptyset, A\}$. Let $f: A \rightarrow P[A]$.

If $f[\alpha] = \emptyset$ then $\alpha \notin f[\alpha]$. If $f[\alpha] = A$ then $\alpha \in f[\alpha]$.

Thus $(f[\alpha] = \emptyset) \Leftrightarrow (\alpha \notin f[\alpha])$.

Consider:

(1) In steps: define $\Psi = \{x \in A \mid x \notin f[x]\}$, then try $f[\alpha] = \Psi$.

(2) Directly: $f[\alpha] = \{x \in A \mid x \notin f[x]\}$.

(3) Either directly or indirectly via (1) or (2): $\Psi = \{x \in A \mid x \notin \Psi\}$.

The latter is a variant of Russell's paradox: $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin \Psi)$. Thus (1) - (3) are only consistent when $\Psi \neq f[\alpha]$. This is an instance of Conjecture W.

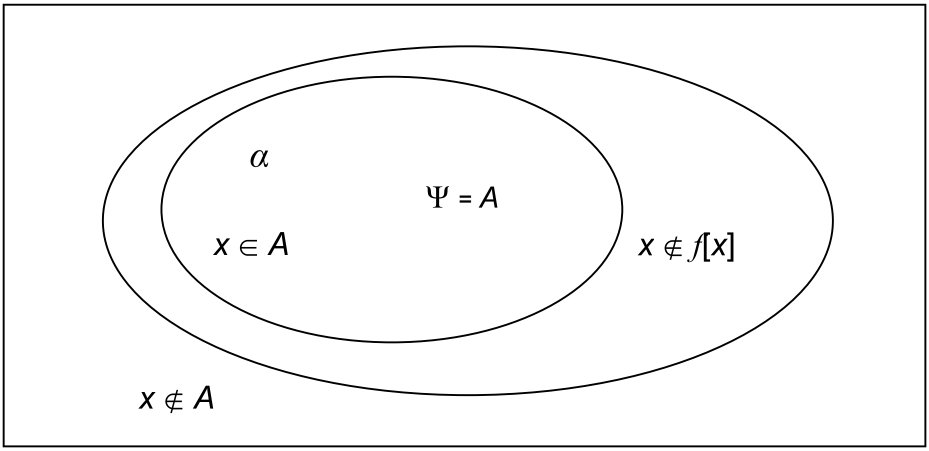
Choosing $f[\alpha] = \Psi$ in (1) assumes freedom that conflicts with the other properties. We have liberty to choose $f[\alpha] = \emptyset$ or $f[\alpha] = A$. This choice defines f and we should write is $\Psi = \Psi[f]$ indeed. This shows why (2) with $f[\alpha] = \Psi[f]$ is tricky. If (2) is an implicit definition of f then it doesn't exist. If it exists then this $f[\alpha]$ will not be in its definition.

Checking all possibilities gives Table 6. The cells are labeled with Δ-*case-numbers*. (The Δ refers to a *difference analysis* (marginal analysis) when a set is extended with single element.) Because of $\alpha \notin f[\alpha]$, the row $f[\alpha] = \emptyset$ is important for us. The case of Δ2 is depicted in a Venn-diagram in Figure 1.

Table 6. Possibilities for $\alpha \in \Psi \Leftrightarrow (\alpha \notin f[\alpha])$ given that $\alpha \in A$

For all cases: $\alpha \in A$	$\Psi = \emptyset$, $\alpha \notin \Psi$	$\Psi = A$, $\alpha \in \Psi$
$f[\alpha] = \emptyset$ $\alpha \notin f[\alpha]$	**Δ1**: $\alpha \in \emptyset \Leftrightarrow \alpha \notin \emptyset$ $f[\alpha] = \Psi$, **impossible**	**Δ2**: $\alpha \in A \Leftrightarrow \alpha \notin \emptyset$ $f[\alpha] \neq \Psi$, possible
$f[\alpha] = A$ $\alpha \in f[\alpha]$	**Δ3**: $\alpha \in \emptyset \Leftrightarrow \alpha \notin A$ $f[\alpha] \neq \Psi$, possible	**Δ4**: $\alpha \in A \Leftrightarrow \alpha \notin A$ $f[\alpha] = \Psi$, **impossible**

Figure 1. The strictly Cantorian set for the singleton, case Δ2: $f[\alpha] = \emptyset \neq \Psi$



Deductive incompleteness

1. Existence of Δ1

An idea is that Ψ in Conjecture E and Conjecture W or Table 6 covers all $\alpha \notin f[\alpha]$. This appears to be false: it doesn't cover Δ1. The cell is declared *impossible*. Let us first verify that it exists as a truth (outside of ZFC), and then accept deductive incompleteness.

Theorem. (ExT1) Case Δ1 exists as a possibility with $\alpha \notin f[\alpha]$.

Proof. We consider the case $f[\alpha] = \varnothing$, so that $(\alpha \notin f[\alpha])$.

Take $q = (\alpha \notin f[\alpha])$ and use tautology T1: $q \Rightarrow (p \Leftrightarrow (q \wedge p))$ for any p, see Table 7.

We are free to take $p = (\alpha \in A)$ for $\Psi = A$, which would give Δ2, or not-$p = (\alpha \in \varnothing)$ for $\Psi = \varnothing$, which would give Δ1. Take the latter, apply modus ponens on q and tautology T1, and find $\alpha \in \varnothing \Leftrightarrow (\alpha \notin \varnothing \wedge \alpha \in \varnothing)$. The equivalence reduces into $\alpha \notin \varnothing$ or $\alpha \in A$. The equivalence is by itself consistent, so that it is possible for $\alpha \in \{ \alpha \}$. Case Δ1 with both $f[\alpha] = \varnothing$ and $\Psi = \varnothing$, fits this equivalence: $\alpha \in \Psi \Leftrightarrow (\alpha \notin f[\alpha] \wedge \alpha \in \Psi)$. We merely establish possibility, and thus the deduction stops here. Q.E.D.

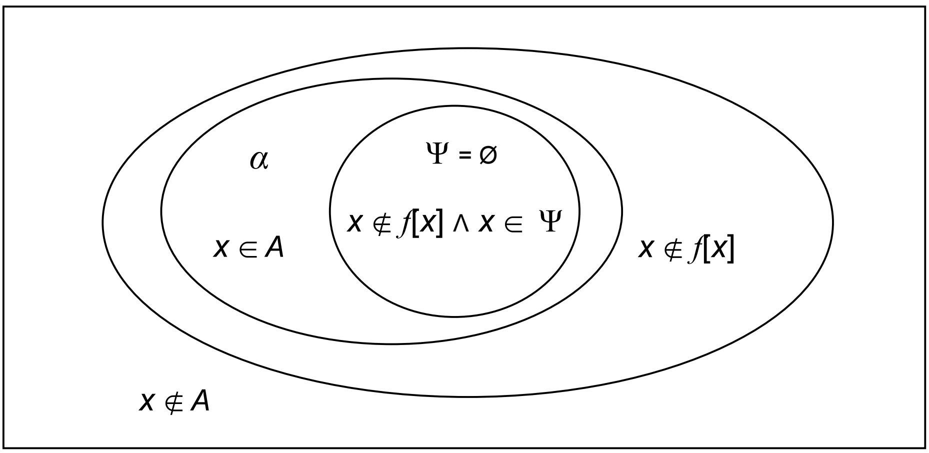
Table 7. Truthtable for T1: $q \Rightarrow (p \Leftrightarrow (q \wedge p))$ **with** $q = (\alpha \notin f[\alpha])$

Case	$\alpha \notin f[\alpha]$	$\Rightarrow$	$(p$	$\Leftrightarrow$	$(\alpha \notin f[\alpha]$	$\wedge$	$p))$
Δ2	1	1	1	**1**	1	1	1
Δ4	0	1	1	**0**	0	0	1
Δ1	1	1	0	**1**	1	0	0
Δ3	0	1	0	**1**	0	0	0

Remark. See page 72 for an indication how Δ1 can exist outside of ZFC based upon other axioms than ZFC. We are tempted to derive inconsistency now, but for understanding of the situation it is better to formally establish deductive incompleteness.

The case shows up by requiring that *all* properties of the case hold, thus jointly $(\alpha \notin \varnothing \wedge \alpha \in \varnothing)$ and not only $(\alpha \notin \varnothing)$. See Figure 2. The diagram uses that $\varnothing \subseteq A$. In this approach for Δ1 or in the figure: when we test $\alpha \in \Psi$, then we test $\alpha \notin f[\alpha] \wedge \alpha \in \Psi$ jointly. From this joint test the decision follows that $\alpha \notin \Psi$, or $\alpha \in A$.

Figure 2. The strictly Cantorian set for the singleton, case Δ1: $f[\alpha] = \varnothing = \Psi$



2. Definition, theorem and proof

Definition. (DeLong (1971:132)): "A formal system is deductively complete if under the intended interpretation there is no truth which is not also a theorem."

Theorem. (D) If ZFC is consistent then it is deductively incomplete.
Proof. Let $A = \{\alpha\}$ have a single element. Thus $P[A] = \{\emptyset, A\}$.
Let $f : A \rightarrow P[A]$ with $f[\alpha] = \emptyset$. Then $\alpha \notin \emptyset$ and $\alpha \notin f[\alpha]$.
Under the intended interpretation, there is the case Δ1 that has $\alpha \notin f[\alpha]$.
Ψ is formulated such that it should contain *all cases* with $\alpha \notin f[\alpha]$.
However, trying to prove that Δ1 fits Ψ, causes a shift to Δ2 or $\Psi = A$ (Conjecture W).
If ZFC is consistent then there is no path to reach Δ1. Q.E.D.

Remark. If there is such a path then ZFC becomes inconsistent.

Inconsistency

1. An implication for the singleton Cantorian set

For the singleton we have $\alpha \in A$, and thus we have $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin f[\alpha])$. It is possible to weaken this by means of another tautology T2: $\forall p,q$: $(p \Leftrightarrow q) \Rightarrow (p \Leftrightarrow (q \wedge p))$. The truthtable for the singleton Cantorian set is in Table 8. The truthtable holds for every f while $\Psi = \Psi[f]$.

Table 8. Truthtable for T2: $(p \Leftrightarrow q) \Rightarrow (p \Leftrightarrow (q \wedge p))$ **for the singleton Cantorian set**

Case	$(\alpha \in \Psi$	$\Leftrightarrow$	$\alpha \notin f[\alpha])$	$\Rightarrow$	$(\alpha \in \Psi$	$\Leftrightarrow$	$(\alpha \notin f[\alpha]$	$\wedge$	$\alpha \in \Psi))$
Δ2	1	**1**	1	1	1	**1**	1	1	1
Δ4	1	**0**	0	1	1	**0**	0	0	1
Δ1	0	**0**	1	1	0	**1**	1	0	0
Δ3	0	**1**	0	1	0	**1**	0	0	0

Consider again $f[\alpha] = \varnothing$. The equivalence on the LHS only allows $\Psi = A$. Look at row Δ1 in Table 8. On the LHS we have Δ1 with $(\alpha \notin \Psi) \wedge (\alpha \notin f[\alpha])$, and the equivalence would declare this combination impossible. However, there is also the relaxed condition on the RHS, that we already encountered in Theorem ExT1.

The crucial step is to distill the RHS from the table. Conjecture E and Conjecture W establish the LHS. Modus ponens with T2 gives the RHS as a separate expression FT2 ('from T2') - provided that we maintain $\Psi = \Psi[f]$:

$$\alpha \in \Psi \Leftrightarrow (\alpha \notin f[\alpha] \ \wedge \ \alpha \in \Psi) \qquad \text{(FT2)}$$

For FT2 we get get Table 9. The same Δ-*case-numbers* apply. Now Δ1 is allowed too: a possible $f[\alpha] = \Psi$ rather than an impossible $f[\alpha] = \Psi$.

Table 9. Possibilities for $\alpha \in \Psi \Leftrightarrow (\alpha \notin f[\alpha] \ \wedge \ \alpha \in \Psi)$**, given** $\alpha \in A$

For all cases: $\alpha \in A$	$\Psi = \varnothing$, $\alpha \notin \Psi$	$\Psi = A$, $\alpha \in \Psi$
$f[\alpha] = \varnothing$ $\alpha \notin f[\alpha]$	$\alpha \in \varnothing \Leftrightarrow (\alpha \notin \varnothing \ \& \ \alpha \in \varnothing)$ $f[\alpha] = \Psi$, **possible**: $\alpha \notin \varnothing$	$\alpha \in A \Leftrightarrow (\alpha \notin \varnothing \ \& \ \alpha \in A)$ $f[\alpha] \neq \Psi$, possible
$f[\alpha] = A$ $\alpha \in f[\alpha]$	$\alpha \in \varnothing \Leftrightarrow (\alpha \notin A \ \& \ \alpha \in \varnothing)$ $f[\alpha] \neq \Psi$, possible	$\alpha \in A \Leftrightarrow (\alpha \notin A \ \& \ \alpha \in A)$ $f[\alpha] = \Psi$, **impossible**: $\alpha \notin A$

Note that $f[\alpha] = \varnothing$ doesn't give a unique Ψ now. Both $\Psi = A$ (Figure 1) and $\Psi = \varnothing$ (Figure 2) are possible. Note that f is still a function and no correspondence.

PM. We can also gain access to Δ4 by another relaxing condition but we are interested in the $\alpha \notin f[\alpha]$ case.

2. A counterexample for Conjecture W

Let us make the latter observations formal. The discovery of Δ1 and tautology T2 gives a contradiction to Conjecture W. While Theorem D stopped looking for a path towards Δ1, we now found that path, namely tautology T2, which gives Theorem Not-W. When Δ1 not merely exists as a truth outside of ZFC (using tautology T1) but also can been proven from Ψ (using tautology T2), then it becomes a counterexample for Conjecture W, which gives Theorem Not-ZFC.

Theorem. (Not-W) For the strictly Cantorian case there are a f and Ψ with $f[\alpha] = \Psi$.

Proof. Let $A = \{\alpha\}$ have a single element. Thus $P[A] = \{\emptyset, A\}$.
Let $f : A \rightarrow P[A]$. (*Remark* 1.) Let $f[\alpha] = \emptyset$. Then $\alpha \notin \emptyset$ and $\alpha \notin f[\alpha]$.
Consider $\Psi = \{x \in A \mid x \notin f[x]\}$. With $\alpha \in A$ then $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin f[\alpha])$. (*Remark* 2.)

Look at Table 8. Use $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin f[\alpha])$ and tautology T2 $(p \Leftrightarrow q) \Rightarrow (p \Leftrightarrow (q \wedge p))$, and apply modus ponens to find FT2: $\alpha \in \Psi \Leftrightarrow (\alpha \notin f[\alpha] \wedge \alpha \in \Psi)$. In this deduction we have maintained the definition of Ψ. The modus ponens is independent of the possibility that also $\Psi = A$ might be derived via another route. The formula FT2 *stands as a separate relation* for Ψ.

A substitution of $f[\alpha] = \emptyset$ and $\Psi = \emptyset$ into FT2 gives Δ1: $\alpha \in \emptyset \Leftrightarrow (\alpha \notin \emptyset \wedge \alpha \in \emptyset)$ that we saw above in Theorem ExT1 and subsequently in Table 9, which reduces to $\alpha \notin \emptyset$, or $\alpha \in A$. The case is consistent by itself, i.e. reduces to $\alpha \in A$. And we have established a path to it. The case has $f[\alpha] = \emptyset = \Psi$. Q.E.D.

Remark 1. In an algebraic version of the proof, we would not assign a value to f but first derive FT2 and only then assign such value. This might however run into the objection that such algebraic application is not possible, and that application of the Axiom of Separation requires a definition of f beforehand. We can do such algebra at the level of the axioms, see below. For this proof we assign f.

Remark 2. At this point in the proof we do not follow the deductive path that leads to a deduction of $\Psi = A$, since this has been already done in Conjecture W. Making this deduction here again confuses the two different proofs. We are now interested in the other path that follows from T2. Inconsistency of ZFC can better be established by the separate Theorem Not-ZFC.

Theorem. (Not-ZFC) ZFC is inconsistent.

Proof. For the singleton, Conjecture E and Conjecture W generate that $\Psi = A$. Theorem Not-W generates the possibility that $\Psi = \emptyset$. Thus it is possible that $A = \emptyset$. This is a clear contradiction. Q.E.D.

Discussion of these two results on ZFC

1. Nominalism versus realism

Recall what we said about holding on to the notion of freedom of definition. Amendment of ZFC will tend to reduce the freedom of definition, unless one allows for a three-valued logic that is strong enough to recognise nonsense.

We can look at Table 6 and Table 9 in horizontal or vertical direction. This reflects the schism in philosophy between *nominalism* and *realism*. (See William of Ockham, page 31.)

(1) The horizontal view gives the *realists* who take predicates as 'real': $\alpha \notin f[\alpha]$ versus $\alpha \in f[\alpha]$. They are also sequentialist: Δ1 & Δ2 versus Δ3 & Δ4.

(2) The vertical view gives the *nominalists* (Occam) who regard the horizontal properties as mere names or stickers, and who more realistically look at $\Psi = \emptyset$ versus $\Psi = A$. They see the table in *even* versus *uneven* fashion: Δ2 & Δ4 versus Δ1 & Δ3.

The nominalist reasoning is: The sets Ø and *A* exist. We are merely discussing how they are referred to. The expression for Ψ is not a *defining* statement but an *inferential* observation. Once the functions have been mapped out, the criteria can be used to see whether the underlying sets may get also another sticker Ψ. We are discussing *'consistent referring'* and not existence.

At issue is now whether ZFC has sufficient logical strength to block nonsensical situations. ZFC has a realist bend. It translates predicates into sets (their extensions). Instead we better employ an axiomatic system to only *test* whether a predicate is useful. Merely cataloguing differently what already exists should not be confused with existence itself. The freedom of definition can be a mere illusion and then should not be abused to create nonsense.

A defining equivalence for Ψ on the LHS of Table 8 results via this tautology into a weaker relation on the RHS that contradicts this definition.

The problem with Conjecture E and Conjecture W is that they *impose* the equivalence on the LHS. This assumes a freedom of definition, whence this assumes that the truthtable on the LHS is true, whence Δ1 is forbidden. But that freedom of definition does not exist. Something exists, that is infringed upon by the definition. When Ψ is the empty set, as in the singleton possibility of Figure 2 then one no longer has the freedom to switch from Ø to *A*, as in Figure 1.

The discussion is not without consequence, see page 79:

> The logical construction $x \notin f[x]$ and only a single problematic element, in badly understood self-reference, should not be abused to draw conclusions on the infinite. There are ample reasons to look for ways how this can be avoided.

2. Diagnosis, and an axiom for a solution set

ZFC blocks Russell's paradox (essentially) by the *Axiom of Separation* (SEP). When the paradoxical $\gamma[x] = (x \notin x)$ is separated from *A* to create some set *R* then the conclusion follows that $R \notin A$, so that the separation cannot be achieved. For the Cantorian set we use $\alpha \in A$, or for the singleton $\alpha \in \{\alpha\}$, so that we can use $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin f[\alpha])$, and there is no separation escape anymore. Separation is an irrelevant solution concept here, and what is at issue is self-reference that requires a fundamental solution.

The diagnosis is that Ψ is a variable (name) rather than a constant. There is a solution set $\Psi^* = \{\emptyset, A\}$, and Ψ is a variable that runs over Ψ^*. Compare to algebra, when one uses a variable *x* with value $x = 0$ in one case and $x = 1$ in another case: then one might derive $0 = x = 1$, but this goes against the notion of a variable. The inconsistency in ZFC is caused by that it does not allow for that Ψ is such a variable.

The following is not in ZFC but will help to understand ZFC.

Definition. An *Axiom for a Solution Set* might be:

$$(\forall A)\ (\exists Z)\ (\exists B)\ ((B \in Z) \Leftrightarrow (\forall x)\ ((x \in B) \Leftrightarrow ((x \in A) \land \gamma[x]))) \qquad \text{(SOL)}$$

This SOL could reduce to the Axiom of Separation (SEP). A way is to eliminate $B \in Z$ as superfluous, with $Z = \{B\}$, or self-evident (which it apparently isn't). Another way is to replace $B \in Z$ by $B = Z$. This imposes uniqueness. When $\gamma[x]$ has more solutions, then a contradiction arises when SEP requires that a single solution B is also the whole set Z. ZFC has the latter effect.

For the singleton $A = \{\alpha\}$ with $f[\alpha] = \varnothing$, Conjecture W finds $B = \Psi = A$ but we find $Z = \{\varnothing, A\} = P[A]$. In itself it is true that $\Psi \in P[A]$, but when the solution set $Z = P[A]$ then it is erroneous to require $Z \in P[A]$.

It is not just an issue of notation. It is not sufficient to suggest to read Conjecture W now as generating a value for the variable, rather than restricting the solution set to that value. For, this reads something into SEP which it does not do: for it really restricts that solution set.

In ZFC Ψ creates the illusion of a unique set, and thus we need amendment of ZFC to correct that. One might hold that Conjecture E and Conjecture W are not necessarily wrong, since one can find for any f a $\Psi[f]$ such that for all α: $f[\alpha] \neq \Psi$. (For the singleton $f[\alpha] = \varnothing$ gives $\Psi = A$.) But the formula of Ψ allows Theorem ExT1 to also find another case with $f[\alpha] = \varnothing$. (For the singleton $f[\alpha] = \varnothing$ also gives $\Psi = \varnothing$.) One can conceive that the two options co-exist, but Conjecture W does not allow for $\Psi = \varnothing$. Thus Theorem Not-W is a real counterexample for Conjecture W.

The freedom of definition used in Conjecture W depends upon the existence Conjecture E. Then something is wrong with Conjecture E, that proved the existence of what Conjecture W uses. The Conjectures were derived in ZFC. Thus ZFC has a counterexample and thus is inconsistent.

Again, consider $f[\alpha] = \varnothing = \Psi$ (Δ1). This is consistent, but is not seen *'easily'* by Ψ, even though it is covered in Table 8 by the falsehood of $\alpha \in \Psi$. In a realist mode of thought, we deduce from $f[\alpha] = \varnothing$ that $\Psi = A$, which is the only possibility on the LHS for $\alpha \notin f[\alpha]$ that is recognised (row Δ2). This is not necessarily the proper response. The problem with ZFC is that it focuses on the LHS and neglects the RHS. We can derive a relaxed condition FT2, and then Theorem Not-W allows to recover Δ1. The latter deductions are actually within ZFC and thus there is scope to argue that Conjecture W presents only part of the picture. However, that part is formulated in such manner that it causes the contradiction in Theorem Not-ZFC. We must switch to a better axiomatic system that covers the *intended interpretation* and that blocks the paradoxical Ψ. The better system blocks the LHS and allows only the RHS.

While this analysis has a destructive flavour on ZFC, it is actually constructive since it indicates what the improvement will be.

3. Logical structure of these deductions on the singleton

The inconsistency shows itself in Table 8 with two cases on the LHS for Conjecture W and three cases on the RHS for Theorem Not-W. In itself it might be possible to use only this table and forget about deductive incompleteness. However, it is useful to build up understanding by first explaining such existence by the use of tautology T1.

The idea that Ψ in Conjecture E and Conjecture W or Table 6 covers all $\alpha \notin f[\alpha]$ appears to be false: it doesn't cover Δ1. Thus when $\alpha \notin f[\alpha]$ then there still exists a case of $\alpha \notin f[\alpha]$. Now, isn't Ψ *supposed* to cover *all* such cases ? The conclusion is: Conjecture E and Conjecture W do not cover the *intended interpretation* (DeLong (1971)). However: since Theorem Not-W deducts this neglected truth, and still is in ZFC, ZFC becomes inconsistent, can prove everything, and the notion of deductive incompleteness loses meaning.

Thus, looking at only consistency would lose sight of Theorem ExT1 and Theorem D.

Since Conjecture E and Conjecture W are well accepted in the literature and Theorem Not-W is new, there is great inducement to find error in it. Indeed, Theorem Not-W allows the deduction of a contradiction in Theorem Not-ZFC, and thus one might hold that it should go. However, its steps are correct. It is more productive for the reader to accept inconsistency of ZFC.

A discussion about self-reference that identifies a contradiction is always difficult to follow. The problem lies not in the identification of the logical framework of the situation but in the inconsistency of ZFC. Potentially the distinction between constant and variable has most effect for clarity. But it also helps to see the distinct roles of the two tautologies T1 and T2.

These tautologies were found to be useful, following an analysis that was inspired by what Paul of Venice (1369-1429) wrote on the Liar paradox. The tautologies generate an amendment to SEP that gives SEP-PV. If SEP in ZFC is replaced by SEP-PV then we get ZFC-PV (see page 89). In this new system of axioms, Table 6 can no longer be derived, but Table 9 can. This is another way to see that Δ1 is a truth that would exist outside of ZFC if ZFC would be consistent.

Part 4. Set theory: ZFC and infinity

The dilemma: which horn to choose

1. Bijection by abstraction for infinity causes that ZFC is inconsistent

In a first reading, the following deduction may seem to be too simple, but there is a logical framework now.

Theorem. (Not-ZFC-BA). ZFC is inconsistent.

Proof. Let A be denumerable infinite, $P[A]$ the power set.
There is a bijection by abstraction, see page 43. Let this be $f: A \rightarrow P[A]$.
ZFC allows the creation of $\Psi = \{x \in A \mid x \notin f[x]\}$,
Because of the bijection there is a ψ such that $f[\psi] = \Psi$.
The direct check on consistency gives: $(\psi \in \Psi) \Leftrightarrow (\psi \notin f[\psi]) \Leftrightarrow (\psi \notin \Psi)$. Q.E.D.

For ZFC, the problem however is not really caused by infinity. The real issue is how ZFC deals with self-reference, as we saw in the former Part for the singleton.
However, when that real issue is not seen then surprising conclusions are possible.

2. Cantor's Conjecture: there is no bijection between set and its power set

The Cantorian set plays a key role in the proof of Cantor's Conjecture on the power set.

Conjecture. (B), (Cantor's Conjecture for the power set, Russell's version, with the bijection). Let A be a set. There is no bijective function $f: A \rightarrow P[A]$.

Proof. Regard an arbitrary set A. Let $f: A \rightarrow P[A]$ be a hypothetical bijection.
Let $\Psi = \{x \in A \mid x \notin f[x]\}$. (*Addendum*, see below.) Clearly Ψ is a subset of A and thus there is a $\psi = f^{-1}[\Psi]$ so that $f[\psi] = \Psi$. The question now arises whether $\psi \in \Psi$ itself. We find that

$$(\psi \in \Psi) \Leftrightarrow (\psi \notin f[\psi]) \Leftrightarrow (\psi \notin \Psi)$$

which is a contradiction. Ergo, there is no such f. Q.E.D.
Corollary. (RND). $\mathbb{N}$ and $\mathbb{R}$ are not equally large. $\mathbb{R}$ is non-denumerable. There are transfinites. A 'bijection by abstraction' is nonsense.

Remarks. The conjecture holds for any set, and doesn't require denumerability. The *Addendum* is often forgotten: This 'proof' relies on (i) existence Conjecture E and (ii) the notion that ZFC provides for well-defined sets anyway. Given this addendum in the 'proof', it should be clearer that if ZFC allows a paradoxical construct then one may feel that ZFC needs amendment. NB. **Appendix G** uses only a surjection rather than a bijection.

3. The dilemma

For this book there is not much of a dilemma. We have deproven Cantor's original 'proofs' of 1874 and 1890/91, and saw inconsistency for the singleton. Theorem Not-ZFC-BA is just a logical consequence of the bijection by abstraction. This result merely shows in another fashion that the strictly Cantorian set is not-well-defined.
All this is a different piece of cake for Cantorian mathematicians though. In the early 1900s they embraced Conjecture B and the implied transfinites. Conjecture B became the corner stone of any course on set theory. Set theory caused that Cantor's original proofs became topics rather for specialists. The transfinites are regarded in 2015 as a hallmark of mathematical achievement. Only proper mathematicians understand those. Who disagrees doesn't understand mathematics.

Well-defined-ness versus the abuse of realism

Page 70 gives the crucial observations on realism versus nominalism.

For a function f from singleton *A* to *P*[*A*] we find that $f[\alpha] = Ø$ or $f[\alpha] = A$. There is no doubt that such a function can exist, or that subsets Ø or *A* exist.

Whether $f[\alpha] = \Psi$ or $f[\alpha] \neq \Psi$ is merely an inferential property, it is not material to the existence of f, and it gives no definition for Ø or *A.* This property *that* Ψ *is irrelevant* holds in general, for both finite and infinite sets.

When we use the self-reference of Ψ to say something about the existence of f then this is an *abuse of an inferential and contingent property*. This Ψ does not necessarily have to support a conclusion on existence, even when a conclusion were true, and the reasoning superficially accurate.

We reach irony:

> This book spends a lot of attention to the Paul of Venice condition and the creation of consistent axioms, partly in order to show that Ψ cannot really block a bijection for the infinite. It is ironic – if that is the proper word – that this analysis is actually superfluous. It is namely sufficient to say that Ψ *is irrelevant anyway*. (However, adherents to Cantor's Conjecture would not believe Occam.)

An analogy is:

> Consider e.g. a theorem *"there cannot be a rational airforce",*
> with definition: An airforce that lets insane pilots fly is irrational,
> and a proof with the Catch 22:
>
> *If a pilot is insane then he should not fly. If he applies for a test on insanity then this is proof of sanity.* Q.E.D.
>
> This abuses a *potential* rule, i.e. no necessary rule. The conclusion seems true for many cases and the reasoning is superficially accurate. This 'proof' makes for a good novel but not for mathematics.

Indeed, for many functions there is no bijection, namely for finite sets. This can be proven by numerical succession, see page 77. The proof in finite cases that uses the Cantorian set does not use a *necessary* condition, and thus is dubious. It is actually invalid, given this observed abuse.

The real issue is infinity. The infinite does not turn the Cantorian set from irrelevance into a necessary condition. One should ask: is there really some set in infinity, that can *only* be described by the Cantorian set, so that we *really need* that set to say something about a surjection ? There is no proof for that. The Cantorian 'set' remains inferential and contingent, and not something that can be used for such a proof on the supposed absence of a surjection.

The inferential and contingent property however can be used to show that ZFC is inconsistent. ZFC namely allows for the formulation of the strictly Cantorian set, with a self-reference that generates an internal contradiction. This hold for the singleton but also for infinity.

Finite sets have no bijection between set and power set

1. Mindmap

The dilemma above is complicated by that Cantor's Conjecture holds for finite sets. The mindmap is:

- For finite sets of arbitrary size - the *potential* infinite – it holds that the power set is always greater than the set.
- That there is no bijection $f : A \rightarrow P[A]$ can be proven by *numerical succession*. [48]
- The method of numerical succession does not work for the *actual* infinite. Cantor's 'diagonal argument' is supposed to work here.
- Cantor's Conjecture allows an elegant union of concepts. Both finite and infinite sets A would share the general property of the lack of a bijection $f : A \rightarrow P[A]$.
- In terms of didactics: once there is an elegant method that also works for the infinite, then numerical succession might be dropped as a method of proof, since there would be this elegant general proof. (But does the 'diagonal method' really work ?)

Our reason to look into Cantor's Conjecture originally was *only* in infinity.

2. Numerical succession for the finite case

Before continuing with ZFC and the infinite, it is useful to establish some of these properties for the finite case, so that we better understand the lure of above mindmap.

Theorem. (FPS) A finite set is smaller than its power set.

Proof. Take $A[n] = \{a_1, ..., a_n\}$. Let $P[A[n]]$ be its power set.

(i) For the singleton the theorem is true. $A[1] = \{a_1\}$ has one element and $P[A[1]]$ has two.

(ii) Assume that the theorem is true for n, with $f : A[n] \rightarrow B$ as a bijection for $B \subseteq P[A[n]]$. Regard $A[n+1]$. Observe that $P[A[n]] \subseteq P[A[n+1]]$. The elements with that bijection can be regarded as allocated and we only have to establish a bijection between $\{a_{n+1}\}$ and elements in $R = P[A[n+1]] \setminus B$. However, R contains elements $\{\{a_1, a_{n+1}\}, ..., \{a_n, a_{n+1}\}\}$, and thus is much too large for a one-to-one relationship. Q.E.D.

3. Examples, and the objections

It may be useful to look at the examples for Conjecture W on page 64 again. The conjecture has been formulated in its weakest form. Bijections $f : A \rightarrow B$ in the finite case will leave great stretches of $P[A]$ untouched.

Overall the Cantorian set does not look unreasonable. For finite cases we can calculate what elements are in the set and what aren't.

There are these objections though:

- It blocks the possibility of a bijection for infinity. See the deproof in **Appendix C**.
- Deductive incompleteness: Ψ promises to collect all $x \notin f[x]$ but doesn't.
- ZFC is actually inconsistent because of it.

[48] Also known as *mathematical induction:* but this is no *induction*, see page 22.

4. Revisit the paradox

The situation causes us to revisit the paradox:

(a) Theorem FPS proves: $\forall n$: $\mathbb{N}[n] < P[\mathbb{N}[n]]$
(b) Potentially, but still unproven is: $\mathbb{N} < P[\mathbb{N}]$ too ?
(c) Can we use abstraction: If ($\mathbb{N}[n]$ @ $\mathbb{N}$) then also ((a) @ (b)) ?

The latter rule in (c) is not defined yet but needs investigating. If this abstraction holds, then we are back to Cantor's conjecture, with $\mathbb{N} < P[\mathbb{N}] \sim \mathbb{R}$.

Let us consider the scheme of abstraction like we saw on page 43. Let us also consider two cases, one for a function itself and one for the statement about existence. On LHS and RHS we simultaneously apply $\mathbb{N}[n]$ @ $\mathbb{N}$

(i) *For any non-bijection*

$$\begin{array}{cccc} \text{not-}b[n]: & \mathbb{N}[n] & \rightarrow & 2^{\mathbb{N}[n]} \\ \downarrow & @ & \downarrow & @ \\ ?: & \mathbb{N} & ?? & 2^{\mathbb{N}} \end{array}$$

(ii) *There cannot be such bijection*

$$\begin{array}{cccc} \neg\exists\, b[n]: & \mathbb{N}[n] & \leftrightarrow & 2^{\mathbb{N}[n]} \\ \downarrow & @ & \downarrow & @ \\ ?: & \mathbb{N} & ?? & 2^{\mathbb{N}} \end{array}$$

(ad (i)) Like on page 43 we can provide a definition of a *"non-bijection by abstraction"*, consisting of the three sides of this square, and arrive at the conclusion that, given this definition, *there is* a non-bijection by abstraction between $\mathbb{N}$ and $\mathbb{R}$.

Indeed, we can easily imagine a function between the natural and the real numbers that is not a bijection. What is missing is the *necessity* that holds for (ii), i.e. Theorem FPS for the finite case. What we need to see is that this would be necessary for infinity as well. Scheme (i) does not provide this. The *bijection by abstraction* in fact shows that scheme (i) does not provide for that necessity indeed.

(ad (ii)) The issue is whether the necessity in Theorem FPS for the finite case can be transferred to infinity. On page 43 there is no existential quantifier in the process of abstraction. The particular bijection for the finite case is abstracted towards infinity. Once this abstraction has been completed, only then there follows the conclusion that it exists.

Above scheme (ii) preloads this quantification into the process of abstraction. Perhaps this is something that one might choose to do.

This book defines abstraction as 'leaving aspects out'. One may well argue that scheme (ii) starts from necessity for the finite case and *only leaves out the finitary aspect* (say, the symbol n). In this approach the necessity transfers to infinity as well. This might be a procedural thing to do, and neglects the special properties of infinity, like Hilbert's Hotel.

In this line of reasoning one might also hold that the above shows that anything can be proven by abstraction, so that the bijection and necessary non-bijection by abstraction are inconsistent, if not nonsensical. This however, would only follow from the earlier choice that one made, of preloading existence in the process of abstraction. Given that this leads to this contradiction, it is a safer conclusion *that such preloading in (ii) cannot be done.*

The above actually illuminates the paradox in the situation. For the finite case there obviously is no bijection, and thus it would seem reasonable that there would be no such bijection for the infinite as well. Cantor's conjecture is only an expression of that seemingly reasonable expectation (in case (ii) above). It is only by looking a bit more sharply at what is involved in the process of abstraction that we can decide that page 43 gives an acceptable formalisation while case (ii) above tries to force too much.

Objectives for this Part

The infinite is special and requires special care. The set-theoretical version of the 'diagonal argument' uses a self-reference that strongly reminds of Russell's paradox. The logical construction $x \notin f[x]$ and only a single problematic element, in badly understood self-reference, should not be abused to draw conclusions on the infinite. There are ample reasons to look for ways how this can be avoided.

The reason to look into Cantor's Conjecture originally was infinity. In the course of this study it was found that there already is inconsistency on the singleton. The need for a change is obvious.

Finding an example in reality for a transfinite object (other than the real numbers, if they really are) would be sufficient to keep them, but it is not necessary to wait till we have found one.

DeLong (1971) explains that an axiomatic system tends to have an 'intended interpretation', so that the axiomatic system is a model for that interpretation. Overall, with an axiomatic system *AS,* the system defines well-defined-ness in its realm. When there is an anomaly ***a*** for *AS*, so that $AS \wedge a$ cause a contradiction, then adherents to *AS* will reject ***a***, but one must always keep in mind that it is also possible to reject *AS.*

Originally, PV-RP-CDA-ZFC presented the condition by Paul of Venice as an anomaly within the intended interpretation of ZFC. Now however, it shows the way to repair ZFC into a ZFC-PV.

To repair ZFC such that the transfinites re-occur again, is dubious. They are a figment of $x \notin f[x]$ confusions, and there is no intended interpretation for them outside of those Cantorian confusions. We have seen three 'proofs' of the non-denumerability of the real numbers: Cantor 1874, 1890/91 and various set-theoretic forms based upon the Cantorian set. There might be other vehicles, but as long there is no necessity, denumerability of the reals, i.e. one kind of infinity, suffices (albeit ordered differently).

The remainder of this Part 4 then has the following objectives:

- Introduce the Paul of Venice consistency condition for sets
- Show how it blocks application of Cantor's Conjecture (though informally, at first)
- Derive it in ZFC
- Show how it makes ZFC inconsistent in the general setting too
- Suggest alternative axioms.

The Paul of Venice consistency condition

Application to Russell's paradox

Russell's set is $R = \{x \mid x \notin x\}$ for naive set theory, and we derived $R \in R \Leftrightarrow R \notin R$, see page 62. Paul of Venice suggested a criterion for the Liar paradox, see page 59. Can we find a similar criterion for sets ?

A formulation is $S = \{x \neq S \mid x \notin x\}$ but this has the suggestion of choice, while the point is that one must show that the property $x \neq S$ is necessary.

Define $S = \{x \mid (x \notin x) \wedge (\text{If}(x = S)\,\text{then}\,(x \in S))\}$ i.e. with the consistency condition inspired by Paul of Venice. [49] The *If*-switch gives a dynamic process of going through the steps, and it is not a mere static implication. Without contradiction we find:

$$S \in S \Leftrightarrow (S \notin S \wedge S \in S)$$
$$S \notin S$$

It is not clear what Russell's set would be, since it is inconsistent; but who wants to work sensibly with a related notion can use *S* without problem.

The dynamic *If*-switch may be replaced by static $S = \{x \mid (x \notin x) \wedge ((x = S) \Rightarrow (x \in S))\}$ but then the truthtable is bigger.

The translation of Paul of Venice's idea to set theory is a bit involved, notably with infinite regress, when a test on *S* on the left causes a test on *S* on the right, which causes a test on the left again, and so on. These issues can be resolved, however. The truthtable of $p \Leftrightarrow (\neg p \wedge p)$ namely allows for a formal decision.

You should indeed recognise the form of the tautologies T1 and T2 used in the deductions on the singleton in Part 3. There the condition surfaced by pure propositional logic, and we used the normal "⋀". Now, however, sets have more elements, and we indeed must handle infinite regress.

A shorthand notation *p* ↑ *q*

A shorthand form is useful. It is only relevant for dealing with infinite regress.

Notation. $V = \{x \mid p[x] \uparrow x \in V\}$, [50] with asymmetric '$p \uparrow q$', stands for the longer $V =$ $\{x \mid p[x]$ unless $(p[x] \wedge x \in V)$ is contradictory (also formally, preventing infinite regress)$\}$. Alternatively $V = \{x \mid \text{If}(p[x] \wedge x \in V) \Leftrightarrow \textit{falsum}\ \text{then}\ \textit{falsum}\ \text{else}\, p[x]\}$ in which the first test can be formal again without infinite regress. In static logic this reduces to $V = \{x \mid p[x] \wedge x \in V\}$ but the idea is the dynamic switch, in which it is tested first whether the *Unless*-condition reduces to a falsehood, formally without infinite regress, and if not, then the unprotected original rule $p[x]$ is applied.

Also: $V = \{x \uparrow p[x]\}$ means $V = \{x \mid p[x] \uparrow x \in V\}$.

Example: In the above we could write $S = \{x \uparrow x \notin x\}$ - and compare this with *R.*

A dangerous shorthand notation

In some texts I have used the form $S = \{x \mid x \notin x \wedge x \in S\}$ as shorthand only – see page 27. This allows students to focus on *S.* Experts however do not regard themselves as

[49] The consistency condition with the exception switch was given in ALOE:129 and applied to Russell. Thus originally the consistency condition was created and this helped finding T1 and T2, not in reverse.

[50] For shorthand I have tried *p* && *q* and *p* &| *q.* The arrow combines a vertical bar with a '⋀' . It remains an asymmetric relation.

students who need education; they quickly recognise that this shorthand form causes infinite regress when $x \neq S$, and then they put this analysis aside, disappointed that it contains such an elementary confusion. However, the shorthand only indicates the intuition by Paul of Venice on the Liar paradox, that must be developed into modern consistency for sets. It is rather curious that this intuition doesn't inspire the experts on set theory. Given this reaction by set theorists it is safer to use $V = \{x \mid p[x] \uparrow x \in V\}$ but sadly with the need of more explanation.

Relevance for the remainder

While a result of this book is that ZFC is inconsistent, another result is that the Paul of Venice condition might be used to repair this, like shown above for Russell. The following sections will make the argument formal.

The **asymmetric** relation $p \uparrow q$ tests on a logical inconsistency of $p \wedge q$, and if there is no conflict, then q is discarded. One can still follow the argument in outline by reading this as '$\wedge$'. The following generalized Cantorian set is called *Pauline*:

$$\Phi = \Phi[f] = \{x \in A \mid x \notin f[x] \uparrow x \in \Phi\}$$

We compare this with $\Psi = \Psi[f] = \{x \in A \mid x \notin f[x]\}$. Does ZFC allow for Φ ? We saw this for the singleton but what is the situation in general ?

Relation to three-valued logic

The Paul of Venice consistency condition gives a solution to both Russell and Cantor. There is no reason for a crisis in the foundations of logic and mathematics and there is no need for a Theory of Types - though you can use them if desired. [51]

We cannot base mathematical conclusions upon an improper way of expressing our statements. E.g. if our form-conventions allow a substitution of "a" and "oo" so that our conclusions about "man" and "moon" are the same, then we can create art, but not necessarily something that we would want to teach as serious mathematics. Discovering what a good notation for well-defined sets is, has been studied even before the discovery of Russell's paradox. Mathematicians thought they had solved it but apparently not.

This book uses *'well-defined'* rather than *'well-formed'.* The context of this book is ALOE that presents three-valued logic, to the effect that logic allows to determine whether an expression reduces to nonsense. This allows leisure on form, so that the Russell set can be said to be of acceptable form – so that it is meaningful for deductive steps – but it turns out to be nonsense and thus not-well-defined. In the same way the Cantorian set would not be rejected merely because of form, i.e. in three-valued logic. In two-valued logic, both Russell and Cantor do not satisfy criteria on well-formed-ness.

For formalisation of an alternative to ZFC there are at least two approaches. One approach is to forbid the formation of *Russell's R* by always requiring the Paul of Venice consistency condition. (This is ZFC-PV.) Alternatively we can allow that *R* is formally acceptable: then we need a three-valued logic to determine that *R* is nonsense. It has meaning, that allows us to see that it is nonsense. (This is BST.) (See below for ZFC-PV and BST.)

[51] ALOE also provided for a solution of the LIar.

Rejection of the proof of Cantor's Conjecture

1. Restatement of ALOE:239

Page 75 gives the common proof for Cantor's Conjecture that a set is always smaller than its power set. The following gives the rejection of this proof by ALOE:239. [52] This rejection assumes that ZFC *might be consistent* while the strictly Cantorian set is nonsensical.

Rejection of Conjecture B. We might hold that above Ψ is badly defined since it is self-contradictory under the hypothesis of a bijection or surjection. A badly defined 'something' may just be a weird expression and need not represent a true set. A test on this line of reasoning is to insert a small consistency condition, giving us $\Phi = \{x \in A \mid x \notin f[x] \uparrow x \in \Phi\}$. See the section above for the notation on '$p \uparrow q$'. Reading it as '$p \wedge q$' still gives an idea of the argument. The bijection or surjection gives that there is a φ such that $f[\varphi] = \Phi$. Now we get:

$$\varphi \in \Phi \Leftrightarrow (\varphi \notin f[\varphi] \wedge \varphi \in \Phi)) \Leftrightarrow (\varphi \notin \Phi \wedge \varphi \in \Phi) \Leftrightarrow \textit{falsum}$$

We find $\varphi \notin \Phi$ without contradiction. This closes the argument against the proof.

Remarks.

(1) The Ψ differs lexically from Φ, with the first expression being nonsensical and the present one consistent. Ψ is part of a lexical description but does not sensically refer to a set. Using this, define $\Phi^* = \Phi \cup \{\varphi\}$ and we can express consistently that $\varphi \in \Phi^*$. So the 'proof' can be seen as using a confused mixture of Φ and Φ^*.

It is remarkable that this explanation by itself is often not enough to recognise the nonsense, i.e. for some people. For them more complex explanations must be added, for infinity the notion of *bijection by abstraction* and for the singleton inconsistency via the tautologies T1 and T2. The nonsense is obscured for finite sets. For example, the $x \notin f[x]$ rule *seems* to work for the singleton. Check what it *does.* Nevertheless, nonsensicality is shown by the above, and the other explanations are only corroborations.

(2) The singleton has no surjection and thus the above does not quite compare. With only one element, $\alpha \notin \Phi$ implies $\Phi = \varnothing$ and $\Phi^* = A$. We however have freedom to take $f[\alpha] = \Phi$ or $f[\alpha] \neq \Phi$. Instead we have always $f[\alpha] \neq \Psi$, so it is no useful building block for a surjection.

(3) The consistency condition in Φ only enhances consistency, but Ψ and Φ still have a different effect. This is an anomaly. Users of ZFC should explain this, see Part 5.

(4) ALOE:239 asumed the common case of a bijection but there is also the standard case of a surjection, see **Appendix G.** Above rejection has been reformulated to cover both.

(5) In writing CCPO-PCWA in 2012 I considered using the more general $\uparrow$-construction, but still preferred the *ad hoc* form $\Phi = \{(x \in A) \wedge (x \neq f^{-1}[\Phi]) \mid x \notin f[x]\}$ for the hypothesis of a bijection. Now, looking at the challenge to ZFC, it seems better not to linger in *ad hoc* solutions but to emphasize the general idea, and use the general notation $p \uparrow q$.

If one feels uncomfortable with the $\uparrow$-switch then it is useful to know that there may be an *ad hoc* definition for Φ indeed. An *ad hoc* format without an inverse function is:

$$\Phi = \{x \in A \wedge f[x] \neq \Phi \mid x \notin f[x]\} \cup \{x \in A \wedge f[x] = \Phi \mid x \notin f[x] \wedge x \in \Phi\}$$

[52] Here we use $p \uparrow q$. ALOE:239 itself uses the 'dangerous' shorthand notation, assuming full formal complexity as on ALOE:129 on the Russell paradox and the condition to prevent infinite regress.

2. Conclusions

1. The common cq. standard proof for Cantor's Conjecture on *any* set is based upon a badly defined and problematic self-referential construct. The proof and the variants evaporate once the problematic construct is disabled and a sound construct is used.

2. The theorem is proven for finite sets by means of *numerical succession* (mathematical induction) but is still unproven for infinite sets: that is, this author is not aware of other proofs. Above we have called it a "conjecture", but Cantor might not have done such a conjecture (without proof) if he would have known about above refutation.

3. What Cantor did know: The distinction w.r.t. the natural and the real numbers would rest (only) upon the specific interval (Cantor 1874) or specific diagonal argument (Cantor 1890/91) – that differ from the (common cq. standard) set-theoretic proof. See Part 2 for the rejection of these original proofs, i.e. page 47 for the proof of 1890/91 and page 51 for the proof of 1874.

4. The transfinites that are defined by using Cantor's Conjecture evaporate with it.

5. The context of ALOE is rather close to naive set theory and definitely not formal ZFC theory. The question is whether Φ is a set in ZFC, or whether it is an entity outside of ZFC. This leads to the next section on Cantorian and Pauline readings of ZFC.

3. Inconsistency seems worse than nonsense

The structure of this book is that we first develop $\mathbb{N}$ and $\mathbb{R}$ with the bijection by abstraction. Hence the dilemma on page 75 is formulated as between the horn of Cantor's conjecture and the horn of the bijection by abstraction. Originally in 2007, the dilemma actually was between Cantor's conjecture and above rejection in ALOE. The bijection by abstraction was found only in 2012. [53]

Somehow it is more interesting in the mathematics community to say that something is *inconsistent* rather than to say that something is *nonsense.* ALOE 2007, restated above, concluded that there was nonsense. It seems that few mathematicians cared, notwithstanding the review by Gill (2008).

In 2014-2015 I decided to look at ZFC myself and spotted inconsistency as discussed in Part 3. Now the reaction should be expected to be a bit different.

Mathematicians are not trained on the three-valued logic in ALOE of course, but this neglect of nonsense does not seem to be a good excuse.

See also the three strategies mentioned on page 60.

For ZFC-theorists, above rejection appears to be too vague or simply unacceptable. In one respect they are right, since they are dealing with ZFC. In another respect they are as wrong as ostriches can be. An axiomatic system *has* an intended interpretation. The rejection above makes eminent sense in the intended interpretation of naive set theory or in the context of the elementary logic of ALOE. Thus, mathematicians should be worried about above rejection. The fact that above rejection is not (yet) in ZFC is no argument to neglect it. Below we will bring it into ZFC, which means that some mathematicians missed the opportunity to do so themselves.

These observations are useful for the *Framework for Discussion*, Part 5. They can best be made here, at the place where the rejection of Cantor's Conjecture started, so that the reader may better appreciate what is involved.

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[53] It is a matter of didactic structure of this book. Starting this Part with the original dilemma would also require that we first develop the notation $p \uparrow q$ w.r.t. infinite regress. An alternative structure would be to start with ZFC so that also the power set can be used from the start. The current structure follows history, starts with numbers and infinity, and only brings in set theory secondly.

Cantorian and Pauline readings of the Axiom of Separation

The discussion now focuses on the *Axiom of Separation* (SEP), stated on page 61, and the condition "*B* is not free in $\gamma[x]$". There are a tolerant *Pauline* reading (supported by Lemma P below) and a defensive *Cantorian* reading of this condition.

1. A Cantorian reading implies the possibility of a Pauline reading

Definition. The **Pauline** reading of SEP takes the axiomatic formula,

$(\forall A)\ (\exists B)\ (\forall x)\ (x \in B \Leftrightarrow ((x \in A) \wedge \gamma[x]))$,

as basic, so that $\gamma^*[x] = (\gamma[x] \uparrow (x \in B))$ with $B = \Phi$ is allowed with the Paul of Venice consistency condition, since $B = \Phi$ is not free but bound by the existential quantifier $(\exists B)$. Thus the formation of Φ in the former section is allowed in ZFC.

The expression $\gamma^*[x]$ is created within the formula and can be taken out for inspection, but one should be careful about conclusions once it has been taken out. The explanatory text in the axioma around the formula is about the whole formula, and not necessarily about expressions once they have been taken out.

Definition. The **Cantorian** reading is that the expression $\gamma[x]$ is created outside of the axiomatic formula, is judged on its own properties (though unstated *why* and *how*), and only afterwards substituted into the formula. It holds: $\gamma^*[x] = (\gamma[x] \uparrow (x \in B))$ is not allowed because $B = \Phi$ would be a free variable *before* the substitution (and not a constant).

Part 5 discusses these two interpretations. There exist not only different forms of ZFC but also different readings. Lemma P however proves that the Cantorian reading implies the possibility of the Pauline reading, i.e. for membership of the *B* which is being defined.

Lemma. (P) For "$(x \in B)$": (The Cantorian reading) $\Rightarrow$ (A possible Pauline reading).

Proof. There is the tautology in propositional logic (see Table 8):

$(p \Leftrightarrow q) \Rightarrow (p \Leftrightarrow (q \wedge p))$

With $p = (x \in B)$ and $q = (x \in A \wedge \gamma[x])$ we get:

$$\big((x \in B) \Leftrightarrow (x \in A \wedge \gamma[x])\big) \ \Rightarrow\ \big((x \in B) \Leftrightarrow (x \in A \wedge \gamma[x] \wedge (x \in B))\big) \qquad (*)$$

Take the Cantorian reading that $(\forall A)\ (\exists B)\ (\forall x)\ ((x \in B) \Leftrightarrow (x \in A \wedge \gamma[x]))$ and eliminate the existential quantifier by some constant set, say $C = C[A, \gamma]$:

$(\forall A)\ (\forall x)\ ((x \in C) \Leftrightarrow (x \in A \wedge \gamma[x]))$

Given that the tautology (*) holds for all *p* and *q,* substitute *C* in *B* in (*), then apply Modus Ponens, and find:

$$(\forall A)\ (\forall x)\ \big((x \in C) \Leftrightarrow (x \in A \wedge \gamma[x] \wedge (x \in C))\big) \qquad (**)$$

Then abstract to an existential quantifier again. There are two ways to do this. The first way is set-preserving, in which the constant *C* is kept on the RHS.

$(\forall A)\ (\exists B)\ (\forall x)\ \big((x \in B) \Leftrightarrow (x \in A \wedge \gamma[x] \wedge (x \in C))\big)$.

The other abstraction considers the whole expression and gives the Pauline reading:

$(\forall A)\ (\exists B)\ (\forall x)\ \big((x \in B) \Leftrightarrow (x \in A \wedge \gamma[x] \wedge (x \in B))\big)$.

Propositional logic also accomodates the exception switch '$p \uparrow q$' (page 80**).** Q.E.D.

The lemma shows that *there is no necessity* to reject the Pauline interpretation.

This book now faces a choice of composition. The (**) relation above can be used to copy the analysis on the singleton, now for the general situation, with proper choice of γ and *C.* However, as it has already been shown that ZFC is inconsistent, there is little advantage in such copying of the argument. It is more interesting to investigate what consequences there would be for the Pauline interpretation and for a development of a ZFC-PV. We proceed on that second course. A first consequence is the following.

2. The Cantorian and Pauline sets may differ

Corollary. (Q) The set created by the Pauline reading is not necessarily equal to the set created by the Cantorian reading.

Proof. The proof of Lemma P used the method of eliminating the existential quantifier by subsitution of a constant set C = $C[A, \gamma]$. Thus the constant set C that satisfies the Cantorian reading also satisfies a Pauline reading. This conclusion is not affected by the later step of abstraction to ($\exists B$). However, abstraction over the whole expression may introduce new solutions. Q.E.D.

Remarks.

(1) The above holds for any set and expression, not just the paradoxical ones.

(2) Inconsistency of ZFC in Part 3 was derived under the Cantorian reading. The Cantorian reading allows $\Psi = \Phi$, and subsequently the derivation for Theorem Not-W and Theorem Not-ZFC. Inconsistency does *not* depend upon the Pauline reading but upon direct propositional logic.

(3) The tautologies T1 and T2 in the proof for Theorem Not-W apply always. Still, the singleton gives an example with Table 6 for Ψ and Table 9 for $\Phi = \Psi$. While that analysis has $\Phi = \Psi$, there also arises the idea to differ. While it may not have been clear there under what circumstances an independent application of the RHS (Φ) was allowed, this clarity has now been given. The Cantorian interpretation always generates a Pauline form for the same sets ($\Psi = \Phi$). There might be a difference however when one would allow existential abstraction from a proposition too: and then the two might start to differ, which gives scope for an adjusted axiom SEP-PV and system ZFC-PV.

(4) It might be useful – e.g. for the expansion to '↑' – to write out some relations for the paradoxical Φ and Ψ. Given the deduction in Lemma P we find for Ψ:

$$\big((x \in \Psi) \Leftrightarrow (x \in A \wedge (x \notin f[x]))\big) \Rightarrow \big((x \in \Psi) \Leftrightarrow (x \in A \wedge (x \notin f[x]) \wedge (x \in \Psi))\big)$$

Given that the antecedens holds ($\forall A$) ($\forall x$), also the consequence holds ($\forall A$) ($\forall x$).

For Φ there is the consequence term only:

$$(\forall A)\,(\forall x)\ \big((x \in \Phi) \Leftrightarrow (x \in A \wedge (x \notin f[x]) \wedge (x \in \Phi))\big)$$

There is the same expression for both now:

$$\text{For } K = \Phi, \Psi\text{:}\ (\forall A)\,(\forall x)\ \big((x \in K) \Leftrightarrow (x \in A \wedge (x \notin f[x]) \wedge (x \in K))\big)$$

With only the latter information it is doubtful whether ZFC is strong enough to derive whether these are just different names for the same set $\Phi = \Psi$ or not. However, Lemma P and Corollary Q make it certain, via another route, that there is ambiguity indeed.

The following question then becomes more acute.

What is the difference between Ψ and Φ ?

1. It should have no effect but it has

The deduction on the singleton poses a challenge to ZFC. Sets *R* and *S* in the discussion on the Russell set on page 80 were in naive set theory, so it has relatively little meaning – for now – to ask about the difference between *R* and *S.* However, Ψ and Φ belong to ZFC – see Lemma P – and thus the question is (more) meaningful. Users of ZFC will have a hard time trying to clarify:

(1) that the Pauline consistency condition should have no effect (it enhances consistency)
(2) but actually can have an effect.

I have considered this question only to some limited extent since I have no vested interest in ZFC. I leave it to users of ZFC to clarify this issue. The following are useful clarifications based upon the little that I could do.

(A) My solution of this issue is that Ψ is badly defined and that Φ is well-defined. I am interested in a convincing argument to the contrary but haven't seen it yet. Note that Ψ at first seems to work for finite sets, see for example the singleton or the examples on page 64. However, we identified for the singleton that Ψ does not cover the intended interpretation and causes inconsistency. While Φ allows more solutions and thus clearly works as a variable, Ψ must be a variable too but gives the suggestion as if it were a single solution and a constant.
(B) Lemma P and Corollary Q allow us to pose the question about Ψ and Φ a bit more acutely. Let us neglect inconsistency because of the singleton and focus (more didactically) on why this inconsistency may not be seen.

One option is to try to determine a contradiction in the general case, similar to the contradiction for the singleton.

2. Changing a function at the margin (Hilbert's Hotel)

A Cantorian is likely to insists on Ψ = Φ for Table 6 but on Φ ≠ Ψ for Table 9. Let us take advantage of a key property of the infinite case. Hilton's Hotel allows the construction of a function *f* such that a Cantorian may sooner accept that both contradictory conditions hold. Let us rework a function at the margin (whence the Δ's). The following is called a *theorem* since it also should hold in suggested ZFC-PV.

Theorem. (ESV, existence of a surjective value for Φ). Let *A* be denumerable infinite, *P*[*A*] the power set. (i) For any non-trivial *h: A* → *P*[*A*] there are *f*: *A* → *P*[*A*] and *φ* ∈ *A* with *f*[*φ*] = Φ = Φ[*A*] = $\{x \in A \mid x \notin f[x] \uparrow x \in \Phi\}$. (When Φ is written without brackets: Φ = Φ[*A*].)
(ii) The direct test has *φ* ∉ Φ without direct contradiction.

Proof. (i) When there is a *φ* ∈ *A* such that *h*[*φ*] = Φ = $\{x \in A \mid x \notin h[x] \uparrow x \in \Phi\}$ then the proof ends with *f* = *h.* (To block repeat application of the proof.)
Otherwise: consider an ordering of *A* = {*a*[1], ...} and let be *φ* = *a*[1].
Let *B* = *A* \ { *φ* } and *g*: *B* → *P*[*A*] as in Hilbert's Hotel: *g*[*a*[*n*]] = *h*[*a*[*n*-1]] for *n* > 1.
Take Φ*[*B*] = $\{x \in B \mid x \notin g[x] \uparrow x \in \Phi^{*}[B]\}$.
(PM 1. Since we use *P*[*B*] and not *P*[*A*], we use a *. Φ*[*B*] exists in ZFC, see Conjecture E: (a) *g*'s domain is *B* and its range is *P*[*A*]; *g* can be regarded as a subset of *B* × *P*[*A*]. Then *g* exists because of the Axiom of Pairing. (b) Φ*[B] exists because of the Axiom of Separation applied to the part without '↑', and then applying Lemma P. N.B. There are the set-preserving Φ1*[*B*] and the free Φ2*[*B*] versions of this theorem.)

Define f: $A \to P[A]$ as:

(a) $x \in B$: $f[x] = g[x]$. Rewrite: $\Phi^*[B] = \{x \in B \mid x \notin f[x] \uparrow x \in \Phi^*[B]\}$.

(b) $x = \varphi$: $f[\varphi] = \Phi^*[B]$

We need to prove that $\Phi = \Phi[A] = \Phi^*[B]$.

Since $(\varphi \notin B)$ also $(\varphi \notin \Phi^*[B])$.

Define: $M = \{x = \varphi \mid (x \notin f[x]) \wedge (x \in \Phi^*[B])\} = \emptyset$. (Margin, '∧', not '↑')

(PM 2. $M = \emptyset$ is in ZFC. Or, do Separation of $\{\varphi\}$ with $\gamma^*[x] = (x \notin f[x]) \wedge (x \in \Phi^*[B])$, in which $\Phi^*[B]$ is not a free variable but a constant given from the above; a different B than in the axiom itself also.)

A union with M allows a rewrite from B to A:

$\Phi^*[B] = \Phi^*[B] \cup M = \{x \in A \mid x \notin f[x] \uparrow x \in \Phi^*[B]\}$ (Substeps 1&2)

Rewrite: $K = \{x \in A \mid x \notin f[x] \uparrow x \in K\}$, for $K = \Phi^*[B]$.

This is using another name for Φ, so that $K = \Phi^*[B] = \Phi$. (Substep 3)

(ii) The direct test on consistency is:

$$(\varphi \in \Phi) \Leftrightarrow (\varphi \notin f[\varphi] \wedge \varphi \in \Phi)$$

Whence it follows without direct contradiction that $\varphi \notin \Phi$.

Q.E.D.

Substeps:

(1) $(\{x \in A \mid \gamma[x]\} \cup \{x \in B \mid \gamma[x]\}) \Leftrightarrow \{x \in A \cup B \mid \gamma[x]\}$

The LHS and RHS mean for the elements of A and B, also allowing for infinity, also when some or all subsets reduce to the empty set:

$\{a_1 \mid \gamma[a_1]\} \cup ... \cup \{b_1 \mid \gamma[b_1]\} \cup ...$

(2) Joining the two sets into K and directly introducing the '↑' notation may be tricky. However, work in the opposite direction. Test $(\varphi \in K) \Leftrightarrow ((\varphi \notin f[\varphi]) \wedge (\varphi \in \Phi^*[B]))$ since the contradiction on the RHS takes ↑-precedence. The RHS *falsum* gives non-membership. Then $(x \in A)$ reduces to $(x \in B)$ so that the original definition of $\Phi^*[B]$ is retrieved.

(3) These are just names. If one allows the rewrite to K then this label K can also be used in the Theorem itself rather than Φ. Who requires a difficult route first defines $\Psi[A]$ then derives $\Phi[A]$ via T2 in Table 8, and then embarks on proving $\Phi[A] = K$, using (2).

Remarks.

(1) Let $Y = \{x \in A \mid x \notin h[x]\}$. With Conjecture W there is no x in A such that $h[x] = Y$. Thus $h[\varphi]$ is unequal to Y and this would not give a bijection. The theorem shows that it is not precluded that one can construct a bijection (by abstraction) by some f however.

(2) Some constructive methods still allow for fixed points (Brouwer). The proposition *"This proposition is true"* is self-referential without much problem.

3. Inconsistency for the general situation

Theorem ESV can be used for the question how Φ and Ψ compare.

Lemma. (DPP, difference Ψ and Φ) Let A be denumerable infinite, $P[A]$ the power set. For any non-trivial h: $A \to P[A]$ there are f: $A \to P[A]$ and $\varphi \in A$ with $f[\varphi] = \Phi$ so that $\Phi \neq \Psi$.

Proof. Theorem ESV generates f with $f[\varphi] = \Phi = \{x \in A \mid x \notin f[x] \uparrow x \in \Phi\}$.

Conjecture W generates $\Psi[f] = \{x \in A \mid x \notin f[x]\}$ so that for all $\alpha \in A$ it holds that $\Psi \neq f[\alpha]$. Thus also $\Psi \neq f[\varphi] = \Phi$. Q.E.D.

Remarks.

(1) Another way: Find $\varphi \notin f[\varphi]$, thus $\varphi \notin \Phi$ and $\varphi \in \Psi$, so that $\Phi \neq \Psi$.

(2) There is inconsistency if also $\Phi = \Psi$. We already derived inconsistency for the singleton. Who has doubts on the singleton may now check on denumerability.

Corollary. (Not-ZFC-2) ZFC is inconsistent for the denumerable infinite.

Proof.
(i) The Cantorian reading generates the possibility of a Pauline reading, see Lemma P. Select the set-preserving case such that $\Psi = \Phi$.
(ii) Do Theorem ESV for the latter case. Thus $\Phi1^*[B] = \Psi^*[B]$ in Pauline format. The Pauline format is important to make this work: to create $M = Ø$ and then have the union with $\Phi1^*[B] = \Psi^*[B]$. Because only $M = Ø$ is included, the result must be equal to $\Psi[A]$.
(iii) While $\Psi = \Phi$, Lemma DPP shows that also $\Phi \neq \Psi$.
Q.E.D.

Remarks.
(1) That ZFC is inconsistent is not the major insight in this book. It are the considerations that count. Major insights are the bijection by abstraction, and that strictly Cantorian sets are actually irrelevant for this kind of existence proofs, see page 76.
(2) Above marginal analysis fits within ZFC without problem because of $M = Ø$. **Appendix H** does a marginal analysis on the original Ψ, and generates these points: (a) Since it is possible that the margin is not empty, there are some complications, and it is better that experts on ZFC explain how it works out, with what properties. (b) It also shows that the situation is highly artificial. We cannot do simple algebra, with assignment of a variable that later finds its value, and we may have to stick to a certain order of manipulations, while there doesn't seem to be an axiom that determines that order. The rationale of such gate-keeping would be to prevent inconsistency, and one would do so when ZFC is regarded as the holy grail. When one accepts however that ZFC is a monster that creates the transfinites based upon badly understood and handled self-reference, then it is easier to see that the order of manipulations is artificial and serves no real purpose.

Amendments to ZFC, giving ZFC-PV or BST

To meet inconsistency we would require the PV-condition in general.

Possibility 1: Amendment by Paul of Venice to the Axiom of Separation:

$$(\forall A)\ (\exists B)\ (\forall x)\ ((x \in B) \Leftrightarrow ((x \in A) \wedge \gamma[x] \uparrow (x \in B))) \qquad \text{(SEP-PV)}$$

Lemma P proves that SEP implies SEP-PV, but there arises a new system when SEP is replaced by SEP-PV. In this case, Ψ is no longer possible, the proof for Cantor's Conjecture collapses, and Ψ becomes ill-formed and nonsensical. My suggestion is to call this the *neat* solution, and use the abbreviation **ZFC-PV**.

This formulation of SEP-PV might also cause a confusion, like SEP does, that $\gamma[x]$ gvies a unique *B,* whence it might be advisable to have a PV-version of SOL that states that solutions need not be unique. However, as shown, application of SEP-PV generates multiple solutions, see Table 9, and hence *B* clearly is a variable.

Another possibility is to move from ZFC closer to naive set theory, discard the Axiom of Separation, and adopt an axiom that allows greater freedom to create sets from formulas.

Possibility 2: Discard the Separation axiom and have extensionality of formulas, a.k.a. comprehension:

$$(\forall \gamma)\ (\exists B)\ (\forall x)\ ((x \in B) \Leftrightarrow (\gamma[x] \uparrow (x \in B))) \qquad \text{(EFC-PV)}$$

This axiom protects against Russell's paradox and destroys the standard proof of Cantor's Conjecture. This resulting system might be called ZFC-SEP+PV.

The Axiom of Regularity (REG) forbids that sets are member of themselves. Instead, it is useful to be able to speak about the set of all sets. Though it is another discussion, my suggestion is to drop this axiom too, then to call this the 'basic' solution, and use the abbreviation BST (basic set theory), thus **BST** = ZFC-SEP+PV-REG. I would also propose a rule that the PV-condition could be dropped in particular applications if it could be shown to be superfluous. However, for paradoxical $\gamma[x]$ it would not be superfluous.

I am not aware of a contradiction yet. I have not looked intensively for such a contradiction, since my presumption is that others are better versed in (axiomatic) set theory and that the problem only is that those authors aren't aware of the potential relevance of the consistency condition by Paul of Venice.

A question for historians is: Zermelo (1871-1953) and Fraenkel (1891-1965) might have embraced the Paul of Venice's condition if they had been aware of it.

Conclusions

1. ZFC is inconsistent. Useful alternatives are ZFC-PV or BST.

2. The earlier conclusions on page 83 about the rejection of Cantor's Conjecture using the informal context of ALOE:239 are valid also in ZFC, since the Pauline set is in ZFC.

3. Users of ZFC who do not accept this should give an answer to above questions, and clarify why they accept Ψ and not Φ that has a better definition of a well-defined set. Deproof would be required for the domino row of Lemma P, Corollary Q, Theorem ESV, Lemma DPP and Corollary Not-ZFC-2.

4. If one holds that ZFC is consistent, against all logic, then one also accepts the construction of a 'proof' for Cantor's Conjecture that generates the transfinites – and the curious *continuum hypothesis* – which makes one wonder what this system is a model for. We can agree with Cantor that the essence of mathematics lies in its freedom, but the freedom to create nonsense somehow would no longer be mathematics proper.

5. It becomes feasible to speak again about the *set of all sets.* This has the advantage that we do not need to distinguish (i) sets versus classes, (ii) *all* versus *any*.

6. The prime importance of this discussion lies in education, see page 25. Mathematics education should respect that education itself is an empirical issue. In teaching, there is the logic that students can grasp and there is the idea to challenge them with more; and there is the wish for good history and and still not burden students with the confusions of the past. My suggestion is that Cantor's transfinites can hardly be grasped, are not challenging, and are burdening rather than enlightening. This book clarifies that highschool education and matricola for a broad group of students, rather also not majoring in mathematics, could be served well with a theory of the infinite that consistently develops both the natural and real numbers, without requiring more than denumerability ($\mathbb{N} \sim \mathbb{R}$), using the notion of *bijection by abstraction*. This book started its discussion for good reason with abstraction.

A major problem in schools would be when mathematics teachers think that Cantor's Conjecture and its transfinites would be a great result and that they would feel frustrated when they would not be in a position to explain it properly – while such frustration would only be based upon a mirage and still show up in behaviour.

Part 5. Framework for discussion

Discussion regarded as a sign of mental incompetence

Mathematicians distinguish between results and philosophy.

The black-and-white image of mathematics would be: Definitions and axioms generate theorems that are proven. Conjectures generate search for proofs. There is no scope for discussion: a proof is valid or not. Who enters a discussion suggests an incapacity to determine validity, and should look for a job in the Department of Philosophy. Who uses diplomatic language ("this analysis causes questions for ...") indicates feelings of uncertainty about the findings and thus apparently doesn't do mathematics.

In practice, research mathematicians (RM) are just people, and they discuss all kinds of things. Some issues are not so clear-cut as RM hold, and then it requires discussion to determine what aspects are clear and what aspects are vague.

The new results in this book will need to be discussed *within* and *for* both school mathematics (SM) and research mathematics (RM). When I propose such a discussion, then this does not imply that I would be uncertain about the results. The suggestion to replace ZFC by ZFC-PV or BST is not an issue of volition and philosophy, but follows from the necessity that mathematics itself generates. But I accept that people will find it hard to drop ideas that they accepted before, so that some discussion is needed.

It may be added that I did not enjoy the idea of rejecting the proof for Cantor's Conjecture, that I originally accepted in 1981-2006, and for the reals till 2010, and that has such an acceptance in mathematics since Hilbert, even in constructivism. But, when one studies logic, one may learn to respect necessity. It must be observed that this author is no expert on ZFC or Cantor, see for background **Appendix D**. This book may reject various proofs but perhaps there are other proofs. Not being an expert generates some uncertainty in one's position, but not on the deductions that one has done.

The following are some suggestions for the Framework for Discussion of the present results. They are based partly upon experience and partly upon the situation itself.

For a discussion it matters what the discussants know. A discussion with persons who have read this book will be different from a discussion with people who haven't.

A discussion with a Cantorian mathematician who is married to the transfinites requires some preparation on the maze of fallacies and misconceptions that one might meet. It is advisable, for example, to look again first at the three strategies on page 60 and subsequently at the observation on page 83 that nonsense doesn't ring alarm bells. The first step in a discussion might well be to restore the awareness of the notion of the intended interpretation.

It was Cantor himself who emphasized the freedom in mathematics. That freedom is limited when the Cantorians themselves do not mention alternatives. Even a university course like Coplakova et al. (2011) currently presents matricola students only with Cantor's Conjecture without mentioning the alternative analysis in ALOE (2007), and thus potentially seduces some students to waste their lives on transfinites. Such a course is indoctrination, not worthy of an academic environment, and it is hypocritical since it claims to be open-minded, as it invites students to check the proofs themselves, while criticism is taken as evidence of failing the course. A discussion with such indoctrinated students might be more complicated since they will think that they were free to check the proof themselves and they will not accept the idea that they have been letting themselves getting indoctrinated.

More on the genesis of this book

ALOE deals with logic and inference and thus keeps some distance from number theory and issues of the infinite. Historically, logic developed parallel to geometry and theories of the infinite (Zeno's paradoxes). Aristotle's syllogisms with *all, some* and *none* helped to discuss the infinite. Yet, to develop logic and inference proper, it appeared that ALOE could skip the tricky bits of number theory, non-Euclidean geometry, the development of limits, and Cantor's development of the transfinites. Though it is close to impossible to discuss logic without mentioning the subject matter that logic is applied to, ALOE originally kept and keeps some distance from those subjects themselves.

But, since logic uses the notion of *all,* it seems fair to ask whether there are limitations to the use of this *all*. Thus it is explained why ALOE 2007 said something about Cantor's Conjecture and why this present book came about.

ALOE rejected Cantor's diagonal argument in 2007 by the Paul of Venice consistency condition, see page 82 above. In 2012 CCPO-PCWA developed the notion of *bijection by abstraction* which is a counterexample to the idea that there would be no bijection between the natural and real numbers. This notion captures the amazement, if not paradox, by identifying the three sides of the square that we know and the fourth side that creates a sense of mystery – see page 43. The notion intends to *emphasize* the mystery, rather than suggesting to stop thinking about it. New ideas are greatly welcome, but the suggestion of transfinites apparently is a misconception.

These results in 2007 and 2012 already suggested that something would be wrong with ZFC that allows Cantor's Conjecture. A discussion about this with mathematicians was hindered by the phenomenon that they tended to regard these results as philosophy and not mathematics. It is quite conceivable that ZFC theorists simply don't have this affinity with both elementary logic and empirical science that I can advise to every student.

A visit to a restaurant in October 27 2014 and subsequent e-mail exchange with Edixhoven (Leiden), co-author of Coplakova et al. (2011), led to the memos Colignatus (2014ab), and the inspiration to think about ZFC. [54] Originally I asked Edixhoven the question here on page 86 on the relation between Ψ and Φ. Edixhoven agreed that the Pauline consistency condition should have no effect, and I asked him to explain that it could have an effect. Since November 2014, see Colignatus (2014ab), I have not received a response even though the question was clear and articulate. Hart (TU Delft), who has invested deeply into the transfinites, simply rejects that Φ belongs to ZFC, thus neglects both the anomaly and its ramifications – including the logic that is now formulated in Lemma P that it belongs to ZFC. In this period I only asked questions and explained the relevance of those, and kept some distance from ZFC since I am no expert.

Seeing that I would not get answers from these experts and having seen ZFC more often in the course of these exchanges, I decided on the morning of Wednesday May 27 2015 to provide for the answers myself, and established major points for the singleton case, notably Table 8, and subsequently also the general Lemma P before noon. It is a joy to see that basic propositional logic still is so useful to resolve such issues like in this book. There was a curious delay in finalising the analysis. I had introduced the different names Ψ and Φ for clarity, given their different definitions. This made it more difficult to see though that Table 8 directly gives the inconsistency for Ψ alone. The rest is essentially didactics.

Colignatus (2015e) reports a breach in scientific integrity.

[54] This book uses ZFC and no other system since Edixhoven said that he restricted his attention to this.

Discussing Cantorian versus Pauline ZFC

Part 3 shows that ZFC is inconsistent, using only the singleton. The Pauline interpretation is not required, since the tautologies T1 and T2 use only propositional logic. The following applies when one does not know about Part 3 or rejects its deduction.

This book discusses the choice of various possible axiomatic systems for set theory. The chosen system defines what is *well-defined according to that system*. ZFC provides for ZFC-sets. BST (to be developed) provides for BST-sets. A common idea is that developers of ZFC have succeeded in capturing the notion of well-defined-ness best. This assumption is often not mentioned in proofs (e.g. page 75, the *addendum*). This book challenges that idea. It may be that ZFC-PV or BST capture it better.

Let us now consider ***Cantorian*** versus ***Pauline*** approaches to ZFC. The following arises when we look at these approaches from the angles of research strategy or didactics.

Page 84 above defines the Cantorian versus Pauline readings of the Axiom of Separation. Lemma P shows that the Cantorian reading implies the possibility of the Pauline one. We take account of two cases:

(α) Lemma P is known and accepted. What are the consequences ? (α1) Does one hold that the Pauline reading does *not* affect ZFC ? Or is some effect accepted as well, and which one ? (α2) Does one also accept Theorem ESV ? (α3) Does one accept the *bijection by abstraction* ? (It is basically only a *definition*.)

(β) Lemma P is not known or not accepted. In this case the different positions are regarded as deriving from a *difference in personal views* only. Philosophy. How does this affect the perception of the challenge to ZFC ? The Pauline view will be ***tolerant,*** the Cantorian view will be ***defensive.*** What are the reasons for tolerance or defence ?

The following was written before inconsistency was establised for the singleton, see Part 3, and the text still is relevant, but it gains most relevance if we assume an exchange of views with a set theorist who has no awareness of Part 3.

1. The Pauline cq. tolerant interpretation

Page 84 presents the view that the condition "*B* is not free in $\gamma[x]$" is satisfied when *B* is bound by the existential quantifier. This is a **Pauline** interpretation. It causes that Ψ and Φ belong to ZFC, so that the question about their differences (and the anomaly) can be discussed *within* ZFC. This book shows that (also) this leads to a rejection of ZFC as a proper axiomatic development for set theory.

This Pauline approach acknowledges the existence of the condition "*B* is not free in $\gamma[x]$" since this highlights the challenge to ZFC. However, ZFC is ambiguous. There are versions available without this condition. We rely on Coplakova et al. (2011:145), but we had to insert the by-line in SEP, see page 61. A notable example is also Weisstein (2015) of MathWorld.

3. Axiom of Subsets: If φ is a property (with parameter p), then for any X and p there exists a set $Y = \{u \in X : \varphi(u, p)\}$ that contains all those $u \in X$ that have the property φ. (also called Axiom of Separation or Axiom of Comprehension)

$$\forall X\, \forall p\, \exists Y\, \forall u\, (u \in Y \equiv (u \in X \wedge \varphi(u, p))).$$

In Hart (2013:29) we find the following formulation that can be judged to be at least *ambiguous*. Its formulation allows the *Pauline* interpretation, i.e. that the test on the free variables of $\gamma[x]$ happens under the existential quantifier. (i) *B* can be regarded as a given or constant, and not a free variable for $\gamma[x]$, and then one finds $y = B$. (ii) Is it really precluded that one starts out with an independent *y* but then deduces that $y = w_1$ would be a solution ? It would be an additional assumption ('clarification of how to read the axiom') to adopt the Cantorian defence below.

Axioma 3. Het Afscheidingsschema. Als ϕ een (welgevormde) formule is met zijn vrije variabelen in de rij x, z, w_1, …, w_n (allen ongelijk aan y) dan bestaat bij elke verzameling x een verzameling die bestaat uit precies die elementen van x die aan ϕ voldoen:

$$(\forall x)(\forall w_1)\cdots(\forall w_n)(\exists y)(\forall z)\big(z \in y \leftrightarrow (z \in x \wedge \phi)\big)$$

2. The Cantorian cq. defensive interpretation

A **Cantorian** approach to ZFC would be to maintain that authors in the world are free, but that only the interpretation of ZFC is acceptable that blocks Φ. This approach is to reject the Pauline interpretation, and deny the challenge.

The defence is: $\gamma[x]$ *originates as an independent expression. It is* ***put into*** *and is not* ***lifted out*** *from the Axiom of Separation and its existential quantifier.*

The Cantorian view remains: the condition "*B* is not free in $\gamma[x]$" allows the creation of Ψ but blocks Φ. This also presumes that Φ cannot be a constant. (Some systems allow that 'variable' might indicate symbols: constants and proper variables.)

This Cantorian approach seems to have the appeal of solidity, i.e. that ZFC exists now for some time, that some researchers find fransfinites attractive and a work of art, and that the view might be maintained even in the face of the Paul of Venice challenge. But it comes at the price of some questions that are not answered except by dogma.

3. Questions for the Cantorian interpretation

(1) It does not matter if something is *put into* or *lifted out* while the relevant issue concerns the scope of the existential quantifier. Yes, there is predicate logic that would allow the creation of $\gamma[x]$. But the idea is that set theory harnesses predicate logic.

(1a) A defence is that $\gamma[x]$ is not lifted out from the formula, but already existed as an independent expression, say as a predicate in predicate logic. This merely shifts the problem to predicate logic, and without looking into that problem there. One idea for set theory was to harness predicate logic, but if one drops this idea then how is one to establish well-defined-ness for predicates ?

(1b) To look at $\gamma[x]$ within the Axiom of Separation *is the easiest way* to see the scope of the existential quantifier. Why would one lift it out ? Why judge it separately ? Axioms are not posed out of thin air, but we generally look for a reason.

(1c) The transfinites are no reason, when they can be diagnosed as an illusion based upon not-well-defined sets. They don't exist in reality, only in fancy because of ZFC. Sticking to ZFC-Cantorian merely because of the transfinites is begging the question.

(2) Why deny the freedom for researchers to adopt the Pauline interpretation ? Didn't Cantor himself argue that mathematics allows for freedom ? Why could ZFC-PV or BST not be fine axiomatic systems, that deserve mention in an introduction course on set theory ?

(3) Beware of theology and the dispute between Gomarus (predestination) and Arminius (freedom of choice). A former version of PV-RP-CDA-ZFC met with criticism that the Pauline approach was based upon a 'misconception' and 'elementary error', and that the use of the consistency condition 'was not allowed'. While the analysis at that time only posed a problem, clearly formulated so that others could understand it, if only they opened their minds to it. But this reader did not see the problem but only error and sin. This reader apparently was so married to ZFC in its Cantorian interpretation, that he did not see alternatives, and he was no longer aware that set theory is about studying axioms for sets

and not just ZFC. Instead, in reality, there are alternatives to ZFC, also alternatives in interpreting ZFC. (It is fortunate that there now is Lemma P, so that the power of the *rational argument in mathematics* can be relied upon to help resolve an issue. In a way though this is too simple, since arguments (1) and (2) were already convincing mathematically. This also holds for the next points.)

(4) While (2) emphasizes freedom, there is also necessity. While the Cantorian approach blocks the question on Ψ and Φ *within* ZFC-Cantorian, ZFC-defenders must acknowledge that the question exists *within* ZFC-Pauline.

(4a) Thus, also, if they reject to answer the question *within* ZFC-Pauline, they must answer it *across* variants of ZFC.
(4b) Thus, please, explain why Ψ in ZFC-Cantorian generates transfinites and Φ in ZFC-Pauline does not ? See page 82. What causes the difference, while the cause is a consistency condition that should have no effect ?
(4c) Rather than neglecting the issue, and getting lost in the illusion of the transfinites, ZFC-defenders might feel obliged to explain why that difference arises. It is not only the consequence of the evaluation $\gamma[x]$, but also the impact of the consistency condition. Is it a *consistency* condition or not ? How can it be that the insertion of consistency can cause the collapse of Cantor's Conjecture and the transfinites ?
(4d) While questions (4a) - (4c) allow for that Lemma P and Theorem ESV are not known, they become more acute when those and their proofs are accepted. One must now answer such questions across systems, say between ZFC and ZFC-PV.

(5) It would help to establish whether this challenge to ZFC, based upon the Paul of Venice condition, is *new* to researchers of set theory or not. I have no knowledge on this.

(5a) If it is new, then perhaps the tradition of ZFC has been based upon an illusion.
(5b) If it is old, then perhaps it was not properly evaluated in the past.
(5c) There is also the variety of formulations of ZFC that needs explanation, compare e.g. Coplakova et al. (2011), Hart (2013) and Weisstein (2015).

(6) How is it with naive notions like the *set of all sets* ? Hart (2015) describes the incongruity of using formal ZFC-sets and informal 'classes'. Would it not be mathematically attractive when these could be brought in line within one consistent system ? The objective is not to limit the freedom in mathematics but to find an adequate system for education, see the Preface to this book.

((7) One cannot persistently neglect Part 3.)

4. Diagram of views when the propositions in Part 4 are known

Let us assume that the propositions in Part 4 are known and accepted.

Figure 3 shows how the views on ZFC relate to each other and to the overall *Intended Interpretation.* The figure indicates only the *expressions* that are allowed. In the Cantorian reading of ZFC, the bijection by abstraction *Б* is rejected, and its Ψ[*Б*] disappears in terms of 'content' (leaving only its grin). But a non-surjective *f* would generate its Ψ[*f*], and thus we can indicate that such an expression is allowed. The advantage of this presentation is also that it shows that the issues can be discussed *across* systems.

Notions for reading the figure are:

(1) The *roman letters* indicate subareas, and the *text labels* their unions.

(2) ZFC-Pauline = $M \cup P \cup R$. ZFC-Cantorian = M. ZFC-PV = $R \cup S$.

(3) The *Intented Interpretation* might be $K \cup M \cup P \cup R \cup S$ but perhaps parts drop out. Ideally, ZFC and ZFC-PV are a model for the Intended Interpretation. Then at least $K = Ø$.

(4) Ideally ZFC and ZFC-PV have the same Intended Interpretation, so researchers have to determine which has to give way. (An important case is Δ1 in Part 3, see page 66.)

(5) Lemma P is that ZFC-Cantorian implies – is a subset of – ZFC-Pauline.

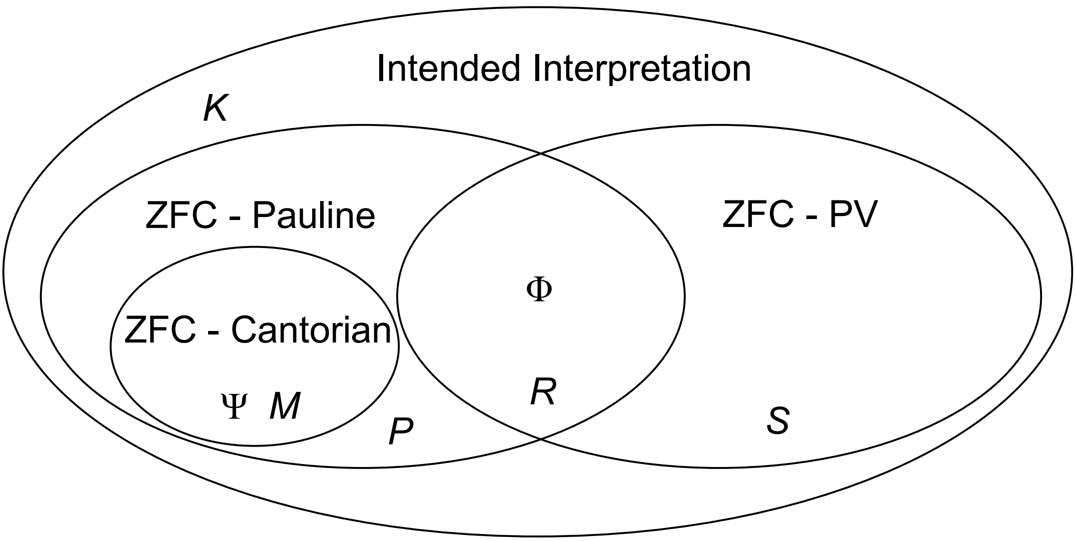
(6) Corollary Q holds that Φ may differ from Ψ. The symbols are distinct in the figure because it looks at the formulas. The sets might disappear e.g. if the bijection by abstraction is rejected. ZFC-Cantorian might hold that on content Φ = Ψ.

(7) Transfinites do not exist out of ZFC-Cantorian (unless there are *really* valid proofs), and thus would not be part of the Intended Interpretation. Thus we should cut out a part of *M* that depends upon Ψ. Thus ZFC-Cantorian has to explain why it includes a part that would not belong to the Intended Interpretation.

(8) ZFC-Cantorian still must explain the difference between Ψ and Φ. One would tend to hold, as in (7), that Φ falls under the Intended Interpretation, so that Ψ has a problem.

(9) This book only looked at *M* and *R*, and didn't look at other areas. Since ZFC is restrictive, it is not unlikely that $S = Ø$ and $P = Ø$. The relevant question is: what can ZFC-Cantorian achieve in *M* that would fall under the Intended Interpretation, but which cannot be achieved by ZFC-PV in *R* ? (The transfinites are excluded because of (7).)

Figure 3. Venn-diagram of ZFC versus ZFC-PV, when the propositions in Part 4 are known and accepted: allowed formulas only



5. Diagram of views when the propositions in Part 4 aren't known

Let us assume that the propositions in Part 4 are not known or accepted. (This is at least the situation before the June 4 2015 version of PV-RP-CDA-ZFC.)

Figure 4 shows how the views on ZFC relate to each other and to the overall *Intended Interpretation.* The figure indicates only the expressions that are allowed. In the Cantorian reading of ZFC, the bijection by abstraction *Б* is rejected, and its Ψ[*Б*] disappears in terms of 'content' (leaving only its grin). But a non-surjective *f* would generate its Ψ[*f*], and thus we can indicate that such an expression is allowed. The advantage of this presentation is also that it shows that the issues can be discussed *within* ZFC-Pauline.

Notions for reading the figure are:

(1) The *roman letters* indicate subareas, and the *text labels* their unions.

(2) ZFC-Pauline = $R \cup M$ (assuming above P = Ø). ZFC-Cantorian = $L \cup M$. (Lemma P found that L = Ø, but one does not know or accept this.)

(3) The *Intented Interpretation* might be $K \cup L \cup M \cup R$ but perhaps parts drop out. Ideally, ZFC is a model for the Intended Interpretation, and at least K = Ø.

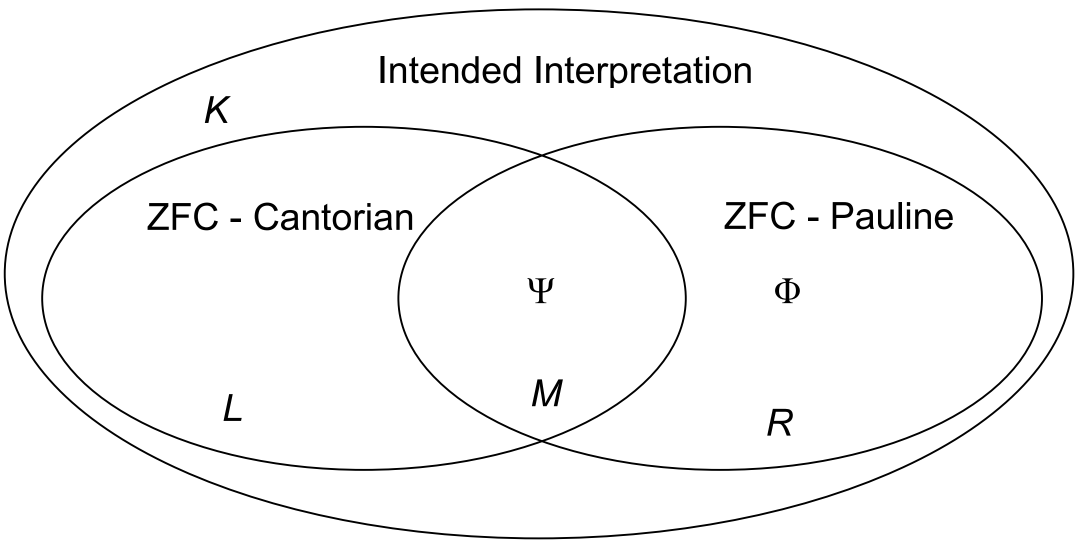
(4) Ideally the two ZFC readings have the same Intended Interpretation, so researchers have to determine which has to give way. (An important case is Δ1 in Part 3.)

(5) Transfinites do not exist out of ZFC-Cantorian (unless there are *really* valid proofs), and thus would not be part of the Intended Interpretation. Thus we should cut out a part of M that depends upon Ψ. Thus ZFC-Cantorian has to explain why it includes a part that would not belong to the Intended Interpretation.

(6) ZFC-Cantorian holds that R would be nonsense: since Lemma P is unknown. But then ZFC-Cantorian must first explain the difference between between Ψ and Φ. One would tend to hold that Φ falls under the Intended Interpretation, so that Ψ has a problem.

(7) This book only looked at M and R, and didn't look at L. (Lemma P found that L = Ø.) The relevant question here is: what can ZFC-Cantorian achieve in L that would fall under the Intended Interpretation, but which cannot be achieved by ZFC-Pauline ?

Figure 4. Venn-diagram of the Cantorian and Pauline readings of ZFC, when the propositions in Part 4 are not known or accepted: allowed formulas only



About: Pierre van Hiele and David Tall: Getting the facts right

In this Framework for Discussion it is relevant to know that not only research mathematics (RM) is at a loss, but also the mathematics education research (MER).

Elegance with Substance (EWS) not only analyses errors on didactic content but also provides the political economy of the mathematics industry, in which MER has a key role. From the book cover text:

> "The failure in mathematics and math education can be traced to a deep rooted tradition and culture in mathematics itself. Mathematicians are trained for abstract theory but when they teach then they meet with real life pupils and students. Didactics requires a mindset that is sensitive to empirical observation which is not what mathematicians are basically trained for."

While MER should be an empirical study, it appears to be misguided by the same tendency towards abstraction. Nowadays there is more emphasis on the application of statistics, but, then the mental training towards abstraction asserts itself again, and the emphasis is on statistical significance (precision) and not on relevance and effect size, see Ziliak & McCloskey (2007). The basic skill of observation tended to be forgotten but fortunately there now is more attention e.g. in the concept of Lesson Study.

I had hoped that researchers in MER would recognise the diagnosis in EWS and respond favourably towards improvement. The result was mixed, and then apparently a few critiques are sufficient to cancel the positive views and make everyone a bit wary.

Apparently there is more to empirical research that is less easily mastered. A key example is the following. Eddie Gray and David Tall coined the word and notion of the *procept*. [55] This suggested to me an openness to empirical observation of how students really study. But the situation appeared to be somewhat differently than expected.

This case is important for this book given the key role of the Van Hiele levels of abstraction, see page 18. Let me refer to my paper on this, and reproduce its abstract:

> "Pierre van Hiele (1909-2010) claimed wide application for the Van Hiele levels in understanding, both for more disciplines and for different subjects in mathematics. David Tall (2013) suggests that Van Hiele only saw application to geometry. Tall claims that he himself now extends to wider application. Getting the facts right, it can be observed that Tall misread Van Hiele (2002). It remains important that Tall supports the wide application of Van Hiele's theory. There appears to exist a general lack of understanding of the Van Hiele – Freudenthal combination since 1957. Part of this explains the current situation in the education in mathematics and the research on this." (Colignatus (2014c))

While this book paves the way for an acceptable treatment of set theory and number theory in highschool and matricola for non-math majors, there are ample reasons to be sceptical that the analysis will be adopted by teachers and researchers in MER. There remains a clear role for Parliament.

[55] Albeit that ALOE in 1981 already used the distinction between statics and dynamics.

Part 6. Philosophical aspects

The context of ALOE

1. Theory of Types, proof theory, or three-valued logic

Though ALOE presents elementary logic, it contains various innovations that are relevant but little-known. The common self-referential logical paradoxes have solution methods: (1) the *theory of types* (levels), (2) proof theory, (3) three-valued logic with *true, false* and *nonsense*. ALOE shows that the latter is consistent and superior, with a way to deal with the three-valued-Liar as well. Three-valued logic has a straightforward interpretation and allows the solution of the Liar paradox and Russell's paradox, while the Gödeliar collapses to the Liar in a sufficiently strong system. Russell's solution with the *theory of types* outlawed self-referential terms, and implicitly declared such forms as nonsensical. The proposal of a three-valued logic thus only makes explicit what Russell left implicit, while it actually allows useful forms of self-reference. Gödel's uncertainty due to incompleteness is replaced by an epistemological uncertainty that some day inconsistency might turn up that proves some assumptions to be nonsensical.

It is a useful tendency to rephrase arguments in terms of two-valued logic. Still, this book allows some self-reference in the definition of sets, and thus has to rely on a solution where such self-reference would cause nonsense. Three-valued logic also makes a useful distinction between existence and non-existence of sensical notions versus nonsense itself. A common notion also in two-valued logic is that nonsensical things cannot exist, but that might also cause the confusion that the nonexistence makes the notions involved sensical.

PM. It seems that (constructivist) Brouwer mixed up the notions of truth and proof. It might be that his interpretation of a double negation might differ from twice a single negation. *"Not-not-A"* might mean *"There isn't a proof that A is not the case"* which differs from *A*. It is somewhat of a miracle that Heyting succeeded in finding apparently consistent axioms. There might be an interpretation in terms of a mixture of both truth and proof.

2. An approach to epistemology

A proposal is the *definition & reality methodology.* Youngsters grow up in a language and culture and learn to catalogue events using particular terms. The issue of matching an abstract idea (circle) with a concrete case (drawn circle) is basic to thought itself. For circles we can find stable definitions and this might hold more in general. Questions like "all swans are white" can be resolved by defining swans to be white. The uncertainty then is shifted from the definition to the process of cataloguing. A black swanlike bird may be important enough to revise the definition of a swan. See *Definition & Reality in the General Theory of Political Economy* (DRGTPE, 2000, 2011a). Definitions in economic models may restrict outcomes which other models may not observe that don't maintain those definitions. In the case of space, my suggestion in COTP (2011c) is that the human concept of space is Euclidean, so that we don't have the liberty to redefine it. Einstein's redefinition of space-time may be a handy way to deal with measurement errors but could be inappropriate in terms of our understanding.

With respect to consciousness, language is a bit tricky here. As people experience consciousness, and this experience is created by (what some models call) atoms and energy in the universe, apparently consciousness is a phenomenon created by the universe as well, and in this sense consciousness is as real as those atoms and energy or the universe itself. While atoms and energy seem to be dead categories without pleasure and pain it is strange that there can be a mind that experiences pleasure and pain. One way to approach this is to say that sound, sight, smell and touch are the senses, but that consciousness then is a 'sense' too. See Colignatus (2011e) and Davis & Hersh (1980, 1983:349). This is vague and speculative and not directly relevant for this present book but it seems relevant enough to at least mention it. The point namely is that abstraction takes place in consciousness or that consciousness might actually be composed of abstractions.

Views on constructivism

The analysis in this book may help the definition of constructivism and the delineation of views on constructivism. A very practical result also concerns mathematics in highschool and matricola. It may be somewhat amazing how philosophical and methodological discussion can boil down to a course in highschool. The real challenge is to avoid rote learning and instead to rekindle the processes of wonder and insight.

A context for this discussion is the *Special Issue* of *Constructivist Foundations* of March 2012 edited by Van Kerkhove and Van Bendegem: *Constructivism In and About Mathematics*. [56] There is the eternal tension between mathematics and engineering. Mathematicians are trained for abstract thought and they may lose contact with limitations that are relevant for constructivism that people will tend to regard as rather practical.

In the same way, the mathematical expositions on constructivism, and thus aforementioned *Special Issue*, may be misleading with regard to proper constructivism, since some mathematical assumptions may hang in the air. Let us coin the term *proper* constructivism indeed for what this present book will try to clarify. This does not concern a new branch of constructivism but it is about what any branch would like to contain, namely a balance between abstraction and practical considerations. For example 'strict finitism' (Van Bendegem (2012)) might perhaps allow more abstraction since it by itself already implies some abstraction; and in other branches the abstraction might have to be reduced in some respects.

The discussion concentrates on the continuum and (its) infinity. The quintessential notion to understand constructivism is this: *what Aristotle called the difference between the potential versus the actual infinite.* While Democritos held that division of matter eventually resulted into atoms, Aristotle held that division of space could be continued forever, and thus he helped Euclid in defining a point as location without size and a line as length without width – note the abstraction. Some authors seem to hold (see page 23) that Aristotle rejected the actual infinite but it seems to me that he would not have rejected the actual infinity of the continuum, e.g. the interval [0, 1]. Classical or non-constructive mathematics tends to allow relatively free assumptions on the continuum and even create higher forms of infinity, the transfinites. My suggestion is that mathematicians since Cantor have been *'too abstract and unrealistic'* (in some sense) about those transfinites. A neoclassical – or *proper* constructivist approach, namely constructivism that looks for the balance of abstraction and practicality – can restore sense in many areas affected by mathematics, not the least in philosophical discourse and in the didactics of mathematics.

Quale (2012) mentions different forms of constructivism, next to solipsism, platonism / realism and relativism. Cariani (2012) uses the perhaps more traditional distinction between three approaches of *platonism* (realism), *formalism* and *constructivism* (including intuitionism and finitism). It is dubious whether there is a convincing experiment to distinguish the one from the other, so these labels are likely to refer to flavours in psychology. Indeed, see Cariani (2012:123, right column). Davis & Hersh (1980, 1983:358-359) regard these three different philosophies as the aspects of a multidimensional phenomenon that have to be considered all in order to arrive at a whole. They regard these three even as extremes in abstraction, and they hold that real (mental) activities by mathematicians are of a more practical kind. The words 'matter' and 'mind' could be overused, imprecise, unrealistic, to describe what is really happening, see Davis & Hersh (1980, 1983:410):

> "Mathematics does have a subject matter, and its statements are meaningful. The meaning, however, is to be found in the shared understanding of human beings, not in an external reality."

[56] http://www.univie.ac.at/constructivism/journal/7/2

Perhaps an example of 'shared understanding' is the ‘average length of 10 cars’, which average may be taken to ‘exist’, though can get different values even when the cars are identified, still depending upon method of measurement and, say, temperature.

The discussion becomes even more complicated when mathematics can hang in the air. Perhaps one type of computer program can be seen as constructivist without any doubt but perhaps there are all kinds of variations in computer programs, not only with a common random generator but also with input from outside measurement instruments, that still might be seen as constructivist in some way or other. Rather than approach the issue head on, this book proposes to take *steps,* see page 39, to identify (the) different views on constructivism that pertain to finitism, continuum and infinity.

Constructivist Foundations is an interdisciplinary scholarly journal and only a smaller part of its readers are professional mathematicians. Constructivists should be aware that the very topic of this book requires some mathematical insight. And, as said, this book takes the approach to link up the philosophical discussion with a highschool course, and that ought to be an acceptable entry level. The level of math required is finished highschool or participation in first-year mathematics and logic at a non-math-major level for a field that uses mathematics, such as economics, physics or biology. The book uses elementary mathematical concepts and notations from logic, set theory and functions. There are some key proofs in the body of the text since it is important that non-math majors can verify that professional mathematicians have gone astray in the most elementary manners. Constructivism has been burdened by irrational winds from mathematical quarters and it is essential to show how Occam helps to cut away the nonsense. Readers who have developed an aversion against mathematics may actually be drawn into the argument and discover how things finally make sense. Indeed, one target of the book is to develop the outline of a course on real numbers in highschool. Most readers might want to hold that in mind, while some might perhaps want to skip some details about where Cantor went wrong.

The reader is invited to read Riedler (2005) again, the first editorial on the constructivist challenge, with the catalogue of ten points for the program to meet that challenge. Interdisciplinarity doesn’t mean sacrificing the standards of quality of one field merely in order to create some umbrella for its own sake however vague it is. Instead, the standards of quality of all fields must be maintained if the results are to be useful. For some researchers perhaps like me it comes across as somewhat curious that other people divide up into disciplines while it ought to be clear that you need all to arrive at the best picture. A paper that closely matches those ten points is Colignatus (2011e), *Brain research and mathematics education: some comments* – included here on page 109 – that argues that brain research on mathematics that is intended to be used for its education may go astray when brain researchers do not see that many concepts in math are quite messed up.

Mathematicians are trained for abstract thought but in class they meet with real world students. Traditional mathematicians resolve their cognitive dissonance by relying on tradition. That tradition however is not targetted at didactic clarity and empirical relevance with respect to psychology. The content in the mathematics curriculum has grown over the ages by conscious construction but also as waste flushed onto the shores. A quick example is that the Arabic numbers like 19 are written from right to left (as in Arabia) while the West commonly writes from the left to the right. In pronunciation there is even a switch in order, compare 19 and 29. Another quick example is that two-*and*-a-half is denoted as two-*times*-a-halve, namely 2½. This kind of mathematical confusion applies to the finite, continuum and infinite as well. It even contains pure errors against logic. It is not only a question how the mind constructs those concepts but also whether our concepts are mathematically sound and not messed up like so much else.

A first step towards clarity is to consider the educational context. At issue is not to educate what is in the books on mathematics but to discover what we really would like to teach. Mathematics education requires a re-engineering, and likely in the constructivist manner. If we do this re-engineering, it helps to have some soundness in philosophy as well.

In the Riegler three-dimensional space of *discipline, school and type of enquiry,* this book then can be located as follows.

- Discipline: the education of mathematics, the philosophy of mathematics, foundations of mathematics, philosophy of science, and methodology of science, with aspects on history, cognitive psychology and epistemology.
- School: linking up to the approach to constructivism in mathematics (not in the Riegler list).
- Type of enquiry: conceptual book, to develop philosophical-argumentative support, though with the understanding that philosophy doesn't hang in the air but at least in this case deals with practical questions in epistemology and the didactics of mathematics.

A simple core of this book

When readers progress through the argument in this book, they may think that it is quite complex, but in fact it is rather simple. There is one major goal and that is to introduce a new definition, namely the notion of *bijection by abstraction* – see pages 23 and 43. Though the intention of this analysis is also a contribution to clarity about the definition of constructivism, namely to make room for abstraction too, that contribution quickly narrows down to emphasising a new definition for a minor though apparently key aspect, namely 'bijection by abstraction'.

If the reader keeps an eye on this major goal of this book, to introduce this new definition, then the other aspects in this book can be moved to the background, and then it would seem that the issue is rather simple. The key notion is that a procedural approach to $\mathbb{N}$ and $\mathbb{R}$ is insufficient for constructivism and that abstraction can be taken as an aspect in *proper constructivism*.

Mathematicians might get lost in proof details but we don't necessarily focus on full formality in all that is said. Purely mathematical papers would focus on full formality but *scientific* papers look at the 'intended application' or 'intended interpretation' of the mathematical model. This book contains (formal) proofs and in that respect it has features of a mathematical book. Still, the major issue is the *intended application to abstraction,* and hence major parts of this analysis might likely not fit in a purely mathematical journal and might perhaps not even be understood by pure mathematicians. Mathematicians who stick to two-valued logic may not be aware that they can produce nonsense.

Georg Cantor (1845-1918) claimed to prove that there was no bijection between $\mathbb{N}$ and $\mathbb{R}$, and he created a whole universe of 'transfinites' to deal with the consequences. The suggestion of this book is that Cantor may not have appreciated what abstraction may entail – even though he actually used the notion. William of Ockham (1288-1348) held that complexities should not be increased without necessity. This book uses Occam's razor to cut away Cantor's universe as overly complex and without a base in necessity. Given the wide acceptance of Cantor's results, the opposition to this analysis will be strong. A Cantorian will tend to hold that the definition of 'bijection by abstraction' is vacuous and irrelevant. The purpose of this book is to at least present the definition, so that discussants know about its existence and possibility.

That said, a fairly quick consequence however is that readers may wish to understand more about the definition and its area of application. This is a somewhat dangerous consequence. At this moment the application is tentative and not fully established by itself. It seems relatively easy to generate all kinds of questions about what such abstraction does entail indeed. Such questions and uncertainty may easily cause the reader to reject this analysis. The reader is invited however to concentrate on understanding the definition, and suspend judgement till after subsequent discussions about the application. On the other hand, one should not fear the obvious, since already Aristotle distinguished between potential and actual infinite.

A prime application is in the area of highschool education, where pupils could be presented with a clear and consistent theory of numbers and infinity without the convoluted Cantorian universe of the transfinites. Teachers of mathematics might feel guilty when they don't explain Cantor's universe but they might be happy if there is a sound alternative and when they can explain more about the wonders of abstraction itself.

A key in the education of mathematics

To aid the discussion, my proposal is to use *evidence based didactics of mathematics* as our anchor in a real world activity, to prevent that we get lost in mathematical inconsequentialism. This actually holds for the philosophical discussion about mathematics anyway. The didactics of mathematics are an excellent sounding board for philosophy, and it seems also a necessary sounding board. It would be somewhat curious to hold that the approaches to clarification would be entirely different for philosophy on the one hand and didactics (of mathematical concepts involved) on the other hand, when the subjects would still be the same. It is more reasonable to assume some overlap.

A reader suggested that my reference to the education of mathematics would be a distracting deviation to what the proper topic of this analysis would be – constructivism with abstraction in the rejection of Cantor in favour of Occam - but this would be a misunderstanding. The misguidedness in mathematics and its application can be quite horrible and there is a huge need for anchors. In standard applications we can refer to engineering, and for the present discussion it is a key insight that we can refer to the education of mathematics. In a way, this very analysis is a development of the didactics on the infinite. Also, when a philosopher would object to 'abstraction' as something quite undefined, then we can refer to the classroom situation, and refer to the Van Hiele (1973) levels of understanding, see page 18, while Colignatus (2011e) contains some comments with respect to research on the brain.

The prime lesson is to beware of mathematical confusions. Apparently it cannot be emphasised enough how important that is. (Mathematical) Philosophies for example relating to the Russell set paradox may be misguided, and this kind of misguidedness has been happening in mathematics overall. Given the suggestion above, the key reference is to education. A *didactic reconstruction* results into another curriculum. Already the term 'didactic reconstruction' causes the question how this is seen in terms of constructivism itself.

The discussion here selects for didactics the teaching for secondary education or for first year (matricola) college and university students who will not be mathematics majors. The discussion in this present book can be complex in itself, but it is directed at the more simplified content that is to be taught. The latter simplified world view would still be true (perhaps in quotes: 'true') but merely be less rich in complexity to allow easier understanding at a more basic level. My paper *Neoclassical mathematics for the schools* (2011f) – here page 25 above – uses the label 'neoclassical' but the approach may also be understood as *proper* constructivist: that is, it is constructivism with some scope for human abstraction. It may help the discussion when such constructivism with abstraction is recognised. When a student can construct a path towards understanding then this will seem more attractive didactics than requiring them to merely 'get it' (or tell them to find another job).

There is an additional advantage of pointing to the mess in education. That mess in itself does not prove anything about particular topics in this book of course. When official dogma is that mathematics in the curricula is perfect, then it is less likely that mathematicians goof on Cantor as well. However, when it is called into attention that mathematics in the curricula is a mess, then the likelihood increases that this may also be with Cantor's results.

Brain research and mathematics education: some comments

July 2011 [57]

Abstract

Combining brain research and mathematics education can be advantageous but it is important to be aware of the risks. The present comments focus on Damasio (2010) *Self comes to mind. Constructing the conscious brain* and Colignatus (2011c) *Conquest of the plane*, a primer for the education of mathematics. Mathematical concepts like line and circle are abstract notions like the other mental events that make up the mind. Education can use this to its advantage. Van Hiele levels of abstraction can guide this research. Freudenthal's "realistic mathematics education" tends to contain too much distraction from the required abstract development, and is not the proper answer to the current problems in mathematics education. The main problem in mathematics education is that mathematicians are trained for theory while when they meet students in class then these are real life students, which requires an empirical mind set. Current mathematics education tries to solve its problems by resorting to tradition but this causes mathematical concepts and notations that can be cumbersome and actually hinder understanding. Such confusions need to be resolved, otherwise the link up of brain research and mathematics education will have no results, or even negative ones. The discussion of brain research applied to abstract notions such as line and circle requires a firm foundation in the philosophy of science, and a perception on the Platonic world frequently favoured by mathematicians.

Introduction

Mathematics education can benefit from brain research in various ways. The literature however contains some confusing concepts. Confusion comes with the risk that wrong concepts would become enshrined in education. The intention of the following comments is to generate clarity.

Damasio (2010) defines the mind as consisting of mental events, both conscious and nonconscious (rather than unconscious). If the image of a circle in the mind, e.g. as the Platonic object, is to be a mental event, then supposedly it might exist also nonconsciously. But thinking about a circle is different from not thinking about it. It seems more likely that the circle comes into being as a mental event because of the thought. Damasio also clarifies that the whole brain is required to create consciousness. If the image of the circle exists only because it is created in the conscious mind, then there would be no similar image in the nonconsciousness since this lacks the whole supporting apparatus. If this holds for all such mental events, does that not imply that nonconsciousness does not use *quite* the same mental events as consciousness ? The model then rather is that mind = consciousness (with the equality sign), while nonconscious events are issues for the brain only and have no mental counterpart but only a supporting role. In other words, the circle exists only in the mind = consciousness while the brain keeps components that can be collected by consciousness to form that circle.

The other source of confusion is, remarkably, mathematics itself.

One example is this. My book *A Logic of Exceptions* (1981, 2007, 2011) (ALOE) shows that three-valued logic is required to deal with the logical paradoxes like the Liar paradox or Russell's paradox, including Gödel's "incompleteness theorems" (i.e. rather confusions). If brain researchers follow current mainstream mathematical thought and are stuck in two-valued logic (like the authors of *Logicomix*), they might miss out on some phenomena in the brain related to three-valuedness. Their findings might be less relevant for good education. (Though they would fit mainstream teaching, and potentially, if two-valuedness is the norm, those brain parts might be 'cured' that support three-valuedness.)

[57] http://thomascool.eu/Papers/Math/2011-07-11-COTP-Damasio.pdf also included in SMOJ.

Another example is this. In mathematics education, Pierre van Hiele created the theory of levels. The same words can relate to different concepts, depending upon the level of understanding by the student. We know from Wittgenstein of the *Philosophical Investigations* with his language games that words are flexible anyway. Still, in mathematics and its education there remains a tendency to think that definitions and proofs can be formulated exactly. If one mathematician formulates the Pythagorean Theorem then another shows his understanding by proving it. If brain research is to try to support education in this, how can it deal with the gap between Wittgenstein's language games and the mathematical claim of exactness ? Pierre van Hiele's levels provide a bridge. But if the mathematical symbolism is formulated crookedly, for example with mixed numbers like 2½ that should rather be conceptualized and also written as 2 + ½, then we need a bridge *plus* a supporting coil and perhaps an additional twist. The circus on fractions in elementary school derives from failed didactics, and this would hinder brain research rather than create scope for its practical use.

As mathematics and its education are best developed within philosophical understanding, the discussion below will also consider Cartesius's "Cogito ergo sum" and consciousness and the self. Mathematics is often understood in Platonic abstraction, and its elements might well be as abstract as the mind itself. A circle as an ideal object might be made of the same 'stuff' as the mind itself.

My books *Elegance with Substance* (2009) (EWS) and *Conquest of the Plane* (2011c) (COTP) have short references to brain research, notably Damasio *Looking for Spinoza* (2003) (LFS). It will be useful to extend a bit and also update to Damasio *Self comes to mind* (2010) (SCTM). Perhaps the title should rather read "Some comments on Damasio and EWS & COTP" but the impression is that there is a wider consequence.

Prolegomena

When we combine philosophical ideas with practical matters such as brain research or didactics of mathematics then all kinds of angles creep in that increase confusion. It will be useful to mention some foundational points without further discussion.

(1) ALOE:179 discusses that there are three kinds of approaches – determinism, randomness and volition – but that there is no experiment to determine which is the true state. At best we can design models that illuminate the different angles and act with pragmatic choice.
(2) Feeling and rationality can be seen as different dimensions rather than as opposites to one another (in only one dimension). (Colignatus (1996) figure 2.) Though they may occur in the mind/brain as a soup, and be linked by necessity, they can be separated by abstract analysis.
(3) One way to see the brain is as a processor of information. Feelings can be seen as information at emotional locations. The idea is to handle information as efficiently as possible, at lowest cost, in particular of energy. A cheap way to handle (conflicting) information is to discard it (cognitive dissonance). An efficient manner is to create definitions with maximal explanatory power. The notion 'all swans are white' can be treated as a definition rather than an empirical proposition that can be contradicted by a black swan.
(4) Morality is related to survival. David Hume correctly diagnosed that 'is' and 'ought' are different dimensions. This distinction however is analytical while the powerful impact of morality derives from the need for survival (of the species). A bridge between 'is' and 'ought' might be to behave as is predicted what will be morally best.

Our concept of reality

When clarifying the terminology for reality, a useful aspect is the concept of *continuity*. An example is a circle. As neurons are discontinuous, how does the brain handle 'reality' ?

Basic reality can be denoted as **reality0** and we will generally not know it. An example is an electron, which is a theoretical term. Kant used the term ‘noumenon’ for such things-in-themselves that we will never fully know. That term however is somewhat problematic since we may still know some things, such as electrical charge (also a theoretical term) of that electron. It suffices to assume reality0 without getting lost in questions about what ‘can’ be known about it.

Then there is the **reality1** created by the map in the brain. Apparently there are layers of cells, and the person who looks at a circle that is drawn on paper or thinks about a circle apparently has nerve cells activated indeed in such a circular form. Clearly, though, those active cells are in discrete number, and the notion of continuity must be added separately.

Subsequently, there is **reality2,** for example a circle in the mind. Reality2 differs from reality0 and perhaps does not deserve the label ‘reality’. Yet it functions in the mind as our reality and thus may be labelled so. Obviously there are versions reality2a, reality2b, … with different flavours (language games).

Note that reality1 is much like reality0, in that we can hardly verify how the brain actually maps reality0. Thus reality1 would rather tend to be a model that we make in reality2. But it can be idenfied as a logical phase.

Within reality2 there are space2 and time2. It is not entirely sure that there are similar aspects within reality0 that we could call space0 and time0. Most likely it is rather useless to query about space0 and time0. It suffices to identify reality0 as a container. It could be sufficient that concepts space2 and time2 work.

Space2 is the concept of space as it naturally arises in the mind, and it is Euclidean. Within that space2 we can imagine other models, for example the non-Euclidean rules for the surface of a sphere. It is dubious whether it is wise to call these models ‘space’ too. A name is only a name, but if there are ontological suggestions then we should be rather careful about those.

My impression is that physicists are hopelessly confused in their verbal explanations about their models of reality. When two objects pass each other in free space then there is no obvious frame of reference, which is the base for the theory of relativity. But if I turn my head then it is silly to take my head as the frame of reference and say that the universe is spinning around – since huge masses would cover huge distances in mere seconds. It makes sense to look for the center of gravity in the universe for a frame of reference. In the 19th century physicists lost much time in discussing the ‘aether’ and they resolved their confusions by concentrating on measurement results. Einstein’s ‘contracting space’ describes such measurements, by resolving all measurement errors into a ‘flexible space’. On the contrary, I think that it is better to remain in Euclidean space and continue speaking about measurement errors. Otherwise the notion of ‘space’ is badly defined. I myself can only understand non-Euclidean ‘space’ like the surface of a sphere when I imagine that sphere within an Euclidean space. A short discussion is in COTP chapter 14.

Consciousness as a primitive concept

Consciousness is a primitive concept. It is understood by the conscious person. If it is not experienced then it cannot be explained. If it is experienced then its existence can be communicated to another person who experiences consciousness as well; but only its existence, and it still cannot be explained what it is.

The brain creates the mind apparently in electrical pulses of a certain length. The eye only has a small sensitive focus and it scans the world to build an image, that fits within those pulses. If awareness indeed has a full delay of 0.2 milliseconds then an atomic blast that destroys the brain leaves a mental after-life of 0.2 milliseconds. Likely though there still is some ‘real-time’ linkage.

Looking for a simple model of this kind I found Bruce Hoeneisen (2002) at Fermi Lab. The short paper and model explain how feedback loops can maintain an image in short-term memory, and how long term learning and recognition can occur. The bio-engineering principle thus seems to be clear.

Terminology

Damasio has the following use of terms:

- Mind consists of mental events, conscious or nonconscious.
- A baby has a mind but still no ego and hence no consciousness (and only nonconsciousness, like sleepwalking, or a state like mankind must have been during much of evolution).
- When "self comes to mind" then the child grows aware and gets conscious.
- A person can sleep and not be aware (in the sense of awake) but still have a mind and a self, for example in dreaming (e.g. as shown from dreams that are remembered).

My suggestion is not to adopt this terminology. In my terminology – that in my perception is rather common – mind and consciousness are other words for the same. The mind is awake **and** aware **and** equipped with a sense of self. Thus mind = consciousness.

My **preferred usage of terms** remains with Merriam-Webster (2011). In particular for **mind** I confer with explanation 2a and 2b (while 2c is Damasio's choice but problematic):

'2a: the element or complex of elements in an individual that feels, perceives, thinks, wills, and especially reasons;

2b: the conscious mental events and capabilities in an organism;

2c: the organized conscious and nonconscious adaptive mental activity of an organism'.

As Damasio clarifies, the whole brain is required to create consciousness. Mind / consciousness only works because there is a large apparatus working at the nonconscious level. For example, the very experience that I think about a circle and handle it, can only happen because of the combined effort of all brain parts. This cannot happen at the nonconscious level since there is no supporting apparatus for it. At the nonconscious level there are only brain events – and not that concept of that circle.

A mental act of thinking about a circle, i.e. The thought of a circle, only 'exists' when awake and aware (which rather is the self). My preference is that thinking about a circle (e.g. as a Platonic ideal circle) can be classified as a 'mental event'. The nonconscious brain may contain a map of a circle at the level of reality1, but it only becomes the mental object of a circle when it enters the mind and becomes part of reality2. It is not a convincing suggestion that reality2 can also exist 'nonconsciously', as if all these mental objects are also available 'nonconsciously'. Reality1 would contain the elements that can be combined to create reality2. (And it does not seem proper to assume that there is a reality3 consisting of 'reality2 turned nonconscious'.)

It would be a (bad) figure of speech to say 'I was nonconsciously thinking about a circle when suddenly the Pythagorean Theorem popped up in my mind.' Properly seen, nonconscious thought is different from conscious thought, and the associated 'circles' would not be the same.

Saying that a baby is unconscious tends to mean that it is not awake, for example due to a car crash that knocked it unconscious. It is only because of brain research that we now employ the term 'nonconscious', and we may use it to mean that a baby has no developed self. The change of language game is a bit awkward / surprising. However, all this should not force us to change our basic vocabulary. I would rather say that a baby has an 'less developed mind' and 'low level of consciousness' and 'less developed self'. Damasio's terminology 'self comes to mind' then would rather be 'the child develops a mind = grows conscious' (by passing particular tests on levels of consciousness).

It is surprising that Damasio in his effort to reach a wider public departs from standard English and attaches different meanings to "mind" and "consciousness" than the standard vocabulary gives us – and adopts the confusing use of Merriam-Webster entry 2c. Perhaps it is the advantage of simple mathematical concepts like line and circle that help us to cement that standard vocabulary as right and proper, and convince Damasio that he should adapt his terms in new editions.

Consciousness as an experience

What standard science cannot explain is the experience of consciousness itself, nor, importantly, sensations of pleasure and pain as they occur in the mind. Inanimate atoms bump around without pleasure and pain, how can it be that e.g. pain feels so horrible in my mind ? It is not sufficient to point to pleasure and pain processes in the brain. What is at issue is the mental experience.

The standard approach is to accept that the universe has human beings who say that they are conscious. In reductionist mode, consciousness is seen as an emergent property of unconscious particles and waves. This is adequate with respect to reality0 but it inadequately denies the mental experience in reality2.

While we are accustomed to recognize the five senses of touch, tasting (touch by molecular form), smelling (touch by molecular form), hearing (movement of air) and seeing (touch by light), and while we are increasingly including other aspects such as balance or sense of temperature or magnetism, it might be a suggestion to see the mind as another 'sense'. The mind is a 'sense' in that it observes something special from reality0 that is not observed by the other senses. One might argue that physical 'matter' and 'electromagnetic waves' create the brain and the brain creates the mind, ergo: the mind must be physical too. Mankind has tended to reject this view. As a conscious mind myself I follow that tendency since for me too it **seems** that there is something quite un-physical about my conscious mind. Let us be exact: it is un-physical2-standard [in terms of standard physical models that we design in reality2], but it will still be physical0 in terms of reality0 since the latter is our definition of all that is.

For me that 'seems' is so strong that I would rather say **appears**. While the brain is physical1 and apparently consists of atoms that bump into each other without pleasure and pain, the mind is non-physical in standard physics, i.e. un-physical2-standard. The brain creates the situation that a conscious human being has that experience of consciousness – with pleasure and pain. Thus arises the hypothesis that a certain constellation of atoms might be sensitive to another dimension that is not directly accessible by the normal five senses (forms of touching) that deal in commonly understood manner with reality0.

The mental experience, including pleasure and pain, regarded as a 'sense', need not be more complex than the normal five senses. It could already exist in single cells, as Damasio explains that those already show reactions that remind of reward and punishment systems. Clearly, we already can make robots with light sensors that 'look for light' to charge their batteries, so that feed and flight mechanisms don't require such a sixth sense. If a cell has a storage of energy it can 'reward' itself by its release. Inanimate feed and flight however do not explain the mental experience, neither in the aggregate. The existence of that robot model does not mean that single cell organisms work like that.

If we agree that reality0 (our definition of all that there is) has a dimension that relates to mental experience, such as pleasure and pain, that are non-physical2-standard, then single cell organisms might use a simpler feed and flight system by directly plugging into pleasure and pain sensations that occur in that mental dimension. Subsequently, the brain builds up from such single cell mechanisms to copy the similar process at the macro level, as feed and flight are evolutionary successful at that level too. There are the two models that the brain either collects and manages myriad sensations of pleasure and pain or mainly magnifies those of some core cells. It remains to be seen whether this hypothesis generates testable propositions or only remains a logical possibility that serves to perhaps satisfy a curious mind.

The above seems just a reformulation of the old suggestion of a Platonic world where ideal concepts might 'exist', and even a human soul as part of a deity. It is not quite the same. The common ground is the acceptance that consciousness does not fit physical2-standard models and thus is non-physical2-standard. The difference is the intention to work with testable hypotheses.

In a discussion on brain research and mathematics education it is useful to have this paragraph on the Platonic world. It is an obvious question "where does this ideal circle exist

?" and apparantly many mathematicians tend to have a Platonic conception about this. The answer suggested here is that abstraction does not imply 'existence' in terms of reality0. Consciousness would however be part of reality0. Our universe has human beings who say that they are conscious. The "Cogito ergo sum" is a valid argument of an intellect observing existence. Pleasure and pain are such that there are no good alternatives but to accept reality0 too. The (creation of the) thought of a circle in a conscious mind does not imply anything other than what happens similarly when a circle is drawn (approximated) on a piece of paper: it is not eternal, like the standard Platonic world would suppose.

PM. Smith, in Descartes (1958), observes that Augustinus already formulated the "Cogito ergo sum" argument but not compactly. For various years I myself adopted the idea that consciousness was rather an illusion in an entirely physical world, yet, an illusion presupposes some intellect that suffers it, which is the "cogito ergo sum" argument. We can observe a dog running after a ball and presume that it has all kinds of illusions, and obviously it is not conscious as in recognizing a mirror image of itself, yet we should not let ourselves be carried away by *sorites* types of paradoxes.

Mind – body equivalence

As Damasio uses 'feeling' for the mind and 'emotion' for the body, I have been tempted to suggest to use Anglo-Saxon English for the mind and Latin-French English for the body. (Shakespeare mixes these uses too, for both the common people and the high court.) But I am afraid that this would not do. It is better to label with 1 and 2, or (our model of the) brain and mind. Thus we have brain [reality1] and mind [reality2], and the question arises how these are linked.

Damasio (2010) forwards the hypothesis – and the Note in his appendix p314-317 is surprisingly the clearest on this: 'that mental states and brain states are essentially equivalent.'

A key paragraph is Damasio (2010:316):

> 'Accepting the hypothesized mental/neural equivalence is especially helpful with the vexing problem of downward causality. Mental states do exert their influence on behavior, as can easily be revealed by all manner of actions executed by the nervous system and the muscles at its command. The problem, some will say the mystery, has to do with how a phenomenon that is regarded as non-physical – the mind – can exert its influence on the very physical nervous system that moves us to action. Once mental states and neural states are regarded as the two faces of the same process, one more Janus out to trick us, downward causality is less of a problem.'

I agree with this. At the same time however he emphasizes that the whole brain is required to generate consciousness. Is this still consistent with that equivalence ? If I study the same proof on the Pythagorean Theorem either sitting in a train or in a chair at home, I suppose that my mental states would be the same but brain elements would be different since in the first case they have to deal with noise and wobbling of the train. Perhaps the hypothesis is saved by the qualifier 'essential'.

I would rather use 'association' instead of 'equivalence'. Logically it would seem strange that the same brain state might generate different mind states (for where would the random element come from ?). Conversely different brain states might generate the same mind state. (We would have to rely on the person who is experimented with whether it indeed is the same mind state.)

Fully adopting 'equivalence' might have the awkward consequence that physical reality0 deterministically generates mental states. I don't mind such models when they enhance clarity but I reject the idea that they are more than models – since what is the proof ? [We will never know reality0 anyway.]

Subsequently, with respect to that key paragraph, I would like to suggest an analogy for the relation between the mental experience and the process of organisation of brain activity.

An analogy is gravity. In the Newtonian model a shift of my hand has an instant impact on the Moon. Possibly at the Big Bang not all dimensions have exploded and there still is an instantaneous connection between all matter. Another model might require gravity waves that travel at the speed of light. A deterministic model also explains the move of the hand, assumes a common cause and then a simultaneous development such that both my hand moves and the Moon is affected. With the latter analogy we may model states of brain and mind, without getting lost in causality.

Thus what remains is that brain mechanisms evolve to ever better organisation, and that the latter is experienced in the mind. It is not that the mind 'causes' organisation but the ever better organisation at the brain level carries the flag of ever better mental states. As long as we are aware of the proper causal order, figures of speech that employ downward causality might help to increase understanding how mental concepts affect actions.

This usefulness is increased by the social environment of the human species. To my taste Damasio does not mention it sufficiently often (neither in the development of the mirror neurons) that mankind developed in a social situation. If we do include it with more emphasis then we see that the flux of social events is quite 'unphysical' as the mind (except for awareness). For example, non-physical minds understand what happens when a mother embraces her child. Physics shows us two masses huddling together, and nothing more. There is no need to suggest that the non-physical [reality2] affects the physical [reality0], since there is a physical apparatus as a common cause. Yet it is true that my conscious understanding of these events includes mental notions. And organised minds communicate in a social fabric, that adds another layer of organisation namely in all kinds of cultural dimensions. Importantly, when notions are encoded in language, the signs and sounds of language impact on the mind (of others), and evolution, which means that the 'unphysical' [reality2] organisation finds a physical [reality0] counterpart.

[Reductionism will try to explain reality2 by reality0, but paradoxically we will tend to hold that reality2 provides our criterion for correctness.]

A note on the flash of insight

Colignatus (2011c) COTP mentions that it can be a good teaching technique to create a conundrum, then drop the punchline, and then all students 'get it' in 'a flash'. I happened to see the project on humor by gymnasium students Riksen, Wenink and Welling (2011), and their reference called my attention to Helmuth Plessner (1970); and this also gave Agnes Heller (2005) and her student Stuart Grant (2008) and Ulrike Günther (2002). I have not studied these references but am struck by Grant's formulation of one of Heller's propositions: 'Laughter is the instinct of reason'. Clearly there is a cognitive element in humor – it must be about something. Clearly, teaching and learning can employ the 'get it in a flash' that also occurs in a joke. It might be, however, that we may define humor as incongruity and that incongruity is the natural state of the mind, and that instead of humor it is serious reasoning that is the exception that needs an explanation. For example, if $1 + 1 = 2$ is serious, and $1 + 1 = \sqrt{4}$ parody, and $1 + 1 = 1 + 2 – 1$ sarcasm, and $1 + 1 = 1$ is irony (by teasingly using + for *), then the serious item only remains after elimination of all incongruity. It is hard work to cut through the forest of inconsistencies and inefficiencies to find something that works seriously. Neurons in a brain branch out with some randomness and the mechanism to align them into something that works might be that 'flash'. With a feedback to consciousness that records with pleasure that something has been learned. This is just a note.

Consciousness and free will

Brain research literature suggests that the brain first starts an action and that the conscious mind records it only with a delay of 0.2 milliseconds. Damasio also discusses the Dijksterhuis experiment on the quality of conscious decision making. It must be observed though that, again, the whole brain is required to construct the mind. Thus it does not make much sense to say that volition arises only after the fact. The sense of free will is already in the whole process of the generation of the state of mind. It still is the whole brain, that comprises the individual, that generates both the act and its conscious state. It is not proper to make another distinction.

It is another thing to observe that a person can react in instinct or in automation and only later correct this. Someone can train his nonconscious mind to react on impulse so that the conscious mind has only the results to deal with. This is great for math exams. These are practical issues on the brain / mind interaction and not essential questions on volition versus determinism.

Proper versus 'realistic' mathematics

Traditional mathematical education relies much on abstraction. Euclid's *Elements* is didactically dubious as a book, but Euclidean geometry with elements such as point, line, triangle and circle might not be too abtract and may be grasped at the different Van Hiele levels, working from the intuitions towards the deductive stage. After the Sputnik launch, America started the New Math education program, that however was didactically destructive. It tells us much about mathematics teachers (of that period ?) that they tried it (collectively), before finally there was a revolt by parents and a few very disgusted mathematicians. In Holland, Freudenthal proposed an alternative both to the traditional programme and the New Math. He adopted notions of applied mathematics to introduce concepts close to the life experience of students and his method was called "realistic mathematics education " (RME). Van den Heuvel-Panhuizen (1998) explained 10 years ago that the project was 30 years old and still "work in progress". She wishes to clarify that RME stands for "making something real in your mind". It tells us much about mathematics teachers (of that period ?) that they tried it (collectively), before finally there was a revolt by parents and a few very disgusted mathematicians. Elementary schools have recently been instructed to work harder on arithmetic, highschools meet with higher requirements of traditional skills, and teachers have managed to introduce more abstract math for the talented.

The didactic point made in EWS and COTP is that the Dutch RME programme, even adapted in the current manner, still is didactically misguided. It does not properly value that mathematics is abstract by nature. The real life contexts can distract the student. RME says that it uses real contexts to work towards abstraction, but in truth it creates confusion. It does not properly distill the abstract notions from those real contexts and the student is left to discover the abstractions himself. Freudenthal says that he wants "guided re-invention" to help the student toward the abstractions but doesn't deliver on the promise. My suggestion is to focus on the notion that thought is abstract by nature, so that events in reality0 have already made a transformation when they are processed in reality2 even at the lowest Van Hiele level. As Van Hiele explains, the jump from one level to the next one is made by classifying, sorting, ordering and abstracting on what has been achieved at that lower level. The student can be given activities that imply this kind of processing and once the ground is fertile the required abstract notions can be given (with that flash of insight).

The true problem in mathematics education is rather that mathematicians are trained for theory while when they meet students in class then these are real life students, which requires an empirical mind set. They try to solve their problems by resorting to tradition in what they teach, but this causes mathematical concepts and notations that are quite cumbersome. EWS advises national parliaments to investigate their national education in mathematics, and COTP provides a primer for mathematical education and an existence proof how it could be done.

Once these awkward confusions are resolved, a reason to link up mathematics education and brain research is that mathematical concepts are abstract and provide mental events, as the mind can be defined as the flux of mental events. Looking at both angles at the same time may give results.

EWS for example refers to Gladwell (2008:228): "we store digits in a memory loop that runs for about two seconds". English numbers are cumbersome to store. Gladwell then quotes Stanislas Dehaene: "(…) the prize for efficacy goes to the Cantonese dialect of Chinese, whose brevity grants residents of Hong Kong a rocketing memory span of about 10 digits." Apparently fractions in Chinese are clearer too. Instead of two-fifths it would use two-from-five. First creating fifths indeed is an additional operation. Perhaps the West is too prim on the distinction between the ratio 2:5 and the number 2/5. Perhaps it does really not make a difference except in terms of pure theory – the verb of considering the ratio and the noun of the result (called "number" when primly formalized in a number theory). In evidence based education we can imagine experiments testing the various variants, with some support in brain scanning. There is a great variety in children and how they learn but perhaps that variety is a bit less when asked very specific operations under a scan.

Conclusions

The subject matter is brain research and the education in mathematics. Small comments in this present paper are the following.

(1) This paper forwards a sharper distinction than Damasio (2010) between reality0, 1 and 2. Damasio of course uses these aspects. Reality2 is nothing but how our mind models reality0. But by explicitly naming them we enhance clarity. For example, continuity might exist in reality0 but still has to be created for reality2 (since neurons are discontinuous). The Van Hiele levels of mathematical understanding are levels in reality2 itself (per mathematical concept). Each level could have different degrees of Gardner's multiple intelligences (http://www.howardgardner.com/FAQ/faq.htm). We better understand the language games and the multiple meanings of the 'circle', if we would want brain research to assist us in teaching and learning about the circle.

(2) Damasio's mind-body equivalence only seems to survive because of his use of the qualifier 'essential'. It is better to use the term 'association' so that various brain states may associate with the same mental state e.g. as strictly defined by the thought about a mathematical concept. It makes sense to refer to mental states in discussion of the organisation of brain activity. For mathematics education it is important to create well-defined concepts, as a useful definition of mental states is that they contain well-defined concepts.

(3) This discussion would provide a better base for the use of abstract notions in the education of mathematics. Mathematical concepts like line and circle are abstract notions (at some Van Hiele level) and made of the same stuff as the mind itself. Education can use this to its advantage. The approach by Freudenthal with 'realistic math' seems to cause too much distraction from the intended abstraction.

Conclusions

This book proposes a neoclassical approach to mathematics that combines constructive methods with a better appreciation and place for abstraction. Infinity is the quintessential case for this because of the clear opposition of *on the one hand* creation of parts and construction via repetition or recursion or the successor function and *on the other hand* completion into some whole with a clear role for abstraction. There are more examples – see page 25 – but infinity takes pride of place.

Traditional mathematics in the 140 years since Cantor 1874 lost a balanced view and ran astray on infinity. The road to recovery and a clearer view on that neoclassical approach came about in steps.

(1) The origin lies in a review of the Liar paradox and Russell's paradox, which in 1980-81 caused a rediscovery of an idea by Paul of Venice (1369-1429) to impose consistency. This was followed in 2007 by an application of this idea to Cantor's conjecture on the power set, see ALOE:239 or page 82 above. This argument was originally given in semi-formal manner within the intended interpretation, and this book brings it formally within ZFC.

(2) A next realisation was that abstraction is involved in $\mathbb{N}[n]$ @ $\mathbb{N}$ and $\mathbb{R}[d]$ @ $\mathbb{R}$. These abstractions are basic to the development of these number systems, and trivial to observe. However, it remains crucial to recognise that these basics *exist,* for jointly they generate the notion of a *bijection by abstraction* – see page 43. This bijection is a clear counterexample to Cantor's argument. This became Part 2 of the book.

(3) The following step was a look at the formal axiomatic system of ZFC, which resulted in Part 3 and Part 4 of this book. These two parts on ZFC came about rather simultaneously. Part 4 reasons at the level of the axioms themselves, which is what ZFC is about, after all. But one also wonders whether an example can be given for some concrete set, and this became the singleton and Part 3. The consistency condition inspired by Paul of Venice allowed the discovery of tautologies T1 and T2. Inconsistency of ZFC then follows from first order logic in Part 3. Inconsistency can also be determined at the level of the axioms in Part 4. These deductions are within ZFC, and the conclusions are on page 90.

(4) All this caused a greater awareness of the distinction between nominalism and realism, and how this applies to the defining predicate of the strictly Cantorian set. The latter appears to be an inferential property that cannot be used as an independent base for further development, but it can be used to deprove the system that allows it – see page 76.

The result is sobering. Mathematicians are trained on abstraction by definition. Many cases already showed how mathematical abstraction can wreak havoc in application to the real world, see EWS. Now however we can observe that the training on abstraction also caused that mathematicians lost their intuition for good mathematics itself. The cause lies in the willful neglect of the intended interpretation, so that nonsense apparently doesn't ring alarm bells – see page 83. This would change when the neoclassical approach gets a chance.

Overall the greatest relevance of these findings is for mathematics education research. It becomes feasible to introduce set theory and number theory within highschool without the burden of the transfinites – as ALOE already relieved logic from the burden of the Liar, Russell's paradox and the Gödeliar. The next step will be the R&D of course materials, and the integration within the existing programme. Hopefully the other innovations suggested by EWS and COTP are considered then too.

Appendices

Appendix A. Versions of ALOE

(1) Colignatus (1981, 2007, 2011) (ALOE) existed first unpublished in 1981 as *In memoriam Philetas of Cos,* then in 2007 rebaptised and self-published. It was both retyped and programmed in the computer-algebra environment of *Mathematica* to allow ease of use of three-valued logic. In 2011 it was marginally adapted with a new version of *Mathematica.*

(2) Gill (2008) reviewed the 1st edition of ALOE of 2007. That edition refers to Cantor's standard set-theoretic argument of the power set and rejects it.

(3) Gill (2008) did not review the 2nd edition of ALOE of 2011. This edition also refers to Cantor's original argument on the natural and real numbers in particular. This edition mentions the suggestion that $\mathbb{N} \sim \mathbb{R}$.The discussion itself is not in ALOE but is in CCPO-PCWA, with both the rejection of the diagonal argument and the introduction of the notion of *bijection by abstraction.*

(4) ALOE is a book on logic and not a book on set theory. It presents the standard notions of naive set theory (membership, intersection, union) and the standard axioms for first order predicate logic that of course are relevant for set theory. I have always felt that discussing *axiomatic* set theory (with ZFC) was beyond the scope of ALOE and my actual interest and developed expertise. This present book is in my sentiment rather exploratory, apart from certainty in the theorems provided.

Appendix B. The traditional view, in Hart (2015)

The article on Paul of Venice and ZFC (PV-RP-CDA-ZFC) was basically written in November 2014 and was updated in the first part of 2015 with some clarifications, also following some comments from others. Only on May 27 I decided to look at ZFC in detail myself.

It so happens that Hart (2015) recently reviews "Cantor's diagonal argument" too, giving the traditional view after Hilbert. He starts with Cantor's orginal arguments on non-denumerability and the diagonal. For foreign readers a discussion of this Dutch article will be difficult to follow, but let me still give my comments now that I am dealing with the subject.

Hart replied to these comments and the text below has been adapted a bit for further clarity. Hart also looked at **Appendix C** and some earlier arguments, and this is discussed in point (6) below and Colignatus (2015de). It must be mentioned that Hart apparently neglected ALOE, Gill (2008) and CCPO-PCWA about which I informed him at the time.

(1) Hart (2015:43) holds correctly that a bijection doesn't have to be used, but only the surjection (i.e. in the mode of thought that the proof would be valid). He however holds incorrectly that the common short proof with the bijection would rely on a 'spurious contradiction' – referring here to Gillman 1987. This would be incorrect if we rely on the common meaning of 'spurious': (a) there is a real contradiction: the assumption of the bijection implies the assumption of the surjection, which causes the contradiction, (b) the context of discussion is infinity, for which we use isomorphisms, and thus injections, and in that case the properties of surjection and bijection are equivalent: and then the shortness of the proof must be appreciated. Indeed Hart (2015:41) explains that Cantor himself also used 'eindeutig' (column 1) and injection (column 3 – below the photograph of 'Georde Cantor'). Overall: the open 'reductio ad absurdum' form and the 'direct' form that Hart suggests are equivalent, and the reference to 'spurious contradiction' is incorrect. PM. Hart (2015:42 first column) suggests that the power set version of Cantor's Theorem was given by Bertrand Russell 1907, also using a 'supposition' and basically using a bijection.

(2) On page 42, third column, Hart agrees that Cantor's distinction between proper sets and improper sets ('classes'), or the distinction between *all* and *any*, still is used informally. Thus mathematics uses both a formal ZFC and an informal naive set system. It is useful to see this confirmed. It remains curious that Hart as a mathematician is happy to live with this incongruity. Hart then discusses the Axiom of Separation, but it gives a wrong impression, because its main weaknesses and alternatives are not discussed. One may write a book or syllabus on 'set theory' but if this only discusses ZFC and its ZFC-sets then this is a biased presentation.

(3) On page 43 Hart mentions the argument concerning $\mathbb{N} \sim \mathbb{R}$ that uses decimal expressions. He states that this particular form does not occur in Cantor's work. This is not quite true. Cantor's proof of 1890/91 uses a binary representation - see Hart (2015:41) – which, for these purposes, is equivalent to using decimals. Hart traces the proof with decimals to Young & Young in 1906, who explicitly refer to Cantor 1890/91, and who explicitly call it his 'second proof'. Thus mathematicians were aware already in 1906 that binaries and decimals are equivalent here. It is curious that Hart in 2015 does not express that awareness. His review of what Cantor originally did thus is biased. (3a) For this proof structure, binaries and decimals are equivalent. (3b) The binaries are mathematically more elegant, since changing an element has only one alternative. The decimals are didactically more useful, since students are more used to decimal expression of the real numbers - which is the representation of the continuum. It would be improper to criticize the decimal form of the proof for being didactic. (3c) It is correct that Cantor claimed that the proof structure was "independent from looking at irrationals" but the proof does *implicitly use* irrationals.

(4) We may wonder why Hart's paper might be biased. It is a good hypothesis that he wants to emphasize that some authors still have questions about Cantor's argument.

(4a) On page 43 Hart refers to Wilfrid Hodges (1998) who discusses "hopeless papers". Hart does not mention Hodges's email to me that I cited in CCPO-PCWA that I informed him about.

(4b) Hart accuses those "hopeless papers" of that they don't check what Cantor did himself originally. This is an improper accusation since such authors discuss a particular argument, that so happens to go by the name of "Cantor's diagonal argument", while it is not always at issue what Cantor himself did – who indeed wrote before ZFC.

(4c) Just to be sure: My own first contact with Hart – in 2011 – was about Cantor 1874. CCPO-PCWA wanted to know whether there were more proofs, and thus also looked at Cantor 1874, and found it inadequate. Hart's page 40 with Cantor 1874 finds a refutation in the appendix of CCPO-PCWA – reproduced above page 51 – but he knows about the latter and does not refer to that refutation.

(4d) Hart suggests that the proof with decimals causes most "hopeless papers", but that this proof can be "thrown in the trash can", because Cantor's original proof from 1874 and his second and more general proof of 1890/91 would be more attractive.

(4d1) This is improper, since it evades the question whether the argument with the decimals is a good deduction or not. Mathematics should not ditch arguments because they cause questions but should answer the questions.

(4d2) It also is an inconsistent argument, see (3): the proofs are equivalent, differ only in binaries versus decimals. Thus Hart suggests to throw Cantor's own proof into the trash can – but doesn't do so.

(4d3) In a personal communication, Hart acknowledges that the binary and decimal proofs are equivalent (without drawing the inference on (3)) but that he only expressed his preference for the aesthetics of the binary form. He is free to state his preference, but the decimal form is the most didactic one, and thus the form *cannot* be ditched.

(4e) Hart holds that such "hopeless papers" and/or internet discussions quickly replace mathematics by *ad hominem* fallacies. An *ad hominem* would be: "You have no mathematics degree and hence I will not listen to your arguments." Obviously Hart presents himself as not falling into that trap. My problem however is that he applies an *'ad gentem* fallacy', by reducing critique on Cantor's Theorem into "hopeless papers" and/or internet ad hominem fallacies. This is a racket or ballyhoo to induce a sentiment amongst his readership to no longer look at critique on Cantor's Theorem, and to join in the putting down of such critics. We thus may understand why Hart (2015) is a biased presentation, unworthy of mathematics that wants to claim to be scientific.

(5) Hart (2015:42, last column): "The best known impossibility theorems in mathematical logic all use a version of Cantor's idea to flip all elements on a diagonal" - and then he refers to Gödel's first incompleteness theorem. This is not quite true. Gödel's theorem uses self-reference. This property was already known in antiquity in the Liar Paradox. Gödel's use of number-coding has historical explanations, like the trust in arithmetic in a period of a foundations crisis in mathematical logic. Gödel's numerical listing is not crucial to the argument. The influence of Cantor should not be made greater than it is. Hart could have known about this, reading both ALOE and Gill (2008) in the same Dutch journal for mathematics, with my refutation of Gödel's two theorems.

(6) Hart does not refer to ALOE or CCPO-PCWA that he knows about, thus misinforms his readership. He reproduces Cantor's 'proofs' of 1874 and 1890/91 without mentioning their refutations. He states the common misconceptions and adds some new ones.

In a personal communication, Hart now has looked at my criticism. It leads too far to look into this here. Colignatus (2015b) reviews the email exchange with K.P. Hart (TU Delft) in 2011 – May 2015. Colignatus (2015c) reviews Hart's response on this **Appendix B** as above. The reader can check that the criticism still stands. Colignatus (2015d) reviews Hart's new combined criticism of May 18 2015 on (i) that version of the paper, (ii) **Appendix C**, and (iii) earlier refutations in CCPO-PCWA – which should cover point (6). There is now also the issue on scientific integrity, see Colignatus (2015e).

Appendix C. Deproving Conjecture W

Conjecture W on page 64 plays a key role in this book. It has the advantages: (1) it would be constructive and avoids the *reductio ad absurdum*, (2) it avoids using concepts like surjection, injection and bijection, (3) it still causes that there can be no surjection and hence no bijection – those are relevant for infinity – and (4) it puts the paradoxical self-reference in center place, as it is essentially a rewrite of the definition ($\alpha \in \Psi$) $\Leftrightarrow$ ($\alpha \notin f[\alpha]$). The conjecture was presented to me by K.P. Hart (TU Delft) in a personal communication in 2012. If no one else presented this conjecture earlier it may be called the Cantor-Hart Conjecture but for now I label it for what it does: it is the weakest conjecture. In 2012 my reply was asking Hart whether he understood the refutation in ALOE of the proof of Cantor's Conjecture that uses the power set and bijection, but I did not receive a response on that. If he had understood, he could have given below refutation of this weakest conjecture himself.

Despite its advantages there is something fishy about the 'proof' for Conjecture W. It has a *'spurious non-contradiction':* the 'proof' looks without contradiction but in fact relies on a hidden assumption that causes a contradiction, but which is not mentioned. When there is an open contradiction then one can infer that something is wrong with *any* assumption, but when there is no contradiction shown then it seems that *all is well.* The contradiction however surfaces once we presume a bijection between the natural numbers and the reals. Since Conjecture W looks innocent and speaks about *any* function without being particular about bijections, it seems that we indeed should cancel that idea of having a bijection. However, the refutation of the *reductio ad absurdum* proof of Cantor's Conjecture on page 82 shows what is actually the problem with the 'proof' for Conjecture W too.

Refutation: While the 'proof' in cases 1 and 2 assumes any f, it ought to distinguish between kinds of functions: the surjections versus the non-surjections. Thus redo the proof.

If f is not surjective, then continue like the 'proof' of Conjecture W on page 64.
If f is surjective, then the definition of Ψ causes a contradiction, so it is no useful Ψ. The closest analogue requires the condition to prevent reliance on hidden contradictions. This gives us Φ. Let us split the subcases on the risk of infinite regress (*ad hoc*, no '↑'):
Define $\Phi = \{x \in A \wedge f[x] \neq \Phi \mid x \notin f[x]\} \cup \{x \in A \wedge f[x] = \Phi \mid x \notin f[x] \wedge x \in \Phi\}$.
Given the surjection there is a φ such that $f[\varphi] = \Phi$. Distinguish cases $\alpha = \varphi$ and $\alpha \neq \varphi$.
Case A. $\alpha \neq \varphi$. All is like the 'proof' of Conjecture W on page 64. Thus:
Case 1: $\alpha \in \Phi$. In that case $\alpha \notin f[\alpha]$. Thus $\Phi \neq f[\alpha]$.
Case 2: $\alpha \notin \Phi$. In that case $\alpha \in f[\alpha]$. Thus $\Phi \neq f[\alpha]$.
Case B. $\alpha = \varphi$.
Case 3: $\varphi \in \Phi$. Then ($\varphi \notin \Phi \wedge \varphi \in \Phi$): contradiction. This case cannot occur.
Case 4: $\varphi \notin \Phi$. Then ($\varphi \notin \Phi \vee \varphi \in \Phi$): no contradiction.
It is false that $\Phi \neq f[\alpha]$ since we have $\alpha = \varphi$ such that $f[\varphi] = \Phi$.
Ergo: The 'proof' fails.
Remarks.
(1) The 'proof' of Conjecture W on page 64 thus holds for ZFC, that allows Ψ. One must explain why one uses a system that allows ill-defined-ness.
(2) For well-defined systems the conjecture cannot stand with the proof that uses Ψ. When we look for something useful that matches Ψ then we find Φ. For some f, namely surjections, this Φ has an element in A, namely $\alpha = \varphi$ such that $f[\varphi] = \Phi$. This holds without contradiction, so that one cannot hold that f cannot exist.
(3) This refutation does not preclude another and real proof for Conjecture W. This may be doubted however.
(4) We have neither shown how such a surjection can be created. We have merely shown that the assumption of a surjection invalidates the 'proof'. It is a fair question: can one actually show a surjection between infinite sets, like the natural numbers and the reals ? For this I refer to the *bijection by abstraction.*

Appendix D. What a mathematician might wish to know about my work

March 26 2013 [58] - edited 2015

Abstract

Mathematicians have contributed to confusions in the areas of logic, voting theory and the education of mathematics itself. While mankind may mistake abstract ideas for reality, mathematicians are not immune for this either. Part of my work has been to correct such mistakes. It would be useful when mathematicians study those corrections with an open mind, so that we can get better logic, more democracy and proper education in mathematics.

Introduction

Mathematics per se is not my target. Over the years I have written some texts that nevertheless may be of interest to mathematicians, like reformulations of logic, voting theory and calculus. These texts are not presented in ways that mathematicians may be used to. My work might be called *applied mathematics,* as it is not developed in an axiomatic context but is in the intended interpretation of some axiomatics that may still need to be developed. Hence some explanation is useful for mathematicians about what to expect about my work, since I would like them to study these books too. Without the explanation below we may expect neglect and misunderstandings from mathematicians, and this would be unfortunate.

I refer to my books ALOE, VTFD, EWS, COTP, EKWAGGG, SMOJ / EWVJ, see the references.

Who is interested in logical paradoxes, will benefit from ALOE. Who is interested in democracy and voting theory, will benefit from VTFD. Who is interested in the impact of ancient mathematics and astronomy on religion, will benefit from SMOJ / EWVJ. Who is interested in the didactics of geometry and calculus, will benefit from COTP. Who is interested in education of mathematics, will benefit from EWS and EKWAGGG. The latter books also rely on economics, when they discuss the mathematics industry and advise to a parliamentary enquiry.

I am an econometrician (Groningen 1982) and teacher of mathematics (Leiden 2008). As a student I chose econometrics because I wanted to find decent solutions for world problems, and I considered the mathematical base as a *conditio sine qua non*. At the interfaculty of econometrics in the 1970s, we as students had our courses in mathematics jointly with students of mathematics, physics and astronomy. Clearly I consider mathematics important but I rather apply it. The didactics of mathematics is an empirical issue as well.

A context of quality

An indication of quality are two favourable reviews by Richard Gill (2008 and 2012) in *Nieuw Archief voor Wiskunde* with respect to ALOE and EWS & COTP. The *European Mathematical Society* website has two favourable reviews of ALOE and COTP too. Recently, Christiaan Boudri (2013) at the website of the *Dutch Association of Teachers of Mathematics* NVVW calls for having an open mind here too. Koolstra and Groeneveld (2013) mention it in the Dutch math email newsletter.

A standard textbook is TSOM with ir. Karel Drenth (TUD, sadly deceased) (2000), as perhaps additional confirmation that I would be able to walk the standard path as well.

A point is that Gill is not specialised in logic and didactics as research fields. This also holds for me, though I did study the subjects that I write about. My subjects are elementary,

[58] http://thomascool.eu/Papers/Math/2013-03-26-WAMMWTKAMW.pdf

so a specialisation to higher levels is not required. Still, the specialists in logic and didactics will tend to defend their specialisms. Everyone can check elementary errors, but will the specialists acknowledge those ?

Standard ways versus re-engineering

The point is now to explain the path into uncharted territory. There are plenty of cranks in the world, and standard mathematicians might tend to catalogue me as one of those, but if they would walk along with me along these new paths, then they might start to wonder whether the standard ways aren't a bit cranky themselves. An option is that they develop the required axiomatics, if that is required to make the new approach acceptable in the mathematics community.

Some mathematicians advise me: 'Present your proposals in various neat articles, phrased in the language that mathematicians like to read, and be done with it.' I am afraid that it doesn't work like that.

Above books *re-engineer* their subjects. In my analysis, standard views in those subjects are misguided in subtle ways. The best approach towards clarity is to reorganise the subject matter, start from scratch, and build on from there with an open mind.

Mathematicians contribute to confusion

A key point is that much confusion in these areas has been caused by mathematicians, who neglect the world and who focus on some *if-then* relationships, where perhaps the logic might be right in some respects, but where the assumptions are (subtly) confused about what the discussion is about.

My books intend to set the record straight, and to invite the mathematicians to reconsider their work, so that this fits in the whole, and thus to stop their contribution to confusion.

This observation is only a rephrasing of the earlier statement: my work is in the *intended interpretation,* the world of application. Thus mathematicians who consider such applications ought to benefit from this effort at re-engineering. Mathematicians are not my target, but the areas of application. Mathematicians interested in those fields better avoid confusion.

Hopefully, you see the problem. Once we enter the world of application, one must study reality and not just mathematics. Selecting only bits and pieces to find *if-then* relationships can contribute to confusion. A mathematician better be precise what the contribution is, and help to clarify what the application really is. Read my books and see how mathematicians have gone astray. They provide lessons on how to communicate on real issues, partly using the language of mathematics, but still focussed on the real issues.

Modesty

My books are modest. They are only what they are about, and not something else. It might sound curious to speak about 're-engineering logic, calculus and voting theory' but the purpose of this description is not to sound curious but to indicate what the books do.

An editor at the journal *Euclides* of NVVW worried that this description of re-organising subjects uses 'big words' but agreed with various points and did not specify what would be wrong, so it is not clear why that description would not be correct. I do not claim that I know everything. I do not claim that am infallible. I just explain where mathematicians go astray at key points in key applications.

My books explain where Hans Freudenthal was in error, and suggest a better alternative, without returning to the old ways of teaching Euclid. Many other authors have promising suggestions too. Let parliament abolish the *Freudenthal Institute* in Utrecht and create the *Simon Stevin Institute,* where researchers from various fields can test what works for pupils and students. I am amazed that ICMI has a *"Freudenthal Medal"* while he has been disastrous for the education in mathematics, with his abstract mind in conflict with empirical reality. He rightly said that education is engineering, thus involves reality, but he was no engineer himself. It actually appears that integrity is at issue, see Colignatus (2014c).

My position is *not* that my books explain how things must be, and that Parliament must impose this on the mathematics community. That would be a gross misunderstanding. What works in education is up to the pupils and students themselves. Parliament must step in to make the funds available for research.

Little contact with mathematicians

As a writer, I have hardly any contact with mathematicians. It is my great regret that these books have been written mostly in a context where mathematicians were not willing to discuss the issues. The book on logic was written in my student days, at first in some interaction with some teachers in logic, but fairly soon in bitter antagonism from their side and with their unwillingness to listen to criticism. The book on voting theory first had some interaction with some mathematicians, but fairly soon in bitter antagonism again. For the book on calculus I have assumed neglect, perhaps I was wrong, and in reality there would have been keen interest. However, one so-called "review" confirms bitter antagonism again.

The books on the education of mathematics EWS, COTP and EKWAGGG have been written with little interaction on these with my colleagues teachers of mathematics. My colleagues have all been fine and capable teachers and pleasant to work with. The focus of our work was on teaching the established programme. My ideas developed over time in notes, but I had little reason to discuss them, since this would distract from the overall focus on the established programme. For the same reason the books were written outside of school, and have not been presented at school. Clearly I want to avoid that colleagues might get the wrong impression that I would teach something else than the established programme. The discussion is best done in a context where improvement of the programme is on the agenda. Clearly, I haven't found that environment yet, given the neglect by such commissions (in Holland: cTWO and NOCW).

The books essentially are invitations to be read and discussed, and if someone has a good counterargument, I will be the first to correct.

In the journal *Euclides* two reviewers of EWS and COTP have selected to start slandering. I have filed a protest with the editors. See my website for the slander and my reply. The reply was not published in the journal. They have decided not to review any book of me anymore. This now holds for EKWAGGG and SMOJ / EWVJ. Slander and now censorship. And these people teach mathematics.

The book editors of *Nieuw Archief voor Wiskunde* recently rejected SMOJ / EWVJ for review, with the argument that it would not fit the readership but that indicates that they have not studied it.

I have submitted some short papers to journals. Editors respond by saying "I do not understand it" and then reject the paper. It would help, and be more decent, to specify what one doesn't understand.

Some original contributions

My books also contain some bits of original contributions to mathematics, but these might perhaps only be understood in the reconstructed framework of those books. They are already available in those books, so there is little advantage in trying to extract them as separate articles. The chance is slim that they might be understood without that reconstructed environment.

Apparently when I want to relay a message A then I also want more internal consistency in the mathematics $M(A)$ that underlies that message, and then I find myself solving problems in $M(A)$. While a pure mathematician would focus on $M(A)$ and think about a possible message A only in a second stage or not at all. Perhaps pure mathematicians might never discover $M(A)$ since they don't see A in the first place. This partly explains why pure mathematicians may have a hard time to accept $M(A)$ anyway. But I do not claim to understand how pure mathematicians think. I can only say: the context and my manner of presentation in that context tend to differ from what (pure) mathematicians may be used to. If they are inflexible then they miss out on some key findings.

Some abstracts

There is a risk in giving abstracts of these books since some subtleties might be lost. The human mind however needs some anchors.

ALOE re-engineers elementary logic. It solves the Liar paradox after 2300 years, and corrects Gödel by showing that his verbal statements and interpretation do not cover his mathematics. VTFD re-engineers voting theory for democracy, and shows that Kenneth Arrow on his *Impossibility Theorem* for collective decision making gives a verbal interpretation that does not cover his mathematics. COTP re-engineers plane geometry, and shows that derivative and integral are algebraic concepts. This is the fundamental understanding that can be used for highschool and first year math for non-math majors, while it remains an open question what Weierstraß and non-standard analysis add to this. SMOJ / EWVJ suggests that religious concepts on the divine are as abstract as mathematical concepts like line and circle, with similar epistemological questions for existence and current research on mind and brain. It is conceivable that religious differences and perhaps even religious wars relate to misconceptions about Van Hiele levels of abstraction as in mathematics education. EWS and EKWAGGG also discuss the mathematics industry and errors in didactics, with a proposal of a parliamentary enquiry to resolve confusion and stagnation.

Parliament

In Holland, the Dijsselbloem *Parliamentary Commission on Education* made the distinction between *what* and *how*. The government determines *what* will be in the programme (say Dutch and math) and the teaching community will determine *how* the subjects will be taught. It is fine that Parliament has decided that the school programme has to include math. The problem is that “math teachers” present something as “math” which it isn’t, see Colignatus (2013a). For example, two-and-a-half is written as two-times-a-half (2½) while it is better to stick to 2 + ½, and learn to see that addition as an end-station, in the same manner as *exp*[2] can be symbolic and doesn’t directly require the use of a calculator. We can understand the use of 2½ from some historical development, but we would all be silly if we were to accept it as decent mathematics. Jan van de Craats solves the issue by mostly using 5 / 2, but this loses the useful feature of a mixed number. Hence, re-engineer the subject. Hence, let Parliament investigate the mathematics industry.

To repair ages of wrong didactics will be a costly affair. Do we have to wait till the USA is open to my analysis, or might Holland take the lead ? I see little other ways of resolution than that members of Parliament look into mathematics education. It suffices that they have had a highschool education with mathematics and then we can explain the current curriculum and the improvements in my books. This will be fun but also necessary for the improvement. I cannot see why mathematicians would be against teaching members of Parliament more about mathematics. If they think that my suggestions for improvement are silly then it should not be difficult to show this. The members of Parliament can also query users of mathematics like economists, physicists and biologists. Finally, note that my target is to establish doubt and to release funds for research. What is an improvement in didactics is an empirical question, and cannot be established by my books only. The error of the current math curriculum is that it relies on tradition and it is time that we see that didactics is an empirical issue.

The economic and ecological crises

Econometrics is: to translate economic theories into mathematical format and test these by statistical methods. Another word is “economic engineering”. As an econometrician I have discussions with economists, mathematicians and statisticians. Misunderstandings in one realm may contaminate misunderstandings in another realm. This would not be logical, but it is a human thing to happen. Mathematicians will not be able to judge details in economic theory, but may still think dark thoughts about my economic analyses, given their apprehensions about my books on the education of mathematics. Economists can get insecure if they think that my mathematics would be improper. The best response is to ask everyone to do their job and to be specific about what one can judge about. My position as an econometrician suffers from maltreatment by the various subprofessions, and this better stops. The world is in economic and ecological crises, my econometric analysis would help a resolution, and it doesn't help when the subprofessions malfunction.

Let me refer to Colignatus (2013b) on bottlenecks against science. Jos de Beus (1952-2013) was a professor of politicology, student of professor or economics J. (Hans) van den Doel (1937-2012), student of Jan Tinbergen (1903-1994), founder of econometrics and winner of the Nobel Prize in Economics, who started as a student of Paul Ehrenfest (1880-1933). Mathematicians have long stories about that Prize etcetera. Point is that Van den Doel didn't quite understand Kenneth Arrow's *Impossibility Theorem*. [59] Jos de Beus was misguided too. These misconceptions have influenced ideas in government circles. It would be such a relief when mathematicians would accept that they contributed to the confusion themselves too, since Arrow gave wrong verbal interpretations to his mathematical result, and others have been copying that.

Conclusion

Mathematics as a profession is not my cup of tea. In my research on key topics in the real world I noticed that mathematics was applied in a wrong manner at key steps in the argumentation. It were especially the mathematicians who advanced such miscomprehension. One might think that people using logic or voting, or the teachers of calculus would be able to correct the misunderstandings of such confused mathematicians, but alas, mathematics also has an aura of authority. If things get complex, follow the specialist, and if a mathematician has a complex paper he or she might be that specialist. However, when I as an econometrician and teacher of mathematics present corrections, I find that mathematicians are not as open to criticism as one would wish. There are some sparks of hope now, but will there be a follow up ?

Let us hope that mathematicians will study my books, learn about the real world, see how important it is to communicate precisely. A sign of success will be when the confusions in voting theory are corrected. A sign of success will be when mathematicians start signing the petition for a parliamentary enquiry into mathematics education, and call on others to help them to save our children from misguided "mathematicians".

The petition is at http://www.ipetitions.com/petition/tk-onderzoek-wiskundeonderwijs.

Unfortunately there is now a breach of integrity of science, see Colignatus (2015e).

[59] In Dutch, see Dutch Colignatus & Hulst (2003, PDF page 85). Also *Kennisnet.nl* and booklets of *Epsilon Uitgaven* still report inadequately, see Colignatus (2013c).

Appendix E. With your undivided attention

April 9 2014 [60]

Both President Obama of the USA and President Putin of the Russian Federation have somewhat illogical positions. Obama repeats the ritual article 5 *“An attack on one is an attack on all”* but the Ukraine is not a fraction of NATO. So what is the USA going to do about the Ukraine ? Putin holds that Russia defends all Russians everywhere but claims that Russia is not involved with the combatants in the Ukraine. His proposed 7 point plan contains a buffer zone so that he creates fractions in a country on the other side of the border. Overall, we see the fractional division of the Ukraine starting, as already predicted in an earlier entry in this weblog.

What is it with fractions, that Presidents find so hard, and what they apparently didn’t master in elementary school, like so many other pupils ? There are two positions on this. The first position is that mathematics teachers are right and that kids must learn fractions, with candy or torture, whatever works best. The second position is that kids are right and that fractions may as well be abolished as both useless and an infringement of the *Universal Declaration of Human Rights* (article 1). [61] Let us see who is right.

An abolition of fractions

Could we get rid of fractions ? We can replace 1 / *a* or *one-per-a* by using the exponent of -1, giving $a^{\neg}$ that can be pronounced as *per-a.* In the earlier weblog entry on subtraction [62] we found the Harremoës operator *H* = -1. [63] The clearest notation is $a^H = 1 / a$. Before we introduce the negative numbers we might consider to introduce the new notation for fractions. The trick is that we do not say that. We just introduce kids to the operator with the following algebraic properties:

$$0^H = undefined$$

$$a\, a^H = a^H a = 1$$

$$(a^H)^H = a$$

Getting rid of fractions in this manner is not my idea, but it was considered by Pierre van Hiele (1909-2010), [64] a teacher of mathematics and a great analyst on didactics, in his book *Begrip en Inzicht* (1973:196-204), thus more than 40 years ago. His discussion may perhaps also be found in English in *Structure and Insight* (1986). Note that $a^H = 1 / a$ already had been considered before, certainly in axiomatics, but the Van Hiele step was to consider it for didactics at elementary school.

From the above we can deduce some other properties.

Theorem 1:

$$(a\, b)^H = a^H b^H$$

Proof. Take $x = a\, b$. From $x^H x = 1$ we get $(a\, b)^H (a\, b) = 1$. Multiply both sides with $a^H b^H$, giving $(a\, b)^H (a\, b)\, a^H b^H = a^H b^H$, giving the desired. Q.E.D.

Theorem 2:

$$H^H = H$$

Proof. From addition and subtraction we already know that *H H* = 1. Take $a\, a^H = 1$, substitute *a* = *H*, get $H\, H^H = 1$, multiply both sides with *H*, get $H\, H\, H^H = H$, and thus $H^H = H$. Q.E.D.

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[60] https://boycottholland.wordpress.com/2014/09/04/with-your-undivided-attention/
[61] http://www.un.org/en/documents/udhr/
[62] https://boycottholland.wordpress.com/2014/08/30/taking-a-loss/
[63] It is one single symbol but still reminds of "-1". Pronounce the operator as "eta".
[64] http://en.wikipedia.org/wiki/Van_Hiele_model

It remains to be tested empirically whether kids can follow such proofs. But they ought to be able to do the following.

Simplification

The expression $10 * 5^H$ or *ten per five* can be simplified into $10 * 5^H = 2 * 5 * 5^H = 2$ or *two each*.

Equivalent fractions

Observing that 6 / 12 is actually 1 / 2 becomes $6 * 12^H = 6 * (2 * 6)^H = 6 * 2^H * 6^H = 2^H$. Alternatively all integers are factorised into the primes first. Note that equivalent fractions are part of the methods of simplification.

Multiplication

$$a\, b^H * c\, d^H = (a\, c)\, (b\, d)^H$$

Comparing fractions

Determining whether $a\, b^H > c\, d^H$ or conversely: this reduces by multiplication by *b d,* giving the equivalent question whether $a\, d > c\, b$ or conversely.

Rebasing

That (a / b = c) ⇔ (a / c = b) may be shown in this manner:

$$a\, b^H = c$$

$$a\, b^H\ (b\, c^H) = c\ (b\, c^H)$$

$$a\, c^H = b$$

Addition

Van Hiele’s main worry was that we can calculate 2 / 7 + 3 / 5 = 31 / 35 but without much clarity what we have achieved. Okay, the sum remains less than 1, but what else ? Translating to percentages 2 / 7 ≈ 28.5714% and 3 / 5 = 60%, so the sum ≈ 88.5714%, is more informative, certainly for pupils at elementary school. This however requires a new convention that says that 0.6 is an exact number and not an approximate decimal, see *Conquest of the Plane* (2011c). The argument would be that working with decimals causes approximation error, and that first calculating 31 / 35 and then transferring to decimals would give greater accuracy for the end result. On the other hand it is also informative to see the decimal constituants, e.g. observe where the greatest contribution comes from.

Another argument is that 2 / 7 + 3 / 5 = 31 / 35 would provide practice for algebra. But why practice a particular format if it is unhandy ? The weighted sum can also be written in terms of multiplication. Compare these formats, and check what is less cluttered:

$$a / b + c / d = (a / b + c / d)\, (b\, d) / (b\, d) = (a\, d + c\, b) / (b\, d)$$

$$a\, b^H + c\, d^H = (a\, b^H + c\, d^H)\, (b\, d)\, (b\, d)^H = (a\, d + c\, b)\, (b\, d)^H$$

Subtraction

In this case kids would have to see that H can occur at two levels, like any other symbol.

$$a\, b^H + H\, c\, d^H = (a\, b^H + H\, c\, d^H)\, (b\, d)\, (b\, d)^H = (a\, d + H\, c\, b)\, (b\, d)^H$$

Mixed numbers

A number like *two-and-a-half* should not be written as *two-times-a-half* or 2½ . *Elegance with Substance* (2009) already considers to leave it at 2 + ½. Now we get $2 + 2^H$.

Division

Part of division we already saw in simplification. The major stumbling block is division by another fraction. Compare:

$$a / b / \{c / d\} = (a / b)(d / c) / \{ (c / d) (d / c) \} = (a / b)(d / c) / \{ 1 \} = (a d) / (b c)$$

$$a b^H * (c d^H)^H = = a b^H * c^H d = (a d) (b c)^H$$

Supposedly, kids get to understand this by e.g. dividing 1/2 by 1/10 so that they can observe that there are 5 pieces of 10^H = 1/10 that go into 2^H = 1/2. Once the inversion has been established as a rule, it becomes a mere algorithm that can also be applied to arbitrary numbers like $34^H (127^H)^H$ = 1/34 / (1/127). The statement "divide per-two by per-10″ becomes more general:

(divide by per-*a*) = (multiply by *a*)

Dynamic division

A crucial contribution of *Elegance with Substance* (2009:27) and *Conquest of the Plane* (2011c:57) is the notion of dynamic division, that allows an algebraic redefinition of calculus.[65]

With $y x^H = y / x$ as normal static division then dynamic division $y x^D = y // x$ becomes:

$y x^D \equiv$ { $y x^H$, unless x is a variable and then: assume $x \neq 0$, simplify the expression $y x^H$, declare the result valid also for the domain extension $x = 0$ }.

A trick might be to redefine y / x as dynamic division. It would be somewhat inconsistent however to train on x^H and then switch back to the y / x format that has not been trained upon. On the other hand, some training on the division slash and bar is useful since it are formats that occur.

Van Hiele 1973

Van Hiele in 1973 includes a discussion of an axiomatic development of addition and subtraction and an axiomatic development of multiplication and division. This means that kids would be introduced to group theory. This axiomatic development for arithmetic is much easier to do than for geometry. Since mathematics is targeted at "definition, theorem, proof" it makes sense to have kids grow aware of the logical structure. He suggested this for junior highschool rather than elementary school, however. It is indeed likely that many kids at that age are already open to such an insight in the structure of arithmetic. This does not mean a training in axiomatics but merely a discussion to kindle the awareness, which would already be a great step forwards.

His 1973 conclusions are:

Advantages

1. In the abolition of fractions 1/*a* a part of mathematics is abolished that contains a technique that stands on its own.
2. One will express theorems more often in the form of multiplication rather than in the form of division, which will increase exactness. (See the problem of division by zero.)
3. Group theory becomes a more central notion.
4. In determining derivatives and integrals, it no longer becomes necessary to transform fractions by means of powers with negative exponents. (They are already there.)

[65] https://boycottholland.wordpress.com/video/

Disadvantages

1. Teachers will have to break with a tradition.
2. It will take a while before people in practice write 3 4^H instead of 3 / 4.
3. Proponents will have to face up to people who don't like change.
4. We haven't studied yet the consequences for the whole of mathematics (education).

His closing statement: "We do not need to adopt the new notation overnight. It seems to me very useful however to consider the abolition of the algorithms involving fractions."

Conclusion

Given the widespread use of 1 / *a*, we cannot avoid explaining that $a^H = 1 / a$. The fraction bar is obviously a good tool for simplication too, check 6 * $(2 * 6)^H$.

Similarly, issues of continuity and limits $x \to 1$ for expressions like $(1 + x)\ (1 - x^2)^H$ would benefit from a bar format too. This would also hold alternatively for $(1 + x)\ (1 - x^2)^D$.

But, awareness of this, and the ability to transform, is something else than training in the same format. If training is done in algorithms in terms of a^H then this becomes the engine, and the fraction slash and bar merely become input and output formats that are of no significance for the actual algebraic competence.

Hence it indeed seems that fractions as we know them can be abolished without the loss of mathematical insight and competence.

Appendix F. Abstraction for the irrational numbers

The reasoning on abstraction makes one wonder when it can be applied to the irrational numbers, for example the square root of 2. Rather than calling such a number 'irrational' it is conceivable to say that it is 'rational by abstraction'. In this case, however, this is merely a play of words. The term 'irrational' is somewhat quaint, in comparison to 'irrational people', but historically useful, because of the conceptual linkages of 'proportion, ratio, logos, calculation, reasoning'.

In the case of 'bijection by abstraction' there is also a shift of perspective because this allows us to regard $\mathbb{N}$ and $\mathbb{R}$ as equally large and only ordered differently.

Let us discuss this issue on the choice of phrases in more detail.

Let us first copy the ancient proof sometimes ascribed to Hippasus that √2 is irrational, i.e. cannot be expressed as a ratio of two integers. Take an isosceles right triangle, with sides 1, then the hypothenuse is $\sqrt{1+1}$ = √2, so we have such a length indeed.

Theorem. There are no integer numerator *n* and denominator *d* such that √2 = $n\ d^{H}$.

Proof. (Hippasus ?) Assume that *n* and *d* exist. Exclude multiples, and take the least values. For example 2 10^H reduces to 1 5^H. Thus *n* and *d* cannot both be even numbers.

Use $n = d\ \sqrt{2}$. Squaring gives $n^2 = 2\ d^2$. Thus n^2 is an even number. Note that the square of an uneven number will always be uneven again. If n^2 is even, it follows that *n* is even, and hence *d* is uneven (since we have simplified).

If *n* is even then we get a new integer number $m = n\ 2^H$ or $n = 2m$. Hence, $n^2 = 2\ d^2$ gives 4 $m^2 = 2\ d^2$ or 2 $m^2 = d^2$. It follows that d^2 is even. From this it follows that *d* cannot be uneven. But we had already derived that *d* is uneven. Contradiction. Hence, there are no such numbers *n* and *d* such that √2= $n\ d^{H}$. Q.E.D.

Our $\mathbb{R}$ concerned the interval [0, 1] and hence we now consider $\sqrt{2}^H$. If $\sqrt{2}^H$ is regarded as a process towards a numerical value then it belongs to S_3 and if it is understood as a completed number then it belongs to S_4. (See page 41.) For $\mathbb{R}[d]$ we can find a best approximation. Since @ has been used for sets, it may be wise to use @@ for numbers. Then:

(1) $x[d] = num[d]\ denom[d]^H \approx \sqrt{2}^H$

(2) ($\mathbb{N}[d]$ @ $\mathbb{N}$) ⇒ ($x[d]$ @@ $\sqrt{2}^H$)

(3) then $\sqrt{2}^H$ might be labelled as 'rational by abstraction'.

The phrase 'rational by abstraction' only is a change of words from 'irrational number', for there is no change in perspective. Changing the words does not add to anything. We still need to specify the numerator and denominator in the steps, and develop notions of convergence, for which Weierstraß is excellent.

Above proof that √2 is irrational uses that it is a completed number. If the number is only in the process of being constructed then the proof collapses, since we cannot use yet that the outcome of squaring is 2 (because of the approximation). Thus, there is scope for a fallacy of composition, w.r.t. being completed or in construction.

In S_3 we would have an argument in each $\mathbb{R}[d]$. Assume that there are numerator and denominator such that $num[d]\ denom[d]^H = \sqrt{2}^H$ (with a factor 10^d cancelling), etcetera, and deduce a contradiction and decide that it is 'irrational[*d*]', also with the meaning that it would not be present in the list (since we haven't made the step towards completed $\mathbb{R}$).

In S_4 we construct the whole $\mathbb{R}$ and then it is present. It is useful to distinguish two cases.

(i) At issue is the label 'rational by abstraction'. There is no reason for this. In each $\mathbb{R}[d]$ we didn't have that $\sqrt{2}^H$ is rational[*d*] and thus there is no transfer by abstraction.

(ii) It can still be called 'irrational by abstraction', which abstracts from particular values of *d*. For each $\mathbb{R}[d]$ we have that $\sqrt{2}^H$ is irrational[*d*] and thus we can consider such transfer by abstraction.

Thus we can agree that abstraction is involved, but it actually supports the term 'irrational by abstraction' rather than 'rational by abstraction'. This line of proof via $\mathbb{R}[d]$ is much less elegant than the original ascribed to Hippasus.

Thus we find a parallel, on one hand the issue on the S_3 - S_4 frontier with respect to √2 on '(ir)rational by abstraction', and on the other hand the issue on the S_5 - S_6 frontier on 'bijection by abstraction'. The parallel is not only a phrase 'by abstraction' but also a scope for a fallacy of composition.

There is a difference however. At the S_3 - S_4 frontier there is hardly a change of perspective, only the "elimination of *d*", if it might be put like that, so that referring to abstraction merely amounts to a different label for the same situation, without illumination. For the issue at the S_5 - S_6 frontier there is a change of perspective. It makes a difference to be able to hold that $\mathbb{N}$ and $\mathbb{R}$ are equally large and only ordered differently. (A key consideration is of course that Cantor's proofs collapse, so that, bearing other proofs, it becomes a philosophical issue to regard $\mathbb{N}$ and $\mathbb{R}$ as different in cardinality. But this is a consequence and not the prime change in perspective.)

The notion of 'completion' shows a similar but still different role in the two contexts. Conventional reasoning is: (i) first construct $\mathbb{R}$, (ii) consider √2 as a completed number, (iii) then consider limiting processes around √2 and within $\mathbb{R}$ and its completed numbers. Alternatively, we can imagine a limiting process that occurs simultaneously while $\mathbb{R}$ is constructed – as is suggested as the proper approach on Cantor 1874, see page 51. The distinction seems to be in the point that the definition of √2 doesn't depend upon the construction of $\mathbb{R}$, while the definition of the diagonal is related to the construction of $\mathbb{R}$.

Appendix G. Cantor's Conjecture with surjection (standard)

While Conjecture B with the bijection on page 75 is *common,* it is useful to mention a form that is rather *standard* and that only uses a surjection.

The following is from a matricola course at the universities of Leiden and Delft for students majoring in mathematics. The online syllabus is by Coplakova et al. (2011), and the issue concerns theorem I.4.9, pages 18-19. We translate Dutch into English.

Conjecture. (S), (Cantor's Conjecture for the power set, using a surjection, see Coplakova et al. (2011:18), I.4.9, replacing their *B* by Ψ):
Let *A* be a set. There is no surjective function $f: A \to P[A]$.

Proof. Assume that there is a surjective function $f: A \to P[A]$. Now consider the set $\Psi = \{x \in A \mid x \notin f[x]\}$. (*Addendum*, see below.) Since $\Psi \subseteq A$ we also have $\Psi \in P[A]$. Because of the assumption that f is surjective, there is a $\psi \in A$ with $f[\psi] = \Psi$. There are two possibilities: (i) $\psi \in \Psi$ or (ii) $\psi \notin \Psi$.

If (i) then $\psi \in \Psi$. From the definition of Ψ it follows that $\psi \notin f[\psi]$ or $\psi \notin \Psi$. Thus (i) gives a contradiction. Thus $\psi \notin \Psi$.

If (ii) then we know $\psi \notin \Psi$ and thus also $\psi \notin f[\psi]$. With the definition of Ψ it follows that $\psi \in \Psi$. Thus (ii) gives a contradiction. Thus $\psi \in \Psi$.

Cases (i) and (ii) cannot apply simultaneously, and hence we find a contradiction. Q.E.D.

Remarks.
(1) The *Addendum* is: This proof relies on existence Conjecture E and on the notion that ZFC provides for well-defined sets anyway.
(2) From the contradiction in this proof, the proper conclusion is not that Cantor's Conjecture is proven, but only that it is proven in ZFC. Either Cantor's Conjecture is true *or* ZFC doesn't yet provide for well-defined sets. Given this addendum, it now should be clearer that if ZFC allows a paradoxical construct then one may feel that ZFC needs amendment.
(3) A refutation of the proof is on page 82.

Appendix H. Marginal analysis and the re-occurrence of Russell's paradox

H.1. Marginal analysis in general

The problem with Cantor's conjectures is not caused by infinity. The real question is how one deals with self-reference. Constructive methods would allow for the notion of a fixed point, i.e. $p = f[p]$. Thus we are not afraid of some kind of self-reference or circularity.

But we are working with ZFC now, so let us formulate some conjectures to test the properties of ZFC. Perhaps ZFC allows the wrong kind of self-reference and disallows the useful kind.

ZFC seems to block the use of a significant set of normal functions. The failure on a single element $\psi \in A$ such that $f[\psi] = \Psi$ – Conjecture W – seems to have too many consequences. The restriction only concerns a single element – though with consequences on infinity – and this suggests the approach of a marginal analysis in constructive manner. Looking at a single element at the margin would show more about the mechanism.

Let us first set up a marginal analysis that fails, so that we can better understand the subsequent marginal analysis that generates the proofs that we desire.

This approach generated the analysis on the singleton in the body of the book. Thus, marginal analysis does have some value. It is not guaranteed that the exploration below has value.

In principle we should be able to take an arbitrary function and reserve a single element, to map this element to a single new set. Let us see what happens when we extend g at the margin with α, which gives function f: $A \rightarrow P[A]$, with $f[\alpha]$ defined at liberty. Let us distinguish the disputable core in the 'proof idea' and the subsequent discussion about this 'proof idea'.

It seems that one can indeed take arbitrary functions and reserve a single element for Φ. Some of the findings of the marginal analysis on Φ have found a place in the main body of the book as well, notably in Theorem ESV on page 86. While the steps for Theorem ESV are clearly within ZFC, the steps for Ψ below might not be. As said, while this book can derive that ZFC is inconsistent, this author is no expert on ZFC – except that ZFC can derive anything. The following on Ψ is still relevant enough for a place in this Appendix.

The conjectures below are not in the overview table on page 11 since they don't play a role in the book itself. A general point is that Ψ is inflexible and causes more questions, but this issue is already dealt with in the body of the text, and this Appendix might only be confusing by repeating that point.

Up to now such a marginal analysis was not very well possible, apparently, because there arises a contradiction that reminds of Russell's Paradox. If the steps satisfy the formal conditions of ZFC then we have another contradiction in ZFC. (This is reflected in Corollary Not-ZFC-2. Such a contradiction can be resolved by the use of the Paul of Venice consistency condition – but then in system ZFC-PV.)

H.2. Testing properties of ZFC at the margin

Conjecture W assumes an arbitrary function and then holds that Ψ exists. Call this *h.* Conjecture H.2 then makes the first element available for mapping to a more relevant $\Psi[f]$. The re-map may cause that $\Psi[h] \neq \Psi[f]$, of course. Still, there is a problematic self-reference that must be dealt with. One way to read Conjecture H.2 is as criticism: Conjecture W assumes that ZFC would properly deal with self-reference but does not *show* that it does.

The following steps in H.2 are looked at more closely in H.3. While H.2 may go too fast the real issue is with H.3. But H.2 gives a nice overview of the general 'proof idea'.

Conjecture. (H.2) Let A be denumerable infinite, $P[A]$ the power set.

(i) For any arbitrary non-trivial $h: A \to P[A]$ there are $f: A \to P[A]$ and $\psi \in A$ with $f[\psi] = \Psi = \{x \in A \mid x \notin f[x]\}$.

(ii) There is a contradiction. (Note that h does not have to be a bijection.)

Proof idea. Consider an ordering of $A = \{a[1], ...\}$ and let $\psi = a[1]$.

(i) Let $B = A \setminus \{ \psi \}$ and $g: B \to P[A]$ as in Hilbert's Hotel $g[a[n]] = h[a[n\text{-}1]]$ for $n > 1$.

Define $f: A \to P[A]$ as:

(a) $x \in B$: $f[x] = g[x]$

(b) $x = \psi$: $f[\psi] = \Psi = \{x \in A \mid x \notin f[x]\}$. (discussion)

(ii) The direct check on consistency gives: $(\psi \in \Psi) \Leftrightarrow (\psi \notin \Psi)$. Q.E.D.

Discussion.

Distinguish two very different issues:

(a) Test whether ZFC allows the definition of this self-referential f.

(b) There is a proper definition.

Given the contradiction it is obvious that f is not well-defined. At issue however is whether ZFC allows the construction (if so, it becomes inconsistent), and how this relates to the Paul of Venice consistency condition (since we might like self-reference for fixed points, even in constructivism).

The standard answer is that ZFC first requires the definition of f and only then allows Separation to create Ψ. Why this artificial sequence ? Why not simultaneously ? Requiring this sequence *prevents one to show* that Ψ is actually nonsensical. Why put on blinders ?

H.3. Taking smaller steps

Let us look in more detail at the steps in conjecture H.2.

Conjecture. (H.3) Let A have at least one element α. $P[A]$ is the power set.

Let $B = A \setminus \{ \alpha \}$ not be empty. Consider arbitrary $g: B \to P[A]$.

(i) There are $f: A \to P[A]$ and a $\psi \in A$ with $f[\psi] = \Psi = \{x \in A \mid x \notin f[x]\}$.

(ii) There is a contradiction. (Note that g does not have to be a bijection.)

Proof idea. (i) $\Psi^*[B] = \{x \in B \mid x \notin g[x]\}$. (With $P[A]$, instead of $P[B]$, there is $\Psi^*[B]$.)

Define: f as: (a) $x \in B$: $f[x] = g[x]$

(b) $x = \alpha$: $f[\alpha] = y$, for some $y \in P[A]$ (e.g. in fixed point form).

Note: $\Psi^*[B] = \{x \in B \mid x \notin f[x]\} = \{x \in B \mid x \notin g[x]\}$.

Let: $M = \{x = \alpha \mid x \notin f[x]\}$ (Margin, discussion)

Ergo: $\Psi^*[B] \cup M = \{x \in A \mid x \notin f[x]\} = \Psi[A]$

Observe that ($\Psi^*[B] \in P[A]$) and ($M \in P[A]$), so that ($\Psi^*[B] \cup M) \in P[A]$.

Take $y = \Psi[A]$.

(ii) Take $\psi = \alpha$. The direct check on consistency gives: $(\alpha \in \Psi) \Leftrightarrow (\alpha \notin \Psi)$.

Q.E.D.

Discussion.

(1) Standardly, ZFC first requires the definition of f and only then allows Separation to create Ψ. Theorem ESV solves this bottleneck by using $M = Ø$. Alternatively, Separation can be done there as required and only afterwards it can be established that $M = Ø$.

(2) If ZFC would allow this deduction, then ZFC is inconsistent. I am no expert on ZFC and would like to hear the views. My guess is that some experts would hold that *M* would be a necessary step for the creation of a surjective function, and that such would not exist in ZFC. This is not the right answer. The question is which axiom has been applied wrongly. Which axiom regulates order ?

The definition of *M* at the margin seems an acceptable application of the Axiom of Separation, namely to set $\{\alpha\}$ and predicate $\gamma[x] = (x \notin f[x])$.

Why does one require that the function has been defined before ? Doesn't the general allocation $y \in P[A]$ allow us to treat f as *some variable, f* ? Wasn't the invention of algebra with the flexible use of variables and formulas a great idea ? Aren't users of ZFC creating artificial barriers to what ought to be natural flexibility in the use of variables and formulas ? Yes, one can understand the hesitation, since this creates inconsistency here, but the cause is not the flexibility but the not-well-defined-ness of Ψ.

(3) From (i) we find:

$$f[\alpha] = \Psi[A] = \{x \in A \mid x \notin f[x]\} .$$

Even though the steps seem innocuous, we derive, using that α is an element of *A:*

$$(\alpha \in \Psi[A]) \Leftrightarrow (\alpha \notin f[\alpha]) \Leftrightarrow (\alpha \notin \Psi[A])$$

The reason lies in the definition of *M.*

Here $(\alpha \notin \Psi^*[B])$ is true and can be dropped:

$$(\alpha \in M) \Leftrightarrow (\alpha \notin f[\alpha]) \Leftrightarrow (\alpha \notin (\Psi^*[B] \cup M)) \Leftrightarrow (\alpha \notin M) \qquad \text{(key step)}$$

$$M = \{x = \alpha \mid x \notin M\} \qquad \text{(Russell !)}$$

We thus have an instance of Russell's paradox. We discussed this when introducing the singleton and nutshell case, on page 65. A singleton, a set with one element, obviously limits our freedom of definition. Now we have a large *A*, possibly infinite. What is crooked here ?

While ZFC blocks Russell's paradox, see page 62, its occurrence here is not direct but indirect by the (circular) steps $M = M[f]$ and $f = f[M]$ in (i), giving a fixed point $M^* = M[f[M^*]]$.

One question is whether ZFC allows the break up into steps as shown.

Creating a ZFC* that blocks such steps and fixed points is not the answer. If ZFC is already this ZFC*, then this is actually a problem. We might like some beneficial fixed point. Now there is a contradiction, but not because of the flexibility of algebra but only because of the paradoxical Ψ.

(4) On the more general issue:

(1) It is not an adequate conclusion that this g does not exist – it is arbitrary – or that it is impossible to reserve an element for a newly created set.

(2) It is not so useful to look into the issue for finite sets, since such sets are smaller than their power sets anyway. The issue remains relevant for infinity, for which the room to select a single element is infinitely larger.

(3) The adequate response is to eliminate this occurrence of contradiction anyhow. Whatever the formal properties of ZFC, we would want to be able to extend a function with a single element while using elementary logic and algebra.

(4) Alternatives have been formulated: ZFC-PV in two-valued logic and BST in three-valued logic. The only reason not to adopt the flexibility of algebra are the transfinites.

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