Background supporting documentation for Appendix B in the paper on Paul of Venice

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1. Introduction

K.P. Hart (2015) - in the Dutch journal of mathematics NAW - reviews Cantor's "Diagonal argument". He presents a view that after the support by Hilbert may be called "traditional" - see Van den Berg (2013) for a perspective on Hilbert.

A refutation of Cantor's diagonal argument can be found in my book "A Logic of Exceptions" (ALOE) (1981, 2007, 2011) which is neglected by Hart. See a review by Gill (2008), also in NAW. Later I updated specifically on Cantor's diagonal argument in *"Contra Cantor Pro Occam - Proper Constructionism with Abstraction"* (CCPO-PCWA) (2012, 2013), on which I informed Hart. See Colignatus (2015a) for the email exchange that followed.

Starting October 29 2014, due to a restaurant discussion with B. Edixhoven, I looked at the relation to the ZFC axioms, which resulted in Colignatus (2015), now with an Appendix B on Hart (2015).

i. This Appendix B in Colignatus (2015) - time stamp May 20 - can stand alone as a "reaction" for the "reaction columns" of NAW ("De Derde Wet").
ii. Colignatus (2015b) contains the rejoinder by Hart w.r.t. Appendix B.
iii. This present discussion supplements this - and will be catalogued as Colignatus (2015c).

One may check that Hart's rejoinder has been used to remove possible sources for misunderstanding in Colignatus (2015) and that Appendix B, but that the arguments still stand.

2. Rejoinder on Hart 2015-05-18: Paul of Venice and ZFC

Normal lines: PKH, May 18 2015
Indented lines: TC, May 19 & 20 2015

On refutation: one does not refute a proof. To refute means to disprove or to deny (The Chambers Dictionary). One can refute (disprove) a statement by demonstrating that it is false, and one can find errors in a proof but not refute it.

(a) See Appendix B point (6) below. There is a problem with the word "rejection". In axiomatics one might reject axioms, but this is not at issue here.
(b) I have looked on the internet again to find a short phrase. Curiously after 2300 years of math there seems to be no short phrase. "To repudiate" exists but seems uncommon.
http://www.thefreedictionary.com/refute
http://www.thefreedictionary.com/repudiate
(c) I have inserted the following text:

"There is a small point on terminology w.r.t. the term "refutation" in the table. A theorem is refuted by a counterexample. For the natural numbers and the reals we can find via the notion of 'bijection by abstraction', Colignatus (2012, 2013). This uses constructive methods that some might not agree with, and it is not the topic of the present discussion. What is refuted in this paper is Cantor's "diagonal argument" in the power set form. Perhaps a better term might be "deproven", in the sense that the theorem is stripped from its proof and no longer can count as a theorem. It may be that the theorem would still hold, but via a different proof. The refutation of 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'."

Section 2.1

In the Addendum on page 4: (b) is unnecessary as *f* is *assumed* to exist.

This point (b) was provided by Bas Edixhoven, see Collignatus (2014) and I tend to agree with him that it is useful to clarify that ZFC does not prohibit this assumption.

What does “ZFC provides for well-defined sets” actually mean? Since you would like to see it proved you should clarify this unambiguously, so that the validity of an argument, for or against, may be judged.

(a) This is the crux of this exercise.
(b) ZFC provides for ZFC-sets. A system V provides for V-sets. The point is that when you arrive at a contradiction in ZFC, then you might as well decided that ZFC is inadequate.
(c) Thus (ZFC & AdditionalAssumption => contradiction) causes you to reject one of the assumptions.
(d) Thus "ZFC provides for well-defined sets” is an hypothesis, the idea that mathematicians have succeeded in formulating a system to capture that notion. The paper now challenges that hypothesis.
(e) The paper indicates alternative axiomatic systems, see the Abstract: ZFC-PV (amendment of de Axiom of Separation) or BST (Basic Set Theory), that might better capture the notion of "well-defined set".
(f) It is remarkable that Hart misses this, and asks "you should clarify this unambiguously" while ZFC-PV and BST are provided. The paper also takes the humble position of indicating the challenge.

That said, it will be very hard to come up with a definition of ‘well-defined’ that will exclude a simple description like $\{x \in A : x \notin f(x)\}$.

> Why would that be hard ? ZFC-PV and BST are provided. They are under your very eyes.

Section 2.2

“We might hold that above Φ' is badly defined since it is self-contradictory under the hypothesis.” Like I wrote above: no matter how I stretch my imagination, the definition of Φ' is still sound. The reason of being ‘self-contradictory’ is not enough for me as this is exactly the point of the proof: to show that the assumption of surjectivity leads to a contradiction.

This seems more like shooting the messenger (the set Φ') for delivering the message that the surjectivity of *f* is untenable.

Replacing a good definition with a bad one — in my eyes the set Φ is badly defined, see below — does not invalidate the proof.

(a) If Φ' satisfies ZFC, then it is a well-defined-according-to-ZFC.
(b) If I do not drop the hypothesis of the surjection (because I just made it), then there still is a contradiction: and this can be explained by that Φ' is badly defined.
(c) Thus well-defined-according-to-ZFC allows for badly-defined objects.
(d) Or, well-defined-according-to-ZFC allows for say badly-defined-in-BST.

Section 3.2

There is an elementary mistake here: after a correct description of the Separation Axiom Scheme — *B* should not be free in ϕ[*x*] — you immediately apply it incorrectly.

> No, I formulate the problem. There is a clear distinction between "misconception" and posing a problem.

In the definition of Φ you use ϕ'[*x*], which stands for

$$\phi[x] \&\& (x \in \Phi)$$

note that the variable Φ *is* free in ϕ'[*x*] as there is no quantifier at all in ϕ'[*x*]. The quantifier ($\exists B$) that you claim binds *B* is *not* part of ϕ'[*x*].

(a) Of course this is so: I alerted the readers to this. This is where the critical look starts.
(b) But the problem is: There is absolutely no reason why a phi[x] ***should*** be studied separately ***outside*** of the quantor. You can choose to do so. But why ?
(c) In this case, with the consistency condition, it is wiser not do so.
(d) Observe that Φ need not be a "free variable" but might also be a constant.
(e) ZFC at least has the ambiguity that ϕ[*x*] can be judged within the existential quantifier (i.e. within the axiom proper) or outside of it as a stand-alone expression. It may be that "defensive" users of ZFC define ZFC such that it should be judged in the latter manner, but one can also see room for 'tolerant' users who see the usefulness of the former.

Therefore the instance of Separation to definition Φ in 2.2 is *not* allowed.

(a) This is not an issue of "allowed" or not. This is a matter how you define and interprete ZFC.
(b) Apparently, you belong to the "defensive" and not the "tolerant" users.
(c) Why would you define ZFC such, that it generates transfinites, and forbids the use of the consistency criterion ?

This is why I do not consider the insertion of the 'consistency condition' an eye-opener, as you hoped it would be, but simply a bad step.

(a) Why quotation marks for 'consistency condition' ? Does it not work like that for the Russell set - in 'naive set theory' (Frege ?) outside of ZFC ? Does it not work like that for the Liar paradox ?
(b) Are you aware that you eliminate a sensible condition merely with reasons of "form" ? The consistency condition is not inconsistent, is it ? At least it should be neutral. So why forbid it ?
(c) You still have not explained the difference between Phi and Phi-accent. I keep asking and never get a reply. Now you misconstrue my critical question as to whether I would have a "misconception" and make an "elementary mistake". Now you answer in terms of "should" and "not allowed" as if ZFC has descended from the sky as the word of the Divine. But Phi-accent under ZFC-defensive and Phi under ZFC-tolerant or BST should produce the same set, since the consistency condition cannot have a material effect. So why do we still see the different mathematical outcome ? This is an anomaly for your belief in ZFC and you should not hide from the problem but face it.

[*A*] To elaborate further: bound variables are, so to speak, invisible to the outside of the formula and, indeed, in any book on logic one finds some kind of discussion on this topic and a lemma to the effect that if one replaces all bound occurrences of a variable in a formula ψ by a variable that does not occur in ψ then the result is a logically equivalent formula.

[*B*] The upshot of this is that one could reformulate the Separation Axiom Scheme to read that *B* should not occur in ϕ[*x*] and not lose any generality at all.

(a) My reference book is Howard DeLong (1971), "A profile of mathematical logic".
(b) To a large extent agreed with [*A*]. For example (There is y such that f[x,y]) is replaced by f[x, Y] for some Y.
(a) (c) I do not follow you on the step from [*A*] to [*B*]. Let us look at the possibility in 3.2.1: $(\forall A)\ (\exists B)\ (\forall x)\ (x \in B \ \Leftrightarrow\ ((x \in A)\ \&\ \varphi[x]\ \&\&\ (x \in B)))$ and then replace the bound *B* by some *H*: $(\forall A)\ (\forall x)\ (x \in H \ \Leftrightarrow\ ((x \in A)\ \&\ \varphi[x]\ \&\&\ (x \in H)))$. Actually *H* =

H[φ]. I do not see a reason why the consistency condition cannot be inserted. Thus the reasoning from [*A*] to [*B*] is mysterious.

This challenge to ZFC has failed.

(a) No.
(b) You have not replied my answer on Phi and Phi-accent.
(c) It is Cantor who emphasised the freedom in mathematics. So why deny people the freedom to challenge ZFC and develop better axioms for set theory ?

The Conclusion

The above should make it clear that I do not agree with this section. Point 1 states the diametrically opposite of what I think of the two constructs: the Cantor/Russell construction is sound; the 'consistency condition' makes it unsound.

In point 6: ZFC is a theory, that is, a collection of formulas in the language of Set Theory; it is not a model. The word 'model' has a completely different meaning in Mathematical Logic.

(a) It is clear that you do not agree, and that you have given no basis for that disagreement.
(b) DeLong (1971) "A profile of mathematical logic" has a discussion of axiomatic systems. One of the properties is that a system tends to have an "intended interpretation". It is said that the system is a model for that intended interpretation. For example, Peano Arithmetic is an axiomatic system that would capture elementary arithmetic as done in elementary school, and be a model for it. Thus I disagree with your description.

3. Rejoinder on Hart 2015-05-07: Appendix B point (6)

Colignatus (2015) (May 1) presented Appendix B with a criticism of Hart (2015).
Hart responded on May 7, and that caused a rejoinder Colignatus (2015b).
The latter rejoiner caused some other comments by Hart May 18, which have been included in the update May 20 version.

Point (6) takes a special position: this is the criticism that Hart has been neglecting the refutation given in Colignatus (2012, 2013).

On May 20, this criticism seems no longer valid, since Hart on May 18 has responded. However, for the paper Hart (2015) the criticism still is valid.

This section looks at

TC May 1: (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.

1. PKH May 7:

> (6) Zoals gezegd: Uw 'weerleggingen' verdienen die naam niet. De enige 'weerlegging' die ik in Uw geschriften

TC May 8:

(a) It would be helpful if you could be more specific about what would be wrong about the refutation in CCPO-PCWA of 2012: both for the form with the decimals (equivalent to Cantor 1890/91) and the Appendix (w.r.t. Cantor 1874).

Please note that in 2012 you reacted to CCPO, now a legacy version, and that CCPO-PCWA was written for a constructivist journal, and that I used your comments for clarification. For the form on the power set, the paper on Paul of Venice of 2015 may be more accessible - and Appendix C refutes your 'direct proof' from 2012.

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2. PKH May 7:

> naam niet. De enige 'weerlegging' die ik in Uw geschriften heb gezien is een uitspraak als onderaan bladzijde 5. Daar spreekt U niet van weerlegging maar van afwijzing. Onder het kopje "2.2 Rejection ..." staat "We might hold that Φ' is badly defined since it is self-contradictory under the hypothesis". De bedoelde hypothese is waarschijnlijk 'f is surjectief'.

TC May 8:

This is the semantics of rejection versus refutation. How does one refute a reductio ad absurdum proof ?
One method is to show that it makes an error so that it can be rejected.

[PM - see the discussion of terminology above]

(b) When you refer to the Paul of Venice paper of 2015, then please observe that I allow for the possibility that the proof on the power set (Russell 1906) might hold in ZFC, but that this would require an explanation about Phi and Phi-accent. Please give that explanation.

(c) My suggestion is also that ZFC may have an ambiguity. The set that is being separated falls under the existential quantifier, and thus is not a free variable (as you suggested in 2012).

[PM. The Paul of Venice paper now has an Appendix E on this]

(d) Indeed, "under the hypothesis" refers to f being a surjection.

(e) There is also something that I came upon in an exchange with Richard Gill. If something is wrong about *f,* then it disappears, leaving only its grin like the Cheshire Cat. However, before we conclude about a contradiction, and then reject a major assumption like on *f*, we must check carefully whether the separate steps are well-defined and properly taken on the minor assumptions.

(f) If Phi-accent is badly defined, then it cannot be used further to prove something.

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3. PKH May 7:

> Mijn conclusie aan het eind van het bewijs is: "de aanname 'f is surjectief' leidt tot een tegenspraak, dus die aanname klopt niet en dus zijn er geen surjecties". In mijn ogen is het bewijs correct.

TC May 8:

Simply deriving a contradiction is not enough. See above point (e) and (f).

(g) A hidden assumption is always that ZFC is adequate. When you derive a contradiction then you must allow for the possibility that ZFC is not adequate.

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4. PKH May 7:

> De rest onder het kopje 2.2 is niet meer nodig: U vind het bewijs niet goed; het is dan niet nodig Φ in te voeren want Φ geeft geen echte weerlegging. De enige manier

TC May 8:

(h) Phi shows that the proof stops and doesn't come to a conclusion. There is no proof.
It is a proper refutation of the idea that there would be a proof.

(i) Formulation of Phi is necessary to show the confusion on Phi-accent.

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5. PKH May 7:

> waarop U mijn kan overtuigen is door expliciet een bijectie tussen A en $\mathcal{P}(A)$ aan te geven.

TC May 8:

(a) CCPO-PCWA gives the "bijection by abstraction". See the email exchange why your rejection of this definition is curious: why reject a definition? http://thomascool.eu/Papers/ALOE/KPHart/2015-05-06-Review-emails-Colignatus-KPHart-2011-2015.pdf
Please note that Cantor cannot give a value for i when he redefines the d[i,i] that he already defined (translating 1890/91 to decimals). My conclusion is that i = infinity, which makes his procedure undefined. Why do you allow Cantor this freedom, but do you require me to fully specify this ?
(b) I am very much surprised that Paul of Venice's consistency criterion does not cause an AH-Erlebnis. Why do you lack this mathematical intuition that it is important ? You declare it unallowable under ZFC - but why do you not allow that ZFC was formulated without awareness of this consistency condition ? It is, please observe, a *consistency* condition. Please explain the difference between Phi-accent in 2.1 and and Phi in 2.2.

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6. KPH May 18:

May 8 (a): I have looked up the 'bijection by abstraction' again. The material on page 16 (2012-0326-CCPO-PCWA.pdf) is inconclusive to say the least: "The @ can be read as 'abstraction'.", "The switch can be interpreted …". So: 'can be' but 'need not'? And what does 'abstraction' entail? The material does not explain anything.

The definition is full of unsubstantiated claims: namely that 'abstraction' somehow creates , , and a map between them. There are no explanations of how this happens or arguments that the effect is as claimed. I do not so much reject the definition but rather the claim that it accomplishes anything.

TC May 20:

(a) It can be read so, but doesn't need to. A formal use should work,
(b) That paper discusses the Van Hiele levels, and assumes that mathematicians know about abstraction.
(c) A new paper on abstraction is Colignatus (2015d): "*An explanation for Wigner's "Unreasonable effectiveness of mathematics in the natural sciences*"
(d) It is not true that " The material does not explain anything". It clarifies that the set of natural numbers is created by abstraction. Unless you can show me the set of natural numbers as a concrete object.
(e) The definition simply sits there. And it is shown how it can be applied.
(f) "I do not so much reject the definition", which is proper, since it is not inconsistent. Given the definition you must conclude that there is a "bijection by abstraction" between N and R Why do you not accept that inference ?
(g) " but rather the claim that it accomplishes anything". The claim is that it solves something for the didactics of mathematics in highschool. What other claim do you think that there is ? It does not change anything about N or R or their properties, so what are you referring to ?

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7. KPH May 18:

Point (b) on page 21 (2012-03-26-CCPO-PCWA.pdf) can be read as a proof that the map is not surjective: we cannot find a member of that corresponds to 1/3.
This makes the whole concept rather unconvincing.

TC May 20:

(a) The paper explains why abstraction eliminates such constructive indentification. The title of the paper states: " *Proper constructivism with abstraction".*

(b) The paper states that it is inconsistent to allow Cantor an unspecified diagonal element while not allowing such unspecification for the bijection.

(c) On May 8 I asked: " Please note that Cantor cannot give a value for i when he redefines the d[i,i] that he already defined (translating 1890/91 to decimals). My conclusion is that i = infinity, which makes his procedure undefined. Why do you allow Cantor this freedom, but do you require me to fully specify this ?" Did you not read or understand this ?

(d) Why is it unconvincing that it helps didactics in highschool ? My question to you is whether you agree that it is clear and consistent, not whether you are "convinced" (on criteria that you do not specify).

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8. KPH May 18:

May 8 (c): see above for why it is *not* an eye-opener.

TC May 20:

(a) The question about the eye-opener was not quite on the decimal diagonal.

(b) The question on the eye-opener is targeted at the impact of the Paul of Venice consistency criterion on naive set theory, when one applies it to the Russell paradox.

The environment of application is unsophisticated at that point, but one can see the impact. Someone knowledgeable in set theory could be expected to have a flash of insight that this could be made to work in sophisticated environment like ZFC too - perhaps requiring some modifications.

(c) Given that I in the period 1981-2015 did not meet a mathematician who had the same flash of insight, I subsequently wrote said paper on Paulus Venetus, with my suggestion on the Axiom of Separation. This is a more complex environment and I do not suppose that this will have the direct effect of an eye-opener.

4. Rejoinder on Hart 2015-05-18: The 1874 proof

This conderns Colignatus (2012, 2013), 2012-03-26-CCPO-PCWA.pdf, Appendix A p30+.

Normal lines: PKH, May 18 2015
Indented lines: TC, May 19 & 20 2015

As to the ‘refutation’ of the interval proof: the redo of the proof does not mention a given sequence of real numbers; the recipe for choosing the next interval is abandoned, indeed there is no relation between the intervals [*a*[*d*],*b*[*d*]] and the numbers in the sequence; the paragraph discussing interiors establishes nothing: what should we make of “… the notion of an ‘interior’ apparently loses ‘grip’ when …” (to me it sounds like nonsense).

(a) A sequence **is given**, the recipe **is applied**, and there **is a relation** between the intervals and the numbers in the sequence:
(b) "[0.1, 0.2], then [0.11, 0.12], [0.111, 0.112] and so on. (Rather nicely we might think of the limit value of 1/9.)"
(c) The counterexample uses a sequence with *all* real numbers. (It is curious that Cantor holds that his method would create a non-real number.)
(d) The conclusion that invalidates the "proof" is:

The suggestion that there is an $\eta \in [\alpha, \beta]$ but $\eta \notin \mathbb{R}$ is erroneous since we see that all elements of $\mathbb{R}$ are represented in X.

(e) The quote "the notion of an ‘interior’ apparently loses ‘grip’ when we take the step of abstraction” is taken from the ***discussion*** of this refutation. In such a discussion an author has more freedom to allow for suggestions.
(f) If KPH finds "to me it sounds like nonsense" on the latter ***discussion*** then he is free to do so.
(g) But he is not free to ignore the conclusion that invalidates the "proof".

5. Rejoinder on Hart 2015-05-18: The 1890 / 91 proof (decimal form)

KPH May 18:

May 8 (b): on page 24 in 2012-03-26-CCPO-PCWA.pdf (I assume you refer to this) there is a very confused presentation of the decimal proof. There are two labels, *D* and *C*, and n_D is used to denote the diagonal number $0.d_{1,1}d_{2,2}\ldots$ and n_C denotes the number obtained by setting

$n_{C,i}$=2 if $d_{i,i}$=1 and $n_{C,i}$=1 otherwise. At this point *C* and *D* do not seem to have any relation with the anonymous map between N and R.

Then suddenly *C* should not be a label but a natural number and, without warning, n_i seems to be the image of *i* under the heretofore anonymous map.

This is seriously bad writing and invalidates any point you want to make on that basis.

TC May 20:

(a) This is seriously bad *reading* and invalidates any criticism you want to make on that basis.
(b) For readers who have not yet looked at page 24: check from the above that "n_D" is a unique symbol for the diagonal number, with "n" as a label for number and "D" as a label for diagonal (in this combination since it need not be obvious that the diagonal is a number), while the definition of n_C implies that C is used as a natural number index.
(c) A comparable flexibility of use is with superscripts: n' (n-accent), n^S (n-label-S) and n^2 (n-square).
(d) If a professional mathematician from TU Delft cannot read properly then I am willing to consider using say D instead of n_D so that we have dealt with this "criticism".
(e) I do not agree with the implied suggestion that a new version of the paper would be required before a discussion on content would be feasible.
(f) I do not observe any criticism by PKH on the content of my refutation, which I asked him to do since 2012.
(g) I maintain my criticism that Hart (2015) does not refer to Colignatus (2012, 2013) and misleads his readership.

6. Rejoinder on Hart 2015-05-18: Appendix C

Normal lines: PKH, May 18 2015

Indented lines: TC, May 19 & 20 2015

> TC finds that PKH's discussion of Appendix C does not deal with the refutation of his proof.

Like I wrote: one can not refute a proof, but one can point out errors or incompleteness.

> See all the way up for a discussion of terminology.

The reason for not accepting the proof on page 11 is mystifying: the proof shows that *all* functions are not surjective. There is no reason to consider cases.

> (a) This cannot be 'mystifying'. It is clearly stated that the proof relies on a hidden assumption "There is no reason to consider cases".
>
> (b) There is every reason to doubt the theorem since it causes mystifying "transfinites", and the proof uses a scheme that reminds of Russell's paradox.

Also, the paragraph starting with 'Negative' provides questions rather than answers.

> These questions are not material to the refutation in section C.3.

• What makes a proof 'proper constructive'?

> This is explained in the paper CCPO-PCWA.

- What is your definition of ‘finite’?

  CCPO-PCWA gives N[n] as a finite set for n in N.

- You “proposed the notion of ‘definition by abstraction’”. Did you ever work out the details?

  This is explained in the paper CCPO-PCWA.

- “If such a bijection would exist …”, so you are not sure that they do.

  (a) President Putin is infamous for interpreting politeness and diplomacy as signs of weakness. In science those tend to be seen as signs of strength.

  (b) Please look at what is being said, rather than how it is said.

Point (1) on page 12 is illogical: you have shown nowhere that bijections between sets and their power sets exist, yet you claim that the proof of their non-existence fails because they exist? That’s circular reasoning at best.

> (a) There is no circularity here in C.4., and, as in this whole paper, the objective is to show that the proof is invalid, and the objective is not to show how a bijection can be created.
>
> (b) You neglect the other points that put (1) into perspective.

7. Some additional points

(1) The application of the consistency condition by Paulus Venetus, in its original shorthand formulation, might cause infinite regress when applied to sets. ALOE solved this by the exception switch. This was shown for the Russell Paradox. Subsequently it was shown, but not immediately published since it seemed rather obvious, that this could also be done for other applications, like for Cantor's Theorem.

Colignatus (2012, 2013) preferred the shorthand formulation for reasons of accessible presentation. Hart (2012) however did not check the longer form in ALOE and came with the "criticism" of .... infinite regress.

See the email exchange in Colignatus (2015a) that it could not be resolved in 2011-2015 that Hart was willing to backtrack and recognise the shorthand presentation for what it was. (He could have seen the solution himself too.)

To avoid this kind of needless discussion, Colignatus (2015) now presents the argument with the exception switch, which makes for a less accessible and less intuitive presentation.

Hart, in his replies of May 7 and 18, however does not state whether he accepts the exception switch - and that his earlier criticism in 2012 was not to the point.

(2) Checking the documentation would find more of such cases, but the above is the one that strikes me most.

Appendix: Original Hart 2015-05-18, using Latex2rtf

[Titel=Some comments on “A Condition …” =cmssq8 (), AuteurA=K. P. Hart, AdresA=Faculteit EEMCS TU Delft Postbus 5031 2600 GA Delft, EmailA=k.p.hart @ tudelft.nl, kolommen=2]

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What does "ZFC provides for well-defined sets" actually mean? Since you would like to see it proved you should clarify this unambiguously, so that the validity of an argument, for or against, may be judged.

That said, it will be very hard to come up with a definition of 'well-defined' that will exclude a simple description like $\{x \in A : x \notin f(x)\}$.

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This seems more like shooting the messenger (the set Φ') for delivering the message that the surjectivity of *f* is untenable.

Replacing a good definition with a bad one — in my eyes the set Φ is badly defined, see below — does not invalidate the proof.

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note that the variable Φ *is* free in $\phi'[x]$ as there is no quantifier at all in $\phi'[x]$. The quantifier $(\exists B)$ that you claim binds *B* is *not* part of $\phi'[x]$.

Therefore the instance of Separation to definition Φ in 2.2 is *not* allowed. This is why I do not consider the insertion of the 'consistency condition' an eye-opener, as you hoped it would be, but simply a bad step.

To elaborate further: bound variables are, so to speak, invisible to the outside of the formula and, indeed, in any book on logic one finds some kind of discussion on this topic and a lemma to the effect that if one replaces all bound occurrences of a variable in a formula ψ by a variable that does not occur in ψ then the result is a logically equivalent formula. The upshot of this is that one could reformulate the Separation Axiom Scheme to read that *B* should not occur in $\phi[x]$ and not lose any generality at all.

This challenge to ZFC has failed.

The Conclusion

The above should make it clear that I do not agree with this section. Point 1 states the diametrically opposite of what I think of the two constructs: the Cantor/Russell construction is sound; the 'consistency condition' makes it unsound.

In point 6: ZFC is a theory, that is, a collection of formulas in the language of Set Theory; it is not a model. The word 'model' has a completely different meaning in Mathematical Logic.

Appendix B

I have already reacted to Appendix B, but have one or two things to say about your reactions.

(1) There appears to be a misconception about the nature of the various proofs discussed in the article in NAW.

If one reads, for example, Russell's argument (page 42, column 1) then there is no assumption that the correlation is onto and the conclusion at the end is that in any one-one correlation at least one class (i.e., subset) is omitted.

Many presentations would, spuriously, preface this presentation with an assumption that the correlation is surjective and, again spuriously, end by saying "contradiction, the correlation is not surjective".

See also below, when I discuss Appendix C.

(2) A look in any book on Set Theory will show that the term 'class' is used judiciously: only for objects of the form $\{x{:}\phi\}$, where ϕ is a formula with x among its free variables. As a consequence the members of a class are sets only, and the informal 'class of all classes' has no formal counterpart in ZFC.

(3) The introduction to Cantor's 1890/91 paper states that its purpose is to give *an easier proof of the existence of uncountable sets without the use of irrational numbers* (paraphrase). Therefore it is incorrect to state that Cantor used binary expansions as he does not mention real numbers or zeros and ones in the main part of the paper.

One may employ the ideas from the paper to create proofs of the uncountability of the real line but, to repeat, that was not the stated purpose of the paper.

(4d) My reasons for not liking the decimal proof as much as the others are

- it requires an arithmetization of the real line
- it requires a choice when a real number has two expansions
- there is not a unique way of using the diagonal to create (a representation of) a real number that does not occur in the list

Both the proof in 1874 and that in 1890/91 avoid choices like these and that is why I like them better. To elaborate: the reason is one of aesthetics, there is nothing mathematically wrong with these arguments; the others are more direct and concise.

6 (a): see below for some remarks about the 'refutation' of a version of the decimal argument. As to the 'refutation' of the interval proof: the redo of the proof does not mention a given sequence of real numbers; the recipe for choosing the next interval is abandoned, indeed there is no relation between the intervals $[a[d],b[d]]$ and the numbers in the sequence; the paragraph discussing interiors establishes nothing: what should we make of "… the notion of an 'interior' apparently loses 'grip' when …" (to me it sounds like nonsense).

May 8 (a): I have looked up the 'bijection by abstraction' again. The material on page 16 (2012-0326-CCPO-PCWA.pdf) is inconclusive to say the least: "The @ can be read as 'abstraction'.", "The switch can be interpreted …". So: 'can be' but 'need not'? And what does 'abstraction' entail? The material does not explain anything.

The definition is full of unsubstantiated claims: namely that 'abstraction' somehow creates , , and a map between them. There are no explanations of how this happens or arguments that the effect is as claimed. I do not so much reject the definition but rather the claim that it accomplishes anything.

Point (b) on page 21 (2012-03-26-CCPO-PCWA.pdf) can be read as a proof that the map is not surjective: we cannot find a member of that corresponds to 1/3.

This makes the whole concept rather unconvincing.

May 8 (b): on page 24 in 2012-03-26-CCPO-PCWA.pdf (I assume you refer to this) there is a very confused presentation of the decimal proof. There are two labels, D and C, and n_D is used to denote the diagonal number $0.d_{1,1}d_{2,2}\ldots$ and n_C denotes the number obtained by setting $n_{C,i}{=}2$ if $d_{i,i}{=}1$ and $n_{C,i}{=}1$ otherwise. At this point C and D do not seem to have any relation with the anonymous map between and .

Then suddenly C should not be a label but a natural number and, without warning, n_i seems to be the image of i under the heretofore anonymous map.

This is seriously bad writing and invalidates any point you want to make on that basis.

May 8 (c): see above for why it is *not* an eye-opener.

Appendix C

Like I wrote: one can not refute a proof, but one can point out errors or incompleteness. The reason for not accepting the proof on page 11 is mystifying: the proof shows that *all* functions are not surjective. There is no reason to consider cases.

Also, the paragraph starting with 'Negative' provides questions rather than answers.

- What makes a proof 'proper constructive'?
- What is your definition of 'finite'?
- You "proposed the notion of 'definition by abstraction'". Did you ever work out the details?
- "If such a bijection would exist …", so you are not sure that they do.

Point (1) on page 12 is illogical: you have shown nowhere that bijections between sets and their power sets exist, yet you claim that the proof of their non-existence fails because they exist? That's circular reasoning at best.

References

Berg, B. van den (2013), 'Hilbert en de bewijstheorie', *Nieuw Archief voor Wiskunde* 5/14 nr. 1, March, pp. 45-48

Bochenski, I.M. (1956, 1970), *A history of formal logic*, 2nd edition, Chelsea, New York

Colignatus, Th. (1981 unpublished, 2007, 2011), *A logic of exceptions,* (ALOE) 2nd edition, Thomas Cool Consultancy & Econometrics, Scheveningen (PDF of the book online at http://thomascool.eu/Papers/ALOE/Index.html)

Colignatus, Th. (2012, 2013), *Contra Cantor Pro Occam - Proper constructivism with abstraction*, paper, http://thomascool.eu/Papers/ALOE/2012-03-26-CCPO-PCWA.pdf

Colignatus, Th. (2014), *Logical errors in the standard "diagonal argument" proof of Cantor for the power set",* memo, http://thomascool.eu/Papers/ALOE/2014-10-29-Cantor-Edixhoven-02.pdf

Colignatus, Th. (2015), *A condition by Paul of Venice (1369-1429) solves Russell's paradox, blocks Cantor's diagonal argument, and provides a challenge to ZFC*, http://thomascool.eu/Papers/ALOE/2014-11-14-Paul-of-Venice.pdf (old link but new file)

Colignatus, Th. (2015a), *Review of the email exchange between Colignatus and K.P. Hart (TU Delft) in 2011-2015 on Cantor's diagonal argument and his original argument of 1874,* May 6 (thus limited to up to then), http://thomascool.eu/Papers/ALOE/KPHart/2015-05-06-Review-emails-Colignatus-KPHart-2011-2015.pdf

Colignatus, Th. (2015b), *Reaction to Hart (2015) about Cantor's diagonal argument,* http://thomascool.eu/Papers/ALOE/KPHart/2015-05-08-DerdeWet-KPHart-with-comments-KPH-and-TC.pdf (old link but new file)

Colignatus, Th. (2015c), this paper

Colignatus, Th. (2015d), *An explanation for Wigner's "Unreasonable effectiveness of mathematics in the natural sciences*, January 9, http://thomascool.eu/Papers/Math/2015-01-09-Explanation-Wigner.pdf

Coplakova, E., B. Edixhoven, L. Taelman, M. Veraar (2011), *Wiskundige Structuren,* dictaat 2011/2012, Universiteit van Leiden and TU Delft, http://ocw.tudelft.nl/courses/technische-wiskunde/wiskundige-structuren/literatuur

DeLong, H. (1971), *A profile of mathematical logic*, Addison-Wesley

Gill, R.D. (2008), 'Book reviews. Thomas Colignatus. A Logic of Exceptions: Using the Economics Pack Applications of Mathematica for Elementary Logic", *Nieuw Archief voor Wiskunde,* 5/9 nr. 3, pp. 217-219, http://www.nieuwarchief.nl/serie5/pdf/naw5-2008-09-3-217.pdf

Hart, K.P. (2011, 2013), "Verzamelingenleer", http://fa.its.tudelft.nl/~hart/37/onderwijs/verzamelingenleer/dictaat/dictaat-A4.pdf

Hart, K.P. (2012), cool.pdf. No title. Comment on Colignatus CCPO 2011 (now a legacy version). See February 29

Hart, K.P. (2015), 'Cantors diagonaalargument', *Nieuw Archief voor Wiskunde* 5/16, nr 1, March, pp. 40-43, http://www.nieuwarchief.nl/serie5/pdf/naw5-2015-16-1-040.pdf

Hodges, W. (1998), 'An editor recalls some hopeless papers', *Bulletin of Symbolic Logic*, 4 pp. 1-16