REACTION TO HART (2015) ABOUT CANTOR'S DIAGONAL ARGUMENT

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May 5 2015 for NAW, with **KPH May 7 & 18** and **TC May 8 & 20**

Hart (2015) reviews Cantor's "Diagonal argument". He presents a view that after the support by Hilbert may be called "traditional". Readers may benefit from Van den Berg (2013) for a perspective on Hilbert. It so happens that a refutation 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) in this journal. 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. In November 2014 I gave an update on the relation to the ZFC axioms. Now in (2015) I have included an Appendix B on Hart (2015). The following is that Appendix B:

TC May 8 & 19 & 20: The Appendix B received a response by Hart on May 7. My rejoinder of May 8 got a response on May 18 which I now also comment on.

I copy his comments into the text below, using a new page per point.

Please use the May 20 2015 version of the Paul of Venice paper (referesh your cache): http://thomascool.eu/Papers/ALOE/2014-11-14-Paul-of-Venice.pdf (old link, new version)

The earlier email exchange 2011-2015 is available at: http://thomascool.eu/Papers/ALOE/KPHart/2015-05-06-Review-emails-Colignatus-KPHart-2011-2015.pdf

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KPH May 7

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Enige opmerkingen over Appendix B

Ik zal de punten in Appendix B nalopen. originele bewijzen

(1) Hart (2015:43) holds correctly that a bijection doesn't have to be used, but only the surjection. 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'). PM. Hart (2015:42 first column) suggests that the power set version of Cantor's Theorem was given by Bertrand Russell 1907, using a bijection.

> (1) Het punt dat ik aan het eind wilde maken was tweeledig: Cantor zelf gaf rechtstreekse bewijzen van de vorm "gegeven een willekeurige functie … conclusie de functie is niet surjectief"; daarnaast gaf ik mijn mening over de structuur van veel van de bewijzen die gepresenteerd worden: men begint met "zei f een bijectie" bewijst vervolgens direct dat f niet surjectief is en schrijft dan "Tegenspraak". Als men begint met "zei f een willekeurige functie", daarna het bewijs geeft en aan het eind concludeert dat f niet surjectief is dan is hetzelfde bereikt zonder "Tegenspraak" te hoeven roepen.

TC May 8: This does not answer to my criticism that a reference to Gillman 1987 on 'spurious contradiction' is inadequate w.r.t. the proof which uses the bijection and reductio ad absurdum method.

Hart p43 column 3:

> Ten slotte: Cantors stellingen en bewijzen zijn direct, niet uit het ongerijmde. De meeste presentaties beginnen met "Stel f is een bijectie …" en roepen aan het eind "Tegenspraak!", zonder dat in het bewijs ooit gebruikt is dat f bijectief verondersteld is. Typisch een geval van wat Leonard Gillman in [8] een "spurious contradiction" noemde.

P.M. Colignatus (2015) Appendix C shows that the Cantor-Hart direct method contains a hidden contradiction, which may be called "spurious non-contradiction".

KPH May 18:

(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.

TC May 19:

(a) There is no misconception of what PK Hart claims.

(b) His claim is wrong.

(c) There is a contradiction. And it is based upon a supposition. Russell states:

> late, i.e., of w. Hence the supposition that w is the correlate of x leads to a contradiction. Hence, in any one-one correlation of all the terms of u with classes contained in u, at least one class contained in u is omitted.

(d) The methods of proofs, what KP Hart calls "direct" and the open "reductio ad absurdum", are mathematically equivalent

(e) KP Hart wrongly applies the term "spurious contradiction".

(f) His rejoinder of May 18 does not respond to the criticism of May 1 (or May 5 for NAW) or the rejoinder of May 8, and merely states that it would be a misconception.

(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 that not its main weaknesses and alternatives are discussed.

> (2) Er is geen sprake van 'incongruity' in het gebruik van klassen: ik gaf aan dat klassen het toelaten stellingen wat bondiger (en wellicht inzichtelijker) te formuleren maar dat ten allen tijde een vertaling in termen van formules en implicaties gemaakt kan worden.

TC May 8: Hart p42 column 1:

> *zij V* zelf natuurlijk geen verzameling is. Dit bracht Cantor er toe onderscheid te maken tussen 'eigenlijke' en 'oneigenlijke' verzamelingen, hetgeen zijn theorie er niet mooier op maakte.

and Hart p42 column 3:

> Overigens wordt Cantors onderscheid tussen 'eigenlijke' en 'oneigenlijke' verzamelingen in de verzamelingenleer op informele wijze nog wel gehanteerd, alleen spreekt men liever van verzamelingen en (echte) klassen.

Subsequently Hart gives an example case how an informal statement that uses classes can be translated into a formal statement with sets. However, this is only an example case.

Hart does not show what he now states: that this could be done for all infomal statements about classes. Consider for example this informal statement: There is a class of all classes.

Perhaps one might hold that mathematicians who have precise statements about ZFC can discuss these in terms of classes without creating confusion - and that they abstain from using class when they have no such translation. This might seem like an ideal situation seen from ZFC. But it might be tested by an empirical enquiry. How often did the informal use of class lead ZFC researchers astray ? And what results have been missed, by not formalizing class ?

I uphold that he describes an incongruity.

KPH May 18:

(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: ϕ}, 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.

TC May 19:

This is incomprehensible:

(a) If such classes satisfy ZFC then they are sets, and there is no reason to give them another name, and there is no reason for Hart to speak about 'informal' use.

(b) Given that Hart really does present a distinction between "verzamelingen en (echte) klassen" and really provides a witness report about such "informal" use, we can presume that classes do not satisfy ZFC - and thus the question is what they are.

(c) It is ambiguous to refer to books on "Set Theory" when the term "set" is reduced to the meaning "satisfying ZFC". For example, Hart might hold that the paper on the challenge to ZFC doesn't need to be discussed in his syllabus for students at TU Delft, since the syllabus discusses ZFC-sets. This would be an improper treatment of Aristotle's original reference to "all".

(3) On page 43 Hart mentions the argument concerning $\aleph \sim \Re$ 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.

> (3) Ik ben bang dat U dit stuk niet goed gelezen heeft. In het artikel van 1890/91 kondigde Cantor aan *geen* irrationale getallen te gaan gebruiken; er staan geen decimale noch binaire ontwikkelingen ontwikkelingen van reële getallen in. Het hoofdbewijs gaat over de verzameling rijen van m-en en w-en — niet eens nullen en enen.
>
> Men kan tussen de regels gaan zitten lezen en Cantor's bewijs ombouwen tot een bewijs, via ontwikkelingen in een of andere basis, dat het interval $(0, 1)$ overaftelbaar is maar, en dit is mijn punt, ik heb dit niet expliciet in Cantor's gepubliceerde werk gezien. Ik ben Cantor's werken gaan lezen om er achter te komen waar en wanneer de aan hem toegeschreven resultaten verschenen waren; ik was voornamelijk verbaasd dat het decimalen-bewijs niet in zijn werk te vinden is.

TC May 8:

(a) Cantor 1890/91 uses binaries. We can agree with Hart's surprise.

(b) Young & Young 1906 use decimals. They hold this equivalent to (a).

(c) There is indeed that formal equivalence.

(d) While (a) might appeal to abstract mathematicians, (b) is more didactic for students who are used to decimal expressions.

(e) Hart p43 column 3 states:

> Wie echter de moeite neemt het verzameld werk van Cantor, [6], door te ploegen zal dit bewijs niet tegenkomen. Ook Joseph Dauben noemt dit bewijs niet in zijn biografie van Cantor [7].

(f) My criticism stands. Hart does not distinguish between formal equivalence and didactic value. Cantor's 1890/91 *is* the diagonal argument. *The mathematical argument does occur in his work*. Only the didactic form with decimals apparently doesn't occur.

What is important about this mathematical argument is that Cantor first defines an element d[i, i] and then redefines it as if he still has the freedom to do so.

I reject the suggestion that I did not read this properly.

KPH May 18:

(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.

TC May 19:

I presume that it is reported correctly that Cantor in 1890/91 **stated** that he did not use irrationals. But he used elements m and w, which is binary, on infinite "co-ordinates" (positions), which is equivalent to the development of decimals, whence he **implicitly used** the decimal expansion of irrationals. One should hope that mathematicians know about "the positional system". You have to look at what Cantor did mathematically and not what he claimed to do. It is amazing that Hart has no grain of doubt in his mind and doesn't ask another mathematician whether Young & Young in 1906 weren't actually right.

Dat nieuwe bewijs begint met "zwei einander ausschließende Charaktere" m en w, en beschouwt dan de verzameling M van alle elementen

$$E = (x_1, x_2, \ldots, x_\nu, \ldots)$$

met oneindig veel coördinaten $x_1, x_2, \ldots, x_\nu, \ldots$, die alle gelijk zijn aan m of w. Deze verzameling heeft meer elementen dan $\mathbb{N}$. Laat

(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.

> (4) Ik weet niet of mijn artikel vooringenomenheid uitstraalt; ik heb het er in ieder geval niet bewust ingestopt. Het doel was vooral de lezer er op te attenderen dat de beschrijvingen van Cantor's werk op zijn zachtst gezegd niet volledig zijn, en eigenlijk niet correct. Ik vind dat de
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> originele bewijzen beter bekend zouden moeten zijn; als dat van vooringenomenheid blijk geeft, het zei zo.

TC May 8:

(i) I am happy to hear that there is no intentional bias. However, I have to point out that the article gives that impression of bias: "that he wants to emphasize that some authors still have questions about Cantor's argument". Hart does not deal with this observation.

(ii) The bias that KPH here refers to - that Cantor's original work should be known better - is not the one that I indicated as the problem.

(iii) There is a distinction between (*) researching what Cantor did, and (**) researching into the theorem and method of proof that are identified by his name. Researching into the latter does not necessarily imply a research on the first. Someone who looks into a fixed point theorem and proof doesn't necessarily have to look into what Brouwer originally did.

(iv) Please observe that Cantor didn't have ZFC. Thus there is ample reason to look at the current state of affairs.

(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.

> (4a) De email van Hodges voegde niets toe aan de onderhavige discussie.

TC May 8: It does. The one-sided reference to Hodges gives a one-sided impression, at the cost of me and Hodges.

(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.

> (4b) Ik beschuldigde niemand van slecht lezen; ik merkte slechts op dat ook hier naamgeving en toeschrijven niet met de werkelijkheid overeenkomen.

TC May 8: This is not a matter of "reading badly". It concerns (iii) above.

(4c) Just to be sure: My own first contact with Hart - in 2012 - 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 - but he knows about the latter and does not refer to that refutation.

(4c) Uw 'weerlegging' van het intervalbewijs verdient die naam niet.

TC May 8: Good to hear. I informed Hart about this refutation in 2012. Let me invite him again to show where the refutation is wrong. See the Appendix in this paper:

http://thomascool.eu/Papers/ALOE/2012-03-26-CCPO-PCWA.pdf

(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.

(4d) De reden dat van mij het decimalenbewijs naar de prullemand mag is niet dat het 'hopeloze artikelen' aantrekt maar de kunstmatige manier waarop de diagonaal moet worden veranderd om een reëel getal te maken dat niet in de rij voorkomt — ik vind die bewijzen gewoon niet mooi.

TC May 8:

(a) But Hart p43 column 2 states, and clearly has this suggestion:

Het is dit argument waar veel mensen een probleem mee hebben. De logicus Wilfred Hodges schreef een lezenswaardig artikel, [10], over de vele artikelen die hij als referee of redacteur onder ogen kreeg en waarin de auteurs hun bezwaren tegen, en weerlegging van, het diagonaalargument hadden neergeschreven.

(b) We established in point (3) that the proof with decimals is logically equivalent to Cantor's diagonal proof of 1890/91. The decimals are only more didactically useful (for students). The method of proof, changing an element, is just as artificial in boths versions. Only in the binary format, the scope for change is limited of course.

(c) Beauty is in the eye of the beholder. We must allow KPH as author his personal preferences. My criticism is directed at the mathematical falsehood that this proof can be ditched without ditching Cantor's original proof too.

(d) When the proof by means of the decimals is refuted, then KPH cannot say that one should look at what Cantor originally did, since because of the equivalence he can determine himself that also the original proof is refuted.

(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.

(4d) De reden dat van mij het decimalenbewijs naar de prullemand mag is niet dat het 'hopeloze artikelen' aantrekt maar de kunstmatige manier waarop de diagonaal moet worden veranderd om een reëel getal te maken dat niet in de rij voorkomt — ik vind die bewijzen gewoon niet mooi.

TC May 8: KPH does not answer to (4d1) and (4d2).

See this paper for a refutation of the decimal format:

http://thomascool.eu/Papers/ALOE/2012-03-26-CCPO-PCWA.pdf

(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 slaughter binge of such critics. We thus may understand why Hart (2015) is a biased presentation, unworthy of mathematics that wants to claim to be scientific.

(4e) De argumenten 'ad hominem' op het net gaan over en weer zowel voor- als tegenstanders van Cantor maken zich er aan schuldig.

TC May 8:

(a) This is not mentioned in the article. The comment about ad hominem fallacies follows directly after reference to Hodges about the "hopeless papers". The readership are mathematicians and they will presume that mathematicians will not use such fallacies.

(b) At issue is not quite the distinction between proponents and opponents. The relevant distinction is about "having an argument that can be checked mathematically" and otherwise ("hopeless", "ad hominem"). Hart reduces the discussion to proponents and opponents, as he states indeed that mathematical arguments are quickly drowned. In combination with (a) this suggests a misrepresentation: Mathematicians of course accept Cantor's Theorem, with proper math, and people who have questions apparently are without proper arguments.

Ad 4 overall: Hart does not acknowledge that his presentation indeed can be criticised for presenting this image.

PKH May 18:

(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.

TC May 19:

(a) Yes, agreed that it is a matter of aesthetics, provided that didactics is part of that (see above).

(b) But my problem with Hart's presentation was that he claimed that the decimal form created most protests against the proof, and subsequently his implied suggestion that people who protest against the proof would lack in (i) historical understanding, (ii) mathematical understanding, (iii) aesthetics, with didactics, (iv) and in the later paragraphs also the link to ad hominem arguments.

(c) PKH on May 18 merely restated what already had been acknowledged. I stated that he as an author definitely had the freedom to express his own preferences for what he considers a neat proof. However, he does not reply to the criticism that this freedom is abused to misrepresent the critique on the proof.

(d) It is simply not true that the decimal form can be thrown in the trash can, since it is a didactically superior form.

(e) If you agree that the binary and decimal form are equivalent, then you should also acknowledge that it implicitly uses irrationals - see point (3).

(f) There are other points than only (4d).

(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 Gödels two theorems.

> (5) Het ene bewijs maakt wat sterker gebruik van diagonaliseren dan het andere; de stellingen van Turing and Tarski wellicht wat meer dan de stellingen van Gödel. Het bewijs van Gödel's stelling dat ik ken maakt een diagonaal en maakt uiteindelijk van één omgeklapt element gebruik om een onbeslisbare formule te produceren.

TC May 8:

(a) Hart p 42 column 3:

> De bekendste onmogelijkheidsbewijzen uit de mathematische logica maken alle gebruik van een versie van Cantors idee om op een diagonaal alle elementen om te klappen. De eerste onvolledigheidsstelling van Gödel, Turings oplossing van het beslissingsprobleem en Tarski's stelling ([14]) over de ondefinieerbaarheid van waarheid in ℕ. Telkens wordt er een welgedefinieerde lijst gemaakt met een welgedefinieerde diagonaal en een geschikte omklapprocedure leidt tot de gewenste onmogelijkheid.

(b) Hart does not refute my criticism. I agreed that Gödel used arithmetic, and a numerical coding of expressions to formalise self-reference in a way that his community would find acceptable. The point remains that logically he used self-reference, known since antiquity, and that Cantor's diagonal argument is not relevant here to arrive at the proof. (Though it is refuted in ALOE.)

I maintain my criticism that Hart's presentation suggests that Gödel would necessarily rely on a diagonal form that was derived from Cantor, and that he makes this method larger than it actually is.

No comment of KPH May 18

(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.

PKH May 18: Responses to refutations.

TC May 20:

Hart defends that Hart (2015) was silent on the criticism since he rejects it. It would have been better to give an indication of the argument and the reason for rejection, so that readers could have checked whether they agreed. (It would also have been better to respond earlier in the 2011-2015 period so that misunderstandings could have been resolved.)

I maintain my criticism that Hart (2015) misinforms his readership.

This discussion itself concerns different topics, and thus the reader is referred to Colignatus (2015c).

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