Mathieu Marion* & Mitsuhiro Okada**

Wittgenstein on Contradiction and Consistency: An Overview

Resumo

Neste artigo, nós fornecemos uma visão geral das observações de Wittgenstein sobre contradições e provas de consistência, prestando atenção ao seu contexto de origem no período intermediário, que estão organizadas em torno de três temas principais: a distinção entre provas de consistência relativas e absolutas; a ideia de contradições 'escondidas'; e a ideia de que uma contradição não gera prejuízos, porque, uma vez derivada, as regras podem ser fixadas e os resultados anteriores podem ser mantidos. Argumentamos que Wittgenstein primeiro distingue entre o cálculo da teoria dos números e o cálculo lógico, que não se aplica a ela. Em seguida, argumentamos que as observações de Wittgenstein equivalem a uma tentativa continuada de desenvolver uma alternativa à visão 'realista' das contradições, de acordo com a qual se um conjunto de axiomas é inconsistente, mas nenhuma contradição foi ainda encontrada, isso significa que uma está à espreita no conjunto de consequências ainda não derivadas, e essa contradição vicia o cálculo como uma doença silenciosa. Também fornecemos réplicas a A. M. Turing e Charles Chihara, que fizeram críticas normalmente consideradas definitivas.

Palavras-chave: Wittgenstein . matemática . lógica . contradição . consistência.

* Université du Québec à Montréal

** Keio University

Abstract

In this paper we provide, paying attention to their context of origin in the Middle Period, an overview of Wittgenstein's remarks on contradiction and consistency proofs, which is organized around three major themes: the distinction between 'absolute' and 'relative' consistency proofs, the idea of 'hidden' contradictions, and the idea that a contradiction does no harm, because once derived the rules can be fixed and previous results can be maintained. We argue that Wittgenstein first distinguished between the calculus of number-theoretic equations and the logical calculus, which does not apply to it. We argue that Wittgenstein's remarks amount to a sustained attempt at developing an alternative to a 'realist' view of contradictions, according to which if a set of axioms is inconsistent, but no contradiction is yet found, it means that one is lurking in the set of yet underived consequences, and this contradiction vitiates the calculus like some silent disease. We also provide rejoinders to A. M. Turing and Charles Chihara, who provide criticisms usually considered definitive.

Keywords: Wittgenstein, mathematics, logic, contradiction, consistency.

1. Organizing Wittgenstein's Thoughts on Contradiction and Consistency[1]

The ideas of a contradiction suddenly occurring within current mathematics and of a proof of consistency that would ascertain that this could never be the case were central to Wittgenstein's thinking about mathematics and the role of mathematical logic, from his discussions in Vienna with Moritz Schlick and Friedrich Waismann in 1930 to his Cambridge lectures on the foundations of mathemat-

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1 In this paper, we will adopt the following usual abbreviations for citing Wittgenstein's works:
BT *The Big Typescript. TS 213,* Oxford, Blackwell, 2005.
LFM *Wittgenstein's Lectures on the Foundations of Mathematics Cambridge 1939*, Ithaca NY, Cornell University Press, 1976.
PG *Philosophical Grammar*, Oxford, Blackwell, 1974.
PR *Philosophical Remarks*, Oxford, Blackwell, 1975.
RFM *Remarks on the Foundations of Mathematics*, revised edition, Oxford, Blackwell, 1978.
RPP1 *Remarks on the Philosophy of Psychology*, Oxford, Blackwell, 1980.
TLP *Tractatus Logico-Philosophicus*, with an introduction by Bertrand Russell, London, translated by D. F. Pears & B. F. McGuinness, Routledge & Kegan Paul, 1961.
WVC *Ludwig Wittgenstein and the Vienna Circle*, B. F. McGuinness & J. Schulte (eds.), Oxford, Blackwell, 1979.

ics in 1939, and beyond. [2] Wittgenstein's remarks are, to say the least, controversial, aiming as they were at what he called with clear disdain:

> [...] the *bugbear* of contradiction, the superstitious fear that takes the discovery of a contradiction to mean the destruction of the calculus. (WVC, 196)

> The superstitious dread and veneration by mathematicians in face of contradiction. (RFM, I, App. III, § 17)

They have been dismissed by more than one antipathetic philosopher such as Charles Chihara, who called them "absurd" (Chihara 1977, 369). Critical attention has been focussed, however, on later remarks found in the *Lectures on the Foundations of Mathematics* – especially because of the exchanges with A. M. Turing on that topic – and *Remarks on the Foundations of Mathematics*, at the expense of earlier ones from the Middle Period, ranging from extensive discussions with Schlick and Waismann[3] to the *Big Typescript*. This bias might be explained by the contingencies of the publication of Wittgenstein's *Nachlass*, e.g., by the fact that the *Remarks on the Foundations of Mathematics* were published first and were for a long time the only text available, or simply by a deeply entrenched prejudice in favour of a so-called 'later philosophy of mathematics'.[4] It is true that Wittgenstein's position shifted through the years, as one can see from these two quotations, separated by a decade:

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2 Readers wishing to find out more about Wittgenstein on contradiction may wish to consult Arrington's original study (Arrington 1969), chapter XVI of (Wright 1980), as well as (Marconi 1984), (Wrigley 1986), chapter 6 of (Shanker 1987), and (Goldstein 1986, 1989). Although it would be tedious to show this, we have tried to provide here a new take on this topic, diverging from these studies. For example, Wright's and Wrigley's studies are premised on an interpretation of Wittgenstein as a 'radical conventionalist', which we think is simply not borne by the text; Goldstein's discussion relates Wittgenstein's remarks to larger issues concerning paradoxes in (Goldstein 1983, 1988 & 1999, 147-160), while we avoid this, etc.

3 Before Waismann's notes were published in their entirety as Wittgenstein and the Vienna Circle (WVC), the discussions on consistency were excerpted and published as an appendix to Philosophical Remarks (PR, 318-346).

4 We tend to agree with (Potter 2011, 135-136) that Wittgenstein never reached a clear formulation of what could be properly called a 'later philosophy of mathematics'.

> […] there can be no such thing as a proof of consistency (if you imagine the contradictions of mathematics as being of the same kind as the contradictions of set theory), that this proof cannot accomplish what it is expected to do. (WVC, 122)

> My aim is alter the *attitude* to contradiction and to consistency proofs. (*Not* to show that this proof shows something unimportant. How *could* that be so?) (RFM, III, 82)

It remains, however, that the later remarks contain a good deal of repetition from earlier claims and hardly any new arguments, while some earlier lines of argument are indeed abandoned, so that no interpretation of these later remarks that does not take into account their genesis can really claim to be wholly right. Since one finds the origin of almost all of Wittgenstein's claims concerning contradiction and consistency, and most of his supportive arguments, in the Middle Period – in particular all the central claims are expressed before 1931 in the conversations with Schlick and Waismann –, it is important to provide an overview of that material in its context if one hopes to obtain a better, if not definitive, interpretation. In this paper, we would like to do just this, a detailed, albeit non-exhaustive,[5] review of Wittgenstein's remarks on contradiction and consistency, with attention to their context of origin. It is of course not possible to provide a detailed evaluation of Wittgenstein's numerous claims and arguments, so we will generally refrain from a definitive assessment; we will merely provide as we go along a partial defence of some specific claims by providing rejoinders to well-known objections voiced by Turing and Chihara, that are often considered as decisive.[6]

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5 As usual, Wittgenstein explores a number of paths, it is not possible to discuss them all, and we selected the claims and arguments that are according to us the more central and important one. We leave aside, e.g., his idea at (WVC, 131) that one "could study logic just as well by means of contradictions" (as opposed to tautologies) – except that one would not know how to apply the calculus – or his claim at (WVC, 139f. & 164) that a calculus containing a contradiction is nevertheless of interest.

6 These criticisms were voiced by Turing during Wittgenstein's 1939 lectures on the foundations of mathematics, as recorded in (LFM), and by Chihara, who, prompted by the publication of these lectures, argued in defence of Turing's objections in (Chihara 1977). For earlier objections, based solely on the text of the *Remarks on the Foundations of Mathematics*, see (Bernays 1967, 521), (Kreisel 1958, 155) or (Anderson 1958, 451). A later paper by Chihara, (Chihara 1982), is a rejoinder to (Wright 1980, chap. XVI) and (Wrigley 1980) (later revised as (Wrigley 1986)). This exchange is of lesser interest in the context of this paper, since part of the debate is premised on the Wright--Wrigley interpretation of Wittgenstein, which we do not share.

The point of this paper is thus primarily to arrange Wittgenstein's claims and supportive arguments in a coherent whole around some key issues. These can be introduced with the following (informal) considerations. A proof of consistency for a given system S is going to be either 'absolute' or 'relative': it can be either obtained directly, through examination of its axioms and/or rules S, in which case it is 'internal' or 'absolute'; or it can be achieved indirectly by showing that each proof in S:

$$\vdash_{S} A$$

can be transformed into a proof in another system S':

$$\vdash_{S'} A,$$

where S' is considered to be 'safer' than S, according to some informal argument to the effect that it is less likely than S to generate a contradiction.[7] In this case, the proof would be 'relative' or 'external'. In this terminology, all 'metamathematical' proofs of consistency, in the tradition of Hilbert's programme, are 'relative' or 'external'. One can argue that Wittgenstein left the door open to 'absolute' consistency proofs, but he could not make sense of 'relative' proofs. His arguments concerning this issue will be presented in section 2.

According to a philosophical viewpoint, which we might call 'realist', once a system with a set of axioms Σ is set up, all their consequences already follow. If the set Σ is contradictory but no contradiction has been so far derived, this merely means that a contradiction occurs undetected in the set of consequences. Nevertheless, this 'hidden' contradiction is not considered to be harmless: it is then said to vitiate the system, because once out in the open it would 'trivialize' the system within a few steps, according to a well-known argument (here in sequent calculus form) involving the rule of disjunctive syllogism:

7 Since Gödel's incompleteness theorems, the idea that a relative proof of consistency of Peano Arithmetic could calm fears of contradiction has become largely otiose, since any proof of consistency (e.g., Ackermann's, Gentzen's, Schütte's, etc.) would be within a system which would be considered less 'safe' than Peano Arithmetic. Strangely, this argument seems never to have occurred to Wittgenstein.

$$\dfrac{\Sigma \vdash \neg A \qquad \dfrac{\Sigma \vdash A}{\Sigma \vdash A \vee B}}{\Sigma \vdash B}$$

An analogous argument was served by Waismann to Wittgenstein as early as December 1930,[8] who mentions the argument again a year later;[9] so he was clearly aware of it. Under this view, for any Σ, we have only at any given point in time derived a limited number of consequences and, although we have not bumped into any contradiction yet, one might still lurk out there, in the shadowy zone of the consequences that are in some sense already there, but not yet inferred. (In the above deduction, assumptions $\Sigma \vdash A$ and $\Sigma \vdash \neg A$ are as yet not inferred.) So, *unbeknownst to us*, we could have in fact derived any *B* from Σ. One source for this idea, mentioned by Wittgenstein,[10] was the development of non-Euclidean geometries in the 19th century, which were at first thought to be contradictory, although no contradiction was forthcoming until it was shown that they were consistent (by relative proofs of consistency).

Those adhering to this view feel the need for a proof of consistency for Σ: only with such a proof would one know that there is no contradiction waiting for us out there, so to speak, and that we are thus safe using Σ. As opposed to this, under a view of the matter that might therefore be called 'anti-realist', no consequence follows from Σ until *we* actually infer it, therefore, there can be no 'hidden' contradiction waiting for us to infer it, say, by accident tomorrow, as we keep inferring more and more of these consequences that are supposed already to be there. There is no doubt that Wittgenstein adopted a similar stance, as early as 1930, in conversations with Schlick and Waismann. The following could serve as an illustration:

> There can be no such question as whether you will some day reach a contradiction by progressing in accordance with the rules.
> I believe this is the essential thing on which everything to do with the question of consistency depends. (WVC, 127-128)

8 See (WVC, 131-132).

9 See (WVC, 197), quoted at the beginning of section 4, below.

10 (WVC, 120).

Wittgenstein also often maintained during these conversations that one could not talk of a contradiction without being given a procedure for searching for one – an obviously 'anti-realist' criterion:

> Can you look for a contradiction? Only if there is a method of looking for it. There can be no such question as whether you will some day reach a contradiction by progressing in accordance with the rules. (WVC, 127)[11]

We will discuss Wittgenstein's remarks on the idea of a 'hidden' contradiction in section 3. It is not our intention to adjudicate here the debate between these opposite views. We wish simply to contribute to the understanding of Wittgenstein's attempt at elaborating this 'anti-realist' view of contradiction.[12]

Finally, one may ask what to do if one obtains both $\Sigma \vdash A$ and $\Sigma \vdash \neg A$. The issue has two dimensions, depending on how we look at it: one may ask what would happen after the contradiction has been found, or one may ask what does the occurrence of that contradiction imply for the conclusions *already* inferred? Wittgenstein's answer to the first question was simply to call for a revision of the rules, e.g., in order to avoid inferring anything from the contradiction. (This answer was famously challenged by Charles Chihara.) One natural inclination concerning the second question would be, under the above 'realist' view, that if a contradiction is to cause damage, then it has already caused damage. The system is vitiated. An illustration of this was provided by Turing when he attended Wittgenstein's lectures on the foundations of mathematics in 1939:[13] if a bridge was constructed with help of a system that turns out later to have been an inconsistent one, then it would likely fall. In line with his answer to the first question, Wittgenstein would argue that all previous derivations are still fine, so can continue using them. To illustrate the point in simple striking terms, one may cite Georg Kreisel:

> Cantor's proofs still stand although they can be embedded in Frege's inconsistent system. (Kreisel 1958, 156)

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10 See also (WVC, 141, 143, 195 & 208).

12 This was the view expressed, alas not in print, by the late Torkel Franzen. We should merely add here a disclaimer: no relation with Sir Michael Dummett's anti-realist programme in the theory of meaning need be implied by the use of the expression 'anti-realism' in this paper.

13 See the discussion at (LFM, 212f.).

This point will be discussed in section 3, along with rejoinders to both Chihara and Turing in section 4.

2. On Absolute *vs*. Relative Consistency Proofs

Wittgenstein's earliest remarks on contradiction and consistency were most probably spurred by his reading Hilbert.[14] (After all, although not altogether entirely absent,[15] such considerations were not central, to say the least, to the logicist tradition in which he was trained.) We need merely to recall here Hilbert's proof of consistency for the axioms of geometry relative to analysis[16] and his realization that an absolute proof of consistency of the latter would be eventually needed, which led to the development of his metamathematical programme. As he put it in 'The New Grounding of Mathematics. First Report':

> […] a satisfactory conclusion to the research into [foundations of mathematics] can only be attained by the solution of the problem of the consistency of the axioms of analysis. If we can produce this proof, then we can say that mathematical statements are in fact incontestable and ultimate truths – a piece of knowledge that (also because of its general philosophical character) is of the greatest significance for us. (Hilbert 1998, 202)

Wittgenstein's annoyance with the mathematician's 'fear of contradiction' may simply be a reaction to Hilbert's notorious sales pitch for consistency proofs in papers that Wittgenstein read, of which this is a good example. [17]

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14 See (WVC, 119 & 137). The conversations with Schlick and Waismann imply that he read, 'Neubegründung der Mathematik' (1922), but there are reasons to believe that he also read 'Über das Unendliche' (1925). See, respectively, (Hilbert 1998) and (Hilbert 1967).

15 One will see below that one immediate source of Wittgenstein's thoughts on consistency is Frege's discussion of the formalists (Thomae and Heine) in volume II *Grundgesetze* der Arithmetik from § 106 onwards – see especially §§ 117-119 (Frege 1980). There are other relevant passages in Frege's *Grundgesetze* that are not mentioned by Wittgenstein, e.g., his argument at §§ 30-1, of a semantic nature, designed to form a sort of consistency proof for his system. See (Heck 2010, 371-376).

16 See (Hilbert 1971, § 9). Wittgenstein refers to this result in (WVC, 147).

17 See footnote 14 above about the papers Wittgenstein most probably read.

As Kreisel often argued, the interest of relative proofs of consistency is not limited to calming a fear of contradiction.[18] But this side of the issue seems to have been missed by Wittgenstein, who never addressed it. At all events, over and above this negative reaction, what was Wittgenstein's stance? What were his arguments? At that early stage of the Middle Period, Wittgenstein was surely thinking mostly in terms derived from his *Tractatus Logico-Philosophicus*; that he could not countenance in that book the idea of a *meta*language is rather uncontroversial. It is no surprise, therefore, that he immediately took exception to Hilbert's idea of a *meta*mathematical proof of consistency; his earliest remarks during the Middle Period are very much interlocked around this rejection. Two lines of argument, one clearly with roots in the *Tractatus*, are worth commenting on. The first line will be discussed presently, the second one in the next section.

One should begin by recalling that in the *Tractatus* Wittgenstein only discussed number-theoretic equations and that he divorced them radically from logic: these equations (of which he gave an account in terms of rewriting rules) are said to be mere 'pseudo-propositions', with the consequence that the laws of logic, which apply to propositions, do not apply to them. The underlying view, one may argue, is that these equations are the mere registers of (elementary) calculations and that there is nothing else to number theory than these. One would naturally object here that this is an unsatisfactorily narrow view, as Wittgenstein does not even raise his discussion to the level of proofs, and of proofs by mathematical induction in particular. The point here is not, however, to start criticizing Wittgenstein, who tackled these issues in the early stages of the Middle Period, but to understand the basis of his arguments. To put his views in broad terms, Wittgenstein was inclined in the Middle Period to think of calculi, like games, as independent of each other, and certainly not as one serving as another's 'foundation' in, say, some sort of inverted pyramid structure, but he would acknowledge that there are fruitful points of contact, e.g., proofs by induction (on the model of Skolem's)[19] providing a contact between the calculus of algebra and the calculus of num-

18 See, e.g., his review of *Remarks on the Foundations of Mathematics*, where, after recognizing that Wittgenstein is right in many of his criticisms of Hilbert, Kreisel writes: "When all this is recognized, the mathematical problem of consistency still stand, and is fruitful: proofs of consistency and, more generally, of independence yield, perhaps, a better control over a calculus than anything else" (Kreisel 1958, 156).

19 See (Skolem 1967).

ber-theoretic equations, such proofs allowing for new calculations;[20] or even between the addition of natural numbers and the union of disjoint classes in logic.[21] With these points in mind, some of Wittgenstein's arguments become transparent. For example, it becomes clear that under such a conception a contradiction cannot take place within the number-theoretic calculations that are registered by the equations – one would only have an error in calculation –, so a contradiction would only appear within the logical system in which the calculations are wrapped, so to speak. That this was Wittgenstein's view is clear from his own words:

> The idea of a contradiction – and this is something I hold fast to – is that of a logical contradiction, and this can occur only in the *true-false game*, that is, only where we make statements. (WVC, 124)

This 'true-false game' is but the very truth-functional calculus from which he had divorced number-theoretic equations in the *Tractatus*.

Wittgenstein notoriously based a number of his arguments in the philosophy of mathematics on a distinction between 'calculus' and 'prose'.[22] In terms of this distinction, a contradiction may occur outside the calculus of number-theoretic equations in what he called the 'prose', but this will not affect calculations *per se*:[23]

> The truth of the matter is this – our calculus *qua* calculus is all right. It does not make sense to speak of contradictions. What is called a contradiction springs into existence as soon as you step outside the

20 On this point, see (Marion & Okada, unpublished manuscript).

21 See, e.g., (LFM, 265), discussed in (Marion 2011).

22 See, e.g., (WVC, 149). Stuart Shanker is probably the first to have attracted attention to the centrality of this distinction in (Shanker 1987), but his understanding of what it amounts to differs from ours, as can be seen from comparing (Shanker 1987, 199-215) to our (Marion & Okada, unpublished manuscript). From our point of view, Shanker simply misunderstood the nature of Wittgenstein's remarks on Skolem's proofs by induction.

23 Given that, following this view of the matter, in what might be called here the 'number--theoretic game' there can be only errors in calculation and no contradiction, Wittgenstein was not worried about the effect of a logical contradiction – as opposed to an error in calculation – on, say, the possibility of a bridge collapsing, a possibility raised by and discussed with Turing in the Cambridge lectures in 1939, at (LFM, 212*f*.). This is one of the many aspects of Wittgenstein's position which needs the sort of detailed assessment that cannot be undertaken here, over and above the brief comments below.

> calculus and say in everyday prose, *Therefore*, this property is true of all numbers, but the number 17 does not have this property.
> In the calculus a contradiction cannot manifest itself at all. (WVC, 120)[24]

This explains Wittgenstein's strongly worded rejection of proofs of consistency in the first quotation of this paper. One should note here that Wittgenstein's argument relies on his own views about inductive proofs:

> The demonstration of consistency in fact proceeds inductively in Hilbert's simple model: the proof shows us, by means of an induction, the possibility that → signs must go on occurring for ever.
> The proof lets us see something. What it shows, however, cannot be expressed by means of a proposition. Thus it is also impossible to say 'The axioms are consistent'.' (Any more than you can say, 'There are infinitely many numbers,' That is everyday prose.) (WVC, 137)

The underlying idea here is that Wittgenstein had been arguing elsewhere in his manuscripts that what a proof of induction in number theory *shows* cannot be *said* using an universally quantified statement, i.e., what the proof shows does not warrant a claim of the sort 'Therefore, for all x …'. This *prima facie* odd claim is quite important to understand since reflection on it led Wittgenstein to what is perhaps his greatest contribution to mathematical logic, the 'rule of uniqueness of a function defined by recursion' – his student Louis Goodstein proved in *Recursive Number Theory* an important theorem according to which in primitive recursive arithmetic the uniqueness rule implies mathematical induction.[25] The above comment on Hilbert's proof of consistency is just an application of his thinking on these issues.

One should have noticed the role of the saying-showing distinction in the above argument. We find it again in this passage, where the notion of relative consistency proof is rejected:

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24 See also (WVC, 142).

25 See (Goodstein 1957, Theorem 2.8 & 3.7-3.81). For a detailed discussion, see (Marion & Okada, unpublished manuscript).

> Consistency 'relative to Euclidean geometry' is complete nonsense. What is going on here is the following. One rule corresponds to another rule (one configuration of a game to another configuration of the game). Here we have a mapping. That's all! Whatever else is said is everyday prose. People say, '*Therefore* the system is consistent.' But there is no 'therefore' here, any more than in the case of induction. […]
>
> *The rules (configurations) of the one group stand in internal relation to one another similar to those of the rules (configurations) of the other group. This is what is shown by the proof and nothing else.* (WVC, 145)[26]

A new idea is introduced here, that has to do with the rejection of the notion of metalanguage. Wittgenstein sees setting up a calculus in a metalanguage in order to study a calculus in an object language as merely setting up a calculus whose rules are about the rules of another calculus: he would not grant that a calculus could ever be a 'theory' in the sense that it would 'describe' anything, let alone describing another calculus. As he put it to Waismann, who had raised the possibility of theory of the game of arithmetic analogous to a theory of chess:

> What is called the 'theory of chess' is not a theory describing anything, but rather a kind of geometry. Of course, it is again a calculus and not a theory. (WVC, 133)

Thus the relation between the above calculi could not be that of 'meta-calculus' to 'object-calculus'; we have instead at best what would be one of the fruitful connections mentioned above:

> I can play with chessmen according to certain rules. But I could also invent a game in which I play with the rules themselves: Now the rules of chess are the pieces of my game and the laws of logic for instance are the rules of the game. In this case I have yet another game and not a metagame.

26 "But there is no 'therefore' here, any more than in the case of induction". For this latter case, see (Marion & Okada, unpublished manuscript).

> What Hilbert does is mathematics and not metamathematics. It is another calculus, just like any other one. (WVC, 120-121)[27]

It can be argued that this just a misunderstanding of Hilbert's metamathematics. We do not wish, however, to comment on this;[28] we would like simply to point out that this line of argument was not pursued by Wittgenstein after the Middle Period. The reasons for this are, however, not immediately clear, and this is not the place to speculate.

We should conclude this section by pointing out, on a more positive note, that Wittgenstein left the door open to absolute proofs of consistency. The possibility of an internal proof (actually of a proof of independence) was explicitly envisaged by Waismann in conversations with Wittgenstein:

> WAISMANN ASKS: Does it not make sense to ask oneself questions regarding an axiom system? Consider, for instance, the propositional calculus which Russell derives from five axioms. Bernays has shown one of these axioms is redundant, and that four are sufficient. He has further shown that these axioms form a 'complete system', i.e. that the addition of another axiom which cannot be derived from these four makes it possible to derive any proposition you write down whatever. For this amounts to saying that every proposition follows from a contradiction. Is this then not a material insight into the Russellian calculus? Or, let me take another case, I choose three axioms. I cannot derive the same class of propositions from them as I can from all five together. Is it thus not possible to regard the demonstration of consistency too as a material insight? (WVC, 128)

27 A similar point is made concerning the connection between the language of number-theoretic equations and that of algebra which he calls here "the system of calculating with letters": "The system of calculating with letters is a new calculus; but it does not relate to ordinary calculus with numbers as a metacalculus does to a calculus. *Calculation with letters* is not a theory. This is the essential point. In so far as the 'theory' of chess studies the impossibility of certain positions it resembles algebra in its relation to calculation with numbers. Similarly, Hilbert's 'metamathematics' must turn out to be mathematics in disguise" (WVC, 136). See also (WVC, 175).

28 One reason for not dealing with this issue is that this was the very topic of Luiz Carlos Pereira's paper at the international colloquium Middle Wittgenstein III, held in Ilha Grande, March 4-11, 2012. We would like to defer our discussion until his paper is in print.

Wittgenstein's reply seems, however, to betray at first a misunderstanding of that idea:

> WITTGENSTEIN: […] I can in no way compare the classes of consequences unless I construct a *new system* in which both groups occur. […] I don't gain a material insight, either: what I am really doing is, again, constructing a new calculus. And in this calculus the proposition 'The one class is more comprehensive than the other one' doesn't occur at all. This is the everyday prose that accompanies the calculus. (WVC, 128-129)

But Wittgenstein does agree, later on, with the idea of proving independence by taking away one of the axioms and replacing it with its negation.[29] At all events, as one knows from Hilbert's programme, the chain of relative consistency proofs has to come to an end, with an absolute proof. But if an absolute proof of the consistency of the rules for a given calculus cannot, according to Wittgenstein, be given in yet another meta-calculus, then it has to come from the inspection of the rules of that calculus themselves (this being a 'method of discovering contradictions', as mentioned in section 1):

> To prove consistency can, I think, mean only one thing: to check through the rules. There is nothing else I can do. (WVC, 137)

> I can always decide […] if a contradiction is present by scrutinizing my list of rules. (WVC,195)

Coming from Wittgenstein, this claim may fail to convince. Worse, Wittgenstein failed to elaborate on this view in later remarks. Given the lack of space, we cannot argue here that one ought not to dismiss this claim too rapidly and we hope to be forgiven for the following appeal to authority: it has been asserted anew recently by Per Martin-Löf, when presenting an absolute proof of consistency for his intuitionistic type theory (Martin-Löf 1996, 53-54).

It is worth noting that this claim about the necessity of an absolute consistency proof squares very well with the 'anti-realist' stance on 'hidden' contradiction, which we are about to discuss. Waismann had another objection: If

29 (WVC, 147-148).

contradictions never occur in a calculus, is there room for proofs of *reduction ad absurdum*?[30] To which Wittgenstein replies:

> What I mean has absolutely nothing to do with indirect proof. There is a confusion here. Of course there are contradictions in the calculus. What I mean is only that it does not make sense to talk of *hidden contradictions*. (WVC, 174)

Wittgenstein's reasoning seems to be this: upon inspecting the axioms/rules of the system, I should see if a contradiction will result,[31] and if I can't find any, then there isn't any hidden one against which a proof of consistency would safeguard me.

3. On Hidden Contradictions

Wittgenstein also developed in his conversations with Schlick and Waismann another, perhaps more fruitful, line of argument, which does not rely on theses from the *Tractatus*, but derives instead from Frege's critique of formalism in his *Grundgesetze der Arithmetik*, vol. II, § 106 onwards.[32] In these sections, Frege argues *inter alia* that a contradiction can only occur in the 'rules of the game' and not in the 'basic configurations of the pieces of the game', and Wittgenstein picks up the idea.[33] Using the analogy between mathematics and the game of chess, Frege mocked Thomae in § 106 for thinking that the rules of chess would describe the starting positions of the pieces on the chessboard:

30 See (WVC, 173).

31 To quote Wittgenstein slightly out of context : "The point is seeing, not proving" (WVC, 146).

32 See (Frege 1980, 182*f.*).

33 See, e.g., (WVC, 124, 132 & 194).

> What would somebody say if, on asking for the rules of the game of chess, he received no answer – being shown instead a group of chessmen on the chessboard? He would probably say he could find no rules there, since he associates no sense with the chessmen and their positions. (Frege 1980, 183) (Quoted at (WVC, 124).)

Let us, for the sake of the argument, forget for a moment axiomatic presentations of logical calculi and think instead of their counterparts in natural deduction calculi, where there are no axioms, only rules. This move is warranted by the fact that, from 1930 onwards, Wittgenstein usually speaks of contradiction and consistency in terms of *rules*. The argument would then be that, the rules of a natural deduction calculus being akin to the rules of chess, a contradiction would have nothing to do with the configuration with which one starts the game (the position of the pieces on the chessboard), but rather with the possibility that applications of the rules would bring about a situation where one would not know anymore which rule to apply; a situation where one would get stuck, so to speak. Wittgenstein illustrates this twice with a cooked-up example:

> […] a contradiction can only occur among the *rules of a game*. I can, for example, have a rule of a game that says: A white piece has to move by jumping over a black one.
>
> If a black piece, then, is at the edge of the board, the rule fails. Thus it may be the case that I do not know what to do. The rule tells me nothing further. (WVC, 124)[34]

His comments, a few moments later in the discussion, are telling:

> After all the rules are instructions for playing the game, and as long as I can play, they must be all right. It is only when I *notice* that they contradict each other that they cease to be all right, and that manifests itself only in this: that I cannot apply them any more. For the logical

34 See (WVC, 194-195), where the same example is invoked.

> product of the two rules is a contradiction, and a contradiction no longer tells me what to do. (WCV, 125)[35]

There seem to be two points made here: first, that the problem is not with the position of the pieces as such but with the rules, because what makes a given configuration problematic is the fact that the rules that are applicable in that configuration don't help us to go on with the game, and, secondly, that a problematic situation of this kind only occurs *at a given stage*, or *in certain positions*. Now, the argument would be that such situations where we would not know what to do should emerge from inspection of the rules of the game: [36]

> In a system with a clearly set out grammar there are no hidden contradictions, because such a system must include the rule which makes the contradiction is [*sic*] discernible. (PG, 303)[37]

To use a simple example due to Diego Marconi, if we define a predicate *P* for natural numbers with these two clauses:

(a) n is *P* if and only if $n > 15$
(b) n is *P* if and only if $n > 14$

35 One could also quote here to § 1096 of the *Remarks on the Philosophy of Psychology*, volume 1: "the interesting games would be such as brought one via certain rules to nonsensical instructions. […] One has received the order "Go on the same way" when this makes no sense, say because one has got into a circle. For any order makes sense only in certain positions" (RPP1, § 1096). Juliet Floyd provided in a recent paper (Floyd 2012) an enlightening commentary of a short set of remarks on Turing Machines from 1947-48, namely §§ 1096-1097 of volume 1 the *Remarks on the Philosophy of Psychology* (RPP1). Floyd succeeded in explaining the diagonal argument presented in § 1097, in relation to Turing's original argument in § 9 of 'On Computable Numbers with an Application to the Entscheidungsproblem' (Turing 1936-37), a very significant achievement. Her discussion emphasizes the consequences of her explanation on Wittgenstein's remarks on the diagonal method and Cantor's proofs that the power set of any countably infinite set is uncountably infinite and that set of real numbers of the interval [0, 1] is not enumerable, i.e., that real numbers cannot be paired with natural numbers. We believe, however, that the passage just quoted shows that the remarks at §§ 1096-1097 are *also* of interest to our understanding of Wittgenstein on contradiction and consistency proofs. This point cannot be substantiated here but will be in a forthcoming paper.

36 The analogy with chess is interesting in this respect, since there are much more possibilities than games ever played, and not only no one got stuck, no one would refrain from playing the game because of a fear of getting stuck.

37 See also a slight variant at (BT, 381).

Then it is obvious from an inspection of (a) and (b) that 15 is both *P* and not *P*.[38]

Still the possibility cannot be ruled out that a contradiction has been overlooked when one was setting up a system, in which case it would indeed surface later.[39] In the (recent) history of logic one find examples of this, i.e., of some of the greatest logicians, who did not see immediately that their systems were contradictory: Gottlob Frege, Alonzo Church, and Per Martin-Löf.[40]

The question now becomes: given that the contradiction will occur only at a certain stage, what to think of the previous stages? In his 1939 lectures, Wittgenstein gave a clear statement of his answer:

> […] suppose that there is a contradiction in the statutes of a particular country. There might be a statute that on feast days the vice-president had to sit next to the president, and another statute that he had to sit between two ladies. This contradiction may remain unnoticed for some time, if he is constantly ill on feast-days. But one day a feast comes and he is not ill. Then what do we do? I may say, "We must get rid of this contradiction." All right, but that does not vitiate what we did before? Not at all.
> Or suppose that we always acted according to the first rule: he is always put next to the president, and we never notice the other rule. That is all right; the contradiction does not do any harm. (LFM, 210)

When Wittgenstein says here that the rules of the game are 'all right' and do no harm *until* we reach a given configuration, he wishes clearly to insist on the fact that the previous moves were taken in accordance with the rules and the fact that we now reach a problematic configuration does not mean

38 (Marconi 1984, 346).

39 This possibility is explicitly envisaged by Wittgenstein, e.g. in such passages: "if I did not see [the contradiction], then it was my fault – perhaps I was too lazy to inspect all cases, or I forgot about one case" (WVC, 195); "You may instruct someone what to do in such-and-such a case; and these instructions later prove *nonsensical*" (RFM VII, § 29).

40 Frege's case is well known, as for the other two cases: when Church published his list of axioms for the lambda-calculus in (Church 1932), he soon discovered by himself that it was inconsistent and he rapidly published a revised version in (Church 1933). A contradiction, now known as 'Girard's paradox', was found in Martin-Löf's first system of type theory (Martin-Löf 1970) by Jean-Yves Girard (Girard 1972). One should note how quickly one found the contradiction and how quickly both system were fixed.

that they were somehow incorrect. The 1939 lectures are well known for his controversial claim that a *soi-disant* hidden contradiction is "as good as gold" until it is derived:

> Is [the contradiction] hidden as long as it hasn't been *noticed*? Then as long as it's hidden, I say that it is as good as gold. And when it comes out in the open it can do no harm. (LFM, 219)

Again, one must be wary of attributing such claims to a 'later philosophy of mathematics' on the basis of ignorance of the texts; this idea is not new in 1939, but at least as old as 1930:

> As long as I can play the game, I can play it, and everything is all right. (WVC, 120)

This view can be seen as a central plank of his 'anti-realist' stance. For this, we have to recall the 'realist' picture, presented in section 1. According to this last, if a set of axioms Σ is contradictory but not initially recognized as such, it means that there is a hidden contradiction, which lurks in the not yet derived consequences of Σ. It also means that this contradiction is doing damage like an undetected cancerous growth. The reason for this is that, according to this view, it does not matter *when* a contradiction occurs; it vitiates the system as a whole irrespective of this temporal dimension. This point is metaphorical, but will become clearer in our discussion of Chihara in the next section. For the moment let us note Wittgenstein's reaction:

> People have the notion that a contradiction that nobody has seen might be hidden in the axioms from the very beginning, like tuberculosis. You do not have the faintest idea, and then some day or other you are dead. Similarly people think that some day or other the hidden contradiction might break out and then disaster would be upon us. (WVC, 120)

> Something tells me that a contradiction in the axioms of a system can't really do any harm until it is revealed. We think of a hidden contradiction as like a illness which does harm even though (and perhaps precisely because) it doesn't show itself in an obvious way. But two rules in a game which in a particular instance contradict each other

> are perfectly in order until the case turns up, and it's only then that it becomes necessary to make a decision between them by a further rule. (PG, 303) (BT, 380)

So, Wittgenstein's idea seems to be this: if a contradiction appears at a certain stage, then it does not invalidate or taint prior derivations, or, to avoid the temporal metaphor, the contradiction is *local*. After all, one could fix the system and find the same valid derivations in the new one. It is worth pondering this last thought for a moment. This is the idea that we illustrated with a quotation from Kreisel at the end of section 1: perfectly valid proofs can occur in an inconsistent system and a contradiction does not invalidate these proofs, one can find them anew in a consistent system. Now, it is worth noting that this must have been Wittgenstein's thought all along, since he had at first radically divorced the calculus of number-theoretic equations from any given logical calculus: a contradiction in the latter would not invalidate any calculation in the former. The least one can say here is that there is a remarkable unity in Wittgenstein's thinking on these issues.

4. Rejoinders

What to do when a contradiction occurs? Wittgenstein would simply insist here that the contradiction can always be eliminated by tinkering with the actual rules, i.e., either remove one of the rules or introduce a new one (the first of the following quotation reprises and extends an earlier one):

> I can, for example, have a rule of a game that says: A white piece has to move by jumping over a black one.
> If a black piece, then, is at the edge of the board, the rule fails. Thus it may be the case that I do not know what to do. The rule tells me nothing further. What do I do in such a case? Nothing is easier than removing the contradiction – I must make a decision, i.e., *introduce a new rule*. (WVC, 124)

41 The point was already made in (WVC, 120).

> If a contradiction is found later on, that means that hitherto the rules have not been clear and unambiguous. So the contradiction doesn't matter, because we can now get rid of it by enunciating a rule. (PG, 303) (BT, 381)[41]

Fixing the rules is exactly what happened with the systems of Frege, Church, and Martin-Löf.[42]

Wittgenstein has, however, another more controversial suggestion. As we saw, he knew the argument, given in section 1 above, that from a contradiction 'A & $\neg A$' one can derive in a couple of steps any B.[43] In 1931, he replied:

> It will probably be said, No, such a calculus [i.e., a calculus with a contradiction, M.M. & M.O.] would be trivial. For from a contradiction every formula follows: you can write down any arbitrary formula, and with that the calculus loses all its interest. To that I would reply, In that case the calculus consists, does it not?, of two parts, of one part that goes as far as the discovery of the contradiction, and of a second part in which it is permitted to write down any formula. The first part is the interesting thing. It will be asked, Does the calculus come to an end? *When* does it come to an end? A very exciting question! (WVC, 197)

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42 Wittgenstein could have known about Church but did not; he knew well, of course, Frege's case and referred to it during his exchange with Turing in his Cambridge lectures in 1939: "[Frege] was led by the normal rules of logic: the rules of such words as "and", "not", "implies", and so on. He was led also by our normal use of words. As we never ask whether "Fox" is a fox or "predicate" is a predicate, the question didn't arise and he never got into trouble […] So it is not quite right to say "Frege might have proved anything else." And this is shown by the fact that Russell, almost immediately on finding the contradiction, found a remedy in the theory of types: "We would never say 'Fox' is a fox; so eliminate that"." (LFM, 229) There are certainly problems with Wittgenstein's characterization of the problem with Frege's system in terms of asking the question 'Is 'fox' a fox?', picked up by Charles Chihara (Chihara 1977, 373*f*.), but Chihara also argues that it is not true that the patch for Russell's paradox was found "almost immediately". Amazingly enough, he bases his argument upon the great lengths through which Russell went to find out and develop his rather unsuccessful ramified theory of types, passing over in silence Ernst Zermelo's conceptually simpler and much more fruitful solution, the *Aussonderungsaxiom*. See (Zermelo 1967, 202).

43 WVC, 132 & 197).

This hardly counts as an enlightening answer, although the idea of the two parts, the 'before' and 'after', is interesting in itself, if not necessarily the right one. But Wittgenstein also famously replied to this argument in his 1939 lectures that one just has to make it a rule not to derive anything from a contradiction:

> One may say: "From a contradiction everything would follow." The reply to that is: Well then, don't draw any conclusions from a contradiction; make that a rule. (LFM, 208)

> *Wittgenstein*: You might get *p*. ~*p* by means of Frege's system. If you can draw any conclusion you like from it, then that, as far as I can see, is all the trouble you can get into. And I would say, "Well then, just don't draw any conclusions from a contradiction." (LFM, 220)[44]

This is one of Wittgenstein's *leitmotifs* about contradiction that has attracted lots of critical attention. Turing is on the record having reacted to this last quotation by saying:

> *Turing*: But that would not be enough. For if one made that rule, one could get round it and get any conclusion which one liked without actually going through the contradiction. (LFM, 220)

In defence of Turing, Chihara offered a deceptively simple argument (Chihara 1977, 371-372), according to which if a set of axioms Σ is inconsistent because one has derived both A and $\neg A$, then one can still derive any proposition B from Σ, without inferring it from the contradiction 'A & $\neg A$', as in the above derivation. His idea is, presumably, that, the set of axioms Σ being itself inconsistent, there could be any number of ways for any B to be inferred from it. The argument he gives is as follows:

(1) A is derivable from Σ, i.e., $\Sigma \vdash A$

And:

44 See also (LFM, 227 & 230).

(2) $\Sigma \vdash \neg A$

Thus

(3) $\Sigma \vdash A \,\&\, \neg A$

But, the argument goes, one can obtain any B without deriving (3) first, because:

(4) $\Sigma \vdash \neg A \rightarrow B$, from (1)

We assume that Chihara, who is not entirely clear, assumes '$A \rightarrow (\neg A \rightarrow B)$', so that:

(5) $\Sigma \vdash B$, by *modus ponens* from (2) and (4).

Reframed in sequent calculus form to make these matters more transparent, Chihara's argument involves only an application of the cut rule, a.k.a., *modus ponens*:

$$\frac{\Sigma \vdash \neg A \qquad \Sigma, \neg A \vdash B}{\Sigma \vdash B}$$

And, according to Chihara:

> Turing's point was a simple one, intelligible, one would think, to anyone with an understanding of elementary logic; yet there are reasons for thinking that Wittgenstein failed to grasp it. (Chihara 1977, 372)

But Chihara's argument is incomplete because he does not say how he obtained the key premise (4), i.e., '$\Sigma, \neg A \vdash B$', in the first place. It can be obtained from another cut:

$$\frac{\Sigma \vdash A \qquad A, \neg A \vdash B}{\Sigma, \neg A \vdash B}$$

So that we have rather:

$$\dfrac{\Sigma \vdash \neg A \qquad \dfrac{\Sigma \vdash A \qquad A, \neg A \vdash B}{\Sigma, \neg A \vdash B}}{\Sigma \vdash B}$$

If so, then Chihara has no argument, since the derivation was not possible without 'going through' the conjunction '$A, \neg A$'. One should note further that '$A, \neg A \vdash B$' might be obtained in two steps, with the rule of 'weakening on the right':

$$\dfrac{\dfrac{A \vdash A}{A, \neg A \vdash}}{\mathrm{A}, \neg A \vdash B}$$

From this, one can get the theorem $\mathrm{A} \rightarrow (\neg A \rightarrow B)$ with two right introduction of '$\rightarrow$', so that the full derivation would look like this:

$$\dfrac{\dfrac{\dfrac{\dfrac{A \vdash A}{A, \neg A \vdash}}{A, \neg A \vdash \mathrm{B}}}{A \vdash \neg A \rightarrow B}}{\vdash A \rightarrow (\neg A \rightarrow B)}$$

From this theorem, Chihara can also obtain any B, if both $\Sigma \vdash A$ and $\Sigma \vdash \neg \mathrm{A}$, by two applications of the cut rule.[45] Again, he has no argument because one had to go through '$A, \neg A \vdash B$'.[46] To the rule of 'weakening on the right' corresponds the '*ex falso quodlibet*' rule in natural deduction systems:

$$\frac{\bot}{\mathrm{A}}$$

45 These application of the cut rule would thus be equivalent to the two *modus ponens* that Chihara appear to rely on at (Chihara 1977, 372).

46 There is a rejoinder to Chihara to the same effect in (Shanker 1987, 241-243).

There are many *bona fide* systems of logic (minimal, paraconsistent, relevant, affine, linear) for which there is, *inter alia*, no 'weakening on the right' or '*ex falso*'. Excluding the '*ex falso*' from one's system fleshes out quite neatly Wittgenstein's idea that one makes it a rule 'not to derive anything from a contradiction'[47] We grant that matters are certainly more complicated than this,[48] but our point is merely that, at this *elementary* level, Wittgenstein's position is perfectly defensible and in no way "absurd". On the other hand, Chihara implies that it is in the nature of an inconsistent system that any formula *B* can be derived in it, so that eliminating '*ex falso*' would merely counter one manifestation of this defect but not eradicate it: we contend instead that it is this essentialist manner of speaking which is, in the end, *obscure*.

Problems with the case of 'a contradiction that might turn up later on' do not, however, stop here. The previous remarks and objections concerned what would happen *after* such a contradiction has been found, and one may now ask: What does the occurrence of that contradiction imply for what happened *before*? On the 'realist' view a 'hidden' contradiction yet to be discovered in a system S is already there, it is just that we are unaware of it: if it is to cause damage, it would also do so without us realizing that it does. So what about inferences already drawn before we discovered the contradiction? The assumption here on the 'realist' view would be that the system S is vitiated from the very beginning and that every deduction within it is somehow tainted. As we saw, Wittgenstein had the opposite attitude, because the contradiction is only appearing at a given stage (and can then be removed or neutralized). During Wittgenstein's 1939 lectures, Turing tried to formulate an argument in support of the 'realist' view, according to which bridges built with help of a system whose inconsistency we are not yet aware of would fall:

> The sort of case I have in mind was the case where you have a logical system, a system of calculations, which you use in order to built bridges. You give this system to your clerks and they build a bridge with it and the bridge falls down. You then find a contradiction in the system. (LFM, 212)

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47 This is the principal reason why Wittgenstein is often presented as a precursor of paraconsistent logics, e.g., in (Priest & Routley 1989, 35-44); we steer clear of this issue here. For further discussions, see (Marconi 1984) and (Goldstein 1989).

48 See, e.g., the discussion at (Marconi 1984, 338-339).

Discussion of the ensuing exchange with Wittgenstein on this particular case would leave us far afield, but one should minimally point out, with Michael Wrigley[49] that, even if it is probably true that bridges built in accordance with an inconsistent system would fall, and even if they would also fall in even greater numbers than those that actually fall that were built with our assumed to be consistent system, it would still remain that bridges we build with the use of our allegedly consistent system *do fall*. Therefore, barring that we are referring here to errors in purely numerical calculations or cases where some possibility, e.g., strong side winds, has been overlooked, it is thus not immediately clear why the fact that bridges built according to an inconsistent system would fall is to count as a defect for the inconsistent system but not for the allegedly consistent one. Let us suppose further that in order to build the bridge the only mathematics involved were elementary numerical calculations, but that the engineers were using a logical system in which these perfectly good calculations were wrapped that happened also to be inconsistent. If the bridge falls, how would the alleged inconsistency of the logical calculus really be at stake? It is true, however, that Wittgenstein does not quite manage, on the spur of the moment, to formulate this sort of reply to Turing. Nevertheless, the fact that it can be provided shows minimally that 'bridges that fall' cannot be invoked so easily against his position.

5. Concluding Remarks

Our task in this paper was to try and organize Wittgenstein's remarks on contradiction and consistency among a few central issues, i.e., to try and reach an *Übersicht* and see from it some new connections. For reasons of space, we did not try and show how our understanding of these remarks differs from other views expressed in the secondary literature. Neither could we spend much time separating the wheat from the chaff or even try and mount a defence of the 'anti-realist' stance that emerged from our overview of Wittgenstein's remarks. With rejoinders to Turing and Chihara in the last section, we merely hoped to have argued for the *prima facie* cogency of some of his claims, but

49 See the reprint of Wrigley's paper, (Wrigley 1986, 351-352).

50 We would like to thank Kai Büttner and Jaap van der Does for their comments on an earlier draft.

we do not claim to have settled any debate, especially in light of the fact that our discussion purposefully remained at a very low level of logical sophistication, so to speak. Again, our task was merely to organize (Wittgenstein's) thoughts, as a sort of prelude for further, more sophisticated discussions; we can only conclude saying that Wittgenstein deserves a better hearing.[50]

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