

Reply to P.T. Johnstone's review of my *Realizability*
September 24, 2010

Two and a half years have elapsed between the appearance of the book (March 2008) and this review (Bulletin of Symbolic Logic 16 (3), September 2010) and whatever may have caused the delay, it is not that the reviewer was slow in reading it; in fact he was one of the first who gave a reaction (first in a private e-mail, then on the categories mailing list).

The Bulletin may have a large backlog. But after reading the review one cannot help thinking that time has been spent on a struggle in the reviewer's mind over what to think of the book, and regrettably this internal conflict did not get resolved before the review was written. The result is a strangely ambiguous piece.

There is some generous praise: [...] *the author's treatment of the effective topos is very well-judged: everything the general reader needs to know about it is here, with the right degree of emphasis.* This is a lot, given that the chapter on the effective topos is about half the book. But somehow, it doesn't seem to make the reviewer happy and in the end, the book is *warmly welcomed* but on a "better than nothing" basis.

What could cause the reviewer's uneasiness with the book, expressed in such awkward sentences as *it is not the account of the subject for which the world is ready?* Incidentally, a naïve interpretation of this phrase would be that the book is ahead of its time, but I doubt that that was the reviewer's intention.

After some preliminary comments the reviewer states his conclusion: *The time may indeed be ripe (indeed, in some respects it is over-ripe) for a book on realizability toposes: but perhaps it isn't quite ripe for this particular book.* As the author (who was, naturally, eagerly anticipating Johnstone's review), I am of course very curious to know what it is that disqualifies *this particular book*, but close study of the review doesn't reveal more than basically one technical point: the reviewer does not agree with the approach to realizability toposes via triposes.

A sentence like *Where the reviewer does part company with the author is with regard to the relative prominence of the first two chapters* seems to want to say more, but in fact the reviewer is silent about chapter 1 and indeed, it seems hard to treat realizability toposes without paying attention to partial combinatory algebras; in chapter 1, I have endeavoured to highlight those properties and constructions of pcas that are relevant for the toposes. But, he does not say anything about it.

The central drawback of the book is then, according to the review, the

emphasis on triposes whereas *it has over the years become clear that a much gentler approach is provided by regarding them [realizability toposes] as exact completions of their subcategories of assemblies.*

I was aware of this, but maybe I was not aware of its *force*. The theory of triposes (I am really sorry that I didn't give due credit to Hyland and Johnstone for the notion¹) is not only used for defining the toposes: it is also used for giving their structure and internal logic. It is not so clear to me how to do this well in the 'exact completion' approach. Apart from this, I use the triposes for their notion of geometric morphism; and I make use of Change of Base and Iteration. Finally, there is a number of 'realizability-like' toposes where it is not a priori so clear what their *categories of assemblies* would be. What I would contend, then, is that the approach via triposes is a defensible choice that does not make a book *partially flawed*.

The reviewer reproaches the author for making *not nearly enough* use of the fact that every realizability topos is the exact completion of a category of assemblies, but he does not give an example of what might have been done with it. Instead, he notices that some properties of the category of assemblies are missing, such as being a quasitopos and having an exemplary subobject. But these are typically the properties one needs in order to prove that the exact completion is a topos; and, as is now clear, I did not choose that route.

I am also castigated for failing to note that things I prove for the effective topos are valid for arbitrary realizability toposes. Well, this is not quite fair. The second paragraph of Chapter 3 states: *This chapter intends to investigate toposes of the form $RT(A)$ in detail; but for definiteness, we focus on the effective topos. Virtually everything we meet in this chapter, carries over to the more general situation, with appropriate changes.*

That means, I have trusted the reader with the ability to carry out these required 'mutatis mutandis's'. For example when I prove (3.2.3) that in $\mathcal{E}ff$ the countable coproduct of copies of 1 does not exist, the reader should be able to deduce that this has to do with the cardinality of the underlying *pca*. It gets tiresome to say continually that this or that holds for all realizability toposes.

The remark that in any realizability topos every injective object is uniform, is a nice one and the reviewer contrasts it with what I prove, namely that in $\mathcal{E}ff$ (and hence, in every realizability topos) every power object is uniform. True, I might have given the slightly more general statement,

¹To avoid a misunderstanding: the Hyland-Johnstone-Pitts paper is, of course, mentioned in the book. But I wrongly credit only Pitts with inventing the notion

which, since in every topos an injective object is a retract of a power object, is a one-line corollary of what I prove.

Reviews are extremely important both for informing the general reader about a book, and for providing the author with an opportunity to learn. Especially when the reviewer is a top researcher in the area the book covers, as is the case here. The present review does not offer the general reader, who might be interested in realizability toposes, a clear-cut opinion on whether or not to read my book, and for me there is only, after some technical matters, an unpleasant, snubbing tone that I cannot understand to deserve.

E-mail discussion on Johnstone's review

Some footnotes later added by JvO (October 5, 2010)

10/01/2010 11:38 AM From: Prof. Peter Johnstone

Dear Jaap,

Thank you for directing my attention to your response to my review of your book. I can understand your feelings of disappointment at the negative aspects of my review, but I ask you to understand that those negative aspects sprang from my own disappointment that your book wasn't quite what I hoped it might be. I'd like, if I may, to reply to some of the points you make.

First, the delay in publication of the review is my fault, not that of the Bulletin of Symbolic Logic. Having read the book (and e-mailed you about the supposed open question on page 235), I started thinking about how to draft my review, but then set it aside because of other more urgent tasks. I'm afraid I then forgot about it for around a year, and only remembered it earlier this year when I was working on the realizability chapter in volume 3 of the Elephant. (I suppose I could blame Steve Awodey, who commissioned the review, for failing to remind me about it; but the primary fault is clearly mine.)

Next, please understand that I am not in any sense opposed to the use of triposes. How could I be, when I was one of those responsible for introducing the concept? (I don't wish to discuss what proportion of the credit for the notion is due to which of the three authors of the HJP paper; but the choice of name was due to me, as I'm sure the other two would confirm.)

However ... the notion of tripos is much more general than that of realizability topos. Every frame is a tripos (a fact which we mentioned in HJP, but which I don't think appears in your book), and the boundaries of "what you can do with triposes" are even more

shadowy and less well explored than those of "what you can do with realizability". Your book gives no hint of this, concentrating almost entirely on the triposes that arise from PCA's (and their various generalizations). Of course, that's what it should be doing: the first book on realizability toposes ought to concentrate on realizability toposes, and not go charging off after little-understood generalizations.

But if you are going to concentrate on realizability toposes, then (as I said in the review) the theory of triposes is not necessarily the best way to get there. The problem is that the topos constructed from a tripos is a rather "floppy" structure: its morphisms are not functions but equivalence classes of functional relations (and the equivalence relation is itself fairly difficult to grasp), and because of this its objects have a habit of turning up in numerous seemingly different, but actually isomorphic, guises. This has the effect that even simple categorical constructions like the construction of equalizers is quite delicate; so it's hard going for readers who haven't met the construction before.

Compare that with a category of assemblies, which is so beautifully simple that you could give it as an example to students in a first course on category theory. Its objects are sets with a certain kind of structure, and its morphisms are simply "structure-preserving" mappings. Construction of finite limits and colimits, exponentials and the natural number object are all done simply and concretely: that is, one takes the appropriate construction in Sets and works out what structure it ought to carry. The fact that this simple-minded construction gives you something very close to a topos (specifically, a quasitopos with NNO and an exemplary subobject, so that all you have to do to get a real topos is to add in the "missing quotients") is so remarkable that it really should have been celebrated in the first book on realizability toposes, and my disappointment with your book (which you have correctly identified from my review) stems almost entirely from the fact that your readers aren't going to learn it from you. (It will, of course, be celebrated in chapter F2 of the Elephant.) It means that all the "floppiness" of realizability toposes resides in the final construction of adjoining quotients; and that construction is one which is well understood in other categorical contexts. For this reason alone, I believe it is a much better way to introduce the subject of realizability toposes to students and others meeting it for the first time.

You complain several times that I don't give examples in my review, to back up my criticisms. (In fact I gave several; but you should bear in mind that book reviews are subject to length limits².) So let me give you one: the proof that injective objects are uniform in any realizability topos. It goes as follows: "Every object of a realizability topos is a quotient of an assembly, which is a subobject of a nabla. If the object is injective, then by the very definition of injectivity it is a quotient of a nabla -- that is, it is a uniform object". Compare that with your two proofs of uniformity in Propositions 2.4.7 and 3.2.6. And incidentally, if you knew that all power-objects in all realizability toposes are uniform, and expected your readers to deduce it from 3.2.6, why did you include 2.4.7 at all? The way it appears, it looks to the reader (to this reader, at least) as if 2.4.7 is "the best you can do" for arbitrary realizability toposes, and 3.2.6 is a stronger result proved only for the effective topos.

Incidentally, you are right to criticize the sentence "It is not the account of the subject for which the world is ready" in my review: it was an inelegant way of expressing what I wanted to say, brought about by the effort to be as concise as possible, and I apologize for it. What I really meant was that I felt your book was "not quite ready for the world", in the sense that you hadn't thought through quite carefully enough what ought to go in and what should be left out -- the duplication inherent in 2.4.7 and 3.2.6 is an obvious example of this. (The "open problem" on page 235 is another, of course: if you'd thought to ask me about it, I could have told you the answer.) Another example: once one knows that all injective objects are uniform, it becomes natural to ask whether the converse holds -- or rather, since the empty assembly is an obvious counterexample, whether every well-supported uniform object is injective. The answer is no, and it would have been good to provide a counterexample at the relevant point³. Yet another: in Theorem 1.7.2, you refer to a functor between categories of assemblies preserving the NNO, but you never tell your readers

²I don't know what these limits are, but there are several reviews in the same issue of BSL which are twice as long as Peter's: pp.415-419, pp.420-423

³One such counterexample in a topos $\mathbf{RT}(A)$ is provided by the object $(\mathcal{P}_{\text{fin}}(A), \sim)$ where $\mathcal{P}_{\text{fin}}(A)$ is the set of finite subsets of A , and \sim is the equality relation inherited from Ω . The obvious embedding of this object into Ω cannot have a retraction, hence this object, although uniform, is not injective

what the NNO in a category of assemblies is! (I complained about that in the review. And don't respond by saying that you described the NNO for assemblies over the first Kleene algebra in 3.1.1 -- that description is one which *doesn't* immediately generalize to other PCA's, because it relies on the coincidence that the underlying set of the Kleene algebra is N.)

Finally, if you feel aggrieved that I don't, in my review, "offer the general reader ... a clear-cut opinion on whether or not to read [the] book", the reason is that I was unable to come to a clear-cut conclusion on this point myself. The best I could come up with was the conditional recommendation with which I finished the review, that "if ... one accepts ... that it is better to have this material available in book form than not to have it, it is ... a book to be warmly welcomed". I'm sorry if that conclusion offends you, but it represents my honest opinion, and readers of the review will have to interpret it as seems best to them.

Best regards, Peter

10/01/2010 02:51 PM From: Jaap van Oosten

Dear Peter,

Thanks a lot - I really very much appreciate your serious reply. If I may say so, this reply is a lot better than the original review. Would you object if I post it on my web site too, along with my Reply?

I would still like to respond to a few things you say.
> your book. I can understand your feelings of disappointment
> at the negative aspects of my review
I was disappointed in the whole review, for the reasons I mentioned: no information about the book's contents in terms a general reader would understand, no real conclusion. But I should confess that part of my disappointment is related to the fact that in your private e-mail of september 26, 2008, you wrote
> I've been reading your book on realizability, which I like a lot, and then when the review appears this turns out to have been just politeness.
> realizability topos. Every frame is a tripos (a fact which
> we mentioned in HJP, but which I don't think appears in your book)
It appears several times: on p.54 as one of the two prime examples of triposes, and in subsection 2.6.1. On pp.81-2 there is a specific example (of a complete Boolean algebra)

> celebrated in the first book on realizability toposes,
> and my disappointment with your book (which you
> have correctly identified from my review) stems
> almost entirely from the fact that your readers aren't
> going to learn it from you. (It will, of course, be
> celebrated in chapter F2 of the Elephant.)

Although disappointment is a personal emotion and hard to quarrel with, I would like this to be backed up by some more argument. If a whole book is disqualified for lacking a specific result, it should be argued that that result is absolutely central to the subject. You have not yet convinced me of that⁴.

You also do not respond to some reasons I state in my Reply, for using triposes: for studying geometric morphisms, because I use triposes over other base categories than \mathbf{Set} , and because for some variations of realizability it is not a priori clear of what sort of 'assemblies' they are exact completions (of course one can read off the category of assemblies from the tripos-to-topos construction and then hide that from the reader). For example, modified realizability.

> You complain several times that I don't give examples in my review
I make this complaint exactly once, and specifically about what one can do with the exact completion result.

> book reviews are subject to length limits.) So let me give
> you one: the proof that injective objects are uniform
> in any realizability topos. It goes as follows:
> "Every object of a realizability topos is a quotient of an
> assembly, which is a subobject of a nabla. If the object is
> injective, then by the very definition of injectivity
> it is a quotient of a nabla --that is, it is a
> uniform object". Compare that with your two proofs of
> uniformity in Propositions 2.4.7 and 3.2.6.

I agree with this criticism. I missed this simple proof. That is why I included 2.4.7 (which is in the general context of toposes of the form $C[P]$, not just realizability toposes) as well as 3.2.6 (which was meant to be understood for realizability toposes). I now see that it is in fact a duplication.

> in 2.4.7 and 3.2.6 is an obvious example of this. (The
> "open problem" on page 235 is another, of course:

⁴How could he? How can I tell "everything the general reader needs to know about [the effective topos]" while omitting a central element of its construction?

> if you'd thought to ask me about it, I could have told
> you the answer.)

We discussed this following your e-mail message which I quote above.
I have certainly brought this up with Martin and Pino. I was honestly
unaware of your solution.

> preserving the NNO, but you never tell

> your readers what the NNO in a category of assemblies is!

The NNO for arbitrary realizability toposes $RT(A)$ is given, but
only on page 268 as a kind of afterthought. I agree that the whole
treatment of categories of assemblies is somewhat incomplete.

> Finally, if you feel aggrieved that I don't, in my review,

> "offer the general reader ... a clear-cut opinion

> on whether or not to read [the] book", the reason is

> that I was unable to come to a clear-cut conclusion

> on this point myself.

It is still my opinion that a reviewer must formulate a conclusion.

If this is difficult, he should tell the readers why it is. Why
certain technical shortcomings really spoil the book.

Anyway, let me reiterate my gratitude for your reaction.

Best regards, Jaap

10/04/2010 01:29 PM From: Prof. Peter Johnstone

Dear Jaap,

Yes, you are welcome to put my previous e-mail on your website
if you want.

I'm afraid that, with our term just starting, I don't have time
for any further detailed discussion with you. So we're just going
to have to agree to differ. But let me just say that my earlier
statement, which you quote, that I was enjoying your book wasn't
just politeness: there are indeed many things about it which I
enjoyed⁵. But there is one thing that I emphatically didn't like,
and that is your failure to tell your readers about the structure
of categories of assemblies. (I don't think I'd really noticed
that absence at the time I wrote to you in 2008.)

Best regards, Peter

⁵So sad, that readers of the BSL did not get to read about them!