ABOUT THE REVIEW IN MATHEMATICAL REVIEWS OF MY PAPER: THE TWO-CARDINAL PROBLEM FOR LANGUAGES OF ARBITRARY CARDINALITY THE JOURNAL OF SYMBOLIC LOGIC 75, NUMBER 3, SEPT., 2010, PP. 785-801 DOI:10.2178/JSL/1278682200

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In this note I make some comments on the review MR2723767 (2011m:03064) appeared in Mathematical Reviews. I can not provide access here to the review or even to my paper, it is copyright material.

1. The review

(1) Paragraph five of the review: "The paper observes that, in L,there is a coarse...."
In the paper (page 788) I give the axioms of a $(\mu, 1)$-coarse morass according to Jensen in terms of pairs of primitive recursive closed ordinals, and I mention that " A proof of the existence of a $(\mu, 1)$-Coarse morass in L can be extracted from [Dev84] for pairs of adequate ordinals"...

An adequate ordinal is admissible or the limit of admissible ordinals (Devlin p. 339 at the Bottom). In particular, they are primitive recursive closed. The proof of the existence of a full $(\mu, 1)$-morass starts in page 344 in Devlin's book. Actually what Devlin really construct is a $(\omega_1, 1)$-morass, but the proof works perfectly good for any regular cardinal other that ω_1. It is just a matter of following this proof, to corroborate that he constructs, in particular, a $(\omega_1, 1)$-coarse morass. In item (b) I should write closed on $\sup(S_\alpha)$, not in μ^+ as the reviewer claims.

(2) Again Paragraph five of the review: "For example, on page 793, lines 1-3, a function is defined and stated to be $\Sigma_1(\{\alpha_1\})$ but it is not....."

The constructions begins in page 792, not in page 793 as the reviewer wrote. I claim that $S_{\alpha_\nu} \cap \nu$ is $\Sigma_1(\{\alpha_\nu)\}$ for every $\nu \in S^1$, which follows from any construction of a $(\mu, 1)$-morass, because this is necessary to succeed on building such a morass. For other proofs of the existence of a $(\mu, 1)$-morass see: L. Stanley, *A short course on gap-one morasses with a review of the fine structure of* L, in: Surveys on set theory A. Mathias (Ed.), London Math. Soc. Lecture Notes Series # 87, 1983, pp. 197-244, or P. Welch, Σ^*-*fine structure*, in A. Kanamori, M. Foreman (Eds), Handbook of Set Theory, Springer-Verlag, pp. 657-736, or the reference [Don81] in the paper.

What is important to us: I am not claiming that the morass maps are Σ_1-preserving for a language which expands LST, I only need the above mentioned fact that $S_{\alpha_\nu} \cap \nu$ is $\Sigma_1(\{\alpha_\nu)\}$ for every $\nu \in S^1$. I also use that $<_L$ is Σ_1-definable to build the functions h_{α_ν}. The sets B_ν appear in $P(L_{\alpha_n u})$. Indeed what we want is to enumerate those sets in a $\Sigma_1(\{\alpha_\nu\})$-fashion. Once we have this enumeration, we can appeal to the morass maps and get the desired preservation.

(3) Paragraph 6. Indeed Lemma 5.4 as stated is wrong, we have to require that the $\vec{x}$ belong to U. But we can take this lemma off the paper, it is not necessary in what follows.

The proof of Lemma 5.6 is unnecessarily complicated, and we do not need Lema 5.4. Let me provide a clearer proof.

Proof of Lemma 5.6. We keep the given proof until the bottom of page 791. We have to show that c is transcendent in $\mathfrak{C}$ over $B \cup U^{\mathfrak{C}}$. First we prove that c is transcendent in $\mathfrak{C}$ over B. Otherwise there exists a formula $\varphi(v, \vec{b})$, $\vec{b} \in B$ in the complete type of c in $\mathfrak{C}$ over B such that

$$(\mathfrak{C}, c, \mathring{B}) \models \varphi(c, \vec{b})$$

and

$$(\mathfrak{C}, \mathring{B}) \models \neg \coprod v \varphi(v, \vec{b}).$$

From the last assertion we get

$$(\mathfrak{C}, \mathring{B}) \models \exists u \forall v \blacktriangleright u \neg\varphi(v, \vec{b}),$$

hence $\neg\varphi(v, \vec{b})$ would appear in $\Sigma_c(v)$ by construction of this set (page 791), thus $(\mathfrak{C}, c, \mathring{B}) \models \neg\varphi(c, \vec{b})$, a contradiction.

Now we show that c is transcendent in $\mathfrak{C}$ over $U^{\mathfrak{C}}$. If this is not the case, as above, we find a formula $\varphi(v, \vec{x})$ with $\vec{x} \in U^{\mathfrak{C}}$, such that

$$(\mathfrak{C}, c, \vec{x})_{\vec{x} \in U^{\mathfrak{C}}} \models \varphi(c, \vec{x})$$

and

$$(\mathfrak{C}, \vec{x})_{\vec{x} \in U^{\mathfrak{C}}} \models \neg \coprod v \varphi(v, \vec{x})$$

Then

$$(\mathfrak{C}, c, \vec{x})_{\vec{x} \in U^{\mathfrak{C}}} \models \exists \vec{x}(U(\vec{x}) \wedge \varphi(c, \vec{x}) \wedge \neg \coprod v \varphi(v, \vec{x}))$$

The formula at the right has only c as parameter, so it belongs to the complete type of c in $\mathfrak{C}$ over the empty set (or over B). Therefore

$$\mathfrak{C} \models \coprod w \exists u \exists \vec{x}(U(\vec{x}) \wedge \varphi(w, \vec{x}) \wedge \neg \coprod v \varphi(v, \vec{x}))$$

The elements $\vec{x}$ belong to U, which is lineraly ordered without maximum, so we can find $u \in U$ with

$$\mathfrak{C} \models \coprod w \exists u \exists \vec{x} \blacktriangleleft u(U(\vec{x}) \wedge \varphi(w, \vec{x}) \wedge \neg \coprod v \varphi(v, \vec{x}))$$

This is a contradiction: the formula at the right has no parameters at all, and if $\mathfrak{A}$ is the original structure of type (κ^+, κ), we get

$$\mathfrak{A} \models \coprod w \exists u \exists \vec{x} \blacktriangleleft u(U(\vec{x}) \wedge \varphi(w, \vec{x}) \wedge \neg \coprod v \varphi(v, \vec{x}))$$

because of $\mathfrak{A} \equiv \mathfrak{C}$. Then

$$\mathfrak{A} \models \forall r \exists w \blacktriangleright r \exists u \exists \vec{x} \blacktriangleleft u(U(\vec{x}) \wedge \varphi(w, r) \wedge \neg \coprod v \varphi(v, \vec{x}))$$

For each $r \in A$ we find $u, \vec{x} \in U$. We have available κ^+ such r's and only κ $u, \vec{x}$'s, then there exist $u, \vec{x} \in U$ such that

$$\mathfrak{C} \models \exists u \exists \vec{x} \blacktriangleleft u \coprod w(U(\vec{x}) \wedge \varphi(w, x) \wedge \neg \coprod v \varphi(v, \vec{x}))$$

hence

$$\mathfrak{C} \models \exists u \exists \vec{x} \blacktriangleleft u(U(\vec{x}) \wedge \coprod w \varphi(w, \vec{x}) \wedge \neg \coprod w \varphi(w, \vec{x}))$$

which is clearly contradictory. ❐

(4) Last paragraph in the review: "Let me add that the argument..."; it is often necessary to cite results from other researcher or own results, and it is not always possible to anticipate any future developments.

2. Personal remarks

- As soon as I have read the comments of the reviewer about the proof of the existence of a coarse morass (see (1) above), I have realized that he does not have any idea about morasses. Someone a little bit acquainted with morasses knows very well that a coarse morass is just a particular case of a full morass. Even worse, he has no idea about the two cardinal transfer problem and what is the paper under review about. This is confirmed by reviewer's comments about Σ_1-preserving maps (See (2) above). The most important point in the paper is how to incorporate slowly predicates in the construction, because we have to keep things in a small cardinality, and one way I found to proceed is with that enumeration of the predicates. But this is done before the morass is being put into action. It seems to me that, if someone is going to write a review, the least we can expect is that he has a reasonable idea of the matter. I find very unprofessional the work of the this reviewer.
- About (3) above: Indeed lemma 5.4 as stated is wrong, and since the given proof of Lemma 5.6 uses 5.4, it is also wrong. Actually, as we make clear above, the problem occurs in the last step of the proof. By construction of the set of formulas $\Sigma(v)$, should be clear for anyone interested on the proof, that the element c is transcendent in $\mathfrak{C}$ over B, and also over $U^{\mathfrak{C}}$ because membership to U is part of our language. In fact, one of the recommendation of the referees was to delete Lemma 5.4, to shorten the proof of the Lemma 5.6 at the end and just to write that the transcendency of c follows immediately from the nature of c. My mistake: I have tried to provide a full proof, when that was unnecessary and my argument is incomplete and wrong. Anyway, above I provide a full proof, may be too long, but the reviewer seems to need extra help to understand a mathematical proof.
- I do not understand at all the motivation of the reviewer when he wrote the last sentence of the review, about to publish a single paper.
- About (4) above. I would like to think that the reviewer do some research by his own.
- I wish my forthcoming papers will be reviewed by a logician or a set-theoretician.

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