Reviewer #1: I've checked the new version with respect to my comments and some of the other reviewers''.

This is a significantly improved version, which deserves to be published.

>Nothing to do here.

Reviewer #2: I have to say that the paper has improved greatly with respect to the first version, but I am still dubious about its content. I will first mention the positive parts pf my review. The introduction is much more clear and also the part presenting the calculus is more convincing, though I have still some criticisms. It is also worth to noticing that many of explanations are in the part where the answers to the reviewers suggestions or criticisms are confined, but these explanations are in fact triggered by the criticisms, thus what the authors do is understandable.

The paper as it is presents mainly an application of the calculus of covalent bond the authors have introduced in a previous paper. In the previous version of the paper the argument was that the calculus has to be changed to deal with the problem (the DNA mismatch repair) but at the very end it seems that this is not needed, though I have not been able to understand to which extent this is true. The calculus is now very much alike to the calculus originally presented in the journal Science of Computer Programming, with some main differences: the prefixes and one concert rule.

>We can explain the change from the first version to this version just for the reviewer.

For the prefix, I must say that I agree with another reviewer complaining on the use of '|' in defining prefixed. It is misleading, as it suggests parallelism but it is not, you do not have any rule for that, and it creates confusion. Use another symbol instead of '|', like $\oplus$ (it indeed resembles some tensor products), and I have to stress that this criticisms has been not considered carefully.

>We can use a new symbol. On the other hand, it is a type of parallelism, so the symbol makes sense.

For the concert rules, we do not have any longer a proliferation of them, but we have just two of them (the concert3 rules are shortcuts). The first one (called concert2') establish that two agents evolving first with a forward action and then with a backward action can concert and perform a concerted action which has a forward and a backward part, the second one (the new one, which is now concert2) involves three participant, in the sense that the second agent does not perform a forward action follower by a backward action, but he backward action can be performed by another agent. The question is then whether it would be possible to have just one of them, namely the one involving three participants, and rewrite the classical one as a special case of this one? I find the answer to the usage of syntactic congruence illuminating (page 4 of the answer part) as you may say whatever you like, instead of stipulating that parts have to be in this way. It is however a minor criticisms, as the authors seem to be little interested in the calculus itself but rather into be able, with a calculus, to model the problem tackled (the DNA mismatch repair).

>For me, that sounds a bit like a sidetrack, and it might not make it easier to understand. Keeping what we had sounds sensible for me.

I consider the paper still unsatisfactory, mainly because of the focus. The problem of the DNA mismatch repair is an interesting one, but for computer scientists I consider much more interesting to understand how a tailored calculus works, and maybe to discover that the original calculus can be improved, retaining the original motivations. In my opinion, the fact that the one proposed here is an extension of the previous one is not properly motivated. However I am not convinced that the paper can be improved, beside the definition of prefix, which has to be properly considered.

>Nothing to do

Reviewer #3: It is OK now - In this revision the authors fully addressed my previous comments.

>Nothing to do here

Reviewer #5: Overall, I think that the current version addresses satisfactorily most of my comments raised in the previous review round. I still find the introduction too vague about the purpose of the paper. It avoids mentioning that the modelling of MMR requires changes in the calculus, which seems insufficient for modelling MMR. I understand that this may be a matter of taste, hence I leave the choice to the authors.

There are a couple of minor issues that should be addressed in the final version.

L51. other the other hand ->

>Changed

L168. Concerted actions are now presented as a 'shortcut' (while it was previously said that "CCB is slightly extended"). What does shortcut mean? It seems to imply that it can be derived from the original calculus. In this case, I would expect to actually see that the rule is derivable from the original semantics (this does not seem immediate since the new concert rules consider labels that are not in the original presentation, the collection of sites) . Otherwise, it should be explicitly stated that the calculus is extended. Note that this is stress again in the conclusions.

This is somehow addressed in the last section

>We've got that on page 20. Perhaps it needs a more explicit wording?

L177. actions1 Reverse -> actions1. Reverse (full stop missed)

>Changed

245. Groups of concerted actions -> groups? Are you referring to elements in AK x und(A)K. Then, they should be just pairs. Moreover, I think this presentation is partial, because the triples as in concert3 act or pairs in concert3 are not included in the enumeration. Note that this (incomplete?) definition of labels may need to be amended in the rest of the paper, e.g., Def 2.

>Considering that we have concert3 as a shortcut, I would just change this from "groups of concerted actions" to "concerted actions".

Fig. 10. The label {e[k],\_f[l]},\_j[m] in the transition in the conclusion of rule concert3 seems wrong. Shouldn't be {e[k],\_f[l],\_j[m]}

>Changed