Dear Editor,
please find below a detailed list of the changes we made to the manuscript. The bulk of the changes followed the referee's comments (starting with "==>"), in the order in which they appeared in the referee report. Each item are separated by a dashed line.
For clarity, we have marked all changes in bold face in the resubmitted manuscript.
With best regards,
R. Rousseau

Response to the referee:

We appreciated the comments of the referee and would like to thank him/her. We are very grateful for all the suggestion he/she sent us which largely improved the quality of this paper.

The comments are addressed in-line in the referee report below preceded by "==>". Each item are separated by a dashed line.
For clarity, we have marked all changes in bold face in the resubmitted manuscript.

Referee Report

I have read the newer version of the manuscript and the authors' reply. The authors have considerably revised the Discussion session, at least partially answering my questions. Now the approximations used by the model are better explained, and more extended citations to previous modeling works are given. The observational arguments in favor of a low nebular magnetic field, as a consequence of the stringent upper limit of the X-ray flux, have been strengthened by the citation of other similar cases.

I am still not fully convinced about the following points.

1. The arguments used to justify the presence of a very low nebular magnetic field, even if the system is assumed to be beyond the reverse shock passage and with the surrounding SNR already in the Sedov phase, are not very convincing. More precisely:

- Models assuming spherical symmetry are criticized in the manuscript, being inconsistent with what the authors mention as "the significant offset of the pulsar from the gamma-ray centroid". In fact, the pulsar offset does not seem to me, when compared with the nebular size (see e.g. Klepser et al. 2008, Fig. 1), at the level so large to make useless even the qualitatively findings of those models (like the presence of a reverberation phase). On the other hand, it sounds strange that, for the time evolution of the magnetic field, formulae taken from models that assume spherical symmetry are then used in the paper.

- About the pressure balance, at later phases, between the pulsar wind nebula and the surrounding SNR, the authors correctly point out (in their reply) that even in these cases the magnetic field could be very low if the pressure is dominated by the relativistic particles. However, since Gelfand et al. 2009 get a much higher nebular field, even taking a low efficiency (10-3) in injecting magnetic field, I am wondering how low the magnetic efficiency at injection should be assumed in this case.

- A related issue comes from what explicitly written at the end of the left column of Page 4, namely "The interaction of the PWN and the SNR reverse shock compresses the PWN, resulting in an increased magnetic field." Even if shortly after it is stated that details about this phase cannot be derived from spherically symmetric models, I would have expected in the assumed PWN evolution at least a sign of PWN contraction and of magnetic field increase, but there is none.

2. In order to show the validity of the quoted errors of their best-fit parameters, the authors have described more quantitatively the statistical method used (basically, that given by "Numerical Recipes"). However, I fear that the main uncertainties are not statistical but are rather dominated by the assumptions introduced (like the special evolution law for the magnetic field).

Considering that there are other published papers providing models with similar levels of approximations, and that the authors do not pretend to draw conclusions far beyond what can be reasonably obtained form these data and this treatment, I believe the manuscript can be published essentially in its present form.

==>We agree with the referee on this point and thank him for pointing them. In this paper, we just applied a simple model consistent with the work done before on other sources and accepted by the community as observed by the referee. A more complicated model would be hard to constrain due to the leak of multi wavelength data on this source and would be out of the scope of this detection paper. Thus we tryied to keep it as simple as possible with reasonable parameters.

Let me just suggest:

1. As for the Tables 2 and 3, that the authors specify that the uncertainties given there are only statistical, while further systematic uncertainties may arise from the some ad hoc assumptions on the PWN expansion and on the evolution of its magnetic field.

==>We added a sentence in each table to mention that the uncertainties are only statistical.

2. page 4, in the lower part of the left column. Please clarify the meaning of the expression "ambient photon fields are static".

===> We developed the sentence to explain it more clearly. This simply means that the photon fields are uniform and do not vary during the evolution time of the electron populations.

3. Here some typos to correct.
- the reference to Abdo et al. 2011 has been deleted, but there is still a link to that reference, in Section 3.2.2, which now appears as a question mark.

==> The authors list of this fermi paper was recently changed to highlight the huge effort done by our collaborator Pat Nolan. We corrected the reference and the link in section 3.2.2.

The paper changed to be Nolan et al. 2011 in the latest version. We fixed the typo.

- page 4, near the bottom of the left column: "as late as late" -> "as late"

==> We fixed it in the text

These changes are minor.

==> In addition to the modifications kindly suggested by the referee, we slightly changed the sentence beginning by « In the 2-10 keV energy range, » in the paragraph on the X-Ray measurements to better introduce the XMM measurement that will be presented in Bogdanov et al. (in preparation). This new sentence now starts with « Based on a 30-ks XMM ».