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

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Referee Report:

In my previous report, apart from other minor issues, I mainly asked for a better explanation of the numerical model. However, I still find it unsatisfactory, so that I still have to request major revisions before publication.

What puzzles me most is that for the three models the have been presented, the authors provide values of various quantities (B_f, E_cut, p, P_o, age), with their relative uncertainties, suggesting a very accurate numerical analysis. But are these fits really constraining, or just illustrative? What is actually computed by the models, and what is just assumed? All this should be stated clearly.

==>We now describe the fitting procedure in greater depth in the Discussion section. The fits are constraining within the model framework. We also included references to Abdo et al. 2010 and Van Etten & Romani 2011, which include a more complete description of the model. The one-zone model applied here is also a quite standard approach, and qualitatively very similar to a number of other models recently published modeling papers: Zhang et al. 2008, Lemiere et al. 2009, Tanaka and Takahara 2010, Mayer et al. 2012. We could include a longer description of the model, but since this model is not new we prefer not to include a full (~1 page long) description of the model, which would shift the bulk of the paper away from the discovery aspect and more towards the modeling aspect. However, we have added a full table summarizing the values of the fitted parameters in the different scenarii described in the paper.

In the original version of the manuscript, for this "one-zone time dependent SED model" there was just a reference to the paper Grondin et al. 2011. Even in that paper there is just a rather qualitative description. From the description given of the models, I understand that they should follow the whole PWN evolution. Right? If so, how can the smoothness of the transition from free expansion to Sedov be "an assumption", or not "a result"? Or is it that these models do not compute the dynamics?

==> The models do no compute dynamics of the PWN-SNR interaction for a number of reasons. The complexity of this interaction introduces a number of new parameters to the model, all of which are very poorly constrained given the lack of multiwavelength data for this source. Second, the offset nature of the gamma-ray source and pulsar precludes applying previously developed (e.g. Gelfand et al. 2009) models which compute the dynamics assuming spherical symmetry. Some past SED models of similar PWNe (de Jager et al. 2008) simply ignore the dynamics of the system in an effort to create a simple model.The dynamics we adopt of r~t^1 followed by r~t^0.3 also appear in Mayer et al. 2012. This is now explained more clearly in the paper. We switch t_sedov to 3 kyr, which is a more appropriate age for a typical SNR. The model depends little on the exact value of t_sedov. Adopting t_sedov = 5kyr changes chi^2 by 1.5, and the best fit parameters by <10%.Adopting t_sedov = 10 kyr changes chi^2 by 3 with best fit parameters consistent with the best fit at t_sedov = 3 kyr.

As for the magnetic field evolution, about the trend "B propto t^(-1.5)" the text also says now "as explained in Van Etten & Romani (2011)", but it should be clear that that approximation is valid only for the free expansion phase. Instead, what will be the field evolution after the transition to the Sedov phase?

For instance, Gelfand et al. 2009 computed the dynamics and find a compression of the PWN size by about a factor 20 (which means a factor 400 in the magnetic field strength!). After my first report, the authors have added the motivation: "The low value of the magnetic field is still reasonable in the Sedov phase if one ignores possible compression from the reverse shock". But how is it possible? In my understanding, a PWN significant compression can be avoided only if the PWN pressure is very high even before the arrival of the reverse shock (which is not the case here).

==> The transition to the Sedov phase is a complex process, and modeling magnetic field oscillations introduces significant complexity and is not well understood for asymmetric reverse shocks. The magnetic field does not simply compress and stay compressed, but instead undergoes a series of compressions and rarefactions. With the adopted simple t^(-1.5) evolution, we ignore the oscillations and adopt a model which aims to reproduce the baseline magnetic field, as clarified in the paper

Afterwards, the pressure must be high. From a very rough analytic calculation I get that right after the passage of the reverse shock (i.e. at the beginning of the Sedov phase) the PWN magnetic field should be around:

180 microG *(n_ISM)^(3/10)*(t_Sed/104 yr)^(-3/5)

where t_Sed is the time at which the Sedov phase begins. For this formula I basically assume pressure equilibrium with the Sedov phase SNR, as stated in the paper.

==> The pressure within a PWN is often assumed to be dominated by the pressure associated with relativistic particles, not by magnetic pressure. Therefore the PWN magnetic pressure need not to be in equilibrium with the SNR pressure.

The stringent upper limit on the X-ray flux precludes a magnetic greater than ~5 uG for the leptonic scenario. Matching the gamma-ray data points requires a significant number of high energy electrons, which would create a booming synchrotron X-ray signal for a field of 180 uG. No matter what the magnetic field evolution of the nebula is or was, the lack of X-rays indicates a low magnetic field currently. The Vela-X nebula was modeled with a similarly low 4 uG field. In the hadronic scenario, the field can be much higher, as is stated in the text.

So a much higher field than that assumed in the paper seems to be a necessary consequence, if one assumes that the associated SNR is already in the Sedov phase. The answer by the authors ("this compression is highly dependent on a number of parameters (SN explosion energy, SN ejecta mass, ISM density, etc.) which are unconstrained. For simplicity, we therefore use a smooth transition at 10^4 years.") is not acceptable, since from the formula written above it is clear that there is only a (mild) dependence on the ISM density, while the other parameters concur to produce t_Sed, which in the paper has been fixed to 10^4 yr. Is this correct?

==> The Sedov phase is expected to occur on a timescale of_ ≈ *3 kyr for an explosion of 10^51 erg, an ejecta mass of 10*M_⊙, and an ambient medium density of 1 cm−3 (Reynolds & Chevalier 1984). Eventually, the inward moving SNR reverse shock collides with the expanding PWN, which can happen as late as late as 5 times the transition to the Sedov phase and may not have happened yet for the case of HESS J1857. All these parameters are not very well constrained and this is why we decided to fix the sedov time. However, as stated above, our fit depends very little on the sedov time. Then, as explained in the paper, a much higher magnetic field is precluded by the X-ray data. The formula above assumes spherical symmetry, while the offset between gamma-ray centroid and pulsar position implies an asymmetry in the nebula. We tried to explain these issues as well as the assumptions that we have used more clearly in the text.

To summarize: on one side I still find unclear what the model does and what it does not do, while on the other side I do not understand why the authors are trying to explain the data with a SNR already in the Sedov phase (I feel in fact that, in that case, it would be hard to reach a self-consistent scenario).

==> We hope that the changes made to the Discussion section in the text, the fact that this type of SED model is widely used to study evolved PWNe, and the responses above will satisfy the referee.

==> In addition to the modifications kindly suggested by the referee, we have made some minor changes to make the paper clearer

==> We added Table 2 which summarize the model parameters.

==> We added the following references to the bibliography :

Reynolds, S. P., & Chevalier, R. A. 1984, ApJ, 278, 630
Van der Swaluw, E., Downes, T. P., & Keegan, R. 2004, A&A, 420, 937
Truelove, J.K., \& McKee, C.F. 1999, ApjS, 120, 299