Response to the referee's report on the manuscript DA11780

Title: A systematic study on the cosmic ray antiproton flux

Authors: Su-Jie Lin, Xiao-Jun Bi, Jie Feng, Peng-Fei Yin, and Zhao-Huan Yu

We appreciate the referee for providing the valuable comments on our manuscript. According to the report we have made major revisions to clarify the questions given by the referee. The response to the referee's report is listed as follows.

> *I have carefully examined the manuscript entitled "A Systematic study on the cosmic ray antiproton flux" authored by Su-Jie Lin, Xiao-Jun Bi, Jie Feng, Peng-Fei Yin and Zhao-Huan Yu. In my opinion, this paper does not meet the standards of Physical Review D and should not be published.*
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> *The main thrust of the analysis presented here is based on a certain number of public numerical codes available for the High Energy Physics Community which the authors blindly use in order to derive the production cross section of antiprotons in proton-proton and more generally nucleus-nucleus collisions. They do so to calculate the antiproton flux at the Earth originating from the interactions of high-energy cosmic ray nuclei on the interstellar material of the Galactic disk. These so-called secondary antiprotons are the dominant source of antiprotons and the background to any search for an indirect signal from Dark Matter particles. As shown in figure 3, these codes are unable to reproduce the CERN ISR data for the* $p + p \to \bar{p} + X$ *at high values of x_F. This translates into a large variance for the antiproton flux predictions of figure 5.*
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> *Using these numerical packages is worthless. What is needed in this game is a good parameterization of the NA49 and NA61/Shine experiments which cover the energy range relevant to secondary antiprotons. The authors use actually the Tan & Ng parameterization – which ironically gives much better fits to the AMS-02 data than the high-energy packages as showed in their table II – but fail to even mention more recent parameterizations such as the one derived in Physical Review D90 (2014) 085017 by Mattia di Mauro et al. or the very recent work arXiv:1701.04866 by Martin Winkler.*

Reply: First of all, we are afraid that we can not agree with the referee's claim that "using these numerical packages is worthless". At present, the only viable physical models to calculate the hadronic interaction with nonperturbative effects in cosmic ray studies are based on the "Pomeron" scenario. The hadronic interaction models adopted in our work are all developed in this description. Considering the high complexity of the hadronic interaction, these models show slight differences in their predictions. This is actually a well known fact in the cosmic ray community that adopting different hadronic interaction models may even affect the final presented measurements. In our work, the complexity of the hadronic interaction and the uncertainties of the predicted antiproton flux are systematically studied.

This is actually a very important issue for interpreting the AMS-02 antiproton fraction data, which has not been fully analyzed and emphasized before.

We agree that the analytical parameterization of the experiments data mentioned by the referee is a viable approach to study the cosmic antiproton flux. We have refereed to those papers in the revised version. However, the parameterization method is an effective approach. Compared with the physical models adopted in our paper, the parameterization method contains less physical principle of the hadronic interaction. We are not confident whether such a parameterization approach is good enough in the full phase space at different energies.

Therefore, we think that both approaches should be equally taken seriously at least. More detailed discussions are given as follows.

- The currently available experiment data on the $p + p \to \bar{p} + X$ process are sparse with a few particular $\sqrt{s}$. Furthermore, the cross sections (like CERN ISR) are usually measured at some certain angles. When extending the prediction to the full phase space and all energies, the parameterization approach may lose the physics guide, although it has well fitted a specific data set. On the contrary, the hadronic interaction models based on the "Pomeron" description are well guided by some physical principles, which would guarantee the overall estimation is reasonable.

- These hadronic models have been carefully studied for many years to fit all the available data. Especially some were tuned recently to fit the new data from CERN LHC. The numerical packages for these models are very complicated and the parameters are generally correlated. The usual practice is that the model parameters should only be adjusted by the authors. They are not supposed to be re-tuned by users. This is different from the case of parameterization forms of the hadronic interaction to fit data. This is also very different from the use of Galprop, whose parameters can be adjusted by users due to its clear and simple structure.

- It is also a usual practice in the cosmic ray community that the results are presented by adopting different hadronic interaction models. The model dependence and the uncertainties induced by the hadronic interaction are taken as a systematic uncertainty in the measurement.

- The hadronic models are not particularly tuned for the CERN ISR data, and thus the high x_{F} expectation do not precisely match the data, as pointed out by the referee. However, as the ISR data have only covered a narrow band (around a fixed outgoing angle) in the kinematic space, such a deviation is not enough to conclude an overall failure of the model at $\sqrt{s} = 53$ GeV. In fact, the precision of hadronic models like EPOS LHC for the p_{T} distribution has been discussed in the literature, such as Phys. Rev. C92, 034906. We think that this deviation is acceptable, and the expectations from these hadronic models are still reliable for the antiproton prediction. We will simply compare the overall difference between the hadronic models and the analytic methods in a following figure, and show that the difference around $\sqrt{s} = 53$ GeV is not the main reason for the variance in Fig. 5 of the manuscript.

- In fact, the "antiproton" flux used in the cosmic ray calculation is a sum of the antiproton and antineutron flux. In the parameterization approach the antineutron flux is assumed to have a fixed ratio to the antiproton flux, while the hadronic models can estimate the antineutron flux more realistically. We show the secondary antineutron-plus-antiproton cross section vs momentum plot in the laboratory system for several CR particle momenta in Fig. [CR_cross_section]. In the $p_{\mathrm{proj}} = 158$ GeV panel, although both the predictions of di Mauro et al. and hadronic models like QGSJETII-04m and EPOS LHC could well fit to the NA49 data, the prediction of EPOS LHC is higher than that of di Maruo et al. by a factor of about 1.5. This is partly caused by the difference of the antineutron estimation.

- The differences due to different hadronic interaction models are directly shown in Fig. [CR_cross_section]. In this work we showed how these hadronic uncertainties affect our estimation of the CR antiprotons.

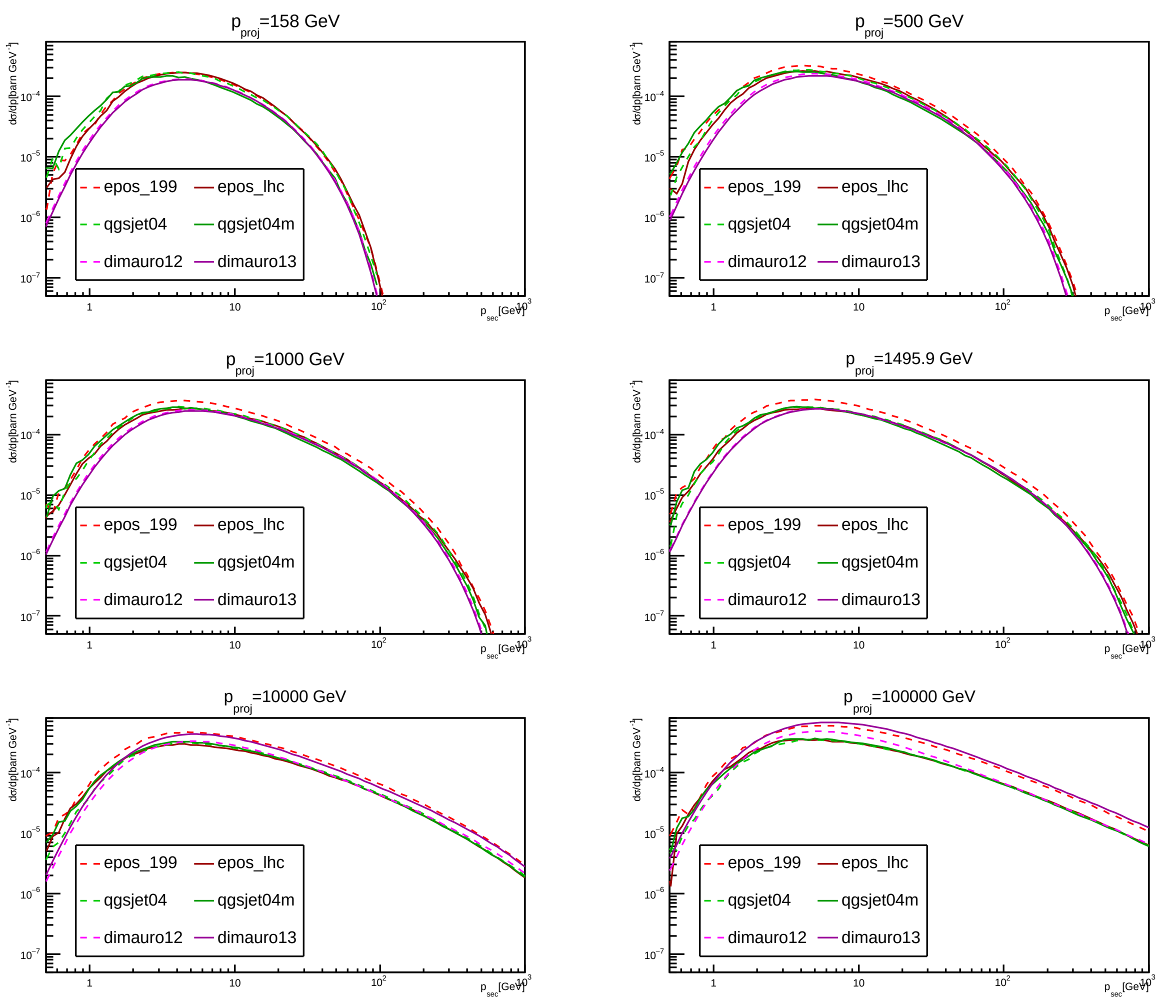

Figure 1: (CR_cross_section) The secondary antineutron-plus-antiproton cross section vs the momentum of secondary product in the laboratory-system, for different incoming primary CR proton momenta

I examine now in detail the paper that falls into four main parts.

1) Cosmic Ray Propagation – The authors use the numerical code GALPROP to determine the parameters which best-fit the AMS-02 data of the boron-to-carbon ratio, a probe of cosmic ray propagation within the Galaxy for energies from 1 GeV to 1 TeV. GALPROP proposes either diffusive re-acceleration (models DR and DR-2) or convection (DC) but both processes are not implemented at the same time in this numerical package. This leads to a misleading interpretation of the errors which the Markov Chain Monte-Carlo yields. In table I, the errors on the δ parameter for instance are small for each of the DR, DR-2 and DC models but the actual uncertainty on δ is given by the large spread – δ varies from 1/3 to 0.51 – between DR and DC.

I have actually a number of comments at this stage.

- There is no discussion on the systematic uncertainties of the B/C analysis. For instance, the authors write that "B/C and 10Be/9Be are sensitive to propagation parameters, but almost independent of primary injection spectra". This has to be proved and a B/C analysis is the proper place for this kind of consideration. In particular, primary spectra are taken to be broken laws. It would be important to study how changing that assumption affects the results.

Reply: We have re-interpreted this problem in the text. Generally speaking, B is produced from C (and also O and N) with the same energy-per-nucleus, and thus their ratio B/C depends only on the amount of matter that C goes through during propagation and is therefore independent of the C flux. Some discussions on this can also be found at arXiv:1011.0037 and arXiv:1001.0551, which are referred to in the manuscript.

- Diffusive re-acceleration and convection are both expected to work. There is no physical reason to consider one without the other.

Reply: The current B/C data would at least require one of the two effects. However, it is not necessary to include both to fit the B/C data (referred to arXiv:1602.02243 and arXiv:1701.06149). We attempt to involve as less effect as possible in the fitting for simplicity. Therefore, we would not involve both of them at the same time. In fact, this strategy has also been adopted in many previous works, such as arXiv:0909.4548, arXiv:1501.04032, and arXiv:1504.04604.

- The parameter R_0 can be redefined within the normalization D_0.

- In the DC model, the authors consider a very strange behavior for the space diffusion coefficient D which they assume to be constant below some rigidity R_0. This was the case in the old days of the Leaky Box model. This behavior is now actually understood as convection overcoming space diffusion at low energies. There is no need to impose a constant diffusion coefficient at low rigidities since the presence of convection leads to that behavior.

Reply: The convection would lead to a similar behavior as a constant low energy diffusion coefficient, but their effects are not exactly the same. We have found that when the B/C and proton fluxes are considered simultaneously, the constant low energy diffusion coefficient could significantly improve the fitting result (see the DC and DC2 cases in arXiv:1701.06149). The convection effect and the constant low energy diffusion coefficient could not replace each other. Moreover, the R_0 is necessary for the DC model, in which δ are different for low and high energies.

- The authors use the Force Field approximation to model Solar Modulation. They should have taken into account a different Fisk Potential for protons and antiprotons. But their use of the Fisk Potential is misleading. They fit it in their B/C analysis while a recent analysis indicates that this parameter is well determined for the period during which AMS-02 has taken data. See table 2 of A&A 591 (2016) A94 by Alexandre Ghelfi et al. for instance. The values quoted in table I and table II of the manuscript do not coincide. They are also different from the value of 724 MV given in the paper by Ghelfi et al.

Reply: We did not give a clear description in the first version to this point. We have tried to fit the AMS-02 and ACE B/C data at the same time, since the AMS-02 data, which is above 0.5 GeV, can not constrain the Fisk potential well by itself. It is found that the Fisk Potential for B/C is dominated by the ACE data, as AMS-02 data has much higher energy. This is the reason why the potential in Table I and Table II are different. We have stressed this issue in the present version. In fact, for different data choices in Ghelfi et al., the potentials for proton would vary from 0.5 to 0.85. Such a range is quite coincide with our result of ϕ_p.

2) Hadronic Interaction Models – I have a few comments here beyond my main criticism.

- The authors do not fit the numerical packages which they use to the accelerator antiproton data. No goodness of fit or chi-squared is provided, whereas in the previous section, a MCMC analysis is performed for the cosmic ray propagation parameters.

- The numerical packages are used as is, without trying to understand which parameters could be tuned to adjust the antiproton data in the appropriate energy range. One is left with the impression that the authors have undertaken an exhaustive survey of the codes available on the market, paying little attention to the physics at stake.

Reply: These hadronic interaction numerical packages have been well developed and carefully tuned with all suitable collider data (other than just the antiproton data) by their authors, and they are not supposed to be re-tuned by users. The packages are maintained by the authors, with upgrading and tuning of parameters to new available data. All these packages are based on an effective field theory called the Gribov-Regge theory, which adopt a phenomenological object "Pomeron" to describe the basic hadronic interactions. The starting points of these packages are the same, while the differences come from the detailed approaches for the "Pomeron" and the tuning parameters. In our work, we aim at surveying the physical indications of the AMS-02 $\bar{p}/p$ result including the possible uncertainties from different strong interaction models. Tuning the parameters in these packages and studying the details of the models are certainly out of the purpose of this work.

3) The astrophysical prediction for the proton-to-antiproton data – The authors use codes which already fail to reproduce the CERN ISR antiproton data. This translates into the poor match between their predictions and the AMS-02 data as illustrated in figure 5. The authors then try to improve the situation by playing with an overall normalization factor $c_{\bar{p}}$ or with different Fisk Potentials for the protons and antiprotons. The best fit is provided by the Tan & Ng parameterization whereas all the codes fail except EPOS 1.99 for the DR2 and DC models.

Reply: Although the numerical packages can not exactly match the CERN ISR data in Fig. 3 at high x_{F}, the variance would not significantly affect the antiproton flux in the concerned energy region of our work, which is from tens of GeV to hundreds of GeV. In order to demonstrate this, we show the production cross sections of secondary antiprotons (plus antineutrons) for different energies of primary cosmic-ray protons in the laboratory system. For a comparison, we also add the results from the parameterization of di Mauro et al. in Fig. 3 of the manuscript and in Fig. [ISR_cross_section] of this reply. For $p_{\mathrm{proj}} = 1495.9$ GeV corresponding to the CERN ISR energy, from Fig. [ISR_cross_section] and Fig. [CR_cross_section], it can be found that the variance in the center-of-mass system with a high x_{F} is translated to that in the laboratory system below 4 GeV and near 1 TeV. Therefore, this variance would only affect the $\bar{p}/p$ ratio below several GeV and near 1 TeV, and does not affect our discussions on antiprotons around hundreds of GeV.

The normalization factor $c_{\bar{p}}$ is introduced because that we attempt to perform a best fit to the AMS-02 data. As can be seen in fig. [Antiproton_flux] that our results can be roughly consistent with the data even without the factor $c_{\bar{p}}$. This is the same as what have been shown in the works with the parameterization methods, such as fig. 9 of arXiv:1701.04866. We guess that if these parameterization methods of are used to fit to the AMS-02 data, a similar factor is also necessary. In fact, this factor has a clear physical meaning, and represents the large relative error of the C $\rightarrow$ B cross section, which is adopted to determine the propagation parameters.

4) Implications for Dark Matter Annihilation – My main concerns here are (i) the lack of a method used by the authors to derive constraints on Dark Matter and (ii) the fact that they derive bounds for configurations where the antiproton AMS-02 data are not reproduced by a secondary component.

- What kind of marginalization is performed ?

- If secondaries do not explain the AMS-02 data, is that because there is an exotic signal or because the high-energy packages are unable to reproduce correctly the antiproton production cross section ?

Reply: We derive the constraints on the dark matter annihilation cross section by a MCMC analysis and a Profile Likelihood method. We have added the corresponding description into the paper.

There could be three marginalization conditions:

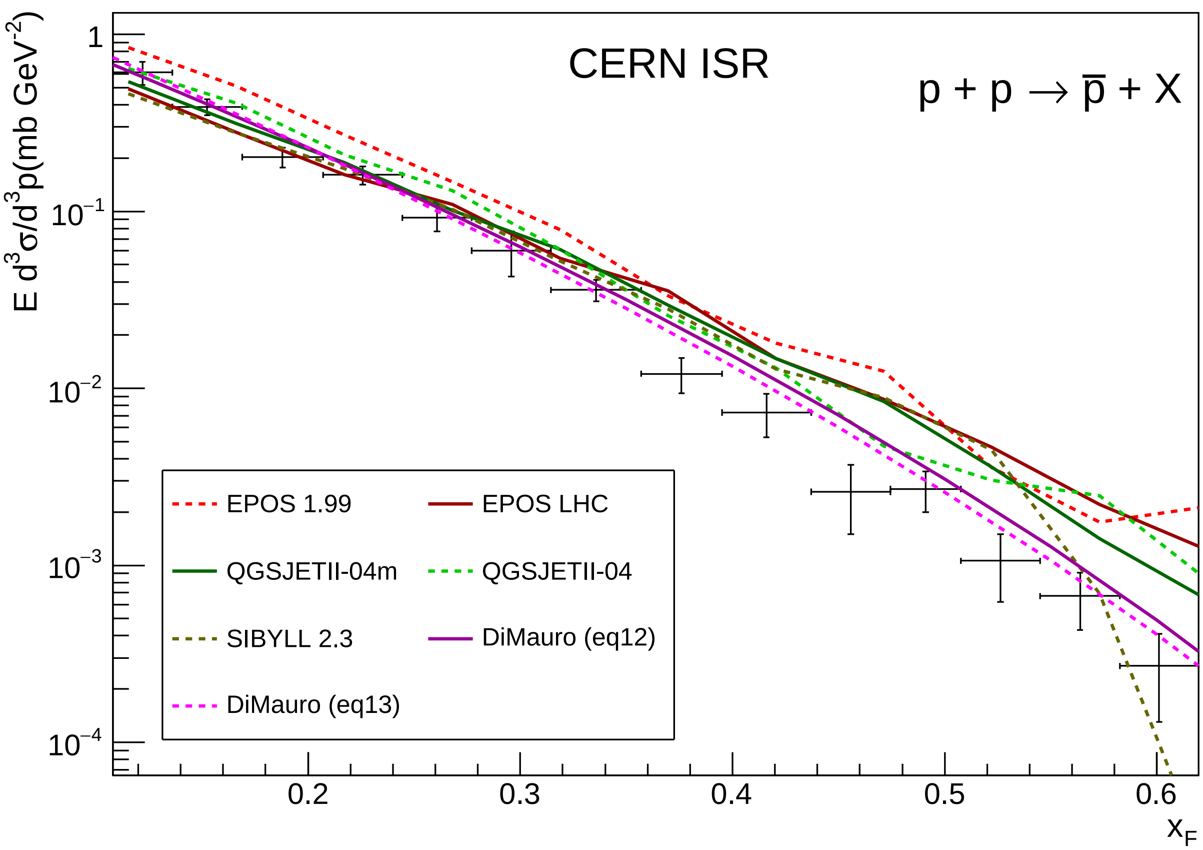

Figure 2: ISR_cross_section

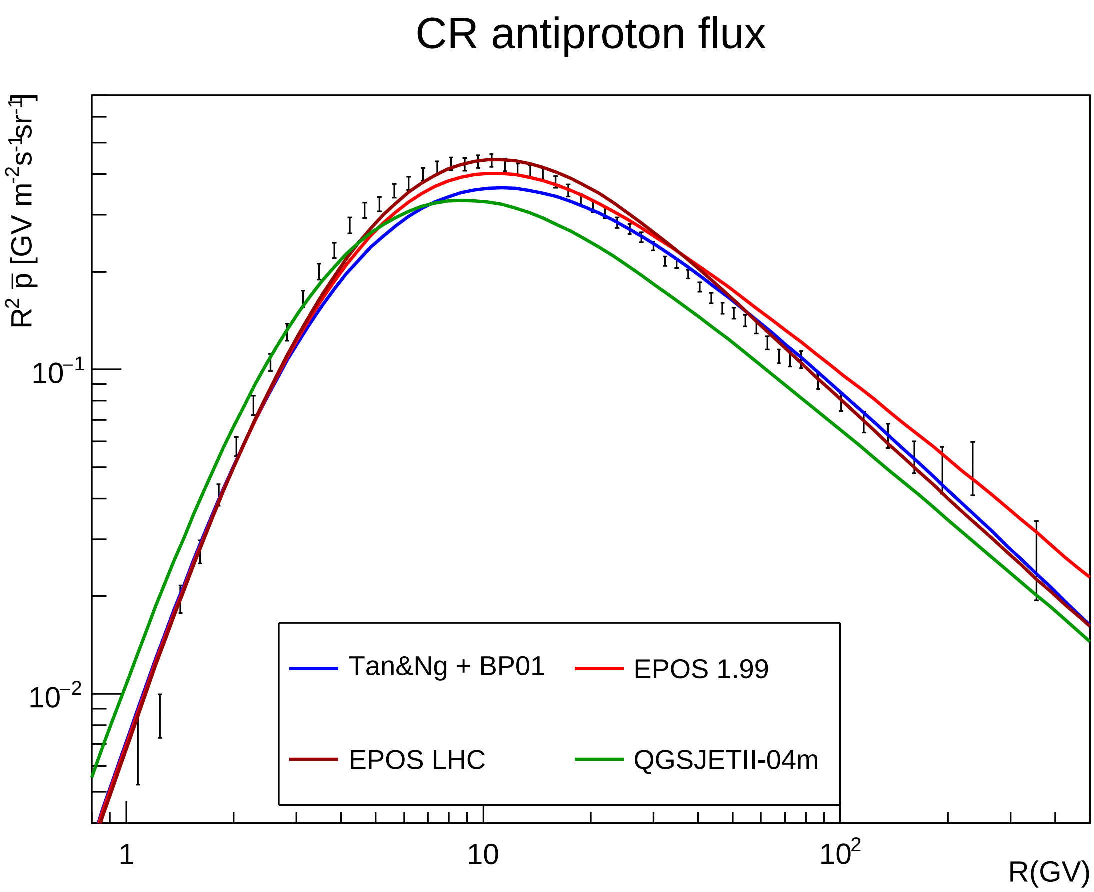

Figure 3: (Antiproton_flux) CR antiproton fluxes from different hadronic models for typical propagation parameters are shown. The differences between the Tan&Ng, EPOS 1.99 and EPOS LHC are comparable with the uncertainty shown in fig. 9 of arXiv:1701.04866

- We set 95% C.L. upper limits on $\langle\sigma v\rangle$ for fixed dark matter masses m_χ via a widely adopted Profile Likelihood method described in Nucl. Instrum. Meth. A551 (2005) 493-503 by Rolke et al. The 95% C.L. two-side boundary is where χ^2 varies from the best-fit value by a factor of 3.84.

- For a specified set of propagation parameters, we could derive the 95% C.L. upper limit described above. We selected a sample of the propagation parameter sets within the 95% confidence region derived from the MCMC analysis of the propagation. The 95% C.L. upper limits corresponding to the different propagation parameter sets would span a band in the $\langle\sigma v\rangle$-m_χ plane. We said this band indicates the propagation uncertainty at 95% C.L.

- If a dark matter signal is considered, we performed a MCMC analysis for m_χ, $\langle\sigma v\rangle$, and relevant parameters with the propagation parameters fixed at their best-fit values. The contours in Figs. 9 and 10 present the 68% confidence regions (projected into the $\langle\sigma v\rangle$-m_χ plane) in this MCMC analysis.

We think that the criterion for a correct hadronic model should be set by collider experiments. If a hadronic model can well fit the current collider data, it should be taken seriously. Therefore, we treat the difference between the hadronic models allowed by collider experiments as a theoretical uncertainty in predicting the $\bar{p}/p$ ratio. If the predictions based on some hadronic models are not consistent with the AMS-02 data, the possibility of the exotic contributions is worth studying. We do not think that whether the prediction can well fit the cosmic-ray data should be taken as a criterion for the hadronic interaction models.

> *As a conclusion, one is left with the overall impression that the work presented here is a very premature analysis which ought to be push much further and refined to reach the level required for a publication in the Physical Review.*
>
> *I therefore recommend rejection of the paper.*

Finally, we emphasize that the hadronic interaction models DO lead to an uncertainty in predicting the antiproton flux. This point has not been fully emphasized in previous studies. We firstly give such a survey of the hadronic interaction models, which are widely adopted in cosmic ray studies, for the explanation of the AMS-02 antiproton-to-proton ratio. We hope that the referee would find that the study helps to clarify this point and has some values.