We would like to express our gratitude to the referee for the appreciation of our work and for providing constructive comments on our manuscript. The valuable feedback has been used to improve the quality and clarity of our work. In response to the suggestions, we address below each of the points raised by the referee and describe the actions that have been taken in the revised version of the manuscript.

1. *Page 3, below (2.8). It is noted that nuclear corrections are not included when interpreting the neutrino structure function data in terms of proton PDFs. It would be good to provide some brief discussion of the size of these and the associated uncertainties.*

   In order to address the need for a comment regarding the sizes of the nuclear corrections in proton PDF fits, we have swapped the order of the last two sentences in that paragraph. The cited references, and the references therein, provide ample discussion regarding the the procedure to include nuclear corrections and their effects.

2. *Table 2.2. It is not clear to me at the point where these numbers are produced exactly how the cross section inputs corresponding to these numbers are calculated. So I think a reference forward to Section 2.5, where the theory settings are described, is needed. Although even then, for completeness giving the PDF set that is used and some uncertainty on the event rates here would be useful.*

   The numbers in Table 2.2 are computed by integrating the event yields shown in Eq. 2.12 in which the differential cross sections are computed using the central value of the PDF4LHC21 set. For completeness, we have added a sentence above Eq. (2.16) that mention the exact PDF set used to compute these numbers.

3. *Page 11. The discussion about consistency between the PDF set and theory settings used to produce the pseudodata and those entering the fit/profiling is in my view not correct or at least too strong. In particular, while it is perfectly reasonable to keep these the same there is definitely no requirement to, as is currently strongly implied in the discussion. In real PDF fits we often see that the fit quality for a given dataset does not follow textbook expectations, with $\chi^2/N \sim 1$. So some inconsistency between data and theory is often observed, rather than being artificial. Indeed, it is precisely because of this effect that tolerances (which are included in e.g. the PDF4LHC profiling) are included. In other words, one could perfectly reasonably generate pseudodata with a different PDF set, or different theory settings in order to emulate this inconsistency. One is free not to, but it should not be suggested that complete consistency is the only option here. It is a choice that is made, and not the only possible one.*

   . . .

4. *Page 15, and Fig. 3.3. Perhaps some explanation of why dV and to a lesser extend uV benefits from charge-lepton identification could be provided?*

   We have slightly rephrased the subsequent sentences to make it clear that the improvements seen in d_v and to a lesser extend u_v can be fully understood by looking at LO expressions of the structure functions in terms of the PDFs for ν and $\bar{\nu}$.

5. *Page 16, and Appendix A. The fact that the FASERν (and SNDLHC) projections lead to a very limited improvement on the PDF uncertainties is rather hidden in a paragraph here, and then in the appendix. In my view, this 'negative' result should be given more prominence. It after all motivates the improvements that might come with the FPF. I would suggest moving this to the main body of the text and starting with this as the first study.*

The main reason the study of the constraints provided by FASERν in the Appendix is because the study in Section 3 is mainly dedicated to the FPF experiments. We nevertheless agree with the referee such findings should be highlighted and used as a motivation for the FPF. In this regard, we have added two sentences in the introductory paragraph of Section to describe this.

6. *Page 22. I am rather unsure about the approach for presenting results here, and in particular in showing numbers without including systematic errors, which are described as being 'optimistic'. Having zero systematic uncertainties is surely unrealistic, rather than optimistic, so I feel as though a clearer justification for this needs to be given. Even more importantly, the labelling of the result without systematic uncertainties as 'FPF' and those with as 'FPF*' is surely the wrong way round, given it implies that the case where the systematic uncertainties are not included is the default in some sense. So these should be swapped, and the rationale behind showing numbers without systematic uncertainties accounted for more clearly presented.*

The referee is surely correct in that zero-systematic uncertainties is unrealistic. However, the reason to also show results with statistical uncertainties only is twofold. First, it substantiates the claim that FPF measurements will be statistically dominated. Second, in the ideal scenario (most optimistic) in which all the sources of systematic uncertainties are under control, the statistic-only case would be the limit in terms of effects. It is also worth noting that the estimation of the systematic uncertainties in our analysis is very much conservative.

Following the referee's suggestion, the results which only includes the statistical errors are labelled "FPF$\star$" while the one that also account for the systematics are labelled "FPF".

7. *Section 4 and elsewhere. Given these are HL-LHC projections, somewhere these should be compared with the HL-LHC PDFs of Ref [34]. This would surely be the fairer comparison, or in any case will give a clearer picture of where things may stand.*

The reason why we did not provide comparisons with the HL-LHC PDFs of ref. [34] is because the pseudodata used to determine these PDFs were produced with the PDF4LHC15 set which does not account for the recent LHC measurements. The most consistent comparisons would be to re-do the analysis done in ref. [34] and then compare the resulting PDFs with our determination.

We now address the comments of referee B

1. *Page 3, lines 34-36 column 2 "since in general the strange and charm ... not expected to vanish". I agree but I suggest adding a reference or a comment supporting this statement.*

We have added the following references which explore strange and charm PDF asymmetries Sufian:2018cpj, Sufian:2020coz

2. *p. 4 l. 30-35 col.2 "Also, to identify... event yields." Why a cut on the hadronic energy should properly simulate a cut on the number of charged tracks emerging from the interaction vertex?*

The charged track multiplicity is expected to grow with the hadronic system's invariant mass. We make this clear and cite a neutrino-Hydrogen interaction multiplicity study (Aachen-Bonn-CERN-Munich-Oxford:1981lfk) as well as a FASER paper (FASER:2019dxq) which also point this out. We also append this sentence:

"..., as the charged track multiplicity is expected to grow with W [?, ?]."

3. *Table 2.1 What is the meaning of the asterisk in "FLArE (*)"? I cannot see it referred to in the caption*

   The meaning of the asterisk was to be clear that this row represents 2 proposed detectors, as opposed to the remaining rows which are each one detector. We make this clear by appending the end of the caption:

   ". . . , which we denote for the two detectors as as FLArE(*)."

4. *p.5 l. 38 col.2 "is made of thin sensitive layers" What is the material/detector the layers are made of?*

   [MF: Not clear, Whitepaper says the same thing. Need to look closer or say that we don't know]

5. *p.5 l.49 col 1 and col 2 ("AdvSND" and "FLArE") Can you put a reference about these experiments? Did you take the info from ref [10]?*

   We have added the original flare proposal paper as well as the whitepaper (ref[10]) to the FLArE section. For AdvSND, we have added included the whitepaper

6. *p.6 l. 50-53 col.2 "Here we neglect efficiency .. simulation." I am aware that this might be difficult to simulate at this stage but I think this effect is non-negligible due to misidentification (a large background could enter this sample). Can you comment further on it? In addition, some guesses on misidentification capabilities can be drawn by the current FASER and SND@CERN capabilities.*

   Indeed, misidentification is not expected to be negligible. While there are no detailed studies on efficiencies, as we point out in the paper charm tagging can be done by through multiple methods, including reconstructing the topology of D-meson decays, and through dimuon events. With multiple techniques, one can draw the conclusion that that charm-tagging efficiency is not small. [MF: looking for quantitative perofrmances/efficiencies, can't find anything. Is there more we can say?

7. *p.9 l. 41-43 col.2 "We note that . . . in our estimation." (see also the conclusions p. 27 l. 52-55 col.1) I am puzzled by this statement. It is well known that flux systematics in Faser and SND@CERN play a prominent role and set the normalization (and shape?) of eqn 2.12. How can you ignore such an important effect in your analyses? Even if the PDF constraints are marginally affected by the normalization of the neutrino flux, some of your considerations about the on-axis (faser and faser2) versus the off-axis experiments (SND@CERN and its upgrades) may be affected by flux uncertainties. Can you comment on it in the paper?*

   Indeed, neutrino flux uncertainties are well established, and the shape and normalization of the flux can vary widely between different models. It is important to note that neutrino measurements actually constrain the product of flux and cross-section - each of these components brings an uncertainty with it, and a full analysis with real data would constrain them simultaneously. We wish to motivate this joint analysis by calculating the impact that FPF data can bring to a PDF fit. In this sense, by taking the flux to be known we can understand the full reach of FPF data on cross-section measurements, analogous to what was done in 2309.10417. Moreover, projections of FPF data on flux measurements has shown that HL-LHC data can bring flux uncertainties to a sub-percent level. We thank the referee for this important point and add the following paragraph after we introduce the flux model in the middle of section 2.3.

   "As pointed out in Ref. [?] there are notable neutrino flux uncertainties, as various event generators do not agree on the forward parent hadron spectra. The spread of the gener-

ators' predictions can be taken as a means of flux uncertainty, in which case there is a $\lesssim 50\%$ uncertainty on the interacting muon neutrino spectrum — if left unresolved this would be a significant systematic. Indeed, there are already projections of FPF measurements which would reduce this uncertainty [?] as well as efforts to describe the uncertainty in a data-driven way while improving the modelling of forward hadronization [?]. However, it is important to note that forward neutrino experiments actually constrain the product of flux and cross-section, and one must be assumed to measure the other. In a full analysis, they would be constrained simultaneously in a joint measurement. In our study, we aim to understand the full impact of FPF data on the PDF fit, thus motivating this future joint measurement. To this aim, we take the neutrino flux to be known and focus on the irreducible systematics associated with event reconstruction."

8. *p.11 l. 33-37 col.1 Are you sure that an assumption of f_{corr} = 0.5 is realistic? The estimates made in ref.[34] are for LHC experiments, not for fixed-target experiments like those of the FPF.*

9. *Fig. 2.4 In the bottom-left plot, I see a sharp increase in the fractional error at about x= 4 e-2. Why?* [MF: Probably a binning effect, will think about it.]

10. *p.19 l.50-53 col 1 and in general along the paper: I cannot see a discussion of the impact of neutral-current events at the FPF. Can you further comment on it?*

There are expected to be roughly as many neutral current neutrino scattering events as charged current events. However, due to the lack of information on both the incoming and outgoing neutrino, the full event cannot be reconstructed, only the total hadronic energy can be measured. One could try to use the total NC event rate as a means of constraining the integrated PDF, however we expect that this would negligibly improve the impact on the PDF constraints as compared to fully reconstructed CC events. [MF: Does this agree with others' understanding?]