Response to the Reviewer RC1

Please note that in the following response, the Review texts are quoted by »...«.

I much appreciate the critical, constructive comments by the present Reviewer. Yet, first, I have a major request for the clarification:

The Major Request to the Present Reviewer:

I read the following paragraph to be the main summary of the Review:

»the structure of the paper could be modified to improve the clarity of the discussion. In particular, some choices are not adequately explained. Adding some references between sections would be helpful to improve the coherence of the paper.«

Unfortunately, I cannot understand how and in what manner the "structure of the paper" must be modified. The Reviewer indeed tries to paraphrase the matter by the two sentences that follow. However, it is not clear how these issues to be reflected upon the "modification of the structure" of the paper. As far as I can tell, all these issues can be solved by elaborating texts at given places without modifying the structure of the paper.

Fortunately, this article is in interactive discussion phase. Thus, I would much appreciate it, if the Reviewer could elaborate on this comment online. This elaboration would be crucial for me to properly revise the manuscript by following the Reviewer's recommendation of the »major revision«, because this very issue is clearly the "major issue" to be addressed.

Summary of the Response:

I emphasize that the present Reviewer does not point to any defect in the methodology adopted in this study. Considering the originality of the methodology, thus, the present manuscript should be accepted for the publication. I frankly admit that the outcome from this methodology (so far) is not a total success. However, the results should be made public for the further common investigations for the progress, rather than bringing them back to my personal drawer.

Lead Paragraph:

I am glad to read that the present Reviewer has followed the »main message« of the present work very well, as seen in the summary given in the beginning. To quote in full:

»This paper investigates a method to predict the evolution of the parameters of assumed probability density functions (PDF), which is applied to different dynamical systems for which an exact solution of the Liouville equation is available. In this paper the method is extended to cases in which constraints are defined over subdomains, the distribution takes different forms in different subdomains and to multidimensional cases.«

Thus, it is rather difficult for me to understand what the Reviewer asks about the »main message« of the present study.

Furthermore, I find it rather unfortunate that the present Reviewer bluntly conclude that »the method fails«: I am more than happy to admit the working of this method is far from perfect. However, as emphasized in the concluding section (L547–550), the present study is attempting a difficult task, most likely next to impossible, of predicting evolution of a distribution only with a limited number of parameters, but in a consistent manner.

Here, the so–called assumed PDF approach already exists for a long time. Yet, this work is original in attempting to predict the evolution of a distribution in a self–consistent manner, and verify the performance taking simply dynamical systems. Such an effort does not exist in the literature, because the existing assumed–PDF schemes are developed case by case with *ad hoc* closures, without a generality. Thus, it is simply not possible to perform the verifications of those schemes by taking simple dynamical systems.

General comments:

In responding to the General Comments, I would first emphasize that the present work is a sequel to the first paper (YLP) under review to ACP, which is also available online. Thus, I've been assuming that any readers, including the Reviewers, would read this ACP paper first before reading this manuscript. For this reason, the introduction only presents the main issues in a very succinct manner, leaving the full discussions to the ACP paper. Especially extensive references are already found in the latter paper. This very simply point will be explicitly emphasized in the final manuscript.

It also follows that the »motivation« of the present paper must also be obvious: only the simplest application is presented in the first paper in ACP. Thus, it must naturally be tested more extensively.

In my own opinion, the abstract and the introduction are already presented in clear and succinct manners: the main result is an inherent difficulty of properly predicting the variances by using only a limited number of PDF parameters, as clearly stated in the abstract.

Yet, I also note that the present version is still too terse to attain even a minimum self–contained reading. Thus, in revision, some further elaborations will be attempted, some of them also remarked in the following.

The specific choices for the weights, σ_l, as well as the assumed PDF form follow the output–controlled distribution principle proposed in YLP. This point as well as the basic idea of this principle will be better emphasized in the final manuscript. See the response to L195–196 below for more.

Finally, the Reviewer asks me for »adding some references between sections«. Yet, to perform this modification, I would need to know what the Reviewer means by that more precisely.

Specific comments:

L4-6: The following sentence will be added to the abstract (if the word number limit permits) in revision for clarify the context better: "The general formulation

developed here is applicable to a wide range of the problems, including the frequency distributions of subgrid–scale variables, hydrometeor size distributions, as well as to probability distributions characterizing data uncertainties."

L8: "the common cause" will be replaced by "a common cause ... due to low dimensionality" in revision to be grammatically correct, and also for making it clear that an exact cause is not known, but also with an addition of the phrase suggesting the required key condition.

L17: The reference to Yano *et al.* (2018, BAMS) will be added here to allude to the weather forecasting, as suggested by the Reviewer, although this reference is already found in YLP.

L50: Meaning of σ_l: Its basic »meaning« must be clear from the phrase that introduces σ_l: "weighting it by σ_l". Thus, it is a weight. Eq. (2.1.7) and the following discussions should further clarify the »meaning« of σ_l. Please note that a more careful derivation and the discussions are found in Sec. 5.1 of YLP, as will be explicitly remarked in revision.

Here, please understand that the presentation here is extremely terse, because all these details are more carefully discussed already in YLP: the readers should refer to it to understand those details. I believe that this point is already implied in the manuscript (*e.g.*, L23, L35), but it will be made more explicit in revision both in the introduction as well as in the beginning of Sec. 2.

L104: A brief description of the output–controlled distribution principle proposed in YLP: the essence of this principle is already introduced in L55–56: "YLP suggest to choose those constraints to be the outputs that are required in a host model." In revision, this sentence will be immediately followed by "and call it the output–controlled distribution principle."

L109: As Eq. (2.2.3) demonstrates, when different constraints are introduced in two subdomains, different forms of distributions are predicted from the maximum entropy principle. Here, the two subdomains are separated by the sign of x (or ϕ), thus the two different distribution forms must be assumed over those two subdomains, as shown in the definition that immediately follows L109. These points will be made more explicit in revision. Here, I also realize that the original presentation was slightly out of order: in revision, the paragraph here will be re–structured to a better order.

Similar remarks will also be added in the beginning of Sec. 2.5 to suggest in what case we further need to introduced different distribution forms also depending on the sign of y. Again, note a slight disorder in presentation here, which will be amended in revision.

Section 3: Visualization of the distribution: here, I assume that the Reviewer is asking for a visualization of the distribution defined by Eq. (3.1). However, this is simply a two–dimensional Gaussian distribution, whose form must be widely known. In fact, the use of the multi–dimensional Gaussian distribution is fairly common in the assumed–PDF literature (*e.g.*, Golaz *et al.* 2002, Larson and Golaz 2005). For this reason, instead of visualizations, those references will be simply added in revision.

L195-196: Choice of σ: the weights, σ_l, are chosen as means and variances throughout the paper, because these are the simplest quantities required as outputs. This basic point will be remarked in an earlier part of the manuscript in revision in association with Eq. (2.1.7).

L255: Choice of σ: please refer to my response to L195–196 just above.

L293-294: A possible solution to improve the solution with an assumed PDF: the reason for the failure to capture the basic evolution of the distribution is discussed in the paragraph (L295–300) that immediately follows. Alternative distribution forms are considered in Secs. 4.2 and 4.3 in order to overcome this defect, as clearly stated in the beginning of each section (L305, L343).

L308: An integral over an infinite domain at each time step, as required, is substantially more expensive numerically than just predicting a few PDF parameters. Obviously a sum of far more than few points is required to obtain an integral with an acceptable accuracy. This computation cost, to be performed at every time step, is much more substantial compared to the cost for simply integrating only few parameters in time, as the present method is designed to do. This elaboration will be added in revision.

L311: "Minor disadvantage": based on the way that the Reviewer is asking the question, it is clear that the Reviewer understands a "disadvantage" that "a distribution can spread to $x < 0$": »the solution covers unphysical values«. Yet, this is only a "minor disadvantage" for the reason explained by the sentence that immediately follows (L312–313).

L348: Purpose of section 4.3: the purpose of Sec. 4.3 is to seek "an alternative possibility" *to Sec. 4.2*. In revision, this additional phrase will be added for a further clarity. This method was actually formulated explicitly, but not presented in detail, because as remarked, no substantial difference from the case with Sec. 4.2 was found. I believe it worthwhile to remark on this unsuccessful alternative attempt, even just briefly, because it is a very natural choice to try.

L540: »This study applies the assumed-PDF approach to dynamical systems for which it is possible to compute the solution of the Liouville equation, but the method generally fails to reproduce the exact solution. What would be the appropriate procedure for cases where the exact solution is not available?«: I do not know the answer to this question. However, it seems to me that it is more constructive to seek better assumed–PDF forms that works better for simple dynamical systems. Then, a similar approach can also be applied to more complex systems, where the direct verifications by the Liouville equation is not feasible.

L566-567: The sentence redundant?: The sentence of concern will be removed in revision.

Technical corrections:

L70: The superscript + in λ_l in the second integral: thank you for pointing out a typo here. The superscript + here will be corrected to − in revision.

Additional technical corrections suggested by the present Reviewer for L134, Caption in Figs. 3 and 4, L481, L527, L530, L532, L535, and L583 will be

performed in revision.