We are grateful to the two reviewers for their constructive and thoughtful comments (Stéphane Vannitsem and the anonymous reviewer) on our manuscript “New insights into decadal climate variability in the North Atlantic revealed by data-driven dynamical models”. We also thank Andrey Gritsun their support as editor. Our responses to reviewer comments are listed below. The requests are in black text and our replies in how we propose responding are in blue font.

Anonymous Reviewer #1

I thoroughly enjoyed reading this paper. The authors use quadratic regression in the spirit of Kravtsov et al. (2005) and a slew of model selection criteria to construct a three-variable representation of decadal coupled dynamics over the North Atlantic. They end up with the model that produces realistic continuous spectra and is able to forecast out-of-sample data at multi-year lead times. The algebraic structure of the model provides essential clues as to the dynamics of the observed climate variability over the North Atlantic region and guides the targeted data analysis to support the emerging hypotheses.

> We are grateful that the manuscript is regarded as an interesting read and offers promising insights.

A couple of personal first-reading impressions on presentation and content:

(1) section 3.1.2 and Fig. 3 are a bit in the way of paper's presentation flow, IMO (with the only message in ll. 308-310 about the key role of precipitation). I'd remove this section or put it in the appendix.

> Thank you for this comment. We do agree, although a novel part of all this analysis is how precipitation acts as a mediating variable, fully coupled and feeding back onto both AMV and NAO. The precipitation terms found by the equation discovery is robust and present in all potential model configurations as shown in Fig. 3. We accept that this does not come across well, as it has been reduced to the last lines 308-310 as reviewers suggested.
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> We intend to respond as follows:
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> - We will raise the emphasis of the role of precipitation and include introductory sentences explaining why we do the analysis shown in Fig. 3.
> - We will align this better to Fig. 3 explaining better its importance and features which were not discussed (e.g. The sudden occurrence of precipitation equations when models exceed 4 terms).
> - As suggested by the reviewer we will shorten and better focus the text of section 3.1.2.
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> We think these proposed amendments would improve the flow of the manuscript. Given the importance of the third variable of precipitation, we feel that if we can tighten the paper wording at this point, as requested by the

> reviewer, then we can keep in Fig. 3 to highlight the important implications of the robust precipitation cross terms while improving readability.

(2) section 3.2 and Fig. 5 suggest skill, but would benefit from including a few concrete numbers in text - and, maybe, comparison with a benchmark forecast (say, persistence, or three-variable LIM model).

> Thank you for this comment. We agree, and we will ensure the text around Fig. 5 quotes correlations. We will also compare the model to a persistence benchmark (we will choose a lead year range (e.g. LYR [1-1]) for persistence calculations to be compared directly to model skill in Fig. 5).

(3) Section 3.3.1 Algebraic structure of the model and the hypothesis of "damped oscillatory mode forced by the atmosphere", connections with linear inverse models, importance of the nonlinear feedbacks involving precipitation, etc. These are the issues the papers prompts (me) to think about. The key difference between the present approach and the previous data-driven approaches, I think, is the focus on a deterministic nonlinear model, rather than stochastic linear model. In my experience, the residual tendencies unexplained by the dynamical operators, say, on the right-hand side of (8–10) are typically large (maybe boxcar running-mean annual smoothing reduces them by a lot, maybe not). The apparent success of the deterministic model indicates that these residual dynamics are unimportant for the phenomenon of interest; at the same time, the "stochastic driving" is internally generated within the model. It is surprising to me that such low-order dynamics generate realistic spectra.

In any case, having the actual model (8-10) provides one with a tool to study the sources of the decadal variability and predictability in this model (by looking, for example, at the importance of various terms in the "dynamical" experiments with, say, suppressed, or one-way coupling between equations, just like it is done with more complex dynamical climate models, or by parameterizing further the model dynamics with linear operators and stochastic driving etc. etc.)

Once again, a very interesting paper!

> We thank you for this assessment. Having read this comment, we will add the following line in the discussion *“The advance we present here is a deterministic nonlinear model instead of traditional stochastic based descriptions”*. We will clarify this point explicitly in the revised manuscript.
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> Based on the comment above we will strengthen the discussion by adding text to emphasise that the advance presented here lies in identifying a low-order deterministic system whose nonlinear structure is able to reproduce key aspects of decadal variability, without the need for explicitly prescribed stochastic forcing. We will also expand the discussion of the role of unresolved processes. While residual tendencies are not explicitly modelled as stochastic noise in our framework, we acknowledge that such processes are present in the real system. Our results suggest that parts of the decadal variability can be

> captured through state-dependent nonlinear interactions. Part of what is often treated as stochastic variability in linear frameworks may instead be represented implicitly through deterministic nonlinear terms.
>
> We will clarify these points explicitly in the revised manuscript.
>
> Also based on the comment above, we are grateful because this has encouraged us to write a more focused discussion with proposed sentences such as *"While increases in computer power will open future possibilities of operating complex dynamical climate models across a large number of parameterisations, in the meantime we believe this data-driven equation discovery approach captures sufficient process representation and can be operated quickly."*

Stéphane Vannitsem Reviewer #2

The manuscript presents a very interesting work on the development of a low-order model to describe the coupled dynamics at monthly time scale between the Atlantic Multidecadal Variability (AMV), the North Atlantic Oscillation and the precipitation over the North Atlantic, the latter being used as a proxy for latent heat exchange and fresh water fluxes between the ocean and the atmosphere. The authors apply a discovery method to isolate the terms appearing on the right-hand side of the dynamical equations, up to quadratic terms. They found several interesting models that do present relevant variability for the North Atlantic. The most interesting model composed of 26 terms displays an erratic behaviour reminiscent of chaotic dynamics. A strong low-frequency variability is identified on time scales between 20 to 35 years. The latter range is reminiscent of the coupled ocean-atmosphere variability identified over this region using a reanalysis dataset (Vannitsem and Ghil, 2017). The manuscript is well written, easy to follow and provide important information on the coupled dynamics between the ocean and the atmosphere over the Atlantic region.

> We are grateful that the manuscript is regarded as an interesting read, easy to follow and provides important information on coupled ocean-atmosphere dynamics in the North Atlantic.
>
> Reading this comment it has encourage us to sharpen the description of the advances in the context of chaotic behaviour. We propose writing "*In this analysis we have used equation discovery to find the most appropriate models and it is of particular interest that most of the candidate equation sets are capable of exhibiting chaotic behaviour.*"
>
> We are pleased to cite (Vannitsem and Ghil, 2017) and we propose writing a few sentences which clarify its direct relevance to the findings in this paper.

A first aspect that is probably useful to mention/address is the role of processes/time scales that are not represented in the exploration, often described by noise. An example is of course the Linear Inverse Modelling (LIM) for which a stochastic noise is used to represent processes that are not described by the deterministic part of the model. In your forecasting setup, it would be nice to have a comparison with the LIM approach to see whether the identified nonlinearities provide a better representation of the dynamics, allowing for a better skill. This question is probably beyond the scope of the current manuscript but is at least worth to mention.

> Thank you for this suggestion. This makes a very valid point of extending our analysis. We agree that unresolved processes and timescales, often represented as stochastic forcing in Linear Inverse Modelling, are not explicitly included in our deterministic framework. Some unresolved processes may be implicitly captured through the nonlinear terms identified by the SINDy models. Robust nonlinear interaction terms suggest that part of the variability often treated as stochastic in linear frameworks may instead be represented as state-dependent deterministic interactions at these timescales. We agree that a direct comparison with a LIM would be very useful. Such a comparison would provide insight into whether the nonlinearities lead to improved predictive skill. We leave it as an important direction for future work.
>
> We will include these new comments above in the discussion/conclusion sections of the revised paper.

A second related aspect is to clarify what is the proportion of variability indeed represented by the dynamical model as compared with the observations. That could allow for making an estimation of the noise that should be added to the description to effectively represent the full variability. This characterization could also allow helping in selecting the best models.

> We thank you for this point. This encourages us to derive one single overall headline statistic, which is the percentage of variance (at a given lead time) explained with the best fitting model across different initial states. We will note its relevance as a potential model selection criterion.

Finally, precipitation is used as an observable of your system. This is indeed an interesting way to see the coupling between the ocean and the atmosphere, but this choice probably would not be my first choice of variables. One could also use both sensible and latent heat fluxes and also the freshwater flux that would allow for discriminating the role of the different variables. It would be nice to have a comment on this and the ability of the approach to handle additional variables and additional nonlinearities.

> Thank you for this point. We agree that precipitation is not the only possible choice of variable to represent air–sea coupling, and that using other quantities (e.g. latent and sensible heat fluxes, freshwater flux) could help discriminate the underlying physical processes.

We chose precipitation for two reasons: Firstly, it acts as an integrated proxy for multiple key processes (latent heat release associated with atmospheric convection, and freshwater fluxes into the ocean, which influence salinity and potentially the AMOC). In this sense, precipitation could capture both diabatic atmospheric processes and oceanic feedbacks. Secondly, precipitation is a useful variable from a climate impacts perspective, particularly European precipitation variability associated with the NAO.

We also note that our aim was to learn low-order, interpretable dynamical models. The SINDy algorithm can easily accommodate additional variables and additional/higher-order nonlinearities, provided there is sufficient data. Extending the framework to include explicit heat and freshwater flux terms would be a valuable direction for future work as suggested by the reviewer, as it would allow a more detailed separation of the mechanisms. However, a limitation would be that increasing the dimensionality of the system makes it challenging to obtain sparse and robust models from limited data.

We will include these new comments in the discussion/conclusion sections of the revised paper.