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An observation-driven time-dependent basis for a reduced description of transient stochastic systems

H. Babaee

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2nd revised submission:	17 October 2019
Final acceptance:	18 October 2019

Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Review History

RSPA-2018-0841.R0 (Original submission)

Review form: Referee 1

Is the manuscript an original and important contribution to its field?
No

Is the paper of sufficient general interest?
Yes

Is the overall quality of the paper suitable?
Yes

Quality of the paper
A paper that may be acceptable after major revision.

Can the paper be shortened without overall detriment to the main message?
No

Do you think some of the material would be more appropriate as an electronic appendix?
No

For papers with colour figures – is colour essential?
Not applicable

If there is supplementary material, is this adequate and clear?
No

Are there details of how to obtain materials and data, including any restrictions that may apply?
Not applicable

Do you have any ethical concerns with this paper?
No

Recommendation?
Reject – article is not of sufficient interest (we will consider a transfer to another journal)

Comments to the Author(s)
I was very confused trying to read this manuscript for I) its motivation and II) its original aspects. The main idea is to extract a time-dependent basis for the description of transient features in general dynamical systems. This problem has been considered in several studies recently.

Motivation:

The author relies on the hypothesis that there are plenty of observations, which is probably a very optimistic scenario for any realistic problem where we have no equations and need to go data-driven. Perhaps a more suitable setup is to consider an ergodic system and try to extract modes in this case. However, this is a much harder problem than the presented one as here the author has the "convenient luxury" of having data to the place he needs them… To this end, I find statements in the abstract and the main body of the paper like "In that sense, the presented reduction is purely observation-driven and may be applied to systems whose models are not known" overoptimistic.

Originality:

Nevertheless, given this hypothesis he obtains an equation to evolve a time-dependent basis that captures the most important directions in phase space. These are equations 2.11 and 2.12 which are supposed to be the main result of this work. It is important to emphasize that these equations are not new. These are the Dynamically Orthogonal (DO) equations (Sapsis & Lermusiaux, Physica D, 2009) (eq. 14 - 13).

Conclusions:

Overall, I find the paper to be a nice exercise demonstrating how one can utilize the DO basis equations in the case where an ensemble of simulations is available. I find this setup very restrictive and not realistic. The core equations are presented as new while they have been published in an identical form previously in several places and without proper citation. I suggest publication to a more focused journal after major revisions that will justify the motivation better and of course clarify the novel elements.

Review form: Referee 2

Is the manuscript an original and important contribution to its field?
Yes

Is the paper of sufficient general interest?
Yes

Is the overall quality of the paper suitable?
Yes

Quality of the paper
A good paper worth publishing in Proceedings.

Can the paper be shortened without overall detriment to the main message?
No

Do you think some of the material would be more appropriate as an electronic appendix?
No

For papers with colour figures – is colour essential?
Not applicable

If there is supplementary material, is this adequate and clear?
Not applicable

Are there details of how to obtain materials and data, including any restrictions that may apply?
Not applicable

Do you have any ethical concerns with this paper?
No

Recommendation?
Major revision is needed (please make suggestions in comments)

Comments to the Author(s)
The author presents an approach for constructing a time-dependent basis from observations of a dynamical system. The approach seems novel, interesting, and of practical value. I have the following comments:

First, the author focuses on representing data, i.e., solving the full dynamical-system model to obtain data (or take measurements) and projecting onto the dynamic space spanned by the dynamic modes. While this is an important task, another important task is deriving a dynamic reduced model that solves the full model in a space that dynamically changes over time. This has been investigated in the model reduction community since a while (dynamic reduced models, online adaptive model reduction, etc). It would be good to highlight somewhere in the introduction that the author focuses on reducing the dimension of given data by representing in a dynamic basis, in contrast to the model reduction that aims to solve in the corresponding dynamic reduced space.

Second, the presentation would be clearer if the author makes the dependence on t of the quantities explicit, i.e., U(t), Y(t). Especially, when it comes to the optimization problem (2.6) it would help to better understand which quantities change with time and which quantities are constant over time. The formulas are not so long that length would become a problem, in my opinion.

Third, the author proposes to find U (and Y) such that the derivative of U (and Y) in time match the derivative of T in time. Is this equivalent to optimizing for U (and Y) that match T (without the derivative in time) and then taking the derivative afterwards? Can the author comment on why the author chooses the first over the second approach?

Fourth, the author proposes a fourth-order time derivative. How does the error of the time derivative approximation affect the overall approach, e.g., the variance approximation in Figure 1? Is a fourth-order approximation necessary or would a first-order approximation work well too? Is the order of the time derivative implicitly defining a sliding window of how many time steps are considered (the higher the order, the further away observations are considered from the current time step in Step 4 of (c))? Please comment.

Fifth, I think an interesting comparison (especially in regard with Figure 8) would be to compare to a localized PCA in time. Compute the PCA modes for a sliding window and compare them to the modes obtained with the proposed approach. Does the approach allow more insights than a sliding window PCA? Are there any other comments that the author has with regarding to localized PCA and the proposed approach?

Decision letter (RSPA-2018-0841.R0)

07-Feb-2019

Dear Professor Babaee:

I am writing to inform you that your manuscript RSPA-2018-0841 entitled "An Observation-Driven Time-Dependent Basis for a Reduced Description of Transient Stochastic Systems" has been rejected in its present form for publication in Proceedings A.

The Editor has made this decision based on the advice of referees, and taking into account their own opinion of your paper. With this in mind we would like to invite a resubmission, provided the comments of the referees and any comments from the Editor are taken into account. This is not a provisional acceptance.

The resubmission will be treated as a new manuscript. Please note that resubmissions must be submitted within six months of the date of this email. In exceptional circumstances, extensions may be possible if agreed with the Editorial Office.

Please find below the comments made by the referees, not including confidential reports to the Editor, which I hope you will find useful. If you do choose to resubmit your manuscript, please include details of how you have responded to the comments, and the adjustments you have made.

Please note that we have a strict upper limit of 28 pages for each paper. Please endeavour to incorporate any revisions while keeping the paper within journal limits. Please note that page charges are made on all papers longer than 20 pages. If you cannot pay these charges you must reduce your paper to 20 pages before submitting your revision. Your paper has been

ESTIMATED to be 22 pages. We cannot proceed with typesetting your paper without your agreement to meet page charges in full should the paper exceed 20 pages when typeset. If you have any questions, please do get in touch.

To upload a resubmitted manuscript, log into http://mc.manuscriptcentral.com/prsa and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Resubmission." Please be sure to indicate that it is a resubmission, and ensure you enter this ID - RSPA-2018-0841 - as the previous submission number.

Yours sincerely

Alice Power
Publishing Editor
Proceedings A
proceedingsa@royalsociety.org

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)
I was very confused trying to read this manuscript for I) its motivation and II) its original aspects. The main idea is to extract a time-dependent basis for the description of transient features in general dynamical systems. This problem has been considered in several studies recently.

Motivation:

The author relies on the hypothesis that there are plenty of observations, which is probably a very optimistic scenario for any realistic problem where we have no equations and need to go data-driven. Perhaps a more suitable setup is to consider an ergodic system and try to extract modes in this case. However, this is a much harder problem than the presented one as here the author has the "convenient luxury" of having data to the place he needs them… To this end, I find statements in the abstract and the main body of the paper like "In that sense, the presented reduction is purely observation-driven and may be applied to systems whose models are not known" overoptimistic.

Originality:

Nevertheless, given this hypothesis he obtains an equation to evolve a time-dependent basis that captures the most important directions in phase space. These are equations 2.11 and 2.12 which are supposed to be the main result of this work. It is important to emphasize that these equations are not new. These are the Dynamically Orthogonal (DO) equations (Sapsis & Lermusiaux, Physica D, 2009) (eq. 14 - 13).

Conclusions:

Overall, I find the paper to be a nice exercise demonstrating how one can utilize the DO basis equations in the case where an ensemble of simulations is available. I find this setup very restrictive and not realistic. The core equations are presented as new while they have been published in an identical form previously in several places and without proper citation. I suggest publication to a more focused journal after major revisions that will justify the motivation better and of course clarify the novel elements.

Referee: 2

Comments to the Author(s)
The author presents an approach for constructing a time-dependent basis from observations of a dynamical system. The approach seems novel, interesting, and of practical value. I have the following comments:

First, the author focuses on representing data, i.e., solving the full dynamical-system model to obtain data (or take measurements) and projecting onto the dynamic space spanned by the dynamic modes. While this is an important task, another important task is deriving a dynamic reduced model that solves the full model in a space that dynamically changes over time. This has been investigated in the model reduction community since a while (dynamic reduced models, online adaptive model reduction, etc). It would be good to highlight somewhere in the introduction that the author focuses on reducing the dimension of given data by representing in a dynamic basis, in contrast to the model reduction that aims to solve in the corresponding dynamic reduced space.

Second, the presentation would be clearer if the author makes the dependence on t of the quantities explicit, i.e., U(t), Y(t). Especially, when it comes to the optimization problem (2.6) it would help to better understand which quantities change with time and which quantities are constant over time. The formulas are not so long that length would become a problem, in my opinion.

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Fourth, the author proposes a fourth-order time derivative. How does the error of the time derivative approximation affect the overall approach, e.g., the variance approximation in Figure 1? Is a fourth-order approximation necessary or would a first-order approximation work well too? Is the order of the time derivative implicitly defining a sliding window of how many time steps are considered (the higher the order, the further away observations are considered from the current time step in Step 4 of (c))? Please comment.

Fifth, I think an interesting comparison (especially in regard with Figure 8) would be to compare to a localized PCA in time. Compute the PCA modes for a sliding window and compare them to the modes obtained with the proposed approach. Does the approach allow more insights than a sliding window PCA? Are there any other comments that the author has with regarding to localized PCA and the proposed approach?

Author's Response to Decision Letter for (RSPA-2018-0841.R0)

See Appendices A & B.

RSPA-2019-0506.R0

Review form: Referee 1

Can the paper be shortened without overall detriment to the main message?
Yes

Do you think some of the material would be more appropriate as an electronic appendix?
No

For papers with colour figures – is colour essential?
Not applicable

If there is supplementary material, is this adequate and clear?
No

Are there details of how to obtain materials and data, including any restrictions that may apply?
Not applicable

Do you have any ethical concerns with this paper?
Yes

Recommendation?
Major revision is needed (please make suggestions in comments)

Comments to the Author(s)
Although the author has made an effort to address my remarks related to the motivation of this work, I still believe that more work is needed regarding the connection with previous work.

Regarding the motivation, the distinction between model reduction and reduced-order modeling is helpful. However, there are some controversial statements in the updated introduction. In particular, it is confusing to read that this work is for "systems whose underlying models are not known or suffer from physics deficiency due to multi-scale physics effects, for example, atmospheric sciences and weather forecasting, computational neuroscience and complex biological systems" while for all the problems shown the available data are not only plentiful but also carefully selected, in the sense that the are chosen with specific boundary and initial conditions. Of course, this is exactly the problem I emphasized in my previous report: this is not case for problems such as "complex biological systems where no models are available" and where data are associated with an ergodic trajectory that is typically sparsely sampled. So what data can be used in this case to apply the method if the only available data is with random initial conditions?

Regarding my second comment about originality: No effort has been made to establish the relationship of the present work (equations derived in section 2) with previous work related to DO/BO/OTD. As I pointed out the derived equations in section 2 and Appendix A, seem identical to those already published in the literature, but there is no single reference in section 2 and no discussion on how the derived equations connect to previous work. In this sense, I cannot recommend to publish something that has already been published, especially for this journal.

I am willing to see the paper one more time assuming the author has made an effort to fix these issues.

Review form: Referee 2

For papers with colour figures – is colour essential?
Not applicable

If there is supplementary material, is this adequate and clear?
No

Are there details of how to obtain materials and data, including any restrictions that may apply?
Not applicable

Do you have any ethical concerns with this paper?
No

Recommendation?
Accept as is

Comments to the Author(s)
The author has addressed all my comments.

Decision letter (RSPA-2019-0506.R0)

19-Sep-2019

Dear Professor Babaee

The Editor of Proceedings A has now received comments from referees on the above paper and would like you to revise it in accordance with their suggestions which can be found below (not including confidential reports to the Editor).

Please submit a copy of your revised paper within four weeks - if we do not hear from you within this time then it will be assumed that the paper has been withdrawn. In exceptional circumstances, extensions may be possible if agreed with the Editorial Office in advance.

Please note that it is the editorial policy of Proceedings A to offer authors one round of revision in which to address changes requested by referees. If the revisions are not considered satisfactory by the Editor, then the paper will be rejected, and not considered further for publication by the journal. In the event that the author chooses not to address a referee’s comments, and no scientific justification is included in their cover letter for this omission, it is at the discretion of the Editor whether to continue considering the manuscript.

In addition to addressing all of the reviewers' and editor's comments please also ensure that your revised manuscript contains the following sections before the reference list:

• Acknowledgements
• Funding statement

To revise your manuscript, log into http://mc.manuscriptcentral.com/prsa and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision.

You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript and upload a new version through your Author Centre.

When submitting your revised manuscript, you will be able to respond to the comments made by the referee(s) and upload a file "Response to Referees" in "Section 6 - File Upload". Please use this to document how you have responded to the comments, and the adjustments you have made. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the referee(s).

IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any unnecessary previous files before uploading your revised version.

When revising your paper please ensure that it remains under 28 pages long. In addition, any pages over 20 will be subject to a charge (£150 + VAT (where applicable) per page). Your paper has been ESTIMATED to be 24 pages.

Once again, thank you for submitting your manuscript to Proc. R. Soc. A and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Yours sincerely
Raminder Shergill
proceedingsa@royalsociety.org

on behalf of
Dr Jan Hesthaven
Board Member
Proceedings A

Reviewer(s)' Comments to Author:

Referee: 2

Comments to the Author(s)
The author has addressed all my comments.

Referee: 1

Comments to the Author(s)
Although the author has made an effort to address my remarks related to the motivation of this work, I still believe that more work is needed regarding the connection with previous work.

Regarding the motivation, the distinction between model reduction and reduced-order modeling is helpful. However, there are some controversial statements in the updated introduction. In particular, it is confusing to read that this work is for "systems whose underlying models are not known or suffer from physics deficiency due to multi-scale physics effects, for example, atmospheric sciences and weather forecasting, computational neuroscience and complex biological systems" while for all the problems shown the available data are not only plentiful but also carefully selected, in the sense that the are chosen with specific boundary and initial conditions. Of course, this is exactly the problem I emphasized in my previous report: this is not case for problems such as "complex biological systems where no models are available" and where data are associated with an ergodic trajectory that is typically sparsely sampled. So what data can be used in this case to apply the method if the only available data is with random initial conditions?

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I am willing to see the paper one more time assuming the author has made an effort to fix these issues.

Author's Response to Decision Letter for (RSPA-2019-0506.R0)

See Appendix C.

RSPA-2019-0506.R2 (Revision)

Review form: Referee 1

For papers with colour figures – is colour essential?
Not applicable

If there is supplementary material, is this adequate and clear?
No

Are there details of how to obtain materials and data, including any restrictions that may apply?
Not applicable

Do you have any ethical concerns with this paper?
No

Recommendation?
Accept as is

Comments to the Author(s)
The paper can be accepted as is

Decision letter (RSPA-2019-0506.R1)

18-Oct-2019

Dear Professor Babaee

I am pleased to inform you that your manuscript entitled "An Observation-Driven Time-Dependent Basis for a Reduced Description of Transient Stochastic Systems" has been accepted in its final form for publication in Proceedings A.

Our Production Office will be in contact with you in due course. You can expect to receive a proof of your article soon. Please contact the office to let us know if you are likely to be away from e-mail in the near future. If you do not notify us and comments are not received within 5 days of sending the proof, we may publish the paper as it stands.

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If you wish to opt for open access then please inform the editorial office (proceedingsa@royalsociety.org) as soon as possible.

Your article has been estimated as being 24 pages long. Our Production Office will inform you of the exact length at the proof stage.

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You can cite the article in advance of publication using its DOI. The DOI will take the form: 10.1098/rspa.XXXX.YYYY, where XXXX and YYYY are the last 8 digits of your manuscript number (eg. if your manuscript number is RSPA-2017-1234 the DOI would be 10.1098/rspa.2017.1234).

For tips on promoting your accepted paper see our blog post:
https://blogs.royalsociety.org/publishing/promoting-your-latest-paper-and-tracking-your-results/

On behalf of the Editor of Proceedings A, we look forward to your continued contributions to the Journal.

Sincerely,

Raminder Shergill
proceedingsa@royalsociety.org

on behalf of
Dr Jan Hesthaven
Board Member
Proceedings A

Appendix A

Reviewer 1:
We thank the reviewer for the insightful comments. His/her comments are reproduced below in regular font and our point-by-point reply is in a blue font.

I was very confused trying to read this manuscript for I) its motivation and II) its original aspects. The main idea is to extract a time-dependent basis for the description of transient features in general dynamical systems. This problem has been considered in several studies recently.

1. Motivation: The author relies on the hypothesis that there are plenty of observations, which is probably a very optimistic scenario for any realistic problem where we have no equations and need to go data-driven. Perhaps a more suitable setup is to consider an ergodic system and try to extract modes in this case. However, this is a much harder problem than the presented one as here the author has the convenient luxury of having data to the place he needs them. To this end, I find statements in the abstract and the main body of the paper like "In that sense, the presented reduction is purely observation-driven and may be applied to systems whose models are not known" overoptimistic.

   While I agree with the reviewer that for many applications acquiring high-fidelity data is cost prohibitive, it is also true that with the advent of extreme scale computing and million-core computing paradigm, concurrent simulations will be routinely possible. Please see reference [21] in the manuscript. In this new paradigm, where the HPC bottleneck is shifting from computing to I/O restrictions, there is a need for scalable data analysis method that can analyze and extract information in an *in-situ* setting. Please see the recent report by the US Department of Energy[1]. The presented method here is one such method. On the other hand, solving the DO equations is also cost prohibitive for many applications, as it requires solving complex system of coupled stochastic ODE/PDE that requires significantly higher resolution compared to the original model. A clarification is added to the manuscript in the Introduction to better motivate the presented method.

2. Originality: Nevertheless, given this hypothesis he obtains an equation to evolve a time-dependent basis that captures the most important directions in phase space. These are equations 2.11 and 2.12 which are supposed to be the main result of this work. It is important to emphasize that these equations are not new. These are the Dynamically Orthogonal (DO) equations (Sapsis Lermusiaux, Physica D, 2009) (eq. 14 - 13).

   The main distinction between the presented method and DO is that DO is *model driven*, while the presented method is *data driven*. This distinction is significant, because: (1) model-dependence of the DO equation prohibits their utility in complex multi-physics/multi-component systems as it would require derivation of interface boundary conditions for the DO modes. For example, in the case of a seemingly simple problem

[1] ASCR Workshop on In Situ Data Management: `https://science.osti.gov/ascr/Community-Resources/Program-Documents`

of UQ of a random boundary condition in a single-domain/single-physics problem, it is still unknown how to determine the boundary conditions for the DO modes in a principled manner. However, this issue is not present in the DB reduction as the DB modes pick up their optimal value at the boundary from the data. The last example in the manuscript is a demonstration of a problem with random boundary condition. (2) The DO formulation requires a case-by-case derivation/implementation, which can be a very time-consuming undertaking. However, the author wrote a (50-line) MATLAB code for the evolution of the dynamic basis and it has used the same program for all of the demonstration examples in the manuscript. This can significantly facilitate the utilization of the presented method to the scientific community.(3) The reliance on the model becomes particularly restrictive for systems whose underlying models are not known or suffer from physics deficiency due to multi-scale physics effects, for example, atmospheric sciences and weather forecasting, computational neuroscience and complex biological systems. These distinctions are updated in the Introduction and a new demonstration case has been added to the manuscript in §3(b) that compares and contrast DO and DB.

3. Conclusions: Overall, I find the paper to be a nice exercise demonstrating how one can utilize the DO basis equations in the case where an ensemble of simulations is available. I find this setup very restrictive and not realistic. The core equations are presented as new while they have been published in an identical form previously in several places and without proper citation. I suggest publication to a more focused journal after major revisions that will justify the motivation better and of course clarify the novel elements.

   I have added a new section to the manuscript and have addressed the reviewers comments in the previous parts.

Appendix B

Reviewer 2:
We thank the reviewer for the insightful comments. His/her comments are reproduced below in regular font and our point-by-point reply is in a blue font.

The author presents an approach for constructing a time-dependent basis from observations of a dynamical system. The approach seems novel, interesting, and of practical value. I have the following comments:

1. First, the author focuses on representing data, i.e., solving the full dynamical-system model to obtain data (or take measurements) and projecting onto the dynamic space spanned by the dynamic modes. While this is an important task, another important task is deriving a dynamic reduced model that solves the full model in a space that dynamically changes over time. This has been investigated in the model reduction community since a while (dynamic reduced models, online adaptive model reduction, etc). It would be good to highlight somewhere in the introduction that the author focuses on reducing the dimension of given data by representing in a dynamic basis, in contrast to the model reduction that aims to solve in the corresponding dynamic reduced space.

   A statement to clarify this point is added in the paragraph of the Introduction.

2. Second, the presentation would be clearer if the author makes the dependence on t of the quantities explicit, i.e., U(t), Y(t). Especially, when it comes to the optimization problem (2.6) it would help to better understand which quantities change with time and which quantities are constant over time. The formulas are not so long that length would become a problem, in my opinion.

   Explicit dependence on (t) is added to the notation.

3. Third, the author proposes to find U (and Y) such that the derivative of U (and Y) in time match the derivative of T in time. Is this equivalent to optimizing for U (and Y) that match T (without the derivative in time) and then taking the derivative afterwards? Can the author comment on why the author chooses the first over the second approach?

   If the optimization problem were formulated versus $\mathbf{U}$ and $\mathbf{Y}$ as control variables instead of $\dot{\mathbf{U}}$ and $\dot{\mathbf{Y}}$, it would have resulted in the classic Karhunen-Loève decomposition. However, our goal in this manuscript is not to build a reduction of $\mathbf{T}(t)$ that is optimal in the second-norm sense, but a reduction that results in an optimal *update* in the low-rank subspace and its coefficients given the change in the data, i.e. $\dot{\mathbf{T}}$. I have added a new demonstration problem to the manuscript that compares the DB reduction versus KL decomposition. It is shown that under linear dynamics, i.e. small stochastic perturbation, DB and KL are almost identical. However, this is not in general true, for example for the highly nonlinear case, as shown in Case II in §3(b). Please see Figures (3)(c) and (3)(d). Formulating the optimization problem versus $\dot{\mathbf{U}}$ and $\dot{\mathbf{Y}}$ has also a computational advantage, in that the evolution equation of $\mathbf{U}$ and $\mathbf{Y}$ results in a *scalable* method with linear computational complexity with respect to number of observation points, number of samples and reduction size as opposed to solving an eigenvalue problem with quadratic/cubic computational complexities, depending how it is formulated. Moreover, KL and DB produce similar reductions only in the linear regimes, as demonstrated in §3(b).

4. Fourth, the author proposes a fourth-order time derivative. How does the error of the time derivative approximation affect the overall approach, e.g., the variance approximation in Figure 1? Is a fourth-order approximation necessary or would a first-order approximation work well too? Is the order of the time derivative implicitly defining a sliding window of how many time steps are considered (the higher the order, the further away observations are considered from the current time step in Step 4 of (c))? Please comment.

   In summary, the accuracy of computing time derivative of the data, $\mathbf{T}(t)$, and the time accuracy of solving the DB evolution equation are very important, especially when Δt between the data snapshots is large. I have investigated this in the last demonstration example in the manuscript. Please see Figure 10(b) and the description in the text in §3(d).

5. Fifth, I think an interesting comparison (especially in regard with Figure 8) would be to compare to a localized PCA in time. Compute the PCA modes for a sliding window and compare them to the modes obtained with the proposed approach. Does the approach allow more insights than a sliding window PCA? Are there any other comments that the author has with regarding to localized PCA and the proposed approach?

   The localized PCA in time is equivalent to the Karhunen-Loève (KL) decomposition, and I have addressed this in my response to Comment 3. A new demonstration case is added that compares the KL reduction to DB in §3(b).

Appendix C

Reviewer 1:
I thank the reviewer for the insightful comments and I am very sorry that I did not clearly explain the differences between this work and DO. The reviewer's comments are reproduced below in regular font and our point-by-point reply is in a blue font.

Although the author has made an effort to address my remarks related to the motivation of this work, I still believe that more work is needed regarding the connection with previous work.

1. Regarding the motivation, the distinction between model reduction and reduced-order modeling is helpful. However, there are some controversial statements in the updated introduction. In particular, it is confusing to read that this work is for "systems whose underlying models are not known or suffer from physics deficiency due to multi-scale physics effects, for example, atmospheric sciences and weather forecasting, computational neuroscience and complex biological systems" while for all the problems shown the available data are not only plentiful but also carefully selected, in the sense that the are chosen with specific boundary and initial conditions. Of course, this is exactly the problem I emphasized in my previous report: this is not case for problems such as "complex biological systems where no models are available" and where data are associated with an ergodic trajectory that is typically sparsely sampled. So what data can be used in this case to apply the method if the only available data is with random initial conditions?

   Data source: As discussed in my previous response, in the era of extreme-scale computing performing concurrent simulations will be routinely possible and there is an urgent need for scalable data analysis methods – in which dimension reduction is one such powerful tool. In fact the author is currently collaborating with the DOE Sandia national lab (Combustion Research Facility) where DNS of combustion is performed concurrently for random reaction coefficients and the dynamic basis is used to study the effect of uncertainty in these coefficients in predicting ignition/extinction (highly transient and finite-time phenomena). For these applications it is extremely difficult to even develop model-driven DO/BO equations due to the very complex flow and detailed chemistry equations, let alone develop stable high-fidelity numerical solvers for the resulting equations. Finally, the presented method can be applied to black box/commercial solvers. The current method can be applied to systems where high-fidelity data from multiple sources can be obtained, for example sea surface temperature measurements from multiple high resolution satellites, or for example, by running the experiments multiple times or as the reviewer suggested replacing the expectation operator with time-average operator for an ergodic random process. Since I have not studies those problems and to avoid confusion I have removed those statements from the Introduction.

   Data sampling: In almost all examples error analysis is carried out, which requires accurate quadrature sampling, for example probabilistic collocation method. However, for demonstration Case (c), part 2, Monte Carlo samples (not so carefully selected) for dimension reduction of high-dimensional problems. Also, demonstration Case (d) is a problem with random boundary condition and deterministic initial condition.

2. Regarding my second comment about originality: No effort has been made to establish the relationship of the present work (equations derived in section 2) with previous work related to DO/BO/OTD. As I pointed out the derived equations in section 2 and Appendix A, seem identical to those already published in the literature, but there is no single reference in section 2 and no discussion on how the derived equations connect to previous work. In this sense, I cannot recommend to publish something that has already been published, especially for this journal.

   Comparison of DB & DO: In fact, in my response to the referee's first review, I had added a new demonstration case to the manuscript (Case (b)), in which the differences between the DB and DO are discussed. I apologize that I was not clear in my response. BO is equivalent to DO and therefore, not discussed. OTD is for deterministic dynamical systems it is equivalent to DO in the linear perturbation regimes. For the proof, please see my papers (Appendix A of reference [14] and Theorem 2.4 in reference [18]). I have added this description to the manuscript in Section 3(c). Following your advice I have also directly compared and contrast DB and DO in Section 2.

   New demonstration case: I have also added another new demonstration case to highlight what distinguishes DB from DO. Please see Section 3(d).

   Theoretical contribution: In this paper a variational principle is presented (for the first time to my knowledge) that allows a rigorous derivation of reductions with time dependent subspaces for stochastic systems. The DO/BO decompositions, so far, lack such a framework. The variational principle is an important contribution of this manuscript as it can guide deriving new time-dependent reduction schemes in a principled manner by manipulating the functional given by Equation 2.4.

I am willing to see the paper one more time assuming the author has made an effort to fix these issues.