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Discussions
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Interactive comment on "Estimating the State of a Geophysical System with Sparse Observations: Time Delay Methods to Achieve Accurate Initial States for Prediction" *by* Z. An et al.

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Overview evaluation:

Techniques for dealing with a sparse observational networks are critically important, particularly for ocean and climate reanalyses that attempt to reconstruct the past state of the Earth system (e.g. Compo et al., 2011; http://www.esrl.noaa.gov/psd/data/gridded/data.20thC_ReanV2.html). The experiment scenarios described here by the authors are perhaps most applicable to the estimation of the global ocean state after the introduction of satellite altimeters, e.g. TOPEX/Poseidon in late 1992 (https://sealevel.jpl.nasa.gov/missions/topex/), with their final set of experiments having a potential application to leverage data from the Global

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Drifter Program (http://www.aoml.noaa.gov/phod/dac/index.php). Thus from a practical point of view, the time-delay method has potential merit for operational scale data assimilation (DA) and reanalysis.

Because of such potential, the authors should give a bit more explicit description about how these ideas compare to common methods like 4DVar or the 4D Ensemble Kalman Filter (EnKF), both of which utilize observations over an extended time window. The authors could give a more thorough depiction of how their ideas could be incorporated in these existing systems in order to facilitate a higher likelihood that an operational center might adopt the approach.

The sea surface height is closely connected to the near surface currents via the geostrophic balance, particularly in midlatitudes. Thus it is expected that unobserved currents would be well constrained by proper estimation of the surface height. For example, sea surface heights and sea surface winds are used to construct an estimate of ocean surface currents for the OSCAR product (http://www.oscar.noaa.gov/index.html). However, the examples given by the authors could perhaps be described as a supplement for the tropical region where this relationship breaks down. For future work, a natural extension would be to address a slightly more sophisticated example consisting of multiple vertical layers and the modeling of the temperature and salinity components of the density. This experiment would give a better test of estimating unobserved variables. For example, observing only temperature while estimating salinity is a challenging problem for ocean reanalyses before the Argo era.

A brief statement could be made about the applicability of the time-delay approach, for example, to the tropical observing system of moored buoys (TAO/TRITON). These are stationary sensors generating data about once every 10 minutes, but the majority of this data is not used in DA because most global scale ocean assimilation systems use analysis cycles that span multiple days. Even a coupled ocean/atmosphere DA system cycling every 6 hours could benefit from better use of this data. I suggest investigating the TPOS-2020 (Tropical Pacific Observing System) effort for the potential to inform the future development of this and other observing systems (http://tpos2020.org).

A weakness in the chosen experiments scenarios that should be acknowledged is that the approach has not been tested on time-delay observations with errors that are correlated in the time dimension. This is particularly important in ocean DA because errors of representativeness often dominate (versus instrument errors). I suggest an experiment, perhaps for future work, in which you run 2 model resolutions. The high resolution run is treated as 'truth', from which observations are drawn. The low resolution model is what you are synchronizing via DA. Set up appropriately, this should give you 'natural' errors of representativeness in the observations that may be correlated in time with the errors of future or past observations. Does the time-delay method still work effectively in this experiment scenario?

The time-delay method is described in comparison to nudging as a baseline. I would like to see the authors compare a simple 4DVar to the time-delay method as well (via experiment) to give context into how their method compares to a more state-of-the-art DA. It seems that the time-delay information for the observations and model state applied with what is essentially a diagonal coupling term emulates a similar effect as the cross-covariances that would in effect apply a non-diagonal coupling term to the innovations computed at different times throughout the window. The authors should discuss how the off-diagonal coupling used in most operational DA relates to the diagonal coupling with time-delay observations used here.

The impact of observation error on synchronization via the nudging approach is not addressed very thoroughly. I'd like to see some evaluation of the sensitivity to observation error in the assessment of the method. The authors should describe how their method is impacted by outliers in the observed data. Is the method sensitive to such outliers? I'd like to see an example.

General Technical Corrections:

The manuscript would benefit from more effort in bridging the nonlinear dynamics synchronization terminology with standard data assimilation terminology. This would particularly benefit the readership of this journal.

There is an odd mixing of tenses throughout the citations, and the inconsistency is distracting. The focus should be on the work presented here. Example substitutions are given in the specific comments below. Also, if the author name in the citation is not used as a part of the sentence, please put it in parentheses.

The discussion of 4DVar from the conclusion section would be much more useful either in the Introduction or in its own section right after the introduction.

The conclusion section is disorganized and needs attention. It should not introduce new concepts in any great detail - instead those should be moved to the body of the paper. Page 14, Lines 24 through the end seem to be a good start to the conclusion section, I advise starting there and reorganizing the rest.

The figures are insufficient. I would like to see a graphical example of the 2-Dimensional domain used for the experiments, including the spatial scale of the layer heights and velocity field. For the drifter experiments, the initial locations for the various experiments should be plotted. Overall, better use of visuals/graphics would greatly enhance the communication.

Specific Technical Corrections:

Page 1:

Abstract:

Line 1:

"The data assimilation process, in which observational data is used to estimate the states and parameters of a dynamical model,"

This is a narrow definition of modern data assimilation. Perhaps reword to indicate that this is one application of data assimilation.

Line 4:

“Since this problem of insufficient measurements is typical across many fields, including numerical weather prediction,”

It’s not clear in what manner the authors feel the measurements are insufficient. A large number of atmospheric measurements are often discarded in operational DA. Is it the quantity or type of observations in the atmosphere that is lacking? The sparsity of observations is certainly a challenge in ocean DA, but this must be considered relative to the timescales and spatial scales of interest. Could the authors please clarify.

Line 5:

“introduced in Rey et al (2014a, b)”

Change to: “introduced [by] Rey et al.”

Line 8:

“For instance, in Whartenby et al (2013) we found that to achieve this goal, standard nudging requires observing approximately 70% of the full set of state variables. Using time delays, this number can be reduced to about 33%, and even further if Lagrangian drifter information is also incorporated.”

Change to:

“While Whartenby et al (2013) we found that standard nudging requires observing approximately 70% of the full set of state variables, we find that this number can be reduced to about 33% using time delays, and even further if Lagrangian drifter information is also incorporated.”

While I understand the comparison to nudging as a baseline/control methodology, could you give an idea of what proportion of state variables must be observed for a more sophisticated DA scheme like 4DVar or an EnKF?

Introduction:

Line 12:

“The ability to forecast the complex behavior of the earth’s coupled ocean, atmosphere system lies at the core of modern numerical weather prediction (NWP) efforts.”

This is true, but the current drive amongst many centers is modeling and forecasting the entire integrated Earth System, including for example atmosphere, ocean, sea-ice, land, aerosol, surface waves, ionosphere, etc. I would suggest either saying “the coupled earth system”, or further specifying examples beyond simply atmosphere/ocean coupling which has been conducted at some centers for many years.

Line 14:

“the observations are completed”

I’m not quite sure what it means to ‘complete’ an observation. Perhaps this could be reworded.

Line 15:

“The latter is indispensable, even if one has a perfect model, as the accuracy of the prediction is crucially determined by the quality of the estimated initial state values: if the state of the model at the end of observation window is inaccurate, the forecasts will be undependable.”

It is my preference to reduce the usage of emphatic adjectives in scientific writings. A possible alternative:

“Even with a perfect model, the accuracy of the prediction is dependent on the quality of the estimated initial state values. Such sensitive dependence on initial conditions was identified by Lorenz (1963; JAS).”

Line 20:

"In our earlier work Whartenby et al (2013) we showed that"

Change to:

"Whartenby et al. (2013) showed that"

Line 23:

I don't think you've specified yet what the 3 dynamical variables are. Even though it is implied by the use of the nonlinear shallow water model, I think you should be explicit.

Page 2:

Line 2:

When the authors mention the use of drifters, does that mean that they are using the position information of the drifters, or simply surface measurements of the model state? Please clarify here.

Line 3:

It is not specified whether the spatial distribution of the observations impacts the estimate of L_s. How does this estimate change when all observations are concentrated in one area, or if the observations are arranging in vertical stripes (e.g. like the TAO/TRITON array), or if they are evenly distributed (which can create aliasing artifacts in the analysis) versus randomly distributed.

There is also ambiguity about whether the state variables are reduced in number only, or in type as well. For example, a collection of global Argo profiles could be cut in half by either (a) reducing the number of floats by half, or (b) eliminating all salinity measurements and keeping only temperature. Both reduce the obs by 50%, but will have drastically different impacts. I'd ask the authors to please be more specific in how the observations are reduced.

Line 6:

"those at ECMWF Cardinali (2013)"

Change to:

"those at ECMWF (Cardinali, 2013)"

Line 7:

"Evensen (2008); Bennett (1992) should be expected"

Change to:

"(Bennett, 1992; Evensen, 2008) should be expected"

Why is Evensen 2008 ("Data Assimilation, The Ensemble Kalman Filter") cited for 4DVar? I'm sure more appropriate references could be used.

Some references that come to mind, among many others are:

Lewis and Derber, 1985: The use of adjoint equations to solve a variational adjustment problem with advective constraint.

Coutier and Talagrand, 1990: Variational assimilation of meteorological observations with the direct and adjoint shallow water equations.

Zupanski, 1997: A general weak constraint applicable to operational 4DVar data assimilation systems.

Line 7:

"While we do not discuss it here in detail, the same results for a 4DVar assimilation method Evensen (2008); Bennett (1992) should be expected."

To which same results are you referring? It is not clear. Do you expect to get all of the same results with 4DVar that you get with nudging? If so, that assertion requires some justification.

Line 11:

"The results appear to be equivalent."

To whom? This statement is ambiguous. Please state clearly whether you found this to be the case in your own research, and if this is part of your research findings, briefly present those results. Otherwise, please list citations of research showing this to be the case. Or, simply remove the statement.

Line 13:

"substantial improvement in reducing the number of observations at each measurement time that we report here."

I think you mean "reducing the number of observations needed to attain comparable accuracy in the forecasts or analysis." Please reword.

Page 3:

Line 1:

Please be careful to acknowledge that this is one application of data assimilation. Model error has many sources. Model parameter estimation can only address a small subset of the many sources of model error.

"In data assimilation we seek to use the information in observations to determine properties of a model that describes the dynamics producing those observations. These properties include unknown parameters as well as the time dependence of unobserved state variables. The model acts as a nonlinear filter coupling the observed states to the parameters and unobserved states."

Perhaps change to:

"We seek to use observations to inform a model of the dynamics producing those observations. Model parameters can be estimated along with the time dependence of unobserved state variables. The data assimilation acts as a nonlinear filter coupling the observed states to the parameters and unobserved states."

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Lines 8 and 24:

Equations (1) and (2) on would be more clearly and compactly defined as matrix equations. Particularly in equation (2), the summation notation and the indices are unnecessarily busy.

Line 9:

"If the dynamics of the system is described by partial differential equations (such as with the fluids in an earth systems model) the ordinary differential equations may be realized by discretizing the partial differential equations on a grid,"

It is worth noting there is a non-trivial amount of discretization error introduced in this process.

Line 18:

"This is crucial, because it establishes a necessary condition on L that is required to synchronize the model output with the data and thereby obtain accurate estimates for the unobserved states of the system, which are also required to make good predictions."

Change to:

"This establishes a necessary condition on L that is required to synchronize the model output with the data and thereby obtain accurate estimates for the unobserved states of the system."

Line 27:

"With enough observations L, a sufficiently strong coupling will alter the Jacobian of the dynamical system Eq. (2) so that all its (conditional) Lyapunov exponents are negative."

Please write out the equations describing the Jacobian of the coupled system (2) and the requirements on its eigenvalues to lead to synchronization. I find this is not well documented in the synchronization literature, even in the references provided. The clearest and most detailed description I have found is in the source code written by P. Bryant (http://biocircuits.ucsd.edu/pbryant/#ScientificSoftware). It would be a great contribution of this manuscript to provide a clearly documented reference for this process, particularly for the audience of this journal.

Line 29:

"This is important, as it establishes a necessary condition on L required to synchronize the model output with the data and thereby obtain an accurate estimate for the unobserved states of the system which are also needed to make good predictions."

It would be preferable if you could state specifically what necessary condition on L is required to synchronize the model. Also, this condition is dependent on the degree of nonlinearity of the system on the timescales of your observation window. A fairly linear dynamical regime would require fewer observations than a more nonlinear regime, measured perhaps using the error doubling time. More detailed discussion would be appreciated.

Line 31:

Change:

"has been shown Abarbanel et al (2009)"

to:

"has been shown [by] Abarbanel et al. (2009)"

Page 4:

Line 3:

"One way to proceed Rey et al (2014a, b); Pazo (2015) involves the recognition that additional information resides in the temporal derivatives of the observations."

This is an interesting idea, that perhaps could be most useful in static observing systems that have high temporal resolution relative to the analysis update cycle. For example, the TAO/TRITON array monitors temperatures at depth at regular 10-minute intervals. However, in NCEP's present operational ocean DA, the data are aggregated to daily or multi-day averages.

Line 9:

Since the use of waveform vs. observation time-derivative seems to be the key idea of the time-delay method in this paper, it would be nice if this mentioned up in the abstract and the introduction.

Line 10:

Please define D_M before you use it. Why is the subscript M used? Perhaps something more connected to 'time delays' could be used. Even using 'K' might make more sense since you use an index k for the time delay while you use the index l for the observations of dimension L.

Line 12:

I think it should be noted that this requires a static observing system producing observations at all points at regular intervals. How difficult would it be to extend this approach to the case when the observing system itself is dynamic?

How does tau relate to delta_t on page 1, line 19?

Line 22:

"DM need only be large enough to effectively increase the amount of information transferred from the L measurements to a value above the critical threshold, Ls."

I understand what is being said here, but it would be nice if the concept of 'amount of information' could be defined more rigorously.

Also, could the authors indicate whether they anticipate D_M to be within the analysis cycle, or perhaps longer than the analysis cycle to the point that observations are 'reused' in consecutive cycles? For example, from page 3, line 6, are the t0,t1,...tN times within the observation window the only candidates for D_M?

Line 24:

The terms "extended space measurement vector" (line 11) and "time delay model vector" (line 24) seem to refer to the same construct in two different spaces. So it would be helpful to the reader to acknowledge this similarity and use a similar terminology for both.

Line 25:

The notation for the quantity S_l,k looks like a matrix. If the authors would like it to represent a vector, it should be explained how the elements of the vector are arranged.

Lines 26-30:

You should make the bold notation consistent between here and the equations in (3) and (4). It seems perhaps you didn't mean for (3) and (4) to be bold if the indices indicate a specific element of the matrix.

Lines 25-31:

I'd like to see these equations represented in their matrix/vector form.

Line 31: Please explain the step between equations (6) and (7) in more detail.

Page 5:

Line 1:

"where repeated indices are summed over."

Which repeated indices? Just use the standard summation notation to keep it clear.

Line 1:

I think you want t to be italicized instead of boldface in G(t).

Line 23:

"So this framework tests the estimation procedure, not the model."

Change to:

"Therefore, this framework tests the estimation procedure absent of model error."

Line 24:

"Removing the issue of model error allows us to assess the weaknesses and strengths of the estimation algorithm and explore in detail the manner in which the unobserved variables are determined."

Change to:

"Removing the issue of model error allows us to assess the manner in which the unobserved variables are determined."

I would argue that the strength of the estimation algorithm cannot truly be assessed until model error is taken into account.

Line 25:

"When successful, it provides confidence that the method may be applied to real data. When it fails, it helps us figure out why."

Change to:

"This is a prerequisite to applying the method to real data."

Line 30:

The term "synchronization error" is reasonable when the observations are perfect.

However when the observations are noisy, I believe it's more common in DA to call the term SE the Root Mean Square Deviation (RMSD). If the observations were known perfectly then SE could also be called the Root Mean Square Error (RMSE). I should also point out that in DA the difference [y-x] is often called the 'innovation' within the context of the update procedure, and the "OMF" (for observed-minus-forecast) outside of that context.

In general, it is my preference to avoid the term "error" (e.g. SE or RMSE) when the observations are not known perfectly (i.e. in real-world experiments) but instead use RMSD. The word 'error' tends to give the wrong impression that a smaller value is better, which is not necessarily true when the observations are noisy.

Page 6:

Line 3:

"so that each state component's contribution to the synchronization error is weighted approximately equally."

It seems that the scaling applied to the observed variables could potentially be highly susceptible to noise or outliers. Is this the case?

Line 9:

"It is crucial to compare SE(t) for both estimates and predictions"

The DA terminology for that would be 'analyses' and 'forecasts', respectively.

Line 10:

"It is crucial to compare SE(t) for both estimates and predictions, as the former is just a 'fit' involving measured quantities, while the latter relies on accurate determination of the unmeasured variables as well."

I think it is worth mentioning again that the former is called "observation-minus-analysis

(OMA)" and the latter is called "observation-minus-forecast (OMF)" in operational applications. It is well understood in the atmospheric DA community that the OMFs are the more important measure for improving forecast skill, as OMAs can be set arbitrarily small by design.

Lines 12-14:

"Accurate estimates alone are not sufficient to validate the model or indicate the success of the estimation procedure, as they do not [contain] any information about the unobserved states."

They do contain information about the unobserved states from past observations propagated through the model from previous analysis cycles. The more important distinction is that the OMAs can be made arbitrarily small, but the observations contain errors and so this is not an indication of a better estimate. Rather, we would like to see consistent statistics of the OMFs over many cases showing a reduction in mean departure (assuming there are no biases in the observation errors).

Line 15:

"We have previously shown that when the synchronization error Eq. (9) decreases in time to very small values, the full state"

I don't think this can be true if there are large/outlier observation errors. At that point you would be fitting the noise and likely disrupting the state estimate and subsequent forecast. Do you mean the average SE decreases in time?

Section 4, Nonlinear Shallow Water Equations:

Line 26:

"We argue that the results presented here, for this simplified model, will be applicable for establishing the initial state of those models and predicting their subsequent behavior."

There is insufficient justification for that argument presented here. There are some additional experiments that must be done with this method before this claim can be stated confidently. A next step would be to apply the time-delay method an example case consisting of multiple layers while computing the temperature and salinity components of the density. This experiment would give a better test of estimating unobserved variables, though this is probably beyond the scope of this work.

Page 7:

Line 5:

I'm curious what impact the wind forcing had on the results. Have the authors tried experiments with multiple wind fields?

Line 7:

"With these fixed parameters the shallow water flow is chaotic, and the largest Lyapunov exponent for this flow is λmax = 0.0325/h $\approx$ 1/31 h."

Please explain how the Lyapunov exponent was computed for this case.

Line 9:

You should clarify that the grid size {16,32,64} is changing resolution over the same domain boundaries.

This result is interesting, because it implies that given an observing network, you can determine the exact scale of features that can lead to synchronization by using that observing network. There is significant interest in transitioning climate models to higher resolutions going into the future (e.g. http://cpo.noaa.gov/ClimatePrograms/ModelingAnalysisPredictionsandProjections/OutreachPublications/MeetingsWorksh while at the same time a number of new satellites are coming on with much higher spatial resolution (e.g. SWOT Altimetry http://journals.ametsoc.org/doi/abs/10.1175/JTECH-D-13-00109.1). The authors are encouraged to explore this concept in more depth.

Line 10:

"we estimated that approximately 70% of the D = 3Nˆ2 degrees of freedom must be observed in order to synchronize the model output with the data"

What observing network did you use? I.e. what was the distribution of observations? Were they stationary? Are they observed all at once, or throughout the observation window. These details will impact the number of observations needed.

Line 16:

"We are confident that despite the numerical challenges associated with scaling the algorithm up to larger D, the results presented here for $N\Delta = 16$ will also remain valid for higher grid resolution."

Does this also hold if the grid resolution is kept constant but $N\Delta$ is increased by increasing the domain size? 50km grid spacing at the equator is about the resolution of many global ocean models run in operations. Do you expect a 1/2° global model to require around 70% coverage? A dynamic observing system tends to require fewer observations to maintain synchronization (e.g. see Penny, 2014, where the Lorenz-96 system is constrained by randomly located observations http://journals.ametsoc.org/doi/abs/10.1175/MWR-D-13-00131.1). A methodology for calcuating Ls for more realistic observing systems would be a valuable contribution.

Line 18:

"In the discussion above, which included reference to the lectures of [Cardinali] Cardinali (2013), we see that the requirement of having to observe[d] 70% of the model dynamical variables exceeds the measurements now available by at least a factor of two; more if the NWP model is larger [yet]."

Change to:

“In the discussion above, which included reference to the lectures of Cardinali (2013), we see that the requirement of having to observe 70% of the model dynamical variables exceeds the measurements now available by at least a factor of two; more if the NWP model is larger.”

Page 8:

Section 5, Results with Time Delay Nudging for the Shallow Water Equations:

The equations on this page are missing equation numbers.

Line 9:

These look a bit like a stream function but usually that’s written in the form: u = ∂Psi/∂y, v=-∂Psi/∂x

Just make sure this is what you intended.

Line 16:

Using a diagonal coupling matrix G ignores spatiotemporal correlations between points. Do you have any comments about the implications of using a diagonal G?

Lines 16-19:

I thought G was the time delay space coupling and g the physical space. But, here is says g is the time delay space coupling term. Is this a typo?

Line 17:

“These are chosen because the height values are several orders of magnitude larger than the flow velocities.”

You could consider normalizing the innovations by the corresponding observation errors (i.e. standard deviation used for Gaussian noise).

Line 23:

Please cite a reference for calculating the average mutual information, using whatever method was used here.

Line 24:

“Furthermore, the results were reasonably stable to changing its value within a few [delta] t.”

I’d like to see some analysis of the sensitivity to delta t to justify this statement.

Lines 26-27:

Do you use a time-delay extending before the beginning of the analysis cycle window [0,T]?

Page 9:

Line 3:

“Since DM $\geq$ 8 produces error values several orders of magnitude smaller than those obtained with DM $\leq$ 6, we expect the state estimates x(T) obtained with DM $\geq$ 8 to be quite accurate when compared with the estimates for DM $\leq$ 6. ”

There seems to be a critical point between D_M=6 and D_M=8, what are the results for D_M=7? How do you explain this bifurcation? Is there a waveform that can only be resolved at this time delay length?

Line 11:

“Just a reminder note here, we used L = 256 = 33% of the total 768 dynamical variables as observed, then used time delay information on the waveform of the measurements to provide the required additional information.”

This reminder is not necessary here in the results section. Instead, the clarification should be made earlier, perhaps when the value of Ls is stated on page 7 line 15.

Line 20:

“As this point is the key theme of this paper, we take the liberty of repeating that the number of physical measurements is just 33% of the overall dynamical variables.”

Yes, but the height has a strong relationship with the currents. What kind of results do you get if you observe only the u or v components of the velocity instead of height? Is 33% still sufficient?

Line 26:

“in accordance with our previous results in Whartenby et al (2013)”

Change to:

“in accordance with Whartenby et al. (2013)”

Lines 31-32:

“When enough information is available, and the coupling is strong enough, these conditional Lyapunov exponents will all be negative, allowing the coupled systems of data and model output to synchronize.”

Do you have a means of computing the conditional Lyapunov spectrum for this system? If so I would like to see these results presented.

Page 10:

Line 7:

“in a true experiment, the success of the assimilation procedure must be evaluated against the predictions - not the estimates.”

Again, if you are going to discuss real-world problems, I suggest using the appropriate terminology: “forecasts - not the analyses.”

Line 19:

“We remark, however, that the overall space of parameters appearing in our formalism has not been thoroughly explored and that by further adjusting these parameters"

One obvious example to maintain prediction skill with reduced observations would be to use non-diagonal coupling terms in your nudging (as is standard for operational data assimilation).

Line 22:

"One would expect, that at some point the resolution should be high enough to not necessitate further measurements."

I don't understand this statement. In a realistic system, as the resolution increases, the resolved features also increase. In that case, I would expect the required observations to increase super-linearly. I suggest more experimentation should be done before making such a claim.

Section 5.1 Measurements with Gaussian Noise

Line 24:

"In operational data assimilation in meteorology, one challenge is that the measurement contains observation error."

This is true in all data assimilation, regardless of the domain.

Change to:

"In operational data assimilation, observations contain measurement errors and systematic biases."

Line 29 and 30:

Missing equation numbers

Line 31:

"and we selected C_data = 106 and C_height = 1652."

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Please list the scale of the observation errors used in the experiments shown in figure 7 within the text.

Page 11:

Line 2-3:

“The time delay nudging method remains robust under imperfect observations.”

I would anticipate there are observation errors that can cause outliers large enough to ‘break’ this method. Have the authors found such cases?

Section 6. Using Drifter Data with Time Delays

Line 9:

The authors should mention here if they append the drifter coordinates to the state vectors, or if they convert the drifter positions to an Eulerian velocity measurement.

Lines 16-19:

“After the initial deployment, the drifters move between grid points providing information not available from grid point measurements alone.”

In order for this to be true, the drifter positions shouldn’t be determined solely by the gridpoints. For example, they might be computed on a finer model grid. However in the next paragraph it is said:

“The dynamics of drifters is described as two-dimensional fluid parcel motion on the surface of the water layer. Since the positions of drifters are continuous values, the velocities of the drifters are estimated by a smooth linear interpolation”

So, if the velocities are determined by a smooth linear interpolation, then aren’t they determined from the grid point measurements alone?

Line 28 and 30:

Missing equation numbers

Line 28 looks as if it should have a left bracket at the beginning of the line.

Page 12:

Perhaps you might also consider an additional case with L = 208 + 20 while N_D = 0 to ensure a fair comparison, or at least give more perspective on the impact of having a portion of the obs drifting versus stationary.

Lines 5-6:

“We have further investigated how the geographic distribution of the drifters influences the size of the synchronization error, although these results are not displayed here.”

Why not? These results would be interesting.

Line 11:

Is it possible to generate an estimated current velocity based on the wind field and heights? Does that velocity correspond well to the drifter observations?

Section 7. Conclusion

Line 15:

“In an earlier paper Whartenby et al (2013) we showed that using standard nudging”

Change to:

“Whartenby et al. (2013) showed that using standard nudging”

Line 22:

“realistic and complex models of the ocean, atmosphere system”

Change to:

“realistic and complex models of the ocean [or] atmosphere”

Line 26:

This discussion about 4DVar should be a section either in the introduction or just after, giving the context of the time delay nudging method relative to the 4DVar method.

Page 13:

Most of the discussion on model error should be moved to the body of the manuscript. It does not seem appropriate in the conclusions.

Page 14:

Line 8:

“The framework presented here allows one to directly estimate the minimum number of observations at each measurement time required for accurate predictions, Ls”

I’m not sure I would characterize it as a direct method for estimating L_s. A brute force application of many values until achieving synchronization seems more indirect. A more direct method would be desirable.

Line 12:

Because ‘almost surely’ has a very specific mathematical meaning, I would suggest to change:

“real data will almost surely fail”

Change to:

“real data will likely fail”

Line 13:

“On the other hand, when the process succeeds, it increases confidence that predictive failures associated with the assimilation of real data arise from inadequacies in the model.”

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Or, inadequacies of the observing system.

Line 14:

“When the model is wrong, as it typically will be in practice”

Change to:

“When the model is imperfect, as will be the case in practice”

Line 16:

“In other words, when predictions fail, our strategy provides a useful diagnostic framework to help determine where to concentrate our efforts: improving the model or collecting more data.”

This statement should be tempered or removed. I don’t see such a tradeoff being made in a real-world scenario, in short because there are far more considerations that go into such decisions than whether one can get a better analysis at a given time. For better or worse, these efforts tend to be independent. There are obvious reasons to develop both the model and increase data collection in parallel. For example, model development can always be postponed, but data collection can never be repeated.

Lines 18-19:

“The inclusion of time delays comes of course with an additional computational cost, mainly associated with the integration steps required to construct the time delay vectors and its Jacobian, as well as solving for the perturbation itself.”

This should be stated at the very beginning when the method is first introduced.

Line 24:

This should be the leading paragraph in the conclusion section.

Page 19:

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Figure 2:

This is not a complete sentence: “In accordance with the synchronization error results.”

Interactive comment on Nonlin. Processes Geophys. Discuss., doi:10.5194/npg-2016-22, 2016.