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Comment

## ***Interactive comment on “Adjoint of the Global Eulerian–Lagrangian Coupled Atmospheric transport model (A-GELCA v1.0): development and validation” by D. A. Belikov et al.***

### **Anonymous Referee #1**

Received and published: 8 September 2015

The manuscript by Belikov et al. presents the development of a new adjoint modeling system – A-GELCA. The novelty of this tool is combining a Lagrangian back trajectory model with an Eulerian adjoint model. The authors provide background on issues related to inverse modeling of CO<sub>2</sub>, which seems to be the intended application of this tool. The model estimates for various configurations (different resolutions of the Eulerian component) are shown compared to CO<sub>2</sub> measurements from seven stations in Siberia. This is followed by evaluation of the model via comparison to forward modeling sensitivities and the Lagrange equality. Lastly, the authors show comparisons of adjoint sensitivities for different model configurations, highlighting the information brought through the coupling of Lagrangian and Eulerian components. The tools presented

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here seem to perform adequately and will be of value for future application studies. My main criticism is a lack of detail in many places in the manuscript, particularly when covering some of the more essential and novel aspects of the model development (how the Eulerian and Lagrangian components were coupled, or how the adjoint code was developed). Further, the article needs much work on the grammar and writing. I believe it will be suitable for publication after addressing these and other issues outlined below.

Comments:

Scope: It seems like evaluation of the forward model is a substantial part of this work; as such, this should be included in the abstract and introduction as one of the aims of the article, and the title itself should reflect this scope.

Abstract and throughout: it seems odd to refer to “development of the adjoint of a Lagrangian model”, since Lagrangian models are self adjoint by construction. So saying “Lagrangian adjoint” seems redundant.

5984.17: this entire sentence is rather vague. Could the authors clarify, quantitatively, what is meant by “effective in reproducing”, “high uncertainty” and “low resolution”? Without any numbers, such statements have little context or impact.

5985.13: Can the authors be any more specific than “a number of studies have proposed improvements” and then citing several papers? What are the improvements, and which are relevant to the topic of this work in terms of those related to resolution, or coupled Eulerian / Lagrangian frameworks?

5985.20: For recent measurement updates, a reference from 1999 doesn't seem very recent.

5986.16: It would take a prohibitively large number of forward model evaluations to evaluate such a matrix for an inversion with the same resolution of an adjoint-based approach.

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5986.24: “Recent studies. . .” It seems odd to switch the discussion here to CO, given the previous focus on long-lived tracers, CO<sub>2</sub> in particular. Why not instead cite / discuss the set of current studies using adjoint models to invert satellite CO<sub>2</sub> data? I believe there are several.

5986.28: “. . .speeds the process of inverse modeling” is only true for high dimensional systems.

5988.20: The background. . .” I didn’t really understand what was being said here or how the modeling setup works in this regards.

5989.3: The description of the coupling of the eulerian adjoint model with the Lagrangian model is rather vague. This statement, that it was coupled at the “time boundary” is made a few times, but to be honest I don’t really know what it means. Given that (a) this coupling is the single most unique and exciting feature of the A-GELCA model and (b) articles in GMD are for the expressed purpose of describing algorithmic model details, this should be clarified in further detail, at the level of making the process understandable and reproducible by a reader.

5989.25: “performs well” is very vague. Can the authors be more specific?

5992.5: Is it that the errors are unbiased or that the background estimate itself is unbiased?

5992.6: This capital bold H applied as a matrix is already linear by definition. If the authors intended to more generally describe a potentially nonlinear forward model operator, they should use capital cursive H.

Did the authors also generate / evaluate a tangent linear model? If not, what is the intended path towards deriving an inverse modeling system (many formulations of which require a tangent linear model, i.e., incremental 4D-Var with CG optimization, etc)? Or will their system only be worth with optimization approaches such as using the BFGS variable-metric quasi-newton algorithm?

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5993.11: Previously (5992.24) a 1x1 scale was referred to as low resolution, but here 1x1 is used for the “high resolution” FLEXPART runs. This is a bit inconsistent. I was expecting FLEXPART simulations to be run at a much finer (i.e. 10’s of km) scale.

The set of measurements used for evaluation (7 sites) seems pretty thin compared to the amount of available CO2 measurements available. The NOAA GMD network alone has more than 100 measurement sites. Now, perhaps forward model evaluation isn’t a goal of this work (see previous discussion, this wasn’t clear), but if it is then it should be done more comprehensively.

5994.7: “We recognize... is quite problematic” I didn’t understand the point that the authors are trying to make here – can they reword?

5994.22: I recognize that there are continuous vs discrete approaches for developing adjoint models, that there are benefits / drawbacks to each approach, and that the authors have adopted the discrete approach for specific reasons. But is it fair to only here mention the benefits of this approach, and none of the drawbacks?

5996: For the forward model sensitivity, use  $\lambda_F$  throughout, not just in equation 5.

5996.14: Why is a perturbation needed for an adjoint simulation? Do you mean forcing? Or that the cost function was defined to be 1 ppm per grid cell?

Section 3: I recognize that the long-term goal is inverse modeling. However, the application and testing of the model thus far is just for sensitivity calculations. It seems then that Section 3 would be better served as a description of adjoint modeling, and the background of how this works, rather than or in addition to inverse modeling, as the latter isn’t actually done in the present manuscript. This would help clarify, for example, the setup of the adjoint calculations that are performed later for validation in 5.2.1, which I don’t believe used a cost function of the type shown here, but rather something different.

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5996.15: The forward sensitivity calculation was performed in how many locations? It seems from Fig 3 that they were done in many grid cells, in order to compare to the adjoint results throughout the domain of this figure, but that would be very expensive, computationally, even using Eq 3. If transport was turned off for the testing, all locations could have been tested simultaneously, but this wouldn't constitute a very meaningful test of the adjoint of the tracer transport model.

5.2.1: What was the state vector used for these tests? CO2 initial conditions? Fluxes? Or flux scaling factors? What are the corresponding units of the results shown in Fig 3?

5997.10: It would probably be good to show results from these tests somehow.

Figs 4-6: These are really interesting results. I found myself, however, having to flip back and forth between these figures to compare across the different modeling approaches. Comparison for a single method across days was much less interesting or relevant to this work. So I would suggest reducing these figures to a single figure that shows the results for a single day but for the 4 methods: eulerian, Lagrangian (native), Lagrangian (aggregated), coupled.

5999: "substantial amount of manual programming effort is required" This should be expanded for a GMD article.

Editorial:

This manuscript needs a thorough proofreading and grammar check prior to publication. I've provided comments below on the abstract and introduction but stopped after that point.

5984.7 tangent → tangent linear

5984.6: paragraph break not needed

5984.11: as results → as a result

5984.11: of Eulerian → of the Eulerian

5984.17: “test experiments” is redundant, suggest just “tests” or “experiments”.

5984.17: shown → shows

abstract: the written tense keeps changing, please try to use a single tense throughout.

5984.20: demonstrates the → is (or was, depending on if you decide to write in the past or present tense throughout) shows to have

5985.18: a density → the density

5985.19: measurements → more measurements

5985.21: global scale CO<sub>2</sub> observation are not existing → global scale in situ CO<sub>2</sub> observations do not exist

5986.10 CO<sub>2</sub> a → CO<sub>2</sub>, a

5986.12: If tracer is a chemically inert → For chemically inert tracers,

5986.15: running multiple times with set → run multiple times with different sets of

5986.19: Seems odd to have the paragraph break here, instead of e.g. line 22.

5986.29: “memory demands” should be minimal for adjoint approaches with inert tracer transport (i.e. linear) models.

5987.1 “It would. . . fluxes” This sentence doesn’t make much sense, and needs to be rewritten.

5987.10: utilize of the → utilize the

5987.11: the adjoint, which → an adjoint that

5987.17: “One goal” → is there another goal of this work? Forward model evaluation perhaps? If so this other goal should also be directly stated. If not, suggest saying

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“The goal”.

Eq 1: why does the “l” index start at 0 and the others at 1?

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Interactive comment on Geosci. Model Dev. Discuss., 8, 5983, 2015.

**GMDD**

8, C1936–C1942, 2015

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