The authors would like to start by thanking Dr. Cotter for his helpful and thorough review, which has greatly improved the clarity, presentation and content of the manuscript. Responses to specific points raised in the review are given below.

In this paper, the authors introduce some new aspects of C-grid and Z-grid schemes for the rotating shallow water equations on polygonal grids. The paper starts each section by collecting together quite a few bits of mathematical structure and previous results about C-grid and Z-grid schemes. These can be found elsewhere but it is nice to see them collected together in this context.

For C-grid schemes, they introduce a new methodology for obtaining schemes that simultaneously conserve energy and enstrophy on arbitrary orthogonal grids, a previously unsolved problem. The authors take the approach of writing out the (possibly over- or under-determined) system of equations that form the constraints, applying the Thuburn et al (2009) decoupling formula and solving the resulting system numerically. There is no proof of solvability for this system but the authors report that a unique solution is obtained in their numerical tests. For Z-grid schemes, the authors extend the Nambu bracket approach to arbitrary orthogonal grids, obtaining energy-enstrophy conserving schemes by construction.

As described in the introduction, this paper is Part I of a series of 3 papers with Part II containing numerical tests and Part III containing linear dispersion analysis. Part I concentrates on exposition of the methods and discussion of their properties. Given the focus of GMD on documenting and describing models and model software, I think that Part I requires:

(a) some evidence that the schemes are practically useful, i.e. that they do not do anything obviously weird. Having conservation properties is a good sign, but I have built plenty of schemes before that have good conservation properties but still lead to terrible numerics. If Part II promises to be a detailed comparison and analysis of results then Part I should at least have some provisional examples that show that things are working as expected.

(b) some evidence that the code provided is a correct implementation of the algorithms described.

A short section (Section 6) has been added the paper with results from the Galewsky et. al ([3]) test case. The simulations look very similar to those obtained with other members of the TRiSK family, indicating that the new schemes are at least comparable. A detailed comparison will be performed in [2]. Evidence of the correctness of the implementation can be found in [1] (through an examination of the conservation properties), and this is mentioned in the revised manuscript.

(c) for GMD, I would expect some discussion as to how the numerical methods were implemented and expressed as code. This is not the same as code documentation, but should describe the main data structures used and how they form an efficient implementation. It may actually be that a merge of Parts I and II makes sense, or some partial repetition between Parts I and II to achieve (a) and (b). Perhaps the solid rotation test plus something else where we can easily check that things are working, like the mountain after the usual 12 days (or however long it is).

Also in Section 6, a brief discussion of the actual implementation has been added. It uses a combination of Python as a driver language with Fortran kernels for the numerics, and employs indirect addressing on a fully unstructured grid. Some thread-level parallelism in the numerics through OpenMP has also been added. This is not a particularly efficient implementation, although it served the purpose of demonstrating the properties of the proposed schemes.

A few other remarks: (1) Its bad form for a referee to ask for a reference to their own paper so feel free to ignore, but you might like to mention that the problem of simultaneously conserving enstrophy and energy on arbitrary grids was solved in the context of compatible finite element methods in McRae, Andrew TT, and Colin J. Cotter. "Energy and enstrophy conserving schemes for the shallow water equations, based on mimetic finite elements"

A reference to this paper has been added to the introduction, and some discussion of compatible finite elements also been included. The present work has been differentiated by emphasizing its use of finite-differences and explicit time stepping that does not require the inversion of a mass-matrix to compute the time tendency terms (although the vorticity-divergence scheme does require the solution of a Poisson problem at each time step to diagnose χ and ψ).

(2) The paper suddenly jumps in with 1-forms, 2-forms, Hodge stars etc without any warning to the reader! You should at least provide some references and a bit of a guide to what is going on, and maybe consider whether the language of differential forms is really necessary for this paper in terms of accessibility to a more general numerics audience.

To clarify the presentation, the references to differential forms and Hodge stars have been removed, and the relevant quantities have been redefined as integrals over the relevant geometric entities. A short section noting the relationship between the proposed C grid scheme and discrete exterior calculus has been added to provide more information for interested readers.

(3) I would like to see some more description of how Equation (61) decouples the problem and how big the resulting uncoupled systems are. Why does it take so long to solve these systems? Why cant they be analysed to check if there is a unique solution?

Some additional discussion of the solution process for $\mathbf{Q}$ has been added to Section 4. Once uncoupled, the resulting systems are quite small (24 coefficients for a square grid cell, 90 coefficients for a hexagonal grid cell), and they are very fast to solve. Although the systems are overdetermined, an exact numerical solution is found. This implies the existence of a solvability condition. As mentioned by reviewer 1, it seems likely that determining solvability condition would enable an explicit, analytic solution for the coefficients in terms of $R_{i,v}$ and $n_{e,i}$. Unfortunately, the authors were unable to determine the solvability condition for the case of general grids. This did not prevent, however, the successful use of a numerical solution to determine the coefficients.

(4) Please can you do a consistency test e.g. on the sphere for the Q operator? That is, take an analytic formula for u,h, interpolate to the grid and apply the Q operator, then analytically compute Q and interpolate to the grid, and compare errors in the L2 norm. Id be especially interested in the cubed sphere case, where we observed lack of consistency for

the Coriolis operator in our non-orthogonal scheme.

(5) If Im thinking about this correctly, then the Q operator should imply a Coriolis reconstruction operator for the linear equations. Is this operator consistent in the limit as dx→0 on e.g. a cubed sphere mesh?

The $\mathbf{Q}$ operator is designed to reduce (in the linear case) to the Coriolis operator W from [4], since for a given $\mathbf{R}$ that is the unique operator that preserves steady geostrophic modes. The new $\mathbf{Q}$ therefore inherits all of the drawbacks of this operator, and in particular as you mentioned its inconsistency. This is a major issue with all TRiSK type schemes. This point has been clarified and further emphasized in the manuscript. A consistency check for $\mathbf{Q}$ is included in [2] and [1], confirming that it is indeed inconsistent on both icosahedral and cubed-sphere grids.

(6) There is no mention of timestepping anywhere. What do you do about timestepping in the code? How do time series of energy and enstrophy look?

This paper concerns itself only with spatial semi-discretization, and any stable time scheme can be employed with the spatial schemes presented here. For the tests performed in [2] and [1], 3rd order Adams-Bashford time stepping was used. The time series of energy and potential enstrophy look quite good, but of course the exact conservation implied by the spatial semi-discretization presented in this manuscript is lost.

(7) What is the relationship of the Z-grid scheme to Heikes et al (2013)? Is it a straight forward extension of the same formulae to arbitrary grids or is another idea needed?

The operators of the Z-grid scheme are the same as those of Heikes et. al (2013), simply with different arguments; and with a different Poisson problem used to diagnose χ and ψ from ζ and μ. These differences arise fundamentally from the use of a Hemholtz decomposition of the mass flux $h\vec{u}$ rather than the velocity $\vec{u}$. This point has been further emphasized and clarified in the revised manuscript.

Bibliography

[1] Christopher Eldred. *Linear and Nonlinear Properties of Numerical Methods for the Rotating Shallow Water Equations*. PhD thesis, Colorado State University, 2015.

[2] Christopher Eldred and David Randall. Total energy and potential enstrophy conserving schemes for the shallow water equations using hamiltonian methods: Test cases (part 2). In Preparation, 20016.

[3] Joseph Galewksy, Richard K. Scott, and Lorenzo M. Polvani. An initial-value problem for testing numerical models of the global shallow-water equations. *Tellus A*, 56(5):429–440, 2004.

[4] J. Thuburn, T.D. Ringler, W.C. Skamarock, and J.B. Klemp. Numerical representation of geostrophic modes on arbitrarily structured C-grids. *Journal of Computational Physics*, 228(22):8321–8335, December 2009.