The authors would like to start by thanking Anonymous Reviewer 1 for their 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.

My most important point is that throughout sections 3, 4, 5 (and Appendices B, C, D) the notation is awkward and potentially confusing. In the papers cited the notation U, for example (no subscript) is typically used to mean the vector comprising all the velocity degrees of freedom while the notation U_e means one component of that vector, that is, the velocity at edge e. In the present manuscript u_e, for example, seems to have both meanings. This then makes many of the formulas difficult to understand, particularly for readers not intimately familiar with the cited previous work. It looks like it will be a rather tedious job to fix this up, but I think is it is essential for the clarity (and correctness) of the paper.

The notation has been modified to use uppercase to denote a vector of all degrees of freedom, while lowercase indicates a single component of the vector, and the subscript indicates the location of the degree of freedom. A hat is used to denote a quantity defined on the dual grid. For example, $\hat{U}$ denotes the vector of all velocity degrees of freedom, while u_e indicates the velocity at edge e. The latter occur only within sums over relevant geometric entities, so the meaning of edge e is unambiguous. The should help clarify the presentation in Sections 3,4,5 and Appendices B, C, D.

As the authors note, the principal novelty (and difficulty) of the proposed C-grid scheme is the specification of a suitable Q operator (section 4) given by some coefficients α satisfying (60). (60) is eventually solved by a least squares method which (lines 441-2) has a unique, exact solution. Could the authors please clarify whether (60) itself is solved exactly? If it is not then the scheme does not quite have the desired properties (though it may do so to a very good approximation). If, on the other hand, (60) is solved exactly, despite being overdetermined, then this suggests that some solvability condition must be satisfied, as Thuburn et al 2009 found in constructing their W operator. (It might even be possible then to write down the solution for the alphas without resorting to a numerical solution?) Either way, there should be something interesting to say.

Some additional discussion of the solution process for $\mathbf{Q}$ has been added to Section 4. Although the systems are overdetermined, an exact numerical solution is found (which was verified by checking that the defining relationships for energy and potential enstrophy conservation held with several sets of random vectors for F_e and q_v). As you have mentioned, this implies the existence of a solvability condition; and it seems likely that determining the 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.

Line 9, line 677 (perhaps elsewhere). Perhaps make it clear that here orthogonal means that there is a dual grid whose edges are orthogonal to those of the primal grid.

This has been clarified in the revised manuscript.

Section 2.2. What is Ω? Are some boundary conditions assumed in writing (7)?

Ω represents the whole entire domain under consideration- either a doubly periodic plane or the sphere. This implies that no boundary conditions are needed. This has been clarified in the revised manuscript.

P6. Define ∇^T; P7 define $\nabla^\perp$

The presence of ∇^T was a typo. The skew gradient operator is defined as $\nabla^\perp = \vec{k} \times \nabla$ on the plane, where $\hat{k}$ is the unit vector in the vertical direction. Both it and the 2D curl $\nabla^\perp \cdot$ have a coordinate independent definition on more general manifolds. This has been clarified in the revised manuscript.

Eq (26). There is the potential for some confusion because (26) is not quite the same expression as that below (2).

A factor of h_s was missing, thanks for catching this!

Line 204-241. Readers not familiar with differential forms and their discrete counter-parts might be thrown by this new terminology. Perhaps explain briefly that 1-forms correspond to edge integrals and 2-forms to cell integrals; that should be enough for most readers to follow.

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.

Eqs (37) (38). Explain the subscripts I and H.

These equations have been rewritten in a way that no longer requires the subscripts I and H, as a part of the change in notation.

Line 268. It is not clear until later that ϕ is an interpolation operator.

This has been clarified in the revised manuscript.

Eq (50). Is there a factor 1/2 missing?

No, when taking functional derivatives of the kinetic energy part of Hamiltonian the factor of 1/2 cancels a factor of 2 that comes from the square of velocity.

Section 3.4. From what we understand about the Hollingsworth instability, a Charney-Phillips vertical grid should reduced the instability compared to a Lorenz grid (rather than avoid it), but it could still be an issue at very high vertical resolution. Also, on a square grid one can rigorously derive a reformulation of the KE so as to rule out the Hollingsworth instability. On general grids this does not seem possible (e.g. Gassmann 2013), one can only minimize the non-cancellation that leads to the instabilty. In practice this seems to be sufficient at least for the published results, but we should not take it for granted that the problem is solved.

The discussion of the Hollingsworth instability has been revised in the updated manuscript. The principal points we are trying to make is that there appear to be several practical approaches to mitigating or avoiding the Hollingsworth instability for similar C grid schemes (reformulation of the kinetic energy expression, use of a Charney-Phillips staggering in the vertical, use of an isentropic or hybrid isentropic vertical coordinate); that these fixes do not affect the properties of the scheme such as energy conservation; and that therefore the possible presence of this instability should not prevent the use of the proposed scheme.

Eq (84). Is K defined?

K is defined in the Appendix, this has been clarified in the revised manuscript.

The definition of FD in (101) is not quite the same as that below (87), which could cause confusion.

This has been changed in the revised manuscript.

Line 679. Surely any Voronoi tessellation / Delaunay triangulation gives an orthogonal primal-dual pair?

This is certainly true, and has been clarified in the revised manuscript. On the sphere, the only quadrilateral orthogonal grid the authors are aware of is the conformal cubed-sphere grid, which still suffers from resolution clustering at the panel corners. The more widely used gnomic cubed-sphere, which does not suffer from resolution clustering, is unfortunately non-orthogonal.

Appendices B and C. It would be helpful to have a brief (one or two sentence) interpretation of what each operator does.

These have been added in the revised manuscript.

Minor points, typos, etc

These have been fixed in the revised manuscript.