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To the Associate Editor, Geophysics:

Re: Paper GEO-2007-0117

This letter is in response to the review received July 13^{th}, 2007 of paper GEO-2007-0117 entitled *Riemannian wavefield extrapolation: Non-orthogonal coordinate systems.* I thank the four reviewers for their comments, and for catching a number of mathematical and language typos in the manuscript. I respond to the individual reviewer's comments below.

I look forward to hearing the response from Geophysics regarding the manuscript corrections.

Sincerely,

Jeff Shragge

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Response to Reviewer 1

1) Sentence "This allows ... " for clarity changed to "The resulting extrapolation operators include additional terms describing non-orthogonal propagation; however, these extra degrees of complexity are offset by smoother coefficients. "

2) I include an example of a 2D benchmark testing results basically to make the reader aware that the method has additional computational and memory overhead. My intention is not to give definitive numbers for how much more costly, because the results would be highly dependent on the particular chosen coordinate system, wavenumber approximations, computational implementation, etc. Accordingly, I have chosen not to include any of the 3D benchmarks or memory requirements beyond the sentence: "For example, a 3D non-orthogonal grid requires an additional 20% memory to store coefficients relative to a semi-orthogonal mesh."

3) "unfavorable mesh conditions" changed to "unfavorable characteristics such as extensive mesh compression/extension or in the presence of singularies."

4) This is indeed a little vague and I have now been more specific in what I am saying. It is not really the smoothing that is the problem; rather, it is grids that exhibit rough and/or discontinuous boundaries: "However, for meshes exhibiting rough and/or discontinuous boundaries, even excessive local smoothing cannot generate coefficients that are smooth enough to be accurately represented with standard extrapolation techniques."

5) I have included the following paragraph better explaining the amplitude contrasts: "Differences in the modeled amplitudes at and above the salt interface in the upper two panels are attributed to differences between finite-difference and one-way wavefield extrap-

olation. Finite differences better models amplitudes in the presence of velocity gradients in the propagation direction, and thus more accurately partitions energy at the top sediment-salt interface. The RWE wavefield underestimates the reflection contribution and allows significantly more energy to be transmitted into and through the salt body. This energy causes the additional multipathing noted in the RWE panel below the salt body, leading the more complicated wavefield behavior relative to the Cartesian wavefield example."

6) The manuscript now discusses in more detail what the "singularity at the pole" issue is: "However, any wavefield extrapolation procedure in near-spherical coordinates must account for the apparent singularity at the poles (i.e. vertically downward). This requires appropriately discritizing the spherical shell making up an extrapolation step in the presence of an ever diminishing solid angle toward the polar regions."

7) I have played around with different projections of the grid and have not been able to generate one that would present greater clarity. Also, any attempt to generate a 3D mesh led to a figure that was much more complicated. I have added the following sentence for clarity: "A 3D point-source coordinate system can be constructed by using a set of concentric shells with spherical Winkel-Tripel gridding of increasing radii".

8) I have changed from percentages to "times factors" that are easier to comprehend: "The most significant observation is that the RWE algorithm is roughly 1.35 times slower than the equivalent Cartesian code. Most of the overhead occurs in the phase-shift and SSF subroutines that are roughly 2.25 and 2.0 times slower, respectively. "

9) "tje" changed to "the"

10) I have added the following sentence addressing the point: "Furthermore, the extra cost of non-orthogonal propagation, relative to that on semi-orthogonal mesh, is $<5\%$ since

this affects only phase-shift operation and occurs outside of the more costly square-root calculation."

11) "the implementation of" has been removed

12) I have added additional sentences that better explain the numbers in Table 1: "A total of 82 frequencies were propagated a total of 511 extrapolation steps, thus requiring 41092 calls to the SSF operator. In addition, the tests involved 112996 calls to the phase-shift routine, or almost three per extrapolation step as this number varied with velocity model complexity."

13) This has been addressed in point 5).

14) The reviewer is correct that the wavefield is not visible. I have reproduced the figure with the wavefield having a greater relative amplitude.

The main purpose for choosing the 3D point example is to demonstrate that 3D RWE is feasible. In addition, I argue that non-orthogonal (or semi-orthogonal) geometry is better because it more stable and better sampled than orthogonal spherical coordinates. Showing a comparative 3D spherical result does not really strengthen this point.

In addition, I generated the corresponding Cartesian figure and have included it in Figure 8. The result shows that the RWE algorithm can overturn waves and that Cartesian extrapolation cannot.

Response to Reviewer 2

1) Page 6: No, this is not absolutely required; however, because this is the first time I use summation notation I thought it might be nice to include just one example.

2) Page 6, line 59: I have appended $i \neq j$ as suggested.

3) Page 7, eq 10: Yes, this was quite a typesetting effort! But the reviewer is correct that it is not really needed and I have removed this equation.

4) Page 7, eq 12: Thanks for catching an error! The reviewer has correctly derived the a_{10} coefficient. This has been amended throughout the paper.

5) Page 8: Indeed, the phase-shift extrapolation routine does impose a limit on the maximum alignment of the grid and wavefield (similar to the case of pure Cartesian propagation). This is addressed partially herein with the SSF approach in Appendix B. In addition, higher-order correction terms (FFD, etc) could be developed too. However, I feel that giving a benchmark (e.g. 30 degrees) would not be wise since this largely depends on the implementation and how much computational effort one want to throw at high-order FD corrections.

6) page 12, line 36: I now explicitly say which equations in Appendix C are being used.

7) page 12, line 42: I have added as recommended "and further restricting to the orthogonal polar case"

8) page 13, line 50: the "redundantly redundant" line has been removed.

9) Page 16, lines 43-47: In this revision I have eliminated this point, as it is not actually that important and doesn't contribute that much information.

10) page 17, line 34: "tje" changed to "the".

11) page 17, line 49: "of" has been added.

12) page 20, line 28: "Lloyd's" has been capitalized.

13) page 31, eq. A-1: I have removed this part to make it more concise and have left the proper definition relating the covariant and contravariant metric tensors.

14) page 21, eq. A-4: I have written more simple discriminant as suggested.

15) page 24, eq. C-6: I have removed the semicolon.

16) page 24, eq. C-8: I have added the correct a_8 term as suggested. Thanks again for catching that!

17) page 25, line 11: This case specifically refers to 2D orthogonal coordinate systems; hence, g^{13} is zero by definition.

18) page 25, line 16: Indeed, "non" is a typo and has been removed.

19) page 27: The excess few words have been eliminated.

Response to Reviewer 3

Abstract

1) "defined" has been used in place of "comprised"

2) I have eliminated "multivalueness" and now just refer to bunching and singularities.

3) "Problems by posing" changed.

Introduction

1) I have changed "steep" to "large" throughout the paper.

2) I have changed "overturning waves" to "turning waves" throughout the paper.

3) I have used "address" instead of "obviate"

4) I am just giving a few examples of recent papers where this has been addressed, and all papers are all kind of "e.g."

5) The reviewer is correct that this is a little ambiguous. I have added that the "local" propagation angle is reduced, which reduces the need for more expensive "global" operators: "The key concept in each of these approaches is that a judicious choice of reference frame lowers the effective local propagation angle, reducing the need for expensive global extrapolation operators and enabling imaging of turning waves."

6) "Generalized Riemannian space" changed to " 3D numerically generated, semi-orthogonal meshes"

7) "degree to which..." changed to " the degree to which wave-propagation direction is

aligned into the computational mesh”

8) ”supposition” changed to ”restriction”

9) ”mixed-domain fields ...” changed to ” a number of mixed spatial and wavenumber domain terms (i.e. simultaneous dependence on x and $\mathbf{k}$) that encode coordinate system geometry.”

10) expanded to ”unfavorable characteristics such as extensive mesh compression/extension or in the presence of singularities”

11) I have changed ”triplications” to ”crossing rays” whenever specifically discussing rays.

12) Indeed this is division by zero due to zero Jacobians. Text changed to ”This generates spatial singularities and singular Jacobians that lead to zero-divisions during wavefield extrapolation. ”

13) I have expanded a little as discussed; however, much of what I do is commented upon in the next paragraph.

14) I now talked about coordinate system ”singularities” instead of ”triplications” throughout the text.

Acoustic WR in 3D generalized Riemannian spaces

1) changed as suggested

2) I have removed ”non-physical” as it didn’t really make too much sense.

3a) I now discuss the coefficient terms and do not really mention the vector $\mathbf{a}$.

3b) I am somewhat hesitant to include a dispersion test in here, because it would greatly depend on which coordinate system and model that I used, as well as the computational implementation and the order of approximation. In particular, I could design the "perfect test" to showcase the method. But this would neither be fair nor be that applicable in practice.

4) Indeed, $3^{10} = 59049$ and not 22599. Good catch!

5) The purpose of the discussion on the Lloyd's algorithm is more to inform the reader that there are methods for approximating the multi-dimensional coefficients in $\mathbf{a}$. I intentionally avoid going into too many of the details as this is somewhat tangential and already covered in much more detail (including examples) in the referred Tang and Clapp (2006) paper.

6) I have added an additional equation (15) that provides my criteria for when to zero a coefficient value rather than recompute it.

Numerical Modeling examples

1) Absolutely there are ways to mitigate these numerical artifacts. I included them largely to show the reader that the same problems in Cartesian wavefield extrapolation craft still exist in the RWE approach. Similarly, similar remedies exist to these problem. In particular to the boundary reflections on the left, these are caused because the coordinate system is truncating at an angle to the plane-wave, which leads to a non-normal incidence and non-zero boundary reflections.

2) changed as suggested

3) the extra "is" has been removed

4a) The reviewer is correct that these are only plane waves at the surface, and that interesting wave-phenomena effect is occurring when the waves go through the turning point, including an apparent wave-guide effect. (I believe that the amplitude response in this region is described by Airy functions.) I have amended the text to be more specific: "The test data consist of ten plane waves defined at the surface by a ray parameter $p_x = -0.5$ s/km. At greater depths the waves are no longer planar and propagate through a turning point before turning upward to the left."

4b) I have also tried to overlay the plane-wave ray paths; however, this made the figure much "busier" and so I chose not to include it.

Generating triplication-free meshes

1) changed as suggested

2) changed as suggested

3) I have added an additional paragraph that discusses the amplitudes in much more detail: "Differences in the modeled amplitudes at and above the salt interface in the upper two panels are attributed to differences between finite-difference and one-way wavefield extrapolation. Finite differences better models amplitudes in the presence of velocity gradients in the propagation direction, and thus more accurately partitions energy at the top sediment-salt interface. The RWE wavefield underestimates the reflection contribution and allows significantly more energy to be transmitted into and through the salt body. This energy causes the additional multipathing noted in the RWE panel below the salt body, leading the more complicated wavefield behavior relative to the Cartesian wavefield example."

4) I have eliminated the discussion on grid anisotropy as this is a secondary issue that doesn't provided much more information.

5) I guess that having the boundaries are at steep angles is not really the issue here. Rather, it is just that there are boundary reflections in the propagation domain where they would not be expected in Cartesian extrapolation.

Implementation Costs

1) changed as suggested

2) changed as suggested

3) Sadly, there is not really really a good metric to tell whether the extra computational cost of the RWE approach is worth it for the extra angular bandwidth. This is something that has to be addressed when being applied in migration algorithms (and is something that I am currently working on ...)

4) changed as suggested

Appendix A

1) There is only 1 determinant; however, the way that I wrote it might look non-symmetric. This has been changed to the regular, more symmetric, form.

2) This is just for notational simplicity and does not have any real physical meaning.

Response to Reviewer 4

1) I have included additional sentences to address this point: "The 3D RWE acoustic wave equation derivation here deviates from that in Sava and Fomel (2006), who introduce nine coefficients expressing the spatial metric tensor derivatives. In this development, I choose to express all metric derivatives in a notation that remains close to the underlying geometry. "

2) The reviewer is indeed correct that I am showing the method "in action" on some more trivial results before getting more complicated. I have added the following introductory sentences before giving specific results: "This section presents numerical modeling examples that help validate the non-orthogonal RWE theory. I begin with the two basic 2D analytic examples of sheared Cartesian and polar ellipsoidal coordinates. I then present a method for generating singularity-free coordinate meshes and illustrate this approach with 2D and 3D Green's function modeling."

3) Generally speaking, singularities were generated in the original RWE implementation because grids had to conform on semi-orthogonal geometry (e.g. a field of rays). In this development, most of the singularities can be avoided just by generating non-orthogonal meshes (usually using analytic coordinate systems). This is the main thrust of the paper. Another issue is eliminating the remaining singularities in the subset of non-orthogonal meshes that are generated by a field of rays. This is the situation that I treat in the latter sections.

However, this aspect is somewhat secondary to the main thrust of the paper, and I feel that a detailed mathematical discussion on potential fields, and differential mesh generation would be well beyond the scope of the paper. (This is also treated in the Shragge reference for the previous years SEG Expanded Abstracts.)

I have made this more clear in the abstract by stating that non-orthogonal meshes can solve most of the singularity problems ("This paper develops a procedure that avoids many of these problems by posing wavefield extrapolation on smoother, but generally non-orthogonal and singularity-free, coordinate meshes. ") and that any remaining singularities can be removed with the additional method ("A method for eliminating any remaining singularities from coordinate systems is presented")