Stripe Patterns in Wind Forecasts Induced by Physics-Dynamics Coupling on a Staggered Grid in CMA-GFS 3.0 by Jiong Chen, Yong Su, Zhe Li, Zhanshan Ma and Xueshun Shen

This is a very interesting investigation into the cause of, and aspects of a remedy for, the appearance of stripes in wind forecasts. The authors:

- present the problem clearly,
- discuss the possible cause,
- present the results of an idealised model to support their hypothesis,
- develop a theoretical model to show what is going on,
- and then show the positive impact of one approach to avoiding the problem.

This is a great example of the scientific method at work and the results are all clearly presented.

I have only one comment that I have listed as a main point which is a suggestion for how I think the authors could give some more insight into what is going on and that would add some value to the presentation.

Other than that, I am happy to recommend publication subject to various rather minor comments/questions/suggestions listed below. The length of some of these might suggest a more major nature but my intention is that of Minor Revision.

Main point

The one aspect of the presentation that I think could be improved is for the authors to give the reader better clarity on what is going on and how it leads to the results seen.

In terms of the analysis section 3.4 there are two models of the damping process:

$$\Delta u^{\alpha}_{i+\frac{1}{2}} = -\Delta t\, \alpha_{i+\frac{1}{2}} u_{i+\frac{1}{2}}, \quad \dots (A)$$

and

$$\Delta u^{\alpha}_{i+\frac{1}{2}} = -\frac{\Delta t}{2}\left[\left(\overline{\alpha}_{i+\frac{1}{2}} + \Delta\alpha_{i+\frac{1}{2}}\right)\frac{1}{2}\left(u_{i+\frac{3}{2}} + u_{i+\frac{1}{2}}\right) + \left(\overline{\alpha}_{i+\frac{1}{2}} - \Delta\alpha_{i+\frac{1}{2}}\right)\frac{1}{2}\left(u_{i+\frac{1}{2}} + u_{i-\frac{1}{2}}\right)\right]. \quad \dots (B)$$

There are two steps that lead to noise being induced by process B.

The first step is common to both A and B. It is perhaps clear to many readers but I think it would be worth being explicit that applying either A or B to even (or perhaps especially) an initially constant field will induce a variation in that field that reflects the variation of α. Such an effect is clear from the results of the control experiment in Fig 7e where the initially uniform wind is damped to zero at one point but remains almost at its initial value upstream of that point – the horizontal variation of u_* is reflected in the wind field.

The next step in the argument is that for process B, the combination of horizontal variation in α and averaging of fields back and forth leads to a dispersive error in how the wind field evolves that is not seen for process A (for which the impact of the damping coefficient is purely local). That this is the case can be seen by rewriting process B as:

$$\Delta u^{\alpha}_{i+\frac{1}{2}} = -\frac{\overline{\alpha}_{i+\frac{1}{2}}\Delta t}{4}\left(u_{i+\frac{3}{2}} + 2u_{i+\frac{1}{2}} + u_{i-\frac{1}{2}}\right) - \frac{\Delta\alpha_{i+\frac{1}{2}}\Delta t}{4}\left(u_{i+\frac{3}{2}} - u_{i-\frac{1}{2}}\right). \quad \ldots(C)$$

In this form two things become apparent:

1. The first term is perhaps not surprising. It has the form of a second-order accurate horizontal diffusion scheme. Any $2\Delta x$ mode, that emerges by whatever route, is invisible to this term and so will not be damped at all. Other modes will also be impacted by the smoothing inherent in the 1-2-1 operator. (In contrast, process A will damp all modes equally as efficiently.)
2. More interestingly perhaps, the second term can be interpreted as a second-order centred-difference advection scheme where the advecting “velocity” is $\Delta x\Delta\alpha/2$. This term is therefore not damping at all. It is also the source of the dispersion issue: When applied to a field with a discontinuous field such a scheme is well known to create upstream propagating noise. Indeed, Fig 3.7a of Durran’s second edition of Numerical Methods for Fluid Dynamics is very reminiscent of the form of the result of process B shown in Fig 7e of the present work.

None of this says a lot more than the authors already present in their work but I feel that adding a brief discussion of the form (C) and making the analogy with centred advection (and its dispersion error and associated noise) might, for some readers, give a bit more insight into what is happening.

Minor comments

(Lnn refers to line number nn.)

1. L13: I am not convinced that the comment about the absence of noise in the static fields is quite correct – see my comment below about L177. I would suggest instead saying something like ‘the structure of the static fields in not consistent with the amplitude of the $2\Delta x$ noise if that noise were forced locally by the static fields’?
2. L80-83: The UK Met Office model does *not* follow this approach. It averages the winds to the cell centre but only uses these averaged winds to evaluate the boundary layer diffusion coefficient (the eddy diffusivity). It then averages the diffusion coefficient back to the wind points and performs the vertical diffusion at the wind points. This is the approach that I would recommend in solving the problem presented in this paper if your infrastructure can support such an approach.
3. L156-157: It would be good somewhere to comment on the stability of the model, i.e. is the amplitude of the stripes approximately constant in that they appear and remain approximately unchanged, or do they grow in time?
4. L177: Without further evidence to support this, I think ‘complete absence’ is too strong. It is clear that there is not the same visual level of noise at the $2\Delta x$ scale as in Fig 2 but that is not the same as there being a complete absence. And given that horizontal averaging of a field cannot create a $2\Delta x$ component then I think that the heart of the later argument lies in there being some forcing of such a component by the physics. It would be interesting to present a spectral analysis of these fields in the same way as Fig 9.

5. L182: It would be useful to the reader to give an indicative latitude and longitude for where the islands are.
6. L209, section 3.3: I think here one is looking for the simplest experimental set-up that shows the noisy behaviour. Given the theoretical model used in section 3.4 I would have thought that it would be best in this section to match as closely as possible that theoretical model, i.e. use a constant advecting wind (thereby losing the complication of nonlinearity) and use a constant eddy viscosity in the vertical (thereby losing the complication of nonzero values of dK/dz . If the hypothesis is correct then the equivalent figures to Figs 7a, b and e should be very similar.
7. L216: If the authors do retain a height varying K then it is probably worth saying that this is an example profile for the purposes of the idealised model rather than the profile used in the full model.
8. Equation (5): I believe there is a missing factor of u_*.
9. L220/236: To save the reader having to work it out for themselves, it would be good to state that the time step is 300s (if I have worked it out correctly!).
10. L241: It would be useful to say whether the noise grows or is stable.
11. L252, Fig 7: It would be worth stating in the caption that ‘x-grid’ means the grid point number not a distance.
12. L255-256: I am not convinced that the words used for either source of ‘noise’ are correct. Earlier it has been stated that there is a complete absence of noise in the surface fields of the full model! The surface forcing is what it is – it is the amplification, or exposure (through dispersion), of whatever $2\Delta x$ component that is at question. Also, I think it is debatable whether that averaging is an inconsistency; I would suggest that it is a discretization choice (albeit perhaps not a good choice!).
13. L272 and following lines: The authors have included one part of the averaging process, from the physics points (cell centres) to the velocity points (cell faces). They have then used Taylor series expansions to estimate the winds at the cell centres. It would be better (and no more difficult) to explicitly include the averaging of the winds to the cell centres and then use Taylor series expansions to obtain estimates for $u(x \pm \Delta x)$ in terms of $u(x)$. This has the effect of changing, in Eq. (10), the denominator 8 to 4, and the denominator 48 to 12. Although this is only a minor change in practice, it better reflects what the model of section 3.3 actually does, and perhaps more usefully, the terms that are proportional to $\Delta\alpha$ are proportional to $u' + \Delta x^2 u'''/6$ which are the terms that arise in a second-order centred advection scheme (as expected from the analysis suggested in the section Main Point).
14. L275: Strictly, Eq. (10) is only the solution to Eq. (8) if $\overline{\alpha}$ and $\Delta\alpha$ are taken as independent of x which they cannot both be over a periodic domain. You could either rewrite (8) in terms of $\overline{\alpha}$ and $\Delta\alpha$ and then postulate a different problem that has those coefficients constant, or simply say that the solution is approximate and only holds locally.
15. L277: Related to the above point, I think here (and everywhere they are used) $\overline{\alpha}$ and $\Delta\alpha$ should be written as $\overline{\alpha}_{i+1/2}$ and $\Delta\alpha_{i+1/2}$ to make their horizontal dependency clear.
16. L277: It would be much better to not include the factor of 1/2 in the definition of $\Delta\alpha$ but carry that explicitly where $\Delta\alpha$ is used. Otherwise the definition is not consistent with the use of Δ elsewhere, e.g., consider what happens if $\alpha \equiv x$, we would then end up with $\Delta x = \Delta x/2$!
17. L280: It would be worth reminding the reader somewhere that the Taylor series expansion is only valid for small values of $k\Delta x$. Otherwise, when $k\Delta x = \pi$, as it can do,

the solution (both (10) and (11)) would be predicted to grow unlimitedly since $\pi^2/8 - 1 > 0$.

18. L303: The approach of this section makes sense and is a nice way of showing that the noise can be controlled. However, it is perhaps worth pointing out that the approach used to do so introduces an arbitrary bias in the direction in which the now piecewise-constant sampling is done, i.e. why choose $u(x)$ instead of $u(x + \Delta x)$? I appreciate that this would not be practicable as a quick experiment, but a better approach might be to always sample upwind, e.g., $u_p(x + \Delta x/2) = u(x)$ when $u > 0$ and $= u(x + \Delta x)$ otherwise.
19. L310: Please say whether this is also applied for the reverse mapping from the physics point back to the dynamics point.
20. L342: I feel that there is still an interesting level of difference in the spectra over the sea, more than 'remarkable consistency' would suggest.
21. L401-409: It is a shame if it is not possible within CMA-GFS to apply the boundary-layer code to wind fields on their native points. This would seem to me to be the preferred solution rather than reintroducing horizontal diffusion. And, as I noted earlier, this is the approach taken within the Met Office's Unified Model.

Typos/editorial comments

1. L13: 'dynamical core' is more standard than 'dynamic-core'
2. L21: 'physics-dynamics' is more standard than 'physics-dynamic'
3. L130: word missing from 'therefore systematically these'
4. L243: 'Compared' should be 'Comparing'
5. L280: There is an overbar missing from the α
6. L284: The exponent in the middle term, together with the factor i, need to be removed

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