

Interactive comment on “Spiciness theory revisited, with new views on neutral density, orthogonality and passiveness” by Rémi Tailleux

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Response to Referee 2

General Comments This manuscript aims to clarify the theoretical foundation for a spiciness variable sought by many oceanographers. The main idea is that, before considering spiciness, one must first construct a good neutral density variable that is materially conserved, and then most any materially conserved function can be used to construct a spiciness variable, simply by constructing its anomaly along neutral surfaces. The author also clarifies that pursuing orthogonality of spiciness and neutral density in $S - \Theta$ space is misguided, and instead that the goal should be orthogonality of their gradients in physical space. Unfortunately, many of the advances of this paper

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are overstated, either lacking justification, detail, or novelty. There are several logical errors as well. These are discussed below. I believe this manuscript has the potential to nicely tie together the theory of spiciness variables, but Major Revisions are required to get there.

Response and proposed changes I thank the referee for his/her careful and comprehensive review, as well as for some thought provoking comments. In addition to addressing the numerous specific suggestions as detailed below, I suggest to implement the following main changes to address his/her most important comments.

1. Rephrasing the abstract somewhat and link the present work with the previous work by Jackett and McDougall (1985). Throughout the manuscript, articulate better the similarities and differences with JM85 and include citation and discussion of McDougall and Giles (1987).
2. Improve the discussion of how to construct $\xi_r(\gamma)$, including a discussion of global versus local considerations
3. Improve the discussion of what is exactly meant by orthogonality in physical space and improve its theoretical justification.
4. Rephrase the various parts that appear to be causes of confusion.
5. Clarify what is meant by 'optimality'
6. Clarify what is meant by first principles justification of neutrality

In the following, I provide some more detailed responses. I did not respond to the more technical comments, which will be done when invited to revise my paper.

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- One of the major points of the paper, that what matters for spiciness is actually the neutral density variable, was made by Jackett and McDougall (1985). The author has acknowledged this in some places, but a reader could easily get the impression that this idea owes to this manuscript. A stand-out example is in the abstract (line 5): stating "contrary to what is usually assumed" is unfair. Anyone who has read Jackett and McDougall (1985) would not assume this. This phrase should be removed, and a citation to Jackett and McDougall (1985) given in the abstract.

Since the referee emphasises rigour and accuracy throughout his/her review, it seems important to point out that my point is actually that what determines the dynamical inertness of spiciness is the degree of neutrality of γ , which is quite different from what the referee says. Moreover, last time I checked, the term 'neutral density' is nowhere mentioned in Jackett and McDougall (1985) (JM85). Rather, JM85 use the term 'isopycnal surfaces', stating at the beginning of their paper: "In this paper we approximate the isopycnal surfaces by surfaces of constant potential density which we call ρ ." Moreover, JM85 also state: "the variations of any variable, when measured along isopycnal surfaces, are dynamically passive [...]". Since such a statement is technically true only on surfaces of constant in-situ density, there is no logical reason why readers would conclude that by 'isopycnal surfaces' JM85 actually mean 'neutral surfaces', especially as neither neutral surfaces nor patched potential density surfaces were yet in widespread use at the time. Even when such surfaces started to become popular, I am not aware that they have never associated with the dynamical inertness of spiciness before my paper.

I don't understand how my "contrary to what is usually assumed" is unfair to JM85, since what such a statement criticises is past studies contending that orthogonality and dynamical inertness are somehow connected, e.g., Veronis (1972). This is obviously not the case of JM85, which my paper praises for being the first to recognise that orthogonality is not connected with dynamical inert-

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ness. This being said, I agree that JM85 deserve a place in the abstract, not for promoting the use of neutral density, but for being the first to recognise the lack of connection between orthogonality and dynamical inertness, as well as for being the first to propose describing spiciness in terms of the isopycnal anomaly of some thermodynamic variable. As regards to the latter point, I don't think I give enough credit to JM85, which I plan on correcting in revising my paper.

As a final remark, I'd like to stress that I am not claiming credit for the idea that spiciness should be used in conjunction with neutral density, because there is no ambiguity that this is implied explicitly or implicitly in most if not all spicity/spiciness papers written post JM85, as is evident in Huang (2011) or McDougall and Krzysik (2015) for instance. I only claim credit for spelling a logical and rigorous argument establishing the link between dynamical inertness and neutrality, which had never been explicitly stated before, as far as I know.

- I find Fig 11 the most interesting aspect of this work. It is essentially a global test of the Jackett and McDougall (1985) idea, repeated here, that it is the anomaly ξ' is dynamically inert. The author's anomaly is defined as relative to a global isopycnal average. The results are evidently meaningful, but it is not entirely clear that more refined results could be obtained by refining the averaging procedure. McDougall and Giles (1987) argued in favor of studying property (salinity) anomalies relative to a local isopycnal average. To study a particular water mass intrusion, the state of the ocean far away should be irrelevant. It would therefore be prudent of the author to discuss the utility of using global isopycnal averages, and to locate the present work relative to the earlier work of McDougall and Giles (1987). I fully agree with the referee and thank him/her for pointing out the highly relevant study by McDougall and Giles (1987), which I was not aware of. I also agree that local versus global definitions of $\xi_r(\gamma)$ need to be discussed in the revised version of my paper.
- Moreover, it would be interesting to add another panel to Fig 11 that tests the

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anomaly of a state variable that is specifically designed to be quite poor as spiciness-as-a-state variable but nonetheless may appear comparably good as spiciness-as-a-property (anomaly).

I am not entirely sure how to construct such a variable or how useful that would be, since it seems to me that Θ is naturally a poor choice of spiciness-as-a-state variable, which performs much better as an isopycnal anomaly. I therefore don't plan on pursuing the referee's suggestion.

- In addition to the question of which geographic data should enter the construction of the $\xi_r(\gamma)$ function, the question of how this data is used must also be asked. Early on in the paper, the author describes this as the isopycnal mean, which presumably implies an arithmetic mean (this should be clarified). However, Section 4 seems to make this more general, stating only that $\xi_r(\gamma)$ is a "suitably constructed function of density only". Should we use an arithmetic mean? If so, why? If we define ξ_r as the best such function, in some kind of a least-squares sense, would we discover that it is an arithmetic mean? Fig 8 provides a trivial example where γ and ξ are linear functions of space. Obviously, the real ocean presents a far more nonlinear problem, for there will not be a suitable function $\xi_r(\gamma)$ that renders $\nabla\xi'$ orthogonal to $\nabla\gamma$. Unless this general issue can be addressed, Section 4.1 is not of great theoretical or practical interest.

Until recently, I must confess that I had no very definite ideas about the best way to construct $\xi_r(\gamma)$ or whether such a best way existed, which is what I had left the question as an open question in my discussion/conclusions. The term 'suitably constructed' was meant to leave it up to the reader to choose what they think best for their particular application, my hope being that referees more expert than I be would pick up on it and perhaps give me some pointers. However, upon giving the matter further thought, it is becoming clearer to me that in order for my framework to be self-consistent, there is only one logical way to construct $\xi_r(\gamma)$, namely as the function that maximises the orthogonality of $\nabla\gamma$ and $\nabla(\xi - \xi_r(\gamma))$.

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Although there is no unique way to construct a cost function, one that is natural and easy to minimise analytically is:

$$E = \int_V [\nabla\gamma \cdot \nabla(\xi - \xi_r(\gamma))]^2 dV = \int_V [\nabla\gamma \cdot \nabla\xi - \xi'_r(\gamma)|\nabla\gamma|^2]^2 dV \quad (1)$$

Minimising such a cost function with respect to $\xi'_r(\gamma)$ leads one to define the latter as an exact solution of the following problem:

$$\int_V |\nabla\gamma|^2 (\nabla\gamma \cdot \nabla\xi - \xi'_r(\gamma)|\nabla\gamma|^2) dV = 0. \quad (2)$$

In the case where exact orthogonality can be enforced, as in the idealised case depicted in Fig. 8, $\xi'_r(\gamma)$ is defined by

$$\xi'_r(\gamma) = \frac{\nabla\gamma \cdot \nabla\xi}{|\nabla\gamma|^2} \quad (3)$$

The above problem defines $\xi_r(\gamma)$ up to an integration constant, which one may fix by imposing that the resulting $\xi_r(\gamma)$ minimises the integral $\int_V |\xi - \xi_r(\gamma)| dV$. As a result, I don't think that the current way I have constructed $\xi_r(\gamma)$ in my paper, namely in terms of a smoothing interpolating polynomial, is well justified. I am currently exploring the possibility of constructing $\xi_r(\gamma)$ as per the method outlined above, and propose to revise my paper accordingly.

- Section 3 provides one way (among many) to nonlinearly scale the $S-\Theta$ diagram so that both axes have common units [density], such that there is a well-defined spiciness variable τ_{\ddagger} that is orthogonal to density on this diagram. However, τ_{\ddagger} is subsequently dropped from the manuscript. It is claimed (line 168) that τ_{\ddagger} is similar to τ_{jmd} , but this is not proven or shown numerically. This manuscript would be considerably stronger if τ_{\ddagger} were tested in Section 4.2 and shown to have some advantage over other spiciness/ spicity variables (including τ_{jmd} , which it

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may well turn out to be very similar to). Otherwise, Section 3 seems to be of limited utility.

I am puzzled by this comment, because the similarity of τ_{\ddagger} to τ_{jmd} is established in Figs. 5 and 6, as well as indirectly in Fig. 7. I did not use τ_{\ddagger} further in my paper because oceanographic sections obtained with it were indistinguishable from those obtained with τ_{jmd} . The primary advantages of τ_{\ddagger} over τ_{jmd} , apart from its associated Jacobian being non-zero over all of (S, θ) space unlike that for τ_{jmd} , are practical and due to its continuous dependence on pressure and having an exact mathematical form in terms of in-situ density that clarifies its dependence on its arbitrary tunable parameters. Since available software for τ_{jmd} is only limited to a couple of reference pressures, this makes τ_{\ddagger} considerably more flexible to use in practice.

- The theoretical argument, opening Section 3, reaches the conclusion that the $S - \Theta$ axes should be rescaled to have density as their common units, but this is commonly known. Huang et al (2018) pursues this, for example. The author does not make it clear why this rescaling of the $S - \Theta$ diagram is superior to other rescalings, even linear ones.

I am puzzled by this comment because my argument is actually the other way around. Indeed, the starting point of my argument explicitly recognises that many investigators have sought to re-scale $S - \Theta$ in density coordinates, however without really justifying it other than by invoking naturalness or convenience. This is expressed Line 120-121 by "So far, studies that have pursued orthogonality in one form or the other have taken it for granted that such a joint system of units should be based on density unit, but without really proving it." What Section 3 explores is whether it is possible to make a stronger and more rigorous case for a re-scaling in density units. I argue that this is possible, the key being to remark that (Lines 122-123 and Equation 4) that "the isopycnal variations of any material

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function $\xi(S, \theta)$ on any given density surface $\gamma(S, \theta) = \text{constant}$ satisfy"

$$d_i \xi = \frac{J}{\gamma_S \gamma_\theta} \gamma_S d_i S = -\frac{J}{\gamma_S \gamma_\theta} \gamma_\theta d_i \theta$$

(my equation 4). This result is important because it states that the isopycnal variations of ξ are all proportional to the quantity $\gamma_S d_i S = -\gamma_\theta d_i \theta$, which has density units, regardless of ξ .

The only point I am trying to make in this section that it is possible to regard the construction of 'spicity'-like and spiciness-like variables as fundamentally relying on similar theoretical foundations, since the two can be interpreted as being orthogonal to density in re-scaled $(X(S), Y(\theta))$ coordinates, the former for a linear rescaling, the latter for a nonlinear one. The referee may be right to think that there is therefore no particular advantage of the nonlinear re-scaling over the linear re-scaling, since $X(S)$ and $Y(\theta)$ rely on the specification of arbitrary constants regardless of how they are constructed, suggesting that spiciness is as arbitrary as spicity in some sense. So far, however, Jackett and McDougall (1985), Flament (2002) or McDougall and Krzysik (2015) have argued that spiciness-like variables are superior to spicity-like variables, even if the physical basis for their arguments is not crystal clear, to say the least.

- In terms of structure, Section 4.2 "Illustrations" is more of a "Results" section, and does not fit well with the theoretical Section 4.1. I recommend splitting Section 4 into two sections, and expanding both, as described above.

I need more time to ponder about to restructure my paper, as I also need to account for the other referees' remarks.

- The author claims that the anomaly ξ' "is the variable optimally suited for characterising ocean water masses" (line 4-5). However, this is not proven, nor is there any discussion about how such optimality would be measured. Claims of optimality appear in several other places in the manuscript. I recommend this loose

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language be qualified and proved, or else changed.

[I agree with this comment. I need to ponder how to best address it in my revision.](#)

- Another one of the major results claimed is that this paper presents a "rigorous and first-principles theoretical justification for... a globally-defined material density variable $\gamma(S, \theta)$ maximising neutrality" (e.g. lines 10, 115-116, 251-252). However, this justification is predicated upon the desire of oceanographers to have a spiciness variable. Though such a variable may be useful to possess, it does not itself have a rigorous and first-principles theoretical foundation, and so cannot be leveraged to justify such a γ .

[I don't think it is true that my theoretical justification for \$\gamma\$ is "predicated upon the desire of oceanographers to have a spiciness variable"; nevertheless, I need to ponder somewhat more about the referee's view to establish whether I agree with it or not. I'll try to address the issue explicitly in my revision.](#)

Specific Comments

- 29: Another citation for thermobaric instability would be apt, here, such as Ingersoll (2005; JPO).

[I agree that this is a good reference to cite](#)

- 71: Some additional conditions are necessary to make this example true. As counterexample, take $\gamma(S, \theta)$ and $\xi(S, \theta)$ as constants: both are material, but the given d does not satisfy property 2, since two distinct points (S_1, θ_1) and (S_2, θ_2) would nonetheless have $d = 0$.

[I think the counterexample is not in the spirit of the paper](#)

- 80: Please provide further detail on the derivation of $\gamma_S S' + \gamma_\theta \theta' \approx 0$. Is one supposed to take the gradient of $\gamma(S - S', \theta - \theta') = \gamma_0$ in the neutral tangent plane, and assume that γ is an approximately neutral density variable? This

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would lead to $\gamma_S \nabla_n S' + \gamma_\theta \nabla_n \theta' \approx 0$, but this differs from the stated equation by the presence of gradients. It is not clear whether the condition $\gamma(S_r(\gamma_0), \theta_r(\gamma_0)) = \gamma_0$ is necessary "for all γ_0 ", or just the γ_0 under consideration.

[I will do so in the revision](#)

- 93-94: Fig 2 does not show, as stated, that "the ability of a variable to characterise water masses is proportional to the degree of orthogonality between $\nabla \xi$ and $\nabla \gamma$..." It simply shows that the spatial gradient of different candidate spiciness variables make different angles with $\nabla \gamma$. Fig 2 can only be interpreted as the author desires by referencing the interpretation of Fig. 1, that SA is a better spiciness variable than the other two. Even still, this is merely an interpretation or a "suggestion" at this stage.

[Fair enough](#)

- Eq. (1): Please provide some details on the derivation of this equation. Tailleux (2016a) also lacks such details.

[This equation is obtained by the Jacobi method. It is explained in much details in Appendix A of Feistel \(2018\): Thermodynamic properties of seawater, ice and humid air: TEOS-10, before and beyond. Ocean Sciences, 14, 471–502.](#)

<https://os.copernicus.org/articles/14/471/2018/os-14-471-2018.pdf>

- Eq. (4): d_i needs to be defined. Also, it needs to be stated that this assumes is a perfectly neutral density variable, rather than "on any given density surface..."

[The expression is correct as stated. \$d_i\$ is defined by introducing the isopycna/diapycnal decomposition \$\nabla F = \nabla_i F + \nabla_d F\$ for any scalar function \$F\$, in which case we have \$dF = \nabla F \cdot dx\$ and \$d_i F = \nabla_i F \cdot dx\$. I am a little bit surprised that this is needed.](#)

- Eq. (6): Units error in the middle expression. X and Y have units of density, so cannot be added to the unitless value 1, which should be ρ_{00} .

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- Eq. (11): This isn't really the total differential of τ_{\ddagger} if it's at fixed pressure.
- 165: τ has not been defined. All that can be said is that τ_0 is an arbitrary constant with units of density, and that $\tau_{\ddagger}(S_0, \theta_0, p) = \tau_0$.
- 168: τ_{\ddagger} is the exact solution to an approximate differential equation, but this does not mean τ_{\ddagger} is an approximate solution of the exact differential equation. Here, Eq. (11) is the "approximate differential equation", which approximately matches (exactly in form, approximately in coefficients) with the "exact differential equation" set out by Jackett and McDougall (1985). If this logic were true, chaos (theory) would not exist.

The referee has lost me

- Eq. (14): How did ρ_{00} become ρ_0 ? I assume the neutral relation $\nabla_i \theta = \alpha(S, \theta, p) \beta(S, \theta, p)^{-1} \nabla_i S$ was used, but this provides the second equality in (14) only if $\rho_0 = \rho_{00}$.
- 196-7: Here, the author states that Section 3 showed spiciness can be theoretically justified to be orthogonal to density in thermohaline space, but elsewhere (e.g. line 208) stated that orthogonality in thermohaline space is "fundamentally ill-defined". This is confusing, to say the least. I remain unconvinced that Section 3 delivered what has been advertised here (line 196-7). Rather, Section 3 just showed that we can define an alternative, but only approximate, equation of state under which orthogonality in thermodynamic space is well-defined. This does not answer the theoretical questions surrounding spiciness in the real ocean.

The referee misunderstands what Section 3 is about. As we all agree, spiciness is a property that is best understood as an anomaly $\xi' = \xi - \xi_r(\gamma)$ for some thermodynamic property and suitably constructed $\xi_r(\gamma)$ to be defined. The theoretical questions that surrounds spiciness in the ocean are therefore:

1. Is there theoretical advantage or justification in using any particular form of

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ξ over another one, or is one free to choose whatever ξ we like? Assuming that there exists some better choice of ξ , should it be defined along the lines suggested by Jackett and McDougall (1985), Flament (2002), McDougall and Krzysik (2015), along the lines suggested by Veronis (1972), Huang (2011), Huang et al (2018), or along some other lines still to be discovered?

2. Once one has settled on a particular choice of ξ , what is the best way to construct $\xi_r(\gamma)$ and if so, what is its theoretical justification?

As regards to point (1), it is clear that Jackett and McDougall (1985), Flament (2002) and McDougall and Krzysik (2015) believe that there are advantages in defining ξ' based on a dedicated spiciness-as-a-state function constructed along the lines that they propose. This is especially evident in the last part of Jackett and McDougall (1985), who argued that their τ'_{jmd} is better than S' or τ'_v , the latter being based on Veronis (1972) variable. By 'fundamentally ill defined' I meant that such a construction has no intrinsic physical meaning since the construction of $X(S)$ and $Y(\theta)$ involve the specification of arbitrarily defined constants regardless of how these are defined, even when these are chosen to be nonlinear.

- 204 and Eq. (15): This is introduced a bit sloppily. No definition is given for \hat{f} so the reader is left to figure that out by understanding Eq. (15) and/or by comparison with $\hat{\rho}$ earlier. Also, $\partial \hat{f} / \partial p = \partial f / \partial p$ is used but not stated in Eq. (15), which would probably benefit by using the latter in the middle expression. Actually, since the same thing appears in Eq. (16), it may be better to simply provide an equation that does nothing more than define $\hat{\nabla}$, thereby eliminating these multi-part equations (15) and (16).

I'll take the referee's comment into account when revising the paper.

- 206: "efficient" does not seem like the right word here. Maybe "compact"?
May be

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- 230: What is meant by "the values of σ_1 contours retained in the nonlinear regression"? Is only some of the data shown in Fig 9 actually used in the nonlinear regression that produces its red lines? And the data that is used has σ_1 values between the largest and smallest of the thick black contours in Fig 10? The caption of Fig 10 helps support this interpretation, but even there it is confusing: the restricted range of σ_1 used to compute the nonlinear regression should be defined by two σ_1 values (a lower and upper bound) rather than four values (the thick contours).
I believe most readers will understand that the lower and upper bounds are given by the leftmost and rightmost contours.
- 250: Jackett and McDougall (1985) should be cited here.
Agreed
- 266-7: What would happen if you used a non-constant reference pressure for τ_{\ddagger} , as suggested here? Actually, it's not clear what this even means: where does a reference pressure fit into τ_{\ddagger} ?
The idea here is to use $\tau_{\ddagger}(S, \theta, p_r(S, \theta))$, which would be the natural approach for use with $\gamma^T = \rho(S, \theta, p_r) - f(p_r)$.
- 270: This claim, that Tailleux (2016b)'s density variable "maximizes neutrality while also being the only one that accounting for thermobaricity", is unfounded. Tailleux (2016b) only compared the neutrality of his density variable against a select few competitor density variables, namely two potential density variables, γ^n of Jackett and McDougall (1997), and a rational approximation of γ^n defined by McDougall and Jackett (2005; JMR). Conspicuously missing is the orthobaric density of de Szoeke et al (2000), not to mention the neutral density of Eden and Willebrand (1999). Moreover, since Tailleux (2016b)'s density variable was custom-built to mimic n of Jackett and McDougall (1997), and the latter exhibits better neutrality (Fig 6 of Tailleux (2016b)), it is unclear how the author can make

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this claim even if orthobaric density had been tested

Orthobaric density is not a purely material variable so does not fit in the present framework. However, McDougall and Jackett (2005) γ_a and Eden and Willebrand (1999)'s variable are presumably affected by thermobaricity on account to the way that they are constructed, so I agree with the referee that I was imprecise.

- 275-6: The author has not shown that ξ' appears to be insensitive to the particular choice of $\xi_r(\gamma)$, since only one method for empirically constructing $\xi_r(\gamma)$ was tested, namely the (arithmetic?) mean.
The use of the verb 'appears' makes my statement a subjective one. Nevertheless, I can try to be somewhat more precise in the revision.
- 278: Isn't ξ' conservative by definition? Since ξ and γ are assumed to be conservative throughout this manuscript, then ξ' should be too.
The term conservative is used as in McDougall (2003) paper on Conservative Temperature. A conservative variable H is one that satisfies an equation of the form

$$\frac{DH}{Dt} + \nabla \cdot F_h = 0, \quad (4)$$

γ and ξ are assumed to be material, not necessarily conservative. γ certainly is not since it is affected by cabbelling and thermobaricity potentially.

- Fig 2: The source data should be restricted to be between, say, 500 and 1500 dbar, to remain near the reference pressure of σ_1 .
I disagree as the point of the figure is to show the distribution of water masses as represented by different spiciness variables.
- Fig 2: The colors are a bit confusing. In the caption, spiciness and spicity are described as brown and orange, respectively – quite similar colors! This seems (to me) to describe more how they appear in the histogram when blended with

C14

other colors, not how they are in the legend.

I'll correct this in the revised version of the paper.

- Fig 9: It is nearly impossible to get much information from these panels. It is likely that most of what we see is due to outliers, and the vast majority of the data is lying on top of itself. Instead of a simple scatter plot, I suggest using a 2D histogram.
I don't understand what would be the purpose of what the referee suggests.
- Fig 11: The colorbars all range between -2 and 2, but the units vary across panels. It would be better to let each colorbar cover the entire range of its variable, or perhaps to cover the variable's range up to two standard deviations, say.
Dr. Zika suggested to normalise each variable by its standard deviation, which I think is a good idea to address the problem.
- Fig 11: Caption: Which contours of σ_1 are shown in white?
This will be clarified in the revised version of the paper

Technical Corrections Thank you for these. These will be taken into account while revising the paper.

- 4-8: "The key results are:" should be "The key results are as follows." and each key result that follows should be a separate sentence. (What comes before a colon must be a complete sentence.)
- 9-10: Same issue as above.
- 19: behaves -> behave
- 28: sopycnal -> isopycnal

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- 48 and 53: At this stage, it's unclear why or when "potential" should appear before "spicity" and "spiciness".
- 50: remove "in general"
- 56, 277, 278: question mark should be a period, or rephrase so that a question is actually asked, rather than stating what the question is.
- 61: signal -> signals
- 67: The statement "checked in any good mathematics textbook" is rather cavalier, and would be better omitted. Simply naming the mathematical object d as a metric is enough.
- 69: Using "1" and "2" to identify data leads to the unfortunate notation of $d(1; 2)$. I'd suggest using A and B instead of numbers.
- 72: The definition of f_i is quite confusingly written, since (γ, ξ) is really meant to say γ or ξ
- 105: This is usually called the "dianeutral vector" not the "neutral vector".
- 120: join \rightarrow joint
- 125: typo in the inline equation: the first S should be θ
- 125: J has already been defined and does not need to be stated again.
- Eq (14) and line 191: τ should be τ_{\ddagger} .
- 192: brackets -> braces in Eq. (14).
- 207: tilde is placed incorrectly, should be over ∇

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- 259: all -> are all
- 260: "the one used in this study": it's unclear what "one" is referring to, since four candidates were tested, and the author's own variable was also presented.
- 263: "as the ... variable" → "as ... variables"
- 264: mimic → mimics
- Fig 2: The x axis label is missing two gradient symbols, in front of σ_1 and ξ . Also, "11" -> "1". Also, "less" → "least".
- Fig 9: "Fig. 11" → "Fig. 1". Also, shouldn't "spiciness" and "spicity" be changed to "potential spiciness" and "potential spicity" throughout this caption? Also, the subscript for τ_{\ddagger} is sideways on the y-axis label of panel (a).
- various: showed → shown

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