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

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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–Θ 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 are overstated, either lacking justifi-

cation, 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.

One of the major points of the paper, that what matters for spiciness is actually the neutral density variable γ, was made by Jacket 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.

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' that 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).

Moreover, it would be interesting to add another panel to Fig 11 that tests the 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).

In addition to the question of which geographic data should enter the construction of the ξ_r 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.

Section 3 provides one way (among many) to nonlinearly scale the S–Θ diagram so that both axes have common units [density], such that there is a well-defined spiciness variable $\tau_\ddagger$ that is orthogonal to density on this diagram. However, $\tau_\ddagger$ is subsequently dropped from the manuscript. It is claimed (line 168) that $\tau_\ddagger$ is similar to τ_{jmd}, but this is not proven or shown numerically. This manuscript would be considerably stronger if $\tau_\ddagger$ were tested in Section 4.2 *and* shown to have some advantage over other spiciness/spicity variables (including τ_{jmd}, which it may well turn out to be very similar to). Otherwise, Section 3 seems to be of limited utility. The theoretical argument, opening Section 3, reaches the conclusion that the S–Θ 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–Θ diagram is superior to other rescalings, even linear ones.

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.

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 language be qualified and proved, or else changed.

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.

Specific Comments

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

71: Some additional conditions are necessary to make this example true. As counter-example, 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 $d=0$.

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 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 current consideration.

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 S_A is a better spiciness variable than the other two. Even still, this is merely an interpretation or a "suggestion" at this stage.

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

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

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}.

Eq. (11): This isn't really the total differential of $\tau_\ddagger$ 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: $\tau_\ddagger$ is the exact solution to an approximate differential equation, but this does not mean $\tau_\ddagger$ 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.

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.

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* $\tilde{\nabla}$, thereby eliminating these multi-part equations (15) and (16).

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

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).

250: Jackett and McDougall (1985) should be cited here.

266-7: What would happen if you used a non-constant reference pressure for $\tau_\ddagger$, as suggested here? Actually, it's not clear what this even means: where does a reference pressure fit into $\tau_\ddagger$?

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 γ^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 this claim even if orthobaric density had been tested.

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.

278: Isn't ξ' conservative by definition? Since ξ and γ are assumed to be conservative throughout this manuscript, then ξ' should be too.

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.

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 other colors, not how they are in the legend.

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.

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.

Fig 11: Caption: Which contours of σ_1 are shown in white?

Technical Corrections

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

48 & 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 -> 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 $\tau_{\ddagger}$.

192: brackets -> braces in Eq. (14).

207: tilde is placed incorrectly, should be over ∇.

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 $\tau_{\ddagger}$ 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 $\tau_{\ddagger}$ is sideways on the y-axis label of panel (a).

various: showed -> shown

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