Are there principles to guide eddy parameterizations?

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Abstract. A “list as long as your arm” has described the range of principles that may be attempted for possible eddy parameterization schemes. We feel uncomfortably an attitude of “try and see” whether any particular principle “works” in any particular application. This note summarises a discussion which followed the body of presentations at ‘Aha. We ask if principles from general physics, especially notions of 2nd Law and entropy, can help clear a way. We ask if such ideas offer practical means to advance practical knowledge, and where major impediments may lie.

A discussion

These pages follow notes taken throughout ‘Aha Huliko’a, and from ideas discussed in part during the ‘Aha discussion time. The issues were framed by David Marshall and further by Peter Killworth. Surveying approaches that have been taken to providing a basis for eddy parameterization, one is daunted by the length of the list and the tentative character of the entries. What “should” eddies do? On the list we find (as examples)

Eddies should flatten isopycnals.

Eddies should maximally dissipate APE (available potential energy).

Eddies should mix PV (potential vorticity).

Eddies should mix layers thickness along layers.

Eddies should maximally dissipate enstrophy.

Eddies should reduce MKE (mean kinetic energy).

Eddies should relax toward certain rectified (“neptune”) flows.

… and so on.

“Should” eddies do any of this? While we pose this list in terms of eddy parameterizations, another ‘Aha topic – stratified mixing – would generate yet another list. To “help”, Bill Young inserted an estimate of the number of active degrees of freedom in the ocean, suggesting 10^{28} “per cell” or 10^{37} if one includes biology. (There was a little dispute about numbers, but the key message is the numbers are “big” – far, far bigger than modern supercomputers are able to prognose, which are more like 10^7 to 10^9.) Given such circumstances, Walter Munk asked if we deem the situation hopeless.

Answering Walter’s question depends upon what ocean modelling seeks to do. If our modelling project amounts to trying to invent a steam engine from molecular dynamics simulation of water vapour, it may well be hopeless in our lifetimes. If we would invent a steam engine based on thermodynamic functions, in part from empiricism and in part from statistical physics, it may not be so hopeless. Importantly we need to turn the huge number of degrees of freedom from threat to opportunity.

When we recognize that we’ve no ability nor practical interest to know the ocean in all its 10^{37} (or whatever) details, we naturally turn to probabilities of oceans. Mel Briscoe asked if we would predict evolution of probabilities distributions or if we limit attention to moments (expectations) from those probabilities. Framing issues in terms of moments might render the task manageable?

Lessons in our coffee cups?

A difficulty may be in part “cultural” insofar as we, as a community, have little orientation toward statistical physics, basing ocean dynamics instead on the classical mechanics of GFD amended with sundry by-guess-and-by-golly mixing coefficients. During ‘Aha two other themes recurred. We were reminded of the oft-cited stirring cream into coffee. And we were reminded of the influence Carl Eckart brought, seeking to base physical oceanography upon underlying physics. Although we cannot invite Carl’s direct input, we might seek in a spirit after Carl to ask why does cream in coffee turn brown. Here I only substitute my own comment. I should hope the answer is not because stirring causes enhanced mixing (diffusion). I should hope the answer is that internal interactions within the coffee cup transition the probabilities of cream and of coffee to a distribution with higher entropy. Practically, the useful representation of this idea may well be that stirring leads to enhanced mixing. We see this as a result following from the underlying basis, after which we might quasi-empirically parameterize cups, teaspoons, manners of agitation, etc.

In the case of stirring cream into coffee, one is quite inclined merely to nod to the entropy discussion before proceeding directly to parameterizing the stirring-mixing. Were the topic of ocean eddies this simple, we would hardly speak of so simple a matter at ‘Aha Huliko’a. But eddies are not simple. Then stirring-mixing intuitions, post-hoc modified by criteria such as listed at the outset of this note, are notoriously unreliable. Was there something more to learn in our coffee cups?

Dynamics of moments of probable seas

The following story is not yet clear, in part because the methods are so little explored. Here I make a sketch, indicating some research directions, results, and relations to issues David and Peter framed.

First we embrace the idea that oceans are known only in probability. The detailed state of the ocean might be expressed in a state vector **y**, whose dimension could well be 10^{37}, or whatever. We don't know y. We only speak of elemental probability $dP=P(\mathbf{y})dy$ that the actual value of **y** falls within a phase volume dy about any given **y**. So far the discussion is aerie-faerie. What we really would like to access are moments of P, such as $\mathbf{Y}=\int \mathbf{y}dP$ with the integral over all **y**. Importantly, the dimension of **Y** need no longer be 10^{37}. We can “project out” as much of **y** as we care not to consider, perhaps taking as only “lumped” (space-time averages) over **y**. Dimensions of **Y** might be only 10^7 or 10^3 or maybe only 10. The key consideration is that these **Y** are moments of probabilities, as Mel contemplated. That distinction can get lost when, for example, we look at output of GCMs and see maps of velocities, temperatures, elevations or whatever. Even when we admit that the **Y** are grid-cell-averaged variables, we tend still to speak of “velocity” rather than “velocity moment of probability of”, for example. So what?

The “so what” gets us when we write dynamic equations. Too easily we look up equations for velocity or temperature or such from textbooks, the only ambiguity arising from nonlinearities which, averaged over space-time volumes need some closure “approximations”. Were we to ask instead for the equation of motion of the temperature moment of the probable state, say, we might (1) grow tired and (2) pause on our way to the textbook. Until we are clear what are the dependent variables in the problem, assuming equations of motion is premature.

Next steps are, in part, familiar. Linear terms in equations of $d\mathbf{y}/dt$ commute with expectation operators, so linear terms in $d\mathbf{Y}/dt$ are “as usual”. Nonlinearities in $d\mathbf{y}/dt$ can be expressed in parts as corresponding nonlinearies among components of **Y**, which again may look familiar following “usual” Reynolds averaging. And there is “more”, the “stuff” that connects the **Y** to all the P(**y**) which we do not know. $d\mathbf{Y}/dt=\mathbf{f}(\mathbf{Y})+\mathbf{X}$, where “f” are “familiar” terms from textbooks and “X” are the new unknowns.

Two route to “X”

The question of “X” should be seen in context of nonequilibrium statistical mechanic, a gloriously unsolved problem. There are two avenues. I have tended to follow Lars Onsager, seeing in “X” the generalized thermodynamic forcing $\mathbf{X}=\boldsymbol{\kappa}\bullet\nabla_{\mathbf{Y}}S$ where $S=-\int \ln(P)dP$ is entropy, $\nabla_{\mathbf{Y}}S$ denotes the gradient of entropy with respect to the **Y**, and $\boldsymbol{\kappa}$ supplies the coupling with which $\nabla_{\mathbf{Y}}S$ forces $d\mathbf{Y}/dt$. As we don't know P, hence we don't know S, or $\nabla_{\mathbf{Y}}S$ and we don't know $\boldsymbol{\kappa}$, all this looks like useless window dressing. Maybe not. If we can determine some $\mathbf{Y}=\mathbf{Y}^*$ for which $\nabla_{\mathbf{Y}}S$ is small (in the sense much smaller than $\nabla_{\mathbf{Y}}S$ at the actual **Y**) we could try to expand $\boldsymbol{\kappa}\bullet\nabla_{\mathbf{Y}}S \approx \boldsymbol{\kappa}\bullet\nabla^2_{\mathbf{YY}}S\bullet(\mathbf{Y}-\mathbf{Y}^*)$. Call $\boldsymbol{\kappa}\bullet\nabla^2_{\mathbf{YY}}S=C$ so it doesn't look so scary and we have only two problems: what is $\mathbf{Y}^*$ and what is C?

$\mathbf{Y}^*$ is usually obtained by thinking about $d\mathbf{Y}/dt=\mathbf{f}(\mathbf{Y})$ under idealized circumstances, where we suppose many excited degrees of freedom while omitting all external forcing and internal dissipation (here regarded as “external” to dynamics of **Y**). Dynamics sometimes are further simplified, e.g. to quasigeostrophy, to make calculation of $\mathbf{Y}^*$ tractable. Subject to integrals of the motion of idealized $d\mathbf{Y}/dt=\mathbf{f}(\mathbf{Y})$, $\mathbf{Y}^*$ is the **Y** that maximizes S, i.e. $\nabla_{\mathbf{Y}}S=0$.

Aside: This point has confused onlookers more than any other. The “theory” appears to be to maximize entropy. But such a result would only apply to a mathematical idealization (an isolated, unforced, nondissipative system) arguably far from

Earth's oceans. What needs be emphasized is that the <u>*whole*</u> <u>*idea*</u> *is to use* ***Y**** *as a means to access* <u>*non-zero*</u> *$\tilde{N}_Y S$ in order to complete the actual equations of motion of actual* ***Y****. This is not a "maximum entropy" theory of anything.*

Couplings C remain to be estimated and, in my work to date, are largely fudged. (**Y*** isn't so great either.) At this time the point is not to find "the answer" (don't I only wish!), but rather to identify the parts of the answer which may yield to successive efforts. At C I encounter the same kinds of semi-empirical, largely fudged, by-guess-and-by-golly estimations which are characteristic of our ability to represent oceanic turbulence.

Here let me mention a second approach to "X", recently advanced by Joel Sommeria and colleagues. The idea is to find an expression for overall production of entropy, dS/dt, which can be maximized with respect to **Y**. **X** is then the force on d**Y**/dt which maximizes dS/dt. Although both the derivation of dS/dt and the assignment of constraints for maximizing dS/dt have raised new issues and new uncertainties, the maximum entropy production approach offers an important complement to the entropy gradient forcing which I have pursued. Happily, Igor Polyakov has compared the two approaches in a case of Arctic ocean modeling and finds pleasingly similar results.

So what?

A reader can well ask: if we are only stirring cream into coffee, isn't this entropy talk a lot of bother? Indeed if only ocean eddies were as simple as the coffee, we should hardly bother. For much of what we do about ocean mixing parameterizations, effort to recast the discussion in terms of entropy calculus would (most likely) only append a superstructure over what – practically – we do anyway. On the other hand, during this 'Aha there were two very different areas of research where results were not "simple" in the sense of coffee turning brown.

First recall David's results in two layer flow over topography. Numerical experiments do not lead to flattening isopycnals (reducing APE) and do not lead to uniform PV. What are eddies doing? The suggestion, which would need to be quantified using actual code for actual geometry of David's experiments, is that eddies move the two-layer flow to nearly the highest entropy it can attain. One approach may be, if the code David used can be run in dissipationless, conservative mode, then the model itself can be let run to reveal **Y***. There is no reason **Y*** should reflect either minimum APE or uniform PV. When actual dissipation and forcing (if present) cause actual **Y** to depart from **Y***, eddy fluxes should arise in the model (testably) approximately proportionally to **Y*-Y**.

Peter reminded us of an older illustration from statistical mechanics, recalling a hypothetical Arctic circulation (from myself from 'Aha Huliko'a, 1993!) in which rectified ("neptune") flows were induced by eddies. While those early results were barotropic, extensions to baroclinic flow apply to David's case.

A different result that stirred controversy during 'Aha was George Carnevale's simulation of internal wave breaking. When George evaluated vertical buoyancy flux, w'b', where buoyancy $b=(\rho_0-\rho)/\rho_0$ is the fractional deficit of density about reference ρ_0 and w is vertical velocity, spectral contributions were positive (upwards) over nearly all k. In particular w'b'(k)>0 over all k that were "turbulent" by any measure of "turbulence". Because the experiments were stably stratified, mean db/dz>0 and the turbulence from internal wave breaking forced b up the b-gradient (on average), "anti"-mixing. This is not stirring the cream into the coffee! What was wrong? Sentiments at 'Aha ranged from (1) the experiments were performed improperly (wrong large scale forcing) to (2) analyzing outcome in "z" is wrong, and density coordinates should be used.

Or maybe George had things right, as indeed (I think) explains the differential diffusion which I report elsewhere in these proceedings. If George was right, why w'b'(k)>0? Again I'll only speculate without direct access to George's output, but I believe that wave "breaking" efficiently scatters potential energy PE (as b'^2) to higher wavenumbers. Over most k, the result was PE > KE at each k. In these experiments without Coriolis, **Y*** for internal gravity modes equipartitions PE and KE. Thus w'b'(k)>0, converting PE $\Rightarrow$ KE, is driven by **Y-Y***.

Does this help?

A shortened list of guiding principles can read only "dS/dt > 0". The ongoing practical challenge is to put this idea to work. Although progress may be slow because the methods are unfamiliar, tangible practical progress is being made.