

Interactive comment on “Thermodynamic Properties of Seawater, Ice and Humid Air: TEOS-10, Before and Beyond” by R. Feistel

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This well written paper surveys some of the theoretical basis underlying the new thermodynamic standard for seawater TEOS-10, while also providing some discussion of remaining issues to be tackled in the future. One of the most striking aspect of TEOS-10 is its dramatic departure from the theory and concepts of the previous standard. While there is no doubt that the new standard represents a major advance over the previous standard, the unfamiliar character of several of its new concepts has also resulted in many oceanographers struggling to grapple with what the new framework actually means and what its actual implications for oceanographic practice and future developments are. This review — but this is also true of the TEOS-10 manual — makes occasional statements and assertions that are by no means self-evident; on the very

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few occasions where this occur, it would therefore be helpful if the author could attempt to be more pedagogical and provide more details on the theoretical justification for some of the most intriguing aspects of the TEOS-10 listed below.

1. Page 10, Line 21-22: **TEOS-10 is the first international seawater where chemical composition anomalies are explicitly accounted for.** This part of the review is very unsatisfactory (but then this is a problem of the TEOS-10 manual as well), as it is not possible from the information given to understand how exactly TEOS-10 account for chemical composition anomalies, nor what is the underlying theoretical justification for it. Indeed, TEOS-10 is presented as providing for the first time a synthesis of all possible thermodynamic information about reference composition standard seawater by means of a master thermodynamic potential (the Gibbs function). The statement 'TEOS-10 accounts for the first time for chemical composition anomalies' suggests therefore that TEOS-10 provides a mechanism for quantifying the impact of composition anomalies on all possible thermodynamic functions, but all what is explicitly discussed is density, which is only one of the many thermodynamic quantities of interest. Moreover, saying that TEOS-10 can account for chemical composition anomalies suggest that it is in principle possible to deduce how all possible thermodynamic quantities are affected by composition anomalies. What the author discusses here, however, is how to compute Absolute Salinity from the knowledge of density, which seems to be the opposite of what is needed. What is the theoretical basis for believing that thermodynamic quantities are only determined by the total mass fraction of the dissolved components? Why is this not discussed in the TEOS-10 manual nor in the present review? Is the Absolute Salinity $S_A = S_R + \delta S_A$ really a single variable, or is it really two more more physical variables? How can we derive a mathematically well-posed problem for S_A in that case? Don't we need an equation for both S_R and δS_A ? And is it really possible to derive an evolution equation for δS_A in terms of a single evolution equation or do we actually several evolu-

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tion equations? Could the author also explain what is Millero's rule exactly, and how is it possible to investigate its validity? Could the author also comment on the possible use of FREZCHEM to construct a Gibbs function for seawater as a function of more than just one composition variable?

2. Page 12, Line 29. **All physical, chemical and oceanographic, theoretical as well as numerical models do actually rely on S_A rather than S_p . Outside oceanography is the only the scientific community recognises salinity.** I don't really understand these statements. What does the author mean by 'Absolute Salinity'? Does he mean density salinity or Reference Composition salinity? Or is the author using the term 'Absolute Salinity' as a generic way to refer to a quantity expressed in standard composition units such as g/kg? For standard seawater, S_p is mathematically equivalent to S_R , since the two are related by a fixed conversion factor, so in some sense, the distinction between S_p and Absolute Salinity is only justified for seawater that differs from standard seawater. It seems to me, however, that from a practical viewpoint, one does not really have the choice at the moment when numerically modelling the ocean but to neglect composition anomalies and to assume fixed composition, since if only one evolution equation is used to describe salinity, it has to be for reference composition salinity S_R or equivalently S_p , since the equations for both quantities are exactly the same but for boundary conditions.
3. Page 19. I find the issues pertaining to SI traceability quite tricky to understand, and I believe that many readers would appreciate a more pedagogical treatment here. To the extent that density of seawater may also be affected by such effects as dissolved CO_2 , air bubbles, microplastic, etc..., which may affect density without contributing to the mass of dissolved tracers, it is unclear to what extent density salinity is always a good proxy for Absolute Salinity? If one cannot be sure that density salinity is a good enough proxy for Absolute Salinity, how confident can we be that 'seawater density is the only promising candidate for

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SI-traceability measurements in the oceanographic practice?' Moreover, isn't it a problem that density also depends on pressure and on the precise value of gravity at the place of measurement?

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