Comment on "An Efficient Method for Determining the Significance of Covariance Estimates"*

HANS VAN HAREN

Netherlands Institute for Sea Research, Den Burg, Netherlands

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Lueck and Wolk (1999, hereafter indicated as LW99) report a computationally faster way to establish the significance of direct flux estimates from ocean data, a novel *aspect* to a *method* originally communicated by B. Ruddick. LW99's novel aspect is the classic relationship between the cross-covariance function and the cross-spectral estimate (Jenkins and Watts 1968), while the method has been used and described before.

Yamazaki and Osborn (1993, hereafter YO93) and Fleury and Lueck (1994, hereafter FL94) used the method suggested by Ruddick to test the significance of their direct heat flux estimates from microstructure measurements, and van Haren et al. (1994, hereafter HOG) used it to test their heat and momentum (internal wave band) flux estimates from acoustic Doppler current profiler (ADCP) and thermistor string data. LW99 also test this method, and less so their novel aspect to it, on vertical momentum flux estimates from ADCP data. In passing, they make conceptual errors in the computation of these momentum flux estimates.

YO93, FL94, and HOG determine the significance levels of cross-covariance estimates at zero lag from a distribution of a limited number of cross-covariance estimates from randomly lagged time series, for which the lags are outside a predetermined range near zero lag. The novelty introduced by LW99 is only equivalent to this method if one can assume statistical independency of *all* estimates outside a certain range from zero lag. Unfortunately, LW99 do not show this. Computationally, their Eq. (3) for the construction of such distribution is indeed much faster than used by YO93, FL94, and HOG. This may be verified using the Matlab built-in function XCOV, which computes the lagged cross-covariance function (J. R. Gemmrich 1999, personal communication).

However, the crux is the *method* of computing the significance levels, and LW99 test it on the statistical significance of momentum flux estimates from ADCP data. The corresponding figures clearly show a misunderstanding by LW99 of the correct computation of such fluxes. This misunderstanding is related to the relatively large horizontal spread of the beams of an ADCP as they are slanted at an angle of typically $\theta = 30°$ to the vertical. As a result, current estimates are averages in the horizontal between opposing beam pairs.

As originally outlined by Lohrmann et al. (1990, hereafter LHR), estimates of vertical components of momentum flux using an ADCP require the assumption of *statistical* homogeneity over the beam spread, rather than the much more restricted condition of *current* homogeneity. Under this assumption of statistical homogeneity the flux estimates are made directly within the volume of a beam, which typically has a spread of about 2°. If we denote the Doppler velocities b_i, $i = 1, \ldots, 4$ in the non-Cartesian beam coordinates, LHR show that the proper momentum fluxes estimated within the beams (subscript b) read, at any depth,

$$-\overline{(u'w')}_b = \frac{(\overline{b_2'^2} - \overline{b_1'^2})}{2\sin 2\theta} \quad \text{and}$$
$$-\overline{(v'w')}_b = \frac{(\overline{b_4'^2} - \overline{b_3'^2})}{2\sin 2\theta}, \tag{1}$$

where the prime indicates fluctuating quantities that remain, in a Reynolds decomposition, after subtracting an appropriate mean from the original signal. The flux estimates (1) are only meaningful when the original data are *not* averaged prior to analysis, that is, when they are collected as "single pings."

When the ADCP is programmed to store ensemble averages of more than one ping, the beam velocities are internally transferred to a Cartesian coordinate system for proper vector averaging, namely, $[b_i, i = 1, \ldots, 4] \leftrightarrow [u, v, w, e]$, where the first three terms denote the Cartesian velocity components, and the fourth term is

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Corresponding author address: Dr. Hans van Haren, Netherlands Institute for Sea Research (NIOZ), P.O. Box 59, 1790 AB Den Burg, Netherlands.
E-mail: hansvh@nioz.nl

indicated as ''error'' velocity, which is generally thought to be redundant. This transfer prior to averaging is performed internally regardless of the output coordinates requested, and the beam coordinate system is only equivalent to the Cartesian coordinate system when the instrument is fixed in space. Expressed in Cartesian coordinates (1) reads, as was shown by HOG,

$$-\overline{(u'w')}_b = -\overline{u'(w' - e')} \quad \text{and}$$
$$-\overline{(v'w')}_b = -\overline{v'(w' + e')}, \qquad (2)$$

so that the use of the error velocity is crucial. Its appearance in (2) is according to the definition given by HOG, which differs to date from the definition given in the technical manual by RDI, the manufacturer of most four-beam ADCPs. Apart from the primes, the definition of u' and v' is as in LW99 [their Eq. (4)] but $w' = \Sigma_i -b'_i/4\cos\theta$, $i = 1, \ldots, 4$, and *opposite in sign* and a composite of data from all *four* beams rather than two, compared to LW99. LW99 fail to inform the reader about their definition of w', which is not standard, and about their computation of the momentum fluxes. Judging from their Eq. (4), the along-channel momentum flux component is estimated under the assumption of statistical homogeneity over the beam spread while, curiously, the cross-channel component under the assumption of current homogeneity. Furthermore, LW99 should have informed the reader about their sampling strategy.

I assume that LW99 used a broadband RDI ADCP, that they covered a range of 30 m, and, judging from the information they give in the caption of their Fig. 5, that data have been stored every 3 s. Then, they either programmed the storage of single-ping data (case 1), or they stored ensemble averages of about five pings, as the typical single-ping time takes about 0.5 s (case 2). For the latter case I assume they were careful in not setting the instrument up using a very long pulse length but rather one that matches their (unknown) depth cell sizes. In case 1, they are free in defining a (wrong) vertical velocity from the beam velocities, although they should have estimated fluxes according to (1). In case 2, they should have used data in Cartesian coordinates, so that w is determined by the instrument, and not as indicated by LW99, followed by (2) to estimate the momentum fluxes properly. Inadvertently, the data presented by LW99 clearly show the importance of the method suggested by LHR and HOG.

LW99 report two examples of flux estimates; one estimate is from a depth close to the instrument in the properly estimated along-channel direction. They find this flux estimate statistically significant. The other estimate is for the cross-channel direction, improperly estimated in the sense of LHR and HOG, *and* from a depth farther away from the instrument. This estimate is statistically marginally significant and, for smaller sample distributions, insignificant. These results have probably nothing to do with the (in)significance of the (cross-) along-channel flux estimates and cannot be related to a difference in the sense of the governing physics. Instead, they seem to be due to the (im)proper way of estimating these fluxes, becoming progressively worse when the beams are more spread.

Similarly, the conclusion by LW99 on the suitable length of the record, which determines the Reynolds decomposition, the fluxes, *and* the levels of statistical significance is not well founded. Although a general ocean spectrum does not show gaps, which would justify the choice of a particular cutoff frequency, LW99 could have elaborated more in some argumentation on the choice of their particular (relatively long) record length of 90 min. Taking an (unspecified) subset of 10 min of ''pseudo'' (?) velocity [their Eq. (4)] data is not a sufficient proof, as a change in sampling length means a change in filter cutoff according to a proper Reynolds decomposition, hence a change in flux estimates and hence in the associated distribution of cross-covariance estimates.

The computational novelty for determining the siginificance levels of direct flux estimates as described by LW99 is highly appreciated. However, their paper, while most of it describes a method suggested about a decade ago through personal communication, falls short in testing this novelty also because the authors disregard the proper estimating of momentum fluxes from ADCP data.

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