# IOVWST Wind Stress Working Group
## May 29, 2014

## 1. Introduction

The Ocean Vector Wind (OVW) Wind Stress Working Group (WSWG) is a subgroup of the International OVW Science Team (IOVWST). Formation of the group was suggested at the 2013 IOVWST Meeting in Hawaii and it organization began in the spring of 2014 with the primary objectives of improving estimates of surface stress from scatterometry.

## 2. Motivation for Working Group Formation

Motivation for the working group can be found in a recent ocean flux remote sensing survey paper by Bourassa et al. (2010 TOS):

- *It is anticipated that scatterometer-derived stresses will soon be available from reprocessed QuikSCAT observations, with regional and seasonal biases proportionally smaller than for stresses determined previously.*
- *Recent studies find that scatterometers, and presumably other wind-sensing instruments, respond to stress rather than wind, accounting for variability due to wind, buoyancy, surface currents, waves, and air density.*
- *This is a tremendous advantage for improved accuracy in other turbulent fluxes because wind stress is more closely related to fluxes than wind: stress observations are believed to account for all sea-state-related variability in surface fluxes of momentum, heat, and moisture.*
- *Because sea state is not well observed from space, this approach should remove one source of error in studies of climate change.*

## 3. Background

The basis for the second bullet is that scatterometers measure surface roughness, and it is generally assumed that surface roughness is more closely correlated with the wind stress on the sea-surface rather than wind speed. Wind stress is proportional to the equivalent neutral (i.e., stability adjusted) wind speed relative to the sea surface, $U_{rN}$, defined at some elevation above the ocean surface (typically 10 m). For this reason, scatterometer wind retrievals are usually defined as the 10-m equivalent neutral wind, $U_{r10N}$, rather than the actual wind at 10 m (Wentz, Climate Working Group Charter). By this definition, the relationship between equivalent neutral wind and stress vectors is

$$\vec{\tau} = \rho_a C_{D10N} \left|\vec{U}_{r10N}\right| \vec{U}_{r10N} = \rho_a C_{D10N} \left|\vec{U}_{r10N}\right| (\hat{i} U_{xr10N} + \hat{j} U_{yr10N}) \qquad (1)$$

where $\rho_a$ is air density, $C_{D10N}$ is the neutral stability drag coefficient at a height of 10 m, and the stress vector is assumed to be aligned with the wind vector. Geophysical model functions (GFMs) are typically determined using buoy observations of wind speed and direction. Buoy wind components are adjusted to $U_{r10N}$ using Monin-Obukhov Similarity (MOS) scaling. For example, the x-component of the equivalent neutral wind vector is given by

$$U_{xr10N} = U_x(z_b) - U_{x0} + \frac{u_*}{\kappa}\left[\ln\left(\frac{10}{z_b}\right) + \psi_m\left(\frac{z_b}{L}\right)\right] \quad (2)$$

where $U_x$ and $U_{x0}$ are the x-components of the mean wind speed and surface current measured on the buoy, respectively; $z_b$ is the height of the anemometer; the friction is defined as $u_*^2 \equiv \left|\vec{\tau} / \rho_a\right|$; $\kappa$ is von Karman's constant; $\psi_m$ is a dimensionless functions that accounts for the effect of atmospheric stability on the wind profile; and $L$ is the Monin-Obukhov length.

## 4. Charge

The development of GMFs based on buoy wind observations provides equivalent neutral winds from scatterometer backscatter measurements. These estimates of $U_{r10N}$ are the starting point for many meteorological applications. However, many ocean applications, including the lower boundary conditions for marine atmospheric models, require the surface stress vector $\vec{\tau}$. Therefore, the primary objectives of the IOVWST Wind Stress Working Group are:

- Improving estimates of wind stress derived from scatterometer estimates of the equivalent neutral wind via a drag coefficient.
- Determining the need for more direct estimates of wind stress from scatterometer measurements of surface roughness via a GMF trained with stress estimates.

## 5. Summary of Potential Research Issues

The following issues have all been considered by the IOVWSTs. The IOVWSTs has a good handle on some of them and significant disagreement or overall lack of understanding exists with other. Several of the latter issues will need to be resolved before we can produce a reasonable accurate climate record of surface stress. On the other hand, early results from the Climate Working Group might be used to address the sensitivity of the Climate Data Records (CDRs) to some of these issues.

1. Currents and stability corrections and consideration
2. Dependence of surface stress on air density.
3. Drag coefficient and surface roughness formulations.
4. Sea-state dependent drag coefficients.
5. Geophysical model function based on surface stress
6. Noise and non-linearity
7. Physical models of scattering and relation to surface stress.
8. Water temperature dependency of surface characteristics (e.g., viscosity, density and surface tension effect on gravity-capillary waves)
9. Extreme wind conditions

## 6. Discussion of Potential Research Issues

The following is based on the email discussion that proceeded the first meeting of the WSWG. I have indicated contributors to the discussion to date in parentheses.

## 6.1 Currents and stability corrections and consideration (Edson, Bourassa, Vandemark)

We have a reasonably good handle on currents and stability corrections and considerations. For example, the direct estimates of the drag coefficient shown in Figure 1 provide evidence that the wind speed relative to the ocean is the appropriate value to use when estimating the surface stress (Edson et al. 2013). This figure relies on direct covariance measurements made from a surface mooring near the northern wall of the Gulf Stream during the CLIMODE program. The buoy was deployed for 15 months and was generally located within but sometime outside the meandering Gulf Stream. Inclusion of currents clearly does a better good of collapsing the data compared to results that use the wind speed relative to earth to compute the drag coefficient. Likewise, several investigations have shown the sensitivity of scatterometer-derived winds to ocean currents (cf. Kelly et al., 2001; Plagge et al., 2012).

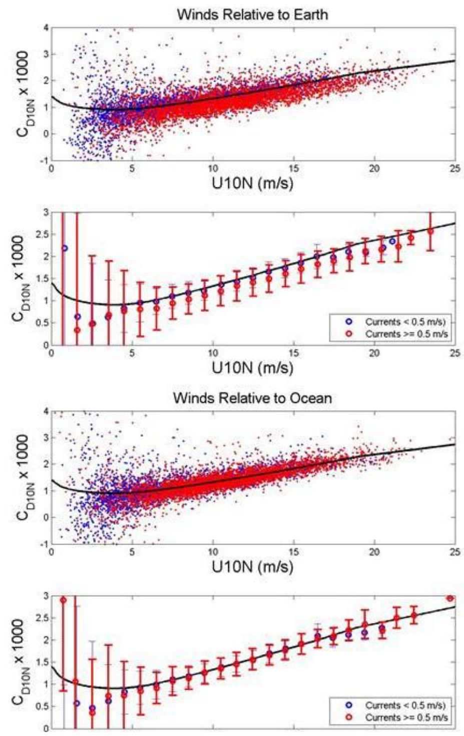

Figure 2. Drag coeffcients computed with direct measurement of the surface stress using wind wind speeds measured relative to Earth (upper panels) and relative to water (lower panels).

Edson et al. (2013) also showed that the measurements of the dimensionless shear used to calculate the stability correction agrees closely with the Businger-Dyer formulations used to determine the GMF in prior studies. The largest differences are under very stable conditions that are most often found in coastal regions with warm air advection over cool water. Figure 2 shows that the stability function used in the current version of the COARE algorithm, which was based on SHEBA data taken in the Arctic, gives good agreement over the entire range of stability. The basic conclusion is that similarity holds in all surface layers (i.e., whether marine, terrestrial, or artic) as long as the measurements are made above the wave boundary layer (WBL). This assumption is generally valid at and above buoy heights. However, simulations and observations indicate that this assumption may break down in deeper WBL over swell and under extreme wind/wave conditions.

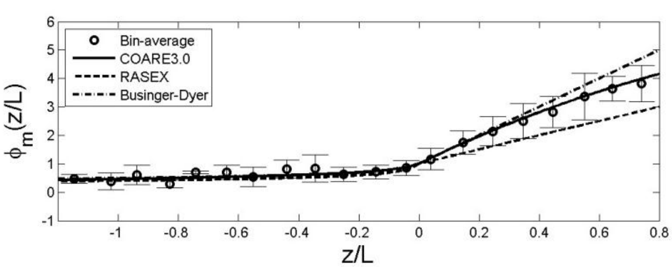

Figure 1. Measurements of he dimensionless shear taken over the ocean compared with several land-based parameterizations.

## 6.2 Dependence of surface stress on air density (Bourassa, Wentz)

We are also getting a good handle of this issue (e.g., May and Bourassa (2010), which is clearly important in the conversion of equivalent neutral winds to stress. Frank Wentz summarized this issue as follows. Currently, most $U_{r10N}$ retrieval algorithms, including ours, do not consider variations in $\rho_a$. Assuming that $\sigma_o$ measurements are more a measure of $\tau$ than $U_{r10N}$, the $\rho_a$ correction for $U_{r10N}$ takes the form

$$U_{r10N} = \sqrt{\frac{<\rho_a>}{\rho_a}}\, U_{r10N}^{retrieval} \quad (3)$$

where $U_{r10N}^{retrieval}$ denotes the current set of VW retrievals, $\rho_a$ is the air density for the given observation, and $<\rho_a> \approx 1.2$ kg/m$^3$ is the average air density over the ensemble of scatterometer measurements used to derive the retrieval algorithm.

Combining (1) and (2) gives

$$\vec{\tau} = <\rho_a> C_{D10N} \left|\vec{U}_{r10N}^{retrieval}\right| \vec{U}_{r10N}^{retrieval} \quad (4)$$

From this, one sees the following:

1. When computing stress from the current set of vector wind retrievals, one should multiply by some globally average air density as opposed to using an air density value at the location of the measurement that one might get from a numerical model. The reason for this is that $U_{r10N}^{retrieval}$ already includes the effect of varying air density.

2. To obtain the true value of $U_{r10N}$, one should multiply the current set of vector wind retrievals by the air density ratio shown in equation (3). Preliminary evidence for this effect can be seen in Figure 3, which plots the ratio of $U_{r10N}$ computed from buoy observations over VW retrievals as a function of the density ratio where $<\rho_a> = 1.16$ kg/m$^3$.

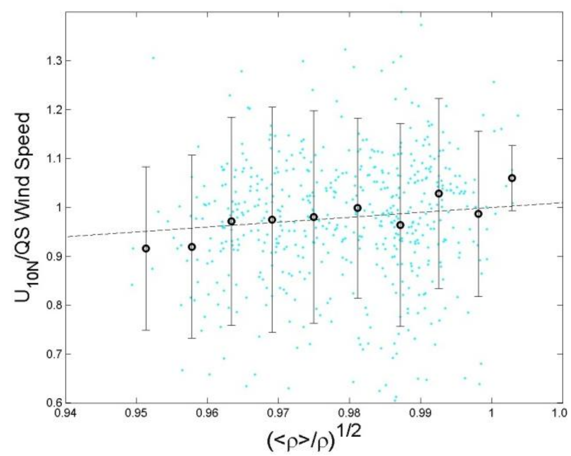


**Figure 3. The ratio of observed to retrieved estimates of ENW versus the appropriate density ratio.**

### 6.3 Drag coefficient and surface roughness formulations (Bourassa, Chelton, Edson).

The neutral drag coefficient is commonly parameterized as a function of $U_{r10N}$ [e.g., Large and Pond 1981; Large et al. 1994] and/or surface roughness [Liu et al. 1978; Fairall et al. 1996, 2003; Liu and Tang, 2004, Bourassa, 2004; Edson et al. 2013]. Any one of these formulations can be used to convert the equivalent neutral wind speed to surface stress using the relationship (1). The wind speed dependent drag coefficient formulations are computed directly, while the surface roughness parameterizations compute the drag coefficient iteratively or through a lookup table as only values for neutral conditions are required. It is far to ask to ask (and it has been!) whether we need to or even can develop a GMF to retrieve stress directly from the radar cross section measurements (see section 6.X). For example, even if the WSWG recommends its development, there may be insufficient data on direct stress measurements to be able to do this.

On the other hand, the global stress database can be used to assess whether conversion of equivalent neutral winds to stress using a neutral drag coefficient. In particular, it would be very useful to determine whether the COARE drag coefficient is better or worse than parameterizations used in the development of the GMF such as the Large and Pond drag coefficient. The consensus of the stress community is that the COARE algorithm is better for applications to in situ data. However, because of its heritage, it may be that the Large and Pond formulation (the version in the appendix of Large et al., 1994) works better with scatterometer data. This is a question that this Working Group can answer.

Towards this goal, Figure 4 shows bin-averaged drag coefficients from direct covariance momentum fluxes measured during the FLIP/MBL, RASEX, CBLAST, and CLIMODE field programs. These are compared against several commonly used parameterization of the drag coefficient including the globally average value determined by ECMWF. The figure suggests that the models are in reasonable agreement for wind-driven seas from about 5 m/s to at least 12m/s. However, the figure suggests that potentially significant discrepancies begin to occur above and below these values and, if the measurements are to be believed, suggest that the more result parameterizations are significantly more accurate.

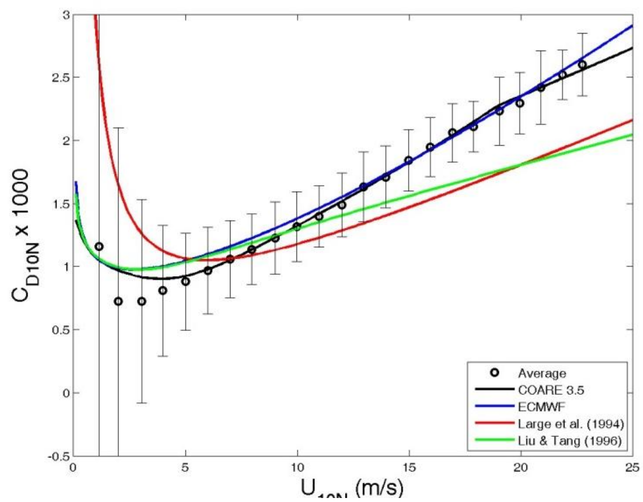

**Figure 4. Neutral drag coeffcient versus the neutral wind relative to th ocean surface adjusted to 10-m.**

*Research Questions/Comments*

- How well are capillary waves parameterized at low wind speeds? In most surface roughness and drag coefficient parameterizations, the surface roughness at very low winds is parameterized using the roughness Reynolds number found in smooth laboratory flow using air-side values of viscosity. However, surface tension effects (e.g., as characterized by Weber number scaling) and water-side viscosity (**issue 8**) are perhaps more relevant (Bourassa et al. 1999).
- How well is gustiness parameterized at low wind speeds? COARE (Fairall et al. 1996) uses the gustiness parameterization of Beljaars and Godfrey, which is based on the convective velocity scale $w_*$. Work on this is ongoing, and the drag coefficient parameterizations should benefit.
- What role do factors like displacement height, sea spray, sea state dependence play at extreme wind conditions and are these different than at more moderate wind speeds?
- What role does swell play at low-wind conditions and misaligned wind and waves play at all conditions? Observations and simulations (e.g., Sullivan et al. 2007) suggest that the reduction of the drag coefficient and equivalent surface roughness to values smoother than smooth flow are due to swell driven wind. Additional observations indicate that direction matters (e.g., Donelan et al. 1997), but most parameterizations are independent of wave direction. This is mainly because role of directionality has proven very difficult to quantify as it often occurs under light and variable wind and stress conditions. However, the effect may be more quantifiable under stronger forcing where wind and wave become misaligned (e.g., in various

quadrants of hurricanes). Of course it situ measurements are difficult to obtain under these conditions, but scatterometry may be able to shed light on this.

### 6.4 Sea-state dependent drag coefficients (Bourassa, Edson, Stoffelen, Vandemark)

Once the sea becomes fully rough, the overall community consensus is that the spectrum of surface gravity waves supports the surface stress. Therefore, after correction of currents and stratification, one can argue that the drag coefficient should be a function of sea state (e.g., wave-height and wave slope) and/or wave-age (i.e., the stage of development at a given forcing). However, many of us have worked for decades searching for this dependence with mixed results. For example, Portabella and Stoffelen (2009) found good agreement between buoy, the ECMWF model and scatterometer derived stresses using two different parameterizations of the drag coefficient – even though one was primarily wind-speed dependent while the other was wave age dependent. Although, perhaps for different reasons, similar agreement is seen between the wind-speed dependent COARE and globally averaged ECMF model. One hypothesis for this agreement is that most of the wind stress is supported by gravity waves with wavelength less than ~10m and that these waves provide a nearly linear relationship between wind speed and wave-age as shown in Figure 5 from Edson et al. (2013). One can also hypothesis that longer wave can modified the exchange, but that their effect is most pronounced under low-wind conditions that are often associated with swell. If this hypothesis is true, then it would suggest that the WSWG focus its attention on sea-state dependency under low winds. Of course, most of our near surface observations of surface stress have been limited to winds below ~25 m/s. Therefore, the role of longer waves remains an issue under extreme wind conditions.

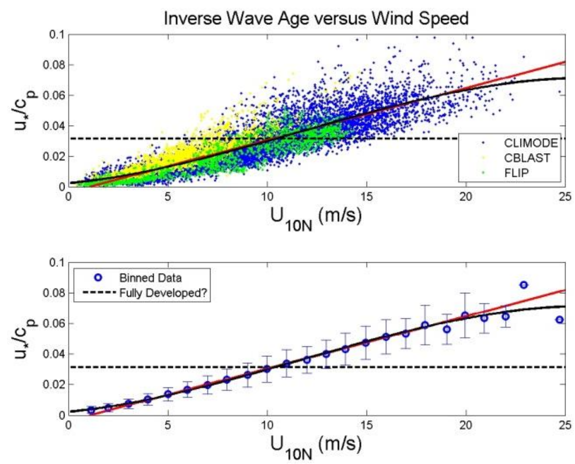


**Figure 5. Neutral wind speed versus inverse wave age. The "fully developed" value is shown by the dotted line.**

*Research Questions/Comments*

- Is the scatterometer derived wind actually more closely aligned with wind stress than that the actual equivalent neutral wind as sea state varies? If so, satellite data deviation from in situ $U_{r10N}$ should track with changes in the drag coefficient and sea state parameters controlling it.
- Another way of asking this questions is: Do all satellite winds, inferred from their ocean surface roughness measurements, reflect a similar estimate of wind stress at the sea surface and do they equally enfold sea state dependent drag variation?
- Can a global perspective on the air-sea drag coefficient be illustrated using a complement of wind and wave model plus multi-sensor satellite data to better define how to use and interpret satellite wind measurements?

### 6.5 Geophysical model function based on surface stress (Portabella)

One thing we've learned with ASCAT (and ERS) is that the model function "shape" in measurement space (i.e., the cone surface) is not going to change (much) whether we use stress-related parameters or wind-related parameters in the GMF. That is, the cone surface represents the best effort so far in fitting the backscatter triplets. Although the shape of CMOD will change in the future, this will be mainly driven by its representation in measurement space rather than by stress versus wind considerations (actually, we already have a pretty good idea on what type of improvements are still required, i.e., mainly at low winds and possibly very high winds). Of course, the interpretation of a point in the cone surface as an area-mean "wind" or area-mean "stress" value matters and can be further explored. As Ad already mentioned, 6-7 years ago we couldn't tell whether a stress-like interpretation was more appropriate than a wind-like interpretation for scatterometer data (although we know that theoretically it's closer to the former).

To move forward in this effort, higher resolution systems will certainly help but, most importantly, we need to improve in noise characterization and noise filtering techniques (at L1, L2, and higher level processing), i.e., we need to improve the signal-to-noise ratio. In other words, to see second (or higher) order effects (e.g., sea state, etc.) we need to retrieve a good signal!

*Research Questions/Comments*

- If we do not have sufficient direct covariance stress estimates to develop a GMF, would it makes sense to use either $(U_{r10N})^2$ or the WSWG recommendation for a bulk estimate of $\tau$ ?

### 6.6 Noise and non-linearity (Bourassa, Stoffelen)

One of the biggest problems in investigating error variances is spatial variability or so-called representation error. If we want to estimate the error of a scatterometer stress measurement we need to sample the integrated footprint of the scatterometer, but we never do! To overcome this problem, we developed ways to estimate representativeness errors by spatial and spectral analyses. Typically, we get 1.2-1.4 m/s wind component representation error variances for buoys, while we estimate scatterometer component wind errors to be 0.6-0.8 m/s for the same data sets (Vogelzang et al., 2011).

Additionally, if scatterometers are responding to stress, and if the noise is Gaussian (not clear that it is), then the noise will be non-linear for wind.   That will change the model function, particularly for very low wind speeds.

*Research Questions/Comments*

- So, how are the stress measurements going to represent the scatterometer IFOV?
- Do we have enough measurements to find residuals that can be associated to something else than U10N?
- For all speeds and directions; for all WVCs or all incidence angles?

### 6.7 Physical models of scattering and relation to surface stress (Stoffelen).

Measured backscatter appears indeed primarily due to surface roughness. In fact, first order Bragg scattering appears a main contribution, but theoretical models have a hard time to get a description to within the precision of the scatterometer measurement. Critical assumptions in describing ocean microwave scattering are among others:

- Electromagnetic closure, no easy way forward here;
- Isotropic Bragg scattering; are breaking and wind-reinforced cm-waves really the same in all directions?

- Roughness spectrum, different spectra provided wildly different backscatter; are spectra always the same? In fact, no, we know they depend on the source function (wind input) which is variable.
- Foam coverage, particularly at strong winds;

*Research Questions/Comments*

- If we do not know in detail how roughness causes backscatter, can we then still assume that bulk aerodynamic roughness is the same as Bragg roughness?
- Then the relationship between roughness and stress; I've seen many publications on this issue, where 10% differences are common. Which relationship do you suggest? Why?

### 6.8 Water temperature dependency of surface characteristics (Grodsky, Stoffelen)

Radar backscatter depends on the spectrum of small scale waves. SST can alter the growth rate of centimeter-scale waves through its impact on air density and water viscosity. These two are competing effects. Rougher seas are generated by denser air over cold SSTs. But, smoother waves are also expected due to higher viscosity and stronger viscous dissipation for colder water.

Ocean backscatter anisotropy is however known/suspected to be affected by other effects too; thus it is relevant to cleanly separate such effects (i.e., wind variability and sea state from SST) if one were to scientifically prove either of these effects.

Note: I chose not to include any more of Ad's email response as I felt the Working Group meeting would be a much better place for such discussions.

### 6.9 Extreme wind conditions

Mark and I will continue working on this, Tim Liu will be presenting a talk on this at the meeting, and I look forward to a discussion about this at the meeting.

## 7. Sensors

The OVW sensors to be considered are the following:

- Ku-band Scatterometers: NSCAT, QuikSCAT, OSCAT, RapidScat, CNSA HY-2A
- C-band Scatterometers: ERS-1, ERS-2, ASCAT-1, ASCAT-2
- L-band Scatterometers: Aquarius and SMAP

## 8. Overall Goal

The overall goal of the Wind Stress Working Group is to advance our understanding of the many issues related to estimating surface stress from scatterometers.

## Appendix A:  Email Discussion as of May 28, 2014

### 31 March, 2014, 7:12 PM:  James Edson

Dear All,

At the last IOVWST meeting, there appeared to be enough interest on scatterometry stress retrievals to put together a Working Group for our next meeting.   I volunteered to organize this group and this represents my initial attempt.

The first item of business is to populate the SRWG, so I am sending this out to potentially interested IOVWST members.  Thus far, I've included:

Mark Bourassa
Dudley Chelton
Jim Edson
Melanie Fewings
Tim Liu
Ralph Milliff
David Moroni
Larry O'Neill
Marcos Portabella
Ernesto Rodriguez
Ad Stoffelen
Lisan Yu
Doug Vandemark
Frank Wentz

**Therefore, I ask you to confirm your interest and provide suggestions for additional members.**

In the meantime, I am going to put together a Draft Charter for the SRWG to provide some guidance for what we hope to accomplish with this group.    Briefly, here's my take on this for our initial discussions:

We would rely on the assumption that measured backscatter is  primarily due to surface roughness generated by surface stress, such that wind stress is more naturally related to backscatter than wind speed (even after correction for currents and stratification).  Therefore, an objective is to take advantage of the growing number of *in situ* direct covariance stress vector measurements as well as state-of-the-art bulk aerodynamic stress estimates  to develop GMFs that directly retrieve surface stress vectors from scatterometry.

I'd be happy to hear you take on this as I put together the Draft Charter.   Don't worry, I've got pretty thick skin!

Best,
Jim Edson

## 22 March, 2014, 1:38 PM:  Dudley Chelton

Hi Jim,

I am willing to participate as a member of this Working Group. But I'm not sure that I agree that it is a foregone conclusion that the eventual recommendation will be to develop a GMF to retrieve stress directly from the radar cross section measurements. I am pretty confident that there is not sufficient data on direct stress measurements to be able to do this. But perhaps you can persuade me that I'm wrong.

On the other hand, I do believe that the global stress database can be used to assess whether conversion of equivalent neutral winds to stress using a neutral drag coefficient. In particular, it would be very useful to determine whether the COARE drag coefficient is better or worse than the Large and Pond drag coefficient. I suspect that the latter is better with QuikSCAT data, but have never actually looked into this rigorously. I know that the stress community feels that the COARE algorithm is better for applications to in situ data. But, because of its heritage, it may be that the Large and Pond formulation (the version in the appendix of Large et al., 1994) works better with scatterometer data. This is a question that this Working Group can answer.

-Dudley

## 1 April, 2014, 5:33 PM:  Ad Stoffelen

Dear Jim,

Thanks for the initiative! I'd be much interested in the elaborations of this group. Since you have a thick skin, let me take apart your statement in pieces below and add some reservations or challenges.

Measured backscatter appears indeed primarily due to surface roughness. In fact, first order Bragg scattering appears a main contribution, but theoretical models have a hard time to get a description to within the precision of the scatterometer measurement. Critical assumptions in describing ocean microwave scattering are among others:

- e.m. closure, no easy way forward here;
- isotropic Bragg scattering; are breaking and wind-reinforced cm-waves really the same in all directions?
- roughness spectrum, different spectra provided wildly different backscatter; are spectra always the same? In fact, no, we know they depend on the source function (wind input) which is variable.
- foam coverage, particularly at strong winds;

If we do not know in detail how roughness causes backscatter, can we then still assume that bulk aerodynamic roughness is the same as Bragg roughness?

Then the relationship between roughness and stress; I've seen many publications on this issue, where 10% differences are common. Which relationship do you suggest? Why?

After correction of currents and stratification, I presume we are left with sea state dependence? I've been looking for that my entire scientific career, as Gerbrand Komen and Peter Jansen promised such dependence when I started my career at KNMI. After Portabella and Stoffelen published on ocean stress, Peter said that he thought this would only matter in extreme conditions (we found statistically the same sea state dependence in buoys as that what we found in scatterometer winds for a given (collocated) data set using Peter's ECMWF WAM sea state parameters). In strong off shore winds (e.g., those passing over Scotland) I've been looking for sea state effects for 60 knot winds (I think those are extreme), but saw the same scatterometer speeds left and right of Scotland (while even the largest waves do probably not pass through!) and right off the coast, well, this is some 30 km. It remains of course interesting to systematically investigate such effects and look for sea state dependence, certainly when moving towards the coast with our processing. Publications I've seen that find sea state dependence statistically do not carefully sample, or just add another sea state variable to minimize variance. Obviously, more variables explain more variance in case of incomplete or inadequate sampling, just poor statistics, but no new physics.

One of the biggest problems in investigating error variances is spatial variability or so-called representation error. If we want to estimate the error of a scatterometer stress measurement we need to sample the integrated footprint of the scatterometer, but we never do! To overcome this problem, we developed ways to estimate representativeness errors by spatial and spectral analyses. Typically, we get 1.2-1.4 m/s wind component representation error variances for buoys, while we estimate scatterometer component wind errors to be 0.6-0.8 m/s for the same data sets (Vogelzang et al., 2011). So, how are the stress measurements going to represent the scatterometer IFOV? Do we have enough measurements to find residuals that can be associated to something else than U10N? For all speeds and directions; for all WVCs or all incidence angles?

If we find a geophysical variable, independent of U10N, that should be associated with scatterometer backscatter, I think we should very carefully elaborate how we are ever going to determine its importance. I'm excited to keep on searching and will join you though! Science is not supposed to be simple and I hope that the above piecewise qualification of the problem helps in addressing it in the appropriate way.

So, please, do not see the above statements as a discouragement, but rather as an attempt to start the discussion. I look forward to your draft or reflection on this message.

Best regards,

Ad

## 1 April, 2014, 11:31 PM: Mark Bourassa

Ad and Jim,

The seastate dependency is an interesting issue and there are several very different models that give very similar answers where we have a lot of data to tune the models, and differ wildly at the extremes. In all likelihood we are missing several important physical considerations at wind speeds >30m/s, but these are quite rare. Yesterday I chaired a session with a series of wildly different presentations on how to model these conditions, each being put forth with great confidence but limited understanding of the basic theory that converts their data (if any) to a model, as well as skipping some potentially huge consequences of their assumed physical processes. We could get bogged down in this for over a decade. I suggest focusing on more common and reliable conditions, then moving outward.

Additional issues:

1) Influence of swell. There are some very interesting implications that could come from swell related dependency. Scatterometry might be a very good way of addressing this issue, in association with other observations.

2) There is an observed, stress-like, dependency of U10EN errors on air density that has been found by me (May and Bourassa, 2010) and Frank Wentz.

Cheers,
Mark

## 2 April, 2014, 8:36 AM: Ad Stoffelen

Indeed Mark,

Good points! Let me reiterate that I do agree we can move forward and certainly with air density, probably the least controversial of all. Also Hans Hersbach tested this effect convincingly in the past.

There are indeed many more ways we can look at sea state too. For example, we now reject local points affected by what I've always called "confused sea state". With Marcos we are looking into some of these points now, those near tropical rain bursts. Really exiting!

Cheers

Ad

## 2 April, 2014, 8:37 AM: James Edson

Hi Ad, Mark, Dudley et al.,

This is exactly the type of discussion I was hoping for. I know exactly what Mark means by "bogged down" as I spent (too many) years (decades?) trying to figure out the impact of wave directionality (especially swell) on the fluxes. At the end, I decided to focus on open ocean conditions and I think we've made some progress/consensus there (see Edson et al., JPO, 2013).

So, I am looking forward to working with this group and will try to incorporate your thoughts into my draft. That will really give you something to tear apart!

Cheers,
Jim

## 9 April, 2014, 7:12 PM: Frank Wentz

Jim,
Sorry in taking so long to reply. Yes I am interested in participating. My major interest is about the question of whether it's best to simply compute stress from the standard 10-m equivalent neutral wind retrieval or does it require its own retrieval algorithm and model function. Either way, specifying the drag coefficient is the major problem.
Frank

## 8 May, 2014, 10:43 AM: Marcos Portabella

Dear Jim et al,

First of all, sorry for my late reply. Indeed, I'd like very much to contribute to the stress WG. At this point, I only have a few remarks to add to this interesting discussion:

1) One thing we've learned with ASCAT (and ERS) is that the model function "shape" in measurement space (i.e., the cone surface) is not going to change (much) whether we use stress-related parameters or wind-related parameters in the GMF. That is, the cone surface represents the best effort so far in fitting the backscatter triplets. Although the shape of CMOD will change in the future, this will be mainly driven by its representation in measurement space rather than by stress versus wind considerations (actually, we already have a pretty good idea on what type of improvements are still required, i.e., mainly at low winds and possibly very high winds). Of course, the interpretation of a point in the cone surface as an area-mean "wind" or area-mean "stress" value matters and can be further explored. As Ad already mentioned, 6-7 years ago we couldn't tell whether a stress-like interpretation was more appropriate than a wind-like interpretation for scatterometer data (although we know that theoretically it's closer to the former).

2) To move forward in point 1), higher resolution systems will certainly help but, most importantly, we need to improve in noise characterization and noise filtering techniques (at L1, L2, and higher level processing), i.e., we need to improve the signal-to-noise ratio. In other words, to see second (or higher) order effects (e.g., sea state, etc.) we need to retrieve a good signal!

Looking forward to seeing you in Brest.

Best regards,

Marcos

## 15 May, 2014, 1:52 PM: Frank Wentz

This email is directed to the Wind Stress Working Group:
Attached is a short comment clarifying how air density affects our vector wind/stress products.
See you in Brest,
Frank

**A Comment of the Effect of Air Density on Vector Wind and Stress Measurements**
Frank J. Wentz
May 15 2015

Scatterometers measure surface roughness, and it is generally assumed surface roughness is more closely correlated with surface stress $\boldsymbol{\tau}$ than the actual wind speed at 10 m. For this reason, scatterometer wind retrievals are usually defined as the 10-m equivalent neutral wind, called $\mathbf{U}_{10EN}$, rather than the actual 10-m wind $\mathbf{U}_{10}$. To obtain $\mathbf{U}_{10}$ from $\mathbf{U}_{10EN}$ one needs information on the stability of the atmospheric boundary layer, which is not contained in the scatterometer measurements.
The relationship between $\mathbf{U}_{10EN}$ and surface stress $\boldsymbol{\tau}$ depends on the air density $\rho$. For the same $\mathbf{U}_{10EN}$, cold heavy air will produce more stress (and roughness) than lighter warmer air. This effect is expressed by the surface stress equation:

$$\boldsymbol{\tau} = \rho C_{D10} \left|\mathbf{U}_{10EN}\right| \mathbf{U}_{10EN} \qquad (1)$$

where $C_{D10}$ is the neutral stability drag coefficient at a height of 10m and is a function of $\mathbf{U}_{10EN}$. Currently, most $\mathbf{U}_{10EN}$ retrieval algorithms, including ours, do not consider variations in $\rho$. Assuming that $\sigma_o$ measurements are more a measure of $\boldsymbol{\tau}$ than $\mathbf{U}_{10EN}$, the $\rho$ correction for $\mathbf{U}_{10EN}$ takes the form

$$\mathbf{U}_{10EN} = \sqrt{\frac{\langle\rho\rangle}{\rho}}\ \mathbf{U}_{10EN,retrieval} \qquad (2)$$

where $\mathbf{U}_{10EN,retrieval}$ denotes the current set of VW retrievals, $\rho$ is the air density for the given observation, and $<\rho>$ is the average air density over the ensemble of scatterometer measurements used to derive the retrieval algorithm (≈1.2 kg/m$^3$).

Combining (1) and (2) gives

$$\boldsymbol{\tau} = C_{D10}\langle\rho\rangle\left|\mathbf{U}_{10EN,retrieval}\right|\mathbf{U}_{10EN,retrieval} \qquad (3)$$

From this, one sees the following:

1. When computing stress from the current set of vector wind retrievals, one should multiply by some globally average air density as opposed to using an air density value at the location of the measurement that one might get from a numerical model. The reason for this is that $\mathbf{U}_{10EN,retrieval}$ already includes the effect of varying air density.

2. To obtain the true value of $\mathbf{U}_{10EN}$, one should multiply the current set of vector wind retrievals by the air density ratio shown in equation (2).

## 15 May, 2014, 2:37 PM: Senya Grodsky

Frank,
it is not that simple.

Radar backscatter depends on the spectrum of small scale waves. SST can alter the growth rate of centimeter-scale waves through its impact on air density and water viscosity. These two are competing effects. Rougher seas are generated by denser air over cold SSTs. But, smoother waves are also expected due to higher viscosity and stronger viscous dissipation for colder water.

I am attaching our GRL paper that shows that the air density and viscous effects almost compensate each other in the C-band. But, viscous effect dominates in higher frequency Ku-band.

--Senya

**Does direct impact of SST on short wind waves matter for scatterometry?**
Semyon A. Grodsky,Vladimir N. Kudryavtsev, Abderrahim Bentamy, James A. Carton and Bertrand Chapron

Scatterometer radar backscatter depends on the relationship linking surface stress and surface roughness. SST can alter the growth rate of centimeter-scale waves through its impact on air and water density and water viscosity. This SST-dependency has not been included in the standard Geophysical Model Functions. This study uses a radar imaging model to evaluate this SST-dependence and compares the results to observations from QuikScat Ku-band and ASCAT C-band scatterometers. A SST correction could raise wind speeds by up to 0.2 ms_1 in the storm track region of the Southern Ocean for C-band scatterometers. For the higher frequency Ku-band scatterometers, a SST-induced reduction up to 0.4 ms_1 is predicted south of 60_S, where SST is cold and winds are moderate.

## 15 May, 2014, 3:16 PM:  Frank Wentz

Senya,
Of course it's not that simple, and I did not want to imply that it is.
And there may indeed be other effects that wash out the air density effect.
I just think it is important that air density be explicitly considered when defining the terminology discussed in the note.
Some researchers may be applying a variable air density correction to the wind retrievals to get stress and this would, in effect, be double bookkeeping.
Frank

## May 17, 2014, 4:38 PM:  Hans Bonekamp

Hi Frank,

for the discussion: for scatterometers, is the assumption on sigma0 measurements in your comment valid if the GMF is trained against u10 or u10en (not tau)?

see
http://earth.eo.esa.int/pcs/ers/scatt/articles/CMOD5N.pdf
https://earth.esa.int/pub/SCATTEROMETER/ecmwf_rep/cmod5.pdf

Cheers,
Hans

**CMOD5.N: A C-band geophysical model function for equivalent neutral wind**
Hans Hersbach

**Abstract**
This document describes the evaluation of a C-band geophysical model function. This model function, called CMOD5.N, is to provide an empirical relation between C-band backscatter as sensed by the space-born ERS-2 and ASCAT scatterometers, and equivalent neutral ocean vector wind at 10-metre height (neutral surface wind) as function of (scatterometer) incidence angle. CMOD5.N embodies a refit of CMOD5, a C-band model function which was previously derived to obtain non-neutral surface wind, in such a way that its 28 tuneable coefficients lead, for given backscatter observation, to an enhancement of 0.7ms□1 in wind speed. The value of 0:7ms□1 is chosen to be independent on wind speed and incidence angle, and incorporates the average difference between neutral and non-neutral wind (_ 0:2ms□1 ) and for a known bias of CMOD5 (_ 0:5ms□1 ) when compared to buoy wind data. The quality of the CMOD5.N fit is tested for the AMI scatterometer on ERS-2 and ASCAT instrument on METOP-A for July 2007 and January 2008. From this it is found that winds inverted with CMOD5.N are on average 0:69ms□1 stronger than winds determined from CMOD5. As function of wind speed and incidence angle, fluctuations are well within 0:05ms□1 . Differences in wind direction are small. ASCAT and ERS-2 wind speed obtained from CMOD5.N compares on average well with operational neutral wind from the European Centre for Medium-RangeWeather Forecasts (ECMWF). In comparison with nonneutral wind, local, seasonally dependent biases between scatterometer and ECMWF model are reduced. Besides effects introduced by orography and ocean currents, a residual stability-dependent bias between scatterometer and neutral wind remains, which is likely connected to a previously reported non-optimality in the ECMWF boundary layer formalism by Brown *et al.* in 2006.

## 15 May, 2014, 5:17 PM:  Ralph Foster

Hi all,

I haven't really had time to digest this, but two points are possibly in order. First, it's never really been clear to me if a true stratification/height correction was done to the cal/val winds used in the GMF development. I've been under the impression that some sort of "bulk" coefficient was applied.

Second, I should check my work before emailing, but I think the formula is something like:

rho*( k*(U10N - U10)/psi(10/L) )^2 = tau

since we assume that between 10 m and the surface, the mean profile is M-O-like.

In this case, rho is "local" and L is going to depend on the surface buoyancy flux and the stress. So, as in all surface layer dynamics, an iterative solution is needed. Psi is relatively well known for unstable to near-neutral stable stratification.

In light winds, the assumption gets worse since it's possible to have nearly zero mean wind and significant stress from processes like convective inflow. Common fixes are to combine u* with a convective velocity scale, w*.   U*eff = (u*^3 + const*w*^3)^(1/3). The added complexity is that w* depends on the surface buoyancy flux and the PBL depth, although results are usually insensitive to h. Commonly h=~700 m is used across the board.

The assumption is also less good for relatively large footprints over smaller-scale changes in SST (fronts, eddies, gulf stream, etc).

Ralph

## 15 May, 2014, 5:18 PM:  Frank Wentz

Hans,
Rather than beginning an email discussion on this and not knowing who in the long list of recipients is interested this,
I think it is best to hold the discussion as part of the Stress WG in a few weeks.
The point of the memo was to  shine light on one factor, air density, and its implications, so we could discuss it in the WG.
Thanks,
Frank

## May 15, 2014, 5:31pm:  Hans Bonekamp

Yes agreed, over and out, Hans

## 17 May, 2014, 2:37 PM:  Ad Stoffelen

Thanks Senya, for your contribution on SST. Unfortunately, the manuscript does not help me much as GMF developer. Let me explain why.

Your theory suggests an important effect of viscosity, which obviously would be of great interest to confirm. Methods exist to test such dependence; both in measurement space and in wind space. I plan to briefly discuss these in the Climate Working Group. Please also see the comment of Marcos Portabella on this wind stress topic.

For example, you appear to suggest that the ocean backscatter isotropy is affected by SST (figure 4). If this were true, the conical surface in measurement space would be different at low SST and high SST; this can be tested obviously. In fact, I tested this in the past and did not find such dependence, but today much more and better data (ASCAT) is available for verification. Ocean backscatter anisotropy is however known/suspected to be affected by other effects too; thus it is relevant to cleanly separate such effects (i.e., wind variability and sea state from SST) if one were to scientifically prove either of these effects.

Others ways to verify these effects are in the wind or stress domain, but here large additional uncertainties come into play. Therefore, analytical comparisons need to be even more careful. The main point is spatial and temporal verification, where changes of 0.2 m/s do occur within about 30 minutes. One could argue that such changes average out by taking many samples, but these changes do have spatial and temporal patterns to them. For the same reason can one not use buoys as truth for 25km area-average scatterometer winds for calibration without complications. Another important remaining error, as you state in your manuscript too, are GMF errors. Generally, C and Ku band GMFs have different errors. I attach plots from Hans Hersbach at ECMWF, who collocated calibrated scatterometer 10m neutral-equivalent winds with independent ECMWF background winds and plotted them on spatial maps. Since the winds were calibrated against ECMWF winds, the underlying GMFs may be regarded as intercalibrated in this case. Clearly, ASCAT and QUIKSCAT differ from each other spatially, but there does not seem to be a sharp SST-induced gradient near 60S. More maps are online available at http://research.metoffice.gov.uk/research/interproj/nwpsaf/scatter_report/2014_01/map.html . A third complication arises when dependent data sets with correlated error are compared. So, never use the ECMWF analysis to compare scatterometers with. ECMWF assimilates scatterometer data and thus scatterometer errors are assimilated too! We are confident though, that such biases disappear after a few hours max. and therefore the ECMWF background is used for comparison. A last point, which we know also changes winds by as much as 0.1 m/s, are interpolation errors; they also change the small-scale spectrum. For example, it took us (and ECMWF) quite a while to figure out how to get appropriate U10N from the ECMWF MARS archive ERA-interim reanalyses. Without all these precautions it is difficult to make progress in GMF development.

The comparison in your manuscript is not in line with the above since:

1) ASCAT and QSCAT are collocated for large time differences;
2) When ASCAT and QSCAT have smaller time differences, these collocations are in very particular sampling regimes (near the poles), where errors due to wind variability, harmonic GMF errors, etc. may be amplified in the particular sample;
3) The Ku-band and C-band GMFs are empirical and have varying speed- and direction-dependent error; moreover, QSCAT undetected rain and false alarms in the screening will result in speed, direction and geographically-dependent biases;
4) The ECMWF field is not used at full resolution (reduced Gaussian grid) and the ECMWF analysis has a complicated relationship with both observation data sets, since these are assimilated;

In my view the IOVWST should upgrade its product evaluation capacity in order to evolve to more advanced products faster. This is, share high-quality collocation data bases and software. Then international resources may be better spent to advance scatterometer products. It is a complex process though, but I will suggest some steps forward at the IOVWST.

See you all soon,

Ad