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Comment

Interactive comment on “Modeling of wave-induced irradiance variability in the upper ocean mixed layer” by M. Hieronymi et al.

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by Martin Hieronymi (MH)

Thank you very much indeed for the detailed and valuable comments from Referee # 1 (R #1). We believe that we can adequately respond to all of the specific comments and we hope that the actions taken in response to the review help to improve the paper in the interest of the referee.

R #1: Page 2113, line 23: “The horizontal extension of the model domain has to be large enough to ensure that the horizontal losses due to domain-leaving rays are negligible.” This statement is correct but not the key point here. The authors are interested in the irradiance at individual spatial locations, not over the whole domain. In this case,

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the horizontal extension is not the key. As explained later, the authors had to cut off the edge of the domain for statistics calculations (take into account 400 m out of a 500 m domain).

MH: The sentence refers to the irradiance pattern of a single light beam, which has a total width of 100 m. The photon access for the MC procedure is at a single point at the top located directly in the middle (± 50 m). Even beyond 50 m in x-direction (horizontally) some scattered light exists (see next comment) but it is not further considered – this is the key message of the sentence. In the next step these individual 100 m x 100 m irradiance patterns of single light beams, which are independently of one another, are superposed accordingly any requested wave field. At both lateral edges of the total model domain (500 m in the given example) 50 m are cut off to ensure that all scattered and unscattered light is considered.

MH: “The model domain, in which the Monte Carlo calculations for a single beam irradiance pattern are conducted, has to be large enough to ensure that the horizontal losses due to domain-leaving photons are negligible.”

R #1: Page 2113, line 25: “typically below 0.01 % of the total downwelling irradiance per depth level”. The word “depth level” needs to be better defined.

MH: “With the given model input values (Table 2), these losses at the side amount to less than 0.01 % of the total downwelling irradiance per water depth compared to a model domain with 200 m width. The model size requirements and the conceptual error of our model concerning the downward scattering from the underside of the wave modulated sea surface are discussed in Hieronymi (2011).”

R #1: Page 2114, line 6: “N_max is selected to be 40”. How was this selected? Did the authors make this selection by looking for a 1.0e-06 residual intensity?

MH: According to Eq. 5 and depending on the IOPs (Table 2), the remaining photon weight is around 1.0e-06 after traveling 40 times the mean free scattering path length

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of 12.6 m ($\exp[-0.028 \cdot 40 \cdot 12.6] = 7.434e-07$). This is roughly the same if we apply 40 times the weight reduction with the single scattering albedo ω_0 , where $w_{\text{new}} = w_{\text{old}} \cdot \omega_0$ (in our case: $\omega_0 = 0.739$). However, the maximum number of scattering events N_{max} is almost never achieved within the 100 m x 100 m model domain.

MH: “With the given IOPs (Table 2), N_{max} is selected to be 40; after traveling more than 500 m ($N_{\text{max}} \cdot \langle l_s \rangle$) through the water body on average the “light beam” does not contribute an important intensity anymore (Eq. 5).”

R #1: Page 2119, line 15: “... and equivalent HydroLight (HL) runs”. In what sense are they equivalent runs? It is mentioned later in this section that there are inherent differences in the diffuse sky light, the scattering properties of the water, and sea surface realizations. Then what are the same?

MH: The input parameters for HydroLight calculations are the same as our measurement conditions, this applies to the wavelength, the refractive index of water, the sun zenith angle, the surface insolation and wind speed (Table 1), as well as IOPs (Table 2) and an assumed sky diffuseness of 10 % each. Both compared methods, MC and HL, use the Petzold scattering phase function VSF for average particles. In this sense, computations are equivalent. However, two different approaches are compared, which obviously leads to some differences in the results. Remember, HL solves the scalar radiative transfer equation. D’Alimonte et al. (2010) discuss some sources of potential deviations between a MC code and HL, for example that the used VSF apply different angle discretizations of the photon scattering direction, which could affect the scattering pattern and in particular the forward scattering (we used a higher interpolated scattering angle discretization). Probably less important is the implementation of the diffuse skylight (10 % only), which is isotropic in our model whereat HL uses an idealized sky model. The sea surface is realized in HydroLight by using the wind-depending Cox-Munk wave slope statistics. In our model, the sea surface is a continuously wave profile that accounts for a fully developed sea state (with same local wind conditions), with actually higher probability of steeper wave slopes. Thus, our slope distribution

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resembles a Cox-Munk distribution with slightly more wind (the PDF skewness is not considered). The differences in sea surface realization are important at low sun positions but negligible at high sun (which here is the case) [Hieronymi, 2011].

R #1: Page 2120, line 3: “the bias continuously grows to less than 20 % in 100 m depth”. Apparently, a 20 % bias is not negligible. As explained later, although this can be a result of different model parameters, it is an indicator that the build-up of lateral losses of diffuse radiation is showing up. It remains unknown how much this lateral losses may have affects the simulated variability at depths. For example, will it cause an underestimation or an overestimation of the flash count? By how much?

MH: Reasons for different results with HL and MC are discussed in the above points. Test have shown, that the best fit of E_d mean values (with $|\varepsilon| < 5\%$) can be achieved in cases with more scattering particles in the water (Chl between 0.4 and 0.6 mg/m³). Thus, it is likely the sensitivity to scattering properties that is crucial for differences. In particular with very clear water (our case), where absorption and scattering are relatively low, some diffuse radiation escapes at the edges (± 50 m horizontally) and is not further comprised for the determination of the E_d mean value. The lateral losses actually increase if the (0.1 mm wide) wave facet is strongly inclined and thus the air-to-water transmission angle is large. In this case, the entire irradiance pattern shifts aside, more scattered light escapes the area under consideration especially in greater depths below 50 m. In cases with strong wind and steep waves, large wave slopes appear more frequent, and although the lateral losses are still small, the bias of the E_d mean increases due to the build-up of these lateral losses. However, by far the most radiation is very close to the direct initial light beam (within ± 10 m), whose direction is determined by the wave slope, even in 100 m depth. It is primarily the narrow light beam that causes the reported irradiance variability at depths. Figure 1 shows the spreading shape of the light beam which is conical at greater depths. We are convinced that the 100 m x 100 m grid is sufficient to capture all relevant diffuse radiation and that the lateral losses do not cause any significant false estimation of the

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flash count.

R #1: Page 2122, line 27: “(MC model)”. The simulation results were not really from a direct MC model, but from a combination of MC and superposition. Referring it to “MC model” may cause unnecessary confusion.

MH: “(based on the superposition of MC calculated single beam light fields)” and “The third MC based model ...”

MH: In order to better distinguish, that we use two general model approaches, we change the sentence Page 2111, line 20: “Two model approaches are chosen to deal with the variety of dimension requirements, a Monte Carlo based model for large-scale irradiance simulations (with $dx = 1$ cm and 10 cm horizontal resolutions) and a simplified ray tracing model for small-scale near-surface conditions (with $dx = 2.5$ mm).”

R #1: Page 2126, line 6: “normalized per one metre”. This is true for the top panel, but not for the middle and bottom panels, where it says “per 100 m” in the figure.

MH: “(normalized per 1 m, and 100 m respectively)”

R #1: Page 2126, line 17: “which is much deeper than so far observed”. As far as I know, all observations of irradiance variability were on temporal variations not on spatial variations. If so the authors are comparing “spatial flashes” to “temporal flashes”. Can the authors clarify on this?

MH: The statistics of spatial and temporal underwater light fields should be identical, they are linked via the spatiotemporal change of the water surface (dispersion relation Eq. 2).

MH: “Our model with $dx = 10$ cm spatial resolution shows light flashes of $\chi = 1.5$ even down to 35 m water depth, which is much deeper than so far observed with temporal irradiance measurements.”

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