The average daily wind speed for the 81 days of the study period is 3.8 m/s with 9 days of no wind. Assuming that the precipitation is measured by a precipitation gauge shielded with an Alter-shield, the total precipitation over the study period would increase from 142.5 to 161.3 mm, or 12.9% by considering wind undercatch, as per Goodison et al. (1998), and assuming that daily trace events contributed half of the measurable amount of precipitation, i.e., 0.127 mm. For the 81 days in 2000–2001, the increase would be from 91.3 to 136.7 mm, or 49.7%.

Sublimation from the snowpack can be estimated using the latent heat-flux equation, i.e., using wind speed and vapor-pressure deficit with the assumption that surface vapor pressure is saturated and can be computed from the ambient air temperature (Fassnacht 2004). A surface roughness height of 0.01 m was used, as per Walter et al. (2004). From the comparison study, the snowpack sublimation was computed to be 26.5 mm (0.327 mm/day) and occurred 37.5% of the time. Since the humidity is high in the area (the average relative humidity is 91.2%), the sublimation is likely vapor-pressure limited (the average vapor-pressure deficit was 0.468 mb). Even if the process was not vapor-pressure limited, then the latent heat flux would be an overestimate.

While Walter et al. (2004) do not present the sublimation rates, the computed snow fluxes were only a portion of the snowpack sublimation computed from the latent heat flux.

The formulation used to estimate fresh snow density could not be found in Goodison et al. (1981). Other formulations exist in the literature, as summarized by Fassnacht and Soulis (2002), all of which use higher densities than the formulation presented by the authors. While The authors' formulation decreases at temperatures colder than −15°C, Fassnacht and Soulis (2002) suggested that the density of fresh snow may actually increase at temperatures colder than −16°C due to a general decrease in snow crystal size at cold formation temperatures. Using a fresh snow density, such as presented by Hedstrom and Pomeroy (1998), resulted in only 23.4% of the snowdrifting flux computed by Walter et al. (2004).

Snow data are available from the Pullman, Wash., site. Using these data and data from surrounding NWS stations, the observed SWE was divided by the snow depth to compute an approximate fresh snow density (Fig. 2). The plot illustrates that there is a large variation in fresh snow density, with few daily observations of 50 kg/m^3 fresh snow density.

Based on the average daily air temperature, precipitation can fall as snow at air temperatures warmer than 0°C (Fassnacht and Soulis 2002). This would increase snow accumulation.

The authors corroborate the model's snowmelt performance by comparison to observed snowmelt at four sites (Danville, Vt., Bloomville, N.Y., Easton, Minn., Troy, Ind.). However, the snowdrift model is not used in these areas, and while it is used for Pullman, the snowmelt component of the model is not used. A simulation of the model's snowpack performance for snowdrifting plus accumulation and snowmelt at the same site would better illustrate the model's capabilities and the integration of the simple snowdrift formulation within the distributed hydrological model, as per the paper's title and its objectives.

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Closure to "Simple Snowdrift Model for Distributed Hydrological Modeling" by M. Todd Walter, Donald K. McCool, Larry G. King, Myron Molnau, and Gaylon S. Campbell

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We appreciate the discussion of Fassnacht and Brazenec, which emphasizes the difficulty in parsimonious modeling on snow processes and suggests some potential improvements to the approaches we used in our snowdrift study. They speculate that our study may have underestimated snowfall, overestimated snowdrift, and overestimated sublimation. While these may be true, our methods were not without justification, as discussed below.

With regard to snowfall, we are of course aware of the commonly employed correction factors but chose not to use them here because ground measurements of new snow generally agreed well with gauge measurements. Admittedly, we made few such comparisons but the limited data we had did not suggest the need for a correction. Also, although Fassnacht and Brazenec note that snow may fall at temperatures greater than 0°C, it is also true that rain may fall at temperatures less than 0°C, so we did not think that 0°C was an unreasonable temperature at which to partition precipitation into rain and snow.

Fassnacht and Brazenec suggest that we underestimated the density of new-fallen snow, which would lead to an overestimation of snowdrift. They also correctly note that the equation we used to estimate new snow density is not in Goodison et al. (1981); indeed, we modified an equation by Barry et al. (1990) and "mined" data from Goodison et al. (1981) and other parts of the *Handbook of Snow* (Gray and Male 1981) to test our relationship. Barry et al. (1990) proposed

$$\rho_{\mathrm{ns}} = 50 + 1.7(T_w + 15)^{1.5} \tag{1}$$

where ρ_{ns}=new-snow density (kg m^{-3}) and T_w=wet-bulb temperature. We found that, for the range of temperatures we observed, $(T_w+15)^{1.5}$ was linearly correlated with $(T+15)$ with a regression slope of two; T is air temperature. Thus we proposed

$$\rho_{\mathrm{ns}} = 50 + 3.4(T + 15) \tag{2}$$

Note, we used T_w at saturated air-vapor density in our regression, and we recognize that Eq. (2) is a rough approximation. However, there is tremendous scatter in the ρ_{ns} data (e.g., Fassnacht and Brazenec Fig. 2), and it is unlikely that any equation as simple as Eqs. (1) or (2) will meaningfully capture the observed variability—in fact, Eq. (2) lies well within the "data cloud." Goodison et al. (1981) also show that the new-snow density changes within hours of falling, so it was not obvious how to estimate this characteristic with meaningful precision. We agree with Fassnacht and Brazenec that, when data are available, it is best to use snow measures directly rather than approximations like Eqs. (1) and (2).

We admittedly handled sublimation in a crude and unsatisfactory way. We assumed large sublimation of the drifting snow. Specifically, and based loosely on Tabler (1975) and Pomerory and Li (2000), we assumed sublimation was proportional to the wind's carrying capacity as long as there was enough new snow available. We are not sure how Fassnacht and Brazenec concluded that Pullman, Wash., was a humid area (average relative humidity =91%), but the Western Regional Climate Center Web site shows monthly average regional humidity rarely exceeds 80%. Regardless, their point is well taken that sublimation should be modeled more mechanistically than we did in our paper.

Fassnacht and Brazenec also noted that we corroborated our snowmelt model with data from locations other than Pullman. Note that "Troy, Ind.," is a mistake in the journal article; the location should be Troy, Id., which was ~30 km from the site where drift measurements were made. We direct readers to Walter et al. (2005) for complete evaluation of the melt model. Incidentally, while on the topic of errata, Eq. (7) should be

$$\Delta u_{\mathrm{lee}} = -\exp\left(\frac{4(Z_{\max} - Z_i)}{Z_{\max}}\right)$$

Our mistake.

Final Thoughts

We are heartened by the increasing frequency with which mechanistic hydrological models are being developed, used, and improved in lieu of temptingly simpler black-box models. We would like to thank Fassnacht and Brazenec for their thoughtful comments and we acknowledge Fassnacht's and colleagues' substantial contributions to the field of snow hydrology. With respect to snowdrifting, we also recommend the excellent publications of Marks, Winstral, and their coauthors.

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