

Review of EGU sphere-2026-5223

The authors present their analysis of SABER temperature observations to derive gravity wave induced heat fluxes and heating rate between about 20 and 100 km altitude. The authors employ the gravity wave polarization relations to estimate the wave-induced vertical wind fluctuations from SABER observations of the temperature fluctuations. The heat flux and heating rate profiles are then computed and compiled globally over a large latitude range for all 12 months of the year. The paper is well written and organized and the results would be of considerable interest to the atmospheric sciences community. Heat flux observations are difficult to make because they require accurate knowledge of both the wave-driven temperature and vertical wind fluctuations in the atmosphere. In particular, the heat flux depends critically on the phase difference between the wave temperature and vertical wind fluctuations, which is almost, but not quite 90° . Until now, such observations have only been made by Na Doppler lidars and only in the mesopause region at a handful of locations. Thus, the current study, if valid, would be a significant contribution to our knowledge of gravity effects in the stratosphere, mesosphere and lower thermosphere. Unfortunately, I have serious concerns about the validity of the approach utilized by the authors to derive their heat flux profiles.

Non-dissipating gravity waves propagate conservatively in the atmosphere so that the wave amplitude increases with increasing altitude as $\exp(z/2H)$, where z is altitude and H is the density scale height. The polarization relations employed by the authors were derived by Vadas (2013) for wave fluctuations scaled by $\exp(-z/2H)$ (eq. 13, Vadas, 2013), so indeed the polarization relations apply to pure sinusoidal oscillations give by eq. 1 in the manuscript. However, the authors then apply the polarization relations to a wavelet (see eqs. 5 and 7), which is clearly not a sinusoid. What is the justification for this? More importantly, what errors accrue by assuming that the polarization relation for a wavelet is identical to that for a pure sinusoid?

A more serious problem is that the Vadas (2013) polarization relations apply only to non-dissipating, fully compressible wave fluctuations, when in fact all waves experience some dissipation (e.g. Guarino et al., 2024; Jandreu et al., 2026, <https://doi.org/10.1029/2026GL124048>). Furthermore, not all wave fluctuations are fully compressible. Many investigators have argued that the wave fluctuations may be regarded as incompressible (e.g. Walterscheid, 1981; Liu, 2000) and more recently, Gardner (2024, fig. 1, <https://doi.org/10.1029/2023JD040436>) showed that compressibility varies depending the amount of dissipation. How are the author's results affected by wave

dissipation and compressibility? What is the justification for assuming no dissipation and full compressibility?

As noted previously, the heat flux is strongly dependent on the phase difference between the temperature and vertical wind fluctuations. Chu et al. (2022, eq. 10) used the Vadas (2013) polarization relations to show that for a non-dissipating, fully compressible wave, the sensible heat flux is $\overline{w'T'} \simeq -\frac{A_T^2}{2\bar{T}} \frac{\omega}{m}$. For upwardly propagating waves the vertical wave number m is negative, and so the heat flux is positive. Indeed, Chu et al. (2022, fig. 5a) and Gardner & Liu (2007, fig. 6) observed positive heat fluxes with Na Doppler lidars above 95 km where dissipation was expected to be small or moderate. Yet, this same approach, applied to wavelets by the authors, yields negative heat fluxes! Why the difference and how do the authors determine which vertical direction, up or down, their observed wavelets are propagating?

Lu et al. (2017) measured the phase difference between the T' and w' fluctuations of numerous monochromatic gravity waves between 85 and 100 km and found that the mean phase difference was 2.6° larger than the phase difference predicted by eq. 2. They attributed the difference to wave dissipation. In other words, the additional phase shift introduced by dissipation can lead to negative sensible heat fluxes, but the author's approach of using Vadas (2013) polarization relations for non-dissipating, fully compressible waves cannot produce heat flux observations of real waves that experience dissipation and that may be only partially compressible. Gardner (2024, table 1) derived an approximate solution to the advection-diffusion for heat transfer, which includes the impact of wave dissipation and partial compressibility. Jandreu et al. (2024) adopted this approach to bound the wave-driven thermal diffusivity. Perhaps, the authors could adopt a similar approach to bound the sensible heat flux using their SABER observations.

The goal of the author's research is quite important, but I believe their approach of using the polarization relations for non-dissipating, fully compressible gravity waves is fatally flawed. I recommend the paper be returned for major revisions.