

## ***Interactive comment on “The global climatology of the intensity of ionospheric sporadic E layer” by Bingkun Yu et al.***

**Anonymous Referee #1**

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This paper uses the same methodology developed in a previous study (Chu et al., 2016) for deriving sporadic E properties from COSMIC GPS data. This study goes further by examining both the occurrence frequency and strength, as a function of season, latitude, longitude and height. A number of interesting observations are made. The authors then show that some of these observations can be explained by using winds from a global chemistry-climate model (WACCM) to calculate wind shear and hence ion convergence.

One curious omission is discussion of gravity waves, which are not explicitly resolved in WACCM if their horizontal wavelengths are smaller than  $\sim 200$  km. Many of these waves penetrate into the lower thermosphere, and appear to be a major cause of sporadic E layers in the polar cap, where the near-vertical magnetic field reduces significantly the

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effectiveness of wind-shear in converging ions into layers. The authors appear to be using winds averaged over an unspecified period to determine wind shear – yet there is no discussion of how valid this is, since sporadic E often have short lifetimes of only hours.

Another point is that the height distribution of sporadic E (Figure 1) shows a relatively large proportion of layers appearing between 40 and 90 km. This is dismissed here as an artefact, based on the fact that sporadic E should not form below 90 km because the ion-neutral collision frequency is too high (page 3). The explanation for the artefact is unclear – reference is made to an “RO” event, though this term is not defined in the paper – but appears to be caused by slant viewing geometries. If that is the case, how reliable is the entire distribution of layers, including those above 90 km? The authors ought to show that the distribution they are using agrees with ionosonde measurements at a particular location.

The paper ends on a vague and rather disappointing note: “It indicates that, in addition to the vertical windshear effects, other processes such as magnetic field effects, meteoric mass influx into the earth’s atmosphere and chemical processes of metallic ions are also likely to play a dominant role in the geographical and seasonal variations of Es layers.” There is no attempt to explain how these “chemical processes”, the meteoric mass influx, or unspecified “magnetic field effects” could explain the observations which do not accord with the wind shear theory.

Specific points which need to be addressed: p. 4, line 10: why do you state “may be caused”? You have applied several different models, including WACCM. What do they tell you about the anomalies? The specified dynamics version of WACCM would be quite informative. You also give no details about the version of WACCM output that you use, etc.

p. 4, line 17: surely the wind shear mechanism should be most effective at the geomagnetic equator when the magnetic field is horizontal? Why do you state that a vertical component is required for ion convergence?

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- p. 4, line 32: the sentence “The ionization of Es layers is persistently magnetic fields trapped in the polar regions” make no sense.
- p. 5, line 18: the sentence “One of the unsolved issues in the ionosphere is that the well pronounced seasonal dependence of mid-latitude Es layers does not have a comprehensive explanation, which is inexplicable from the windshear theory” contradicts your own conclusion that wind shear does explain many of the mid-latitude features!
- p. 5, line 24: you suddenly mention Fe<sup>+</sup> ions here. Why only Fe<sup>+</sup>, where do they come from? What is the evidence?
- p. 6, line 1: why do you take the wind from WACCM, and the atmospheric composition from MSIS? This is inconsistent. This calculation should be performed using composition and winds from the same model.
- p. 6, line 19: the windshear theory does not explain formation of Es layers at high geomagnetic latitudes, because the magnetic field is nearly vertical. There is evidence that within the polar cap gravity waves play a dominant role in Es formation (see, e.g. the papers by John MacDougall from Western Ontario). This is not discussed anywhere.
- p. 6, line 25: did you derive equation (3) here? If not, it should be referenced.
- p. 6, line 32: why are the regions of ion convergence different when the magnetic declination angle is included. You seem to imply that agreement with the observations is worse. What does that imply? Is equation (3) incorrect?
- p. 7, line 11: why would energetic particle precipitation and cosmic rays generate Es in the polar region? Most cosmic ray ionization occurs around the tropopause, and EPP would not create a thin layer of ions.
- p. 8, line 2: I thought this range of IDP input had been considerably reduced in the Carrillo-Sanchez et al., GRL 2016 paper (which followed the one you cite). What difference would it make anyway to sporadic E formation?

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Corrections: Need to indicate in the figure captions for Figure 3, 4, 5, 6, 7, 8 and 9 whether this is data averaged over the whole period or for a single year.

References: many do not have the complete initials of the authors

p. 1, line 3: “show a high . . . distribution and . . .”

p.1, line 6: “. . .bands. Simulations results show that . . .”

p. 1, line 12: “. . .layers are thin-layered . . .”

p. 1, line 19: “equatorial region”

p. 2, line 1: “. . .irregularities and their sharp . . .”

p. 2, line 33: “. . . models, namely the Whole . . .”

p. 3, line 2: “. . . time. Section 5. . .”

p. 3, line 6: “. . .behind earth’s . . .”

p. 3, line 7: “. . .signal is received at . . .”

p. 3, line 16: “. . .time series . . .”

p. 6, line 9: “. . . seasons. They also showed . . .”

p. 7, line 31: “. . .provides a much greater . . .”

There are many other grammatical errors in the paper which need to be corrected.

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