

Peer Review

Review of: "Improving Our Knowledge of the Solar Near-Surface Shear Layer: The Special Case of the Leptocline"

S. N. Breton¹

1. Osservatorio Astrofisico di Catania, Catania, Italy

Dear Authors,

I have read with interest the preprint of your article “Improving Our Knowledge of the Solar Near-Surface Shear Layer: The Special Case of the Leptocline”. I believe drawing attention to the peculiarity of the dynamics of this upper region of the solar convective envelope is of interest not only for helioseismology, but also in the general context of the modelling of solar-type stars.

The review as it is already reads well, I believe nevertheless that clarifying and extending some of the aspects you discuss will improve the didactic dimension of your article, please therefore find below the list of my comments and suggestions:

Abstract

I think it should be good to clarify in a few sentences that this article is a work of review that summarises the current level of knowledge available on topics related to the dynamics of the leptocline.

Introduction

First paragraph

1. I would say that these subsurface layers are of crucial importance for a third reason, that is of interest primarily for helio- and asteroseismology, in the way that subsurface layer dynamics affect solar acoustic mode properties (the so-called “surface effects”, e.g. Ball & Gizon 2014), making it difficult to match observed and modelled frequencies. This is a key aspect not only for the Sun but for any solar-type oscillators (where related issues on parameter constraints can

worsen than in the solar case, as surface effect corrections are usually calibrated with the Sun as a reference, see e.g. Bétrissey et al. 2023).

2. “The first addresses”: I might be wrong but I am not sure a reason can address something, I would suggest rephrasing the beginning of this sentence.
3. Following the mention of radial differential rotation in the abstract, it might be good to signal already here that differential rotation in the Sun can be latitudinal and radial.

Third paragraph

1. “*Helioseismic studies*” : for the sake of readers non-expert in the domain, I would suggest providing here some review and textbooks references.
2. “*We also need to understand how the turbulent convection interacts with solar rotation*”: You can probably mention that semi-analytic theories have been developed in this direction (e.g. Stevenson 1979, Bessila et al. 2025) and that different convection/rotation regimes have been examined in MHD simulations (e.g. Miesch et al. 2006, Brun et al. 2017, 2022, Hotta et al. 2022 - although near-surface layers are most often not treated in such simulations due to the computing power and spatial resolution it would require).
3. “This paper aims to present” → “This paper aims at presenting”

Fourth paragraph

1. As you gave the thickness of the tachocline in solar radius, can you indicate the fraction that 8 Mm represents in terms of the same unit ? (it should be something like 0.013 R_{sun} if I made the conversion correctly)
2. Please state explicitly that the data you are using are the one collected by the MDI and HMI instruments (this is what I am understanding from the references used there and the nature of the inversion you have in what follows). In addition to the references to the MDI/HMI Scherrer et al. papers, please provide the references to the SoHO (Domingo et al. 1995) and SDO (Pesnell et al. 2012) spacecrafts.

Section 2

First paragraph

1. The references provided for SoHO and SDO are actually the references for MDI and HMI. As stated in the previous point, please provide the actual SoHO and SDO references in addition to the MDI/HMI references.
2. “inversion techniques to the rotational splitting” : the reference [17] (Schou et al. 1997) is about the determination of the solar seismic radius, I believe at this point the correct reference should be reference [19] (Schou et al. 1998) ?

Second paragraph

1. It seems that the url link to the JSOC was supposed to be in a footnote but apparently something in the formatting went wrong ?
2. (Fig 1) It might be useful to recall here that seismic rotational inversion gives maps that are symmetric in latitude, hence the necessity to show only one quarter of the meridional plane.

Third paragraph

1. “These results” → “The averaged rotation profile shown in Fig. 1”
2. “In particular the results reveal” → “In particular they reveal”

Fifth paragraph

1. Figure 2: Again, for readability, it might be interesting to have the x-axis of the left panel both in Mm and in solar radius.
2. “The gradient that remains constant, ≈ -1 in the deep NSSL”: would not it be better to state that the gradient takes values roughly between -0.5 and 1 in the deep NSSL ?
3. “sharply increases its negative value to ≈ -1.5 ” → “sharply decreases to reach a value of ≈ -1.5 ”
4. “These data do not resolve sharp variations near the surface, smoothed with so-called averaging kernels (e.g. Schou et al.[19])” → it might be good to clarify that these are the inversion profiles that are actually smoothed out with the averaging kernels. Could you provide an estimate of the spatial scale on which the smoothing acts ?
5. Figure 3: the figure has only one panel but three are described in the legend, the description of panel a. and b. seem to refer to what is shown in Figure 2.

Sixth paragraph

1. Do you have any hypothesis concerning the origin of this discrepancy ? Could it be related to the choice of solar model made to perform the inversions ?

Section 3

First paragraph

1. Figure 4: Panel b and c would probably be easier to compare if the same colorbar was used for both of them.

Third paragraph

1. “the variations are less pronounced and have more complicated structures, which, however, resemble the overlapping extended cycles of the torsional oscillations”: can you provide a map of the torsional oscillations in order to make the comparison more straightforward ?

Section 4

First paragraph

1. Here, you might also remind that the variation of low-degree acoustic modes properties with the magnetic activity cycle is a phenomenon that has been known for long (e.g. Pallé et al. 1989, Salabert et al. 2018, García et al. 2023), although, due to the complex topology of the problem, it did not allow yet for a quantitative inversion of the properties of the subsurface magnetic field.

Second paragraph

1. “defined as f-mode of solar oscillations” → “referred to as f modes in the context of solar oscillations”.
2. Figure 5 : panel c y-axis states that the DeltaR parameter is given in m, should not it be km as in panel d ?

Section 5

Can you comment on the level of realism of the simulation that you mention in this section ? In addition, can you outline the configuration that future numerical simulations would have to consider in order to provide us with a better understanding of the dynamics of the leptocline ?

Section 6

First paragraph

1. “the top 15% of the solar convection zone, the depth range $\approx 30\text{--}35$ Mm)” → a parenthesis seems to be missing.
2. “It will be important to develop a synergy of helioseismic observations and numerical simulations”: if possible, could you provide more insights on the priorities in this direction, in particular concerning helioseismic observations and instrumental requirements for future instruments ?

Declarations

Potential competing interests: No potential competing interests to declare.