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Response to the Referee

Many thanks for your very constructive report. The manuscript has been revised following your comments and suggestions. Please note that in the revised manuscript, all of the paragraph except for 'Simulation Model' are rewritten, then we just list the paragraph rather than to say the lines in the reply.

In what follows, your comments are listed in their original order, followed by our response and the corresponding revisions. Please note that the page numbers referred to in this letter are for the revised manuscript. In addition, all new references introduced during this revision have been properly included in the reference list.

The paper focused on the study of double layers (DL) during reconnection. By PIC simulations, the authors claimed that the double layers can be created during reconnection due to the beam instability in the plasma sheet boundary layer. The isolated electrostatic waves (double layers, electron holes (EH)) have been observed in previous simulations and are believed to play a key role during reconnection. Electron holes are frequently observed by the spacecraft measurement during reconnection. Double layers are frequently observed in the ionosphere. Only recently, Ergun et al. reported the double layers in the plasma sheet in the magnetotail. But, they could not confirm the relation between the double layers and reconnection in the work. This year, Wang et al. presented the first evidence of double layers during reconnection[Wang et al., GRL 2014], and found that the double layers are moving away from X-line along the separatrix region. The double layer can accelerate charged particle effectively on its way. In this paper, the simulation domain is only about 25.6 * 12.8 di which is too small to simulate the plasma boundary layer, since the plasma sheet boundary layer denotes a huge region between the lobe region and plasma sheet in the magnetotail. In my view, the so-called 'plasma sheet boundary layer' in the text corresponds to the separatrix region during reconnection. Then, the simulation results are basically consistent with the observations [Wang et al., GRL 2013; 2014]. Based on the observations, DL and EHs are observed together. EHs are observed in the high potential side of the DL. Moreover, the DL propagates in the local thermal velocity. However, the authors found that the DL almost does not move in the simulation.
The authors need to provide a reasonable explanation.

Thanks for pointing this out. Please take a look at the parts that are added on page 6 and figure 4, 5.

According to the observations, the spatial size of the DL is about Debye length. But, it reaches the ion inertial scale in the simulation.

Thanks for pointing this out. We have carefully improved the quality of the figure 2. Please take a look at the lines 9-11 on page 5.

In addition, the authors claimed the DL is produced by the beam instability. Then, please show the evidence also. The language should be improved as well.

Thanks for pointing this out. Please take a look at the parts that are added on page 5-6 and the figure 2-5.

The quality of the figures should be improved. In some figures, no physical quantity can be found, like Figures 3 and 4.

Thanks for pointing this out. We have improved the queality.