1 Overview:

Review of "Large XCH_4 anomaly in summer 2013 over Northeast Asia observed by GOSAT" by Ishizawa et al.

Ishizawa et al.'s manuscript identify anomalously high GOSAT observations over Japan in summer 2013. They present surface observations (total column and *in situ*) that seem to corroborate these GOSAT observations and use model simulations to interrogate the source of the methane anomaly. The manuscript is fairly well written. However, I have some serious concerns with the manuscript. In particular, the authors need to account for the seasonal cycle and topography before the data can be used to answer their questions.

2 Major comments:

2.1 Source of the anomaly

The source attribution for this anomaly is severely lacking. The wind patterns do not seem to fit with their discussion. It appears that only the 850 hPa winds in August 2013 would have actually brought high-CH_4 airmasses from China (and only to the northern part of Japan). The only thing that seems to really stand out in the rest of the panels is the flow from the Pacific towards southern Japan, why would Saga have a large anomaly from this air? Hysplit runs would be more convincing for showing the sources of the airmasses. As for the CTM results, does the CTM capture the duration of this anomaly? What does a 2012 and 2013 timeseries of XCH_4 in the Japan region look like? Does the anomaly show up? What about the simulated surface sites?

2.2 Real or noise?

The authors claim that GOSAT is able to detect synoptic-scale XCH_4 enhancements. It's not clear to me that GOSAT was actually able to pick this up and that it's not just an artifact of the analysis. There are other periods in the record that GOSAT seems to do quite poorly compared to TCCON. For example, June/July 2012 in Figure 4 seems to be a $\sim$20 ppb anomaly in TCCON that GOSAT misses. Why is the former anomaly real and the latter just noise? The two sets of TCCON data are the only thing that makes me think this "anomaly" was real and I'm not convinced that GOSAT actually observed it.

Additionally, the authors claim that the modeled XCH_4 in August 2013 are lower than 2012 because of these strong zonal winds. However, the GOSAT observations don't seem to support this (Figure 2). Why would the GOSAT observations pick up the Japan high anomaly but not the China low anomaly?

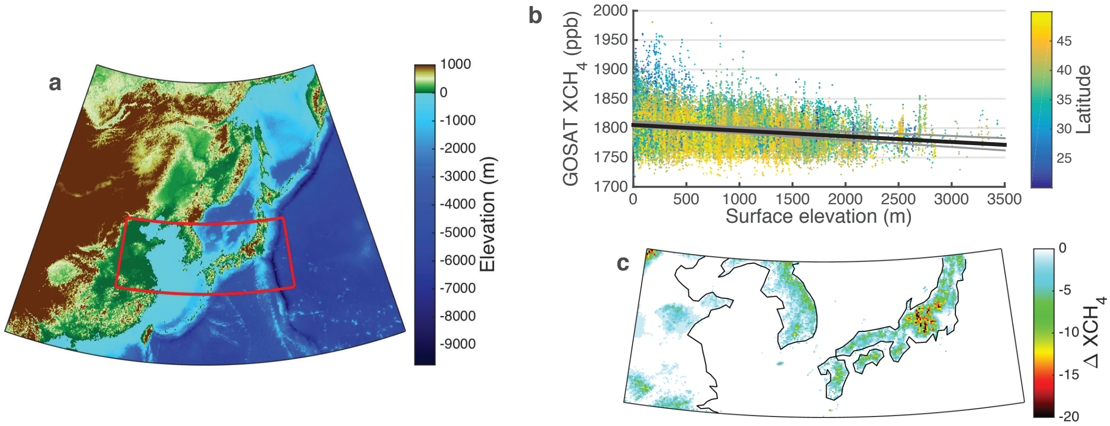

Figure 1: **Relationship between surface topography and XCH_4.** (**a**) East Asian elevation. Red box is the region considered in Ishizawa et al. (115–145°E, 30–40°N). (**b**) Relationship between summertime (JJA) GOSAT XCH_4 in the extratropical Northern Hemisphere (20–50°N) using the SRON RemoTeC retrievals. Black line is the error-weighted least-squares regression and gray lines are for 10° latitude bands. (**c**) Anomalous methane due to surface topography in the region considered in Ishizawa et al.

2.3 XCH_4 in different parts of a region are not directly comparable

Figure 1 shows a simple example of how topography can impact the XCH_4. This is why papers like Kort et al. (2014; GRL) computed anomalous methane by removing the bias due to topography. By averaging GOSAT observations over a large region you could be inducing a sampling bias. For example, if you have a higher density of GOSAT observations over Korea in 2012 and then in 2013 you have more observations over Bejing you will almost certainly have a higher "regional" XCH_4 simply due to topography. This effect can be up to 20 ppb in parts of Japan (near Mt. Fuji).

Additionally, in 2013 and 2014 you see an increase in GOSAT observations over Japan (bottom panel of Figure 2b). If these happened to be over Tokyo (lower elevation) it could explain part of this "Large XCH_4 anomaly". What is the spatial distribution of the GOSAT observations? A figure showing the location of the GOSAT observations would be helpful (maybe observation density).

2.4 Seasonal cycle

I've got a few issues with the treatment of the seasonal cycle:

1. Remove the seasonal cycle in your data. The anomalies seem to be on the order of 20 ppb, this is comparable to the peak-to-peak amplitude of the seasonal cycle. How much of this is seasonal?

2. The seasonal cycle in XCH_4 is not necessarily reflective of emissions. The seasonal cycle in the total column does not always follow the seasonal cycle in the emissions

(cf. the Bloom et al., 2010 discussion of SCIAMACHY columns and wetland emissions in the Amazon). Changes in stratospheric methane induce higher order harmonics that don't peak when emissions peak. Figure 5 from Saad et al. (2014; AMT) is a nice illustration of this. So statements like, "The summertime high XCH_4 must be partially attributed to the seasonal biogenic CH_4 emissions from rice paddies and natural wetlands underneath East China and Korea." are not well founded.

3. Figure 7 is presented as "CH_4 and XCH_4 in August and September 2012 and 2013, with respect to surface CH_4 and XCH_4 at South Pole". This does not make sense to me. Why would the authors present this as the difference between the Asia and the South Pole? They have different seasonal cycles. CH_4 concentrations at 40°N and the South Pole are 6-months out of phase (Northern hemisphere peaks when the Southern hemisphere is at a minimum). This makes interpretation of the plot nearly impossible. Are differences between years due to changes in a different (not shown) hemisphere? Are changes between August and September due to changes in the Southern hemisphere?

3 Minor comments:

Incomplete literature review
The authors don't seem to have cited any of the previous literature on this topic. The last paragraph on page 24997 briefly mentions a couple studies that used *in situ* observations to estimate methane fluxes but completely neglects the satellite studies (which are the more relevant studies to this work). Examples of relevant studies: Bergamaschi et al. (JGR 2007, JGR 2009, JGR 2013), Fraser et al. (ACP 2013), Monteil et al. (JGR 2013), Wecht et al. (JGR 2014), Kort et al. (GRL 2014), Cressot et al. (ACP 2014), Houweling et al. (ACP 2014), Turner et al. (ACP 2015), and Alexe et al. (ACP 2015) to name a few.

Page 24997, Lines 23: Miller et al. (2013) also use aircraft data.

Page 24999, Lines 16–17: How are you deducing the large methane sources in China? Bottom-up inventories, EDGAR, something else?

Page 25000, Lines 16–18: As I mentioned in the major comment, you can't compare the XCH_4 values. There are biases due to topography, for example, that you have not accounted for.

Page 25003, Lines 18–21: Wind patterns don't seem to support this.

Page 25004, Line 3: This is very coarse resolution, is this resolution sufficient to resolve these sort of spatial patterns? I'd rather see this plotted without the spatial interpolation, that way we can see the actual model resolution.

Page 25005, Line 4–8: However this isn't seen in the GOSAT data. So if this argument were true and GOSAT can pick up the synoptic event then why isn't it seeing this lower XCH_4 over China?

Figure 4: Does this have the seasonal cycle removed?

Figure 8: Shouldn't surface observations in Sept 2013 be lower than average since the air is mostly coming from the Pacific? How is this air coming from China? Especially at Saga.

4 References:

Bloom *et al.*: Large-scale controls of methanogenesis inferred from methane and gravity spaceborne data. *Science* **327**, 322-5, 2010.

Kort *et al.*: Four corners: The largest US methane anomaly viewed from space. *Geophys. Res. Lett.* **41**, doi:10.1002/2014GL0615053, 2014.

Saad *et al.*: Derivation of tropospheric methane from TCCON CH_4 and HF total column observations, *Atmos. Meas. Tech.*, **7**, 2907-2918, doi:10.5194/amt-7-2907-2014, 2014.