Review of **S. S. Peng et al.** Inventory of anthropogenic methane emissions in Mainland China from 1980 to 2010, ACP 2016
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The paper provides a consistent time series of CH4 emissions from China from 1980-2010. China is an important contributor to total global CH4 emissions and a better understanding of the sources and possible mitigation options is relevant for the scientific community. Methane emission inventories for China have been made before and as such the work is not novel but the compilation of the different sources and the consistent time series make it certainly worthwhile. Also, as discussed in the paper, the discrepancies between various existing estimates for China is substantial and the investigation of the causes or at least identification of sectors that are most uncertain is important for both the global and the Chinese CH4 budget. I think that for several sources the review of emission factors and especially possible trends in these emission factors or the emission controlling variables over the time period could be more in-depth and that this could still further improve the inventory. On the other hand, an inventory includes many sources and a balance between total time spend on each category and the overall result needs to be found. I would recommend the paper for publication but would like to see several points discussed in more detail or added. If for some reason the authors find it unrealistic or over-demanding to make those changes, some argumentation why this is not feasible or out of scope should be provided.

First of all, as lined out in the beginning of section 2.1., the methods of the IPCC GHG inventory guidelines were followed. The authors then search and use for several, but not all, sources more representative Chinese emission factors. I think it would be valuable to also have a full IPCC emission factor only emission calculation, next to the final result of the paper. This is 1) the easiest way to understand what the impact of the country specific emission factors (EFs) on the total Chinese emission estimate is. 2) In the comparison with the other sources such as EDGAR or EPA – again it would be very useful to know if these estimates' are in line or higher / lower than using avg IPCC EFs. Since the structure followed by the authors is based on the IPCC methodology, my feeling is making an "base-line" avg IPCC EF calculation is not a very demanding task. There are good arguments why the current approach is more accurate but it would provide a very useful benchmark for comparing the impact of more detailed information as well as in the comparison with EDGAR and EPA values.

An important aspect of the paper is the long time series. Something that is not well discussed is whether the activity data and emission factor data really cover the temporal changes. For example if the emission factors are based on using a certain technology but this technology was not used before 1990, the EF might not be representative for the 1980-1990 period. While there are good reasons to use it as best guess, the trend 1980-1990 is then highly uncertain and much less reliable than 1990-2010. I would like to discuss that in more detail for the CH4 emission from rice agriculture.

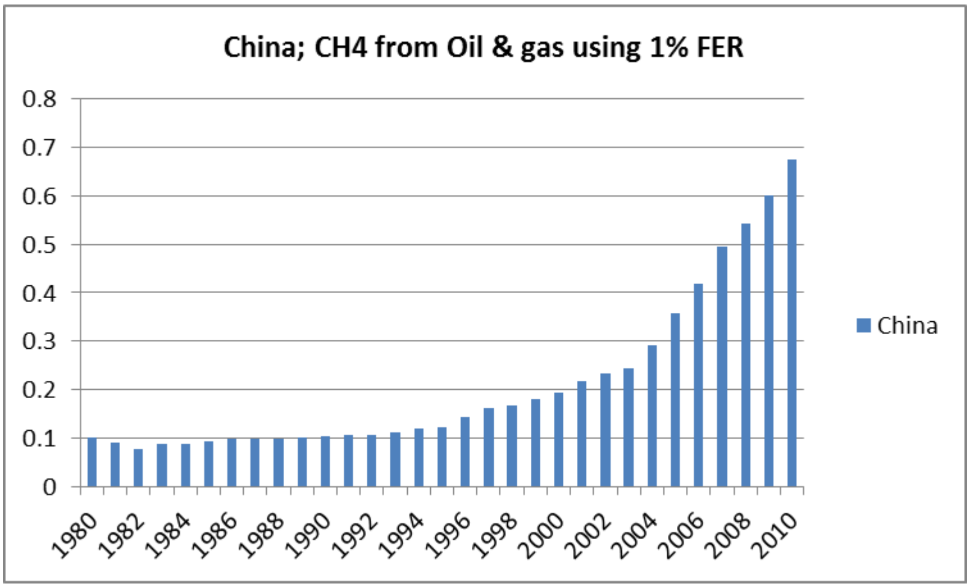
CH_4 emissions from rice agriculture

In section 2.2.2 the authors explain their approach to calculate CH_4 emissions from rice. While it is clearly acknowledged in the paper that the emission factors depend on such things as organic matter (OM) input and water management, no trends in these controlling factors are discussed. Denier van der Gon and Neue (1995) and Denier van der Gon (1999) have provided a simple, empirical impact relationship for CH_4 emission from rice fields with OM input versus chemical fertilizer. A ~5 t OM/ha input creates a doubling of the CH_4 emission, a 10 t OM/ha triples the CH_4 emission. Peng et al use an assumption based on Yan et al (2003) that 50% of the rice paddies received organic input. While that may be the case at a certain moment in time for the trend in CH_4 emission it is crucial to understand the trend in the OM input because it is such a strong driver of CH_4 emissions from rice fields. Denier van der Gon (1999) compiled the green manure statistics, fertilizer production and harvested rice area statistics in China over the period 1960-1995. Especially from the mid-1970s onwards the production of fertilizer in China grows tremendously but the harvested rice area remains the same or declines somewhat. It is a logical hypothesis that the every year increasing availability of fertilizer (urea) started replacing the much more labor-intensive use of OM incorporation. While reliable statistics for total OM use are lacking, the green manure statistics support this hypothesis. From 1980-1990 the harvest rice area slowly declines, the fertilizer production rapidly increases and the planted green manure area roughly halves. The green manure statistics are available at the regional level and show for example a much stronger impact in the Central and east China (See Figure 3 and table 1 in Denier van der Gon, 1999). The impact of less OM input in the rice field is further enhanced by the change of rice varieties from traditional to high yielding varieties. The main trait of these high yielding varieties is that they are very responsive to N fertilizer and allocate (or invest) a much smaller part of their total net primary production in the below ground root system (which will be the OM for the next growing season). This trend is described by Denier van der Gon (2000) but that paper does not give data for China – nevertheless the high yielding varieties have also been introduced in China and it will also have contributed to making less OM available for CH_4 production in Chinese rice soils. This reviewer would therefore argue that the the trend for CH_4 from rice as shown in table 3 of the paper, strongly underestimated the trend between 1980 and certainly 1990. An educated guess would be that the year 2000 value is realistic and in line with most available estimates as discussed by the authors but the emissions from rice should show a declining trend in emission since 1980 mostly due to lower OM input into the rice cropping system which is in line with the strong growing availability of urea fertilizer. The authors could use the trends and data compiled in Denier van der Gon 1999 or references therein which would result in a CH_4 emission from rice cultivation in an estimated range of ~15 Tg/yr. As a result the trend would be rather similar to EDGAR (Fig 2 in the paper), although the absolute emission level remains lower. Indeed, as mentioned by the authors, the increasing trend in the EDGAR estimate after 2003 is remarkable and not easily understood but that is outside of the scope of the paper.

CH4 Emissions from Oil and Gas industry

Emissions from natural gas production sites are characterized by skewed distributions, where a small percentage of sites—commonly labeled super-emitters—account for a majority of emissions. (Zavala-Araiza et al., 2015). The importance of these super-emitters in the O&G sector is a rather new insight and probably not well represented in the current emission factors. It only surfaced due to large numbers of measurements that showed the "fat tail distribution" of the EFs. Therefore, I would argue that using standard emission factors may well lead to underestimation for the emissions from this sector. Moreover, the emission factors used in the paper appear really low. I would like to see a very simple "sanity check" on these numbers. When taking the total calculated CH4 emission from the Oil and Gas industry for example in 2000 (0.1 Tg / yr) or 2010 (0.3 Tg/yr) (see Table 3 in the paper); what share is due to the gas industry and what percentage of total natural gas production is this? And does it make sense over time? At a first glance it seems a really low estimate that is presented here. To get a feeling I have taken the data from Schwietzke et al., 2015 and looked at the CH4 emissions from china from Natural gas industry only if a Fugitive Emission Rate (FER) of 1% is assumed (see figure below). This leads to a factor 2 higher emissions than reported by Peng et al. and the gap is much bigger because in the below estimate oil industry is not included whereas Peng's estimate includes both oil and gas. While this does not mean that the presented estimated in the paper is wrong, I would like to see more discussion and think that expressing the FER as a % of the production is a very useful thing to do to show that really low % are currently assumed in this paper whereas recent measurements in the US and Canada found FER's of 2-4% more realistic.

Constant global avg. Fugitive Emission Release (FER) of 1% of natural gas production only: data taken from Schwietzke et al., 2014. The figure does not include the oil sector emissions yet but these are available from Schwietzke et al and would further increase the emission estimate.



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Data table for figure O&G CH4 emissions derived from Schwietzke et al., 2014:

year	China
1980	0.102085
1981	0.090967
1982	0.077018
1983	0.087126
1984	0.088541
1985	0.092382
1986	0.09784
1987	0.099922
1988	0.09923
1989	0.102085
1990	0.102691
1991	0.10633
1992	0.107797
1993	0.112865
1994	0.119005
1995	0.121574
1996	0.143898
1997	0.161981
1998	0.166086
1999	0.179778
2000	0.194534
2001	0.216379
2002	0.233012
2003	0.244863
2004	0.29098
2005	0.356372
2006	0.417752
2007	0.494509
2008	0.542839
2009	0.601392
2010	0.67398