Review of the paper “Increasing emissions of HCFC-123 and HCFC-124 may be due to leakage during HFC-125 production” by Western et. al.

This paper identifies sources of HCFC-123 and HCFC-124 emissions that point to their unexpected persistence in the atmosphere despite phase-out regulations under the Montreal Protocol. The work shows measurements of these species taken at several regional and global stations, which are combined with inverse modelling to estimate emission rates, especially from Asia, Europe, and the United States. The authors discuss that these emissions are by-products in the production process of HCFC-125, a non-ozone depleting substance, used in many applications instead of regulated compounds. Thus, the authors propose that current levels of HCFC-123 and HCFC-124 come from emissions generated during the production process of HCFC-125. In my view, this source identification is an important scientific contribution that fits within the scope of ACP and is a meaningful finding for future regulations of ODS. However, there are some points that require clarification prior to acceptance for publication:

Figure 1 (a, b) shows that the atmospheric abundance of HCFC-124 has always been greater in the northern hemisphere. At the same time, there are very few measurements in the southern hemisphere. Granted that most of the production is done in the northern hemisphere, it seems strange that this species never mixed completely in the atmosphere in 30 years. Could it be that a notorious lack of measurements in the southern hemisphere is introducing a bias in the calculation? Can authors offer some quantitative idea of how the global mean is affected by this aspect?

Authors conclude that HCFC-124 emissions have being increasing globally but the source region for excess emissions is not clear because emissions from Europe, US, and eastern Asia have not increased since 2015. However, looking at Figure 5, 6, and 7, this conclusion seems counterintuitive. Looking at the entire time of the trends presented in the figures, emissions have fallen consistently in Europe and the US, but they have increased in eastern Asia. Thus, it seems that one of the source regions of HCFC-124, precisely, is eastern Asia. Hence, there seems to be either an apparent contradiction or a tacit implication that the authors should clarify.

The previous point is also evident if the figures in the paper are seen as a whole. According to the authors, emissions of HCFC-124 from the US dropped to almost half the amounts in 2015 (0.5 to 0.26 Gg yr^{-1}). Emissions in Europe have dropped in line with phase out regulations. However, emissions of HCFC-124 from eastern China more than doubled in 15 years (0.2 to 0.45 Gg yr-1) and industrial regions are identified. It seems that the authors feel conservative at directly stating that eastern China is a source region. Could the authors comment on this?

It would be beneficial to the paper and the readership that authors include a summary of the HCFC-125, HCFC-124, HCFC-123 inventories or reported values from every region and contrast the inversion model results with the reported values.

The authors use inverse modeling to estimate emissions from source regions (Europe, US, and eastern Asia) for which they use a priori emission estimates. Did the authors consider using a priori values (even if arbitrary as done in this and other papers) to estimate/model potential emissions from other regions such as from Brazil in South America?

Looking at the entire trend in Figure 3a, it shows a global decrease of HCFC-124 from 1997 until 2019, when authors indicate that emissions begin to increase again. In Figure 3b, HCFC-123 emissions have continuously increased for the same time period, although with greater uncertainty. In the past 5 years or so, the speed of increase of HCFC-123 seems to have decreased. If both are byproducts in the production of HCFC-125, how is it that only HCFC-124 generally decreased while HCFC-123 generally increased? Although the authors discuss uncertainties in HCFC-123 and acknowledge the different trends, it is not entirely clear/convincing why both trends are so different. The authors should better clarify this portion of the paper.

Also, with respect to Figure 3a, since 2019 HCFC-124 emissions seem to go up again, which authors point to in the text. However, there are many bumps just like that (or bigger) in previous years in the entire trend and yet as a whole the trend is negative since about 99. Could it be that this “bump” is similar to the previous ones and the trend could still be negative overall?

The authors recognize that the global increase in HCFC-123 and 124 emissions does not currently represent a threat to the ozone layer recovery, but they present the important implication of whether other ODS or GHG could be emitted (or are being presently emitted) as intermediates from the production processes of fluorochemicals. This perspective is eye-opening. Could the authors offer some more discussion on potential substances or industrial processes that should be investigated more closely?