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Interactive comment on "Russia's black carbon emissions: focus on diesel sources" *by* N. Kholod et al.

Anonymous Referee #2

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Overall, this is a good effort at building a better emissions inventory for Russian on-road and off-road diesel use (though excluding military diesel usage.) I am glad the authors account for super-emitters.

However, I have some concerns regarding the uncertainty propagation, the super-emitter fraction, and relatively unexplained emission factor data. Once these are clarified, this manuscript should be acceptable for publication.

One final concern is that the current submission has no explanation of differences between this paper, and the on-road BC emissions estimate published earlier by the first author (Kholod and Evans, http://dx.doi.org/10.1016/j.envsci.2015.10.017) While the current paper is more detailed, the bottom line figure appears the same - in 2015, Figure 1 of Kholod and Evans shows 20,000 tons of BC from on-road Russian sources,

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Discussion paper

similar to the current paper. Maybe the complicated model of the current submission is not needed?!

Detailed comments: Lines 181-184: authors claim “Russia does not have large-scale production of diesel passenger cars”, but also say 98% of diesel cars were either imported or produced in Russia by foreign companies. Not sure how this affects the overall emissions, but what fraction of on-road diesel cars are made in Russia by Russian or non-Russian companies? (Also, since this paper focuses on diesel cars, the line about “foreign-make cars, both gasoline and diesel” is superfluous.)

Lines 203-204: what higher emissions standards do imported diesel vehicles meet? Euro 6? Or were imported vehicles always produced to meet a higher standard than necessary for Russia?

Lines 240-251: The authors rely on the Bond et al. (2004) assumption of super-emitter fraction as 10%, even though they cite several more recent studies that show super-emitters can be as high as 13-15% of the fleet, even in California. Given the lack of studies in Russia, and the authors’ literature survey of the Russian fleet (36% of trucks and 23% of buses older than 20 years), using the old Bond et al. (2004) assumption will likely bias their emissions inventory low as the authors acknowledge at the end. The authors should investigate the sensitivity of their results to this fraction, and perhaps try higher values (15-30%) for the super-emitter fraction?

Lines 286-287: What is the basis for their assumption of 40-20-40 on urban roads, rural roads, and highways?

The authors use NIIAT data for on-road emission factors, but the actual source of that data is not clear – are these based on measurements or on estimates based on emissions standards? The authors present the data used; a brief explanation of the source methodology will be helpful, since these NIIAT publications do not appear to be easily accessible online. (I checked their website. I don’t know Russian.)

The conclusions should note that the results exclude military diesel usage emissions; in particular, these could be large sources of sulfate PM, and possibly also BC.

While the authors present a comprehensive list of potential uncertainties with their emissions inventory estimate, they don't propagate the uncertainties through, which would be helpful. From their list, it appears the emission factors could produce uncertainties of +/-30% or so, while the bias due to low super-emitter fraction (10% when 15-20% might be more appropriate) could increase the overall BC estimate by as much as 40%!

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