**Response to Reviewer #2**

The authors would like to thank the reviewer for the thorough analysis of the paper.

The paper has benefitted from the comments, and the revised version is significantly improved.

These are the responses (in red) to the reviewer’s comments (in black). All modifications to the paper are reported in this document and highlighted in red in the manuscript pdf.

**This paper discusses the simulations of jet noise with different grid densities and works towards predicting far-field sound covering a "larger" range of frequency using the combination of fine and coarse grid sets. The decomposition of pressure field in both near and far field shows that the finer mesh can capture acoustics of high frequency and azimuthal mode. The paper is acceptable for publication, subject to some revisions. There is one general concern:  
The results look promising but this is not clear yet how the large-scale turbulent structures can be isolated from small scales, which lays the foundation of the currently presented sound spectra relay method. I would think the large scale could get affected if the small scales cannot be properly resolved. Especially the grid used here is very coarse, i.e. 5 or 10 million for second order scheme at such high Re number jets.**

This paper tries to show how coarser grids could still be effective in correctly capturing the low frequency behaviour of jets. A good subgrid scale modelling should be able to model the unresolved scaled and their effect on the resolved one. Indeed, for such High Re numbers, 5M or 10M points might not be enough, and the results show how in those coarser grids the potential core length is underpredicted for the round nozzle and the shear layer transition is delayed. Nevertheless, the acoustic analysis consistently shows, through frequency decomposition and azimuthal modes, that low-frequency and low-mode content does not vary significantly with mesh refinement. One could argue that maybe the 20M grid (or even 40M) might be more reliable as a “coarse” case, but this would not invalidate the main message of the paper, i.e. the potential for combining spectra from grids with successive refinement.

**Specific comments are listed below:  
(1) Table 1, better to express the time step non-dimensionally.**

Time step is now expressed non-dimensionally. **(2) P5, in addition to the grid refinement strategy, it is worth giving a table of the grid spacings of one reference grid set, i.e. 80M or 10M. in terms of D/dx, D/dr at least in the region of interest, e.g. along the lipline. It will indicate how well resolved at each level of grid set.**

Grid spacings relevant parameters are now reported in Table 1. **(3) P7, as to the turbulence modelling, it is not clear that it is wall resolved or wall modelled LES for the moment. It seems to be wall resolved but how the inlet turbulence near the wall is treated. As it is shown by Bres et al. 2018, the inlet turbulence has profound effects on jet noise prediction. How does it agree with experiment inlet flow condition?  
BRÈS, G. A., JORDAN, P., JAUNET, V. & LE RALLIC, M. 2018 Importance of the nozzle-exit boundary-layer state in subsonic turbulent jets. J. Fluid Mech. 851, 83-124.**

A brief discussion on the state of the internal boundary layers has been added in section 2.2, as follows:

*Recent studies focused on the effects of nozzle inflow and boundary layer turbulence on jet development and noise generation \citep{bogey\_influence\_2012,bogey\_identification\_2013,bogey\_study\_2016,bres\_importance\_2018}. The introduction of turbulence was shown to improve the flow field and sound prediction for jets originating from a straight cylindrical pipe, at an increased computational cost and complexity due to the generation of synthetic turbulence, near-wall grid refinement and wall modelling. However, the strong sensitivity to the resulting boundary layer characteristics requires adequate knowledge of the experimental conditions, which are not available for the case studied in the present work. Additionally, the presence of a contraction angle within the SMC000 and SMC006 nozzles generates a favourable pressure gradient which thins the nozzle boundary layer and has a stabilizing effect on the turbulence, forcing the boundary layer towards partial relaminarization \citep{uzun\_prediction\_2011}. For these reasons, in the present simulations, no wall treatment was specified and no turbulence was introduced at the inlet.* **(4) P9," the coarser grids used in this study are able to capture the large-scale behaviour of the jet, despite the lack of smaller-scale information." This comes to the general concern. It needs to be proved by showing that the integral length scales along the lip line are the same on both the coarse and fine grids.**

Hao, could you help me with this?

It is worth noting that there is limit on how coarse the coarser grid can be in order to still capture the majority of the turbulent energy, i.e. the coarsening process cannot go on for ever. In our case, the coarser grids are not exactly coarse in this regard. In fact, our lip-line and radial statistics have shown that the coarser grids capture similar amount of fluctuation as the fine ones do.

We agree with the reviewer in the sense that once LES (for instance, Smag or Sigma models) is employed the subgrid scales will not be accounted for in terms of backscatter effects. However, as a matter of fact, jet dynamics is less prone to backscatter effects. As conclude by Sukuzi and Colonius (JFM 565:198-226, 2006) in their well agreed prediction and experiment results, the influence of small-scale turbulence on large-scale structures are negligible. In some way, our results confirm this. However, that’s not to say that if we keep coarsening grids much further this would still be negligible as it may not capture the flow correctly to begin with, and comparison turbulent statistics should be able to identify that.

A statement is now added to xx page …

**(5) Figure 5, why does R0-40M centerline result look worse than the 10M or 20M result?**

We do not have an explanation for this discrepancy. It should be noted that the 40M case is the first case to match the experiment for x/D<10. This improved behaviour might have an unwanted effect downstream which requires an even finer mesh (80M) to counteract it. However, it is also possible that this discrepancy might be mitigated with longer runtimes.

**(6) Figure 7, the round nozzle should be chevron nozzle.**

This has been corrected. **(7) Figure 7, 80M tends to slightly overpredict the potential core, what is the reason? Will it continue to overpredict when refining the mesh?**

Given the lack of information on the experimental measurement error, we cannot draw strong conclusion about the relative accuracy of the different grids. Considering also the plot of centerline turbulence on the right, it looks like the 40M and 80M grids show very similar behaviour, which is an indication of refinement convergence. **(8) Figure 8, the profile at 0.5-2.5 is not satisfactory. Is it due to the initial boundary layer state?**

This should not affect the objective of the paper. However, a brief comment has been added, as follows:

*The agreement with experiments is good for all grids, with some discrepancy at $x/D\_{j}=0.5$, 1 and 2.5 in the outer region, possibly due to the absence of numerical inflow turbulence.* **(9) Figure 9, what is the cut-off frequency for S-10M and S-80M?**

In the energy spectra the cutoff frequency corresponds to the LES SGS cutoff wavenumber, where the filter is based on the grid cell size. In this region of the mesh the cells are very fine (with deltaX/D around 0.005). This gives an estimated cut-off frequency well above the max frequency shown in the figures St=10. The authors believe the energy drop at x/D=0.2 is not due to grid limitation as in the far-field noise spectra, but to the transition behaviour of the shear layer, where the smallest structures have not yet developed. **(10) P13, "this (transition process) will not necessarily have a strong effect on the strength of the noise sources in this region of flow." I don't think it is true from my experience. This process will have strong impact on the noise spectra at forward arcs where polar angle is greater than 90 degree, which isn't shown in the current paper. Therefore, the noise source is affected by this transition process, mainly fine-scale turbulence mixing noise.**

The authors agree that a well captured fine-scale turbulence will affect the noise generation, especially at high frequencies. The idea behind that sentence was to anticipate the results presented in the following section, in which indeed the grid refinement improves the high frequency content, but with limited effect on the low-to-mid frequency range. The sentence has been modified as follows:

*As will be shown in Section \ref{sec:Acoustic-analysis}, this grid refinement effect on turbulence will produce a spreading of the spectra towards higher frequencies, with limited effect in the low and medium frequency range.*  **(11) Figure 11, was this figure generated by performing azimuthal averaging or decomposition?**

The data used to produce the Figure is not volume data, but cut-plane data. No azimuthal analysis was therefore possible. The filtering was performed over 1/12th octave bands. All this is now clear in the text, as follows:

*The temporal Fourier transform of the pressure fluctuation on the $x$-$y$ cut-plane is filtered at Strouhal numbers $\text{St} = 0.3$, 0.6, 1.0 and 2.0 over 1/12th octave bands, and the real part of its inverse transform is shown in Figure \ref{fig:single\_freq\_2} for cases S6-10M and S6-80M.* **(12) P16, "limitations on the high-frequency far-field information come mainly from the imperfect short-distance propagation to the FW-H surface, rather than from under-resolved sources." Please provide evidence that the under-resolved sound source is not important by using 80M and 10M grid sets.**

The authors do believe that a coarser grid will generate less fine sources within the jet plume. But the short-distance noise propagation to the FW-H surface will reduce the cut-off frequency even further. Figure 12 (now Figure 14, *Pressure fluctuation along the dash-dot lines*…) shows that at St=2 the 10M grid is still producing noise at a similar strength as the 80M grid, but it does not travel far enough to reach the FW-H surface. The sentence has been modified for clarity, as follows:

*This means that the imperfect short distance propagation to the FW-H surface will produce a loss of high frequency information even if the corresponding noise sources are well resolved in the jet plume*. **(13) Equation 7, should PSD be defined in log10 as dB is used in the figure?**

Equation 7 has been modified following the remark. **(14) Figure 14 and especially Figure 15, the sound is significant underpredicted. What is the reason? Will it affect the quality of the simulation?**

Does the reviewer refer to the 30° angle spectra in those Figures?

It is true that the sound is underpredicted, especially in the chevron case, as described in the text.

Regarding the overall quality of the simulations, the authors believe that all other results show a very good agreement with experiments, and the overall focus of the paper is not affected by this discrepancy.

Regarding the cause of this discrepancy, it is not easy to identify. At these low angles, this could be due to some of the grid parameters (such as grid stretching in the axial direction), but that would mean that the results should improve with finer grids (See new Table 1), whereas they stay the same. This is why we identified the SGS model, which might have a strong effect for all grids, as a possible cause. However, since this link is not clear, we have rewritten the paragraph as follows:

*In the serrated case, the $\theta=30\text{\textdegree}$ spectra of Figure \ref{fig:psd-chevron-30} do not show a noticeable dependence on grid refinement, thus confirming the suitability of the coarse grids for low-frequency dominated noise, although they all present a lower sound level at mid-to-high frequencies compared to the experiments*, *the cause of which has not been identified*. **(15) P20, first sentence, "due" -> due to, and how to justify it is the effects of SGS model not due to the fact that the grid is too coarse.**

See response to previous comment. **(16) Equation 8, missing a variable /phi in p(t).**

Equation 8 has been corrected. **(17) Equation 9, should it be log10?**

Equation 9 is now expressed with log10. **(18) Figure 17, Is it the azimuthal decomposition of far-field sound? Does it also mean that the sound generated by near-field flow structures of the same azimuthal mode?**

Yes, the caption has been modified to “*PSD of far-field azimuthal modes*…”.

It does not necessarily mean that the azimuthal decomposition of the sources follows the same behaviour, as the far-field observers receive a signal from multiple sources within the plume.  **(19) P24, the paper gives the method to estimate St\_min, but how to estimate the cut-off frequency of predicted far-field sound, i.e. St\_max? The current prediction is not very satisfactory as the industry requires St\_max around 10 but only 3 for the current simulation with 80M. It seems that it is impossible to get there with this method.**

The high cut-off frequency St\_max, as explained in a previous comment, depends strongly on the grid refinement in the short-distance propagation towards the FW-H surface. In section 4.1, the pressure fluctuations of Figure 12 (now Figure 14, “*Pressure fluctuation along the dash-dot lines*…”) are sustained until “*the number of points per wavelength drops below 25*”, which is a common observation for this kind of simulations.

The following sentence has been added in Section 4.3 to provide a method to estimate St\_max:

*A first estimate for $\text{St}\_\text{max}$ can be derived from the grid cell sizes in proximity of the FW-H surface, with the requirement of 25 points per wavelength, as discussed in Section \ref{sec:near-field}. The analysis of the Fourier-decomposed pressure waves, e.g. in Figure \ref{fig:single\_freq\_2}, can yield a more accurate estimate of $\text{St}\_\text{max}$, without the need for comparison with experiments.*

In terms of reaching higher St\_max for industrial applications, a result of this paper is that very fine grids only need to run for a short time, allowing for even finer simulations than the ones presented to be affordable. More importantly, a possible strategy to increase St\_max could be to produce grids with more carefully designed refinement. In the present paper the authors were interested in consistency between grids, but it is expected that a zonal refinement in the proximity of the nozzle lips would yield even better results for the same global grid size. A sentence has been added as follows:

*The grid strategy applied in this paper was chosen to ensure consistency in the successive refinement. For the discussed approach to be viable for even higher cut-off frequencies, as is typically required in an industrial context, a more carefully designed zonal grid refinement is necessary, to yield broader noise spectra for the same global grid size.* **(20) P24, "a) a general spectral correlation exists among different grid resolutions;" is not clear to me. What is the spectral correlation among grid resolutions? Better to write in specific.**

The sentence has been modified as follows:

*Different noise frequency ranges and modes are affected in a different way by the grid resolution*.