MNRAS: MN-20-5012-L: Response to the Referee Report

Dear Editor,

We thank the referee for their detailed and comprehensive review of our manuscript. We have majorly revised our paper accordingly with a more careful analysis of our key result and better constraints on our main conclusions. We address below all the points raised in detail and attached a PDF of our revised manuscript with key changes highlighted in yellow. The paper has been restructured from a Letter into a full journal article, and what was the 'Materials and Methods' section has been superseded by Section 2 titled 'Burst Analysis', where we describe in detail the changes to our analysis and the justification for our methodology. We recommend reading the response to Point 5 in this report first as this summarizes the new analysis and changes the context in which many of the other points were made. Point 7 also has relevant discussion in regards to the robustness of the results. In this report the points raised by the referee are in bold and our responses are in plain text.

We hope that our revised manuscript will prove satisfactory.

Sincerely,
M. A. Chamma

Editor's Comments:

Point 0:

Please make sure you justify publication as a Letter, especially with a lengthy Supplementary section. While interesting, you should explain why this would not be better as a publication in the Main Journal with the Supplementary material integrated into the main text.

Response:

We agree with the editor that much of the lengthy supplementary section is better suited in the main text and have accordingly restructured our paper into a full journal article. The material that was in the supplementary section is now largely found in Sections 2 and 3, and Appendices A and B.

Reviewer's Comments:

The paper presents an important and unexpected relationship in the signal observed from three repeating fast radio burst (FRB) sources. Moreover, the authors link this result to their previous model, which satisfactorily describes the observed trend. The result is certainly interesting and worth publication. The analysis is well presented and the manuscript is concise and generally clear. I have substantial comments, however, on the robustness of the result that I report in the following. I hope that addressing these comments will strengthen the results of the paper and increase its impact. Despite presented in detail somewhere else, the paper could also benefit from an additional description of the model previously developed by the authors. I will present my major concerns first, followed by a list of additional possible improvements. There are two main points in the analysis that could impact the central observational result of the paper presented in Fig. 1. The first point concerns the details of the autocorrelation function (ACF) used by the authors and the second one concerns the effect of dispersion measure (DM) on the analysis.

I list in the following my concerns regarding the ACF analysis.

Main Points:

Point 1:

Although the authors explain in the introduction the difference between frequency drift within and among single components of a burst, I find a general confusion throughout the manuscript on the subject. The confusion is partially due (but not limited to) the fact that the term "sub-burst drift" is generically used in the literature to indicate the latter effect, while the authors use the same term to indicate the former. I realize the same group defined the terms in their published paper by Rajabi et al. 2020. However, I would urge the authors to replicate the definition here and, eventually, even coin a new term (something on the line of "sub-burst slope" could be an idea) to avoid any misunderstanding from the readers.

Response:

While we prefer the terms and definitions used in Rajabi et al. (2020), we recognise the potential for confusion. We therefore adopted the term "slope" instead of "drift" for individual sub-bursts throughout the text. We added the following footnote in the new Sec. 1.1, where the model of Rajabi et al. (2020) is now summarised:

"We denote an FRB event or burst as containing one lone or many pulses of radiation, as is observed for the so-called sad trombone effect, for example. A single pulse in an event containing several pulses will be defined as a sub-burst. Rajabi et al. (2020) used the term "sub-burst drift" to describe the observed signal delay t_{D} as a function of the measured frequency ν_{obs} within an individual sub-burst (as in equation (1)). However, since this term is also sometimes found in the literature to denote delays between separate sub-bursts (as in the sad trombone effect), we will instead opt for "sub-burst slope" to describe the internal drift within an individual sub-burst to avoid any possible confusion."

Point 2:

It is not mentioned how the ACF is run on single components of the bursts. This is central to the paper and the method to select single components and cut them in time, with the consequent problem of having a very short baseline for the ACF, should be discussed in detail. This also applies to the measurement of ν_{obs}.

Response:

In the original manuscript the supplementary material briefly mentioned that the components are separated into distinct waterfalls before running the ACF analysis. For bursts with resolved multiple components, which is the minority of bursts analysed, the components are separated by identifying valleys in the time series of the data. The components are then padded with a background sample of the burst in order to account for the short baselines that can arise. Some bursts are not clearly resolved, but whenever the sub-burst slope changes mid-burst an attempt is made to separate components. The measurement of ν_{obs} occurs on each of the separated components.

We have addressed this point with the penultimate paragraph of Section 2.1 (Effect of Dispersion Measure). Additional text regarding ν_{obs} can be found in the last paragraph of Appendix A (Measuring Burst Properties).

Point 3:

Most of the bursts from FRB 121102 are coherently dedispersed and have a higher time resolution, which implies sub-components are generally resolved, whereas bursts from the other two FRBs analyzed have much lower time resolution and single components cannot be resolved in most cases (e.g. see Figure 1 of Michilli et al. 2020; arxiv 2010.06748). Therefore, the authors are likely comparing different phenomena (i.e. the "sad-trombone effect" and the "sub-burst drift") on the same Fig. 1 for different sources. I am thus very surprised that the two populations lie on the same fitted line given that the authors state that the latter produces "a steeper frequency drift". Even in the unlikely case that most of the low-resolution bursts have a single component, I am still surprised by the lack of substantial outliers caused by accidentally include two or more single components in the same ACF due to low S/N or poor time resolution. It is even possible to see by eye that some bursts have unresolved components, such as Bursts 14, 15, 18 23, and 36 of Fig. 7 among others. Is it possible that sad trombone and sub-burst drift follow the same relation? It seems unlikely from the model presented by the authors.

Response:

As explained our the response to the previous point, we do our best to separate individual sub-bursts whenever possible. But as is correctly pointed out by the referee it is likely that there are cases where multiple sub-bursts are blended and treated as one in our analysis. Although it may at first seem surprising that our analysis of these occurrences yield results consistent with those obtained for individual sub-bursts, it is actually to be expected when considering our model and the underlying equations. That is, the main relation that is central to this paper, i.e., equation (1), stems from a more general equation (i.e., equation (6) in Rajabi et al. 2020) that ultimately relates time intervals in the observer's frame with observed frequency intervals and time intervals in the FRB rest frame

$$\Delta t_{\mathrm{D}} = -t_{\mathrm{D}} \left(\frac{\Delta\nu_{\mathrm{obs}}}{\nu_{\mathrm{obs}}} - \frac{\Delta\tau'_{\mathrm{D}}}{\tau'_{\mathrm{D}}} \right). \tag{0.1}$$

Equation (1) in the paper results when $\Delta\tau'_{\mathrm{D}} = 0$, which will not only apply for individual sub-bursts but also for groups of closely temporally located sub-bursts in the FRB rest frame with $\Delta\tau'_{\mathrm{D}} \simeq 0$. One therefore expects that blended sub-bursts should follow the same scaling law that is central to the paper; our results are thus not surprising when considered in this context.

Other groups of sub-bursts where $\Delta\tau'_{\mathrm{D}} \neq 0$ will rather follow the law applicable to the sad (or happy) trombone effect given as equation (3) in our paper, but more thoroughly discussed in Rajabi et al. (2020); see equation (8) there.

I report in the following my comments on the DM that could produce a trend similar to the one reported in Fig. 1. The authors acknowledge that a wrong DM value could produce a spurious rotation in the 2D Gaussian used to fit the ACF together with an increase of the pulse width. This effect, as recognized in the Supplementary Material, could produce a trend similar to the one observed. The authors show that this would not influence the results of the paper. I do not agree with the conclusion and list my main points of concern.

As this is one of the key concerns of our manuscript we address this point in great detail in Points 5 to 7.

Point 4:

The plot in Fig. 2 of the Supplementary Material should be produced in the same parameter space as Fig. 1 on the Main Text to present a fair comparison.

It is shown in Fig. 2 of the Supplementary Material that a wrong DM would cause a global shift of the vertical axis for a single source. However, what about different sources, possibly with different ΔDM values used for each source? Should this not produce a jump among the values of different sources?

Response:

(This point is in reference to the angle vs. duration plot that is now Figure 3 of the paper)

In order to more clearly prove that DM variations do not affect our conclusion that a burst's sub-burst slope is inversely proportional to its duration, we move away from the argument previously made with this figure and performed a new analysis where each burst has its properties measured across several DMs. The response to Point 5 explains this change in analysis in more detail.

Fig. 2 of the Supplementary Material (the plot of sub-burst angle vs. sub-burst duration) is now Figure 3 of the paper. As it is no longer part of the argument supporting our conclusions, it has been moved to Section 3.1, where we simply discuss the connection between DM variations and rotations of a burst's autocorrelation function.

Point 5:

Only the case of a globally wrong DM is discussed. However, if the DM of a single source evolves with time (as is the case with FRB 121102, see for example Hilmarsson et al. 2020), ΔDM will be different for different bursts. This will not only introduce an additional spread in the data points but also determine a certain trend as ΔDM(t) varies with time.

As a consequence of the previous point, caution is required when using a sample of bursts de-dispersed to a single DM value. How was this DM value selected and how well does it work for the full sample? The timescale and amplitude of DM variations for a single FRB source are unknown. Therefore, the authors should either estimate a DM value for every single burst (this would be my preferred solution, as it would also give an uncertainty from the DM error), show how much DM variation within a realistic uncertainty region will affect the result (considering, for example, both long-term and short-term DM variations), and/or discuss very carefully what the assumptions are (e.g. no DM variations within a certain amount of days) and justify them.

Response:

In order to properly address this point and the ones that follow we decided that the best path forward was to determine what range of DM values have been observed for each source and make multiple measurements of each sub-burst slope and temporal duration across that DM range. As mentioned by the referee, one option is to estimate a DM value for each burst. However, due to the ambiguity some bursts have in terms of whether an S/N optimized DM or a structure optimized DM is appropriate (see Figure 1 of Gajjar et al. 2018, for example), as well as the possibility of long- and short-term DM variability with time, it is more prudent to choose a large range of DMs based on values of the DM that have been observed up until now.

Thus for each source we determine a range of DMs that is much broader than the DM uncertainty found for each individual burst from their respective publications. For example, from the FRB121102 data we used from Michilli et al. (2018), the DM for Burst #6 is found by aligning it vertically to obtain DM$= 559.7 \pm 0.1$ pc/cm^3, a very precise determination. However, since it is mentioned that the dispersion measure variations in the context of other bursts from FRB121102 are likely at the $\lesssim 1\%$ level, for this dataset we adopt a range of DMs = 554.1–565.3 pc/cm^3 that corresponds to variations at the 1%

level, and make the safe assumption that the true DM of any of the bursts we analyse lies within this range. To obtain an estimate of the error due to these DM variations, we measure the sub-burst slope and temporal duration of each burst across a grid of these DMs, and the range of measurements obtained gives us a measurement uncertainty due to the DM uncertainty. Because of the large DM range adopted, the uncertainty found in this way is almost definitely larger than the true uncertainty. Despite this, the data still indicate an inverse trend between the sub-burst slope and temporal duration.

Other considerations go into determining the DM range to use. For instance, DMs that yield predominantly positive drift rates or that clearly still have the $\nu_{\rm obs}^{-2}$ dependence (i.e., are distorted) are excluded. In practice we push the range to as low a DM as possible since these drift measurements are negative (and physical) while some higher DMs are excluded for some individual bursts due to positive (non-physical) slopes.

In the case of FRB 121102, Hilmarrson et al. (2021) mentions that its DM roughly increases at 1 pc/cm^3 per year (roughly from 557-563 pc/cm^3). Data from Gajjar et al. (2018) indicates that the DM can vary more broadly than that, with the DM of some bursts being found to be as high as 636 ± 11 pc/cm^3, whereas Spitler et al. (2014, 2016), Scholz et al. (2016), and Law et al. (2017) indicate a range of DMs between 553 and 569 pc/cm^3. It is apparent that the phrase "DM of a source" is akin to saying "the weather in London", i.e., a quantity that can be expected to not change drastically but whose precise value changes with time or from burst to burst. The ranges we have chosen for this source and the others properly encapsulate the variation observed for the subset of bursts selected.

All the details of this analysis can be found in Sections 2.1 (Effect of Dispersion Measure) and 2.2 (Measurement Exclusions and Fitting), which were added to address this point. Figure 1 has also been extensively updated to reflect this analysis. The capped lines for each burst now show the ranges of measurements possible yielded by the DM range analysis as well as shaded regions that reflect the possible fits to the equation $A/t_{\rm w}$ given the range of possible measurements.

Point 6:

Special caution is required for FRB 121102. The authors use different DM values for the three samples of bursts from FRB 121102 coming from three separate studies. This is an arbitrary choice and is likely to affect the result. For example, Michilli et al. 2018 selected a single DM for their sample from the shortest burst, which will thus be vertical in the waterfall plot by definition. The other bursts may or may not be de-dispersed at the right DM value. Given that this affects the sub-burst drift measured by the authors in the paper, this is a relevant issue for the manuscript. Moreover, the DM of different samples from FRB 121102 have been selected by different authors using different methods. The fact that they lie on the same line could just

be an artifact of a wrong DM value.

Response:

This is largely addressed by considering a range of DMs instead of a single DM for each burst (see Point 5). Different DM ranges are used for the FRB121102 data used from Michilli et al. (2018) and Gajjar et al. (2018) to better suit the bursts used, but both ranges are broad and cover the DMs of multiple bursts. Note that the single burst from Josephy et al. (2019) is displayed 'as is' and was not part of the DM range analysis (we did not arrange access to the burst waterfall). As seen in the redone Figure 1 of the paper, the points from FRB121102 still exhibit the predicted trend and the capped lines indicating the range of possible measurements for these bursts indicate that the trend is not an artifact of the DM, as it holds (to varying degrees) for all the DMs considered.

Point 7:

Reading the manuscript for the first time, I would have been less cautious if presented with the following simple simulation. Let us take a simple, single-component, Gaussian burst. For each frequency, let the width be of the order of the shortest burst detected for that source at that frequency. If the burst is incorrectly dedispersed to a number of realistic ΔDM values, how would it appear in Fig. 1? From the analysis presented in the supplementary material, I expect it would describe an almost vertical line, is this correct? I would be worried if instead, it followed a relation similar to the one presented in Fig. 1.

Response:

With the new analysis and the redone Figure 1 of the paper, the question of if incorrect de-dispersion leads to the predicted relationship can be answered by the capped lines indicating the range of measurements across their respective DM ranges. For all the data from FRB 121102, we can see that the temporal duration measurements are tightly constrained and only the drift measurements vary significantly, resulting in an almost vertical line as mentioned by the referee. The same is true for the data from FRB 180814.J0422+73, however the range of duration measurements is larger.

Only for the data from FRB180916.J0158+65 do we see significant variability in both measurements, but the trend traced by the measurements of each burst across the DM range do not line up with the predicted relationship we hope to demonstrate. Figure A in this response shows two diagnostic plots of the traces made by the burst measurements as their underlying burst DM is varied. Each point is a burst measurement with the symbol denoting the DM the measurement it was taken at. The lines connecting the points indicate that those points are from the same burst and therefore trace the changes in the burst measurements for different DM values. Overlayed are the fits to the model performed at different DMs. For comparison we show a plot for the subset of FRB 121102 bursts used from Gajjar et al. 2018 and the same plot for the data from FRB180916.J0158+65. The traces shown for the data from Gajjar et al. (2018) represents the ideal case where the DM primarily affects just the sub-burst drift rate measurement and the temporal duration remains largely unchanged, thus we see the traces are vertical. The traces for FRB180916.J0158+65 however can be seen to form a nasty web, indicating a complex dependence of the measurements on the DM that may be caused by the structure of the underlying bursts and/or the lower time resolution of its waterfall. Despite the complex traces, a majority of the points follow paths that are distinct from the path made by the model fits. Several of the bursts follow mostly vertical paths. Two bursts in particular certainly do follow paths that are mostly horizontal (i.e., constant drift but varying temporal duration), and could be confused with the model fits. These problematic points can be identified in plots like Figure 1 of the paper as the points whose capped lines show little vertical range but a broad horizontal range. Figure A indicates that the bursts with both a broad horizontal and vertical range have underlying measurements that follow paths that are distinct from the predicted relationship.

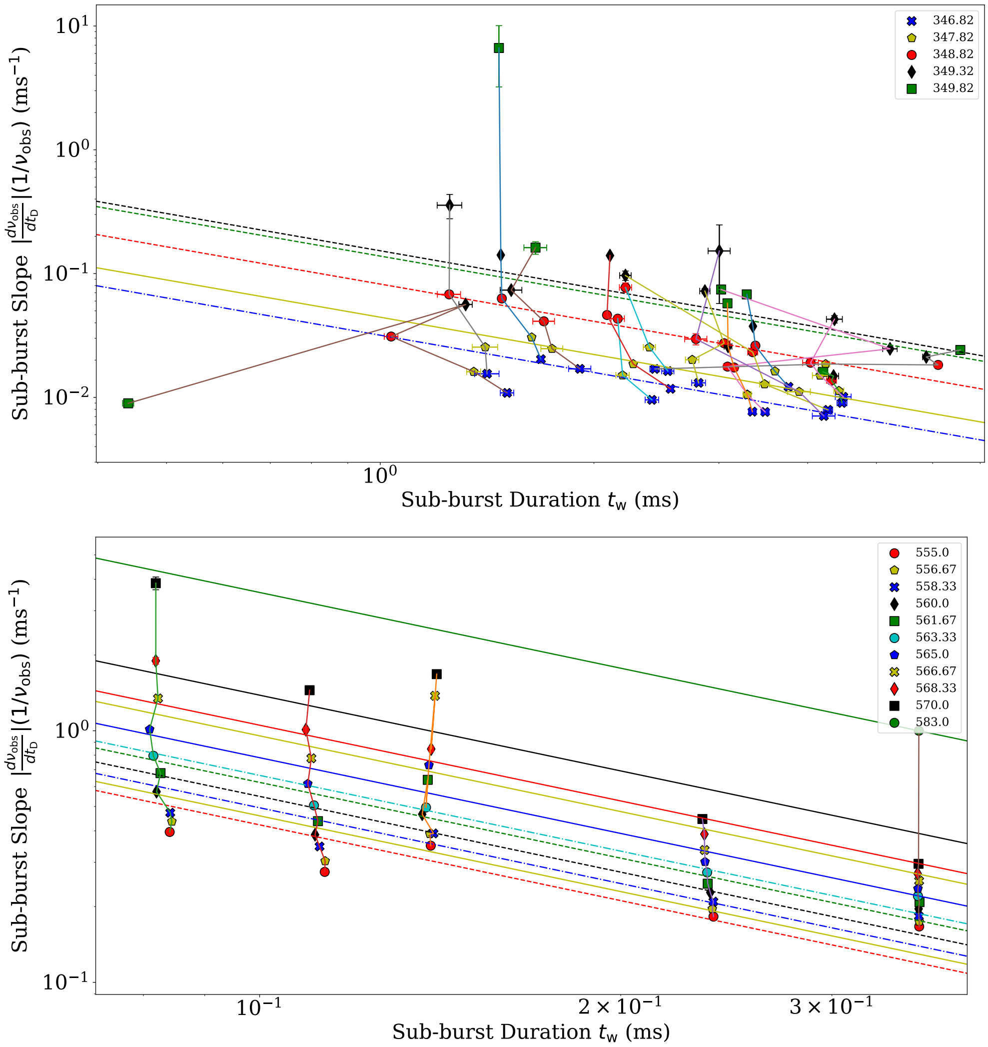

Figure A: Diagnostic figures for Point 7. Each panel shows the sub-burst slope and temporal duration measurements made for all the bursts at each of the trial DMs. The top panel shows the data from FRB 180916.J0158+65, and the bottom shows data of the sub-bursts used for FRB 121102 from Gajjar et al. (2018). The different symbols denote the DM that the measurement was made at. The jagged lines connecting the points group the measurements that come from the same sub-burst. Thus these lines trace the change in measurements for a sub-burst as its DM is changed. The inclined lines running from left to right are fits to the predicted relationship performed at each of the trial DMs. The sub-burst measurements from Gajjar et al. (2018) have mostly vertical traces, indicating the ideal scenario where a change in DM simply results in a change in the sub-burst slope measurement. The traces for the FRB180916 data are more complex, but trace paths that are distinct from the model fit, and many bursts trace roughly vertical paths. However, two sub-bursts from FRB 180916 can be seen to trace mostly horizontal paths and might be confused with the model fit.

A list of additional possible improvements follows.

Additional Improvements:

Point 8:

Title: it could be more explicit about what the shared law is

Response:

The title has been changed to 'Evidence of a shared spectro-temporal law between sources of repeating fast radio bursts'. This is to better capture the details and intent of the paper without being overly technical.

Point 9:

Why other samples for FRB 121102 are not considered? For example, from Hessels et al. 2019, already cited by the authors. It is OK if the authors decide not to include every single burst from the source (although that would be preferable) but it should be clearly stated why the authors choose to use only these particular samples.

Response:

We used all the samples from FRB 121102 that we were able to arrange access to, which did not include all the data that are available. Ideally we would like to use every burst from every source possible, but the fact of the matter is that the data availability of FRB data at this stage of the field is limited in several ways. While many authors have been extremely generous with their time in not only providing data to use but also supporting us with some data problems (such as acquiring a noise mask from the CHIME/FRB team in order to properly use the FRB180814 data), this 'email overhead' can add days if not weeks to the analysis, as a lot of different people need to be contacted for data from different sources or even for different bursts from the same source.

The ideal solution to this problem is for groups collecting FRB data to provide their data whenever possible in a format that is easily accessible online, publicly, and with example scripts and documentation to reduce the need for support. This is of course a challenge due to the size these datasets can grow to but also due to the fast and competitive pace of research in this field. However until asynchronous access (i.e., without the need for bothering someone) to these datasets is arranged, studies such as this one that require large samples across different telescopes and different sources will be hampered. The website https://chime-frb-open-data.github.io/ is significant progress in that direction, however a lot of data are still unavailable, and other FRB groups do not provide their data in a similar way, or with support. Open sourcing data is an intensive task and requires committed resources, but is worth it in terms of the benefits to the larger scientific community as well as the public.

The following sentence has been added to the end of the first paragraph of Section 2 ('Burst Analysis'): "These data sources were chosen purely due to their ease of accessibility and the support available. Ultimately we aim to extend this analysis to as many sources and bursts as possible."

Point 10:

In case the authors are not already aware, the detection of FRB 180916 down to 110 MHz recently reported could be useful for the present study, although no clear sub-bursts are observed. The authors may want to check the relative papers by Pleunis et al. 2020 and Pastor-Marazuela et al. 2020.

Response:

We thank the referee for the references. As we noted in our response to Point 9 above, it is our plan to include as much data from as many sources as possible in future analyses and papers.

Point 11:

Amiri et al. 2020 should be cited as CHIME/FRB Collaboration 2020.

Response:

We have updated the paper accordingly.

Point 12:

There are double parentheses and parenthesis inside other parentheses in multiple places. These should be avoided.

Response:

We have avoided all or almost all of these occurrences.

Point 13:

FRB 180916 has been firstly reported by CHIME/FRB Collaboration 2019 (ApJ 885; arxiv 1908.03507) and not by Chawla et al. 2020 as currently cited.

Response:

We have corrected this mis-citation.

Point 14:

Plot labels are sometimes too small. I believe the general rule is that they should be at least the font size of the caption.

Response:

We have increased the size of the legend text, which was the primary offender of this rule, and increased font size elsewhere where necessary.

Sec. 1

Point 15:

"the large number of proposed models". The authors could cite Platts et al. 2019 here.

Response:

We have updated the paper accordingly.

Point 16:

"a reduction in the temporal duration of individual sub-bursts" should be edited to "an average reduction in the temporal duration of individual sub-bursts"

Response:

We have updated the paper accordingly.

Point 17:

Kirsten et al. 2020 does not report the detection of a galactic FRB but of weaker bursts from the same source.

Response:

We have removed this reference from the relevant section.

Point 18:

A "happy trombone" effect, despite rare, is not unique to the galactic FRB, see for example burst 6 of Hilmarsson et al. 2020.

Response:

We have added this example to the Introduction as well as a reference to Day et al. (2020, MNRAS, 497, 3335) for FRB 190611 (page 2, first column).

Point 19:

How do the authors distinguish one FRB event formed by single components and two distinct FRB events close in time? Is it possible that the "happy trombone" is actually two distinct events observed at different frequencies? This caveat should be mentioned.

Response:

The following text was added at the end of the new Sec. 1.1:

"We also note that within the context of our triggered model, individual sub-bursts belonging to a single FRB event all results from the same background trigger signal. Their sequence of appearance in time, as seen by the observer, will vary depending on the physical properties of the medium where individual sub-bursts emanate from (which will affect the delay time in the corresponding rest frame τ'_{D}) and its velocity β (and therefore frequency ν_{obs} relative to the observer). Although we expect sub-bursts belonging to a single FRB event to be clustered in time, it is also possible that sub-bursts belonging to different events be observed relatively closely in time."

Point 20:

"dynamical spectrum (i.e., frequency vs. time)" I have a few issues with this definition. Firstly, it should be "dynamic spectrum". Secondly, "dynamic spectrum" is used in the literature to refer to the on-pulse region of a pulsar observation, something different from the current definition. I know that the term has been used in FRB studies as well but I argue that it is not correct and the more common "waterfall" should be used instead. Finally, the definition should be "(i.e. the signal intensity as a function of frequency and time)" or similar.

Response:

We have replaced occurences of "dynamical spectrum" with waterfall throughout the manuscript and have defined 'waterfall' in the Introduction, page 2, first column.

Point 21:

"We further provided" and "We then argued that" should be edited to "Rajabi et al. (2020) provided" and "Rajabi et al. (2020) argued that", respectively. I find it awkward to refer to another paper as "we" and this form is used in multiple places throughout the paper. I suggest modifying this using "Rajabi et al. (2020)" instead of "we".

Response:

We have updated the paper accordingly.

Point 22:

A quick summary of the basic idea of the authors' model and what the actual FRB source could be may be useful in the introduction.

Response:

We have added a new Sec. 1.1 ('The triggered dynamical model of Rajabi et al. 2020') that briefly summarize the model of Rajabi et al. (2020), which is central to our paper.

Point 23:

It is not clear to me what τ'_W represents exactly. The authors state it is "the corresponding sub-burst proper delay in the FRB reference frame". Delay of what?

Response:

As stated in that portion of the text, $\tau'_{\rm w}$ is the FRB-rest frame counterpart to the temporal duration $t_{\rm w}$ (not the delay) of the sub-burst measured in the observer's frame. As is also stated at the same location in the paper the delays $\tau'_{\rm D}$ (in the FRB frame) and $t_{\rm D}$ (in the observer's frame) are in relation to the arrival of the trigger signal in these reference frames.

Point 24:

The authors state that "This closeness between the values obtained for A is rather remarkable and points to the existence of a single and common underlying physical phenomenon responsible for the emission of FRB signals in the three sources." It would be interesting a discussion about the physical consequences of these similarities. What does the fact that the signal delay is always 12 times larger than the signal duration can teach us about the emission mechanism? The constancy of A among different sources is not a requirement for the model developed by the authors, do the authors thus have an explanation of why that is the case?

Response:

This is an interesting question, which is central to our continuing studies on FRBs using Dicke's superradiance. A longer delay relative to the signal duration is a feature that is inherent to superradiance, as it takes some time before coherence can be established in a physical systems. Although a factor of 12 or so is consistent with results typically obtained in superradiance experiments and simulations, there are factors that can affect this figure (e.g., the level of population inversion, time-scales of non-coherent processes that tend to inhibit superradiance, etc.) that have to do with the details of superradiance theory. This topic is outside the scope of the present paper, but is broadly discussed in our earlier papers listed in the references. However, we are currently working on a project aiming to investigate this aspect within the general setting of the relativistic model considered in this paper and Rajabi et al. (2020), and will be the subject of a future publication from our group.

Point 25:

The values obtained for β are larger than 0.9. Since they are limited by the frequency coverage, they are likely larger. Do these values represent an issue for the model? What kind of acceleration process can keep this kind of speed for multiple years (at least the lifespan observed for FRB 121102)? I would be curious about a discussion of this in the paper.

Response:

Although we focus on superradiance for our FRB model, the dynamical relativistic model discussed in this paper and in Rajabi et al. (2020) is general in scope and could, in principle, be applied more generally to other types of models. Although there are no issues for superradiance in principle (the realisation of superradiance in a given reference frame is independent of its speed relative to other frames), there are still many unanswered questions as to the specific details of environments that could provide the conditions necessary to realise the observed spectro-temporal characteristics of FRBs. It is, at this point, much too early to venture beyond the types of models we have published so far. Importantly, we feel that our present work on a shared spectro-temporal law for repeating FRBs can possibly help in refining (and discriminating between) models of different types.

Point 26:

The meaning of the sentence "This spectral extent is the result of motions within a given FRB rest frame from where a sub-burst centred at ν_{obs} originates." is unclear to me, I suggest rewording and clarify it.

Response:

Because of its significant width the spectral bandwidth of an individual FRB sub-burst signal due to a given physical process is likely caused by the velocity range covered by the radiators (whatever they are) through the Doppler effect.

We have augmented the sentence to "This spectral extent is the result of motions (through the Doppler effect) within a given FRB rest frame from where a sub-burst centred at ν_{obs} originates."

Point 27:

What is β' and how is it defined?

Response:

β' is not a quantity that enters in our analysis. The parameter $\Delta\beta'$ does, however, and is properly defined as the "velocity change in the FRB frame." The corresponding velocity change in the observer's frame is $\Delta\beta$; the two quantities are related through equation (B5) in the updated paper.

Point 28:

$\Delta\beta' \sim 0.08$ looks like a relatively narrow range to me, why do the authors say that it "covers a wide range of velocities"?

Response:

The figure $\Delta\beta' \sim 0.08$ is evaluated from observational data from FRB 121102 over a wide range of frequencies, and amounts to 8% of the speed of light. As this is the range of motions that is needed on average to cover (half of) the spectral extent of an individual sub-burst (e.g., a spectral width of 160 MHz at 1 GHz), it is in our opinion a substantial range. However, the statement "covers a wide range of velocities" does not pertain to $\Delta\beta'$ but to the range covered by β, which could be as high or higher than 0.9.

This is also related to the point that follows, which has to do with the definition for an FRB rest frame. As stated earlier, we have now added Sec. 1.1 in the Introduction that summarizes the relativistic model introduced by Rajabi et al. (2020). This should clear the issue. The text in the paragraph where this statement is found (after equation (6) in the updated paper) has been clarified by specifying what velocity ranges β and $\Delta\beta'$ correspond to (as explained above).

Point 29:

I have difficulties following the sentence "We thus have a picture where [...] widths of sub-bursts." For example, what do the authors mean by "FRB rest frames"? Is it the rest frame of the source on one FRB event (which can be formed by multiple sub-bursts)? I suggest clearing the paragraph better. Also, as suggested in the Introduction, more words could be spent to describe the model that is central to the paper.

Response:

As stated in our answer to the previous point, we have added Sec. 1.1 in the Introduction that summarizes the relativistic model introduced by Rajabi et al. (2020) to clear any confusion. To answer the question here, a FRB rest frame moving at velocity β relative to the observer is the origin of an individual FRB sub-burst. This FRB rest frame hosts a range of velocities $\Delta\beta'$ ($\Delta\beta$ in the observer's frame) about β that are responsible for the spectral extent of the sub-burst (e.g., about 160 MHz at 1 GHz). An FRB event can consist of a large number of sub-bursts, as in Burst 11 of Gajjar et al. (2018).

Supplementary Material Materials and methods

Point 30:

The analysis description could be more detailed. For example, it is not clear which structure-optimization code the authors use to determine the DMs, what is the downsampling factor used, how sub-bursts are divided, what SNRs are too low.

Response:

As discussed in Points 2 and 5, Section 2 ('Burst Analysis') has been added to explain how DMs are chosen, and how sub-bursts are divided. The downsampling factor is typically the same as that used in the data's source publication.

Point 31:

A better acronym for signal-to-noise ratio is S/N.

Response:

Although both acronyms can be found in the literature, we originally used "SNR" since it is the one favoured by the IEEE (see www.ewh.ieee.org/soc/ias/pub-dept/abbreviation.pdf). However, due to the possible confusion with "supernova remnant" in astrophysics we implemented the change suggested by the referee.

Point 32:

"multiple sub-bursts from short duration pulse trains should be expected to have a single canonical DM". This is not necessarily true, e.g. in case multiple sub-bursts are emitted in plasma at different heights or if they travel different paths from the source to the observer.

Response:

This phrase is no longer in the manuscript. As described in Point 5, we decided to dedisperse each sub-burst to several DMs and use multiple measurements to account for such variations. This includes bursts whose components were separated.

Point 33:

"we choose a single DM per source since multiple sub-bursts [...] obey the inverse relationship [...] and therefore supports the simplification of using a single DM for the analysis." This is circular reasoning that does not support the assumption. As an example, let us assume that the DM from a source is rapidly varying with time. Assuming a constant DM would result in a certain relation between width and slope of sub-bursts in the waterfalls but this would not demonstrate that the DM was indeed constant.

Response:

We agree with the referee that choosing a single DM is not an appropriate simplification for this analysis. The changes to our analysis are described in Points 5–7.

Point 34:

"the time-averaged frequency series" I believe the authors just mean spectrum? Also, it would be important to specify whether the spectrum is calculated only using the on-pulse region (which I strongly suggest) or the whole time range (i.e. including the off-pulse region).

Response:

We have replaced the phrase with just 'spectrum'. We use the whole time range, and this has little bearing on the result. A sentence has been added in the last paragraph of Appendix A.

Point 35:

"We used the scipy.odr.RealData package, which uses orthogonal distance regression and incorporates the uncertainties on the data to find a fit." It is not clear to me why it was chosen to use a complex algorithm to perform a weighted average. Is it to deal with missing frequency channels?

Response:

Orthogonal distance regression better incorporates the uncertainties on the data when searching for a fit by using the uncertainties on both axes of data, i.e., both the uncertainties of the sub-burst duration and sub-burst slope are used when searching for a fit. In practice, however, this algorithm had similar results compared to the standard `scipy.optimize.curve_fit`.

Point 36:

In Figure 1, it would be beneficial to report the same plots at ΔDM=0 pc/cc. The top row looks well de-dispersed to me and a comparison would be good to have.

Response:

This is in regards to what is now Figure 2 of the manuscript. For reference the bursts at $\Delta\mathrm{DM} = 0$ can be found in Figures 7–10 of the manuscript.

Point 37:

As discussed above, I think that constant variations in DM as presently discussed do not cover the full picture of possibilities. For example, choosing the best DM for every individual burst (which would also provide uncertainties for the DM that could be propagated and included in Fig. 1 of the main text) would be a way to perform the same analysis less prone to artifact derived from DM evolution.

Response:

Please see Points 5–7.

Point 38:

The analysis in the Supplementary Material mostly refers to the rotation angle of sub-bursts. However, it would be clearer to convert that to a sub-burst drift, which is the quantity central to the study.

Response:

Please see Point 4.

Error Introduced by Frequency Band Masking

Point 39:

I find "pixel" a strange naming, why not using channels and time bins? How is the choice of 25 channels justified? What is the error induced on the sub-burst drift, which is central to the paper?

Response:

We use 'pixel' for the sake of generality, but have added the equivalent values using a typical time and frequency resolution. The error induced on the sub-burst slope for the example used in that discussion has been added, and is of the order of 5%.

Point 40:

The narrow-band nature of the emission process

I suggest specifying that the temporal width here refers to that of sub-bursts (as opposed to Gajjar et al. 2018, where it refers to the width of the envelope).

Response:

This is something that we emphasized in Rajabi et al. (2020). We have added a similar footnote (the second) in the corresponding discussion in Sec. 1.1 in the new version of the paper.

Point 41:

"it further follows from equation (5) that the rest frame frequency ν_0 cannot change significantly as a function of ν_{obs}, as this would affect the inverse relationship observed in the data." The use of "significantly" here is ambiguous and should be replaced with a clearer term. Given the large scatter in the current data, ν_0 can change significantly but within certain (fair large I believe) limits that the authors could provide. The same applies to the rest of the section.

Response:

This is a good point raised by the referee. For the discussion referred to here (relating to equation (2) in the new version of the paper) we have removed the quoted sentence and modified the one preceding it to (now in Sec. 1.1):

"Although the burst temporal duration exhibits a fair amount of scattering at a given frequency (which could also be inherent to τ'_{w} in equation (2)), the predicted behaviour is consistent with the observations."

The referee is correct that the evidence gathered from Figure 7b) of Gajjar et al. (2018) does not significantly restrict ν_0. However, it is the combination of the corresponding three predictions stemming from the model of Rajabi et al. (2020) that is more constraining for ν_0. That is, combining the change in the temporal width t_{w} with the observed frequency ν_{obs} discussed above with the relative delay between sub-busts measured for the sad trombone effect as a function of ν_{obs} (asserted from Figure 3 of Hessels et al. 2019, and discussed following equation (3) in Sec. 1.1) and the results we present in Figure 5 in Sec. 3.3 (related to equation (8); updated version of the paper) that restricts the range of ν_0. While for the latter we are also limited to "the predicted behaviour is consistent with the observations," the sad trombone effect is significantly more constraining. That is, we can see from Figure 3 of Hessels et al. (2019) that even a change of, say, 50% would be significant.

We have therefore added the following sentence at the end of Sec. 1.1 (for the paragraph following equation (3)) when referring to Figure 3 of Hessels et al. (2019):

"For example, a change of 50% in ν_0 would markedly affect the appearance of the figure."

We also reworded the brief discussion in Sec. 3.3 to reflect these points and tone down our conclusion. Notably, when discussing Figure 5 we end this part of the text with:

"While there is some scatter in the data, the result is consistent with the expected lack of dependency on ν_{obs}. Any deviation could easily be accounted for with the uncertainty on the DMs and inherent variations in τ'_{D}. The combination of this result with the temporal narrowing and sad trombone effects discussed in Section 1.1 for FRB 121102 provides evidence for the narrow-band nature of the emission process."

Point 42:

The value of B looks by eye a bit low in Figure 4. Did the authors use some weighted mean? Also, its error looks pretty small, was it calculated from the spread of the data points?

Response:

Figure 5 (in the updated paper) has been reworked in view of our new analysis using the ranges of DMs for the central result presented in Figure 1. The fit value has been updated as well.

Point 43:

Why is this section in the Supplementary Material? It is an interesting result that should be included in the discussion, in my opinion. Also, it is not an analysis used for the conclusions in the main text, which is what I believe should go into the Supplementary Material.

Response:

While the detailed calculations leading to the equations for β^+, ν_0 and $\Delta\beta'$ can be found in Appendix B, we have moved the discussion and the figure (now Fig. 5 in the revised version) centred on the narrow-band nature of the emission process to the main text, in the new Sec. 3.3.

Point 44:

Determination of β^+, ν_0 and $\Delta\beta'$ The authors use a method to estimate $\Delta\beta'$ based on the trend between ν_{obs} and $\Delta\nu_{obs}$ observed by using multiple bursts. As an alternative method, is it possible to give some constraints from single FRBs? For example, Michilli et al. 2018 presented a burst with a duration of 30 us, which constraints the size of an emitting region to less than 10 km modulo relativistic effects, and a bandwidth larger than 800 MHz. What is the speed distribution necessary in this small space to give the observed bandwidth, under reasonable assumptions about the narrowness of ν_0? Or, on the other hand, is it possible to constrain the extent of ν_0 considering that particles are limited to the speed of light in the emitting region? Similarly, it should be possible to set constraints on the minimum size of the emitting region for the longest-duration sub-bursts (even though it can be possible to argue that maybe there are unresolved components in this case).

Response:

Depending on the model one uses it is certainly possible to give constraints on the size of the emitting medium for any FRB sub-burst, such as the one referred to by the referee. However, one should be very careful in not accounting for relativistic effects when attempting determine the size of the emitting region, as it can scale by as much as $\gamma^2 = 1/\left(1-\beta^2\right)$ in some cases (see the well-known Prob. 4.1 in Rybicki and Lightman, for example). As we show in our paper, it is likely that FRB 121102 is highly relativistic, as more data at higher (and lower) frequencies may reveal when they become available. I would therefore be very weary to assume that the size of emitting regions for FRBs could be as small as ~ 10 km, for example.

Furthermore, the argument at the root of such estimate for the size of a source does not apply to all kinds of physical processes and systems. For example, the time-scale setting the duration of pulses in superradiance is inversely proportional to the length of the emitting region (i.e., along the line of sight), not its cross-section on the sky. If, as is usually done, one sets the geometry of the superradiance system by assuming a Fresnel number of unity (this is a constraint for a coherent signal), then the linear size of the emitting region on the sky scales with the inverse of the square root of the pulse's duration. Clearly, this is a very different behaviour compared to the model underlying the estimate provided by the referee. Accordingly, it was shown in Houde, Rajabi, Gaensler et al. (2019, MNRAS, 482, 5492) that superradiance systems with cross-sections on the order of 1000 km could readily reproduce FRB signals with durations on the order of ms in their rest frames. Once one considers relativistic motions, beaming effects, etc., (as done in Rajabi et al. 2020) these time-scales would be transformed accordingly and could readily account for significantly shorter (or longer) bursts also observed (such as the one from Michilli et al. 2018 referred to by the referee); see equation (2) in the new version of the paper.

That being said, the method presented in the paper is general under the assumption of a narrow-band emission process and FRB rest frames moving along the line-of-sight. The speed distribution $\Delta\beta'$ we calculate within an FRB rest frame is the one needed to account for the signals observed. With these estimates in hand, then one could, in principle, use their model to work out the details of the emission source from the corresponding theory. But as discussed in the previous paragraph, the size of the region could be highly dependent on the model and physical process at play.

Point 45:

Do the authors have any theory about what the emitting regions could be with similar sizes and velocity distributions? Some sort of km-scale explosions?

Response:

Please see our response to the preceding point. The results obtained so far are consistent with superradiance but much work remains to be done.
