MNRAS: MN-20-5012-L

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Dear Prof. Houde,

Copied below are the comments on your manuscript entitled "A shared law between sources of repeating fast radio bursts", ref. MN-20-5012-L, which you submitted to Monthly Notices of the Royal Astronomical Society.

Major revision of your manuscript is requested before it is reconsidered for publication.

You should submit your revised version, together with your response to any comments from the editor and reviewer at https://mc.manuscriptcentral.com/mnras. The deadline for this is two months from today.

Enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions". Under "Actions," click on "Create a Revision". Please ensure that you also respond to any comments from the editor or assistant editor.

IMPORTANT: do not submit your revised manuscript as a new paper!

When submitting your revised manuscript, you should provide details of any changes you make to the original manuscript. Changes to the manuscript should also be highlighted (e.g. in bold or colour), to assist the referee and editor.

We look forward to receiving your revised manuscript.

Regards,

Claire

Claire Williams
Assistant Editor (MNRAS)
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cc: all listed co-authors.

Assistant Editor's Comments:

If this is is to remain as a Letter then the supplementary material format will need changing - it doesn't need the author list, institutions, contact details etc and the figures will need to be renumbered so they are unique from those in the paper. An online appendix is usually the best way of doing this. Please see https://academic.oup.com/mnras/pages/general_instructions#2.7 Catalogues and online-only material for more information on online material.

Scientific Editor's Comments:

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.

Reviewer's Comments:

Reviewer: Michilli, Daniele

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.
- 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.

- 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}.
- 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.

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.
- 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?
- 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.
- 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.
- 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.

A list of additional possible improvements follows.

- Title: it could be more explicit about what the shared law is
- 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.
- 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.
- Amiri et al. 2020 should be cited as CHIME/FRB Collaboration 2020.
- There are double parentheses and parenthesis inside other parentheses in multiple places. These should be avoided.
- 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.
- Plot labels are sometimes too small. I believe the general rule is that they should be at least the font size of the caption.

Sec. 1
- "the large number of proposed models". The authors could cite Platts et al. 2019 here.
- "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"
- Kirsten et al. 2020 does not report the detection of a galactic FRB but of weaker bursts from the same source.
- A "happy trombone" effect, despite rare, is not unique to the galactic FRB, see for example burst 6 of Hilmarsson et al. 2020.
- 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.
- "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.
- "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".
- 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.

Sec. 2
- 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?
- 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?
- 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.
- 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.
- What is β' and how is it defined?
- $\Delta\beta' \sim 0.08$ looks like a relatively narrow range to me, why do the authors say that it "covers a wide range of velocities"?
- 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.

Supplementary Material

Materials and methods
- 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.
- A better acronym for signal-to-noise ratio is S/N.
- "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.
- "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.

- "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).
- "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?
- 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.
- 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.
- 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.

Error Introduced by Frequency Band Masking
- 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?

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).
- "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.
- 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?
- 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.

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).
- 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?