
Supplementary information

**A bright millisecond-duration radio burst
from a Galactic magnetar**

In the format provided by the
authors and unedited

Peer Review File**Manuscript Title: A bright millisecond-duration radio burst from a Galactic magnetar****Editorial Notes:****Reviewer Comments & Author Rebuttals****Reviewer Reports on the Initial Version:****Referees' comments:****Referee #1 (Remarks to the Author):**

Referee: Evan Keane

First of all congratulations on the very nice work described in the paper. Overall I think this paper is very well presented and it reports an important discovery, a radio burst from a well-known magnetar that is very bright and so is plausibly a Galactic FRB. In combination with the other detections in X-ray and radio the picture becomes clear. It tells us that at least some FRBs can be explained from such flaring magnetars, long thought to be an explanation but seeing is believing. As such it is an important result.

The paper is well presented and most of my comments are clarifying details or highlighting slight inconsistencies (e.g. between abstract and main text). I was wondering if the actual beam model used could be made available as I think this is an important ingredient in understanding how you made your scientific interpretations? To me this seems crucial. I like the Methods section; the Crab work is very

interesting. And it is great that you have these outriggers to do such measurements and calibrations. Despite this I still include comments that might make the main text more clear in a standalone sense, as some readers may not read the Methods alongside the main paper

--Abstract--

A few minor things on the abstract:

I don't think reference 4 is the best one to illustrate that FRBs are from cosmological distances - maybe something where very high DM ones have been found and/or the recent paper from Macquart et al. on the DM-z relation?

I don't think you should omit a citation to this paper:

<https://ui.adsabs.harvard.edu/abs/2019ApJ...887L..30K/abstract> in relation to repeating FRBs.

When you say that such a burst in a nearby galaxy would be indistinguishable from a typical FRB this is true but you need to say "up to redshift Z" and give whatever Z is, explicitly.

The next sentence "bridges a large fraction of the radio energy gap" is vague - give numbers. How many of the FRBs known (and give that number) could be explained by this SGR being scaled to their distance. Even if it is only 1 it is still something. But you are alluding a bit too tangentially to it being more - just state the number. You specifically say the energy released in radio for this SGR is 3×10^{34} erg. You also say this is 1000x more than any previous Galactic magnetar radio activity. You should also then say, explicitly, what the (average or median or max or whatever - up to you) energy of an FRB is, here.

I think that magnetars are "at least some FRBs" is a true statement but does this SGR strongly support the notion that all FRBs could be magnetars? Is that what you are saying? _Just based on this abstract text_ I am not sure so I suppose a bit of re-phrasing might help so as not to lead to mis-interpretation of what you mean.

--Main Text--

You should explicitly say the fitted scattering tails are the same for both components if they are.

Given the amount of instrumental effects this far off axis, and the S/N levels of the bursts, it seems remarkable upon reading it that you have the scattering pinned down so well. Figure 1 doesn't really make it clearer as to how you might have done that. I am wondering if there is some way you could combine the current Figure 1 and Extended Data Figure 1, on page 43, somehow? I think that is a crucial thing for understanding/'seeing' what you've fit.

By the way, what is the S/N of each component in the brightest beam detection? And indeed in all the other beams where it was detected. I could not find that anywhere, nor in your ATel. I think the S/N of the event in the CHIME data (and ARO of course) should be reported. Table 1 would be a good place for this as well as in the main text and/or Figure 1's caption. If it is there somewhere, and I've just missed it, then please read this as a request to make it more prominent.

Related - I suppose the relevant second or so of data from all 1024 beams will be made available upon request to interested readers? If not I would suggest that is very important to do.

Is there no CHIME publication for this comb-like effect that you could reference? I think you should, even if it's a technical memo from the observatory or something like that. It is a critical piece of the puzzle for understanding how you went from your observed signal to your scientific interpretation.

When you mention that you pinpoint the bursts to 0.3deg of the SGR and it is this in combination with the known X-ray flaring that identifies the source, that is not the full picture. Surely it is these in combination with the STARE2 detection, which was also able to pinpoint its detection, also had the exact same DM, and a pulse that lined up with your 400-800 MHz arrival time - right? FAST was also actually pointed at the SGR when it saw its very weak pulse with the exact same DM. These should be mentioned here also.

Again the DM fit precision suggests a S/N value but can you state it explicitly?

The STARE2 number is now not just " $> 1.5 \text{ MJy*ms}$ " so it is a very interesting point where you identify which of your 2 components it is most likely to be based on the spectrum in the CHIME band. Are your arrival times in the CHIME band, especially given the precision in your DM measurement, sufficient to predict the arrival time of each component in the STARE2 band at the level where you can distinguish based on timing alone which one is which, and indeed rule in/out that there was a third burst? It seems like you should be able to do this. That seems like a good thing to work out to strengthen what you have based on the spectra. Actually, in this regard is there anything to be said in terms of the 'DM constant' and the differing values of this in use - does that limit you in the above? I am alluding to this paper: <https://arxiv.org/abs/2007.02886>

These MJy*kpc^2 and radio band energy numbers could do with some context or comparison - what is the next brightest thing in the radio transient sky? What is the Crab? Any kind of comparison - I think these numbers, in these units, will not mean a lot to most readers so a citation at least (maybe as well) would help.

The radio:gamma ratio being 10^5 x higher than SGR 1806-20 is a nice illustration of how wide the magnetar range is for this! How much wider would it need to be to explain an FRB at $z=0.1, 0.5, 1, 2, 3$ etc.? Might be a good spot to mention this (after line 123).

When you mention the other previous X-ray outbursts, how they were 14x and 11x times weaker in X-ray and what a 3-sigma limit implies for the relative radio to gamma-ray ratio, I find it a bit cryptic. I am not sure I can reverse engineer all those things to get absolute numbers and be confident that what I've done is correct. I would like if you explicitly said what your sensitivity limit was in janskies (to say a 1-ms pulse) for where you were pointed at the time, so what 10-sigma corresponded to each time.

I find the text (lines 155-168) to be much clearer than the earlier text I commented upon re: "narrowing the energy" or luminosity gap.

You mention FRB180110 as the most luminous FRB. A small thing is whether you should not switch between luminous and energetic? Maybe not so important when everything is $\sim 1\text{ms}$ but still. And of course there are much shorter-in-time FRB signals known. But more seriously you and the FRB Catalogue seem to work out energy differently. That's fine but I mean even systematically it is not like your numbers and its are scaled. I know it is all very ballpark estimate kind of stuff and the purpley-orangey haze in Figure 2 covers the right range I'm sure. I just think you should be certain you have the very brightest one if you are mentioning one in particular. And if you are doing something different and more

correct than FRBCAT you could elaborate in the Methods as I'm sure that would be of interest to many people.

But anyways, 10^9 times more energetic than this SGR - yikes! I think earlier statements should be modified to be consistent with this. Are you basically saying it would be incredible if magnetars could be that bright (and/or it would be very rare) but this SGR event being reported was incredible + some physical models say it's possible, so maybe? Is that the angle?

On page 11, everything that you are saying is common in FRB pulse morphology and spectra is of course also common in pulsars, i.e. extreme spectral variation pulse to pulse etc. "or be found in the outskirts" seems like you are missing a word or two around here?

I think your conclusion at the end of the main text is correct and well put. I would just modify the conclusion remarks in the abstract to be consistent with that.

--Methods--

I think again there needs to be some detail of the model used. "We fit a model" with no details and no citation to somewhere the details can be found is a bit of a hole. This could probably be easily be remedied by just referencing some observatory report/document. Or is your subsequent (very nice) discussion of the Crab, and the solar work, basically what this is built upon?

"which is subdominant" --> "which are subdominant"

"to assess telescope surface figure" --> missing a word or two here?

Referee #2 (Remarks to the Author):

A bright millisecond-duration radio burst from a Galactic Magnetar

This work is a report of a novel result from the CHIME/FRB project. The work describes the detection of an extremely intense radio burst from SGR J1935 +2154, a magnetar in our own galaxy and explains why this is likely a Fast Radio Burst. The work also provides compelling reasoning for how this bridges the gap in observations of radio bursts from galactic magnetars to date with observations of extragalactic FRBs and provides a compelling case for magnetars as the likely origin of some FRBs.

There is clear novelty in this work, aside from describing an original observational result, this work provides a missing piece in the broader puzzle of the origin of FRBs. Aside from its originality, the work is written in an extremely clear way, with succinct explanations of the results and addressing the broader astronomy community as this is a result of interest beyond the FRB community. The work also presents a number of intriguing puzzles for the community going forward.

There is a clear link with the STARE2 results. STARE2 appear to have observed a higher fluence measurement and an explanation for this difference is not provided. An explanation for this, be it an instrumental issue or timing, would be important to provide.

The authors assume a fiducial distance of 10 kpc as being within the distance range of 6.6 – 12.5 kpc range estimated. Given this assumption does not appear to be motivated, how robust is the rest of this work to that assumption? i.e. do any conclusions substantively change significantly at either end of the distance range chosen? If they do not, then it is worth being clear that this is the case or if there is some range in outcomes, perhaps an analysis presenting that range is worth providing.

There are a number of uncertainties in the data, in large part it seems due to data from the side lobes. However, this is well handled by the team and does not impact the overall result which is consistent with that observed by other radio telescopes and the observations in the X-Ray – and the authors have explained each uncertainty and how they have treated them so far. It is very clear that more data will be useful but this result stands on its own.

I will comment that one puzzle posed by this observation, the upward march in frequency of the two components is intriguing and deserving of further study, especially with more data of other FRBs with a similar pattern.

Overall, this is an extremely exciting result, and a beautiful piece of work to extract the signal from data from the side lobes of the detector. It is an excellent, well written article, my congratulations to the authors.

A tiny point – would be to standardise the way that SGR is labelled. Either with a J or without in a uniform way across the articles to be published on this work.

Author Rebuttals to Initial Comments:

We thank the referees for their constructive comments. Below we reproduce the referees' report with our responses interspersed in italics.

Note that the line numbers in the referees' comments refer to the PDF uploaded with our initial submission and the line numbers in our response refer to the PDF uploaded with our resubmission and they are different.

Referee #1 (Remarks to the Author):

--Abstract--

A few minor things on the abstract:

I don't think reference 4 is the best one to illustrate that FRBs are from cosmological distances - maybe something where very high DM ones have been found and/or the recent paper from Macquart et al. on the DM-z relation?

The reference is not to illustrate that FRBs are from cosmological distances; it is a review article on FRBs. It is appropriate since it is cited at the end of the sentence that introduces FRBs, so we have left it as is.

I don't think you should omit a citation to this paper:

<https://ui.adsabs.harvard.edu/abs/2019ApJ...887L..30K/abstract> in relation to repeating FRBs.

The reference has been added.

When you say that such a burst in a nearby galaxy would be indistinguishable from a typical FRB this is true but you need to say "up to redshift Z" and give whatever Z is, explicitly.

We have added that the burst would be detectable at a distance up to 12 Mpc.

The next sentence "bridges a large fraction of the radio energy gap" is vague - give numbers. How many of the FRBs known (and give that number) could be explained by this SGR being scaled to their distance. Even if it is only 1 it is still something. But you are alluding a bit too tangentially to it being more - just state the number. You specifically say the energy released in radio for this SGR is 3×10^{34} erg. You also say this is 1000x more than any previous Galactic magnetar radio activity. You should also then say, explicitly, what the (average or median or max or whatever - up to you) energy of an FRB is, here.

As suggested by the referee in later comments, we have reworded the last sentence of the abstract to summarize our conclusions from the last paragraph of the main text.

I think that magnetars are "at least some FRBs" is a true statement but does this SGR strongly support the notion that all FRBs could be magnetars? Is that what you are saying? _Just based on this abstract text_ I am not sure so I suppose a bit of re-phrasing might help so as not to lead to mis-interpretation of what you mean.

The text in question has been replaced as per the above comment.

--Main Text--

You should explicitly say the fitted scattering tails are the same for both components if they are.

We now state "with a single scattering function fit to both burst components" in the main text.

Given the amount of instrumental effects this far off axis, and the S/N levels of the bursts, it seems remarkable upon reading it that you have the scattering pinned down so well. Figure 1 doesn't really make it clearer as to how you might have done that. I am wondering if there is some way you could combine the current Figure 1 and Extended Data Figure 1, on page 43, somehow? I think that is a crucial thing for understanding/'seeing' what you've fit.

The Nature guidelines on Figures request that "only the data most central to the conclusions are presented in the main figures" and "Figures should be as small and simple as is compatible with clarity". Since the details of the scattering fits to the data are not central to the conclusions we feel that it is appropriate that they are included in the Extended Data Figure 1 where interested readers can refer.

By the way, what is the S/N of each component in the brightest beam detection? And indeed in all the other beams where it was detected. I could not find that anywhere, nor in your ATel. I

think the S/N of the event in the CHIME data (and ARO of course) should be reported. Table 1 would be a good place for this as well as in the main text and/or Figure 1's caption. If it is there somewhere, and I've just missed it, then please read this as a request to make it more prominent.

We now report the S/N of the brightest beam detection for the burst derived from our burst fitting. It is provided in Table 1 and a detailed explanation on how it is measured has been added to the Methods.

Related - I suppose the relevant second or so of data from all 1024 beams will be made available upon request to interested readers? If not I would suggest that is very important to do.

We will be providing the data for all the beams that we have data for in a doi linked repository.

Is there no CHIME publication for this comb-like effect that you could reference? I think you should, even if it's a technical memo from the observatory or something like that. It is a critical piece of the puzzle for understanding how you went from your observed signal to your scientific interpretation.

Unfortunately there is no reference that we are aware of specifically for the comb-like structure. However, we now provide in the Methods, in response to one of the referee's later comments, references to the papers Ng et al. 2017 and Masui et al. 2019 which contain the formulae used to describe the CHIME/FRB synthesized beams. From these formulae, the comb-like frequency response of a sidelobe can be derived.

When you mention that you pinpoint the bursts to 0.3deg of the SGR and it is this in combination with the known X-ray flaring that identifies the source, that is not the full picture. Surely it is these in combination with the STARE2 detection, which was also able to pinpoint its detection, also had the exact same DM, and a pulse that lined up with your 400-800 MHz arrival time - right? FAST was also actually pointed at the SGR when it saw its very weak pulse with the exact same DM. These should be mentioned here also.

We were confident in the association before we were aware of the STARE2 and FAST detections. Indeed, we asserted the association in our ATel before we were aware of the other detections. So, in no way did STARE2 or FAST influence the association that we assert in our ATel and thus paper. The converse is not true, i.e. the STARE2 and FAST teams were fully aware of our discovery when they discovered the bursts in their data.

Again the DM fit precision suggests a S/N value but can you state it explicitly?

We now report the S/N of the brightest beam in Table 1.

The STARE2 number is now not just " $> 1.5 \text{ MJy*ms}$ " so it is a very interesting point where you identify which of your 2 components it is most likely to be based on the spectrum in the CHIME band. Are your arrival times in the CHIME band, especially given the precision in your DM measurement, sufficient to predict the arrival time of each component in the STARE2 band at the level where you can distinguish based on timing alone which one is which, and indeed rule in/out that there was a third burst? It seems like you should be able to do this. That seems like a good thing to work out to strengthen what you have based on the spectra. Actually, in this regard is there anything to be said in terms of the 'DM constant' and the differing values of this in use - does that limit you in the above? I am alluding to this paper:

<https://arxiv.org/abs/2007.02886>

When we wrote our manuscript the STARE2 preprint was not available. The STARE2 numbers are more precise in their preprint, but we feel that revising our draft based on the pre-publication numbers in their arxiv preprint may lead to incorrect values cited in our paper: they are also currently revising their draft and we can't be confident that its final form and the numbers therein won't change. So we feel it is appropriate to use the preliminary number that's in the STARE2 ATel.

These MJy*kpc^2 and radio band energy numbers could do with some context or comparison - what is the next brightest thing in the radio transient sky? What is the Crab? Any kind of comparison - I think these numbers, in these units, will not mean a lot to most readers so a citation at least (maybe as well) would help.

Figure 2 provides a comparison for the fluences and energetics of Galactic sources with those from the SGR 1935+2154 event. To make the comparison more explicit when it comes to Galactic sources, we have added a sentence highlighting the fluence and energy of the brightest Crab giant pulses to the text.

The radio:gamma ratio being 10^5 x higher than SGR 1806-20 is a nice illustration of how wide the magnetar range is for this! How much wider would it need to be to explain an FRB at $z=0.1$, 0.5, 1, 2, 3 etc.? Might be a good spot to mention this (after line 123).

We are unsure how to interpret what is needed to "explain and FRB at $z=0.1$..." but we do two things to address what we believe to be the spirit of this comment:

- 1) At line 124, we cite constraints on the radio:gamma-ray ratio on extragalactic FRBs which are most constraining for repeating FRB 121102. This constraint is a lower limit as it is based on the non-detection of gamma-ray emission rather than radio.*

2) *At line 169, we now present the energy of the corresponding radio counterpart to the 2004 SGR 1806-20 giant flare (the most luminous gamma-ray emission from a Galactic magnetar) assuming the same radio:gamma-ray efficiency as measured for the 2020 April 28 SGR 1935+2154 event. This shows that extragalactic FRBs could be explained with a more energetic gamma-ray counterpart if we assume the same radio:gamma-ray efficiency.*

When you mention the other previous X-ray outbursts, how they were 14x and 11x times weaker in X-ray and what a 3-sigma limit implies for the relative radio to gamma-ray ratio, I find it a bit cryptic. I am not sure I can reverse engineer all those things to get absolute numbers and be confident that what I've done is correct. I would like if you explicitly said what your sensitivity limit was in janskies (to say a 1-ms pulse) for where you were pointed at the time, so what 10-sigma corresponded to each time.

In the main text, we state what our 3-sigma flux limit is at the time of those bursts given the source's location in the CHIME beams for our detection threshold of 10. This is our best estimate of the sensitivity to a conservative 99.7% (3-sigma) confidence level based on analogous FRB detections. Our conversion from S/N to flux currently depends on comparing the spectra of calibrator sources to detected FRB spectra and this is likely what the referee finds cryptic. Due to uncertainties in the beam response and the RFI environment, we unfortunately don't have a simple estimate of the sensitivity for a given sky position assuming a theoretical

1-ms pulse.

I find the text (lines 155-168) to be much clearer than the earlier text I commented upon re: "narrowing the energy" or luminosity gap.

We have rewritten the abstract sentence in question based on the referee's later comment on our concluding paragraph.

You mention FRB180110 as the most luminous FRB. A small thing is whether you should not switch between luminous and energetic? Maybe not so important when everything is ~1ms but still. And of course there are much shorter-in-time FRB signals known. But more seriously you and the FRB Catalogue seem to work out energy differently. That's fine but I mean even systematically it is not like your numbers and its are scaled. I know it is all very ballpark estimate kind of stuff and the purpley-orangey haze in Figure 2 covers the right range I'm sure. I just think you should be certain you have the very brightest one if you are mentioning one in particular. And if you are doing something different and more correct than FRBCAT you could elaborate in the Methods as I'm sure that would be of interest to many people.

Since we are talking about the fluence and distance the value we are comparing is energy (i.e. the diagonal lines on Figure 2), so we have changed “luminous” to “energetic”. We don’t mean to assert that FRB 180110 is absolutely the most energetic detected FRB, that depends heavily on the poorly constrained distances to the sources. But, it is certainly one of the most energetic and in Figure 2 it is the one that reaches to the highest energy. We’ve changed the text to say “For example” and “one of” to be clear that we are using it as an example of an energetic FRB. We disagree that we absolutely need to be certain we have to have the brightest in order to mention one as an example.

But anyways, 10^9 times more energetic than this SGR - yikes! I think earlier statements should be modified to be consistent with this. Are you basically saying it would be incredible if magnetars could be that bright (and/or it would be very rare) but this SGR event being reported was incredible + some physical models say it's possible, so maybe? Is that the angle?

To help clarify this and demonstrate that high-energy magnetar activity does span many orders of magnitude already, we now present at line 169 the energy of a radio counterpart to the 2004 SGR 1806-20 giant flare (the most luminous gamma-ray emission from a Galactic magnetar) assuming the same radio:gamma-ray efficiency as measured for the 2020 April 28 SGR 1935+2154 event.

On page 11, everything that you are saying is common in FRB pulse morphology and spectra is of course also common in pulsars, i.e. extreme spectral variation pulse to pulse etc.

We are not aware of such extreme pulse to pulse variations in the spectra of pulsar single pulses in the literature. Since the referee did not provide any examples, we are unable to address this assertion.

"or be found in the outskirts" seems like you are missing a word or two around here?

We’ve changed it to “or to be found in the outskirts” which reads a bit better.

I think your conclusion at the end of the main text is correct and well put. I would just modify the conclusion remarks in the abstract to be consistent with that.

We have reworded the abstract sentence to be a summary of this paragraph as suggested.

--Methods--

I think again there needs to be some detail of the model used. "We fit a model" with no details and no citation to somewhere the details can be found is a bit of a hole. This could probably be

easily be remedied by just referencing some observatory report/document. Or is your subsequent (very nice) discussion of the Crab, and the solar work, basically what this is built upon?

We now cite Ng et al. 2017 and Masui et al. 2019 which contain the formulae used to describe the CHIME/FRB synthesized beams.

"which is subdominant" --> "which are subdominant"

Done.

"to assess telescope surface figure" --> missing a word or two here?

We have removed the word "figure" to address this.

Referee #2 (Remarks to the Author):

There is a clear link with the STARE2 results. STARE2 appear to have observed a higher fluence measurement and an explanation for this difference is not provided. An explanation for this, be it an instrumental issue or timing, would be important to provide.

STARE2 is observing in a different frequency band than CHIME (1.4 GHz vs 400-800MHz) so a different fluence is not a discrepancy: the burst is just brighter at those frequencies. In the manuscript we say in regards to the STARE2 fluence: "Our observed spectrum of the second component rises steeply towards the top of the 400--800\,MHz band and so a higher fluence at 1.4\,GHz would be consistent"

The authors assume a fiducial distance of 10 kpc as being within the distance range of 6.6 – 12.5 kpc range estimated. Given this assumption does not appear to be motivated, how robust is the rest of this work to that assumption? i.e. do any conclusions substantively change significantly at either end of the distance range chosen? If they do not, then it is worth being clear that this is the case or if there is some range in outcomes, perhaps an analysis presenting that range is worth providing.

The results that are derived from the assumed distances (e.g. energies) have differences of several orders of magnitude from the values to which they are compared. Therefore, a difference in the distance of 30% would not change the conclusions that are derived from those comparisons.

There are a number of uncertainties in the data, in large part it seems due to data from the side

lobes. However, this is well handled by the team and does not impact the overall result which is consistent with that observed by other radio telescopes and the observations in the X-Ray – and the authors have explained each uncertainty and how they have treated them so far. It is very clear that more data will be useful but this result stands on its own.

I will comment that one puzzle posed by this observation, the upward march in frequency of the two components is intriguing and deserving of further study, especially with more data of other FRBs with a similar pattern.

We certainly agree. But, it is out of the scope of this work.

Overall, this is an extremely exciting result, and a beautiful piece of work to extract the signal from data from the side lobes of the detector. It is an excellent, well written article, my congratulations to the authors.

A tiny point – would be to standardise the way that SGR is labelled. Either with a J or without in a uniform way across the articles to be published on this work.

The standard in the field is to not include the J when using the SGR prefix as we have done in our paper. We'll relay the referee's concern in regards to all articles using the same standard to the Nature editorial staff in our resubmission cover letter.

We have also made the following changes in addition to those suggested by the referees:

- *We have corrected the geocentric arrival time as it was incorrect by ~2 ms due to a bug in our conversion code.*
- *In the Figure 1 caption we have provided the reference frequency for the arrival time to a greater precision.*