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Fw: MNRAS: MN-13-1261-MJ

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Dear All,

Please find below the referee report on our paper submitted to MNRAS.

with regards
Neelam

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Dear Ms panwar

I attach the reviewer's comments on your manuscript entitled "Young Stellar Population of Bright-Rimmed Clouds BRC 5, BRC 7 and BRC 39", ref. MN-13-1261-MJ, which you submitted to Monthly Notices of the Royal Astronomical Society.

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

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I look forward to receiving your revised manuscript.

Regards,

Claire

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Assistant Editor's Comments:

The editor handling your paper is on vacation but in the meantime the report is being made available to you.

Reviewer's Comments:

The authors present new optical observations of stars in and around 3 bright-rimmed clouds. Including archival IR data, they study the ages and properties of the in- and out-cloud population, exploring the mechanisms of triggered star formation. While this study is clearly interesting, the paper is not clear in several aspects that should be addressed before it can be accepted for publication. My main comments and suggestions are listed below.

1. Clarity.
The main goals of the paper are never clearly stated, but somehow listed separately (and in some cases, repeated) along several sections of the paper. From reading the paper, these would be:
1) Study of the spatial distribution of YSO on small scales.
2) Study of the age (and age-related properties) variations between in- and out-cloud objects.
3) Conclusions on star-forming/triggered mechanisms.
Maybe citing them clearly in the introduction and following them in the discussion would make the paper more clear and concise.

For instance, the cloud structure is described first in section 4, then again in 7.1, and then in 7.2 we learn that one of the regions had been ruled out for triggered SF. Why not to move it all to 7.1? Section 7.1. also has some information that should rather be with the description of observations, like the kind of emission that dominates in the four WISE bands.

Another point that is not clear is how the already available information is presented. In section 4, all the regions are described in a similar way. But then in 7.2 and again in 7.4 they talk about Morgan et al. (2009) having ruled out triggered star formation in BRC 39, even though the sections are not clearly written and I am not sure whether the present results confirm or reject this hypothesis. To follow a logical line, the known information should be clearly presented when the regions are described, including any suggestions that the regions may or may not be triggered. Then, in the discussion, the authors could explain whether their new results are in agreement with the previous ones, or not.

The paper contains much repetition and sentences that do not really convey any useful information. One example is the sentence in the abstract "The spatial distributions of YSOs in the studied BRC regions show aligned distributions". Another unclear sentence is "Although infrared wavelengths can penetrate dense medium and are particularly useful to unravel the sources hidden in the silhouette of dust layers, these wavelengths also become transparent to the background sources, especially for...". What is the silhouette of dust layers? How can a wavelength become transparent? Do the authors just mean that extinction is lower in the IR than in the optical so you can see objects inside the cloud and in the background?
There are more examples throughout the text, I would encourage the authors to correct them, shortening the text.

2. Mechanisms of triggered star formation.
One of the main arguments of the paper is that the observed irregular distribution of stars favors the RDI mechanism, rather than the C&C one. Nevertheless, the authors never really discuss the spatial distribution of the observed BRC in detail. How do they quantify the anisotropy of the YSO distribution? How are initial inhomogeneities in the cloud (filamentary structure, density variations) reflected in the resulting YSO in both scenarios? How are projection effects dealt with? How "regular" would be the intervals between YSO resulting from C&C, considering that the initial clouds do not need to be homogeneous?

It is true that most optical, IR, and X ray studies favor RDI (e.g. Sugitani et al. 1991,1995; Ogura et al. 2007; Koenig et al. 2008; Getman et al. 2007, 2009), but it is not clear how the present study can help to distinguish between RDI and C&C. If the text will accept RDI from the beginning, then the discussion about C&C may be shortened.

The authors also mention similar studies of other BRC done by the same group (Paper I, Paper II). In fact, two of the studied BRC are associated to IC1805, which had been studied in relation to the larger W3 complex by Oey et al 2005. It would be very interesting to put the results from this work in a broader context by comparing to observations of general triggered star formation in the region.

Similarly, BRC39 is related to cluster Tr37. There are previous studies of triggered star formation in other cloudlets in the region, in particular, the study of Getman et al. 2012 related to the IC1396A globule, and also previous suggestions of an age gradient suggestive of triggered star formation throughout the region (Sicilia-Aguilar et al. 2005, Barentsen et al. 2005). It would be also very interesting to put the new results on BRC39 in a broader context, especially since the authors seem to rule out triggered SF in this region later on.

The studies listed above targeted different BRC that would have been triggered by the same population of massive stars. It may be thus very interesting to compare the results here to results in previously studied BRC. This is especially relevant in section 7.5, where they aim to

complete the picture of global triggered star formation. Are the previous and the new results consistent? Could one trace a propagating bubble of triggered star formation all around the massive stars? Oey et al. 2005 already talked about large-structure SF in the IC 1805 region, and Patel et al. 1995, 1998 also discussed it for the extended IC1396 and CepOB2 region. It would be very interesting to discuss whether the results here confirm or reject the previous picture of global SF in these regions.

In order to put these results in a broader context, it may be also interesting to include a table with information like number of objects studied, distance to the O stars in the regions, relative sizes of the triggering/triggered regions, ages of in-cloud vs off-cloud objects.

In 7.4, the authors check the possibility of triggered star formation.

BRC39 is ruled out immediately without any further discussion just based on previous results by other groups (Morgan et al. 2009). If this region was ruled out from the beginning, I don't understand why the authors included any discussion on the possibilities of triggered star formation up to section 7.2, where it comes as a surprise. Then, in 7.5, the authors discuss again the results on BRC 39, and just from the discussion it does not seem substantially different from the other two, but the final conclusion is that "BRC 39 is excluded from the sample of the triggered BRCs (Morgan et al. 2009)". Since the work of Morgan et al. seems to be so highly relevant to the study that it is presented without further discussion, the authors should at least make a small summary of it (what kind of observations? what are the results that rule out completely triggered star formation?), probably when the regions are presented for the first time. If this region is different to the other two, it would offer a superb comparison to present to the reader the fundamental differences between triggered and non-triggered BRC, and they may also want to cite it in the abstract.

Also in 7.4, the authors discuss about the asymmetric spatial distribution of objects, but they never explain how they solve the issues of projection, nor how asymmetric the regions are and how one can quantify this asymmetry.

The authors conclude that triggered star formation is favoring high- and intermediate-mass stars in BRC5 and BRC7. This is one of their important results, being listed in the abstract, but I don't really see a place where this is discussed in detail. The authors should first identify what high-mass stars are (OB stars or AF stars?) From the V vs V-I diagrams, one would think that there is a reasonable number of low-mass stars in the clouds. In addition, the authors should discuss the completeness of the survey and whether it is feasible to reach very low masses when looking for e.g. optical counterparts in highly extincted regions.

The authors also mention age differences between the in- and off-cloud objects. Such age differences have in fact been provided as a proof of triggered SF in other regions, although it is always complicated to derive accurate ages of young, variable, and very extincted objects. The authors mention this age difference in the abstract, and the ages are given in the tables, but the discussion of the ages, their potential problems, and whether there is a significant statistical difference between objects in- and off-cloud is never clearly done.

The ages are discussed a bit in 7.3 and 7.4, but although they mention "mean ages" several times, I do not find the place where the mean ages have been discussed. The authors also say that stars with ages over 5 Myr have to be ruled out from the triggering process, but they never discuss how they could be affected by uncertainties. Most young clusters have a few objects that seem to be members but appear in the wrong place according to isochrones, which could be caused by variability or formation history (e.g. Baraffe et al. 2009). In principle, finding some stars older than 5 Myr does not seem enough to rule out triggered star formation.

3. Observations.
The authors use WISE data to confirm the status of their YSO candidates. They explain that they use only data that have low uncertainties and no confusion flags (in fact, this is mentioned several times in the paper, when one would have been sufficient), but they do not explain how the large WISE beam can be affected by substantial cloud emission in the region. Even with Spitzer, there has been substantial discussion about the requirement of using small apertures to avoid contamination by cloud emission (e.g. Megeath et al. 2004; Mercer et al. 2009, among many others).

Since by definition a large fraction of the objects appear over a bright cloud, how can this affect the selection and the resulting SEDs of the identified YSO? Can it also lead to misidentification of objects with excesses that are caused by cloud emission? This effect could be particularly important for W3 and W4, since W1 and W2 can be compared to Spitzer, for which the authors performed small aperture photometry. Although the authors say several times that they prefer Spitzer to WISE data for the shorter wavelengths, did they try a comparison of both to check if there is any contamination from cloud emission? If they find contamination in W1 and W2, one would expect it also in W3 and W4.

The authors use Halpha emission to identify the stars (Sec 5.1), but never specify what kind of Halpha data they have (photometry, spectroscopy?). The data came from two catalogs (Ogura et al., Nakano et al.). Since this is an important part of the YSO identification, it would be good to describe it together with the rest of archival data. It may be also interesting to describe what types of objects would these data select: CTTS only, or also WTTS? How can contamination by dMe stars be ruled out? Since these are HII regions, how can nebular emission be corrected for a proper identification of the objects?

4. Membership selection.
The authors describe several criteria for membership selection, but it is not clear how the final list of members is produced. Do the authors select objects with Halpha AND in the right place of the JHK diagram AND IR excesses, or is it objects with Halpha OR right JHK colors OR IR excesses? From Fig 5, I guess it is rather an "or", although several sources are selected based on more than one criterion.

Can the authors estimate the contamination of the sample by comparing different indicators?

One of the points of the paper is that the triggered star formation seem to favor intermediate-mass stars, but the authors do not describe the mass limits of the present survey. Given that the BRC are at considerable distances and that the clouds have by definition high extinction, how complete is it the survey for objects with lower masses?

Minor questions:
- Although I am not sure whether this was a problem that appear at the journal, the pdf I received had a very low quality (the text is blurry and pixelized). For the revised paper I would appreciate if the pdf has a better quality. I also contacted the journal about this, since I am not sure where the problem comes from.

- There are some bold-face paragraphs in the text. What do they mean?

- There are some Class I sources marked in the V vs V-I diagrams. How did they obtain optical photometry of the very embedded Class I objects? How was it corrected from its high extinction

to put it in the plots?

- Some more repetitions: the use of good-quality flags for WISE data is stated several times throughout the paper (3.3., 5.3, 7.3). Saying it once when the data are presented would be enough.

- In 7.5 the authors say that "the cloud acquired its shape in a previous episode of photoevaporation by now-extincted stars". Extincted would in principle mean "affected by high extinction or high Av", but I guess here the authors refer to stars that are already gone (after exploding as a SN? any hints about SNe in the region?).

- There are several typos throughout the text.