

REFeree'S REPORT FOR "Probability of causal contact between interstellar civilizations through MonteCarlo simulations"

This paper deals with an important issue of the dynamics and causal structure of interstellar communication (understood in the widest sense of the word). The manuscript has a rather novel quantitative approach and, in spite of some limitations of the model proposed, offer several interesting and important results. It also gives a rather wide cross-section of the relevant prior literature and correctly positions the research presented in a wider revival of the interest in numerical studies of astrobiology and SETI. It is indeed encouraging to see more studies such as this in recent years. However, all this is marred by a presentation which can only adequate called horrible. **Therefore, I cannot in good conscience recommend this manuscript for publication in the "International Journal of Astrobiology" without major revisions** suggested below.

1. By far the most important problem I have with this manuscript is that it is, frankly, very badly written; its positive message is, therefore, distorted and almost lost in the noise. The manuscript as it now stands is written in convoluted and often confusing language, requiring much proof-reading. For instance, consider the following sentence: "In the approach of this work, we used computer simulations to address the problem of the probabilities of causal contacts between location in the galaxy the possibility of sending and receiving messages." (p. 12) This is **very bad** language, even for the present reviewer who is a non-native English speaker as well. Worse, it is incomprehensible: is it the probability of the hypothesis that there is a causal link **between** location and possibility, or the probability that the contact is indeed causal, or the probability that causal messages could be sent between different locations in the Galaxy (the Milky Way needs to be capitalized, by the way)? What is the "problem of probabilities"? It's not a problem, it is the question how big or small the probabilities are. Also, it seems that at least two commas are missing in the sentence – and, I repeat, a professional proof-reader is likely to find more, rather than less, language infelicities. It is manifested even in the title: the common spelling of "Monte Carlo" is with the separation. "ad-hoc" (p. 7) is not written with a dash. "These distributions arise any time a phenomena is characterized by commonly occurring small events and rarely produced large events" (p. 7) gets the singular wrong twice. "and the with of the galactic disk is neglected" (p. 8) is a typo, but the typo which could be found and corrected

- by the simplest spelling/grammar checker; not to use it in the *Anno Domini* 2020 is simply irresponsible and unserious. The manuscript is, unfortunately, peppered by literally dozens of such examples and they **must** be corrected prior to publication.
2. The manuscript describes much numerical work – and correctly so – but is entirely silent on what **any** numerical work needs to present: the technical aspects of the simulation. The discussion of “Complexity of the model” in section 2.2. is useless in this respect. It would be useful for researchers in the field to at least get a rough handle on the CPU times required for running the model, on what kind of machines, using what software, parallelized or not, etc., in order to be able to compare it with other approaches for astrobiological large-scale modelling which have started to be used in recent years (as the authors correctly note in the references to other studies).
 3. The presentation of the parameters of the model and its results violates a number of conventions employed throughout the astronomical sciences, which the authors would do well to heed. First, professional astronomers and astrobiologists used parsecs (pc), kiloparsecs (kpc) and their multipliers as the units of distance; “light year” is an outdated unit appropriate only for popular science and undesirable in the research context. When “max” or “min” occurs in subscript or superscript it should be in roman font, not italic. The authors should refrain from listing times in years when the timescales are so long as they are in the manuscript; instead, they should express everything in kiloyears (kyr) or megayears (Myr) or simply use the exponential notation everywhere and express results in nits of 10^3 yrs or 10^6 yrs. These might seem as minor points, but taken together with the item #1 above, they make the manuscript look like an unfinished, unpolished job which the authors themselves do not take seriously enough.
 4. Fixing the size of GHZ as the authors do is unrealistic for several reasons. While I understand that the timescales obtained in the model (and plotted in the figures) are short in comparison to the timescale of Lineweaver et al. (2004), this should be explicated and discussed in the model setup (i.e., in the section 2). Its even worse when other work – cited by the authors, ironically! – is taken into account, showing that outer parts of the Galactic disk has higher statistical weight within the distribution of habitability.
 5. Why is the minimal value of τ_a set to 4000 yrs? One could easily envision scenarios in which the interval between awakenings is realistically smaller. Also, what would happen if an infinite maximal range of the message is added to the proposed values? This could be envisioned not just as an

- emitted message, but rather in terms of setting an astroengineering artefact detectable from all over the Galaxy.
6. Why is the x -axis in Figure 6 not extended to 10^6 years? It is not obvious from the lower panel diagram that nothing interesting happens beyond 700 kyr.
 7. Referencing should be improved. Some existing items lack the required bibliographic data. Došović et al. (2019) lack the manuscript id. (it's A98). Then, we have "Dayal P., Ward M., Cockell C., 2016, arXiv e-prints" without even arXiv number/link (and this has actually been published since). Even worse, Drake (1962) lacks, well, the **entire bibliographic record**. Hippke (2017) has in the meantime been published. Rahvar (2017) is cited twice in the same group of citations on page 5. And, of course, there are some relevant references which need to be cited in relation to individual topics. Let me just mention the two important publications of Sir David Bates, who actually first posed, in a rudimentary form, the central question of the present study: Bates, D. R. 1974, "CETI: put not your trust in beacons," *Nature* **252**, 432-433; Bates, D. R. 1978, "On making radio contact with extraterrestrial civilizations," *Astrophysics and Space Science* **55**, 7-13. In contrast to Prantzos (2013) and some of the other sources cited, Fermi's paradox can indeed be formulated entirely independently of the parameters of the Drake Equation, as shown in detail in Ćirković, M. M. 2018a, *The Great Silence: The Science and Philosophy of Fermi's Paradox* (Oxford University Press, Oxford); this is useful for some of the other points raised. Finally, the model of Hair, T. W. and Hedman, A. D. (2013), "Spatial dispersion of interstellar civilizations: a probabilistic site percolation model in three dimensions," *International Journal of Astrobiology* **12**, 45-52 should be discussed in Section 1.

In conclusion, let me re-emphasize that **this paper certainly deserves serious consideration in IJA** after major improvements, notably in terms of presentation and language, have been made.