



SDI Review Form 1.6

Journal Name:	British Journal of Mathematics & Computer Science
Manuscript Number:	Ms_BJMCS_32184
Title of the Manuscript:	On the Hyper-Poisson Distribution and its Generalization with Applications
Type of the Article	Short research papers

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This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound.

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PART 1: Review Comments

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
Compulsory REVISION comments	The manuscript describes how to fit Mittag-Leffler distribution (and the hyper-Poisson as a particular case) to real data with MLE. Comparisons with other distributions, some of them permitting under dispersion, are included. I have no clear idea about which is the objective of the work. Abstract includes a series of questionable comments instead of a general view of the main ideas. I don't see the success of the inclusion of the hyper-Poisson distribution in the more general family MLFD since nothing is included about the new α parameter in the MLFD. To my knowledge, author does not model the mean and the dispersion parameters in the "GLM" context (as it ensures in page 113, first paragraph), but the location parameter λ. Additionally, I think something must be said in order to justify that HPP and MLFD are GLM.	
Minor REVISION comments	About the abstract: - In my opinion, 41.56% of zeros in German national registry data is not enough to say that they are zero-inflated. Anyway, it is surprising this comment, since data are not analysed as zero-inflated. - About the fact that theoretical means and variances do not match with their corresponding empirical versions: the author ensures that this is due to the truncation of the infinite range of possible values. I think it may depend on the estimation procedure. - Sincerely, it is totally unclear for me the question about	



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	the last category of data. In relation to that, author says that “most results in the literature just assume that...”. I would like to see unless some of these results. I insist that perhaps there is something that I do not understand, but in my opinion the question about the infinite range of the distributions and the finite range of data is totally trivial. Of course real data has finite range, because they are real data; and of course the theoretical distributions have infinite range, and propose non-zero probabilities to non-observed data. Which is the problem? I have an advice to the author in relation to the introduction. I would avoid to classify distributions such as the Poisson Inverse Gaussian, NB-Lindley and others as “exotic”. Neither to consider that “The literature is saturated with several models for over-dispersed or under-dispersed count data”. HP parameters in Table 2 are λ and β, nor α and β. About the conclusions, in my opinion the author needs a longer experience in the fit of real data before to emphasize, for example, (a).	
Optional/General comments		

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