We would like to thank the reviewers again for their time and valuable advice in improving our paper. We would like to apologize for not providing appropriate citations, which we thought we did, and have added three of the recommended references in the revised form, one to provide further information about the task allocation problem by citing the very valuable review by Brambilla, one to serve as an example for simple task allocation via probabilistic switches, and the other to make it clear that our work does not address *task* *partitioning* – dividing a problem into a number of parallel and consecutive tasks – but *task allocation.* (Five of the six provided references are on task partitioning and share subsets of authors, so we decided to cite only one two papers out of this group.)

We have also added a reference to recent work in the journal Swarm Intelligence that investigates adaptive response threshold methods in a new paragraph in the discussion, where we make clear that the analysis presented here is so far limited to constant response thresholds. We also added language throughout that makes clear that response threshold methods do not require communication and only very simple perception – the stimulus – and are therefore very simple to implement and ubiquitous in nature and swarm robotics.

Finally, we share the editor’s and the reviewers’ concerns with the applicability of game theory techniques due to their strong assumptions on global information and communication. We therefore explicitly state in the related work section “These approaches are not applicable here, where we consider systems with imperfect information and no communication between agents. For this class of systems, there exists a branch of game theory in which information about characteristics of the other players is incomplete. These class of games are known as Bayesian games…”, and further explain why this small subfield of game theory is particularly suited for swarm robotics.

*Associate Editor:  
  
The reviewers suggest that a further revision is needed before the manuscript can be accepted for publication (I have suggested minor revisions, but in fact the required revisions are a bit more than minor, although less than major).  
  
In particular, Reviewer #1 points out that the articles that he suggested should be included in the literature review have not been included (contrary to what stated by the authors in their answer; I have personally compared references in the original and revised version and none on task allocation has been added). It is indeed true that the manuscript mentions old works in task allocation and disregards a rather large number of recent ones, many of which have been published in this journal. The authors should provide a more up-to-date picture of the results achieved by the community.*

We have added a reference to recent work of Castello et al. as well included two references on “Task Partitioning”, which we believe to be a distinct problem. We have also added a reference to the overview by Brambilla.

*Reviewer #3 correctly points out that a discussion of implementation aspects should be included. One of the major comments made by Reviewer #3 on the original submission concerned the lack of an empirical validation. The answer given by the authors to this comment is not satisfactory. It is certainly true, as the authors point out in their answer, that the manuscript makes an important theoretical contribution. Nonetheless, it is fundamental that the authors provide convincing elements indicating that the ideas proposed can be have a practical application. Otherwise, the contribution made in the manuscript loses all its relevance to swarm robotics.*

We added language and references throughout the manuscript that response threshold methods are widely used in swarm robotics and have very minimal hardware requirements, which we believe speaks for the practicality of the method. We are not sure how we can empirically show that the resulting allocations are a Bayesian Nash Equilibrium other than by a formal proof. The problem is that the BNE is not optimal and there are a number of well-known policies that will improve allocations, but require additional perception and/or communication (see related work).

*Although the manuscript makes an original contribution in framing task allocation within the context of game theory, other authors have previously proposed the adoption of concepts from game theory in swarm robotics. Unfortunately, practical issues have typically prevented the uptake of these concepts. The fact that the robots are not capable of precise perception and localization, bandwidth limitations in communication, limited computational capabilities, etc. are the typical barriers that prevent the practical implementation of effective distributed algorithms based on game theory.*  
We are explicitly making the case that the majority of multi-agent game theory is not applicable to swarm robotics with limited computation and communication, and identify the subfield of Bayesian games, which is applicable to a simple policy such as a response threshold without communication.

*The author should include in the manuscript a discussion of the challenges raised by the implementation of the ideas proposed. In particular, they should discuss which capabilities the robots should have to perform the required computation and to acquire/exchange the required information.*  
We added language to the related work and the discussion that emphasize that response thresholds only require perceiving a global stimulus, no communication.

**Reviewer #1**: The paper has addressed most of my comments, although some remained partially open.  
- In the discussion answering my first concern on optimality of the BNE, the authors state:  
" ... but we believe that there is still no definition for optimal multi-robot task allocation that is broad enough to compare equilibrium strategies to."  
I did not particularly understand what are the characteristics of the sought definition of optimality. Also, definitions of optimality in engineering and in evolutionary biology might not be the same. Do the author mean that the community needs a definition that encompasses both? (Is that even possible?). I suggest to further clarify the above sentence.

**ANSHUL**

*- The expression "across length scales" is still present in the abstract and in the introduction. I propose to remove it as it's not adding anything to the manuscript.*

Done.  
  
- About the answer to the comment on systems without knowledge of the stimulus (leaf-cutting ant paper). In that paper, ants in the cache were not recruiting other ants (they cannot communicate). Ants moved from the source to the cache using a simple probabilistic switch (independent of any stimulus). Ants were then going from the cache to the source whenever they did not encounter items on their way. In a way, I see this as a mechanism that is even simpler (i.e., has less capability requirements) than the threshold model. Therefore, there could be mechanisms that are even more suited as a baseline strategy (I'm referring now to the added last paragraph in the discussion). This is why I was asking whether, in a future extension, this framework could be used to model also these mechanisms (this would be needed to make the approach more general and not specific for threshold-based).

**This is a very interesting idea. A probabilistic switch, together with a time-out or another switch to return to the initial state, leads to a well known equilibrium that is determined by the ratios of probabilities. As such it is the most basic form of division of labor, which leads to constant allocations independent of the environment. We added this discussion to the related work section to introduce response threshold methods as the simplest possible form of adaptive task allocation and will use the provided reference as an example. We also added a sentence to the discussion that the sigomoidal response threshold function approaches a constant probability when the variance with which the stimulus can be perceived increases.**

*- Finally, I wanted just to point out that, although the authors claimed to have included some of the references I proposed, I did not see any of these references in the bibliography.*

See above.  
  
  
*Reviewer #3: The authors have satisfactorily revised the paper to address the major comments in the first round of reviews. The contribution of the revised paper is clearer. Since the intended goal of this work is to implement the algorithm on robot hardware, it would be appropriate if the authors address the implementation aspects of their proposed algorithm - some discussion along the lines of what would be the challenges and issues that someone trying to implement this algorithm would expect to face? That would not only make the paper stronger, but, most importantly, would allow researchers interested to extend this work with some insight about how to proceed.*

Thanks. We have added language throughout the paper that emphasizes the minimal requirements for implementing a threshold strategy.