Reviewer #1: The paper can be placed in the context of studies of task allocation through individual-level models within swarm robotics and other swarm systems. The paper, at the best of my knowledge for the first time, frames these type of systems as Bayesian games, in which each individual in a swarm is playing a game with other individuals. The strategy of each individual consists in choosing whether to participate in the game (task) or not depending on the level of stimulus perceived from the environment. The stimulus is task dependent and represents the level of need of a task to be performed. The main contribution of the work is to prove that a set of threshold-based strategies, commonly used in biology and engineering to model these types of systems, and whereby each individual does not need to have capabilities to communicate nor to model the behaviour of other individuals, is a Bayesian Nash Equilibrium (BNE), meaning that no individual has an incentive to change its strategy for something else.  
  
The paper brings a fresh set of novel ideas to the swarm robotics community. It successfully convinces the reader that this framework within game theory can be successfully used to model task-allocation systems, and that it is sufficiently general to model a wide set of possible scenarios. The paper is also very well written and clearly structured. I therefore do recommend the publication on Swarm Intelligence, while I still have a set of comments that could be used to improve the paper and to further foster the discussion.  
  
My first high level comment is on the main message/insight that can be drawn by showing that threshold-based models are a BNE. In the discussion section, the authors correctly state that a BNE strategy is not necessarily the optimal strategy, but just a strategy that can't be further optimized at the individual level provided other agents have a similar threshold-based strategy. This fact basically mean that being a BNE alone cannot be a justification for these models to be chosen as "best" engineering methods. Furthermore, I would also add that being a BNE is also not a sufficient condition to claim that these are the best models for similar biological systems. In fact, if placing the study in an evolutionary perspective, it is not (yet) shown that a BNE strategy composed of threshold-based models cannot be invaded by any other (possibly different) strategy. In other words, it is not (yet) shown that the BNE is also an Evolutionary Stable Strategy. The reason why I'm saying  
this is not to penalize the paper (which still is a very nice incremental contribution), but to encourage the authors to include a discussion on this aspect.

***Thank you very much for stimulating this discussion. We believe the wide perseverance of simple thresholds models to be indeed a hint toward it being an evolutionary stable strategy, but we agree that the mere existence of a BNE is by no means a sufficient condition. We toned claims on this regard down throughout the manuscript. We have also enhanced the discussion with this regard. Finally, we have clarified that this property makes its application in an engineering context attractive, but by no means an optimal choice. [WORK IN PROGRESS]***

The rest of my comments are either aimed at clarifying better some aspects of the paper, or to point at some literature that in my eyes should be included as reference as well.  
- Title: what does "across length scales" mean? I did not see an explanation anywhere in the paper. I would actually remove this part from the title and add something concerning game theory. Alternatively, the authors could explain what this means in the body of the paper.

*We have removed “across length scales” from the title. Here, length scales refer to the presence of the phenomenon in a variety of species ranging from social insects to humans, i.e. different scales, see also the first sentence of the manuscript. We changed the wording to “different length scales”*  
  
- Page 3 line 38: random task allocation strategies -> shouldn't it be "probabilistic" task allocation strategies?

*Thanks for catching this.*  
  
- The authors mentions the presence of a supplementary materials, but do not provide a link to it.  I suggest to fix this.

*Thanks for catching this editing mistake.*  
  
- Section 2 properties a. and b. On property a., they mention "for any a\_i \in A\_i and g", but the previous formula does not have a\_i in it. Can you clarify? On property b., half of it is explained formally (situation where all agents participate), the second part only in words (all agents do not participate). I suggest to explain both first formally, and then by words, for better clarity.

*We removed the phrase a\_i \in A\_i to increase clarity and made the description of property b more verbose.*  
  
- Page 4 line 39: is it possible to maybe plot the example utility function for some specific values of the parameters? I'm really curious about its shape.  
  
- Page 5 beginning (end of Sect 2): this part is particularly hard to follow. I suggest the authors to explain with words the meaning of the above formula in full (specifically the part s(x) \in argmax ... is particularly obscure to me).  
  
- Page 5 line 55: what do the authors mean with "original" strategies here?  
  
- Page 6: to be really honest, I am not an expert of Bayesian Game Theory myself, therefore I could not really follow the proof of Lemma 2 or the need for it at all. First, I did not understand what the mapping L exactly is. Second, why one would need this mapping to be continuous, and what this actually means for the purpose of this problem. Third, the proof itself is quite hard to follow (I also had hard time to understand some definitions, for instance line 8-10 defines s as a threshold strategy, \alpha as a threshold vector, but then \alpha\* becomes a threshold strategy again). I believe this is the part of the paper that need a more heavy rewriting. The authors could also move this to the supplementary materials/appendix, but they would need still to explain the main purpose/utility of this lemma to prove Theorem 1.  
  
- Page 7: the threshold t is indicated in bold, but it's a scalar number right? I find this confusing as bold is typically used to indicate vectors.

*We changed the format of T to \mathcal{T} throughout the paper*

- Page 7 line 37: why are I\_xi > t IID random variables when x\_i are ? I don't find this passage very obvious, please motivate  
  
- Fig 2: I would maybe show less lines from Eq 1 but add few lines from the the actual sigmoid, to show how the two relate to each other.  
  
- One of the main assumptions of the paper is the knowledge of this stimulus signal. I wonder whether a discussion can be included in the paper about how it could be possible to model systems where it is not possible to obtain a (not even noisy) estimate of the stimulus. For instance, still in the context of task allocation, this recent work <http://journals.plos.org/ploscompbiol/article?id=10.1371/journal.pcbi.1004273> (which I do recommend to include as reference) successfully evolved a task-allocation-like collective behaviour where individuals seem not to have access to a stimulus estimate (for instance, robots that are dropping objects from the top of the slopes do not have an idea on the state of the cache, but still manage to switch between tasks in an effective way). How could these situations be modelled using this framework? What about other collective behaviours (the authors mention flocking at some point)? A similar discussion could help in establish the generality  
of the approach, which I do find very promising.  
  
- Besides the above work, the related work section could be extended to include more recent works done in swarm robotics on task allocations. Example are:  
  
G. Pini, M.Gagliolo, A. Brutschy, M.Dorigo, and M. Birattari (2013). Task partitioning in a robot swarm: A study on the effect of communication. Swarm Intelligence. Volume 7, Issue 2-3, pp. 173-199. (Bib)  
G. Pini, A. Brutschy, C. Pinciroli, M.Dorigo, and M. Birattari (2013). Autonomous task partitioning in robot foraging: an approach based on cost estimation. Adaptive Behavior. Volume 21, Issue 2, pp. 117-135. (Bib)  
A. Brutschy, N.-L. Tran, N. Baiboun, Frison M., G. Pini, A. Roli, M. Dorigo and M. Birattari (2012). Costs and benefits of behavioral specialization. Robotics and Autonomous Systems. Volume 60, Issue 11, pp. 1408-1420. (Bib)  
G. Pini, A. Brutschy, M. Frison, A. Roli, M. Dorigo and M. Birattari (2011). Task partitioning in swarms of robots: An adaptive method for strategy selection. Swarm Intelligence, Volume 5, Numbers 3-4, pp. 283-304. (Bib)  
  
and more can be found in this swarm robotics survey (which I do suggest to include as well):  
  
<http://link.springer.com/article/10.1007%2Fs11721-012-0075-2>  
  
  
  
Reviewer #2: This paper is to a certain extent an interesting piece of work that somehow still seems to be preliminary. I like the idea of trying to formulate a formal model using game theory that clarifies/supports the use of response threshold strategies in natural swarms or within mathematical and engineering formulations. Having said that the authors claim that the paper provides a formal analysis framework for threshold-based task allocation in multi-agent systems; I don't see such a framework in this paper to be honest; it is indeed shown that in a simple setting BNE is achieved, but these results are not providing a formal framework within which to study multi-agent or swarm threshold-based task allocation strategies.  
  
From my point of view, it seems that the lemmas and theorem given are reasonable (and their proofs sound), but I am very doubtful that the contribution is very significant - at least this is hard to judge from the paper in its current form. The paper seems almost cut short. Section 1 seems to set the stage for a much longer paper, but after the proofs the discussion and conclusion sections are almost non-existent. The figures seem unnecessary (one conceptual illustration and one plot of results that are perfectly predictable according to the way the program that generated them was written). For instance I do not see what the added value is of figure 1. I would also recommend the authors to nu use subsections in the first section but just try to describe what you are studying in this paper, what the proposed solution method entails and what the contributions are.  
  
I am not sure whether the background section is really complete. for instance there is quite a bit of work on large coordination and anti-coordination games for multi-agent task allocation (think for instance of the work Shoham, Grenager and Powers on dispersion games; or the work of Tumer et al.) Assuming that all agents know the total number of other agents sound like a strong assumption to me. As already indicated I find the discussion and conclusion a bit light weight. The first line of the conclusion is a rather vague statement, which I am not sure of that it is really very well backed up. The entire conclusion sounds a bit too speculative in my view.  
  
  
  
Reviewer #3: This paper presents a technique to model a threshold based aggregation mechanism using a class of competitive games called global games. The idea to combine or model a biological evolutionary process with game-theoretic decision making is interesting, and the technique could offer advantages such as system stability if all players or swarm units follow the Bayes-Nash equilibrium strategy. The paper appears to be well written, with very little typos. The proposed model is analyzed theoretically to show existence of a stability criteria and the proofs appear sound. However, in its current form the research seems to have certain fundamental shortcomings. Below are some points to make the paper stronger.  
  
1) The motivation of the paper should be improved, especially stressing on the idea why it is relevant to use games, like global games as a formal model for task aggregation. What advantages does modeling the process as a game offer? The introduction section of the paper should be rewritten while describing and motivating the problem and summarizing the key contributions and results of the paper, instead of getting into description of model parameters in the very second paragraph. Also, as in conventional in journal papers, a section-wise summary of the paper should be provided at the end of the first section.  
  
2) In the introduction section, the parameter \tau or stimulus seems to be introduced abruptly. \tau is a crucial variable in the model, but it is best to introduce mathematical parameters while describing the mathematical framework. Also, while introducing \tau, several references to the use of similar stimulus parameters in other disciplines, are made. Moving this discussion to the related work section would improve the flow of the paper.  
  
3) The description of modeling the task allocation process as global game not clear. In Bayesian games, which are used as the main formal model in this paper, the uncertainty in other players' actions is modeled as players' types. The type of the player then determines the action that each player takes. Each player is assumed to know the joint distribution over types of all players. The modeling used by the authors does not seem to follow this canonical model. It is not clear what the notion of a player's type in the proposed model would be. In the proposed model, it looks like the type and action are combined into one parameter as the strategy function. The authors should elucidate this point further. It would also be useful to give a formal description of a global game at the beginning of section 2.  
  
In a similar vein, there are several types of multi-robot task allocation - MR vs. SR, MT vs. ST and TA vs. IA, that are described in authors' cited paper by Gerkey04, on which they base their task allocation model. It would be useful to give a formal definition of the task allocation model used in the paper to clearly explain which task allocation type the authors are considering.  
  
In Section 2, page 5, lines 4-5, the reviewer is not convinced how the parameter 'g', (the number of other agents or players that influence the utility of the the action a\_i) can be realized as stimulus function, \sum s\_j(x\_j). Specifically, how does an agent i observe the value of \sum s\_j(x\_j) for all j \neq i?  
  
4) One of the challenges in implementing a game as an interaction strategy in physical systems such as multi-robot systems is to resolve conflicts between multiple Nash equilibria. The authors prove that there exists Bayes Nash equilibria (BNE), but have not shown that the BNE is unique, that is there exists exactly one BNE. Because this is a one-shot game, where agent select their actions simultaneously without observing each others actions, if there are multiple BNE, then multiple agents might end up selecting different BNE (as the criteria is to play only \_a\_ BNE, not the same BNE). This would result in the agents playing a non BNE strategy as different agents are playing different equilibrium strategy, and the stability of the system would break down. This is a very critical point in the entire paper, as without this, the entire premise of using BNE would be questionable. The authors should address this aspect carefully as it critical to the validity of their proposed  
approach.  
  
5) A rather serious weak point of the paper is the lack of empirical validation, esp. in light on comment no. 4 above. It would be very useful, and the make the paper much stronger if empirical results are provided, even in simulation using robotic agents following the proposed model for task allocation. Currently, the analysis is done for a single task, but in real-life situations there are bound to be multiple tasks. It would be essential to see how the system behaves with multiple tasks, for different numbers of agents and tasks. Also, empirical validation of the system with different stimulus parameter (\tau) threshold and different noise (\eta) would be useful to understand the system's behavior. Empirical validation is also conventional in most swarm-based system papers. It would make this paper much stronger.  
  
Overall, the idea in the paper is interesting, but, in its present form the paper has certain crucial shortcomings. If the paper is made stronger while addressing above points, it will make a nice contribution to the SI journal.