Research Statement
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My research focuses on control for mobile sensor networks. In particular, Multi-Robot Task Allocation (MRTA) problems, and distributed motion control coordination. My results, although inspired by biological systems and social networks, are general and rigourously proved.

Overview of previous results

In the context of MRTA, I studied how to attain an optimum balance between *coverage* and *sensing*. I obtained optimal solutions under various cost schemes [1,2,5] that rely only on local information. Unlike previous results in the area, I consider both tasks that are of effectively different nature, and different realizations of the same task simultaneously.

By looking at coordination problems, I improved the existing algorithms for two of the most common distributed robotic tasks: *coverage* and *rendezvous*. For coverage, I extended the approach from Cortés et. al. [10] to non-convex regions [4,6]. This generalization is highly relevant since most real world environments are non-convex. My result thus makes coverage algorithms practical for real world scenarios, including homeland security, environmental monitoring and ocean pollution control. Moreover, since the coverage problem can be shown to be equivalent to the quantization problem solved by Lloyd's algorithm in signal processing, my extensions now allow its implementation in applications which are not restricted to a convex space.

As for rendezvous, I solved the general problem by exhibiting it as a particular realization of *consensus algorithms*. My solution [3,8] describes how the rich literature on consensus protocols can be applied to analyze and solve the rendezvous problem, and also shows how most of the previous approaches to rendezvous are a particular realization of my solution. Although the relationship between rendezvous and coverage was not unnoticed in the literature, it was not until my paper [3] that an explicit description of the nature of this relationship was presented.

Future research directions

I am currently pursuing two problems derived from the results of my PhD thesis: What happens when communication restrictions are taken into account for motion control algorithms, and how robust consensus is to noise.

In my previous work, the communication channel is assumed to be perfect. This assumption is clearly unrealistic but, surprisingly, its effect on the final behavior of the distributed network is negligible: Recall that the *physical motion* of the agents is the result of the communication exchange, and it evolves on a different (slower) time scale than communication. In [7] I formalize this point, and present an explicit description of how fast a robot can move in the formation, so quality parameters like probability of error in closing the feedback loop can still be ensured, even though some communication messages may be lost. I extend this discussion to present minimum requirements in the length of the time slot necessary to guarantee the implementation of the *average-consensus* algorithms, which require synchronism in order to work. Following these results, I plan to study how the limitations on the channel capacity would affect the performance of the network when analyzing the maximum distortion that can be allowed for the network to perform as expected. Researchers in both the Information Theory and Control communities have agreed that joint work is needed to address some of the overlapping open problems in both areas [11].

The question of how robust consensus is to noise appeared when studying the rendezvous problem. It was assumed that each agent was capable of having a correct observation of the other robots in the formation. This is an unrealistic assumption, and hence the estimated location was modeled as the real location perturbed by some noise. When written as a consensus protocol, the rendezvous problem can be formulated as a linear system under the influence of noise. We derived bounds for the worst-case scenario in [8], and observed that such a worst-case bound is conservative for the *typical* realization. We present some first results about the behavior of such a typical realization in [9], where it can be observed that the problem has two parts: First, how it behaves with respect to the typical noise (which was the problem studied in the paper) and second, how it behaves with respect to the typical consensus matrix. We derived how the second largest eigenvalue of the matrix appears on the evolution of the noisy system. Currently, we are trying to derive how the *typical* value of this eigenvalue will affect the evolution.

Studying the rendezvous problem as a consensus problem allows one to look at the evolution of noisy consensus algorithms which, I believe, remains an open problem, despite some partial results for particular configurations. I presented in [8] the relation between the second largest eigenvalue of the consensus matrix and the evolution of the system under noise and highlighted how to use such dependency in the context of the rendezvous problem in [9]. By attaining a more complete description of how the average consensus matrix affects the output on a noisy system, enough tools would be obtained to solve the noisy rendezvous problem, and apply such algorithms in signal processing. Although consensus algorithms have already been used in signal processing, they have been used *after* the processing stage has taken place. A deeper understanding would allow us to implement them directly into the processing stage rather than as a final *clean-up* step, reducing the computational costs of alternative filter strategies.

The generalizations to the coverage task that I presented in [6, 8] have in common that the agents need to know before the deployment the region in which they are going to be performing the task. This includes knowing all possible obstacles in the terrain which, for practical applications, might be unreasonable. It is necessary to develop a *robust* coverage algorithm that can adapt itself to changes (first on the interior, then on the boundary) of the relevant workspace. Once this is accomplished, the results can be extended to different scenarios, and the task allocation algorithms I proposed in [1, 2, 5] will thus be generalized.

Previous results assume that all the agents are behaving according to the protocol rules, and if some robot starts malfunctioning, this would become evident because its decisions would contradict previous exchanges of information. However, there are few results on how to detect an agent that malfunctions consistently with the protocol. Solving this problem would solve questions about the robustness of the network, and have applications in communications systems and computer networks, by allowing us to detect harming sources that otherwise would look consistent with the network's correct mode of operation. The underlying idea is that as time passes, the window of operation for such malfunctioning agents gets reduced, and hence the actions they can take to mislead the system get reduced until either the agents *behave* according to the network expectations, or they expose themselves, getting purged out of the system as a consequence. Derivation of robust protocols is necessary, and control theoretic guarantees on them are required to ensure that, even if the malfunctioning agents cooperate among themselves, their scope of operation gets reduced as time goes to infinity, and that effectively each agent, based on the information it gathers, can raise valid suspicions based on the actions of any other agent in the system.

Funding opportunities

Problems related with the time evolution of distributed networks appear, among others, in communications, biology and social sciences. Funding agencies and the private industry are aware of this situation, and funding options for working in this area are offered regularly. Just to mention one example, DARPA classified *The Dynamics of Networks* (understood as the development of mathematical tools for the modeling of distributed networks evolving under time) as one of their *23 mathematical challenges*. I expect that new exciting, challenging and difficult research problems would need to be faced in the years to come as new applications are developed and implemented.

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