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I use the lens of dynamic game theory to understand the emergent properties that arise from relational structure of multi-agent systems in the areas of applied dynamical systems and mathematical biology. My main interest is in the innovation of new modeling techniques, building novel tools that co-opt and expand on existing methods in ODEs, PDEs, and non-local equations to attack the fundamental problem of multiscale emergence. I am a mathematical generalist and examine new techniques for game-theoretic modeling that span from fundamental models with pure dynamic game theory to applied models with varying degrees of specificity. On the foundational side, I have mainly focused on simple, elementary game-theoretic interactions like coordination games, and I have spent most of my time in graduate school considering new methods to build models of coordination through continuous extensions in time, space, and strategy. The main goal of this research is to introduce new, more powerful tools to the study of coordination and develop theory which is consistent across all coordination models, regardless of the domains in which they are embedded.

I am excited by the breadth of scope and diversity of opportunities for interdisciplinary collaboration my work enables. I consider a wide variety of domains and use a wide variety of tools to answer questions that arise from an even wider variety of applications. I have already enjoyed collaboration with researchers in disciplines ranging from behavioral ecology and evolutionary ecology to economics and systems engineering to explore how my basic mathematical results can provide novel insights in these areas. My research has been published, not only in academic journals for a mathematical audience, but also on topics ranging from group foraging to pandemic models to trade networks. In this way, my research experience can be classified into two modes: my fundamental focus - **developing new game theoretic modeling techniques for high-structure, multiplayer games**, which involves applied dynamical systems and PDEs, and my application focus - **using dynamic game theory to produce and analyze basic models in complex systems**, to which I apply my experience in modeling, numerical methods, and high performance computing.

1 Novel models of coordination

My main research focus for the past several years, and the topic of my dissertation, has been models of coordination. Coordination games (a general set of game theoretic interactions in which a player taking on the same strategy as a co-player results in a higher payoff) are well understood when every player interacts with all other players equally, but when relational structure is added to the game and players interact with one another inhomogeneously, the dynamics of this game are far more complicated [1, 2]. The general goal of this research effort was to interrogate the interaction between relational structure and coordination dynamics. I approached this problem through both modalities: applied dynamic game theory and innovative modeling techniques.

1.1 Applications of dynamic game theory

In the past decade, the pursuit to better understand the coordination game has involved many simulation studies and numerical experiments[3, 4, 5]. This approach has found success, especially among those who study coordination for its application's sake. Although the simulation approach is not the main focus of my research, I recognize its importance, especially for building intuition for the behavior of a system and for producing usable results in the application area. To that end, I find it important to make sure that, in everything I do, a strong numerical component makes the results understandable to a wider audience. This often involves going back to the fundamental methods in dynamic game theory to examine and reexamine system behaviors.

Simulations and numerical methods

I began my research into coordination dynamics with a simulation study[6]. My coauthor and I considered the critical case of coordination games in which no strategy offers any intrinsic benefit to the players. Part of this project involved estimating the size of the basin of stability for the consensus equilibrium (the equilibrium in which every player plays the same strategy) through myopic best response. The most crucial elements of this task were first to understand how the best response operator worked on the system and then how to correctly and efficiently implement it.

The model of the pure coordination game we were working from involved stochastic tie-breaking, and so resolving basins of stability in the state space is not so easy as it is in a deterministic model. Instead, our approach, which required us to think of the collection of basins of stability as a partition of unity over the state space, allowed us to estimate the size of the basin of stability for certain equilibria without having to find the actual partition of unity over the state space. Our approach relied both on the brute force provided by the cluster computer at the National Institute for Modeling Biological Systems (NIMBioS) and some clever algorithmic design which changed the time complexity of finding non-trivial equilibrium states for a particular graph (a question that we conjecture to be $\mathcal{NP}-$complete) to space complexity, tracing out paths through the state space under myopic best response. By saving only endpoints of these flows through the state space, we draw a great deal of insight about the system. In addition to the measurement of the size of the basins of stability for all equilibria found through the simulation (up to isomorphism), we also developed the basis for several conjectures about coordination dynamics in general settings. Some of these conjectures are addressed later in my research, while others remain unanswered.

Applications beyond numerical methods

Of course, applications of dynamic game theory are useful far beyond the numerical methods. I find these foundational applications so important to my research, not only for the intuition-building on unfamiliar systems but also for construction and validation of novel modeling techniques. I will discuss several examples of modeling techniques in dynamic games that I have innovated or invented in the next section, but in order for any such innovation to be usable, the foundations of dynamic game theory are required. When discussing the classification of equilibria in new models of coordination, it was necessary to describe the novel systems in terms of existing equilibria. Moreover, having an understanding of qualities of dynamic games that unite systems beyond a single domain (e.g., the bandwagon principle for coordination games) and being able to make arguments with it using foundational dynamic game theoretic results are crucial in the effort to innovate modeling techniques.

1.2 innovative modeling techniques

The work involving applications of dynamic game theory I did early in the course of this research made it possible to start to expand on the modeling techniques already used in coordination and to develop entirely new modeling techniques. This area of research involved much more of the fundamental focus on developing new modeling techniques, but even within this focus, I found ways to incorporate applied dynamical systems from a variety of domains. I think that the flexibility to translate a model from one domain into another (e.g., discrete time-discrete space to continuous time-continuous space) is crucial for an applied mathematician and much of the work that I have done and am continuing to do demonstrates this.

Re-framing the problem

The flexibility to restate problems through modifications of the domain is extremely important and one of the main tools of my research. In dynamic game theory, when we think of modifications to the domain, we often think of continuous extensions in time or space and these types of modifications are crucial in my research (see later paragraphs), but equally as important are modifications which may be more specific to the game.

For the coordination game, my first modification of the problem was to forget the method of strategic profiles, which had been used ubiquitously across all studies into the structured coordination game (e.g., [1, 2]). Instead, I considered only the boundary of strategies at equilibrium. This reduction in the state space makes it much easier to classify equilibria for a wider variety of asymmetrical domains. From this, we were easily able to find methods to detect and remove isomorphic solutions from numerical solutions and improve efficiency to catalogue equilibria of the coordination game on very small graphs. This same re-framing made possible the analytical study of structured coordination in larger, more general discrete domains. This research is still ongoing, but it is easy to see how reduction to the boundary information may be helpful when you consider coordination in a planar graph. The boundaries of connected strategic communities form cycles through the dual graph and, when that dual is simple, the partition boundaries form what we call the partition boundary subgraph. By considering coordination in the dual problem, we take a local maximization problem and turn it into a global minimization problem. Studying this dual object is more straightforward and we can rely on existing literature about minimal subgraphs and related topics.

By considering the question with flexibility in the domain, I was able to open up many new avenues for examination. This particular novel modeling approach to coordination in discrete settings allowed for new approaches in two different settings: (1) In the equilibrium setting, using a result involving the isomorphisms of equilibrium strategy, we were able to prove that the partition boundary subgraph is entirely sufficient to describe an equilibrium strategy profile with particular assumptions on the graph. This allows us to make determinations about what structures in the graph admit non-trivial equilibria and which kinds of graphs are "indecomposable" (meaning that they only admit the consensus equilibria) through convexity-type arguments. Thus, attempting to understand this problem through its dual gives some helpful insight into structured coordination games as a whole. (2) In the dynamic setting, there are many issues that prevent one from describing the dynamics entirely in the dual. In particular, the unfortunate demand for global information in the local decision-making of the individual in the myopic best response dynamics prevents a fully local description of the dynamics through the partitioning method. However, the modifications to the game that are required to resolve this issue prompt us to think about the extension from the graph to a continuous setting.

Adapting game theoretic fundamentals

I also take advantage of domain flexibility to borrow and adapt more powerful tools from non-native domains. Another example of such a novel approach to modeling coordination in my work that exemplifies this involved considering a continuous strategy space by way of mixed strategies and a continuous time extension so that I could co-opt standard techniques in ODEs to describe the stability of non-trivial equilibria. To model coordination in continuous time poses another issue in the dynamic case. Continuous adaptations for myopic best response (such as gradient ascent methods, which I will mention later) do not work because the gradient of fitness may lead outside of the strategy space. Instead, inspired by models of competitive exclusion and replicator dynamics, I formulated a version of the replicator equation to model mixed strategies of many individuals rather than the mixture of pure strategies in a well-mixed population. This captures a "better response" dynamic in the population, and we can use this method to describe some of the dynamics we see in the structured coordination game, but again we are limited in this pursuit, this time by the size of the system.

However, having proven a certain equivalence between equilibria in the ODE model and in the discrete model, we can assess the stability of pure strategy equilibria easily through standard methods in ODEs. One of the many issues with studying the structured coordination game is that non-trivial equilibria are hard to find in large graphs. This is because of the size of the basins of stability of these non-trivial equilibria relative to that of the consensus equilibrium. Although it is easy to check whether a strategy profile is an equilibrium, determining the size of its basin of stability is a far more complicated problem. Although this research is also ongoing, It is among my immediate next goals to show that the dimensions of the stable manifold for pure strategy equilibria can be used to relate the structure of the graph to the stability of particular non-trivial equilibria.

Borrowing from nonlocal equations and PDE theory

Through flexibility in the domain and innovative adaptation of existing techniques, we can use a wide range of new tools to answer long-standing questions. This ability is exciting, but even more exciting is the ability to tackle new modeling questions that require nonstandard techniques. The last novel modeling technique I used to study coordination involves another continuous extension. In this study my coauthor and I considered a continuous extension in the strategy space, not through mixed strategies, but through comparable strategies [7]. When you consider the pure coordination game (when the payoff matrix is I_m), using a comparable strategy "blurs" the payoff matrix a bit and gives the off-diagonals small positive values. In the continuous case where the strategy space does not have finite dimension, we replace this payoff matrix with a "recognition function." This continuous extension into the strategy space is interesting on its own because it shows us that the properties of coordination games, namely the bandwagon property, are maintained even when the strategy space is distorted. However, this extension becomes very exciting when we examine it in a continuous player space.

The extension into a continuous player space is a personal favorite way to re-imagine dynamic games because it allows us to use far more powerful tools that we would be able to use otherwise. This is on full display for this continuous extension of the "comparable strategy" coordination game. In any pairwise interaction under the comparable strategy concept, the payoff for a player playing strategy u_1 against a player playing a strategy u_2 is given as $w(u_1, u_2) = \rho(u_1 - u_2)$ where ρ is the recognition function which satisfies certain growth conditions and regularity conditions. When players play this game among themselves in a continuous player space, we use a "familiarity kernel" to describe how players interact and the resulting payoff of a player $x \in \Omega$ in a strategy profile given by $u \in C_b^0(\Omega;\mathbb{R})$ is given by $\int_\Omega k(x,y)\rho(u(x)-u(y))dy$ and the time dynamics of the game under myopic best response, with certain hypotheses in place can be described by

$$\frac{\partial}{\partial t}u = \int_\Omega K(x,y)\rho'(u(x)-u(y))dy$$

The resulting nonlinear nonlocal diffusion equation is worth studying on its own because of its significance in the field of non-local equations, and the fact that it can also be used to help describe coordination dynamics in certain settings makes it all the more interesting. In this research, I worked out some standard existence and uniqueness type results as well as some regularity results, which allowed us to use numerical methods to visualize solutions to the initial value problem. These numerical experiments showed a peculiar behavior in solutions where the recognition function had compact support. I observed that, at equilibrium, strategy profiles collapsed into discrete bands of strategies. That is, if u is a solution to $0 = \int_\Omega k(x,y)\rho'(u(x)-u(y))$, then the image of the domain $u(\Omega) = \{y_0, y_1, ..., y_m\}$ was a finite collection of points. I later proved this observation to be true using a modified maximum principle-type argument and gave a lower bound for the separation of the bands.

Throughout all of this work on coordination, I achieved a balance between my two main modes of research. I spent time studying existing models and practicing using applied dynamical systems and standard game theoretic modeling techniques when I carried out the simulation study and in carrying out numerical experiments (Found in the repositories [8, 9]) for each of the aforementioned projects. Those numerical experiments and the standard techniques I employed early on also informed the new modeling techniques I developed when I considered the partition boundary subgraph or any of the continuous extensions. Through both of these modalities, I have been able to identify new and interesting results about the relationship between relational structure and coordination. For instance, throughout each of these projects, I have found that the standard results in coordination (that when all players interact, the only stable configuration is consensus) does not hold up if changing strategies is costly to the player. I have also shown that, in the continuous setting, a higher degree of connectedness counterintuitively has the potential to result in a lower degree of consensus. These results and others have a great many applications in the social sciences and behavioral ecology.

2 Interdisciplinary modeling work

Another important part of my research is my commitment to applications of game theoretic modeling in a variety of domains. I have been lucky enough to get the opportunity to conduct both modalities of my research in many applied settings. In applications from behavioral ecology to trade networks, I have applied my research wherever there are many agents interacting in complex ways. In settings like these, I lean heavily into the applications of dynamic game theory, and my skills as a generalist in this area have come from opportunities to work on a wide range of modeling problems.

Behavioral Ecology

Early in my research career, I studied a behavioral ecology question of group foraging through the adaptive dynamic approach[10]. In addition to practicing this fundamental technique in evolutionary game theory and getting my first experience with scientific computing, I also developed a novel approximation of the spread of alert signaling through groups of foragers as a function of time spent in vigilance. The adaptive dynamic approach requires the assumption of a well-mixed population, so if I were to revisit this question, I might investigate the ways that that assumption could be weakened so that position could be more accurately included. However, this work did provide theoretical support and even an adaptive mechanism for the 'many eyes' hypothesis of group vigilance.

Epidemiology

Because of my association with the National Institute for Modeling Biological Systems (NIMBioS) and the NSF Center for Analysis and Prediction of Pandemic Expansion (APPEX), I also got the opportunity to work on projects relating to pandemic modeling. Most pertinent to my current research was a paper I authored about the built environment micro-biome[11]. Although there was no game theoretic modeling involved, the work was a highly interdisciplinary effort to understand how the form of the built environment impacts the dynamics of the micro-biome. In this project, I got to work with academics and practitioners from many domains to synthesize a new perspective on how the micro-biome impacts models of infectious disease of all types. This paper was the first of an intended (and forthcoming) series of papers for the purpose of incorporating a better understanding of disease dynamics in buildings into models of pandemic risk. The current work to this end is taking the results of that synthesis and turning it into an evaluation of risk based on the structure of a building. Getting to work on this and other modeling projects [12] with people from many disciplines has been a great opportunity to learn about the skills necessary to make models and modeling results accessible to a wide audience of academics and practitioners.

Trade networks

Most recently, I have had the opportunity to practice my applied game theoretic modeling skills in a project, also associated with the APPEX center, related to the trade of wildlife and the associated risk of disease spillover[13]. Game theoretic models of trade networks are not novel but, as part of this working group, I developed a new way to incorporate the probability of infection, not just into the calculation of spillover risk, but into the payoff structure of the model to capture the bidirectional coupling of economic incentives and health and safety practices in the wildlife trade. I was able to employ fundamental modeling techniques in dynamic game theory and think about numerical methods for solving nonlinear problems related to this model (found in the repository: [14]). Between these skills and the repeated practice of making these model results useful and accessible to the biologists and the wildlife managers in the group, this project and ones like it have helped me to become a better applied mathematician and have informed my goals for my research in the future.

Throughout each of these projects, and projects I've played smaller roles in (e.g.,[15]) I have been able to flex both modalities of my research. Of course, these projects in applied game theoretic modeling lean further to the applied side of my research, but they have been instrumental in forming my understanding of the fundamentals of game theoretic modeling, and they inform how I think about the requirements on a game theoretic model to make it both powerful and useful. As I plan the next phases of my research, both modalities will continue to be important in the way that I investigate any question in game theoretic modeling.

Future directions

My vision for my future research is clear but highly flexible: I will maintain a strong effort both in developing novel modeling techniques and in applying game theoretic models in areas where they are needed. Highly structured multiplayer games are plentiful in all sorts of application areas and novel techniques for modeling these kinds of games can improve our understanding of these systems in many domains. My main goal will be to continue studying extensions into continuous player spaces for different kinds of multiplayer games, not only the coordination game. I am certain that nonlocal equations and other PDE techniques will prove to be extremely powerful, not only in extending multiplayer games to more complex modeling scenarios, but also in the analysis and evaluation of existing game theoretic models. Extending game theoretic models by way of nonlocal modeling will allow me to study the dynamics of these systems and their equilibria with a more powerful set of tools to describe things like regularity and domain effects on equilibrium dynamics. Moreover, I will leverage the existing literature on nonlocal numerical methods and nonlocal boundary value problems to tackle questions about how to simulate high-structure multiplayer games efficiently and how to think about these games fitting into larger systems, making them far more accessible than has been possible with the current, traditional methods.

My research seeks to push the boundaries of dynamic game theory and game theoretic modeling through the innovation of new modeling techniques and the application of applied dynamical systems to existing game theoretic models in a wide variety of application areas. I am excited to continue my research in high-structure multiplayer dynamic games, but I am even more excited to see the way that new collaborations, both within math and in many new application areas, shape my research in the future.

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