University of Zurich[UZH]

UZH Department of Economics

Blümlisalpstrasse 10
CH-8006 Zürich
http://www.econ.uzh.ch

Marek Pycia
Professor of Economics
Chair of Organizational Economics
marek.pycia@econ.uzh.ch

Zürich, 2019

Report on the PhD Dissertation of Rajani Singh

Summary Assessment

The dissertation studies optima and equilibria of a class of dynamic games. I think that it clearly demonstrates a PhD level competence. There is some interesting material here and some of the results are already published in two papers joint with Agnieszka Wiszniewska-Matyszkiel, one in Topological Methods in Nonlinear Analysis, 51:23–54, 2018, another in IEEE Transactions on Automatic Control (forthcoming).

I recommend moving to the next step of the PhD granting procedure.

Disciplinary Differences

Before a more detailed discussion let me comment on my perspective. My background is in game theory and its applications as understood from the perspective of currently mainstream economics. As far as I can say this is not the perspective taken by the PhD candidate who studies at a department of mathematics. This difference of perspectives matters for the interpretation of my comments because writing them I maintain the perspective of an economist because this is the place where I am qualified and also because I think these are the type of comments I can make that are of most use to the PhD candidate.

Please keep this disclaimer in mind when reading my detailed comments. E.g. the game theory literature as I know it is pretty much ignored in the dissertation and I comment on this while discussing the literature overview presented in the paper. Despite that the literature positioning seems arbitrary to me, I assume that it makes sense from the perspective of the candidate. Chapter 3 provides the main illustration of the differences between economics and mathematics; I discuss it below.

Content

The dissertation focuses on the following problem: consider a set of parties that can fish in a fish reservoir in time periods 1, 2, 3, etc. In between time periods the remaining fish multiply. The fishing parties optimize a discounted sum of their in-period utilities (the in-period utility is quadratic in the amount of fish caught that period). The dissertation analyzes both the non-cooperative problem (aka the Nash equilibrium) and the cooperative problem (aka the optimal solution).

This is a fairly canonical perfect information dynamic game with a state variable (the quantity of fish in the reservoir). In general such games have lots of equilibria—basically anything that satisfies both feasibility constraints and provides the participants with a minimum sensible utility can be supported as an equilibrium. Implicitly for this reason, and following a long tradition in studying of such games, the dissertation focuses on Markovian equilibria in which each participant's fishing decisions depend only on the quantity of fish available but not on the time of the fishing.

Chapter 1 presents some broad discussion of game theory, and the substantive results are in Chapters 2-6.

Chapter 2 studies a basic model as above. There is no literature discussion and I am not sure what in this chapter is new. I gather the key insight is encapsulated in Corollary 13. Putting aside my concerns about the validity of the proof, the insight is a correction of some sloppiness in unspecified papers studying the same or related models.

My concerns about the proof of Corollary 13 goes back to the proof of Theorem 10. There I have run across some confusion between sufficient and necessary conditions (I provide details below). While I hope it is me who is confused and not the author, I don't have confidence in this proof. It is of course only one of many results in the dissertation and the issue does not substantially affect my overall positive assessment.

Theorem 18 in this chapter shows that value functions in a problem studied there in does not belong to a certain class of functions. In the dissertation summary and in Chapter 4, this result is then referred to as a counterexample to the Guess-and-Verify method of finding solutions. I am at a loss what to make of it. How can one find a counterexample to Guess-and-Verify? Theorem 18 shows that a certain guess is wrong but I don't see how we can interpret it as more than this.

Chapter 3 shows that the strategies in a two-player two-period version of the above game can be discontinuous. This is achieved by taking the math of the fisheries model at face value beyond its usual applicability. In standard fisheries models the more fish a fisherman extract, the higher the fisherman's utility. Because of the focus on monotonic utility, only certain parameter range of the quadratic utility formulation is usually considered—precisely the part that gives us monotonic utility. With monotonic utility, the strategies in the extraction problem are continuous.

The dissertation breaks this standard assumption and consider an environment in which too high extraction levels decrease utility. While the dissertation doesn't discuss it, this range of parameters—usually ignored in the literature—is of actual independent interest: it captures the fact that disposal of fish is not free. In the paper it is combined with a somewhat less natural (but also relevant in some environments) assumption that all fish need to be extracted from the fishery in the second period. The latter assumption is embedded in the terminal condition (unlike in standard finite horizon games with increasing utility this condition is not redundant for the purposes of Chapter 3).

While I like the focus on costly disposal (without taking a stance whether it is new or not), as an economist I find the discontinuity reported uninteresting. I don't find it surprising given the non-standard assumptions of non-monotonicity of utility and the need to leave the reservoir empty at the end of period 2. The total utility as a function of quantity extracted in period 1 is then the sum of an n-shaped utility in period 1 and u-shaped utility in period 2. If we add them up, we can easily get a total utility which goes up, then down, and then up again, creating discontinuities in optimal strategies. Such discontinuities are quite common in some classes of economic problems (searching for ironing would bring up examples).

The dissertation claims that there is a common belief in continuity of this solution. While the dissertation does not provide evidence for this claim, I am willing to take it at face value. (This lack of literature discussion is a broader issue: in general the chapters in the dissertation are not very precise in how they engage the literature).

I think however that we run here into a clear difference of perspective between mathematics and economics. From the perspective taken in the dissertation this common belief is then wrong because for a slightly different set of parameters it is not true, and pointing this out is of crucial methodological value. From an economic perspective this is nothing more than misunderstanding the model at hand: for an economist changing the parameters in a quadratic utility so as to make the utility non-monotonic means we left the models all those claiming continuity study and we are entering a completely different model. In this different model the presence of the discontinuity is a fairly straightforward and familiar phenomenon; no surprise there. In other words I am guessing that the disconnect happens because the author has a very different understanding of the papers she tacitly criticizes than the authors of these papers or the economists reading them. (As an aside: the criticized papers should make the parameter range assumptions explicit and I suspect many do so; other criticized papers might be sloppy or might consider the matter so obvious as to not be worth mentioning.)

Chapter 4 resembles Chapter 2; it relaxes a particular balancing assumption on discount rates and restricts attention to single-agent decision problems.

Chapter 5 is in my opinion the most interesting part of the dissertation. It shows that in a class of fish extraction problems studied therein, the strategies are interestingly insensitive/ robust to errors in calculation of value functions. I think it would be interesting to try to understand what drives this robustness, on a more conceptual level, beyond the calculations of this chapter. With the caveat that I am not an expert in numerical methods, if the resulting point is new it could be quite influential.

Chapter 6 solves a version of the model in Chapter 2, but now with continuous time.

Further Comments

I am including below some more specific comments. None of them needs to be introduced in the dissertation, which I think fulfills the PhD requirements in its current shape. They are intended as help in future work or in publishing the not-yet published parts of the dissertation.

Abstract: “consumers” are usually understood as many decision makers not one.

"Correcting previous results"—the dissertation claims in several places to correct things but none of these places convinced me any correction was needed.

"There are quite a few results in nonzero-sum dynamic games and if the constraints appear (...), then the results are very rare." The claim strikes me as obviously false. There are multitudes of thousands of papers on nonzero-sum dynamic games with constraints.

Chapter 1. In the context of the Bellman principle—I think that the validity of the Bellman approach was for the first proven in Ralph Strauch's PhD dissertation.

The claim that "Dynamic games are the only appropriate tool to model decision making" strikes me as too strong and not needed.

The claim that "Linear quadratic dynamic games seem[] to be the best researched class of games" is almost surely false, and again not needed. There are many well-studied game classes.

Page 3 paragraph on Reddy and Zaccour is hard to parse. Are we now restricting attention to their work? Also, no specific results were mentioned in the preceding paragraph which makes parsing the claim that they cannot be applied hard to assess.

Page 3 classification of the literature of dynamic games leaves most of the literature unaccounted for. (And evolutionary game theory is not all that recent). From my perspective the selection of which parts of dynamic game theory is included is fairly arbitrary, and the selection of papers illustrating the divisions is (with some exceptions) even more so.

Even when narrowed down to the questions of the extraction of common resource, the focus of the dissertation, the literature review omits both the leading mainstream game theorists who worked on related topics (e.g. Lloyd Shapley, Edward Green and Robert Porter, George Akerlof and Janet Yellen, Herve Moulin, Drew Fudenberg, and David Levine for extraction/ public good games and e.g. Albert Kyle, Dirk Bergemann, Stephen Morris, and Xavier Vives for linear-quadratic games) and such non-game-theoretic luminaries as Elinor Ostrom who received the Economics Nobel Prize for her work on precisely these questions.

Chapter 2. The introduction of the game should be more careful. E.g. symbol s_i is used in points 4 and 5 before it is defined in point 6 (and even there it is defined only implicitly). The symbol s_i is earlier is introduced in Chapter 1 in the context of normal form games, but this is not the context studied in Chapter 2, and the mapping is missing. I wasn't able to fully decode the meaning of s—I am guessing it refers to the realization of S(X)? These basic concepts should be defined, otherwise the reader is left guessing what the story is about. (This issue is treated better in Chapter 6). All this notwithstanding I can with reasonable confidence guess what is being meant from the context of how the symbols are used.

(In point 10: "more interesting"—do you mean "most interesting"?)

I mentioned above a concern about the validity of the proof of Theorem 10 (and hence Corollary 13). The key puzzle for me is that on one hand you refer to the terminal/ transversality condition as the sufficient condition for optimality of the Bellman solution and, indeed, it does usually play this role including in Theorems 2,3, and 4 of Section 1.3.2 on which you built. On the other hand the structure of the proof of Theorem 10 seems to hinge

University of Zurich[UZH]

UZH Department of Economics

on the transversality condition being necessary: in several places you conclude that the terminal condition fails and as far as I can see infer from this that the possibility considered is not an optimal solution. Is my concern valid?

Chapters 2-3. You should explain your terms. What are open loop equilibria and symmetric feedback equilibria? I gather I can look it up in [91] but you are not explicit that you use the terms introduced in that earlier work. Note that these terms are much less standard than lots of terms you introduced.

The language layer in the paper is very good. Some tiny issues I caught at this level:
* in Abstract in the sentence quoted above "and" seems to stand in for "but";
* the sentence on the boundary of pages viii and ix seems off;
* Page 2 sentence on Blackwell "gave" not "given".
* Page 43 "In [91], ..." "the" is missing before "non-existence".

With best regards,

Marek Pycia

Marek Pycia