Marketing Science - Decision on Manuscript ID MKSC-22-0247.R1

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28-Aug-2024

Dear Author:

The review process for your Marketing Science manuscript, MKSC-22-0247.R1, entitled "Dynamic Pricing using Monotonic Nonparametric Bandits," is now complete. I have received comments from a subset of the review team that had processed the paper in the previous round (R2 was not available). Their reports are combined in the attached file.

The reviewers are mostly happy with the progress made in the revision; however, they also suggest some minor revisions. Therefore, I invite you to respond to the reviewer(s)' comments and revise your manuscript.

1. Many of R1’s comments are expositional. This is your paper and I do not want to be too prescriptive, but please make an honest effort to address these comments, which will improve the paper’s potential impact.

2. R1.7 are “must have comments.” I was also a bit concerned about the number of typos in the paper. For example:
-bottom of Page 4: “sign restriction in a different GP space”
-middle of Page 5: “higher expected total rewards than our benchmark algorithms”
-bottom of Page 32: “happened when there were 100 arms”

3. The AE asks that you delineate more candidly the main contribution of the paper, and provides a list of references that should be discussed.

Given the degree of alignment between R1 and the AE, I think it won't be necessary to send the next version to R1, it may go straight to the AE.

Given the above revision task, I hope you will be able to resubmit the paper in three-four weeks. When submitting your revised manuscript, please make sure that you respond to the above comments and the comments made by the review team in a separate document. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewers.

*Starting in May 2022, Marketing Science has implemented a competing interests policy (https://nam12.safelinks.protection.outlook.com/?url=https%3A%2F%2Fpubsonline.informs.org%2Fpage%2Fmksc%2Fcompeting-interests-policy&data=05%7C02%7Cvineet.kumar%40yale.edu%7C7ec4f8ef030d473888dd08dcc7789bad%7Cdd8cbebb21394df8b4114e3e87abeb5c%7C0%7C0%7C638604568105133100%7CUnknown%7CTWFpbGZsb3d8eyJWIjoiMC4wLjAwMDAiLCJQIjoiV2luMzIiLCJBTiI6Ik1haWwiLCJXVCI6Mn0%3D%7C0%7C%7C%7C&sdata=iTFjfswEIkVuJYZw4%2F0IIhzDAKhWWaURD9V6KwPCUDI%3D&reserved=0). Please add a "Funding and Competing Interests" section above the references section and include all relevant declarations here. Please do not include author names as the peer review process is double blind.

Once again, thank you for submitting your manuscript to Marketing Science and I look forward to receiving your revision.

Sincerely,

Prof. Olivier Toubia
Senior Editor, Marketing Science

ScholarOne Manuscripts Guidelines for Submitting Revisions

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Associate Editor
Comments to the Author:
This is a revised manuscript. The authors have made a conscientious effort to address the concerns from the previous round. In this round, only Reviewer 1 was available. This reviewer appreciates the effort the authors have undertaken and is largely satisfied with the revision. The main remaining concerns for this reviewer are being more upfront and clear regarding the contribution, and improving certain parts of the exposition to improve the readability of the paper.

The authors have also largely addressed the concerns raised by me in the previous round. They have broadened the range of algorithms against which their proposed method is tested, they have established the specific gains from including the monotonicity conditions which is the primary novel feature introduced by them. Regards my third comment on initial vs. long-run performance gains they have suggested a variant that resets and restarts the demand learning at set intervals which

can help in environments where the timing of likely demand shifts is known in advance.

I recommend closely following all of Reviewer 1's comments on rewriting the Introduction as well as parts of the exposition. I too found the opening discussion vague regarding the contribution of the current paper and needs to be toned down. Credit to prior work on non-parametric bandits and on exploiting correlation in adjoining prices needs to be to be clearly presented upfront. It is also important to acknowledge that this is a highly active area of research with many recent papers such as the ones mentioned later below. The main contribution of the current paper - namely adding monotonicity conditions - should be delineated more candidly. For example, the abstract makes its appear that correlation in demand at nearby price points has not been considered in earlier work. Similar to Reviewer 1's comment regarding toning down the claims about "economic theory" I did not follow the claims regarding "any domain-specific restrictions... can be incorporated into this framework".

Other work that appears to be relevant are:

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Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author
I thank the authors for responding to my comments. Previously, my main comments were:
a. Positioning the paper against Ringbech and Huchzermeier (2019) and other GPTS papers.

b. Clarify the added value of GP and contrast that against no GP and polynomials.
c. Add theoretical discussions and strengthen simulation results.
d. Discuss, in a more systematic manner, the value of the two information externalities.
These points are mostly addressed. My current comments focus on expositional stuff. In my opinion, the paper still needs significant rewriting before it becomes clear and accessible. So, I encourage the authors to spend time on writing in the next round.

1. On the previous R1.1:

The paper has significantly expanded the scope of algorithms it studies, and hence the positioning has changed and become naturally stronger.

However, the contribution to the literature is not sufficiently elaborated in the introduction. In fact, that section does not cite any existing works, hence making the contribution vague to readers and up to their own interpretation. Further, the literature review section is a bit disorganized and jumps between topics. I encourage the authors to spend significant effort to improve the paper's writing.

2. On the previous R1.2

2.1. I very much appreciate the authors' effort in expanding the scope of algorithms they test, systematically documenting their performance, and giving detailed discussions on the mechanisms that drive the results. These results to me are quite clear and interesting.

I also thank the authors for clarifying that the method is an approximation. I'm okay with this. But the writing is still unclear about that point. The reader might wonder why one needs to estimate the basis functions (why not update like a standard GP). It is useful to spend some time clarifying what one is doing there. Also see my comments responding to the previous point R1.5.

2.2. Thank you for responding to this question.

2.3. I agree the high dimension problem does not belong to this paper.

3. On the previous R1.3:

Thank you for adding the theory. Maybe I missed this, but are these results mentioned at all in the main paper? I would encourage the authors to discuss theoretical properties at least in a paragraph or a small section, and then relegate details to the appendix.

4. On the previous R1.4 and R1.6:

Thank you for adding more simulations and more mechanism discussions. This part is clear to me now.

5. On the previous R1.5:

I thank the authors for responding to this comment. Although, I have to point out that, to me, the paper is still not very well written. I offer some suggestions they can consider in a separate point below.

5.1. Thank you. The algorithms are much clearer now. Although I find it strange to leave the Gaussian process to the appendix, given it is core to many algorithms tested in the paper.

5.2. The comment still applies to the current draft. Section 3 is too loose to serve any real purposes. Maybe it's just me, but I only start to understand what the algorithms are doing when I reached page 21, with quite a bit of guesswork here and there, while having to constantly refer to the appendix.

6. Continuing from the previous point, I have some suggestions on writing:

6.1. Instead of framing section 3 by as what MAB is, consider framing it as a description of what the pricing problem is (a downward sloping nonparametric demand, decision is price, online testing as customers flow in, how reward is defined, etc.).

6.2. Then, restructure most of the current section 3 (where the authors currently discuss in loose terms different "components"), centering the discussion around the two crucial steps of the approach:
- a way to approximate demand (GP, step functions, kernel, etc.).
- an algorithm following which test prices are played (UCB or TS).

6.3. Clearly define what GP is versus no GP (which treats demand as a step function). And then clearly define what UCB and TS are. This is the paper's core, so in my opinion it should be in the paper.

6.4. Then talk about monotonicity restrictions (M), and how it is implemented with GP.

6.5. Then talk about the information externality intuitions from GP and from M.

6.6. Finally, give the algorithm flowchart just as a summary.

This is just one way of writing based on my understandings. Maybe my understanding is still wrong or imprecise. But perhaps the authors can find a way to rewrite the paper and make it more accessible.

7. Additional minor comments

7.1. The word "dynamic pricing" confounds forward-looking pricing decisions, which is a different literature in operations, economics, and marketing. I think the authors' concept of dynamic pricing is closer to "dynamic online pricing" as in Misra et al 2019.

7.2. Here, the word "demand" is used to refer to sales quantity

or demand curve. My understanding is that one usually uses demand to refer to the demand curve as a function, not the dependent variable of that function (which is more commonly referred to as "sales quantity," "quantity," or "sales").

7.3. Weaken the claim "guaranteed to satisfy criteria based on economic theory." Here, the authors' reference of the term "economic theory" refers to monotonicity. Whereas downward sloping a reasonable first-order shape restriction on demand, much of the microeconomic theory makes other micro-founded restrictions on the demand function, such as Slutsky Symmetry (Compiani, 2022, Abaluck, Compiani and Zhang, 2023). The authors are not going into the micro-founded demand estimation business, so I would just suggest they weaken the statement that their demand curve satisfy "economic theory."

7.4. There are still numerous typos in this draft, which is atypical for a revision, signaling that the authors' effort on writing does not match their effort on the analysis.