COMMENTS TO THE AUTHOR:

Reviewer #1: Review comments on "Pay for the option to pay? The impact of improved scientific information on payments for ecosystem services"

The author identifies one major challenge in producing (and paying for) ecosystem services: the uncertainty surrounding the link between the action on a land parcel and the desired ecosystem service outcome(s). It is a completely legitimate concern, and the contention is made that ongoing research improves the quality of ecological information (acceptable assumption). One other assumption that probably needs to be stated at the outset is that the value of a particular parcel may very well be related to the state of other parcels elsewhere. Realizing that the paper is not about such endogeneities, it's ok to proceed, but the implicit assumption that the value for the parcel is separable should probably be made explicit.

I have added an explicit discussion of this point on pg. 4, paragraph 4.

The other feature of the setup is that waiting for future improved information may run the risk that the parcel is developed or otherwise disposed of, foreclosing the option to use it for ES provision. Thus the natural option value context arises where one is willing to pay today to preserve the option to conserve the parcel in the future. A dynamic (2-period) model is used to understand how the improvements in scientific information affect optimal payment today. Uncertainty in both the needed payment to the owner of the land and the ES value is present in the model. Development value and ES value, as well as the signal about ES arriving at time T are assumed to follow a joint distribution. The buyer has the ability to make conservation offers to the landowner in both periods (periods are not temporally explicit). The model does not have an analytical solution but the solution proceeds from buyer's final period offer, to landowner's decision prior to the final period, to the buyer's decision in the initial period.

Allowing for correlations, the author is able to form each of the respective agents' optimization problems using conditional distributions. In particular, in the second step (landowner's prior period decision), a unique development value threshold appears likely. Given this threshold, the buyer's initial period decision can be characterized. An option value (in a weak sense) is established due to the presence of learning about the land's ES value in the future. The direct value of improved future information is clear for the second period. The indirect effect through its impact on the threshold development value is explored in the numerical simulations.

First set of simulations removes the correlation between ES signal and development value. Figure 3 does a nice job outlining cases which map fairly well to inefficiency cases in PES programs. The key insight in this section seems to be that initial policy adjustment (even with a perfect signal) leads a small increase in total expected conservation value.

Explicitly allowing correlation between ES signal and development value. Positive correlation (up to a point) increases the value to the buyer from customizing the first period offer. However, such gains are still quite modest.

An interesting result is that landowners are not responding to low current offers even if they believe that new information will lead to substantially higher offers in the future. To the extent that uncertainly is in the ES value and not in the development value, it appears to be an intuitive result. The author does point out that landowners may be willing to accept smaller current offers if they are uncertain about future development values (as they likely are). In any case, isolating the impact of ES value uncertainty is valuable.

Overall, this is a very carefully crafted (applied) theoretical paper that does a very nice job parsing the many critical regions implied by the model and provides a good way to bound the effects of future information on tailoring today's offers for a wide range of relative parameter values. I think it's an effort that once again highlights that even on the face of it simple setups can hide a fairly rich set of implications. I think this work does have a place in the literature on conservation under uncertainty.

A few small suggestions to improve:

1) motivate the conservation setup. A two-stage conservation is envisioned, could you give an example? Temporary easements or CRP contracts may help in adding to the motivation part.

I have added additional discussion about the motivation for the two stage set-up. As I noted in the text, while CRP provides a nice example of the timing, the model presented here does not apply to CRP because CRP pays farmers to remove land from agricultural production and restore it to a “natural” state. CRP is thus an “asset building” program and the irreversibility that drives the effects in the model presented here does not apply in the CRP setting.

2) can you provide a better interpretive argument for the parameter choices to argue that the cases presented likely cover the realistic relationships present between conservation and development values?

I have added a new section to the text describing how the parameters from the model relate to several existing ecosystem service payment plans. I have also added additional statements throughout the model development to tie the model variables more closely to their real world meanings.

3) p. 11, second paragraph "…optimal first period offer and how large valuable adopting…" 'large' to be deleted?

Change made as requested.

4) p. 12, first line: buyer's

Change made as requested

Reviewer #2:

The authors use a two-period dynamic stochastic optimization framework to analyze how uncertainty in availability of information pertaining to the conservation value of the land affects the decision of the conservation planner today. While the approach is novel and the numerical simulations provide key insights, this study lacks a case study application to motivate and better explain some of the findings. Additionally, the manuscript also lacks details on how the numerical simulations are carried out. Also, in its current form, the methods/model and results are mixed together and it is difficult to clearly follow some of the key assumptions and approaches the authors used to arrive at the results. Below I provide more detailed comments and suggestions:

1) define CARA risk preferences the first time you use it

Thank you for noticing this oversight. I have replaced the abbreviation with the full description since it is not used again in the text.

2) It is a little difficult to distill the key results of the study as the manuscript is currently presented. It would help to clearly separate the methods/model section from the results section. For example, in section 2.1 (page 7), the 3 paragraphs following the text: "from this figure, we observe several effects....." can be moved to a separate section (e.g. "results") and some of the text in section 3 and 4 that highlight the key assumptions can be moved here.

I appreciate this concern about the separation of the model and the figures describing the predictions of the model. I have re-organized the sections of the paper keeping the model itself in Section 2 and combining all of the discussion of the model’s predictions in a single section with several subsections. Moreover, the text now emphasizes that these figures are exploring the predictions of the theoretical model under a variety of parameters.

3) Please put in main headings (e.g. Results) for section 3 and 4. Also, maybe section 3 and 4 can be combined as sub-sections under one main heading.

See previous comment. I have combined all of the predictions into a single section and used more descriptive subsection headings.

4) One of the limitations of this study is restricting the conservation buyer's choice of optimal use of the land in conservation only when "e<0". In reality, there may be situations when conservation is important even in scenarios when "e<0". While I understand the necessity of such assumptions to develop a tractable model, it is important to point out this limitation and what intuitions your results can provide for such cases (if any).

I have added additional detail into the manuscript explaining the meaning of the variables in more depth and illustrating this through a discussion of how the model in the paper relates to existing descriptions of several real world PES programs.

Given the definition of the variables in the model, *e* represents the true benefit of conservation over development, accounting for all possible benefits (to the buyer) of conservation. If a parcel provides different types of ecosystem services, as is common, *e* represents the sum of the value the buyer places on each individual service, less any value provided by the parcel to the buyer in its developed state. So if conservation is important to the buyer (i.e. more desirable than development), then according the definitions in this model, *e*>0. Part of the reason this value is unknown to the buyer is precisely because it is very difficult to measure all the possible benefits a parcel may provide.

As the paper now makes clearer on pg. 4, if *e*<0, this means that the buyer of ecosystem services will receive more benefits if the land is developed than if it is conserved. This option exists in the model to reflect the reality that not all buyers of ecosystem services will want every parcel developed. The text now makes it clearer (on pg. 5) that while there may be other benefits of conservation that accrue to someone other than the buyer, these benefits would not be considered.

5) Figure 2 is missing labels for x-axis; also the title of Figure 2 seems incomplete (the Impact of Changing Parameters on what?)

The revised figure includes an *x*-axis label (on the bottom row) and is now titled “Impact of Changing Parameters on the Marginal Benefits and Costs of Increasing the Final Period Offer”

6) Applying this to an actual case study would make some of the results more tangible for the reader. Even if it is not possible to apply a full case study, it would benefit the reader if many of the assumptions that the author makes are motivated with real life scenarios (wherever possible)to make the methods and results less abstract.

Applying the model to a specific case study is challenging because of the large degree of uncertainty about parameter values that drives the model motivation. However, I have adopted your suggestion of motivating the model and the definition of terms by drawing parallels with several real world PES schemes. I have also included a new section (Section 4) that discusses how the results relate to existing PES programs.

7) I find a few instances of typos and grammatical errors. Please proof read the manuscript carefully.

Thank you for pointing these out. I have corrected any typos and grammatical errors that I found and hope that I have caught them all.

8) Many of the figures are not self explanatory. Please provide axis labels wherever missing and also clear titles and captions to explain the figure. For example, in Figure 8, it is not clear from the title what does the gain (shown on y-axis) refer to. There are similar issues with Figure 2, 5 and 6.

I have updated the labels and titles of most of the figures in the text.

9) Please provide more details about how the numerical simulations were carried out either as a separate sub-section under methods or as part of the appendix.

I have created a new section in the appendix describing how the numerical results were generated and created a zip file containing all of the Matlab code used to generate the results that I will make available to readers upon request. Please let me know if you would like the zip archive at this point.