Associate Editor Comments to Author (Professor Chris Chambers):
Associate Editor: 1
Comments to the Author:

I have now obtained evaluations of your Stage 2 submission by three of reviewers who reviewed the Stage 1 manuscript. As you will see, all are positive and the manuscript should be suitable for final acceptance following minor revision to consider additional discussion points.
Concerning R2's point about H5 being dropped, as this happened during the Stage 1 review process no further comment is necessary (it is not a protocol deviation).
Concerning the stylistic points about tenses and method content: please do update the tenses from future tense to past tense and I also recommend reporting in the method section was actually done rather than what was planned. Where these differ, it a footnote should be added explaining what changed. However, please avoid major structural changes (e.g. reporting of pilot data should be maintained).
I look forward to receiving your revised Stage 2 submission and response.

We thank you for your comments and clarifications. We have adjusted the tenses throughout the manuscript as suggested, and have updated the methods section to include what was actually done. In particular, we have spotted one or two places where the modelling methodology was slightly different compared to what was originally written in the Stage 1 manuscript. We have indicated these differences with footnotes, but if you have any further comments or requests for improving clarity here, we are very happy to adjust as necessary.

Comments to Author:
Reviewer: 2

Comments to the Author(s)

The Introduction, rationale, and stated hypotheses are identical to the Stage 1 submission, so I do not need to comment on them again. The authors appear to have carried out the experiment and analysis as planned. The simulated data analysis in the Stage 1 submission indicated that the experiment should have the power to detect the predicted effects if they exist. Interestingly, the primary hypothesis (H1), that participants would show a preference for selecting scarce targets, was not supported by the data. While there were weak trends the expected direction, the evidence for a scarcity effect did not reach the pre-specified threshold, either in the dataset as a whole or in the feature and conjunction conditions separately. Of the three secondary hypotheses, two were supported by the data. The H2 hypothesis was that foraging behavior would be "stickier" in the conjunction condition, as indexed by a higher value of the parameter b_s, and that was observed. Similarly, H3, that there would be a larger proximity bias (σ_ρ) in the feature condition

than in the conjunction condition, was supported. However, the authors also predicted that the σ_ρ would be >10 in both conditions, which does not appear to be the case from Figure 4; the σ_ρ value for the conjunction condition is < 10, which is not commented on by the authors. H4, that the relative direction parameter would be negative, was not supported. Hypothesis H5, that target-by-target predictions would be more accurate in the feature condition, seems to have vanished from the manuscript.

Thanks for noting we did not comment further on H4 - we have now done this, both in the results (L493) and the discussion (L623).

For clarification, H5 was removed during the original Stage 1 review, predominantly because we found it tricky to fit this question in the Study Design Table recommended in the initial round of reviews, and so we decided it was not a good question for a pre-registered hypothesis.

The authors conclude that the "scarcity effect" observed in other domains (e.g., evaluating the taste of yogurt – Sehnert et al. 2014) may be primarily a phenomenon of higher-level cognition, rather than a low-level attentional bias to rare items. Another, closely related possibility might be that the scarcity effect may be generated by top-down expectations about scarcity, rather than perceptual estimates of prevalence. This may seem like a restatement of the authors' point, but it is subtly different in (to me) interest ways. In this experiment, participants are not told which stimulus is scarcer in the display (as far as I can tell from the method), they simply can see in the display that there are fewer green circles than red circles; even if they cannot tell at a glance which of the conjunction targets is scarcer, they will quickly experience scarcity. In contrast, my impression is that in the social psychology experiments that inspired/supported Commodity Theory, scarce items are defined by the experimenter ("this is scarce, this is common"); I could be wrong, having just barely scratched this literature. In any case, my point is that this distinction corresponds to the distinction between top-down expectations and bottom-up experience in the prevalence literature. Since these constructs have different effects on search behavior in prevalence experiments, the same may be true in foraging. While scarcity effects might be "higher-level cognition" in nature, they may also have effects on "low-level attentional bias" if manipulated in the right way, for example telling people that a certain target is rarer in general as opposed to having the target be rarer on the screen. This may have important implications in domains like medical image reading, where a given target might be "scarce" in that it rarely shows up in the population, yet "common" in a particular image (e.g., a patient with a rare type of tumor all over their lung).

Thanks, this is a really interesting point, and we now discuss it in the manuscript (see paragraph beginning L547).

The authors also point out that this project proves the power of their novel modeling approach, which is perhaps the more important finding. I think it might be worth a little speculation on other problems that could be approached with this method.
Thank you - we have added some further comments on the broad utility of this modelling approach (see L649).

Todd Horowitz

Reviewer: 3

Comments to the Author(s)

The preregistered report stage 2 is fully in accordance with the accepted stage 1 manuscript. In particular, the rationale, hypotheses, data, and analyses, are the same as the approved stage 1 submission, and exploratory analyses are informative and have been deemed justified. The conclusions are justified given the data.

I must say that I was surprised that the analyses revealed no evidence for a scarcity bias. However, the different analyses conducted by the authors and the convincing discussion of the results provide interesting insights for future research on human visual foraging. This is a solid piece of work.

The only concern I have regarding the manuscript is its clarity/organization. I'm not very familiar with the Registered Reports format, but I feel that some sections were useful in stage 1 but could be removed in stage 2. I am specifically referring to pp.5-14, where I would suggest directly describing the final methods rather than focusing on what was planned in stage 1. For example, I find it confusing to wait until the results section to get information about the final number of participants and their demographics. Similarly, there is redundancy between the "planned analyses" and the analyses and hypotheses presented in the results section. I believe it would be clearer to have a single "data analysis and hypotheses" section placed somewhere between the methods and the results section.

Additionally, is it necessary to keep the simulated and pilot data sections in the final version? In stage 2 I would suggest focusing only on the obtained results, especially since the accepted stage 1 manuscript (where all this info is provided) is linked in the paper.

On a similar note, in the abstract and methods section, I find the use of the future tense confusing. Now that the study has been conducted, I believe the past tense would be more appropriate.

We have been asked by the Editor to avoid major structural changes. However, we have adjusted the tenses accordingly and hope this improves the reading experience somewhat.

That said, again, I'm not very familiar with the Registered Reports format, so if the Editor has other recommendations, I apologize for this potentially unnecessary long comment!

In what follows, I share some thoughts and suggestions, just to ensure we haven't overlooked anything in the data:

1/ Regarding the absence of evidence for a scarcity bias: could it be that participants need time to learn which target is scarcer? My suggestion is to analyze the time course of the scarcity effect across trials. This could be investigated by separately analyzing the first five and last five trials for each condition and participant, to see if a potential scarcity bias emerges in the last half of the experiment.

Thank you for this suggestion - we have now tried this analysis but didn't see any stronger effects in the later trials. Full details are available in the supplementary materials and in section 5.5 of the main paper (L515).

2/ In the conjunction condition, the squares are "rotated". I understand that there is a trend in the data suggesting a preference for red squares over green circles. Could this be related to the "uniqueness" of each rotated square in the display? In a given trial, there is only one square per rotation angle, while all circles have the same orientation (since they can't be rotated). This uniqueness of squares might attract attention over circles. To test this hypothesis, are there any trials or conditions with green-square targets? If there is no preference for rotated-green squares over red circles, then my reasoning may not hold.

Unfortunately, we did not test any trials or conditions with green-square targets: in general, the exact shapes/colours of targets have not been of primary interest in previous foraging experiments. However, we agree with your suggestion that uniqueness may be important, and have added some discussion on this at L599.

3/ On P.4 of the introduction, the authors describe research from Shen et al. (2000) and Sobel and Cave (2002) showing that the distractor ratio affects search performance in single-target visual search, and they claim this reflects a 'scarcity bias'. However, I'm not sure this is fully comparable to the manipulation of target scarcity during foraging, since target selection and distractor inhibition may rely on different mechanisms. Actually, this may be a nice way for you to follow up on this study: would the results change if distractor scarcity, rather than target scarcity, were manipulated during foraging?

We completely agree that manipulating the ratio of distractors is not fully comparable, but wanted to highlight that some of the findings in this literature might have similarities to ‘scarcity biases’. We agree that looking at manipulating distractor ratios would be a very interesting follow up study!

Reviewer: 4

Comments to the Author(s)

I do not have many comments – this is a preregistration that I previously reviewed and now the authors are reporting the results of their preregistered research. The results look fine and the authors conclusion is that there is no evidence that observers value scarce items more highly than others.

*My main comment is about the writing style. The authors have written this such that they are saying, “we WILL do x and analyze Y”. And then describe the results of what they intend to do. This sounds fine when it is an *intended* data collection, but as a paper it doesn’t work well. I recommend avoiding this back-and-forth time travel, and write this as a more straightforward regular paper: “We did X, analyzed Y, and our results were X”. I think that this would massively improve the readability of the paper.*

Thanks for this comment - we have now edited the tenses throughout the manuscript to hopefully improve clarity.

In the intro, when the authors discuss the arguments of Tagu & Kristjansson, I recommend talking about their point about “selection balance”, where the idea is to determine what tips performance and selection decisions in one way or another.

The concept of the selection balance may be something to think about in terms of the authors interpretation of their results. The authors do not find any preference for scarce items and argue that this reflects that they do not find the scarce targets more rewarding. But could it be that they DO actually find them more rewarding but some other aspects shift the preferred selections?

It is our understanding that the introduction and methods of registered reports should not be modified (beyond changing tenses, or reporting any methodological deviations). However, we have included some further discussion of selection balance as suggested, beginning on L562.