University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

Dear Dr. Snower, Dr. Armstrong, and Dr. Tuckett,

Thanks very much for the opportunity to revise my article "Toward a Cognitive Science of Markets: Economic Agents as Sense-Makers" for *Economics*. The reviewers both provided detailed and insightful comments, and I have endeavored to incorporate as many of their points as possible. As a consequence of the reviews, I significantly restructured the paper and highlighted a number of points emphasized by the reviewers. My sense is that this has substantially strengthened the article.

Below are my specific thoughts on the reviewers' comments.

Reviewer #1 (Magda Osman)

> (i) Is the contribution of the paper potentially significant?
> I can see that the submission makes a reasonable contribution to a broad readership. The main point it makes is that Economics and early psychology (behaviourism) have had their problems. More specifically, the piece makes the argument that there are limitations in both because they simply look at observable behaviour and just look at the conditions under which behaviour is shaped and maintained to make inferences about how the two are related, without saying anything about the contents of the black box, namely the mind. This isn't a new argument (e.g., Meder, Le Lec, & Osman, 2013), but the added value of the piece is that it takes an interesting narrative position in describing where the limitations have existed in psychology and economics in order to advance the argument that cognitive science and behavioural economics are the way forward. The point being that both cognitive science and behavioural economics have much to gain from each other, and should make greater efforts to converge in their methods and theoretical/computational modelling approaches. By framing the piece in such a way as to introduce the disciplines to each other in the context of dating is a novel spin, though on this note, there are many sentence where the author takes some artistic license that forgoes the accuracy of the details for an entertaining and engaging characterisation of the disciplines it discusses.

I am delighted that the reviewer found some of the paper's arguments to be interesting and novel. However, I am actually making a stronger claim than I think the reviewer may be recognizing, namely that *behavioral* economics itself—at least the versions practiced by most economists—has inherited many of the bad habits of (neoclassical) economics that limit the explanatory power of that science. Many behavioral models are just neoclassical models with one or more assumptions relaxed. This is absolutely defensible from the perspective of maintaining continuity within economic science, which I do not think we should be ditching by the wayside. For understanding much 'normal' economic activity I suspect these models provide great insight. But what I *am* saying is that in cases where existing neoclassical models don't give us much of a starting point—e.g., in cases of Knightian uncertainty or biased mental models—these tweaked models aren't going to give much more insight because they don't open up the black box of the mind. I've sought to clarify earlier on that my target is not just neoclassical economics here but also much of behavioral economics that suffers from similar problems.

As for areas of artistic license, as I explain below I have restructured the paper significantly and—for better and for worse—it is no longer so artistic. I believe the concerns about historical accuracy should no longer be relevant as I have removed most of the historical references from the article.

> It is a long piece, and it attempts to cover a lot of ground, but it might be best improved by being a little more focused. One way it might achieve this is by taking a phenomenon that has been explore in economics and in psychology, and to show what the consequences have been in taking an approach that focuses only

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

> on observable factors (e.g., conditions of the environment, choice behaviour). From this it can then illustrate how research in behavioural economics or cognitive science (though I'm not sure how that would be achieved since cognitive science spans many disciplines, and there are new off shoots such as Decisions Sciences which do the job of the what the author is proposing in his concluding section) has helped mitigate the limitations because they provide richer models for predicting behaviour based on models that precisely characterise the internal cognitive process that guide behaviour. Doing something like this might help the reader see how the different areas of research that the piece covers (e.g., heuristics and biases, moral psychology, causal cognition, evolutionary psychology, cognitive constraints, developmental psychology, decision-making under risk/uncertainty etc...) specifically inform a set of stylized facts in economics/psychology that have been hampered by a narrow focus on observable.

Thanks for the frank feedback on this, which is related to a comment from Reviewer 2. This was a tough call for me, because I actually quite liked the paper's previous approach of making broad points about the relevance of cognitive science for economics, and then going into quite a lot of detail in a few areas. However, in light of both reviewers' comments, I now think the more 'artistic' section may actually be more suitable as a standalone opinion piece and have therefore reorganized the article quite a lot. The three sections on the different sense-making tools have been retained with relatively minor changes, but I added a section to the introduction describing the three main economic puzzles addressed by the underlying cognitive science—decision-making under Knightian uncertainty, economic booms and busts, and voter psychology over economic policy. I also added a new paragraph to the first two sense-making sections (on heuristics and stories) tying them more explicitly to these issues (this was already done quite explicitly for the third section, so this was left mostly unchanged).

> The concluding section makes the point that the where cognitive science and behavioural economics might usefully converge could lead to the "cognitive science of markets". It isn't clear what role macroeconomics plays in this union, presumably it should be involved in the collaborative relationship being proposed at the end. More to the point, in proposing something akin to a cognitive science of markets, I'm not sure what exactly the objectives/questions are that it would address; it seems like there are other disciplines that are not mentioned would be relevant and contribute to this new discipline, such as social psychology, sociology, political science, management, organisational science. In other words, if we are to understand market behaviour, then it makes sense to have some basic idea of social cognition, and social behaviours, and there are many disciplines that do this outside of the very broad remit of cognitive science. The piece doesn't talk much about social cognition but clearly economic behaviour is informed by social aspects of our behaviour, and if one goes down the route of including moral cognition as the author does, then by extension one is also saying something about social behaviours (e.g., Osman & Weigmann, 2017).

As I've reoriented the piece around the cognitive science of sense-making in particular, the rewrite largely avoids these problems. However, as I still include a brief section on what a cognitive science of markets might contribute, I do add that social cognition is one area of cognitive science that is relevant (of which moral psychology is, I would argue, a part).

> In short, the piece does say some interesting things, and raises some interesting points, but could benefit from being more focused. It isn't quite a review, and it isn't quite a thought piece, or opinion piece, but instead seems to a hybrid of these three. If the author considers going down the opinion piece route, which I think it ought to be, then it is more obvious that the piece is a reflection of what the author's personal take is on various issues, that way the author is somewhat protected from criticism regarding the precision of some of the points made, and the author's depiction of the sciences.

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

As I noted above, I have now clarified the contribution of the article—it is a review article bringing the cognitive science of sense-making into contact with economics.

(ii) Is the analysis correct?
My answer to this has in part been addressed by some of the points I made in response to the first question. However, I can highlight some examples to illustrate the types of inaccuracies I've observed. And, the extent to which these should be addressed depends on the way in which the piece should be revised. As an opinion piece it might be easier for the author to assert that how he is characterising the different disciplines is based on his take, rather than a reflection of how others might see things.
For instance, behaviourism is presented in a significantly unfavourable light.
"But both fields eventually fell prey to behaviorism—the idea that the only ultimate target of explanation is observable behavior, and that internal mental states cannot enter into such explanations—probably under the joint influence of logical positivism, popular in early 20th century philosophy of science." (page 3).
"A consequence of behaviorist tendencies in psychology was an absurdly impoverished vision of human nature as governed solely by patterns of reward and punishment. This vision was demolished by a series of results that demonstrated, even in non-human animals, internal states such as cognitive maps (Tolman, 1948) and motivations beyond material reward-seeking (Harlow, 1958)." (page 3).
Given the different movements in psychology, in terms of key changes in thinking, it is of course true that behaviourism (or some of its forms such as radical behaviourism) was a counter point to movements such as introspectionism (William James), Psychodynamic psychology (Freud), and Gestalt Psychology. But, it isn't the case that these movements have no relevant bearing on psychology today, or other related disciplines for that matter. Behaviourism has given rise to associative learning/reinforcement learning, which is foundational to many models that cognitive psychologists use to model and predict behaviour, and these same models can be found in behavioural economics, neuroeconomics, and in the decision-sciences. Moreover, there are elements of behaviourism that feature in popular areas of interest in behavioural economics and psychology/cognitive science at large, such as behavioural change frameworks (e.g., nudging). In fact, the insights from behaviourism were relevant to behavioural change programs in the 50's, and many famous behaviourists, such as Watson, were employed to provide insights to help develop behavioural interventions that were tested in marketing and consumer research; much in the same way that applied psychologists advise on behavioural interventions that are employed by government today.

The distinct disciplinary boundaries and research approaches that are described in the piece don't reflect the much more opportunistic approach that researchers take in psychology, behavioural economics, cognitive science etc... So, it isn't the case that behaviourism is defunct, it has mutated and is very much alive in current research efforts across a broad range of disciplines. Also, another example of an inaccurate description of the history of psychology and cognitive science is that cognitive psychology through Chomsky wasn't the only trigger to cognitive science. Cognitive science has its origins in other disciplines, and has also mutated (in the direction of decision sciences, or behavioural science) and has become much more inclusive (e.g., subsuming neuroscience and neuroeconomics). Because of this, it might be useful for the author to provide illustrations of where the disciplinary boundaries lie as the author might see them so that the reader has a clearer idea of what the author means, so that they don't have to take their comments on face value without evidential support.
For instance, researchers at many conferences that are badged as behavioural economics are cognitive scientists that are invited to present their work there, and many psychologists that are considered as behavioural economists (e.g, Camerer, Ariely, Loewenstein, Kahneman, Thaler, Sunstein) present at a range of different conferences that are not exclusively behavioural economic/economic in flavour. So, there is more cross speak between disciplines than seems to be purported in the piece. For this reason, I return to the point that I made in response to the first question, which is that it might help the reader if the piece focus on

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

> some core examples to illustrate where mutual efforts to share methodological approaches and theoretical insights between behavioural economics and cognitive science would target the limitations that area currently faced by not collaborating.

I accept many of the historical points made by the reviewer, and I think the current version of the manuscript avoids most of these concerns as that portion of the manuscript has been mostly removed. That said, I don't think the reviewer's comments about behaviorism undercut the central point I was making in that section—that the behaviorism of Watson and Skinner, like much neoclassical economics and (I argue) most of behavioral economics, ignores internal mental states in its explanations.

As an aside, I agree that cognitive science and some parts of behavioral economics are very much un-siloed disciplines that involve heavy cross-disciplinary talk, but to me the central problem is that only a very specific brand of behavioral economics makes its way into mainstream economics—behavioral economics that lends itself to modeling in a broadly neoclassical framework. I think this is fairly apparent if one looks at the behavioral papers published in the top economics journals. Much of this work is very interesting, but it seems to me that a very narrow range of ideas is seriously considered in these places and therefore do not propagate into the mainstream of the discipline (of course, behavioral field journals are more open, but tend to be much less widely read). That said, since I no longer discuss this issue in depth, I think the revision probably avoids these concerns about overstating the extent of disciplinary boundaries.

Reviewer #2 (Richard Bronk)

> I wanted to like this discussion paper more than I did in the end. There is much of interest in it and its avowed topic – the (potential) contribution of cognitive science to (behavioural) economics – is important. But the paper has two flaws: first, it's style and format are uneven – varying from being an amusing and engagingly written opinion piece to a learned review of literature to an exploration of a few of the author's pet research themes. And, secondly, the paper's coverage is so broad that it results both in some questionable generalisations and a few obvious lacunae. I would suggest that any subsequent iteration of the paper should resolve whether it is trying primarily (a) to provide a comprehensive review of academic literature in the field(s), or (b) to discuss a few aspects of the author's own research within an overarching thematic framework, or (c) to provide a literary think piece for a wider readership on the role that has or can be played by cognitive science in expanding our understanding of economics. All three potential papers could be valuable.

Thanks very much for this comment, which was echoed by Reviewer 1. Given the comments of both reviewers, I've decided that the front-end of the paper probably is better-suited as a stand-alone piece and have instead focused more on the sections on the cognitive science of sense-making. I think the paper's new framing is much clearer in its goals compared to the previous version.

> It is also a shame in a paper examining how economic agents make sense of the uncertain future that there is very little discussion of the role of imagination in constructing novel futures (thereby contributing to uncertainty) or of the psychological and physiological drivers of agents' use of contingent social imaginaries (and stories) to help make sense of the future and decide how to act.

To some degree, this is a difference in language and terminology rather than a difference in focus. I talked quite a bit about prediction and mental simulation in the previous version of the paper, which are closely related to imagination. I would also argue that the limitations posed by uncertainty actually should be decomposed into

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

several different categories of cognitive and environmental limits, as I do in the article, since our minds rely on different strategies for circumventing each of these limits. That said, since I agree with the reviewer's general views on this I have clarified the areas of the article that are effectively discussing imagination, and I have explicitly made this point (as well as highlighting some of the reviewer's later points) early in the new version of the paper.

> Let me make a few more substantive comments and criticisms on the paper before outlining my own view about the contribution that cognitive science could make to economics:
> - At least since the ground-breaking work of Kahneman and Tversky (2000), it has become a commonplace to argue that economics has a lot to learn from psychology and cognitive science. In this sense, Johnson is exaggerating how novel his call for the two sets of disciplines to talk to each other is.

I don't think this is quite fair, as even the previous version of this paper acknowledged the existence of behavioral economics early-on, including liberal citations to Kahneman and Tversky, as well as other founding figures in the movement such as Thaler and Loewenstein. As I noted above in my response to Reviewer 1, my target for criticism is not just neoclassical economics, but precisely the behavioral economics that purports to address the problems with neoclassical models. Nonetheless, given that both reviewers thought I was overclaiming on the novelty of my critique, I have now tried to make this point very clearly in the opening paragraphs of the article.

> - Nevertheless, the author is onto something important when he criticises the *positivist* slant of modern economics – its lack of interest in *internal* mental states (because such states cannot be measured or verified from outside), and its lack of interest in the psychological *source* of preferences, biases and, dispositions. Economic models and theories pride themselves on being parsimonious and resting on testable hypotheses; and, as a result, they often resemble the proverbial black-box explanations of behaviour. But the author needs to explain more why this is problematic – because, in important areas, standard economic models have signally failed to explain or predict what is going on, particularly where economic agents are coping with radical uncertainty. The lack of psychological realism of economic models only matters for users of economics because it is implicated in those models' failure to explain or predict economic behaviour in complex and innovative markets.

I agree with the reviewer and have endeavored to clarify this early and often.

> - Johnson is also correct to argue that behavioural economics 'builds off' the 'skeletal vision of human nature' (p4) implied in standard economic models – or, as I put it (Bronk, 2009; Beckert and Bronk, 2018), it contents itself with providing 'bolt on' amendments to standard theory and its core model of agents as rational optimisers. The reason for this is that, by limiting itself to bolting on predictable observed biases and framing tendencies, behavioural economics promises to allow standard economic models (so refined) to remain predictive of future behaviour. In a passage (p4) that appears to refer back to Kuhn's vision of a monstrously complex mature science like Ptolemaic astronomy, incorporating multiple exceptions to a core (false) assumption (Kuhn, 1996), Johnson argues that the behavioural economics programme builds a 'grotesque assemblage of disembodied organs grafted onto the skeleton of *homo economicus*'. More crucially, he argues that by limiting itself to adding qualifications to the core model of agents acting as rational optimisers of utility, behavioural economics fails to build in other positive (indeed, essential) drivers of economic behaviour and sense-making. So far, so good.
> - From page 5 onward, the paper starts to glance at a dizzying array of important intellectual issues without being able to do justice to many of them: collective action problems; the importance of

intellectual pluralism; the relative merits of lab and field experiments (p6); the distinction between risk and uncertainty (p7); institutions and why they matter for economic behaviour (p17); etc. It would have been much more effective to have discussed these issues only in relation to the core themes of the paper – the contribution of cognitive science in understanding the role of heuristics and stories in guiding economic behaviour. So, for example, Tuckett (2011) and Beckert and Bronk (2018) demonstrate why economic agents faced with radical uncertainty (as opposed to risk) – where the agents cannot calculate objective probability functions – are much more prone to relying on simplistic stories or contingent imaginaries and the emotional solace and confidence they inspire. It is above all when studying behaviour in conditions of uncertainty that economics needs to supplement its core models with new ones informed by psychology and cognitive science. If the author had homed in on this topic, he could have demonstrated the need for the two disciplines to talk to each other (and build in findings from his own research) without any requirement to make huge sweeping generalisations about the nature of economics and cognitive science in the round.

This was one of the comments that most influenced my decision to restructure the article—I think there is a lot of truth here. In my re-write of the article, I have zoomed in on a few key puzzles about markets, while still retaining the broad goal of introducing readers to the literature on sense-making in cognitive science. Many of the issues mentioned above—while always intended as illustrations of key points rather than as foci of discussion in themselves—are no longer included as these were from a section of the article that has now been excised.

- I applaud the author's core focus on 'the human capacity to make sense of the world around us', which – given the uncertainty, novelty, and complexity of the environment in which economic agents operate – is both more challenging and imperative than standard economics assumes. And I agree with Johnson's insistence (p10) on the need to understand how we do in fact succeed in discerning clues about what is going on in situations where we are unable either to predict the future in probability terms (as economics assumes) or to update prior hypotheses and probabilities in a Bayesian manner. Much of what Johnson says about the role of heuristics (and the challenges faced in using them to choose between different hypotheses) is useful and interesting. But his discussion does not really integrate the full cognitive (and social science) challenge implied by novelty and radical uncertainty and their mental correlate – the human imagination.
- Johnson correctly mentions that agents must often *create* hypotheses rather than *infer* from them the data before them (p10), and that they operate under information constraints and are often unable to assign probabilities to different hypotheses (p11). What he does not spell out is that these information constraints are not merely due to our shortcomings as knowing agents nor to the complexity of the environment – both constraints that can be somewhat lessened by learned strategies. Instead, the information constraints on economic decision-making are due above all to the *radical indeterminacy* of any economic environment characterised by constant innovation and novelty – works of the imagination – and by the *contingent imaginaries* generated to envisage the reality that might as a consequence emerge (see Beckert and Bronk, 2018). The future is not already 'out there' waiting to be discovered but has yet to be created by how we collectively imagine and will the future to be. To quote Shackle (1992 [1972]), 'What does not yet exist cannot now be known'.
- One of the intellectual mysteries is why economics and psychology have so far largely ignored the role of imagination both as a cause of uncertainty (imagination is the ultimate source of mutations in thought and behaviour) and as our greatest tool for coping with that uncertainty (through the contingent imaginaries and options we construct and share). It is the newly imagined options, shared imaginaries, and contingent interpretations we generate (about how others will react to these novelties) that ensure that the future cannot be a statistical shadow of the past – and that we must seek creatively to make

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

sense of the future as it emerges. It is the human imagination that makes the standard-economics assumption of *homo economicus* – a mere calculating machine – particularly inappropriate. It follows that the great challenge for cognitive scientists who want to help economists make sense of our dynamic economic environment is to show how the human mind generates imaginaries and uses them in conjunction with calculation to generate the conviction to act – see Tuckett (2011) etc.

- One way that imaginaries become determinate enough to coordinate action (and be a suitable object of study by social scientists) is by being crystallised in shared narratives or stories. And such stories are one of the tools of sense-making that Johnson correctly focuses on. There is now a lot of work on the role of narratives in economics – in texts mentioned in this article and well beyond (e.g., Beckert, 2016). Some of this work looks at how government agencies like central banks use narratives to structure the expectations of others (e.g., Holmes, 2014), and there is much interesting work to be done in studying what are the key psychological drivers of the success or failure of group narratives as an expression of political, market, or technocratic power. Above all, though, no psychological account of the role of narratives in helping us make sense of the world can be complete without considering their *social* construction and the *social* roles they play in conferring legitimacy and confidence. There is very little mention of social psychology (or the contribution of sociology) in this paper – beyond telescoped references to herding and social networks (p15).

I agree with many (perhaps all) of these points and have incorporated many of them into the rewrite of the paper, often in prominent locations. That said, I did not always make these central points of discussion because in some cases these take me afield from the intended focus of the article. For example, I agree that social psychology is important, and now emphasize the significance of sociology as well, but a detailed discussion of these literatures falls beyond the (already quite broad) scope of what I aim to cover in this article. My purpose is to introduce the reader to several useful literatures in cognitive science and to identify how they can help shed light on economic puzzles. Just as this does not mean I reject neoclassical economics as relevant to market activity (in fact, I think a large fraction of market activity is probably well-characterized by neoclassical models), I also do not mean to reject sociology as irrelevant. Instead, neoclassical and sociological perspectives get little attention here because this paper is not a review of such models, but instead of cognitive science principles and phenomena as they are relevant to markets.

- One of the most original and interesting parts of the paper discusses how a propensity to zero-sum thinking might be responsible for a number of politically and economically salient preferences (against free trade etc). Here, though, it would be interesting for the author to go beyond discussing why this is important in policy terms to present experimental or physiological evidence for whether or not such a disposition actually exists and why (psychologically) this might be so. Particular care needs to be taken to distinguish *social* (learned) from (innate) *physiological* conditioning of response and preferences.

I am very happy the reviewer enjoyed that part of the paper. In fact, the underlying empirical papers that I describe provide quite a lot of experimental evidence for the propositions here, and I think the previous version of the paper did not make clear enough that many of the points in this section are not speculation but rather a review of specific experimental studies. I've now pointed out earlier in this section that we are reviewing specific experimental studies.

- In this case and more generally, cognitive science will be more likely to influence economics if it is seen to rest – in a market context – on clear systematic evidence and well-understood social, psychological, and physiological processes rather than merely an alternative set of interesting general hypotheses and anecdotally or contingently-observed tendencies. How, for example, has the new science of brain plasticity changed the argument about the reversibility of socially-learned cognitive behaviours in a

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

> market context due to changes in policy or narrative? What is the psychological or physiological explanation for why narrative frames appear to condition expectations and decisions more effectively than undigested available data? Does the evidence suggest that human beings have an innate or socially conditioned psychological tendency to minimise their recognition of the degree of uncertainty they face? Are they physiologically hard-wired to prefer group narratives to constant individual experimentation with diverse explanations for emerging patterns? And is there any consistent evidence for these hypothesised cognitive tendencies? Johnson is right that cognitive science has much to offer economics in the years to come.

I am very pleased that the reviewer agrees with me that cognitive science has a bright future in its future interactions with economics. I fully agree that experimental evidence on these questions is much more informative than mere speculation, which is one of the reasons that I wrote this article—to supplement economists' speculations about the role of (for example) narratives and stories with a summary of the available experimental evidence as to how precisely these processes work. Much is to be done, but I think we have an exciting beginning for developing better theories of market activity.

Reviewer #3 (Ekkehart Schlicht)

> This is a very stimulating contribution offering a treasure-trove of references.
>
> Let me add some comments from my (slightly different) point of view.
>
> I would think that social psychology is as relevant as cognitive science for the problems discussed in the paper. The analysis of "causal forks" and "causal chains" is a central topic in classical attribution theory (Heider, Kelley), which is usually considered part of social psychology. The classical reference would be the book "Social Psychology" by Solomon Asch. Asch points out that cognition and emotion are closely connected. This connection is sometimes lost when focusing mainly on cognition.

Thanks very much for pointing out this connection. I agree that attribution theory is absolutely relevant, and indeed much of the work I reference in the article has grown out of that tradition. While I would argue that this older work is largely superseded by the more precise, newer work that I cite, I agree that it is important to acknowledge the roots of this research tradition. I therefore now include relevant citations.

> You seem not to address the orthodox position of economists regarding the use of heuristics etc., but maybe a paragraph on this topic may be useful. Alfred Marshall, Armen Alchian and evolutionary economists have taken that position. Here it is assumed that people follow routines and heuristics, but they change them: They copy successful routines, drop unsuccessful ones, and vary behavior. So there is variation and selection, and in the end successful behaviors survive. The Friedman-Savage example of the expert billiard player who cannot apply the laws of physics to the movements of the billard balls but plays nevertheless as if could do so, epitomizes this.
>
> I think that this is, theoretically speaking, a very strong argument for side-stepping behavioral considerations in many economic settings, yet I think that it is misleading because there are many examples where we observe behaviors that contradict with that stance. In order to understand the persistence of suboptimal, and even costly, routines, I have urged in my book "On Custom in the Economy" that social psychology (including cognitive elements) have to be brought into economics, very much in the Asch tradition (where sense-making is central, by the way). I attach the preface to the 2018 paperback edition.

University of Bath School of Management
Building 8 West, Quarry Road
Bath BA2 7AY, United Kingdom

Thanks very much for pointing out the relevance of the Marshall/Alchian position to the heuristics debate, and for making me aware of your very interesting work. I've now mentioned this intellectual tradition when discussing heuristics, which I see as broadly aligned with Gigerenzer and company's "adaptive heuristics" view.

I would like to thank both reviewers very much for their extremely thorough and thoughtful comments. Grappling with these comments has, I think, greatly improved the paper.

Sincerely yours,

Sam Johnson