

Responses to referee reports and comments on "How to cope with (new) uncertainties - A bounded rationality approach"

We are grateful for the referee reports and other comments concerning our paper. One adequate response would be a complete revision of its submitted version. Another one are detailed responses of a constructive rather than defensive type. Opting for the second alternative we shall address some (hopefully most) of the points raised. As can be learned from the comments and referee reports that are available along with the paper on the Webpage of this Journal the points raised basically fall into two classes: 1. Omissions or insufficient recognition of relevant other research or arguments; 2. Deficiencies in our arguments and suggested improvements. Let us respond to the two classes in turn.

1. Omissions or insufficient recognition of relevant research

Doing full justice to the literature on decision-making in a single paper is impossible. There must be omissions. Still, some omissions are justifiable while others are not. Moreover, omissions are sometimes a result of ignorance and sometimes deliberate. In sum, four types of omissions may occur:

Omissions of literature or previous research may be:	justifiable	non-justifiable
deliberate	1	2
non-deliberate	3	4

Table 1

Due to increasing specialization, division of labor and knowledge in science it is one central function of comments and referee reports to provide hints to other research that has been

overlooked or possibly misinterpreted in the paper under scrutiny. The present journal's commendable policy of making the interaction between commentators, referees and paper authors transparent to a wider audience induces us to respond in some detail and guided by the four cases.

Case 1. Our discussion of the work of Herbert Simon and Gerd Gigerenzer was deemed insufficient. We do not agree as far as Simon is concerned but concede that our omission of Gigerenzer is presumably unjustified.

The sub-title referring to bounded rationality acknowledges how much the paper owes to Simon. He is a Mertonian intellectual giant on whose shoulders we stand. His influence (and that of Reinhard Selten) is pervasive in the paper. It would have been strange to explicitly draw attention to this influence at every possible instance. In sum, that we left detailed references to Simon out was deliberate and, we believe, justified.

As far as the work of Gerd Gigerenzer is concerned, there are arguments on both sides. Rather than defending ourselves we seize the opportunity to make a few observations concerning the approach of Gigerenzer and his followers.

Gerd Gigerenzer shares with Reinhard Selten the focus on actual decision processes. In this regard both follow Herbert Simon and so do we. As far as the rational in "bounded rationality" is concerned we take it that criteria of what is rational cannot be deduced from a priori principles of pure reason. We believe that a concept of "workable" rationality has to be derived from "best practices". We argue similar to Nelson Goodman's (see (Goodman and Putnam 1983)) approach to explicate induction and also to the reflective equilibrium search John Rawls proposed ((Rawls 1951),(Rawls 1971)). The concept of rationality that we envision is a posteriori and dependent on facts. But it is not a mere description of empirical facts. It is a stylized image of what as a matter of fact seems to work well; where "working well" is meant to imply: first, that there are tested law-like regularities on which decision-support technologies can rely in bringing about

certain consequences and, second, that the results are well esteemed according to standards derived from the *given* aims, ends or values of *real* people.

Decision-makers will use technologies of decision support for improving their practices of decision-making only if the prescriptions of the technologies are reasonably close to their real practices and abilities of dealing with uncertainties. Useful advice (including advice on how to cope with uncertainties) must pick its addressees up where they do rather than where they might or should "stand". Gigerenzer's concept of ecological rationality is in line with this.

However, how well Gigerenzer type heuristics can qualify as decision support when confronted with genuinely new uncertainties seems open. It seems to us that there is a tension between acknowledging that decision problems involve truly new uncertainties and the view that which heuristic or rule is to be applied is ecologically determined.

That for genuinely "new" uncertainties there are no adapted tools is not the fault of Gigerenzer's approach. It is, in a way, an almost analytical consequence of the novelty of the problem and the uncertainty involved. Still, one level up, there might conceivably be heuristics operative in decision preparation that can be used when repeatedly confronted with new problems (the novelty of problems is encountered repeatedly but these are not repeatedly the same or similar situations). Ancient topoi catalogues partly were also meant to fulfill such roles but never really did. If decision makers are repeatedly confronted with situations that are genuinely new a more constructive forward looking approach seems necessary.

To illustrate, if from many possible adaptive strategies with ambiguous relevance the decision ecology suggests to use a heuristic like "take the best" then it will be directly applied to a set of alternatives with uncertain outcomes. Yet truly new uncertainties seem to go beyond this. The practical prescription would be something rather abstract and vague like: "when confronted with genuinely new problems always ask yourself first what kinds of results you do not dare to neglect in your model of the situation you are confronted with". Such a kind of rule ("topos") is one step removed from a rule directly relevant for choice making (like take the best).

It seems to us that ecological rationality and the adaptive fit of heuristics refer to decision-making in situations with certain repetitive traits and sufficient similarity with preceding experiences. Here it is clearly of great value and referring to established practices well adapted to (sufficiently stable) human purposes. We took it that the topic of "how to cope with new uncertainties" was meant to refer to situations for which experience of cases sufficiently similar to trigger adapted responses is lacking. The new uncertainties that we imagined are cases for which there are no "pre-adapted" heuristics for coping directly with substantive problems.

There can conceivably be heuristics of mental model building (as in (Tetlock 2015)) to cope with new uncertainties for which no substantive precedence exists. However, there will always be situations in which the only adequate process will be one of critical reflection.

In sum, we believe to be justified in omitting a more extended discussion of the otherwise highly appreciated research of Simon and Gigerenzer. But we readily admit that we are genuinely uncertain whether the ways of mental model building that might emerge from being repeatedly confronted with new uncertainties should also be classified as heuristics.

If so the exemplary flow chart of the paper (on which we shall comment again below) could be interpreted as expressive of tools or heuristics for dealing with genuinely new uncertainties. Decision-makers are repeatedly confronted with the genuinely new and are uncertain about how to cope with it. Since this happens more than once they may develop strategies one level up that help them to cope.

Case 2. The well-established literature on case based decision theory, CBDT (Gilboa and Schmeidler 2010) – including case based prediction (Gilboa and Schmeidler 2012) – was also deliberately left out. Yet, thinking twice, after receiving early on an email comment – not published on the page – to this effect, we believe that CBDT should have been discussed more extensively by us. CBDT acknowledges that in situations of uncertainty neither the probability distribution nor even the state space are known. In this regard, CBDT does not nurture the illusion that we can hedge our ignorance or can at least marginalize it as practically irrelevant.

In short, CBDT is not subject to our main reservations against conventional ways of coping with uncertainty as if it were risk. We were nevertheless induced to leave out CBDT. First, it is broadly "inductivist" which seems epistemologically problematic to us. Second, it seems well adapted only to situations that are in some ways not genuinely new but similar to situations in memory to an extent that will provide guidance for the new case at hand. Third, and more importantly and generally, CBDT has inherited some of the behaviorism of the rational choice paradigm (as emerged from revealed preference theory).

CBDT seems basically an "externalist" account of overt behavior. Even if it were used to capture cognitive processes (including, in semantic parlance, an intensional concept of preference as an attitude rather than representing it extensionally through "possible worlds") the account would remain externalist. It ultimately treats meaning and understanding of the situation as it appears to the actor as irrelevant (we further comment on the so-called internal point of view and its relation to CBDT and other approaches briefly below).

Despite the preceding reservation, that an "externalist" CBDT account of choice making by boundedly rational individuals in a "large" (genuinely uncertain) world can be given is a major achievement. CBDT can do without the – as we believe inadequate and dangerously misleading -- Bayesian assumption that a probability measure over a full state space adequately captures the essentials of uncertainty. New cases of choice making -- new uncertainties -- are dealt with in terms of similarity to old cases *in memory*. A weighing according to similarity with old cases rather than according to the probability of (sets of) states takes place (the similarity measures giving rise to weighing need not sum to "1"). We appreciate the rather surprising fact that nevertheless a weighted sum representation of decision behavior can be axiomatically characterized.

If decision-makers are informed that some of the axioms that guarantee that their choices can be *represented as if* maximizing a function (in memory-dependent ways) are *violated* in overt behavior then this (external) feedback information may induce choice-makers to revise their *internal* decision processes that lead to the observed behavior. If it is impossible to provide an

externalist representation of choice behavior "as if" maximizing some function (based on probability or similarity weights) then this indicates a lack of consistency in choice making. To the extent that individuals have an interest in consistency of their overt choice behavior they have a "means to given ends" reason to revise their internal decision-generating processes such that consistency emerges. The "technology" and the *precepts* and *prescriptions* that can guide them in their decision-making from their *internal* point of view (if x then y *should* be done) should be altered towards consistency then.

In view of the preceding CBDT can play a legitimate role in improving decision behavior. It can provide information on the level of the choice of rules. But CBDT is not a set of rules of decision-making that can be applied in so-called within rule choices or from an internal point of view (a statement of this classical distinction between choices of rules and within rule choices in economics is (Brennan and Buchanan 1985)). The flow chart presented in the paper is meant prescriptively as indicating to the decision-maker in her first person actor perspective what she "should" do. This perspective certainly could be associated with CBDT as well but is originally alien to it.

CBDT does not prescribe the decision *process* from an internal point of view of the actor.

The preceding may be discarded by some decision-theorists as a subtle conceptual nicety of merely philosophical interest. However, an understanding of the respective roles of internalist and externalist perspectives is rather crucial for an adequate account of cognitive processes as they appear to the decision-maker herself (from her own internal point of view). A reconstruction of this self-understanding is necessary for empirically adequate explanations of what we observe. Following prescriptions is among the *causes* of action and therefore relevant for explanatory as well as normative purposes. (Rule following should be considered a cause of behavioral regularities even though it may on a still deeper level be itself the result of regularities and laws of nature).

In sum, the deliberate omission of CBDT seems justifiable in view of CBDT's relative neglect of the process of generating decisions as perceived from an internal point of view of the decision-maker. But it should be strongly emphasized that CBDT can provide an instructive externalist account of coping with uncertainty without assuming probability weighting over a partition of the full state space.

Case 3 and 4. Konstantinos Katsikopoulos points out that we have not linked our views to what he calls "soft OR" and what is obviously known under this heading. The very existence of the literature to which Konstantinos Katsikopoulos draws attention escaped our attention.

Therefore the omission was non-deliberate. We are very grateful for the hint -- and the more so, since some of the inspiration for our views on bounded rationality derived from OR problems like the so-called secretary problem. In these cases "hard OR" solutions rely on outrageously unrealistic incentives or an unjustified Bayesian setup that we regard as highly problematic.

We should have discussed soft OR. Our complete omission seems non-justifiable. But we are nevertheless rather critical of many aspects of some bits and pieces of soft OR that we looked at. We share the view expressed in the literature that in coping with messy real world problems it is inadequate to be guided primarily by which "hard" OR methods are available rather than by the problems themselves. As can be shown in experimental settings it seems possible to capture fundamental uncertainty in the secretary-search problem without making outrageously unrealistic assumptions. Relying on a concept of satisficing in sampling first and then in search is possible and reasonably successful (see Güth and Weiland 2011).

One should also be aware that even in the most successful scientific endeavors there were long phases in which concerns of mathematical tractability rather than substantive real world problems were guiding research. For instance, as Max Albert reminds us, when Newton tried to apply his general theory to the planetary system he had to start with extremely simple and completely unrealistic models of two bodies moving in interaction with each other (initially fixing one in complete violation of the concept of mutual attraction of masses). Gradually, first

Newton and then others worked the math out. They were guided by what they could handle in a rigorous way rather than by the "relevance" of the problems (see Albert 2013).

Still, rather than modeling the world according to availability of rigorous models it is necessary to be guided by substantive problems of the real world. These problems become more complicated by the fact that the social world is populated by individuals who can understand the theories used to understand and describe it. To the extent that knowledge of the theory of the world would change the world itself independent theorizing is practically very difficult if not impossible (see on theory absorption among boundedly rational actors (Güth, Werner and Kliemt, Hartmut 2004) and generally (Dacey 1981), (Morgenstern and Schwödiauer 1976)).

We acknowledge that in this regard social theories are different from theories about entities that cannot understand the theories about the behavior of the entities. We resent, however, the illusions created by adherents of some kind of superiority of "the humanities" that allegedly command superior knowledge that empirically minded rigorous approaches cannot provide (see on the closely related problem of the limits of expert judgment in politics (Tetlock 2009) and also more generally the modern classic (Meehl 2013)). So-called "critical theory" invoked by some soft OR approaches (e.g. Mingers 2008) is about the worst foe of empirically minded critical thinking. In the end what really matters is nomological knowledge about the world that can be used technologically for the pursuit of given aims ends or values (as required in (Robbins 1935) for rational normative economic argument).

We believe that tools borrowed from the humanities can still have pre-technological uses. They could conceivably lead to prescriptions of how to deal with new uncertainties that are comparable to the suggestions contained in the flowchart of the paper. But eventually the procedures that are meant to help us in forming mental models of the action situation should themselves be put to the test of whether or not they do lead to better results (better according to the given aims, ends, or values of the advisees). Technological claims that certain procedures lead to better ways to deal with messy problems marred by uncertainty must be tested (e.g. in the ways that Tetlock describes for short range predictions in (Tetlock 2015)).

In sum, we are grateful to Konstantinos Katsikopoulos for making us aware that we non-deliberately and non-justifiably omitted a discussion of soft OR.

To the extent that soft OR is not making an effort to show empirically that it can come up with standards whose observance will improve decision quality we are skeptical about it. In this sense we endorse Max Albert's criticism and constructive proposal. We share his view that a critical rational scientific approach provides a model of how to cope with new uncertainties. To this we turn next.

2. Deficiencies of arguments and how to develop remedies

An obvious deficiency of our type of argument is that we do not present sufficient evidence on the workings of the procedure we propose. We propose the flow chart at least implicitly as a remedy for some deficiencies of traditional conceptions of coping with new uncertainties. We agree that "... idealistic ways of conceiving agents are perpetuated because research has very rarely been descriptive of 'in vivo' decision—making processes outside the laboratory." ((Tuckett et al. 2015), 3) We do not offer studies of such "in vivo" processes ourselves (for experiments see the list in section 4 below).

We believe that giving advice of how decision-making entities should cope with new uncertainties we first need to know how they in their best practice do try to cope with them. We did not provide such evidence in the paper. Neither did we provide evidence showing that advice concerning decision-processes can have causal effects on actual behavior of decision-makers and be advantageous for them. As far as the efficacy of advice for improving actual practice is concerned there are some first research bits and pieces. With respect to improvements of practices of coping with new uncertainties one can on the one hand refer to philosophy of science discussions of the kind and role of research and of critical thinking in best scientific practice and on the other hand imagine a research agenda concerning practices of coping with problems for which no established practices yet exist. With respect to the latter the comments of Max Albert seem to be right on target and to raise very relevant and interesting issues.

Already in his Abstract Albert poses the crucial question: “According to G uth and Kliemt, rational decision making is based on knowledge about the causal relations between actions and relevant consequences. Uncertainty is characterized by the absence of such knowledge. What, then, is the basis for rational decision making under uncertainty?” (Albert comment, this journal)

Procedures like those represented by the flow chart must be “good” instruments to reach our ends in situations of uncertainty. We believe that the quality of advice should be tested in the laboratory exactly along the lines Max Albert suggests (some explorative studies on satisficing behavior in the spirit of the flow chart are to be found in the studies listed in section 4).

We fully agree that “In a sense, science provides the paradigmatic case of rational decision making under uncertainty” (Albert comment, 7). Though we have been implicitly thinking along these lines we did not state it explicitly. As Max Albert rightly says it seems desirable and possible to combine the vision of the flow chart with a general critical attitude that is trying to incorporate empirical evidence. But nevertheless the aim must be to structure the critical method and adapt it to the specific context of decision problems of a certain type. It is here that laboratory studies and hybrid studies that combine experiments in the laboratory and the field enter the picture. Though they are still few accounting for exploratory experiments that incentivize scenario generation as well as the formation of aspiration levels and ways to adapt them has been beyond the scope of our paper. It should be emphasized, however, that the “technological view” on science that we like Max Albert endorse is in itself very hospitable to efforts of adapting it to management issues. Those who endorse the vision of developing evidence based management (see Rousseau 2012; EBM as in case of its successful older brother in medicine) should be aware, though, that the project is about methods to gather evidence rather than presenting evidence on the substantive problems and their solution.

The aim is to study what improves outcomes in situations of uncertainty. This is in itself a situation of uncertainty and studies must follow the testing view of critical rational exploration.

Effects of extending and modifying the flow chart of the paper or some alternative process proposal must be explored. It may also well be that Max Albert's hypothesis that at each stage of a dynamic process of model formation general empirical psychological knowledge may be used for "checking the current model for typical biases and modeling mistakes" (Albert 7). To explore how critical thinking may be supported by this or other additional loops in the flow chart is certainly worthwhile. It is, however, always necessary that prescriptions must be implementable by boundedly rational individuals. The flow chart could be amended in many alternative ways and controlled decision-making experiments in a situation of uncertainty could be run with and without the amendment.

A certain lack of examples was also criticized. This criticism is fair enough but we are somewhat skeptical that adequate discussions of examples and case studies could have been provided within given constraints. For instance we were ourselves thinking about Björn Vollan's example of climate change. What can be said within a context like ours can be said only without going into details. Moreover, climate change is not an action problem that could be tackled by a single decision-making entity (like voters who can vote on but none can literally choose the president). The problem is one of model formation in a very special "one off" constellation. Here, as Vollan rightly states, "one important question may be how one can learn about uncertainty when one can only take one irreversible decision and has not really the possibility to learn from others' choices." Reliable evidence can be gathered and hypotheses concerning it can be tested only for the repetitive aspects of problems.

3. Other comments

Let us re-iterate that the approach that we suggest is "internalist". The prescriptive internalist interpretation in particular of the flow chart is alien to the prevailing externalist representations of the results of choice processes in economics. The conventional approaches represent decision and choice *outcomes* rather than the reasons and processes underlying their emergence. This is analogous to the difference between the classical concept of utility – that figured among the reasons for preferring something – and modern utility that merely represents choices or

proclivities to choose and to decide but is itself not among the reasons for preferring. Individuals do not choose an alternative because it has higher utility in the modern sense of that term.

The prescription "maximize your utility" does not tell anything about what to do. Applied to the modern utility notion whatever meaning it has is derived from consistency and the fact that consistent choices can from the external point of view be described "as if" maximizing. As a prescription it is strictly speaking meaningless. The prescriptions that will guide our decision making processes from an internal point of view such that they appear as if we were maximizing utility must be found in the first place.

The best criticism of a purely externalist account of human (rule following) behavior we owe to the legal philosopher Herbert Hart (himself inspired by Wittgenstein). Hart showed in a principled way that it is impossible to account for all relevant behavioral regularities in terms of regularly expected consequences of acts of choice taken separately. Hart's account covers in terms of underlying philosophical principles empirical insights and criticisms of conventional choice modeling as put forward in particular by cognitive psychologists and management theorists, too (see for a paradigm recent example of this genre the programmatic statement by (Tuckett et al. 2015) and in particular the reference to (Katsikopoulos 2014) and the use of the term "idealistic").

For cognitive psychology reasons of risk communication, Pascal's Original Sin of representing uncertainty or the genuinely unknown as if it were risk or partially known should be avoided by all means. If we accept that we cannot know what we once shall know then we cannot have a rational prior over a known state space concerning what can be learnt. When we tried to go back to the very roots of rational decision theory we, at least in a way, came up with more or less the same conclusion as those who like Max Albert fundamentally reject Bayesianism. The admirers of Pascal hedge the deserved admiration for his formal genius by some critical empirical thinking. But if we want to

better cope with new uncertainties we better start by acknowledging openly our ignorance rather than camouflage it by presenting uncertainty as if it were risk.

4. Some explorative laboratory studies

In the comments it was pointed out that references to explorative experimental studies of Güth et. al. concerning several levels of the flowchart were missing or at least hard to track down (in particular on the home pages of the Max Planck institutes of Jena/Bonn). We list some explorative studies chronologically below. It may be worth mentioning that the flowchart grew out of explorative experimentation rather than free speculation.

Fellner, G., Güth, W., Maciejovsky, B., 2004. Illusion of Expertise in Portfolio Decisions: an Experimental Approach, *Journal of Economic Behavior and Organization* Vol. 55(3), S. 355-376.

Güth, W., Levati, V., Ploner, M., 2008. Is Satisficing Absorbable?: An Experimental Study, *Journal of Behavioral Finance* Vol. 9(2), S. 95-105.

Güth, W., 2008. (Non)Behavioral Economics: a Programmatic Assessment, *Zeitschrift für Psychologie* Vol. 216(4), S. 244-253.

Güth, W., Levati, V., Ploner, M., 2010. Satisficing in strategic environments: a theoretical approach and experimental evidence, *Journal of Socio-Economics* Vol. 39(5), S. 554-561.

Güth, W., 2010. Satisficing and (un)bounded rationality: a formal definition and its experimental validity, *Journal of Economic Behavior and Organization* Vol. 73(3), S. 308-316.

Güth, W., Kliemt, H., 2010. (Un)Bounded Rationality in Decision Making and Game Theory - Back to Square One?, *Games* Vol. 1(1), S. 53-65.

Berninghaus, S., Güth, W., Levati, M., Qiu, J., 2011. Satisficing search versus aspiration adaptation in sales competition: experimental evidence, *International Journal of Game Theory* Vol. 40(1), S. 179-198.

Güth, W., Weiland, T., 2011. Aspiration formation and satisficing in search with(out) competition, *New Zealand Economic Papers* Vol. 45(1/2), S. 23-45.

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