Reply to referee report #1

Thank you very much for the long and detailed report. However, I don't agree with most points of critique but they show me that I have to make my arguments more clear.

- **Purpose of the paper**

  I never said that one purpose is a restatement of Keynes's liquidity preference theory (= point (ii)). Exegesis of Keynes and exegesis of the broad exegetic Post-Keynesian literature is clearly not my ambition. The referee is right when claiming that point (iv) is crucial: If existing formal representations of hard uncertainty would be convincing then there is no need to put forward an alternative view. That's trivial. Since my critique is very general (section 2) there is no need to discuss these approaches in all mathematical details. Target (iii) is indeed the main purpose of the paper. Aim (i) is mentioned only in the concluding remarks and has more the character of an outlook. It is stated that the approach aims to incorporate uncertainty into macro models, it is not the purpose of the paper to do that. Hence it is a bit unfair to impute this to me. It is not complcated to incorporate the behavioral heuristic $\lambda^a(\beta)$ (p. 10) into a model of financial markets as a part of a macro model. Why should this be "wishful thinking"?

- **The term "fundamental uncertainty"**

  The referee uses the term exclusively in quotation marks in order to indicate that I use the term in a "wrong" way. I accept the point that I should be more precise in disentangling the different notions of uncertainty. However, much of the existing literature (partially cited in the paper) have to come under the same critique since verbal interpretations of Keynesian, Knightian, fundamental or hard uncertainty are often imprecise if authors are primarly interested in formal representations, while such authors who are verbally very precise (or fussy) usually didn't provide any modeling advice. The referee claims that "this is not a terminological issue". I do not fully agree. The pragmatic question is whether we gain important insights and modeling advice when we follow a specific terminology as suggested on p.2 of the report, or whether this ends up in a controversy about who is more Catholic than the Pope. If "fundamental uncertainty" is exclusively defined as the Keynesian case of $w = 0$, then it describes the artificial case of zero knowledge, zero data (or zero memory) and hence zero considerations about reasonbale decision making. Is this case worth of any theoretical discussion or theorizing? Or should it be treated as an artificial and uninteresting border case of what the referee calls "hard uncertainty"? Since my main point is that the agent is by principle not able to *know* the underlying structures, the parameters, and distributions of randiom variables since his knowledge *must* be treated as incomplete (in absence of a "true" model), and therefore he should *not* completely trust his own beliefs, the character of uncertainty is sufficiently clear. I regret that the referee was "unable to find a clear definition, explicit or implicit" of what I call fundamental uncertainty, but I am not able to relate to this critique. If the reader prefers to call this "hard" uncertainty instead of "fundamental", I have no problems to replace the words. But I doubt whether this would matter.

By the way: In case of $w = 1$ the knowledge is *believed* to be complete (in words of the referee), and the individual will rely on a single probability function. This is the Bayesian case since probability distributions remain subjective, and therefore it is "classical uncertainty", not "risk" as the referee claims. Note, that a Bayesian individual does not neccessarily know the true model, he may also fully believe in a misspecified model. Since $w = 1$ is an attribution of the individual, he acts naively *as if* there is classical uncertainty. I would call this perfect self-confidence.

- **The formal representation of uncertainty**

As I made clear in section 2, the way I follow is not to provide the $n + 1$st mathematical approach to complex belief structures. It is sufficient to know that the agent has *some* beliefs which are derived by *some* incomplete knowledge and *some* observations of past data (eventually including some information processing errors). I am not so much interested in how these beliefs are formed. The "formal representation" is quite modest: The fact that *beliefs are dispersed* makes evident to each individual that his view of the world is imcomplete and he should not be so naive to trust his own cosiderations and beliefs. The main point is that the agent is *aware of this fact* and he *responds to this fact*.

The referee is puzzled about the concept of belief dispersion as a meaningful expression of uncertainty. If a single agent forms beliefs derived from some knowledge and data, then – for the modeler – these beliefs are taken from a probability distribution. If he knows that he possesses knowledge about the "true" model he would act under "soft uncertainty". Such a knowledge is not possible. Hence he becomes uncertain since he is aware of the fact that his beliefs are just *one* possibility to form expectations about the variables. This becomes more evident if one takes different agents into consideration with different beliefs. It is then evident for the agent that the variable will be drawn from a probability distribution different to that distribution he believes in because there is no a priori reason that his beliefs are superior to those of other agents. I wonder how this setting could collapse to rational expectations, as the referee supposes (p.3). I strongly claimed for the absence of the idea of a "true model" which is essential for rational expectations but the purpose of the paper is not a methodological discussion of the RE paradigm.

A further objection is related to my assumption that beliefs are dispersed around the ex post realized values. The referee claims that this is neither in line with the notion of uncertainty nor with the microeconomic arguments. On p.7 I argue why this assumption does not contradict uncertainty. Each single agent knows that his believed distribution and the realized distribution will differ, so there is reason for self-consciousness. Assume that agent i forms expectations about a random variable x. From prior knowledge and data he believes that x is normally distributed with mean $\bar{x}_i$ and variance σ_i^2. The ex post realized distribution, however, differs from his beliefs and he might face a systematic error $\bar{x}_i \neq \bar{x}$. Therefore $\bar{x}_i$ are not rational expectations. But if we have a lot of agents (or, alternatively, we assume that the single agent is drawn from a set of agents with different beliefs), the assumption $E_i[\bar{x}_i] = \bar{x}$ is justifiable for methodological reasons. Recall, that in the mean of all beliefs the resulting average behavior is biased compared to an omniscient representative agent. If the modeler imposes systematic belief errors in the mean of all agents then the specific structure of the presumed error would "explain" the behavior. Hence any aribtrary behavior could be "explained" by a corresponding ad hoc presumed error structure. I would refuse to call this a scientific explanation. In contrast, I explain behavioral regularities (here: liquidity preference) by the benefits they create by controlling the impact of a *neutral* error on the decision behavior (see e.g. Heiner 1983 in AER for this idea).

- **Epistemological issues**

  The referee supposes that I invoke an "updated version" of Occam's razor to criticize existing formal representations of hard uncertainty. This is only partially correct. What we could observe are environmental data and the behavioral regularities (as a response to these data). The "full-fledged axiomized theories of decision under hard uncertainty" (p.3) explain the behavior by a very rich structured set of *non-observable* entities, including ambiguity preferences and principles of rational decision making. The consistency and formal elegance is undoubtedly glorious. The problem is that we have too much degrees of freedem in the set of unobservable entities so that such models could be fitted to any behavior and therefore may become unfalsifiable. Therefore I also deny the referee's claim (p.3) that the antecedent conditions of those theories are "observable" because the experimental economics literature gives "support to many of them". If, for example, an ambiguity aversion theory is tested by an experiment, then we are able to "estimate" ambiguity aversion parameters from the data – by *a priori* presuming consistent behavior. Is the behavior then "explained" by the theory or does the data "support" the theory? Perhaps the agents have very different ambiguity preferences but they behave in an inconsistent way? If there are any empirical contradictions, the modeler is free to impose additional structural parameters which could be estimated by experimental data. In which sense we have "observed" these entities? Especially preferences are such an entity which could easily be enriched with fancy things like "intrinsic motives" etc.. You can "see" such preferences and beliefs if and only if you accept rational (consistent) choice and a certain model structure a priori. The reason why orthodox economists follow this path of theorizing is that they do not have to leave the paradigm of rational choice. I am not interested in this kind of "act rationality" (Aumann). I haven't claimed that I waive for any non-observable entities (this is impossible), but what I need are simple beliefs and simple preferences. Deviations from rational choice are permitted, instead. This allows to relieve the explanans to some extent from fancy non-observable entities.

- **Bounded rationality**

  Each choice which could not be derived from a calculus, based on beliefs and preferences, is not a rational choice in the orthodox terminology. If choice behavior systematically differs from rational choice but follows a pattern which is more or less in line with the individual's preferences, it is boundedly rational. I decline the referee's claim that there is a need for a "well specified metric of rationality boundedness" (p.4) before talking about bounded rationality. This is a nihilistic argument to choke any attempt to model boundedly rational behavior. I made clear that chhosing λ according to the *rule* $\lambda^a(\beta)$ is a deviation from the portfolio calculus. The essential point the referee unfortunately hasn't recognized is that this rule is not an ad hoc assumption (which is a usual objection against models of bounded rationality), but it is justified by the fact that this rule improves the performance in terms of subjectively expected utility and is therefore "rule rational" (again: Aumann). It is also in line with contemporary portfolio literature dealing with estimation and model uncertainty as discussed on p.8, and it confirms the empirically relevant assertion, that agents respond to increasing uncertainty by a higher liquidity preference.

- **Final comments**

I appreciate that the referee read the paper carefully and made very detailed critical comments. I have learned that I have to formulate my arguments more precisely to avoid misunderstandings. But with regard to the *content* of the objections and the asserted reasons why many of the ideas are "puzzling" I do not agree with the referee. One of the few things I have frankly to admit is that there are many misprints and language errors which call for copyediting by a native speaker.