

## Response to referee 1

### Summary

The referee first challenges the terminology of the paper.

A generally accepted terminology for uncertainty and risk is still emerging. This paper can at best contribute to this ongoing process.

Second, the referee thinks that the model's calculus is either wrongly explained or simply wrong. Faced with these two options, it must be maintained that the first holds as there is no proof (yet) of the latter.

Several minor issues relating to the notation, wording and the motivation will be dealt with in a revised version.

### Detailed responses to the referee

I would first like to thank the referee for his / her careful reading of the paper and then take this opportunity to respond to the voiced concerns.

The two main issues of the report are related to my use of terminology and to the suggested model.

As regards the terminology, I agree with the referee that the distinction between risk and uncertainty could be discussed in depth in its own right. As the exact wording and terminology of risk and uncertainty are not the focus of the paper, I explicitly refrain from this exercise at large (p. 2). I do stress, however, the fact that I refer to uncertainty as non-quantifiable measures. According to my terminology, therefore, events which can be described by probability distributions do not qualify as uncertainty while risk, which builds on (quantifiable) probabilities, does so. A thorough discussion would further look into the issue of how to distinguish between quantifiable (probabilities) on the one hand and probabilities which exist but cannot be quantified on the other.

I am grateful to the referee for pointing out this important issue. I am confident that other contributions to the journal's special issue on radical uncertainty will help clarify this aspect.

In the same vein, I do agree that the number of papers featuring the word “uncertainty” alone is not an exhaustive proof of the relevance of the topic. In fact, this number is a rather arbitrary choice for motivating the paper. A maybe more convincing one is the editors' decision to devote a special issue to this very topic alongside the apparent penetration of mainstream economics journals by previously heterodox, Post-Keynesian language such as radical uncertainty, or Keynesian uncertainty.

With respect to the entertained model the referee considers two options: the model's calculus is either wrongly explained or simply wrong. Due to lack of proof of the latter, it must be assumed that it is correct but deserves a better explanation. To start with, the correct notation of the error term in equation (2) should be  $\varepsilon_i(t)$  in order to match it with the other variables' notations.

The referee objects to the assumptions that each price quote reflects individual voting decisions by calling them “very strong” assumptions. These assumptions are indeed crucial. The standard alternative to these assumptions are given by my equation (2). In brief, though the voting itself is not unusual, it is usually assumed that the price reflects decisions which are not individual but collective or, rather, made by representative(s).

I cannot see why assuming that individual decisions are subject to a common rule such as (maybe time varying) probability distribution function is a weaker or a preferable assumption to letting individuals be what they are: individuals. In fact, since the assumption of probability distribution function – be them Gaussian or some general Levy, or any other – for individual behaviour more often than not is the centrepiece for describing and measuring volatility or return processes that assumption would need a very careful justification, not its opposite.

It would obviously be a tremendous advantage for all sorts of asset price analyses if one could devise a reliable probability distribution function for prices of assets or their returns. Unfortunately, the referee's references and whole libraries testify to the failure of all related efforts.

This failure sets the scene for my model. It simply shows that dropping the assumption of a common rule for individual decision making creates uncertainty in the sense that the average market price does not converge as the number of individuals increases and the (empirical) variance becomes a function of the number of individuals participating in the market. Studying the suggested references, I feel reassured by the independent finding of a positive association between trading frequency and volatility (especially Gorishkan et al., 2000, but also Plerou et al., 2008) which I consider the main testable implication of my theoretical model.

Furthermore, the referee claims that my price setting mechanism is flawed (because it would build on Walrasian theories) and advises to cast the market dynamics in a synchronicity v asynchronicity dichotomy along the lines of the excellent Donier and Bouchaud (June 2015) paper. Setting apart the detail that this particular paper dates from after submitting the refereed one, it also lacks an important feature which I consider extremely desirable. My model is void of any exogenously given “fundamental” price but establishes the actual price endogenously. Moreover, in my model this endogenous price has features which are akin to Donier and Bouchaud (June 2015)'s Brownian motion-like “fundamental” price. It should be noted that the very empirical fact that actual prices look (somewhat) like a Brownian motion demands a structural explanation; an assumption is just not enough. I suggest to use uncertainty as the explanation and deliver an according model.

There probably are other, maybe simpler (or less obscure for that matter) ways to describe the genesis of uncertainty. The key (intended) contribution of my model is to bridge the gap between market behaviour of individuals and actual market outcomes that fit the empirical evidence instead of ending up with some unresolved “puzzles”. It is understandable that theoretical economists as well as econophysicists will find it difficult to accept the notation because of their fields' traditions. It might be easier for time series scholars to the extent that I try to draw on their representation of covariance non-stationary processes. In any case, I am aware that the lack of a canonical model which would serve the key purpose poses a considerable challenge to both parties, author and reader.

To wrap up this discussion, based on my model and empirical evidence I am confident that we can regard individualism as a source of uncertainty which constitute asset prices whose properties closely fit the features of actual prices.

I would finally like to thank the referee again for giving me the opportunity to critically scrutinise and hopefully improve the current version of my submission.

Christian Mueller

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