Reply to comments on "The order of knowledge and robust action"

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First of all, I want to express my thanks to the referee and the two commentators for the toil of reading my paper and commenting on it. Comment # 1 (in the sequence of appearance) finds the basic dichotomy between distinguishable states and a set of non-distinguishable events useful for thinking about general and specialized investments from an applied point of view. This is very encouraging and alleviates the unsureness about the value of my thoughts. My reply concentrates on comment # 2 and the referee report which point to the downsides of my contribution. Both report and comment # 2 share the criticism that the paper is ineffective in the way it is written and its contribution to the literature remains unclear. Their approaches, however, come from opposite sides. Whereas the report is asking for a more standard economic paper and illustration by a toy model or example, commentator # 2 is asking for more rigor and clearness from the perspective of abstract theory.

For solving the tension between demand for a simple toy model on the one side and more rigorous abstract reasoning on the other side, I will sharpen the principal argument in the most simple abstract way, without using any detailed structure like probability distributions, and try then to translate the argument into the language of simple tools or procedures in economics and business. Before turning to this I give a motivating example and refer to some literature.

The most salient example of the successful career of standard economic risk analysis and its tragedy was the sophistication of finance and the financial crisis. A huge set of new financial instruments has been developed over the last decades – with the claim to generate high returns and reduce risk at the same time. Ideally, financial innovations expand the set of states that is

spanned by independent financial instruments and increase market complete-ness (Magill and Quinzii, 1996). At the macroeconomic level, a richer set of state-dependent financial instruments allows to finance new specialized tech-nologies, which are risky but highly productive and thus foster development (Acemoglu and Zilibotti, 1997). Now, if the number of financial products traded in the financial market increases by a factor of ten or more (Studer, 2015), one may worry if indeed they help to deal better with economic un-certainty or they rather increase the risks to which an economy is exposed. If one asks which products have problematic features or are designed in a sloppy way, then one gets essentially two answers: i) It is the system, not the single product. ii) The products don't account properly for the correlation with all the other products. From this one may conclude that people are naive or the clever ones are cheaters. I think we should add to these answers a further one: The increased sophistication in dealing with uncertainty in the financial market is based on a false pretense of knowledge – not only in the financial industry but in the scientific community as well.[1] The knowledge needed for dealing with uncertainty in risk analysis is the joint distribution of events on the entire state space.[2] Hence, the knowledge needed for accurate risk

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[1]Caballero (2010) prominently pointed to "the pretense-of-knowledge syndrome" in economics and emphasized that the lesson to be drawn from the financial crisis was to give up the pretension.

[2]Reader comment # 2 asks what it means to have knowledge about distributions. My understanding of knowledge is pragmatic: What do I need to know for solving a problem? For instance, if I want to price a security I need to know the state-contingent performance of the product and the probabilities with which states are realised. Or if I want to choose a portfolio, I need to know my endowment, my risk attitude, the set of feasible instruments, their state-contingent performance and the probabilities of the realisation of states. The fact that I can apply my own subjective probabilities does not change the fact that I need to know them for solving the addressed problem. More formally, in the language of

analysis rises with the complexity of the state space. Rational dealing with uncertainty therefore requires a systematic effort to bring the complexity of the state space in line with our knowledge about possible future conditions. In Falkinger (2014) I elaborated the argument in the specific context of financial innovations in a general equilibrium. In the paper I presented here I tried to outline the general nature of the argument.[3]

From the example of financial innovations we get the following important insight: Uncertainty in economics is generated by "nature" but also by the model we use to "produce the future".[4] The production of the future includes both the design of instruments (technologies, financial products) and the allocation of resources on the instruments. In my paper I denoted the first object by f and the second by x. In other words, the problem of rational dealing with uncertainty from a system's point of view is not only how to play given lotteries but also how to design the lotteries so that the future economic development is good according to some (subjective or social) value standards. This fact may be the main source of misunderstandings in the communication with "behavioristic" (Schmeidler, 1989, p. 584) representations of the problem of uncertainty. In a world with innovation the consistent

theoretical computer science, problem solving requires to put input into a problem solver. Whoever uses the problem solver has to let the problem solver know the required input. Whether the user of the problem solver comes to the input by forming subjective beliefs or by evaluating data, the complexity of the user's task rises with the complexity of the required input; in the context discussed here, with the sophistication of the state space.

[3]False pretense of knowledge and careless dealing with models is a problem that goes far beyond the aspects of uncertainty and risk that are addressed in my work. This has recently be pointed out by authors like Hellwig (2015), Pfleiderer (2014) or Romer (2015).

[4]In the language which Schmeidler (1989) calls Bayesian nomenclature, one therefore would need to distinguish between "nature states" ω and "model states" ϑ, where ϑ is a "nature event" that comprises indistinguishable nature states.

derivation of "number(s) used in calculating the expectation (integral) of a random variable" (Schmeidler, 1989, p. 573) is a necessary requirement but not sufficient for dealing with economic uncertainty – regardless of whether the numbers are objective or subjective probabilities. We play the lotteries that we have designed, or more precisely: Some agents, for instance, the households, play the "lotteries" which technical or financial engineers design.[5]

What is a rational, that is, a logically consistent way of accounting for the knowledge that choices are based on models and models are based on limited knowledge about the world? As the sentence makes clear immediately, this requires to transcend somehow the economic analysis in the narrow sense, looking at it from outside in a language that addresses economic models as objects. At the level of standard economic analysis, the most basic primitives of the language are a choice x in a model M. (In the case of my paper the core elements of M were $\Theta, \{f_\vartheta | \vartheta \in \Theta\}, (\pi_\vartheta)_{\vartheta \in \Theta}, \mu_\Theta$; and choice x was an allocation x_Θ.) Model M is an abstract representation of a real world W.[6]

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[5] Suppose that $\Theta = \{\vartheta_\nu\}_{\nu \in N}$ is a partition of $\Omega_K \subset \Omega$ with $\Omega - \Omega_K \neq \Phi$ and let Θ' be a partition of Ω. In my approach targeted instruments (e.g. state-contingent securities or specialized technologies) are used for $\vartheta_\nu \in \Theta$ and part of the resources is invested in a robust instrument. For determining the quantities invested in these alternatives probability distribution π_Θ on Θ and μ are used. In Schmeidler's (1989) approach, if I understand him correctly, the instruments would be defined with respect to Θ', and if we are not sure about the objective $\pi_{\Theta'}$ we determine the quantities invested in the alternatives $\vartheta \in \Theta'$ by evaluating the expected consequences, using a set of numbers $\{\upsilon(\vartheta) | \vartheta \in \Theta'\}$ with $\sum_{\vartheta \in \Theta'} \upsilon(\vartheta) < 1$ and $\upsilon(\Omega) = 1$. The non-additive subjective probability υ reflects that we are not sure about $\pi_{\Theta'}$ or that something apart from $\vartheta \in \Theta'$ may happen. But why then design lotteries for Θ' rather than ask: What can be distinguished in a way that additive probabilities can be assigned more or less reliably and what is the part about which we have no specific knowledge to distinguish events?

[6] Paul Romer's (2015) criticism of "mathiness" calls into mind that economic modeling

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It expresses our knowledge about W. Choice x is a men-made object, chosen on the basis of M, the consequences of which are experienced in the real world. They depend on the performance of x in W. The valuation of the consequences is a normative issue and usually a subjective thing. Let

$$V(x, W)$$

denote the value of x in W.

Rational choice of x in M usually means to maximize the value of the consequences of x according to the knowledge about W as captured by the model M. That leads to

$$V(x^*[M], W), \quad x^*[M] \equiv \operatorname*{argmax}_x V(x, M).$$

An omniscient decision maker would have an ideal model M_∞ that captures all relevant features of W correctly so that the value of the model consequences of the optimal choice coincides with the value of its real world consequences:

$$V\left(x^*[M_\infty], M_\infty\right) = V(x^*[M_\infty], W).$$

Actually, however, $V(x^*[M], W)$ may fall short of $V(x^*[M], M)$ for any feasible model M, due to limited knowledge. Let $q(M)$ denote the (relative)

requires to tie the abstract and formal components of a model to the real world to be modeled. Pfleiderer (2014) speaks of Chameleons who within M pretend to talk about W and, if confronted with W, come up with the excuse that M is just a toy model. Hellwig (2015) points out that, in particular in the application of economics to policy questions, our discipline lacks a good practice for selecting an appropriate model for a given context. I share the view that disciplining our modeling by careful checking that M captures essential features of W in an appropriate way is the most important requirement for scientific analysis.

deviation of actual and model value of an optimal choice:

$$q(M) \equiv \frac{V(x^*[M], W)}{V(x^*[M], M)} \leq 1.$$

This assigns to models a quality attribute. $q(M)$ expresses how appropriate or reliable model M is for guiding economic decisions. Of course, we cannot determine the quality of M within M. Ideally, we would wish to experiment with different models from a set of potential models $\mathbf{M} \equiv \{M \text{ is model for } W\}$ and collect for each $M \in \mathbf{M}$ deceptions and surprises from comparing the model values of the respective optimal choices with the experienced values.[7] My approach was to look for model characteristics that might arguably be used to form reasonable priors about $q(M)$. Formally, this means to define a structure on $\mathbf{M}$ that allows to compare models in $\mathbf{M}$ according to some order which can be then related to model reliability.[8]

In the standard economic model of decision making under uncertainty the core model components are state space and probability distribution on the space. Therefore, I tried to work out a formal structure that allows to order state spaces according to characteristics which are related to the knowledge

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[7]The approach "let the data speak" works well if a narrow set of hypotheses is consider. The idea to select theories by verifying or falsifying implications of a theory is more questionable. At a more pragmatic level, the question in the context here is: How many observations on W do we need to assess probability distributions π_Θ for different state spaces Θ and to compare their reliability? Can we generate these observations, before a structural break puts us in a world W'?

[8]There are similar problems in other areas, for instance, the complexity of a program in a formal language. One can execute a program and measure the computing time, provided the program stops in the time span available. Or you can try to look for a suitable complexity measure on the set of programs. Nested *Do loops* or *If then* clauses may be simple indicators of computational complexity, for example.

requirements for correct assessment of the probability distribution on a state space. I came to the conclusion that granularity and coverage are suitable characteristics for ordering state spaces according to knowledge requirements. Then, I hypothesized, for a given level of knowledge available, a model with higher knowledge requirements tends to be less reliable.

Apparently, my presentation of this approach was confusing - both at the abstract theory level and more so at the level of practical application.[9] Having tried to outline the abstract argument more clearly, I turn now to the translation of the argument into simple procedures from the standard economic tool box.

One of the most common economic tools is demand and supply analysis. For guiding our ordering of models about the world, we can assign to a model the pieces of information required for finding an optimal choice. This gives us for $M \in \mathbf{M}$, a list $K^d(M)$, which expresses the knowledge requirements

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[9]One aspect which was particularly confusing was that I used the word knowledge in two different contexts, as comment #2 pointed out. One context was the knowledge about probabilities. I hope I could clarify the meaning of knowledge in this context by my earlier footnote on the knowledge about distributions. Another context is the invention of new technologies. In this context, I used the symbol G to express the stock of general technological knowledge, from which engineers and entrepreneurs can draw Know-how for creating new specialized technologies. This way of modeling the process of technological innovations is inspired by the endogenous growth literature in which $G > 0$ is the aggregate productivity level, reflecting the so far accumulated technological Know-how.

From this pool of knowledge engineers can draw Know-how for designing technologies which are targeted to performance in clearly specified conditions. The performance under real world conditions, however, is a different matter. This is why two types of knowledge are relevant: Laboratory knowledge - we might call it technical Know-how, and knowledge about the relationship between laboratory or model conditions and real world conditions.

of a model. We may call K^d the demand of knowledge. In the context of my paper, which focuses on the modeling of state spaces, I proposed to use granularity (n_Θ) and coverage (μ_Θ) as characteristics which describe the knowledge requirements of a model. The idea was that an increase in granularity and coverage raises the number of states that have to be measured.[10] On the supply side, we have a data generating process which reveals information about the world. This gives us, at time t some pool of knowledge $K^s(t)$; we could call it the supply of knowledge. Now, the idea of my paper was that at time t, at which the supply of knowledge is given, the quality of a model declines if its demand of knowledge is high. So I concluded that $q(\Theta) = R(n_\Theta, \mu_\Theta)$, is a reasonable prior for assessing the quality of a risk model under uncertainty.[11]

I am not sure whether the demand and supply jargon is useful or misleading for the practical choice of the framework in which we should make our choices about uncertainty. But I am convinced that some systemic form of documenting the information needed to solve a model and its comparison to the information available is crucial for economic decision making under

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[10]In general, one could think of other characteristics as well for describing the knowledge requirements of a model. For instance, number and dimensions of functions and order of derivatives needed to solve the model. To give an example, which I did not address in my paper: For answering economic questions in an expected utility framework one usually needs, apart from knowledge of the probability distribution, information on the risk aversion and how it changes with income. This involves information about the third derivatives of the utility function. One may therefore ask if utility functions are an appropriate language to express risk attitudes.

[11]It is worth mentioning that granularity (n_Θ) and coverage (μ_Θ) are at the same time key determinants of the value $D(n_\Theta, \mu_\Theta)$ of specialization and diversification. Thus, the advantages of a differentiated state space Θ and its costs (in terms of reliability) are both controlled by the same characteristics of Θ.

uncertainty. This sounds trivial but I don't observe it in economics as a standard disciplining method required like, for instance, correct calculation of solutions in a given model.

Another simple tool, used in the more applied sphere of management and policy, is to make lists of priorities. A good way to adopt this tool for our context is to put us in the shoes of an expert who is asked for advice by a manager or policy maker. What a responsible decision maker wants to hear is neither the one and only one optimal solution under specific assumptions nor different solutions under all possible assumptions. The question to be answered is rather this: Tell me the two or three alternatives which you think are the best for my goal. In the expert language this means: The goal is V, tell me $x^*(M)$ for the two or three models M which you find most reliable. Maybe the decision maker asks for one or four alternatives. In any case, the lesson we should learn is to order our state space framing of an uncertain world along a hierarchy of reliability.

I would like to close with a remark on "rational inattention". A first requirement of rational accounting for limited knowledge is to put a weight (in my paper μ) on the fact that their are things out there which I do not know but may be relevant. Rational inattention recommends to focus a limited perception and information-processing capacity on important things. How do I know what is important and what is not before seeing it? More specifically, my approach suggests to prepare for unknown but potentially relevant events by robust measures - at the cost of foregone gains from more sophisticated measures. The rational inattention approach emphasizes to react to the events in the chosen focus of attention and to ignore the rest. Now, at some level of abstraction one could argue that my argument, to bring the

complexity of the model we use for dealing with uncertainty in line with our knowledge about the states of nature, has also the flavor of rational choosing our focus of attention. In the end, the bottom line is this: Rational dealing with limited knowledge is not an individual choice problem. In terms of the introductory example: The financial crisis was not caused by the fact that the one or the other individual made errors in risk assessment or had a wrong focus of attention; rather it was the consequence of coordination of agents by unreliable models of risks. So if rational inattention is an appeal to the scientific community to focus attention and effort on reliable modeling, then I am happy to join. But as long as it is meant as advice to individual agents to be clever, I am out.

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