ON CONCEPTUALIZING RISK: A COMMENT ON HOFFMANN

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ABSTRACT: Hoffmann (2018) attempts to reconstruct a typology of risks deemed more accurate and useful to both economists and risk managers than currently received views on the subject within mainstream economics/finance and Austrian economics. This comment argues that his criticisms of the Misesian approach and his case for an alternative are unconvincing. We explain weaknesses in his criticisms of the Misesian approach and outline some problems with his constructive task of building up the alternative.

KEYWORDS: Austrian economics, risk, uncertainty, complexity, probability

JEL CLASSIFICATION: B4, C1

I. INTRODUCTION

Drawing on the general literature on risk and uncertainty, as well as Mises, Knight and Weaver, Hoffmann (2018) attempts to reconstruct a typology of risks deemed more accurate and useful to both economists and risk managers than currently received views on the subject within mainstream economics/finance and Austrian economics. In particular, the author emphasizes what

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his approach and the Knightian/Misesian one have in common and where they differ. Formally, this is done by identifying two "research gaps" in the Misesian literature—a lack of conceptual clarity in dealing with risk and uncertainty (1) and a lack of justification for the view that classical probability theory is irrelevant when dealing with human action (2)—and trying to close them.

In what follows, we focus on some reasons why both his criticisms of the Misesian approach and his case for an alternative strike us as unconvincing, although this is not to deny that the paper is thought provoking and displays valuable information. First, we explain why his criticisms of the Misesian approach appear to us as weak, and second, we outline some issues with his constructive task of building up the alternative.

II. WEAKNESSES IN THE CRITICISMS OF KNIGHT/MISES

While Hoffmann (pp. 2–3) delves into some epistemological considerations, in order to make some proposals regarding the requirements of a proper definition of risk, he nevertheless neglects to identify what could be the epistemological grounding for the Misesian position, as if it did not have any, before telling us about the two research gaps that allegedly characterize it. It is true, as the author suggests, that Mises is less than perfectly explicit regarding the proper scope of application of classical probability theory. However, the impression left that the traditional dichotomy of risk and uncertainty could be considered as an *ad hoc* piece of theorizing, somehow independent of the praxeological edifice and its justifications, is unwarranted. On the contrary, as can be inferred from Mises's discussions, as well as Hoppe's (2007) defense and elaboration of it that the author refers to without ever mentioning why Hoppe thinks Mises is right, Mises's views on this particular topic are arguably grounded in his general epistemology. If they are flawed, ultimately it must then be either that Mises's epistemological views are wrong, or that he inconsistently applies them to the particular questions under consideration (or a combination of both). But the author provides no assessment of the sort. It seems obvious to this commentator, in any case, that the author is on shaky grounds when identifying some research gaps in Mises's approach without first paying some attention to those considerations.

At the risk of oversimplifying, the Misesian approach on probability, risk and uncertainty that the author describes, can be defended along the following lines:

The distinction between risk and uncertainty and their fields of application mirrors the methodological dualism Mises advocated between the natural sciences on the one hand and economics on the other and derives from it.[1] According to Mises and his followers, sound economics has to be structured as statements logically derived from and implied in the so-called axiom of action (the "logic of action" or "praxeology"). Action has to be understood as purposeful behavior. It implies the necessity of choice regarding the use of some scarce means to arrive at some ends. All the categories of goods, value, cost, profit and loss, etc. are implied in this insight which is considered by Mises as valid knowledge derived *a priori* of experience, via discursive reasoning. The axiom is self-evident in the sense that one cannot deny it without performative contradiction since any attempt would have to be an action itself, using some means to arrive at some end, etc.

One implication of the axiom is that action in general and therefore any production process takes time and that the future must be uncertain to the actor. For there would be no choice to make if future courses of events were known in advance in a world of complete certainty (Mises, 1949, p. 105). Actors must lack perfect foresight then. When acting, they must rely on their more or less probable knowledge about the world.

Now, for our purpose here, a relevant implication is, as Hoppe (1995, p. 78) puts it, that "action presupposes a causally structured observational reality but the reality of action which we can understand as requiring such structure, is not itself causally structured." Action itself is not causally structured since it is purpose-directed. The actor chooses to use scarce means in some ways instead of some other ways to arrive at some ends and by necessity, chooses to abandon or postpone the fulfillment of other ends. On the other hand, action presupposes the "constancy

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[1] This, incidentally, helps explain why Knight's views came to be typically associated with the Austrian school after Mises systematized and refined the theory of knowledge and the corresponding method used by the Austrian economists, and as the Chicago school became unambiguously positivist.

principle," "time invariant operating causes" in the actor's environment, or a "causally structured" physical reality in which action takes place. In Mises's words, "causality is a category of action." The reason is that the very idea of action implies interference in the actor's environment in order to produce a preferred state of affairs compared to the course of events without such an action. Success and errors must be ever present possibilities as long as there is action, and being able to conceive of a course of events and its successful deviation initiated by an actor means he can grasp some relationships between things which stay constant over time. There cannot be any meaningful concept of success and error, planning and therefore action under complete randomness or indeterminacy in the actor's environment. The range of applicability of teleology and causality must therefore be clear and are determined *a priori*. Action has to be categorized teleologically, as purpose-directed, and the non-acting entities in the actor's environment must be categorized causally (Mises, 1949, p. 107; Hoppe, 1995, pp. 77–81).

Now the insight here is that there are two categorically different realms of phenomena and that different methods are required to learn about them accordingly. On the one hand, the actor will have a less than complete knowledge of causally structured natural phenomena. On the other hand, he will lack knowledge of his own and other people future actions. As for the methods, there is no way one could identify fundamental laws of action by treating it as some causally structured movements of bodies that one has to experiment with to find the cause and effect relationships *a posteriori*, and there cannot be *a priori* knowledge of specific causal relations apart from the fact that they are causally structured.

In the realm of natural phenomena, the constancy principle allows us to project past observations regarding peculiar cause and effect relationships into the future. In other words, actors can hypothesize some specific time invariant causes at work and test their views thanks to experiments. The more tests are made, the more the relationships can be confirmed or discarded. That is how natural sciences proceed, of course. At some point, it becomes known with practical if not absolute certainty that combining two atoms of hydrogen and one atom of oxygen produces a molecule of water. Or, some engineers are able to build and operate high speed rail networks which work most of the time without significant

technical failure. Now, sometimes observations of natural phenomena do not shed light on all the relevant cause and effect relations, but still allow actors to discover some regularity that can be expressed in terms of a numerical probability distribution. That is what Mises (1949, p. 107) refers to when discussing "frequency" or "class probability."

The important consideration here is that the very possibility of being able to identify a class and the related probability distribution of some event presupposes that it is ruled by causality. No quantitative constant can be expected as a rule from an acting entity. That is why frequency or class probability can strictly be applied only in the field of natural sciences and that is why Knight's concept of risk should apply to this realm only.

Now, not every event can fit the "ruled by causality" category. People act—people choose, that is—and choices cannot be predicted on the basis of time-invariant causal laws. A particular action is not the automatic answer to an external stimulus but the deliberate employment of chosen means to reach chosen ends. Different actors or even the same actors facing the same situation at different times can make different choices. Therefore, there can be no question of grouping some acts in a class of supposedly homogeneous events (Mises, 1949, pp. 110–113). This is the realm of "case probability." This is why Knight's concept of uncertainty should apply to actions only.

One may also refer to Hoppe's (2007) elaboration of why action is intractable by frequency theory. In a nutshell, we may typically "know of no rule how to distinguish one bottle from another as far as breakage is concerned," (Hoppe [2007, p. 14], referring to the manufacturing of beers in a factory for instance) so that a class may meaningfully be identified and probability calculus applied. However, understanding (*verstehen*) via verbal communication with other actors puts us "in a position to precisely distinguish one actor from any other actor and one action of a given actor from any other" (Hoppe, 2007, p. 17). Hence, as Knight puts it, in most cases in daily life, "there is no valid basis of any kind for classifying instances." That is, "the essential and outstanding fact is that the 'instance' in question is so entirely unique that there are no others or not a sufficient number to make it possible to tabulate enough like it to form a basis for any inference of value about any

real probability in the case we are interested in" (Knight, 1921, p. 226). Should a particular manufacturer expand production? With no valid basis for classification, limited knowledge of the possible outcomes and no calculation of the sort insurance deals with being possible, actors must then resort to "intuitive judgment" and "estimates" in "any typical business decision." Being irreducible to fixed costs, they permanently leave room for errors in judgment, hence the existence of profits and losses.[2] Typical business decisions being based on such estimates, failure to forecast future prices and quantities is perfectly normal and results in bidding up factors of production "too much" or "not enough" in relation to their marginal productivity.

Now perhaps that approach is flawed, but where is it exactly? Why is the identification of risk with the frequency interpretation of probability naïve, in light of Mises's whole system? Is it, for instance, that his methodological dualism is wrong? Shall we get rid of the whole edifice? If not, why not? What shall we keep, why, and how does that affect our treatment of risk or uncertainty? Unfortunately, the author does not give us a clue, since he does not treat Mises's take on risk and uncertainty as a part of a larger system. Instead, the author takes another route. He occasionally alludes to other paradigms or builds his case for another framework and in light of it, incorporates elements of Misesian thought which fit and rejects those who supposedly do not. This is not necessarily prob-lematic, although a possibly enlightening discussion of the above considerations is lost in the shortcut. If one refers to or builds an alternative paradigm, demonstrates it to be the truth on the matter, one may spare oneself a thorough analysis of the Misesian—or any other—view on uncertainty and risk and its relationship to Mises's epistemology and simply point out that this view must be wrong to the extent that it deviates from the said truth.

An example of such an "external" critique of the Misesian approach is when Hoffmann (2018, p. 21) claims he is justified in asserting that, "we can reason about human action and choices probabilistically" as Luce and Raiffa (1957, pp. 19–23) show or,

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[2] These errors should not be confused with technical failures, when one's techno-logical recipes do not work, which essentially have to do with our grasp of the laws of nature.

referring approvingly to Hájek (2011), that the frequency interpre-
tation of probability is flawed anyway, so that the Misesian iden-
tification of risk with the frequency interpretation of probability is
naïve. Apart from the fact that it is hardly obvious how both claims
could be held at the same time, the problem is that the author does
not tell us what are the objections exactly, and why we should
consider them as valid.[3, 4]

III. WEAKNESSES OF THE ALTERNATIVE PARADIGM

More constructively, Hoffmann (2018, pp. 11–14) lays down the
foundations of an alternative paradigm, by providing the reader
with four requirements that a sound definition of "risk" should
meet, and tries to sort out what is right and wrong in the Misesian
approach, in light of that new framework. The requirements are
(1) that "risk should be defined in such a way that it can be distin-
guished between risk per se (what risk is) and how risk is measured,
described or managed"; (2) "risk should be defined in such a way
that it can be distinguished between what risk is and how risk is
perceived as well as that the definition does not presuppose an
interpretation of either objective or subjective risk"; (3) "risk should
be defined in such a way that it is helpful to the decision-maker in
lieu of misguiding her in many cases, and, thereby, the risk defi-
nition should capture the main *pre-theoretic* intuitions about risk";
and as a weaker requirement (4) "Risk should be defined in such
a way that it does not divert attention away from systemic effects
that have an impact on not only the actor, but also on other actors."
While these requirements sound by and large reasonable, the main
issue is the following: the author tells us that their notion of risk (in
a broad sense, or "risk I") is introduced "in a deductive manner by

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[3] As a matter of fact, a cursory look at the relevant section in Luce and Raiffa's book,
called "Individual decision making under risk," reveals that its authors do not
assign numerical probabilities to human acts at all. The probabilities discussed
there are those of the outcomes of a gambling game such as a lottery!

[4] Yes, one can point toward objections in the literature to virtually any view under
the sun, but if merely pointing out that stance A runs counter to stance B was
deemed decisive to make a case for stance A, one could have as well demonstrated
that stance A is wrong by pointing out that stance B exists. And if one can "prove"
one thing and its opposite by the very same procedure, this should say something
about the procedure.

postulating four requirements that a risk notion should meet." Yet what is the epistemological status of those postulates? As far as the present writer can see and for our purposes here, it is clear that, at least, the Misesian treatment of probability, risk and uncertainty, can be thought of as grounded in an identifiable epistemology. It is far less clear that the alternative proposed by the author has such firm grounding.

In addition, why does the risk definition provided actually suit those requirements? It is hardly obvious that it does and that it fills research gap I, as intended, for it is quite close to Mises's notion of probability (except for the uncommon inclusion of desirable outcomes) which allegedly does not: risk is "the real or realistic possibility of a positive or negative event the occurrence of which is not certain, or expectable but only more or less likely. However, the probability that the positive or negative event will occur does not have to be known or be subject to exact numerical specification." (Hoffmann, 2018, p. 16). In fact, it turns out that the concept includes as subcategories the familiar Knightian concepts of risk in the narrow sense (later called Risk II) that the author finds problematic in other sections of the paper, and uncertainty: "Thus, the term 'risk' is not used as an antonym to 'uncertainty,' as is customary in decision theory, but rather as a generic concept that covers both 'risk in a narrower sense' (what Knight calls measurable uncertainty) and 'uncertainty.'" (Hoffmann, 2018, pp. 16–17) What is the improvement then?

Now it is true that further elaborations of the author reveal that he deals with additional distinctions, Knightian risk and uncertainty being one among others. This is another consideration that leads him to disagree with Mises on the scope of classical probability theory. For not only human action could sometimes be made tractable by it. When it is not, when we deal with (deep) uncertainty instead of risk narrowly understood, this would not so much be because of some feature inherent to human action but because we are in the presence of what Weaver (1948) calls "organized complexity." In other words, we are "dealing simul-taneously with a sizable number of factors which are interrelated to form an organic whole. Interactions and the resulting interde-pendence lead to emergence, i.e., to the spontaneous appearance of features that cannot be traced to the character of the individual

system parts and, therefore, cannot be fully captured in probability statistics nor sufficiently reduced to a simple formula." (Hoffmann, 2018, p. 22) Again here, it is unclear what is the epistemological status of the proposal, it is unclear why we are supposed to adopt Weaver's view. But even if we do not dive into the deep waters of epistemology, it should be clear that the proposal is not as plausible as the author wishes it to be. For we can conceive of situations in which we deal with human choices without organized complexity. For instance, the range of possible choices of a shipwreck survivor alone on a desert island or in a lifeboat would be very limited and there would be no interaction to speak of (at least no interactions between human actors). Yet, if what makes some choices intractable by probability theory is organized complexity, would that not mean that we can predict the choices of this person, using classical probability theory? Now the author would have to tell us how we could do so.

IV. CONCLUSION

Aside from some apparent internal inconsistencies, the main problem with the author's thesis is the lack of a systematic analysis of how both the praxeological treatment of risk and uncertainty on the one hand and his own on the other are or can come to be known and validated. His apparent eclecticism leaves his approach with shaky foundations.

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