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# PROBABILITY AND UNCERTAINTY: THE LEGACY OF GEORGESCU-ROEGEN

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# Probability and Uncertainty: the legacy of Georgescu-Roegen

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**Abstract**

In this paper we consider Georgescu-Roegen's approach to uncertainty, showing that his characterization of expectations cannot be reduced to any probabilistic decision making model. Drawing upon Georgescu-Roegen lesson a lexicographical utility function is proposed and analyzed in the mark of his own peculiar scientific methodology. It is demonstrated that such a formulation can be useful in solving usual failure of the expected utility model, such as the Ellsberg paradoxes. The epistemic limits of our re-construction are considered, too.

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# 1 Introduction.[1]

In his introduction to Georgescu-Roegen's 1966 volume "Analytical Economics", Paul Samuelson writes: "Professor Georgescu-Roegen has been a pioneer in mathematical economics. The times have almost caught up with him; but unlike the hare, he moves ahead of his pursuers in a divergent series. ... For in Georgescu-Roegen we have a scholar's scholar, an economist's economist" (Analytical Economics, Introduction, vii).

Almost forty years later we can undoubtedly agree with Samuelson's words. Indeed, Georgescu-Roegen (GR) has been a master of economics. His contributions in consumer and producer theory, in institutional and mathematical economics (to cite just a few) are milestones of the economic discipline; his studies of thermodynamics and natural resources in economics have opened an entirely new field of research (bioeconomics).[2]

Throughout his whole career GR showed a profound and continuous interest in the foundations of the mathematical structures that he contributed to build up. His deep and sound training in mathematics and his brilliant mind helped him not to slip into the "mathematical trap" choosing the easy way, namely, constructing elegant yet unrealistic abstract formalizations that pay few or null attention to their epistemic premises and factual consequences. Quoting Samuelson: "... Because he is so superlatively trained as a mathematician, he is quite immune to the seductive charms of the subject, being able to maintain an objective and matter-of-fact attitude towards its use" (Analytical Economics, Introduction, xi).

In the fifties GR entered the debate about the nature of uncertainty and beliefs' representation, which had always been an open argument in the economists' agenda, but that was becoming an "hot topic" in those days mainly thanks to the clear axiomatic treatments by the founding contributions of Von-Neumann and Morgenstern (1944) and Savage (1954).

The scientific debate has evolved since then in a well known way. On the one hand, the development of the expected utility model, both in its objectivist and subjectivist interpretation, has opened a whole new branch of

[1] In the whole text, citations of Georgescu-Roegen (1954, 1958) refer to page numbers of the reprint (Georgescu-Roegen, 1966).

[2] The scientific biography about Georgescu-Roegen is quite vast. For a survey of some of his contributions, along with a critical assessment of the economic debate that stemmed from them, see the special issue of *Ecological Economics* dedicated to GR (Aa. vv. 1997). For some biographical notes, see Maneschi and Zamagni 1997.

economic analysis which has made his way through the discipline becoming a founding element of any course in microeconomics (at least at intermediate levels), without ceasing to provide fruitful grounds for more advanced speculations. On the other hand, there has been a flourishing of contributions that, starting from the factual counterevidences of the theoretical constructions that were being put forth (Allais 1953, Ellsberg 1961), pointed out the fallacies, or, at least, shortcomings, of what was going to become the "main-stream" approach. These contributions highlighted the need to find both a theoretical enlargement of the structure that stemmed from the Von-Neumann and Morgenstern and Savage analyses and a deep re-thinking of the theoretical premises over which this structure is grounded. Calling on the early contributions of Knight (1921) and Keynes (1921) theese contributions distinguished between risk and uncertainty, where the latter is to be considered different in its deep nature[3] from the former, namely, from probabilities.

Interestingly enough, GR contributions in these fields have been substantially neglected by the subsequent literature.[4] Even his biographers, highlighting his contributions to the theory of consumer's choice,[5] point out his lessons about preferences' discontinuity in utility representation, without making references to GR's opinion about the very nature of beliefs representation's problem, which was for him indistinguishably tied to the nature of uncertainty.

This is an interesting puzzle from the standpoint of the hystory of economic thought, which alone might justify an analysis of GR's lessons. However, we believe there is more than just historical curiosity that motivate the re-thinking about GR's contributions. Indeed, GR's analysis of choice under uncertainty should be seen as a typical lesson about how is or should be the use of mathematics in economics. For, GR's opinion was that a model should not derive consequences that depend more on its internal theoretical

[3] "Apparently, the cause of persisting in the idea that uncertainty and probability are identical resides in the fact that both concepts are related to the impossibility of an exact prediciton. Nevertheless, they are entirely different *essences* [in note: Knight prededed Keynes in noticing and analyzing this difference]." (Georgescu-Roegen, 1954, p. 527-28, my italics)

[4] Interestingly enough, Ellsberg has been the only scholar working in decision making under uncertainty who made an explicit reference to GR's contributions (Ellsberg, 1961, footnote 8, p. 659 and 2, p. 664).

[5] See Zamagni, 1979, and Maneschi and Zamagni, 1997.

structure than on the characteristics of the problem for the solution of which it has been constructed. This need appears to us particularly evident for models of decision making under uncertainty and it motivate us to shed new light on GR's legacy.

In our work we investigate the problem of choice under uncertainty, which is tied to the issue of the nature and proper formalization of expectations for agents who aim at choosing what is best for them in an uncertain situation, i.e., looking for what they *expect* to be their best. The open questions that GR addressed and that we consider in our work are *i*) what expectations are; *ii*) which relationship they have with probabilities, and *iii*) how we can provide a representation for preferences coherent with logic and factual behaviors. In section 2 we address these questions, reconstructing GR's definition of expectation and highlighting a possible interpretation of the concept of credibility of an expectation. In section 3 we propose an interpretation of GR model in terms of a lexmin utility function, and show that our formalization can be useful in solving the expected utility paradoxes pointed out by Ellsberg. In order to do so, a discussion of the possible use of the principle of Insufficient Reason in GR's model is proposed. In the concluding remarks of section 4 we discuss the limit of our interpretation of GR's model in terms of its epistemic structure; this, we argue, may (at least partially) explain the scarce impact that this element of GR's analysis has had in the subsequent literature.

# 2 Expectations and Uncertainty.

The problem of representing agents' decision making under uncertainty is tied to the issue of defining expectations. An expectation, for GR, is a *state of mind* about a fact, a statement, an object, about which the agent does not have an absolute knowledge.[6] Knowledge is a primitive of such a definition. Assuming it, we can represent an expectation by means of the following triple:

$$\varepsilon = (i, E, p) \tag{1}$$

where $i$ denotes the individual, $E$ the (set of) evidence that is available to her and $p$ the statement, or proposition, that she is predicting.[7]

[6] Georgescu-Roegen (1958), p.243

[7] While there are no problems in understanding $i$ and $p$, it is necessary to clarify what

Notice that, from definition 1, it appears evident that the concept of expectation is more general than probability, since it does not rely on any "spurious" definition of "degree of belief" or similar that has been employed to define probabilities. Indeed, the problem of understanding what an expectation is and what is its relationship with the probability depends precisely on studying the link between $E$ and $p$. For GR, probabilities are just one class of expectations, yet not an univocal one. It is that set of expectations for which a single numerical measure can be constructed calling on some logical argument about the nexus between $E$ and $p$ that justifies the use of that measure. This set is not univocal because it varies according to the different definitions of probability that have been proposed by several theories. Each model, i.e. each theory, cannot be satisfactory in its attempt to provide a single measure to uncertainty, since the latter is something that, in general, "... cannot be meaningfully connected to real [or complex] numbers..." (GR 1958, p. 242). But this is exactly one of the point that GR's addresses in its approach to mathematization of economics or, for what it matters, to the general problem of science, i.e., the relationship between the world and its measures. There is no exhaustive definition of probability exactly because there are several possible theories that can all be equally defined and criticized. Each theory is justified because of need to encompass in a structured and logic relationship the reality that it analyzes. However, any reality is far more complex than the model constructed over it. In GR's terminology, models, based on logic, are *aritmomorphic*:[8] discrete, sharp representations of the reality which leave qualitative attributes outside the description; the

GR supposes that evidence is. $E$ can be interpreted as a specific subset of the knowledge, namely, that subset that is available to $i$ when thinking about $p$. Even if, ideally, the evidence encompasses all the knowledge of the individual (GR calls this "the Principle of Absolute Knowledge"), it has to be admitted that in "real life" $E$ includes just a subset of the whole "ideal" $E$, namely, that set of evidence, opinions, etc. that $i$ "can bring into sharp focus at the proper moment" (Georgescu-Roegen, 1958 p.244). Such an explanation, intuitive as it might appear, implies some difficulties about the type of knowledge we assume that $i$ holds, or, to say it more clear, what $i$ knows about what *she* does not know. We postpone the discussion about this point to section 4.

[8] " [...] the domain of Logic -conceived as Princpia Mathematica- is limited by rigidly set and sharply drawn boundaries. The reason for this is that *discrete* distinctions consitutes the very essence of Logic [...]. [...] The fundamental principle upon which Logic rests is that *the property of discrete distinctions should cover not only symbols but concepts as well.* [...] Since any particular real number constitutes the most elementary example of a discrete distinct concept, I propose to call any such concept *arithmomorphic.* [...] Arithmomorphic concepts [...] *do not overlap*" (Georgescu-Roegen, 1966, p. 21).

reality is a *dialectical* continuum, i.e., it is a continuum of entities that are different yet non-completely distinguishable one from the other.[9]

It is therefore not surprising that for GR there is no "correct" definition of probability, and that each one implies unacceptable restrictions about the type of uncertainty that it measures. Let us consider explicitly the Subjectivists' approach to probability.[10] In the subjective model, internal consistency (addition of probabilities of mutually exclusive events and multiplication of probabilities of independent events) is the only criterion that is needed to define a probability $\pi$ as the numerical coefficient that measure the subjective degree of belief in $p$. This claim is thoroughly criticized by GR; the point is made extremely clear and is worthwhile citing it in full: " [...] if all events could be expressed as Boolian polynomials of some elementary events that need only to be mutually exclusive, the structure of the beliefs of any individual would be completely characterized by the manner in which he would distribute probabilities to these elementary events. This probability distribution is otherwise arbitrary and does not have to reflect any stochastic aspect of the material world. In maintaining that such a theory is fully adequate to deal with rational actions in the face of uncertainty, the Subjectivist is exactly like a geometrician who would claim that any geometry topologically equivalent to that of the material world is all we need to explain our

[9] "A vast number of concepts [, ...] among them are the most vital concepts for human judgments, like "good", "justice", "likelihood", "want", etc. [, ...] have no arithmomorphic boundaries; instead, *they are surrounded by a penumbra within which they overlap with their opposites.* [...] It goes without saying, to [this] category of concepts we cannot apply the fundamental law of Logic, the Principle of Contradiction: "B cannot be both A and non-A". On the contrary we must accept that in certain instances at last, "B is both A and non-A" is the case. [...] I propose to refer to these concepts that may violate the Principle of Contradiction as *dialectical* " (Georgescu-Roegen, 1966, p. 23).

[10] In the 1958 paper, GR discusses and criticizes also the "Classical" school, the Frequentist one and the ultra-subjectivist approach of Shakle (1949, 1955). Notice that such a classification is not common nowadays, due to the inclusion of Shakle's view as a specific "ultrasubjectivist" probability theory. Shakle's works are now considered either as a non-coherent theory of probability (see GR's opinion about the unclear and vague wording of Shakle's axiom 1, 3 and 7 (Shakle, 1949, p. 131-2), in Georgescu-Roegen, 1958, note 80) or as an open yet incomplete hint that opened the path towards non-probabilistic measures (Basili and Zappia, 2003). One could explain the GR's classification on an historical basis, since the problem of probability's foundations was in the fifties a more open argument than it appears now, mainly because of the breakthrough of Savage contribution and the following unifying approach of Anscombe and Aumann (1963) which is now widely accepted, at least as a starting point for further theoretical advances.

understanding and use of space properties. For, certainly, a theory of probability cannot concern itself only with the internal consistency of the acts of an individual, as maintained by Savage [in note: Savage, Foundations, pp. 56-57. "Because that theory [of personal probability] is a code of consistency for the person applying it, not a system of predictions about the world around him"]. Ordinary logic would suffice for this." (Georgescu-Roegen, 1958, p. 259).

Moreover, also the attempt to justify probability as the *measure* of the expectation for all type of uncertain situations cannot be shared, since it is not always the case that such a measure exists. Consider the following two properties of any measure (and thus of the subjective probability one): comparability and ordinal measurability. The former refers to a specific structure of the preferences (or any binary relationship that expresses $i$'s choices), namely, being a chain (a completely ordered set). The latter relies on the possibility of representing comparability unambiguously by a chain of real numbers, i.e. "where to each element one can assign a real number which will completely identify its relative ranking" (Georgescu-Roegen, 1954, p.201, original text in italics). GR was among the first (Georgescu-Roegen, 1954) to understand that ordinal measurability implies comparability, but the converse need not be true, since any chain is a lattice, but not all lattice are ordinally measurable (e.g., see Topkis, 1998, ex. 2.2.3 b and 2.6.7). Consider the following example:[11]

**Example 1** *let $X^2 \subset \mathbb{R}^2$ be a compact set, and assume that an ordering $\succsim_O$ exist such that $(x'_1, x'_2) \succsim_O (x''_1, x''_2)$ $iff$ $x'_1 \geq x''_1, x'_2 = x''_2$, or $x'_2 \geq x''_2, x'_1 = x''_1$; the ordering $\succsim_O$ is not ordinally measurable.*

The problem of the definition of a measure $\pi$ in the subjective theory is exactly that, within this approach, ordinal measurability needs to be assumed to define the "single" probability, without any justification than the need to provide a measure for probability, which is a tautology.[12] This appears clear

[11] Which is drawn upon GR's example suggested in Georgescu-Roegen, 1958, p. 245.

[12] In a paper devoted to the general definition of measures and their properties (Georgescu-Roegen, 1964) GR prove that continuity is a necessary yet non-sufficient condition for ordinal measurability (Georgescu-Roegen, 1964, Example 7). A further topological condition is needed, namely measure-homogeneity (named after the similar condition for r.v. functions). However, this assumption is implicit in the space over which probability is defined in the subjectivists' approach.

in the Savage axiomatization, where axioms P1-P4 guarantee just the existence of what Savage calls a "qualitative probability",[13] while the continuity axiom P6 is needed to derive a "quantitative" probability $\pi$.[14]

Given the rationale outlined above, one can share GR's view that the probability measure $\pi$ captures just "some" of the world complexity.[15] This does not imply that a numerical probability cannot be employed in all those cases in which there is some evidence that might "suggest" its use, provided that these measures are qualitative ranked according to the logical link between $E$ and $p$. Consider the following example:[16]

**Example 2** *Suppose that there are four urns, whose evidence can be summarized as: $E_1$ : "in the urn $U_1$ one half of the balls are white and one half are black"; $E_2$ : "the frequency of white in 3,426 independent extractions from urn $U_2$ was $\frac{1}{2}$"; $E_3$ : "two independent extractions from urn $U_3$ resulted in one black and one white"; $E_4$ : "the urn $U_4$ contains some balls". Let $i$ be asked to name a betting quotient for each urn and then choosing her bet among urns[17]. She will typically rank preferences[18] as $E_1 \succ E_2 \succ E_3 \succ E_4$, even though the probability (the "betting quotient") is $\frac{1}{2}$ for urns $U_1$, $U_2$, $U_3$ and (possibly) $U_4$.*

The reason is that there is a *qualitative* ranking of the estimates of the frequencies embedded in the evidence. The estimate of the proportions of balls is "sure" in $E_1$ (since it is known), less sure in $E_2$ , and even less in $E_3$. It is completely unknown in $E_4$. In a sense, it is as if there is an ordering

[13] A simple ordering applied to events, such as the relationship "not more probable than" (Savage, 1954, Theorem 1, p. 32).

[14] Savage, 1954, par. 3.3, p. 33 and following.

[15] For GR, if the reality can be interpreted as a phase diagram, each measure of probability (classic, frequentist, subjective or ultrasubjective) can be represented as a path along it. Notice that all paths start from and end at the same starting points, the absolute necessity and impossibility (which correspond to the logical necessity of $p$ given $E$ and to its complement) which share the same meaning for all possible theories of uncertainty (see Georgescu-Roegen 1958, par. vi 1, 2, and figure 6-2). This point seems to be an extension of the Keynesian interpretation of probabilities (Keynes 1921).

[16] Adjusted from the example in Georgescu-Roegen 1958, p. 266. Notice that it is an early (and more structured version) of the 2-colour Ellsberg paradox (Ellsberg, 1961).

[17] Suppose for simplicity that she chooses the same bet, i.e., $p$ is equal for all $E$.

[18] Throughout the paper it is assumed that preferences are set on expectations. However, defining preferences on expectations is equivalent to setting preferences on evidence, whenever the same $i$ is to choose among the same $p$, as in the example.

of what GR defines the *credibility* of each probability measure, that depends on the "quality" and amount of information $i$ has.

This stimulate us to interpret GR's decision making criterion as a lexicographic one, composed of a probabilistic judgment and its subjective qualitative component, namely, its credibility.

This point will be further discussed in the next section. However, before doing so, we need to clarify what credibility is (or better what we belive GR meant it to be). Denote $m \in \mathbb{N}_+$ as the number of white ball drawn from an urn, $n \in \mathbb{N}_+$ the number of black, and let $U_{m,n}$ indexing urns according to the amount of $m$ and $n$ extracted from it. Assume that the prediction $p$ is: "the next ball extracted will be white". Clearly, the space of all possible urns is $\mathbb{N}_+^2$. A certain frequency can be represented on a $m, n$ space as the straight line whose slope is $\frac{m}{m+n}$. The frequency represents the probability, or, to use GR's terminology, the *betting quotient.*[19] There is another parameter that has to be taken into account, the *credibility* of the betting qoutient. This should not be intended as a numerical parameter that measure the "degree of uncertainty" (Dow and Werlang, 1994, Marinacci, 2000), ranging from risk to Knigtian uncertainty, according to the amount of observation available; if this were the case, GR's expectation could be reduced to a capacity, i.e., a non-additive probability. The crucial point is that the credibility embeds a subjective qualitative aspect that makes impossible to fuse it together with the probability into a single capacity measure. Consider the following example, proposed by GR, in which he immagined an agent who is asked to choose among bets having the same frequencies:

**Example 3** *Suppose there are four urns ($U_{0,3}$; $U_{0,6}$; $U_{3,0}$; $U_{6,0}$), whose evidence can be summarized as: $E_1 : m = 0, n = 3$; $E_2 : m = 0, n = 6$; $E_3 : m = 3, n = 0$; $E_4 : m = 6, n = 0$ . Let $p$ be: "next ball drawn is white" and assume that the consequence of $p$ is positive (betting on white is the most desired outcome). An individual $i$ will typically rank preferences between $E_1$ and $E_2$ and between $E_3$ and $E_4$ as $E_1 \succ E_2$, $E_4 \succ E_3$, even though the probability (the "betting quotient") is the same between both pairs of urns.*

This example shows that there are two dimensions of the credibility of a betting quotient. The first one referres to the amount of observation the decision maker has, i.e., $m+n$. Let us call this dimension of the credibility its

[19] The justification for this is the (weak) law of large numbers (see GR, 1958, p. 268, l. 17).

*index*. The second one depends on whether the evidence is favorable or not, in the sense that the evidence agrees with the desired one: "This principle also sees intuitive [...]. The individual has a stronger belief in the hypothesis that the urn $U_{0,6}$ contains only nonwhite balls than in the same hypothesis for $U_{0,3}$" (Georgescu-Roegen, 1958, p.268).[20]. This second dimension of credibility is its *favorableness*. We can therefore define credibility as the composed function:

$$C(\varepsilon) = C(\iota(\varepsilon), f(\varepsilon)) \tag{2}$$

defined on expectatons,[21] where the *index* of credibility is $\iota(\varepsilon)$, that depends on the amount of evidence $m + n$, and the second dimension $f(\varepsilon)$ is the *favorableness* of the credibility that depends on whether the evidence $m + n$ agrees or not with the agent's desired outcome.

# 3 Lexmin Utility and the Ellsberg Paradoxes.

On the basis of our interpretation of GR we have defined *credibility* as a parameter, depending on evidence, that is related to the betting quotient without being a substitute for it. On the other hand, according to GR, the betting quotient too is a subjective parameter in the sense that it depends on $i$'s subjective utility; therefore, it should be intended as an expected utility rather than an expected value: "[Ramsey, (1926), p. 172 ff.] proposed to measure the subjective probability by the betting quotient the individual is willing to accept on the given uncertain events, [...]. But Ramsey [...] realize[d] that in order to obtain the correct measure, the betting quotient has to be expressed in terms of utility, not in terms of money" (Georegescu-Roegen, 1958, p. 263).

These arguments lead us to follow the intuition of GR and propose a model of decision making under uncertainty based on GR's arguments. On the basis of our reconstruction of his analysis, the model can be formalized as a lexicographic utility function that depends hierarchically on the expected value of a certain expectation and its credibility. Let us see how. Define the

[20] This principle is sufficiently general to apply to all frequencies, and not only to extreme ones as in Example 3

[21] Recall that for a given $i$ and a given $p$, there is a biunivocal correspondence between evidence and expectations, and thus setting choices on expectations is equivalent to defining them on evidence.

space of all expectations as $\Theta$, which can be intended as a space of lotteries whenever expectations are of the type described in example 2,[22] but that in general need not to be restricted as such and suppose that a function $U : \Theta \to \mathbb{R}^2$ exists. We can represent GR' decision making model by means of a lexicographical utility function $U$:

$$\varepsilon_1 \succeq \varepsilon_2 \Longleftrightarrow U(\varepsilon_1) \geq_{GR} U(\varepsilon_2) \tag{3}$$

where $\geq_{GR}$ is the following lexicographic[23] ordering:

$$U(\varepsilon_1) \geq_{GR} U(\varepsilon_2) \Longleftrightarrow \begin{cases} \exp(\varepsilon_1) > \exp(\varepsilon_2) \\ \exp(\varepsilon_1) = \exp(\varepsilon_2), \quad C(\varepsilon_1) \geq C(\varepsilon_2) \end{cases} \tag{4}$$

*exp* indicates the probabilistic expected value and $C$ is the credibility function $C : \Theta \to \mathbb{R}$, defined in Equation 2. It is possible to prove[24] the existence of the lexmin function in 3 for quite general structures[25] for $\Theta$ (which include the lottery space of example 2 as a subset), provided that Von Neumann and Morgenstern axioms holds w.r.t. the preference relation defined in 4, except for the Archimedean one.

We can represent graphically this decision making criterion in the space $m, n$. See figure 1.[26] Along a straight line (starting from the origin) an agent has the same betting quotient. Preferences are represented by the arrows: the first criterion, the betting quotient, is represented by the choice among straight lines, denoted by the continuous arrow. Thus, in figure 1, $b \succ a$ since $\frac{m_b}{m_b+n_b} > \frac{ma}{m_a+na}$. The second criterion, the credibility, is denoted by dashed arrows and represents choices along a straight line. Both the index of credibility and its favorableness are used to compare any two points along the line (such as $a$ and $a'$, $b$ and $b'$); favorableness, in particular, specifies the direction of the arrow, i.e., whether it points towards the origin or away from it. From this, it should be self-evident that the index of credibility and

[22] When we say that expectations can be intended as a lottery space in Example 2 we mean that numeric probabilities (the betting quotient) exist or can be inferred (according to one theory or another) for all four urns of the example. This is not in contrast with GR's assumption about the "general" non-measurability of expectations, provided that any measure "leaves room" for a qualitative residual (captured by the credibility).

[23] Complete, antisymmetric, preordering.

[24] See Martinez-Legaz, 1998, Theorem 5.2.

[25] It is sufficient that $\Theta$ is a mixture set. See Martinez-Legaz, 1998, p.357.

[26] Adapted from Figure 3-3 in GR 1953, p. 210 and Figure 6-3 in GR 1958, p. 269.

the favorableness of credibility are not to be taken as two distinct criteria in the lexmin utility function, i.e., are not the second and third moment of the process of elicitation of preferences, but are both encompassed in the second component of the lexmin, namely, in the credibility of the expectation.

[figure 1 about here]

The lexmin utility of Equation 3 appears to be further we can go in formalizing GR's intuitions without providing an axiomatic basis for its foundations.[27] Here, we intend to show that, if our conjectures are to be agreed on, the GR utility function in 3 has some interesting properties which positively justify its adoption as a criterion of decision making under uncertainty. In particular, we show that it is coherent with the patterns of behavior highlighted by the Ellsberg paradoxes.

Consider the following version of the paradox:

**Example 4 (Ellsberg two-colors paradox)** *Suppose that $i$ has four expectations: $\varepsilon_1 = (E_1, p_1)$, $\varepsilon_2 = (E_2, p_1)$, $\varepsilon_3 = (E_1, p_2)$, $\varepsilon_4 = (E_2, p_2)$; where evidences are: $E_1$ : "in the urn $U_1$ one half of the balls are red and one half are black", $E_2$ : "in the urn $U_2$ there are some red and some black balls"; and previsions are: $p_1$: "next ball drawn is red"; $p_2$: "next ball drawn is black", where it is assumed that the consequence of each $p_i$ is positive (guessing right the prevision is the most desired outcome). $i$ will typically rank expectations as $\varepsilon_1 \succ \varepsilon_2$, $\varepsilon_3 \succ \varepsilon_4$, even though the probability (the "betting quotient") is the same for all urns.*

The utility function defined in Equation 3 "solves" the paradox, in the sense that its ranking of preferences agrees with the observed (or conjectured) ranking of the Example:

**Claim 1** *The ranking of the utility defined in 3 w.r.t. expectations defined in Example 4 is $U(\varepsilon_1) > U(\varepsilon_2)$, $U(\varepsilon_3) > U(\varepsilon_4)$, since $exp(\varepsilon_1) = exp(\varepsilon_2)$, $C(\varepsilon_1) > C(\varepsilon_2)$; $exp(\varepsilon_3) = exp(\varepsilon_4)$, $C(\varepsilon_3) > C(\varepsilon_4)$.*

The difference in credibility are justified by the observation that the index of credibility for $\varepsilon_1, \varepsilon_3$ is at its maximum[28] while it is null for $\varepsilon_2, \varepsilon_4$,[29] (favorableness is equal across these expectations). A similar argument can be made about the three-coulor version of the paradox:

[27] Which is far beyond the scope of our paper.

[28] It is infinite, since it corresponds to the frequency limit.

[29] There is no frequency that justifies the betting quotient of $\varepsilon_2$ and $\varepsilon_4$.

**Example 5 (Ellsberg three-colors paradox)** *Suppose that* $i$ *has four expectations:* $\varepsilon_1 = (E_1, p_1)$, $\varepsilon_2 = (E_1, p_2)$, $\varepsilon_3 = (E_1, p_3)$, $\varepsilon_4 = (E_1, p_4)$; *where evidence is* $E_1$ : *"in the urn* $U_1$ *one third of the balls are red and two thirds are either black or yellow", and previsions are:* $p_1$*: "next ball drawn is red";* $p_2$*: "next ball drawn is black",* $p_3$*: "next ball drawn is either red or yellow";* $p_4$*: "next ball drawn is either black or yellow", where it is assumed that the consequence of each* $p_i$ *is positive (guessing right the prevision is the most desired outcome).* $i$ *will typically rank expectations as* $\varepsilon_1 \succ \varepsilon_2$, $\varepsilon_4 \succ \varepsilon_3$, *even though* $\varepsilon_1 \succ \varepsilon_2 \Rightarrow \pi_{p_1} > \pi_{p_2} \Rightarrow \pi_{p_3} > \pi_{p_4} \Rightarrow \varepsilon_3 \succ \varepsilon_4$, *which contradicts the observed (or conjectured) rank of expectations if* $\pi_i$ *is a probability.*

In making the following claim, we focus on the first dimension of credibility, namely, its, index. The discussion of favorableness is postponed to the end of this section.[30]

**Claim 2** *The ranking of the utility defined in 3 w.r.t. expectations defined in Example 5 is* $U(\varepsilon_1) > U(\varepsilon_2)$, $U(\varepsilon_4) > U(\varepsilon_3)$, *since* $exp(\varepsilon_1) = exp(\varepsilon_2)$, $C(\varepsilon_1) > C(\varepsilon_2)$; $exp(\varepsilon_3) = exp(\varepsilon_4)$, $C(\varepsilon_4) > C(\varepsilon_3)$.

Our claims hold true if $i$'s behavior does not depend on the expected value of each expectation but on its credibility. A sufficient (yet not necessary) condition for this is assuming that the Principle of Insufficient Reason[31] (IR) holds. In other words, it is as if the agent conceives the betting quotient as the subjective probability, but does not trust completely such a conjecture if it is based only on a logical rule adopted *ex-ante* (such as the Principle of IR), rather than on evidence. Clearly, the Principle of IR can be criticized in several ways.[32] GR hymself was quite critical about it. Discussing Carnap's probability theory (Carnap, 1950) he notices: "it is also clear that his rules [the rules that Carnap claim should be employed to define the a priory probability] [...] involve the Principle of Insufficient Reason. A discussion of these rules will disclose, for the $n$th time, the slippery handles of that principle" (GR, 1958, p 257). Another interesting (indirect) objection to it was posed

[30] See note 36 below.

[31] Also called Principle of Indifference, claims for equal treatment of "similar" cases.

[32] This point is too vast to be discussed here. See Keynes, 1921 and Carnap, 1950. Notice that not all authors share the same critical point about it; witness, for instance, Savage's opinion: "the principle of insufficient reason has been and, I think, will continue to be a most fertile idea in the theory of probability" (Savage, 1972, p.64). See also Sinn, 1980.

by Ellsberg. In his famous article (Ellsberg, 1961) he claims that the notion of "credibility" for GR is similar to the notion of "ambiguity" that he proposes;[33] yet, according to Ellsberg, GR's lexmin preference ordering does not solve a modified version of the paradoxes, if the principle of IR holds, since it does not allow to compensate credibility with expected payoff: "Many subject will still prefer to bet on [$p_1$ in $U_1$] than [$p_1$ in $U_2$] in our [two-color] example, even when the proportion of red to black in [$U_1$] is lowered to 49:51 [...]. But at some point , as the "*unambiguous*" likelihood becomes increasingly unfavorable,their choices will switch" (Ellsberg, 1961, p. 664). We can notice two points about Ellsberg's statement. First of all, according to the analysis we have developed here, Ellsberg's first claim that GR's credibility is somehow similar to his concept of ambiguity does not appear correct.[34] In fact, credibility for GR is a more complex concept than Ellsberg's ambiguity: the latter depends on the degree of confidence about the probabilistic judgment (see Eichberger and Kelsey, 1999), while the former has two dimensions, the index ad the favorableness of the evidence, which depend on the amount of evidence and the relationship between the betting quotient and the desired outcome. The second reply to Ellsberg's observation involves the problem of whether in settings such as those suggested by the modified version of the two-color paradox it makes sense to assume, like Ellsberg conjectures that GR does, that $i$ keeps on maintaining a 50:50 expectation about the drawing of red in the unknown urn. We believe that Ellsberg conjecture about GR's model is not fully convincing. The justification for a 50:50 expectation on the ambiguous relies on the Principle of IR, if this is to be taken thoroughly as a compelling argument in favour of equidistribution of probabilities across mutually exclusive events that are supposed to be *equally likely*. It is apparent that such an interpretation of the Principle would be a tautology.

We believe that we can relax this assumption and still show that the pattern of preferences supposed by Ellsberg agrees with GR's one, provided that the model is "closed" by means of some extension of the Principle that is justified on the basis of some logic and positive rationale. Recall that the

[33] Ellsberg's decision making criterion has been axiomatized by Eichberger and Kelsey (1999), who have shown that it can be represented by means of a Choquet Integral, given some "degree of confidence" on the probabilistic measure $\pi$.

[34] However, it has to be noticed that Ellsberg himself admitted that " These highly pertinent articles [Geogescu-Roegen 1954 and 1958] came to my attention only after this paper had gone to the printer, allowing no space for comments here" (Ellsberg, 1961, note 8, p. 659).

rationale behind the IR is that *similar cases* should receive the same treatment if there is no reason (evidence) that claims for a deviation. However, the definition of "similar cases" is not univocally determined *ex-ante*. It is generally assumed that the category of similarity refers to events *within* urns (i.e. within the same level of uncertainty); however, we might as well assume that $i$'s consider similarity *across* urns (i.e. between choiches that embed different levels of uncertainty) in all those cases in which there is no evidence that justifies a different composition of the urns. In other words, we can suppose that when $i$ is asked to compare two urns (where the betting quotientis is known for just one of them) she assumes that the frequencies of the unknown one correspond to those of the observed one if there is no evidence that contradicts it. Let us call this principle as Principle of Comparability. It can be regarded as an extension of the Principle of IR that takes into account the psychological principle according to which individuals, when analyzing some uncertain problem, refer to the most similar case they have in mind to make a decision about it.[35]

We can easily show that GR's utility function solves the modified paradox,[36] provided that the principle of Comparability replaces the Principle of IR. Define $\varepsilon_n$ as the vector of expectations about $p_1$ (drawing a red ball) from $U_1$ in Example 4 for a sequence of evidence $E_n$ in which, in each step, a black ball replaces a red one in urn $U_1$ (i.e., $E_1 : m = 49, n = 51$; $E_2 : m = 48, n = 52$; etc.), and let $\varepsilon_n^*$ be the vector of expectations about $p_1$ from $U_2$ that is pair-wise compared with each $E_n$. If the principle of IR was to hold, $\varepsilon_n^*$ would be a sequence of $\frac{1}{2}$, and Ellsberg's opinion would be verified. However, by adopting the Principle of Comparability we have that $\exp(\varepsilon_n) = \exp(\varepsilon_n^*)$; $C(\varepsilon_n) \neq C(\varepsilon_n^*)$. Recall equation 2. Credibility depends on two dimensions: the credibility index (i.e. the amount of observable frequency that justifies a certain betting quotient), and the favorableness, i.e., whether the betting quotient is against or in favor of the desired result. We can suppose, therefore, that there exists a certain threshold of expectations $\hat{\varepsilon}^*$ such that $\forall \varepsilon_n^* > \hat{\varepsilon}^*$, $\frac{\partial C}{\partial \iota}|_{\bar{f}} > \frac{\partial C}{\partial f}|_{\bar{\iota}} \Rightarrow C(\varepsilon_n) > C(\varepsilon_n^*)$; $\forall \varepsilon_n^* < \hat{\varepsilon}^*$, $\frac{\partial C}{\partial \iota}|_{\bar{f}} < \frac{\partial C}{\partial f}|_{\bar{\iota}} \Rightarrow C(\varepsilon_n) < C(\varepsilon_n^*)$. In words, for expectations above the threshold the dislike of such an expectation due to its low index of credibility

[35] The literature about psychological effects in choice is too vast to be reviewed here (see Kahneman and Tversky, 2000 for applications to decision making under uncertainty). Notice that our Principle of Comparability reflects the spirit (yet not the formalization) of the Case-Based Decision Theory of Gilboa and Schmeidler (2001).

[36] Notice that our claims 1,2 would not be affected.

(as compared to the infinite credibility of the corresponding $\varepsilon_n$) dominates the favour that low-credibility expectations have with respect to high-credibility ones when they are made w.r.t. unfavorable statements (i.e. whenever the more credible betting quotient represents a "bad news"), and therefore the overall credibility function of $\varepsilon_n^*$ is lower than the credibility of the "known" expectations (those drawn upon the urn of known composition). The reverse is true for expectations below the threshold. It follows that (our interpretation of) GR's model, extended to encompass the Principle of Comparability, solves Ellsberg paradoxes and is immune to the criticism to GR's decision making criterion made by Ellsberg himself. It is also true that our claims 1 and 2 still hold[37]; this justifies our claim that the principle of IR is not necessary, and that can be usefully replaced by a different *a priori*, such as the Comparability Principle.

# 4 Concluding Remarks.

Even if evidence $E$ is a primitive of the expectation, we already noticed that GR intended it in an operational way, i.e., as the amount of evidence that $i$ is able to recall at a certain time to make a decision about $p$; it is, thus, a subset of the whole amount of knowledge that *we* suppose that $i$ can hold. This leaves open the question whether such a definition is coherent with the model we have outlined above for representing $i$'s expectations. The problem must be tackled in the mark of GR's methodological approach to the scientific method. We already highlighted GR's distinction between *arithmomorphic* and *dialectical* concepts, where the former are logic (as distinguished from taxonomic or lexicographical) representations of the latter. The model we have set is, by its very nature, *arithmomorphic*. However, the definition of *arithmomorphism* as the realm of Logic, in which concepts *do not overlap,* imposes on the decision maker $i$ very stringent constraints on the type of knowledge we can assume that she can hold, leaving no room for contexts in which $i$ is *unaware* of some of the consequences.

[37]For claim 1, favorableness is constant since the betting quotient is $\frac{1}{2}$. Claim 2 holds true if the betting quotient for $p_2$ is above the threshold below which $f$ dominates $i$. This seems to us a viable conjecture: we very much doubt that there would be any paradox if the odds of the red ball were (far) below 1/3.

Let us consider this point more carefully.[38] It is common[39] to represent the information that an agent holds by means of an accessibility function that specifies the set of those *states* (elements, propositions, whatever constitutes the domain of knowledge) across which she "cannot distinguish". In such a setting, information defines for each set an equivalence class about the knowledge that $i$ holds and, as it is well known, equivalence classes are either disjoint or coincident: there cannot be any overlap of the boundaries of knowledge.[40] This implies that one and only one of the following statement must be true: $i$ believes with certainty in a given proposition; she believes with certainty in its opposite; she is not sure about it. There is no room for *unawareness*[41] of a given state, defined as that situation in which some information that exist (i.e., it can be logically defined or inferred) simply does not come to the agent's mind. Without unawareness there is no room for what the agent cannot imagine or recall (Dardi, 2004). This implies that in the model outlined above it is supposed that $i$ must know what she does not know, excluding the possibility of representing uncertainty about events that may happen but that *ex ante* were not even conceived.

This point appears clear in the applications we have considered in section 3, where we needed to suppose that some *a priori* probability existed; the only room for uncertainty was in the credibility of these measures, that depend on the frequency that support it and its favorableness only. This can be perfectly suited for a limited definition of uncertainty, namely, uncertainty about which measure should be attached to the states of the world (as it happens in Ellsberg's examples); it cannot be applied to uncertainty about whether the representation model is complete or not and what it is that it does not capture. To say it using GR's terminology, our reconstrction does not capture the uncertainty about the boundaries of the dialectical reality (which cannot be captured by any arithmomorphic representation). Obviously, this does not reduce the importance of using arithmomorphic *simile*s: " no [one ...] has ever denied either the unique ease with which thought

[38] We maintain the discussion at an informal level. For an analytical survey of the logic assumptions embedded in modellistic representations and their epistemic consequences see Dardi (2004).

[39] At least in game theory, where problems of knowledge (and common knowledge) have been thoroughly analyzed (see, for instance, Fudenberg and Tirole, 1991).

[40] In other words the information sets are a partition of the available knowledge.

[41] For the impossibility of using the accessibility function in case of unawareness see Dekel *et al.* 1998.

handles arithmomorphic concepts or their tremendous usefulness. For these concepts possess a built-in device against most kind of errors of thought that dialectical concepts do not have" (Georgescu-Roegen, 1966, p. 27). However, it does lower both the interpretation of the model as a positive analysis of the reality and its viability as a normative construction.

Clearly, GR himself was well aware of this problem: "there is a limit to what we can do with numbers, as there is to what we can do without them" (Georgescu-Roegen, 1958, p. 275). In other words, " [...] there seems to be no other recommendation for dealing with Knightian uncertainty than the common advice: "get all the facts and use good judgment". But what is "good judgment"? The concept seems to resist any attempt at an objective definition that also would be operational *ex ante*. [...] Together with gathering, presenting and analyzing in a logical fashion as many facts as possible, to detect and to use good judgment constitute the only mean by which we can respond to living without divine knowledge in an uncertain world" (Georgescu-Roegen, 1958, p. 275).

Perhaps, it was GR's self-awareness of the limit of his own analysis that may have restrained many scholars to follow his example and that has discouraged researchers to undertake the difficult task to understand GR's analytical framework and setting it within his coherent yet unorthodox methodological approach.[42] However, we believe that, as for the other parts of GR's analytical *corpus*, his lessons about expectations and uncertainty will provide a fertile ground of analysis, rich of fruitful insights and methodological hints, for those who will like to get inside the deep nature of the problem of expectations.

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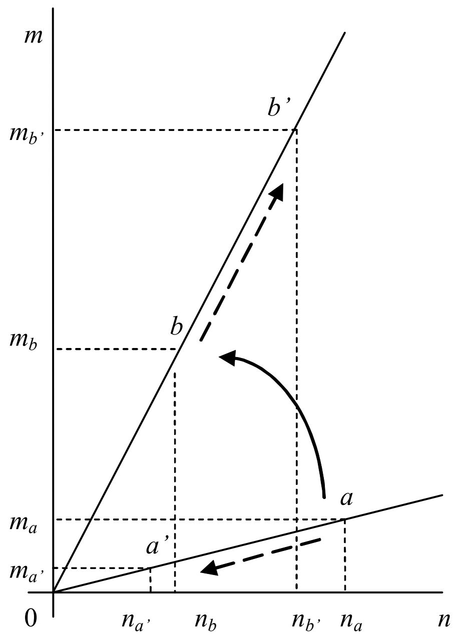


Figure 1: