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JOHN MAYNARD KEYNES AND JOHANNES VON KRIES*

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This essay sheds light on the fundamental influences of a German logician on Keynes' ideas on uncertainty, a source referred to explicitly by Keynes as inspiration for his "weights of the arguments". This study suggests that many of Keynes' most noted ideas on uncertainty, such as the existence of non-numerical probabilities as well as the "weights of the arguments", date back to Johannes von Kries, 1886.

Apart from filling an apparent gap in the history of economic thought, the historical reconstruction put forth in this essay supports an innovative view of Keynes' ideas on uncertainty. It appears, in fact, that non-probabilistic uncertainty is basically cognitive in nature, due essentially to the imperfection of the analogies drawn by the human mind in its attempt to frame empirical experience by classifying empirical facts into mental categories.

* Although I assume full responsibility for the ideas expressed in this essay, I wish to express my gratitude to Marco Dardi and to Giorgio Rampa for the stimulating discussions I had with them, and to acknowledge the influence I received from these two articles:

DARDI, M. (1991), "Interpretazioni di Keynes: logica del probabile, strutture dell'incerto", *Moneta e Credito*, n. 173, pp. 59-88.

LUNGHINI, G. and RAMPA, G. (1996), "Conoscenza, equilibrio e incertezza endogena", *Economia Politica*, a.XIII, n. 3, pp. 435-475.

1. Introduction

Although any modern interpretation of *The General Theory of Employment, Interest and Money* focuses on uncertainty, in this work Keynes did not explicitly define what this concept meant to him. There is merely a clue to it in a footnote at the beginning of Chapter XII, where he investigates the process of collective expectations formation (Keynes 1936:148): "By 'very uncertain' I do not mean the same thing as 'very improbable'" he writes, pointing to chapter VI of his *A Treatise on Probability* (Keynes 1921) for a detailed treatment of the matter.

It is quite disappointing to read at the beginning of chapter VI of this treatise (Keynes 1921:77): "The question to be raised in this chapter is somewhat novel; after much consideration I remain uncertain as to how much importance to attach to it.". The "novel question" is that of the "weights of the arguments", alleged to support probability statements, an issue which has received extensive comment and a huge number of interpretative attempts.

None of these studies, however, has paid attention to the last section of chapter VI of *A Treatise on Probability*, where Keynes reveals to have based his statements on ideas found in two articles by A. Meinong and A. Nitsche (Meinong 1890; Nitsche 1892). Examination of these papers is important, because both appear to be nothing more than comments on a book that dates back to 1886: Johannes von Kries, *Die Principien der Wahrscheinlichkeitsrechnung* (von Kries 1886).

Von Kries is, in many respects, an outsider in the literature on probability, reflecting possibly a rather idiosyncratic scientific curriculum among his probabilist peers. Johannes von Kries, from 1880 a professor with the University of Freiburg, was originally a physiologist. From a concern with the functioning of sensory organs he was naturally led to issues of a more psychological nature, such as perception and cognition, and from there he moved on to logic and probability theory.
In his youth Johannes von Kries happened to make practical use of probability theory in order to assess the effectiveness of new drugs: in such situations, the crucial issue is not so much to count the fraction of recoveries, as to evaluate the similarity of a huge number of diseases and cures in order to define a suitable set of "events". Von Kries also remarked how closely this situation paralleled that encountered by Wilhelm Lexis in his evaluation of mass behaviour (Lexis 1877), as well as how different it was from games of chance, where the set of possible events is a given.

This experience, so different from that of mathematicians who depart from an analysis of games of chance, led him to formulate a probability theory where the crucial issue is the construction of a possibility set by drawing analogies among empirical experiences. Digging into von Kries' work one can discover non-numerical and non-comparable probabilities, as well as a possible forerunner of the "weights of the arguments". One can also realise that his interpreters, Meinong and Nitsche, misunderstood von Kries' innovative ideas and confused them with more standard issues, and that a large part of the misinterpretation which permeates their articles later passed on to Keynes' work.

This essay aims to reconstruct the development of the idea of non-numerical probability from von Kries to Keynes, and to illustrate the place that the "weights of the arguments" have in it. Section 2 is devoted to a brief exposition of von Kries' ideas, section 3 illustrates the alterations these ideas underwent in the hands of Meinong and Nitsche, while section 4 examines the three available versions of *A Treatise on Probability* in order to assess to what extent von Kries' ideas were accepted by Keynes. Finally, section 5 revisits some widespread interpretations of Keynes' ideas on uncertainty in light of von Kries' work.

2. Johannes von Kries

Die Principien der Wahrscheinlichkeitsrechnung by Johannes von Kries (The Principles of Probability Calculus) must have enjoyed a wide following after its first publication in 1886, if in 1927 a reprint was necessary. Nevertheless, this reprint of the 1886 edition had a new preface where - among else - von Kries laments a half-hearted reception of his "*Spielräume* theory" among logicians. Instead, in retrospect, it is precisely the idea of *Spielräume* - an expression which could be translated imperfectly as 'possibility spaces' - which may be von Kries' most original contribution to probability theory. It is exactly on this issue that he is likely to have influenced Keynes' ideas on uncertainty.

Von Kries' ideas cannot be understood having in mind the usual opposition between subjective and objective views of probability. Both views, in fact, draw from the assumption of a given set of possible events, be they in the empirical reality or in an individual's mind. Von Kries instead tackles the problem of the induction of a set of events from empirical experience to the mind of an individual. This is why his *Spielräume* fit so poorly into our current idea of 'possibility spaces': far from being well-defined objects fixed once and for all, von Kries' *Spielräume* depend on empirical experience as well as on perception, and may have a more or less definite character in different situations.

The view of probability as a logical relationship might be a more suitable framework to approach von Kries. But while this view usually focuses on the relationships among given premises and consequences, von Kries is primarily concerned with the process by which this set of premises and consequences is established from empirical experience. According to von Kries, this process is analogy: by drawing analogies between the observed cases an individual constructs the abstract propositions on which probability relationships can be defined. Hence uncertainty is not exclusively probabilistic: unsound analogy can undermine the validity of conclusions as well. Von Kries' own words might be useful to explain the concept:

> If under certain conditions we observed the occurrence of some fact, and, then, in direct reference to that experience, under conditions that we find to be similar to the previous ones, we expect the same course of events, we draw an analogy. Since this analogy is based on recalling cases already observed that are similar to a present one, it is obvious that its validity increases with the number of these cases, but also that it depends in some non trivial way on the grade and kind of similarity of the past cases with one another, as well as of the past cases with the present one.[1]
>
> (von Kries 1886: 16)

Clearly, von Kries is dealing with such a difficult subject as induction, and his position is to stress that inductive reasoning cannot arise out of mere repetition of the same experience: rather, analogy between slightly different empirical experiences is the source of inductive reasoning. His most original conclusion is that, if analogy is less than perfect, probabilities cannot be expressed in numerical terms. The argument is that less-than-perfect analogy depends on qualitative differences in the features of the observed cases, and that qualitative differences, by their very nature, cannot be described in numerical terms. Again, let us have von Kries himself speak:

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[1]. Wenn wir unter irgend welchen Umständen mehrmals den Eintritt eines gewissen Erfolges beobachtet haben, und sodann unter Umständen, welche wir den früheren ähnlich finden, mit unmittelbarer Berufung auf jene Erfahrung den gleichen Verlauf erwarten, so machen wir einen Analogie-Schluss. Da derselbe sich auf die Heranziehung schon beobachteter Fälle gründet, welche einem gegenwärtigen ähnlich sind, so ist ohne Weiteres ersichtlich, dass seine Sicherheit mit der Zahl jener Fälle wächst, ausserdem aber auch, dass dieselbe in einer nicht einfach angebbaren Weise von dem Grade und der Art der Uebereinstimmung abhängt, welche jene Fälle untereinander und mit dem gegenwärtigen besitzen.

(...) there exist logical relations that link things known with certainty to others for which they provide a large or small probability, without a numerical mass existing for it. Actually only the strict dependency that in deductive reasoning links premise to conclusion can transpose to the second the certainty of the first. With other logical relations it is not so. Less than ever with analogy. If we have observed one or more cases of a certain kind occurring in a certain way, and we expect the same course for a similar case, this expectation does not share the certainty of the premises on which it is based. Taken for granted these premises, this expectation is just more or less probable. But this logical relation cannot be expressed numerically. The probability of an outcome increases with the number of cases that have come to be known, but depends also on the grade and the kind of similarity of these cases, and especially on the similarity between the case the current expectation refers to, and the cases that occurred in the past. But there is no reason to assume that the [similarity] assumptions have the same value.[2]

(von Kries 1886: 26)

In order to transpose this in modern terms let us reflect that, within von Kries' framework, what we call 'events' are not objects that exist in themselves. Rather, they are the product of the classifying activity of a human mind: an individual observes an empirical fact, takes it as qualitatively equivalent to somewhat similar facts that happened in the past, forms a category for these facts which he calls 'event X', and measures the probability of this event with respect to the other events he had defined. But since it is up to the individual to give similarity judgements, the same empirical facts could have been classified in different categories, that would have represented different "events" having different probabilities. Clearly, this classification process has some degree of arbitrariness, and to the extent that the classification of empirical facts into 'events' is arbitrary, numerical probabilities cannot be expressed. To do otherwise would mean comparing empirical facts of different qualities, which is the same logical error as summing apples and pears. Furthermore, let us also note that the very

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[2]. (...) Verhältnisse des logischen Zusammenhanges giebt, welche, indem gewisse Dinge als sicher gelten, für andere eine mehr oder weniger grosse Wahrscheinlichkeit constituiren, ohne dass für diese ein numerisches Maas existirt. In der That kann ja nur der völlig feste Zusammenhang, welcher bei dem deductiven Schlusse die Conclusion an die Prämissen knüpft, die Sicherheit dieser letzteren unverändert und ohne Abzug auf jene übertragen. Bei anderen logischen Verhältnissen ist dies anders. So zunächst beim Analogie-Schlusse. Wenn wir einen oder mehrere Fälle von gewisser Art in einer bestimmten Weise haben verlaufen sehen und für einen ähnlichen Fall den gleichen Verlauf erwarten, so teilt diese Erwartung ja selbstverständlich nicht die Sicherheit derjenigen Voraussetzungen, auf welche sie sich gründet; sie ist - jene als sicher angenommen - immer nur mehr oder weniger wahrscheinlich. Das hier Statt findende logische Verhältniss hat nun gar nichts zahlenmässig Darstellbares; die Wahrscheinlichkeit des früher beobachteten Erfolges steigt mit der Zahl der bekannt gewordenen Fälle und hängt ausserdem von dem Grade und der Art der Aenlichkeit ab, welche die einzelnen Fälle untereinander und insbesondere der gegenwärtig zu beurteilende mit den früheren zeigt. Für eine Aufstellung gleichwertiger Annahmen aber fehlt hier jeder Anhalt.

arbitrariness of analogy statements allows the construction of alternative probability relations from the same empirical evidence: these probabilities, defined on qualitatively different sets of 'events', can be neither expressed numerically, nor compared to one another.

Sometimes numerical probabilities can, obviously, be expressed: these are the cases where empirical facts are qualitatively equal one another, so that it is not necessary to draw any analogy at all. It is the case, for example, of the games of chance we usually refer to when we talk about probability. However, situations in general can be very different from one another, and even the reliability of numerical probabilities can be a matter of degree. In the last chapters of his book, von Kries examines how sound numerical probabilities are in games of chance, in the physics of his time, namely, thermodynamics, and in the social sciences. Towards the end of the book, we find what perhaps can be considered a forerunner of Keynes' "weights of the arguments".

In fact, at a certain point von Kries acknowledges that, in practice, it can be useful to express uncertainty numerically even when the arbitrariness of analogy undermines the theoretical foundations of any numerical probability, and that this is what individuals actually do. The rationale for doing this, according to von Kries, is the application to non-numerical probabilities of a subjective *Taxirung* (evaluation) that expresses the confidence in the reliability of the analogies underlying them. However, von Kries worried about the subjective character of this procedure:

> We can now examine the question of whether probabilities of any kind can be expressed in numerical form, so that they can be compared with actual numerical probabilities. It seems possible to evaluate a generic probability in such a way as to produce a numerical probability that has the same degree of certainty. In principle there is no objection to such a procedure, but it is necessary to be aware of what it means and of the difficulties that underlie it. When we evaluate numerically the probability of an analogy at 5/6 and then consider a case of an expectation where the dimensions of the possibility spaces of alternative outcomes are in the ratio 1:5, these are completely heterogeneous contexts, incomparable by their very nature. Consequently, their point of comparison is merely a psychological one. What is compared is the psychological certitude in the two contexts: this is all they have in common.[3]

[3]. Es darf nun die Frage aufgeworfen werden, ob nicht eine zahlenmässige Darstellung auch anderer, ganz beliebiger Wahrscheinlichkeiten in der Weise Statt finden kann, dass dieselben mit eigentlich numerischen verglichen werden; es erscheint denkbar, jede Wahrscheinlichkeit zu taxiren, indem man diejenige zahlenmässige Wahrscheinlichkeit angiebt, welche mit ihr gleichen Sicherheits-Grad zu haben scheint. Gegen derartige Abschätzungen ist nun zwar principiell gar nichts einzuwenden; es ist aber notwendig, wol zu beachten, welche Bedeutung sie haben und welchen Schwierigkeiten sie unterliegen. Wenn wir den Wahrscheinlichkeits-Wert eines Analogie-Schlusses zahlenmässig taxiren und auf $5/6$ angeben, so sind die logischen Verhältnisse jener Analogie und einer freien Erwartungsbildung, bei welcher die Spielräume in dem Grössen-Verhältniss 1:5 stehen, vollständig heterogen und ihrer Natur

(von Kries 1886: 181)

Interestingly, von Kries later remarks that what makes some probabilities non-numerical is the subjective character of analogy relations, just like in the *Taxirung* whereby any probability can be made numerical, but subjective.

The subjectivity in establishing what we must consider as equally possible was the essential reason for limiting the numerical form of probability to particular cases. This subjectivity arose from the indeterminacy of certain logical relations, and it is documented by the fact that no grade of psychological certainty can be accurately attributed to any of these relations. Consequently, our *Taxirung* also lacks the essential condition so that for each logical relation there corresponds a precise and easy-to-imagine grade of subjective certainty.[4]

(von Kries 1886: 183)

Thus the importance von Kries attributed to subjective evaluation (*Taxirung*) was actually very little, even if he recognised that it was able to yield numerical probabilities in any situation. What he did not like was the very fact that this evaluation was subjective, as were the probabilities this process yielded.

What remained of all these issues in the articles written by A. Meinong (1890) and A. Nitsche (1892)? Very little, actually, together with some misunderstanding.

3. Meinong and Nitsche

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nach unvergleichbar. Der Vergleichspunkt beider ist demgemäss ein lediglich psychologischer; verglichen wird die psychologische Gewissheit, welche in dem einen und dem anderen Falle Statt findet; diese ist das einzige beiden Fällen Gemeinsame.

[4]. Die Willkürlichkeit in der Ansetzung dessen, was uns als gleich möglich gelten soll, war der wesentliche Grund, welcher die numerische Gestaltung der Wahrscheinlichkeiten auf besondere Fälle einschränkte. Diese Willkürlichkeit, welche aus der Unbestimmtheit und Unbestimmbarkeit gewisser logischer Verhältnisse entspringt, documentirt sich auch darin, dass es gar nicht angebbar erscheint, welcher Grad psychologischer Gewissheit irgend einem solchen mit Recht zukomme. Und demgemäss fehlt es auch für die in Rede stehende Taxirung an der wesentlichen Voraussetzung, dass sich mit jedem logischen Verhältniss ein bestimmter, deutlich vorstellbarer Grad subjectiver Gewissheit factisch verbände.

Meinong's article (Meinong 1890) is a review of von Kries' book, followed by Meinong's own comments.

It is, unfortunately, a biased review. Analogy has very little space, to the point that it is seldom mentioned. Nevertheless, von Kries' idea of the existence of non-numerical probabilities is retained: but in this way, the concept is as if suspended in space.

Von Kries provided examples of a number of concrete situations where analogy plays a key role in probability statements, examples taken mainly from the evaluation of the symptoms of possible diseases in order to assess the survival probabilities of patients, and from the evaluation of mass behaviour in order to assess the probabilities of socio-economic regularities. But he also carried out a thorough examination of games of chance, anxious to highlight which conditions make probabilities numerical. None of the examples taken from sociological and medical statistics survived Meinong's survey; on the contrary, some examples of games of chance were taken to be the main content of von Kries' work. The following example is typical of the issues von Kries was interested in; at the same time, it helps to explain how his commentators could shift away from his original concern with analogy (von Kries 1886: 8).

Let A and B denote two urns. Let us suppose that a person who must extract one ball from both urns knows that urn A contains an equal number of white and black balls, while all she knows about urn B is that the balls it contains can be either white or black. According to what von Kries called *Princip des mangelnden Grundes* (principle of lacking reason), this person should assume in both cases equal probabilities 1/2 to extract a white or a black ball. Yet it is intuitively evident that the uncertainty relative to urn A is not the same as the uncertainty relative to urn B.

This conceptual experiment reappeared about one century later in the economic literature, and is known today as "Ellsberg's paradox" (Ellsberg 1961). Von Kries' discussion of "Ellsberg's paradox" is definitely unorthodox, and worth reporting:

> We could say equally well that the most simple assumption is that the urn is either filled with balls of only one colour, or with a mixture of balls of both colours. Hence, if the urn contains 1000 balls we should attach higher probabilities to the urn containing a thousand, five hundred or no black balls than, say, to the urn containing 873 black balls.[5]

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[5]. Wir könnten recht wol auch sagen, dass die Füllung des Gefässes mit Kugeln bloss einer Sorte, und anderseits eine zufällige Durcheinandermischung beider Sorten am ehesten anzunehmen sei; es würde danach, wenn tausend Kugeln vorhanden sind, den Annahmen, dass tausend, dass fünfhundert oder dass gar keine schwarz sei, grössere Wahrscheinlichkeit zugeschrieben werden müssen, als etwa der, dass 873 schwarz seien.

(von Kries 1886: 34)

In light of the rest of the book, it seems reasonable to interpret this passage in the sense that the assumptions which are deemed to be "most simple" by the person extracting the ball is a matter which depends on analogies this person draws with previous situations where she faced another urn containing balls: if we are led to assume a 1:1 proportion between white and black balls, it is only because urns are usually employed to produce equal probabilities of extracting each colour. Meinong, instead, like modern commentators of "Ellsberg's paradox", is not able to go beyond the *prima facie* impression according to which both urns suggest probability 1/2 for each of the two colours (Meinong 1890: 58).

However, the most important part of Meinong's article is the second section where he expresses his personal views. Here Meinong puts forth an example of his own, remarkable because - contrary to the rest of his article - it entails analogy-making, and also because Meinong is led to introduce, besides probability, a second magnitude to characterise uncertainty.

For a geometrically exact die, the probability of obtaining a number greater than 1 is 5/6. Now, if we face for the first time an object that looks approximately like a die, what is the probability of obtaining a value higher than 1? Perhaps we would like to abstain from making any valuation. But suppose practical considerations compel us to take sides: in this case the probability of obtaining a value greater than 1 would also be set at neither more nor less than 5/6. Yet psychologically this case is not the same as the previous example: loosely speaking, this time the judgement has much less certainty. And this certainty is not what we have called "presumption grade", since if this grade had changed, one's position between Yes and No had also. We must recognise that we have to do with presumptions having a higher and lower value.[6]

(Meinong 1890: 71)

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[6]. Für einen geometrisch genauen Würfel betrüge die Wahrscheinlichkeit, im nächsten Wurf mehr als 1 zu werfen, $5/6$. Wie nun wenn wir zum ersten Mal einen Körper vor uns haben, der ungefähr einem Würfel gleichsieht, wie viel beträgt die Wahrscheinlichkeit hier mehr als 1 zu werfen ? Vielleicht möchte man sich einer Vermutung darüber am liebsten enthalten; aber gesetzt, praktische Rücksichten zwingen uns, Stellung zu nehmen, dann wird auch hier weder mehr noch weniger als $5/6$ angesetzt werden können. Aber psychologisch steht dieser Fall dem ersten nicht gleich: vulgär sagt man wohl, die Vermutung habe diesmal viel geringere Sicherheit; doch ist dies nicht dasjenige, was wir oben Vermutungsgrad nannten, denn mit diesem Grade hätte sich ja die Stellung zwischen Ja und Nein geändert. Man erkennt natürlich, daß wir es hier mit der mehr- und minderwertigen Vermutung zu thun haben.

The "presumption grade" Meinong refers to is probability: Meinong said this is the "first dimension" of presumption, a dimension that he characterised by the extreme values 'Yes' and 'No' that it can attain. But there exists also a "second dimension" of presumption, apparently, of which he speaks in an important passage that immediately follows the one quoted above:

(...) the psychic phenomenon of presumption does not appear to span the dimension characterised by the extremes Yes and No only, but also a second dimension, according to which more or less reliability can be attached to a presumption (...)[7]

(Meinong 1890: 72)

Meinong is not able to say anything more precise about this second dimension of presumption, except that the difference between the two dimensions is likely to be of the same kind as the one between 'intensity' and 'quality'.

According to von Kries, probabilities based on objective empirical facts can be non-numerical because analogies by their very nature cannot capture the qualitative features of these facts to their full extent; nevertheless, subjective numerical probabilities can always be computed by means of a subjective evaluation (*Taxirung*) of the non-numerical ones. Von Kries, however, was less interested in the subjective numerical probabilities resulting from this *Taxirung* process. Meinong, instead, looked for a magnitude able to express the extent to which the *Taxirung* process had taken place before a numerical (subjective) probability was available. Unfortunately, during this shift in perspective, von Kries' clarity was lost. It is perhaps due to this lack of clarity that Meinong's article was harshly criticised by Nitsche.

Nitsche's point follows from his epistemological premises, that he coherently retains throughout his article. To be short, Nitsche refuses the status of 'evidence' to thoughts that are present in the mind simply because of lack of contradictory experience (Nitsche 1892: 34-35). He also stresses that von Kries' *Princip des mangelndes Grundes* (principle of lacking reason) should be substituted by the *Princip des zureichendes Grundes* (principle of sufficient reason): in this way - according to Nitsche - empirical experience can only be interpreted in one way, since no alternative interpretations can be entertained just because there is no reason to exclude them. Perhaps, it is more correct to say that Nitsche aims to rule out interpretation *tout court*:

[7]. (...) das psychische Phänomen des Vermutens erweist sich nicht nur in der durch die Extreme Ja und Nein gekennzeichneten Dimension variabel, sondern auch noch in einer zweiten, indem jeder Vermutung mehr oder auch weniger Sicherheit zukommen kann (...)

empirical facts are evaluated for what they are, without any need to make any analogy with past experiences.

Nitsche focuses on Meinong's example reported above, purporting a different interpretation of it: instead of thinking to an observer who evaluates the similarity of 'an object that looks approximately like a die' to 'a geometrically exact die', he thinks to the physical properties that make 'an object that looks approximately like a die' differ from 'a geometrically exact die'. Since the only effect of physical imperfections can be that of changing the probabilities by which the faces of the die appear, the distance of our 'object that looks approximately like a die' from a 'geometrically exact die' can be measured by simply throwing it. Nitsche does concede that uncertainty is not fully expressed by probability, and that a second variable is necessary. But this second variable is, in this example, the number of times the die has been thrown in order to measure probabilities: the second uncertainty variable is the dimension of the sample, in the purest Bayesian tradition.

Evidently, with Nitsche all original insights of von Kries have been lost. The reason seems to be that, unlike von Kries, his commentators focused on games of chance only.

4. John Maynard Keynes

Let us now come to the main point of this essay: To what extent did von Kries, Meinong and Nitsche influence Keynes? To this aim we will scrutinise Keynes' *A Treatise on Probability* (Keynes 1921), as well as its early version, *The Principles of Probability*. This is Keynes' doctoral dissertation, of which we have the final version (Keynes 1908) and a provisional draft (Keynes 1907). In spite of the profound changes that characterise the evolution of Keynes' thought throughout his life, the period 1907-1921 can be considered to be stable enough to ignore the differences between the above texts. Thus, quotations will be drawn indifferently from these three works, according to convenience.

Unfortunately, neither the above texts nor Keynes' handwritten notes are able to tell us to what extent Keynes relied on reading von Kries' original writings, or rather the interpretations Meinong and Nitsche gave of them. However, a careful consideration of

Keynes' philosophical background allows us to draw a fairly faithful reconstruction of Keynes' attitude towards von Kries.

Let us begin with the first criticism Nitsche made against von Kries, as reported in the previous section: that his "principle of lacking reason" should be substituted by the more standard "principle of sufficient reason" in order to avoid multiple interpretations of empirical evidence. Keynes, instead, with his "principle of non-sufficient reason" (Keynes 1907, 1908) or "principle of indifference" (Keynes 1921) writes in essentially the same spirit as von Kries, of whom he reports a number of examples (Keynes 1907: 95-101, 1908: 77-82, 1921: 48-55). These examples, all along the same line of reasoning as von Kries' discussion of "Ellsberg's paradox", aim to show that the "principle of indifference" does not provide a unique way to assign probabilities: true that according to this principle equal probabilities must be attached to all events taken in consideration if no empirical evidence suggests otherwise, but the choice of the events to consider is to some extent arbitrary.

Similarities between Keynes and von Kries are even more striking if we observe that both authors claimed that probabilities are in general non-numerical, and that not all of them are comparable. Moreover, whenever Keynes tries to explain exactly what he means, he talks about induction, analogy, similarity (Keynes 1907: 52-69, 1908: 28-48, 1921: 21-44). Let us report a particularly illuminating passage:

> The type of probability which is clearest in this connection for the purpose of example is that of induction or generalisation. (...) we may sometimes have some reason for supposing that one object belongs to a certain category if it has some similarity to other known members of the category, (e.g., if we are examining the question whether a certain picture is to be ascribed to a certain painter), and the greater the similarity the greater the probability of our conclusion. But we cannot in these cases measure the increase; we can say that the presence of certain peculiar marks in a picture increases the probability that the artist of whom those marks are known to be characteristic painted it, but we cannot say that the presence of these marks makes it two or three or any other number of times more probable than it would have been without them. We can say that one thing is more like a second object than it is like a third; but there will very seldom be any meaning in saying that it is twice as like. Probability is, so far as measurement is concerned, highly analogous to similarity.
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> (Keynes 1908: 32-33)

But although Keynes recognises his debt to von Kries when he discusses the "principle of indifference", he never mentions him in relation to the idea of non-numerical probabilities. In the preface of the first version of *The Principles of Probability*, Boole and von Kries are the only thinkers to whom Keynes acknowledges to "owe a great deal"; but a few pages later, in the appendix to Chapter I, Keynes writes that "(...) Von Kries arrived at his brilliant but distorted theories." (Keynes 1907). Keynes did esteem von Kries' probability theory, but did not embrace it:

I do not agree with the solution whose demonstration is the object of his book, but there can be little doubt that his is by far the most original and important work on this branch of the subject which has been published during the last quarter of a century.

(Keynes 1908: 71)

At a certain point, Keynes seems to promise he will explain the reason of his double-sided attitude towards von Kries:

I do not agree with those of their [the "German logicians", as he had expressed himself a few lines earlier] solutions and qualifications with which I am acquainted; but I shall reserve what criticism I have for a later stage.

(Keynes 1907: 87)

Unfortunately, a footnote immediately specifies:

I regret that this Chapter has not been written. It would have been mainly concerned with a detailed criticism of Von Kries' doctrine of 'Spielräume'. It is useless to attempt an exposition of this elaborate theory briefly.

(Keynes 1907: 87)

This ghost chapter was never written. On the contrary, in *A Treatise on Probability* the previous declarations of critical esteem for von Kries became just a polite recognition of his importance in the general development of probability theory (Keynes 1921: 95), and his name is not even further mentioned in the preface.

Subsequently, we cannot directly read why Keynes could not accept von Kries. Nevertheless we can easily reconstruct his difficulty by comparing the philosophical background of the young Keynes with the idea of probability that had inspired von Kries.

The young Keynes was a follower of the philosopher George Edward Moore, highly influential in the Cambridge environment at the beginning of this century. Moore was a neo-platonic realist, who claimed that concepts like 'beauty' or 'goodness' are simple and undefinable entities of an objective world, with which we get acquainted by direct intuition. The later Keynes of *My Early Beliefs wrote*:

> Moore had a nightmare once in which he could not distinguish propositions from tables. But even when he was awake, he could not distinguish love and beauty and truth from the furniture. They took on the same definition of outline, the same stable, solid, objective qualities and common-sense reality.
>
> (Keynes 1938)

Yet the young Keynes did accept this doctrine, and did apply it to probability theory. Probability relations became for him objects that exist on their own, their perception requiring no interpretation:

> I am inclined to believe that we possess some power of direct inspection in the case of every judgment of probability. By this I mean that relations of probability are things that can be directly perceived, just as many other logical relations are by general admission objects of intuition, so that there is no inherent ground why we should not be directly aware of <u>any</u> particular relation of probability.
>
> (Keynes 1908: 89)

However profoundly these beliefs might have changed by the time of *A General Theory of Employment, Interest and Money*, they were professed by Keynes at least up to the publication of *A Treatise on Probability*: "By 1921, the Moorean nature of his probabilities is clear and unmistakable: they are objective, indefinable, and Platonic." (Bateman 1996: 55).

Von Kries' epistemology was quite different. According to von Kries, to perceive does not merely mean to see 'objects', as e.g. probability relations. Rather, it means to recognise similarities and to trace analogies until empirical evidence is classified into categories that are, to some extent, necessarily arbitrary. Implicit in his treatment of analogy is the conviction that individuals never know all the possible qualities of empirical reality, which might be infinite in number and in any case appear as such to the individual: analogy-making is a way to simplify an enormous amount of different qualities by drawing analogies that can never be perfect. It is the less-than-perfect character of analogy-making that makes probabilities non-numerical, in the sense that they cannot take any objectively determined value (even if a subjective numerical evaluation is always possible).

Keynes devoted the whole Part III of *A Treatise on Probability* to induction and analogy, and there stressed the importance of analogy for induction. Still, 'analogy' is for him a very different concept than it was for von Kries, since Keynes focuses on empirical experiences where only a finite number of qualities can appear:

Now it is characteristic of a system, as distinguished from a collection of heterogeneous and independent facts or propositions, that the number of its premisses, or, in other words, the amount of independent variety in it, should be less than the number of its members. But it is not an obviously essential characteristic of a system that its premisses or its independent variety should be actually finite. We must distinguish, therefore, between systems which may be termed finite and infinite respectively, the terms *finite* and *infinite* referring not to the number of members in the system but to the amount of independent variety in it. The purpose of the discussion, which occupies the greater part of this chapter, is to maintain that, if the premisses of our argument permit us to assume that the facts or propositions, with which the argument is concerned, belong to a *finite* system, then probable knowledge can be validly obtained by means of an inductive argument.

(Keynes 1921: 279)

(...) we can justify the method of perfect analogy, and other inductive methods in so far as they can be made to approximate to this, by means of the assumption that the objects in the field, over which our generalisations extend, do not have an infinite number of independent qualities; that, in other words, their characteristics, however numerous, cohere together in groups of invariable connection, which are finite in number. This does not limit the number of entities which are only *numerically* distinct. In the language used at the beginning of this chapter, the use of inductive methods can be justified if they are applied to what we have reason to suppose a finite system.

(Keynes 1921: 285)

As a logical foundation for analogy, therefore, we seem to need some such assumption as that the amount of variety in the universe is limited in such a way that there is no one object so complex that its qualities fall into an infinite number of independent groups (i.e. groups which might exist independently as well as in conjunction); or rather that none of the objects about which we generalise are as complex as this; or at least that, though some objects may be infinitely complex, we sometimes have a finite probability that an object about which we seek to generalise is not infinitely complex.

(Keynes 1921: 287)

Dealing with a finite number of qualities, Keynes can understand analogy in terms of collecting objects that share some of these qualities, generalisation as a restriction of the number of common qualities, induction as generalisation of the premise and concentration of the conclusion (Keynes 1921: 247-259). Non-numerical probabilities, according to Keynes, occur when it is not clear how many common qualities an analogy is grounded upon:

> There is a vagueness, it may be noticed, in the number of instances, which would be required on the above assumptions to establish a given numerical degree of probability, which corresponds to the vagueness in the degree of probability which we do actually attach to inductive conclusions. We assume that the necessary number of instances is finite, but we do not know what the number is. We know that the probability of a well-established induction is great, but, when we are asked to name its degree, we cannot. Common sense tells us that some inductive arguments are stronger than others, and that some are very strong. But how much stronger or how strong we cannot express. The probability of an induction is only numerically definite when we are able to make definite assumptions about the number of independent equiprobable influences at work. Otherwise, it is non-numerical, though bearing relations of greater and less to numerical probabilities according to the approximate limits within which our assumption as to the possible number of these causes lies.
>
> (Keynes 1921: 288)

The neo-platonic Keynes assumed it was possible to identify some kind of 'elementary particles' of empirical experience, elementary qualities, finite in number, by which every empirical experience would be constituted. Hence, Keynes' non-numerical probabilities are due to a lack of knowledge about the true composition of empirical experience in terms of its elementary particles. Contrary to von Kries, no discourse about the impossibility of a numerical evaluation of similarity is implied.

Whenever Keynes speaks about similarity it is only to make an example: his aim is simply to say that probabilities can be non-comparable just as qualitatively different objects can not. Keynes may draw analogies to explain his non-numerical probabilities, but the subjects who should perceive these probabilities do not. For example, the passage on similarity quoted above (Keynes 1908: 32-33) ends saying that "Probability is, so far as measurement is concerned, highly analogous to similarity": note that according to Keynes probability is <u>analogous to similarity</u>, not <u>grounded on analogies suggested by similarities</u> as according to von Kries. The following passage is even more explicit:

> But the closest analogy is that of similarity. When we say of three objects A, B, and C that B is more like A than C is, we mean not that there is any respect in which B is in itself quantitatively greater than C, but that, if the three objects are placed in an order of similarity, B is nearer to A than C is. The analogy is specially close because, as in the case of probability, there are <u>different</u> orders of similarity. For instance, a book bound in blue morocco is more like a book bound in red morocco than is one bound in blue calf; and a book bound in red calf is more like the one in red morocco, than is one in blue calf. But there is no comparison between the degree of similarity, which exists between books bound in red morocco and blue

morocco, and that which exists between books bound in red morocco and red calf. I must ask the reader to pay special attention to this illustration, as the analogy between orders of similarity and probability is so great that its apprehension will greatly assist the ideas I wish to convey. At the same time the analogy must not be pressed too far. I do not mean that probability is a kind of similarity, and my only object is to show by example that, when I speak of distances between probabilities, I am using the term 'distance' in a sense which would be equally legitimate as applied to objects in an order of similarity.

(Keynes 1908: 38)

Once the difference between the conflicting epistemologies of Keynes and von Kries is made clear, we can single out from *A Treatise on Probability* a passage that gives us a clue to the 'ghost chapter' Keynes never wrote:

His [von Kries'] discussion of the philosophical character of probability is brief and inadequate, and the fundamental error in his treatment of the subject is the *physical*, rather than logical, bias which seems to direct the formulation of his conditions. The condition of *Ursprünglichkeit*[8], for instance, seems to depend upon physical rather than logical criteria, and is, as a result, much more restricted in its applicability than a condition, which will really meet the difficulties of the case, ought to be.

(Keynes 1921: 96)

Among von Kries' conditions for probabilities to be numerical, that of *Ursprünglichkeit*[8] is the most important: it means that the events to which the probabilities refer must be 'original', 'ultimate', 'elementary', in the sense that it must not be possible to further subdivide them into more basic components. This is what happens in games of chance, where the possible events correspond to the faces of a die or to the numbers of a roulette, but not in the typical situations we encounter when we do medical or sociological statistics, where the definition of the set of possible events is necessarily arbitrary to some extent and where the "principle of lacking reason" does not provide a unique, 'objective' set of probabilities.

It is interesting to remark that, by focusing on what he called "finite systems" where elementary qualities are supposed to exist and to be finite in number, Keynes limits his attention to probabilities that satisfy von Kries' *Ursprünglichkeit*[8] condition. If nevertheless Keynes' probabilities can be non-numerical, it is only because he concedes that the elementary qualities that constitute empirical experience may be

[8]. Substantive derived from the adjective *ursprünglich*, which means 'original'. A translation of *Ursprünglichkeit* with 'originality' would be obviously uncorrect; 'being original', 'being ultimate' or 'being elementary' convey better its meaning.

imperfectly known. Von Kries did not care whether such elementary qualities existed at all: he acknowledged that, in many cases, individuals must behave as if they would not exist, drawing analogies that are imperfect because they collect different qualities in the same container. These analogies are not susceptible to any objective numerical evaluation because the extent to which the qualities of empirical experience are different cannot be quantified, not because the face qualities of empirical experience depend on elementary qualities that may not be known. No wonder that, from Keynes' point of view, von Kries' *Ursprünglichkeit*[8] condition had a too physical flavour and too little logical foundation.

Since according to Keynes non-numerical probabilities arise out of a lack of knowledge of the elementary qualities that constitute empirical evidence, no subjective evaluation can be a remedy. This is obvious, since no subjective evaluation can substitute lacking information. Coherently, Keynes did not accept von Kries' "*Taxirung*".

Keynes resorted to Meinong's "second dimension of presumption" instead. But again, it had to be something objective, in the sense that it must not depend on the way empirical experience is perceived by the human mind. Meinong's example about "an object that looks more or less like a die" does not perfectly suit this scheme, it is still too close to von Kries. Nitsche's shift of concern from the perception of the qualities of an object to the qualities themselves appeared to fit better into Keynes' Neo-Platonism:

> Meinong, who does not develop the point in any detail, distinguishes probability and weight as 'Intensität' and 'Qualität', and is inclined to regard them as two independent dimensions in which the judgment is free to move - they are the two dimensions of the 'Urteils-Continuum'. Nitsche regards the weight as being the measure of the reliability (Sicherheit) of the probability, and holds that the probability continually approximates to its true magnitude (reale Geltung) as the weight increases. His treatment is too brief for it to be possible to understand very clearly what he means, but his view seems to resemble the theory already discussed that an argument of high weight is ‘more likely to be right’ than one of low weight.
>
> (Keynes 1921: 85)

This embrace with Nitsche puts Keynes' probability theory on a dangerous course, however. In fact, the "weight of the arguments" risks becoming an indicator of the dimension of the sample, which is quite a standard Bayesian procedure. Several concrete examples provided by Keynes point in this direction:

Suppose, to take a concrete case, that we are investigating the question of a possible connection between sunspots and commercial crises and are estimating the evidence derivable from recorded instances of proximity, putting on one side other considerations. We begin by possessing information with regard to five crises, and we find that four of these were associated with sunspots and one was not; this we will call case A. Another investigator having before him a different series of five crises finds that three were associated with sunspots and two were not (case B). A third investigator, who is able to examine ten crises, finds that eight were associated with sunspots and two were not (case C). Now the [weight of the arguments on which] the conclusion [is] drawn in case B is, it seems to me, the same as that [of the arguments on which] the conclusion [is] drawn in case A, whereas the [...] probability is manifestly different; and, further, the [weight of the arguments that support] a conclusion regarding some general proposition which is solely based on the inductive examination of five instances is the same in all cases. The [weight of the arguments on which] the conclusion is based in case C, on the other hand, is greater than that in either of the other cases (...).[9]

(Keynes 1908: 50)

Yet the "weight of the arguments" is not just this. "Weight" can be non-numerical and non-comparable, just like probability:

Where the conclusions of two arguments are different, or where the evidence for the one does not overlap the evidence for the other, it will often be impossible to compare their weights, just as it may be impossible to compare their probabilities.

(Keynes 1921: 78)

The measurement of [weight] is not dissimilar in the difficulties it presents to that of [probability]. Only in a limited number of cases is it possible to compare the [weights of the arguments that support] two probabilities in respect of more and less. It is clearly possible where the conclusion in question is the same in each case and the evidence in the one includes and exceeds the evidence in the other (...). (...) Where the conclusions in two [probability relations] are different, or where the evidence for the one is not contained in the evidence for the other, it will often be impossible to compare [the weights of their arguments] (...).[9]

---

[9]. In *A Treatise on Probability* (Keynes 1921) Keynes distinguishes between "probability" and "weight of the arguments" that support a probability statement. Without any change of meaning, in the second version of *The Principles of Probability* (Keynes 1908) he had distinguished between "magnitude" and "value" of probability, and in the first version between "magnitude" and "intensity" of probability. He also notes (Keynes 1907) that he uses "intensity" where Meinong had used "quality", and "magnitude" where Meinong had used "intensity". In order to ease understanding to the modern reader, supposed to be acquainted with the terminology of the *Treatise*, "magnitude of probability" has been replaced by "probability", while "intensity of probability2 and "value of probability" have been replaced by "weights of the arguments" that support a probability.

(Keynes 1908: 49)

Clearly, if the amounts of empirical evidence that support two probability statements are made out of different elementary qualities, neither these probabilities nor their weights can be compared. In the case they can, Keynes' "weight" reduces to some measure of the dimension of the sample upon which probabilities are computed.

We can say that both Keynes and von Kries derive the idea of non-numerical probabilities by the impossibility of comparing qualitatively different instances. Keynes committed himself to what he called "finite systems", for which a kind of 'elementary particles' of empirical evidence could be defined: so according to Keynes probabilities cannot be numerical if they are grounded on pieces of evidence that contain qualitatively different particles. On the contrary, von Kries was concerned with those "infinite systems" Keynes had excluded from his enquiry: since speaking of elementary qualities of empirical evidence makes no sense in this case, in general any two pieces of empirical evidence are qualitatively different, non-comparable, and only subjectively susceptible of numerical estimation.

However, Keynes did acknowledge that exclusion of "infinite systems" is arbitrary:

Now an assumption, that *all* systems of fact are finite (in the sense in which I have defined this term), cannot, it seems perfectly plain, be regarded as having absolute, universal validity in the sense that such an assumption is self-evidently applicable to every kind of object and to all possible experiences. It is not, therefore, in quite the same position as a self-evident *logical* axiom, and does not appeal to the mind in the same way. The most which can be maintained is that this assumption is true of *some* systems of fact, and, further, that there are some objects about which, as soon as we understand their nature, the mind is able to apprehend directly that the assumption in question *is* true.

(Keynes 1921: 291)

Evidently, to the extent Keynes and von Kries investigated different cases, their probability theories are more complementary than conflicting. But we can go even further perhaps. The above passage is actually a fundamental one, because it discloses an interpretation of Keynes as focusing on "infinite systems" after the publication of *A Treatise On Probability*.

The often-quoted passage where Keynes seems to accept Ramsey's subjective idea of probability continues with the following qualification:

So far I yield to Ramsey - I think he is right. But in attempting to distinguish 'rational' degrees of belief from belief in general he was not yet, I think, quite successful. It is not getting to the bottom of the principle of induction merely to say that it is a useful mental habit. Yet in attempting to distinguish a 'human' logic from formal logic on the one hand and descriptive psychology on the other, Ramsey may have been pointing the way to the next field of study when formal logic has been put into good order and its highly limited scope properly defined.

(Keynes 1931)

This passage hints to "the next field of study" after formal logic - which applies to "finite systems" - would be "put into good order", suggesting to focus on the "human logic" that - we are tempted to fill in - governs "infinite systems". Moreover, this "human logic" is to be found by distinguishing it from formal logic on one hand, and "descriptive psychology" on the other: whatever Keynes might mean with an expression like "descriptive psychology", the very mention of a psychological plane brings him close to von Kries.

If these considerations make sense, Keynes' late acceptance that "probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief" (Keynes 1931) could be interpreted as a second, closer approach to von Kries, rather than as an unconditioned acceptance of Ramsey's subjective probability theory. However thin the basis for this interpretation might appear, it is worth to note that it is consistent with the idea of uncertainty that underlies Keynes' *General Theory*.

5. Beyond Keynes

Keynes's ideas on uncertainty have been the subject of countless speculations about "what he did really mean". The above reconstruction of the influence of Johannes von Kries on the development of Keynes' thought has been presented in the hope it might contribute to a more precise framing of his concept of uncertainty within the historical setting he operated in, as well as with respect to the tradition that departed from him. The last issue is the topic of the present section, which is a cursory survey of the concepts of uncertainty more or less related to those of Keynes, now reviewed in light of von Kries' work.

5.1 Knight's distinction between "risk" and "uncertainty"

Frank H. Knight developed his famous *Risk, Uncertainty and Profit* (Knight 1921) independently from Keynes' contemporary *A Treatise on Probability* (Keynes 1921); nevertheless, his name is often associated with that of Keynes, and their views are often superficially considered as more or less equivalent. Perhaps surprisingly, Knight actually seems to have more commonalties with von Kries than with Keynes, in spite of an absolute lack of any direct or indirect influence of the German logician on the American economist.

Similarly to von Kries, Knight claims that "(...) all reasoning rests on the principle of analogy." (Knight 1921: 202). But most important, he gives a rough account of how analogy-making works by means of classification of empirical facts into proper categories:

> But *workable* knowledge of the world requires much more than the assumption that the world is made up of units which maintain an unvarying identity in time. There are far too many objects to be dealt with by a finite intelligence, however unvarying they might be, if they were all different. We require the further dogma of identical similarity between large number of things. It must be possible not merely to assume that the *same* thing will always behave in the same way, but that the *same kind* of thing will do the same, and that there is in fact a finite, practically manageable number of *kinds* of things. Hence the fundamental rôle which *classification* has always played in thought and the theory of thought. (...) But even this is not enough. If the classification of objects be restricted to the grouping of things in *all* respects similar or substantially identical, there would still be a quite impossible number of *kinds* of things for intelligence to grasp. Even in the sense of practical degrees of completeness of similarity, identity to ordinary observation, our groups would be far too small and too numerous. It is questionable whether classification would be carried far enough on this basis to be of substantial assistance in simplifying our problems to the point of manageability. It is not that kind of world. And even abstracting from mere differences in degree such as size and the like, for which intelligence readily makes allowance, the same would still hold true. It is clear that to live intelligently in our world, - that is, to adapt our conduct to future facts, - we must use the principle that things similar in some respects will behave similarly in certain other respects even when they are very different in still other respects. We cannot make an exhaustive classification of things, but must take various and shifting groupings according to the purpose or problem in view, assimilating things now on the basis of one common property (mode of behavior) and now on the basis of another. The working assumption of practical inference about the environment is thus a working number of properties or *modes of resemblance* between things, not a workable number of kinds of things; the latter we do not have. That is, the properties of things which influence our reactions toward them must be sufficiently limited in number and in modes of association for intelligence to grasp.

(Knight 1921: 205-206)

The last lines of the above quotation are extremely important, because they highlight the degree of arbitrariness in the formation of the categories whereby empirical facts are classified. If this choice can be to some extent arbitrary, an individual might feel uncertain about how the known empirical facts should be classified: this is the root of Knight's famous distinction between "risk" and "uncertainty".

The issue is that of the similarity of the present situation with the situations a decision-maker has faced in the past: if the present situation is qualitatively so different from any other that it cannot be classified in any common category, no sound analogy is possible. In this case, Knight says that we have "uncertainty", not merely "risk". While the second can be evaluated by means of a suitable probability distribution and adequately insured against, in the first case only "estimates" are possible.

For the purpose of a comparison between Knight and von Kries, it is interesting to note that Knight laments that "estimates", although numerically expressed, do not arise out of any objective measuring procedure:

> The liability of opinion or estimate to error must be radically distinguished from probability or chance of either type, for there is no possibility of forming *in any way* groups of instances of sufficient homogeneity to make possible a quantitative determination of true probability. Business decisions, for example, deal with situations which are far too unique, generally speaking, for any sort of statistical tabulation to have any value of guidance. The conception of an objectively measurable probability or chance is simply inapplicable. The confusion arises from the fact that we do estimate the value or validity or dependability of our opinions and estimates, and such an estimate has the same *form* as a probability judgment; it is a ratio, expressed by a proper fraction. But in fact it appears to be meaningless and fatally misleading to speak of the probability, in an objective sense, that a judgment is correct.

(Knight 1921: 231)

In light of the arguments exposed in the previous section, the similarity between Knight's "estimates" and von Kries' "*Taxirung*" is impressive.

5.2 Non-Exhaustive Set of Events

In *Decision, Order and Time in Human Affairs* G.L.S. Shackle attempted to lay the foundations of a Decision Theory that would not assume the set of alternatives as exogenously given to the individual, but would allow the individual to construct his own set of alternatives by means of the invention of "unpredictable hypotheses" (Shackle 1961). In Shackle's own words:

(...) we think of uncertainty as more than the existence in the decision-maker's mind of plural and rival (mutually exclusive) hypotheses amongst which he has insufficient epistemic grounds of choice. Decision, as we mean the word, is creative and is able to be so through the freedom which uncertainty gives for the creation of *unpredictable hypotheses*. Decision is not choice amongst the delimited and prescribed moves in a game with fixed rules and a known list of possible outcomes of any move or sequence of moves. There is no assurance that any one can in advance say what set of hypotheses a decision maker will entertain concerning any specified act available to him. Decision is thought and not merely determinate response.

(Shackle 1969: 6)

Shackle's Decision Theory clearly intended to be the counterpart of subjective expected utility maximisation, as well as any other procedure based on attaching probabilities to some exhaustive set of events. The crucial issue is whether the set of possible events is exhaustive or not, that is, whether the set of possible events is able to describe whatever situation might or might not occur. It is not a matter of contemplating a finite or infinite set of events; rather, the issue is that an individual may think something may happen which he is not even able to imagine, something not described by any of the events he is making allowance for.

Within Bayesianism such a case can be dealt by assuming that the individual contemplates a "residual event", defined by exclusion as anything the other events do not cover: hence, to suppose that the set of possible events is not exhaustive means assuming that for practical purposes no residual event defined by exclusion can be of any use to a decision-maker. The reason why a residual event may not do is that by definition an event defined by exclusion does not capture any precise feature of reality, but simply puts anything the other events do not describe into a black box. Decision-making might require the ability to distinguish novel features that emerge in the empirical reality by means of novel events, whose definition implies enlarging the set of possible events. In Shackle's words, this means that the human mind is able to create "unpredictable hypotheses", since "decision is thought and not merely determinate response".

Although Shackle did not make any reference to Keynes, some interpretations of the role of uncertainty in *The General Theory of Employment, Interest and Money* tend to attribute to Keynes an idea of uncertainty similar to that of Shackle. P. Davidson, for example, writes that "(...) an environment of true uncertainty (...) occurs whenever an individual cannot specify and/or order a complete set of prospects regarding the future, because the decision maker either cannot conceive of a complete list of consequences that will occur in the future; or cannot assign probabilities to all consequences (...)" (Davidson 1991).

This interpretation rests mainly on Keynes' comments to the criticisms expressed to *The General Theory of Employment, Interest and Money* just after its publication. In particular, the two following passages are quoted most often:

> The future never resembles the past - as we well know. But, generally speaking, our imagination and our knowledge are too weak to tell us what particular changes to expect. We do not know what the future holds.

(Keynes 1937a)

> Actually, however, we have, as a rule, only the vaguest idea of any but the most direct consequences of our acts. (...) By ‘uncertain’ knowledge, let me explain, I do not mean merely to distinguish what is known for certain from what is only probable. The game of roulette is not subject, in this sense, to uncertainty; nor is the prospect of a Victory bond being drawn. Or, again, the expectation of life is only slightly uncertain. Even the weather is only moderately uncertain. The sense in which I am using the term is that in which the prospect of a European war is uncertain, or the price of copper and the rate of interest twenty years hence, or the obsolescence of a new invention, or the position of private wealth owners in the social system in 1970.

(Keynes 1937b)

Some of the statements contained in the above quotations seem to support Davidson's interpretation of Keynes' idea of uncertainty, notably those about the future that "never resembles the past" and the fact that we have "only the vaguest idea of any but the most direct consequences of our acts". Others are less clear-cut: 'a European war', 'the price of copper and the rate of interest twenty years hence', 'the obsolescence of a new invention' and 'the position of private wealth owners in the social system in 1970' are events that seem to exhaust the possibility space they refer to: Europe can be either in war or not, a new invention can be obsolete or not, the price of copper and the interest rate take values within well-defined intervals, as well as the position of private wealth owners. On the other hand, the occurrence of these events depends in turn on future political developments and future technological breakthroughs: events that, by their very nature, cannot be imagined. Thus, although the events mentioned by Keynes form superficially an exhaustive set of events, to closer scrutiny they depend on events that do not exhaust what reality can produce (note that the same happens when we define a residual event: the underlying non-exhaustive set of events is just hidden by an exhaustive one).

By definition, a non-exhaustive set of events is liable to be enlarged whenever a new possibility appears in the mind of a decision-maker, as well as restricted if the decision-maker decides to ignore the possibility that some event occurs. Keynes' probability theory seems to deal with this issue in connection with the "weights of the arguments", and at this point some influence by von Kries is vaguely acknowledged:

> The phenomenon of "weight" can be described from the point of view of other theories of probability than that which is adopted here. If we follow certain German logicians in regarding probability as being based on the disjunctive judgment, we may say that the weight is increased when the number of alternatives is reduced, although the ratio of the number of favourable to the number of unfavourable alternatives may not have been disturbed; or, to adopt the phraseology of another German school, we may say that the weight of the probability is increased, as the field of possibility is contracted.
>
> (Keynes 1921: 84)

In the case of games of chance an exhaustive set of events is at hand, and probabilities can be computed without any need to resort to analogies whatsoever. If von Kries assigned such a crucial role to analogy, it was because his probability theory was designed to account for decision-making in situations where empirical reality provides happenings with qualitatively new features, that must be grouped by means of analogies in order to define new events, able to comprise the new happenings. Clearly, the set of possible events is enlarged (or restricted) by this procedure, which necessarily postulates that any set of events is provisional and non-exhaustive.

Incidentally, this remark leads us to discard the claim that non-additive probabilities capture Keynes' and Knight's ideas on uncertainty (Gilboa 1987; Schmeidler 1989; Sarin and Wakker 1992), since even non-additive probabilities refer to an exhaustive set of events. In fact, the merit of non-additive probabilities is to combine the kind of uncertainty expressed by additive probabilities with the kind of uncertainty expressed by the dimension of the sample into a single magnitude. But in spite of the importance of this result in reconciling probability theory with situations such as "Ellsberg's paradox", this enterprise has clearly nothing to do with induction and analogy.

5.3 Non-Ergodicity of Economic Variables

Among the many facets of the long-standing debate between "rational expectations theorists" and "Keynesians" we find, in the Keynesian camp, Davidson's claim that Keynes' ideas on uncertainty can be cast in terms of non-ergodicity of economic variables (Davidson 1982).

Stochastic processes are said to be 'ergodic' if time averages coincide with space averages, which implies that space averages can be measured observing one single realisation of a stochastic process for a sufficiently long time: for instance, ergodicity means that the expected value of a random variable can be approximated by the sum of a sufficiently large number of realisations of that variable, divided by the number of realisations. Stationarity is a necessary condition for ergodicity, although not a sufficient one; in any case, since time series can be detrended with respect to a given series of values, non-stationarity due e.g. to a well-known cyclical component does not cause non-ergodicity.

Non-ergodicity can only be caused by unpredictable disturbances. Even this is not enough, however, since it has been proven that if the observed economic variables are stable around some equilibrium value, ergodic properties follow (Kurz 1994a, 1994b). Hence, non-ergodicity can only arise from structural innovations that destroy the equilibrium the economic system was eventually approaching.

Thus, non-ergodicity implies an economic environment where qualitatively new features continuously appear, so that individuals rely heavily on analogy-making in order to organise empirical evidence into a coherent framework. Conversely, if this is the normal operating mode of decision-makers, the economic variables they compute are exposed to changes that are unpredictable in principle, and that can undermine any stability assumption.

5.4 Case-Based Decision Theory

Case-based decision theory (Gilboa and Schmeidler 1995, 1996, 1997) fits particularly well into the line of thought we have observed developing from von Kries to Keynes. In fact, it does not assume that individuals dispose of an exhaustive set of events whose identification is self-evident in the setting where uncertainty arises, as it happens in the case of games of chance; on the contrary, it is explicitly designed to cope with ill-defined situations where the set of events is still to be framed. Case-based decision theory claims that what individuals actually do is to compare the situation they face with the "cases" they previously met by means of a "similarity function" they are endowed with. This similarity function is derived from the preferences of the decision-maker; while in the first version of the theory similarity was evaluated with respect to previous decision problems only (Gilboa and Schmeidler 1995), a more refined version assumes that the similarity of the acts undertaken in the past by the decision-maker is evaluated as well (Gilboa and Schmeidler 1997). Case-based decision theory naturally lends itself to modelling "satisfying" behaviour; at the same time, if some decision problem is presented to the decision-maker sufficiently often, case-based behaviour approaches "optimising" behaviour (Gilboa and Schmeidler 1996).

Although Gilboa and Schmeidler make some reference to Keynes and Knight (Gilboa and Schmeidler 1995: 608, 622), in this essay it is evidently more sensible to draw a comparison with von Kries, with whom both Keynes and Knight have been already compared.

Von Kries and case-based decision theory have two basic points in common: (i) both distinguish between facts that objectively happen in the empirical world and the way these facts are interpreted by the human mind, and (ii) both approaches view analogy-making as the link between mind and reality. Consequently, both von Kries and case-based decision theory are able to entertain the kind of uncertainty that stems from imperfect analogy. However, profound differences are to be found with commonalties.

Von Kries did not attach any great importance to the numerical probabilities that came out of a subjective evaluation of similarity between cases (the *Taxirung* process): to him, both non-numerical probabilities and subjective numerical probabilities were of little use in the statistician's practice. Consequently, von Kries' main concern was to elicit conditions for probabilities to be numerical, in the sense that they had to be amenable to measurement by some generally accepted procedure.

No such preoccupation affects Gilboa and Schmeidler, for whom subjective expected utility is the benchmark to refer to. Considerations about kinds of uncertainty that cannot be expressed numerically are also completely absent in their work, consistently with their full-title acceptance of the idea of subjective probability. Correspondence between von Kries' "*Taxirung*" and case-based decision theory is perfect instead, to the point that the second can be considered a formalisation of the first.

Possibly, the limit of case-base decision theory resides in the fact that the similarity function is given once and for all, instead of being developed by the individual along with the experiences he makes of the world. A decision theory that recognises that individuals generally face unpredictable situations should also allow them to change the criteria by which they evaluate the similarity of the problem they are trying to solve to the problems they encountered in the past. Von Kries did not touch this issue at all, however: he actually never specified how analogies are traced by individuals, so that Gilboa's and Schmeidler's assumption of a fixed similarity function is perfectly compatible with von Kries' framework, even if, in some sense, it restricts its potentialities.

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