The European Journal of the History of Economic Thought

ISSN: 0967-2567 (Print) 1469-5936 (Online) Journal homepage: http://www.tandfonline.com/loi/rejh20

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Euro. J. History of Economic Thought 16:3 489–510 September 2009
Routledge
Taylor & Francis Group

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1. Introduction

This paper examines Keynes's views on the logic of probability, in the sense of the field concerning rational human behaviour in an uncertain world (hence connected to, but distinct from, the pure theory of probability, in the sense of a field of mathematics). As we shall see, these views are at the basis of Keynes's theory of employment, interest and money as well as to his attitude towards econometrics; they also constitute an important contribution in themselves to the logic of probability and to the use of statistical tools in economics. In other words, Keynes's *Treatise on Probability* is here considered as an attempt both to provide an illustration of a (partly new) system of propositional logic, aiming at rationalising human behaviour, and to bring into perspective the relations between it and the real world.[1]

Keynes devoted a great deal of research time – after gaining top marks in the mathematical Tripos in 1905 – to writing and revising his *Treatise on Probability*. It was first unsuccessfully submitted as a King's College fellowship dissertation in December 1907 and then again, successfully, in December 1908, to be finally published in 1921 in a revised and expanded form. These were important formative years in his research career, and – as we shall see – the position he arrived at then changed little in later years.

I begin by recalling the main aspects of the classical and frequentist approaches to probability in the simplest possible terms, focusing on their basic characteristics. I then go on to consider Keynes's own views, and their relation to the subsequent subjective (or personalistic) approach, while also

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1 This latter aspect is stressed by Carabelli: in summarising her interpretation of Keynes's method, she says that for Keynes 'Probability did not belong to speculative knowledge, but served as a guide of life' (Carabelli 1988: 234).

The European Journal of the History of Economic Thought
ISSN 0967-2567 print/ISSN 1469-5936 online © 2009 Taylor & Francis
http://www.tandf.co.uk/journals
DOI: 10.1080/09672560903101369

briefly outlining the implications these views hold for interpretation of Keynes's *General Theory*. Finally, I consider the impact all this has for the application of statistical analysis (and econometrics) to economics.

2. The classical and frequentist approaches to probability

The classical definition of probability – number of favourable cases over number of possible cases – was initially proposed when – with James Bernoulli (1654–1705), and his *Ars conjectandi*, published posthumously (Bernoulli 1713); and, subsequently, his grandson, Daniel Bernoulli, (1700–1782) – probability calculus emerged from the study of 'regular' games, such as dice. It implies the full specification of the space of events in a finite number of 'atomic' events (for instance, the six faces of a die), considered equiprobable on the basis of the so-called Principle of insufficient reason, or Principle of indifference: there is no reason to consider one face of a die more probable than any other. Probability calculus is then assigned the task of computing the probabilities of complex events, such as two or three throws of a die showing the same face, or giving as total a preassigned value. It is a clear definition, but valid only within narrow limits. For instance, it is not applicable to other games (such as chess) where some element of human ability enters; or, more generally, whenever the very delimitation of the space of events and its partition into a finite number of atomic events proves difficult and/or involves an element of subjective evaluation.[2]

Keynes follows what at the time were familiar lines in criticising the classical definition of probability, focusing on the (very common) arbitrariness of a partition of the space of events into atomic events, to which the Principle of indifference is then to be applied.[3] Let us try here to approach

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2 The Italian mathematician Francesco Paolo Cantelli (1875–1966; quoted by de Finetti 1989: 180) considered the field of probability as composed of different sub-fields, to which different approaches are suited: the 'urn' scheme for the cases in which equally probable atomic events can be defined; 'frequencies' for fields like insurance; and 'bets' for fields such as horse races. de Finetti remarks that the mathematical treatment of the three fields can be analogous – which is true, once probability distributions are assumed as given; what Cantelli's examples stress is that in the different cases probability distributions are obtained in different ways, since the phenomena under consideration differ in nature. There is some similarity between Cantelli's ideas and Gillies's (1973) thesis that an objective interpretation of probability is the appropriate one for classic games of chance.

3 Among other sources, Keynes (1921: 45 ff.) singles out von Kries's *Die Principien der Wahrscheimlichkeit* of 1886.

the issue from a slightly different viewpoint, considering the application of probability theory to a game of dice. In this case, assuming a 'perfect die', the computation of probabilities on the basis of the classical definition takes on the form of deduction from given premises, rather than of inductive reasoning based on experience of a series of past events. As a deduction, the probability statement is certain.[4] However, as soon as we consider the result of a game played with one particular real die, the perfection of which cannot be assured, uncertainty may reappear. We must be careful, however, in specifying where exactly uncertainty crops up. Proposition *h*, describing the state of the world, may now be rephrased as 'we are considering a real game of dice, and given the circumstances (for instance, our acquaintance with the players) we consider it very likely/rather unlikely that the real die being utilised is for practical purposes indistinguishable from a perfect die'; proposition *p*, our statement of probability, could be something like 'we consider it very likely/rather unlikely that the usual results of mathematical probability theory concerning the game of dice[5] apply to this specific game'. Thus we have here a probability statement concerning a specific game of dice where we are possibly unable to define a quantitative probability. The example is helpful in that it stresses the difference between mathematical (deductive) probability theory and the field of probability statements concerning real events. Clearly, it is this latter field in which economists are interested. And, in this case, our knowledge may be expanded by taking into account past events, such as the results previously obtained by the use of that specific die; but it is no simple task to specify *a priori* how the additional knowledge should be utilised.

The idea that probability has to do with inductive knowledge underlies the frequentist approach to probability prevailing at the time Keynes began working on the subject. Specifically, we should recall that it was supported by John Venn (1834–1923), author of *The Logic of Chance* (Venn 1866), fellow of Caius College in Cambridge as from 1857 and master of the same college from 1912. According to the frequentist approach, the probability of an event is the limit to which the relative frequency of the event tends in

4 'In order that we may have a rational belief in a proposition *p* of the degree of certainty, it is necessary that one of two conditions should be fulfilled – (i) that we know *p* directly; or (ii) that we know a set of propositions *h*, and also know some secondary propositions *q* asserting a certainty-relation between *p* and *h*' (Keynes 1921: 17). Here, the second condition applies, with proposition *h* stating that we are considering a 'perfect' die, propositions *q* providing the logical bridge between the 'state of the world' represented by *h* and the conclusion represented by *p*, and proposition *p* stating, for instance, that the possibility of obtaining the same face when twice throwing the same die is one-sixth.

5 Which have the nature of propositions *q* in the previous footnote.

successive observations (stochastically independent from each other) of some variable, for instance the stature of conscripts or the throw of a die, or repeated independent measures of the same magnitude, when the number of observations tends to infinity.

As with the classical definition, the frequentist definition implies an objective view of probability. The objective nature of the probability statement lies in the fact that it is considered to depend on the intrinsic properties of the phenomenon under consideration. One of the main supporters of this definition, Richard von Mises, stressed that, rigorously speaking, it can only be applied to 'collectives', namely to successions of uniform events only differing in some observable characteristic that is the object for scrutiny, when the principle of causality holds for the sequence of observations (i.e. where no regularity occurs, so that it is impossible to devise a winning strategy concerning the order of the sequence). Clearly, this definition excludes from the field of probability all 'singular' events; that is, all events that are not part of a collective.[6]

While the background to the classical approach to probability was provided by a mathematical interest in 'regular' games (such as dice), the frequentist approach grew on the ground fertilised by the Gaussian (normal) curve. This curve had originally been proposed (by Carl Friedrick Gauss, 1777–1855) as representing the frequency distribution of measurement errors in physical sciences, but soon (prompted by Adolphe Quételet, 1796–1874) interest focused on regularities in the social field, such as the stature of soldiers (cf. Stigler 1986; Hacking 1990). In the wake of this research, in economics (mainly under Marshall's lead) the term 'normal' substituted the term 'natural' to indicate the theoretical values of variables such as prices or the wage rate. In any case, as specified above (limit frequency of an event in an infinite series of replications), the frequentist definition of probability still retained an implicit reference to 'regular' games and, in the natural as in the social field, to the idea of 'intrinsic laws' independent of the observer, which only wait to be discovered by the scientist.

Keynes's critique of the frequentist approach[7] goes further than limiting it to a set of phenomena to which the law of large numbers applies. It is, rather, a broadside critique, coloured by Humean scepticism, of the possibility of scientific induction; that is, the possibility of inferring from a series of observations, however long, some regularity to be considered as certain. An infinite series of replications of an event cannot be actually

6 Cf. Costantini (2004: 72–9).

7 The frequentist approach had already been criticised by Edgeworth. Cf. Baccini (2001) and Stigler (1999: 87–128).

observed; a finite series of observations cannot provide the foundation for scientific induction, since it may be contradicted by another finite series of observations. (In other words, each finite series of observations, however long, is a 'singular event', not a 'collective'.) Even if we have got heads 100 or 1000 times in a row tossing a 'regular' coin, we know that we cannot infer that heads will come out again on the next toss with probability equal to one; if we have obtained heads 60% of the time in a long series, we may quite reasonably still believe that the coin is 'regular' and therefore maintain that the 'real' probability of heads is 0.5. As Hume maintained, experience of past events plays an important role in shaping our beliefs, but can always be contradicted by subsequent events.[8] (We will return to these issues below, in Section 5).

3. The Keynesian approach to probability

Confronted with this situation, Keynes follows a different course. In illustrating it, we focus on three elements: his definition of the probability statement, his notion of the 'weight of the argument', and his 'theory of groups'.

First of all, Keynes defines probability as the 'degree of rational belief' in a proposition (hypothesis) on the basis of the available evidence. Hence, by itself probability is not an objective property of the phenomenon under examination, but a logical relation, established by the observer, between the available evidence on the one hand and the proposition under consideration ('primary proposition') on the other. The logical relation (or 'secondary proposition')[9] may differ from one observer to another, because

8 As Keynes commented (1921: 302), 'Hume's sceptical criticisms are usually associated with causality; but argument by induction – inference from past particulars to future generalisations – was the real object of his attack. Hume showed, not that inductive methods were false, but that their validity had never been established and that all possible lines of proof seemed equally unpromising.' Popper's famous example of the white and black swans ([1934] 1959: ch. 1, §1) is a variation on this theme. (Although beyond our scope here, we may recall that, while sharing Hume's scepticism about induction, Keynes did not seem to share the Scottish philosopher's scepticism about moral judgement (cf. Carabelli and De Vecchi 1999: 279–80). In any case, let us not lose sight of the fact that scepticism is a far cry from nihilism, as summarised in the motto 'anything goes'.)

9 'It will be convenient to call propositions [...] which do not contain assertions about probability-relations, ''primary propositions''; and propositions [...] which assert the existence of a probability-relation, ''secondary propositions''' (Keynes 1921: 11).

of differences in knowledge, such as the extent of evidence available to each of them, but also because of differences in their individual intellectual abilities. At the same time, the probability statement retains some empirical correlate in the reference to the available evidence, which acts as a constraint on the rational observer.

Because of this latter aspect, the simple dichotomy between objective (classical and frequentist) and subjective probability theories is inapplicable, or at least requires the term 'objective' to be aptly specified. For instance, if the term 'objective' is specified as in Gillies and Ietto-Gillies (1991: 394) – 'Objective interpretations consider probability to be a feature of the external physical or material world which is independent of human consciousness, human belief or human knowledge' – then Keynes's theory cannot be classified as 'objective'. It can rather be classified as belonging to the 'epistemological interpretations', which 'identify probability with the degree of knowledge, or degree of belief, or degree of rational belief held by a human being or a group of human beings' (Gillies and Ietto-Gillies 1991: 394). This category then includes logical (Keynes, Carnap), subjective (de Finetti, Ramsey, Savage), and 'intersubjective' (Gillies and Ietto-Gillies) theories.[10]

In Keynes's theory of probability, there is no objective rule to establish how the empirical evidence should affect the probability statement, or as to how additional evidence should change it, so as to establish a bi-univocal correspondence between evidence and a 'rational' probability statement;[11] however, there is the idea that the subject must somehow take the available evidence into account.[12] In fact, together with internal consistency (no contradictions) in the system of beliefs, this is what distinguishes rational from irrational behaviour.[13] Keynes's definition is thus a variant of what is commonly labelled by probability theorists as the 'logicist' definition of

10 'If a group does in fact agree on a common betting quotient, we shall call that betting quotient the *intersubjective* or *consensus* probability of the social group' (Gillies and Ietto-Gillies, 1991: 399).

11 In the *Treatise on Probability*, Keynes seems not to deny such a bi-univocal correspondence, but to make it specific to each individual (due to different individual abilities).

12 'What we know and what probability we can attribute to our rational beliefs is, therefore, subjective in the sense of being relative to the individual. But given the body of premisses which our subjective powers and circumstances supply to us, and given the kind of logical relations, upon which arguments can be based and which we have the capacity to perceive, the conclusions, which it is rational for us to draw, stand to these premisses in an objective and wholly logical relation.' (Keynes 1921: 19). See also the quotation given in footnote 14 below.

13 Carabelli (1988: 99) points to 'Keynes's concept of rationality as reasonableness or practical rationality'.

probability, according to which the probability statement is a logical relationship connecting a hypothesis – a ‘primary proposition’ – to the available evidence (or, more precisely, to ‘our knowledge’, as Keynes (1921: 4) puts it).

Thus, according to Keynes, the probability relation is an objective one, in that it is ‘independent of our opinion’.[14] However, he also stresses that the probability relation is a statement made by an individual agent at a specific moment in time, since it depends on the state of knowledge – which may be different from one person to another and which may change over time. This point would become important later in his research when, dealing with the stock exchange or the financial markets in general, Keynes was to construct his theory on the basis of the fact that different agents may have (commonly have) different expectations; that is, different evaluations of the situation and its perspectives.

This point – the objective nature, in some sense of the term, of the probability statement – is important because of its connection with an ethic of individual responsibility. We cannot discuss here the influence of Moore on Keynes, or Keynes’s differences from Moore’s ethics. Briefly, the link between the probability statement and the knowledge available to the subject making the probability statement distinguishes the ‘rational’ from the ‘irrational’ belief; each person has a moral responsibility to choose among courses of action on the basis of a ‘rational’ evaluation of their consequences, which are in general uncertain; if I do my best in this direction I am morally on safe ground even if it turns out that I am wrong in my evaluation (think of a physician making a diagnosis and prescribing a cure).[15]

14 ‘It is without significance to call a proposition probable unless we specify the knowledge to which we are relating it. To this extent, therefore, probability may be called subjective. But in the sense important to logic, probability is not subjective. It is not, that is to say, subject to human caprice. A proposition is not probable because we think it so. When once the facts are given which determine our knowledge, what is probable or improbable in these circumstances has been fixed objectively, and is independent of our opinion’ (Keynes 1921: 4).

15 On this point see Cristiano (2005); on Moore and Keynes, see Bateman (1988), Carabelli (1988), and O’Donnell (1989). Raffaelli (2006: 162), after recalling Moore’s idea ‘that we must fall back on customary rules of conduct’, remarks that in an unpublished paper ‘Keynes blames Moore’s conservative conclusion on the implicit acceptance of a wrong theory of probability – the then dominant frequency theory […]. Whereas in Moore’s theory probability judgements “can be confirmed or refuted by future events”, Keynes grounds their validity in themselves: “a statement of probability always has reference to the available evidence, and cannot be refuted or confirmed by subsequent events”.’

In the second place, Keynes interprets probability not as a set, possibly axiomatic, of theorems, but as a field of logic,[16] or more precisely as a system of propositional logic. This system appears to be built in such a way as to contribute to our understanding of rational human behaviour under uncertainty. The latter (uncertainty) is seen as the general case, while perfect certainty and absolute ignorance are its limit boundaries. There are two aspects to this: first, the probability attributed to a proposition – be it expressed as a numerical magnitude or not[17] – is commonly less than one (where one corresponds to certainty) even though more than zero (which corresponds to impossibility); second, the probability statement is itself commonly 'uncertain' (not fully reliable).[18]

This means that there is an additional dimension: the 'weight' we can attribute to the logical statement of probability connecting a proposition (hypothesis) to the available information. Keynes defines the 'weight of the argument' as the basis of evidence upon which to rest our probability statement. 'As the relevant evidence at our disposal increases [. . .] the *weight* of an argument [. . .] always increases'. However, Keynes adds, when we compare different probability propositions, 'only in a restricted class of cases can we compare the weights of two arguments in respect of more and less'.

Thus, the weight of the argument should not be interpreted as a measurable magnitude. It is by way of a similitude, utilised for introducing

16 'Probability [is here conceived] as a branch of logic', while 'in the learned world [. . .] Probability is oftener reckoned with Mathematics than with Logic' (Keynes 1921: xxv).

17 Keynes insists repeatedly on the fact that it is not always possible to express probabilities as quantitative magnitudes. For instance: 'There appear to be four alternatives. Either in some cases there is no probability at all; or probabilities do not all belong to a single set of magnitudes measurable in terms of a common unit; or these measures always exist, but in many cases are, and *must remain*, unknown; or probabilities do belong to such a set and their measures are *capable* of being determined by us, although we are not always able to determine them in practice' (Keynes 1921: 33).

18 Knight (1921; esp. 233) distinguishes between risk and uncertainty, the first being the case involving quantitative probabilities and the second the case in which probabilities are unmeasurable. Such a distinction is not to be found in Keynes. Here the distinction would have to keep account of the additional dimension of the 'weight of the argument': one might define as risk the case where not only probabilities are measurable, but also the weight of the argument is considerable, so that the agent has – with good reasons – full confidence in his or her own (quantitative) probability statement. Keynes's own definition of risk (Keynes 1921: 348 ff.) is different, indicating the product of the 'net immediate sacrifice to be made in the hope of obtaining A' and the 'probability that this sacrifice will be made in vain'.

the reader to the notion, and not as a precise definition, that Keynes tentatively expresses the weight of the argument as 'a balance [...] between the *absolute* amounts of relevant knowledge and of relevant ignorance respectively'.[19] With additional evidence, the probability statement may be revised, while the weight of the argument increases; it may so happen that the two vary in opposite directions, with the probability assigned to the proposition decreasing while the weight of the argument increases.

Let us notice that, when Keynes refers to 'the relevant evidence at our disposal', this means that the weight of the probability statement depends on the evidence at the disposal of the subject who is responsible for the probability statement.[20] The relevant evidence at the disposal of each individual agent may be different and/or be differently interpreted; this allows us to distinguish between the weight of the argument and the 'confidence' of the subject on her/his probability statement, where 'confidence' retains a psychological connotation.[21]

In the third place, Keynes (1921: ch. 11) defines a 'theory of groups'.[22] The 'group' is specified in a purely logical way. It is a set of propositions with two components. The first sub-set is made up of those propositions (independent of each other) that define the group as its 'premisses'; the second sub-set includes those propositions that are logically derivable from the premisses. Within each 'group' we may define a system of statements of

19 Keynes (1921: 77). Hence the weight of the argument is a different concept from probability: 'The weight, to speak metaphorically, measures the *sum* of the favourable and unfavourable evidence, the probability measures the *difference*' (Keynes 1921: 84).

20 Let us recall, however, that Keynes interprets the probability statement as 'independent of our opinion'. Thus, the weight of the argument could perhaps be correlatively interpreted (although Keynes does not explicitly say so) as determined not by the evidence at the disposal of a single individual agent, but as some sort of common evidence (possibly identifiable with the evidence in principle available to a good *pater familias*).

21 An anonymous referee suggests that 'weight and confidence might be considered as correlative terms'. We may add that an increase in the relevant evidence, which according to Keynes always increases the weight of the argument, may decrease the confidence of the subject in a probability statement, whenever the new evidence brings to light the importance of additional, highly uncertain, circumstances hitherto ignored (with a variant of an old adage, 'only the sage realises how deep is her/his ignorance'). This possibility is suggested by Runde (1990: 283), who provides an in-depth discussion of the notion of the weight of the argument; his interpretation is slightly different from the one presented here, since he does not distinguish between (objective) weight and (subjective) confidence.

22 Keynes (1921: 134 fn.) notes his indebtedness to W. E. Johnson in this connection, but without providing bibliographical references. Johnson (1858–1931), a logician, was Fellow of King's College, Cambridge from 1909.

probability (be it quantitative probability or otherwise); the theory of probability ensures their logical consistence.[23]

This notion is substantially different from von Mises's 'collectives'; rather, it may vaguely recall the later Wittgenstein's construct (in the *Philosophical Investigations*; Wittgenstein 1953) of 'language games'.[24] Like it, Keynes's notion of 'groups' seems to point in the direction of isolation of specific problems for separate enquiry. In other words, a general methodological indication we may infer from Keynes's notion of groups is that, in our attempt to build theoretical representations of reality, we should not consider all aspects simultaneously in an encompassing model, but rather focus on separate subsets of issues. For instance, with direct reference to Keynes's groups, roulette will constitute such a group, dice another group, and card games still other groups (distinct from one another, although for all of them the probability statement may be made with perfect confidence if we can assume that the roulette, the die or the cards are unbiased); daily events on the bond and money markets constitute another such group (where, however much evidence one can collect, the weight of the argument never reaches a maximum and the personal nature of the probability statements is clearly apparent); while forecasts concerning the relative profitability of coal, oil or gas-fuelled power plants over their lifetime form yet another group (where not only the weight of the argument is low, but also it would be quite difficult to assign numerical values to probabilities for many of the variables involved in the exercise, although trying to reason on the basis of the available information is a commendable way to evaluate the investment choice between different kinds of power plants). As we shall see in Section 7, this aspect has important consequences in Keynes's economic theorising.

4. The subjective approach

In opposition to objective (classical and frequentist) approaches (which may be reduced to the role of a special case, in their new framework), de Finetti (and Ramsey in Cambridge) developed a subjective (or

23 'We define a group as *containing* all the propositions logically involved in any of the premisses or in any conjunction of them; and as *excluding* all the propositions the contradictories of which are logically included in any of the premisses or in any conjunction of them' (Keynes 1921: 134). When *a* is true, then *a* is true; therefore, although Keynes does not explicitly say this, the group also contains its own premisses. Thus, the first sub-set mentioned above is included in the second sub-set, which coincides with the group.

24 Cf. Roncaglia (1978: 121–4).

'personalist') approach, which triumphed after the Second World War in the formulation given it by Savage.[25] According to this approach, the probability statement is fully subjective: it is a 'state of the mind', not a 'state of nature'. More precisely, with an 'operational' definition, it can be determined as the lowest betting odds one would accept on a given event. Each subject is considered able to quote betting odds for all sorts of events; the 'supply price' and the 'demand price' for each bet are assumed to be equal; that is, the subject is assumed to be indifferent between outcomes.[26] The mathematics of probability is entrusted with the task of ensuring the logical consistency of each subject's book of bet offers. In fact, consistency is all that matters: a coherent – and firmly believed in – system of rules for reading one's health in tea leaves is considered equally 'rational' as asking the advice of the best physicians. (When applied to economic theory, this means that the focus of attention is wholly on logical rigour, while realism is left aside.[27])

Consistency is defined as the impossibility of devising a book of bets (a 'Dutch book') such as to ensure winning, whatever happens, against the book of bet offers under consideration. Thus subjective probabilities come to constitute the foundation for decision theory, with action (represented by a book of bet offers) based on a consistent set of probabilities dominating over all inconsistent ones. The difference with Keynes is that here the probability is a subjective evaluation made by an individual, 'revealed' through betting, while in Keynes it is a 'rational' evaluation, conditional on the extent of knowledge the individual agent has in the specific case under consideration. 'Rational' does not imply reference to an omniscient maximising agent, as in the marginalist construct of the

25 See the essays collected in de Finetti (1989, 1993), Ramsey (1931) and Savage (1954).

26 The subject must also be assumed to be indifferent towards betting or not betting. Otherwise the 'supply' and the 'demand price' for a bet will differ by an amount sufficient to compensate the subject for accepting to enter the betting arena. In general, professional gamblers (such as casino owners) require such a compensation.

27 We should recall here, however, de Finetti's (1989: 136) subtle answer: 'Invece di cercare di riportare tutto all'oggettivo, si può ottenere chiarezza facendo il percorso inverso, riducendo cioè ogni concetto di questo tipo al soggettivo. Il valore di un concetto risulterebbe allora da un'analisi delle ragioni profonde e essenziali che ci hanno spinto, forse inconsciamente, a introdurlo e che ci danno una spiegazione della sua utilità' (My translation: 'Rather than trying to relate everything to the objective, we can get clearness by following the opposite route, that is by reducing every concept of this kind to the subjective. The value of a concept would then result from an analysis of the deep and essential reasons which led us, possibly unconsciously, to introduce it and which give us an explanation of its usefulness'.)

homo oeconomicus; rather, it recalls the notion of Adam Smith and other classical economists of the economic agent as a good *pater familias* trying to determine what to do when confronted with a complex world, where clear-cut rules of behaviour, univocally applicable to each situation, are not available to provide satisfactory guidance.

After publishing the *Treatise on Probability* in 1921, Keynes did not take up the subject again, except for some occasional remarks. Thus, we do not have a clearly stated, well-developed illustration of his reaction to the subjectivist approach to probability. Apart from his reaction to Tinbergen's work and his attitude towards statistical inference, which we look at below, we find relevant evidence in only two contexts; namely his 1931 obituary of his friend Frank Ramsey, and a 1938 letter to Hugh Townshend.

> Ramsey argues, as against the view which I had put forward, that probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief, and he succeeds in showing that the calculus of probability simply amounts to a set of rules for ensuring that the system of degrees of belief which we hold shall be a *consistent* system. [...] 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. (Keynes 1931: 338–9)

This passage has been interpreted in contrasting ways: as abandonment of Keynes's original position in favour of the subjectivist approach to probability (Bateman 1987, 2003; Raffaelli 2006: 172), and as a simple tribute to a deceased friend, with no substantial changes in Keynes's views (Carabelli 1988); there are also some intermediate views, with different balances between change and stability in Keynes's position (O'Donnell 1989, 2003; Dardi 1991).

With respect to the aspects considered in the present paper, in the passage quoted above Keynes appears to be rephrasing Ramsey's criticism of his own views in such a way as to leave aside Ramsey's critique of the possibility of 'perceiving' some probability relations and to focus on a point – the subjective nature of the probability statement – on which Keynes himself agrees, when interpreted as pointing to the fact that the probability statement is made by an agent, under circumstances of knowledge that are specific to the agent. But a crucial point of dissension – the fact that the probability statement relies on the agent's relevant knowledge, and is independent of her/his opinion – is then gently but sufficiently clearly elicited.[28] Nothing, moreover, is said here of the subjectivist idea that a

28 Thus, Keynes's theory cannot be classified as subjective, though it cannot be classified as objective in a strict definition of the term (as the one given by Gillies and Ietto-Gillies 1991: 394 – quoted above in Section 3).

system of numerical degrees of belief can always be established through betting statements. In this respect, a letter to Hugh Townshend of 26 July 1938 confirms Keynes's 1921 position:

> on my theory of probability, the probabilities themselves, quite apart from their weight or value, are not numerical. [...] One arrives presumably at the numerical estimations by some system of arranging alternative decisions in order of preference, some of which will provide a norm by being numerical. (Keynes 1979: 288–9)

5. Bayes's theorem

The difference between the frequentist, the subjective and the Keynesian approaches can be illustrated by the fact that they attribute different roles to statistical analysis. Discussion has in this respect traditionally focused on Bayes's theorem, originally published in 1764, which shows how the probability attributed to a hypothesis H is modified by the acquisition of new relevant evidence E:[29]

$$\Pr(H/E) = \Pr(H)\Pr(E/H)/\Pr(E),$$

namely, the 'final' probability – the probability of our hypothesis H given the 'new' evidence E – is equal to the 'initial' probability of our hypothesis multiplied by the prior probability of the evidence E if the hypothesis H holds (namely, what is called the 'likelihood' of the hypothesis), divided by the prior probability of the evidence. Bayes's theorem is widely considered the basic pillar for statistical inference, since it points out the 'scientific' rule according to which we can modify our probability assessment of any event on the basis of additional evidence. For instance, if in a long sequence of dice throws the same face nearly always shows up, the initial (prior) probability for that face ($\Pr(H) = 1/6$), based on the assumption of a 'regular' die, appears less and less defendable as the game goes on.

This example points to an aspect that is often left out from illustrations of Bayes's theorem: the role of the 'general state of knowledge' (K) in determining our prior probabilities. It is what we know about the die – that it is a more or less regular cube – that leads us to our prior probability of 1/6 for each face. If we have no other information, and

29 Cf. for instance Costantini (2004: 43, 48, 55). It should be stressed that Bayes's theorem was originally developed in the context of the classical approach to probability. Its association with the subjective approach – as when the latter is labelled as 'subjective (Bayesian)' – is quite misleading, from the point of view of the history of probability analysis. It is only when the role of our 'general state of knowledge' is introduced, that Bayes's theorem can be re-interpreted on logicist or subjective lines.

even more if we learn that the die is well made with no apparent defect, the results of our sequence of throws, although unusual, may not induce us to change our prior probability. However, if we knew that the die in question were owned by a professional player with a bad reputation of dishonesty, our initial prior probability would not be held with great confidence, and would soon be abandoned as the game unfolded. We should then write:

$$\Pr(H/E\text{and}K) = \Pr(H/K)\ \Pr(E/H\text{and}K)/\Pr(E/K).$$

Our 'general state of knowledge' K not only determines our prior probabilities, but also the weight of the argument we place on them; hence, it also influences our readiness to change our probability evaluations in the light of new evidence. (Thus, for instance, when in the Italian state lottery the number 53 did not show up for 180 weeks in a row, this did not change the basic assumption of hundreds of thousands of betters that there was no cheating on the part of those in charge of the lottery.[30])

Pr(E/H) is called the likelihood function, and is at the centre of the frequentist interpretation. In fact, frequentists minimise the role of prior probabilities, focusing statistical analysis on the full set of available evidence (past and newly acquired) on the variable under consideration. Thus, assuming uniform initial probability distributions, they are able to compute 'intervals of confidence' for accepting or rejecting the hypothesis H in the light of evidence E. For instance, if E/H is extremely unlikely – say $\Pr(E/H) = 0.001$ – then we should reject our hypothesis when confronted with evidence E.

There are two shortcomings to this. First, estimating the probability distribution for E/H may be easy in some instances (dice or roulette first and foremost: in general, all 'regular' games), more difficult in most others (those that do not belong to von Mises's 'collectives', or when it is intuitively absurd to apply the Principle of indifference because of the impossibility of devising an adequate full partition of the space of events). Second, as we have just seen in the examples above, even if Pr(E/H) is very small, rejecting the hypothesis H may be wrong,[31] and occasionally our good sense tells us that this is the case: in other words, in considering Bayes's formula in full we attribute such a high weight to our 'prior probability',

30 In other terms, the weight of the argument does not appear directly among the variables involved in Bayes's theorem; its role lies in influencing the way in which the new evidence affects the values assigned to prior probabilities, Pr(H/K) and Pr(E/K).

31 Symmetrically, accepting the hypothesis H when Pr(E/H) is very high may also prove a mistake.

Pr(H), that we are unwilling to abandon it, even in the face of serious evidence to the contrary.[32]

Thus, Keynes appears to consider Bayes's theorem not as an 'objective' rule for statistical inference, but rather as a piece of logical analysis the application of which to real-world decisions is necessarily founded on evaluation of the relevant magnitudes, including in most instances the form of the likelihood function, but especially our prior probabilities and the weight we can attribute to them.[33] Statistical analysis cannot be considered a mechanical, 'objective' method of scientific enquiry: it is, rather, a tool to be handled with care and caution, guided by intuitive ability driven by experience and knowledge of the phenomenon under consideration.

6. Keynes on statistical inference and econometrics

Keynes goes most of the way with the subjectivist position in criticising any mechanical application of statistical inference. This was clear when, nearly two decades after publication of the *Treatise on Probability*, he was asked to evaluate Tinbergen's (1939) econometric exercises on business cycles for the League of Nations, and expressed serious doubts, both about the techniques adopted and about the underlying method (Keynes 1939, 1973: 285–320).[34]

There are three different aspects intermingled in Keynes's critique: first, there is a more complex and less mechanical view of business cycles (different episodes display different characteristics; the factors in action

32 After recalling 'the necessity in general of taking into account the *a priori* probabilities of the different causes', Keynes (1921: 197) remarks: 'If a cause is very improbable in itself, the occurrence of an event, which might very easily follow from it, is not necessarily, so long as there are other possible causes, strong evidence in its favour.'

33 Cf. Keynes (1921: 192–205 and 413–17), where Pearson's use of the theorem is criticised. Subjective probability theorists agree on this. Thus de Finetti vehemently criticises 'those mechanical recipes which indefinitely enrich the statistical recipe books: those recipes which, according to the appropriate definition introduced by Irving J. Good, simply represent ''adhockeries'' [... namely] decision methods operating as a black box, as a mysterious and unprecise object into which data and questions are introduced at one end and ready-made answers and advice are obtained from the other end' (de Finetti 1993 [1978]: 514).

34 Among the literature on this episode, let us recall Theil (1963), Patinkin (1976), Phelps (1980), Lawson (1985), Pesaran and Smith (1985), Carabelli (1988: 173–93), and Bateman (1990). Lawson (1989) illustrates the background to this debate in the history of the development of econometrics.

may not only differ from one episode to another, but are also multifarious in their nature, some of them having the nature of quantitative variables and others of unmeasurables); second, he advances some technical criticisms specific to Tinbergen's work; and third, he proposes a basic critique of method concerning the limits of induction, greatly limiting the scope for statistical inference.

Keynes's specific criticisms of Tinbergen's econometric techniques have come in for comment from a number of modern econometricians. Some dismiss them as badly formulated and outdated, certainly nowadays if not already in the 1930s.[35] Others recognise that, expressed in modern terms (omitted variables and unobservables; inadequacy of the statistics employed; spurious correlation, simultaneity and multicollinearity; inadequacy of linear functional forms; unjustified specification of time lags; structural instability), they are correct with respect to Tinbergen's analysis and point to present-day good econometric practice.[36] What is more, the developments that econometrics has seen since Keynes's times ('error in variables models to deal with unobservables; simultaneous equations models to deal with interdependence; Box-Cox transformations to deal with uncertain functional forms; distributed-lag and time-series methods to deal with dynamics; and random coefficient models to deal with parameter change') 'increase the flexibility with which the problems can be approached, [but] do not solve them' (Pesaran and Smith 1985: 139).

In fact, the 'outdated' form Keynes adopted to express his criticisms is indicative of a more basic issue: what Keynes points to with his specific comments is the distance between Tinbergen's chosen field of business cycles and von Mises's notion of the 'collective' as the proper object of probabilistic analysis. Inference, and specifically statistic–probabilistic inference, should thus be handled with great caution. Keynes (1973: 315) recognises that he has 'not noticed any passage in which Professor

35 It may not be entirely irrelevant in this respect, although it is clearly not a decisive point, to recall that Keynes was elected President of the Econometric Society in 1944 and 1945 (Patinkin 1976: 1092). This notwithstanding, Samuelson (1948: 156 fn.) decreed: 'Keynes' critical review of Tinbergen's economic business cycle study for the League of Nations reveals that Keynes did not really have the necessary technical knowledge to understand what he was really criticising'.

36 See, for instance, Hendry (1980: 396): 'Forty years after Keynes wrote, his review [of Tinbergen's 1939 book, Keynes 1939] should still be compulsory reading for all who seek to apply statistical method to economic observations'. See also Pesaran and Smith (1985).

Tinbergen himself makes any inductive claims whatever. He appears to be solely concerned with statistical description.' But he perceives a tendency (for instance in Loveday's preface to Tinbergen's book) to use econometric analyses to 'prove or disprove a qualitative theory' or give 'a quantitative guide to the future' (Keynes 1973: 315), and he is emphatically against this. Economic (or, more generally, social) issues are of a different kind from the sort of issues tackled by the natural sciences, in that 'the most important condition' for the application of statistical inference/econometric tools ('that the environment in all relevant aspects, other than the fluctuations in those factors of which we take particular account, shall be uniform and homogeneous over a period of time'; Keynes 1973: 316) is not satisfied. Thus, Keynes concludes, econometrics is a 'branch of statistical alchemy' (1973: 320).

The point is repeated a number of times, and with great clarity, for instance, in a letter to Harrod (Keynes 1973: 299–300), which deserves quoting at length:

> In chemistry and physics and other natural sciences the object of experiment is to fill in the actual values of the various quantities and factors appearing in an equation or a formula; and the work when done is once and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought. [...] All the statisticians tend that way. [...] The art of thinking in terms of models is a difficult [...] practice. The pseudo-analogy with the physical sciences leads directly counter to the habit of mind which is most important for an economist proper to acquire. I also want to emphasise strongly the point about economics being a moral science. [...] It deals with introspection and with values [...] with motives, expectations, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether it is worth while falling to the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth.

All this, it should be stressed, does not imply rejection of statistical analysis tout court. On the contrary, 'The specialist in the manufacture of models will not be successful unless he is constantly correcting his judgement by intimate and messy acquaintance with the facts to which his model has to be applied' (Keynes 1973: 300). That of the economist is an art, although based on the craft of logical tools and statistical analyses of the available empirical data.

We thus come back to the relationship between theory and statistical analysis as expressed by the interplay of prior probabilities and the likelihood function in Bayes's theorem (Section 5 above): such interplay is necessary for the progress of knowledge in economic affairs, but no mechanical rule can be imposed on it. Thus, Keynes says, there should be no 'uncertainty as to who, the economist or the statistician, is in the saddle and who the patient ass' (1973: 319).[37]

7. Keynes's *General Theory*

The implications Keynes's ideas on probability hold for his economic theory have been the object of a wide and constantly growing literature;[38] here I limit myself to outlining a few points.

First, there is the rejection of a 'general equilibrium' viewpoint: although all economic events may be considered related to each other, each theoretical proposition concerning each individual link embodies an element of uncertainty, and the uncertainty piles up, so to say, if we consider successive links, so the theoretician had better make do with considering one or a few links at the time (the 'short causal chain' method). Thus, Keynes's '*General Theory*' is a set of separate theories (concerning the determination of the interest rate on the money markets, the influence of interest – together with long run expectations – on investments, the influence of investments on levels of production and employment) that, although belonging to one same 'view' of how a monetary production economy works, cannot be unified into a single all-embracing model (such as Hicks's IS-LM) without losing sight of the basic role of uncertainty and its different characteristics in the different fields of the economy (for different kinds of decisions and for different groups of agents).[39]

Second, there is distinction to be made between economic agents and decisions according to the 'kind' of uncertainty involved: entrepreneurs have to be kept distinct from financiers, and both from households; among

37 This is clearly a reaction to what Keynes perceived as the risk of statistical techniques coming to dominate over economic reasoning; a better metaphor could be that of a cooperative game.

38 As Bateman (1990: 378) remarks, 'Keynes had a clear interest in induction from the time of his first forays into economics, and [...] it influenced the ideas about measuring economic variables, the appropriate application of statistical methods to economic data, and model specification'. Carabelli (1988) and O'Donnell (1989) provide book-length discussions of the subject and many bibliographical references; among the most recent literature, see the essays collected in Runde and Mizuhara (2003).

39 Due to the role of uncertainty, Keynes's notion of equilibrium too is different from the traditional one: 'Keynes's boot-strap theory of economic equilibrium, dependent only on widespread conventional trust in its stability, was novel' (Raffaelli 2006: 177).

entrepreneur's decisions, those concerning investments have to be kept separate from those concerning production levels.[40]

Third, one of Keynes's main original contributions, and one of the pillars of his theoretical construction – namely, his theory of liquidity preference – is a direct offspring of his views on probability. It is uncertainty that explains why agents prefer liquid assets to illiquid ones (and are correspondingly prepared to pay a premium for liquidity) as a way to keep – at least in part – their options open (or better, more open than would otherwise be the case) in the face of unforeseen developments of the economy.[41]

8. Conclusions

Keynes walks a tight rope between the Scylla of the uncritical empiricism of the frequentist tradition, the logical foundation of which he criticises, and the Charybdis of the solipsism of the subjective approach, which risks focusing on logical consistency alone while ignoring the fact that the subject is somehow anchored to the real world. The old dilemma of the conciliation between the body and the mind, between external reality and thought, cannot have a definitive theoretical solution; however, according to Keynes this does not mean that good sense (what he calls rationality) cannot provide a bridge, albeit, admittedly, imperfect: we must somehow – as well as we can – find our way in an uncertain world. Although we cannot logically 'prove' the existence of the external world, let alone whether and how it conforms to our representation of it, we should try and keep account of the information we believe we have about it.

There is not a unique scientific way of translating information into theories and decision-making processes. However, while differing from one agent to another, personal probabilities will retain intrinsic reference to an 'objective' probability – where 'objective' does not mean a magnitude endowed with real existence, but a construct of our own mind based on the assumption that we behave 'rationally', taking into account what we know about the situation under consideration.[42] (Similarly, in the *Theory of Moral Sentiments* Smith (1759) referred to the 'man from within', the invisible

40 As far as I know, this point – which is related to the first one – has not been made by previous commentators. Gillies and Ietto-Gillies (1991: 404–8) identify 'intersubjective' probability (see above, footnote 11) behind Keynes's entrepreneurs' long-term expectations.

41 On this, see Runde (1994), who also provides a critical illustration of other interpretations (Tobin's, Davidson's, Makowsky's) of the relationship between uncertainty and liquidity preference in Keynes.

42 See the quotation from Keynes (1921: 19) given above in footnote 12.

arbiter for our moral conduct that, while somehow representing an objective constraint on our behaviour, keeps into account each person's state of knowledge. As a result of this, when judging a certain behaviour the invisible arbiter may evaluate differently one person from another because of differences in their state of knowledge, even if the external circumstances of their actions are the same). Economists are not automata pre-programmed to give automatic answers (which, apart from errors, should be identical regardless of who answers) to all questions following pre-programmed rules; but at the same time they are not free to say whatever they like. For economists, as for social scientists in general, there is an overall moral rule guiding their research: a deontology of research should be adopted, according to which the 'facts' should be seriously taken into account and an open mind should be kept when considering rival theories. In a sense, the requirement for the good scientist is the same as that for the good practical agent: to behave like a good *pater* (or *mater*) *familias* in disentangling our way between theoretical (deductive) reasoning and 'messy acquaintance with the facts'.

Acknowledgements

A preliminary draft of this paper was presented at a conference organised by the Faculty of Statistics, University of Rome 'La Sapienza', on 16 January 2003. Thanks for comments and suggestions are due to Anna Carabelli, Alessandro Cristiani, Marco Dardi, Massimo De Felice, Carlo Panico, Mario Tonveronachi, and to two anonymous referees; the usual caveat applies more strongly than usual.

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Abstract

Most interpreters agree that Keynes had a wide-ranging, complex, 'vision of the world', which underlies his theoretical contributions. Whenever this is forgotten, as happens in the so-called neoclassical synthesis, not only the original Keynesian spirit goes lost but also, and especially, we lose substantive bricks for our theoretical constructions. The paper considers an important instance of this general rule; namely Keynes's views on the logic of probability, meant as the field concerning human behaviour in an uncertain world (hence connected to, but distinct from, the pure theory of probability, meant as a field of mathematics). The paper begins by recalling the main aspects of the classical and frequentist approaches to probability and the main criticisms they received, pertaining among other things to the limits of their applicability. We then consider Keynes's own views, stressing three aspects: the definition of probability as pertaining to the field of logic, the notion of uncertainty and of the 'weight of the argument', the 'theory of groups'. We then discuss the subjective approach of de Finetti, Ramsey and Savage, and contrast it with Keynes's own views. Finally, we consider the implications of our analysis for the interpretation of Keynes's *General Theory*, and of his attitude towards econometrics.

Keywords

J. M. Keynes, probability, risk, uncertainty