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## Keynes's A Treatise on Probability is based on Boole's formal, mathematical, symbolic logic: Keynes was a formalist and logicist

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### Abstract

*J M Keynes's work in his A Treatise on Probability (1921) is based on the formal application of Boole's relational, propositional logic and interval valued probability as presented in his The Laws of Thought (1854). Keynes then based his A Treatise on Money and General Theory on his work in Part II of the A Treatise on Probability as presented in Chapters X through XVII. This work is the basis for Keynes's comments on mathematical and statistical methods and application in chapters 6,7 and 8 of Volume I of his 1930 A Treatise on Money and chapters 4 ,12 and 17 of the General Theory on using approximation, inexact measurement and reasonable calculation as opposed to precise ,exact, unreasonable mathematical expectations.*

*Keynes's clear statements in his Treatise on Probability about the nature of his technical work can only lead to the following conclusion -Keynes was a formalist and a logicist .All existing orthodox and heterodox "interpretations" of Keynes's works are attempts by economists to reinterpret Keynes's work ,so as to try to fit him into some version of either Benthamite utilitarianism (precise probability and precise utility a la James Tobin,1958) or a nihilist position(Joan Robinson ,G L S Shackle ,T. Lawson ,Post Keynesians ,Institutionalists).*

*Keynes's position is so clearly defined and presented by him in his A Treatise on Probability that the only conclusion possible is that both orthodox and heterodox economists have never read Keynes's A Treatise on Probability ,except possibly for small , scattered bits and pieces cobbled together in a confused and confusing manner .The justification for this decision not to read the A treatise on Probability is based on the false claim that F P Ramsey had shown that Keynes's formal, Boolean, relational , propositional logic was erroneous .Accepting Ramsey's false claims thus made it impossible for economists to see that the*

underlying foundations of the *A Treatise on Money and General Theory* are built on the formal, mathematical analysis presented in Part II of Keynes's *A Treatise on Probability*.

Keynes later extended his propositional logic to include an introductory use of predicate (1st order) logic as defined on p.56 and applied in chapter 33 of the *A Treatise on Probability* on pp.405-425.

**Keywords:** Boolean relational, propositional logic; mathematical, formal, symbolic logic; objective, logical, probability relation; related versus unrelated propositions.

## Section 1. Introduction

The paper will be organized in the following manner. Section Two will deal with Keynes's construction of his relational propositional logic in Chapters I and II of the *A Treatise on Probability* (TP,1921). They were built on Boole's chapters I,XI and XII. Section Three will deal with Keynes's extensive, advanced application of Boole's relational, propositional logic in Parts II, which was built on Boole's work in chapters XVI to XXI of *The Laws of Thought* (LT,1854). Section Four will deal with the applied results of Keynes's work on interval valued probability in chapters XV and XVII of Part II of the TP. Section Five will show that Keynes's comments on pp.39-40 and pp.43-44 in his chapter Four of the *General Theory* (GT,1936) are simply summaries of his previous work already done in chapters XV and XVII of the TP, but applied specifically to economics. Keynes's discussions in chapters 12 and 17 of the GT on reasonable calculation, as opposed to the unreasonable calculations based on "strict" and "exact" mathematical expectations, which were criticized on pp.161-163 of the *General Theory* (GT,1936), lead to Keynes's discussions on p.240 concerning the estimation and calculation of marginal efficiency of capital values. Keynes makes it very clear that such calculations must incorporate both probability (risk) and weight (confidence) estimates. This then leads back to Keynes's conventional coefficient of weight and risk,  $c$ , in chapter 26 of the TP, which is discussed by Keynes in his last exchange with Townshend in December 25, 1938 in his letter of reply to Townshend's question of 'how does one connect the concept of liquidity preference in the GT to the concept of evidential weight in the TP', while also incorporating Keynes's non numerical, interval valued probabilities. The only possible answer is by the use of Keynes's  $c$  coefficient.

Section Six concludes that present day academicians, writing on Keynes, are completely confused and intellectually lost. This is due to three factors. The first factor is relying on the errors of Joan Robinson, a mathematically inept, confused, innumerate and illiterate economist. The second factor is relying on Frank P. Ramsey's incomprehensible errors about Keynes' logical theory of probability, as presented in 1922 in *Cambridge Magazine* and in 1926 in "Truth and Probability", published in 1931. The third factor is to try to combine Keynes with Robinson and Ramsey, as done by the Post Keynesian, Institutional and Heterodox schools.

This 50 years of confusion all comes to a head in the following conclusion reached by B. Bateman, in his role as the special editor, selected by the *Journal of the History of Economic Thought* in December, 1921, to honor the 100<sup>th</sup> centenary of the publication of Keynes's TP. Bateman asserts that, after over 45 years of study of Keynes's TP by economists, the following conclusion is asserted to hold: "...we are still faced with unresolved, fundamental questions..." (Bateman, 2021) about Keynes's *A Treatise on Probability*.

It is an obvious conclusion that, if after nearly 50 years of alleged study of Keynes's TP by economists, which has resulted in the publication of many thousands of journal articles and hundreds of books, economists are still facing "unresolved, fundamental questions" about the TP, then these economists simply do not know what they are doing or talking about. The reason for this conclusion is that they are (a) unable to follow the rigorous application of Keynes's Boolean algebra and logic (relational, propositional and predicate (1<sup>st</sup> order) logics in Parts II and V of the TP which lead to Keynes's exposition in chapters XV and XVII concerning (b) the applications of interval valued probability and inverse probability.

## Section 2. Keynes's Boolean relational, propositional logical foundation for his chapters I and II of the TP: Boole's chapters I, XI and XII

J M Keynes was very clear about the importance of the relational, propositional logic that he was going to deploy in the *A Treatise on Probability*:

"This chapter has served briefly to indicate, though not to define, the subject matter of the book. Its object has been to emphasize the existence of a logical relation between two sets of propositions in cases where it is not possible to argue demonstratively from one to the other. This is a contention of a most fundamental character. It is not entirely novel, but has seldom received due emphasis, is often overlooked, and sometimes denied. The view, that probability arises out of the existence of a specific relation between premiss and conclusion, depends for its acceptance upon a reflective judgment on the true character of the concept. It will be our object to discuss, under the title of Probability, the principal properties of this relation." (Keynes, 1921, pp.8-9).

Keynes is very straightforward and up front in admitting that

".... It is not entirely novel, but has seldom received due emphasis, is often overlooked, and sometimes denied." (Keynes, 1921) as he had already pointed out in a footnote on p. 5 that

"The point was emphasized by Boole, *Laws of Thought*, pp. 7 and 167." (Keynes, 1921, TP, p.5)

Unfortunately, the last fifty years of work by all orthodox and heterodox schools of economics on Keynes's logical theory of probability still have no idea about Keynes's version of Boole's relational propositional logic, which is built directly on the original relational, propositional logic of George Boole. Keynes gives an excellent preview of Boole's approach in chapters I and II before presenting a full scale discussion in chapters X, XI and XII that lead to the heart of his system-Axiom (i), which is fully covered on pp.134-138 in chapter XII.

The result of this ignorance of the Boolean foundation for Keynes's work in probability permeates all of the work of all "fundamentalist Keynesians" and "Keynes scholars". It leads to incomprehensible claims about Keynes as is illustrated, for example, in Misak's 2020 biography of F P Ramsey.

It is simply impossible for any solid, concrete research on Keynes's *A Treatise on Probability* to emerge until the erroneous works of the last fifty years on Keynes, published by economists, historians and philosophers, are consigned to history's trash dump.

It will be shown that no economist writing on Keynes's *A treatise on Probability* has any idea that Keynes's relational, propositional logic, introduced in chapters I and II of the TP and employed in great detail in chapters X-XVII, is an improved version of Boole's original approach to relational propositional logic as presented in 1854 in *The Laws of Thought* (LT,1854), which Boole introduced in chapters I, XI and XII of LT. No economist could possibly read Part II of the TP, which is a far more advanced version of what Keynes presents in his introductory chapters I and II, because they have no idea about what Keynes was doing in chapters I and II. All economists, philosophers and historians writing on Keynes interpreted Keynes's chapters I and II of the TP as being based on a mix of Plato's metaphysical relations and Moorean Intuitionism. Given that Part III of Keynes's TP is built on Part II and that Part V is built on Parts II and III, it is easy to conclude that there are no economists who had any idea about the technical, mathematical and statistical corpus of Keynes's logical theory of probability, even though economists have been writing hundreds of books and many thousands of articles on Keynes's TP for about 50 years.

The conclusion will be that the work of some of the heterodox economists examined in this paper directly contradicts the vastly superior, original assessments made by F. Y. Edgeworth, B. Russell and C.D. Broad over 100 years ago in 1922.

The major reason for the many errors of omission and commission that permeate their work is their acceptance, to either a greater or lesser degree, of the entirely bogus claims made by F P Ramsey in 1922 and 1926 about Keynes's relational propositional logic. Ramsey himself never understood at any time in his life what the original work done in propositional logic by George Boole involved.

The acceptance of Ramsey's claims about Keynes's theory were simply accepted as being correct by economists, either in part or as a whole. Ramsey's utterly preposterous claims were then substituted for Keynes's theory, so that practically nothing written by the economists examined in this paper has any foundation in what Keynes actually presented as his theory of logical probability in the TP in 1921.

### Section 3. Keynes's advanced Boolean logic and algebra in Part II of the TP

Keynes could not be clearer about what he intended to do in Part II of the TP. On the very first page, Keynes states the following:

"... In Part II. I pass to its

Formal Logic. I am not certain of how much positive value this Part will prove to the reader. My object in it is to show that, starting from the philosophical ideas of Part I., *we can deduce by rigorous methods out of simple and precise definitions* the usually

accepted results, such as the theorems of the addition and multiplication of probabilities and of inverse probability. The reader will readily perceive that *this Part would never have been written except under the influence of Mr. Russell's Principia Mathematica*. But I am sensible that it may suffer from the overelaboration and artificiality of this method without the justification which its grandeur of scale affords to that great work. *In common, however, with other examples of formal method*, this attempt has had the negative advantage of compelling the author to make his ideas precise and of discovering fallacies and mistakes. It is a part of the spade-work which a conscientious author has to undertake, though the process of doing it may be of greater value to him than the results can be to the reader, who is concerned to know, as a safeguard of the reliability of the rest of the construction, that the thing can be done, rather than to examine the architectural plans in detail. *In the development of my own thought, the following chapters have been of great importance. For it was through trying to prove the fundamental theorems of the subject on the hypothesis that Probability was a relation that I first worked my way into the subject; and the rest of this Treatise has arisen out of attempts to solve the successive questions to which the ambition to treat Probability as a branch of Formal Logic first gave rise.*" (Keynes, 1921, p.115; italics added). And "Probability is concerned with *arguments* (author's note - Keynes's emphasis), that is to say, with the "bearing" of one set of propositions upon another set. If we are to deal formally with a generalized treatment of this subject, we must be prepared to consider relations of probability between any pair of sets of propositions, and not only between sets which are actually the subject of knowledge. But we soon find that some limitation must be put on the character of sets of propositions which we can consider as the hypothetical subject of an argument, namely, that they must be possible subjects of knowledge. We cannot, that is to say, conveniently apply our theorems to premisses which are self-contradictory and formally inconsistent with themselves. (Keynes, 1921, p.116)

and "The distinction between the Relational Logic of Inference and Probability, and Mr. Russell's Universal Logic of Implication, seems to be that the former is concerned with the relations of propositions in general to a particular limited group. Inference and Probability depend for their importance upon the fact that in actual reasoning the limitation of our knowledge presents us with a particular set of propositions, to which we must relate any other proposition about which we seek knowledge. The course of an argument and the results of reasoning depend, not simply on what is true, but on the particular body of knowledge from which we have set out. Ultimately, indeed, Mr. Russell cannot avoid concerning himself with groups. For his aim is to discover the smallest set of propositions which specify our formal knowledge, and then to show that they do in fact specify it. In this enterprise, being human, he must confine himself to that part of formal truth which we know, and the question, how far his axioms comprehend all formal truth, must remain insoluble. But his object, nevertheless, is to establish a train of implications between formal truths; and the character and the justification of rational argument as such is not his subject." (Keynes, 1921, pp.118-119). There is thus no conflict between the different goals and uses of a relational, propositional logic.

Finally,

"Passing on from these preliminary reflections, *our first task is to establish the axioms and definitions which are to make operative*



our symbolical processes. These processes are almost entirely a development of the idea of representing a probability by the symbol  $a/h$ , where  $h$  is the premiss of an argument and  $a$  its conclusion. It might have been a notation more in accordance with our fundamental ideas, to have employed the symbol  $a/h$  to designate the argument from  $h$  to  $a$ , and to have represented the probability of the argument, or rather the degree of rational belief about  $a$  which the argument authorizes, by the symbol  $P(a/h)$ . This would correspond to the symbol  $V(a/h)$  which has been employed in Chapter VI. for the evidential value of the argument as distinct from its probability. But in a section where we are only concerned with probabilities, the use of  $P(a/h)$  would have been unnecessarily cumbersome, and it is, therefore, convenient to drop the prefix  $P$  and to denote the probability itself by  $a/h$ .”(Keynes,1921,p.119;italics added).

This is one of the very few points where I disagree with Keynes. For the average reader,  $P(a/h)$  is vastly superior to  $(a/h)$ , especially with regards to Keynes’s earlier analysis on p.40, where  $P$  on p.40 is now the relation of probability discussed on p.119. $a/h = P$  could very well lead to confusion, given that Keynes had already used the notation  $a/h = \alpha$ :

“If the conclusion  $a$  bears the relation of probability  $P$  to the premiss  $h$ , or if, in other words, the hypothesis  $h$  invests the conclusion  $a$  with probability  $P$ , this may be written  $aPh$ . It may also be written  $a/h = P$ . This latter expression, which proves to be the more useful of the two for most purposes, is of fundamental importance. ... The value of the symbol  $a/h$ , which represents what is called by other writers ‘the probability of  $a$ .’ (Keynes,1921, p.40; italics added. Keynes could, of course, have added that there would be some purposes when it was not most useful)

Keynes had already given a precise definition in chapters I and II as follows: “Let our premisses consist of any set of propositions  $h$ , and our conclusion consist of any set of propositions  $a$ , then, if a knowledge of  $h$  justifies a rational belief in  $a$  of degree  $\alpha$ , we say that there is a probability-relation of degree  $\alpha$  between  $a$  and  $h$ . \*This will be written  $a/h = \alpha$ .” (Keynes,1921, p.4).

Further, Keynes had already used  $\alpha$  to express a rational degree of belief:

“...Let our premisses consist of any set of propositions  $h$ , and our conclusion consist of any set of propositions  $a$ , then, if a knowledge of  $h$  justifies a rational belief in  $a$  of degree  $\alpha$ ...” In conclusion, it is clear beyond any possible doubt that Keynes’s analysis is a formal (or symbolic or mathematical) logic. Unfortunately, both A. Carabelli and S. Dow have denied this in all of their writings on Keynes’s analysis in the TP going back 47 years:

“Thus, Keynes seemed to see his work on probability as ....an approach which was based on probability, ordinary discourse and common sense....it was a logic of opinion .... which stemmed from Aristotle’s doctrines of rhetorical argument and practical wisdom...” (Carabelli,1988, pp.149-150)

Supposedly, Keynes viewed probability

“...as “opinion” ...confined to the realm of rhetoric.”(Carabelli,1988,p.234)

Therefore, Keynes’s view was that “... probability was grounded on ordinary practice and therefore to be approached by the tools of

ordinary language...rather than by formal and artificial language.”(Carabelli,p.234).

Her severely erroneous mischaracterizations of Keynes’s use of formal, Boolean logic first appear to have been written in 1985:

“His logic of probability ...is to be carefully distinguished from the later developments of probability worked out by Carnap, in the Russellian tradition.” (Carabelli,1985, p.165)

and

“The ‘logician’ interpretation of Keynes is thus based on a hasty reading of Keynes’s text. In various passages Keynes did indeed speak of the logical character of his notion of probability...But this fact does not mean necessarily that, as a logic, it was a logic of the formal type.”(Carabelli,1985,p.166).

or

“Until quite recently, Keynes’s contribution was grouped within the logicist approach to probability, mainly thanks to R. Carnap’s 1950 reading of it.” (Carabelli,1985, p.177).

All of these conclusions reappear in her 1988 book on pages 23 and 145, where it is asserted that Keynes’s logic was not “a logic of the formal type”, but that it was an “ordinary discourse logic.”:

“3.1.1. Keynes’s view of probability, whose basic aspects were considered in the previous chapter, was centred on some general key doctrines. As I have already noted, these doctrines were not always explicit and expressed in univocal and coherent form. Hence the necessity not only of a close reading of Keynes’s text, but also of a sort of systematic reconstruction of Keynes’s approach to key epistemological topics, together with an attempt to clarify his position within its historical intellectual context. Such a task, which will be attempted in the present section, will enable one, for instance, to grasp the fact overlooked in a superficial reading of the Treatise, that Keynes (as we will see in Chapter 8) did not usually adopt the term ‘logical’ in the sense of formal logic, but in the sense of ordinary language logic, that is, in a sense which was actually antithetical to it. This explains the above-mentioned uncritical ranking of Keynes within the so-called logicist approach to probability.”(Carabelli, 1988,3.1.1,p.23)

Surely, it could be expected that Carabelli would explicitly fixed these very severe errors in her latest contribution published in November, 2021? It is impossible to discern any answer in her comments on Keynes’s approach.Carabelli’s latest assessment of Keynes’s formal, symbolic logic is still unclear, vague, ambiguous and confused:

“What is Keynes’s probability? Keynes’s probability is logical judgement; better, It is reasonable judgement. It is *having some reasons to believe*.” Probability is concerned with *arguments*” (TP,126; original italics).” (Carabelli,2021, p.5(first paragraph of section 5)

Of course, Carabelli’s quote from page 126 of the CWJMK ,1973 edition of the TP is on page 116 of the 1921 edition that I have presented above. Keynes’s argument form involves an explicit application of formal ,Boolean, relational ,propositional logic .The one sentence quoted by Carabelli appears after Keynes’s extensive statement on page 115 that his logic is a formal ,symbolic logic .So, while the many claims about some “ordinary discourse logic “ have,for the most part , disappeared ,what has replaced it is deliberate vagueness and ambiguity .A good one sentence quote to

use, if Carabelli was actually interested in correcting her myriad errors from her 1988 book and earlier /later articles, would have been taken from page 115 of the TP :

“In Part II. I pass to its Formal Logic.” (Keynes,1921, p.115).

This simple sentence is crystal clear and easy for a reader to grasp and understand. Carabelli fails to state the obvious.

Dow simply repeats what Carabelli has stated about ordinary logic and ordinary language. I will use two examples. I examined 50 of her publications and, in each publication, I found the same type of imaginative claims made about Keynes's so called ordinary discourse logic, all without any citations to the TP:

“Keynes explained how ordinary language could provide a basis for knowledge about organic systems.... ordinary logic proceeds pluralistically ... This was a key feature of Keynes's philosophy; ordinary logic required ordinary language.” (Dow,2017,pp.20-21- Dow claims that this is in Keynes's *A Treatise on Probability* 1921(1973))

There is no such discussion as claimed by Dow above anywhere in Keynes's TP. This entire claim has just been created out of nothing by Dow. Dow's claims are very similar to the types of claims involving made up definitions about Keynes, definitions without any page citations, used by Frank Ramsey in his reviews of Keynes's TP in 1922 and 1926. No page citations are given to the TP by Dow.

Consider another example:

“Keynes was thus diverging from the endeavours of Russell and (the early) Whitehead to build a complete mathematical system based on classical logic. His (human, or ordinary) logic was more suited to conditions of uncertainty. As with Hume's epistemology, Keynes founded his logic on common sense and convention and he employed ordinary language.”<sup>14</sup>

Just as the scientist organizes observations according to prior conceptualizations and patterns, so the individual or group in society has to apply judgement to observation.

... The theory of probability was thus subjective in the sense that the evidence brought to bear and its assessment in relation to other sources of knowledge involved judgement. But it was objective in the sense that anyone in the same circumstances and with the same understandings of the evidence would arrive at the same judgement.<sup>15</sup>

When Keynes turned to economics, this epistemology had strong methodological implications. (Chick 2003). First, he regarded economics as an art. While the father had privileged deductive theory as the core of the discipline, with values imported later and 'unscientific' methods only introduced at the stage of policy application, the son privileged the art of application at the core.

For him, the requirements of practical application determine the methodology of theory development and, given the open-system nature of the subject matter, that methodology was pluralist. Ordinary logic required multiple strands of reasoning and evidence which could lend weight to argument, for economists as well as economic agents.” (Dow,2017, p.35).

Dow's claim, that

“... His (human, or ordinary) logic was more suited to conditions of uncertainty. As with Hume's epistemology, Keynes founded his

logic on common sense and convention and he employed ordinary language.”<sup>14</sup>

is completely wrong, as Keynes dealt with uncertainty by interval valued probability or decision weights, which was based directly on the extensive relational, propositional, formal, Boolean logic of Chapters X-XIV of Part II of the TP. There is no such thing as Dow's ordinary logic based on common sense and convention. All of this has been made up in Dow's imagination much like Ramsey's claims about an Axiom I (Ramsey,1922,p.3) that does not exist.

It is unclear to me how much of an influence on Carabelli and Dow has been exerted by J. Robinson, and then T. Lawson, in their ongoing efforts to promote the false belief that Keynes was an anti-formalist, anti-logicist and anti-mathematical thinker. Lawson himself is completely ignorant of the formal, mathematical foundations contained in the TP, which underlie Keynes's GT. Lawson has never grasped the concept of interval probability. Basically, Lawson has absolutely no idea of the connections that exist between Boole's 1854 *The Laws of Thought* (LT) and Keynes's 1921 *A Treatise on Probability* (TP).

The grave intellectual dangers made in taking Carabelli's work too seriously are illustrated in the following statement in P. Clarke's very recent 2023 book, *Keynes in Action*, published by Cambridge University Press 36 years after Carabelli (1988):

“Here is the basis for Anna Carabelli's pithy contention:

‘Keynes's probability shared all the attributes of Moore's concept of goodness: it was a simple notion, unanalyzable, indefinable, non-natural, directly perceived or intuited and objective’ (Carabelli,1988, p.31).” (Clarke,2023, p.123)

All of Carabelli's claims in the quote above are false. They follow directly from the claims first made by F P Ramsey in a 1923 Apostles paper, titled “Induction: Keynes and Wittgenstein”, that very severely misinterpreted Keynes's Boolean, relational, propositional logic as being based instead on Plato and Moore and not Boole.

## Section 4. The applied results following from Chapters X-XIV and Chapter XV of the TP

Chapter XV in Part II of the *A Treatise on Probability* is where the final word is provided by Keynes on measurement, who then applies interval valued probability throughout the rest of the book. Keynes was very explicit on pp.37-38 of the *A Treatise on Probability* that what he was going to do on pp.38-40 was only to provide a provisional introduction.

Consider the following statements by Keynes:

“It will not be possible to explain in detail how and in what sense a meaning can sometimes be given to the numerical measurement of probabilities until Part II. is reached. But this chapter will be more complete if I indicate briefly the conclusions at which we shall arrive later. It will be shown that a process of compounding probabilities can be defined with such properties that it can be conveniently called a process of addition. It will sometimes be the case, therefore, that we can say that one probability C is equal to the sum of two other probabilities A and B, i.e.  $C = A + B$ . If in such a case A and B are equal, then we may write this  $C = 2A$  and

say that C is double A. Similarly, if  $D = C + A$ , we may write  $D = 3A$ , and so on.” (Keynes,1921, p.37).

In other words, in Part II Keynes investigated whether probabilities are always additive or not. Keynes is also very clear that no detailed analysis is going to be provided on pp.38-40 of chapter III of the TP. He will “... indicate briefly the conclusions at which we shall arrive later .”

These conclusions are valuable but were only understood by Kyburg alone. Based on these pages, Kyburg did point out that Keynes’s graphical analysis on pp.38-40 of the TP demonstrated his intuitive understanding of Boole’s analysis of interval probability and of Boole’s analysis of glb ‘s and lub’s ,as contained in pp.293-325 of the LT; however, Kyburg refused to give Keynes any credit whatsoever for his mathematical specification of interval valued probability in Parts II and III of the TP in any publication during his lifetime .(See Kyburg,1964,1970,1988,1991,1992,1995,1999,2002,2003,2006 and 2010)

Continuing, Keynes states that “An endeavor will be made later to explain in a systematic way how and in what circumstances such orders can be established. The argument for the theory here proposed will then be strengthened. For the present it has been shown to be agreeable to common sense to suppose that an order exists in some cases and not in others.” (Keynes,1921, p.38).

It is in chapter XV of the TP, which is titled “Numerical Measurement and Approximation of Probabilities “, that these questions are answered in a systematic way:

“It is evident that the cases in which exact numerical measurement is possible are a very limited class, generally dependent on evidence which warrants a judgment of equiprobability by an application of the Principle of Indifference.... *The sphere of inexact numerical comparison is not, however, quite so limited. Many probabilities, which are incapable of numerical measurement, can be placed nevertheless between (author’s note-Keynes’s emphasis) numerical limits. And by taking particular non-numerical probabilities as standards a great number of comparisons or approximate measurements become possible.* If we can place a probability in an order of magnitude with some standard probability, we can obtain its approximate measure by comparison. This method is frequently adopted in common discourse.” (Keynes,1921, pp.159-160; italics added).

It is on these pages that various misinterpretations of the diagram on p.38 of the TP, as being an illustration of ordinal valued probability by Keynes, have been based. Keynes makes it clear that not just one problem is being formulated and solved:

“It is not worthwhile to work out more of these results here. Some less systematic approximations of the same kind are given in the course of the solutions in Chapter XVII. In seeking to compare the degree of one probability with that of another we may desire to get rid of one of the terms, on account of its not being comparable with any of our standard probabilities. Thus, our object in general is to eliminate a given symbol of quantity from a set of equations or inequations. If, for instance, we are to obtain numerical limits within which our probability must lie, we must eliminate from the result those probabilities which are nonnumerical.

This is the general problem for solution. (55)

A general method of solving these problems when we can throw our equations into a linear shape so far as all symbols of probability are concerned, is best shown in the following example:—... where  $\lambda, \mu, \nu, \rho, \sigma, \tau, \upsilon$  represent probabilities which are to be eliminated, and limits are to be found for c in terms of the standard probabilities a, b, d, e, and 1.”(Keynes,1921,pp.162-163)

Thus,  $\lambda, \mu, \nu, \rho, \sigma, \tau, \upsilon$  on page 163 are non-numerical probabilities that are just like the non-numerical probabilities V, Z, W, X, Y, U from the diagram on p.39. Keynes restricts this example to the case where “when we can throw our equations into a linear shape.” However, Keynes makes it crystal clear that Boole’s technique works with nonlinear equations also in his footnote on page 161 of the TP:

“\*In Boole’s Calculus we are apt to be left with an equation of the second or of an even higher degree from which to derive the probability of the conclusion; and Boole introduced these methods in order to determine which of the several roots of his equation should be taken as giving the true solution of the problem in probability. In each case he shows that that root must be chosen which lies between certain limits, and that only one root satisfies this condition. The general theory to be applied in such cases is expounded by him in Chapter XIX. of The Laws of Thought, which is entitled “On Statistical Conditions.” But the solution given in that chapter is awkward and unsatisfactory, and he subsequently published a much better method in the Philosophical Magazine for 1854 (4th series, vol. viii.) under the title “On the Conditions by which the Solutions of Questions in the Theory of Probabilities are limited.” (Keynes,1921, p.161).

All of the four curvatures in the diagram on p.39(p.42 of the CWJMK version, Vol.8) of the TP are quadratic, second order, parabolic equations, to which Boole’s technique can be applied in order to arrive at a root which has an upper and lower bound or limit.

The claim, then, that the diagram on p.39 (p.42 of the CWJMK version,Vol.8) is an illustration of an application of ordinal probability, has no support at all.

The diagram on p.39(p.42 of the CWJMK version, Vol.8) was an introductory, initial illustration of Keynes’s interval valued probability approach showing nonlinearity and non-additivity of the different paths illustrated by parabolas that Keynes then explained in much greater detail in chapter XV and XVII of the TP, as opposed to the linear and additive OAI. Chapter XV must be read and understood as supplying the final analysis promised by Keynes on pp.37-38 of the TP. Nowhere in chapter 15, or in chapters 16 or 17, is there any mention of ordinal probability whatsoever.

In conclusion, the diagram on p.39 can only be understood after chapter XV has been absorbed. Keynes’s great concern with the addition property (additivity) in Part II simply means that any interpretation that Keynes’s system of probability is ordinal doesn’t make any sense because ordinal probability can’t be multiplied or added. It is an oxymoron to state that Keynes is concerned with the property of additivity and at the same time that his theory is an ordinal one in light of all of the worked-out interval estimate problems contained in the TP.

Keynes’s alternative formulation to the use of interval valued probability is his conventional coefficient of weight and risk, c, from chapter 26. This is an alternative to the much more difficult



interval valued probability analysis that Keynes provided in chapters 15 and 17 of the TP. The initial probabilities are additive in chapter 26. Keynes then transforms the additive probabilities into decision weights (conventional coefficients) by multiplying by  $[2w/(1+w)]$  so as to introduce non additivity and multiplying by  $[1/(1+q)]$  in order to introduce nonlinearity. It is impossible for  $p$  and  $q$  to be ordinal probabilities, since Keynes explicitly stated that they were additive. Finally, Keynes recognized that the common man uses interval valued probability, not ordinal probability, which is simply far, far too weak to allow for any sort of analysis which is supposed to serve as a guide to life.

## Section 5. From Chapter XV(XVII) and XXVI of the TP to Chapters 4,12 and 17 of the GT

Consider the following repetition of Keynes 's Part II, TP discussion in chapter XV of the TP, as well as his extensive deployment of Parts I, III, and V of the TP in chapters 6,7, and 8 of Volume I of his 1930 *A Treatise on Money* (TM,1930), in the GT:

*“The fact that two incommensurable collections of miscellaneous objects cannot in themselves provide the material for a quantitative analysis need not, of course, prevent us from making approximate statistical comparisons, depending on some broad element of judgment rather than of strict calculation, which may possess significance and validity within certain limits.”*

But the proper place for such things as net real output and the general level of prices lies within the field of historical and statistical description, and their purpose should be to satisfy historical or social curiosity, a purpose for which perfect precision—such as our causal analysis requires, whether or not our knowledge of the actual values of the relevant quantities is complete or exact—is neither usual nor necessary.”(Keynes 1936,pp.39-40;italics added)

and

“It is my belief that much unnecessary perplexity can be avoided if we limit ourselves strictly to the two units, money and labour, when we are dealing with the behaviour of the economic system as a whole; reserving the use of units of particular outputs and equipments to the occasions when we are analyzing the output of individual firms or industries in isolation; and the use of vague concepts, such as the quantity of output as a whole, the quantity of capital equipment as a whole and the general level of prices, to the occasions when we are attempting some historical comparison which is within certain (perhaps fairly wide) limits avowedly unprecise and approximate.”(Keynes,1936,pp.43-44,italics added).

Again, Keynes's contrast is between imprecise and inexact measurement on the one hand, which he recommends, as opposed to precise and exact measurement which he rejects on the other hand. Keynes's general opposition to exact, quantitative methods is not an admonishment against the use of formal, mathematical and statistical methods, in general. It is an admonishment against the use of precise point probability combined with advocacy for the use of imprecise interval probability or decision weight approaches to decision making under uncertainty.

It is obvious, if a academician has read Part II of Keynes's TP, that the claim made about Keynes by the Fundamentalist Keynesians (J. Robinson, Shackle, Skidelsky, etc.), that he was an advocate of

Marshall's dictum, that one should burn one's formal mathematical analysis after obtaining the result, and, instead, make only a verbal English prose presentation, is unsupported.

The following is an example of how Keynes would have proceeded to deploy his Part II approach. Consider the following interval valued probability  $[.3,.8]$ , where we will interpret the lower bound, .3, to be a greatest lower bound (glb), and interpret .8, the upper bound, to be a least upper bound (lub). Note that all rational decision makers will agree just as they would agree that there is only one rational estimate when using Keynes's version of the Principle of Indifference, that there can be only one lub and one glb. However, while the decision makers have the exact same information or data set, and have the same skill set of technical, mathematical, statistical, logical and probabilistic techniques, they can have differing emotional responses to the decision situation above. The interval probabilities are deficient in their evidential weights, so a decision maker can lack confidence in the interval estimate. This would appear to mean that he should remain liquid and delay making a decision, hoping new evidence will result in a narrower range between the upper and lower probability bounds, such as  $[.5,.6]$ , sometime in the future. Here Keynes, in 1936, adds his new concept of optimism-pessimism, animal spirits, in the same chapter where he introduced his additional concept of changes in the evidential weight as measured by the "state of the news". Positive animal spirits (optimism) will lead a decision maker to concentrate on the upper bound while negative animal spirits (pessimism) will lead him to concentrate on the lower bound.

Keynes's important point here is that proactive, macroeconomic government policy can not only add evidential weight, so as to decrease somewhat the range of the interval estimate, thereby increasing confidence, but also create a positive atmosphere promoting the creation/generation of optimistic animal spirits:

“This means, unfortunately, not only that slumps and depressions are exaggerated in degree, but that economic prosperity is excessively dependent on a political and social atmosphere which is congenial to the average businessman. If the fear of a Labour Government or a New Deal depresses enterprise, this need not be the result either of a reasonable calculation or of a plot with political intent;—it is the mere consequence of upsetting the delicate balance of spontaneous optimism. In estimating the prospects of investment, we must have regard, therefore, to the nerves and hysteria and even the digestions and reactions to the weather of those upon whose spontaneous activity it largely depends.” (Keynes,1936, p.162)

and

“But individual initiative will only be adequate when reasonable calculation is supplemented and supported by animal spirits...” (Keynes,1936, p.162).

Thus, animal spirits can serve only as a supplemental and supporting function. They are not a primary variable, but a secondary one. The primary tool of analysis is reasonable calculation based on interval valued probability and decision weights allowing the businessman to incorporate the important concept of degree of confidence in the estimate of his probability into his decision making calculus.

The entire analysis above can be presented again using Keynes's decision weight approach that he called a conventional coefficient of risk and weight in chapter XXVI of the TP on p.315.

It is in the final Keynes-Townshend exchange in late 1938, where Townshend asks Keynes to pinpoint where the discussion is that ties Keynes's concept of evidential weight of the TP to his GT concept of liquidity preference that Keynes tells Townshend that his discussions of probability are linked to his discussions of risk premium while his discussions of weight are linked to the liquidity premium. The only chapter that has such an explicit discussion of both probability and weight simultaneously occurs on p.315 of chapter XXVI as regards Keynes's c coefficient. There is NO other analysis that combines weight and probability in the TP. Here Keynes refers Townshend back to his p.240 comment:

“The owners of wealth will then weigh the lack of “liquidity” of different capital equipment(s) in the above sense as a medium in which to hold wealth against the *best available actuarial estimate* of their prospective yields after allowing for risk. The liquidity-premium, it will be observed, is partly similar to the risk-premium, but partly different; — the difference corresponding to the difference between the *best estimates* we can make of probabilities and the *confidence* with which we make them.[7] When we were dealing, in earlier chapters, *with the estimation of prospective yield, we did not enter into detail as to how the estimation is made*: and to avoid complicating the argument, we did not distinguish differences in liquidity from differences in risk proper. It is evident, however, that in *calculating the own-rate of interest we must allow for both*.

There is, clearly, no absolute standard of “liquidity” but merely a scale of liquidity — a varying premium of which account has to be taken, in addition to the yield of use and the carrying-costs, in estimating the comparative attractions of holding different forms of wealth.” (Keynes,1936, p.240; italics added).

Again, the only method of calculating estimates that “... distinguish differences in liquidity from differences in risk proper. It is evident, however, that in calculating the own rate of interest we must allow *for both*...” is that occurring on p.315 of the TP that involves the c coefficient. There is no other possibility.

The reader is advised to see the Brady, 2024 paper in *Theoretical and Practical Research in the Economic Field*.

## Section 6. Conclusions

Economists are simply ill-equipped and ill-trained intellectually to grasp why the logical foundations carefully developed by Keynes for his TP are also the logical foundations for the theory of macroeconomics presented by Keynes in 1930 in Volume I of the TM and in the GT in 1936 because only Keynes's interval valued /decision weight approach can deal with the uncertainty negatively impacting the macro economy. Economists are tied to either a concept of probability that is numerical, additive and linear, as demonstrated by J. Tobin in 1958 or to claims that probability, however defined, can play no role when faced by fundamental uncertainty, by which is meant ignorance, a term created by two nihilists, Joan Robinson and G L S Shackle, who simply confused/conflated Keynes's uncertainty (situations of partial knowledge and partial ignorance)with their fundamental uncertainty(ignorance). See my references to Shackle (1938,1939,1949a,b,1952,1953,1955,1961a,b )and Dimand (2021) for an example of this ongoing confusion in the field of economics

and philosophy. This result is an intellectual mess of confused and confusing papers written by economists who are simply ignorant of how Keynes showed how to deal with decision making under uncertainty by the use of interval valued probability and decision weights. Of course, Keynes's technical work has been advanced even further by T. Hailperin (1986). His followers, who continue to mine the fertile fields of Boolean logic and algebra, have advanced further still.

Economists thus continue to advance the 235 year old claims of Jeremy Bentham about decision makers being able to calculate accurately far into the future using exact sets of numerical probabilities .It is simply not possible ,in such an environment, for Keynes's contributions ,or Adam Smith's much earlier version of Keynesian interval valued probability in his *The Wealth of Nations* , to be understood ,much less applied , taught and implemented in government policy decisions.

An attempt to remedy the vast ignorance that permeates practically all economics journals about Boole and Keynes was made in Brady and Arthmar (2012) and Arthmar and Brady (2016,2017,2018) was made with respect to the journal, *History of Economic Ideas*. However, the 2021 symposium on Keynes's 1921 *A Treatise on Probability*, edited by C. Zappia in the HEI, demonstrated that none of the 2021 articles in HEI had the slightest awareness/understanding of the Boole -Keynes connection that permeates Keynes's work.

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