Scholars Journal of Economics, Business and Management OPEN ACCESS
Abbreviated Key Title: Sch J Econ Bus Manag
ISSN 2348-8875 (Print) | ISSN 2348-5302 (Online)
Journal homepage: https://saspublishers.com

Why is Clive Bell, an Artist with no Training in Logic, Mathematics, Statistics or Probability, being cited as an Expert on J M Keynes's *A Treatise on Probability?*

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DOI: 10.36347/sjebm.2024.v11i04.005 | **Received:** 21.03.2024 | **Accepted:** 27.04.2024 | **Published:** 30.04.2024

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Abstract **Review Article**

Ramsey's criticism, that "… the obvious one is that there really do not seem to be any such things as the probability relations he describes", ignored Keynes's analysis, for example, on page 36 of the *A Treatise on Probability* that required the propositions linking the premises and conclusion had to be similar. Ramsey ignored the fact that Keynes's method was based on using Boole's relational, propositional logic that required that the propositions had to be connected, related or similar before the formal, mathematical, symbolic logic could be applied. Keynes pointed out that the "…analogy *between orders of similarity and probability is so great* that its apprehension will greatly assist that of the ideas I wish to convey" (Keynes, 1921, p.36; italics added). Ramsay failed to grasp Keynes's analogy between similarity and probability before criticizing Keynes. Ramsey's main example of supposed errors in Keynes's analysis relies on examples, such as his "My carpet is green, Napoleon was a great general "(Ramsey, 1922, p.3), which involves dissimilar and unrelated propositions which are not connected. Keynes's introductory comments on p.36 were then explored in Part III of the A Treatise on Probability in far greater depth and detail by Keynes. Keynes's objective, probability relations are simply objective relations connecting old, known situations with new situations that can be shown to be related. Human pattern recognition skills involve using resemblance functions based on past memory that projects past knowledge of old situations into new situations, where there are similarities that are seen to exist between the old, known situation and a new, unexplored situation. One then can come up with a rational degree of belief regarding how some new situation will play out, given the similarities that exist between the old and new situations. All of Ramsey's examples attacking Keynes's theory involve examples such as the green carpet -Napoleon example above. Herbert Simon independently rediscovered some parts of Keynes's Part III analysis in the A Treatise on Probability that dealt with the connection between intuition and induction when he started to analyze the decision-making capabilities of tournament chess players in Over the Board competition, where the players must make decisions under time constrain (a clock), in the 1950's. Simon was a part of the developing fields of Cognitive Science and cognitive psychology that started in the 1950's. Keynes's positions on intuition, induction, similarity (dissimilarity), resemblances, analogy and pattern recognition are all accepted basic conclusions in these fields. Ramsey's concept, that supposed humans were capable of calculating exact, precise probabilities, using the purely mathematical laws of the probability calculus to make decisions, is completely rejected except as a very special case. One very special case would be correspondence (postal) chess, where games can last for 3 years. Another special case would be chess computers, like Deep Blue, that were allowed to bring their "book" knowledge of opening, middle, and end game positions (gigantic libraries containing all known variations of past played games) with them to the chess board. Garry Kasparov, the then World Chess champion, defeated Deep Blue 4-2 in 1996 and lost a rematch 2.5-3.5 in 1997. No cognitive scientist or cognitive psychologist would accept Ramsey's claims about decision making by humans, be it in 1950 or 2020, because all the empirical and experimental evidence is completely against Ramsey's position on the ability of humans to use and calculate accurately with exact, precise probability. It is disturbing to find in recent reviews of Misak's book, including Misak's own commentary on Ramsey, a reliance on a completely and technically ignorant artist friend of Keynes, Clive Bell, who had no idea about what Keynes was doing in the A Treatise on Probability. The reason is simply because he was technically illiterate and had no knowledge of mathematics, statistics, logic, or probability. I find it strange that he is considered a legitimate source that supposedly validates Ramsey's false comments about Keynes's "objective

probability relations". The argument seems to go something like this, where (a), (b) and (c) are the premises of the argument expressed as propositions and (d) is the conclusion of the argument, expressed as a proposition:
Given that:

a) Ramsey was a great mathematical and logical genius
b) Clive Bell knew, lived and talked with Keynes for many years
c) Clive Bell stated that Ramsey's critique demolished Keynes's logical theory of probability

Therefore,

d) Ramsey's critique demolished Keynes's logical theory of probability.

This is what Misak's argument amounts to.

Keywords: Ramsey's criticism, *A Treatise on Probability*, Clive Bell, Keynes's.

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SECTION I. INTRODUCTION

The paper will be organized in the following fashion. Section Two will examine the claims made in the latest review of Misak's book that uses Bell as an expert witness. An examination of this review, which is typical of the claims made by various academics concerning Ramsey's so called argument demolishing Keynes's theory over the last 100 years, shows that it is highly questionable, at best. These claims usually rest or start with Braithwaite's claim that he read the A Treatise on Probability between two academic terms in Cambridge in mid-1921, but discovered his error by reading Ramsey's critique. It is basically composed of the same types of erroneous analysis presented above in (a), (b), (c) and (d). Section Three will examine what I believe to be the main source for the many errors made about Keynes's theory that exists as of 2020-The 1973 Editorial Foreword of Brathwaite's placed at the front of the 1973 CWJMK edition, Volume 8, of Keynes's A Treatise on Probability, by Donald Moggridge and Elizabeth Johnson. Braithwaite's 1973 foreword is very weak intellectually.

Section Four will conclude that there is no evidence that any academic has demonstrated any grasp of Keynes's imprecise, interval valued approach to probability that Keynes based on Boole, but substantially improved, in 1921.This is due to the general acceptance among academicians working in the field of decision making of Ramsey's extremely poor papers on Keynes in 1922 and 1926.

Section 2. Clive Bell, an artist, was a pseudo expert on Keynes's Logical Theory of Probability

Consider the following claim made by Harrod in 1957:

"The volume of personal recollections before us[1] contains an article about our former editor, J. M. Keynes, and a notice in these pages, therefore, seems appropriate. Mr. Clive Bell was an intimate friend of Keynes over a great many years, and future historians will accordingly, whether professional economists like it or not, assign high authority to his testimony" (Harrod, 1957, p.692).

This might be an accurate conclusion as regards some of Keynes's non technical activities. However, it is simply false as regards Keynes's A Treatise on Probability.

Consider the following statement by Mukherjee:

"In 1921, John Maynard Keynes published his Treatise on Probability to great acclaim. Russell hailed it as "undoubtedly the most important work on the probability that has appeared for a long time". The dissenting voice was that of the undergraduate Frank Ramsey who published a critique in the Cambridge Magazine. He pointed out problems with Keynes's theory which he would later expand in his paper on "Truth and Probability" (1926). Keynes considered Frank's criticisms were serious and fundamental.

Roy Harrod wrote:

The only criticism that disturbed Keynes at this time came from . . . an undergraduate at Trinity, Cambridge, who had recently arrived from Winchester.

. . This was Frank Ramsey . . . [whose] . . . criticism carried more weight with Keynes than any other, and it is not clear that he felt that he had a satisfactory answer to it.

Clive Bell, who lived in the same house as Keynes, remarked that Frank had "made a rent" in Keynes's theory "which caused the stitches to run*". This set the pattern and tone of the relationship between Frank and Keynes…*." (Mukherjee, 2020; italics added).

Pace Mukherjee, Keynes never was concerned about Ramsey's critique because Ramsey (a) never understood the analogy between probability and similarity, which was basic and fundamental in Keynes's theory, that Keynes spent nearly a page on (page 36) in Part I introducing it in chapter III,(b) never understood that Keynes's "mysterious, non numerical probabilities" were interval valued probabilities that were imprecise and did not, could not, obey the purely mathematical laws of the probability calculus, and (c) never understood Keynes's nonlinear, non-additive decision weights approach contained in chapter 26 of the TP in his c coefficient.

There were three reviewers who did understand the fundamental nature of the analogy between

probability and similarity. Their names were Francis Ysidro Edgeworth, Bertrand Russell, and C D Broad.

Harrod had no idea what Keynes was talking about because he was a frequentist who, like Ramsey, only accepted the concept of exact, precise additive probability.

Where are the statements, made by Keynes, where Keynes supposedly states that Ramsey's "…. criticism carried more weight with Keynes than any other, and it is not clear that he felt that he had a satisfactory answer to it." (Harrod, 1951) in the 30 volumes of the Collected Writings of John Maynard Keynes (CWJMK, 1973)?

The answer is that there are none. There are no such statements anywhere in the 30 volumes of the CWJMK.

Keynes did not reply to Ramsey because Ramsey never understood, just as Mukherjee, Harrod, Misak, Jeffreys, Monk, Good, Mellor, Gillies, Braithwaite, Mini, Hacking, Skidelsky, and many, many other writers on Keynes still do not understand after 100 years, the fundamental and basic connection that exists between probability and similarity logics for Keynes that Edgeworth, Russell, and Broad did understood. (See references below).

In 1937-1938, Hugh Townshend (see references) engaged Keynes in a detailed discussion about the connections between the A Treatise on Probability (1921; TP) and the General Theory (GT,1936) regarding Keynes's Liquidity Preference (LP) theory of the rate of interest. Keynes could not have been clearer. Keynes stated that LP rested on "my theory of probability" that rests on Keynes's (a) non-numerical (non-additive) probabilities and (b) the evidential weight of the argument from chapters 6 and 26 of the TP. There is no mention made of F P Ramsey.

Keynes then told Townshend that he had to go back and reread what Keynes had written on pages 148 and 240, which are the two pages in the GT that deal with the TP.

Consider the following statement by Mini:

"Clive Bell, who knew Keynes as well as anyone, writes that after the war 'when. [Keynes] took up the manuscript of his old dissertation with a view to making a book, he would …. occasionally hand me a much-corrected sheet saying …" Can you remember what I meant by it?" [36].

Keynes didn't understand in 1920 what he had written in 1907 or 1911 because his outlook and metaphysical presuppositions had changed.

Meanwhile, he made extensive corrections that do not fit with his earlier presuppositions but which create a veritable tower of Babel among interpreters of Probability" (Mini, 1999, p.45).

Mini, of course, is correct that the various academic, economist interpreters of the A Treatise on Probability (for example, Skidelsky, Moggeridge, O'Donnell, Carabelli, Runde, Weatherson, Davis) *have made a gigantic, intellectual mess out of Keynes's work* because they have all based their assessments of Keynes's technical relational, propositional formal, mathematical, symbolic logic on the strident claims made by F P Ramsey in 1922 and 1926 in his reviews (See Ramsey in references).

We again consider Mr. Clive Bell 's claims, an artist who had NO TRAINING in mathematical logic, probability, statistics or mathematics. The only suggested corrections Keynes would have taken seriously would have been those suggested by Bertrand Russell, William Ernest Johnson, C D Broad, and F.Y. Edgeworth, who assisted Keynes in the years of 1919-1920 and 1921 as co editor of the Economic Journal. Why Keynes would show Mr. Clive Bell material that he knew that it was not possible for Mr. Bell to understand is certainly a strange and bizarre claim that is simply accepted as fact by Misak and others.

Now consider the following claims made by Dostaler:

"Clive Bell, who lived in the same house as Keynes at the time the latter was putting the final touches on his manuscript, recalled: 'And after that war, when he took up the manuscript of his old dissertation with the view to making a book, he would…occasionally hand me a much corrected sheet saying… can you remember what I meant by that (C. Bell, 1956, p.59)" (Dostaler, 2007, p.59; also see Dostaler, 2006, p.5).

Again, fundamental errors in introductory logic are on display in Dostaler's retelling of Bell's story.

Now consider C. Misak's 2020 reliance on the same type of claims made by Clive Bell. Misak's story is nearly identical to those that have been told time and again by Monk, Mellor Gillies, Good, H. Jeffreys, R. Braithwaite, Robert Skidelsky, and many, many others:

"Misak tells the story of how economists like John Maynard Keynes and philosophers like Ludwig Wittgenstein and Bertrand Russell reacted to Ramsey's devastating critiques.

"Keynes took it very well," Misak says.

"Clive Bell, a friend of Keynes, said that Ramsey made a rent in Keynes's theory that caused all the stitches to run. So if you picture a garment, Ramsey made a little tear in it and all the stitches ran — and the whole garment fell apart."

The theory in question was Keynes's famous treatise on probability. At the time, Keynes was probably the most influential scholarly voice in Britain, and was so respected at Cambridge that it was suggested the university change its name to 'Keynes-bridge.'(Misak, C. (2020), radio broadcast).

Of course, there is a very severe, intellectual problem here, since Clive Bell had absolutely no idea about the fact that Keynes's theory of probability is an interval valued approach that was built on Boole's original, formal, mathematical, relational, propositional, symbolic logic as contained in his 1854 *The Laws of Thought*. In order to make a dent in Keynes's interval valued theory, a critic would have to first put a dent in Boole's theory. No such paper has ever been published in the 19th, 20th and 21st centuries.

Consider again the following statement from Mini above, which I have demonstrated has very severe, intellectual deficiencies in it:

"Clive Bell, who knew Keynes as well as anyone, writes that after the war 'when [Keynes] took up the manuscript of his old dissertation with a view to making a book, he would …. occasionally hand me a much corrected sheet saying …" Can you remember what I meant by it ?"'

Keynes didn't understand in 1920 what he had written in 1907 or 1911 because his outlook and metaphysical presuppositions had changed" (Mini, 1999, p.45).

Misak's reliance on some story told by Mr. Clive Bell as evidence, regarding Bell's own assessment of the effect of Ramsey's claims about Keynes's theory on Keynes, is simply illogical, given Bell's ignorance about an approach to probability that he could NEVER, EVER grasp in his lifetime due to his own ignorance of mathematics.

I would like to obtain from Misak an explanation of what precisely she thinks Mr. Clive Bell is talking about with regard to the supposed stitches in Keynes's theory all coming out, given that Keynes's theory is an interval valued approach to probability that neither Bell nor Ramsey ever had any inkling about.

Section 3. The editorial foreword of Braithwaite contained in the 1973 CWJMK version of the TP: quite puzzling, indeed?

In a 1931 paper, examined by Brady (2016), Braithwaite made the false claim that it was Harold Jeffreys who had first put forth a logical theory of probability in 1919, two years before Keynes did in 1921.Now this deliberately ignores the fact that Bertrand Russell had made extensive use of Keynes's 1908 Second Fellowship dissertation at Cambridge University, England, in a book published in 1912, titled "The problems of Philosophy", that specifically acknowledges Keynes contribution:

"I have derived valuable assistance from unpublished writings of G. E. Moore and J. M. Keynes: from the former, as regards the relations of sense-data to physical objects, and from the latter as regards probability and induction. I have also profited greatly by the criticisms and suggestions of Professor Gilbert Murray" (Russell, 1912, Preface; boldface added).

Of course, Keynes's logical theory of probability was discussed in Russell's book in 1912, which is seven years before the publication of the Wrinch-Jefferys article.

Forty two years later, Braithwaite is more subtle in his misrepresentation. He merely insinuates that Jeffreys was first. Braithwaite finally acknowledges that Russell's book incorporated a discussion "…to some of his ideas…" (Braithewaite, 1973, p. xv), but "… (except for an article by Dorothy Wrinch and Harold Jeffreys in 1919 ,which Keynes had not seen)the Treatise contains the first publication of …. A logical probability relationship…" (Braithwaite, 1973, p.xvi).

Of course, there is no "except" or but. Keynes's work was acknowledged in 1912 by Russell explicitly as regards specific work done by Keynes in "…. probability and induction" and not as the very vague "…some of his ideas" as claimed by Braithwaite.

However, very quickly, Braithwaite starts making claims for which he provides no evidence at all, but simply makes empty assertions:

"Keynes wrote the Treatise at a time when mathematicians were discovering the conditions required for an axiom set in any field to be formerly satisfactory; and the axiomatic development (in Part II of the Treatise) of the theorems of the probability calculus has serious formal defects. Keynes insisted that most probability relationships are not measurable, and indeed that many pairs of probability relationships are incomparable so that the set of all probability relationships cannot be arranged in simple one -dimensional order between the two extremes of certainty of truth and certainty of falsehood…Keynes's thesis that some probability relationships are measurable and others unmeasurable leads to intolerable difficulties without any compensating advantages" (Braithwaite, 1973, pp .xvi-xvii).

Of course, contrary to the false intellectual assessment of Keynes's work presented by Braithwaite above, Keynes's logical theory of probability approach leads explicitly to imprecise, interval valued probability using upper and lower probability bounds to define non -additive probabilities. These non-additive probabilities,

as well as Keynes's decision weight approach represented by his c coefficient of chapter 26, is concerned with the representation of uncertainty using inexact measurement and approximation which it is simply impossible to analyze with Ramsey's additive, linear approach to probability.

Braithwaite claims that the axiomatic development (in Part II of the Treatise) of the theorems of the probability calculus has serious formal defects. I can find no such defects in Part II of the TP at all. Braithwaite fails to specify or enlighten the interested reader about where these "serious formal defects "appear. What chapters is he talking about? What sections of each chapter do these so called, alleged defects appear? What are the page numbers of the TP where these so called "serious formal defects 'appear'? Braithwaite is completely silent on precisely where these so-called errors appear, which are really figments of Braithwaite's imagination.

Braithwaite's foreword can only lead to a decision on the part of a reader of Braithwaite's forward to skip all of Part II of the TP because it is erroneous.

The fact of the matter is that Braithwaite never ever actually read Part II of the TP. From his statement above, he appears to have read only pages 30 and 34 of the TP in chapter III, which is the standard approach taken by philosophers, historians, psychologists and economists.

Some years ago, I was contacted by some students at Cambridge University, England. They informed me that students seeking or expressing an interest in working on Keynes were required to read the 1973 CWJMK version of the TP. Of course, reading Braithwaite's completely erroneous assessment would seriously damage and undermine the message that Keynes was trying to convey about the importance of imprecise probability in decision making, in general, and decision making, in particular, in economics and finance, since it is impossible to deal with the uncertainty problem except by using either interval valued probability or decision weights. Braithwaite's intellectual misrepresentation of Keynes's work, engaged in in order to advance F. Ramsey's competing approach ,explains the total confusion emanating from the publications of Cambridge University trained and associated economists and philosophers over the last 45 years starting with E R Weintraub's confused and confusing assessment linking Keynes and Shackle, when ,in fact, the decision making systems they present are diametrically opposed to each other as Shackle's system rejects all probability concepts and uses possibility instead.

Unfortunately, there are much more additional misrepresentations made by Braithwaite.

Braithwaite claims the following:

"A more serious criticism of Keynes's theory is that it supposes that the logical interpretation of 'probability' applies to every field in which the term is used…But to show that a frequency theory will not explain the sense of 'probability ' used in the context of rational belief does not show that it cannot adequately explain the probabilities which occur within scientific statements, e.g. That the probability of a radium atom disintegrating within 1622 years is ½ (which is what physicists mean by saying that 1622 years is the 'half-life' of a radium atom). These propositions are undoubtedly empirical and so present insuperable obstacles to being incorporated into a logical theory of probability. Keynes never explicitly discusses such probabilities…." (Braithwaite, 1973, pp.xvii-x).

Of course, Keynes did discuss how such probabilities, for example in chapters 8,16 (pp.172-174 dealing with Faraday and Maxwell) and 32, which can easily be integrated into a logical theory of probability.

(The reader should note that Keynes is not dealing with this problem in chapter 24 of the TP, as falsely claimed by Braithwaite (Braithwaite,1973, p.xviii).Chapter 24 is concerned with epistemological versus ontological uncertainty, where Keynes makes it clear that his concept of logical probability emphasizes epistemological uncertainty and not ontological uncertainty):

"Whilst no general criterion of choice seems to exist, where of two alternative classes neither includes the other, it might be thought that where one does include the other, the obvious course would be to take the narrowest and most specialized class. This procedure was examined and rejected by Venn; though the objection to it is due, not, as he supposed, to the lack of sufficient statistics in such cases upon which to found a generalization, but to the inclusion in the class- concept of marks characteristic of the proposition in question, but nevertheless not relevant to the matter in hand. If the process of narrowing the class were to be carried to its furthest point, we should generally be left with a class whose only member is the proposition in question, for we generally know something about it which is true of no other proposition. We cannot, therefore, define the class of reference as being the class of propositions of which everything is true which is known to be true of the proposition whose probability we seek to determine.

And, indeed, in those examples for which the frequency theory possesses the greatest prima facie plausibility, the class of reference is selected by taking account of some only of the known characteristics of the quaesitum, those characteristics, namely, which are relevant in the circumstances. In those cases in which one can admit that the probability can be measured by reference to a known truth-frequency, the class of reference is formed of propositions about which our

relevant knowledge is the same as about the proposition under consideration. In these special cases we get the same result from the frequency theory as from the Principle of Indifference. But this does not serve to rehabilitate the frequency theory as a general explanation of probability and goes rather to show that the theory of this Treatise is the generalized theory, comprehending within it such applications of the idea of statistical truth-frequency as have validity.

'Relevance' is an important term in probability, of which the meaning is readily intelligible. I have given my own definition of it already. But I do not know how it is to be explained in terms of the frequency theory. Whether supporters of this theory have fully appreciated the difficulty I much doubt. It is a fundamental issue" (Keynes,1921, p.104).

In chapter 32, in sections 5 and 6 of the TP, pp.396-400, Keynes makes it clear that he will accept any statistical frequency statement as evidence if it passes the Lexis-Q test for stability (constancy of the answer as in the radium example used by Braithwaite above).

Let us actually make use of Keynes's logical, objective relation between conclusions and relevant evidence P, where a is the conclusion and h represent the different evidence statements in the form of propositions.

Let h_1 represent a proposition concerning frequent events; let h_2 represent a proposition concerning infrequent events; let h_3 represent a proposition about non frequent events. Then P (a/h_1, h_2,h_3), where there is no relevant h_2 and h_3 evidence ,reduces to P(a/h_1)= ½ for the probability of the decay of a radium atom. Furthermore, V(a/h_1) =1, given that all the available evidence establishes this probability to be a constant (stable value) satisfying the Lexis-Q test for stability.

Braithwaite, who, like Ramsey, had no idea that Keynes's objective, logical probability relations are by analogy extremely close to similarity relations between a and the h_i,(i=1,….,n,n+1….),based on analogy, pattern recognition, and resemblance, simply does not understand what he is talking about when he claims the following:

"….and in a paragraph in which he speaks of probability being 'relative' in a sense to the principles of human reason …he throws over entirely his doctrine of specific objective probability -relations" (Braithwaite, 1973,p.xxi)

Of course, Keynes's comment on p.32 (p.35 of the 1973 edition) needs to be read in connection with his assessment of the extremely close connection between his relational, propositional logical conception of probability and similarity made on in chapters I,II,III and XII of the TP, which Keynes developed in Part III of the TP .However, neither Ramsey not Braithwaite, Ramsey's disciple ,ever read Part III.

The only conclusion possible from the examination of Braithwaite's editorial Foreword introducing the 1973 CWJMK's A Treatise on Probability, Volume 8, to a reader is to undermine any serious consideration of the soundness of Keynes's imprecise and non-additive approach to probability on the part of the reader. Braithwaite was an advocate of Ramsey's competing approach to probability, which must be precise and additive.

It is clear that, for example, Skidelsky, Moggridge, Carabelli, and O'Donnell read this version of the TP from their citations to it .The result was that they all incorporated the serious and severe error of relying on Ramsey's evaluation and assessment of the TP in 1922 and 1926 into their work on Keynes. This would explain Skidelsky's rejection (1992, pp.60-100) of Keynes's logical theory of probability and his rejection of Keynes's definition of uncertainty on p.148 of chapter 12 in footnote 1 of the GT, that uncertainty is a function of the Evidential Weight of the Argument, V.

SECTION 4. CONCLUSIONS

Why would the Economic Journal in 1957 publish a paper,"Harrod's " Clive Bell on Keynes" (December, 1957), is beyond my comprehension and understanding. What possible knowledge and expertise could Bell have possibly had regarding the GT, TP, and the two volumes of the October, 1930 A Treatise on Money, that would have been of interest to a practicing economist? The apparent answer, that Bell lived and talked with Keynes in a house they lived in, would logically require Bell to have also published articles in the *Economic Journal* on Keynes's TP, TM and GT. Why would alleged scholars base their arguments on claims that are dubious, at best.? What possible evidential support can be assessed about Keynes's technical works, based on the fact that Bell lived in a house with Keynes? I don't have any answer. However, all such arguments contain very serious flaws and should never have been able to have made it through the refereeing process at the Economic Journal.

What has been going on now for about 100 years, in all areas of academia involved in a study of Keynes's intellectual work, is ignorant commentary, especially on Keynes's TP, that is all based on one source only, F P Ramsey. The reason for such commentary is that current and past economists and philosophers are simply not equipped intellectually and mathematically to be able to follow Keynes's analysis. So they decided to follow the assessments of Ramsey. This is abundantly clear in S. Bradley piece in the 2019 Stanford Encyclopedia of Philosophy, where he claims that the only diagram in the TP, contained in chapter III on page39 (page 42 of the CWJMK 1973 version), represents Keynes's theory of probability.

Academics can't grasp the proper role played by intuition and perception in Keynes's approach to decision making under time constraint because they have no idea about the art/science /sport of tournament chess or why the academic study of master level chess games by cognitive scientists and psychologists results in a nearly complete rejection of Ramsey's belief in a decision makers power to make exact, definite, precise calculations of probabilities in decision making, a belief which is rejected by practically all tournament chess players.

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