

J M Keynes ,like Benoit Mandelbrot, was right .They (Econometricians, Statisticians,) do not Know What They are Doing

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Abstract-

Section 1.) Introduction

Stephen Stigler (SS) makes a number of claims about the *Treatise on Probability* (TP, 1921) using a 1923 review of the TP by R. Fisher as the source for his assessment:

“Keynes was one of the odd couple of twentieth-century economics, being for about fifteen years the co-editor of the *Economic Journal* with Francis Edgeworth. While they are both on my list as statisticians, they were polar in more than simply their personal characteristics. I included Keynes in the phylum “Unbalanced” simply because his immense reputation in

economics was so far out of kilter with his reputation in statistics. He is on my list as a statistician solely because of his 1921 *Treatise on Probability*, a work that had and continues to enjoy a reputation principally among philosophers, a reputation that seems to me to far outstrip its influence or merits. I would view this as an instance of what Robert K. Merton has called the Matthew Effect in Science: ``To him that hath shall be given;’ ’ that is, the *Treatise on Probability* is a work whose glory is reflected from Keynes’s undoubted brilliance and his great influence in economics.

The book is based upon a wonderfully wide reading in the early history of probability, and as a written work it sings with Keynes’s magnificent prose style.

But while the book exhibits a modicum of philosophical and logical care, it also shows a lack of mathematical attention to detail and no sensitivity at all to the striking statistical developments of the period 1880 to 1920.

Two of the more statistically informed readers among his contemporaries either damned it faintly or denounced it in ringing terms. The philosopher of science and geophysicist Harold Jeffreys was polite to Keynes in an initial review, but after Keynes’s death, Jeffreys inserted a pointed jab in the preface in 1947 to a new edition of his own *Theory of Probability*. There, Jeffreys explicitly attributed any similarity between their works to a common influence by W. E. Johnson,

noted that the philosophical approach adopted by Keynes had been published earlier (in 1919) by Wrinch and Jeffreys, and stated that Keynes's distinctive contribution in the book was in fact an error, an error that Keynes had later retracted after it was pointed out by Jeffreys and Ramsey. Ronald A. Fisher, the twentieth century's greatest statistician, was even more critical in a 1922 (sic) book review. Fisher attacked the book from nearly every angle: for faults in logical definition, for its sneering criticism of nearly every writer mentioned, for a large number of mathematical errors, for vagueness of definition, for misunderstanding basic Bayesian inference, for obscurity in exposition, and particularly for Keynes's 'apparent lack of acquaintance with modern developments of Statistical Science.'

To a degree Fisher's criticism could be judged unnecessarily harsh, the case of a young (thirty-two-year-old) brilliant Cambridge graduate, Fisher, pouncing upon a slightly older (thirty-nine-year-old) brilliant Cambridge graduate, Keynes, who had trod perilously close to the boundaries of his mathematical competence (and over those boundaries in some cases)."(Stigler,2002,p.161).

We also have an additional claim made by SS that severely misrepresents Edgeworth's assessment of the TP.I deal with Jeffreys in a separate paper:

"Edgeworth himself reviewed Keynes's book, and his review was essentially as scathing as Fisher's, and on much the same

points. But Edgeworth was so polite in his expression that a reader could come away with a quite contrary impression.

Keynes would have understood, but the soft words required no response. Keynes did not see the need to reply. E. B. Wilson also reviewed the book and also politely judged it wanting (Mirowski (1994), reprints correspondence on this between Wilson and Edgeworth). Neither Keynes nor Edgeworth did extensive empirical work, a trait shared by about half my list of 39. Keynes removed himself from probability after the book was published, a wise investment in human capital, in my view.”(Stigler,2002,p.163)

SS has overlooked the fact that Edgeworth reviewed the TP twice; the other review was for MInd. Contrary to SS, neither review is scathing, although Edgeworth carefully uses a set of “if-then “ statements to question the generality of some of Keynes’s criticisms .In fact, Edgeworth asked for help in his Mind article concerning the evaluation of the nature and application of Keynes’s coefficient of weight and risk,c, from chapter 26 of the TP(see Arthmar and Brady,2012).

Fisher’s review for the Eugenics Society appeared in 1923 ,not 1922. The reader is asked to see my papers on R Fisher and Pearl listed in the references for a detailed refutation of practically all of their claims about the TP. Both reviews are truly poor works that should never have been published in their present form .

This paper will be structured in the following fashion. We will examine Stigler's conclusions, which are all based on a series of claims made by R. Fisher. The same type of claims also appear in the review of the TP by Pearl. According to Stigler, "... Fisher attacked the book from nearly every angle: for faults in logical definition, for its sneering criticism of nearly every writer mentioned, for a large number of mathematical errors, for vagueness of definition, for misunderstanding basic Bayesian inference, for obscurity in exposition, and particularly for Keynes's `apparent lack of acquaintance with modern developments of Statistical Science.'" (Stigler, 2002, p.161)

We will take each claim that Stigler claims was derived from Fisher and examine the support Fisher provides for it in his 1923 review in a section of this paper. Section 2 will deal with "...for faults in logical definition." Section 3 will deal with "...for its sneering criticism of nearly every writer mentioned ." Section 4 will deal with "...for a large number of mathematical errors ." Section 5 will deal with "...for vagueness of definition ." Section 6 will deal with "...for misunderstanding basic Bayesian inference." Section 7 will deal with "...for obscurity in exposition." Section 8 will deal with "...for Keynes's `apparent lack of acquaintance with modern developments of Statistical Science.'" Section 9 will deal with SS's severe misconstrual of Edgeworth's review. Section 10 will conclude our paper. That conclusion will be that Fisher had absolutely no idea about what Keynes was doing. Fisher deliberately misquoted ,as well

quoted out of context ,from the TP,in an attempt to misrepresent Keynes's analysis.

The following strange assessment of Keynes's non-additive ,Boolean approach to probability, based on indeterminate probabilities, made by Fisher ,should have provided sufficient evidence that R .Fisher had no idea about what Keynes was talking about or doing in the TP before he wrote his 1923 diatribe:

'It should be remarked that likelihood, as above defined, is not only fundamentally distinct from mathematical probability, but also from the logical " probability " by which Mr. KEYNES ...has recently attempted to develop a method of treatment of uncertain inference, applicable to those cases where we lack the statistical information necessary for the application of mathematical probability. Although, in an important class of cases, the likelihood may be held to measure the degree of our rational belief in a conclusion, in the same sense as Mr. KEYNES' " probability," yet since the latter quantity is constrained, somewhat arbitrarily, to obey the addition theorem of mathematical probability, the likelihood is a quantity which falls definitely outside its scope" (Fisher,1922,p.327)

Given that , in general ,Keynes's probabilities are interval valued probability estimates that are non additive and nonlinear , the claim that "...as Mr. Keynes' " probability," ... is constrained, somewhat arbitrarily, to obey the addition

theorem of mathematical probability...” ,no one should have taken Fisher review seriously. Unfortunately, as is the case with SS, this is not the case .It is not surprising that no economist at SS’s presentation speech raised a single objection to the series of unfounded claims in SS ‘s article. Apparently , the vast majority of practicing economists and econometricians also can’t comprehend what an interval valued probability is. Nor can they fathom what this means for current statistical and econometric practice ,which is founded on the assumptions of additivity and linearity that underlie the Subjective Expected Utility (SEU) approach, as put forth by F. Ramsey, B. De Finetti, L. Savage ,Milton Friedman and James Tobin, not to mention Muth, Sargeant, Lucas, and a host of others.

Section 2.) Fisher’s First Claim according to SS

SS’s first claim is that Fisher’s critique shows that there were “... faults in logical definition.”

What exactly were Keynes’s faults in logical definition according to Fisher?

“Mr. Keynes gives a great deal of space to formal proofs: Part II. of his book (71 pages)(sic) is practically composed of a symbolical logic in which all the laws of probability are duly "proved." Curiously enough, however, no definition of

probability whatever enters into these proofs. Probability is introduced surrepticiously(sic), not in a definition of probability, but in the definitions of addition and multiplication! No proof is given that these definitions are satisfied by the ordinary arithmetical processes, when the probabilities are given numerical values. As a matter of fact, the addition and multiplication theorems of probability, are only known to be true, when probability is used in the ordinary statistical sense. This whole elaborate structure of symbolical logic thus proves nothing whatever about Mr. Keynes' "probability." Such a logical lacuna would be of little importance.....”(Fisher,1923,pp.46-47).

This massive error on Fisher’s part follows directly from the incorrect assessment made in his 1922 article about Keynes’ logical probability approach requiring the addition axiom. Keynes ’s exposition is needed since he is going to include the case of non additivity in his analysis. Keynes had provided a symbolic logic that includes explicitly the case of non additivity. The logical lacuna is Fisher’s , not Keynes’s. Apparently, Fisher believed that he had found logical flaws in the TP that mathematical logicians with many times his very limited logical skills, such as J Nevile Keynes, Bertrand Russell, Alfred North Whitehead, William Ernest Johnson, and CD Broad , had not found. Thus ,if the assessment of Keynes’s symbolic logic in the TP comes down to a dispute between Bertrand Russell and R

Fisher, it is obvious that Fisher's position is erroneous. Fisher needed to have stayed in his fishbowl of biology, molecular biology, and genetics, where the limiting frequency interpretation of probability has application

Section 3.) Fisher's Second claim according to SS

Fisher's second claim was that Keynes had engaged in "... sneering criticism of nearly every writer mentioned ."

Direct quotes from chapter 26 of the TP will suffice to dispose of this claim :

"A more correct doctrine was brought to light by the efforts of the philosophers of the Port Royal to expose the fallacies of probabilism. "In order to judge," they say, "of what we ought to do in order to obtain a good and to avoid an evil, it is necessary to consider not only the good and evil in themselves, but also the probability of their happening and not happening, and to regard geometrically the proportion which all these things have, taken together." Locke perceived the same point, although not so clearly. By Leibniz this theory is advanced more explicitly; in such judgments, he says, "as in other estimates disparate and heterogeneous and, so to speak, of more than one dimension, the greatness of that which is discussed is in reason composed of both estimates (i.e. of goodness and of probability), and is like a rectangle, in which there are two

considerations, viz. that of length and that of breadth. . . . Thus we should still need the art of thinking and that of estimating probabilities, besides the knowledge of the value of goods and evils, in order properly to employ the art of consequences.”(Keynes,1921,pp. 308-309)

Continuing,

“Bernoulli’s maxim, that in reckoning a probability we must take into account all the information which we have, even when reinforced by Locke’s maxim that we must get all the information we can², does not seem completely to meet the case. If, for one alternative, the available information is necessarily small, that does not seem to be a consideration which ought to be left out of account altogether. 8. The last difficulty concerns the question whether, the former difficulties being waived, the ‘mathematical expectation’ of different courses of action accurately measures what our preferences ought to be—whether, that is to say, the undesirability of a given course of action increases in direct proportion to any increase in the uncertainty of its attaining its object, or whether some allowance ought to be made for ‘risk,’ its undesirability increasing more than in proportion to its uncertainty.

In fact the meaning of the judgment, that we ought to act in such a way as to produce most probably the greatest sum of goodness, is not perfectly plain. Does this mean that we ought so to act as to make the sum of the goodnesses of each of the

possible consequences of our action multiplied by its probability a maximum? Those who rely on the conception of ‘mathematical expectation’ must hold that this is an indisputable proposition. The justifications for this view most commonly advanced resemble that given by Condorcet in his “Réflexions sur la règle générale, qui prescrit de prendre pour valeur d’un événement incertain, la probabilité de cet événement multipliée par la valeur de l’événement en lui-même,” where he argues from Bernoulli’s theorem that such a rule will lead to satisfactory results if a very large number of trials be made. As, however, it will be shown in Chapter XXIX. of Part V. that Bernoulli’s theorem is not applicable in by any means every case, this argument is inadequate as a general justification.” (Keynes, 1921, pp.313-314)

“In the history of the subject, nevertheless, the theory of ‘mathematical expectation’ has been very seldom disputed. As D’Alembert has been almost alone in casting serious doubts upon it (though he only brought himself into disrepute by doing so), it will be worth while to quote the main passage in which he declares his scepticism: “Il me sembloit” (in reading Bernoulli’s *Ars Conjectandi*) “que cette matière avoit besoin d’être traitée d’une manière plus claire; je voyois bien que l’espérance étoit plus grande, 1^o que la somme espérée étoit plus grande, 2^o que la probabilité de gagner l’étoit aussi. Mais je

ne voyois pas avec la même évidence, et je ne le vois pas encore, 1^o que la probabilité soit estimée exactement par les méthodes usitées; 2^o que quand elle le seroit, l'espérance doive être proportionnelle à cette probabilité simple, plutôt qu'à une puissance ou même à une fonction de cette probabilité; 3^o que quand il y a plusieurs combinaisons qui donnent différens avantages ou différens risques (qu'on regarde comme des avantages négatifs) il faille se contenter d'ajouter simplement ensemble toutes les espérances pour avoir l'espérance totale.”²

In extreme cases it seems difficult to deny some force to D'Alembert's objection; and it was with reference to extreme cases that he himself raised it. Is it certain that a larger good, which is extremely improbable, is precisely equivalent ethically to a smaller good which is proportionately more probable? We may doubt whether the moral value of speculative and cautious action respectively can be weighed against one another in a simple arithmetical way, just as we have already doubted whether a good whose probability can only be determined on a slight basis of evidence can be compared by means merely of the magnitude of this probability with another good whose likelihood is based on completer knowledge. There seems, at any rate, a good deal to be said for the conclusion that, other things being equal, that course of action is preferable which involves least risk, and about the results of which we have the most complete knowledge. In marginal cases, therefore, the coefficients of weight and risk as well as that of probability are relevant to our conclusion. It seems natural to suppose that

they should exert some influence in other cases also, the only difficulty in this being the lack of any principle for the calculation of the degree of their influence. A high weight and the absence of risk increase pro tanto the desirability of the action to which they refer, but we cannot measure the amount of the increase ." (Keynes,1921 ,314-315)

and

"Bernoulli's second axiom, that in reckoning a probability we must take everything into account, is easily forgotten in these cases of statistical probabilities. The statistical result is so attractive in its definiteness that it leads us to forget the more vague though more important considerations which may be, in a given particular case, within our knowledge. To a stranger the probability that I shall send a letter to the post unstamped may be derived from the statistics of the Post Office; for me those figures would have but the slightest bearing upon the question." (Keynes,1921,p. 322)

I have shown that Keynes cited Locke, D' Alembert, Bernoulli, Condorcet, and Leibniz in chapter 26 with no "sneering criticism". The real problem here is that Fisher is an intellectual totalitarian who will accept no criticism of his limiting

frequency interpretation of probability. Keynes's criticisms are , for the most part, directed against advocates of the limiting frequency interpretation of probability who attempted to extend the reach of the limiting frequency interpretation into areas where it can't be applied reliably.

Section 4.) Fisher's Third Claim according to SS

Fisher's third claim was that there were " a large number of mathematical errors" in the TP. In fact ,there are **very few** mathematical errors in the TP(these few, minor errors are in chapter 17) , although there were (and still are many typographical errors in the CWJMK ,Volume 8-See Brady 1996 and 1997) a great number of typographical errors that any diligent reader with a BA in mathematics would be able to spot. However, once these typographical errors are corrected, it is obvious that there were no significant or major mathematical errors that would have overturned a single result presented by Keynes in the TP. This is what one would expect , given the fact that William E. Johnson ,an acknowledged applied mathematics expert, constantly went over the TP with Keynes .The reader is asked to read my paper on Fisher(and Pearl) at SSRN for the details.

Section 5.) Fisher's Fourth Claim according to SS

Fisher's Fourth claim is that there was "... vagueness of definition" in the TP. This is basically a repeat of Claim 1:

" Curiously enough, however, no definition of probability whatever enters into these proofs. Probability is introduced surrepticiously(sic), not in a definition of probability, but in the definitions of addition and multiplication! No proof is given that these definitions are satisfied by the ordinary arithmetical processes, when the probabilities are given numerical values. As a matter of fact, the addition and multiplication theorems of probability, are only known to be true, when probability is used in the ordinary statistical sense. This whole elaborate structure of symbolical logic thus proves nothing whatever about Mr. Keynes' "probability." Such a logical lacuna would be of little importance....."(Fisher,1923,pp.46-47).

For Keynes, following George Boole, probabilities are either determinate, in which case you can use one numerical to represent the probability, or they are indeterminate, in which case you have to use two numerals to represent the probability. Fisher, like Pearl and Richard von Mises, is completely ignorant of Boole's breakthrough logic of partial entailment that requires the use of interval valued probability if the weight of the evidence does not equal, approximate, or approach 1.

Keynes makes the first attempt in history to deal axiomatically with interval valued probability .Of course, Fisher is now outside of his fish bowl. He doesn't know or understand what Keynes is attempting to do . Fisher's comments are stupid and foolish.

Section 6.) Fisher's Fifth claim according to SS

Fisher's fifth claim was that Keynes analysis in the TP showed a "misunderstanding (of) basic Bayesian inference". We will have to deal with two basically different points here. First, all of Keynes's probabilities of the form $p(h, e)$ are conditional probabilities so that the conditional probability is $p(h/e)$, where h stands for hypothesis and e stands for evidence. There are no marginal probabilities. All probabilities must be relative to evidence. Thus ,Keynes argues that a change in evidence results in a new probability, and not a revised, updated probability. Thus ,Keynes substitutes the concept of Keynesian Support ,based on the weight of the evidence, w , for Bayesian conditionalization.

Second, Keynes's principle of indifference requires positive symmetrical evidence .It is impossible for a probability to exist if there is no evidence. A major criticism of Laplace's Principle of Non –Sufficient Reason was that ,in the absence of any evidence, it could be asserted that equal probabilities could be

assigned. This is the basis for Richard Von Mises gross misinterpretation of Keynes's Principle of Indifference that "Keynes makes every effort to avoid this dangerous consequence of the subjective theory, but with little success.... It does not occur to him to draw the simple conclusion that if we know nothing about a thing, we cannot say anything about its probability". The curious mistake of the 'subjectivists' may, I think, be explained by the following example. If we know nothing about the stature of six men, we may presume that they are all of equal height. This application of the Principle of Indifference is also legitimate from the point of view of Keynes's rule" (R. Mises, 1957, pp. 75-76), and that "The peculiar approach of the subjectivists lies in the fact that they consider 'I presume that these cases are equally probable' to be equivalent to 'These cases are equally probable,' since, for them, probability is only a subjective notion". (R. Mises, 1957, p. 76). Richard von Mises' claims regarding Keynes are as "off the wall" as those of Fisher. All of Keynes's probabilities are conditional probabilities. There must be relevant evidence or there is no probability. Von Mises claim, that the inspection of a die can yield no relevant evidence (R. Mises, p. 76), is so nonsensical that this chapter of the book should never have been published. Again, we have someone outside his fish bowl. Therefore, Keynes correctly assigns equal prior probabilities (a uniform or rectangular distribution) to propositions only if there is symmetrical evidence which is complete. The weight of the evidence, w , must equal 1. Thus, since a die's 6 sides are

symmetrical to each other and this symmetry is directly observable before the die is tossed, the probability of each of the six sides is $1/6$, which means that no tosses of the die need to be observed, empirically, before the prior probability is assigned. Of course, these are conditional probabilities and not marginal probabilities.

Keynes, in chapter 30, is dealing with the problem of choosing between two urns, one urn with a known number of white and black balls and the other urn with an unknown number of white and black balls. This problem is one of two of Ellsberg's famous "paradox" problems. Keynes discussed this problem in depth in chapters 4 and 6. It is clear that Fisher would have no idea about what Keynes was doing, because he had no idea about what an "ambiguous" probability was. Keynes demonstrates that while the probability of both urns must be $1/2$, the weight of the evidence favors the urn with the known number of white and black balls.

Fisher also fails to recognize that Keynes deals with the Bernoulli (binomial) distribution by using the Normal approximation to the binomial and not the binomial distribution. Keynes correctly recognizes what the problem of inverse probability entails. Fisher's assertion that Keynes does not correctly understand this question is simply an empty claim supported by deliberately misquoting and/or partially quoting Keynes out of context.

Section 7.) Fisher's Seventh Claim according to SS

Fisher's seventh claim was that Keynes's analysis in the TP showed an " ... obscurity in exposition."

The only thing that is obscure is how Fisher could have written his review in 1923 and believe, as he did in 1922 (see above) that Keynes's "logical " probabilities obeyed the addition axiom as a general case. In fact, Keynes's logical probabilities , which are interval valued probabilities, in general, obey the addition axiom only if the decision maker has a complete information – evidence-knowledge base, so that the weight of the evidence, w , equals ,approximates or approaches 1 so that the intervals become smaller and smaller as w approaches 1 and his preferences are linear .The confusion of Fisher is practically identical to the confusions of Frank P. Ramsey on this point,with his talk about "Mr. Keynes's mysterious degrees of belief and non numerical probabilities. Neither Fisher or Ramsey ever had any inkling about what an interval valued probability entailed.

One can say that ,for both of them, ignorance was bliss.

Section 8.) Fisher's Eighth Claim according to SS

Fisher's eighth claim according to SS was that Keynes's analysis in the TP showed an "... apparent lack of acquaintance with modern developments of Statistical Science." from the late 1870's through the early 1920's.

Actually, Keynes's analysis and critique of the use of the Normal Distribution in chapter 17 of the TP is that

"The popularity of the normal law, with the arithmetic mean and the method of least squares as its corollaries, has been very largely due to its overwhelming advantages, in comparison with all other laws of error, for the purposes of mathematical development and manipulation. And in addition to these technical advantages, it is probably applicable as a first approximation to a larger and more manageable group of phenomena than any other single law. So powerful a hold indeed did the normal law obtain on the minds of statisticians, that until quite recent times only a few pioneers have seriously considered the possibility of preferring in certain circumstances other means to the arithmetic and other laws of error to the normal. Laplace's earlier memoir fell, therefore, out of remembrance. But it remains interesting, if only for the fact that a law of error there makes its appearance for the first time." (Keynes,1921,p.234)

A careful reading of chapter 17 of the TP demonstrates that Keynes was well aware of “...modern developments of Statistical Science.”

Basically , the “modern developments “ are all related to an analysis running from the Limiting Frequency interpretation of Probability to the Strong Law of Large Numbers to the Central Limit Theorem to the Normal probability distribution. This approach works in areas where the main ,or only, relevant evidence is relative frequencies. Thus, neutrinos, bosons ,quarks, electrons ,protons, gas particles, molecules ,cells, germs ,chromosomes, genes, coins, dice, urn balls, mass assembly line production (quality control),weight, height, heart beat ,pulse, blood pressure, etc., are the type of phenomenon that the “modern “ statistician can apply the normal distribution to.

However, this approach breaks down quickly in most areas of social science, liberal arts, and especially in economics, business, and finance, outside of studies of the Keynesian consumption function and business inventories. It would especially breakdown in studies of investment expectations, business cycles and macroeconomic forecasting, where decision makers are NOT using the additivity ,complementarity, and linearity assumptions of the purely mathematical laws of the probability calculus ,upon which to base their expectations of the future, that are assumed by econometricians. Keynes states:

“In particular if any such law of sensation, as that enunciated by Fechner, is true (i.e. that sensation varies as the logarithm of the stimulus), the arithmetic mean must break down as a practical rule in all cases where human sensation is part of the instrument by means of which the observations are recorded.*(Keynes,1921,p.241: the footnote notes that “This was noticed by Galton”).

Keynes’s conclusion is clear. You can’t use the normal distribution in most areas of social science and liberal arts, especially in economics ,business, and finance.

Section 9. Edgeworth’s review of the TP

SS make the following claim:

““Edgeworth himself reviewed Keynes’s book, and his review was essentially as scathing as Fisher’s, and on much the same points. But Edgeworth was so polite in his expression that a reader could come away with a quite contrary impression.”
(Stigler,2002,p. 203)

Anyone who reads Edgeworth’s review will soon realize that SS’s claim doesn’t make any sense at all.Indeed, the opposite is

the case. Edgeworth correctly acknowledges Keynes's contributions.

First, Edgeworth spends considerable space talking about Keynes's chapter 17 results on Inverse probability and the principal averages, as well as Keynes's advocacy of Chebyshev's Inequality in Part V, topics that are not ever mentioned by Fisher in his review.

Edgeworth expresses some agreement with Keynes and some disagreement that are correctly framed as "if-then" statements. A careful analysis shows that Edgeworth would not accept Keynes's criticisms if you are dealing with the entities studied by chemists, biologists and physicists.

Edgeworth first demonstrates his vast superiority over Fisher by providing the **best** one paragraph summary of the TP ever written:

"Suffice it that many probabilities which are incapable of numerical measurement can be placed " between two numerical "measures " (p. 32). We thus obtain the idea of a " finite probability " as one which exceeds some numerical probability (p. 257). Such measurements play a leading part in induction. To establish a generalization it is necessary that " with the experience we have " actually had there are finite probabilities, however small, derived "from some other source, in favour of the generalization " (p. 238). Round this nucleus of finite probability, through the operation of repetition and likeness, science grows. " An argument from "induction must

always involve some element of analogy, and, 'on the other hand, few arguments from analogy can afford to "ignore altogether the strengthening influence of pure induction "[repetition] " (p. 255)." (Edgeworth,1922,pp.107-108)

Edgeworth understands that Keynes's logical approach is built on Boole's interval valued probabilities .Fisher never grasped any of the basic ideas expressed in Edgeworth's one paragraph above. Unfortunately, neither do any of the other statisticians or econometricians who have written on Keynes's TP.

Edgeworth then goes on to discuss Keynes's work on the Principle of Indifference:

"He steers a safe course between the incautious expressions of the classical writers and the extreme scepticism professed by Dr. Venn. He seems to admit that we have direct knowledge of certain equi-probabilities, prior to statistical observation."
(Edgeworth,1922,p.108)

Edgeworth deals appropriately with Keynes's support for using Chebyshev's Inequality:

"He has also improved the superstructure, too, at several points, though not, we think, at all the points which he has retouched. We do not share his predilection for Tchebycheff and the Russian school. Tchebycheff's principal contribution to our subject is a simple formula for approximating, or rather

finding an inferior limit, to a certain probability which is often required by the mathematical statistician. The required fraction corresponds to the area intercepted between an assigned tract of the abscissa and the normal error-curve, or more generally the locus which represents the frequency-distribution of an aggregate formed by the addition of several constituents. The formula has, besides its simplicity, two advantages over the classical theory. It holds good when the number of constituents is small; and it is applicable over a larger range, to a greater distance from the centre of gravity, than the classical formula. Cases may occur in which these advantages are decisive; and the statistician who requires a safe limit to the possible deviations of some aggregate may have to resort to the formula of Tchebycheff, or to find some other inequation on the lines of the Russian school. But in the case of the Binomial considered by Mr. Keynes (p. 356), the advantages of the proposed formula are less conspicuous. If Bernoullian conditions are fulfilled, when the number of observations is tolerably large, say above 25, the classical formulae give values for short ranges much more accurate than the inferior limits which Mr. Keynes has deduced from the Tchebycheff formula (loc. cit.). For long ranges there is available a geometrical progression which furnishes a superior limit for the tails of the Binomial beyond any assigned point, and so a superior limit for the central part of the area. If the number of observations is small, tables of factorials are available for the calculation. But, as Mr. Keynes reminds us, Bernoullian conditions may not hold good: the

statistics, for instance, might be characterised by instability in Lexis' sense of the term, and then the classical formulae would have to be applied with caution. Similar remarks apply to the asymmetrical Binomial instanced by Mr. Keynes (p. 359). If the number of observations is fairly large, and the sortition is of the pure Bernoullian type, the more important of the results laboured by Mr. Keynes (pp. 358-361) are obtainable directly from the theory of Laplace as extended by Poisson. For instance, the formula cited by Mr. Keynes for the difference between the area above and that below the centre of gravity (p. 359) follows at once from the " Second approximation to "the generalised curve of error " treated by Professor Bowley in his Elements of Statistics (Ed. 4, p. 442). We concede something to Mr. Keynes' contentions on behalf of the Russian school. But we cannot subscribe to his daring dictum: " The Laplacian mathematics, although it still holds the field in most text-books, is really obsolete, and ought to be replaced by the very beautiful work which we owe to these three Russians." (Tchebycheff, Markoff and Tchuprow) (p. 358)."

(Edgeworth, 1922 ,pp. 110-111).

We now come to Edgeworth's assessment of Keynes's assessment of Laplace :

" In judging of Laplace, Mr. Keynes is at a disadvantage due to a cause which is creditable to him-his own originality. He has

made a new contribution to an old problem. The problem arises in connexion with errors of observation. Suppose the law of frequency, according to which the errors are dispersed about the true value of the object under measurement, to be known. Then by inverse probability we may infer from a given set of observations what is the magnitude to which the observations relate (p. 194 et seq.). For instance, if the distribution of errors is known to be normal, it may be inferred that the arithmetic mean will give the most probable value of the object measured. There is thus suggested the problem: what is that law of frequency which corresponds to the use of any particular mean of the observations--the Arithmetic, Harmonic, or other Mean? What is that law from which it would be deducible that the said mean afforded the most probable value of the qucesitum ? It has been usual to conclude that the normal law of frequency corresponds in the sense indicated to the Arithmetic Mean. But Mr. Keynes has found a more general solution (first published in this Journal, Feb., 1911). Whereas hitherto there has been found for the logarithm of the required function a constant minus $K(x-x_r)^2$, where K is a constant, x is the sought true point, x_r is one of the observations (supposed all equally good). Mr. Keynes finds the more general expression $\phi'(x)(x-x_r)^2 - \phi(x)$, where ϕ is an arbitrary function (p. 197). We omit an additional term, $\psi(x_r)$, as ruled out by the assumption entertained by those who set the problem that the error of an observation is a function only of its distance from the true point. This is

certainly a remarkable generalization of the received theorem.
“(Edgeworth,1922,p.111)

Now,however,

“Having found that (in the case of symmetry) the use of the Arithmetic Mean corresponds to the assumption that the observations are distributed according to the normal error-function, our author might naturally argue that, as Laplace prescribes the use of the Arithmetic Mean, therefore he assumes distribution according to the normal error-function. But this argument requires an additional premiss: namely, that Laplace, in combining given observations, was aiming at the same quaesitum as that above described, namely, the most probable value of the true object, that value from which the given observations would most probably have resulted. But this premiss is very far from being true. Laplace recommends as the proper combination of the given observations, not that function thereof which is most frequently right, but that which, account being had of the extent and frequency of the errors incurred in the long run, shall by its use minimize the detriment incident to error...”(Edgeworth,1922,p.112)

Therefore,

“We therefore strongly dissent from all that Mr. Keynes has written implying that Laplace supposed the observations to be distributed according to the normal law of error. The method of

least squares was not a " corollary " of the normal law (p. 202). It is not, as we interpret Laplace, true that " the Theory of Least Squares simply " develops the mathematical results of applying to equations of " observation which involve more than one unknown that law of " error which leads to the Arithmetic Mean in the case of a single " unknown " (p. 210). It is *not* " the application of the same considerations as those which we have just been considering " (p. 309) -namely the inverse method well employed by Mr. Keynes, as above mentioned." (Edgeworth1922,p.112).

Edgeworth 's review is a balanced one,especially if we also consider his review of the TP for Mind. SS's bizarre claim that Edgeworth's assessment of the TP is similar to Fisher's assessment has no support.

Section 10.) Conclusions

SS made a number of claims based on the work of Fisher concerning J M Keynes's mathematical skills.SS fails to support even one of his claims. None of these claims has any support. This is because SS never read the TP in his life.He has read the book reviews about the TP .Fisher 's "review" is ,in fact, a worthless diatribe . There is not a single point argued by Fisher against Keynes that is sound in Fisher's entire review.

Edgeworth, on the other hand, makes an assessment that is quite balanced .

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