"A terrible piece of bad metaphysics"? Towards a history of abstraction in nineteenth- and early twentieth-century probability theory, mathematics and logic

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Citation for published version (APA):

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Remarks on the idealist and empiricist interpretation of frequentism: Robert Leslie Ellis versus John Venn

0. Introduction

In many accounts of the history and philosophy of probability theory one finds the statement that the frequency interpretation of probability – as developed by Robert Leslie Ellis (1817-1859) and John Venn (1834-1923) – is to be regarded as the British empiricist reaction against the rationalism of the traditional theory.\(^1\) The goal of this paper is to show that this claim is misconceived and to argue, instead, that there was no unified ‘British school’ of frequentism during the nineteenth-century.\(^2\)

I have divided my arguments for this claim into two sections. In section 1, I indicate that neither Ellis nor Venn rejected the traditional theory of probability \textit{in toto}. Where Ellis tried to reconstruct only the metaphysical – rather than mathematical – part of its foundations, Venn merely objected to its ‘inverse’ extrapolation of the calculations of artificial events to natural events. In section 2, I will argue that Ellis and Venn supported two irreconcilable positions vis-à-

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2 For more or less detailed accounts of Venn’s views on probability see Keynes (1921, chapter 3), Kilinc (1999), Verburgt (submitted for review a), Verburgt (submitted for review b), Wall (2005), Wall (2006b). The only comprehensive analyses of Ellis available are Kilinc (2000) and Verburgt (2013).
vis the assumed ‘Bernoullian’ starting point of frequentism; i.e. the definition of probability as the limit of a relative frequency.

1. Traditional probability theory and frequentism

1.1 Robert Leslie Ellis on traditional probability theory

In the opening passage of his essay of 1844 ‘On the foundations of the theory of probabilities’ Robert Leslie Ellis wrote the following:

‘The Theory of Probabilities is at once a metaphysical and mathematical science. The mathematical part of it has been fully developed, while […] its metaphysical tendencies have not received much attention. This is the more remarkable, as they are in direct opposition to the views of the nature of knowledge, generally adopted at present’ (Ellis 1844, 1).

As may be clear from this passage, Ellis agreed with the calculations of traditional probability theory and objected, merely, to the metaphysical views on which they are based. According to Ellis, there ‘has not yet been philosophy enough’ (Ellis quoted in Shairp & Tait 1873, 481) to expel the influence of the ‘school of [Étienne Bonnot de] Condillac and the sensationalists [which] were in the ascendant when the theory of probabilities received its present form’ (Ellis 1844, 1). The influence of this ultra-empiricist school essentially includes the rejection of all reference to a priori truths and the, subsequent, attempt to establish these truths ‘as mathematical deductions from the simple notion of probability’ (ibid.). Now, Ellis wants to replace these ‘sensationalist’ metaphysical foundations of the traditional theory by foundations that are based on ideal elements of knowledge so as to show that the mathematical calculations of

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3 This essay was read to the Cambridge Philosophical Society on February 14, 1842 and published in its Transactions in 1844.

4 For an account of the philosophical roots of Ellis’s account of probability theory see, for instance, Kilinc (2000) and Verburgt (2013).
probability are, in fact, premised on certain axioms supplied by the mind.\textsuperscript{5} That is to say that for Ellis, the truth of the ‘first principles’ of the theory – that the ‘probability of an event is the number of equally possible ways in which it may take place, divided by the total number of ways which may occur on the given trial’ and that ‘if the probability of a given event [is] correctly determined, the event will, on a long run of trials, tend to recur with frequency proportional to this probability’ (ibid., 2) – is secured ‘a priori’, rather than by means of mathematics or with reference to external phenomena. Although both these principles are ‘generally proved mathematically’, it seems to Ellis that they are ‘ultimate facts’ – ‘the evidence of which must rest on an appeal to consciousness’ (ibid., 2). This statement is argued for by pointing to the impossibility of inventing a case in which the ‘judgment that one event is more likely to happen than another’ (ibid., 3) can be combined with the belief that on the long run it will not occur more frequently. In effect, Ellis regards as futile the mathematical demonstration of such propositions. The passage in which he justifies this claim by means of an example is worth quoting in full:

‘A coin is to be thrown 100 times; there are 2\textsuperscript{100} definite sequences of heads and reversed, all equally possible if the coin is fair. One only of these gives an unbroken series of 100 heads. A very large number give 50 heads and 50 reverses; and Bernoulli’s theorem shows that an absolute majority of the 2\textsuperscript{100} possible sequences give the difference between the number of heads and reverses less than 5. If we took 1000 throws, the absolute majority of the 2\textsuperscript{1000} possible sequences give the difference less than 7 [...] and so on. [...] But this is not what we want. We want a reason for believing that on a series of trials, an event tends to occur with frequency proportion to its probability [...] But, although a series of 100 heads can occur in one way only, and one of 50 heads and 50 reversed in a great many, there is not the shadow of a reason for saying that therefore the former series is a rare [...] event, and the latter [...] an ordinary one. [...] In Bernoulli’s theorem, it is merely proved that one event is more

\textsuperscript{5} For instance Porter, in his The Rise of Statistical Thinking, mistakenly remarks that Ellis actually entertained a ‘sensational philosophy’. This may have to do with the fact that Ellis’s writings are, at some points, somewhat opaque – consider, in this context, his statement that ‘I [Ellis] shall be satisfied if the present essay does no more than call attention to the inconsistency of the theory of probabilities with any other than a sensational philosophy’ (Ellis 1844, 2).
probable than another, i.e. by the definition can occur in more equally possible ways, and that there is no ground whatever for saying [...] that it is a more natural occurrence’ (ibid., 4, my emphasis).

Although what is here referred to as ‘Bernoulli’s theorem’\(^6\) may be ‘true [and] important’ (ibid.), its essential defect lies in its inability to prove the truth of the undeniable proposition that an event, in the long run, occurs proportional to its probability. In other words, the theorem is unnecessary for ‘it leaves the matter just where it was before’ (ibid.). According to Ellis, the reason for this is twofold. On the one hand, no mathematical deduction ‘from premises with do not relate to laws of nature can establish such laws’ (ibid.) since, on the other hand, these are ‘supplied by the mind itself which is ever endeavouring to introduce order and regularity among the objects of its perceptions’ (ibid., 5). His ‘idealist’\(^7\) reconstruction of the traditional foundations of probability, thus, consisted of the claim that, on the one hand, its calculations are grounded on the mental phenomenon of expectation that, on the other, must be assumed in so far as it is premised on the belief in the regularity of nature that, as a ‘general law of nature’, cannot be deduced from calculations. For Ellis, laws of nature or fundamental axioms such as that ‘on the long run, the action of fortuitous causes disappears’ (ibid.) and the ‘idea of an average among discordant results’ (ibid.) cannot but be supplied by the mind. Importantly, when Ellis goes on to deny that probabilities are to be taken as the measure of any mental state, he is not embracing empiricism, but pointing to the centrality of a priori regulative mental principles that guide the practice of mathematical probability.\(^8\)

This is also the case in the context of Ellis’s criticism of ‘a more celebrated application of the [traditional] theory [...] to events whose causes are unknown’ (ibid., 7 & 10) – that is, ‘inverse probability’. He defines this application as the aim to determine the ‘a priori probability’ of the cause of an event from the event itself. Ellis expresses the opinion that ‘in reality, the a priori probability

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6 I will return to Ellis’s views on ‘Bernoulli’s theorem’ in section 2.2.
7 In Verburgt (2013) this characterization of Ellis’s reinterpretation of probability theory is qualified in light of his renovation of Bacon’s empiricist theory of induction – as put forward under the direct influence of William Whewell. In brief, it is claimed here that both Ellis and Whewell used ‘idealist’ views to supplement or perfect, rather than overcome or dismiss, the Baconian method.
8 For this point see, especially Daston (1994), Verburgt (2013).
of a given event has no absolute determinate value independent of the point of view in which it is considered. Every judgment of probability involves an analysis of the event contemplated’ (ibid., 8). This claim is explained by means of the following example:

‘Take the case of a vessel sailing up a river. The vessel has a flag. What was the a priori probability of this? Before any answer can [...] be given [...] we must know (1) what circumstances the person who makes it rejects as irrelevant [...] ; (2) what circumstances constitute in his mind the ‘trial’ [...] (3) What idea he forms to himself of a flag. [...] Unless all such points were clearly understood, the most perfect acquaintance with the nature of the case would not enable us to say what was the a priori probability of the event: for this depends, not only on the event, but also on the mind which contemplates it’ (ibid., 9).

In another example in which not one, but a succession of similar events is taken into account, Ellis remarks that also here the ‘expression applied to determine the probability of a common cause among similar phenomena’ (ibid., 11) rests on a ‘petitio principii’; ‘we assume that all the phenomena are allied; that they are the results of repetitions of the same trial, that they have the same simple probability [etc.]’ (ibid.). As in the case of the calculation of ‘direct’ probabilities, the problem with traditional calculations of ‘inverse’ probabilities is that they are unable to prove these fundamental assumptions. Here it suffices to note that for Ellis the estimates of the ‘inverse’ application – or, as he calls it, ‘the theory a posteriori of [...] inductive results’ (ibid.) – are illusory in so far as they falsely assume that it is possible to mathematically found what is, in fact, a truth of the mind; namely the regularity of nature.

1.2 John Venn on traditional probability theory

In the second chapter of his Logic of Chance of 1866, Venn – reflecting on the classical examples of games of chance – remarks that the calculations of these kind of events have ‘infected the whole science [of probability] with an a priori tendency, which has biassed [sic] the minds of its followers in other applications’ (Venn 1866, 27). This ‘a priori tendency’ is defined by Venn as the idea that the basis of such calculations is formed by the ‘a priori conditions on which the series depends’ (ibid.) – conditions which are satisfied at the moment that
one is able to ascertain that the events of which the series consists are ‘equally likely’. But, Venn asks, ‘What is the meaning of the expression ‘equally likely’? To such a question [...] two forms of reply are possible. The one of these would seek an explanation in the state of mind of the observer, the other would seek it in some characteristic of the things observed’ (ibid., 28). The first reply is dismissed not only for making probability into a branch of psychology, but also because of the impossibility of making sense of the notion ‘equally likely’ a priori. The second reply – which holds that ‘the events [...] would occur with equal frequency in the long run’ (ibid., 29) – is embraced for its definition of ‘equally likely’ as the result of an empirical ‘sequence of events’. Venn, thus, writes: ‘[W]e shall find that [the] tacit restrictions on the a priori plan are really nothing else than a mode of securing an experimental result [or] a compendious way of saying, Let means be taken for obtaining a given result. Since [...] it is upon this result that our inferences ultimately rest, it seems to me simpler [...] to appeal to it [...] as the groundwork of our science’ (ibid., 31).

Yet, it is only after having summarized the advantages of the psychological or ‘a priori’ approach – neatness, accuracy etc. – and having acknowledged that ‘in many cases it would be a real hardship to be debarred from appealing to it’ (ibid., 32) that Venn touches upon his central criticism. This, essentially, concerns the application of this approach to events other than those connected to games of chance – events of ‘the most purely experimental character’ (ibid., 33), namely facts of nature. Referring to a passage from Laplace’s *Essai Philosophique* in which he, after ‘speaking of the irregularity and uncertainty of nature as it appears at first sight’ (ibid., 33), notes that ‘when we look closer we begin to detect “a striking regularity which seems to arise from design [...] [b] ut [which], on reflection, [...] is soon perceived [to be] nothing but the development of the respective probabilities of the simple events which ought to occur more frequently according as they are more probable’ (ibid., 34), Venn writes that

‘[i]f this remark had been made about the succession of heads and tails in the throwing up of a penny, it would have been intelligible, though [...] not philosophically correct. It would simply mean this: that the constitution of the body was such that we could anticipate what the result would be when it was treated in a certain way, and that experience, in the long run, would justify our anticipation’ (ibid.).
It is this extrapolation of the (a priori) conditions connected to the calculations of simple events to those of natural events – or ‘objectification’ of a state of mind - that Venn characterizes as ‘unmeaning’. In other words, also from his remark – uttered in chapter 13 titled ‘On direct and inverse probability’ – that the idea of so-called ‘inverse probability’ arises ‘from the attempt to force the Calculus of Probability upon a class of subjects which do not properly belong to it’ (ibid., 204), it may be inferred that Venn here criticized mainly the attempt to calculate the probability of an unknown cause from a known event in the context of natural, rather than artificial, series.9

This criticism is argued for by means of an elaborate discussion of the distinction between these ‘artificial’ and ‘natural’ series. Although the difference between them, as Venn puts it, can only be ascertained in their ‘ultimate form’,10 where ‘natural uniformities at length fluctuate, those afforded by games of chance seem fixed for ever’ (ibid., 17). That is to say that ‘the one tends without any permanent variation towards a fixed numerical proportion in its uniformity [and] in the other the uniformity is found at last to fluctuate […] in a manner utterly irreducible to rule’ (ibid.). If Venn acknowledges that it is only with regard to ‘artificial series’ that it is possible to make rigorous, i.e. mathematical, inferences, it is crucial to witness ‘the kind and extent of error that would be committed if in any example we were to substitute an imaginary series of the former [artificial] kind for any actual series of the latter [natural] kind which experience may present to us […] [This] substitution […] is equivalent to saying, Let us assume that the regularity is fixed and permanent’ (ibid., 18). Nonetheless, Venn – in full recognition of this error – claims that this substitution must be made in order to transform probability theory into a general science. He, thus, writes that even though

9 In the preface, Venn makes a similar remark when he criticizes ‘the belief that Probability is a branch of mathematics trying to intrude itself on to ground which does not belong to it’ (Venn 1866, vii).

10 Venn writes that ‘[t]he difference […] between them would not appear in the initial stage, for in that stage the distinctive characteristics of the series of Probability are not apparent; nor would it appear in the subsequent stage, for the real variability of the uniformity has not for some time scope to make itself perceived. It would only be in what we have called the ultimate stage, when we suppose the series to extend for a very long time, that the difference would begin to make itself felt’ (Venn 1866, 18).
Remarks on the idealist and empiricist interpretation of frequentism: Robert Leslie Ellis versus John Venn

In examining the series of statistics which arise out of any of [the] natural uniformities we should generally find that as a matter of fact the series tent at length to lose their regularity [...] this will not suit our purposes. What we do therefore is to make a substitution, and employ instead a series which shall be regular throughout. It is by this substituted series that we do in reality make our inferences [...]’ (ibid., 23).

In other words, Venn acknowledges the necessity of a process of idealization in which the ‘things’ about which probability makes inferences are brought into a shape ‘fit for calculation’ (ibid., 56). As Venn himself admits, this makes his position vis-à-vis the traditional theory somewhat diffuse: he first criticizes it for treating natural series as if they are artificial series, but then goes on to argue that this substitution – underlying its ‘inverse application’ – is, in fact, inevitable. From his scarce remarks about ‘ideal’ (natural) series it may be inferred that Venn finds recourse in the claim that where he himself explicitly characterizes these series as ‘mere fiction’ and ‘artifice’ (ibid., 58), the traditional theory pretends as if natural series of themselves resemble – and can be treated as – artificial series.11 I will return to this issue in subsection 2.2.

To conclude this section I would like to draw attention to the connection between the views of Ellis and Venn as discussed so far. Venn’s criticism of traditional probability boiled down to rejecting its assumption that it is possible to extrapolate the idea – connected to the mathematical calculations of games of chance - that events, in the long run, will occur with a frequency in accordance with their (‘a priori’) probabilities to natural events, or ‘facts of nature’. According to Venn, this can only be done by abstracting from the empirical fact that where the uniformity of artificial series of games of chance is fixed, that of series in nature at length fluctuates. Ellis, for his part, held that the traditional calculations of simple events depend on the mental expectation of the

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11 It is all the more remarkable that, after having introduced the notion of ‘substitution’ as a necessary precondition for reasoning about series of natural events, Venn discusses an example where the substitution is ‘accidentally’ unnecessary – namely a game of chance! ‘In most cases a good deal of alteration is necessary to bring the series into shape, but in some – I refer of course to games of chance – we find the alterations [...] needless’ (Venn 1866, 58). Here, Venn, thus, treats ‘artificial’ series as less artificial than natural series.
regularity of series which, as such, cannot be proved mathematically. Where Ellis, accordingly, defined the fundamental principle of probability theory as the recognition of the a priori truth of the (non-mathematical) axiom that series of events tend toward regularity, Venn’s reinterpretation of probability theory consisted of dismissing exactly this statement as a non-empirical dogma. As I will argue in the following section, this disagreement between Ellis and Venn can only be understood with reference to their particular, ‘idealistic’ and empiricist, position vis-à-vis ‘Bernoulli’s theorem’.

2. Bernoulli’s heritage: probability as the limit of a relative frequency

It was in Part IV of his Ars Conjectandi that Jakob Bernoulli revolutionized probability theory by claiming that it is possible to determine some unknown probabilities by experiment; this procedure consists of calculating estimates of the proportions within an unknown ‘probability set’ by means of repeated observations.\(^{12}\) This is commonly taken as the basic starting point of frequentism – the definition of probability as the limit of a proportion within a sequence of trials that is to be approximated a posteriori. Now, although some interpreters have remarked that Ellis and Venn failed to recognize that Bernoulli’s ‘theorem’ was the same as theirs,\(^{13}\) they both principally questioned the foundations of the traditional theory via a criticism of its ‘inverse’ application – albeit, as became clear, for opposing reasons. In what follows I would also like to point out that neither Ellis nor Venn straightforwardly adopted Bernoulli’s ‘frequentist’ starting point. This, in turn, is what explains their opposed positions vis-à-vis the foundations of frequentism as such.

2.1 Ellis’s ‘semi-Bernoullian’ frequentism

In his ‘On the foundations of the theory of probabilities’ Ellis rhetorically asked whether ‘we [are] prepared to admit, that our confidence in the regularity of nature is merely a corollary from Bernoulli’s [sic] theorem? That until this

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\(^{12}\) For the meaning of Part IV of Jakob Bernoulli’s Ars Conjectandi for the philosophy of probability see, for instance, Hacking (1971), Schafer (1996) and Hald (2007, chapter 2).

\(^{13}\) See, for instance, Hald (2007, 77).
theorem was published, mankind could give no account of convictions they had always held, and on which they had always acted?’ (Ellis 1844, 1). It has already been made clear in subsection 1.1 that Ellis considered ‘Bernoulli’s theorem’ as an a priori truth supplied by the mind itself, rather than as a result of mathematical demonstration. Now, in ‘Remarks on the fundamental principle of the theory of probabilities’ published in 1854, Ellis translates this fundamental principle of probability – i.e. that ‘[o]n a long run of similar trials, every possible event tends ultimately to recur in a definite ratio of frequency’ – into philosophical language as follows:

‘[W]e may in the first place remark that the phrase ‘similar trials’, expresses the notion of a group or genus of phenomena to which the different results are subordinates as distinct species [...] [I]t is less obvious how the idea expressed by a ‘long run of trials’ [...] is to be expressed [because] its own nature is negative and indefinite [and] impl[ies] merely the absence of the limitations inseparable from [...] any finite number of [...] cases whether contemplated as actually existent or as about to be developed within definite limits of space and time’ (Ellis 1854, 605).

According to Ellis, the problematic fact that ‘[w]hen individual cases are considered we have no conviction that the ratios of frequency of occurrence depend on the circumstances common to all the trials’ (ibid., 605) will only disappear ‘when we consider the genus in its entirety [i.e.] in what may be called an ideal and practically impossible realization of all which it potentially contains’ (ibid., 605-606) – that is, in the ‘limit’. On the basis of this statement Ellis reformulates the fundamental principle of probability theory as: ‘The conception of a genus implies that of numerical relations among the species subordinated to it’ (ibid., 606) and, subsequently, writes that because of this reformulation the traditional controversy between ‘realism’ and ‘nominalism’ is of importance for probability theory. As he has it: ‘[I]n what relation [...] do these conceptions [of the ‘ideal’ and ‘impossible’ realizations of a genus, LV] stand to outward realities? How can they be made the foundations of a real science, that is, of a science relating to things as they really exist? We are by such questions led back to what was long the great controversy of philosophy: - I mean the content between the realists and the nominalists’ (ibid.). Ellis argues that in order to make probability theory into a science it is necessary to entertain a realist
position which holds that ‘universals are not mere figments of the mind, but [...] have a reality of their own’ (ibid.).

His reasons for this claim are twofold. Firstly, he contends that the nominalist position – according to which the ‘grouping of phenomena together is merely a mental act wholly disconnected from outward reality and altogether arbitrary’ (ibid.) – cannot even be considered as a philosophically viable option; for Ellis, ‘man in relation to the universe is not spectator ab extra, but in some sort a part of that which he contemplates, and that the rebus avolsa ratio, which is [...] the fundamental postulate of nominalism, is therefore inconceivable’ (ibid.). Secondly, he answers the question of why and how facts and ideas correspond with reference to the fact that ‘in every science [...] the former is the realization of the latter’ (ibid.). This also explains how Ellis attempts to reconcile his metaphysical idealist reconstruction of probability theory with his realism vis-à-vis its philosophical foundations; facts approximately realize ‘a priori’ ideas which underlie experience.

This is of crucial importance for an understanding of Ellis’s views on ‘Bernoulli’s theorem’. To his statement that facts are realizations of ideas, Ellis adds that ‘as this realization is of necessity partial and incomplete [...] this correspondence is but imperfect and approximate. It is only when in thought we remove the action of disturbing causes to an indefinite distance, that we can conceive the absolute verification of any a priori law. Only on the horizon of our mental prospect [...] the fact and the idea are seen to meet’ (ibid., my emphasis). That is also to say that it is only ‘in idea’ that it is possible to introduce the ‘tendency inherent in a series of successively developed results to restore the balance of frequency of occurrence’ (ibid., 607) – which, as such, is to be explained with reference to the above-mentioned ‘ideality’ of the limit connected to the ‘ideal’ consideration of a genus in its entirety. When Ellis, thus, concludes that ‘[t]here is nothing absurd in the notion of a restorative and balancing tendency, though the grounds on which it is commonly assumed indicate much confusion of thought’ (ibid.) it may, by now, be clear that Ellis here aims at an idealist transformation of Bernoulli’s ‘frequentist’ starting point. In other words, he argues that the notion of a ‘limit’ can only properly be introduced, not as a cor-

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14 For an account of the intricate connection between Ellis’s ‘idealism’ and ‘realism’ see Kilinc (2000) and Verburgt (2013).
ollary of ‘Bernoulli’s theorem’, but, when it is both deemed possible to have knowledge of the (infinitely) long run on which ‘fortuitous causes’ disappear as well as admitted that ideal genera of probability have a reality of their own in so far as they are the realizations of an idea imposed on nature.

2.2 Venn’s ‘anti-Bernoullian’ frequentism

As pointed out in subsection 1.2, the notion of a ‘series’ – which Venn defined as ‘a large number or succession of objects’ (Venn 1866, 6) – was of crucial importance for his renovation of probability theory. He disagreed with traditional probability theory mainly for extrapolating, in a non-hypothetical fashion, the ‘a priori’ series of artificial events to the ‘experimental’ series of events in nature.\(^\text{15}\) Where the first ‘tends without any permanent variation towards a fixed numerical proportion in its uniformity’, in the second ‘the uniformity is found at last to fluctuate […] in a manner utterly irreducible to rule’ (ibid., 17). For Venn, series of natural events can be defined as peculiar kinds of classes of objects of things of which it must be assumed that they combine individual irregularity with aggregate regularity in the long run.\(^\text{16}\) As Venn himself acknowledges, such a formulation is reminiscent of Jakob Bernoulli’s theorem which holds that ‘in the long run all events will tend to occur with a frequency proportional to their objective probabilities’ (ibid., 35). Yet, he immediately adds that both this theorem as well as the very accompanying notion of ‘objective probability’ must be rejected.

‘This theorem of Bernoulli seems to me one of the last remaining relics of Realism, which after being banished elsewhere still manages to linger in the remote provinces of Probability. It is an illustration of the inveterate tendency to objectify our concepts even in the cases where the conceptions had no right to exist at all. A uniformity is observed; some-

\(^\text{15}\) This was, of course, exactly Bernoulli’s move when he, in a letter to G.W. Leibniz, argued that – put in modern terminology – there is not only a ‘fundamental probability set’ for games of chance, but also, for instance, for diseases. Bernoulli, thus, wrote: ‘If now in place of the urn you substitute the human body, young or old, which contains the tinder of diseases like an urn contains stones, you can in the same way determine how much nearer the one [the old man] is to death than the [young man]’ (Bernoulli quoted in Hacking 1971, 220).

\(^\text{16}\) Recall Venn’s introduction of ‘ideal’ or ‘substituted’ natural series.
times, as in games of chance, it is found to be [...] connected with the physical constitution of the bodies employed as to be capable of being inferred beforehand [and] this constitution is then converted into an ‘objective probability’ supposed to develop somehow into the sequence which exhibits the uniformity’ (ibid., 36).

As in his general criticism of the traditional theory, Venn dismisses the ‘objectification’ legitimized with reference to the analogy between games of chance and natural events –the inference from the existence of the ‘physical constitution’ of artificial events (the ‘objective probability’) to the existence of the ‘physical constitution’, capable of being known before its actual occurrence, of natural events. In his own words, the ‘very questionable objective probability is assumed to exist [...] in all the cases in which uniformity is observed, however little resemblance there may be between these and games of chance’ (ibid.).

Venn points out that he criticizes Bernoulli’s theorem especially for obscuring the following two, closely related, ‘positive truths’: ‘firstly, the gradual change of type, and secondly, the distinction between the actual series about which we reason and the substituted series we employ in reasoning about it’ (ibid., 37). In the context of this first truth, Venn remarks that the ‘doctrine of an objective probability [...] presupposes a fixed type. It seems merely the realistic doctrine of an ideal something which is perpetually striving, and gradually, though never perfectly, succeeding in realizing itself in nature’ (ibid., 38). According to Venn, it is this ‘realistic doctrine’ which assumes the existence of ‘fixed types’ in nature that has been refuted by the ‘Theory of Evolution’. Thus, in the second edition of the Logic of Chance published in 1876 Venn writes that

‘[n]o one who gives the slightest adhesion to the Doctrine of Evolution could regard the type [...] as possessing any real permanence and fixity [...] [When] the constant [causes, LV] undergo a gradual change, or if the variable ones, instead of balancing one another suffer one or more of their number to begin to acquire a preponderating influence, so as to put a sort of bias upon their aggregate effect, the mean will at once being [...] to shift its ground. And having once begun to shift, it may continue to do so, to whatever extent we recognize that species are variable’ (Venn 1876, 42-43).
In other words, from the theory or doctrine of evolution it can be inferred, on
the one hand, that there are no ‘fixed types’ in nature and, on the other hand,
that the development of (series of) events in nature cannot be understood, as
can the (series) of artificial events, in terms of succession. This, however, is
precisely what ‘Bernoulli’s theorem’ presumes when it claims that, in the long
run, natural events – like artificial events – occur with a frequency proportional
to their ‘objective probability’.

About the second truth – the distinction between actual and substituted series
– it suffices to say that it, as already pointed out in subsection 1.2, brought Venn
into a somewhat uncomfortable position. The full importance of the distinc-
tion becomes apparent with reference to the definition of probability as the
limit of a relative frequency of an attribute in a series – which, as I mentioned
above, is commonly taken as the traditional starting point of frequentism.
Venn himself asks: ‘[H]ow can we have a ‘limit’ in the case of those [natural,
LV] series which ultimately exhibit irregular fluctuations?’ (Venn 1866, 109).
The obvious problem to which Venn alludes is that the notion of an ‘ultimate
limit’ of a natural series necessarily involves the ‘Bernoullian’ assumption of
an ‘absolute fixity of the type’ (ibid.)! Now, because nature does not, except in
games of chance, ‘present us with this absolute fixity […] our only resource is to
invent such a series, in other words, […] to substitute a series of the right kind’
(ibid.). That is to say that since the series ‘we actually meet with show a change-
able type and the individuals of them will sometimes transgress their licensed
irregularity […] they have to be pruned a little into shape’ (ibid., 56). After this
process of idealized substitution the form ‘in which the series emerges is that
of a series with a fixed type and with its unwarranted irregularities omitted’
(ibid., 56-57). This indicates that Venn’s rejection of ‘Bernoulli’s theorem’
forced him to transform finite, ‘experimental’, series of natural events drawn
from actual experience into infinite series of ‘quasi’-natural events drawn from
potential experience. It also suggests that his empiricist reconstruction of tradi-
tional probability theory was premised on the creation of new, non-empirical,
foundations.17

To conclude this final, second, section I would like to point to the fact that
where Venn’s reformulation of the traditional theory was premised on the

17 For this point, see Verburgt (submitted for review b).
rejection of ‘Bernoulli’s theorem, that of Ellis hinged on its idealist reinterpretation. In a similar vein, where Venn objected to the theorem for holding on to the ‘realistic’ doctrine of ‘fixed types’ in nature, Ellis considered this realism as an essential precondition for probability theory as such. Despite the rejection of the theorem, Venn was forced to acknowledge the necessity of somehow incorporating the notion of a ‘limit’ of a series connected to the it into his definition of probability; he did this by inventing, or hypothesizing, infinite series of ‘quasi’-natural events drawn from potential experience. Interestingly, Ellis legitimized a similar ‘invention’ by characterizing it as an a priori axiom supplied by the mind. In opposition to Venn, Ellis considered these infinite, potential series as existing in nature in the form of ‘universals’.

3. Conclusion

My goal in this paper was to show that, contrary to received opinion, the work of Ellis and Venn cannot be regarded as either the empiricist reaction against the rationalistic foundations of traditional probability theory or as constituting a unified ‘British school’ of frequentism. As was pointed out in subsections 1.1. and 2.1, Ellis was in total agreement with the mathematical calculations of (inverse) probability, but attempted to replace their ‘sensationalist’ metaphysical foundations for ‘idealist’ ones. Interestingly, his work can thus be considered as the idealist reaction against what he conceived of as the empiricist foundations of traditional probability theory. In direct opposition Venn, Ellis also took a specific ‘Bernoullian’ realism vis-à-vis kinds in nature as a precondition for probability theory. In subsection 1.2, I argued that Venn, for his part, first and foremost criticized the traditional theory’s ‘inverse application’ for treating natural events and series as if they are artificial events and series. In subsection 2.2, it was made clear that Venn objected to ‘Bernoulli’s theorem’ for its reliance on the ‘realist’ objectification – under the heading of ‘objective probability’ – associated with this extrapolation.

Now, much hinges on Ellis and Venn’s respective position with regard to the assumed fundamental principle, or ‘Bernoullian’ starting point, of frequentism: that of probability as the limit of a relative frequency. Venn’s empiricist rejection of the validity of this principle in the case of natural, rather than artificial, series went hand in hand with his acknowledgment of the necessity of embrac-
ing it as an assumption. This, in turn, forced him to create his ‘non-empirical’ natural series that differed from those implied by Bernoulli’s theorem in so far as they are recognized as substitutions of actual, empirical natural series. According to Ellis, the theorem must be transformed from being a result of mathematical calculation into an a priori axiom. Venn and Ellis, thus, both treated it as the starting point of frequentism, albeit in a different way; where Venn considered it as the result of a process of idealization, Ellis made it into an ideal truth of the mind. Remarkably, the only point of agreement between them seems to be that the foundations of frequentism are the result of a ‘mental’ construction – the validity of which, in the case of Venn, is premised on its conformity with actual experience and, in the case of Ellis, is a priori.

On the basis of these insights I would like to propose that there were, in fact, two, rather isolated, ‘schools’ of British frequentism – Ellis’s idealist and Venn’s ‘quasi’ empiricist school – neither of which can straightforwardly be considered as the ‘empiricist’ reaction against the whole of traditional probability theory; most importantly, because they were, actually, in direct conflict with each other as far as the foundations of frequentism were concerned.¹⁸

¹⁸ My thanks to Gerard de Vries, who first suggested to pursue the research on the topic of this paper.
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