Borel on the Heap*

Paul Égré   Anouk Barberousse

Abstract

In 1907 Borel published a remarkable essay on the paradox of the Heap ("Un paradoxe économique: le sophisme du tas de blé et les vérités statistiques"), in which Borel proposes what is likely the first statistical account of vagueness ever written, and where he discusses the practical implications of the sorites paradox, including in economics. Borel's paper was integrated in his book *Le Hasard*, published 1914, but has gone mostly unnoticed since its publication. One of the originalities of Borel's essay is that it puts forward a model of vagueness as imprecision, making particular use of the Gaussian law of measurement errors to model categorization. The aim of our paper is to give a presentation of the historical context of Borel's essay, to spell out the mathematical details of his model, and to provide a critical assessment of his theory. Three aspects of Borel's account are particularly discussed: the first concerns the comparison between Borel's statistical account and posterior degree-theoretic accounts of vagueness. The second concerns the anti-epistemicist flavor of Borel's approach, whereby the idea of statistical fluctuation is used to undermine the notion of sharp boundary for vague predicates. The third concerns the problematic link between Borel's model of vagueness as imprecision and the notion of semantic indeterminacy. An English translation of Borel's original essay is appended to this paper.

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1 Introduction

In 1907 Émile Borel published a remarkable article in the journal *La Revue du Mois*, entitled "Un paradoxe économique: le sophisme du tas de blé et les vérités statistiques". The paper, whose English translation we present with this paper (*Erkenntnis*, this issue), is undoubtedly one of the earliest contributions on the sorites paradox written in the twentieth century, and possibly the first extended analysis on the topic ever written. Borel integrated it a few years later in his 1914 book *Le Hasard*, where it corresponds, with a few minor modifications, to paragraphs 47 to 51 of chapter 5 entitled "Les sciences sociologiques et biologiques", discussing some applications of probability theory to sociology and biology, including topics in economics and psychology.

Despite the success of Borel's 1914 book in France between the two Wars and the considerable legacy of Borel's mathematical work in fundamental areas of mathematics, Borel's reflections on the paradox of the Heap appear to have gone unnoticed for more than a century. On the other hand, interest for the topic of vagueness and for the sorites paradox has been constantly increasing since the 1970s, making the translation and reissue of this paper both needed and timely. Indeed, like most of Borel's other books since the death of their author, *Le Hasard* has been out of print for several decades now, and to the best of our knowledge the book was never translated into English. Borel, however, returned to the topic toward the end of his life in 1950, and again devoted a whole chapter of his short book *Probabilité and Certitude* to the paradox of the Heap. Unlike *Le Hasard*, Borel's 1950 book was translated into English after Borel's death in 1963, but here again Borel's reflections appear not to have drawn much attention, and both editions too have been out of print since. While mostly self-contained, that chapter is furthermore a different text, best read in our view as an addendum to the 1907 piece, in that it explicitly introduces clarifications or updates on points left unclear in the original 1907 essay and in related writings by Borel from the same period (see our Appendix below in particular).[1]

In his original essay, Borel presents a probabilist account of the sorites paradox and outlines a degree-theoretic conception of vagueness, both of which are certainly the first of their kinds. The paper, moreover, puts special emphasis on the practical implications of the phenomenon of vagueness, as Borel's main goal is to discuss a particular manifestation of it in economics. Despite the lack of filiation between Borel's work and posterior analyses of vagueness, the text antedates some of the more recent approaches to vagueness, even though several of the ideas it contains remain completely unprecedented until today.

The first goal of our paper is to give an exposition of Borel's paper, putting it back in its historical context, but also explaining how it compares with more recent theories of the sorites paradox. As we shall see, Borel proposes a statistical treatment of vagueness as imprecision, with special emphasis on the theory of measurement errors: part of our effort is to explain the mathematical details of his model and to show how it relates to

[1] The English translation does not include the original footnotes of the 1950 French edition, incidentally, in which Borel makes cross-reference to his earlier work on the sorites.

Borel's foundational concerns about measurement and psychophysics. The second main goal of our paper is to provide a critical assessment of Borel's approach to vagueness and the sorites paradox. Borel presents two distinct statistical perspectives on vagueness: one concerns lexical vagueness, namely the variance affecting linguistic judgments across individuals. The other concerns what we call practical vagueness, namely the imprecision in the individual application of sharp criteria in categorization. Both perspectives are used by Borel to argue against the idea of sharp cutoff points in sorites series, conferring to Borel's approach a rather striking anti-epistemicist orientation. Despite this, Borel does not give any elaboration of the link there might be between these two distinct perspectives, nor does he question the compatibility of his imprecision model with a view of the kind the epistemicist would favor. We discuss this connection. We also show the affinity between Borel's account of vagueness and ulterior degree-theoretic accounts, most notably Smith's fuzzy account, by explaining in what sense the use of probabilities allows Borel to circumvent what Smith calls the 'jolt problem' and the 'location problem' in sorites series (Smith 2008). Despite this connection, we argue that Borel's account bears more affinities to supervaluationism than to standard fuzzy accounts in which degrees of truth are not seen as derivative upon yes/no judgments.

Our paper is structured as follows. Sections 2 and 3 give some elements on the historical background of Borel's essay. Sections 4 to 7 give a detailed exposition of Borel's theory, in particular regarding his taxonomy of heap phenomena, his understanding of the major premise of the sorites, and his use of probability theory to deal with categorization and category change. Section 8 provides a critical assessment of Borel's account, examining the pragmatic picture of meaning boundaries for vague expressions that results from his account, and discussing the connection between lexical vagueness and practical vagueness. Further elements concerning Borel's attention to psychophysics and the connection between vagueness and the problem of intransitivity in discrimination are presented in an appendix.

By the attention given to historical, epistemological and mathematical aspects of Borel's essay, our paper ends up being longer than it might have been if we had focused on a single of those aspects. Not all sections carry equal weight, however. Readers not interested in historical aspects may wish to skip sections 2 and 3 to directly focus on the more analytical parts of our work.

2 *La Revue du Mois*

Borel's essay on the Heap paradox appeared in *La Revue du Mois* (The Monthly Review), a journal which Borel created in 1905 with his wife Camille Marbo (born Marguerite Appell, and daughter to mathematician Paul Appell) after he was awarded the Petit-d'Ormoy prize by the French Academy of Science (Marbo 1967). The first issue appeared in January 1906. The aim of the journal, whose board included scientists and scholars from different disciplines (among whom several distinguished colleagues to Borel, such as physicists Paul

Langevin and Jean Perrin) was to serve an educational and enlightening purpose, namely to instruct a general readership on a wide range of topics spanning from scientific subjects, to social and economic issues, to military and political affairs (see Guiraldenq 1999, Gispert 2012). The journal got regular issues published between its inception in January 1905 and June 1915, when due to war circumstances its activities went on stand-by. It appeared again after the War but only for a short period, between 1919 and December 1920, when its publication stopped for good.

The project of *La Revue du Mois* gives a good illustration of Borel's constant interest and preoccupation for didactic matters, from the early 1900s until the end of his life.[2] Specifically, "Un paradoxe économique" reflects Borel's aim to defend and popularize the value of the probability calculus in the treatment of a wide range of scientific and practical problems. The same effort toward making the probability calculus part and parcel of the general scientific education and mathematical instruction can be seen in his book *Le Hasard*, which stands equally as a popular science book and as a philosophical manifesto. It is also reflected in the publication of a series of mathematical treatises by Borel on the probability calculus, including Borel's *Éléments de la théorie des probabilités*, first published 1909, his book *Probabilités, Erreurs* (written with R. Deltheil and published 1923, later augmented with R. Huron), and finally the four volume handbook *Traité du calcul des probabilités et de ses applications*, published in 1939.

The personal context of the publication of Borel's "Un paradoxe économique" also corresponds to a time when Borel gradually shifted his theoretical interests from pure mathematics to applied mathematics, being particularly attracted by the subject of probability theory, a topic on which Borel subsequently taught several courses.[3] Today Borel's early mathematical work in analytical function theory remains what Borel is most famous for, in particular for his pioneering contribution to the field of measure theory (later to become the basis of descriptive set theory, as well as of probability theory). A very substantial part of Borel's work, however, was either directly or indirectly driven by a sustained interest in mathematical physics and with a view toward all mathematized sciences more generally. This is clearly witnessed in his book *Le Hasard*, whose second part discusses some of the applications of statistical methods to the emerging sciences of biology, economics, and even psychology (chapter 5), and also to physics (chapter 6), including astronomy and cosmology.[4] The third part of *Le Hasard*, finally, concerns what Borel calls the "practical value of the probability calculus", what in more modern terms can be viewed as a discussion of

[2]Borel exerted several academic responsabilities in relation to primary as well as higher eduction. See Guiraldenq 1999 and Gispert 2012 for more biographical details.

[3]See Graham and Kantor 2009: 62. Borel wrote to his wife in 1909: "Not having any more strength for high mathematics, I will go safely to work in probability and statistics following your uncle Bertrand. It is not much compared to my earlier works in mathematics, but it is useful!" (from Marbo 1967). Borel in this note alludes to Joseph Bertrand, a relative of Paul Appel's wife, and prominent probability theorist.

[4]Borel wrote at least three important contributions to the field of the foundations of statistical mechanics, namely Borel 1906, Borel 1903, and Borel 1915.

the central role of probability in decision theory in a broad sense.[5]

Those aspects of Borel's work are important to bear in mind in order to appreciate the singular perspective taken by Borel on the sorites paradox in his 1907 paper. In the light of the current literature on the topic, it is quite intriguing to see that Borel conceived of the sorites paradox first and foremost as an *economic* paradox, as expressed in the title and opening of the paper, that is as a puzzle with practical implications, and not merely as a *logical* puzzle. Borel's attitude in this regard is characteristic both of his mathematical inspiration – a problem is well-defined on the condition that it can be sufficiently generalized – but also of his positivist inspiration and constant concern for the pragmatic dimension of mathematics – a problem is interesting particularly if it is has practical implications.[6] In retrospect, it may also be seen as the expression of Borel's well-known skepticism, shared with several of his French peers, and most notably Poincaré before him, for questions of pure logic.[7]

3 Vagueness and Zeno's paradoxes around 1900

Before examining the details of Borel's account of vagueness and the sorites paradox, it is worth noting that Borel in his paper uses neither the now familiar word "vagueness" (whose French counterpart is: "le vague") nor the Greek-derived term "sorites" (whose French counterpart is: "le sorite"). For the latter, although he uses the term 'paradox', he also speaks of "the sophism of the heap of wheat", a characterization of the paradox shared by several other scholars at the time. In this section, we first review the (rare) occurrences of the words 'vagueness' and 'sorites' in the writings of Borel's contemporaries, as well as the place occupied by Zeno's paradoxes – under which Borel subsumes the Heap.

Peirce is usually credited for having raised the word "vague" to the status of a philosophical notion in the corresponding 1902 entry to Baldwin's *Dictionary of Philosophy and Psychology*. Peirce, however, does not refer to the sorites argument in his entry on vagueness, though the Heap is mentioned in subdefinition 2 of the entry "Sorites" of the

[5] This is also the topic of the last part of Borel's *Traité du calcul des probabilités*, entitled "Valeur pratique et philosophique du calcul des probabilités". The general topic of the practical value of science was much discussed at the time, for instance by Poincaré (1902-1908).

[6] Talk of a positivist inspiration in Borel's philosophy of science is not fortuitous. The title of chapter 5 of *Le Hasard*, "Les sciences sociologiques et biologiques", for instance, is inherited from A. Comte's division of the sciences.

[7] A good example of Borel's rather dismissive attitude towards logic is Borel (1907a) entitled "La logique et l'intuition en mathématiques", where Borel criticizes the views of L. Couturat. Borel writes, for instance: "A logical formula is a phenomenon like the fall of a body or like a tree and mathematics are a natural science in which logic plays no more role than in the other natural sciences" ("Une formule logique est un phénomène comme la chute d'un corps ou comme un arbre et les mathématiques sont une science naturelle dans laquelle la logique ne joue pas plus de réle que dans les autres sciences naturelles"). Despite this, Borel maintained a profound interest for the set-theoretic paradoxes, throughout his carrier. See in particular Borel (1946).

same dictionary (an entry coauthored by Peirce and Baldwin).[8] Russell (1923) is often considered the first to have stated an explicit connection between lexical vagueness and the sorites paradox in the version of the Bald man (a version not alluded to by Borel in his text).[9] As a matter of fact, the association of the word "vague" with the paradoxes of the Heap and the Bald Man can be found explicitly in the French philosophical tradition before Borel and even before Peirce.[10] It appears, in particular, in C-B. Renouvier's *Traité de logique générale et logique formelle*, who concludes a discussion of an argument by Spencer with the following comment (1874: 316):

> "My argument only superficially resembles the famous argument of the Heap, also known as the Bald Man, which is rightly considered a sophism. What makes the sophism, in these latter examples, is the idea that a heap, compared to a number of grains of wheat, for example, or a bald head, compared to a fixed number of hairs, are vague concepts, which by their nature exclude numerical precision. Hence to ask how many grains or hairs more or less make or do not make heapness or baldness, is like asking what determinate number of objects is needed to add up to a concept representing an indeterminate number. The question therefore is absurd."[11]

This quote by Renouvier is valuable to appreciate Borel's presentation of the puzzle, for it shows that Borel is not the first to present the argument of the "heap of wheat" as a sophism. In particular, Renouvier presents the sorites as a misconceived puzzle akin to a category mistake (seeking determinateness where indeterminateness is presupposed).[12]

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[8]Peirce uses the word "sorites" and "heap" in his writings, but mostly to talk of chains of arguments, as is quite common at the time. Viz. CP 4.45 Cross-Ref:††: "A necessary inference from a single premiss is called an immediate inference, from two premisses a syllogism, from more than two a sorites.". The entry "Sorites" of the *Dictionary of Philosophy and Psychology* (1902: 557) mentions both meanings to the word, namely "a chain of syllogisms" as first meaning (given by Peirce), and "applied to a Megarian sophism of the 'Heap"' as second meaning (given by Baldwin). The latter gives a cross-reference to the entry "Sophism", also coauthored by Peirce and Baldwin, where the Heap appears under the second definition given there for "sophism", namely "a false argument which, without deceiving, is difficult to refute logically" (1902: 556).

[9]Russell in particular writes: "Baldness is a vague conception; some men are certainly bald, some are certainly not bald, while between them there are men of whom it is not true to say they must either be bald or not bald"

[10]Peirce 1902 entry notes that the French word for "vague" is "vague", possibly an indication that Peirce had found it used in French writings.

[11]"Mon argument n'a que l'apparence de l'argument célèbre du *Tas*, autrement dit du *Chauve*, lequel passe à très bon droit pour sophistique. Ce qui fait le sophisme, dans ce dernier, c'est l'idée qu'un tas comparativement à un nombre de grains de blé, par exemple, ou qu'une tête chauve, comparativement à un nombre de cheveux fixe, sont des idées vagues, qui de leur nature excluent la précision numérique. De là vient que demander combien de grains ou de cheveux en plus ou en moins font ou ne font pas le tas ou la calvitie, c'est demander quel nombre déterminé d'objets il faut pour constituer un total dont l'idée répond à un nombre indéterminé. La question est donc absurde."

[12]Frege is another major figure and contemporary of Renouvier, Peirce and Borel, who emphasizes the connection between the sorites and indeterminateness in a brief passage. In the paragraph §26 of *Begriff-*

Whether or not Borel had read Renouvier is not something we know, although we can conjecture that Renouvier's treatise is representative of the philosophical knowledge that might have been available to an educated French student at the end of the 19th century (at the time of Borel's education).

Another noteworthy aspect of Borel's presentation is that he attributes the sorites paradox to Zeno at one place in his paper ("Zeno's argument"). This attribution is unexpected, given the present consensus (based on Diogenes Laertius, see Moline 1969) that the paradox should be attributed to Eubulides of Miletus, who is also considered the inventor of the argument of the Bald Man. However, as Williamson (1994: 9) reminds us, "the Heap may have been inspired by a different puzzle, the Millet Seed, propounded by Zeno of Elea a century earlier". Borel's comment is consistent with that hypothesis, but it is rather unclear whether Borel is indeed knowledgeable in making this attribution, or whether he is simply mistaken.[13]

In 1907, the very year of publication of Borel's paper, Henri Bergson in particular published *L'évolution créatrice*, an ambitious and controversial book in which Bergson discusses notions of transformation and evolution. A section of Bergson's book is devoted to the discussion of Zeno's paradoxes.[14] Bergson includes in that discussion the example of the passage from childhood to adolescence and to adulthood. About that Bergson writes, in particular (1907, IV. 312):

> "Nothing would be easier, now, than to extend Zeno's argument to qualitative becoming and to evolutionary becoming. We should find the same contradictions in these. That the child can become a youth, ripen to maturity and decline to old age, we understand

sschrift, he makes a short comment about the fact if the concept 'heap of beans' was *hereditary* in the sequence determined by the relation for b to contain one bean less than a (i.e. if the concept was closed under the relation in question) then 0 beans would make a heap. He points out that heredity fails because of "the indetermination of the concept "heap"". Similarly Peirce (1902:748) insists that vagueness gets its origin in the fact that "the speaker's habits of language [are] indeterminate". Borel himself in the text talks of the "necessary indeterminacy of verbal definitions".

[13]According to Moline (1969), most Greek scholars seemed to agree, already in Borel's time, that the paradox should be attributed to Eubulides rather than Zeno. For example Gomperz's book *Greek Thinkers* vol. 2, whose English translation from German appeared in 1905 (and whose first volume was translated in French in 1908), clearly attributes the sorites to Eubulides, also giving an explicit discussion of Zeno's Millet Seed for comparison. Interestingly, Black (1937) mentions that the paradox is "sometimes attributed to Zeno" and cites J. Burnet *Greek Philosophy* (1928: 114) as a reference. As a matter of fact, Burnet does not refer to the sorites in that passage, he cites a dialogue from Simplicius about Zeno's Millet Seed with a different purpose in mind. Also, although our research is certainly not exhaustive, the various texts written about Zeno's paradox by French scholars around the turn of the 19th century, including Bergson 1907, do not contain any explicit mention of the sorites paradox but only discuss Zeno's four classic problems.

[14]Broader interest for Zeno's paradoxes at that period in the French philosophical community is testified by the publication, in the first issue of the *Revue de Métaphysique et de Morale* in 1893 of several papers on Zeno's paradoxes, some of which in relation to Renouvier's work. See the papers by G. Brochard, F. Evellin, G. Milhaud, G. Lechalas, G. Noël, all grouped in the same issue. Those papers, however, deal with the classic paradoxes about the impossibility of movement, not with the Heap.

when we consider that vital evolution is here the reality itself. Infancy, adolescence, maturity, old age, are mere views of the mind, possible stops imagined by us, from without, along the continuity of a progress."[15]

It is quite possible for Borel to have been prompted by Bergson's reflections on Zeno's paradoxes, all the more since Borel himself published a rather critical review of the vision of mathematics developed by Bergson that same year 1907.[16] In that paper, Borel criticizes Bergson for actually *underestimating* the role played by intuition and cinematic imagination in the work of modern mathematicians (such as Weierstrass and Lie). One of Bergson's leading ideas in *Creative Evolution* and in the rest of his work is indeed the claim that geometric intuition would give us only a static view on the underlying dynamics of processes. Basically, Borel criticizes Bergson for failing to acknowledge the importance of movement and transformations in modern geometry, and therefore for misrepresenting geometric intuition in his account. Interestingly, Borel ends his criticism of Bergson with the following remark (1907b: 2109):

> "Once accustomed to those forms of thinking, one feels in front of Zeno of Elea's sophisms the same indulgent surprise as before a four-year-old child who asks that the stars be given to him: a few years later, you will be able to explain to him why it is impossible, but then he will not be asking anymore."[17]

In the light of this rather critical comment, one wonders whether Borel might have wished to provide an example of an analysis of one of Zeno's paradoxes specifically informed by a rising branch of mathematics, namely the probability calculus. Borel's account of the sorites in this respect differs significantly from the spirit of Bergson's opposition between the discrete and static character of verbal categories vs. the dynamic and continuous character of reality. Borel in particular does not hold the view that verbal categories are mere artefacts, and he does not solve the puzzle of the Heap merely by calling on the concept of continuity. His emphasis lies elsewhere, that is in the importance of statistical facts underlying categorization processes.

[15] "Rien ne serait plus facile, d'ailleurs, que d'étendre l'argumentation de Zénon au devenir qualitatif et au devenir évolutif. On retrouverait les mêmes contradictions. Que l'enfant devienne adolescent, puis homme mûr, enfin vieillard, cela se comprend quand on considère que l'évolution vitale est ici la réalité même. Enfance, adolescence, maturité, vieillesse sont de simples vues de l'esprit, des arrêts possibles imaginés pour nous, du dehors, le long de la continuité d'un progrès."

[16] See Borel (1907b) and also Borel (1908a), a short follow up to Bergson's response in the *Revue de Métaphysique et de Morale*.

[17] "Lorsqu'on s'est habitué à ces formes de pensée, on éprouve devant les sophismes de Zénon d'Elée le même étonnement indulgent que devant un enfant de quatre ans qui demande qu'on lui décroche les étoiles: quelques années plus tard, on pourra lui expliquer pourquoi cela est impossible, mais il ne le demandera plus."

4 Lexical vs. practical vagueness

The first originality of Borel's paper in relation to the literature on vagueness and the sorites written since then is that it starts out with a taxonomy of concrete manifestations of the paradox of the Heap, of which the phenomenon of linguistic vagueness, namely the difficulty of assigning non-arbitrary bounds of application to most of our ordinary vocabulary, is only one aspect, and even the one Borel considers the less interesting.

More generally, Borel proposes a threefold classification of Heap-like situations. The first category in his taxonomy is what we can call *lexical vagueness*, that is the problem of specifying the definition and limits of application of ordinary verbal categories. The examples given by Borel are all typical examples: Where is the limit between childhood and adulthood? Where lies the difference between a house and a palace? When exactly does a shade of color turn into a distinct shade? The analysis of that family of problems is the topic of the first section following the introduction to the paper, in which Borel presents his views about the relation between linguistic meaning and language use.

The second category corresponds to cases of *practical vagueness*, that is categories explicitly related to practical interests (such as "acceptable (for a manuscript to publish)", "sufficiently large (for a room to rent)", "worth buying (for a ticket")). It includes problems of individual decision-making of two kinds: on the one hand situations that concern the application of an explicit and predetermined rule of choice; on the other hand situations in which the choice rule is only implicit and partly determined. Examples of the first kind of predicament for Borel are about the imprecise application of some administrative rules meant to be precise. The main example concerns the rules of acceptance of a manuscript by a publisher, as a function of the manuscript size and of precise bounds of acceptance and rejection. An example of the second kind of predicament is the choice of a room as a function of the size of the room and of underspecified criteria of acceptance and rejection. The latter subcategory of problems is dealt with more briefly at the end of the same section. The analysis of both kinds of examples is actually the core of Borel's probabilistic conception of vagueness, and it occupies the second section after the introduction.

The third category finally, which we may dub *economic vagueness*, is the economic paradox intended by the title, dealt with in the penultimate section of the paper. The problem concerns the incidence of small variations of wholesale price on variations of retail price for some good. The point of this example is that the scale of measurement for variations of wholesale price is more fine-grained than the scale for retail price, and that variations along one scale are not exactly aligned with variations along the other scale. Obviously, this third class of problems can be seen a particular instance of practical vagueness, since retail price is indeed a function of individual choices in the setting of prices. The reason Borel presents it as a category of its own may have to do with the fact that it constitutes a generalization of the problem of yes/no categorization discussed in the previous two categories (see section 7 below).

A look at Borel's taxonomy reveals two commonalities behind Borel's categories. First of all, Borel fundamentally conceives of all soritical situations as situations of individual decision-making. Whether in the verbal or in the practical domain, the three categories of examples reviewed by Borel are cases in which a subject has to make a decision whether or not to include an object under a category. Secondly, all examples have the same abstract structure, as revealed in Borel's target example: how exactly does a small variation of some input parameter along a given scale of measurement affect the inclusion of an object within a given category, when the scale relevant for the output category is itself coarser-grained?

A third aspect worth emphasizing about Borel's classification finally is that it covers different aspects of the phenomenon of vagueness as we conceive of it today. In his discussion of lexical vagueness, Borel talks of the "necessary indeterminacy of verbal definitions", and points to the "ambiguity" of common definitions. When dealing with the second category of Heap-like situations, Borel uses a model of statistical error to account for the imprecise or approximate application that is commonly made of precise rules. He thereby accounts for so-called tolerance effects, namely for the way in which precise rules get to be relaxed in practical applications.[18] Finally, in his treatment of the sorites as an economic paradox, Borel's analysis points toward the problematic relation between scales that are calibrated in different ways.[19] As a result, we may say that Borel's account proposes a unified perspective on at least three aspects of the phenomenon of vagueness: about vagueness as semantic indeterminacy (see sections 5 and 9 below), about vagueness as imprecision (see section 6 below), and finally about the connection between vagueness and granularity (see section 7 below).

5 Probabilities and the tolerance principle

In order to appreciate Borel's treatment of vagueness for the three categories of problems he distinguishes, let us first examine the way in which probabilities allow him to explain away the paradox. His account presents some significant commonalities with subsequent degree-theoretic treatments of the sorites, in particular regarding the treatment to give of the main premise of the sorites. His statistical inspiration, however, differs in important ways from what later led to fuzzy logic, and comes in fact closer to supervaluationism.

Modern accounts of the sorites paradox are usually classified depending on how they handle the major premise of the sorites, usually called the *tolerance principle* after Wright (1976), or sometimes the inductive premise, namely the principle that:

(1) if some object a_n is P, and if a_n is very similar to a_{n+1} in the relevant respects, then a_{n+1} is also P.

In the case of the Heap, this is the premise that if n grains of wheat do not make a heap,

[18] The word "tolerance" does not appear in Borel (1907), but it is used in Borel (1950) to qualify the permitted error around some standard.

[19] See in particular Hobbs (2000) for a recent perspective on this problem.

then neither do $n + 1$ grains. When the logic is assumed to be bivalent and classical, acceptance of that premise leads to paradox. The rejection of that premise, on the other hand, logically implies that there has to be a least number n of grains such that n grains do not make a heap, but $n + 1$ do.

Borel is aware of the dilemma, and in his essay he abstains from saying either that the premise is true or that it is false. The main reason is that Borel explicitly denies the presupposition that the right way to think of how we reason in such cases is in terms of a "yes or no answer". Nevertheless, he gives a clear statement that he considers the denial of the main premise of a sorites to be misconceived:

> "Should we say that 2342 grains, for instance, do not make a heap, whereas 2343 grains make one? This is obviously ridiculous."

On the other hand, Borel's account implies that the tolerance principle is not perfectly true either, for he writes:

> "there is no sudden leap, but an imperceptible decrease down to a zero probability (...) There is never absolute demarcation, but always imperceptible shading-off. (...)"

With regard to the current literature on vagueness, the first passage above arguably sets Borel at odds with modern epistemic theories of vagueness (see in particular Sorensen 1988, 2001; Williamson 1994), according to which every vague predicate comes with an underlying but unknowable sharp cut-off.[20] But the second quote clearly indicates that Borel does not accept the naive version of tolerance either. For Borel, a difference in the respects relevant to adjudicate whether an object is P, however slight, must be matched by some difference in the probability of judging the object P, however slight. For Borel, however, this does not mean that the difference will necessarily be easily detectable in practice. Borel (1950:108) is much more explicit than Borel (1907a) on this. Borel asks:

> "Can it be admitted that the small difference of ten grains added to a thousand will make enough difference in the coefficient of probability for this alternation to be revealed by a sufficient number of experiments?".

His answer is that the number of experiments needed to statistically reveal such a difference may often be too large in practice. But this is not an argument against the theoretical validity of the principle.

Although Borel never states an explicit abstract alternative to the tolerance principle, his analysis therefore suggests the following generalization as a way out of the dilemma:[21]

[20] Note that Borel writes: "There appears no logical way out of this dead-end; it is therefore *not possible to know* what is a heap of wheat" (our emphasis). Borel's remark – which sounds like free indirect speech in this context – is nothing like an endorsement of necessary ignorance about some determinate fact of the matter, since Borel denies that there is anything like absolute boundaries to be known.

[21] See Egre 2009 and Lassiter 2011 for probabilistic versions of the tolerance principle along the lines of (2), though based on assumptions distinct from Borel's; see Egré 2011b for the suggestion to interpret Smith's closeness principle in probabilistic terms.

(2) if a_n and a_{n+1} are similar in respects relevant for the application of the predicate P, then the probability of judging a_n to be P and the probability of judging a_{n+1} to be P must be close

While stated in terms of probability, such a principle suggests a comparison between Borel's account and fuzzy accounts of vagueness involving degrees of truth, which make the tolerance principle not true to degree 1, but almost perfectly true. N. Smith's (2008) recent account of vagueness in particular proposes the following *closeness principle* as a weakening of the tolerance principle:

(3) if a_n and a_{n+1} are close in P-relevant respects, then the degree of truth of $P(a_n)$ will be close to the degree of truth of $P(a_{n+1})$

The closeness principle is used by Smith to block the paradox and to solve what Smith calls the *jolt problem* for sorites series, namely the situation in which two highly similar cases receive semantic verdicts that are very far apart. The same concern is at work in Borel's approach, in the idea that probabilities avoid "sudden leaps". Furthermore, probabilities can also account for the lack of what Borel calls an "absolute demarcation" along the series. This matches what Smith dubs the *location problem* for vagueness, namely the problem of having to postulate a hidden but determinate cutoff for a vague category. Indeed, any item for which the probability of inclusion under a category is strictly between 0 and 1 is a potential location for category change, making no particular point a privileged location.

While avoiding jolts and determinate boundaries, probabilities can also explain that subjects do change categories along a sorites series: without actual leaps at particular locations, there would be no probabilities to compute, an no shift between categories.[22] As his discussion of the economic paradox shows, Borel considers as a psychological *illusion* the idea that if an individual is ready to pay some good n cents, the individual is equally likely to pay the same good $n+1$ cents. In other words, probabilities offer a middle way between the two horns of the paradox, that there would be absolute demarcation between two consecutive individuals on the one hand, and that there would be no demarcation at all on the other.

Despite the affinity between Borel's emphasis on probabilities and Smith's fuzzy account of vagueness, it would be inadequate, both conceptually and historically, to conflate Borel's talk of probabilities with talk of degrees of truth in the sense usually intended in fuzzy logic.[23] At best, we may say that Borel outlines a pragmatic treatment of vagueness that

[22] Borel (1950: 108) is even more explicit on this: "the use of a coefficient of probability allows one to introduce a discontinuity in place where there is an apparent continuity".

[23] On the relation between probability theory and many-valued logic, see Dubois and Prade's (2001) valuable clarification. See also Edgington (1997) and Schiffer (2003) regarding conceptual differences between probabilities viewed as reflecting subjective *uncertainty*, and intermediate degrees of truth taken to represent *ambivalence* in the case of vagueness. Schiffer's perspective, like Edgington's, rests upon a subjective or Bayesian conception of probability. However Borel's 1907 use of probability in the text relies exclusively on frequentist considerations, from which Borel departed only later (see Borel 1950). See Borel (1914, chapter

corresponds to one attested way of making sense of degrees of truth, namely in terms of statistical frequencies. This view of degrees of truth is generally considered inadequate by fuzzy logicians, however, for whom truth degrees are not seen as derivative from yes/no judgments.[24] On the other hand, Borel's convention to assign a probability of x to $P(a)$ provided $x\%$ of the members of a group judge the sentence true bears a strong affinity to the supervaluationist treatment of degrees of truth sketched by Lewis (1970) and elaborated by Kamp (1975). The proposal is basically to define the degree of truth of a vague sentence as the proportion of precisifications in which the sentence is true.[25] On Kamp's picture, degrees of truth are therefore parasitic on bivalent verdicts, exactly as they are for Borel. The affinity between Borel's approach and supervaluationism can furthermore be used to substantiate Borel's rejection of the sharp boundary principle, namely the idea that postulating a value n such that $P(n)$ and $\neg P(n+1)$ is "ridiculous". We may take Borel to mean that no two consecutive numbers n and $n+1$ are such that all competent speakers would unanimously judge that n grains of sand make a heap, and unanimously judge that $n+1$ grains don't, or equivalently, that no two consecutive numbers are such that the probability of judging $P(n)$ is 1 and the probability of judging $P(n+1)$ is 0. Such a principle constitutes a particular case of the closeness principle (2) stated above.[26]

Besides supervaluationism, a closer kin to Borel's approach is M. Black 1937's statistical treatment of vagueness, published thirty years later. To the best of our knowledge, Black was not acquainted with Borel's reflections on vagueness, but Black's account is undoubtedly the closest in spirit to Borel's, since like Borel, Black explicitly sets an analogy between the 'variations in the boundary' of a category and the fluctuations in the measurement of a physical magnitude. Black in particular writes:

> The crude notion of the "fringe" is therefore replaced by a statistical analysis of the frequency of deviations from strict uniformity by the"users" of a vague symbol (1937: 430).

1 and §87) for an early discussion of the distinction between subjective and objective probability, which indicates that Borel saw frequentism as a way to bridge the two perspectives.

[24]Hajek (1998: 4) in particular describes as a "frequentist temptation" the idea of interpreting "non extremal truth degrees as relative frequencies". Borel's discussion of linguistic judgments may be compared to Hajek's remarks:

> "there have been attempts to explain non-extremal truth degrees as some relative frequencies. For example, take the sentence: "Sagrada Familia is beautiful" and ask n people "is Sagrada Familia beautiful?" allowing them to say only "yes" or "no". Imagine 70% of them say "yes". Can you take 0.7 to be the truth degree of our sentence? This causes problems because "beautiful" in your question was two-valued (yes-no, say "beautiful"$_2$) whereas in your sentence you deal with fuzzy "beautiful" ("beautiful"$_f$, say)."

[25]See Lewis (1970) Kamp (1975: 134), and Kamp and Partee (1995). See also McGee and McLaughlin (1995). We are indebted to T. Williamson for pointing out this connection.

[26]Compare with Lassiter (2011) and Égré (2009, 2011b).

Black, more specifically, defines for each item in a sorites series the *consistency of application* of a predicate L, roughly as the ratio within a group of the number of observers judging that L applies to the number of observers judging that the negation of L applies.[27] Borel's remarks concerning the statistical coefficient attached to the application of a predicate within a given population is exactly in line with Black's posterior analysis and remarks.

Unlike Black in 1937, Borel does not attempt to even sketch what a logic of vague predicates would look like.[28] The reason is that Borel nowhere countenances probability coefficients as degrees of truth in the sense of fuzzy logic, namely as objects of truth-functional computation: such objects were introduced only 13 years later by Łukasiewicz (1920) and in a different context (originally, in relation to the problem of future contingents).[29] For one thing, unlike Łukasiewicz, Borel is not interested in the behavior of logical connectives. More fundamentally, while Borel's account of language use proposes a frequentist interpretation of the probability of application of vague predicates, his discussion there rests on the assumption that individual judgments are yes/no judgments.

Consequently, although Borel's discussion of language use is undeniably in the spirit of subsequent accounts of vagueness based on degrees of truth, it does not offer to change the logic. The point of Borel's paper is not to deny the principle of bivalence, a principle Borel does not even question (unlike Black in his own essay, or the supervaluationists). His claim is that a good account of the use we make of vague predicates in terms of "yes" and "no" judgements should take account of statistical laws about how such judgments are pronounced, individually and collectively.

[27] Black's definition is more precise, since it defines the consistency of application as the limit of this ratio when the number of observations and the number of observers tend to infinity. See Black (1937: 442) for details.

[28] Black's account, unlike Borel's, did have a significant influence on the development of fuzzy logic. See in particular Goguen 1969 an Zadeh 1975 for reference to Black's work. Goguen, in particular, stresses the importance of Black's consistency profiles toward the project of solving the sorites paradox within a deductive system. Lakoff (1973), another influential degree-theoretic account of vagueness, pursues the same tradition, but taking additional inspiration from the work of psychologist E. Rosch on typicality ratings to introduce membership degrees.

[29] Łukasiewicz's infinite-valued logic was proposed in 1922 as a generalization of his three-valued logic. About the infinite-valued logic, Machina (1976: 191) writes: "I know of no place in which Łukasiewicz himself indicated any interest in thinking of his logic as a logic of vagueness. In fact he seems to have had a quite different interpretation in mind – an interpretation of the values as probabilities." Possibility was in fact the driving notion for Łukasiewicz, although inspection of Łukasiewicz's work reveals several connections between probability and many-valuedness, starting with Łukasiewicz (1913), where "the truth value of an indefinite proposition" is defined as "the ratio between the number of values of the variables for which the proposition yields true judgments and the total number of values of the variable." About his infinite-valued system, Łukasiewicz (1930: 173) writes: "it would be most natural to suppose (as in the theory of probabilities) that there are infinitely many degrees of possibility, which leads to the infinite-valued calculus." However, Łukasiewicz presents as an open issue the specification of the exact link of his infinite-valued calculus with probability theory, and points out in the same text (fn. 22) that his 1913's account of probability does not rest on the logical notion of many-valuedness introduced in his posterior work.

6 Error theory and the Gaussian law

We have seen that Borel, in his discussion of lexical vagueness, outlines a frequentist solution to the question whether an object belongs to a category, namely by assuming that the answer is a probability coefficient determined by the percentage of Yes answers in a given population. This solution is not particularly illuminating, however, for it gives no indication of how individual decisions are taken concerning category membership. The core of Borel's approach is given by the next section of his paper in which he analyzes a problem of individual categorization by means of the probabilistic theory of errors.

The main example discussed by Borel in that section is that of a publisher's employee in charge of the acceptance or rejection of manuscripts depending on the number of characters they contain. The structure of the example is particularly interesting, for this time, Borel assumes that there are precise limits of acceptance in principle, namely that the rule is to accept all and only manuscripts that have between 250,001 and 300,000 characters. In principle, therefore, the graph of the decision function for manuscripts should look as on top of Fig. 1, with three discontinuous parts. Assume that this graph represents the percentage of manuscripts accepted for each number of characters. This would correspond to a case of perfect discrimination, where 100 percent of manuscripts having exactly between 250,001 and 300,000 characters are accepted, and 0 percent of manuscripts outside these boundaries are accepted. However, the point of Borel's example is that the actual pattern of acceptance and rejection is not adequately described by a discontinuous function such as this one. Rather, the actual function is a smoother function describing the probability for a manuscript to be accepted, as a function of the number of characters. The bottom graph of Figure 1 gives a representation of the sort of function intended by Borel (as computed from what Borel calls the law of deviation, see below).

While Borel chooses not to give mathematical details, he makes explicit reference to what he calls the law of deviations ('loi des écarts'). To understand exactly what Borel intends, it is therefore necessary to figure out what this 'law of deviations' refers to (the name is no longer standard) and how it helps model a situation where a certain quantity is measured with error. The model Borel has in mind relates to the probabilistic theory of errors investigated by Laplace and Gauss, and on which several probability theorists will continue to work after Borel: the situation described by Borel is the situation of someone who has to measure a certain quantity with a probability of errors for each trial, on the assumption that the probabilities of positive and negative errors are equal. This situation was first imagined by Gauss himself in his theory of *accidental* errors of measurement.[30]

The expression 'loi des écarts', which has fallen out of use and which appears rather idiosyncractic to Borel, makes a quick appearance in Galton (1892), who mentions the "very curious theoretical law of "deviation from an average"" (1892: 22) and refers to Quételet (1849) in that passage. Borel himself uses the expression in his 1909 course *Éléments de la*

[30] See Stiegler (1986) and Fisher (2010) for the mathematical and historical details of Laplace's and Gauss's works.

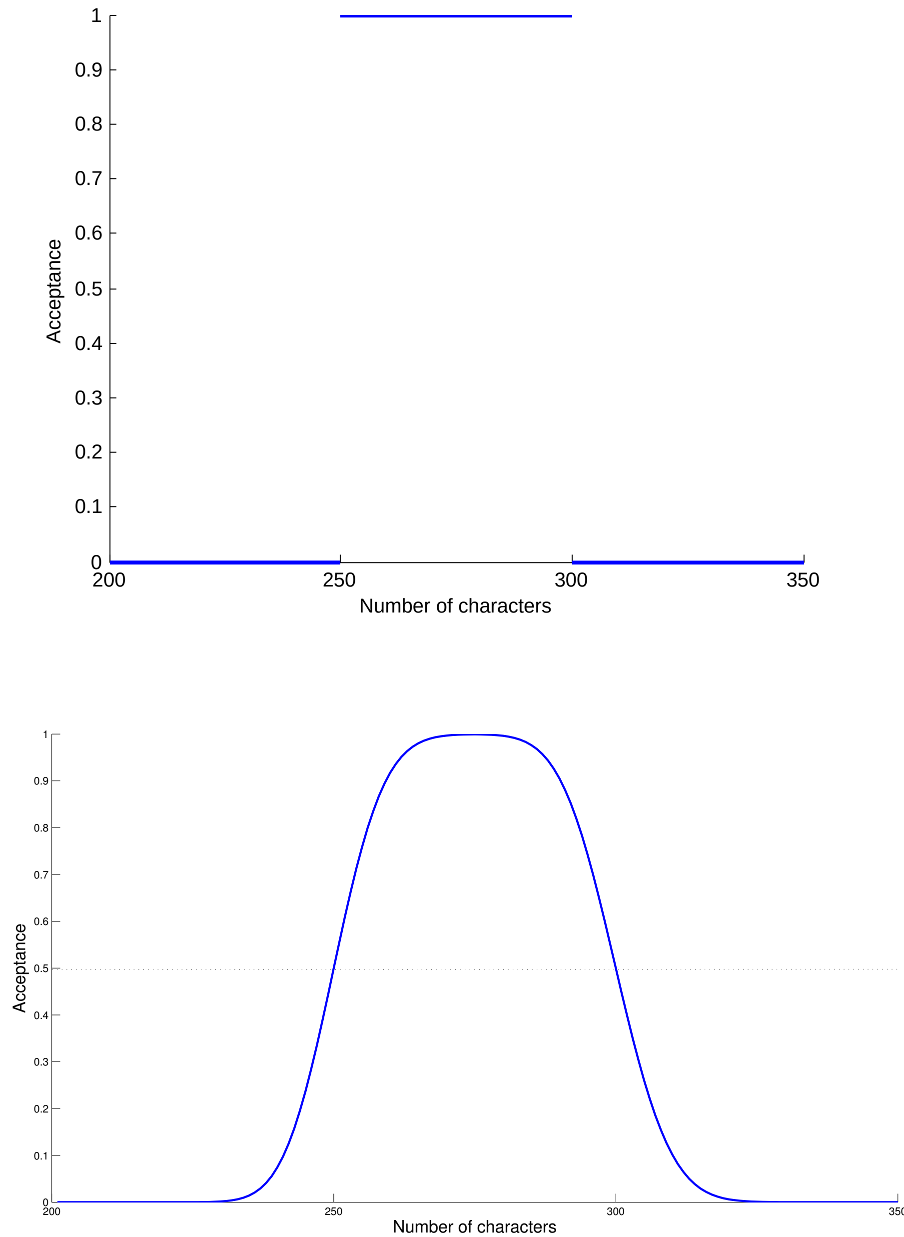

Figure 1: Ideal acceptance (top) vs. gradual acceptance (bottom)

théorie des probabilités, and again in his 1923 book, coauthored with R. Deltheil, entitled, *Probabilités, Erreurs.* It still appears in the 1954 edition of the book, a modernized reedition coauthored with R. Huron, where it is called "loi normale des écarts" (§32). The expression "loi des écarts" is another name for the the Gaussian law by which the probability of deviation from the expected value of a random variable can be computed.[31] In the case of a repeated Bernoulli trial, the "loi des écarts" gives the probability that the actual number of success deviates within certain limits from the expected number of success. When the number of trials is large enough, this probability is given with reasonable approximation by the Gaussian or normal distribution.[32]

As it turns out, the law of deviations described by Borel is a particular case of what is now known as the 'Central Limit Theorem', which was proved in full generality by Lindeberg and Lévy around 1920, following work by Lyapounov.[33] In the case of Lévy, it was directly in relation to the theory of errors, as testified by the number of articles written by the latter between 1922 and 1931 on the subject.[34] Because Borel chooses not to give the mathematical details of his analysis of the manuscript example, it will be useful to give an indication of how his main claim could be derived from the Central Limit Theorem.

Borel in his paper asks about the probability for a manuscript of 24999, 250000, or 250001 characters to be accepted. His answer is that there is one chance in two for the

[31]See Borel & Deltheil (1923), §18, p. 39: "On donne souvent le nom de *loi de Gauss* à la loi des écarts que nous venons d'exprimer" (One often gives the name of Gaussian Law to the law of deviations we just stated).

[32]The expression "déviation" or "écart" used by Borel as early as 1909 is not exactly what is currently known as the "standard deviation" (a term first introduced by Pearson, see Pearson 1896, and Stiegler 1986: 328), though it is closely related. The most complete reference on the notion of "deviation" and its use by Borel is certainly Borel, Deltheil and Huron's 1954 revised edition of Borel and Deltheil 1923, itself a revised edition of Borel 1909. BDH compare several notions of deviations and introduce the notion of "écart-type" (standard deviation) that was absent from earlier editions. The basic expression "écart" is used by Borel 1909 and until BDH 1954 to denote $k - np$ where k is the number of favorable outcomes of a Bernoulli trial with probability p. Thus, for n trials, the expression "écart" refers to the deviation of successes from the mean. They use the term "écart réduit" (reduced deviation) for $\lambda = \frac{k-np}{\sqrt{npq}}$, that is the ratio of "écart" to "écart-type" or standard deviation. As BDH point out on §38, Borel and Deltheil (1923) define the notion of "écart réduit" as $\mu = \frac{k-np}{\sqrt{2npq}}$ (there they mention the name "écart étalon" for $\sqrt{2npq}$). The version BDH give for the "law of deviations" is that the probability for λ to be between a and b is $\frac{1}{\sqrt{2\pi}} \int_a^b e^{-\frac{x^2}{2}} dx$. BDH on §39:77 introduce a notion of "écart probable ou écart moyen", which is defined as the mathematical expectation of the absolute value of the deviation. Borel and Deltheil 1923: 41 have a notion of "écart quadratique moyen", defined as "la valeur dont le carré est égal à la moyenne du carré de l'écart" (the value whose square is equal to the mean of the square of the deviation). This corresponds to the notion of standard deviation.

[33]We refer the reader to LeCam (1986) and Fischer (2010) for a detailed history of the central limit theorem.

[34]See *Oeuvres complètes* de P. Lévy, vol. 3. In particular: "Le Théorème fondamental de la théorie des erreurs", p. 71-83. Lévy presents interesting reflections on different ways to state and establish the law of large numbers in chap. 5 of the second part of his treatise *Calcul des probabilités*, which includes the now called central limit theorem. His remarks suggest that Borel had obtained the theorem only by analytic methods and under certain restrictions. Cf. p. 250.

manuscript to be accepted in that case, because for the employee "the chances that he makes a mistake of a few units more are equal to the chances that he makes a mistake of a few units less". One way to make the paradigm followed by Borel explicit is to imagine that each character of the manuscript has a given probability of being counted for 1, for ± 1, ± 2, and so on. In the simplest case, we may assume that the employee has an equal probability ε of either omitting a character or of miscounting it for 2 instead of 1. To each character i there corresponds a random variable X_i, such that $P(X_i = 0) = p\,(X_i = 2) = \varepsilon$, and $P(X_i = 1) = 1 - 2\varepsilon$. The problem put by Borel is the following: given a manuscript of n characters, what is the probability of its being accepted, given the fact that the employee is fallible? The probability sought is thus: $P(250.001 \leq \sum_{i=0}^{n} X_i \leq 300.000)$.

This random variable has a finite expectation $\mu = 1$, and a variance $\sigma^2 = 2\varepsilon$. The central limit theorem states that:

$$P(n\mu + \alpha \cdot \sigma\sqrt{n} < \sum_{i=0}^{n} X_i < n\mu + \beta \cdot \sigma\sqrt{n}) \xrightarrow[n \to \infty]{} \Phi(\beta) - \Phi(\alpha)$$

where $\Phi(x)$ is the cumulative Gaussian distribution.

Because the expectation is equal to 1, it can be seen on those grounds that the probability of a manuscript of 250001 characters to be accepted is indeed very close to $\frac{1}{2}$. Given the very small variance, the probability for a manuscript of $n = 249000$ characters to be accepted is of about 3 in a million if we suppose that the probability ε of errors is $\frac{1}{10}$. The results informally given by Borel in the text actually correspond to a much flatter distribution (Borel assumes that 450 out of 1000 manuscripts of 249000 characters should be accepted), but the structure of the model is exactly the same. On the other hand, it may be checked that the probability for a manuscript of say 275000 characters to be accepted will be practically equal to 1 in this case, which means that the chances of the number of characters to be miscounted below 250000 or above 300000 are negligible when the probability of correctly counting each character is as high as 0.8.

The bottom plot of Figure 1 was generated by applying the Central Limit Theorem, but assuming the number of characters to range between 200 and 350. That is, for the same random variable with mean 1 and variance 1/5, and for each number of characters between 200 and 350, we computed the probability that the number of estimated characters should be comprised between 250 and 300, using the Gaussian approximation. For 249, the computation yields a probability of acceptance of about 40%, much closer to Borel's actual example. Note that the acceptance function, although built using the cumulative normal distribution, is not itself a Gaussian density function, despite the bell-shaped appearance of Figure 1. As the variance decreases, and as precision increases, the range of values with acceptance nearly equal to 1 makes a wider plateau, and the decrease to 0 gets much steeper on either side around 250 and 300 (see Figure 2, where variance is decreased by a factor of 10). In that sense, the top plot on Figure 1 (ideal or precise acceptance) may really be seen as a limiting case of the bottom one (gradual and imprecise acceptance).

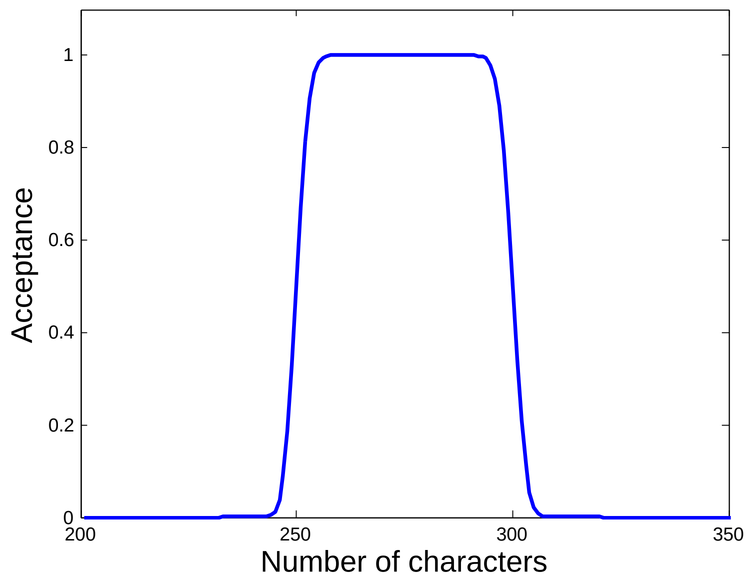

Figure 2: Increasing Precision ($\mu = 1$, $\sigma = \frac{1}{50}$)

To give a further illustration of Borel's example, Figure 3 gives the histogram of a particular numerical simulation representing 1000 blocks of 25000 samplings of the random variable above specified.[35] This can be taken to represent 1000 measurements of a manuscript of 25000 characters, made with the same error parameters as above (mean equal to 1 and variance to 1/5). As can be seen from the histogram, the distribution of manuscripts in this case is roughly centered around the expected value of 25000 characters. More precisely, in this case, the result of the simulation was that 496 out of 1000 manuscripts were estimated to have at least 25000 characters – and no manuscript was found to have less than 24500 or more than 25400 characters. Assuming the boundaries for of acceptance of a manuscript to be between 25000 and 30000, this therefore is a case in which the rate of acceptance is only slightly below a half for a manuscript of 25000 characters, but still close enough, in agreement with Borel's account.

7 Granularity and the economic paradox

Borel's analysis of the Heap culminates with an application to economic theory, and more specifically to a foundational issue in the theory of price variations. Although this application is probably the least expected (even in Borel's time), Borel describes it as "the most

[35]The simulation was run with Matlab. We used 25000 instead of 250000 samplings for reasons of memory limitation.

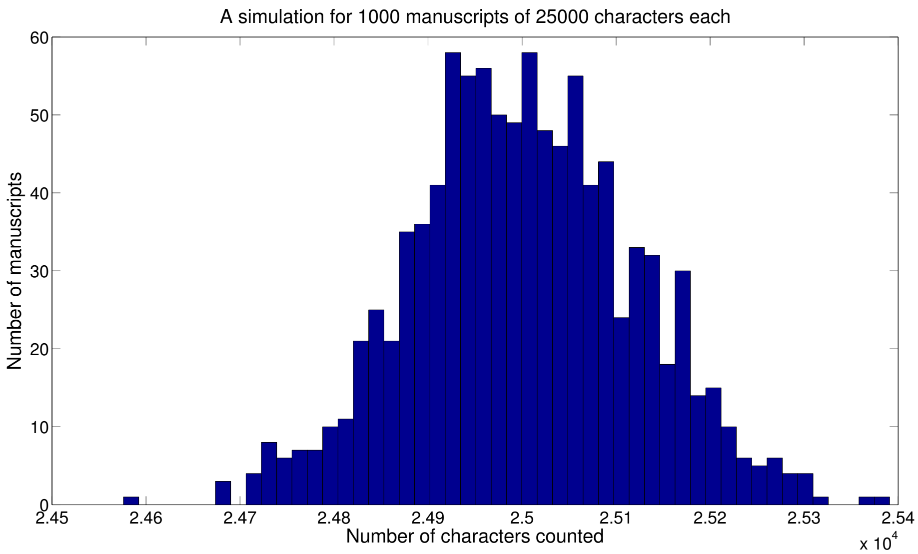

Figure 3: A simulation based on Borel's example

frequent and important case" in his paper. Borel's strategy in that section is the same we saw at work in other sections: viewed abstractly, his aim is to refute a misconception about the law of variation relating distinct scales.

The first two categories of problems considered by Borel indeed concern the relation between what today we would call a *nominal* scale (Yes vs. No, Accept vs. Reject) on the one hand, and a *ratio* scale on the other, that is a scale with fixed ratios between differences and a zero point (viz. the numerical scale for the number of characters in a manuscript; or the scale of measurement for ages in years; or the metric scale for sizes).[36] For those two categories of problem, we may summarize Borel's argument by saying that the law describing the distribution of items on the nominal scale, as a function of their properties along the ratio scale, is a probabilistic law.

A major originality of the economic example is that Borel generalizes his account to the relation between two ratio scales with distinct unit intervals. Thus, in the case of wholesale price, the unit interval is half a cent. For retail price, the unit interval is five cents. As in the previous case, however, the problem in this case consists in the fact that the scale for retail price is more coarse-grained than the scale for wholesale price. The two scales, in other words, have different granularities, so that the effect of a change of one unit on one scale over the over is unobvious. Accordingly, the economic paradox can be seen as providing a generalization of the problem of tolerance as phrased by Wright (1976), which

[36]On the definition and properties of measurement scales, see Stevens (1946). Stevens distinguishes between two kinds of nominal scales, Type A and Type B scales. As a typical example of a Type A scale, he gives the numbering of football players in a team. He describes a Type B scale as one in which "each member of a class is assigned the same numeral". What we describe as a Yes vs. No scale is a type B nominal scale in Stevens' sense.

Figure 4: The model of price variations refuted by Borel

can be presented abstractly as the problem of determining how variation of a parameter along an interval or ratio scale will affect variation along a Yes-No nominal scale.

The basic misconception refuted by Borel in this generalization remains the illusion of a fixed boundary, namely the idea that in order to see a diminution of 5 cents in retail price, one would need *exactly* 10 consecutive diminutions of half a cent in wholesale price. In Figure 4, we represented the associated function for the intended relation between increases (equivalently decreases) in wholesale price and increases (equivalently decreases) in retail price. As on top of Figure 1, the resulting function is a staircase function. The meaning of such a function is that a switch from 9 to 10 along wholesale price is assumed to be pivotal for retail price, whereas any shift between 0 and 9 is assumed not to make any difference.

The implausibility of this model is that it implies that some positions along one scale are privileged to make a difference along the other scale, contrary to observation. Borel however admits that if wholesale price were to diminish constantly by half a cent every week over a year, wholesale price would decrease by 26 cents over a year, which would have a sufficiently large effect on retail price to occasion a theoretical diminution of 25 cents on retail price. Thus, Borel admits that *on average*, 10 diminutions of half a cent on retail price will effect a diminution of 5 cents on retail price. Although the *global* dependence between retail price and wholesale price is assumed to be *linear*, the upshot of the different granularities between the two scales is that the *local* effect of a shift in wholesale price on a shift in retail price can only be described probabilistically, by saying that each shift of 1/2 cent in wholesale price has only a probability of 1/10 to occasion a shift of 5 cents in retail price.[37]

Three further points should be stressed about this example. The first is that the economic version of the paradox corresponds to a dynamic version of the sorites (often called a *forced-march* sorites since Horgan 1994). In contrast to the previous examples, the point of the discussion is to describe the effect of a process of step by step progression along the value of the parameter (this is not so, for example, in the manuscript case). The second point is that Borel's solution accepts that there will indeed be a category shift in a such a process, but the shifting point can only be determined probabilistically, it has no privileged position. This example further substantiates Borel's initial claim that it is absurd to assume that there is a determinate position for cutoffs (what Smith 2008 calls the *location problem* for vagueness). The third point is that it would be a mistake to think that solving the sorites paradox for Borel essentially comes down to substituting a continuous function for a discontinuous function. If that were the case, it would suffice him to postulate that the covariation between wholesale price and retail price is described by a linear real valued function (or by a quadratic function, or by whatever continuous function over the reals). It would be a misunderstanding to envisage Borel's theory in this way. This would not solve the problem, moreover, precisely because the scales are assumed to be discrete in this case.

8 An assessment of Borel's account

Having surveyed the main themes of Borel's paper, we propose a general discussion of the benefits and limitations of Borel's account with regard to what are considered the main *explananda* of a theory of vagueness. We start by a discussion of two of those explananda, namely the prospect of unifying the main symptoms of vagueness, and the problem of higher-order vagueness. In the remaining subsections we focus on the ways in which Borel's account of practical vagueness as imprecision can be articulated with the phenomenon of lexical vagueness, namely with the problem of providing truth-conditions for vague expressions.

8.1 The symptoms of vagueness

Today, vague predicates are standardly characterized by three main 'symptoms', namely as predicates that are sorites-susceptible, that have borderline cases, and that have blurry boundaries (see Keefe 2000, Smith 2008, Egré and Klinedinst 2011). Although Borel does not tease apart these notions explicitly, all three are represented in the text, and probably the main virtue of his account is to allow a unified treatment in statistical terms.

[37]As a consequence, the decrease of retail price can be viewed as a binomial random variable taking two values, either 5 or 0, with respective probabilities $\frac{1}{10}$ and $\frac{9}{10}$. For 52 trials, one for each week, the expectation of this random variable is 26, which is the expected decrease on retail price for a constant weekly diminution of half a cent over one year.

We saw that sorites-susceptibility is implicitly characterized in terms of the principle of *probabilistic closeness* discussed above in section 5 – that is, adapting Smith's terminology, as the principle that slight modifications of the P-relevant respects of an object are matched by slight modifications of the probabilities of their inclusion under the category P. Similarly, the phenomenon of blurry boundaries, usually presented as the lack of a sharp transition between a category and its negation is captured by the *smoothness* of the probabilistic acceptance functions attached to vague predicates, such as pictured in Figures 1 (bottom) and 2.

As emphasized throughout our discussion, probabilistic closeness, like the smoothness of acceptance functions, both concern the statistical dependency between the characteristics of an object and its inclusion under a category. This dependency is a global feature, representing how a subject (or population of subjects) would behave over a class of similar situations. At this global level, Borel's approach avoids both the *location problem* and the *jolt problem* that Smith identifies for most theories of vagueness: that is, given a smooth acceptance curve, there is no privileged position for a category switch, and there is no "sudden leap" either. But at a local level, namely when looking at how subjects individually deal with category membership along a given sorites sequence, Borel's view can accommodate the fact that the subjects' acceptance functions are necessarily jolty, with identifiable switches along the sequence. The strength of the statistical view is obviously to reconcile these two antagonistic aspects of categorizing behavior (located and jolty on one trial, uncertain and and smooth when averaged over several trials).

The notion of a borderline case finally, what Borel calls "un cas douteux" or "un cas intermédiaire", receives two characterizations in the text. It is initially presented as a case for which yes-no judgments between subjects will disagree, so indirectly, as one for which the probability of inclusion (as represented by the frequency of judgments) is strictly between 0 and 1 (compare again with the supervaluationist picture of borderline cases). In the second part of the text, Borel's approach suggests a more precise definition, namely that a borderline case of an acceptable manuscript, for example, is one for which the probability of acceptance (for a single subject) is sufficiently close to .5. In that particular case, a borderline case of an acceptable manuscript is one that has roughly equal chances of being accepted or rejected. The interest of Borel's account, however, is that this probability is *derived* in his model, for a borderline case is initially presented in his discussion as a case that lies near enough the acceptance threshold that is relevant to the employee's categorization. It remains a contingent fact, however, dependent on the hypotheses of Borel's model (the value of the threshold of thresholds, the shape of the error function), that a manuscript of n characters, with n as the acceptance threshold, should have a probability of acceptance of 1/2. For example, we may consider a trivial case, in which the error of miscounting a character for 0 or 2 is 1/10 in each case, but such that only manuscripts of 2 or 3 characters are accepted. In that case the probability of acceptance of a manuscript of exactly 2 characters is 82/100. Thus, there is no necessary connection between the characterization of a borderline case as one that lies near some acceptance threshold and the probability 1/2 of acceptance, even though the use of large numbers makes this probability salient.

8.2 Higher-order vagueness

One aspect of the phenomenon of blurry boundaries pertains to the problem of higher-order vagueness, namely the difficulty of drawing sharp boundaries between borderline cases and clear cases of application of a predicate. This problem, which is discussed and presented in Russell (1923), is not countenanced by Borel.

One reason, arguably, could be that the use of probability is a way of cutting the problem at the root. For example Hampton (2007), who uses typical sigmoid psychometric functions, as generated by cumulative Gaussian distributions, to deal with vagueness, writes:

> "there are no discontinuities in the function, so we can neatly avoid the problem of second order vagueness associated with dividing the scale into clear and borderline regions. The function is continuously graded, and simply asymptotes at 1 or 0."

This asymptotic behavior is a potential difference with degree-theoretic accounts of vagueness, often accused of drawing a line between the degree 1 and degrees less than 1 (see Williamson 1994).

It is unclear, however, whether the introduction of probabilities is by itself sufficient to solve the higher-order vagueness problem. In one important sense, higher-order vagueness concerns the semantic relation between the vague category P and the vague category "clearly P". Borel's account is simply silent on this issue. More generally, whether this particularly theory succeeds at solving the higher-order vagueness problem will depend on the extent to which it can get rid of any notion of hidden semantic boundaries. As we saw, however, the manuscript example rests on some underlying notion of boundary, and what should be clarified is the status of those predefined boundaries.

8.3 Two kinds of uncertainty

The main objection that can be made to Borel's account concerns the hypothesis that category membership is relative to predetermined boundaries along a scale of measurement. If all vague categories could be modeled along the lines of Borel's model of acceptance and rejection for the manuscript, this would imply that all vague categories come with underlying criteria of acceptance that are sharp. However, if we think of categories like "heap", "house", or "child", the objection that comes to mind is that there seems to be nothing like a predetermined and precise criterion of acceptance and rejection as assumed in the manuscript case.

More generally, there are two kinds of uncertainty in relation to vagueness that Borel does not distinguish. One concerns the *uncertainty regarding the stimulus*, the other concerns the *uncertainty regarding the position of the boundary.* The manuscript case is an instance of uncertainty regarding the value of a stimulus, but for which the location of the boundaries is known. This kind of uncertainty is the hallmark of *imprecision* about the stimulus. In a wide range of cases, however, there is no uncertainty about the stimulus even though there remains an uncertainty about where to put the boundary for acceptance or rejection. For example, we may know someone's age very precisely, and remain uncertain whether to count the person as old or not. These two uncertainties appear to be of very different kinds. The latter, in particular, appears to correspond to what Borel calls the "necessary indeterminacy of verbal definitions", namely to the fact that, even as all the information about an object is known with precision, some uncertainty remains concerning the category proper.

The difference between the two kinds of uncertainty is briefly pointed out by Borel in the text, however, when he writes:

> "In other cases, it can happen that one does not set oneself a rule *a priori*, but that one deduces the mean rule from the facts themselves. For example, when one wants to buy a piece of furniture, one does not always have an absolutely fixed idea of the dimensions that one desires; however, there are some models that one will certainly rule out as too small, and some others that one will certainly rule out as too big. In fact, if one sees many models and notes those whose dimensions seem acceptable, one will observe that these dimensions lie around a certain mean value, according to the aforementioned laws."[38]

What Borel suggests is that even when no definite rule is set in advance for inclusion in a category, categorization happens *as if* there were such a rule implicitly.[39] Unfortunately, Borel does not work out the consequences of this view so as to make a link with his initial discussion of vague lexical categories.

[38] "Dans d'autres cas, il arrivera que l'on ne se fixe pas de règle *a priori*, mais que l'on déduise la règle moyenne des faits eux-mêmes. Par exemple, lorsque l'on veut acheter un meuble, on n'a pas toujours une idée absolument arrêtée des dimensions que l'on désire; cependant, il est certains modèles que l'on écartera sûrement comme trop petits, et certains que l'on écartera sûrement comme trop grands. En fait, si l'on voit beaucoup de modèles et que l'on note ceux dont les dimensions paraissent acceptables, on constatera que ces dimensions se répartissent autour d'une certaine moyenne, suivant des lois déjà rappelées."

[39] This kind of 'as if' arguments occurs at several points in Borel's analysis. For instance, when Borel, in section 52 of *Le Hasard*, discusses the status or normal distributions in nature, he takes inspiration from Quételet's example of the average man to write:

> "Generally speaking, one can say that measured sizes satisfy the same laws as measurement errors; everything happens as if the same man, whose size were equal to the mean, had been measured a large number of times by quite awkward observers or only having very imperfect measurement instruments at their disposal."

To clarify this issue, let us think of the way in which Borel's error model, which is used to deal with cases of practical vagueness, might be generalized to account for typical cases of lexical vagueness. Borel's treatment of a predicate like "acceptable (for a manuscript)" makes use of essentially two parameters: the so-called rule of choice, or *value of a criterion*, namely of a threshold or predefined boundary along a scale of measurement, and the *precision* with which each relevant unit is represented on that scale.[40] The same two parameters can be used to deal with more standard examples of vague linguistic expressions.

Consider a predicate like "tall". Contrary to "acceptable for a manuscript", which specifies a closed interval with a lower bound and upper bound for acceptance, "tall" appears to involve only one criterion value, namely the height such that being above that height would count as being tall, and such that being below it would count as not being tall.[41] The second parameter concerns the imprecision regarding the estimation of each unit i, namely the various probabilities of under- or over-estimating it (representable by a random variable X_i). Hence, the probability of judging "x is tall", for someone whose actual height is n units, would be given by: $P(\sum_i^n X_i \geq \alpha)$, with n as x's actual height, and α as the criterion value.

There are several limitations to such an account. First of all, it would only apply to predicates for which an interval scale of measurement is available (a vague word like "house" is obviously harder to regiment in that way); but arguably, this is a problem for all theories, as soon as several dimensions of comparison need to be integrated. More fundamentally, this model only represents uncertainty about the stimulus, but not the uncertainty concerning the criterion. What can Borel's account offer about the uncertainty that affects the criterion?

As we see it, Borel would agree that the value of this criterion, just like the precision that concerns the stimulus, is subject-relative and interest-relative.[42] Under the postulation of such a threshold, it is entirely consistent with Borel's "as if" argument to assume that individuals do not have access to their own criterion. The opening of Borel's text about the value of testimonies indicates that when we issue judgments involving vague expressions, we are moreover uncertain about other people's use of the same expressions, hence uncertain about where they would in turn put their criterion for inclusion or exclusion relative to a category. This uncertainty about each other's "rule of choice" is obviously an essential component to linguistic vagueness, and it too is susceptible of a probabilistic treatment (see in particular Frazee and Beaver 2010 and Lassiter 2011), but such a treatment goes beyond what Borel's considerations can offer in the text. In that sense, it is fair to say that

[40] We use the terms "criterion" and "threshold" interchangeably in the current context, and irrespective of their distinct meanings in modern psychophysics (viz. McNicol 1972). The notion of "criterion" is convenient to convey the idea of a subject-relative acceptance/rejection value.

[41] Compare with the standard semantic treatment of gradable adjectives, where "tall" means "significantly taller than s", with s some standard value, as in Fara (2000).

[42] In particular Borel's overbrief account of lexical vagueness seems to us to fit with most of Fara's remarks about the interest-relativity of vague expressions.

the uncertainty regarding the criterion remains a missing component of the account.[43]

8.4 Semantics or pragmatics?

Borel's account of vagueness in terms of error and imprecision raises a more fundamental problem, namely whether his theory has anything to say about the truth conditions of vague expressions, or whether it is to be seen essentially as a pragmatic theory of judgment and categorizing behavior. Consider the epistemicist perspective on vagueness. Couldn't an epistemicist agree with all of Borel's remarks, but argue as follows: *although, from a statistical point of view, our judging behavior shows no sharp cutoff point as to where we draw the boundary of vague expressions, the truth-conditions of vague expressions require that there be such a sharp cutoff point.*

For example, in the manuscript example, there are *de jure* boundaries for acceptability, they determine sharp cutoff points, even though in practice the employee errs around those values. Shouldn't there be a similar *de jure* cutoff point for "tall", for example, or for "thin"? Or should we say that anything goes, and that a speaker is free to set the boundary anywhere he or she pleases?

Prima facie, Borel's remarks may suggest some sympathy for the latter position. His considerations on the importance of testimonies for verbal definitions indicate that he agrees that meaning supervenes on use, so that for Borel, the judgments involving an expression are constitutive of its meaning. But the epistemicist too insists that meaning supervenes on use (see Williamson 1994). For Williamson, this supervenience thesis is not incompatible with the idea that at any moment in time, the use of a vague expression by the community determines a sharp but unknowable meaning.

There have been attempts to provide a statistical definition of the notion of unknowable but sharp cutoff. Benovsky (2011) for instance proposes that the cutoff for a vague expression is determined by some way of computing the average of all sharp delineations for that expression. For a work like 'heap', a delineation above the mean value would be correct, one below it would be incorrect. Even though Williamson himself (1992: 155) appears to be skeptical of that way of articulating the supervenience of meaning on use,[44] one may still wonder whether Borel would not endorse a view along those lines.

Sure enough, Borel agrees that statistical surveys are needed to map out the meaning of an expression, but throughout his text he maintains a more neutral stance about the

[43] As pointed out above (see fn. 23), Borel's 1907 use of probability rests almost exclusively on frequentist considerations. Only later did Borel express an inclination toward the Bayesian definition of probability. See for instance his review of Keynes in Borel (1924), which Savage's 1972 bibliographical supplement to Savage (1954) presents as "the earliest account of the modern concept of personal probability known to me".

[44] "If one sticks to actual speakers of English, there is no prospect of reducing the truth conditions of vague sentences to the statistics of assent and dissent, whether or not one accepts the epistemic view of vagueness." Williamson considers that the supervenience relation is rather one on which our use of a vague expression tracks a sharp property.

notions of correct and incorrect uses. We believe that Borel too would view as too crude a reduction of "the correct meaning" of a vague expression to a sharp range of particular uses. Borel does regard as important what he calls "the majority opinion" (*l'opinion de la majorité*) in order to settle semantic debates, but he also draws attention to the conventional character and fragility of statistical thresholds.[45]

As we see it, the general direction of Borel's account is rather antagonistic to the epistemic view, simply because the idea of a unique privileged cutoff point seems to go against the whole spirit of his account: as explained, we find a closer affinity with supervaluationism, namely with the idea of a plurality of cutoffs compatible with the meaning of an expression (see Fine 1975). More fundamentally, the underlying boundaries used by Borel in his error model are better viewed as individual decision criteria than as hidden semantic boundaries. This does not mean that Borel would accept the theory on which any judgment regarding category membership for a vague expression can count as correct. 'Correct', however, as applied to language use, is itself a vague and interest-relative expression, and any precise definition is a matter of convention (in Poincaré's sense, as endorsed by Borel).[46] Although Borel gives no indication on how individual decision criteria (the α value in our example above) are selected and constrained, it is reasonable to suppose that, for cases in which linguistic communication between individuals matters and for which no explicit convention is enforced yet, the position of an agent's criterion for a vague expression (such as "tall") depends in part on where he expects the others to put their own criterion. When applying a word like "tall", we are likely estimating the expected cutoff value for tallness based on the common use. But this is not the same thing as saying that a correct use of "tall" would be one for which one's criterion must lie exactly above some definite mean value. A correct use of "tall" may simply be one for which the position of one's criterion is sufficiently close to someone else's criterion as to prevent miscommunication. Consequently, we are tempted to conclude that a Borelian account of meaning, if it were to be developed, would definitely be more along the lines of a pragmatic theory of coordination and should see the notion of a fixed semantic boundary as an idealization.[47]

[45]See in particular Borel (1950:100), probably the most explicit text on this element of arbitrariness; see also Borel 1914: 263-62: "If, out of 1000 trials, 520 declare A heavier and 480 B heavier (I am supposing that dubious answers are not included), the deviation observed is among those that would happen frequently in a series of 1000 answers drawn from a toin coss; one must not conclude anything, except a strong presumption that a new series of 1000 trials will produce an outcome just as uncertain" (Si, sur 1000 expériences, 520 déclarent A plus lourd et 480 B plus lourd (je suppose qu'on laisse de côté les réponses douteuses), l'écart observé est de ceux qui se produiraient fréquemment dans une série de 1000 réponses tirées à pile ou face; on ne doit donc rien en conclure, sinon une forte présomption pour qu'une nouvelle série de 1000 expériences fournisse une résultat aussi incertain)". On the value of opinion polls, and their application in psychophysics, see the Appendix.

[46]See Borel's own emphasis in the text on talk of probabilities as "a reasonable convention".

[47]On vagueness and coordination, see in particular Parikh 1994 and Lipman 2009.

9 Conclusion: legacy and perspectives

As said at the outset of this paper, Borel's note on the heap paradox has gone unheeded since its original publication. Because of that, the impact it could have had on later developments of either the logic or the psychology of vagueness is virtually non-existent. Even in the area of fuzzy logic, where Borel's influence might have been the most expected, we have seen that Borel's work has remained out of the mainstream.

Unsurprisingly, the idea that vagueness should be approached with a statistical perspective in mind has been proposed independently a number of times since 1907, starting with Black's work, and including more recently with attempts to establish a closer connection between lexical meaning and either probability or statistics (Lassiter 2011, Solt 2011), to enrich degree-based logics (Edgington 1997, Schiffer 2003, Smith 2008, MacFarlane 2010), supervaluationist accounts of vagueness (Simons 2010), or more generally to connect vagueness with imperfect discrimination (Hardin 1988, Egré and Bonnay 2010, Fults 2011), ambiguity (Egré 2011b) or stochastic behavior (Franke et al. 2010). In comparison to those accounts, however, a distinctive feature of Borel's is undeniably the use of a statistical law, namely a limit theorem of the probability calculus, to model categorization behavior.[48]

In the area of psychology, probabilistic models of categorization and discrimination have been more widespread ever since the birth of psychophysics (see for example Luce 1959, McNicol 1972 and Hardin 1988 on models of discrimination, and Hampton 2007 on threshold models for categorization), but mostly because probability theory has established itself as an essential tool for more than a century now. At the time Borel was writing on the Heap, however, the idea that probability could be fruitfully applied to psychology was not as entrenched, even though it was acquiring momentum, in particular with the extraordinary development of psychophysics, and with the bridge drawn by scientists such as Quételet, Galton and Pearson between physics and the social sciences (all three authors being referred to in Borel 1914). Even in comparison to more recent accounts of vagueness that incorporate a probabilistic dimension, Borel's account remains highly original by his emphasis on error theory on the one hand, and on the weight put on individual decision-making on the other, including on psychological aspects of decision processes.[49]

From a philosophical point of view, we take the leading idea of Borel's account to be his resistance to absolute demarcations for vague concepts. Importantly, for Borel this does not mean that vague concepts are not to be assigned boundaries on particular

[48] Faulkner (2010) proposes a suggestive reconstruction of the status of vague boundaries for Wittgenstein, making use of the Law of Large Numbers to model boundary fluctuations around a mean value, but based mostly on passing remarks by Wittgenstein.

[49] Decision-making in a broad sense, since utilities are not part of Borel's picture. Note that several other remarks in Borel's text are likely to surprise readers well acquainted with some of the recent aspects of the literature on vagueness, such as his passing emphasis on the importance of round numbers, a topic that has also received a significant amount of attention in the recent literature in natural language semantics (see Hobbs 2000, Krifka 2007, Bastiaanse 2011).

occasions of use. Again, the gist of Borel's approach must be distinguished from the Bergsonian idea, advocated for example by Dawkins (2004) for biological categories (and to which van Deemter (2010) has called particular attention in relation to vagueness) that "lines are impositions of the discontinuous mind". Rather, Borel's idea is that our assignment of boundaries fluctuates both within and across individuals, and that this fluctuation is to be handled statistically. In particular we have argued that Borel's account puts him at odds with the epistemic view of vagueness, on which vague categories come with unknowable sharp lines. On the other hand Borel's model of vagueness is equally a model of imprecision, compatible with the idea that we treat sharp boundaries with some approximation. His error model, because of that, remains compatible with a stronger brand of semantic externalism, such as endorsed by the epistemicist, on which meaning boundaries are 'out there' to be estimated. In our view, however, a more fruitful way to take up Borel's perspective where he left it would be to keep the idea that vague lexical categories are in large part indeterminate, and to maintain that hidden semantic boundaries for vague concepts are simplifications and convenient idealizations. The task, however, would be to develop a fuller account of lexical vagueness, one maintaining a more internalist perspective, on which the *explanandum* would concern the relative positions of individual decision criteria in situations of communication.

Appendix: Psychophysics and the problem of nontransitivity

To a modern eye, Borel's emphasis on the law of errors in relation to problems of categorization is reminiscent of the importance of the same law of error in psychophysics for questions of discrimination. Psychophysics was still a young science in 1907, and Borel showed interest for the problem of psychophysical measurement. Two other essays of the same period testify that Borel saw a connection between the sorites paradox and the problem of nontransitivity in discrimination. The most explicit discussion of that connection, however, only appeared much later in Borel (1950), when Borel revisited the paradox of the Heap.

In 1909, Borel published a short paper entitled "Le continu mathématique et le continu physique", in which he responds to Poincaré's paper "Le continu mathématique" (Poincaré 1893). In that paper, Poincaré discusses the Fechnerian problem of the comparison between different sensations of weight (making explicit reference to Fechner). The puzzle discussed by Poincaré concerns the lack of transitivity in the indiscriminability of sensations. A weight A of 10g and a weight B of 11g can be felt to produce identical sensations, and so will the weight B and a weight C of 12g, however the weight A and the weight C will be felt to be distinct. Poincaré presents the relations $A = B$, $B = C$ and $A < C$ as a "the formula of the physical continuum". Based on it, Poincaré makes the hypothesis that the invention of the mathematical continuum might have been prompted by an effort to analyze away the "intolerable" contradiction resulting from that definition of the physical continuum.

A section of Borel's 1909 paper (reproduced in Borel 1922 under Note III) is entirely devoted to the discussion of this example, and part of its interest in the present context is that Borel establishes an explicit connection between this example and his 1907 paper on the Heap. Borel disagrees with Poincaré on the idea that the definition of the identity between physical magnitudes should lead to contradiction. He contends that intransitivities only result from imperfection in measurement. About physical measurement, Borel writes the following:

> "If we measure a magnitude by comparing it to a fixed standard, we obtain so many decimals certainly correct followed by a last decimal about which there is some uncertainty; different experimenters, equally careful, obtain different values for this last decimal, so that the observations can be summed up in such a form as the following: 30 per cent of observers find a 5, while 50 per cent found a 6 and 20 per cent found a 7; another magnitude very near to the first leads to results which are analogous, but not in general identical. The phenomenon is analogous to that which takes place in the *sophism of the heap of wheat*."[50]

[50] "Si l'on mesure une grandeur par comparaison avec un étalon fixe, l'on obtient un certain nombre de décimales sûrement exactes suivies d'une dernière décimale sur laquelle plane quelque incertitude; des expérimentateurs divers, également soigneux, obtiendront des valeurs diverses pour cette dernière décimale,

Borel's analogy with the heap paradox is very allusive in this passage, but what the last sentence suggests is that for him small differences in physical magnitude will necessarily imply corresponding statistical differences regarding the judgments based on them. The remark may be connected to the comment Borel makes in the last section of his 1907 paper regarding individual decisions to buy in relation to slight differences in price. Borel presents as a "psychological illusion" the idea that a slight difference in price makes no difference regarding the propensity to buy.

The rest of Borel's 1909 note adds some indications about the way to evade intransitivities in the measurement of physical magnitudes. Borel in particular makes the following addition in the same text and in a footnote:

> "the physical continuum differs from the mathematical continuum in that because experience only allows us to achieve a limited approximation, a certain minimum difference is necessary for us to be able to distinguish two very near elements.[Fn. 2. In making this observation, we should not be thought to fall prey to Mr. Poincaré's objection; the minimum difference is not an absolute constant, but depends on the experimental conditions and often also on the value or nature of the measured quantities. Consequently, two magnitudes A and B that direct measurement procedures would not allow us to distinguish can be differentiated if one has the good idea – or good luck – to compare them both to a suitably chosen magnitude C, that direct experience does not distinguish from B, but distinguishes from A; those new experiments diminish the *minimum separabile* for A and B. Another procedure for the diminution of this minimum is the repetition of experiments and the application of the probability calculus; this is a very interesting issue to which I will very likely have the occasion to return some day."][51]

In talking about the "miminum separabile" between two magnitudes, Borel points to the central question investigated by Fechner in his *Elements of Psychophysics*, namely the

de sorte que les observations se résumeront sous une forme telle que la suivante: 30 p. 100 des observateurs trouvent un 5, tandis que 50 p. 100 trouvent un 6 et 20 p. 100 trouvent un 7; une autre grandeur très voisine de la première conduira à des résultats analogues, mais non en général identiques. Il se passe là un phénomène analogue à celui qui se produit dans le *sophisme du tas de blé"*. (from Borel 1922, English edition, translated by A. Rappoport and J. Dougall, modified by us.

[51] "le continu physique se distingue du continu mathématique en ce que l'expérience ne permettant jamais d'atteindre qu'une approximation limitée, une certaine différence minimum est nécessaire pour l'on puisse discerner deux éléments très voisins. [Fn 2. Il ne faudrait pas croire que nous retombons, en faisant cette constatation, sous le coup de l'objection de M. Poincaré ; la différence minimum n'est pas une constante absolue, mais dépend des conditions expérimentales et souvent aussi de la valeur ou de la nature des quantités que l'on mesure. Par suite, deux grandeurs A et B que des procédés directs de mesure ne permettraient pas de distinguer, pourront être différenciées si on a l'heureuse idée – ou la chance – de les comparer toutes deux à une grandeur convenablement choisie C, que l'expérience directe ne distingue pas de B, mais distingue de A; ces nouvelles expériences diminuent pour A et B le *minimum separabile.* Un autre procédé pour la diminution de ce minimum est la répétition des expériences et l'application du calcul des probabilités ; c'est là une question fort intéressante sur laquelle j'aurai sans doute l'occasion de revenir un jour".]

problem of measuring the difference *threshold* or *limen* required between two magnitudes to produce a difference in sensation. Fechner himself, in his *Elements*, first made use of the probability calculus and of Gauss's law of errors to give a practical and theoretical answer to his problem.

From Borel's 1909 note, it is not possible to determine whether Borel had a clear notion of Fechner's contribution to psychophysics. However, a paper published in 1908 entitled 'Le calcul des probabilités et la méthode des majorités' in the psychology journal *L'Année psychologique* shows that Borel was well acquainted with Fechner's work, at least through the manual of experimental psychology of Titchener (1905), which Borel discusses to some length in that paper. The content of Borel's 1908b paper was later included as chapter 9 of *Le Hasard* (§99 to 107, with the addition of two introduction and conclusion paragraphs 98 and 108), entitled "La valeur scientifique des lois du hasard", where Borel discusses the applications of the probability calculus to several issues in psychology.

Borel's paper on the majority method is related to his paper on the heap paradox in two ways. First of all, Borel questions the value of opinion polls to establish facts. One example he discusses, with cross-reference to his paper on the heap, concerns the value of opinion surveys for the establishment of facts about language use. Secondly, a large section of that paper, corresponding to paragraph §105 in Borel 1914, is a discussion of the method Fechner defined as the method of right and wrong cases (also called the method of constant stimuli), specifically designed by Fechner to propose a probabilistic model for the measurement of the threshold.[52]

Fechner's method of constant stimuli is introduced and illustrated at paragraph 22 of Titchener's manual on the example of lifted weights, and Borel uses Titchener's figures (1905:107) to make his point.[53] Borel's use of Titchener's data is particularly striking, for it shows that Borel thought that an adaptation of the method of constant stimuli was indeed a correct way to get a reliable estimate of the "minimum separabile" between two magnitudes. In Titchener's experiment, the same subject is shown pairs of weights taken from a fixed set, each time with the same standard weight S present in the pair, and asked for each trial to issue a comparative judgment on the pair (which weight is felt heavier, or

[52] See Fechner 1860: 84:

> "In studying the theory of probability, to which my interest in the development of our methods drove me again and again, the following considerations occurred to me: (1) according to our procedure the measure of sensitivity for differences could be represented by the value usually designated by h, which, according to Gauss, affords a measure of the precision of observations, as long as precision depends only on the sensitivity for the perception of differences under comparable modes of procedures"

Interestingly, Fechner got the assistance of the German mathematician Möbius to define a rigorous probabilistic solution of the problem of discriminating the difference between two weights. See Stigler (1986) for a detailed presentation of Fechner's account and its relation to Gauss's law.

[53] Titchener's chapter 2 in that manual is entirely devoted to a presentation of the Law of Error and its relation to measurement in physics and psychophysics.

whether the two weights are felt equal).[54] The experiment thus provides, for each pair, the frequency of judgments of each kind ($C > S$, $C = S$, $C < S$). Based on those frequencies, the difference limen, or increase of weight d such that $S + d$ is felt as minimally distinct from S is determined by statistical methods.

The proposal made by Borel is to transpose this method to an analysis of collective judgments. That is, instead of considering 100 trials performed by the same observer, Borel proposes to consider each trial as made by a different observer. First, Borel explains how probabilities can help to determine, relative to the series of measurement, when the two weights C and S are equal, and then the first lower value in the series for which C is reliably felt as lighter than S. Basically, in that passage Borel illustrates how the binomial distribution can help to infer whether the difference between two means is statistically significant. For instance, in Titchener's table, a weight C of 1071g is felt lighter than the standard S of 1071g 33 percent of the time, and heavier 37 percent of the time, while a weight C of 1021g is felt lighter 53 percent of the time, and heavier 10 percent only. This points to a difference threshold of about 40g, a result confirmed independently by Titchener's calculations. Borel's main point, however, is to argue that, relative to a set of actual data like Titchener's, one can linearly interpolate data points between 1021 and 1071, so as to refine the estimate of the threshold by the same statistical method. He finds that a weight of 1051g would give 41 percent of 'lighter' judgments vs. 22 'heavier', still a reliable correct difference, thereby reducing the differential threshold to 20g.[55] The general conclusion Borel draws is that the majority method, applied to collective judgments, allows one to refine what he in 1909 calls the "minimum separabile" between two physical magnitudes.[56]

Borel's 1908 remarks on psychophysical measurement only cast light on the second of Borel's proposed responses to Poincaré, however, namely on the value of repeated measurements. They say nothing about his second suggestion, that is about the introduction of a third element to improve on the direct comparison between two elements. In paragraph 49 of Borel (1950), however, Borel eventually picked up the problem where he had left it.[57]

[54] In Titchener's setup, this includes cases for which the subjects is unsure either way.

[55] Borel in a footnote indicates what is now called the p-value for statistical significance – in that case a value of 0,024 – in that case the probability of finding the distribution in question in case the difference between the two groups were due to chance.

[56] Several aspects of Borel's 1908b paper would call for further commentary, but would take us too far afield. In particular, Borel makes several interesting comments on the use of statistical methods to establish differences. One comment he makes is that "if one blindly took on board Gauss's law, or any other precise mathematical law, to express individual errors, one would necessarily be led to the conclusion that precision increases as the square root of the number of observations and can, as a result, be made as high as one wishes)." ["si l'on acceptait d'une manière aveugle la loi de Gauss, ou toute autre loi mathématique précise, pour exprimer les erreurs individuelles, on serait forcément conduit à la conclusion que la précision croît comme la racine carré du nombre des observations et peut, par suite, être rendue aussi grande que l'on veut" (1908b: 268)]. This problem, incidentally, is the starting point of Peirce and Jastrow's 1888 contribution to psychophysics.

[57] See Borel (1950), pp. 104 sqq., the section entitled "Physical continuity according to Poincaré".

Forty years after his response to Poincaré, Borel wrote about his own paper: "I made, at that time, some objections to that definition [of the physical continuum by Poincaré] to which Poincaré did not respond, which allows one to think that he accepted them".[58] Borel, however, is much more explicit about his solution than he was in 1908. His proposal is that, from the assumptions $A \sim B$, $B \sim C$, and $A > C$ one can actually logically infer that $A > B > C$. His brief argument is that if it were the case that $B \leq C$ on the continuum, then it would follow that $(A - B) \geq (A - C)$, contradicting the assumption that $A \sim B$, and similarly if it were true that $B \geq A$, then it would follow that $(B - C) \geq (A - C)$, contradicting $B \sim C$.

Borel's solution to the problem is particularly striking in that it antedates an abstract method for undoing intransitivities first sketched by Goodman (1951) – more briefly Russell (1940) – and only made fully precise in 1956 by Luce in the context of his theory of semi-orders. Specifically, Goodman assumes that two sensory qualities or qualia can be directly indiscriminable (can 'match', in Goodman's terminology), but he accepts, like Poincaré, that direct indiscriminability (matching) is a nontransitive relation. However, Goodman considers that for two qualia to be identical, it is not sufficient for them to be directly indiscriminable: they must also be indiscriminable from exactly the same qualia. Consequently, two qualia a and b can be directly indiscriminable, but will be indirectly discriminable if there is a quale c such that c is directly indiscriminable from the one but directly discriminable from the other.

Goodman's method only focuses on identity, however, and not so much on ordering proper. In that respect, the closest elaboration of Borel's thoughts is definitely Luce's theory of semi-orders, which Luce presents as a way of obtaining "a non-statistical analogue of the 'just noticeable difference' concept of psychophysics". What Luce 1956 spells out is indeed a general method for extracting a *weak order* preference relation (inducing a transitive indifference relation) from a *semi-order* preference relation (in which the associated relation of indifference is typically nontransitive). Luce's method relies on essentially the same reasoning as the one detailed independently by Borel (1950) on Poincaré's intransitivity problem, but using a more algebraic perspective. The significance of Luce's result is explained in greater detail in Luce 1959, where Luce discusses the correspondence between the probabilistic treatment of the notion of Just Noticeable Difference and the way in which a ratio scale can be obtained from a set of imperfect discriminations.[59]

[58] That remark, and the footnotes to Borel (1950), do not appear in the English translation of Borel (1963)'s reissue.

[59] A more recent account of the connection between intransitivity, semi-order, and the way to obtain a weak order out of a semi-order is van Rooij (2010). On indirect indiscriminability, see also Williamson (1994). On the discussion of probabilistic treatments of phenomenal predicates and the problem of transitivity, see Hardin (1988), whose account comes out close in spirit to the first of Borel's proposed answer to Poincaré's predicament, and also Voorhoeve and Binmore (2006). For a discussion of Luce's canonical weak order as resulting from a semi-order – which coincides exactly with Borel's argument in the example at hand – see Lehrer and Wagner (1985).

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