Two Conceptions of Weight of Evidence in Peirce's *Illustrations of the Logic of Science*[*]

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Abstract: Weight of evidence continues to be a powerful metaphor within formal approaches to epistemology. But attempts to construe the metaphor in precise and useful ways have encountered formidable obstacles. This paper shows that two quite different understandings of evidential weight can be traced back to one 1878 article by C.S. Peirce. One conception, often associated with I.J. Good, measures the balance or net weight of evidence, while the other, generally associated with J.M. Keynes, measures the gross weight of evidence. Conflations of these two notions have contributed to misunderstandings in the literature on weight. This paper shows why Peirce developed each conception of weight, why he distinguished them, and why they are easily mistaken for one another.

Peirce scholars are accustomed to hearing (and to claiming) that Peirce anticipated this or that philosophical development commonly credited to another thinker. Less often than Peirce scholars would like, but perhaps more often than they tend to acknowledge, philosophers who are not given to Peirce exposition credit the founder of pragmatism with important ideas. Carnap, Popper, Good, Ullian, and O'Donnell, among others, have found in Peirce's 1878 essay, "The Probability of Induction" the first clear formulation of the notion of weight of evidence.[1] The situation actually involves an embarrassment and a confusion of riches, however. Peirce formulates two quite distinct notions of weight of evidence, each of which has been influential. One anticipates Keynes's conception of the weight of argument, first broached in his 1921 *A Treatise on Probability*. Peirce develops this sense of weight as part of a critique of conceptualist (or, as we would now say, Bayesian) approaches to probability. But Peirce also develops a conception of weight of evidence that favors conceptualism, and this has been picked up by Bayesians like Good. Peirce goes to considerable trouble to distinguish the two notions, but almost all commentators have either conflated the two notions or ignored one in favor of the

[**] References to Peirce's work are to the *Writings of Charles S. Peirce: A Chronological Edition* and have the form: W[Volume Number] [Page number].

other.[2] Revisiting Peirce's reasons for developing two conceptions of evidential weight and the uses to which he puts them promises to tell us a great deal about Peirce and about some impressive early developments in the philosophy of probability. It will also help us understand and avoid some confusions that have pervaded the literature. Most importantly, it can help us get clear about the uses to which various conceptions of evidential weight can be put in our thinking about probability.

This paper begins with a brief discussion of just how and how centrally the notion of probability figures in Peirce's *Illustrations of the Logic of Science*. It then sketches Peirce's innovative proposal for measuring the appropriate intensity of belief in a given evidential situation. Peirce supplements this proposal with a suggestion for how conceptualists or Bayesians ought to measure what Peirce calls the weight of evidence. The suggested measure is the sum of the logarithms of the likelihood ratio for each piece of evidence. But Peirce goes on to argue against conceptualism, in part by appealing to a notion that he does not refer to as evidential weight but which anticipates what Keynes means by that term. The paper discusses how readily the two notions of evidential weight are conflated and surveys Peirce's reasons for distinguishing them. Some attention is given to the issue of what the discussion of weight can teach us about Peirce, but for the most part, the focus remains on what Peirce can teach us about weight.

1. Probability in the *Illustrations*

Discussion of Peirce's *Illustrations of the Logic of Science*, published in *Popular Science Monthly* in 1877-78, has been dominated by the first two of the six essays. Peirce typically thought of "The Fixation of Belief," which contains his most influential presentation of his theory of inquiry, and "How to Make Our Ideas Clear," in which he presents his pragmatic maxim, as one paper. Both essays descend from the talk that Peirce famously gave to The Metaphysical Club in Cambridge in the early 1870's, which William James makes central to the founding narrative of pragmatism. Manuscript material makes it clear that the best-known sections of "Fixation" were drafted by the end of 1872.[3] In these sections, Peirce discusses the differences between doubt and belief;~~,~~ argues that the settlement of opinion is the sole end of inquiry;~~,~~ contrasts the scientific method of fixing belief to its competitors;~~,~~ and concludes that, once one has reflected on the options for settling belief, only the method of science can be deliberately adopted.[4] The published version of the paper, however, begins with two sections that link the doubt-belief theory to a larger project. In the first section, Peirce provides some historical illustrations of the logic of science, culminating in a discussion of Darwin, Maxwell, and others who have brought the statistical method into 19th-century science. Peirce maintains that "each chief step in science has been a lesson in logic," and he is convinced that probability looms large in the chief steps being taken in the science of his day (W3 244). Accordingly, he aims to draw out for his readers the lessons in logic implicit in the probabilistic revolution. Peirce drew on his earlier material for the first of his "Illustrations," but added the prefatory material he did largely because he had decided to make probability the central topic of his "Illustrations" as a whole (see de Waal 2014, p. 10). Peirce follows "Fixation" and "How to" with "The Doctrine of Chances," "The Probability of Induction," and "The Order of Nature," before concluding the series with "Deduction, Induction and Hypothesis." Peirce had long been thinking about probability, under the influence of De Morgan, Boole, and others. At the time he was preparing his *Illustrations* for publication, he seems to have decided to bring together his doubt-belief theory of inquiry, his pragmatic maxim for clarifying difficult ideas, and his thoughts about probability

The text of the *Illustrations* sometimes sounds apologetic about the doubt-belief theory and the pragmatic maxim, but enthusiastic about the material on probability. Near the end of "How to," Peirce promises that he "will not trouble the reader with any more Ontology at this moment. I have already been led much further into that path than I should have desired; and I have given the reader such a dose of mathematics, psychology, and all that is most abstruse, that I fear he may already have left me" (W3 275). Peirce rightly insists on the importance of the material in "Fixation" and "How to," but he suggests that its value derives from its contribution to the succeeding discussion of probability. "The reader who has been at the pains of wading through ["How to"] shall be rewarded in ["The Doctrine of Chances"] by seeing how beautifully what has been developed in this tedious way can be applied to the ascertainment of the rules of scientific reasoning" [W3 275]. As he transitions from the work for which he would become known to his much less influential treatment of probability, Peirce remarks that "[w]e have, hitherto, not crossed the threshold of scientific logic. It is certainly important to know how to make our ideas clear, but they may be ever so clear without being true. How to make them so, we have next to study" (W3 275-6). Though his successors have not followed him in this, Peirce clearly thinks that his *Illustrations* center on the treatment of probability therein.

Peirce begins the first of the papers on probability, "The Doctrine of Chances," by insisting on the transformative power possessed by the notion of continuous quantity. "It is not," Peirce says, so much from *counting* as from *measuring* … that the advantage of mathematical treatment comes" (W3 276-7). Peirce cites Lavoisier's chemical revolution as his exemplar of the advantages that accrue from making measurement central to scientific practice, but he goes on to make a less obvious point. The notion of continuous quantity "has a great office to fulfill, independently of any attempt at precision…[I]t is the direct instrument of the finest generalizations (W3 277). By emphasizing continuity, naturalists, "the great builders of conceptions," classify and generalize in illuminating ways. Continuity gives us "an idea of a leaf which includes every part of the flower, and an idea of a vertebra which includes the skull," and it provides the materials from which clear notions of a species are built. Peirce promises "to make a great use of this idea in the present series of papers" (W3 278). He makes it explicit that "[t]he theory of probabilities is simply the science of logic quantitatively treated," and he makes it equally clear that he expects to sow for logic the benefits that the naturalists have reaped. Logic, for Peirce, includes ampliative as well as explicative inference, and so he is well positioned to insist on continuities between problems of logic and of the theory of probability. "The general problem of probabilities is, from a given state of facts, to determine the numerical probability of a possible fact. This is the same as to inquire how much the given facts are worth, considered as evidence to prove the possible fact. Thus the problem of probabilities is simply the general problem of logic" (W3 278). This task of assessing how much certain facts are worth considered as evidence bearing on another fact has often invited metaphors about the force, strength or weight of evidence. As we will see, Peirce approaches this task as the measurement theorist he is, and he does indeed forge novel conceptions that allow for arguments to be classified in valuable new ways.

Peirce warns us, however, that "this branch of mathematics is the only one, I believe, in which good writers frequently get results entirely erroneous" (W3 279). The sense in which probabilities and logic illuminate one another, for instance, was commonly misunderstood in Peirce's time. "Some writers," Peirce says, "have gone so far as to maintain that, by means of the calculus of chances, every solid inference may be represented by legitimate arithmetical operations upon the numbers given in the premises" (ibid). Such misguided approaches to the theory of evidence (Peirce has Quetelet in mind) flourish because the fundamental concepts involved in the theory of probability remain in dispute. And so Peirce presents the conception of probability as the raison d'etre, within the *Illustrations*, for the presentation of the pragmatic maxim. That maxim recommends that we "Consider what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object" (W3 266). Peirce's pragmatic elucidation of meaning focuses, not on practical effects, but on conceivable practical effects. A full clarification of what is involved in attaching a degree of probability to a statement or argument could only be attained by tracing out every way in which such an attachment could make a difference to potentially envisioned rational conduct. Peirce's pragmatism about meaning is more like a sophisticated version of operationalism than it is like a practicalism.[5] Like much of Peirce's work, it is geared toward stable belief in the indefinite long-run, not toward desire-satisfaction in the short or medium term.

Peirce elucidates the conceivable practical effects involved in judgments of probability by noting that probability attaches to inferences and that it constitutes a special case of the notion of inferential validity. He invokes the conclusion of an anti-psychologistic argument he offered in Section II of "Fixation" (see W3 244). The validity of an argument "does not depend on any tendency of the mind to accept it, however strong such tendency may be; but consists in the real fact that, when premises like those of the argument in question are true, conclusions related to them like that of this argument are also true" (W3 280). When an argument is described as a valid probable argument, it is claimed to be a member of a genus such that when the premises are true, the conclusions are true "for the most part." Despite some important loose ends, Peirce offers a plausible answer to the question posed by his pragmatic maxim. He claims that the "real and sensible difference between one degree of probability and another, in which the meaning of the distinction lies, is that in the frequent employment of two different modes of inference, one will carry truth with it oftener than the other" (W3 280).

Peirce is particularly insistent that the meaning of probability statements must be grounded in "existing facts." Of a given inference considered by itself, "the only possible practical question" is whether the conclusion is true or not. But Peirce's pragmatic clarification of the notion of reality in "How to" issued in the result that the very idea of a fact "depends on the supposition that sufficient investigation would cause one opinion to be universally received and all others to be rejected" (W3 280). So Peirce unabashedly appeals to a fact about the success of a given mode of inference at yielding true conclusions from true premises in the long run. "The very idea of probability and of reasoning," says Peirce, assumes that the number of inferences to be performed is indefinitely large (W3 284). Permissive though this notion of an "existing fact" is, Peirce is anything but permissive about conceptions of probability that do not ground the notion in such facts. He explicitly claims that the notion of probability cannot meaningfully be applied to a single case, considered in itself. If a given inference is strictly and genuinely considered unrepeatable, (Peirce considers the case of an entirely selfish person drawing a red or a black card with eternal happiness on the line), there is no truth-maker for the statement that the conclusion would be true a certain fraction of the times when the premises are true, and so "there can be no sense in reasoning in an isolated case at all" (W3 282). He grants that "it would be folly to deny" that an agent ought to draw from the pack with the larger proportion of cards leading to endless happiness, and so concludes, after some suggestive and puzzling argumentation, that a rational agent cannot treat the case as unique and unrepeatable. "[L]ogicality inexorably requires," says Peirce, "that our interests shall not be limited. They must not stop at our own fate, but must embrace the whole community" (W3 284). This issue, which Hillary Putnam has named "Peirce's Puzzle," is not really on the agenda for this paper. It appears here in order to show how seriously Peirce takes the claim that statements involving probability claims must represent a real fact. Peirce's criticism of the conceptualist (aka Laplacian or Bayesian) tradition for its failure to ground probabilities in facts derives from a deeply felt and quite general philosophical commitment. As Isaac Levi has argued, Peirce does not object in principle to interpreting probability statements in terms of credal states, but he does insist that we find a way to understand such states as expressing or as suitably constrained by facts (see Levi 1995, pp. 65-66 and p. 75).[6]

2. Probability and Belief

If "The Doctrine of Chances" draws heavily on the pragmatic maxim from "How To," its successor, "The Probability of Induction," makes important use of the doubt-belief theory from

"Fixation." In "Probability," Peirce contrasts what Venn calls the materialistic view of probability, according to which probability statements report facts about the frequency with which one kind of event accompanies another kind, with De Morgan's conceptualistic view, according to which probability concerns the degree of belief that ought to attach to a proposition. Though we know that Peirce will side with materialism, he offers novel and important arguments on behalf of conceptualism. Peirce pronounces it "incontestable" that the chance of an event, taking into account all known relevant information, should have an "intimate connection" with our degree of belief concerning the event. By "chance," Peirce means the ratio of favorable to unfavorable cases, so that an event with a probability of one half has an even, or 1 to 1 chance of occurring. Crucially, chances can be greater than one; if we accept the folk wisdom that two-thirds of those directly struck by lightning survive, then the chance of survival, given that one has been directly struck, is 2 to 1. When we're evaluating the strength of evidence for and against a given hypothesis, Peirce's notion of the chance of an event can be stated in more modern garb as the odds for hypothesis H given evidence E, or P(H\E)/P(~H\E). This is, of course, equivalent to P(H\E)/1-P(H\E). Clearly enough, odds can be derived from knowledge of probabilities and probabilities can be derived from knowledge of odds.[7] We will soon see why Peirce chooses to focus on chances (or odds) rather than probabilities.

Peirce's discussion in this section of "The Probability of Induction" is quite compressed, and it has been interpreted rather variously by its commentators. Peirce defines chances in terms of frequencies or, as he puts it, ratios of cases. This (combined with a misunderstanding of how Peirce uses "chance") has led Schum to complain that Peirce "seems not to have appreciated any distinction between chances (the aleatory conception) and epistemic probability." (Schum 1994, p. 214). Levi, on the other hand, presents Peirce with an explicitly Bayesian or conceptualist argument. As Levi reads him, Peirce "took the conceptualist practice of invoking insufficient reason to be claiming that the prior odds P(H)/P(~H) = 1 and with that took the 'final odds' (that is, the odds for H given E) to be equal to the 'likelihood ratio' P(E\H)/P(E\~H)" (Levi 2010, p. 40). In Schum's defense, it should be pointed out that Peirce does not invoke anything like a principle of insufficient reason at this point in his argument. In Levi's defense, Peirce makes it clear that he takes himself to be mustering a defense of conceptualism, which is a view about epistemic probability, not about chance in the aleatory sense. I suggest that Peirce's reliance on frequencies does not betray any confusion about the nature of epistemic probability; nor does it involve the begging of any questions against his conceptualist opponents. Peirce is not, at least in "Probability," primarily interested in such questions as whether probability statements are "about" degrees of belief or long-term limit frequencies (or, as he will argue later in his career, dispositions). This is confirmed by his claim that "the great difference" between conceptualism and materialism concerns not what probability statements are "about" but rather the fact "that the conceptualists refer probability to an event, while the materialists make it the ratio of frequency of events of a *species* to those of a *genus* over that *species*, thus *giving it two terms instead of one*" (W3 292, emphases in original). This, as we saw above, is the kind of elucidation of the "meaning" of probability statements that Peirce has in mind. And this is why the construal of probabilities and chances in terms of frequencies doesn't (yet) beg any issue between conceptualists and materialists. If conceptualists can account for the legitimacy of probabilistic inferences in terms of degrees of belief, then the view will have been vindicated, for Peirce's purposes.[8] So Peirce is simply focusing on favorable cases for construing probabilities as degrees of belief. Any conceptualist will favor some principle linking degrees of belief to objective frequencies or to some suitable substitute like truth values of propositions.[9] Peirce may have developed his defense of conceptualism along the lines suggested by Levi or he may just have started with well-behaved cases, ones in which we can govern our belief on the basis of unproblematic statistical frequencies involving events which are probabilistically independent of one another. Such cases minimize the conceptualist's truck with controversial issues about prior probabilities and principles of indifference, and so Peirce avoids such matters when putting the best face on conceptualism.[10]

Why, though, does Peirce formulate his argument on behalf of conceptualism in terms of chances (or, in the modern sense, odds) rather than probabilities? Because, he says, "chance is a quantity which may have any magnitude, however great" while probabilities range only from zero to one (W3 293). Because the probability scale has upper and lower boundaries, evidence tends to make less of a difference to probabilities near the extremes.[11] Slightly less informally, if a prior probability is near one, it can be very difficult for evidence to raise that probability significantly, even if that same evidence would have made a big difference had the prior probability been closer to .5. Peirce, in trying to make the best case he can on behalf of conceptualism, wants to avoid this consequence of using probabilities to directly measure evidential force. As Schum puts it, "an item of evidence whose force is graded in terms of the ratio of posterior to prior *odds* will have the same measured force in changing a prior belief regardless of the strength of a prior belief," while measures of evidential force in terms of the difference between prior and posterior probabilities or the ratio of posterior to prior probabilities lack this feature.[12]

As we've seen, Peirce wants to focus on cases in which the odds are derived from observed frequencies. Peirce may, as Levi suggested, have started from a ratio of prior to posterior odds of 1; he may instead have employed direct inference from frequencies to degrees of belief on behalf of conceptualism; either way the result is the same. Whether or not Peirce used a version of Bayes' Theorem, he does recognize a consequence of Bayes' Theorem, namely that the ratio of the posterior to the prior odds for H given E is identical to the likelihood ratio P(E\H)/P(~E\H).[13] No conceptualist will balk at the suggestion that if I have a large body of evidence summarized by P(E\H)/P(E\~H) and no other relevant considerations to go on, that is where I should set my odds for H given E. Nobody will deny that a measure of belief intensity should somehow take into account chances in Peirce's sense, but Peirce has a great deal more to say about the "intimate connection" between intensity of belief and chance.

The heart of Peirce's defense of conceptualism is worth quoting at length:

> Belief is certainly something more than a mere feeling; yet there is a feeling of believing, and this feeling does and ought to vary with the chance of the thing believed, as deduced from all the arguments. Any quantity which varies with the chance might, therefore… serve as a thermometer for the proper intensity of belief. Among all such quantities there is one which is peculiarly appropriate….As the chance diminishes the feeling of believing should diminish, until an even chance is reached, where it should completely vanish and not incline either toward or away from the proposition. When the chance becomes less, then a contrary belief should spring up and should increase in intensity as the chance diminishes, and as the chance almost vanishes (which it can never quite do) the contrary belief should tend toward an infinite intensity. Now, there is one quantity, which, more simply than any other, fulfills these conditions; it is the *logarithm* of the chance. (W3 293-294)

The conceptualist's notion of the degree of belief which ought to attach to a proposition is here construed as a measure of the proper intensity of the feeling of belief. Peirce grants that believing involves more than feelings; earlier in the *Illustrations*, he credits belief with three properties. It is something of which we are aware (this presumably derives from the feeling discussed above), it appeases the irritation of doubt (doubt stimulates toward its own removal while belief tends to remain settled), and it governs action by establishing habits (to the extent that we possess a firm belief, we are prepared to act confidently in situations to which the belief applies).[14] So Peirce is not by any means reducing belief to a feeling, nor does he accuse conceptualists of doing so. Instead, he is praising conceptualism for the elegant resources it possesses for linking evidence and appropriately intense belief.

Peirce reinforces this defense of the Laplace/De Morgan/Bayes tradition in two ways. One appeals to Fechner's psycho-physical law which states that the intensity of a sensation varies in direct proportion to the logarithm of the external force which produces the sensation. Treating the chance as the external fact which generates the belief leaves the logarithm of the chance as the appropriate measure of belief intensity. The result is a kind of belief thermometer modeled on psychologically successful scales like the decibel measure of the intensity of sound. He then claims that "belief ought to be proportional to the *weight of evidence*, in this sense, that two arguments which are entirely independent, neither weakening nor strengthening each other, ought, when they concur, to produce a belief equal to the sum of the intensities of belief which either would produce separately" (W3 294, emphasis added). So, says Peirce, since the logarithm of a product is the sum of the logarithms of its factors, we have another argument for treating the logarithm of the chance as our belief thermometer. Peirce takes himself to have produced an impressive argument in favor of conceptualism constituted by the simplicity, elegance, and plausibility of the connection between measures of probability and of the appropriate feeling of belief.

3. Net Weight of Evidence

It is in developing this last reason for linking degree of belief and the logarithm of the chance that Peirce introduces the term "weight of evidence." As Isaac Levi has noted, Peirce treats the metaphors of weighing and of balancing evidence as interchangeable. In this he differs importantly from Keynes, as we shall see. Peirce locates his notion of weight of evidence within a practice he calls the balancing of reasons. Keeping in mind that Peirce restricts his discussion to cases in which the arguments in question are independent of one another, we see him offering a characterization of something we might call, following Levi, the net weight of evidence: "Take the sum of all feelings of belief which would be produced separately by all the arguments *pro*, subtract from that the similar sum for arguments *con*, and the remainder is the feeling of belief which we ought to have on the whole" (W3 294). Despite the compression of Peirce's account, the reader has no trouble discerning that it is resolutely normative. The logarithm of the chance is taken to represent the facts which "produce" the belief, and then the feeling of belief ought to remain proportional to this magnitude. Peirce says nothing about how well or poorly we respond to the facts that our probability assessments aim to track. He is describing how an appropriately calibrated belief thermometer would work. Just as our auditory system, if functioning properly, ought to issue in results that remain proportional to the log of the stimulus that produced the sound, our cognitive apparatus ought to respond to the log of the evidential stimulus.

Conceptualists like I.J. Good have credited Peirce with anticipating this notion of weight of evidence and have retained (or arrived independently at) Peirce's term for this important measure of one's evidential situation. Good, like Peirce, identifies weight of evidence with the logarithm of the likelihood ratio.[15] Interestingly, Turing seems independently to have come up with a measure of evidential weight that deploys odds, logarithms, and the analogy with the decibel system almost exactly as Peirce did. Turing proposed the term "ban" for the unit in which weight of evidence is to be measured (when the logarithms are to the base 10). "Ban" derives from Banbury, the town in which the forms used by Turing, Good, and the other codebreakers were printed. Keeping within the Humean/Peircean tradition of treating probabilities as (in part) felt perceptions, Good characterizes a deciban (one-tenth of a ban) as "about the smallest unit of weight of evidence that is perceptible to human judgment," so he takes the analogy with decibels to be particularly apt.[16] Good illustrates with the following example:

> [S]uppose we are trying to discriminate between an unbiased die and a loaded one that gives a 6 one third of the time. Then each occurrence of a 6 provides a factor of ${}^{1/3}/_{1/6} = 2$, that is, 3 db [decibans], in favor of loadedness while each non-6 provides a factor of ${}^{2/3}/_{5/6}$ = 4/5, that is 1 db, against loadedness. For example, if in twenty throws there are ten 6's and ten non-6's, then the total weight of evidence in favour of loadedness is 20 db, or a Bayes factor of 100. (Good 1985, p. 254).

Apparently before becoming aware of Peirce's work, Good proposed "weight of evidence" as a terminological improvement upon Turing's "decibannage." Contemporary conceptualists, under the influence of Ramsey, tend not to think of weight of evidence in terms of appropriate feelings of belief.[17] But with or without that component, Peirce's desiderata for a precise measurement of the extent to which evidence favors a hypothesis have been accepted even by philosophers unimpressed by his critique of conceptualism.

4. Gross Weight of Evidence

Having offered some novel and important arguments on behalf of conceptualism, Peirce proceeds to reject the view in favor of a position more like Venn's. Peirce will soon object to conceptualism in the expected ways, by attacking the intelligibility of an appeal to a principle of indifference and by denying the legitimacy of personal probabilities not appropriately grounded in facts. So Peirce does look backward toward Venn and forward toward Bertrand in his critique of conceptualism. But he begins with something more idiosyncratic and more important for our purposes. To make his point, Peirce deploys a standard probabilistic set-up in which black and white beans are drawn from an urn (with replacement):

> When we have drawn a thousand times, if about half have been white, we have great confidence in this result. We now feel pretty sure that, if we were to make a large number of bets upon the color of single beans drawn from the bag, we could approximately insure ourselves in the long run by betting each time upon the white, a confidence which would be entirely wanting if, instead of sampling the bag by 1000 drawings, we had done so by only two. Now, as the whole utility of probability is to insure us in the long run, and as that assurance depends, not merely on the value of the chance, but also on the accuracy of the evaluation, it follows that we ought not to have the same feeling of belief in reference to all events of which the chance is even. In short, to express the proper state of our belief, not one number but two are requisite, the first depending on the inferred probability, the second on the amount of knowledge on which that probability is based. (W3 295)

Peirce doesn't use the term "weight of evidence" here; as we have seen, he used it a page earlier to describe the way in which conceptualists combine evidence by adding the logarithms of the chances of independent arguments together. The magnitude currently under discussion is put forward as part of an objection to conceptualism and is the notion that Carnap, among others, reads as an anticipation of Keynes's conception of weight of argument. In his 1921 *A Treatise on Probability*, Keynes writes:

> As the relevant evidence at our disposal increases, the magnitude of the probability of the argument may either increase or decrease, according as the new knowledge strengthens the unfavourable or the favourable evidence; but *something* seems to have increased in

> either case, -- we have a more substantial basis upon which to rest our conclusion. I express this by saying that an accession of new evidence increases the *weight* of an argument. New evidence will sometimes decrease the probability of an argument but it will always increase its 'weight.' (p. 71)

Keynes contrasts the balance metaphor and the weight metaphor, and clearly has gross, not net weight of evidence in mind. As we will see below, some philosophers have treated Good's notion of weight of evidence as a competitor to Keynes'. Others have held that it competes with standard measures of degree of confirmation like P(H/E) – P(H) or P(H\E)/P(H).[18] Good seems to think he has formulated a conception that, in one stroke, improves upon these two measures of confirmation and on the Keynesian notion of evidential weight.[19] This is odd because what Keynes means by weight of evidence isn't a competing conception of the balance of evidence but instead something quite different. He is trying to quantify the amount of evidence relevant to the conclusion, not the amount of support provided for the conclusion. So Keynesian weight, unlike the Turing-Good notion, is independent of probability.[20] Like Peirce, Keynes thinks that at least two numbers will be needed to express one's state of belief, and that one can go up while the other goes down.

We can return to Peirce's example to get a feel for Keynesian weight of evidence. Someone who has drawn a small number of beans, the vast majority of which are black, has good evidence, good reasons for belief, in one clear sense. Such evidence as she has bears decisively, to use Joyce's term, on what she is to expect. The net weight of her evidence might not be large, but it has clear directionality; in this case it clearly favors the hypothesis that the bag contains many black beans. Someone who has drawn a much larger number of beans from the bag (with replacement and stirring) but has drawn an approximately equal number of black and white beans is in a less favorable situation for having an expectation about what color the next bean will be. But her judgment is in some clear sense more settled or more stable than the belief of the other inquirer. Brian Skyrms, for instance, writes that increasing the number of trials increases the "resilience" or resistance to change of the probability judgment. And Joyce characterizes Keynesian weight in terms of the concentration and stability of credences in the face of changing information.[21] Unlike Good's conception of weight, gross or Keynesian weight lacks directionality or valence; the weight of evidence for H will always be the same as the weight of evidence for ~H. Our second inquirer's evidence is weighty in Keynes's sense but not in Good's, and this does seem like an independent dimension of evidential goodness.[22] Such evidence would (on its own) produce no strong feeling of belief according to Peirce's method of balancing reasons.

We thus see that the two conceptions of weight of evidence that have attracted the most attention both get broached, within a page of each other, by Peirce in 1878. This has led to some confusion in the literature since those who credit Peirce with the notion of weight of evidence generally have either Good's conception or Keynes's in mind, and are typically unaware that Peirce can be cited as a source for the other notion of evidential weight.[23] Ullian's discussion is illustrative:

> Further underlining should be given to one of Peirce's contributions to the subject of probabilistic reasoning, the concept of *weight of evidence*. Carnap, Popper, and I. J. Good are among those who mark Peirce as the first (by far) to introduce it. Even though we may have a continuing belief that the probability of a certain character's being present is ½, it may happen that as we accumulate data we have more and more reason to believe that this is the probability. We judge the chance as before, but that judgment is itself rendered more likely. (p. 96)

Carnap thinks of Peirce as anticipating Keynes. Good thinks of Peirce as anticipating Turing and Good, and Popper somewhat runs the two together, so the appearance of agreement is quite misleading. Ullian then goes on to describe Peirce's view as a combination of Keynes-like and Good-like characteristics. Keynes is a relationist about probability and so denies that probability judgments based on more evidence are more likely to be true than those grounded on less evidential weight. Probability judgments for Keynes are about the relationship between the evidence and the conclusion, and are, when true, logically true. For this reason, Keynes struggles to articulate why we should prefer beliefs grounded in more evidence to those grounded in less.[24] And Good's notion of weight of evidence has a valence and so won't in general fit Ullian's example.

Confusion is furthered by the fact that it is tempting and sometimes correct to see Good and Keynes as offering competing theories of the same thing. Net weight and gross weight each have an intuitive claim to serve as the referent of "weight of evidence." But neither is trivial to measure, and an attempt to characterize one kind of evidential weight has often been criticized as a misguided attempt to measure the other. The confusions have been mediated, not only by assumptions about what "weight of evidence" means (aggravated, as noted above, by confusing citations of Peirce), but also by competing views about the importance of these notions for understanding evidence and inquiry. Good, for example, says that Keynes' approach "is like interpreting weight of evidence as the weight of the documents on which they are printed" and adds that it is "if not horseradish, it is at least a crummy concept in comparison with the explicatum of weight of evidence that I support" (Good 1985, p. 267). Joyce, on the other hand, assumes that "weight" means Keynesian weight and faults Good for offering a measure of evidential relevance as if it represented evidential weight (Joyce 2005, p. 165). Thinkers like Keynes and L.J. Cohen have treated Keynesian weight as a measure of how close a body of evidence comes to proving or disproving a claim, and such a notion readily gets conflated with Good's measure of evidential support. Finally, the two conceptions have competed for a role in solving issues like the stopping problem.[25] Both Good's and Keynes's approaches have seemed to various thinkers like they would help one decide when one's evidence is good enough so that it no longer makes sense to seek more evidence.[26] Keynes, however, has been vindicated in his assessment that, "[a] little reflection will probably convince the reader that this is a very confusing problem," and the intractability of that issue has, I suspect, contributed to the persistent failure of discussions about weight to engage one another (Keynes 1921, p. 77).

For this and other reasons, Keynes remains unconfident about the importance that should attach to his notion of evidential weight. He begins the relevant chapter in *A Treatise on Probability* by remarking that "The question to be raised in this chapter is somewhat novel; after much consideration I remain somewhat uncertain as to how much importance to attach to it" (Keynes 1921, p. 71, also see p. 76). In a later chapter he considers the question of whether we ought (ceteris paribus) to be guided in our actions by probability assessments grounded in weightier evidence, and pronounces the question "highly perplexing," even adding that "it is difficult to say much that is useful about it" (Keynes 1921, p. 313). Peirce, on the other hand, seems quite confident in the importance of Keynesian weight.

> It is true that when our knowledge is very precise, when we have made many drawings from the bag, or, as in most of the examples in the books, when the total contents of the bag are absolutely known, the number which expresses the uncertainty of the assumed probability and its liability to be changed by further experience may become insignificant, or utterly vanish. But, when our knowledge is very slight, this number may be even more important than the probability itself; and when we have no knowledge at all this completely overwhelms the other… (W3 295).

The different assessments of the importance of Keynesian weight derive, in part, from the perhaps surprising fact that Peirce, the founder of pragmatism, is less concerned about practical decision-making than is Keynes. The economist worries about the significance of weight because whenever a decision must be made, we can only go on such evidence as we have, regardless of its weight.[27] For Peirce, on the other hand, "the whole utility of probability is to insure us in the long run," as we saw in a passage above. The connections between theory and practice constitute a complex and challenging issue for Peirce scholars.[28] When facing "vital crises," he often suggests that we should appeal to tradition, habit, or sentiment, rather than trying to reason our way to a solution.[29] Probability statements concern arguments and their ratios of truth preservation. They inform us about the risks inherent in acting on the assumption that the conclusion of an argument will be true, given that its premises are true. The meaning of such statements, as we have seen, includes every way in which they might guide reasoning. Their value crucially depends on the confidence with which they can be asserted, so Peirce sees Keynesian weight as essential to the role played by probability judgments.

While it is clear that Peirce is bullish and Keynes cautious about more or less the same magnitude, there is probably no fact to the matter about whether they have quite the same concept in mind. As recent scholarship by Runde, Weatherson, and Joyce indicates, there is probably no single concept fitting the various uses to which Keynes puts the concept of weight. The same is no doubt true of Peirce, who said less about the issue than Keynes did. Keynes sometimes wants weight of argument to be a measure of the total amount of relevant evidence, and he sometimes wants it to be a measure of the ratio of our total relevant evidence to the ideal evidential situation, and he sometimes wants it to be an inverse measure of uncertainty. These can come apart, as when I learn new evidence that also makes me newly aware of the depth of my ignorance. I thereby increase my total evidence and increase my uncertainty by learning about the extent of relevant information of which I'm ignorant. So Weatherson, for instance, preserves the link between weight and uncertainty by dropping what some have considered the essential feature of Keynesian weight, viz. that it always increases when new relevant evidence is learned.[30] Peirce, for his part, tends to identify gross weight of evidence, the resistance of the probability judgment to be changed by further evidence, and an inverse measure of probable error. The measure of Keynesian weight "expresses" (inversely) "the uncertainty of the assumed probability and its liability to be changed by further experience" (W3 295). Peirce explicitly invokes the term “probable error” in a footnote to the passage quoted above. Peirce’s preferred strands of the notion of weight can come apart as well, as Keynes himself showed in the case of probable error. Keynes pronounces it “obvious that the determination of probable error is intrinsically a different problem from the determination of weight” (75). Probable error, like uncertainty, can increase as new relevant information is presented, since new relevant information can increase the dispersion among supported probability estimates. Keynes grants that there is a practically important connection between gross evidence and probable error, but insists that “the connection is casual,” contingent, and unsupported by theoretical considerations. This perhaps undersells the point, since Keynes also recognizes that in situations which are favorable to sampling, increases in sample size will decrease probable error. Peirce is probably more confident than Keynes is that large chunks of inquiry can be assimilated to statistical sampling, and it is also open to Peirce to decouple the gross amount from evidence from the reduction of probable error, as Weatherson has done for Keynes in the case of uncertainty. Finally and unsurprisingly, neither Peirce nor Keynes seems to have anticipated the recent emphasis in epistemology on ways in which evidence can be defeated. Clearly enough, such work complicates any attempt to make simple sense of the notion of gross evidential weight. In short, there are several noteworthy candidates for the title “(gross) weight of evidence,” and it’s understandable that Peirce and Keynes couldn’t always keep them straight.

5. Weight of Evidence, Stability, and the Argument against Conceptualism

Philosophers today are clearer than Peirce was about the nuances involved in Keynesian weight. On the other hand, as argued above, contemporary discussions of weight of evidence lack the clarity that Peirce achieved about the distinction between net and gross evidential weight.~~But he gains some clear-headedness by not having the term “weight” tug him simultaneously toward what he and Good mean by it and what Keynes means by it~~. Peirce might, in fact, be somewhat blind to the similarities between the two notions. He shows no inclination to apply the weight metaphor to Keynesian weight of evidence. Peirce’s mix of insight and ignorance within two pages intrigues. As we have seen, the *Illustrations* begin with Peirce’s famous discussion of fixed or settled belief, before focusing on probability. The very metaphor of weight of evidence suggests deep conceptual connections to the settlement of opinion. Weighty things require, ceteris paribus, large forces to move them. So Good’s measure of evidential weight can be thought of as a measure (among many) of the resistance of a probability judgment to be altered by further evidence. The weightier one's evidence in this sense, the more evidence unfavorable to a hypothesis it will take to diminish one's degree of belief in the hypothesis. "More" here can range over both the intensity of the evidence (represented by a probability) and the extensiveness of the evidence, (represented by Keynesian weight). The fact that a strong balance of evidence (i.e. a judgment backed by high weight in Good's sense) will often require a significant quantity of evidence (that is, weighty evidence in Keynes's sense) before being displaced provides yet another connection between the two notions and another opportunity for confusion. Though I will not pursue it here, this notion of resistance to alteration in the light of further evidence represents an aspect of what Peirce means by stable or settled belief in "Fixation" and elsewhere.[31] It is odd that Peirce draws no explicit connection between what he calls weight of evidence and his notion of stable belief; if he counted on his readers to make the connection, that expectation has been disappointed. It is even odder that Peirce connects his discussion of the amount of evidence on which a probability assessment is based neither to the weight metaphor nor to his discussion of stable belief. We've seen non-Peirceans like Joyce and Skyrms use terms like "settled" and "resilient" in characterizing the properties or effects of weighty evidence. Peirce himself treats Keynesian weight as a measure of a probability assessment's "liability to be changed by further experience," but he says nothing else linking probability to stability. The two conceptions of evidential weight can be thought of as measuring two distinct components of stability, and it remains to be seen how effectively these quantitative treatments of stability can be integrated with the better-known qualitative treatment that stability receives in the first of the *Illustrations*. One might think, for instance, that Good's notion measures the extent to which one's evidence settles the matter of what to believe, while Keynes's notion measures the extent to which one's belief is itself a settled state.[32] The papers on probability in the *Illustrations* suggest that we have missed important nuances in Peirce's conception of stable belief in the first paper of the series.

The main thing preventing Peirce from seeing the links between the two conceptions of weight, no doubt, is the fact that he presents Good's conception of weight of evidence as part of an argument for conceptualism about probabilities, while Keynes's conception of weight is supposed to be the centerpiece of an argument against conceptualism. Why, exactly, is the Keynesian notion supposed to present a problem for conceptualists? Peirce argues that "when our knowledge is very slight, this number [i.e. Keynesian weight] may be even more important than the probability itself; and when we have no knowledge at all this completely overwhelms the other, so that there is no sense in saying that the chance of a totally unknown event is even (for what expresses absolutely no fact has absolutely no meaning), and what ought to be said is that the chance is entirely indefinite" (W3 295-6). Peirce seems to be making two different points here. He is less than a page away from arguing that there is no non-arbitrary, consistent way of dividing up the space of probabilities, and so part of the argument is supposed to dovetail with something like Bertrand's Paradox. Peirce is arguing that there's no acceptable way to get prior probabilities that "express a fact," either through some principle of indifference or through a more subjectivist approach. But Peirce is running an argument somewhat akin to Popper's Paradox of Ideal Evidence in tandem with this argument.[33] Without tracking something like Keynesian evidential weight, Peirce suggests, the conceptualist cannot distinguish between the feeling of belief appropriate to a run of twenty black beans at the start of sampling and a net difference of twenty beans after a thousand samples have been taken. "[A]ccording to the rule of *balancing reasons,* since all the drawings of black beans are so many independent arguments … an excess of twenty black beans ought to produce the same degree of belief … whatever the total number drawn" (W3 296). So Peirce's rather quick and casual critique of conceptualism seems to have two main components. First, conceptualists cannot get the prior probabilities they need without trying magically to transform ignorance into knowledge. In addition, they allegedly cannot make room for a notion of Keynesian weight of evidence and cannot make their Turing-Good notion of evidential weight reflect important differences in evidential situations without supplementing it with the Keynesian one.

Bayesians and other conceptualists have, of course, developed powerful responses to these 1878 criticisms. Peirce often has philosophically unsophisticated conceptualists like Quetelet in mind, and he could not have anticipated the Bayesian innovations of the last hundred years. In particular, the ability of Bayesians to represent ignorance via imprecise credences goes a long way toward disarming Peirce's most deeply felt objection, namely that Bayesians try to convert ignorance into knowledge.[34] Peirce also seems not to have anticipated the prospect of a reconstruction of the notion of gross evidential weight along conceptualist lines. Keynes and Joyce are conceptualists, after all, and recent work has shown that Bayesians have resources for developing a notion of Keynesian weight out of conditional or second-order probabilities. So, unless Peirce makes his insistence that probabilities must be grounded in facts question-begging against conceptualists, he does not have decisive arguments against his contemporary opponents, however effective they might have been against Quetelet, and others who, as Peirce describes them, would reduce inductive inference to arithmetic.

Though contemporary conceptualism is more formidable than Peirce could have realized, the alternative to it that he presents in “Probability” is remarkable for its time and remains ~~formidable~~ credible today. This frequentist approach figures crucially in Peirce’s argument against conceptualism and, in particular in his argument that Keynesian weight is more important than Turing-Good weight. Peirce contrasts the ambition of conceptualism with his more modest approach, which more closely restricts itself to the facts. Conceptualism tries to assign probabilities to hypotheses, which Peirce interprets as a misguided attempt to ascertain how frequently a particular statement would be found true. In a justly famous line, Peirce critiques this approach: “The relative probability of this or that arrangement of Nature is something which we should have a right to talk about if universes were as plentiful as blackberries” (W3 299). Instead of asking how probable it is that the fact will accord with our conclusion, says Peirce, we should ask the probability that our conclusion will accord with the fact. So, instead of trying to assign probabilities to all of the possible hypotheses concerning the distribution of black and white beans in the bag, Peirce anticipates Neyman-Pearson confidence interval estimation. He assigns probabilities to procedures rather than hypotheses and uses Keynesian weight to determine the probability that a given procedure will yield a result within a specified range of the correct value. This is why two numbers, not one, are needed to represent our epistemic situation, and it is why the balance of reasons (conceived of as Good suggests) does not adequately reflect what the data tell us. We are asking, of this world, how trustworthy or reliable our procedure is. We are not asking of all possible (or nearby) worlds, how often our conclusion is true. Treating data statements as inputs into a procedure, according to Peirce, prevents us from making knowledge out of ignorance by assigning probabilities to hypotheses when we do not have frequencies on which to ground such assignments, while allowing us to avail ourselves of the real utility of the notion of probability, viz. telling us the degree of assurance that attaches to our procedures in the long run. As Peirce says, each of us is an insurance company (see W3 283).

What is noteworthy here is not only the extent to which Peirce anticipated the Neyman-Pearson approach to statistical inference. As Deborah Mayo has argued, Peirce engages with philosophical issues about ampliative inference to a much greater extent than do Neyman, Pearson, and their successors. Much has been learned about conceptualist and materialist approaches to probability since Peirce's time, but I hope to have made a case that much can yet be learned by revisiting underappreciated texts by thinkers who were wrestling with these issues. In particular, I hope to have shown that the notion of weight of evidence has an earlier and more central place in the history of philosophical thinking about probability than has usually been understood, and that it remains a fruitful topic today.

[1] See Carnap, p. 554, Popper, p. 406, Good 1985, p. 267, Ullian, pp. 96-97, and O'Donnell, p. 76.
[2] Levi in his 2011 deserves credit for a clear recognition of the distinction, but he does not pursue the matter.
[3] See W3, pp. 14-109.
[4] For more on the role of deliberately choosing, rather than merely picking, a method of inquiry, see Kasser 2011, pp. 233-234.
[5] For a recent and detailed discussion of the pragmatic maxim, see Burke 2013.
[6] I am grateful to Cheryl Misak for directing me to a more recent paper by Levi in which he seems to deny that Peirce countenaces degrees of belief, even when betting rates can be grounded in statistical information. See Levi 2012, p. 157.
[7] For some details involved in the derivations, see Schum, p. 216.
[8] For a somewhat similar discussion, see Levi 1995, p. 67.
[9] See Joyce 2005, p. 156.
[10] I do not mean to be making the stronger claim that Peirce's way of thinking about probabilities is fully *neutral* between conceptualist/Bayesian and materialist/frequentist approaches. As Adler argues, partial belief "points inward toward the inquirer" and one cannot "look through" partial belief to the world, as one arguably can for full belief. So I do not mean to deny that Peirce's insistence that probability statements must rest on facts puts conceptualists at a disadvantage. I merely insist that it's not an insurmountable one. See Adler 2002, p. 237.
[11] Unfortunately and surprisingly, Schum's valuable discussion gets Peirce quite wrong on this point. Schum writes that "one trouble with Peirce's idea is that logarithms of Pascalian probabilities are always less than or equal to zero." That's true enough, but Peirce makes it quite plain that he is taking the logarithms of chances, not probabilities. Schum is misled by Peirce's use of "chance." ~~He mistakenly insists that~~ Schum doesn't see the connection Peirce forges between chances and odds. He then goes on to commend a log-odds approach, not seeing that it is Peirce's.
[12] Schum, p. 217. Once again, Schum's valuable discussion is insufficiently charitable to Peirce. Schum claims that "one matter Peirce did not consider is that evidence having any force at all causes a *change* in belief, which we must determine in order to grade the force of evidence in Pascalian terms" (ibid). Peirce's discussion is too compressed, but a mere consideration of the fact that "Probability" is part of a serious that continues Peirce's doubt-belief theory of inquiry as articulated in "Fixation" renders it highly improbable that Peirce ignored the fact that we always set out from a state of belief and that inquiry involves the updating of prior opinions. On the other hand, it must be granted to Schum that Peirce's distrust of principles of indifference (on which more below) and his insistence that probability judgments must represent a fact (as discussed above) make it difficult to see how, exactly, prior states of belief are to be represented for Peirce. Schum is right that weight of evidence can't be made to link up to appropriate feelings of belief unless there is some prior belief state involved. I am only gesturing in the direction of an answer here. This matter deserves a paper of its own.
[13] For further discussion see Levi 2010, pp. 39-40, and Schum, pp. 218-220.
[14] See W3 247 and 263.
[15] Good sometimes credits Turing rather than Peirce with the suggestion of the log-likelihood measure. See Good 1983, pp. x-xi and 36-38. The complications arise because of a dispute about how adequately Peirce characterized what it is for two arguments to be independent of one another. For an exchange between Good and Levi on this topic, see Good 1981 and Levi 2011, p. 40. For Good's treatment of the issue more generally, see Good 1984. Schum, p. 229, credits Good and Turing with being the first to propose the log-likelihood measure of weight. But this is because Schum has misunderstood Peirce, as we've seen.
[16] Good 1985, p. 253. Also see Good 1983, p. 124 and Gillies, p. 144.
[17] Schum is an important exception. Cheryl Misak has pointed out that the interpretation of Ramsey on this score is more contentious than is generally recognized.
[18] See, for example, the exchange between Good and Seidenfeld in Good 1985 about whether Good's conception of weight of evidence captures Popper's notion of corroboration. For a contemporary survey of Bayesian approaches to incremental confirmation, including the log-likelihood ratio, see Fitelson.
[19] See Good 1985, p. 253 and p. 267.
[20] O'Donnell, p. 70, suggests a qualification. When the probability of a statement is 0 or 1, he suggests, weight is automatically at a maximum.
[21] This is a simplification. What Joyce actually says is that Keynesian weight "stabilizes not the probabilities of the chance hypotheses themselves, but their probabilities discounted by the distance between X's chance and its credence" (p. 166).
[22] By "independent," I don't mean that the value of Keynesian weight can't be captured in terms of, say, belief probabilities. Joyce offers an illustration of how that might work. I just have in mind the minimal sense that improvement along one dimension of one's evidential situation is compatible with weakening along another.
[23] Levi 2011, as noted, is the shining exception. He alone seems to be clear about the distinction between net and gross weight of evidence in Peirce, but he doesn't seem particularly interested in developing it. That paper, despite its title, does not offer a sustained examination of the two approaches to evidential weight, but instead a comparison of

Peirce and Keynes on a number of issues.
[24] See Cottrell on this point.
[25] See Good 1985, pp. 264-268.
[26] For a bit of discussion of nearness to proof and of the stopping problem, see Levi 2011.
[27] For some discussion, see Adler, p. 252.
[28] For a recent treatment, see Misak.
[29] This forms a crucial component of Peirce's solution to Peirce's Puzzle, discussed earlier.
[30] Weatherson, pp. 52-53. As Joyce points out, this amounts to identifying Keynesian weight with what Joyce calls specificity of evidence, i.e. the extent to which the evidence favors one hypothesis rather than its competitors.
[31] The most extensive discussion of the notion of settled belief in Peirce is in Kasser.
[32] This formulation is merely suggestive and omits many important matters of detail. I elide the distinction Joyce draws between specificity, viz. "the degree to which the data discriminates the truth of the proposition [in question] from that of others" (p. 154) and both force and weight, and I ignore such matters as that a belief can be highly resilient in the face of some kinds of data while remaining highly susceptible to being undermined by other kinds of data.
[33] See Popper, pp. 406 ff.
[34] See Joyce, pp. 170-171.

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