

Nine Remarks Provoked by Two Recent Papers by Grünbaum (1977)

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HISTORICAL NOTE

This paper was written in 1977, but not published. Notes have been added in 2010 and 2014 amplifying and, in some places, correcting the text. They are indicated by small roman numerals in the margin, and are to be found, together with an augmented bibliography, on pp. 15–24.

0 Introductory Metaremark

The titles of two recent papers [1976a], [1976b] by Professor Grünbaum¹ pose two questions, negative answers to which would seem to strike at the heart of the methodology developed by Sir Karl Popper ([1934] and later writings). Grünbaum does not exactly answer these questions in the negative, nor in the affirmative either; after involved discussion he simply concludes that Popper has in neither case adequately defended an affirmative answer (pp. 22f., 136). On the subject of the first of these questions — ‘Can a theory answer more questions than one of its rivals?’ — it seems to me that Grünbaum’s exposition is substantially correct — as substantial as it is correct, no doubt, but nevertheless on pretty well the right lines. However, I am rather unimpressed by Grünbaum’s impressive arguments for his conclusion. What is more, a negative answer here, though undeniably serious, does not appear to be a complete disaster. My first remark elaborates on this.

With respect to Grünbaum’s second question — ‘Is the method of bold conjectures and attempted refutations *justifiably* the method of science? — I am oppositely placed. Here I do think that a negative answer would be a catastrophe (I assume that the word ‘justifiably’ is not taken too seriously); but I do not think that there are any valid grounds for condoning one. In his treatment of this question Grünbaum discusses briefly (and, in my view, unintelligibly) a probabilistic argument (see my fourth remark), and at considerably greater length a sheaf of arguments deriving from Popper’s theories of verisimilitude (seventh remark). In the course of these criticisms he makes a host of misleading and even misguided comments concerning content measures (second remark), the determinateness of probability measures (third remark), qualitative verisimilitude (fifth remark), quantitative verisimilitude (sixth remark), and the supposed role of the method of induction by enumeration (eighth remark). Nowhere, however, does he mention the simple logical argument to which falsificationism makes appeal; an argument which — in my view, anyway — gives a clear affirmative answer to Grünbaum’s second question. This is the substance of my ninth, and concluding, remark.

¹ Grünbaum has published at least two other related papers [1976c] and [1976d] in the course of the year. The task of sifting these for truth I leave to others. Incidentally, since the two papers under advisement here appear in the same volume of this *Journal*, I shall in references not distinguish between them, but merely cite a page number.

Two considerations have roused me to utter these nine remarks. One is the apparently merciless thoroughness with which Grünbaum's two papers are written, which is exaggerated, if anything, by the prestigious setting in which they appear. The papers are not easy to read; and many, I suspect, will prefer to accept their conclusions than to dismiss them unread. The second consideration is more personal: throughout his second paper Grünbaum makes frequent, on the whole distinctly flattering, references to published work of my own. But the aspects of my papers that he praises are certainly no more important than the aspects that he disregards, and it is in these neglected portions that I have already argued for stronger versions of some of the points Grünbaum attempts to make and against weaker versions of many of the others (see especially my first, fifth, sixth, eighth, and ninth remarks). Of course I am sympathetic to some of the things that Grünbaum has to say. But it must not be supposed that, because Grünbaum appears not to disagree with me, I therefore do not disagree with him.

1 Questions and Answers

In his [1972], pp. 52f., Popper suggested that we could compare the contents of two set-theoretically incomparable (in particular, conflicting) theories by collating not just their consequences but the questions that they answer; two theories have the same content if they answer the same questions, and so on. He asserted further that by these means we could compare the contents of Newton's and Einstein's theories of gravitation, and 'Einstein's has the greater content' (*loc. cit.*). Section II of my [1975b] was devoted to showing that for quite general reasons the proposal cuts no ice: if B does not entail A , and B is not complete, then there is a question that A answers and B does not. For let a be a consequence of A that is no consequence of B , and c be a sentence that is not decided by B . Then to the question 'Is either a or c true?' A gives a Yes answer and B gives no answer. Amongst incomplete theories, therefore, this method of comparing contents reduces to the standard ('Tarskian') method of set-theoretical comparison.

The argument is of course a trivial one, in the sense of being intellectually undemanding. But that is hardly a good reason, I should have thought, for discounting it. Grünbaum, however, does no more than mention the argument: '... David Miller ... has given some avowedly only trivial examples to impugn the thesis which I have called "Q"' (pp. 10f.). He at once proceeds to consider five sets of counterexamples to the view that Einstein's theory answers more questions than does Newton's. They consume more than eleven pages of text. Yet as Watkins ([1978], p. 361) observes, every one of the damaging questions that Grünbaum supplies rests on some presupposition that is part of Newton's theory but denied by Einstein's. Every one of them is therefore 'answered' by Einstein's theory if we permit the repudiation of a presupposition to count as an 'answer'. At the end of his paper Grünbaum himself notes this (p. 22): 'Any question asked in either theory which is predicated on an assumption denied by the other or not even *statable* in the other's conceptual framework will ... at best ... be obviated by the other'. He then mentions an example due to Glymour that 'would seem to show that countenancing obviation of a question as being an "answer" does not disarm *this* example as a counterexample' (*loc. cit.*). Unfortunately Grünbaum gives us precious little idea what this counterexample of Glymour's is (it is referred to also on p. 12). All that does seem clear is that, like all the others, it depends on some special features of the theories of Einstein and Newton.

Thus although Glymour's example may show that these two theories are not comparable, it hardly constitutes a demonstration that no two conflicting theories can be compared for

question-answering capability. Indeed, I do not see any of Grünbaum's examples as amounting to anything like a general proof. For suppose that we do not allow 'obviation' of a question to count as an 'answer' (though I am sure we should). Grünbaum has certainly not shown that for any two theories A, B there is bound to be a question askable in A that is 'predicated' on an assumption' denied by B (or even on any assumption at all)²; nor (see pp.21f.) is it the case that A and B need each have a different 'conceptual framework'. Grünbaum's discussion, then, even when most charitably regarded, falls somewhat short of the level of generality of my own.

If conflicting theories are not comparable, as they seem not to be, in the way Popper suggested, is there any hope of comparing them in some other way? As Grünbaum rightly emphasizes (p. 1, paragraphs (ii), (iii)), it is of some importance for rationalists that the answer should be affirmative. (Grünbaum's paragraph (i) is of much less importance; see the seventh remark below.) At the moment, I admit, we do not have much reason for imagining that the answer *is* affirmative. But the problem has not yet attracted much attention, and no very concerted effort has been expended on it. Watkins's ingenious and elegant alternative solution ([1978], pp. 336–370, 372–376) is, in my opinion, demonstrably defective (*op. cit.*, pp. 371f.). But it shows how little are the possibilities yet exhausted. We must not of course try to ignore the problem, but neither must we despair too soon. 'Bend your brain to the problem, Jeeves. It is one that will tax you to the uttermost.' (Wodehouse [1930a], p. 103; [1930b], p. 101).

2 Content Measures

According to Popper [1959], appendix *vii, 'There can be no doubt that the content or the logical strength of two universal theories can differ greatly' (p. 373). But 'all universal theories, whatever their content, have zero probability' (*loc. cit.*). Thus 'these differences in content and in testability cannot be expressed immediately in terms of the absolute logical probability of the theories a_1 and a_2 , since $p(a_1) = p(a_2) = 0$. And if we define a measure of content, $C(a)$, by $C(a) = 1 - p(a)$, as suggested in the book [[1959]], then we obtain, again, $C(a_1) = C(a_2)$, so that the differences in content which interest us here remain unexpressed by these measures' (p. 374).

The last three pages of appendix *vii are taken up with a discussion of this problem. Popper suggests a number of ways in which we may represent what he calls the "fine structure" of content, and of logical probability . . . [so as] to differentiate between greater and smaller contents and absolute probabilities even in cases where the measures $C(a)$ and $p(a)$ are too coarse, and insensitive to the differences; that is, in cases where they yield equality' (p. 375).

Despite this apparently clear appreciation on Popper's part of the issues involved, Grünbaum writes (with ct instead of C for the measure of content) that Popper 'believes himself entitled to claim that whenever B unilaterally entails A , $ct(A) < ct(B)$ ' (p. 111). He denounces it as a 'serious inconsistency between Popper's account of the relation of logical probability to Tarskian content, on the one hand, and his *reductio ad absurdum* attempt to establish "the impossibility of an inductive probability" ([1959], p. 363), on the other' (p. 131). In support of this attribution Grünbaum produces (p. 130) some quotations from Popper's [1972] that *could* be taken to express the view that *probability decreases strictly monotonically with increasing*

²The following is perhaps a way of constructing such a question whenever A and B conflict; let a be a consequence of A that is denied by B . Then the question 'What is the explanation of a ?' is, according to Grünbaum, 'at best obviated' by B . See his example 4(a), p. 22.

content, but only, I think, by someone who was unfamiliar with those last pages of appendix *vii of Popper's [1959]. Grünbaum quotes from this appendix (in the last quotation above) and also (p. 130) refers to it.

viii There is, of course, no inconsistency (let alone 'serious inconsistency') in what Popper says. Nor need there be, in my opinion, even if Popper acquiesces in what Grünbaum imputes to him, the assertion that if B unilaterally entails A then $ct(A) < ct(B)$ and $p(B) < p(A)$. Of course, he would then have to modify the thesis that universal hypotheses all have zero probability. But all this is possible, and the critique of inductive logic largely unaffected, if our probability measures are allowed to take values not solely in the reals but in some non-Archimedean extension thereof; that is, if we choose a *strictly positive* measure, for which $p(A) = 0$ if and only if A is a contradiction. (A finitely additive such measure can always be found if our non-Archimedean field is large enough; see Nikodým [1960].) It would then follow that the stronger of two comparable theories would have a definitely lower probability. And arguments such as Popper's ([1959], appendix *vii) might incline us to distribute no more than infinitesimal probability to any universal theory. Compare Jeffrey [1975], p. 150: 'Popperians would do well to count $p(H)$ as infinitesimal but not 0 when H is a universal scientific hypothesis The fact that Popper has not used them need not mean that infinitesimals are unPopperian.' Indeed, one would do well to assign different orders³ of infinitesimals to theories of markedly different strength.

ix Under such an appropriation it turns out that for any theory B and non-tautological prediction E from B we will have $p(B, E) > p(B)$. For when none of the relevant probabilities is zero this condition is equivalent (see Popper [1959], p. 388) to $p(E, B) > p(E)$; which manifestly holds when B entails E and p is strictly positive. Thus *the probability of a theory may be raised by finite evidence*. However, *no finite evidence can raise the probability of a theory to an infinitesimal of a lower order*. It continues to hold, therefore, that 'a higher degree of corroboration will, in general, be combined with a lower degree of probability' (Popper [1959], p. 363). In other words, the introduction of infinitesimals, though of some value in other fields (see the sixth remark below), can offer very little help to inductive logic.⁴

3 The Determinateness of Probability Measures

x In addition to foisting on Popper the view that logical probability is a strictly positive measure, Grünbaum also ascribes to him the thesis that (p. 132) 'though not normally known to us, the measures $ct(B)$ and $ct(A)$ are *determinate*'. He mysteriously qualifies '*determinate*' with the phrase 'at least up to assuring a linear order such that $Cn(A) \subset Cn(B) \rightarrow ct(B) > ct(A)$ '. But since a strictly positive function p that takes values in an ordered field is bound to generate a linear ordering satisfying this condition, the qualification reduces the thesis to the earlier one. Moreover, Grünbaum's expressed doubts (*loc. cit.*) in no way tell against such a weak 'determinateness'. I presume therefore that something more must be intended.

But just what more is not entirely clear. Grünbaum's *references* to Popper [1972] support, as far as I can see, *at most* the view that Popper uses 'the logical probability of A ' and ' $p(A)$ ' as singular terms or definite descriptions (rather than as variable terms). Grünbaum's *doubts*

³ Two infinitesimals ξ and η are of the same order if for some standard real number r we have $\xi \cdot r = \eta$. But if ξ is infinitesimal its powers ξ^2, ξ^3, \dots are of progressively higher orders.

⁴ For some of the issues surrounding modern uses of 'inductive probability', see my [1975a], especially pp. 141f.

are first that ‘logical probability measures are ... language-dependent’ (a defect that for some reason he thinks Tichý’s heavily language-dependent [1976] might help us to overcome); and secondly (here I summarize) that there are for any language L endlessly many distinct probability measures permitted by the calculus of probability.

But the uncontroversial fact that there is no unique abstract probability measure on L does not in itself show that ‘the logical probability of A ’ is not a singular term (compare the case of physical probability). Nor (despite Howson [1973], p. 157) would this follow from the fact (if it is a fact) that for no contingent A and numeral λ is ‘ $p(A) = \lambda$ ’ a logical/mathematical theorem.⁵ It is something like this, I suspect, which is customarily meant by the ‘indeterminateness’ of a probability measure. And, indeed, Popper endorses this sort of indeterminateness, as is apparent from the passages that Grünbaum cites, and from his [1963], addendum 4 (Popper clearly means *numerals* by the term ‘numerical values’). xi

Grünbaum’s only objection to the thesis that logical probability is strictly positive was that (p. 130; see also p. 131) ‘the mathematical probability calculus does *not* entail it’. One might well ask: which ‘probability calculus’? Why does Grünbaum restrict himself to a theory which lacks this thesis? After all, it merely formalizes the condition of strict coherence. On the question of ‘determinateness’, too, he seems to *criticize* Popper for assuming more about logical probability than that it is a probability measure. But a geometer, for example, may well assume more about a distance function d than that it obeys the metric axioms. Perhaps the proliferation of available metric operations for space means that the distance between two points is ‘indeterminate’ or ‘fictitious’. But then the fact that we get along so well notwithstanding must surely suggest that the same may be true in the theory of logical probability. For reasons such as these I am unconvinced that the ‘indeterminateness’ of logical probability gives rise to any serious problem. xii

4 Severity of Tests

I come now to a section of Grünbaum’s [1976b] that I find genuinely bewildering: his section 2. He purports to prove there that Popper is unable to explain the virtue of imposing severe tests; but that, surprisingly, the Bayesian can explain it. It seems to me, however, that Grünbaum’s argument rests on a couple of quite elementary muddles, and that once these are cleared away little substantive objection remains.

Grünbaum in this section simply assumes the existence of some ‘logical’ probability measure, talking in fact (p. 108) of ‘*inductive* mathematical probabilities of hypotheses[,] which are, of course, anathema to Popper’; a qualification I fail fully to understand (given what he says later about content). Although I shall follow Grünbaum in the assumption that unless B contradicts

⁵ It is worth comparing the situation here with the Sorites paradox (Black [1963], Cargile [1969]). Some numbers are small, some numbers are not small; it follows by elementary reasoning that there is a (unique) least positive number that is not small. If $n + 1$ is this number then n is small while its successor is not. This conflicts with what seems an obvious principle: the result of adding 1 to a small number is itself small. In particular, any attempt to ‘specify’ n seems to lead to an absurd conclusion.

What the argument does show is that we may construct a ‘mathematical’ number designator u , namely ‘the least number that is not small’, that has a unique reference despite the fact that for no numeral λ is the sentence ‘ $u = \lambda$ ’ a logical/mathematical theorem. In these terms the Sorites paradox can be seen to be caused by an illicit inference from there being no numeral λ such that ‘ $u = \lambda$ ’ is plausible to the conclusion that for no number n is ‘ $u = n$ ’ is plausible. But of course $u = u$ is extremely plausible. See also my [1977], where, incidentally, I argue that there is no intelligible distinction between ‘mathematical’ and ‘factual’ number designators.

A the probability of A given B exceeds zero, I shall boycott his adoption of Reichenbach's kinky notation for relative probabilities. Thus the probability of A given B will be written $p(A, B)$. (Minor changes will accordingly be made in some of the quotations from Grünbaum.)

Let B henceforth be background knowledge. Grünbaum initially supposes (pp. 107f.) that prediction C is a consequence of hypothesis H , but inconsistent with B . Thus $p(C, H) = 1$ and $p(C, B) = 0$. Given that we have in our possession no other theories that predict C , the experiment E that tests for C is the simplest case of what Popper calls a 'severe' or 'crucial' test of H . For on the basis of background knowledge B we certainly do not expect H to survive the test.

According to Grünbaum, 'the fact that H makes a prediction C which is incompatible with ... B does *not* justify the following contention of Popper's: A *deductivist* is entitled to expect the experiment E to yield a result *contrary* to C , unless H is true' (p. 108).

xiii There are two insidious ambiguities in the statement of this supposed contention: one concerns the word 'unless', the other the word 'expect'. The word 'unless' suggests that what is at issue is a (material) conditional with *antecedent* ' H is false', and some such consequent as ' $p(C)$ is small' or ' $p(C, B)$ is small'. However, there is a great difference between such a conditional 'if D is true then $p(A) = r$ ' and the corresponding statement of relative probability ' $p(A, D) = r$ '. Admittedly, writers seldom distinguish the two in vernacular transcriptions (see Popper [1966], pp. 62f., and Miller [1966], pp. 148f.), but this need not be symptomatic of any confusion. In this instance, both the quotation Grünbaum gives from Popper (p. 107) and the one he gives from Salmon (p. 108) must be read as statements of relative probability; if only because the 'conditional' interpretation makes probabilities unwelcomely 'dependent on the facts' (see also Popper [1963], addendum 2). Grünbaum himself seems to favour this interpretation when he refers (p. 109) to 'the assertion — made by Popper and Salmon alike — that the value of $p(C, B \ \& \ \sim H)$ is very low'; yet only a few lines earlier he had been stressing that in this formula C must be a 'successful outcome', which suggests that for him $p(C, B \ \& \ \sim H)$ depends on whether C is true or false. However, since neither Popper nor Salmon would condone such a dependence, I am now going to reject this interpretation out of hand.

The ambiguity concerning 'expect' is really a question of whether or not our probabilities are relative to the background knowledge B . My own view is that Popper contends simply that $p(C, B \ \& \ \sim H)$ is small (see the previous paragraph). In the case under advisement *this* contention can hardly be controverted, since $B \vdash \sim H$ and $p(C, B) = 0$. But what Popper says, of course, is *not* justified if it is interpreted to mean that $p(C, \sim H)$ is small; that is, if p is not relativized to B . Still less is it justified if interpreted to mean something like 'if H' (is true and) contradicts H then $p(C, H')$ is small', the only interpretation that, as far as I can see, Grünbaum's *argument* has any bearing on. But that there is no justification for these alternative interpretations is clear from the more general case where $p(C, B)$ is assumed to be small, but not to be zero.

This case Grünbaum also discusses. He shows, essentially, by means of Bayes's theorem, that if $p(C, B \ \& \ H) = 1$ then the smaller is $p(C, B)$ the smaller is $p(C, B \ \& \ \sim H)$. (For if $p(C, B \ \& \ H) = 1$ then $p(C, B \ \& \ \sim H) = [p(C, B) - p(H, B)] / [1 - p(H, B)]$. Indeed $p(C, B \ \& \ \sim H) < p(C, B)$.) He asserts, however, that 'unlike Popper, the Bayesian can then appeal to ... [Bayes's theorem] to deduce correctly that the value of $p(C, B \ \& \ \sim H)$ is very low' (p. 109); so that 'while Popper failed to furnish a deductivistically sound rationale for [this claim] ... the Bayesian is able to underwrite [it] cogently' (p. 110).

I am reluctant to impute to a philosopher I respect a really harebrained thesis, but the only explanation I can give of these quotations is this: Grünbaum thinks that a non-Bayesian is precluded from using Bayes's theorem. (For a relevant comment see Popper [1974], note 68, pp.1185f.) Since Bayes's theorem is a theorem of the calculus of probability and, in the form Grünbaum uses it, not essentially different from the multiplication law, this position is quite obviously absurd. Yet there seems to be no other reason why Popper should be barred from giving the derivation that the Bayesian is allowed to give. xiv

For further comments on severity, relating to a paper of Grünbaum's that I have not seen, I would refer the reader to Watkins [1978], pp.353–355. xv

5 Qualitative Verisimilitude

In section 3(b)(i) of [1976b] Grünbaum sets out to show that 'all the basic defects of Popper's theory of *qualitative* verisimilitude can be demonstrated by the most elementary logical devices familiar to a beginning student of Venn diagrams' (p.114). If A and B are theories the assumption that B is false was the only assumption needed in Tichý [1974] and Miller [1974] to show that Popper's condition (\diamond)

$$\text{Either } A_T \subset B_T \text{ and } B_F \subseteq A_F \quad \text{or} \quad A_T \subseteq B_T \text{ and } B_F \subset A_F$$

could never hold (here A_T, A_F are respectively the truth content and the falsity content of the theory A). Grünbaum trisects this assumption of B 's falsity into 'mutually exclusive special cases' (p.115), namely those where in addition (a) $B \vdash A$; (b) $B \vdash \sim A$; (c) A is independent of B . He notes that Tichý's proof 'encompasses these three cases with admirable elegance and conciseness' (*loc. cit.*)⁶, but feels that in cases (a) and (b) much more elementary considerations are sufficient, as well as being more illuminating. I do not intend to argue against this preference, though I do not share it. But I do wish to make a couple of critical comments on Grünbaum's line of attack.

Grünbaum first considers case (a) when A and B are not logically equivalent. Here, he says, we have an elementary proof that goes through whether B entails A or not. It is sufficient for the proof that B is false and $A \not\vdash B$. xvi

But what is the virtue of separating a situation into three 'mutually exclusive' special cases if *the proof of one of these cases is not exclusive to it*? Clearly, the division into (a), (b), (c) is not a very appropriate one! If one wants to break up the proof one might be better off considering just two cases subsidiary to B 's falsity: (e) $A \not\vdash B$; (f) $A \vdash B$. The proof in case (e) is just Grünbaum's proof in case (a). And for (f) we prove the result very simply by noting that A is false, so that $A \in A_F$; but $A \notin B_F$ unless A and B are interderivable, so that $B_F \not\subseteq A_F$ and (\diamond) fails. This proof for case (f) assumes that A is finitely axiomatizable, just as Grünbaum's proof in case (a) assumes the same for B . In case (c) Grünbaum also assumes that A is finitely axiomatizable. xvii

But the Miller/Tichý result does not depend on any such restriction to finitely axiomatizable theories. Grünbaum's proof, therefore, is considerably less general than either Tichý's proof or my own.⁷ To my mind this loss of generality is a high price to pay for the doubtful advant- xviii

⁶ On p.121 Grünbaum also introduces a case (d). I mention this briefly in the next note.

age of having a more ‘immediate’ proof, for the force of Tichý’s and my criticism of Popper’s condition (\diamond) is not that there are *few* cases of false theories comparable by verisimilitude, but that there are *none*. That there are clear examples of theories intuitively, but not formally, comparable for truthlikeness was, I think, apparent from the very start. What (\diamond) purported to show was that at least in *some* simple cases a genuine comparison was possible. A complete refutation therefore is only to be achieved by showing that in fact there are no such cases.

6 Quantitative Verisimilitude

xix The numerical measure of verisimilitude that Popper proposes in [1963], addendum 3 (and also in [1972], Chapters 2 and 9), has been much less discussed than has his qualitative theory. Students have thought, perhaps, that as this latter theory fares so ill there is little chance of success for a quantitative theory erected on its base. I would largely agree with this pessimistic assessment. Yet despite the paucity of published commentary, it does not seem to me that Grünbaum’s ‘critique of Popper’s *quantitative* verisimilitude ... [contains] results ... not redundant with those of others’ (pp. 114f.). Grünbaum’s discussion in section 3(c)(i), indeed, impresses me as excessively weak, not only in what it seeks to prove, but in the way it professes to prove it. I shall indicate here briefly how an application of the calculations of section 4 of my [1974] can lead to a substantial refocillation.

xx In his [1974] Tichý showed essentially that the measures of truth content, falsity content, and verisimilitude of a false theory A all depend only on the measure of its content. (He also had other complaints (*op. cit.*, p. 159) against Popper’s measure of verisimilitude, complaints that I have been at pains to rebut; see my [1974], pp. 175–177.) This result was independently established in my [1974]. (See also my [1975b], p. 162; the publication ‘1975’ announced there is not now likely to be published in anything like its intended form.) Let us use, as I did there, $\alpha, \beta, \dots, \tau, \dots$ for the probabilities of the theories A, B, \dots, T, \dots . In view of the discussion in my second remark above, I shall assume that α exceeds zero, though perhaps is infinitesimal, as long as A is consistent. (Thus I regard the discussion of section (vi) of my [1972] as superseded.) We can then show that if A is false its truth content $ct_T(A)$, defined by $1 - p(A_T)$, is $1 - (\alpha + \tau)$, [and] its falsity content $ct_F(A)$, defined by $1 - p(A, A_T)$, is $\tau/(\alpha + \tau)$. (Grünbaum’s definition on p. 131 is defective.) If A is true, of course, its truth content equals its content $1 - \alpha$ and its falsity content is zero. It is easily seen that for false theories $ct_T(A)$ and $ct_F(A)$ increase together. It follows that a numerical version of condition (\diamond), what Grünbaum calls ‘Condition C’:

$$\begin{array}{l} \textit{Either} \quad ct_T(A) < ct_T(B) \quad \textit{and} \quad ct_F(B) \leq ct_F(A), \\ \textit{or} \quad \quad ct_T(A) \leq ct_T(B) \quad \textit{and} \quad ct_F(B) < ct_F(A) \end{array}$$

⁷ Of course Grünbaum’s proof could be extended to cover the unaxiomatizable case, but then it would be a lot less ‘immediate’. The same cannot obviously be said of Hempel’s proof (see my [1974], the note on p. 171) as reported by Hattiangadi [1975]; for this involves the construction of the conditional $B \rightarrow A$, which is not even defined generally for unaxiomatizable theories. Tichý’s own proof is actually presented only for finitely axiomatizable theories; but certainly no change is needed for it to embrace the general case.

My own proof ([1974], pp. 171f.) can be much simplified by assuming that *either* (e) $A \not\vdash B$ (as above) *or* (g) $B \not\vdash A$ obtains. By my THEOREMS 1 and 3 respectively it follows in either case that their truth and falsity contents are not even weakly comparable; so that if they are comparable, we must have A equivalent to B . I am grateful to Professor Grünbaum for helping me to find this simplification; I would note too that Grünbaum’s case (d) is effectively covered by the other part of my THEOREM 3.

can only hold if B is true. I used this simple result in my [1974] to argue that it was not the sparseness of the \subseteq -relation that was really to blame for the vacuity of the condition (\diamond).

I also noted (*op. cit.*, p. 168, note 2) that nevertheless one could, according to Popper's *measure* of verisimilitude, have a false B closer to the truth than a suitable A was. It is over to this result, for a slightly modified measure, that Grünbaum's $\mathbf{3}(c)(i)$ is given.

Grünbaum considers only Popper's unnormalized measure of verisimilitude, $Vs(A) = ct_T(A) - ct_F(A) = p(A, A_T) - p(A_T)$. Popper's normalized measure, which I shall call VS , is the following:

$$VS(A) = \frac{p(A, A_T) - p(A_T)}{p(A, A_T) + p(A_T)}.$$

Contrary to *desideratum* (i) of [1963], p. 396, the functions $Vs(A)$ and $VS(A)$ do not increase and decrease together.⁸ Thus Grünbaum's result, as it stands, does not apply directly to Popper's preferred measure. xxi

Grünbaum's demonstration that Condition C is not a 'necessary condition' for $Vs(A) < Vs(B)$ is oddly lacking in both explicitness and rigour. He merely asserts (pp. 128f.) that A may exceed B in truth content, yet exceed it even more in falsity content; or alternatively that B may exceed A in falsity content, yet exceed it even further in truth content. He gives no examples to show that this actually does happen. Here I shall just indicate briefly how they may be constructed.

Suppose that A and B are both false and $Vs(A) \neq Vs(B)$. Then one or other of these Vs values is less than the other. Yet Condition C never holds between two false theories. Thus any false A and B for which Vs takes different values will provide the needed counterexample.

Assume $\beta < \alpha$. Then it is easily checked that $Vs(A) < Vs(B)$ if and only if $\tau < (\alpha + \tau)(\beta + \tau)$. In particular, therefore, if $0 < \tau < \beta^2 < \alpha^2$ then $Vs(A) < Vs(B)$.

A similar result holds for VS . Here it is sufficient for false A and B that $\tau^2 < \alpha\beta$. Thus if $0 < \tau < \beta < \alpha$ we have $VS(A) < VS(B)$. We might paraphrase these results as follows: the stronger of two false theories is closer to the truth, provided it is not *too* strong. I do not intend to defend this conclusion here, but I shall refer to it in my next remark. xxii

Grünbaum seems surprised (p. 128) that 'the requirements for greater *quantitative* verisimilitude are much *less stringent* . . . than the requirements for an excess of *qualitative* verisimilitude' (p. 129). But it is hardly surprising, given that a linear ordering is richer than a partial ordering. Indeed, if Condition C were to generate a linear ordering, we would be able to measure verisimilitude by truth content alone.

7 Verisimilitude and Falsificationism

The main purpose of Grünbaum's paper is to assess the importance of Popper's claim that 'the stronger theory, the theory with the greater content, will also be the one with the greater verisimilitude *unless its falsity content is also greater*' ([1972], p. 53). For Popper had held that

⁸ For a simple counterexample take the Carnapian language \mathfrak{L}_3^2 , which has two primitive predicates and three individual constants. There are 64 state descriptions. Let T be the true one and A and B false ones. Since their probabilities can be assigned independently (provided they sum to less than 1) we put $\alpha = 1/64$, $\beta = 7/64$, $\tau = 1/64$. It is routinely checked that $Vs(A) = 30/64 < 48/64 = Vs(B)$ but $VS(A) = 30/34 \not< 48/64 = VS(B)$.

this assertion ‘forms the logical basis of the method of science — the method of bold conjectures and of attempted refutations’ (*loc. cit.*).

Grünbaum calls the first claim ‘Proposition P ’. It is true, but has nothing like the importance Popper once attached to it. For if B is stronger than A its falsity content will also be greater than A ’s, unless they are both true (Miller [1974], p.168; Grünbaum quotes this on p.114). Thus in no interesting case can anything be concluded about their relative verisimilitude.

Grünbaum also attributes to Popper a proposition called P° , which is like P except that ‘the theory with the greater content’ is here taken to mean ‘the theory with the greater *generalized* content’. Since, however, we have no ‘generalized sense of content’ (my first remark above) we are in no position to assess this proposition. Grünbaum rightly, I think, says (p. 114) that it is ‘at best ill-founded’ (though he also says on the same page that it is ‘at best unfounded’). I shall say no more about it here except to suggest that if in P° we generalize the idea of content we must be prepared also to generalize the idea of falsity content. (See my [1975*b*], pp.166*f*.)

A second variant of P that Grünbaum considers is called $P^\#$. This is a quantitative variant, likewise ‘at best unfounded’ and ‘at best ill-founded’ (*loc. cit.*), but also described by Grünbaum as ‘at best gratuitous’ (p. 113). I shall show that, for all that, it is *true*.

In slightly amended notation Proposition $P^\#$ is the following (p. 113):

$$\text{If } ct(A) < ct(B) \text{ and } ct_F(B) \leq ct_F(A) \text{ then } Vs(A) < Vs(B).$$

The left conjunct of the antecedent amounts to $\beta < \alpha$. Thereafter there are four cases, depending on the truth values of A and B . Remember that if A is true then $Vs(A) = ct(A)$.

If A and B are both true then $P^\#$ is clearly true. If A is true and B is false, $P^\#$ is vacuously true, because $ct_F(B) \not\leq ct_F(A)$. If A and B are both false it is also vacuously true, since the antecedent reads: $\beta < \alpha$ and $\tau/(\beta + \tau) \leq \tau/(\alpha + \tau)$.

If A is false and B is true, $P^\#$ becomes

$$\text{If } \beta < \alpha \text{ and } 0 \leq \tau/(\alpha + \tau) \text{ then } \alpha/(\alpha + \tau) - (\alpha + \tau) < 1 - \beta.$$

The consequent here amounts to saying $\beta < \alpha + (\tau + \tau/(\alpha + \tau))$, and so holds if $\beta < \alpha$.

The first three cases go through in the same way for VS . In the fourth the consequent becomes

$$\frac{\alpha - (\alpha + \tau)^2}{\alpha + (\alpha + \tau)^2} < \frac{1 - \beta}{1 + \beta},$$

which is the same as $\alpha\beta < (\alpha + \tau)^2$, and again follows from $\beta < \alpha$.

Thus the quantitative counterpart $P^\#$ of Proposition P is *provable*, contrary to what Grünbaum says on p.134. Can it therefore be used to vindicate the method of science? Grünbaum says that it is ‘unavailing’ (*loc. cit.*), for the irrelevant reason that ‘ B ’s unilateral entailment of A does not guarantee that the antecedent $ct(A) < ct(B)$ in the antecedent of Prop. $P^\#$ will be satisfied’ (as we saw in my second remark, this need not be so). But I would agree with his conclusion: it *is* largely unavailing. The main reason was noted at the end of my sixth remark: the stronger of two false theories is closer to the truth, provided it is not *too* strong. (In a rich language this may be satisfied by all axiomatizable theories that we are likely to take seriously.) Thus falsity content drops out of sight. (A related result may be obtained when B is false and A true; provided B is *sufficiently* stronger than A we are bound to have $Vs(A) < Vs(B)$.)

It should be noted that both P and $P^\#$ hold for all theories, axiomatizable or not.⁹ There is no need to invoke Popper's 'theorem on truth-content', or a quantitative variant (Grünbaum, p. 134). The variant suggested by Grünbaum — that truth contents increase monotonically with contents — is of course false in general (let A be the truth content of a false B), but it cannot be proved without additional assumptions even for axiomatizable A and B . For the same reason no general proof is possible that if B entails A unilaterally then $ct_T(A) < ct_T(B)$. But the latter does of course follow from $A_T \subset B_T$. Grünbaum's discussion on pp. 134f. seems hardly aware of these simple facts. xxvi

It is less than surprising, given the weakness of Popper's theories of verisimilitude and generalized content, that one cannot employ P , P° , and $P^\#$ to vindicate the falsificationist method. But these failures cast no more shadow on this method than the existence of an invalid proof does on the truth of a theorem. In my ninth remark I shall briefly remind the reader how the method of conjectures and refutations is to be vindicated. xxvii

8 Induction by Enumeration

Popper has often stressed that 'not only are all theories conjectural, but also all appraisals of theories, including comparisons of theories from the point of view of their verisimilitude' ([1972], p. 58; quoted by Grünbaum on p. 123). How then might one set about testing these conjectural comparisons? Elsewhere ([1975b]), pp. 187f.) I have observed that it is enough if '“experience” can inform us, at least in some cases, that one theory is not closer to the truth than is another'. That is, the claim that B has greater verisimilitude than A must be testable, but it need not be 'verifiable'; for, if we persistently fail to refute the claim, it is corroborated, and we may (with no 'good reason', of course) conjecture it to be true. xxviii

This remark is intended to be quite general; that is to say, such testability is a desideratum of any theory of verisimilitude. The theory incorporated in (\diamond), however, is so weak that there seems little point in investigating how we can refute, or corroborate claims like ' B has greater verisimilitude than A '. Grünbaum nevertheless undertakes this task (pp. 122–125), and concludes that Popper is compelled to rely on 'induction by enumeration'. I shall argue that this conclusion is mistaken.

Suppose B entails A unilaterally. Then the conjecture ' B has greater verisimilitude than A ' says simply ' B is true'. This theory can surely be tested; and therefore it can be corroborated, notwithstanding Grünbaum's warning (pp. 122f.) '*not* to conflate putative corroborations or refutations of the stated conjecture concerning ... falsity-contents ... with ... [those] of the competing theories B and A '. As this passage indicates, Grünbaum is chiefly concerned with the testing of conjectures about falsity contents, in particular the conjecture $B_F = A_F$. He asserts that 'the only possible rival of the conjecture $B_F = A_F$ is $A_F \subset B_F$ ' so that 'if the conjecture $B_F = A_F$ is to be corroborated ... the ... test outcomes ... would either have to refute the rival hypothesis $A_F \subset B_F$ or would have to be “novel facts” with respect to the latter' (p. 123). Now since the 'rival' $A_F \subset B_F$ is, to the extent that it disagrees with $B_F = A_F$, *existential*, it is xxix

⁹ There is a problem here. The class of all deductive theories, axiomatizable and non-axiomatizable, does not constitute a Boolean algebra (or even a complemented lattice), so does not carry a probability measure of the normal sort. (See also Popper [1959], appendix *v.) However, the lattice can be embedded in a larger Boolean algebra; and so if there is a measure on the algebra, it will provide a probability for each deductive theory. For an alternative approach to the problem see my [1976].

not as it stands a falsifiable hypothesis; and, because it is so weak, there seem to be few, if any ‘novel facts’ with respect to it. Thus, says Grünbaum, $B_F = A_F$ can only be warranted by ‘induction’ from ‘those B -predictions which turn out to be *false* [and] are likewise predictions made by the theory A ’ (p. 122).

xxx Although I should usually want to deny that a theory can *only* be tested and (corroborated) if we have already formulated a genuine rival, I am prepared to grant here all Grünbaum’s premises, *except* the supposition that ‘the only possible rival of the conjecture $B_F = A_F$ is $A_F \subset B_F$ ’. It is hard to see why this should be true. After all, B may be a generalization of the theory A ; and on the basis of our background knowledge we may have a pretty shrewd hunch that if B commits any errors that A does not then those errors will be in some narrowly limited domain. Thus we might formulate in rivalry to $B_F = A_F$ the hypothesis $A_F \cap C \subset B_F \cap C$, where C is a drastically restricted (perhaps even finite) class of sentences. Should it turn out that, contrary to expectations, $B_F \cap C = A_F \cap C$, we must be entitled to claim that $B_F = A_F$ has passed a severe test. Thus there is no reason to suppose that conjectures concerning comparative falsity content cannot be corroborated.

xxx The same considerations apply, by and large, to what Grünbaum calls ‘incompatible competitors’ — for example Einstein’s and Newton’s theories. The conjecture $E_F \subseteq N_F$ is here equivalent to the conjecture that E is true. Thus it is surely testable, and so corroborable. Such corroboration does not help us very much, of course. But that is not my point.

9 Falsificationism

The falsificationist method that Popper has recommended is the method of proposing *bold conjectures* and of deliberately attempting to eliminate by *severe tests* the false ones among them. If the method is a good one, why is it a good one? Can it in any way be ‘vindicated’?

xxxii Not many philosophers now would defend the view that our theories can be obtained solely from experience. But they have to come from *somewhere*. A falsificationist would maintain that, provided there is ample opportunity to get rid of the *false* ones and the *irrelevant* ones, it does not really matter a hoot where our theories come from: it may be conscious cerebration, tradition, divine revelation, alcoholic frenzy, random walk through a dictionary, or an ‘inductive machine’ or ‘algorism’. Indeed, the word ‘conjecture’ is meant to imply nothing about the origin of our ideas; only something about the tentative manner in which they are to be entertained.

If we are interested in discovering the truth, we shall want to eliminate (as candidates for the truth) the false conjectures as quickly as possible. The more falsifiable they are, the more easily this will be done; *provided*, that is, we set out determinedly to falsify those that are false. In a nutshell this explains the virtues of *bold conjectures* and *attempted refutations*.

xxxiii Is falsificationism then (in Grünbaum’s words (p. 105)) ‘conducive to the discovery of truth’? One might ask similarly: ‘Is regular medical attention conducive to good health?’ Medical science is powerless in many cases to prevent disease, but it may be able to cure it. So although someone who regularly sees a doctor may have the same chance of contracting a streptococcus infection as does someone who distrusts all doctors, he is much more likely to recover from it than is the other. Thus medical care is conducive to health *not* in the sense that one is less likely to fall ill, but in the sense that one is more likely to be cured of any sickness to which one does succumb.

Falsificationism is ‘conducive to the discovery of truth’ in the same way. It is not pretended that one is more likely to hit on the

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Notes (2010)

- i The paper, written in the early part of 1977, was submitted to *The British Journal for the Philosophy of Science*. One of the Editors (J. W. N. Watkins) criticized it both substantially and stylistically, but expressed a willingness to publish it if some changes were made. The paper was criticized also by Grünbaum, to whom I had sent a copy. I was disinclined to temper my criticisms in the way that Watkins suggested and, after some deliberation, I put the paper aside. Somewhat later I decided to destroy what I had written, and to hope that Grünbaum's interventions in the debates on verisimilitude, the progress of science, and related issues, would for the most part obsolesce. That this has not happened is obvious, for example, from the article (revised on 9.ii.2009) on Karl Popper in the *Stanford Encyclopedia of Philosophy*, which says that 'in the 1970's a series of papers published by researchers such as Miller, Tichý, and Grünbaum in particular revealed fundamental defects in Popper's formal definitions of verisimilitude'. (The *Encyclopedia's* article on truthlikeness (revised on 9.v.2007) does not cite Grünbaum's writings.) Other works that mention Grünbaum's papers benevolently include O'Hear (1980), p. 48, who sets out to 'consider how criticisms by Tichý . . . , Harris . . . , Miller . . . and Grünbaum [[1976b]] . . . have shown the inadequacy of Popper's original attempts to formalize the concept of verisimilitude'; Rivadulla (1986), Chapter IV, entitled 'Lógica de la investigación y verosimilitud. La disputa Popper–Tichý–Grünbaum–Miller–Niiniluoto', especially § 5.3 and § 6.2; Martínez Solano (2004), Chapter 8, § 3.2.3; and Antiseri (2006), p. 81 and p. 262. Grünbaum's [1976a] is one of the five papers selected for the section on verisimilitude in O'Hear (2004). It may be noted in passing that one standard work of philosophical reference gives a different account of the fate of Popper's initial proposal concerning verisimilitude: '[Popper's] theory', says *The Penguin Dictionary of Philosophy*, 'provoked a lively debate, in which Imre Lakatos, Pavel Tichý, Graham Oddie and others raised important objections' (Mautner 2000, p. 591). Hempel's contribution, reported in Hattiangadi [1975], as well as in my [1974] (see footnote 7 above), should by no means be overlooked.

In the interests of identifying more precisely what is worthy, and what is not, in Grünbaum's voluminous criticisms of Popper's philosophy, it now seems best to make my comments publicly available, even though I should now write them differently if I were writing them from scratch. The only critical commentary on Grünbaum's papers that is known to me is Rochefort-Maranda (2003), who agrees with Grünbaum on some points but attempts to defend Popper with regard to others.

I am indebted to Alain Boyer, Diego Rosende, and Kenneth Hopf, for encouragement, and to the Archives of the London School of Economics for providing me with an (unfortunately incomplete) copy of the draft typescript. The present digitally remastered version follows the original typescript closely, but minor slips in the text have been silently corrected if they have no bearing on its content, and some missing details in the references and the References have been added in dark blue type. In particular, all the original references, including page numbers, to a 1977 manuscript of Watkins's entitled 'Reply' have here been changed to references to the published version, Watkins [1978]. Some of the notes of clarification and emendation that have been added in 2010 originated in my correspondence with Grünbaum in 1977. I have not made a serious effort to take account of the host of relevant publications of the last 30 years. Dates in parentheses, rather than brackets, refer to items in the Additional References at the end. In the final note, note **xxxv**, I summarize my criticisms. Only minor errors and irregularities have been corrected since 2010.

- ii I am mortified that the proof given in the text appears to assume that if B implies neither a nor c then it does not imply their disjunction $a \vee c$. This is an elementary howler (which has more than once been made in published proofs of the inadequacy of Popper's theory of verisimilitude). The proof can be repaired straightforwardly in the case that A and B are assumed to be mutually inconsistent, for then there must exist at least one consequence a of A whose negation $\sim a$ is a consequence of B , and for any such a , if B entails $a \vee c$ then B entails c . This assumption of the incompatibility of A and B is implicit in the treatment in Miller [1975b], § II, and explicit in the treatments of Watkins [1978], §§ 7f., and of Popper in supplementary remark (3) on pp. 367–371 of Appendix 2 of his (1979).

What should have been said in the more general case is that although A gives an affirmative answer

to each of the questions ‘Is either a or c true?’ and ‘Is either a or $\sim c$ true?’, B gives no answer to at least one of them. For since B has neither c nor $\sim c$ as a consequence, it cannot answer either question in the negative; and since it does not have a as a consequence, it cannot answer both questions in the affirmative. We should not allow the disjunctive form of these questions to mislead us into dismissing them as technical trickery. Every sentence, after all, is logically equivalent to some disjunction. The same point could have been made by using the two conjunctive questions ‘Are $\sim a$ and c both false?’ and ‘Are $\sim a$ and $\sim c$ both false?’; or by using the two conditional questions ‘Is a true if c is true?’ and ‘Is a true if $\sim c$ is true?’; or in other ways. For instance, if $\{c_i \mid i \in I\}$ is a set of pairwise exclusive and collectively exhaustive sentences, none of which is implied by the theory B , then B cannot answer all the questions ‘Is a true if c_i is true?’. Although the construction given in my [1975b] was artificial, such questions might well be of genuine interest. A and B might perhaps be theories about earthquakes, a the statement that the Longmenshan Fault will be significantly active in 2011, and the c_i statements about the number of reservoirs completed in China during 2010. Whereas we should not expect A or B to settle singular questions about the damming of rivers, it is not impossible that A could imply a and B not do so. Then each of the questions ‘Will the Longmenshan Fault be significantly active in 2011 if there are i reservoirs completed in China during 2010?’ is answered affirmatively by A , but at least one of them is not answered either affirmatively or negatively by B . The example is plainly open to generalization to cases where A asserts a unconditionally and B asserts it only under some conditions (if at all). The conditions themselves need not be singular statements (as they are here). The example is, if anything, improved if ‘if’ is interpreted as stronger than the material conditional.

I suppose that it was the disjunctive form of the example of my [1975b] that induced Grünbaum (who is venially misquoted in the next paragraph of the text) to call them ‘avowedly only trivial’. His letter of 14.iii.1977 contained an assurance that it was only the examples that he deemed trivial, not the argument leading to them, and that he regarded my results and his own as complementary.

- iii This curt dismissal of Grünbaum’s counterexamples now seems to me to be both unfair to Grünbaum and unfair to the truth. Its peremptoriness may be explained, if not excused, by my conviction at the time (and ever since) that Grünbaum’s arguments had been superseded before they were published.

The discussion in the text is a little unfair to Grünbaum because it pays scant attention to the complexity of his discussion, and hardly reveals what we can learn from some of his proposed counterexamples. It is unfair to the truth because (following Watkins 1978, p. 361, and Popper 1979, p. 368) it crudely lumps all the counterexamples together as questions resting on presuppositions that are part of Newton’s theory but not of Einstein’s. In reality there are various syntactical and semantical mechanisms at work. It is not even true to say that Grünbaum’s counterexamples all concern the comparison of Newton’s and Einstein’s theories; the single question in Set 3 ([1976a], p. 21) concerns the special theory of relativity and ‘the Maxwell–Lorentz ether theory’, and the single question in Set 4 (*op. cit.*, pp. 21f.) concerns only variants of classical celestial mechanics (see also *op. cit.*, pp. 8f.).

Grünbaum’s other counterexamples are also grouped according to their subject matter: Set 0 concerns the geometry of physical space; Set 1 concerns the appropriate space–time transformations; and Set 2 concerns the velocity of light. Much more suitable for our purposes, however, is a grouping according to logical structure. Grünbaum’s text offers three types of questions that one theory (usually Newton’s theory N) is claimed to answer and its successor theory (usually Einstein’s theory E) cannot answer: (α) questions that contain singular terms that, according to the successor theory, are empty; (β) conditional questions whose antecedents are denied by the successor theory; (γ) explanatory questions. Questions (0a) and (0b) on p. 11, and (2a) on p. 16 are of type (α). Question (1a), on p. 13, looks like a question of type (β), but this is open to dispute (as noted below); question (2b), on p. 17, falls squarely into this category. Questions (1b) on p. 13, (2c) on p. 18, (2d) on p. 19, and (3a) on p. 21, are of type (γ). Let me first say why Popper’s proposal for the characterization of the content of a theory by the set of questions it answers has nothing to fear from questions of types (α) and (β).

There is a well-known way of understanding questions of type (α) as questions with uniformly negative answers. The affirmative answer to Grünbaum’s question (0a) is the sentence ‘The geometry of the three-dimensional spaces in which gravity acts in accord with the laws of motion and gravitation

is Euclidean’, which, according to Russell’s theory of descriptions (1905), asserts that there is one and only one such geometry. According to Grünbaum’s presentation, Einstein’s theory denies this uniqueness claim, and therefore delivers a negative answer to question (0a), ‘Is *the* geometry of the three-dimensional spaces in which gravity acts in accord with the laws of motion and gravitation Euclidean?’. Russell’s analysis of sentences containing empty terms was, to be sure, contested by Strawson (1950), and defended by Russell (1957). Grünbaum does not mention this controversy, and offers no argument as to why this way of defusing counterexamples of type (α) may not be adopted.

Questions of type (β), if that is what they are, are bewilderingly treated by Grünbaum, but the logical situation is unambiguous and they should really cause no serious problems. The clearest example of such a problem is the generic question (2b), which has the form ‘What happens under ‘initial conditions I involving the motion of a positive mass particle . . . with a super-light velocity?’ (p. 17), which may have a determinate answer within N but is, admittedly, bound to be an odd question to ask within Einstein’s theory E . Since E implies the sentence $\sim I$, it automatically answers in the affirmative the question ‘Is $(E \cdot I) \rightarrow F$ true?’, whatever sentence F is (here \cdot represents conjunction). Yet Grünbaum concludes that ‘since $E \cdot I$ entails every contrary of F as well as F , E cannot answer the question (2b)’. This conclusion seems just to be a mistake on Grünbaum’s part. Question (2b) is not ‘*obviated as ill-conceived*’ by Einstein’s theory, as he describes it, but answered in a multitude of different ways. (This does not mean that E is inconsistent; the different answers are conditionals of the form $I \rightarrow F$. The question is also, of course, answered in many different ways by N , but the conjunction of N ’s answers does not imply $\sim I$.) The situation is the same with regard to other questions of type (β). To what extent question (1a), ‘Are any two punctal event having a non-zero *spatial separation* which is *invariant* in *all* inertial frames under the Galilean transformations necessarily simultaneous?’, is such a question is not clear to me. It is speculatively recast on p. 15 by Grünbaum as a conjunction of three questions; of which the first receives a negative answer in Einstein’s theory, and the second and third are decidedly of type (β). But Grünbaum, at once withdraws this reformulation, writing (*loc. cit.*): ‘while (1a) is querying only whether one *assumedly non-empty* set is included within another *assumedly non-empty* set, E asserts that both of these sets are empty. For precisely this reason, E cannot give an unambiguous yes-or-no answer to (1a) but can only obviate it, whereas N does answer it affirmatively.’ This formulation of question (1a) suggests that Grünbaum regards it after all to be of type (α), since ‘the non-empty set of pairs of punctal events having a non-zero spatial separation which is invariant in all inertial frames’ and ‘the non-empty set of pairs of punctal events that are necessarily simultaneous’ are treated as empty terms within the theory E . I am by no means sure that this is the best way to construe question (1a).

As for explanatory questions (those of the form ‘Why . . .?’, ‘How . . .?’, and so on, including Grünbaum’s question (4a) on p. 22) I now think that Popper should have ruled them out of consideration at the outset, and have formulated the broader idea of content in terms only of yes–no questions. (I therefore withdraw the suggestion made in footnote 2.) The trouble with explanatory questions is that explanation and deduction are so closely linked — only what is logically implied by a theory can be explained by it — that a comparison of the sets of explanatory questions that two theories answer may not differ from the comparison of their logical contents. For example, a contingent sentence and its negation are never comparable by the explanatory questions that they answer (unless they have no explanatory muscle), since they share no contingent consequences. Yet what lay behind Popper’s proposal was presumably the idea that, if A is a singular sentence, A and $\sim A$ should be comparable.

I am not of course putting forward this drastic emendation of Popper’s proposal — that it be restricted to yes–no questions (or no–yes questions, as Noyes 1996, §2, is wont to call them) — in order to defend it against criticism, since I do not wish to defend it against criticism. My wish in this note is only to acknowledge that one part of Grünbaum’s criticism has some force, and to emphasize that the appealingly simple response that can be made to it is not enough. The question, raised in the text, of whether obviating a question counts as answering it, need therefore not be further pursued. Grünbaum’s examples are better laid to rest in other ways.

iv In his written comments Watkins observed that this alone constitute a serious problem for Popper’s

thesis that Einstein's theory is preferable to Newton's. This is true. But remember the universalistic tenor of the title of Grünbaum's paper [1976a].

In a letter of 5.iv.1977 to Grünbaum I remarked that his emphasis on the specific issue of whether the sets of questions answered by Newton's and Einstein's theories are comparable, rather than on the general issue of how we can compare the contents of theories that are not related by logical implication, seemed somewhat characteristic of his approach to philosophical problems. In a lecture that he had recently delivered at the LSE (and is, in one form or another, still delivering, as Grünbaum 2009 makes evident), he had attacked Popper's criterion of demarcation by arguing that psychoanalysis is, contrary to Popper's judgement, empirically falsifiable, and therefore according to the criterion has to be granted scientific status. In the discussion of Grünbaum's lecture I had raised the objection that philosophical theses are not refuted, though their significance may be abridged, by the piling up of historical examples, however salient. On the deep and abiding significance of the falsifiability criterion of demarcation, in particular, I have not changed my mind (Miller 2007, § 1; 2014, § 1).

- v On the sensitivity of Watkins's method of content comparison to the predicament of language dependence, see § 1.1 and § 3.0 of Chapter 11 of my (2006a). This chapter aims to demolish all the principal attempts to defend language dependence as an acceptable feature of comparisons of verisimilitude and content. For references to the most recent fireworks, see Miller (2008) and Miller & Taliga (2008).

One theory of content not adverted to in (2006a) is the 'new theory of content' of Gemes (1994). This theory too suffers from the bane of language dependence. For if u and v are atomic formulas of some propositional language, then according to the definition BCPL1 (p. 605), u is a 'basic content part' of the conjunction $u \& v$, whereas $u \leftrightarrow v$ is not. In an equivalent language with atomic sentences v and w (the latter being understood to be a translation of $u \leftrightarrow v$), w is, in the same way, a basic content part of $w \& v$, whereas $w \leftrightarrow v$ is not. Since $u \& v$ and $w \& v$ are intertranslatable, as are u , $(u \leftrightarrow v) \leftrightarrow v$, and $w \leftrightarrow v$, what has been shown is that u both is and is not a basic content part of $u \& v$. It would be surprising if the theory of verisimilitude that Gemes (2007) builds on this definition of content were not language dependent in much the same way as is the theory of Tichý (1974) and Tichý (1976). It is remarkable that Gemes (2007) is not alert to this possibility.

- vi The three quotations, in this paragraph and the next, from Popper [1959], appendix *vii, are on p. 386, *loc. cit.*, p. 387, and p. 388 of the repaginated Classics edition (2002a).
- vii The text cited by Grünbaum is from p. 363 of the edition of Popper [1959] that was current in 1977. It is on p. 374 of Popper (2002a).
- viii Grünbaum's bleakly unsympathetic reading of Popper's (admittedly imperfect) texts on the variation of probability with content takes little account of the stylistic commonplace of using 'less than' as shorthand for 'less than or equal to' and for 'not greater than' (but not the other way round). In his letter of 14.iii.1977, Grünbaum defended his reading, and countered by asking whether it was fair to him (Grünbaum) to draw attention to the possibility, which he had never denied, of letting the probability measure p take values in a non-Archimedean field. Since intellectual exchanges are not competitions, I can see nothing unfair in my attempt to advance the discussion in this way. Added December 2014: the first author, to my knowledge, to 'suggest the introduction of infinitesimals into the Calculus of Probability' was Polya (1954), Chapter XV, p. 139.
- ix The reference here should be to Popper [1959], p. 389 (p. 404 of Popper 2002a). The text quoted later in the paragraph from *op. cit.*, p. 363, is on p. 374 of (2002a).
- x $Cn(\Gamma)$ is the consequence class of the set Γ , in Tarski's sense; that is, the class of its logical consequences. A is a theory if $A = Cn(A)$, and is a finitely axiomatizable theory if $A = Cn(\{a\})$ for some sentence or proposition a . It should be noted that although Grünbaum's extra condition is superfluous if A and B are finitely axiomatizable theories, in other cases it needs more work. The problem is mentioned in footnote 9, and also in note xxvi below.
- xi The paper Miller [1977] that is mentioned in footnote 5 was published in a revised form as Miller (1978c).

- xii** As reported in my fourth remark, Grünbaum himself gave an example of an interesting (though not unfamiliar) qualitative result that holds for all probability measures, and therefore holds for logical probability even if logical probability is indeterminate. What was shown is that if, in the presence of background knowledge B , the hypothesis H implies the prediction C , then $p(C, B \ \& \ \sim H) < p(C, B)$. For more results of this kind see § 4 of my (2010).

At several places, not mentioned by Grünbaum, Popper expressed strong reservations concerning the need for a determinate metric of logical probability, and endorsed ‘the significance of the topological approach’ ([1959], p. 404f; 2002a, pp. 421f.). For some discussion, see § 4.0 of my (2006b). Added December 2014: a similar point is made by Polya *op. cit.*, Chapter XV.5.

- xiii** Much of this paragraph may seem pedantic. Yet at least two other authors have thought it worth the effort to distinguish between the two statements ‘if D is true then $p(A) = r$ ’ and ‘ $p(A, D) = r$ ’ (and between both of them and the statement ‘ $p(D \rightarrow A) = r$ ’). See Cohen (1977), Chapter 3, note 21, and Butterfield (1992), p. 252, and (1993), pp. 2271f., where further references are given.

The reference in the text to addendum 2 of Popper [1963] is a little mysterious. I may have intended to refer to formula (22) of addendum 3, which states that $p(D \rightarrow A) - p(A, D) = ct(A, D) ct(D)$, and hence is never negative. But I may have wanted to call attention to the odd phrase ‘the severity of ... [a] test interpreted as supporting evidence’, on which more is now said in Miller (2010), § 1(e).

- xiv** Bayes’s theorem is no more exclusive to Bayesians than Pythagoras’ theorem is exclusive to Pythagoreans. The multiplication law mentioned later in the paragraph is the (universalized) identity $p(a \ \& \ b, c) = p(a, b \ \& \ c)p(b, c)$, which is axiom B2 of the system of Popper [1959], appendix *v.

In his letter of 14.iii.1977, Grünbaum agreed that, to the extent that it is a theorem of the probability calculus, Bayes’s theorem is as freely available to deductivists as it is to Bayesians. He indicated further that his objection to Popper’s interpretation of the usefulness of severe tests was that a deductivist is not permitted to substitute into Bayes’s theorem the values of ‘inductive’ probabilities. This can hardly mean that the deductivist may not conclude that if $B \ \& \ H \vdash C$ then for every probability measure p that is strictly positive, $p(C, B \ \& \ \sim H) < p(C, B)$, so that if $p(C, B)$ is small then $p(C, B \ \& \ \sim H)$ is smaller still (as shown more precisely below). The force of Grünbaum’s criticism of deductivism continues to elude me. At times, for example after reading his description of background knowledge as ‘those *particular* theories with which scientists had been working by way of historical accident’ (pp. 107f.), I wonder if his objection is that since many items in background knowledge B may be untrue, probabilities relative to B may have quite misleading values; that is, $p(C, B)$ may not measure the true uncertainty of the outcome C , and the fact that $p(C, B)$ is low does not mean that a test with outcome C is severe, or that it has ‘a good chance ... [of] weed[ing] out *false* theories’ (p. 108). This is no doubt so. It would still be so if the truth of B (which may be assumed not to be logically true) were guaranteed, inductively or otherwise. As already explained, to say that $p(C, B)$ is low is not to say that if B is true then $p(C)$ is low. According to Popper, judgements of severity are made, rightly or wrongly, relative to background knowledge B , which is not dogmatically maintained, but subject to correction. It is not pretended that they have more authority than this. Whether this is Grünbaum’s objection to deductivism’s advocacy of severe tests as a way of exposing and eliminating error, I cannot say. I decline to speculate further on what his opaque text might mean.

The simple probabilistic theorem expounded by Grünbaum, in which it is assumed that ‘ C ... is predicted deductively by the conjunction of B and H ’, and the numerical example on pp. 109f. with which he illustrates it, can be profitably improved in this way. Suppose that $p(C, B) = \varepsilon > 0$ and that $p(C, B \ \& \ H) = 1 - \delta < 1$, where, in interesting cases, both ε and δ are small. It follows that

$$\varepsilon = p(C, B) = p(H, B)(1 - \delta) + (1 - p(H, B))p(C, B \ \& \ \sim H),$$

by the theorem of total probability, and therefore that

$$(0) \quad p(C, B \ \& \ \sim H)(1 - p(H, B)) = \varepsilon - (1 - \delta)p(H, B).$$

Suppose that $p(H, B) < 1$. We shall show that the inequality $p(C, B \ \& \ \sim H) \leq p(C, B)$ holds provided that $\varepsilon + \delta \leq 1$. For if $\varepsilon + \delta \leq 1$, then $\varepsilon - (1 - \delta)p(H, B) \leq \varepsilon - \varepsilon p(H, B) = \varepsilon(1 - p(H, B))$, whence by (0),

$$(1) \quad p(C, B \ \& \ \sim H)(1 - p(H, B)) \leq \varepsilon(1 - p(H, B)).$$

In the unrealistic and unimportant case of $p(H, B) = 1$, the value of $p(C, B \ \& \ \sim H)$ is indeterminate, but if $p(H, B) < 1$ we may conclude that $p(C, B \ \& \ \sim H) \leq \varepsilon$; that is, that $p(C, B \ \& \ \sim H) \leq p(C, B)$, as before. It is a simple consequence of (0) that, provided $p(H, B) < 1$, the value of $p(C, B \ \& \ \sim H) = (\varepsilon - (1 - \delta)p(H, B))/(1 - p(H, B))$ increases both as ε increases and as δ increases.

Grünbaum showed also, by means of a numerical example, that ‘the occurrence of C does *not* make the truth of H more probable than its falsity’ (p.110). In fact, by the multiplication law, $p(H, B \ \& \ C)p(C, B) = p(C, B \ \& \ H)p(H, B)$, so that $p(H, B \ \& \ C) = (1 - \delta)p(H, B)/\varepsilon$. In a severe test, $\varepsilon \ll 1 - \delta$, and therefore $p(H, B \ \& \ C) \gg p(H, B)$. But if $p(H, B)$ is small enough, for example if $p(H, B) \ll \varepsilon/(1 - \delta)$ then $p(H, B \ \& \ C)$ will also be small. If there remain inductivists who are tempted to claim that a theory that triumphs in a severe test acquires a probability close to unity, then the deductivist wants no part of it. It may be remembered that in [1972], Chapter 2, §33, Popper considered and decisively rejected this conclusion, but endorsed something rather weaker in terms of verisimilitude (for critical comments, see Miller 1994, Chapter 2, §2*i*, and p. 49).

More interesting, perhaps, than these more or less familiar calculations, are the values to be assigned to Popper’s two measures of degree of corroboration when a severe test of hypothesis H against background knowledge B yields the predicted outcome C . Even more interesting are the values obtained both by the probability and the degrees of corroboration, when the anticipated outcome is not realized. I have investigated and answered these questions in §4 of my (2010).

- xv** The paper in question is Grünbaum (1978). In Chapter 2, §2*e*, of my (1994) I have tried to explain why the falsificationist insistence on severe tests is uncontaminated by inductivist misconceptions.
- xvi** Grünbaum’s proof assumes that B is finitely axiomatizable, so that $B \in B_F$. Since A does not imply B , we do not have $B \in A_F$. It follows that $B_F \not\subseteq A_F$, and therefore that (\diamond) fails.
- xvii** Rosende has pointed out that the proof of case (*f*) offered in the text is fallacious. What is shown there is not that $B_F \not\subseteq A_F$ but that $A_F \not\subseteq B_F$. Indeed, $B_F \subseteq A_F$ holds automatically if $A \vdash B$. The shortest proof of case (*f*) that I know is Hempel’s (see footnote 7): since B is false, and A and B are not interderivable, the conditional $B \rightarrow A$ belongs to A_T but not to B_T , which implies that (\diamond) fails.
- xviii** Despite toiling for seven pages (pp.115–121), Grünbaum never supplied an alternative proof of the unsatisfiability of (\diamond) when the theory B is false, even for finitely axiomatizable A and B . When he reached case (*c*) on p.121 he acknowledged that ‘[t]here are no particular features of this special case which are sufficient to exhibit [the result] in a more immediate and elementary way than Tichý’s general proof’, and abandoned his quest. A proof of the Miller/Tichý result (as I called it in the text) that is shorter than Tichý’s own proof proceeds via the theorem of Keuth and Vetter that if B is any false theory then B_T and B_F have the same cardinality. See Miller (1994), Chapter 10, §4*a*.
- xix** This refers to **3(c)(i)** of Grünbaum [1976*b*].
- xx** Grünbaum [1976*b*], p. 127.
- xxi** *Desideratum* (i) on p. 396 of the edition of Popper [1963] current in 1977 is on p. 534 of the repaginated Classics edition Popper (2002*b*).
- xxii** The condition for $VS(A) < VS(B)$ is stated rather loosely. What should have been said is that if $\beta < \alpha$ then $VS(A) < VS(B)$ if & only if $\tau^2 < \alpha\beta$. Note that if $\tau = 0$ then $VS(A) < VS(B)$ if & only if $Vs(A) < Vs(B)$, even though VS and Vs may assume different values.
- xxiii** This point may need to be spelt out. As in note **x**, write $Cn(A) \subset Cn(B)$ to mean that B has greater content than A . Proposition P is logically equivalent to the following proposition P^\bullet : if $Cn(A) \subset Cn(B)$, and $A_F \not\subseteq B_F$, then B has greater verisimilitude than A does. But if $Cn(A) \subset Cn(B)$, and B is false, then $A_F \subset B_F$, and so P^\bullet is vacuously true; and if $Cn(A) \subset Cn(B)$, and B is true, then $A_T = Cn(A) \subset Cn(B) = B_T$ and $A_F = B_F = \emptyset$, and hence by (\diamond) , B has greater verisimilitude

than A does; again P^\bullet is true. Grünbaum's proof of P on p.4 appeals to Popper's 'theorem on truth-content' (1966), but this is unnecessary, as observed in the text to note 9.

xxiv Note that provided that $\tau \neq 0$, each subcase of $P^\#$ holds even if $\beta = \alpha$. Grünbaum's remark that ' B 's unilateral entailment of A does not guarantee that $\dots ct(A) < ct(B)$ ' is therefore doubly irrelevant.

xxv I have not changed my mind about Popper's quantitative theory of verisimilitude, but it now seems to me to be too hasty to dismiss, on these grounds alone, any claim that $P^\#$ may make to 'vindicate' the method of science. In a real sense, it does this as well as can reasonably be expected. I therefore wish to moderate the antepenultimate sentence of the present section, replacing 'that' by 'if'.

There are indeed theories of verisimilitude, for example the theories of my (1978d), (1984), and (2009), that imply that the stronger of any two false theories is at least as close to the truth as is its rival. This result provokes the so-called 'child's play objection' raised in note 2 on p.157 of Tichý [1974], that, given a false theory, no effort is required to find a theory that is at least as close to the truth, and may be closer. Other theories based on the same idea, such as that of Kuipers (1982), manage to avoid the objection (see §1 of Chapter 10 of my 2006a, especially Tables 10.0 and 10.1). A rebuttal of the child's play objection would be out of place here, but it may be noted that both *absolute confirmation* and *incremental confirmation*, in the sense in which these terms are standardly used in confirmation theory, are prey to similar misgivings. Any theory weaker than a given theory A has at least as high a probability (degree of absolute confirmation) on the same evidence C , while any theory B satisfying $A \& C \vdash B \vdash A$ has at least as high a degree of incremental confirmation (support) by C as has A ; that is, $p(B, C) - p(B) \geq p(A, C) - p(A)$. This result hardly depends at all on how incremental confirmation is measured (note 8 of Popper & Miller 1987). Less obvious is the fact that $A \vee C$ too is always at least as well supported by C as A is (*op. cit.*, Theorem 4).

xxvi In Theorem 1 of the paper [1976], referred to in footnote 9, it is shown that the function q defined on deductive theories by $q(A) = \inf\{p(a) \mid A \vdash a\}$ satisfies the addition law $q(A) + q(B) = q(A \vee B) + q(A \& B)$, as well as more immediate laws of probability. The problem of extending a relative probability measure from an algebra of sentences to the corresponding algebra of theories is unsolved. For some steps in the right direction, and some in the wrong direction, see Miller (1978a), (1978b).

xxvii In his letter of 14.iii.1977 Grünbaum drew my attention to the final paragraph of his [1976b], which indeed leaves open the possibility of what he calls a 'deductivist rationale' for the method of conjectures and refutations. That Grünbaum endorsed no answer to the question raised by his [1976b] (or to the question raised by his [1976a]), is recognized at the outset of my paper. His criticism is typically philosophical rather than scientific, objecting to the validity of Popper's arguments rather than to the truth of his conclusions. Much of my initial response, I am afraid, also falls into this category.

xxviii In my present judgement, this is most unsatisfactorily expressed. The claim ' B has more verisimilitude than A ' was conjectured to be true before it was tested. The failure to refute it left it where it was. See my (2006a), Chapter 4, §1, and my (2007), §1.

xxix To refute the conjecture ' B has greater verisimilitude than A ' we need to refute each disjunct of (\diamond). One way to do this is to find a true consequence of A that is not a consequence of B . Another way is to find a false consequence of B that is not a consequence of A . But neither of these is necessary. Any false consequence of B provides a refutation of ' B has greater verisimilitude than A ', since the latter cannot hold when B is false.

xxx What I meant here was ' \dots to deny that a theory can be tested and (corroborated) *only* if \dots '.

xxxi A silent appeal is here made to Theorem 1 of my [1974], which in the notation of the present paper says: if A is false and $A_F \subseteq B_F$ then $B \vdash A$.

xxxii It would have been more accurate to have written here 'critical manner', in order to bear witness to the falsificationist or critical rationalist maxim that only theories that are open in principle to rejection should be open in practice to acceptance (Miller 2006a, Chapter 4, §1, and (2007), §1). The statement in the text, that the source of our conjectures is of no methodological interest, has to be

qualified (as Hopf has reminded me) by excluding, or at least discouraging, those inductivist methods of theory formation that stick closely to the data, and never yield independently testable theories.

xxxiii I do not deny that the more serious effects of cancers, dental cavities, and some other progressive medical problems, can often be avoided by means of a programme of regular medical checks. No doubt in medicine there is some sense in the adage ‘Prevention is better than cure’, though it is unfortunate and dangerous that the medical profession (especially the British Medical Association) now dedicates itself to the totalitarian ambition of controlling our lives in the interest of preventing illnesses from which, in the cases of most of us, we are never going to suffer. If we want to learn, in contrast, we must be eager to make errors and thereafter to cure them. See Miller (1983), pp. 9f.

xxxiv At this point the typescript provided by the Archives of the London School of Economics comes to a stop (although the notes and bibliography are intact). Because the pages are not visibly numbered, I cannot say how many pages have been mislaid, but I suspect that it is not more than one.

If I were rewriting the paper in 2010, I should continue as follows:

[It is not pretended that one is more likely to hit on the] . . . truth by means of uninhibited conjecture than by some more disciplined method of formulating new hypotheses. What is maintained is that a regime of severe critical testing can be expected to uncover and to eliminate more errors, and therefore lead to more revisions and more progress, than does a regime of accumulating confirmations and taking account of errors only when they become impossible to ignore. It need hardly be said that a systematic policy of searching for, and attempting to eradicate, errors cannot be demonstrated to be more successful, or even more efficient, than a less adversarial approach to empirical investigation. No method of exploring the unknown can be known in advance to be effective. But what can be demonstrated is that from confirmations nothing can be learnt that is not already known. For an extended discussion, see my (2006a), Chapter 4, § 1, and my (2007), § 1.

xxxv My principal criticisms, under the nine subtitles of the original text, may be summarized as follows:

- 1 **Questions and Answers** Many of the questions that Grünbaum cites as being answered by Newton’s theory, but not by Einstein’s, indicate that Popper should from the start have excluded explanatory questions from his widened definition of content. Grünbaum’s other examples are easily taken care of. There is a more general demonstration that Popper’s proposal does not work.
- 2 **Content Measures** Grünbaum exaggerates the difficulty of comparing numerically the contents of universal theories (to all of which Popper ascribes probability zero), and neglects most of what Popper wrote about ‘the fine-structure of probability and content’ ([1959], appendix *vii). Neither Popper nor Grünbaum takes advantage of the possibility of admitting infinitesimal probabilities.
- 3 **The Determinateness of Probability Measures** Grünbaum’s complaint that Popper endorses no method of measuring content and of logical probability takes no account of the fact that epistemologically significant results can be obtained in the absence of such determinateness. Grünbaum himself appeals to an informative theorem that holds however probability is measured.
- 4 **Severity of Tests** Grünbaum argues that deductivism, unlike Bayesianism, is unable to explain the virtue of subjecting theories to severe tests. Even after resolving an apparent conflation of the conditional ‘if D is true then $p(A) = r$ ’ with the corresponding statement of relative probability ‘ $p(A, D) = r$ ’, I have been unable to discover a coherent interpretation of Grünbaum’s discussion.
- 5 **Qualitative Verisimilitude** Grünbaum’s extended but fragmentary exposition considers only finitely axiomatizable theories. It fails to add to, elucidate, or improve on, earlier, more general, and more complete proofs that Popper’s qualitative definition (\diamond) of verisimilitude is unsuccessful.
- 6 **Quantitative Verisimilitude** Using Popper’s measures of truth and falsity content, Grünbaum formulates a quantitative counterpart of (\diamond), and suggests that it may fail when the theories involved are comparable by his (unnormalized) measure of verisimilitude. Not much more is said.

- 7 Verisimilitude and Falsificationism** Popper claimed that ‘the theory with the greater content ... will also be the one with the greater verisimilitude *unless its falsity content is also greater*’. Grünbaum dismisses as ‘unfounded’ a quantitative version $P^\#$ of Popper’s claim, and infers that $P^\#$ ‘cannot serve to vindicate severe tests’. This line of thought is confounded by a proof of $P^\#$.
- 8 Induction by Enumeration** Grünbaum maintains that the statement that B and A have the same falsity content can be corroborated only by ‘induction’ from the falsified predictions that they share. It is shown that Grünbaum makes assumptions here that no deductivist need accept.
- 9 Falsificationism** The main purpose of Grünbaum [1976*b*] is to cast doubt on Popper’s claim that considerations of verisimilitude underlie ‘the logical basis of the method of science — the method of bold conjectures and of attempted refutations’. It is noted that, whether this be so or not, the good sense of the falsificationist approach is open to a simpler explanation or ‘vindication’.

The paper now reads to me like an unusually prolix report from a referee who is trying to spell out how these two papers of Grünbaum’s might be brought into something like publishable shape. My opinion is unchanged that, although the papers contain some points of interest, *The British Journal for the Philosophy of Science* performed a signal disservice to its readers by publishing them in this form.

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