Carnap-Confirmation, Content-Cutting, & Real Confirmation

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1. Introduction

The attempt to explicate the intuitive notions of confirmation and inductive support through use of the formal calculus of probability received its canonical formulation in Carnap's *The Logical Foundations of Probability*. It is a central part of modern Bayesianism as developed recently, for instance, by Paul Horwich and John Earman.[1] Carnap places much emphasis on the identification of confirmation with the notion of probabilistic favorable relevance. Notoriously, the notion of confirmation as probabilistic favorable relevance violates the intuitive transmittability condition that if e confirms h and h' is part of the content of h then e confirms h'. This suggests that, pace Carnap, it cannot capture our intuitive notions of confirmation and inductive support. Without transmittability confirmation losses much of its intrinsic interest. If e, say a report of past observations, can confirm h, say a law-like generalization, without that confirmation being transmitted to those parts of h dealing with the as yet unobserved, then it is not clear why we should be interested in whether h is confirmed or not.

In section 2, after rehearsing the problem of the failure to meet the transmittability condition, I introduce a new concept of content in order to define a new notion of confirmation, called real confirmation, which satisfies the transmittability condition. The notion of real confirmation is also defined in terms of probabilistic favorable relevance, albeit not Carnap's simple identification of confirmation with favorable relevance. Section 3 contains a brief proof of the difference between Carnap's notion of confirmation and the new notion of real confirmation. In section 4 the new notion of content introduced in section 2 is utilized to define the notion of evidence e cutting the untested content of hypothesis h. It is then argued in sections 5 and 6 that the classical paradoxes of confirmation pose no special threat to probabilistic notions of confirmation since they all involve cases of content-cutting in the absence of real confirmation. In section 7 and 8 the failings of more traditional Bayesian solutions to Hempel's Raven Paradox are discussed and contrasted with the solution offered in sections 5 and 6 resulting in the surprising conclusion that some of the traditional Bayesian solutions implicitly assume a deductivist, inductive sceptical, rationale. In section 9, after demonstrating that Carnap's identification of the intuitive notion of irrelevance with the notion of probabilistic irrelevance yields highly unintuitive results, the new notion of content is used to define a new notion of irrelevance, called real irrelevance, which allows us to better capture our intuitive judgments of irrelevance. The notion of real irrelevance is also defined in terms of probabilistic irrelevance, albeit not Carnap's simple identification of irrelevance with probabilistic irrelevance. In section 10 we consider how the notion of real confirmation bears on the confirmation of theories as opposed to the confirmation of simple law-like hypotheses. It is argued that theory confirmation is in the final analysis also best analyzed in terms of real confirmation, albeit in terms of the real confirmation of theory parts, rather than in terms of evidence simply being favorably relevant to theory.

[1] See Horwich (1982), esp. Chapter 3, and Earman (1992) esp. Chapters 3 and 4. Since all the points I wish to make can be made using Carnap's work as a foil I have reframed from explicitly addressing my analysis to the work of particular Bayesians. Given the multiplicity and varieties of Bayesians this makes for a much more focused approach.

In sum, all this suggests that Carnap's project of defining confirmation and inductive support in terms of probabilistic favorable relevance, a project much cherished by many present day Bayesians, is still a viable option as long as it does not proceed through a simple identification of confirmation with probabilistic favorable relevance.

2. Carnap-Confirmation, Content and Real Confirmation

A parable: June is anxious. Her daughters Laura and Sophie have just taken the Bar exam. She wants to know if they have passed and rings her secretary Bob who has been making the relevant inquires at the Bar Association. Unfortunately Bob is a bit flighty and all he can remember from his inquiries is the net result that either Laura passed or Sophie failed. On hearing this June is relieved for she takes the claim 'Laura passed or Sophie failed' to confirm the claim that 'Laura and Sophie both passed'!

The moral of this parable will be revealed shortly.

In his *The Logical Foundations of Probability* Carnap tried to capture one notion of confirmation in terms of the relation of probabilistic favorable relevance (see Carnap 1962, preface to the second edition, pp.xvi-xx, and §86, pp.462-468). For Carnap, e is favorably relevant to h just in case the posterior probability of h on the evidence e is greater than the prior probability of h, that is, if P(h/e) > P(h). Carnap's proposed definition may be rendered as

D1 *e confirms h* $=_{df.}$ *P(h/e) > P(h).*

D1 simply identifies the intuitive notion of confirmation with the notion of probabilistic favorable relevance. We shall hereafter refer to D1 confirmation as the favorable relevance notion of confirmation, and, occasionally, as Carnap-confirmation.

Now note, evidence e can be favorably relevant to hypothesis h even though e is not favorably relevant to every content part of h. For instance,

(1) Ken is in Sydney

is, presumably, favorably relevant to

(2) Ken is in Sydney and The moon is blue,

though it is not favorably relevant to (2)'s content part

(3) The moon is blue.

In other words, (1) Carnap-confirms (2) but does not Carnap-confirm every content part of

(2). Where e Carnap-confirms h and every content part of h we might say e really confirms h. Thus we have the definition

D2 *e really confirms h* $=_{df.}$ *P(h/e)>P(h) and for any content part h' of h, P(h'/e)>P(h').* [2]

Equivalently, we might say that e really confirms h if and only if e is probabilistically favorable relevant to h and every content part of h. Carnap and his successors, while trying to explicate intuitive notions of confirmation and inductive support in terms of the probabilistic notion of favorable relevance, never tried to employ the notion of e being favorably relevant to every content part of h. The reason for this is that they always took for granted the classical notion that the content of a statement is given by the class of its non-tautologous logical consequences. That is, Carnap, and his successors, worked with the following classical notion of content

D3 *h' is part of the content of h* $=_{df}$ *h ├ h' and h' is non-tautologous.*

Under this understanding of content it follows that e is only favorably relevant to h and every content part of h in those cases where P(h/e)=1.

Proof: Assume P(h/e)≠1.

Case 1. Suppose P(h)=0 or P(h)=1 or P(e)=1 or P(e)=0. Under any of these conditions it is clearly not the case that P(h/e)>P(h).

Case 2. Suppose 0<P(h),P(e)<1. Then 0<P(~e v h),P(~(~e v h))<1. Also ~(~e v h) ├ e. Therefore P(~(~e v h)/e)>P(~(~e v h)).[3] Now for any p and q, if P(~p/q)>P(~p) then P(p/q)<P(p). So P(~e v h/e)<P(~e v h) and h ├ (~e v h).

So, where P(h/e)≠ 1, e is either not favorably relevant to h or e is not favorably relevant to h's its (non-tautologous) consequence (~e v h).

In other words, given the classical notion of content, as defined by D3, the notion of real confirmation, as defined by D2, is near vacuous.

The notion that the (logical) content of a statement is given by the class of its (non-tautologous) logical consequences is by far the dominant conception of content among logicians and philosophers of science. Indeed it is shared by both inductivists, such as Carnap and Salmon, and anti-inductivists, such as Popper (for example, see Salmon 1969, p. 55, Carnap 1935, p. 56, and Popper [1959] 1972, p. 120, 1965, p. 385). Elsewhere (Gemes 1994) I have argued against this identification of content with (non-

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[2] This definition, though perspicuous, involves redundancy since, under all the theories of content considered here, for any contingent h, h counts as part of the content of h.

[3] It follows from Bayes theorem, P(p/q) = P(q/p) x P(p)/Pq), that where 0<P(p),P(q)<1 and p├q (and hence P(q/p)=1), P(p/q)>P(p).

tautologous) consequence class. The chief problem with this notion of content is that it allows that for any contingent p and q, as long as the negation of p does not entail q, there will always be some part of p that "includes" q, namely 'p v q'. This has the result that any two (contingent) theories/statements T and T', such the negation of T does not entail T', share some common content, namely 'T v T''. Given this notion of content Newton's laws share common content not only with Einstein's relativity theory but also with your favorite crackpot theory, say, Dianetics or the medical theory of Paracelsus. Further, on this notion of content, 'Fa1 v ~Fa2', for instance, counts as content common to both 'Fa1 & Fa2' and '~Fa1 & ~Fa2'. Yet, prima facie, 'Fa1 & Fa2' shares no common content with '~Fa1 & ~Fa2'. Moreover, if 'Fa1 v ~Fa2' counts as part of the content of 'Fa1 & Fa2' then it follows that '~Fa2' conclusively confirms part of the content of 'Fa1 & Fa2'!

The classical notion of content makes a particularly poor combination with probabilistic favorable relevance accounts of confirmation and not simply because, as noted above, it renders vacuous the D2 notion of real confirmation. D1 combined with D3, also yields unacceptable consequences for the notion of partial confirmation. For instance, that combination yields the result that the observation of a white raven a, that is 'Ra&Wa', conclusively confirms part of the content of the claim that all ravens are black, '(x)(Rx ⊃ Bx)'; and the observation of a black raven b, that is 'Rb&Bb', disconfirms part of the content of that claim. In particular, 'Ra&Wa' conclusively confirms '(x)(Rx ⊃ Bx)'s D3 content part '(x)(Rx ⊃ Bx) v Ra&Wa', and 'Rb&Bb' disconfirms its D3 content part '(x)(Rx ⊃ Bx) v ~(Rb&Bb)'. The same problems apply to the question of the confirmation of theories. Given D1 and allowing the 'h' of D3 to stand for theories, yields the result that evidence E will always confirm part of theory T and disconfirm part of T.[4] Now Bayesian decision theorists may care little for the notion of partial confirmation, after all, their focus, leaving the question of utilities aside, is simply on the question of what is the probability of T on the evidence E and what is the probability of various rivals given E. But for those of us interested in old fashioned confirmation and truth the notion of partial confirmation is crucial. For typically we will regard evidence E as being confirmatory for some parts of a broad theory T and being irrelevant to other parts. We want to know not simply whether the posterior probability of T on E is higher than the probability of T prior to the addition of E. We want to know what parts of theory T evidence E is favorably too and what parts it is irrelevant to. To know this we have to first get right what counts as part of a theory. The question of the partial confirmation of theories is taken up again in section 8. below.

There are in fact a host of other problems associated with the classical notion of content however this is not the appropriate forum to air them.[5]

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[4] To be more exact, the combination of D1 and D3 and the trivial claims that P((x)(Rx ⊃ Bx)v(Ra&Wa))<1, P((x)(Rx ⊃ Bx)v(Rb&Bb))<1, P(Ra&Wa)>0, and P(Rb&Bb)>0, yields the result that 'Ra&Wa' conclusively confirms part of the claim '(x)(Rx ⊃ Bx)' and 'Rb&Bb' conclusively disconfirms part of the content of '(x)(Rx ⊃ Bx)'. Similar trivial assumptions need to be made to make good the claim the evidence E always confirms part of theory T and always disconfirms part of T.

[5] For a rehearsal of some of these problems see Gemes 1990, 1993, and 1994.

The following is a simplified version of a definition of content offered in Gemes (1994), but see also Gemes (1996), for formal propositional and quantificational languages: Where α is a variable over well-formed-formulae (wffs) of the language in question and β is a variable over wffs and sets of wffs of the language in question,

D4 *α is a content part of $\beta =_{df}$ (i) α is a non-tautologous consequence of β and (ii) for some ϕ logically equivalent to α there is no consequence φ of β such that φ is stronger than ϕ and every atomic wff occurring in φ occurs in ϕ.*

We say φ is stronger than ϕ just in case φ entails ϕ but ϕ does not entail φ. The reference to logical equivalents ensures that the content part relationship is closed under logical equivalence, i.e. if ϕ and φ are logically equivalent then ϕ is a content part of β iff φ is a content part of β.

On this account of content it does not follow that for arbitrary (contingent) *p* and *q*, where the negation of p does not entail *q*, *p v q* is part of the content of *p*. According to D4, 'Fa1 v ~Fa2' is not part of the content of 'Fa1 & Fa2' since 'Fa1' is a consequence of 'Fa1 & Fa2' that is stronger than 'Fa1 v ~Fa2' and contains only atomic wffs that occur in 'Fa1 v ~Fa2'.

Given this new notion of content it does not follow that for arbitrary (contingent) *e* and *h*, provided *e* does not entail *h*, *h v ~e* is part of the content of *h*. So given this new notion of content, it does not follow that e only really confirms h where P(h/e)=1. In other words, given the classical D3 notion of content, the D2 notion of real confirmation is trivial, but given our new D4 notion of content it is substantive.

It is this notion of real confirmation, rather than the Carnapian notion of favorable relevance, that is best suited to explicate many of our ordinary notions of confirmation, inductive support, and having reasons for belief. For instance, the ordinary notions of inductive support and confirmation are transmittable over contents, that is, if e confirms/inductively supports h and h' is part of the content of h then e confirms/inductively supports h'. For inductivists it is just this feature that makes inductive support interesting. Evidence dealing with past and present observed events is taken to support generalizations and from such confirmed generalizations consequences concerning future events are drawn. However without transmittability no such consequences could be drawn.[6] Where confirmation is identified with non-transmittable D1 Carnap-confirmation rather than transmittable D2 real confirmation we loose at least some of the intuitive link between the notion of evidence confirming a hypothesis and evidence giving reason to believe a hypotheses. For instance, while the observation of a black raven Abe D1 Carnap-confirms the claim that Abe is a black raven and all other

[6] To some extent confirmation without transmittability is akin to what Popperians call 'corroboration'. It is simply a report on past performance devoid of implications for future performance.

ravens are pink, it, intuitively speaking, does not give reason to believe that claim.[7] The fact that intuitively the observation of black raven Abe does not give reason to believe the hypothesis that Abe is black and all other ravens are pink is however reflected in the fact that that observation does not D2 really confirm that hypothesis.

Carl Hempel tried to capture something like the transmittability requirement by offering the following condition of adequacy for any theory of the confirmation of universal statements by observational evidence

S.C.C. If an observation report confirms a [universal] hypothesis H, then it also confirms every consequence of H (Hempel 1965, 31).

A broader version of this adequacy condition would stipulate

S.C.C.1 For any e and h if e confirms h then e confirms every consequence of h.

Now presumably in framing S.C.C. Hempel did not have in mind any old disjunctive consequence of h. Thus he did not have in mind the idea that if 'a is a raven & a is black' is to confirm 'All ravens are black' then it must confirm its consequence 'All ravens are black or a is a non-black raven'. More likely he had in mind the idea that if 'a is a raven & a is black' confirms 'All ravens are black' then it confirms its consequence 'If b is a raven then b is black.' That is to say, the intuition behind Hempel's S.C.C. is better expressed by the condition of adequacy

T.C. If an observation report confirms a hypotheses H then it also confirms every content part of H,

and its more general version

T.C.1 If e confirms h then e confirms every content part of h,

where content part is taken in the sense of D4 - we use the initials 'T.C.' to indicate that these are transmittability conditions. Our new notion of content is needed not simply to define a probabilistic notion of confirmation that meets the transmittability condition but also to give a precise formulation of that very condition.[8]

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[7] It follows that 'Abe is a black raven' Carnap-confirms 'Abe is a black raven and all other ravens are pink' provided that the former claim has a prior probability less than 1 and the later has a prior probability greater than 0.

[8] A caveat: Perhaps if we are dealing with a merely quantitative notion of confirmation rather than a qualitative notion we may adhere to the Hempelian Special Consequence Condition without recourse to the new notion of content. However, merely quantitative notions (e.g. confirmation as probability over a given value r) has well-know drawbacks; for instance, such quantitative notions allow that e may decrease the probability of h from it's initial (prior) probability yet nevertheless e still confirms h.

The D1 Carnapian notion of confirmation as probabilistic favorable relevance does not meet any of the above transmittability conditions. Hence it does not fair well as a stand in for the ordinary notion of confirmation. It does not meet S.C.C. and its generalization S.C.C.1 because, generally, *e* (observational or otherwise) does not confirm *h's* consequence $h \vee \sim e$. It does not meet our reformulated transmittability condition T.C.1 because of cases like that of

(1) Ken is in Sydney

and

(2) Ken is in Sydney and The moon is blue

mentioned above. In this case (1) is presumably favorably relevant to (2) but not to its content part

(3) The moon is blue.

However the notion of real confirmation does meet our reformulated transmittability conditions. It is real confirmation, as defined by D2, rather than Carnap-confirmation, as defined by D1, that captures the intuitive transmittability requirement, and hence better approximates the intuitive notion of confirmation. Similarly, inductive support is best defined in terms of real confirmation rather than Carnap-confirmation. This is a theme we shall return to in section 4 below.

The Carnapian D1 definition of confirmation, in flaunting the transmittability conditions, lays itself open to a charge Clark Glymour pressed effectively against hypothetico-deductive theories of confirmation. Glymour, (1980, pp. 133-5), (1980a, passim), notes that according to canonical hypothetico-deductive theories of confirmation where $h \vdash e$, for any arbitrary g, provided g is consistent with h and e is non-tautologous, e confirms h&g. By the same token D1 yields the result that were $h \vdash e$, for any arbitrary g, provided $P(h\&g)>0$ and $P(e)<1$, e confirms h&g (for more on this cf. section 10. below).

Returning now to our parable of June the hopeful mother we see what is wrong with her claim that the evidence 'Laura passed or Sophie failed' confirms the claim that 'Laura and Sophie both passed'. Intuitively while that evidence is entailed by and in turn confirms the claim 'Laura passed' it does not confirm the conjunction of that claim with the claim 'Sophie passed'. While that evidence is probabilistically favorably relevant to that conjunction it is not probabilistically favorably relevant to every content part of that conjunction.[9] In particular, it is unfavorably relevant to the claim 'Sophie passed'. The moral to our parable of June is that she has erred in identifying confirmation with mere

[9] Note where H is 'Laura & Sophie both passed' and E is 'Laura passed or Sophie failed', since $H \vdash E$, it follows that provided $P(H)>0$ and $P(E)<1$, $P(H/E)>P(H)$. So for a Carnapian, given those minimal conditions, it follows that E confirms H.

favorable relevance. Rather she should identify confirmation with favorable relevance to every content part and hence take less joy from Bob's report.

Before proceeding it will be useful to pause and provide a formal demonstration of the difference between confirmation, as defined by D1, and real confirmation, as defined by D2. Readers not enamored of technical details might skip to section 4.

3. A formal digression being an existence proof of the possibility of a measure function yielding Carnap-confirmation with and without real confirmation

We proceed by constructing a formal language which under an obvious interpretation is suitable for framing hypotheses about the roll of dice pair a_1 and a_2. The "dice language" DL is a propositional language with only two logical constants 'a_1' and 'a_2', and only the six predicates '1', '2','3','4','5' and '6'. DL contains then 12 atomic sentences; '$1a_1$', . . '$6a_1$', and '$1a_2$', . . '$6a_2$'. We call a basic pair the set consisting of an atomic sentence and its negation. A state description is a conjunction which contains as conjuncts one and only one member from each basic pair. Now we construct measure m as follows: We assign the probability of 1/36 to each of the 36 distinct state description that contains exactly one of '$1a_1$', . ., '$6a_1$' and one of '$1a_2$', . . , '$6a_2$'. All other state descriptions get a probability of 0. From these initial probabilities we calculate further probabilities in the standard manner, e.g. the probability of any statement α being true under measure m, (i.e. $P(\alpha)$), is the sum of the probabilities of those state descriptions that entail α. Thus $P(1a_1) = 6/36 = 1/6$.
Now, under an obvious interpretation of the constants and predicates of DL, the statement

(4*) Die a_1 came up even and a_2 came up odd

translates into DL as

(4) $(2a_1 v\ 4a_1\ v\ 6a_1)\ \&\ (1a_2\ v\ 3a_2\ v\ 5a_2)$.

The statement

(5*) Die a_1 came up even

translates as

(5) $2a_1\ v\ 4a_1\ v\ 6a_1$

The statement

(6*) a_2 came up odd

translates as

(6) $1a_2$ v $3a_2$ v $5a_2$.

The statement

(7*) Die a_1 came up six

translates as

(7) $6a_1$

and, finally,

(8*) Die a_1 came up six and a_2 came up one, three or six

translates as

(8) $6a_1$ & ($1a_2$ v $3a_2$ v $6a_2$).

We take the content of wffs of DL to be defined by D4 above, Carnap-confirmation to be defined by D1 above, and real confirmation to be defined by D2 above.

Now (4) has as its content parts (4), (5) and (6) and any statement logically equivalent to one of those three. Since logical equivalents stand in the same confirmation relationships, to see if a given statement really confirms (4) we need only ascertain whether the statement in question is favorably relevant to (4), (5) and (6).

Under measure m, P((4)) = 1/4, P((4)/(7)) = 1/3. So under m, (7) Carnap-confirms (4). Yet (6) is a content part of (4) and under m, P(6) = 1/3 and P((6)/(7)) = 1/3. So, under m, (7) does not really confirm (4). For (7) to really confirm (4) it would have to be favorably relevant to every content part of (4), including (6).

On the other hand, under m, (8) both Carnap-confirms and really confirms (4). (8) really confirms (4) since (8) is favorably relevant to every content part of (4). In particular, under m, P((4)) = 1/4 and P((4)/(8)) = 2/3, P((5)) = 1/2 and P((5)/(8)) = 1, and, P((6)) = 1/2 and P((6)/(8)) = 2/3. Now, as noted above every content part of (4) is equivalent to one of (4), (5) and (6), so since, under m, (8) is favorably relevant to (4), (5) and (6) it is favorably relevant to every content part of (4).

In summary then (7) Carnap-confirms (4) but does not really confirm it, while (8) both Carnap-confirms and really confirms (4). Thus measure function m is a function where there are cases of Carnap-confirmation without real confirmation and cases of Carnap-confirmation with real confirmation.

4. Real Confirmation Vs Mere Content-cutting

Consider the case of statements

(7*) Die a_1 came up 6

and

(4*) Die a_1 came up even and die a_2 came up odd

mentioned above. (7*) confirms (4*) not because it lends credibility to those parts of (*4) untested by (7*), namely

(6*) Die a_2 came up odd.

Rather, (7*) confirms (4*) because it deductively entails a content part of (4*), namely

(5*) Die a_1 came up even.

In an obvious sense, (7*) cuts the questionable content of (4*) down to its second conjunct (6*).

This type of confirmation might aptly be called content-cutting. Thus we have the definition,

D5 *e content-cuts h* $=_{df}$ *e entails a content part of h*.

Under the classical D3 construal of content this notion of content-cutting is a more or less trivial relationship. Where content is taken as (contingent) consequences then, provided the negation of *e* does not entail *h*, *e* will always entail some content part of (contingent) *h*, namely *hve*. For instance, under the D3 notion of content each of

(7*) Die a_1 came up 6

(7') The moon is round

and

(7!) Die a_1 came up 3

content-cuts

(4*) Die a_1 came up even and die a_2 came up odd

since each of the disjunction of (7*) and (4*), the disjunction of (7') and (4*), and the disjunction of (7!) and (4*) count as content parts of (4*) and are respectively entailed by (7*), (7') and (7!).

Under the D4 notion of content offered above content-cutting is not trivial since it is not the case that for any arbitrary contingent *h*, if the negation of *e* does not entail *h*, *h v* e is a content part of *h*. For instance, under the D4 neither the disjunction of (7*) and (4*), nor the disjunction of (7') and (4*), nor the disjunction of (7!) and (4*) count as content parts of (4*).

The combination of D5 and D4 yields the intuitively correct results that (7*) content cuts (4*) while (7') and (7!) do not.(7*) content cuts (4*) because it entails (4*)'s D4 content part(5*).

In some cases of confirmation e confirms h merely because e content-cuts h. That is to say, in some cases of confirmation, for instance the confirmation of (7*) by (4*), and of

(2) Ken is in Sydney and The moon is blue,

by

(1) Ken is in Sydney,

confirmation occurs wholly through content-cutting, without any increase in the probability of the deductively untested parts of the hypothesis in question. In such cases the confirmation is still the result of a purely deductive relationship between *e* and *h*, albeit a deductive relationship between *e* and a part of *h*. For genuine inductive support *e* must really confirm *h* beyond content-cutting *h.* For *e* to inductively support *h* there must be real confirmation that does not come wholly from content-cutting. In the case where *e* deductively entails *h*, *e* conclusively confirms *h* through mere content-cutting. In such cases *e* cuts the untested content of *h* down to nothing. In cases such as those of (7*) and (4*) and (2) and (1), while the respective evidence statements do not entail the respective hypotheses, what confirmation they give comes wholly through content-cutting in the absence of real confirmation. This belies the popular notion that where *e* confirms *h* and *e* does not entail *h*, *e* inductively supports *h*. There is deductive support short of full deductive entailment.

Elsewhere I (a) argue that attempts to define inductive support in terms of deductive support short of full entailment amount to a form of inductive scepticism, and (b) further develop the claim that real confirmation rather than simple Carnapian favorable relevance is the notion needed to build an inductive logic.[10] For the moment I will attempt to further motivate the distinctions between Carnap-confirmation, real confirmation and

[10] Cf. Gemes 1994a and 1994b. In respect of the claim that attempts to define inductive support in terms of a notion of deductive partial entailment lead to inductive scepticism the path breaking paper is Salmon 1969.

content-cutting by showing how they shed light on the relevance of the classical paradoxes of induction (Hempel's raven paradox, Goodman's grue-green paradox, and the paradox of instance confirmation) to attempts to define confirmation in terms of probabilistic favorable relevance. Along the way we shall note how this approach differs from more traditional Bayesians approaches to the paradoxes of induction.

5. Content Cutting and the Paradoxes of Induction

To see how our new notions of real confirmation and content-cutting shed light on the classical paradoxes of confirmation we shall consider analogs of those paradoxes as they arise in a simple formal language. But first we need to register a small caveat.

In attempting to show how light is shed on the paradoxes certain claims about the possibility of constructing various measure functions will me made. However to avoid the proliferation of technical details the exhibition of a measure function having the properties described will be left to appendix I below.

Consider the Carnap type language L_3. L_3 contains only the three individual constants 'a_1', 'a_2' and 'a_3'. Let us interpret the L_3 predicate 'B' as being equivalent to the English predicate 'is black', 'R' as the English predicate 'is a raven', 'E' as the English predicate 'is an Emerald', 'G' as the English predicate 'is green' and 'G*' as the Goodman grue-type predicate 'is identical to a_1 or a_2 and is green or is not identical to a_1 or a_2 and is blue'. For convenience we assume that L_3 contains no other predicates. Besides these individual constants and predicates L_3 contains an individual variables 'x_1', 'x_2', etc, the logical operators 'v', '&', '~', and '$\supset$', and the grouping indicators '(' and ')'. The well-formed-formulae of L3 are defined in the usual manner.[11]

Now suppose we adopt a measure function m that yields the result that for any (contingent) universal sentence S of L3 and any instance I of S, I Carnap-confirms (i.e. is favorably relevant to) S. In particular, suppose under m

(9) Ra_1 & Ba_1

Carnap-confirms

(10) (x)(Rx $\supset$ Bx)

and

(11) ~Ra_1 & ~Ba_1

Carnap-confirms

[11] Carnap's L_3 would not, of course, contain such non-L distinct predicates.

(12) $(x)(\sim Bx \supset \sim Rx)$.

Then by the equivalence principle - that wherever e Carnap-confirms h, e Carnap-confirms any logically equivalent of h - our measure function m yields the result that (11) Carnap-confirms (12)'s logical equivalent (10).[12]

Yet note, even accepting that (11) Carnap-confirms (10), we may still claim that this confirmation is a case of mere content-cutting without real confirmation. That is, we can claim that (11) Carnap-confirms (10) merely because it deductively entails (10)'s content part

(13) $Ra_1 \supset Ba_1$.

At the same time our adopted measure function m might yield the result that (11) does not Carnap-confirm (10)'s content part

(14) $Ra_2 \supset Ba_2$

while (9) does.[13]

Once we realize that we are ultimately interested in real confirmation (as defined by D2) rather than mere Carnap-confirmation (as defined by D1) we may happily concede that both the observation of a black raven and the observation of a white shoe (i.e. a non-back, non-raven) Carnap-confirms the claim that all ravens are black. The white shoe Carnap-confirms through mere content-cutting. This leaves us free to deny that both really confirm that claim. In particular, we may claim that only the black raven really confirms the claim that all ravens are black.

Let me try to forestall objections by granting that this solution to Hempel's paradox does not show that, or why, the black raven, rather than the white shoe, really confirms (10), the claim that all ravens are black. Indeed nothing I have said is inconsistent with the claim that it is the white shoe, rather than the black raven, that really confirms (10), or even the inductive skeptical claim that neither really confirms (10). To show that the black raven really confirms (10) would be to solve Hume's problem not Hempel's. Hempel's paradox arises from the conjunction of the equivalence principle and the claim that instances confirm their generalizations. My solution is in the claim that admitting that (10) is Carnap-confirmed by a white shoe no longer seems paradoxical one we realize that we are ultimately interested in real confirmation rather than mere Carnap-confirmation.

More formally, Hempel's paradox is generated by the following set of prima facie plausible yet prima facie incompatible claims

[12] For Carnap's probabilistic notion of confirmation it follows from the way probabilities are constructed under measure functions that the equivalence principle always holds.
[13] For an exhibition of a measure function having this property see Appendix I below.

(A) Any (contingent) generalization is confirmed by its instances.
(B) If e confirms h and h' is equivalent to h then e confirms h'.
(C) A non-black non-raven does not confirm the claim that all ravens are black.

Yet this air of paradox depends on an equivocation on the notion of confirmation. This becomes apparent when we disambiguate the different notions of confirmation using our distinction between real confirmation and Carnap-confirmation. The disambiguated versions of (A), (B) and (C) are

(A') Any (contingent) generalization is Carnap-confirmed by its instances.
(B') If e Carnap-confirms/really confirms h and h is equivalent to h then e Carnap-confirms/really confirms h'.
(C') A non-black non-raven does not really confirm the claim that all ravens are black.

There is no hint of logically inconsistency between (A'), (B'), and (C').

Now of course (B') and (C') are inconsistent with

(A'') Any (contingent) generalization is really confirmed by its instances.

However (A'') is not an intuitively acceptable thesis and, more importantly, accepting a natural measure function which commits one to (A') need not commit one to (A'').

The particular problem posed by the ravens paradox for probabilistic theories of confirmation is that many natural measure functions yield the result that both a black raven and a non-black non-raven Carnap-confirm the claim that all ravens are black, yet, intuitively, only the black raven confirms that claim. Those who favor probabilistic accounts of confirmation seem to face the following dilemma: Either they reject a huge class of natural measure functions or accept that a non-black non-raven confirms the claim that all ravens are black. Now we see they do not need to take either of these unappealing options; rather they can claim that adopting a measure function which has the consequence that a non-black non-raven Carnap-confirms the claim that all ravens are black does not commit one to the claim that the non-black non-raven confirms that claim in the intuitive sense of confirmation since that sense is better identified with real confirmation as defined by D2 than Carnap-confirmation as defined by D1.

Now let us consider a version of Goodman's paradox.

Suppose we know that a_1, a_2, and a_3 are all emeralds but have so far observed only a_1 and a_2, yielding the evidence statement

(15) $(Ea_1 \& Ga_1) \& (Ea_2 \& Ga_2)$

Given our interpretation of the predicates 'G' and G*' from (15) we may deduce

(16) $(Ea_1 \& G^*a_1) \& (Ea_2 \& G^*a_2)$.

Now if we adopt the principle that instances always confirms their generalizations we would be committed to claiming that our evidence confirms both of the generalizations

(17) $(x)(Ex \supset Gx)$

and

(18) $(x)(Ex \supset G^*x)$.

While (17) and (18) are consistent with each other when combined with our background information that a_1, a_2 and a3, are emeralds (17) and (18) yield the conflicting claims

(19) Ga_3

and

(20) G^*a_3.

But note, adopting the principle that instances always confirm their generalization is not the same as adopting the principle that instances always really confirm their generalization. In the case of (16) and (18) we are free to claim that (16) confirms (18) through mere content-cutting and hence that it is not favorably relevant to the untested part of (18) relative to (16), namely

(21) $Ea_3 \supset G^*a_3$.

In other words, we are free to claim that (16) only Carnap-confirms (18) through content-cutting and does not really confirm (18). At the same time we may claim that (15) both Carnap-confirms and really confirms (17). In particular we may claim that (15) is favorably relevant to the untested part of (17), namely

(22) $Ea_3 \supset Ga_3$.

Note, it is not being claimed that one must accept that (15) really confirms (17) while (16) only Carnap-confirms but does not really confirm (18). One sceptical of our normal inductive practices might claim that it is (16) that really confirms (18) while (15) Carnap-confirms but does not really confirm (17). Alternatively, an inductive sceptic will claim that neither of (15) or (16) really confirm their respective generalizations (17) and (18). Again, we are not here trying to solve the problem of inductive scepticism. Rather it is being claimed that the claim that instances Carnap-confirm, which entails that both the green emerald and the grue emerald Carnap-confirm their respective generalizations, does not spell trouble for probabilistic conceptions of confirmation once we take note of the distinction between confirmation and real confirmation.

In summary then, our solution to the paradoxes of induction, as they arise for the cases of the grue emeralds and the non-black non-raven, is that they all involve cases of Carnap-confirmation by mere content-cutting. They are not cases of real confirmation.

6. The Problem of Instance Confirmation

Both the Raven paradox and the Grue paradox result from the principle of "instance confirmation":

(A) Any (contingent) generalization is confirmed by its instances.

Now consider a generic universal generalization

(23) $(x)(Px \supset Qx)$.

We can distinguish two notions of an instance of (23). Commonly, an instance of such a sentence is taken to be any sentence which results from eliminating the quantifier, replacing the remaining free variables with a individual constant and replacing the conditional operator, in the case of (23), with a conjunction operator. For example, in this sense,

(24) $Pa_1 \& Qa_1$

is an instance of (23). If we adhere more stringently to the logicians notion of an instance, we get a slightly different notion of instance. For the logician an instance of a universal statement is what results from applying the rule of universal instantiation to the statement in question. In this sense

(25) $Pa_1 \supset Qa_1$

is an instance of (23).

Hereafter, we shall use the term strong instance for the first type of case. We will use the term weak instance to refer to the second case. So (24) is a strong instance of (23) and (25) is a weak instance of (23).

Now it follows from the probability calculus that where e is a weak instance of, and hence a consequence of, h, e Carnap-confirms h, provided $0<P(h)$ and $P(e)<1$. While no such result is derivable for strong instances, under many natural measure functions it follows that strong instances Carnap-confirm. [14] What does not follow from the calculus or the calculus combined with natural measure functions is that instances weak or strong always really confirm their generalization.

[14] This is true of Carnap's favored measure function m*. Cf. Carnap 1962, 563.

Here then we have an answer to the problem of instance confirmation as it arises for probabilistic conceptions of confirmation.

The paradoxes of induction have often been taken to show that attempts to define the intuitive notion of confirmation by recourse to the probabilistic notion of favorable relevance are doomed to failure. The argument here is roughly as follows

(i) Intuitively, the grue emerald and the white shoe do not represent cases of confirmation of the relevant generalizations. And more generally, intuitively, instances do not always confirm.

(ii) Given the identification of confirmation with the probabilistic notion of favorable relevance it pretty much falls out of the calculus that the grue emerald and the white shoe confirm the relevant generalizations. And more generally it falls out that instances confirm.

Therefore

(iii) Attempts to capture the intuitive notion of confirmation using the probabilistic notion of confirmation are fatally flawed.

The problem with this argument is that it assumes that any attempt to capture the intuitive notion of confirmation in terms of the probabilistic notion of favorable relevance will proceed by simply identifying confirmation with favorable relevance ala the Carnapian D1. However, as we have seen, there is an alternative to this simple identification. We may identify the intuitive notion of confirmation with what we have here called real confirmation. In this case mere favorable relevance will not be sufficient for intuitive confirmation. Rather we only get intuitive confirmation where the evidence e is favorably relevant to every content part of h. That is to say, the intuitive notion of confirmation is better represented by our notion of real confirmation, as defined by D2, than by Carnap-confirmation, as defined by D1.

The intuitive notion of confirmation is transmittable over content parts and does not give rise to the paradoxical Goodman-Hempel cases. These facts have militated against probabilistic notions of confirmation. What we have seen above is that once we have replaced the traditional notion of content with the one offered above we are in a position to define a new probabilistic notion of confirmation which more truly accords with the intuitive notion.

While our distinction between real confirmation, Carnap-confirmation and mere content-cutting allows us to dissolve the air of paradox posed by Hempel's non-black non-raven and Goodman's grue emerald cases for attempts to explicate confirmation in terms of probabilistic favorable relevance it still leaves us with an important lesson to be learned from those cases. In its simplest form the lesson is this: We should be wary of any attempt to construct a merely formal, in particular, a merely syntactical, theory of real confirmation. A syntactically based principle such as

(A) Any (contingent) generalization is confirmed by its instances.

is only acceptable when 'confirms' is interpreted in the sense of Carnap-confirmation.[15] This is not to say that we are obliged to accept (A) as a principle governing Carnap-confirmation. The crucial point is that should we adopt a measure function which yields the result that strong instances, in the absence of specific background information, Carnap-confirm their generalization this in itself no longer suggests an air of paradox once we accept that the intuitive notion of confirmation is captured by real confirmation rather than Carnap-confirmation.[16] Where 'confirms' is understood in terms of real confirmation (A) is unacceptable. This is no small lesson. Since we really are interested in real confirmation rather than mere Carnap-confirmation, the real lesson here then is that we cannot hope for a merely syntactic account of a truly substantive notion of confirmation.

7. Traditional Bayesian Solutions I: Hosiasson-Alexander-Mackie

Before proceeding it behooves us to consider more traditional Bayesian solutions to the paradoxes. While this will help further clarify some of the advantages of identifying confirmation with real confirmation rather than Carnap-confirmation, readers not sympathetic to the traditional Hosiasson-Alexander-Mackie or I.J Good type solutions might skip to section 9.

To keeps matters simple we will address only the paradox of the ravens.

It is commonly claimed, for instance in Alexander [1958, p.232], that the generic positive instance

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[15] Note, where confirms is understood as D2 real confirmation (A) is incoherent. To see this consider the predicates 'Gx' and 'G*x' of a language where individual constants name uniquely and where 'G*x' abbreviates '(x=b & ~Gx) v (x≠b & Gx)'. Then according to (A), combined with D2's notion of confirmation, 'Ga' really confirms '(x)(Gx)' and hence is favorably relevant to its content part 'Gb', and 'G*a', which is logically equivalent to 'Ga', really confirms '(x)(Gx*)' and hence is favorably relevant its content part '~Gb'.

[16] I.J. Good and others have shown that relative to certain background information strong instances can Carnap-disconfirm their generalizations (Cf. Good 1967). The claim here is that in the Hempelian context of no background information the claim that strong instances Carnap-confirm their generalizations is acceptable. This is not to say that we are obliged to accept it. The crucial point is that should we adopt a measure function which yields the result that strong instances, in the absence of specific background information, Carnap-confirm their generalization this in itself no longer suggests an air of paradox once we accept that the intuitive notion of confirmation is captured by real confirmation rather than Carnap-confirmation. For more on the problems with Good style solutions to the paradox see section 7. and, especially, 8. below.

(9) Ra_1 & Ba1

better supports its generic generalization

(10) $(x)(Rx \supset Bx)$

then does

(11) $\sim Ra_1$ & $\sim Ba_1$

only if P(Rx)<P(~Bx).[17] This is often cited as part of the traditional Bayesian solution to Hempel's paradox of the ravens. The basic idea here being that our tacit assumption that P(Rx) is much less than P(~Bx) makes the degree of confirmation of (10) by (11) look so negligible compared to degree of confirmation of (10) by (9) that we are inclined to count (11) as not confirming (10) at all.

In fact, as demonstrated in Appendix II below, P(Rx)<P(~Bx) is not a necessary condition for P((10)/(9))>P((10)/(11)).

While Alexander [1958] explicitly claims that R(Rx)<P(~Bx) is a necessary condition for P((10)/(9))>P((10)/(11)) the argument he marshals for explaining the paradox actually depends on the claim that P(Rx)<P(~Bx) is a sufficient condition for P((10)/(9))>P((10)/(11)). In particular, in proposing a solution to Hempel's Raven paradox, Alexander claims that the fact that we naturally assume that the probability of something being non-black is greater than the probability of its being a raven explains why we are inclined to take the black raven as providing greater confirmation for the claim that all ravens are black than is provided by the non-black non raven. But assuming that the probability of a thing being non-black is greater than the probability of a thing being a raven only provides a clear rationale for taking the black raven to provide more confirmation than the non-black non-raven if the probability of something being non-black being greater than the probability of its being a raven is sufficient to ensure that the black raven provides more confirmation than the non-black non-raven.

Now, as shown in Appendix II below, P(Rx)<P(~Bx) is not sufficient to ensure that P((10)/(9))>P((10)/(11)).

The claim that P(Rx) being less than P(~Bx) is necessary for P((10)/(9))>P((10)/(11)) bears a marked resemblance to the claim that the condition that there are more non-Bs than there are Rs is a necessary condition for P((10)/(9))>P((10)/(11)). Indeed it is natural to assume that there are more non-B things than there R things if and only if P(Rx)<P(~Bx). That the idea that there are more non-Bs than there are Rs is a necessary condition for P((10)/(9))>P((10)/(11)) is flawed is suggested by the consideration of the case of the confirmation of the claim 'All ultimate

[17] Alexander uses 'ϕ' and ' φ' were we use 'R' and 'B' but nothing hangs on this difference.

particles are charged'. Here it seems that one could well believe that there are more ultimate particles then there are non-charged things yet still take a charged ultimate particle as providing better confirmation for that claim then that provided by a non-charged non-ultimate particle. By the same token, imagine you are examining, quantifying over, a particular species. You know that 99% of the species have gene G and at least 50% have hormone H. You also know that '(x)(~G ⊃~Hx)' is true. That is to say, you know that having G is a necessary condition for having H. You are now wondering if having G is a sufficient condition for having H. That is to say, you are assessing whether '(x)(Gx ⊃ Hx)' is true. Here clearly the number of the domain that have G (i.e. 99%) exceeds the number of the domain that lack H (i.e. at most 50%). Yet here 'Ga&Ha' lends greater confirmation to '(x)(Gx ⊃ Hx)' than does '~Ga&~Ha'.

Mackie [1963, p.267], picking up from Alexander, explicitly claims that the condition that there are more non-Bs than there are Rs is a necessary condition for P((10)/(9))>P((10)/(11)).[18] As shown in appendix II below this claim is false.

Analogous to the Alexander case, while Mackie explicitly claims that there being more non-Bs than Rs is a necessary condition for P((10)/(9))>P((10)/(11)) his deployment of this alleged fact in his solution of the raven paradox suggests that he is implicitly assuming that there being more non-Bs than Rs is a sufficient condition for P((10)/(9))>P((10)/(11)). In fact both Alexander and Mackie cite as a precursor Hosiasson-Lindenbaum [1940] who actually does not in fact claim that there being more non-Bs than Rs is a necessary condition for P((10)/(9))>P((10)/(11)). Rather she claims, [1940, p.137], that ceterus paribus, there being more non-Bs than Rs is a sufficient condition for P((10)/(9))>P((10)/(11)).[19] Now what exactly is involved in this ceterus paribus clause is never clearly specified. However for our purposes it will suffice to simply note that there being more non-Bs than Rs is a not sufficient condition for P((10)/(9))>P((10)/(11)) - for proof see Appendix II below.

In fact, while neither the condition that P(Rx) is less than P(~Bx) nor the condition that there be more non-Bs than Rs is sufficient to guarantee that P((10)/(9))>P((10)/(11)) the following set of conditions are jointly sufficient for P((10)/(9))>P((10)/(11)) :

(i) P(Ra/(x)(Rx ⊃ Bx))=P(Ra)
(ii) P(Ba/Ra)=P(Ra)
(iii) P(~Ba/(x)(Rx ⊃ Bx))=P(~Ba)
(iv) P(Ra)<P(~Ba).

(For a demonstration of this claim see Appendix III below.)

Now at this point the degree of complication and dubiousness of these conditions, especially condition (iii), calls for a diagnosis of what has gone wrong with the

[18] Cf. Mackie [1963], p267. Mackie makes the claim using the symbols 'ϕ' and 'φ' where we use 'R' and 'B'. Of course nothing hangs on this difference.
[19] Hosiasson-Lindenbaum uses 'A' where we use 'R' but nothing hangs on this difference.

Lindenbaum-Alexander-Mackie solution rather than any attempt to revive it. After all, that solution was predicating on explaining the basis of our intuitive judgment that the non-black non-raven does not confirm the claim that all ravens are black. Now it is one thing to claim that we naturally assume that $P(Rx)<P(\sim Bx)$ and quite another to say that we naturally assume each of (i) to (iv).

Diagnosis: What leads one to suggest that if $P(Rx)$ is less than $P(\sim Bx)$, or, alternatively, if there are more non-Bs than Rs, then 'Ra&Ba' lends more confirmation to '$(x)(Rx \supset Bx)$' then is lent by '~Ra&~Ba'? The thinking that leads to this conclusion, I suggest, is that there are two obvious ways one might conclusively confirm a universal material conditional such as the claim that all ravens are black. One might check all the ravens to see if any of them is non-black or one might check all the non-black things to see if any of them is a raven. Now where there are many more non-black things then there are ravens, or alternatively where $P(Rx)$ is substantially less than $P(\sim Bx)$, searching the space of non-black things and coming across a non-black non-raven leaves one with a much bigger remaining search space then one is left with when one is checking the smaller space of ravens and discovers a black raven. A single black raven represents a much larger percentage of the raven search space then a single non-black non-raven represents for the non-black things search space.

Now note, the implicit reasoning here is that confirmation comes from cutting down search space; the less space left to be searched the greater the confirmation. Now in fact if this is one's reasoning one would naturally conclude that were one makes no presumption about the ratio of ravens to non-black things and hence makes no presumption about the size of the relevant search spaces one would count a black raven and a non-black non-raven as equally confirming the claim that all ravens are black. Interestingly this exact conclusion is reached by Mackie

> If we consider the problem in the setting Hempel proposed it, namely without reference to any additional knowledge, then we must say that if the observation of a black raven confirms h ['h' being 'All ravens are black'] then an observation of a non-black non-raven equally confirms h. (Mackie [1963], p.266)

Now a robust inductivist sees things differently. An instance confirms a generalization not simply through cutting down the search space of potential falsifiers but by probabilifying other instances of the generalization. Finding black raven a_1 confirms all ravens are black not simply because it eliminates raven a_1 as a potential falsifier but because it gives some reason to believe that other ravens are black. Only a deductivist, for instance a Popperian, sees confirmation - a Popperian would of course say corroboration - as coming solely through this type of content cutting. The Hosiasson-Mackie-Alexander Bayesian solution to the raven paradox of confirmation with it's implicit assumption that confirmation comes solely through cutting the search space of potential falsifiers harbors an unhealthy resemblance to inductive skepticism.

The Hosiasson-Mackie-Alexander solution relies on a tacit deductivist assumption that confirmation comes through deductive cutting of search space. Our solution, offered in section 5 above, relies on the explicit inductivist notion that some instances probabilify the remaining instances while others do not. In as much as we are inductivists this can be counted as a significant virtue of our solution.

To put the point another way: The traditional Bayesian solution treats the difference in the confirmation afforded by the black raven and the confirmation afforded by the non-black non-raven as a merely quantitative difference; the black raven lends more Carnap-confirmation to the claim that all ravens are black then is lent by the non-black non-raven. Our explanation in terms of real confirmation attempts to explain the difference as a qualitative one; the black raven really confirms the claim that all ravens are black while the non-black non-raven only Carnap-confirms that claim.

8. Traditional Bayesian Solutions II: The I.J. Good Solution

Recall in section 5. above it was shown that even accepting a measure function that yields the result that strong instances are always probabilistically favorably relevant to their generalization need not yield the unintuitive result that a white shoe confirms 'All ravens are black'. We can avoid this result by identifying confirmation of h with favorable relevance to every content part of h, ala D2, rather than identifying confirmation of h with mere favorable relevance to h, ala Carnap's D1.

Now Good (1967) argues that we should reject the claim that strong instances always Carnap-confirm their generalizations. His point is that in the light of particular background information a strong instance may be probabilistically unfavorably relevant to its generalization. This, as noted earlier, is to move way from the setting considered by Hempel, namely, that in which no background evidence is taken into account. For some such a setting is frankly inconceivable. But what Good misses here is the realization that there are cases where, even in light of one's background theory, evidence e can be favorably relevant to hypothesis h yet intuitively e does not confirm h.

For instance, one might have a background theory which leads one to increase one's subjective probability for '$(x)(Rx \supset Bx)$' on the evidence of '$\sim Ra \& \sim Ba$'. Yet this alone should not commit one to the claim that '$\sim Ra \& \sim Ba$' confirms, in the intuitive sense of 'confirms', or alternatively, gives reason for believing, '$(x)(Rx \supset Bx)$'. After all, in such a situation it could well be that '$\sim Ra \& \sim Ba$' leads one to increase one's subjective probability for both '$(x)(Rx \supset \sim Bx)$' and '$(x)(Rx \supset Bx)$' while not increasing one's subjective probability for either '$Rb \supset Bb$' or '$Rb \supset \sim Bb$'. Thus suppose one's domain of quantification is aviary A which one knows to contain nothing but 2 birds a and b at least one of which is a raven. Then finding that a is a non-black non-raven might lead one to increase the probability of both '$(x)(Rx \supset \sim Bx)$' and '$(x)(Rx \supset Bx)$' merely on the grounds that a has been eliminated as a potential defeater of either of these generalizations. Yet even if the observation of a non-black non-raven leads one in this case and in light of one's background theory to increase the probability of both '$(x)(Rx \supset \sim Bx)$' and '$(x)(Rx \supset Bx)$' this should not be sufficient to saddle one with the claim that that observation confirms, gives reason for believing, each of those generalizations. The point here is that even if one has a background theory that commits one to increasing the probability of '(x)(Rx ⊃ Bx)' on the observation of a particular non-black non-raven this alone should not commit one to the claim that that non-black non-raven has confirmed '(x)(Rx ⊃ Bx)'.

The paradox of the Ravens as originally presented by Hempel was generated by a number of assumptions including Nicod's claim that strong instances always confirm their generalization. Good tries to avoid the paradoxical results by simply denying Nicod's claim. But for Carnapians and Bayesians who identify confirmation with favorable relevance paradoxical results can be generated without direct appeal to Nicod's claim. The paradox is generated by the clash of the claims

(D) There are background theories b and measure functions/prior probability distributions m such that relative to m and b $\sim\phi a \& \sim\varphi a$ is probabilistically favorably relevant to $(x)(\phi x \supset \varphi x)$ yet intuitively in some such cases $\sim\phi a \& \sim\varphi a$ does not under those conditions confirm $(x)(\phi x \supset \varphi x$.

(E) If $\sim\phi a \& \sim\varphi a$ is favorably relevant to $(x)(\phi x \supset \varphi x)$ relative to measure function/prior probability distribution m and background theory b then $\sim\phi a \& \sim\varphi a$ under those conditions confirms $(x)(\phi x \supset \varphi x)$.

For a further instance, relative to my background theory that room X contains a finite number of people half of them being men and the other half being women, the observation that Jane in room X is a woman wearing a dress might make me increase the probability that all the people in room X are wearing a dress just because it eliminates Jane from a finite pool of potential falsifiers. Still, contra (E), this observation need not, for me, confirm the claim that everyone in the room is wearing a dress. In particular, if that observation does not lead me to increase the probability of the claim that all the men in room X are wearing dresses then I can plausibly say that while that observation is probabilistically favorably relevant to the claim that all of the people in room X are wearing dresses it does not confirm that claim.

The fact, cited by Good, that occasionally relative to background evidence a positive instance is not probabilistically favorably relevant to its generalization does not remove the air of paradox from the claim that wherever an instance, relative to give background information and a given measure function/prior probability distribution, is probabilistically favorably relevant to a generalization that instance confirms that claim.

My suggestion is that Carnapians and Bayesians should accept (D) and reject (E).

9. <u>Carnap-irrelevance and Real Irrelevance</u>

Above I argued that Carnap's identification of confirmation with probabilistic favorable relevance, ala D1, conflicts with various intuitive judgments of confirmation. I will now show that a similar flaw affects attempts to identify the ordinary intuitive notion of irrelevance with probabilistic irrelevance.

According to the traditional probabilistic definition of irrelevance,

D6 *e is irrelevant to h* $=_{df}$ *P(h/e) = P(h)*.[20]

Less widespread, though intuitive enough, is the following definition of probabilistic independence,

D7 *e and h are probabilistically independent* $=_{df}$ *Every member of the set {e, ~e} is irrelevant to every member of the set {h, ~h} and vice ve*rsa.

In fact, the relationships of irrelevance and complete probabilistic independence are equivalent (see Carnap 1962, Chapter VI). This suggests that the irrelevance relationship defined by D6 is a fairly strong relationship. Nevertheless, I will now argue, that it is not sufficiently strong to capture our intuitive notion of irrelevance.

Consider the following statements

(26) Die a_1 came up odd and die a_2 came up even,

(27) Die a_1 came up odd and die a_2 came up 1,3,5, or 6.

Intuitively, P((26)) = 1/4 and P((26)/(27)) = 1/4. So according to D6, (27) is irrelevant to (26). Yet, intuitively, (27) is not irrelevant to (26). At least it is not irrelevant to (26) in the same way that, say, the claim

(28) The moon is made of green cheese

is, though, presumably, P((26)/(28)) = P((26)/(27)) = 1/4.

Normally we use the notion of irrelevance in such maxims as "Ignore irrelevant evidence." Yet clearly a gambler about to bet on (26) would be ill advised to ignore the

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[20] Carnap 1962, 348, offers this type of definition. Note, rather than defining e's being irrelevant to h, Carnap defines i's being irrelevant to h with respect to (background) evidence e. Our D6 may be seen as the limiting case of Carnap's definition where Carnap's e is taken to be any tautology. More generally, in the rest of this chapter, we will write of 'e having relationship X to h' where Carnap writes of 'i having relationship X to h with respect to e'. Nothing substantial depends on this substitution, it is merely a matter of notational convenience.

evidence statement (27). D6 gives us a precise enough notion of irrelevance. However, in as much as it is not the notion of irrelevance appropriate to capture our most important intuitions about irrelevance it is precisely useless. The main reason we want a notion of irrelevance is so that we can express what evidence we can safely ignore in assessing a given theory. Clearly it is not simply irrelevant evidence as defined by D6.

Interestingly, Carnap himself tried to define stronger notions of irrelevance than that of D6. Presumably he did so because he realized that sometimes statements which are probabilistically irrelevant to each other are in an intuitive sense relevant to each other. In particular, he defined the notions of "extreme irrelevance" and "complete irrelevance" as follows,

D8 *e is extremely irrelevant to h* $=_{df}$ *e is irrelevant to h and for every j, if j* $\vdash$ *e then j is irrelevant to h*,

D9 *e is completely irrelevant to h* $=_{df}$ *e is irrelevant to h and for every j, if e* $\vdash$ *j then j is irrelevant to h*.[21]

In fact, the relationship of extreme irrelevance as defined by D8 is near empty. For any e and h, provided P(h/e)>0 and P(h)<1, e is not extremely irrelevant to h, since there always is some j, namely Úh&e¿ such that jÃe and j is not irrelevant to h. Similarly, the relationship of complete irrelevance is near empty. For any e and h, provided 0<P(e v h)<1 and P(h)>0, there is always some j, namely e v h, such that eÃ j and j is not irrelevant to h. In particular, on these definitions

(28) The moon is made of green cheese

is neither extremely nor completely irrelevant to

(26) Die a_1 came up odd and die a_2 came up even.

More promising, for a stronger notion of irrelevance than that offered by D8 or D9, is the following

D10 *e is really irrelevant to h* $=_{df}$ *e is irrelevant to every content part of h.*

Presumably, such a definition did not occur to Carnap because he labored under the traditional D3 notion of content as consequence class. Given this notion, real irrelevance, as defined by D10, is an empty notion. There is always some consequence of h, namely Úh v e¿, such that e entails, and hence is not irrelevant to, that consequence. In particular, where content is understood as consequence class, (28) is not really irrelevant to (26).

[21] Carnap's definitions of extreme irrelevance and compete irrelevance (Carnap 1962, 411 and 415) are notationally different from D7 and D8 above, and are offered only with respect to limited languages. These restrictions in no way effect the substance of the points made here.

However where content is understood in our new way, as defined by D4, D10 has exactly the right consequences. Understood this way

(28) The moon is made of green cheese

is really irrelevant to

(26) Die a_1 came up odd and die a_2 came up even

while

(27) Dies a1 came up odd and die a_2 came up 1,3,5, or 6

is not.

The content parts of (26), as defined by D1 above, are

(29) a_1 came up odd

(30) a_2 came up even

and (26) itself (and any logical equivalent of these three). Presumably, P((29)/(28)) = P((29)), P((30)/(28)) = P((30)), and P((26)/((28)) = P((26)). So (28) is really irrelevant to (26). On the other hand, (27) is not really irrelevant to (26). For instance, presumably, P((29)) = 1/2 and P((29)/(27)) = 1, so P((29)/(27)) ≠ P((29)).

The everyday intuitive notion of irrelevance, according to which (28) is irrelevant to (26) and (27) is not irrelevant to (26), is not captured by the Carnapian notion of irrelevance as defined by D6. It is however captured by our new notion of real irrelevance as defined by D10. This new notion of real irrelevance, like our new notion or real confirmation, can only be given substance by using our new D4 notion of content.

10. Confirmation of Theories Vs. Confirmation of Simple Hypotheses

So far I have primarily argued that the Carnapian notion of confirmation as probabilistic favorable relevance is too weak a notion to capture the intuitive notion of confirmation. In its place I offered the notion of real confirmation, that is confirmation as favorable relevance to every content part of a given hypothesis. Now perhaps it may be argued that real confirmation is too strong a notion to capture the intuitive notion of confirmation. The force of this objection becomes clear when we move from a consideration of the confirmation of simple hypotheses, for instance the law-like universal conditionals which are the focus of most of the discussion above, to consideration of broader theories. It is one thing to claim that there are ample cases, such as that of the observed black raven, where the relevant observation statements are favorably relevant to every content part of a simple hypothesis, such as 'All ravens are black'. It is quite another thing to claim that in the typical case of confirmation of a complex theory of broad content that the relevant observational evidence is favorably relevant to every content part of the theory. For instance, the CERN experiments which confirmed Bell's Theorem are generally taken to confirm Quantum Mechanics (QM) even though they are not taken to be favorably relevant to the dynamical postulates of QM such as the Schrodinger's wave equations.

To consider the merits of this objection it will help if we first back up a bit and review some of the recent work on the question of whether confirmation should be seen as widely distributed to theories as wholes or more finely distributed to particular postulates of theories.

As briefly noted in section 2., Glymour trenchantly attacks hypothetico-deductive (H-D) accounts of confirmation on the grounds that they are far too promiscuous in where they find confirmation. In particular, he objects that where consistent h entails contingent e, for any h' such that the conjunction of h' and h is consistent, e H-D confirms both h and the conjunction of h and h'. So if e H-D confirms a part of a theory h it will confirm the theory as a whole. This is a large part of the motivation for Glymour's bootstrapping account of confirmation. Glymour's account allows that evidence e may selectively bootstrap confirm part of a theory without bootstrap confirming the theory as a whole. As noted in 2. above the charge of promiscuity holds similarly for the Carnapian notion of confirmation as favorable relevance. In particular, where h entails e, it follows that e Carnap-confirms both h and the conjunction of h and h' provided $P(e)<1$ and $0<P(h\&h')$.

Let us consider an example.

Let h be Kepler's first law that the planets move in elliptical orbits around the sun and h' be the third law which states that for any two planets the ratio of the square of the periods of orbit is equal to the ratio of the cubes of the mean distances from the sun. Let b be some background evidence about the observed positions of a given planet over the course of a couple of months. Let e be some prediction of the future position of the relevant planet which is derivable from the conjunction of b and h.[22] Now in this case, relative to b, e H-D confirms both h and the conjunction of h and h'. By the same token, given fairly trivial assumptions we have the result that, relative to b, e Carnap-confirms both h and the conjunction of h and h'. So if we assume the identification of confirmation with Carnap-confirmation ala D1, we have the result that e Carnap-confirms both h and the conjunction of h and h'. To see how counter-intuitive these results are let h'' be the negation of h', or, if you prefer, a variant of h' which says that for any two planets the ratio of the square of the periods of orbit is equal to the ratio of the cubes of the mean distances from the sun multiplied by 3. Then we have the result that, relative to b, e H-D

[22] e is not to be derivable from b alone.

confirms h, the conjunction of h and h' and the conjunction of h and h". Similarly, making the appropriate trivial assumptions, and assuming the D1 identification of confirmation with Carnap-confirmation, we have the result that relative to b, e confirms h, the conjunction of h and h' and the conjunction of h and h".[23]

Now note, if we take confirmation in the sense of real confirmation we do not get any analogs of these results. Given appropriate measure functions we get the result that in the case above, relative to the background evidence b, e really confirms h but does not really confirm either the conjunction of h and h' or the conjunction of h and h".[24] Real confirmation allows for the type of selective confirmation without which, according to Glymour, any account of confirmation is hopeless.

Now there is some prima facie tension here between the intuitions behind our consideration of the putative objection to real confirmation that it does not allow for various cases of circumscribed observational evidence confirming a broad theory and the intuitions behind our acknowledgment of Glymour's charge that we should be wary of accounts that distribute confirmation too widely. However when we consider particular cases I think we see that the notion of real confirmation delivers the right results. In particular, it delivers the result that given a substantial theory of wide enough scope and content, for instance a theory of planetary motions incorporating Kepler's law, and a circumscribed body of observational evidence, for instance evidence about the orbit of a single planet, that evidence will only really confirm particular parts of theory but not the whole. Does this in fact preclude the possibility of really confirming such a wide ranging theory? Not at all. What it does require is that for real confirmation of such a theory we need a wide body of observational evidence. This result seem exactly right.

But still, if we take the intuitive notion of confirmation to be captured by real confirmation rather than mere Carnap-confirmation then what are we to make of such common pronouncements as "The results of the CERN experiments devised to test Bell's Theorem confirm QM? After all surely the relevant results are not favorably relevant to every content part of QM? Let me parry that question with another, do we want to say that "The results of the CERN experiments devised to test Bell's Theorem confirm the conjunction of QM and the medical theory of Paracelsus?" What these questions point to is that the term "confirm" and its cognates are, especially in contexts dealing with theories rather than simple hypotheses, used in a lot of different ways which are sometimes only loosely related. Often we say such things as "x confirms theory y" with the intent of

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[23] It follows from the calculus that where conjunction of h and b entails e, and hence the conjunction of h, h' and b and the conjunction of h, h" and b entail e, e Carnap confirms h relative to b, e Carnap-confirms the conjunction of h and h' relative to b, and e Carnap-confirms the conjunction of h and h" relative to b, provided $0<P(e\&b)<P(e)<1$, $0<P(h/b)<1$, $0<P(h\&h'/b\&e)$, and $0<P(h\&h''/b\&e)$.

[24] The crucial point here is that it does not follow from the mere fact that where h entails e any measure function m which yields the results that e really confirms h relative to b and $0<P(h\&h'/b\&e)$, and $0<P(h\&h''/b\&e)$, does not thereby yield the result that, relative to b, e really confirms the conjunction of h and h', and the conjunction of h and h".

communicating the information that x confirms some core claim(s) of y over the some core claim(s) of y's well known rival z. In our language we would capture this claim by saying x really confirms some core claim(s) of y over various core claim(s) of z. For example, in saying that the CERN results confirm QM we typically mean that those results really confirm the collapse postulates of QM over the rival claim of Einstein-Rosen-Podorsky that those postulates are radically incomplete. That we take the CERN results to really confirm the collapse postulates rather than merely Carnap-confirm them is shown by the fact that nobody who takes the CERN results as confirmatory would say that those results have not increased their confidence that future experiments are also (now more) likely to coincide with the predications of the collapse postulates. Often we say such things as "x does not confirm theory y" meaning not that x fails to really confirm any core claim of y but simply that x fails to really confirm some particular core claim of y. For example, we say that the CERN results do not confirm the conjunction of QM and the medical theory of Paracelsus meaning that the CERN results are not favorably relevant to the medical theory of Paracelsus.[25] Since our everyday speech is of a rather loose variety it is not surprising that a more formal analysis such as that which generally identifies intuitive confirmation with favorable relevance to every content part fails to give an single formal analog for all our everyday locutions. What we hope is that it can capture, whether directly of through various circumlocutions, those intuitions we couch in everyday jargon that are worth preserving in our more considered formal analyses. On the other hand, if we are to simply insist that the intuitive notion of confirmation is to be understood in all cases as Carnapian favorable relevance, we will not simply be stuck with saying that the CERN results confirm the conjunction of QM and the medical theory of Paracelsus but also with saying that the CERN results confirm QM and the theory that QM is correct until 2000 and false afterwards.

John Earman, (Earman 1992, p.242), has offered a version of the real-confirmation-is-too-strong-a-relation objection. Basically he argues that since real confirmation rarely occurs between a broad theory and circumscribed body of evidence the best we can say in such cases is that the evidence really confirms some substantial part of a theory. But in this case, Earman objects, all the old problems of finding confirmation too easily come back. For instance, consider any theory that contains the consequence that all emeralds are grue. Here the evidence statement that all emeralds observed to-date (1994) are green really confirms a substantial part of the theory, for instance, that part which says that all emeralds observed before the year 2000 are grue. The core of Earman's objection is that real confirmation of a theory is too strong a requirement for intuitive confirmation of a theory yet real confirmation of some part is just too easy. I think what this shows is that we have to be rather careful in considering what

[25] Note, those who insist that the ordinary language term 'confirms' be uniformly understood as meaning the same as 'is probabilistically favorably relevant to' would then have to say that the commonsensical pair of prima facie true claims "The CERN results confirm QM" and "The CERN results do not confirm the conjunction of QM and the medical theory of Paracelsus" are to be respectively translated as the presumably true claim "The CERN results are probabilistically favorably relevant to QM" and the presumably false claim "The CERN are not probabilistically favorably relevant to the conjunction of QM and the medical theory of Paracelsus".

counts as a part of a theory. Elsewhere I have given an account of the notion of a natural axiomatization of a theory. That account demands that every axiom of a natural axiomatization be a content part of the theory in question. Now note that any theory that has as a consequence that all emeralds are grue cannot have as a content part, hence as an axiom of any natural axiomatization of it, the claim that all emeralds examined before the year 2000 are grue.[26] I think we often loosely speak of evidence confirming a theory when we believe that evidence really confirms some axiom of an envisaged natural axiomatization of the theory. Indeed, in designing experimental set-ups this is typically what scientists do, they focus on some specific parts of a theory, where the parts are axioms of an envisaged though perhaps not ever actually expressed version of a natural axiomatization of the theory, and try to get results which really confirm or refute those axioms.

The canvassed objections to our account of intuitive confirmation as real confirmation actually help bring out some of the strengths of that account. Taking confirmation as D2 real confirmation helps us accommodate Glymour's insight that confirmation should not be distributed too broadly. This is an insight that confirmation as mere D1 favorable relevance fails to accommodate. It, especially when coupled with the notion of a natural axiomatization, also allows us to make clear why in certain cases where x is favorably relevant to theory y we talk of x confirming y (e.g. the CERN results and QM) and in other cases were x is favorably relevant to theory y we do not say x confirms y (e.g. the CERN results and the conjunction of QM and Paracelsus, the CERN results and the conjunction of the claims that QM is true before 2000 and false afterwards).

Our everyday use of the term 'confirms' and its cognates when used in connection with the question of the confirmation of simple hypotheses, typically law-like universal conditionals, is best captured by uniformly taking confirmation to be real confirmation, that is favorable relevance to every content part of the hypothesis in question. When 'confirms' is used with respect to complex theories there is no single analysis that fits all our everyday uses. This is true for both Carnap's analysis of confirmation as 'favorably relevance' and the alternative analysis of confirmation as 'favorable relevance to every content part'. What I do believe, for the reasons outlined above, is that those everyday uses are best explicated by recourse to various constructions in terms of real confirmation, (for instance, really confirming some core claim, failing to really confirm some core claim) than constructions in terms of simple Carnap-confirmation. But this matter obviously deserves further investigation.

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[26] Let 'Ex' stand for 'x is an emerald', 'Grx' stand for 'x is grue' and '2000x' stand for 'x is observed before 2000'. Then where T entails '(x)(Ex ⊃ Grx)', '(x)((Ex&2000x) ⊃ Grx)' cannot be an axiom of any natural axiomatization of T since '(x)((Ex&2000x) ⊃ Grx)' is not a content part of T. It is not a content part of T since '(x)(Ex ⊃ Grx)' is a consequence of T that is stronger than '(x)((Ex&2000x) ⊃ Grx)' and contains only atomic wffs that occur in '(x)((Ex&2000x) ⊃ Grx)

11. <u>Conclusion</u>

Carnap initiated a project of attempting to clarify intuitive epistemic notions such as those of confirmation, inductive support, and irrelevance in terms of probabilistic favorable relevance and irrelevance. This project was seen to have various drawbacks. Among the major drawbacks is that it yields a technical notion of confirmation that does not guarantee that confirmation is transmittable over content parts, and, given plausible assumptions, it generates paradoxical cases of confirmation (Hempel's and Goodman's paradoxical cases). The Carnapian identification of irrelevance with probabilistic irrelevance also leads to highly counter-intuitive results. While these results have been generally regarded as drawbacks of the Carnapian project, I have shown that in fact they only militate against that project when it takes for granted the classical notion of content as consequence class. Given an alternative notion of content, which I argue in "A New Theory of content I: Basic Content" is highly plausible in its own right, the Carnapian project can easily be developed in a way that renders it unencumbered by such drawbacks. By the same token, none of this militates against the core of Bayesian confirmation theory which attempts to explicate the intuitive notion of confirmation in terms of the relationship of probabilistic favorable relevance.[27] It only militates against that particular version of Bayesian confirmation theory that follows Carnap by simply identifying confirmation with probabilistic favorable relevance. The explication of the intuitive notion of confirmation in terms of the technical notion of real confirmation developed above should be seen as a version of the Bayesian project.

The introduction of our new notion of content also allows us to define the useful notions of real confirmation and content-cutting which, besides allowing for a version of the Carnapian project unencumbered by the above mentioned drawbacks, also promises us clearer insight into the vexing question of how the theses of inductivism and inductive scepticism might best be expressed within the language of the probability calculus. Furthermore it also gives us tools for elucidating exactly what counts as theory confirmation. But these are projects whose full elaboration is best left to another time.[28]

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[27] The Bayesian Horwich similarly gives a general characterization of the Bayesian project in terms of the general attempt to explicate confirmation in terms of probabilistic terms (Cf. Horwich (1982, p. 13). This suggests that Horwich, who in fact endorses a variant of Carnap's D1 definition of confirmation, could nevertheless recognize our D2 definition of confirmation as being within the spirit of Bayesianism

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Appendix I

It was claimed above that it is possible to construct a measure function yielding the result that

(9) Ra_1 & Ba_1

both Carnap-confirms and really confirms

(10) $(x)(Rx \supset Bx)$

while

(11) $\sim Ra_1$ & $\sim Ba_1$

Carnap-confirms but does not really confirm (10).

To prove this we consider the Carnap type language L2 which contains only the two one-placed predicates 'B' and 'R' and the two individual constants 'a_1' and 'a_2.

Let m@ be the measure function which assigns values to the 16 state descriptions of L_2 as follows,

Ra_1	Ba_1	Ra_2	Ba_2	5/64	Ra_1	Ba_1	Ra_2	$\sim Ba_2$	1/64
Ra_1	Ba_1	$\sim Ra_2$	Ba_2	4/64	Ra_1	Ba_1	$\sim Ra_2$	$\sim Ba_2$	4/64
Ra_1	$\sim Ba_1$	Ra_2	Ba_2	1/64	Ra_1	$\sim Ba_1$	Ra_2	$\sim Ba_2$	4/64
Ra_1	$\sim Ba_1$	$\sim Ra_2$	Ba_2	4/64	Ra_1	$\sim Ba_1$	$\sim Ra_2$	$\sim Ba_2$	5/64
$\sim Ra_1$	Ba_1	Ra_2	Ba_2	4/64	$\sim Ra_1$	Ba_1	Ra_2	$\sim Ba_2$	4/64
$\sim Ra_1$	Ba_1	$\sim Ra_2$	Ba_2	4/64	$\sim Ra_1$	Ba_1	$\sim Ra_2$	$\sim Ba_2$	4/64
$\sim Ra_1$	$\sim Ba_1$	Ra_2	Ba_2	4/64	$\sim Ra_1$	$\sim Ba_1$	Ra_2	$\sim Ba_2$	5/64
$\sim Ra_1$	$\sim Ba_1$	$\sim Ra_2$	Ba_2	4/64	$\sim Ra_1$	$\sim Ba_1$	$\sim Ra_2$	$\sim Ba_2$	7/64

Under m@, both (9) and (11) Carnap-confirm (10) - P((10))=40/64, P((10)/(9))=13/14, and P((10)/(11))=48/64. Under m@, only (9) really confirms (10). (9) really confirms (10) because it Carnap-confirms each of (10)'s three logically distinct content parts, namely (10),

(13) $Ra_1 \supset Ba_1$

(14) $Ra_2 \supset Ba_2$.

(9) Carnap-confirms (13) and (14) since P(13)=50/64, P((13)/(9)=1, P(14)=50/64, and P((14)/(9))=13/14. (11) does not really confirm (10) because it does not Carnap confirm (10)'s content part (14) - P(14)=50/64 and P((14)/(11))=48/64.

It is similarly possible to construct a measure function yielding the result that

(15) $(Ea_1 \& Ga_1) \& (Ea_2 \& Ga_2)$

both Carnap-confirms and really confirms

(17) $(x)(Ex \supset Gx)$,

while

(16) $(Ea_1 \& G^*a_1) \& (Ea_2 \& G^*a_2)$.

Carnap-confirms but does not really confirm (18) $(x)(Ex \supset G^*x)$.

The exhibition of such a measure function can be found in Gemes (1991) p.48-49.

Appendix II

(1) The condition that P(Rx)<P(~Bx) is not necessary for P((10)/(9))> P((10)/(11)). Proof: Consider the language L_2 whose universe of discourse includes just two individuals. For simplicity we suppose the language L_2 to have only the 2 predicates 'R' and 'B' and the two individual constants 'a' and 'b'. Let m be the measure function which distributes the following probabilities to the 16 possible state descriptions of L_2:

Ra Ba Rb Bb 10/16	Ra Ba ~Rb ~Bb 1/16	Ra ~Ba ~Rb ~Bb 2/16
~Ra ~Ba Rb Bb 1/16	~Ra ~Ba Rb ~Bb 2/16	All other state descriptions get 0

In this case P(Rx)=13/16, P(~Bx)=5/16, P((10)/(9))=1 and P((10)/(11))=1/3. So under m, it is not the case that P(Rx)<P(~Bx), yet P((10)/(9))>P((10)/(11)). This shows that the condition that P(Rx)<P(~Bx) is not necessary for P((10)/(9))>P((10)/(11)).

(2) The condition that P(Rx)<P(~Bx) is not sufficient for P((10)/(9))>P((10)/(11)).

Proof: Consider the measure function m1 which distributes probabilities to the 16 state descriptions of L_2 in the following manner:

Ra Ba Rb Bb 2/16	Ra Ba Rb ~Bb 2/16	Ra ~Ba Rb Bb 2/16
Ra ~Ba Rb ~Bb 1/16	~Ra ~Ba ~Rb ~Bb 9/16	All other state descriptions get 0.

In this case P(Rx)=7/16, P(~Bx)=12/16, P((x)(Rx>Bx)/Ra&Ba)=1/2and P((x)(Rx>Bx)/~Ra&~Ba)=1. So under m, P(Rx)<P(~Bx), yet it is not the case that P((Rx>Bx)/Ra&Ba)>P((x)(Rx>Bx)/~Ra&~Ba). This shows that the condition that P(Rx)<P(~Bx) is not sufficient for P((10)/(9))>P((10)/(11)).

(3) The condition that there are more non-Bs than there are Rs is not necessary for P((10)/(9))>P((10)/(11)).

Proof: Consider the language L_3 whose universe of discourse includes just three individuals. For simplicity we suppose the language L_3 to have only the 2 predicates 'B' and 'R' and the three individual constants 'a', 'b', and 'c'. Let m_2 be the measure function which distributes the following probabilities to the 64 possible state descriptions of L_3:

Ra Ba Rb Bb Rc Bc 1/7	~Ra ~Ba Rb ~Bb Rc Bc 1/7
~Ra ~Ba Rb Bb Rc ~Bc 1/7	Ra ~Ba ~Rb ~Bb Rc Bc 1/7
Ra Ba ~Rb ~Bb Rc ~Bc 1/7	Ra ~Ba Rb Bb ~Rc ~Bc 1/7
Ra Ba Rb ~Bb ~Rc ~Bc 1/7	All other state descriptions get 0.

Under m_2 the number of ravens is at least 2 (i.e. under m_2, P((x)(y)(Rx & Ry & x≠y)) = 1) and the number of non-black things is at most 2 (i.e. under m_2, P((x)(y)(z)(((~Bx & ~By & x≠y) ⊃ (~Bz⊃ (z=x v z=y)))) = 1), yet P((x)(Rx>Bx)/Ra&Ba)=1/3 and P((x)(Rx>Bx)/~Ra&~Ba)=0. So under m_2, it is not the case that the number of non-Bs is greater than the number of Rs, yet P((10)/(9))>P((10)/(11)). This shows that the condition there are more ~B's than Rs is not necessary for P((10)/(9))>P((10)/(11)).

(4) The condition that are more non-Bs than Rs is a not sufficient for P((10)/(9))>P((10)/(11)).

Proof: Consider the language L4 whose universe of discourse includes just four individuals. For simplicity we suppose the language L4 to have only the 2 predicates 'B' and 'R' and the four individual constants 'a', 'b', 'c' and 'd'. Let m_3 be the measure function which distributes the following probabilities to the 256 possible state descriptions of L4:

~Ra	~Ba	~Rb	~Bb	~Rc	~Bc	~Rd	~Bd	1/13
Ra	Ba	Rb	~Bb	~Rc	~Bc	~Rd	~Bd	1/13
Ra	Ba	~Rb	~Bb	Rc	~Bc	~Rd	~Bd	1/13
Ra	Ba	~Rb	~Bb	~Rc	~Bc	Rd	~Bd	1/13
Ra	~Ba	Rb	Bb	~Rc	~Bc	~Rd	~Bd	1/13
~Ra	~Ba	Rb	Bb	Rc	~Bc	~Rd	~Bd	1/13
~Ra	~Ba	Rb	Bb	~Rc	~Bc	Rd	~Bd	1/13
Ra	~Ba	~Rb	~Bb	Rc	Bc	~Rd	~Bd	1/13
~Ra	~Ba	Rb	~Bb	Rc	Bc	~Rd	~Bd	1/13
~Ra	~Ba	~Rb	~Bb	Rc	Bc	Rd	~Bd	1/13
Ra	~Ba	~Rb	~Bb	~Rc	~Bc	Rd	Bd	1/13
~Ra	~Ba	Rb	~Bb	~Rc	~Bc	Rd	Bd	1/13
~Ra	~Ba	~Rb	~Bb	Rc	~Bc	Rd	Bd	1/13

All other state descriptions get 0.

Under m_3 the number of ravens is at most 2 - i.e. under m_2 P((x)(y)(z)(((Rx & Ry & x≠y) ⊃ (Rz ⊃ (z=x v z=y)))) = 1 - and the number of non-black things is at least 3 - i.e. under m_3, P((∃x)(∃y)(∃z)(~Bx & ~By & ~Bz & x≠y & x≠z & y≠z))) = 1. Yet under m_3, P((x)(Rx>Bx)/Ra&Ba)=0 and P((x)(Rx>Bx)/~Ra&~Ba)=1/7. So under m_3, it is the case that the number of non-Bs is greater than the number of Rs, yet it is not the case that P((10)/(9))>P((10)/(11)). This shows that the condition there are more ~B's than Rs is not sufficient for P((10)/(9))>P((10)/(11)).

It is perhaps worth noting that while the measure functions m, m_1, m_2, and m_3, introduced above, are all rather unusual they all preserve exchangeability of individual constants.

Appendix III

Assume: (i) $P(Ra/(x)(Rx\supset Bx))=P(Ra)$
(ii) $P(Ba/Ra)=P(Ba)$
(iii) $P(\sim Ba/(x)(Rx\supset Bx))=P(\sim Ba)$
(iv) $P(Ra)<P(\sim Ba)$.

Note, where P(Ba/Ra)=P(Ba), P(~Ba/~Ra)=P(~Ba), as demonstrated in Gemes (1991), Appendix II.

Now $$P((x)(Rx\supset Bx)/Ra\&Ba) = \frac{P(Ra\&Ba/(x)(Rx\supset Bx)) \times P((x)(Rx\supset Bx))}{P(Ra\&Ba)}$$

$$= \frac{P((Ra)/(x)(Rx\supset Bx)) \times P((x)(Rx\supset Bx))}{P(Ra) \times P(Ba/Ra)}$$

$$= \frac{P(Ra) \times P((x)(Rx\supset Bx))}{P(Ra) \times P(Ba)} \quad \text{[Sub. by (i), (ii)]}$$

$$= \frac{P((x)(Rx\supset Bx))}{P(Ba)}$$

$$P((x)(Rx\supset Bx)/\sim Ra\&\sim Ba) = \frac{P(\sim Ra\&\sim Ba/(x)(Rx\supset Bx)) \times P((x)(Rx\supset Bx))}{P(\sim Ra\&\sim Ba)}$$

$$= \frac{P((\sim Ba)/(x)(Rx\supset Bx)) \times P((x)(Rx\supset Bx))}{P(\sim Ra) \times P(\sim Ba/\sim Ra)}$$

$$= \frac{P(\sim Ba) \times P((x)(Rx\supset Bx))}{P(\sim Ra) \times P(\sim Ba)} \quad \text{[Sub. by (iii), (ii)]}$$

$$= \frac{P((x)(Rx\supset Bx))}{P(\sim Ra)}$$

Now where P(Ra)<P(~Ba), P(Ba)<P(~Ra), and hence

$$\frac{P((x)(Rx\supset Bx))}{P(Ba)} > \frac{P((x)(Rx\supset Bx))}{P(\sim Ra)}$$

Hence, assuming (i)-(iv),

$$P((x)(Rx\supset Bx)/Ra\&Ba)>P((x)(Rx\supset Bx)/\sim Ra\&\sim Ba)$$

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