Notes for Week 8 of *Confirmation*

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1 Carnapian Explication: The Basics and a Canonical Example

In chapter 1 of *Logical Foundations of Probability*, Carnap describes the methodology he plans to adopt in his development of confirmation theory. He calls it *explication*. Here's how Carnap characterizes explication:

> The task of ***explication*** consists in transforming a given more or less inexact concept into an exact one or, rather, in replacing the first by the second. We call the given concept (or the term used for it) the ***explicandum***, and the exact concept proposed to take the place of the first (or the term proposed for it) the ***explicatum***. The explicandum may belong to everyday language or to a previous stage in the development of scientific language. The explicatum must be given by explicit rules for its use, for example, by a definition which incorporates it into a well-constructed system of scientific either logicomathematical or empirical concepts. . . . if a solution for a problem of explication is proposed, we cannot decide in an exact way whether it is right or wrong. Strictly speaking, the question whether the solution is right or wrong makes no good sense because there is no clear-cut answer. The question should rather be whether the proposed solution is satisfactory, whether it is more satisfactory than another one, and the like.

Carnap lays down four requirements (*desiderata*) for an explicatum in a satisfactory explication:

> A concept must fulfil the following requirements in order to be an adequate explicatum for a given explicandum: (1) similarity to the explicandum, (2) exactness, (3) fruitfulness, (4) simplicity. . . .
>
> (1) The explicatum is to be similar to the explicandum in such a way that in most cases in which the explicandum has so far been used, the explicatum can be used; however, close similarity is not required, and considerable differences are permitted.
>
> (2) The characterization of the explicatum . . . (e.g., in the form of a definition), is to be given in an exact form, so as to introduce the explicatum into a well-connected system of scientific concepts.
>
> (3) The explicatum is to be a fruitful concept . . . useful for the formulation of many universal statements (empirical laws in the case of a nonlogical concept, logical theorems in the case of a logical concept).
>
> (4) The explicatum should be as simple . . . as the more important requirements (1), (2), and (3) permit.

Carnap's canonical example is that of providing an explication of the pre-scientific explicanda "warm" and "warmer than". "Warm" is a *qualitative* (*classificatory*) explicandum and "warmer than" is *comparative*. According to Carnap, we can think of *temperature* (in its precise, scientific sense) as an explicatum for "warmer than". Since temperature is a quantitative concept, we can use it to formulate both comparative an qualitative explicata. For the explicandum "x is warmer than y", we have the explicatum "$t(x) > t(y)$", where $t(x)$ is the temperature of x (operationalized in the modern scientific way). Similarly, we could use a *threshold* explicatum "$t(x) > r$" for the qualitative explicandum "x is warm". Carnap views the *quantitative* concepts as the "most scientific". This seems to be because they are (generally) the most exact and fruitful. However, Carnap does not think that quantitative explications are always required (or even always possible):

> If it is a question of an explication of a prescientific concept, then a situation of the kind described, where we do not succeed in finding an adequate quantitative explicatum, ought not to discourage us altogether from trying an explication. It may be possible to find an adequate comparative explicatum.

Carnap considers a (fictitious) case in which we are unable to measure small differences in volume of samples of mercury (but we can measure large differences). As a result, me may only be able to say "if x is warmer than y (in the prescientific sense) and we bring a body of mercury first in contact with x and later with y, then it has at the first occasion a greater volume than at the second." We may not be able to say precisely what the temperature (of x or y) is. Here, we could still have an adequate comparative explicatum, even though we wouldn't have an adequate quantitative explicatum. Carnap also says that if "no important laws containing [a quantitative] concept have been found", then this could be another reason to favor a purely comparative explication over a quantitative explication. He also says that these points are important in the case of confirmation. As I have mentioned already, I agree with this (we'll say more about that later).

In his discussion of temperature and warmth, Carnap makes several other important points. He points out that some disagreement (dissimilarity) between explicatum and explicandum will be inevitable, but this needn't warrant the rejection of the explication as inadequate. He clarifies the requirement of similarity as:

> The concept Temperature is to be such that, in most cases, if x is warmer than y (in the prescientific sense, based on the heat sensations of the skin), then the temperature of x is higher than that of y.

He then discusses an interesting case in which

> ...I enter a moderately heated room twice, first coming from an overheated room and at a later time coming from the cold outside. Then it may happen that I declare the room, on the basis of my sensations, to be warmer the second time than the first, while the thermometer shows at the second time the same temperature as at the first.... Experiences of this kind do not at all lead us to the conclusion that the concept Temperature defined with reference to the thermometer is inadequate as an explicatum for the concept Warmer. On the contrary, we have become accustomed to let the scientific concept overrule the prescientific one in all cases of disagreement. ...Thus the experience described above is now formulated as follows: "I believed that the room was at the second time warmer than at the first, but this was an error; the room was actually not warmer; I found this out with the help of the thermometer".

This is interesting. On the one hand, Carnap seems to be saying that "a few" instances of dissimilarity (in which the explicandum holds but the explicatum fails) are OK. On the other hand, he seems to be suggesting that (in the case at hand) we *change our mind* about whether the explicandum holds, so as to bring it into agreement with the explicatum. He seems to be saying that although the explicandum *appears* to hold, it "really" does not. He says "but this was an error", and "I found this out with the help of the thermometer". Can we learn about an explicandum by studying an explicatum? We'll return to this key question below.

When it comes to the converse of the above desideratum ("in most cases, if the temperature of x is greater than the temperature of y, then x is warmer than y"), Carnap explicitly rejects it:

> When the difference between the temperatures of x and y is small, then, as a rule, we notice no difference in our heat sensations. This again is not taken as a reason for rejecting the concept Temperature. On the contrary, here again we have become accustomed to the new, scientific concept Warmer*, and thus we say: "x is actually warmer than y, although we cannot feel the difference".

Carnap says he rejects this "explicatum holds ⇒ explicandum holds" direction *even with the caveat* "in most cases". On the other hand, he claims to accept the ⇐ direction *with* the caveat "in most cases". But, his discussion of both directions involves rather unusual cases, and he also suggests that the explicatum operates as a *corrective* for appropriately applying the explicandum. Carnap clarifies things as follows:

> Since the thermometer has a higher discriminating power than our heat sensations, the scientific concept is superior to the prescientific one in allowing more precise descriptions. The procedure leading from the explicandum to the explicatum is as follows. At first the prescientific concept is guiding us in our choice of an explicatum (with possible exceptions, as discussed earlier). Once an explicatum is defined in a relatively simple way, we follow its guidance in cases where the prescientific concept is not sufficiently discriminative. It would be possible but highly inadvisable to define a concept Temperature in such a way that x and y are said to have the same temperature whenever our sensations do not show a difference. This concept would be in closer agreement with the explicandum than the concept Temperature actually used. But the latter has the advantage of much greater simplicity [and, presumably, *precision*?] both in its definition—in other words its method of measurement—and in the laws formulated with its help.

The picture seems to be something like this. We begin with clear-cut cases in which the explicandum holds (or does not), and we develop an explicatum (or explicata) that (approximately?) systematizes these clear-cut ("desiderata") cases in a precise, fruitful, and simple way. Then, by applying the explicatum to cases in which there is no clear-cut verdict explicandum-wise, we can actually learn something about the explicandum (*i.e.*, refine or correct our pre-scientific usage of the explicandum). In the case of warmth and temperature, the picture seems pretty clear. But, one wonders how this story is supposed to go with logical, epistemological, normative, and other sorts of *philosophical* explicanda. Strawson was deeply skeptical that the method of explication was of any use at all in the philosophical domain (*i.e.*, where the explicanda arise in the course of "natural language philosophy"). Next, we'll take a look at Strawson's worry, and Carnap's reply.

2 Strawson's Worry About Explication and Carnap's Puzzling Reply

Strawson worries that Carnapian explication is of little or no use when the explicanda arise in the context of natural language philosophy (or, more generally, in the context of non-scientific discourse). He laments:

> It seems prima facie evident that to offer formal explanations of key terms of scientific theories to one who seeks philosophical illumination of essential concepts of non-scientific discourse, is to do something

> utterly irrelevant—is a sheer misunderstanding, like offering a textbook on physiology to someone who says (with a sigh) that he wished he understood the workings of the human heart.

Carnap's reply to Strawson's worry is puzzling. He offers the following analogy:

> A natural language is like a crude, primitive pocketknife, very useful for a hundred different purposes. But for certain specific purposes, special tools are more efficient, e.g., chisels, cutting machines, and finally the microtome. If we find that the pocket knife is too crude for a given purpose and creates defective products, we shall try to discover the cause of the failure, and then either use the knife more skillfully, or replace it for this special purpose by a more suitable tool, or even invent a new one. [Strawson's] thesis is like saying that by using a special tool we evade the problem of the correct use of the cruder tool. But would anyone criticize the bacteriologist for using a microtome, and assert that he is evading the problem of correctly using the pocketknife?

Patrick Maher's rejoinder on behalf of Strawson is apt. He says:

> Of course, nobody would criticize the bacteriologist, but that is because the bacteriologist's problem was not about the pocketknife. However, the relevant analogy for "one who seeks philosophical illumination of essential concepts of non-scientific discourse" is someone who seeks knowledge of proper use of the pocketknife; Carnap has offered nothing to satisfy such a person. Carnap seems to have thought that we don't need to take problems about ordinary language very seriously because, when such problems arise, we can develop a new more precise language that serves the same purposes and avoids the problems. But in many cases our purpose is to resolve a problem about a concept of ordinary language, and Carnap has not indicated how a new more precise language can serve that purpose.

Before getting into the details of Maher's response, I will digress for awhile and talk about modern deductive logic—from the point of view of Carnapian explication. Then, we'll return to confirmation theory.

3 Does Modern Deductive Logic Provide Carnapian Explications?

Consider the precise, formal (and *scientific* in Carnap's sense) modern (classical) concept of *logical consequence*. Actually, there are many concepts of logical consequence in the modern (classical-logical) literature. Within classical logic, there is sentential logical consequence, monadic predicate logical consequence, first-order consequence, second-order consequence, *etc.* For simplicity, let's focus on (classical) *sentential* logical consequence. There are "semantic" and "syntactic" characterizations of this concept. I'll focus on the "semantic" characterizations. We start with a sentential language $\mathcal{L}_S$, with some set of truth-functional connectives. Then, we introduce the notion of an interpretation of a sentence p of $\mathcal{L}_S$ as an assignment of truth-values to all the atomic constituents of p. And, we say that q is a logical consequence of a set $\{p_1, \ldots, p_n\} = \mathcal{P}$, which we abbreviate $\mathcal{P} \vDash_S q$, iff there is no interpretation on which all the $p_i \in \mathcal{P}$ get assigned $\top$ and q gets assigned $\bot$. Let's agree that this leads to a very precise, fruitful, and simple notion of sentential logical consequence. Indeed, this sounds like a good candidate for an explicatum. But, if $\mathcal{P} \vDash_S q$ is an explicatum, then what is the corresponding explicandum? Several possible candidates come to mind (here, I will assume for simplicity that $\mathcal{P}'$ is a set of propositions and q' is proposition — or perhaps English language sentences — that are in some relevant sense "faithfully captured" by $\mathcal{P}$ and q in $\mathcal{L}_S$):

(a) The ordinary language concept (if there be such) $\ulcorner\mathcal{P}'$ implies $q'\urcorner$ or $\ulcorner q'$ follows from $\mathcal{P}'\urcorner$.

(b) The ordinary language concept (if there be such) $\ulcorner$the argument from $\mathcal{P}'$ to q' is a good argument$\urcorner$ (do we need to add: in some sense that doesn't depend on whether the elements of $\mathcal{P}'$ are *actually true*?).

(c) The ordinary language concept $\ulcorner$it would be reasonable to infer q' from $\mathcal{P}'\urcorner$.

The explicatum $\mathcal{P} \vDash_S q$ is, of course, not perfectly similar to any of these explicanda. [Note: the first two explicanda might themselves be seen as logical, whereas the third sounds clearly epistemic.] Carnap requires that, in most cases, (a) q' follows from $\mathcal{P}'$, or (b) the argument from $\mathcal{P}'$ to q' is a good argument, or (c) the inference from $\mathcal{P}'$ to q' would be a reasonable inference to make $\Longrightarrow \mathcal{P} \vDash_S q$. In this example, Carnap's requisite "explicandum holds $\Rightarrow$ explicatum holds" direction sounds (to my ear) not so good — even "in most cases". For one thing, many (most?) good arguments (even in a *deductive* sense) are good even though their *sentential* forms are *invalid*. And, presumably, it can be reasonable to infer q' from $\mathcal{P}'$ in *many* (probably even *most*!) cases where the underlying argument is not deductively valid (in *any* sense). How about the other direction, which Carnap rejects (even when restricted to "most cases"): "explicatum holds ⇒ explicandum holds". Actually, this direction sounds better to my ear in the present case. Of course, there are still some dissimilarities, but "in most cases" this direction doesn't sound so crazy. Here is one dissimilarity of this kind (how many are there?). Let $\mathcal{P}$ be inconsistent. Then, $\mathcal{P} \vDash_S q$, for *any* q. But, many will say that some of the possible explicanda (a)–(c) will fail to obtain in some of these cases. Moreover, it will also sound quite odd to many to say that we should use this as a *corrective*—especially for (c). Presumably, there will be some cases in which an agent ϕ knows that q' is false, but they also know that their belief set $\mathcal{P}'$ is inconsistent. And, presumably, in this case, (c) will fail to be true, as applied to ϕ. I guess it is less clear as to whether (a) and (b) are satisfied in such cases. Perhaps we could argue that (a) and (b) are pre-theoretically imprecise and vague (like warmth was above), and we can now say something like "I used to think that some q's don't follow from some inconsistent $\mathcal{P}'$s, but now I know that anything follows from an inconsistent set of propositions/English sentences (thanks to my helpful explicatum $\mathcal{P} \vDash_S q$)." I guess we *could* say something like that, but it seems a little different than what's happening in the temperature/warmth case. Moreover, it's unclear whether Carnap thinks we *need* to be able to (plausibly) say something like this — even in cases like (c). In any case, such asymmetries between the two "directions of fit" between explicatum and explicandum are quite interesting, and it seems that they have not received enough attention in the literature. I'll return to this point, below, when I discuss the "old evidence" problem and the "grue" paradox.

4 Maher's Response to Strawson on Carnap's Behalf

Maher's response to Strawson is three-fold. He characterizes it as follows:

> Suppose our problem is to determine whether or not some sentence S of ordinary language is true. If we apply the method of explication to this problem, we will construct explicata for the concepts in S, formulate a corresponding sentence S' using these explicata, and determine whether or not S' is true. This does not by itself solve the original problem—that is Strawson's point—but it can greatly assist in solving the problem, in three ways. (i) The attempt to formulate S' often shows that the original sentence S was ambiguous or incomplete and needs to be stated more carefully. (ii) If the explicata appearing in S' are known to correspond well to their explicanda in other cases, that is a reason to think that they will correspond well in this case too, and hence to think that the truth value of S will be the same as that of S'. (iii) We can translate the proof or disproof of S' into a parallel argument about the corresponding explicanda and see if this seems to be sound; if so, we obtain a direct argument for or against S. In these ways, explication can provide insights and lines of argument that we may not discover if we reason only in terms of the vague explicanda.

Specifically, Maher uses an example that we've been talking about for several weeks now: Nicod's Condition. Maher takes the salient explicandum to be the following, which he calls an "ordinary language concept":

($\star$) q is made more probable by p (or, p raises the probability of q).

Here, Maher reads Nicod as using a natural language concept (or, at least, a vague, pre-scientific notion of "inductive probability-raising"), which he calls "confirmation". Before getting into Maher's examples and discussion, let me make a few preliminary remarks. First, it is not clear to me that there *is* an ordinary language concept ($\star$) of the sort Maher claims to be talking about here. It helps to fill-in some background here. In his paper "The Concept of Inductive Probability" (now also on the website), Maher claims that there is a logical, pre-theoretic, ordinary language concept of (conditional) inductive probability: "the probability of q, given p". He argues for this claim (mainly) by appealing to the following "existence criterion":

(EC) Inductive probabilities exist iff there are true sentences in ordinary language which assert that a probability has a certain (precise or imprecise) value, where 'probability' is used in the inductive sense.

Then, he claims that "practically all competent speakers of ordinary language will assent to":

(WB) The probability that a ball is white, given that it is either white or black, is 1/2.

Maher (in his paper "Probability Captures the Logic of Scientific Confirmation", also on the website) offers a formal, Carnapian theory of conditional probability (more on those next week) as an explicatum for this ordinary language explicandum "inductive probability". Let's call Maher's Carnapian explicatum for inductive probability $\Pr_{\top}(q \mid p)$. And, let's use $\mathfrak{P}(q \mid p)$ for the explicandum "the inductive probability that q, given that p" (if there be such!). In his response to Strawson, Maher does *not* use $\Pr_{\top}(q \mid p)$ as his example of a "helpful explicatum" [for $\mathfrak{P}(q \mid p)$]. Rather, he wants to talk about a helpful explicatum for the explicandum ($\star$), which he calls "confirmation". It is not quite clear what he means by this. First, as he points out:

> If we attempt to explicate the concept of confirmation we soon realize that whether or not evidence E confirms hypothesis H depends not only on E and H but also on the background evidence...

Thus, it seems that it isn't really ($\star$) he's trying to explicate, but the following more complex explicandum:

($\star_K$) q is made more probable by p, relative to background evidence K (or, p raises the probability of q, relative to background evidence K).

Maher will view this, already, as an illustration of the "usefulness" of explication. This, he will claim, is an example of prong (i) — a case in which the process of explication reveals that "making more probable" is actually a *three*-place relation, and not a two-place relation as the naive, pre-scientific ($\star$) would suggest.

This is also, already, a bit worrisome, since Maher wants the explicandum to be a *logical* (not epistemic) concept. But, he's already talking about "evidence" here, which seems to indicate that the relation is epistemic (*i.e.*, inherently involving an agent ϕ who *possesses* some body of background *evidence*). This is unfortunate. I will take the liberty of using the term "corpus" instead of "evidence" here. That leads to the following further clarification of the explicandum, which (I assume) is a relation between *propositions*:

($\star_K$) q is made more probable by p, relative to background corpus K (or, p raises the probability of q, relative to background corpus K). [We can abbreviate this as: $\mathfrak{P}(q \mid p \,\&\, K) > \mathfrak{P}(q \mid K)$.]

Now, Maher claims that his Carnapian explicatum $\Pr_{\top}(q \mid p)$ for $\mathfrak{P}(q \mid p)$ can be used to construct an adequate explicatum for ($\star_K$). Presumably, this explicatum for what Maher calls "confirmation" will be as follows:

($\mathfrak{C}$) $\Pr_{\top}(q \mid p \,\&\, K) > \Pr_{\top}(q \mid K)$ [which we can abbreviate as follows: $\mathfrak{C}(q, p \mid K)$]

Now, we're ready for Maher's example of a "helpful application" of an explicatum. His example involves the following "natural language sentence", which he attributes to Nicod (and Hempel as well, I suppose):

(NC) A law of the form "All F are G" is confirmed by evidence that something is both F and G, relative to no background evidence.

Again, Maher's use of the term "evidence" in the formulation of (NC) is misleading, and confuses the issues at hand. So, I will switch to a more "non-epistemic" way of stating the "ordinary language claim" at issue:

(NC) All universal generalizations ⌜All F are G⌝ are confirmed by the (any) proposition that some object is both F and G [⌜$Fa \,\&\, Ga$⌝] — relative to "empty background corpus" $K_{\top}$.

Now, Maher's three-pronged response to Strawson moves to prong (ii). He claims that his explicatum ($\mathfrak{C}$) is "known to correspond well to its explicandum ($\star_K$) in other cases". It's not quite clear what he means by this. Again, in his paper "Probability Captures the Logic of Scientific Confirmation", Maher does argue that ($\mathfrak{C}$) corresponds well to its explicandum ($\star_K$) in various cases. We'll talk more about that next week. For now, let's just grant him this premise here. Prong (iii) of his response to Strawson involves "translating the proof or disproof of S' into a parallel argument about the corresponding explicanda". Here, S' is:

(NC′) For all F, G, and x: $\mathfrak{C}[(\forall y)(Fy \supset Gy), Fx \,\&\, Gx \mid \top)$.

Maher *proves* (in "Probability Captures the Logic of Scientific Confirmation") that (NC′) is *false* for his explicatum ($\mathfrak{C}$). He then reasons (by analogy, I guess?) that, since ($\mathfrak{C}$) is similar to ($\star_K$) in *other* cases, it is reasonable to infer that ($\mathfrak{C}$) is also similar to ($\star_K$) in *this* case. Thus, it is reasonable to infer that (NC) is also false. That's not quite the end of the story. We still need to "translate the proof that (NC′) is false back into the explicandum language", and see if it makes sense there as well. Maher does not include this "translation" in his paper on explication. One needs to look back at the earlier paper (once more) to find this. Unfortunately, when one does this, one finds the following passage (we have seen this one before):

> According to standard logic, 'All unicorns are white' is true if there are no unicorns. Given what we know, it is almost certain that there are no unicorns and hence 'All unicorns are white' is almost certainly true. But now imagine that we discover a white unicorn; this astounding discovery would make it no longer so incredible that a non-white unicorn exists and hence would disconfirm 'All unicorns are white.'

This is certainly a nice example, cast in "ordinary language". But, note how it says "given what we know". That's worrisome, since it seems to appeal to *non-empty* (indeed, *empirical*) background corpus/knowledge to establish a claim which is supposed to be made "relative to empty background corpus $K_\top$". When I asked Maher about this apparent problem with the "translation back into explicandum terms", he said:

> That's a good question. But I did not, and would not, call the unicorn example a "translation" of Nicod's condition with no background evidence, both for the reason you mention, and also because it is a specific example. Yes, I did say, in 'Explication Defended', that I "showed that the proof that (NC′) is false makes intuitive sense when translated back into qualitative explicandum terms", but that does not mean the unicorn example is a translation. The "showing" that I refer to here depends partly on the unicorn example, but also on the paragraph that follows it. The unicorn example is there because it is intuitive and the (qualitative) structure of the probabilities in it is the same as in the situation with no prior evidence but a low *a priori* probability for something to be an F. Once we see how the probabilities work in the unicorn example, we can see that in general, when the prior probability of something being an F is low, Nicod's condition can be violated.

This is an interesting response. But, the paragraph following the unicorn example is simply an Englishy re-description of the *Carnapian* $\Pr_\top$-countermodel to (NC′). So, I don't see how that can add much to the plausibility of the falsity of (NC) in explicandum terms. Moreover, there is a deeper problem lurking here. Recall that Maher's main reasons for believing in the existence of "inductive probabilities" involve examples in which the probability in question is conditional upon some *non-empty, empirical* background corpus. However, (NC′) explicitly involves Carnapian "inductive probabilities" that are conditional upon *tautological* background. Thus, the sorts of inductive probabilities that Maher must establish the existence of in *this* context are of the form $\mathfrak{P}(q \mid K_\top)$. But, he has given us no reason to believe that inductive probabilities *of this kind* exist — much less that $\Pr_\top(q \mid \top)$ *is a good explicatum for* $\mathfrak{P}(q \mid K_\top)$. As a result, Maher has given us no reason to believe that "confirmation relations relative to empty background corpus ($\star_{K_\top}$)" exist either. This is a non-trivial objection to Maher's approach. To see the problem, ask yourself the following question:

- What is "the probability that a ball is white, given that it is either white or non-white"?

Is it also 1/2? If we begin to multiply such questions, things quickly start to look problematic. In the end, it is not at all clear (to me) that there *is* an ordinary language concept "the probability of p, *relative to empty background corpus*". And, I don't see how any of Maher's arguments help to establish the existence of such "a priori" inductive probabilities. Moreover, *even if there were* such an ordinary language concept, Maher gives no reason to think his explicatum $\Pr_\top(p \mid \top)$ will be similar to *this* explicandum in *any* cases. So, his "analogical argument" is doing *very* heavy lifting. [We'll talk more about "logical probability" next week.]

One last note, as a prelude to next week's discussion of Carnap's various remarks about explicating "confirmation". When Carnap clarifies the various explicanda that he thinks are salient to the explicata in his treatise, he mentions the following (which I think *is* a *bona fide* ordinary language concept):

(†) E provides some positive evidence for H (I would add: for an agent ϕ in a context C).

As we'll see next week, Carnap was a bit confused about firmness *vs* increase in firmness explications of (†). But, assuming an increase in firmness explication of (†), we might go for something like the following explication (which many contemporary Bayesians seem to have adopted, at least implicitly):

(€) $\Pr(H \mid E \,\&\, K) > \Pr(H \mid K)$, where K is ϕ's total evidence in C, and Pr is some probability function.

Of course, there will be the question of *which* probability function Pr is "appropriate" for (€). We'll talk more about that later. For now, I want to briefly discuss another problem that people have seen with (€). If $K \vDash E$, then (translating back into explicandum terms) according to (€), E cannot evidentially support any H for ϕ in C. This is sometimes called "the problem of old evidence". We'll talk more about this later in the seminar. But, I want to end today's notes by asking whether Carnap would have seen this as a problem. For Carnap, this would be, at least, a *relevant* case of dissimilarity: explicandum holds $\not\Rightarrow$ explicatum holds. But, since he requires only that this hold "in most cases", one wonders how bothered he would have been by *this* case. Moreover, perhaps he would have gone for the "corrective strategy", and said something like "I used to believe that things I already knew (with certainty) provided me with evidence for other things, but now I see this was mistaken (thanks to my helpful explicatum €)." Again, that sounds implausible to me, and so this also seems to differ from the temperature/warmth case. When we get to "grue", we'll see that *it* involves (at most) a counter-example to the *other* direction "explicatum holds $\Rightarrow$ explicandum holds", which Carnap *rejects*. So, why should he have seen "grue" as a problem for (€) *at all* — *qua* explicatum for (†)?