Notes for Week 4 of *Confirmation*

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1 Some Background Issues in the Keynes Readings

1.1 Keynesian Conditional Probabilities

There is an ambiguity in Keynes's discussions involving probability. Sometimes he talks about agents knowing (or believing) that some objective probability statement (sometimes having a logical flavor, but usually having an epistemic flavor) is true, and sometimes he talks about a rational agent's degrees of belief (or *credences*) having certain properties. These, of course, are different. An agent ϕ can know (or reasonably believe) that a claim $\Pr(p \mid q) = r$ is true, where Pr is some kind of objective probability, and at the same time have reasonable degrees of belief which are such that $\Pr_{\phi}(p \mid q) \neq r$. Indeed, for all Keynes tells us, it can even be the case that an agent ϕ falsely (but reasonably) believes that $\Pr_{\phi}(p \mid q) = r$. Some have defended "bridge principles" or "direct inference" principles which connect certain sorts of objective probabilities with rational degrees of belief, in certain contexts. But, this is not an issue that Keynes takes up in any detail. That's why his discussion is often ambiguous in this way. I will assume that he is trying to characterize (or give constraints on) $\Pr_{\phi}(p \mid q)$ itself, and not giving condtions under which it would be reasonable to believe that some class of objective probability statements are true.[1] Later, when we discuss Carnap, we will take the opposite stance, since it is clear that Carnap is talking about logical probabilities, and he realizes that substantive epistemological principles are need to connect them to credences.

Keynes makes it clear that he doesn't think $\Pr_{\phi}(p \mid q)$ always has a precise numerical value (or, at least, that we may not always be able to *determine* such values, either in the first or third person). But, since he is most concerned (in the chapters we're reading) with relations of *probabilistic relevance*, it will be *comparative* claims such as $\Pr_{\phi}(p \mid q \,\&\, K_{\top}) > \Pr_{\phi}(p \mid K_{\top})$ that are most important for us.[2] Keynes thinks that some such comparative claims can be true (or, at least, plausible), even if we cannot determine precise numerical values for $\Pr_{\phi}(p \mid K_{\top})$ or $\Pr_{\phi}(p \mid q \& K_{\top})$. I am using $K_{\top}$ to denote an *"a priori"* background corpus — it is supposed to express/summarize certain "background propositions" that can be known *a priori* by ϕ, or, at least, those which are known *prior* to observing some "foreground" evidence that's being considered. As such, $\Pr_{\phi}(p \mid K_{\top})$ can be thought of as an[3] epistemically rational *"a priori"* probability of p. Relative to such *"a priori"* background(s), some propositions will be favorable to (*i.e.*, positively correlated with) p and some will be unfavorable to (*i.e.*, negatively correlated with) p. This is an early rendition of an (epistemic) *"a priori" probabilistic relevance conception of confirmation/disconfirmation*.[4] One of the key questions we will be discussing this week is how Keynsian probabilistic relevance confirmation relations are supposed to look for *universal* p's, and *instantial* q's. And that will set the stage for much of the subsequent history.

1.2 Keynes's Historical Discussions of Induction and Causation

Keynes (chapter 23) gives a brief summary of the history of induction (in the modern sense). [His discussion is rather similar to Milton's in various ways, including its concentration on universal induction, which is almost exclusively what Keynes has in mind in the readings for this week.] His main complaint about Mill and Bacon is that they fail to "apprehend the relativity of all inductive arguments to the evidence, [and]

[1] One reason for this is Keynes's insistence that the probabilistic methods he develops can be applied to *any* domain of inquiry — even mathematics and logic. It is difficult to imagine how non-trivial *logical* probabilities could be countenanced in cases such as: Pr(Goldbach's Conjecture | GC is true for the first 1M cases & the Peano Axioms for arithmetic are true). I agree with Keynes that we *should* be able to say non-trivial things about *epistemic* probabilities in such cases. That is largely beyond the scope of the seminar.

[2] It is interesting to note that Keynes borrows much of the mathematical theory of probabilistic relevance from his teacher W.E. Johnson (*e.g.*, it seems that he just *copied* most of the salient theorems on probabilistic relevance from Johnson's notes). I'll return to this point, below, in my discussion of an odd remark Keynes makes about his response to the Humean "circularity" charge.

[3] I say "an" rather than "the" here, since $K_{\top}$ may not consist of *all* of ϕ's "*a priori*" background evidence. At least, Keynes doesn't say anything that requires $K_{\top}$ to contain *all* of ϕ's "*a priori*" knowledge. He often focuses just on those "parts" of the background that are relevant to the probabilities at issue in a given context. He has a rather naïve conception of how "parts" of $K_{\top}$ are combined.

[4] As we'll see later in the course, Carnap discusses a similar notion of confirmation, which he calls "initial confirmation". But, Carnap's relation is a *logical* one, which only bears on *epistemology* when we *apply* inductive logic to an agent ϕ in an epistemic context. Moreover, Carnap's use of the term "a priori" in this connection is more standard and consistent than Keynes's.

the element of uncertainty which is present, more or less, in all the generalisations which they support." Keynes's approach to confirmation makes claims of inductive support relative both to background and foreground evidence, and it doesn't even require that the generalizations in question have *high* probability (much less probability 1) given our total evidence — since *relevance* of the foreground evidence (relative to the background evidence) is itself crucial for Keynes. Keynes also complains that Mill and Bacon (and others) were not explicit (and/or careful) enough about what presuppositions are required in order for their inductive methods to work. Keynes thinks that certain inductive presuppositions are required for his own (any?) methods to work. In *this* sense, he agrees with the "popular" reading of Hume. On Hume, he remarks:

> Hume's sceptical criticisms are usually associated with causality; but argument by induction — inference from past particulars to future generalisations [note the emphasis on *universal* induction here] — was the real object of his attack. Hume showed, not that inductive methods were false, but that their validity had never been established and that all possible lines of proof seemed equally unpromising. The full force of Hume's attack and the nature of the difficulties which it brought to light were never appreciated by Mill, and he makes no adequate attempt to deal with them. Hume's statement of the case against induction has never been improved upon; and the successive attempts of philosophers, led by Kant, to discover a transcendental solution have prevented them from meeting the hostile arguments on their own ground and from finding a solution along lines which might, conceivably, have satisfied Hume himself.

Thus, Keynes seems to think his own approach is something that might have satisfied Hume himself. And, he also is reading Hume as making claims about universal induction, as opposed to either causation or singular predictive induction (although, Keynes worries that predictive induction may be easier to ground, in his discussion of his own methods of Analogy and Pure Induction — more on this issue below).

Keynes's discussion of causation (275–277) is interesting. [I'll only discuss it briefly here, since Keynes does not think it is the crux of the issue in the Humean tradition of induction.] He distinguishes several senses of the term "cause". The most interesting of these (from a modern perspective) is what he calls *causa cognoscendi* (277). Basically, what Keynes is describing here is an early version of *probabilistic causality*.[5]

2 Keynes on the Humean Circularity Charge

2.1 Keynes on "The Principle of the Uniformity of Nature"

> The Principle of the Uniformity of Nature, as I interpret it, supplies the answer, if it is correct, to the criticism that the instances, on which generalisations are based, are all alike in being past, and that any generalisation, which is applicable to the future, must be based, for this reason, upon imperfect analogy. We judge directly that the resemblance between instances, which consists in their being past, is in itself irrelevant, and does not supply a valid ground for impugning a generalisation.

Here, Keynes identifies "uniformity" with the assumption that (in applying his methods of Analogy and Pure Induction — to be discussed below) we can safely ignore *temporal* and *spatial* (dis)analogies among instances of universal generalizations. He thinks this is required, since otherwise it would introduce a disanalogy between past instances (*i.e.*, all instances we have observed in the past) and future instances to which the universal generalization applies (which, he thinks would spuriously undermine the *"a priori"* probability of the generalization — see below for more on his approach to Analogy and universal induction). I won't dwell on this here. But, it is worth thinking about what Keynes might have said about "grue" in this connection. Surely, we cannot ignore the fact that all "grue" instances in our sample have been observed prior to t. That is part of the antecedent property of the universal generalization itself! Perhaps Keynes would have followed Hempel and Carnap in banishing such predicates from the realm of inductive methods. Indeed, Keynes seems to lean in the direction of banishing "positional" predicates on page 256, when he says:

> When I say that position is irrelevant, I do not mean to deny that a generalisation, the premiss of which specifies position, may be true, and that the same generalisation without this limitation might be false. But this is because the generalisation is incompletely stated; it happens that objects so specified have the required characters, and hence their position supplies a sufficient criterion. Position may be relevant as a sufficient condition but never as a *necessary* condition, and the inclusion of it can ouly affect the truth of a generalisation when we have left out some other essential condition. A generalisation which is true of one instance must be true of another which *only* differs from the former [in] its position in time or space.

[5] Sewall Wright, working at around the same time, was a pioneer of this field, which has gone through many incarnations since. There has been a ton of work related to probabilistic causality since 1915. See Eells's *Probabilistic Causality* for a recent survey.

2.2 Keynes on "Limited Variety"/"Absolute Finiteness"

Aside from "uniformity", Keynes also identifies another claim that he thinks is a pre-condition for the operation of his (or any?) inductive methods. He calls it "Limited Variety"/"Absolute Finiteness" (258):

> As a logical foundation for Analogy ... we seem to need some such assumption as that the amount of variety in the universe is limited in such a way that there is no one object so complex that its qualities fall into an infinite number of independent groups (*i.e.*, groups which might exist independently as well as in conjunction); or rather that none of the objects about which we generalise are as complex as this; or at least that, though some objects may be infinitely complex, we sometimes have a finite probability that an object about which we seek to generalise is not infinitely complex.

The background metaphysical picture here seems to be as follows. First, Keynes imagines that there are a finite number of "generator properties" $\mathbb{P}_1, \ldots, \mathbb{P}_n$. All "apparent properties" (F, G, etc.) arise out of this finite basis. For instance, F might arise out of $\mathbb{P}_1$ & $\mathbb{P}_3$ & $\mathbb{P}_9$, *etc.* Moreover, Keynes thinks we presuppose that the qualities of an object are "bound together in a limited number of *groups*, a sub-class of each group being an infallible symptom of the coexistence of certain other members of it also." (252) Another way of characterizing "Limited Variety" is in terms of these "groups" of qualities. As Keynes explains (253):

> ... if we find two sets of qualities in coexistence there is a finite probability that they belong to the same group, and a finite probability also that the first set specifies this group uniquely. Starting from this assumption, the object of the methods is to increase the finite probability and make it large.

In this connection, Keynes thinks that for *universal* induction we must *also* assume:

> ... a finite probability that the set of characters, which condition the generalisation, are *not* the possible effect of more than one distinct set of fundamental [*i.e.*, "generator"] properties.

But, because Keynes sees no way to justify such an assumption, he wonders whether "our conclusions should be in the form of inductive correlations [what we're calling *predictive* inductions], rather than of universal generalisations". We won't have time this semester to discuss what Keynes says about predictive induction. But, it is worth noting that he was worried that predictive induction might be the only "firmly grounded" sort of induction, because of this "problem of the plurality of sets of generator properties", which Keynes seems to think *only* plagues *universal* induction (*why* does he think this is peculiar to universal induction?).

Three things about this part of Keynes's discussion are worth noting.

- First, he assumes that the presence of certain sub-classes of groups of qualities are *infallible* indicators of the coexistence of certain other members of that group of qualities. Why *infallible*? Why not just require some positive degree of relevance of the presence of certain sub-groups for certain other members of that group (in the "finite probability"/"positive relevance" over "certainty" style of the rest of the chapter)? Is this a concession to traditional inductive skeptics that *somewhere* "down deep" an assumption of *infallibility* has to be made in to get our story about induction off the ground. This *sounds* like a retreat to a more traditional *infallible-foundationalist* epistemology. But, is it?

- Second, "finite probability" is here being contrasted with *certainty* (I will return to this in the next sub-section, below). It *sounds* like a weaker requirement than requiring certainty (*modulo* my worry above that this seems to be a "finite probability" of the existence of a certain *infallible* sign). But, this is misleading. If we had certainty here, rather than finite probability, it would actually *undermine* Keynes's argument. This is because *certainties cannot be correlated with anything* (more on this next).

- Third, I'm dumbfounded as to what all of this metaphysics is supposed to imply about *epistemic* probabilities. Go ahead and assume anything you want about the "absolute finiteness" of an underlying "realm of generator properties". How does any of this do anything to secure Keynes's claims about the *epistemic rationality* of certain probabilistic relevance relations (or certain *"a priori"* ranges of probability values), concerning the "apparent properties" involved in our inductions? I don't get it.

2.3 Keynes's Response to the Charge of "Circularity"

Keynes explicitly addresses the Humean "circularity" charge on pages 259–260. He begins §11 here by saying something quite puzzling: that — up until then — he had been supposing

> ... it is necessary to make our assumptions as to the limitation of independent variety in an absolute form, to assume, that is to say, the finiteness of the system, to which the argument is applied, *for certain*. But we need not in fact go so far as this.

This clearly indicates that Keynes thinks it would be *better* for his arguments if we assumed that his "absolute finiteness" assumption (which I'll call $\mathcal{F}$) were known *with certainty* by our would-be inductive inferrer ϕ. That is, he seems to be saying that assuming $\Pr_{\phi}(\mathcal{F} \mid K_{\top}) = 1$ would be better for his argument than assuming "merely" that $\Pr_{\phi}(\mathcal{F} \mid K_{\top}) > 0$. This couldn't be farther from the truth. For, his response to (his interpretation of) the Humean "circularity" charge *requires* that $\Pr_{\phi}(\mathcal{F} \mid K_{\top}) \in (0, 1)$. In general, he says:

> If our conclusion is C and our empirical evidence is E, then, in order to justify inductive methods, our [*"a priori"*?] premisses must include, in addition to E, a general hypothesis H such that $\Pr_{\phi}(C \mid H)$, the *"a priori"* probability of our conclusion [*viz.*, E's probability relative to *"a priori"* background corpus H], has a finite value. The effect of E is to increase the probability of C above its initial *"a priori"* value, $\Pr_{\phi}(C \mid H \& E)$ being greater than $\Pr_{\phi}(C \mid H)$. But the method of strengthening C [relative to background corpus H] by the addition of evidence E is valid quite apart from the particular content of H. If, therefore, we have another general hypothesis H' and other evidence E', such that $\Pr_{\phi}(H \mid H') > 0$, we can, without being guilty of a circular argument, use evidence E' by the same method as before to strengthen the probability H [relative to background corpus H']. If we call H, namely, the absolute assertion of the finiteness of the system under consideration [the assumption I've been calling "$\mathcal{F}$"], the *inductive hypothesis*, and the process of strengthening C [relative to background corpus H] by the addition E the *inductive method*, it is not circular to use the inductive method to strengthen the inductive hypothesis itself, relative to some more primitive and less far-reaching assumption.

This is something like what we were discussing last week, as a possible response to the "circularity" charge. But, in order for this to work, we *cannot* have $\Pr_{\phi}(H \mid H') = 1$, since then *nothing* could "strengthen the probability H [relative to background corpus H']". Thus, *we must* ***not*** *assume that H is certain*, if Keynes's own response to the "circularity" charge is going to work. This makes Keynes's discussion rather misleading.

Having clarified that important presupposition of Keynes's response, let's think in more detail about how this response is supposed to work. To make the example concrete, what if we want to know how learning that $E \overset{\text{def}}{=} Fa \,\&\, Ga$ (where a is previously unobserved, say) bears on the probability of $C \overset{\text{def}}{=} (\forall x)(Fx \supset Gx)$. Keynes seems to be making the following assumptions about the *"a priori"* background conditions H/H'.

(i) Keynes wants this to be a case in which $\Pr_{\phi}(C \mid E \,\&\, H) > \Pr_{\phi}(C \mid H)$, where H is a proposition summarizing all the requisite *"a priori"* background assumptions required (by Keynes's lights) for Keynes's methods to work in this sort of case. H will, it seems to me, include not only the "absolute finiteness" assumption ($\mathcal{F}$), but also the "uniformity principle", and also all previous analogcial evidence, which Keynes thinks is relevant in a case like this. Let's just grant this step, for now (since this isn't what being defended *here*). We'll have more to say about this below in the discussion of Analogy.

(ii) Keynes also wants this to be a case in which some *other* evidence E' confirms H itself, relative to some other (presumably, also *"a priori"*?) "more primitive and less far-reaching" background assumption H'. That is, Keynes also wants this to be a case in which $\Pr_{\phi}(H \mid E' \,\&\, H') > \Pr_{\phi}(H \mid H')$.

(iii) Keynes realizes that he must therefore be assuming that $\Pr_{\phi}(H \mid H') > 0$ in this case, since, otherwise $\Pr_{\phi}(H \mid E' \,\&\, H') = 0$, and we won't have (ii).

(iv) Keynes does not seem sensitive here to the fact that he also needs to assume $\Pr_{\phi}(H \mid H') < 1$, since, otherwise $\Pr_{\phi}(H \mid H') = 1$, and we won't have (ii).

(v) Moreover, the question arises as to what E' and H' might be (this is analogous to Mike's worry about the E' in my reconstruction of Stroud's Humean argument from last week). Presumably, E' would have to itself be instantial evidence that is relevant (in Keynes's framework) to H, given some other "more primitive and less far-reaching assumption" H'. But, what would such E's and H's be like, exactly?

(vi) Finally, in the footnote on this page, Keynes also suggests that he is presupposing "that if H' supports H, it strengthens an argument which H would strengthen". I don't see why any such assumption is required here. This is puzzling, especially since the theorems of Johnson that he refers back to in the footnote (which he admits he just copied from Johnson's notes, and which he doesn't seem to fully grasp) do not seem to bear on this case. This suggests that Keynes was not completely on top of the mathematical theory of probabilistic relevance that he's leaning so heavily upon in his arguments.

3 Keynes on Analogy and Pure Induction

Keynes's discussion of Analogy and Pure Induction is quite interesting, and it anticipates much of the literature we're going to read over the next several weeks. It's amazing that this part of his book isn't cited more in connection with the historical development of confirmation theory. The basic picture Keynes seems to have here is that (provided that the background assumptions $K_\top$ about "uniformity", "absolute finiteness", and "plurality of sets of generator properties" are in place), universal induction has two components: Analogy and Pure Induction. The role of Analogy is to help set the "a priori" probability of a universal generalization, and then the role of Pure Induction is to raise this "a priori" probability *via* numerous subsequent instantial observations. Thus, the two work in concert, according to Keynes. Naturally, he discusses Analogy first.

3.1 Keynes on Analogy

Before digging into Keynes's fascinating chapter on Analogy, I should mention that his usage of the term "Analogy" is somewhat non-standard. Typically, "analogy" is used to describe a kind of *singular predictive induction* (*e.g.*, the inference that I have a mind, based on some analogy between John – who clearly has a mind – and myself), rather than a kind of *universal induction.* Later, when we read Carnap, we will see that he makes use of "analogy", but only in the context of singular predictive induction, not universal induction.

Keynes has a very subtle and complex discussion of Analogy. I will simplify things (greatly!) by restricting my comments to (in his terminology) the case of *perfect* analogy, where our knowledge of *all* the salient analogies is *complete.* Indeed, I will go even farther than that, and I will consider a (highly idealized!) case in which the *only* analogical evidence ϕ has is a *single* proposition $\mathcal{E}$ of the form $\pm Ea \mathbin{\&} \pm Ga$, which says of an object a (say, the only object ϕ has seen in the past to have any "apparent properties" whatsoever) is (or is not) an emerald and is (or is not) green. And, we're interested in how this sort of "analogical evidence" will bear on the "a priori" probability of the universal claim $\mathcal{H} : (\forall x)(Ex \supset Gx)$. That is, we're trying to get some grip on $\Pr_\phi(\mathcal{H} \mid \mathcal{E} \mathbin{\&} K_\top)$, where $\mathcal{E}$ can take four possible values: $Ea \mathbin{\&} Ga$, $Ea \mathbin{\&} {\sim}Ga$, ${\sim}Ea \mathbin{\&} Ga$, and ${\sim}Ea \mathbin{\&} {\sim}Ga$, and (presumably) $K_\top$ includes all the necessary pre-conditions for induction, discussed above.[6] Keynes thinks some of these instances of $\mathcal{E}$ are "a priori" favorable to $\mathcal{H}$, some are "a priori" unfavorable to $\mathcal{H}$, and some are irrelevant. Moreover, for still others, Keynes is unsure whether they favorable or not.

Let's start with the "easiest" instance of $\mathcal{E}$, from Keynes's perspective: $Ea \mathbin{\&} {\sim}Ga$. Keynes seems to think that $\Pr_\phi(\mathcal{H} \mid Ea \mathbin{\&} {\sim}Ga \mathbin{\&} K_\top) = 0$. This doesn't seem crazy, since $Ea \mathbin{\&} {\sim}Ga$ *entails* ${\sim}\mathcal{H}$. However, this is not as benign an assumption as it might appear. Here, Keynes is presupposing a kind of *logical omniscience* that goes beyond the axiomatic basis of the probability calculus. In probability calculus, *Boolean* relations between statements in a sentential language are guaranteed to be enforced by the axioms. But, the axioms do not (strictly speaking) entail that $\Pr_\phi(\mathcal{H} \mid Ea \mathbin{\&} {\sim}Ga \mathbin{\&} K_\top) = 0$. This is because the predicate-logical entailment relation is distinct from the Boolean, *propositional* entailment relation presupposed in the axioms.[7] Of course, Keynes is free to *assume* $\Pr_\phi(\mathcal{H} \mid Ea \mathbin{\&} {\sim}Ga \mathbin{\&} K_\top) = 0$, if he likes, but I just want to point out that this does presuppose a non-trivial bit of logical *knowledge* on the part of the agent ϕ (this is especially pressing for Keynes, since he thinks we can apply these methods even to *arithmetic*, *etc.*). Putting this issue to one side (we may briefly return to it later in the course), I'll move on to other instances of $\mathcal{E}$.

The second "easiest" instance (in Keynes's thinking) seems to be the "positive instance" $Ea \mathbin{\&} Ga$. Keynes assumes that $\Pr_\phi(\mathcal{H} \mid Ea \mathbin{\&} Ga \mathbin{\&} K_\top) > \Pr_\phi(\mathcal{H} \mid K_\top)$, *i.e.*, that $Ea \mathbin{\&} Ga$ is "a priori" favorable to $\mathcal{H}$. This is a very common assumption in the literature, and it forms the basis for much of the subsequent literature on universal induction/confirmation. It became known as "Nicod's Criterion", after Hempel wrote his paper about Nicod's approach to universal induction. As we'll see next week, Nicod just takes this from Keynes directly. As such, it would be more accurate to call this assumption the "Keynes Criterion". We will spend a

[6]Actually, it is unclear whether or not $K_\top$ is supposed to include all these presuppositions. Keynes says that agents may not be aware that such "inductive hypotheses" are being presupposed whenever inductive methods are used. But, he says that:

> Whether or not anything of this sort is explicitly present to our minds when we reason scientifically, it seems clear to me that we do act exactly as we should act, if this were the assumption from which we set out.

Therefore, I guess if we include these sorts of background assumptions in the $K_\top$'s of our statements about "a priori" epistemically rational degrees of belief, *i.e.*, in all terms of the form $\Pr_\phi(\cdot \mid \cdot \mathbin{\&} K_\top)$, we're assuming Keynes is an *access-externalist* about them?

[7]Unfortunately, Keynes's own rendition of the "axioms" of probability calculus contain a bizarre admixture of epistemic and logical language. Some of the axioms talk of "certainty", and some talk of "impossibility". I will ignore this, and assume a more modern formalization of probability calculus for the remainder of my discussion. I actually think this is being *charitable* to Keynes.

lot of time talking about this later. I won't dwell on it now. But, I will note that Keynes doesn't seem to think that much in the way of an argument is needed for this. I think this may be because he is a bit confused about the logical relationships between $\mathcal{H}$ and $Ea \,\&\, Ga$. We'll return to this point in the next section.

The third "easiest" instance (in Keynes's thinking) seems to be $\sim Ea \,\&\, \sim Ga$. Here, again, Keynes assumes that $\sim Ea \,\&\, \sim Ga$ is "a priori" favorable to $\mathcal{H}$. As such, he simply embraces what later became known as "The Paradox of Confirmation". Clearly, he doesn't think there is anything paradoxical about this assumption. Historically, Hempel is usually credited as the first confirmation-theorist to endorse a theory according to which $\sim Ea \,\&\, \sim Ga$ confirms $(\forall x)(Ex \supset Gx)$. But, this is (again) historically inaccurate. Clearly, Keynes was "way ahead" of Hempel in this regard. Again, we'll spend a lot of time talking about this later in the course.

The "hardest" instance (in Keynes's thinking) was $\sim Ea \,\&\, Ga$. He says in a footnote (230) that he is "disposed to think that we need not pay attention to" such instances. But, he adds, "the question is a little perplexing." So, his official view seems to be that $\sim Ea \,\&\, Ga$ is "a priori" neither favorable nor unfavorable to $\mathcal{H}$ — that it is *irrelevant* to the "a priori" probability of $\mathcal{H}$. But, he seems tentative on this score. As we'll see later, Hempel's theory of confirmation implies that $\sim Ea \,\&\, Ga$ *confirms* $\mathcal{H}$. We'll address this issue (and the other issues above) in some detail when we get to our discussion of Hempel (and Carnap).

3.2 Keynes on Pure Induction

We'll spend a lot of time talking about what Keynes says about Pure Induction next week, since that's one of the main complaints Nicod has about Keynes. For today, I'll just say something brief to set the stage for subsequent discussion. Basically, Keynes's goal is to explain how learning *many* "positive instances" $\mathcal{E}_i$ can raise the "a priori" probability of $\mathcal{H}$. Indeed, he aims to show that the "a posteriori" probability of $\mathcal{H}$ can, in this way, be driven all the way to 1 — in the limit as the number of learned "positive instances" grows without bound. In other words, he aims to identify conditions on $K_\top$ under which the following obtains:

$$\lim_{n\to\infty} \Pr_\phi(\mathcal{H} \mid \&_{i=1}^{n} \mathcal{E}_i \,\&\, K_\top) = 1$$

To make a long story short, Keynes, assumes (basically) the following three things:

(a) For each i, $\Pr_\phi(\mathcal{E}_i \mid \mathcal{H} \,\&\, K_\top) = 1$.

(b) $\Pr_\phi(\mathcal{H} \mid K_\top) \in (0, 1)$.

(c) $\lim_{n\to\infty} \Pr_\phi(\&_{i=1}^{n} \mathcal{E}_i \mid \sim\mathcal{H} \,\&\, K_\top) = 0$.

Keynes says that "under suitable conditions" (a)–(c) are true and that they imply that the limit claim above is true. Moreover, he seems to think that (b) and (c) are implied by his "absolute finiteness" assumption $\mathcal{F}$. There is a fair amount of controversy about the details of what Keynes is up to here. Much of the controversy is about the status and origin of (b) and (c), and about whether (a)–(c) really do secure the desired limit claim. On this score, for next week, I recommend that (in addition to the Nicod) you read the salient part of Braithwaite's review of Nicod (pp. 488–489), and the part of the Hosiasson–Lindenbaum paper that is about the Keynes-Nicod debate (page 145). I will bracket discussion of these issues until next week.

In closing, I want to make one remark on (a)–(c). If Keynes is assuming (a)–(c), then it should be no surprise that he thinks "positive instances" are favorable to universal generalizations. For, as Keynes was surely aware, (a)–(c) *jointly entail* $\Pr_\phi(\mathcal{H} \mid \mathcal{E}_i \,\&\, K_\top) > \Pr_\phi(\mathcal{H} \mid K_\top)$. At this point, one should wonder why Keynes is assuming (a). After all, $\mathcal{H}$ *doesn't entail* any of the $\mathcal{E}_i$'s. What $\mathcal{H}$ entails are *conditional* instances of the form $Ea \supset Ga$ (*i.e.*, instances in the modern, *logical* sense). But, $\mathcal{H}$ does *not* entail "positive instances" of the form $Ea \,\&\, Ga$. This is a very common mistake that is made over and over again in the literature (Earman remarks on several contemporary versions of this error in the reading posted on the course website). When we get to our discussion of Hempel and Carnap, we will see that some contemporary authors presuppose that Ea is entailed by the background corpus K (on the grounds that ϕ knows "a priori" that ϕ is "observing emeralds for their color" or "sampling from the class of emeralds"). While it is clear that $K_\top \vDash Ea$ would ensure (a), it is also clear that Keynes is *not* assuming this. Moreover, as we'll see in a few weeks, Hempel (and Carnap) insist that it is crucial that we do not make any such assumptions about $K_\top$. Of course, Keynes could modify everything in these chapters by uniformly replacing "$Ea \,\&\, Ga$" with "$Ea \supset Ga$". But, that would substantially change everything he says (sometimes in subtle and/or *bad* ways). Moreover, doesn't it seem a bit odd to speak of people learning $Ea \supset Ga$? And, would Humean "constant conjunction" have to give way to "constant conditional"? This is a non-trivial issue, since probabilistic relevance in *non*-monotonic. We'll discuss the insidious role of non-monotonicity when we get to Hempel.