Scientific Explanation & Scientific Realism: Day 2

- Administrative:
  - Three hour meeting tonight (with a break!) & perhaps next week
  - I'll present stuff for 1 or 2 more weeks – then students
  - We'll start scheduling presentations over the next week (email)
- Brief Review & Finishing-up From Last Time
  - The move away from Inductive Logic to Epistemic Relativity
  - I–S, Maximal Specificity & Related Problems
  - Probabilistic Laws, Statistical Laws & the Interpretation of Pr
  - High Probability *vs* Probabilistic Relevance
- Statistical-Relevance (S–R)
  - A "relevant" conception of Pr-explanation (& its own problems)
- Railton's D–N Model of Probabilistic Explanation
  - Propensities, probabilistic laws, and theories which entail them

From Inductive Logic to Epistemic Relativity 1

- Keynes, Carnap, and others believed that Inductive Logic was possible. That is, they believed there was a quantitative relation of "argument strength" or "confirmation" $\mathfrak{c}$, which satisfied (at least) two key desiderata:

($\mathcal{D}_1$) The relations of deductive entailment and deductive refutation should be captured as limiting (extreme) values of $\mathfrak{c}$ with cases of 'partial entailment' and 'partial refutation' lying somewhere on a $\mathfrak{c}$-continuum (or range) between these extreme values of the confirmation function.

($\mathcal{D}_2$) Inductive logic (*i.e.*, the *non*-deductive relation $\mathfrak{c}$ between propositions that is characterized by inductive logic) should be *objective* and *logical*.

- It is easy to satisfy ($\mathcal{D}_1$). Just take the strength of an argument from P to C to be $\mathfrak{c}(C, P) = \Pr(C \mid P)$, since $\Pr(C \mid P) = 1$ if $P \vDash C$, and $\Pr(C \mid P) = 0$ if $P \vDash {\sim}C$.
- This $\mathfrak{c}$ isn't sensitive to the *relevance* of P to C [Fred Fox], but it satisfies ($\mathcal{D}_1$).
- The tough one is ($\mathcal{D}_2$). And, it is because philosophers ultimately came to view ($\mathcal{D}_2$) as unsatisfiable that they abandoned the Inductive Logic Project.

From Inductive Logic to Epistemic Relativity 2

- Why did people come to think that ($\mathcal{D}_2$) can't be satisfied? Basically, because:
  - They assumed $\mathfrak{c}(C, P) = \Pr(C \mid P)$. And, they thought that if $\mathfrak{c}$ is going to be logical, then $\Pr(\cdot \mid \cdot)$ itself must also be logical — "logical probability".
  - The search for "the logical probability of C, given P" was underway. But, this search ran into insuperable difficulties, which I won't go into here.
  - At some point, people decided that there was no such thing as "a priori" or "logical probability", but only *a posteriori* or *epistemic* probability.
  - So, people abandoned "logical confirmation" in favor of "epsitemic confirmation": the degree to which E confirms (*i.e.*, supports or weighs evidentially in favor of) H, relative to (total) background knowledge K.
  - Coffa nicely summarizes The Received View about confirmation:

    Although the syntactic form of expressions like "hypothesis *h* is well-confirmed" may mislead us into believing that confirmation is a property of sentences, closer inspection reveals the fact that it is a relation between sentences and knowledge situations and that the concept of confirmation cannot be properly defined . . . without reference to sentences intended to describe a knowledge situation.

From Epistemic "Inductive Logic" to I–S Explanation 1

- There are various dimensions along which D–N explanation can be generalized or refined. The first dimension is what I will call the *logical* dimension, and it has to do with the *standard* by which arguments are judged.
- In D–N, the argument must meet a deductive-logical standard: Deductive Validity. The natural way to generalize this is to move to *confirmation* or *inductive strength* as the *logical* standard by which the argument is judged.
- But, you can only do this if you think there *is* a quantitative inductive-*logical* standard! Since Hempel did not think there was such a thing (almost nobody does anymore!), I–S became a non-logical, epistemic theory of explanation.
- So, Hempel went from Deductive Logic to "Inductive Logic" as the "logical" standard for explanations (thinking of explanation, still, in *inferential* terms), and his (quantitative) theory of "Inductive Logic" is epistemic. That's why.
- Sadly, we can't borrow the standard used in epistemic treatments of "inductive logic", since our total background knowledge K *entails* the *explanandum*!

From Epistemic "Inductive Logic" to I–S Explanation 2

- Since K entails the explanadum, we will always have $\Pr(C \mid P \,\&\, K) = 1$, using the "inductive logical" standard. Thus, every explanatory argument from any P to any C will meet the "logical" standard of having high $\Pr(C \mid P \,\&\, K)$.
- As such, we need to place some sort of restriction on K, so that arbitrary putative explanations aren't able to trivially meet the "logical" standard of evaluation — enter the Requirement of Maximal Specificity (RMS).
- Before we get to (RMS), we need to talk about a second dimension along which we can generalize D–N. That is what I will call the *nomological* dimension, which has to do with the conception of "law" that we assume.
- Hempel generalizes his notion of "law" (which is *extensional*), by moving from universal generalizations to "statistical laws" (still extensional!). A statistical law is simply a statement of relative frequency in a population.
- Such "laws" will be of the form $\ulcorner\Pr(\phi x \mid \psi x \,\&\, K) = r\urcorner$, and they are to be read as: $\ulcorner$Within the reference class K, the proportion of ϕs among the ψs is $r\urcorner$.

I–S, The Requirement of Maximal Specificity & the Reference Class Problem 1

- The simplest schema for an I–S explanation of Gb (relative to K!) would be:

$$\begin{array}{l} \Pr(Gx \mid Fx \,\&\, K) = r \\ Fb \,\&\, K \\ \hline\hline Gb \end{array} \quad [r]$$

- So, the problem for naive I–S raised by the fact that our *total* K entails Gb becomes an instance of the *reference class problem*. Which class to which b belongs is the *reference class* for an I–S explanation of Gb [of strength r]?

(RMS) If $Fb \,\&\, K$ implies that b belongs to a class F_1 and that F_1 is a subclass of F, then $Fb \,\&\, K$ must also imply a statement specifying the statistical probability (*viz.*, the *relative frequency*) of G in F_1:

$$\Pr(Gx \mid F_1 x) = r_1.$$

And, $r_1 = r$, unless the probability statement in question is simply a theorem of probability theory proper (*e.g.*, $\Pr(G \mid F_1 \,\&\, G) = 1$).

I–S, The Requirement of Maximal Specificity & the Reference Class Problem 2

- (RMS) is a pretty strong condition. To be in possession of an I–S explanation of Gb [of strength r] based on $\Pr(Gx \mid Fx \,\&\, K) = r$ and Fb, relative to one's background knowledge K is to know (*i.e.*, for one's K to entail) that there is no subclass F_1 of F such that $\Pr(Gx \mid F_1 x) \neq r$, except for those F_1s which make this true by probability theory alone. That's a pretty strong claim!
- At this point, it is useful to mention another important distinction: the metaphsyics versus the epistemology of explanation. Hempel is supposed to be telling us about the metaphysics of explanation – what an explanation *is*.
- On his account, explanation *is* epistemically relative. This gives the epistemology of explanation an interesting "higher order" aspect. To know that you have I–S explained Gb to a certain degree of strength, you need to know that your knowledge K satisfies (RMS). That's interesting (and hard!).
- While Coffa is OK with *confirmation* being epistemically relative, he is not at all happy with the epistemic relativity of Hempel's account of *explanation*:

> . . . the possibility of a notion of true explanation . . . is not just a desirable but ultimately dispensable feature of a model of explanation: it is the *sine qua non* of its realistic, non-psychologistic inspiration. It is because certain features of the world can be deterministically responsible for others that we can describe a concept of true deductive explanation . . . If there are features of the world which can be non-deterministically responsible for others, then we should be able to define a model of true inductive explanation.

- It's important to be clear on where Coffa thinks Hempel goes wrong. It seems to me that Coffa is objecting to the *inferential framework* of Hempel's programme. That is, Coffa seems to be rejecting the following analogy:

$$\frac{\text{Deductive Logic}}{\text{“Inductive Logic”}} :: \frac{\text{Deterministic Explanation}}{\text{Indeterministic (or Probabilistic) Explanation}}$$

- Coffa rejects this because he thinks (1) Deductive Logic is not epistemic, (2) Inductive Logic is epistemic, and (3) Explanation (of any kind) is not epistemic. Hempel, on the other hand, simply *adopts* a "logical" *framework*.
- Note, also, that the symmetry thesis seems to require that either both or neither confirmation/explanation (in the general inductive case) is epistemic.

I–S, Traditional Inductive Logic *&* *Relevance*

- We will return to the nomological dimension when we discuss Railton's paper. But, I want to say a bit more about the "logical" dimension.
- One thing that Hempel inherits from orthodox Inductive Logic is the assumption that the strength of an explanation (*viz.*, an argument from P to C, relative to K) goes by the size of the *conditional probability* $\Pr(C \mid P \;\&\; K)$.
- But, we have discussed an argument that seems to call this into question:

  (P) Dennis Rodman has been taking birth control pills for the past year.
  Therefore, (C) Dennis Rodman is not pregnant.
- Given our background knowledge K, P is (intuitively) *probabilistically irrelevant* to C, and it seems to have no *explanatory* relevance either. But, $\Pr(C \mid P \;\&\; K)$ is very high, simply because $\Pr(C \mid K)$ is very high.
- These sorts of cases have led people to require *probabilistic relevance* of the explanans. As it turns out, there's only one measure of argument strength that satisfies ($\mathcal{D}_1$), plus relevance. That is the *likelihood ratio*: $\frac{\Pr(P \mid C \;\&\; K)}{\Pr(P \mid {\sim}C \;\&\; K)}$.

High Probability *Versus* High Probabilistic Relevance

- Irrelevance cases seem to indicate that HP is *not sufficient* for explanation.
- Jeffrey: high probability is *not necessary* for explanation either:

  Consider a genuinely indeterministic coin which is biased strongly (p = 0.9) toward heads when tossed. Suppose that if it is not tossed the coin has probability of 0.5 of being in either the heads or tails position and that whether or not the coin is tossed is the only factor that is statistically relevant to whether it is heads or tails. According to the IS model, if the coin is tossed and comes up heads, we can explain this outcome by appealing to the fact that the coin was tossed (since under this condition the probability of heads is high) but if the coin is tossed and comes up tails we cannot explain this outcome, since its probability is low . . . The fact that the coin has been tossed is the only factor relevant to either outcome and that factor is common to both outcomes once we have cited the toss . . . we left nothing out that influences the outcome.
- Woodward: such arguments presuppose that "it is not possible for all of the information that is relevant to some M to be insufficient to explain it. . . . It is far from self-evident that this assumption is correct." More later from Railton.

Statistical–Relevance Explanation I

- Salmon, Greeno, Jeffrey, and others were among the first to question the high probability requirement of the I–S account.
- As we have just seen, Jeffrey argued that stochastic processes generate outcomes with varying probabilities, and that we understand the low probability outcomes as well as we understand the high probability outcomes.
- Salmon and Greeno formulated theories in which the key probabilistic fact is a fact about probabilistic *relevance* — not just the (high, posterior) probability of the explanandum, given the explanans.
- Accounts involving relevance as the key attribute face special problems of their own — problems not faced by the I–S account.
- The most important of these is known as *Simpson's Paradox*. Nancy Cartwright describes a good example illustrating this paradox.

Statistical–Relevance Explanation II

- In the early 80's there was a positive correlation between being female (F) and being rejected from Berkeley's graduate school (R).
- This (initially) raised some suspicions about the possibility of sexual discrimination in the admissions process for Berkeley's grad school.
- Symbolically, $\Pr(R \mid F) > \Pr(R)$. That is, being female is *statistically relevant* to being rejected from Berkeley grad school. Or is it?
- If we *partition* the applicants according to the department to which they applied: $\{D_1, \ldots, D_n\}$, then the correlation disappears!
- That is, $\Pr(R \mid F \;\&\; D_i) = \Pr(R \mid D_i)$, for all i.
- Should we still be suspicious about sexual discrimination? Or, more relevantly here, should we still think that the gender of the applicant is *explanatorily relevant* to why they got rejected (or accepted)?

Statistical–Relevance Explanation III

- Simpson's Paradox forces the statistical relevance theorist to make some special maneuvers. Enter the notion of "homogeneous relevant partition".
- Salmon's original S–R account is intended to provide an answer to the question "Why does this (member of the reference class) A have the attribute B?" If the question is not stated this precisely, then Salmon suggests using "pragmatics" to determine the reference class (ghosts of Hempel's ambiguity).
- An S–R explanation (of why this A is a B), consists of the prior probability of B (given A), a homogeneous relevant partition $\{A \,\&\, C_i\}$ of A with respect to B, the posterior probabilities of B in each cell $A \,\&\, C_i$ of the partition, and a statement of the location of the individual in a particular cell $A \,\&\, C_k$:
  - $\Pr(B \mid A) = p$
  - $\Pr(B \mid A \,\&\, C_i) = p_i$
  - $\{A \,\&\, C_i\}$ is a homogenous relevant partition of A with respect to B
  - b is a member of $A \,\&\, C_k$

Statistical–Relevance Explanation IV

- To fix ideas, let's return to the Berkeley graduate school example. Let b be some applicant (A) who was rejected (B). And, we want to know why this applicant (b) was rejected (*i.e.*, why this A (b) is a B).
- A *partition* $\{A \,\&\, C_i\}$ of a class A is a collection of mutually exclusive and exhaustive subsets of A. Each subclass $A \,\&\, C_i$ in the partition $\{A \,\&\, C_i\}$ is called a *cell* of the partition.
- A partition $\{A \,\&\, C_i\}$ of A is *relevant* with respect to B if the probability (*i.e.*, the relative statistical frequency!) of B in each cell $A \,\&\, C_i$ of the partition is different from each other cell, *i.e.*, $\Pr(B \mid A \,\&\, C_i) \neq \Pr(B \mid A \,\&\, C_j)$, all $i \neq j$.
- A partition F is *homogeneous* with respect to B if *no relevant partition (wrt B) can be made within F*. *Objectively* homogeneous = no relevant partition can be made *in principle*; *epistemically* homogeneous = no relevant partition is *known* (presumably, by the explain*er*). Here, the interpretation of Pr is crucial!
- In the case at hand, we do know a relevant partition of A with respect to B:

- That partition is $\{A \,\&\, C_i\}$: the partition in which the C_i are the *genders* of the applicants. If C_1 = Male, and C_2 = Female, then:

  $$\Pr(B \mid A) = p,\ \Pr(B \mid A \,\&\, C_1) = p_1,\ \Pr(B \mid A \,\&\, C_2) = p_2$$

  Here, $p_2 < p < p_1$. Therefore, the partition $\{A \,\&\, C_i\}$ is *relevant* to B.
- Intuitively, the partition $\{A \,\&\, C_i\}$ is *not* "explanatory" with respect to B. This is because there is a further set of (intuitively) *relevant factors* $\{D_i\}$ such that:

  $$\Pr(B \mid A \,\&\, C_1 \,\&\, D_i) = \Pr(B \mid A \,\&\, C_2 \,\&\, D_i), \text{ for all } i$$

- Does this mean that the original partition $\{A \,\&\, C_i\}$ is *not homogeneous* with respect to B? If so, that would undermine an explanation of Ab which appeals to the $\{A \,\&\, C_i\}$ partition (*i.e.*, an explanation in terms of *gender*). Intuitively, that's the answer we want, and that's what "homogenaeity" is supposed to do.
- We can't tell from the information so far whether the finer-grained partition $A \,\&\, C \,\&\, D$ is relevant. For that, we'd need to check and see whether, *e.g.*, $\Pr(B \mid A \,\&\, C_2 \,\&\, D_i) \neq \Pr(B \mid A \,\&\, C_2 \,\&\, D_j)$, for $i \neq j$. As it turns out (in the example at hand), the answer is YES. Is this a vindication of the S–R model?

Statistical–Relevance Explanation V

- Intuitively, "homogeneity" is *intended* to block the explanatoriness of gender for acceptance in the Berkeley grad school example. And, Salmon's definition of "homogeneity" does seem to to the trick in this case. But, will it always?
- If Pr is just a *statistical frequency*, then there may be further "relevant" partitions of the data in Salmon's sense. Or, there may happen to be no further "relevant" partitions. Is *statistical* relevance *explanatory* relevance?
- Interesting fact about the Berkeley Case: If one partitions *further within* $C \,\&\, D$, according to Z = the first letter of the applicant's last name is between "F" and "K", then *this* is a further *statistically relevant* partition á lá Salmon.
- I don't think we'd want to say that $C \,\&\, D \,\&\, Z$ is an *explanatorily* relevant partition, nor would we want to say that the existence of this finer-grained partition *rules-out* the explanatory relevance of the coarser-grained $C \,\&\, D$.
- Statistical relevance can be *misleading* about explanatory relevance. What we want is "homogeneous" in the sense of "including all the *explanatorily* relevant factors". Idea: *causal* relevance? This is where Salmon goes next …

Railton's D–N Theory of Probabilistic Explanation 1

- Before getting into the details of Railton's account, it helps to think more about the "logical" and "nomological" dimensions of Hempel's programme.
- As I have mentioned, people gave up on inductive logic *qua logic*, and this explains why Hempel went "epistemic" in his I–S theory of explanation.
- There is a way to remain *logical* in the inductive case. I say confirmation is a *ternary* relation between premises, conclusions, and *probability models*. Once a model is specified, the degrees of confirmation are logically determined.
- The question then becomes: what is the correct probability model for a given inference? That, I claim, is not a logical question but a pragmatic one.
- If we're using this inference as (or in) an explanation, then we want an *explanatorily salient* probability model. But, where do those come from?
- In a sense, Railton's approach is very amenable to this way of thinking about inductive logic. Railton says that the appropriate probability models will come from *our best scientific theory of the phenomenon in question*. Makes sense.

Railton's D–N Theory of Probabilistic Explanation 2

- Getting back to the nomological dimension, we need to ask whether "statistical laws" in the Salmon/Hempel sense are really the kinds of laws that are featured in explanations of indeterministic events. It seems they are not.
- Here, again, we need to carefully distinguish metaphysical and epistemic aspects of explanations. If one looks at our best indeterministic physical theories, it seems that they are not – metaphysically – statistical.
- Example: QM models of the position of an electron in a hydrogen atom seem to involve *probability*, but *not* of a *statistical* variety. It isn't just that large ensembles of hydrogen electrons happen to exhibit statistical behavior *en masse*. It seems that the probabilities attach to *token events* (single cases).
- What is a single-case probability? We won't get into that too much here, but think of it as a *disposition* of an object to instantiate a property (in a context).
- Railton invites us to look at QM explanations like this as involving the *deduction* of *probabilistic* laws concerning *single-case probabilities* of particles instantiating properties at times (in contexts). Railton's example:

Railton's D–N Theory of Probabilistic Explanation 3

- (a) All nuclei of U^{238} have (single-case) probability $1 - e^{-\lambda_{238} \cdot \theta}$ to emit an alpha-particle during any interval of length θ, unless subjected to environmental radiation. [Is this really a Hempelian Law? See below.]
  (b) U was a nucleus of U^{238} at time t_0, and was subjected to no environmental radiation before or during the interval $[t_0, t_0 + \theta]$.
  Therefore, (c) U had (single-case) probability $1 - e^{-\lambda_{238} \cdot \theta}$ to emit an alphaparticle during the interval $[t_0, t_0 + \theta]$.
- Railton says that (a)–(c) constutute a Deductive–Nomological Probabilistic Explanation of U's emitting (or not emitting?) an alphaparticle during the interval $[t_0, t_0 + \theta]$, provided that we provide the following "supplements".
- (d) A derivation of law (a) from our "best theory of alpha-decay."
- (e) The D-N inference from (a) and (b) to (c).
- (f) A parenthetic addendum to the effect that U did (did not?) alpha-decay during $[t_0, t_0 + \theta]$. [Would (a)–(c) explain both a decay and a non-decay?]

Railton's D–N Theory of Probabilistic Explanation 4

- We still have a monotonicity problem. Single-case probabilities (and propensities or dispositions, generally) also depend on which other factors are present in the token case. Which factors should be included in the "law"?

  > . . . there is a great deal more we could say about U's decay. Deliberately left out of [the supplement] are innumerable details about the experimental apparatus (temperature, pressure, location, etc.), about the beliefs and expectations of those monitoring the experiment, and about the epistemic position of the scientific community at the time. These facts are omitted as *explanatorily irrelevant* to U's decay because they are *causally irrelevant* to the physical possibility for decay that obtained during the interval in question, and to whether or not that possibility was realized.

- Railton seems to presuppose here that our best scientific theory of the phenomenon in question will isolate for us the *explanatorily relevant* factors by codifying the *causally relevant* factors (for a given experimental set-up).
- Causal relevance again rears its head at the foundation of judgments about explanatory relevance. After we discuss Harman, we'll move on to causal E.

Railton's D–N Theory of Probabilistic Explanation 5

- Has Railton avoided the problems with D–N? Won't he still face many of the same "irrelevance"s, both of the logical kind and the nomological kind?
- Since Railton is still assuming classical universal generalizations with chance consequents, he is still susceptible to the paradoxes of material implication, isn't he? Shouldn't the "laws" involve some kind of (modal) connection between antecedent and consequent? Is "$(\forall x)[Fx \supset \Pr(Gx) = r]$" a *law*?
- Moreover, the standard of classical logical entailment still also seems susceptible to irrelevant "logical tricks" a la Kaplan et al. Like:

$$T.\ (\forall x)(\forall y)[Ix \vee (Py \supset \Pr(My) = r)]$$
$$C.\ (Ic \vee {\sim}Pd) \supset \Pr(Md) = r$$
$$\therefore E.\ \Pr(Md) = r$$

- Important Distinction: "the conditional probability of A, given B, is r" *vs* "if B, then the probability of A is r". "$\Pr(A \mid B) = r$" *vs* "If B, then $\Pr(A) = r$".
- Must a "logical" probabilistic account of explanation involve conditionals with chance consequents? Why not a purely probabilistic approach?

Inductive-Nomological Explanation? 1

- Why not try for a theory of Inductive–Nomological explanation? That is, why not use our approach to Inductive Logic to provide an inductive generalization of Hempel's approach? This is not so easy, but here's a try that's in the spirit:
- Say we're trying to explain Gb-at-t on the basis of Fb-at-t', where it is assumed that a field of background factors $\mathcal{B}$ (other than F or G) obtained at t'.
- Let's assume that our best theory (a la Railton) of such phenomena states that, relative to $\mathcal{B}$, the single-case probability of Gb-at-t, given Fb-at-t' is r. Arguably, then, our theory gives us an explanatorily salient probability model $\mathcal{M}$ with which to judge the strength of the following inductive argument:

$$\begin{array}{c} \mathcal{B}\text{-at-}t' \\ Fb\text{-at-}t' \\ \hline\hline Gb\text{-at-}t \end{array} \qquad [r]$$

- Following Hempel, we might let r be the conditional probability in $\mathcal{M}$ of Gb-at-t, given Fb-at-t' and $\mathcal{B}$-at-t'. That is, we might say $\Pr_{\mathcal{M}}(Gb\text{-at-}t \mid Fb\text{-at-}t' \,\&\, \mathcal{B}\text{-at-}t') = r$.

Inductive-Nomological Explanation? 2

- It seems to me that the correct theory of inductive logic should not only get the deductive cases right (as the extremes), but it should also be sensitive to probabilistic relevance (in $\mathcal{M}$). This leads to the following "relevant" theory:
- **I–N**. Fb-at-t' I–N explains Gb-at-t to degree r, relative to $\mathcal{B}$-at-t', according to probability model $\mathcal{M}$, if the likelihood-ratio (in $\mathcal{M}$) of Gb-at-t on Fb-at-t', given $\mathcal{B}$-at-t' is r. That is, if: $\dfrac{\Pr(Fb\text{-at-}t' \mid Gb\text{-at-}t \,\&\, \mathcal{B}\text{-at-}t')}{\Pr(Fb\text{-at-}t' \mid {\sim}Gb\text{-at-}t \,\&\, \mathcal{B}\text{-at-}t')} = r$.
- This requires our theory to tell us that Fb-at-t' is *relevant* to the single-case probability of Gb-at-t, given $\mathcal{B}$-at-t', and that this relevance has a certain degree of strength. [Recall Fred Fox's pill-taking and his non-pregnancy.]
- Also, this should resolve the "homogenaeity" problem, and problems involving Simpson's Paradox in the same way that Railton's D–N approach resolved the monotonicity (or reference class) problem faced by I–S.
- If we follow Railton, we will be appealing, implicitly, to causal relevance in our claim that our best theory tells us which additional factors to ignore.

Inductive-Nomological Explanation? 3

- What happened to the distinction between the nomological and the logical dimensions of explanation? In the traditional inferential accounts (D–N, I–S, Railton's D–N–P, etc.), we had (i) "laws" as *premises in*, and some standard *for* the *evaluation of* explanatory arguments. It seems (i) is gone with I–N.
- In my I–N account (which I am not endorsing, but filling-in for historical purposes) the probability model $\mathcal{M}$ is doing double duty – as a player in both the nomological and logical aspects of explanation. How does this work?
- From a logical point of view, the model is just an abstract component of the confirmation function – there to facilitate judgments of argument strength.
- From a nomological POV, $\mathcal{M}$ contains information about the nomological structure of single-case propensities, as expressed by our best theories.
- It is important to keep in mind this dual aspect of probability models, as they are being used in the I–N model of explanation that I have sketched. I wonder what Coffa would have said about I–N? [Railton seems to think it's cute.]