Recall (**Lectures 18,18b) :**

- **Logical empiricists**, like Rudolf Carnap and Carl Hempel, who helped established the 'traditional' or 'classical' research tradition in the philosophy of science[1], set the stage for contemporary theories of **scientific explanation** by stripping away from this enterprise any reference to metaphysics.[2]

- Carl Hempel, in particular, sought to characterize his notion of scientific explanations in a **logically rigorous manner** (in a manner he hoped would mimic the character, content, and success of *proof theory* in metamathematics) in which, he claimed, can be fundamentally characterized either as **deductive-nomological (DN)**, or **inductive-statistical (IS).** Emblematic of the **hypothetico-deductive schema**, the former (DN) are characterized by the **argument schema**[3]:

1. $C_1, C_2, \ldots, C_n$
2. $L_1, L_2, \ldots, L_m$

$\therefore E$

where: the premises/explanans are subdivided according to :

$C_1, C_2, \ldots, C_n$ **:** statements describing particular facts
$L_1, L_2, \ldots, L_m$ **:** statements describing **general laws**
E: The explanandum (what demands an explanation)

- Moreover, the **DN** schema is often described as the **covering-law** model: General (universal) laws[4] with particular facts 'cover' the explanandum *E* (which can either be

---

[1] Recall **Lecture I**: from a meta-philosophical perspective such a tradition was wholly committed to an **anti-metaphysical** picture insofar as any theoretical term that was (at least in principle) incapable of being reduced to set of observation-terms/sentences was considered to be meaningless. Theories and scientific knowledge is general was considered to be reducible to or 'explicated' (rationally reconstructed) by sets of *sentences* in some language ideally characterized by some formal (logically regimented—usually FOPL—by default) language Λ (i.e., the sentential/syntactical view--**footnote 1, Lecture I**). These considerations evinced a *verificationist* theory of meaning (a statement is meaningful if and only if it is verifiable in principle.) **However, as the term 'empiricists' (and the earlier progenitors: 'postivists') connotes, such a tradition was scientifically *anti-realist*:** scientific theories were seen as instrumentally reliable and rated accorded to their predictive power. **Aside from being amenable to a rigorous (logical/syntactic) reconstruction, any residually *semantic* notions, including, for that matter, considerations concerning 'truth', were written off as having (at best) *epistemic*, or *pragmatic* efficacy, but didn't constitute the fundamental character of scientific knowledge or reasoning.**

[2] Recall R. Carnap's 'demarcation criterion' for scientific explanation necessarily involving **laws of nature.**

[3] Recall **Lecture II**.

[4] Recall **Lecture XVIII**: *Causal laws* (i.e. laws expressible in terms of causal processes) are examples of such laws: i.e. every *causal* explanation of some phenomenon is DN, but the converse doesn't hold. A counterexample illustrating that not all DN explanations are causal, would include (according to Hempel) invoking any general mathematical law (like Boyle's Law, i.e. $PV = nRT$) expressing *simultaneous quantities*. For example, in the case of Boyle's law, isolating P for instance produces $P = {}^{nRT}/_{V}$. Though in such a scenario, one could perhaps speak of a *change* in n, (or T, or V) as 'causing' a change in P, **it would still be erroneous to say that any of the terms on the right hand side are in any mutually causal**

some brute empirical fact or some other general law) insofar as *E instantiates* (i.e. acts as an *instance of*) a conclusion *deduced* from the premises/explanans.

- Explanations characterized by the **IS** schema were considered to be more problematic by Hempel. On the one hand, he (and Carnap) sought to characterize them as **broader generalizations of the DN schema** applied to laws having a *statistical* or probabilistic character.[5] On the other hand, for Hempel, such a generalization also *weakened* the IS schema (in comparison to the robustness of the DN schema). Such weakenings produced problems of **epistemic ambiguity** not shared by the DN schema. For a given IS schema:

1. $C_1, C_2, \dots, C_n$
2. $P_1(R_1|\cap_{k1}F_{k1}), P_2(R_2|\cap_{k2}F_{k2}), \dots, P_m(R_m|\cap_{km}F_{km})$
================================== **[*p*]**
E

where: $C_1, C_2, \dots, C_n$ **:** statements describing particular facts
$P_1(R_1|\cap_{k1}F_{k1}), P_2(R_2|\cap_{k2}F_{k2}), \dots, P_m(R_m|\cap_{km}F_{km})$**:** statements describing probabilistic/statistical laws (where, for $1 \leq i \leq m$: R_i are the regularities and $\cap_{ki} F_{ki}$ represent the particular background factors[6]. Note that $\cap_{ki} F_{ki}$ is simply shorthand for $F_{1,i} \cap F_{2,i} \cap \dots$ where the first numerical index refers to the particular index fo the particular factor F while the second index I of course refers to the particular probabilistic law $P_i(R_i|\cap_{ki}F_{ki})$

E: The explanandum, with degree of support p.

- **Note1:** The degree of support p need not be the same as the particular probabilistic law(s) comprising 2. in the *explanans.* (However, for the toy examples Hempel offers, p have the same value(s) as the particular probability(ies) of the aforementioned probabilistic laws.) In more general terms, however, p could be some *function* of some (or all) of the probabilistic laws P_1, P_2, …
- **Note2:** Recall the Problem of Induction[7]; Hempel qualifies his **IS** cases so that they are 'close to' the **DN** schema, namely, that $p \approx 1$ (i.e. that degree of inductive support is close to unity). **In this respect, the epistemic ambiguity Hempel grappled with <u>is completely unrelated</u>**

---

interrelationship: mathematically, after all, they represent in the above formula for *P* three separate *independent variables*, i.e. *independently varying* quantities.

[5] Examples are easy to come by: such laws of a statistical variety would include the strong (i.e. statistically significant) correlation between hypertension and heart disease, etc.

[6] Recall **<u>Lectures XV –XVII</u>**: *empirical* probabilistic claims, whether alluding to frequency ratios or more general probabilistic inferences alluding to specific empirical contexts (via Bayes' Theorem or otherwise) are inevitably conditional.

[7] I.e., how to warrant any conclusion whose informative scope exceeds that of the union of its premises. (Recall the case of the ravens: forming some conclusion concerning *all* ravens being black, based on *some observed instances of black ravens* is obviously such a case, where the presumed scope of information of the conclusion needless to say far exceeds the union (total) scope of the premises. Those of you who chose the first topic in Paper I certainly are acquainted with this issue☺.

<u>**to the issue of inductive support**</u> (i.e., the <u>degree of belief</u> by which one claims E has been explained or accounted for by the explanans.)

<u>**Recall the problem**</u>: Two (or more) IS schema, *with the same logical form*, *can account for contradictory explananda*, given modifications of the terms of the explanans. According to Hempel, this *cannot* arise in the DN case: either the deductive argument is *sound* (and therefore *valid*[8]) and hence accounts for E, or not. **On the other hand, in the case of the IS schema, since the *explanans* include probabilistic laws, modifying any particular fact or law merely changes the *value*(*s*) of the probabilities, a *semantic* matter[9], but not the logical form.** Examples (cited in <u>**Lecture 18b**</u>) include:

W : Property of being a warm and sunny day.
N : Property of being November at Stanford.
n : Nov 27th .
S : Property of being a successor to a cold and rainy day at Stanford.

2c) $P(W \mid N) = 0.95$
Nn
========= $[p = 0.95]$
Wn

2d) $P(\sim W \mid S) = 0.8$
Sn
========= $[p = 0.8]$
$\sim Wn$

In both instances, the arguments **2c)** and **2d)** have the same logical form…all that was altered were the probabilities in the laws[10] and the (logically contingent) particular facts. To see that this ambiguity *cannot* arise in the case of DN, consider the (admittedly ridiculous) "law" that $P(W \mid N) = 1$ which is equivalent to the conditional: $\forall x(Nx \rightarrow Wx)$**.** In that case, its counterpart is obviously $P(\sim W \mid N) = 0$, which means that; $\forall x(Nx \rightarrow \sim Wx)$ is strictly **false**. Hence any explanation incorporating the "law" $\forall x(Nx \rightarrow \sim Wx)$ is **unsound**, thus cannot account for E.

Of course, as discussed in <u>**Lecture 18b**</u> Hempel assumed such an 'ambiguity' could be resolved through stipulation of a condition of maximal specificity, which involves specifying a **minimal reference class** from the set of total scientific knowledge (at time t) K_t , which is a set with the maximal number of conditions necessary for the propert(ies) for the particular causal law to fall under. For example, in the above case, the reference class for **2d)** is the **subset** of all days in November in Stanford in which their preceding days were cold and rainy. This is obviously *strictly contained* (i.e. is a *proper subset*) of all the days in November at Stanford. But the reference class of the probabilistic "law" in **2c**) is the latter set mentioned above, and *not* the former.

---

[8] Recall <u>**Lecture II**</u>.
[9] Recall **footnote 1.,** <u>**Lecture 18b**</u>
[10] As shown in <u>**Lecture XVI**</u>, $P(\sim A) = 1 - P(A)$ (for any event A). Hence the probabilistic law in **2d)** could be re-cast in the form: $P(\sim W \mid S) = 1 - P(W \mid S)$, i.e. $P(W \mid S) = 0.20$, which of course shows that it's the same one as in **2c)**, with its particular probabilistic value altered.

Responses to Hempel (Kitcher, and others)

- As mentioned above and in **Lectures 18, 18b** Hempel sought to reduce the phenomenon of scientific explanation to an issue involving ***logical form***, in which scientific explanation basically served an *epistemic* role, i.e. its explication (or rational reconstruction) revealed a unique schema by and through which scientific reasoning and knowledge can be *extended*. Conversely, *explaining* some scientific theory (for Hempel, Carnap, & co.) did *not* require resorting to some metaphysical notions or principles like 'fundamental causal structure,' 'unity,' etc. **Recall that Hempel would argue that such aforementioned principle were 'metaphysical' (i.e. meaningless) if in principle they could *not* be reduced to observation terms/sentences or laws of nature.**

  Armed with such metaphilosophical presuppositions, Hempel claimed to have been able to label some of these concerns as 'pragmatic,' 'semantic', and 'contextual'[11] the following major objections to his theory:

  1. **The Problem of Relevance**

  This objection was raised by Wesley Salmon. Consider the following DN "explanation:"

  1. C_1, C_2,
  2. L_1
  ____
  $\therefore E$

where:

C_1 **:** Bob takes birth control pills (i.e., *Bb*)
C_2 **:** Bob is a man, (i.e., *Mb*)
L_1**:** No man who takes birth control pills becomes pregnant, i.e.:
$\sim\exists x(Mx \wedge Bx \rightarrow Px) \equiv \forall x[Mx \wedge Bx \wedge \sim Px]$
E: The explanandum: Bob hasn't become pregnant ($\sim Pb$)

Because of its *logical form,* this rather particularly bizarre DN scheme still qualifies as explanatory according to Hempel. **But what one recongnizes as intuitively bizarre concerns the (presumed) *explanans* as being clearly *irrelevant* to the explanandum!** More precisely, L_1 and C_1 are: C_2 and $L_1^* \equiv \forall x[Mx \wedge \sim Px]$ would have done the job.

  2. **The Problem of Asymmetry**

  This objection was raised by Sylvain Bromberger. Consider the following DN "explanation:"

  1. C_1

[11] Hence some of the concerns were for him insignificant from a syntactic/logical form perspective. He did, however, recognize that his account needed to address pragmatic issues to *some* extent. "Hempel clearly needed an account of the pragmatic of explanation" (Kitcher, 413) beyond just offering the analogy with metamathematics and that his schema functioned in a similar manner as "rational reconstructions."

2. L_1

$\therefore$ ***E***

where: ***C_1*****:** This Barometer (*a*) is falling rapidly (*Ba* & *Fa*).

L_1**:** Whenever a barometer falls rapidly, a storm approaches: $\forall x(Bx \wedge Fx \rightarrow Sx)$

E: The explanandum: A storm is approaching at this particular day (*d*), place (*l*) and time (*t*): *Sdlt*

Because of its *logical form,* this rather particular DN scheme still qualifies as explanatory according to Hempel. However, though one would argue that a rapidly falling barometer *indicates* an approaching storm, **it's quite another thing to say a rapidly falling barometer *explains* the storm's occurrence. If anything, it makes far more sense to state the converse: a rapidly approaching storm explains the falling barometer.**

Kitcher uses the flagpole example (viz. specification of causal laws) discussed in **Lecture 18** concerning the asymmetry problem (p. 412, 425 viz. the discussion of the axiomatization of mathematical groups.)

3. The Problem in the Specification of Laws

It remains unclear **how a law-like generalization can be distinguished form some accidental one** (like "the sleeves of all my shirts have coffee stains".) Essentially what *would* distinguish the two would presumably answer Hume's problem of induction. Hempel thought a *syntactic* maneuver would suffice here, but this *semantic* issue obviously still arises.

Kitcher discusses Goodman's problems of projectible predicates (412) (recall the 'grue' issue of warped mentioned in **Lecture XVII)**

There are other objections to Hempel, but the above three beg the question for other strategies for accounting for the nature of scientific explanation. Phillip Kitcher sought to preserve what he considered were some of the strongest **methodological & logical** features of Hempel; namely that **scientific explanations that resort to theory** can be characterized as some form of a **deductive/ derivative activity**: "Theoretical explanation provides some support for the Hempelian idea that explanation is derivation…[n]onetheless, Hempel's account of theoretical explanation is underdeveloped, and with good reason." (428)

Recall from your previous readings of Kitcher that he is a ***scientific realist***. **Note also that scientific realists need not be strict or strong metaphysical realists** (insofar as stipulating that there exists one unique ontology "**[T]**ruth" tof the world that science shall 'uncover'). Scientific realists can resort to comparatively weaker epistemic and methodological issues[12]: whether (in the case of later Kitcher) arguing that theories function as 'maps' which though sharing obviously some correspondence with features of the world (i.e. "**[t]**ruth") are also

[12] In this regard, recall the introductory lecture on realism: Nicholas Cusanis (15th cent) with his 'inscribed polygon in a circle' illustration of the minds attempts to comprehend the world, in which 'no polygon, no matter how many sides are added ever converges to the circle is an early example of this weaker epistemc notion of realism.

inevitably and irreducibly characterized by the pragmatic en epistemic interests of the theorists/mapmakers. One sees a version of Kitcher's idea in this earlier paper: For he argues that the **global methodological goal**[13] of science is a **search for greater understanding:**

> **The search for understanding is…a fundamental goal of the [scientific] enterprise [conceived of in a global methodological manner]. The quest may take many different forms in different historical and disciplinary contexts, but it is tempting to think that there is something that underlies the various local endeavors, something that makes each of them properly be seen as striving after the same goal. (419)**

For Kitcher, this necessarily involves a notion that scientific explanation is an **endeavor aimed at unification**. In this regard, Hempel's scheme of *explanans* as premises and *explananda* as conclusions isn't precise enough:

> *[I]deal* explanations are derivations. Here there is agreement…with Hempel…But on the [unification] systematization account, [though] an argument is considered as a derivation…**An ideal explanation [nevertheless] does not simply list the premises but shows how the premises yield the conclusion.** (431)

Kitcher characterizes this systematic schema in terms of a set *E*(*K*) defined as "the explanatory store" over the set of the deductively closed and consistent set of sentences *K* characterizing the beliefs of a scientific community endorsed at a particular time (i.e. Hempel's notion of scientific knowledge):

> *E*(*K*) is to be the set of derivations that best systematizes *K*, and I shall suppose that the criterion for systematization is unification. ***E*(*K*), then, is the set of derivations that best unifies *K*.** (431)

In the remaining sections of his paper, Kitcher "precisifies" the notion of unification in rather detailed fashion, but the essential point is a simple one, which he paraphrases Michael Friedman:

> *E*(*K*) [is] the set of arguments that achieves the best tradeoff between minimizing the number of premises used and maximimizing the number of conclusions obtained. (431)

Kitcher (like many realists…recall Boyd with his 'virtuous circle' defense of scientific realism viz. confirmation theory) is invoking the above notion of unity as an *inference to the best explanation* (IBE) for an epistemic defense of scientific realism. Similar to Boyd, in his criticisms of vanFraassen, Kitcher (as other realists) argue that the burden of proof rests upon the anti-realist's shoulders on just how to provide an account for such unifying activity in scientific explanations: to write it off as a 'pragmatic virtue' (in the case of van Fraassen) strikes realists as question-begging: *what makes this a virtue in the first place*?

Kitcher of course draws heavily from vanFraassen's pragmatic accounts of explanation (as answering 'why?' questions) but argues that his unifiying account tightens up van Fraassen's approach. (van Fraassen of course introduced his approach to account for the pragmatic problems confronting Hempel's model mentioned above.)

---

[13] A controversial notion. Note that Laudan believed in no such thing, as he argued the methodologies of science were underwritten by their research tradition

For van Fraassen, an explanation (or answer to a why question) is characterized via the following ordered triple (414-415):

<P_k , *X*, ***R*** > where: P_k is the topic of the 'why?' question
X is P_k *'s* contrast class (every "why P_k ?" question is a "why P_k instead of alternatives listed in *X*? " question)
R is the relevance relation (with respect to the explanatory context from which *X* is drawn.)

Though illuminating in ways that Hempel's account wasn't, nevertheless vanFraassen's epistemic anti-realism commits him to an an excessively unconstrained notion of *R*: "Is uggest that van Fraassen's..discussion of why-questions is best seen not as a solution to all the problems of the theory of explanation, but as a means of tackling problems of the first type [i.e. the problems of relevance, asymmetry, and lawhood directed against Hempel.]" (417)