Hypothetico-deductivism is a first approximation to scientific practice. In its simplest form, predictions are deduced from a hypothesis and if they prove true on observation this confirms the hypothesis. "Deduction" here is used loosely as meaning something like "acceptable mathematical reasoning." But often more is needed in scientific practice to accomplish the deduction, that is, a substantive background theory. This leads to the version of hypothetico-deductivism with which Glymour's discussion begins: "a sentence h is confirmed by a sentence e with respect to a theory T if e is true and h & T is consistent and h & T entails e... but T does not entail e." Glymour finds this account untenable because of the problems of *irrelevant conjunction* (for any sentence A that is consistent with h & T, the conjunction A & h is confirmed by e with respect to T if h is) and *trivial theories* (if e is true and not valid and S is any consistent sentence such that the negation of e does not entail S then S is confirmed by e with respect to a true theory, namely $(S \rightarrow e)$). Grimes discusses a third problem of *irrelevant disjunction*: If e confirms h in the hypothetico-deductive way, then a weaker statement $(e \vee I)$ can confirm h where I is an irrelevant disjunct.

Merrill (***Philosophy of science***, vol. 46 (1979), pp. 98–117) offered a reformulation of hypothetico-deductivism with an explicit clause designed to rule out hypotheses that are irrelevant conjunctions. The leading idea is to say that in the case of direct confirmation, h should not be logically equivalent to the conjunction of two strictly weaker sentences at least one of which is confirmed by e with respect to T in the standard hypothetico-deductive way. Glymour shows that Merrill's definition is satisfied only in the trivial case where e entails h, and obtains the same result for a modified version of Merrill's theory suggested by a referee. If e does not entail h, there is always a way to represent h as a prohibited conjunction. In the trivialization of Merrill's theory the conjuncts that Glymour considers are $(T \rightarrow e)$ and $((T \rightarrow e) \rightarrow h)$.

Waters suggests that the problems of irrelevance in confirmation are not problems of hypothetico-deductivism *per se* but rather problems of the classical logic used in the deductions. He suggests relying instead on relevance logic; in particular, the system ***R***. This, however, cannot be the whole story, since the inferences from p & q to p and from p to $p \vee q$ are as valid in relevance logic as in classical logic. Rather, Waters advocates a version of the Merrill approach combined with relevance logic. He supposes that there is a natural division of empirical claims into "unit statements." Scientific hypotheses are conjunctions of these unit statements. A hypothesis H is directly confirmed relative to a theory T only if (A) H and T are consistent; (B) $H, T \vdash e$; (C) for every unit statement S of H, $(H - S, T) \nvdash e$ (where $H - S$ is conjunction of all the conjuncts of H except for S). The sort of conjunctions that Glymour constructs are not to be counted as "unit statements." So far, this proposal only addresses the problem of irrelevant conjunction in hypotheses, and has nothing to do with relevance logic.

Waters, however, sees residual problems with this approach which he wishes to resolve by interpreting scientific theories as being formulated in the language of relevance logic. One of these is the problem of trivial theories. On the foregoing proposal it is still true that any true piece of evidence confirms any unit statement with respect to some true theory by Glymour's argument. Use of relevance logic in formulating scientific theories is supposed to block the argument: "But this $[H \rightarrow e]$ is not necessarily a true theory if we use ***R***-logic, for in system ***R***, $H \rightarrow e$ does not follow from e." This quick remark calls for some discussion. I take it that the relevant implication from H to e need not be true when e is; but the truth function $\sim H \vee e$ is. Disjunctive syllogism fails to deliver a relevant implication, however. Waters's proposal must then be to require the derivability conditions in (A)–(C) to be rephrased as relevant implications. There is no discussion in this paper of semantics for ***R*** and its appropriateness for a theory of contingent scientific truth.

Waters's proposal is (and is intended to be) programmatic. Implementation requires a theory of "unit statements" and how scientific hypotheses are built out of them, and the development of relevant arithmetic, analysis, topology, etc. adequate to deducibility in the broad sense required in science. It does not address the problem of irrelevant disjunction which was (subsequently) raised by Grimes.

Grimes's discussion begins with a defense of the simplest and most straightforward version of hypothetico-deductivism. If (non-tautological) e is classically derivable from (consistent) h and e is true, then e confirms h. If auxiliary hypothesis T is required for the derivation then rather than considering a relativized notion of confirmation, Grimes simply says that e confirms the conjunction, h & T. In the case of a trivial theory, $h \rightarrow e$, this conjunction is equivalent to h & e, and so the problem of trivial theories is a special case of irrelevant conjunction. Grimes believes that irrelevant conjunction is not a problem. If e is a non-tautological consequence of a consistent statement, h & I, where I is an irrelevant conjunct, then *e* is "part of the content" of *h* & *I* and showing that *e* is true should confirm *h* & *I*, at least to some degree.

After arguing that in this way the consequences of hypothetico-deductivism that Glymour and Waters take as untenable are, in fact, correct, Grimes introduces the problem of irrelevant disjunctions. But I fail to see why his defense of hypothetico-deductivism against the problem of irrelevant conjunction does not apply equally well here. If *h* entails *e* and you are informed that the disjunction of *e* with some irrelevant (logically independent contingent) *S* is true, this information rules out possible situations in which *h* is false. Then, by Grimes's previous reasoning, the disjunction should confirm *h*, at least to some degree. Grimes, however, does not think that the evidential disjunction with irrelevant disjunct is intuitively *part of the content of h* in the way that *e* is part of the content of *h* & *I* in the case of irrelevant conjunction. So Grimes introduces a notion of *narrow consequence* designed to rule out disjunctions with irrelevant disjuncts, and requires the confirming statement to be a narrow consequence of the hypothesis in the reformulation of hypothetico-deductivism.

The nature of the discussion in these three papers suggests that it would be useful to devote more attention to what Carnap called "the clarification of an explicandum." Grimes devotes some attention to this task in his discussion of content, but the notion of content itself needs to be clarified. Carnap (***Logical foundations of probability***, XVI 205), in discussion of Hempel, distinguished two possible explicanda that might be associated with "*e* confirms *h*": (1) The probability of *h* conditional on *e* is greater than its unconditional probability. (2) The probability of *h* conditional on *e* is greater than some fixed value. These two explicanda have different logical properties. Hypothetico-deductivism is false for (2) but by Bayes's theorem is approximately true for (1). If *h* entails *e* and both *e* and *h* have non-extreme prior probabilities then $\mathrm{pr}(h \mid e) > \mathrm{pr}(h)$. This can be thought of as a probabilistic version of Grimes's defense of irrelevant conjunction. In fact, Carnap explicitly discusses the problem of irrelevant conjunction which was raised by Barrett (VI 103) and discussed by Hempel (X 104). Barrett took irrelevant conjunction to show that "not every observation which confirms a sentence need also confirm all its consequences"; the conjunction is confirmed but not the irrelevant conjunct. Hempel took irrelevant conjunction to be a counterexample to the converse consequence condition; the conjunction is not confirmed. Carnap suggests that Hempel and Barrett may have different explicanda in mind. I suspect that this may also be true in the discussion reviewed here. BRIAN SKYRMS

WILLIAM A. DEMBSKI. *Randomness by design*. ***Noûs***, vol. 25 (1991), pp. 75–106.

Although most of us have definite intuitions about random processes, the notion of a random sequence is notoriously hard to formalize. There is no lack of definitions (e.g. various definitions involving ideas from complexity theory), but there exists no generally accepted mathematical theory. The existing variety of definitions reflects a variety of uses: foundations of probability, cryptography, simulation, and lower bound arguments, to mention a few. To some this situation suggests that randomness, though useful heuristically, is not a coherent mathematical notion. These problems concerning randomness become particularly acute in the case of random number generators: In what sense can an algorithmic procedure be said to imitate true randomness? How do we select the statistical tests that algorithmically generated random numbers have to pass?

In the article under review, the author introduces what he considers to be a new philosophical perspective on the study of randomness, and he offers some suggestions for a formal implementation of his proposals. The first part I found hard to understand, and in discussing it I will stay close to Dembski's wording. (Dembski describes this article on p. 102 as an "expository paper," but apparently there is no more technical account on which it is based.) According to the author, the most common approach to randomness is probabilistic: sequences are random when they pass "sufficiently many" statistical tests. The problem with this approach is that one can always construct tests that rule out some given sequence. This problem does not really arise for randomly generated strings: "Truly random strings are supposed to be generated according to some probability distribution and for this reason—and this reason alone—pass statistical tests" (p. 77), but for strings generated by a (deterministic) algorithm there is a genuine problem. (The quoted remark does not make sense to me; randomly generated strings can fail to pass statistical test as well.)

Dembski proposes to cut through this Gordian knot by reversing the previous procedure: we now start with a given collection of "patterns" (i.e., tests or, more generally, regularities) and we say that a string that violates all patterns in the collection is random. That is, randomness is not absolute, but