Petrus Hispanus Lectures

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After Logical Empiricism

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I
After Logical Empiricism

Call it what you will—and it was mainly its enemies who called it 'logical positivism'—logical empiricism was less a position than a movement, a loose confederation of activists in Vienna, Berlin, Prague, Warsaw, Uppsala, London, ..., and even Cambridge (Massachusetts). The shape of the movement was significantly influenced by historical contingencies like the execution of Janina Hosiasson-Lindenbaum (1899-1942) by the Gestapo in Vilna, silencing one of probabilism's clearest early voices.[1]

Still, the movement did originate with certain salient features, persistent and position-like, but fluid. Notable among these was the LOGICISM that promised to made sense of the non-empirical character of mathematics by rooting it in logic. Another was the EMPIRICISM that defined the remainder of the realm of the meaningful. But both of these went through serious changes of shape and fortune in response to hardcore metamathematical developments (notably, the limitative theorems of Gödel and Tarski, Turing and Church) or in response to other, no less effective, dialectical moves—especially, Carnap's, in tandem with Neurath, from the phenomenalism of *Der logische Aufbau der Welt* (1928) to the physicalism of *Der logische Syntax der Sprache* (1933).

What happened to logical empiricism?

I think that both its logicism and its empiricism ran their course, but were far from vanishing without a trace. On the contrary, I think they can be seen to have been transformed, within the movement, into what are now well-established successor disciplines. It is the empiricism that mainly interests me here, but let us begin with a look at an unestablished successor to the logicism that intrigues me.

[1]Some of her writings: 'Why do we prefer probabilities relative to many data?', Mind **40** (1931) 23-36. 'On confirmation', *Journal of Symbolic Logic* (1940) 133-148. 'Induction et analogie: comparaison de leur fondement', *Mind* (1941) 351-365.

1.1 Logicism Lite[2]

Here is the core of logicism as I see it, presented as a metaphysical question and its dissolution.

Q: How do the natural numbers latch onto the world?

A: They don't—e.g., when we explicate 'There are exactly 3 dodos' we speak of dodos, but of not numbers:

> There exist distinct dodos, x, y, z, and no others,

or, in logical notation,

$$\exists x\, \exists y\, \exists z[Dx \wedge Dy \wedge Dz \wedge x \neq y \wedge x \neq z \wedge y \neq z \\ \wedge \neg\exists w(Dw \wedge w \neq x \wedge w \neq y \wedge w \neq z)].$$

For short:

$$\exists_{\bar{3}} x\, Dx,$$

where '$\bar{3}$' is to be replaced by the numeral for 3 in your favorite notation—e.g., perhaps, $\bar{3}$ = the successor of the successor of the successor of 0,

$$\exists_{0'''} x\, Dx.$$

Aside from the biological term 'D', only logical terms appear here.

Logicism Lite says: IT'S THE DATA THAT ARE LOGICAL.

Carnap's full-blown (1931) logicist thesis about what he called simply 'mathematics' was a heavy logicism, according to which

- mathematical concepts are explicitly definable in logical terms, and
- mathematical theorems are then seen to be logical truths.

Our lighter logicism counts number-theoretical laws as logical for the same reason that physical laws were once counted as empirical: BECAUSE OF THE CHARACTER OF THEIR INSTANCES.[3]

Example. Logicism lite does not count Fermat's Last Theorem as shorthand for a logically valid formula, but as a generalization that derives its status as logical from the particular data it is responsible to—data like

[2]The ideas in this section were floated in 'Logicism 2000: A Mini-Manifesto' in *Benacerraf and his Critics,* Adam Morton and Stephen P. Stich, eds.: Blackwell, Oxford, 1996, pp. 160-164. For a more serious, independent development of such ideas, see Fernando Ferreira, 'A Substitutional Framework for Arithmetical Validity', *Grazer Philosophische Studien* **56** (1998-9) 133-149.

[3]It is no longer respectable (and rightly so!) to think that Newton's inverse square law of gravitation is logically equivalent to the set of its observational consequences. But Fermat's last theorem IS logically equivalent to the set of its variable-free consequences.

$$3^3 + 4^3 \neq 5^3 \ ,$$

which are established by invalidity of schemas like

$$\exists_{\bar{3}^{\bar{3}}+\bar{4}^{\bar{3}}} x A(x) \leftrightarrow \exists_{\bar{5}^{\bar{3}}} x A(x)$$

via recursive contextual definitions of $+$, $\times$, etc. in the subscripts of numerically definite quantifiers. Now any decision procedure for monadic 1st-order logic with '$=$' delivers the data about particular sums, products, and powers to which Fermat's last theorem is answerable.[4]

1.2 Thespian Fictionalism

As to the other salient metaphysical question about number theory, 'What *are* the natural numbers?', the answer is: ANY $\boldsymbol{\omega}$-SEQUENCE WILL DO. The simplest choice is the sequence of numerals that you use, whether that is '0', '$0'$', '$0''$', ... or '0', '1', '2', ..., or whatever. Your numerals are an ontological freebie: they are there anyway, in the syntax of your number theory. For natural number theory itself, as for the theory of real numbers, there is no "correct" domain for its variables.

What I am suggesting is not particularly that number theory is a fiction, but that it is a play, which can be performed with different casts. It's still "Henry the Fifth," whether it's the Laurence Olivier or the Kenneth Branagh production. And it's still "Number Theory," whether the rôles of 0, 1, 2 ... are played by '0', '1', '2', ... or by '0', '$0'$', '$0''$', ... or by $\emptyset, \{\emptyset\}, \{\{\emptyset\}\}, \ldots$.

[4]Fernando Ferreira's (op. cit.) light logicism has no need of these recursive definitions. His concern is with the language L_a of first-order arithmetic without function symbols, with '$=$' and $\neq$, and with the six further predicate symbols *Suc, Add, Mul,* $\overline{Suc}$, $\overline{Add}$, $\overline{Mul}$ under the interpretations $Suc(x,y)$ iff $x' = y$, $Add(x,y,z)$ iff $x + y = z$, ..., $\overline{Mul}(x,y,x)$ iff $x \times y \neq z$. Where I reduce truth of '$7 + 5 = 12$' to validity of '$\exists_{\bar{7}+\bar{5}} x A(x) \leftrightarrow \exists_{\overline{12}} x A(x)$', which is then reduced, in stages, to validity of '$\exists_{\overline{12}} x A(x) \leftrightarrow \exists_{\overline{12}} x A(x)$', he reduces truth of '$Sum(7,5,12)$' or of '$\neg Sum(7,5,12)$' to validity of '$\exists_{\bar{7}} x A(x) \wedge \exists_{\bar{5}} x B(x) \wedge \neg \exists x (A(x) \wedge B(x)) \rightarrow \pm \exists_{\overline{12}} x (A(x) \vee B(x))$', where in the reduct of '$Sum(7,5,12)$' the sign '$\pm$' is dropped, and in the reduct of '$\neg Sum(7,5,12)$' it is replaced by '$\neg$', which is then eliminated step-by-step. (The negation, '$\neg$', of a formula A is defined by interchanging $=$ and $\neq$, $\wedge$ and $\vee$, $\forall$ and $\exists$, *Suc* and $\overline{Suc}$, *Add* and $\overline{Add}$, and Mul and $\overline{Mul}$. Then $\neg\neg A = A$.) Using Saul Kripke's treatment of substitutional quantification ('Is there a problem about substitutional quantification?', *Truth and Meaning,* G. Evans and J. McDowell, eds., Oxford, Clarendon Press, 1980, pp. 325-419), Ferreira defines, relative to any given first-order language L and any sentence S of L_a, a scheme $\mathcal{S}$ of *substitutional sentence forms* over L, and proves the following: (a) If S is a true sentence of L_a, then every member of $\mathcal{S}$ is true. (b) If S is a false sentence of L_a, then not every member of $\mathcal{S}$ is valid.

Now, what about the real numbers? Here I take logicism's successor discipline to be the theory of measurement, the topic treated in volume 3 of *Principia Mathematica* and developed (independently of that) into an applied mathematical discipline later in the 20th century.[5]

Volume 3 of *Principia* aimed to lay foundations for the conceptual apparatus of physics, in a theory of measurement (part VI, 'Quantity'). Like the natural numbers, the rationals were "defined away", as when 2/5 was defined as a relation between relations.[6] And physical magnitudes were analyzed as relations:

> We consider each kind of quantity as what may be called a vector-family, i.e., a class of one-one relations all having the same converse domain, and all having their domain contained in their converse domain. In such a case as spatial distances, the applicability of this view is obvious; in such a case as masses, the view becomes applicable by considering, e.g., one gramme as ... the relation of a mass m to a mass m' when m exceeds m' by one gramme. What is commonly called simply one gramme will then be the mass which has the relation + one gramme to the zero of mass.[7]

The basic move in measurement theory is to axiomatize the theory of the comparative versions of real magnitudes like mass (where the primitive term of the comparative version the relation *is at least as massive as*) so as to mirror the properties of the relation $\geq$ between reals. What I am suggesting is that different empirical magnitudes (mass, length, temperature, desirability, etc.) correspond to different productions of a play, *"Reals!"*, for which the axiomatic script is the theory of complete ordered fields. The suggestion is that abstract productions—models within pure set theory—are at best first among equals.

EXAMPLE: Bolker's Representation theorem for the relation $\succeq$ of weak preference between possible states of affairs (propositions).[8] Here, A and B are any members of the field $\mathcal{F}$ of the relation $\succeq$, and '$X \succeq Y \succeq Z$' means that $X \succeq Y$ and $Y \succeq Z$.

AXIOM 1. $\mathcal{F}$ ($= \mathcal{A} - \bot$) is a complete,[9] atomless Boolean algebra $\mathcal{A}$ with the

[5] See *Foundations of Measurement,* David H. Krantz, R. Duncan Luce, Patrick Suppes and Amos Tversky (eds.): New York, Academic Press: volumes 1-3 (1971, 1989, 1990).

[6] Quine's example: the relation 2/5 holds between the grandparent relation and the great-great-great grandparent relation. ('Whitehead and Modern Logic', The Philosophy of Alfred North Whitehead, P. A. Schilpp (ed.), Northwestern University Press, 1941, p. 161.)

[7] *Principia Mathematica,* vol. 3, p. 233.

[8] Ethan Bolker, 'A Simultaneous Axiomatization of Utility and Subjective Probability', *Philosophy of Science* **34** (1967) 333-340. Also, chapter 9 of *The Logic of Decision* (2nd ed., 1983).

[9] A Boolean algebra is *complete* iff every set of elements of the algebra has a supremum and an infimum in the algebra. The supremum of a set of propositions is implied by every element of the set (it is an upper bound) and implies every upper bound (it is the least upper bound). Similarly, the infimum is the greatest lower bound.

null element removed.

AXIOM 2. Weak preference is transitive and connected.

Definitions. Preference: $A \succ B$ iff $A \succeq B \not\succeq A$.
Indifference: $A \approx B$ iff $A \succeq B \succeq A$.

AXIOM 3, "Averaging." If A and B are incompatible,[10] then
(a) $A \succ \ A \vee B \ \succ B$ if $A \succ B$, and (b) $A \approx \ A \vee B \ \approx B$ if $A \approx B$.

AXIOM 4, "IMPARTIALITY."[11] If (a) A, B, C are incompatible
and (b) $A \approx B \not\approx C$ and (c) $A \vee C \ \approx B \vee C$, then
(d) $A \vee C' \ \approx B \vee C'$ whenever C' is incompatible with A and B.

AXIOM 5, "CONTINUITY." If the supremum, S (or infimum, I) of an implication chain[12] in $\mathcal{F}$ lies in a preference interval, then all members of the chain after (or before) some element C will lie in that interval.

BOLKER'S EXISTENCE THEOREM. If $\succeq$ satisfies axioms 1-5, then there exist a probability measure pr and a signed measure[13] sg on $\mathcal{A}$ with the following property:

$$(\Theta) \qquad A \succeq B \text{ iff } des(A) \geq des(B), \text{ where } des(X) =_{df} \tfrac{sg(X)}{pr(X)}.$$

This takes some proving.[14]

The actors in Bolker's production are INDIFFERENCE CLASSES, where your indifference classe for A is the set $\varphi(A)$ to which a proposition belongs if and only if you are indifferent between it and A. If we write '$\geq$' between '$\varphi(A)$' and '$\varphi(B)$' to indicate that $A \succeq B$, we get a simulacrum of (Θ) that takes no proving:

$$(\Theta') \qquad A \succeq B \text{ iff } \varphi(A) \geq \varphi(B).$$

What does take proving is that φ, whose values are certain subsets of $\mathcal{F}$, acts in all relevant respects[15] like des, whose values are certain real numbers. Or, since real numbers are only rôles, what takes proving is that the values of φ do play the rôle of real numbers: That, rewritten according to (Θ'), the script given by axioms 1-5 can be seen to have the usual axioms for the reals as a subplot, so that the

[10] I.e., if $A \wedge B = \bot$, where $\bot$ is the null element of $\mathcal{A}$.

[11] When conditions (a) and (b) hold, C is a "test proposition" for equiprobability of A and B; and the test is passed or failed depending on whether or not (c) holds. This axiom says that the test is impartial in the sense of not depending on which particular test proposition C is chosen.

[12] I.e., a set of propositions linearly ordered by the relation A IMPLIES B ($A = A \wedge B$).

[13] As signed measures need only be countably additive, sg may take values less than 0 and greater than 1.

[14] As in Bolker's doctoral dissertation, *Functions Resembling Quotients of Measures,* (Harvard University, April 1965), where the representation theorem is proved in chapter 9 (which is largely reproduced in the 1967 'simultaneous axiomatization' paper) as a corollary of earlier parts of the dissertation. These appear, somewhat condensed, as 'Functions Resembling Quotients of Measures', *Transactions of the American Mathematical Society* **124** (1966) 292-312.

[15] —in spite of having to satisfy further conditions as well, e.g., averaging and impartiality.

trajectory of $\varphi(X)$ through the indifference classes will simulate the trajectory of $des(X)$ through any other production of *"Reals!"*.

A final question: How can coarse-grained magnitudes (temperature, desirability) provide casts large enough to fill the enormous stage needed for the real numbers play? (Pure set theory suffers no such embarrassment.) Here is an answer, addressed to the case of your *des* function.

The crucial question is: Can your actual weak preference relation $\succeq$ be extended to a relation $\succeq'$ that satisfies all of Bolker's axioms? If not, your preferences are incoherent, and the fault is with your script, not with the production company. But if it can, then there are relations $\succeq'$ that satisfy the Bolker axioms and agree with $\succeq$ wherever the latter is defined. According to Bolker's representation theorem, each such completion of your weak preference relation determines a function φ whose values can provide a full cast for *"Reals!"* .

1.3 Empiricism Lite?

Here is a sample of mid-20th century heavy empiricism:

> Subtract, in what we say that we see, or hear, or otherwise learn from direct experience, *all that conceivably could be mistaken;* the remainder is the given content of the experience inducing this belief. If there were no such hard kernel in experience—e.g., what we *see* when we think we see a deer but there is no deer—then the word 'experience' would have nothing to refer to.[16]

Heavy empiricism puts some such "hard kernel" to work as the "purely experiential component" of your observations, about which you canot be mistaken. Lewis himself thinks of this hard kernel as a proposition, since it has a truth value (i.e., true), and has a subjective probability for you (i.e., 1). Early and late, he argues that the relationship between your irrefutable kernel and your other empirical judgments is the relationship between fully believed premises and uncertain conclusions, which have various probabilities conditionally upon those premises. Thus, in 1929 (*Mind and the World Order,* pp. 328-9) he holds that

> the immediate premises are, very likely, themselves only probable, and perhaps in turn based upon premises only probable. Unless this backward-leading chain comes to rest finally in certainty, no probability-judgment can be valid at all. . . . Such ultimate premises

[16]C. I. Lewis, *An Analysis of Knowledge and Valuation,* Open Court, LaSalle, Illinois, 1947, pp. 182-3. Lewis's emphasis.

> ... must be actual given data for the individual who makes the judgment.

And in 1947 (*An Analysis of Knowledge and Valuation,* p. 186):

> If anything is to be probable, then something must be certain. The data which themselves support a genine probability, must themselves be certainties. We do have such absolute certainties in the sense data initiating belief and in those passages of experience which later may confirm it.

In effect, Lewis subscribes to Carnap's view[17] of inductive probability as prescribing, as your current subjective probability for a hypothesis H, its "degree of confirmation", $pr_t(H) = c(H|D_1 \wedge D_2 \ldots \wedge D_t)$, given all of your fully believed data sentences D_i, from the beginning (D_1) to date (D_t).

Lewis's arguments for this view seem to be based on the widespread but erroneous idea that

(a) such conditioning on certainties is the only rational way to form your degrees of belief, and

(b) if you are rational, the information encoded in your probabilies at any time is the conjunction of all your data propositions up to that time.

Against (a), I point to the existence of a way, prescribed under identifiable circumstances by the rules of the probability calculus, in which you must update your old probabilities in the light of experience which has led you to assign certain new probabilities short of 1 to the answers to a multiple-choice question on which your new observation bears—say, new probabilities r, y, g for the color (R, Y, G) of a traffic light:

$$new(H) = r \cdot old(H|R) + y \cdot old(H|Y) + g \cdot old(H|G)$$

("Probability Kinematics")

If you were sure of the color—say, red—your new probabilities would be $r = 1$, $y = 0$, $g = 0$, and the formula would reduce to ordinary conditioning: $new(H) = old(H|R)$.

The broadest general sufficient condition mandated by the probability calculus for this mode of updating is *constancy of your conditional probabilities given the*

[17]See, e.g., Rudolf Carnap, *The Logical Foundations of Probability,* University of Chicago Press (1950, 1964).

possible answers ($A = R, Y, G$) *to the question, as your probabilities for the answers change* (i.e., here, to r, y, g):[18]

$$new(H|A) = old(H|A) \text{ for } A = R, Y, G \qquad \text{(“Invariance”)}$$

Against (b), I point to cases where experience conveys information that you cannot incorporate into your judgmental probabilities simply by conditioning on some proposition in your probability space.

Example. Is the shirt blue or green? There are conditions (blue/green color-blindness, poor lighting) under which what you see can lead you to adopt probabilities—say, $\frac{2}{3}$, $\frac{1}{3}$—for blue and green, where there is no hardcore experiential proposition E you can cite for which your $old(Blue|E) = \frac{2}{3}$ and $old(Green|E) = \frac{1}{3}$. This E would be less accessible than Lewis's ‘what we *see* when we think we see a deer but there is no deer’, since what we think is not that the shirt is blue, but that $new(\text{blue}) = \frac{2}{3}$, $new(\text{green}) = \frac{1}{3}$.

With $\mathcal{E}$ as the proposition $\boxed{new(\text{blue}) = \frac{2}{3} \wedge new(\text{green}) = \frac{1}{3}}$, it is actually possible to expand the domain of the function *old* so as to allow conditioning on $\mathcal{E}$, in a way that yields the same result you would get via probability kinematics. Formally, this $\mathcal{E}$ behaves like the elusive E of the preceding paragraph.[19] Perhaps $\mathcal{E}$ could be regarded as an experiential proposition in some very light sense. It may be thought to satisfy the certainty and invariance conditions, $new(\mathcal{E}) = 1$ and $new(H|\mathcal{E}) = old(H|\mathcal{E})$, and it does stand outside the normal run of propositions to which we assign probabilities, as do the presumed experiential propositions with which we might hope to cash those heavily context-dependent epistemological checks that begin with ‘looks’. Perhaps. But my own inclination is to withhold the term ‘empiricism’ in this very light sense as misleadingly atavistic, and speak simply of “probabilism”.

[18] *Proof.* In the presence of invariance, the following instance of the law of total probability reduces to probability kinematics: $new(H) = new(H|R)new(R) + new(H|Y)new(Y) + new(H|G)new(G)$.

[19] Following Brian Skyrms, *Causal Necessity* (Yale, 1980), Appendix 2, note that, where $\mathcal{E} = \boxed{new(D_1) = d_1 \wedge \ldots \wedge new(D_n) = d_n}$, if (1) $old(D_i|\boxed{D_i = d_i}) = d_i$ and (2) $old(H|D_i \wedge \mathcal{E}) = old(H|D_i)$, then (3) $old(H|\ \mathcal{E}) = d_1 old(H|D_1) + \ldots + d_n old(H|D_n)$. In the presence of certainty, $new(\mathcal{E})=1$, invariance, $new(H|\mathcal{E}) = old(H|\mathcal{E})$, reduces to $new(H) = old(H|\mathcal{E})$, and—as with probability kinematics—we have (4) $new(H) = d_1 old(H|D_1 + \ldots d_n old(H|D_n)$.

1.4 Appendix: Notes on Heavy Empiricism

1.4.1 Mach's Moustache

Fig. 1.

> ... I lie upon my sofa. If I close my right eye, the picture represented in the accompanying cut is presented to my left eye. In a frame formed by the ridge of my eyebrow, by my nose, and by my moustache, appears a part of my body, so far as visible, with its environment. My body differs from other human bodies—beyond the fact that every intense motor idea is immediately expressed by a movement of it, and that, if it is touched, more striking changes are determined than if other bodies are touched—by the circumstance that it is only seen piecemeal, and, especially, is seen without a head. ...[20]

As illustrated, Mach's moustache frames a photographic parable of the evidential proposition E for an intert fragment of visual perception.

[20] Ernst Mach, *The Analysis of Sensations and the relation of the Physical to the Psychical,* Chicago, Open Court, 1897; New York, Dover, 1959, pp. 18-19.

1.4.2 Quine's Irritations of our Sensory Surfaces

—say, the retina. The trouble is that a chronological record of the pattern of irritation of rods and cones would be gibberish without a correlated record of activity in the proprioceptive circuitry monitoring position of eyes in head, head on torso, etc. Here is his broader (and, I think, latest) word on the subject:[21]

> . . . we can imitate the phenomenalistic groundwork of Carnap's *Audbau* in our new setting. His ground elements were his elementary experiences; each was the subject's total sensory experience durimng some moment, or specious present. What can we take as the physical analogue? Simply the class of all sensory receptors that were triggered at that moment; or, better, the temporally ordered class of receptors triggered during that specious present. The input gets processed in the brain, but what distinguishes one unvarnished input from another is is just what receptors were triggered and in what order. Here is a fitting physical correlate of the global sensory experience of a moment. I call it a *global stimulus.*

1.4.3 Inner-outer hocus-pocus

Use of the familiar inside/outside the skin contrast as an explanatory placebo is bogus because the whole body is part of the "external" world. Consider Quine's *global stimulus:* 'The input gets processed in the brain, but what distinguishes one unvarnished input from another is is just *what receptors were triggered and in what order.'* The "unvarnished inputs" are triggerings of "receptors"—but these must include triggerings of introceptors, inputs that register orientations of eyes in head, head on shoulders, etc. And do these latter include inputs to the muscles governing these orientations, which are part of a feedback loop containing those introceptors? Quine's breezy analogy overlooks these questions.

1.4.4 Conscious experience is too slender a base.[22]

"Blindsight"[23] is a striking illustration. In certain primates—humans, among them—some 90% of optic nerve fibres project to the striate cortex at the very back of brain via the dorsal lateral geniculate nucleus in the midbrain. The non-geniculo-striate 10% seem to provide visual capacities of which patients whose

[21] Willard van Orman Quine, *From Stimulus to Science,* Harvard University Press, 1995, pp. 16-17.

[22] See pp. 3-5 of *Probability and the Art of Judgment* (Cambridge, 1992).

[23] *Blindsight, a Case Study and Implications,* L. Weiskrantz (Oxford, 1986), pp. 3-6, 14, 168-9.

striate cortex is removed are unaware. One such patient ("D.B."), who 'could not see one's outstretched hand, . . . seemed to be able to reach for it accurately.' In effect, D.B. formed rather reliable judgments about the location of the unseen hand. (He thought of it as guesswork.) The rest of us—epistemologists and others—who may well make some use of the the occult 10%, are similarly oblivious to its input.

1.4.5 Neurath's Protocol-Processor[24]

> There is no way to establish fully secured, neat protocol sentences as starting point of the sciences. There is no *tabula rasa.* We are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry-dock and reconstruct it from the best components. . . . (p. 92)
>
> . . . Fundamentally it makes no difference at all whether Kalon works with Kalon's or with Neurath's protocols . . .. In order to make this quite clear, we could think of a scientific cleaning machine into which protocol sentences are thrown. The 'laws' and other 'factual statements', including protocol statements, which have their effect through the arrangement of the wheels of the machine, clean the stock of protocol statements thrown in and make a bell ring when a 'contradiction' appears. Now either the protocol statement has to be replaced by another or the machine has to be rconstructed. *Who* reconstructs the machine, *whose* protocol statements are thrown in, is of no consequence at all; everybody can test his 'own' as well as 'others'' protocol statements. (p. 98)

1.4.6 Probabilistic Enhancement of Dogmatic Protocols[25]

Driving to work, radios tuned to NPR, Ann and three of her colleagues all hear an actor—they know it's Gielgud or Olivier—doing 'To be or not to be'. On arrival they write protocol sentences on cards (e.g., 'Ann's protocol at 9 AM: At 8:45 AM I heard Gielgud') and drop them into the protocol box. The *Protokollmeister* collects the four cards and prepares a single protocol for the Neurath machine (e.g., 'Master protocol at 9:05 AM: *It was Gielguid*'), like this:

> The master protocol says it was Gielgud if at least three of the individual protocols said it was Gielgud, and otherwise says it was Olivier.

[24]"Protokollsätze", *Erkenntnis* **3** (1932-3) 204-214. Translations here are from Otto Neurath, *Philosophical Papers 1913-1946*, Reidel (1983).

[25]Edward Moore & Claude Shannon, "Reliable Circuits Using Less Reliable Relays", *J. Fanklin Inst.* **262** (1956) 191-297

Perhaps the *Protokollmeister* regards the four individuals as equally reliable—and as not very reliable. He thinks they are all pretty good at recognizing Gielgud's voice, and really bad at recognizing Olivier's. For each of them—say, Ann—he judges:

pr(Ann says 'Gielgud'|It is Gielgud) = 80%
pr(Ann says 'Gielgud'|It is Olivier) = 60%

Fact: He must judge that the master protocol, *MP,* does better:

$pr(MP$ says 'Gielgud' | It is Gielgud)=82%
$pr(MP$ says 'Gielgud' | It is Olivier)=47%[26]

And many more 80%/60% protocols can make a 99%/1% one.

[26] *Proof.* pr(3 'Gielgud's) $= 4p^3(1-p)$, where $p = .8$ (or .6) if it was (or not) Gielgud. Then pr(3 or 4 'Gielgud's) $= 4p^3(1-p) + p^4 = p^3[4(1-p) + p]$. If $p = .8$ (or .6), this = .82 (or .47).

II
Radical Probabilism

What used to be called 'dogmatism'[27] is the view that judgment is and ought to be a matter of all-or-none acceptance or rejection of propositions—i.e., as close as makes no odds, assignment of subjective probabilities 1 or 0 to propositions. But suspense of judgment is also possible: not every proposition need be accepted or rejected.

What I call 'probabilism'[28] is the view that we do better to encode our judgments as subjective probabilities that can lie anywhere in the whole unit interval [0,1] from 0 to 1, endpoints included. The thought is that this provides a richer palette, allowing a more nuanced representation of our judgmental states. And again, suspense of judgment is possible: not every proposition need be assigned a subjective probability.

What I call 'radical probabilism' is the view that probabilistic judgment need not be rooted in underlying dogmatic judgments: it can be probabilities all the way down the roots.

2.1 Four Fallacies

Perhaps the best way to start expanding these remarks is to examine some common misunderstandings.

2.1.1 "All probabilities are conditional."

This is often the expression of a stalwart empiricism: 'Your probabilities depend on your experiences—including, perhaps, your experiences of other people's probabilistic protocols.'

[27] E.g., by Sextus Empiricus.

[28] This is not to be confused with what the ancients called by that name.

Perhaps so. But it is a further step—and a blunder—to suppose that therefore your new probabilities are obtainable by conditioning your old probabilities on your current experience, $old \overset{e}{\mapsto} new$. This is a blunder because e ("my current experience"[29]) is
(1) not a proposition in the domain of *old*, but
(2) a particular existent—your current experience in full detail—
(3) which is definable ostensively but not purely descriptively.

It makes sense to write 'new(H)=old$_e(H)$' where 'e' is an index and 'old_e' is an ostensive definition of *new*, but by (1) it makes no sense to write '$new(H) = old(H|e)$'. This point would be blunted if in (2) the particular were a particular fact, a true proposition in the domain of *old*, and (3) were false, for then a description of e could be used as a sentence, and e might be a proposition in the domain of *old* after all. But according to (1)-(3), the 'e' in 'new(H)=old$_e(H)$' is an index identifying old_e as your sequel ("*new*") to *old* after your new experience, whatever that may have been: $old \overset{e}{\mapsto} new(=_{df}\ old_e)$.

2.1.2 "Probabilism eliminates suspense of judgment."

Probabilism does offer a way of replacing blunt dogmatic suspensions on the one hand and rash rejections and acceptances on the other by probabilistic judgments corresponding to betting odds.

EXAMPLES: .01 (odds 99:1 against), .50 (evens), and .90 (odds 9:1 on), where the salient dogmatic options might have been rejection, suspension and acceptance, respectively.

In such ways, probabilism can offer more acceptable treatments of cases where none of the three dogmatic options seem quite right; but it also allows for indeterminacies. These can be more nuanced than dogmatic indeterminacies, which are simple suspensions.

To compare the two frameworks here it will be useful to introduce the term 'proBAsition' for a set of complete probability assignments *pr* to the sentences of a language (or, what comes to the same thing, to the propositions those sentences express[30]). The terminological conceit is: just as a proPOsition is a set of complete truth valuations *val* of the sentences of L, so a proBAsition is a set of complete probability assignments to the sentences of L.[31]

[29] I have been using double quotes to indicate simultaneous use and mention. Here, 'e' denotes your current experience, and abbreviates the words 'my current experience', i.e., in your mouth.

[30] If the arguments of *pr* are sentences of, not propositions, we need an extra axiom: either 'If A logically implies B, $pr(A) \leq pr(B)$' or 'If A and B are logically equivalent, $pr(A) = pr(B)$.'

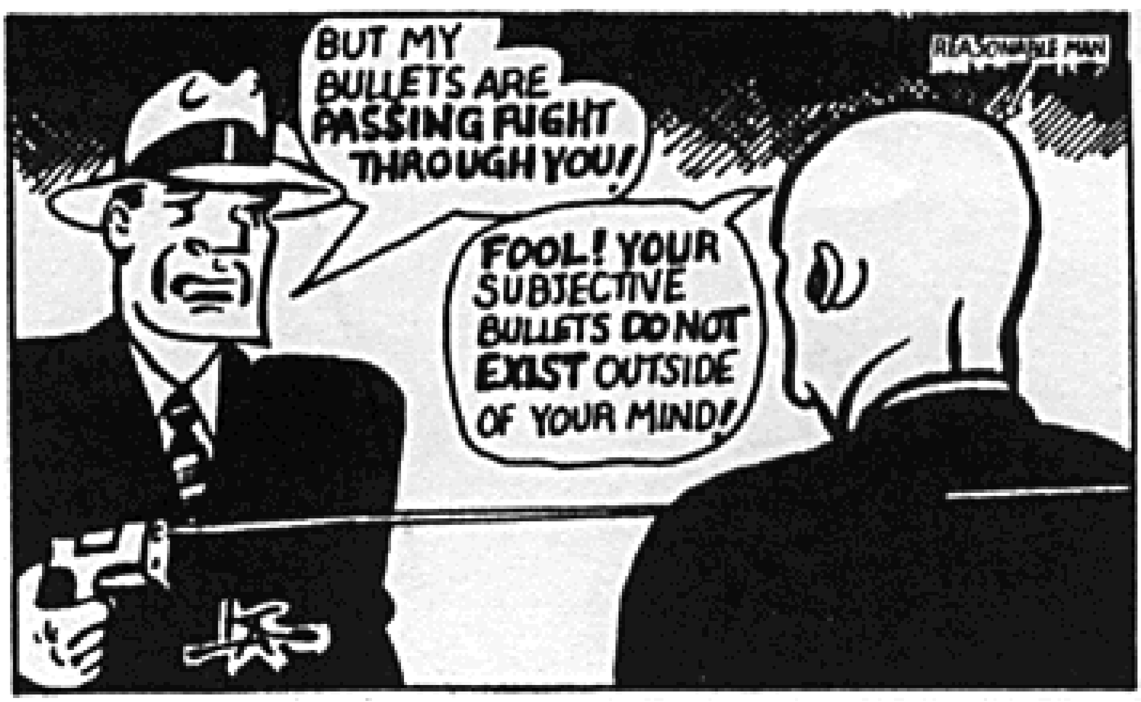
[31] If we take the truth values to be 1 and 0, for true and false, then, formally, a truth valuation is the special case of a probability assignment in which only the extreme probabilities are assigned.

Now probabilistic judgmental states of mind relative to a language L are representable by probasitions. And the corresponding dogmatic states are representable by propositions. Such a proposition represents suspension of judgment about a sentence S iff there are (total) valuations in it—say, val_0 and val_1—for which $val_0(S) = 0$ and $val_1(S) = 1$. And a probabilistic state of total suspense about S would be represented by a probasition $\mathcal{P}$ containing, for every number r from 0 to 1, a probability assignment pr for which $pr(S) = r$.

But there is a rich spectrum of probabilistic judgmental states that lie between definiteness[32] and total suspense. Here is a small sample:

- Convex[33] indefiniteness
  (a) $\frac{1}{4} \leq pr(S) \leq \frac{1}{2}$ (b) $pr(S) = .7$ to one digit of accuracy
- Nonconvex indefiniteness
  (c) $pr(S) \in \{\frac{1}{4}, \frac{1}{2}\}$ (d) Independence,[34] $pr(A \wedge B) = pr(A)pr(B)$

2.1.3 "Subjective = gratuitous, data-free"



[32]A probasition $\mathcal{P}$ assigns a definite value to S iff $pr(S) = pr'(S)$ for every pr and $pr' \in \mathcal{P}$.

[33]A probasition $\mathcal{P}$ is convex iff for all $pr, pr' \in \mathcal{P}$ and all $a, 1-a \geq 0$, $a{\cdot}pr + (1-a) \cdot pr' \in \mathcal{P}$.

[34]In example (d) the indefinite relation of judgmental independence of A from B is represented by $\mathcal{P} = \{pr : pr(A \wedge B) = pr(A)pr(B)\}$. Convexity implies that if pr_1 and pr_2 are both in $\mathcal{P}$ then so is $pr_3 =_{df} \frac{1}{2}pr_1 + \frac{1}{2}pr_2$. But suppose $pr_1(A) = pr_1(B) = \frac{1}{4}$ and $pr_2(A) = pr_2(B) = \frac{3}{4}$. Then we have $pr_3(A) = pr_3(B) = \frac{1}{2}$, so that $pr_3(A \wedge B) = \frac{11}{32} \neq pr_3(A)pr_3(B) = \frac{8}{32}$.

Vernacular "subjectivity" has just these connotations[35]—which is why I prefer the term 'judgmental' and Savage preferred the term 'personal'. But the young de Finetti preferred the term 'subjective', and it is now pretty widely understood among probabilists as a term of art, stripped of its connotations of voluntarism and irrationalism.[36]

In fact, as de Finetti points out, judgmental probabilities are always equivalent to certain probabilistic judgments about frequencies, for the following is a law of the probability calculus:[37]

> Your probability for any sentence S = your expectation of the relative frequency of truths in any finite sequence $S_1, \ldots, S_n$ of sentences you see as equiprobable with S.

Then we expect our probabilistic judgments to match the statistical data: in that sense, we see our "subjective" probabilities as objective.

Following de Finetti, we can also identify the conditions under which subjective probabilities do and should converge limiting relative frequencies. As de Finetti shows,[38] the special circumstances under which this happens are those where you regard a sequence of hypotheses as EXCHANGEABLE, in the sense that your probability for truth of some particular t of them and falsity of some other particular f depends only on the numbers t and f, and is independent of which particular t were true, and which particular f false.[39] Under the assumption of exchangeability, as $n \to \infty$ your conditional probability for the $n+1$'st hypothesis, given the truth values of the first n $(= t + f)$, gets arbitrarily close to the relative frequency $\frac{t}{n}$ of truths among the first n.

2.1.4 "The result of two updates is independent of order."

This is true for CONDITIONING on two propositions in turn. And in the case of generalized conditioning on two partitions (D, E) it is true as long as the partitions

[35] Witness the cartoon by KA, reproduced here from "Deontological Dick," *Harvard Law Record,* **86**, No. 7 (April 15, 1988), p. 9.

[36] For particulars about the young de Finetti, see 'Reading *Probabilismo', Erkenntnis* **31** (1989) 225-237.

[37] See chapter II of his 'La prévision', ses lois logiques, ses sources subjectives', *Annales de l'Instutut Henri Poincaré* **7**(1937)1-68. In symbols, $pr(S) = E(\frac{I_1 + \ldots + I_n}{n})$ if $pr(S) = pr(S_1) = \ldots = pr(S_n)$. Here, I_i is the "indicator" of S_i (the random variable that takes the value 1 where S_i is true, and 0 where S_i is false). Proof is by linearity of E, together with the property $E(I_i) = pr(S_i)$.

[38] de Finetti, *op. cit.,* chapter 3.

[39] Thus, with $t + f = 3$ tosses, of which exactly $t = 2$ are heads, the tail is equally likely to turn up first, second or third: $pr(\overline{H_1}H_2H_3) = pr(H_1\overline{H_2}H_3) = pr(H_1H_2\overline{H_3})$.

are viewed as independent, i.e., as long as $old(D_i \wedge E_j) = old(D_i)old(E_j)$ whenever D_i is a cell of the D partition and E_j is a cell of the E partition.[40]

But in the absence of independence, order can affect the outcome. The simplest example is also the clearest: Suppose $D = E$, so that there is only one partition. Here the second assignment simply replaces the first, leaving matters as they would have been if the first update had never happened. Then unless the two assignments are identical, the result will depend on the order of updating.[41]

2.2 When is Conditioning the Way to Go?

Conditioning is the business of updating your old probabilities by making the new agree with the old, conditionally upon some data statement: $new(H) = old(H|D)$.

Note that '$pr(H|D) = \frac{pr(H \wedge D)}{pr(D)}$' is not "the definition" of conditional probability. There are two reasons for this:

(1) In idealized examples, $pr(B|A)$ may be defined when $pr(A) = 0$:
pr(The H_2O is solid | Its temperature is precisely $\pi° F$) = 1.

(2) $pr(B|A)$ may be defined when $pr(A)$ and $pr(A)$ are not:
pr(head|tossed)=$\frac{1}{2}$ although pr(head $\wedge$ tossed) and pr(tossed) are undetermined.

Conditional and unconditional probability are different functions, of different numbers of arguments, connected by the multiplicative law, $pr(A \wedge B) = pr(A)pr(B|A)$. This equation can be solved for $pr(B|A)$ only if both $pr(A)$ and $pr(A \wedge B)$ are defined, and $pr(A \wedge B)$ is positive.

Now when can you consistently set $new(H) = old(H|D)$?

NECESSARY CONDITION #1, "CERTAINTY":
If $new(H) = old(H|D)$, then $new(D) = 1$.

Proof. By hypothesis, $new(D) = old(D|D)$, and by the probability calculus, $old(D|D) = 1$.

But certainty is not *sufficient* for conditionalization. Counterexample: A card is drawn at random from a normal 52-card deck, and you see that it is a heart, so that new(It's a heart) = 1. Then also new(It's red) = 1 and $new(\top) = 1$, so that

[40] For a more general independence condition, necessary and sufficient for commutativity, see Persi Diaconis and Sandy Zabell, 'Updating subjective probability, *JASA* **77** (1982), esp. pp. 825-827.

[41] In the notation of the previous footnote: if $\mathcal{A}$=$\mathcal{B}$ and $A = B$, then $pr_{ab}(A) = pr_b(A) = b$ and $pr_{ba}(A) = pr_a(A) = a$. So, unless the second update simply reiterates the first (unless $a = b$), order matters.

old(It's the queen of hearts | It's a heart)$= \frac{1}{13}$
old(It's the queen of hearts | It's red)$= \frac{1}{26}$
old(It's the queen of hearts | $\top$) $= \frac{1}{52}$.

Sufficiency cannot be be sufficient for conditionalization because it is impossible that

$$new(\text{It's the queen of hearts}) = \tfrac{1}{13} = \tfrac{1}{26} = \tfrac{1}{52}.$$

Taken together with certainty, the following condition is necessary and sufficient for conditioning.[42]

NECESSARY CONDITION #2, "INVARIANCE"
—in three equivalent versions:

"CONSTANT ODDS"
If B and C each imply D, then $\dfrac{new(B)}{new(C)} = \dfrac{old(B)}{old(C)}$.

"CONSTANT PROPORTION"
If B implies D, then $\dfrac{new(B)}{new(D)} = \dfrac{old(B)}{old(D)}$.

"RIGIDITY"
For all H, $new(H|D) = old(H|D)$.

One way to ensure that these three hold: Use a "statistician's stooge" (I. J. Good's term), i.e., someone you trust to give true yes-or-no answers, with no further information, to questions of form "D?". (In the counterexample, above, D = 'The card is a heart'.)

2.3 Generalized Conditioning

Here we begin a general treatment of the sort of updating ("probability kinematics") that was illustrated by the traffic light example in sec. **1.3** above. In general, the thought is that an observation will change your probabilities for the answers to a multiple-choice question from $old(D_1), \ldots, old(D_n)$ to $new(D_1), \ldots, new(D_n)$. The thought is that you view the D's as a partition, i.e., for $pr = old$ and for $pr = new$, you judge that $pr(D_i \wedge D_j) = 0$ if $i \neq j$, and $pr(D_1 \vee \ldots \vee D_n) = 1$. In

[42]For proof of equivalence, see my *Subjective Probability: The Real Thing,* sec. **1.9**. Incomplete 2001 prepublication version on http:\\www.princeton.edu \˜bayesway\.

sec. **1.3** the question was 'What is the color of the light?', n was 3, and $D_1 = R$, $D_2 = Y$, $D_3 = G$.

In the traffic light example your $new(H)$ was obtained as a weighted average of your old conditional probabilities, $old(H|D_i)$; the weights were your unconditional probabilities $new(D_i)$ for the colors. Their values, $new(D_i) = d_i$, were presumably PROBABILITY PROTOCOLS, arising in direct response to your visual experience.

In general we have the formula

PROBABILITY KINEMATICS: $new(H) = \sum_{i=1}^{n} new(D_i) old(H|D_i)$ (1)

This is equivalent to

RIGIDITY: $new(H|D_i) = old(H|D_i)$ for all i (2)

and to the conditions of CONSTANT PROPORTION and CONSTANT ODDS noted in the previous section, with 'D_i' on place of 'D'.

The number by which your old odds on H:G can be multiplied in order to get your new odds is called your 'ODDS FACTOR' or

BAYES FACTOR: $\beta(H : G) =_{df} \frac{new(H)/new(G)}{old(H)/old(G)}$ (3)

When $G : H$ is of form $D_i : D_1$, we abbreviate this:[43]

$$\beta_i =_{df} \beta(D_i : D_1) \quad (4)$$

Now in view of (3) and (4), the following is an identity:

$$new(D_i) = \frac{old(D_i)\beta_i}{\sum_i old(D_i)\beta_i} \quad (5)$$

And in view of (1) and (5), so is the following:

$$new(H) = \frac{\sum_i \beta_i old(H \wedge D_i)}{\sum_i \beta_i old(D_i)} \quad (6)$$

Then updating by factor protocols can always be viewed as a case of probability kinematics, namely, the case where the weights $new(D_i)$ in (1) are not your direct probability protocols, but are computed from factor protocols α_i or β_i. Often, these factor protocols will be those of some other observer, whom you regard as an expert, and whose factor protocols you adopt, and combine, in (5), with your own $old(D_i)$'s to get your $new(D_i)$'s and, then, via (1), your $new(H)$'s.[44]

[43] The choice of D_1 is arbitrary, i.e., the proportions $\beta_{1j} : \ldots : \beta_{nj}$ are the same for all j.

[44] See the end of sec. **2.1** of *Subjective Probability* for A. N. Turing's view of $\log \beta_i$—what I. J. Good calls 'the weight of evidence'—as a direct protocol. The context was the breaking of the "Enigma" code at Bletchley Park, in World War II.

2.4 Collaborative Cognition

2.4.1 Exogenous Factor Protocols[45]

Factor protocols can be better candidates than probability protocols for the role of reports to be pooled, Neurath-style, in the common data base, for the numbers β_i seem to dissect out of the transition $old \mapsto new$ the contribution of the observation itself, leaving the observer's prior probabilities behind.

MEDICAL EXAMPLE. In the light of a histopathologist's factor protocols, you, the clinician, update your prior probability $old(H)$ for your patient's being alive in 5 years. The partition has three cells:

(D_1) Islet cell ca, (D_2) Ductal cell ca, (D_3) Benign tumor.

Here we suppose that the pathologist, having no prior probabilities of her own for the three diagnoses in relation to your patient, sets her $old(D_1) = old(D_2) = old(D_3) = \frac{1}{3}$ arbitrarily, in order to make her Bayes factors easy to compute. (If she has definite priors for the D's, she had better use them; the arithmetic is no real obstacle.) The key assumption is that, as an expert histopathologist, her Bayes factors in response to microscopic examination of the tissue sample are independent of her priors.

Now suppose the probabilities and Bayes factors for diagnoses are the following:

	D_1	D_2	D_3			D_1	D_2	D_3
old	$\frac{1}{3}$	$\frac{1}{3}$	$\frac{1}{3}$		$\downarrow$ *old*	$\frac{1}{6}$	$\frac{1}{3}$	$\frac{1}{2}$
new	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{2}$		*new*	$\frac{1}{9}$	$\frac{2}{9}$	$\frac{6}{9}$
β_{i1}	1	1	2	$\longrightarrow$	$\uparrow \beta_{i1}$	1	1	2
P a t h o l o g i s t					*C l i n i c i a n*			

As shown above, the pathologist's Bayes factors β_{i1} are:
$\beta_1 = 1$ since the odds between D_1 and itself are necessarily 1,
$\beta_2 = 1$ since, as it happens, $pr(D_2) = pr(D_1)$ for $pr = old$ or $= new$,
$\beta_3 = \frac{(1/2)/(1/4)}{(1/3)/(1/3)} = 2$.

Now you, the clinician, will combine your own old probabilities for the diagnoses with the pathologist's Bayes factors to compute your new probabilities for the diagnoses[46]—and, by probability kinematics, for 5-year survival (H_5).[47]

[45] Schwartz, Wolfe, and Pauker, "Pathology and Probabilities: a new approach to interpreting and reporting biopsies", *New England Journal of Medicine* **305** (1981) 917-923. (*Not* their numbers.)

[46] E.g., for the diagnosis D_3 that the tumor is benign, you can apply formula (5) to get the result $new(D_3) = \frac{(1/2)(2)}{(1/6)(1)+(1/3)(1)+(1/2)(2)} = \frac{6}{9}$, as in the table.

[47] E.g., if your $old(H_5|D_i) = .5, .6, .9$ for $i = 1, 2, 3$, formula (1) yields $new(H_5) \approx .80$, whereas, using your $old(D_i)$'s instead of your $new(D_i)$'s, your $old(H_5) \approx .50$.

2.4.2 Factor updating twice on a single partition is always commutative.

MEDICAL EXAMPLE, CONTINUED. Adopting a pathologist's factor protocols $\beta_1, \ldots \beta_n$ as your own, you update your old probabilities on the diagnoses to new probabilities, $old \overset{\beta}{\mapsto} new$. But let us write *old* as P and *new* as P_β, so that the provenance of a probability assignment can be read off its name: $P \overset{\beta}{\mapsto} P_\beta$.

Here, by **2.3**(5), $P_\beta(D_i) = \frac{P(D_i)\beta_i}{\sum_i P(D_i)\beta_i}$.

You have also received factor protocols $\beta'_1, \ldots, \beta'_n$ for the same diagnoses from another sort of expert—say, a radiologist. Adopting those, you now update the $P_\beta(D_i)$'s: $P \overset{\beta}{\mapsto} P_\beta \overset{\beta'}{\mapsto} P_{\beta,\beta'}$. It turns out that

$$P_{\beta,\beta'}(D_i) = \frac{P(D_i)\beta_i\beta'_i}{\sum_i P(D_i)\beta_1\beta'_1}$$

Proof, via (5): $P_{\beta,\beta'}(D_i) = \frac{P_\beta(D_i)\beta'_i}{\sum_i P_\beta(D_i)\beta'_i} = \frac{\frac{P(D_i)\beta_i}{\sum_i P(D_i)\beta_i}\beta'_i}{\sum_i(\frac{P(D_i)\beta_i}{\sum_i P(D_i)\beta_i}\beta'_i)} = \frac{P(D_i)\beta_i\beta'_i}{\sum_i P(D_i)\beta_i\beta'_i}$.

Here you have adopted the experts' protocols in the order $P \overset{\beta}{\mapsto} P_\beta \overset{\beta'}{\mapsto} P_{\beta,\beta'}$, where β represents the pathologist's protocols and β' the radiologist's. But if you had adopted them in the other order, $P \overset{\beta'}{\mapsto} P_{\beta'} \overset{\beta}{\mapsto} P_{\beta'\beta}$, first using the radiologist's β''s to update your $P(D_i)$'s to new values $P_{\beta'}(D_i) = \frac{P(D_i)\beta'_i}{\sum_i P(D_i)\beta'_i}$ and only then using the pathologist's β's to update the $P_{\beta'}(D_i)$'s to new values $P_{\beta',\beta}(D_i)$, the result would have been the same:

$$P_{\beta',\beta} = \frac{P(D_i)\beta_i\beta'_i}{\sum_i P(D_i)\beta_i\beta'_i}$$

Proof: In the previous paragraph, interchange β's and β''s.

Then $P_{\beta,\beta'} = P_{\beta',\beta}$. And, evidently, it would have been the same if you had updated in a single step, using the product protocols $(\beta \times \beta')_i =_{df} \beta_i\beta'_i$:

$$P \overset{\beta \times \beta'}{\longmapsto} P_{\beta\times\beta'} = P_{\beta'\times\beta}$$

2.4.3 Factor updating on different partitions is always commutative.

We prove this by comparing the results of updating $P(H)$ successively on crosscutting partitions $\{D_i\}$ (via factors δ_i) and $\{E_j\}$ (via factors ϵ_j), in different orders:

(1) $P \overset{\delta}{\longmapsto} P_\delta \overset{\epsilon}{\longmapsto} P_{\delta,\epsilon}$, (2) $P \overset{\epsilon}{\longmapsto} P_\epsilon \overset{\delta}{\longmapsto} P_{\epsilon,\delta}$.

We assume that the rigidity conditions hold (1) for the D_i's and (2) for the E_j's, so that by **2.3**(6) we have

(1a) $P_\delta(X) = \frac{\sum_i \delta_i P(X \wedge D_i)}{\sum_i \delta_i P(D_i)}$, (2a) $P_{\delta,\epsilon}(X) = \frac{\sum_j \epsilon_j P_\delta(X \wedge E_j)}{\sum_j \epsilon_j P_\delta(E_j)}$,

(1b) $P_\epsilon(X) = \frac{\sum_j \epsilon_j P(X \wedge E_j)}{\sum_j \epsilon_j P(E_j)}$, (2b) $P_{\epsilon,\delta}(X) = \frac{\sum_i \delta_i P_\epsilon(X \wedge D_i)}{\sum_i \delta_i P_\epsilon(D_i)}$.

By (2a) and two applications of (1a),

$$P_{\delta,\epsilon}(H) = \frac{\sum_j \epsilon_j \frac{\sum_i \delta_i P(H \wedge E_j \wedge D_i)}{\sum_i \delta_i P(D_i)}}{\sum_j \epsilon_j \frac{\sum_i \delta_i P(E_j \wedge D_i)}{\sum_i \delta_i P(D_i)}} = \frac{\sum_{i,j} \delta_i \epsilon_j P(H \wedge D_i \wedge E_j)}{\sum_{i,j} \delta_i \epsilon_j P(D_i \wedge E_j)}.$$

Similarly, by (2b) and two applications of (1b),

$$P_{\epsilon,\delta}(H) = \frac{\sum_i \delta_i \frac{\sum_j \epsilon_j P(H \wedge D_i \wedge E_j)}{\sum_j \epsilon_j P(E_j)}}{\sum_i \delta_i \frac{\sum_j \epsilon_j P(D_i \wedge E_j)}{\sum_j \epsilon_j P(E_j)}} = \frac{\sum_{i,j} \delta_i \epsilon_j P(H \wedge D_i \wedge E_j)}{\sum_{i,j} \delta_i \epsilon_j P(D_i \wedge E_j)}.$$

Then $P_{\delta,\epsilon}(H) = P_{\epsilon,\delta}(H)$: Factor updating on two partitions is always commutative.

This can be proved along the same lines for any finite number of partitions.

2.4.4 Skepticism

Neurath's protocols (sec. **1.4.5**) were imperfectly reliable unprobabilistic statements, in need of screening and revision. But the same goes for probability protocols, and factor protocols.

It is only in rather special circumstances that you would be well advised to simply adopt someone else's probability protocols as your own. The problem is that other observers' $new(D_i)$'s will be determined by their prior judgments as well as by the sensory inputs they see themselves as reporting. To adopt such protocols is to some extent to substitute other prior judgments for your own.

Factor protocols are meant to avoid that problem. In dividing the new odds by the old, the effects of the observer's prior judgments are presumably factored out, so that the result is something like a purely experiential report—a kind of Empiricism Lite. But still, factor protocols must be scrutinized, and sometimes modified or rejected.[48]

EXAMPLE: DOUBLE BILLING? When you adopt the pathologist's factor protocols 1,1,2 in sec. **2.4.1** you update your old probability for benign tumor to $new(D_3) = \frac{2}{3}$. Now suppose you ask for a second opinion, and get 1,1,2 again. A simple soul would update $\frac{2}{3}$ further, to $\frac{(6/9)(2)}{(1/9)1+(2/9)1+(6/9)2} = \frac{4}{5}$. But

[48]Daniel Garber, "Field and Jeffrey Conditionalization", *Philosophy of Science* **47**(1980)142-5.

(a) if you take the second 1,1,2 to represent the independent opinion of a second pathologist you might not update beyond $\frac{2}{3}$, for you might take the second opinion to merely endorse the first. (Together, they say: '1,2,2 *a fortiori.*') Alternatively,

(b) the neatness and identity of the two reports might lead you to think that no observations had been made, and reject both reports as factitious.

Moral: Ultimately, it is your own protocols that you update on. ('Though we sit in the seats of the mighty, we must sit on our own behind."[49]) You need not adopt expert protocols. You may well modify them in the light of your own judgment:

(a) old $\overset{\beta}{\longmapsto}$ $new \overset{1}{\longmapsto} new$ (b) $old \overset{\beta}{\longmapsto} new \overset{\beta^{-1}}{\longmapsto} old$

And this is typical: For the most part, you will factor-update twice on the same partition only (a) trivially, or (b) to undo a mistake. The reason is that even when the protocols come from observers with different sorts of expertese—say, histopathology and diagnostic radiology—you will see them as positively correlated to the extent that you see them as reliable.

2.4.5 Factor updating on independent partitions

On the other hand, if you accept exogenous sets of factor protocols on crosscutting partitions that you see as independent, you may well update successively on both.

EXAMPLE. The two partitions contain diagnoses D_i regarding cancer and E_j regarding heart disease for your patient. You receive Bayes factors δ_i for the D_i's from an oncologist (o), and Bayes factors ϵ_j for the E_j's from a cardiologist (c):

$$\tfrac{new_o(D_i)}{new_o(D_1)} \Big/ \tfrac{old_o(D_i)}{old_o(D_1)} = \delta_i, \qquad \tfrac{new_c(E_j)}{new_c(E_1)} \Big/ \tfrac{old_c(E_j)}{old_c(E_1)} = \epsilon_j.$$

You are a clinician, wishing to provide your patient with informed probability judgments for s-year survival (H_s) for $s = 1, 5$, etc. in the light of these experts' opinions and your own prior probability assignment, *old*. If you initially judge the two diagnostic partitions to be independent, you may well choose to adopt both sets of Bayes factors, in either order—say, δ, ϵ:

$$old(H_5) \overset{\delta}{\longmapsto} old_\delta(H_5) \overset{\epsilon}{\longmapsto} old_{\beta,\beta'}(H_5). \tag{1}$$

Note the rôle played by your original judgment of independence, $old(D_i \wedge E_j) = old(D_i)old(E_j)$. In updating by probability protocols, it is this substantive judgment that guarantees commutativity. But here, where updating is by factor protocols, the rôle of your judgment of independence is to assure you that their automatic commutativity is indeed what you want: See (a) and (b) below.

[49] 'Et au plus élevé trône du monde, si ne sommes assis que sur notre cul.' (Montaigne, 'On Experience')

(a) Updating on Bayes factors δ_i, ϵ_j as in (1) has the same effect as updating on probabilities d_i, e_j as follows,

$$old(H_5) \overset{d}{\longmapsto} old_\delta(H_5) \overset{e}{\longmapsto} old_{\delta,\epsilon}(H_5), \tag{2}$$

where, by **2.3**(5), $d_i = \frac{old(D_i)\delta_i}{\sum_i old(D_i)\delta_i}$ and $e_j = \frac{old_\delta(E_j)\epsilon_j}{\sum_j old_\delta(E_j)\epsilon_j}$. Now by **2.3**(6), $\epsilon_j = \frac{\sum_i \delta_i old(E_j \wedge D_i)}{\sum_i \delta_i old(D_i)}$. Given your judgment that the D's and E's are independent, this $= \frac{\sum_i \delta_i old(E_j) old(D_i)}{\sum_i \delta_i old(D_i)} = old(E_j)$, so that the probabilities d, e in (2) are

$$d_i = \frac{old(D_i)\delta_i}{\sum_i old(D_i)\delta_i},\ e_j = \frac{old(E_j)\epsilon_j}{\sum_j old(E_j)\epsilon_j} \tag{3}$$

(b) If we factor-update in the other order,

$$old(H_5) \overset{\epsilon}{\longmapsto} old_\epsilon(H_5) \overset{\delta}{\longmapsto} old_{\epsilon,\delta}(H_5), \tag{4}$$

the probability-updates that yield the same results are of form

$$old(H_5) \overset{e'}{\longmapsto} old_\epsilon(H_5) \overset{d'}{\longmapsto} old_{\epsilon,\delta}(H_5), \tag{5}$$

where by **2.3**(5), $e'_j = \frac{old(E_j)\delta_j}{\sum_j old(E_j)\delta_j} = e_j$ and $d'_i = \frac{old_\epsilon(D_i)\delta_i}{\sum_i old_\epsilon(D_i)\delta_i}$, which, by **2.3**(6) and your judgment of independence, $= d_i$.

Then your judgment $old(D_i \wedge E_j) = old(D_i)old(E_j)$ ensures that the automatic commutativity in updating by factor assignments δ, ϵ to partitions D, E corresponds to updating by probability assignments d, e to those same partitions—probability assignments whose commutativity is not an automatic result of the formalism, but reflects a substantial judgment on your part.

6 Appendix: Notes on Heavy Radical Probabilism

2.6.1 Beyond Generalized Conditioning

Where the invariance conditions are not satisfied by your upcoming observation, generalized conditioning is no way to go. But there may be other moves you can make:

(1) *The Problem of New Explanation* ("Old Evidence."[50]). Here, seemingly, $P(H|evidence) = P(H)$. Example: Einstein's discovery that the already well

[50]Clark Glymour, *Theory and Evidence* (1980), chapter 3.

known anomalous advance in the perihelion of Mercury was explicable by the GTR. A sometimes applicable solution is "probability reparation."[51]

(2) *Temporal Coherence,*[52] *Reflection,*[53] *Condition M.*[54] These say: "Current probability judgments are current expectations of future probability judgments." This condition holds wherever invariance holds for an observable partition.

(3) *Beyond Temporal Coherence: Expected Irrationality.* Arriving at the party, you give your car keys to a friend to hold until you are home—because you expect that later in the evening it will be your drunken wish to drive home. This wise choice violates (2) above. (So does probability reparation.)

2.6.2 Gappy Probabilities; Fine Points for Mavens

It's OK to have gappy probability assignments—e.g., because...

(1) In updating $old \longmapsto new$ by probability kinematics, you make no use of the prior $old(D_i)$'s, which can therefore be undefined.

(2) In evaluating your desirability for an act, i.e., your conditional expectation of utility given the act, you make no use of the prior probability of the act—which is best left undefined.

Spohn[55] and Levi are troubled by this.[56] Thus, Spohn observes that

$$pr(A) = \frac{pr(B) - pr(B|\bar{A})}{pr(B|A) - pr(B|\bar{A})} \text{ if } pr(B|A) \neq pr(B|\bar{A}),$$

so that if B (a state of nature) is not *pr*-independent of A (an act), and $pr(B|A)$ and $pr(B|\bar{A})$ are both defined, then $pr(B)$ must be undefined if $pr(A)$ is. They seem to view this as a *reductio ad absurdum* of the idea that you can intelligibly have subjective probabilities for propositions you see as within your power to make true or false.

[51] For an elementary exposition and further references, see my *Subjective Probability,* sec. **2.10, 2.11**(8), on http:\\www.princeton.edu \~ bayesway\. See also Carl Wagner, "Old evidence and new explanation III" (forthcoming), which reviews and extends his "Old evidence and new explanation I" and "...II" in *Philosophy of Science* **64** (1997) 677-691 and **66** (1999) 283-288.

[52] Michael Goldstein: *The Prevision of a Prevision JASA* **78** (1983) 231-248. "Prior Inferences for Posterior Judgments", *Structures and Norms in Science*, ed. Maria Luisa dalla Chiara *et al.* (1997) 55-72.

[53] Bas van Fraassen: "Belief and the Will", *J. Phil.* **81** (1984) 235-256.

[54] Brian Skyrms, *The Dynamics of Rational Deliberation* (Harvard, 1990).

[55] Wolfgang Spohn, 'Where Luce and Krantz do Really Generalize Savage's Decision Model', *Erkenntnis* **11** (1977) 114-116.

[56] Isaac Levi, *The Covenant of Reason* (1997) 73-83.

(2) Your judgment $pr(A \wedge B) = pr(A)pr(B)$ of independence makes sense even when you have no P values in mind.

(3) At an early stage of deliberation, old and new probabilities may be "entangled" in the sense that you take the invariance conditions $new(H|D_i) = old(H|D_i)$ to hold for all H and i, but have not yet set numerical values $new(H|D_i) = old(H|D_i) = c_i$. Setting them disentangles *new* and *old*, for you then have separate conditions, $old(H|D_i) = c_i$ and $new(H|D_i) = c_i$, from which the invariance conditions follow.

2.6.3 Mad-Dog Subjectivism[57]

(1) There are no "real" probabilities out there for us to track,

(2) nor are there uniform probability-makers,[58]

(3) but there can be averages or symmetries out there, in view of which we find certain judgmental probability assignments irresistible,

(4) as happens when we use probabilistic theories—notably, quantum mechanics. The probabilities it provides are "subjective" for us, i.e., we adopt them as our judgmental probabilities, but they are objective in the sense (3) of being shared and compelling.

[57] This is pure de Finetti, going back to his "Probabilismo" (1931). For more about it, with further references, see Bas van Fraassen, *Laws and Symmetries* (1989), e.g., pp. 198-199 regarding (4).

[58] E.g., relative frequencies to date do not reliably deliver acceptable probabilities (think: Grue); nor will your dogmatic Protokollsätze to date serve as a condition on some Laplace/Carnap ur-prior that yields acceptable judgmental probabilities for you.