Resposta aos Quacks:

O Princípio de Incerteza de Heisenberg

MINISTÉRIO DOS LOBBIES E DA CULTURA INCULTA

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A leitura da minha recensão crítica de *A Fórmula de Deus* causou algum espanto quanto à minha gozação (ainda hoje me rio!) acerca da interpretação que José Rodrigues dos Santos (o nosso Tintim) deu ao Princípio de Incerteza de Heisenberg. Alguns leitores (que gostaram do meu texto) sentiram-se incomodados (e alguns... são académicos): *então, a incerteza não advém da interacção entre o aparato experimental (o célebre microscópio de Heisenberg) e o objecto observado*?

NÃO! O Princípio de Incerteza muito mais profundo do que um mero *entanglement* entre o aparato experimental e o sistema observado.

Para mostrar que o assunto é bem estudado, deixo uma leitura de um livro sobre fundamentos da física, um clássico entre muitos outros clássicos. Não posso explicar-lhes em poucas palavras a natureza estocástica dos fundamentos da Mecânica Quântica (MQ), mas deixo-os acompanhados de uma deliciosa leitura de Mario Augusto Bunge.

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LEITURA de Mario Bunge, *Foundations of Physics*, Springer Tracts in Natural Philosophy, Volume 10, 1967, pp. 256–257, 264, 265–268.

CONTEXTO Thm.3 e Thm.9 que estabelecem as relações de incerteza de Heisenberg. [1]

Thm.9: *spreads of canonically conjugate variables.* For every $\langle \sigma, \overline{\sigma} \rangle \in \Sigma \times \overline{\Sigma}$, every $\psi \in \mathcal{H}$ that solves Schrödinger equation for $\sigma + \overline{\sigma}$, and every trio $\mathcal{A}, \mathcal{B}, \mathcal{C} \in \{\mho\}$ such that $\mathcal{A} \triangle A$, $\mathcal{B} \triangle B$, $\mathcal{C} \triangle C$, with $A, B, C \in \{Q\}$ and $\mathcal{AB} - \mathcal{BA} = i\mathcal{C}$, the standard deviations of the A and B distributions in the state ψ are related by

$$\Delta_\psi \mathcal{A} \cdot \Delta_\psi \mathcal{B} \geq \frac{1}{2} |\langle \mathcal{C} \rangle_\psi| \ .$$

Proof. By the corollary of Thm.2 and the Schwarz inequality.

[1] Σ and $\overline{\Sigma}$ are nonempty denumerable sets. Every $\overline{\sigma} \in \overline{\Sigma}$ is the environment of some $\sigma \in \Sigma$. For every ordered pair $\langle \sigma, \overline{\sigma} \rangle \in \Sigma \times \overline{\Sigma}$ there exists a Hilbert space $\mathcal{H}$ associated with $\langle \sigma, \overline{\sigma} \rangle \in \Sigma \times \overline{\Sigma}$. $\{Q\}$ is a nonempty family of functions on Σ. $\{\mho\}$ is a nonempty family of operators in $\mathcal{H}$.

(1) This theorem, a generalization of HEISENBERG'S "principle", places a lower bound on the scatters of canonically conjugate properties. An equality can in principle be obtained upon specifying $\mathcal{A}$, $\mathcal{B}$, $\mathcal{C}$, and ψ. **(2)** Once again, nothing has been assumed about measuring devices. Consequently the preceding theorem is supposed to be satisfied by every $\langle\sigma, \overline{\sigma}\rangle$, whether or not $\overline{\sigma}$ happens to idealize an apparatus, and even when there is no $\overline{\sigma}$ — e.g., for a free electron. How could we specify anything about the experimental set-up if we have not even specified the properties A and B? **(3)** The usual interpretation of the random fluctuations as uncontrollable disturbances caused by the apparatus is *ad hoc* since (a) no apparatus variable occurs in the theorem (not even when deduced in the orthodox textbooks) and (b) in general there exists no joint probability distribution for pairs of noncommuting variables, whence no conclusion about the spreads of measurement results can be drawn even applying [the above theorem] to an experimental situation (SUPPES, 1961). **(4)** The widespread belief that the scatter relations ([the above theorem]) can be inferred from an analysis of measurements and then the corresponding commutation relations deduced from scatter relations (HEISENBERG, 1927) is plainly false: (a) Thm.3 is a universal statement involving unobservables, and no such statement can be gotten by induction from the observed instances; (b) every analysis of a measurement procedure is made in some context or other (see Ch.1, 5.1.4), and if QM is not used to this end then some classical theory will be employed (as is usual in HEISENBERG and BOHR'S analyses of gedankenexperiments) — but then no quantal relations will come out; (c) if the scatters of $\mathcal{A}$ and $\mathcal{B}$ satisfy ([the above theorem]), then its commutator can be any of the infinitely many relations $[\mathcal{A}, \mathcal{B}] = i(\mathcal{C} + \mathcal{D})$ with an arbitrary $\mathcal{D}$ subject to the sole condition $\langle\mathcal{D}\rangle = 0$. **(5)** Similar relations occur in CEM and other field theories, also independently of considerations on measurements. This is one more reason for not interpreting them as either indeterminacy or uncertainty relations caused by either The Observer's ignorance or His experimental activity.

Lemma 1: *basic commutation relations.* If $\mathcal{P} \equiv (\hbar/i)\nabla$, then

$$x_i \mathcal{P}_j - \mathcal{P}_j x_i = i\hbar\delta_{ij} \ .$$

Thm.9: *Heisenberg relations.* For every $\langle\sigma, \overline{\sigma}\rangle \in \Sigma \times \overline{\Sigma}$ in a state $\psi \in \mathcal{H}$, the scatters in the position and momentum distribution at any given $t \in T$ are reciprocal:

$$\Delta_\psi x \cdot \Delta_\psi \mathcal{P} \geq \hbar/2 \ .$$

Proof. By axiom *QM 6*, Thm.3 and Lemma 1.

Remarks. **(1)** Since this is a very special theorem, to start philosophical discussions with it is misleading — as much as calling it a "principle". **(2)** Read in terms of particle mechanics, these relations mean that the dynamical state of a system — another classical concept — is not sharply determined since the better the position of a *particle* is "defined" the worst its momentum is "defined". No such indeterministic interpretation is possible in our version of QM because the concept of classical particle is alien to it. **(3)** Similar relations occur in classical field theories and for the same formal reason — namely that the two distributions are related by a Fourier transformation. [...] **(4)** Thm.9 is sometimes "deduced" by reasoning on ideal measurements, e.g., by means of HEISENBERG'S microscope. It is even claimed that the statement was originally inferred from a detailed analysis of measurement procedures. But no such deduction is possible, for reasons given in Remark 4 to Thm.3 and because (a) experimental arrangements are classical describable (as BOHR himself has untiringly emphasized), (b) the HEISENBERG relations happen to be probability statements, and (c) the theoretical spreads require the knowledge of the unobservable ψ. What happens is (a) that, by interpreting x and $\mathcal{P}$ as *classical* variables referring to a point particle, relations *similar* to HEISENBERG'S can be obtained for some examples (but then $\hbar$ is missed); (b) by interpreting ψ as a *classical* wave field, relations *similar* to HEISENBERG'S can be obtained (see Remark 3 above); (c) classical gedankenexperiments can always be imagined to illustrate [the inequation above] and other relations, which is no wonder as they are tailored to that task but not every actual calculation for such idealized situations happens to confirm HEISENBERG'S relation (BECK and NUSSENZVEIG, 1958); [...] **(5)** The Heisenberg relations pass for being an illustration of the *wave-particle duality*, which would in turn be a case of the *complementary principle.* There is no such duality in our version of QM, because it contains neither the concept of wave nor that of particle. Being a stochastic theory, it is only natural that spreads should occur in it alongside averages. And Thm.9 states only that, the narrower the x-distribution, the broader the p-distribution and conversely: this has nothing to do with a complementarity between experimental set-ups (or alternatively modes of description). So much the better, because the complementarity principle is subject to grave philosophical difficulties (BUNGE, 1955b). [...] A frequent reading of HEISENBERG'S relations is this: "One can never know exactly the simultaneous values of the position and the momentum of a system". This presupposes that a quanton has a definite momentum at any time, only for some reason (incompleteness of the theory, or else disturbance by a measuring device) we cannot get to know them accurately. This is not what our version of Thm.9 states: it speaks not about human knowledge and ignorance but about a chunk of reality that has no simultaneous sharp position and momentum, for the excellent reason that it has neither a position nor a momentum *tout court* but position and momentum distributions (by QM 6). For having such distributions it has the *possibility* of going exceptionally either to a sharply localized state or to a state of sharp momentum. But in general it will be in a state in between which, from a classical viewpoint, is an intolerable wishy-washy situation. **(8)** That *measurements* of position and momentum give no simultaneous sharp values follows from the preceding in conjunction with the truism "What does not exist cannot be measured". [...]

[...]

And now to a *pseudotheorem*. It is usually claimed that the relation similar to HEISENBERG'S holds for energy and time, namely

$$\Delta_\psi t \cdot \Delta_\psi E \geq \hbar/2 \ .$$

It could be that, properly interpreted, this formula were true. But it does not belong to QM; in particular, it cannot be proved along the lines of HEISENBERG'S relations. The reasons for this are: (a) t is not an operator in Hilbert space, hence it exhibits no scatter; (b) although $i\hbar\partial/\partial t$ is sometimes said to be the energy operator, only H plays this role, and for a stationary state H has no spreads either (indeed, $\langle H\rangle = E$ and $\langle H^2\rangle = E^2$), whereas [the above formula] is alleged to be completely general. So far there are only heuristic reasons for the so-called Heisenberg 4th relation (actually proposed by N. BOHR). But in any case it cannot be interpreted similarly to HEISENBERG'S relations if only because 'Δt' cannot be a standard deviation as long as t is a "c-number". If one wants to have [the above formula] one must modify QM by introducing a suitable time operator — and then destroy the equivalence between the Schrödinger and the Heisenberg "pictures", as in the latter the dynamical variables evolve *in* time. Contrary to a widespread belief, the same holds for relativistic QM: in this theory, too, t is a classical variable.