New Perspectives in Physics. LOUIS DE BROGLIE, translated by A. J. Pomerans. Pp. 291, Basic Books, Inc., New York, 1962. Price $6.00.

The dust jacket of this book tells us that (italics mine): "A *new* book by Louis de Broglie is bound to be an exciting and pleasurable event . . .. This *new* book of de Broglie is, in some ways, perhaps his most interesting. For in it the founder of wave mechanics, *now* 69, . . ." The title page lists only the date 1962. The fact is that this book is nothing but a translation of an old book, which this reviewer read six years ago, and whose existence is nowhere mentioned. The practice of false labeling, long a problem in the food and drug industry, has thus come to the publishing industry.

Part I (six essays on various topics in physics) is all dated and partly obsolete. Part II (five essays on the interpretation of quantum theory) is still of such interest and importance that all physicists should be aware of it. In two fine sentences, de Broglie states his main constructive point: "History shows clearly that the advances of science have always been frustrated by the tyrannical influences of certain preconceived notions which were turned into unassailable dogmas. For that reason alone, every serious scientist should periodically make a profound reexamination of his basic principles."

Many physicists, trying to do exactly that, have stated their belief that the present (Copenhagen) theory, in particular its claim that ψ represents a *complete* description of physical states, is not based on sound reasoning, and some have suggested possible modifications. This field admittedly has more than its share of crackpots; but when many scientists of unquestioned competence all raise this same point, it is idle to pretend that there is no real difficulty here.

de Broglie discusses an alternative possibility which he proposed in 1927, then abandoned in the face of criticism until 1951, when it was revived and greatly extended by Bohm. He starts by saying, and I think correctly, that if one admits the phenomenon of "reduction of the wave packet" to describe the process of measurement, then one must concede that the wavefunction ψ has only a subjective meaning. Heisenberg [Daedalus **87**, 100 (1958)] has recently stated the same view.

Now if ψ is only a subjective quantity, then according to all pre-Copenhagen standards of common sense, one should ask next, as de Broglie does, "What is the objective thing underlying it?" He suggests a possible answer in the "double solution," according to which besides ψ there is a wavefunction u with a singularity which represents the true (but unknown to us) position of the particle. This singularity is to move in the direction of the phase gradient of ψ, in such a way that knowledge of ψ enables one to make the usual probability statements.

The Copenhagen school of thought will object to this on the grounds that no meaningful distinction exists between what is subjective and what is objective. Indeed, *within the framework of the Copenhagen theory* no such distinction can possibly exist, because ψ already represents a complete description of the physical state; to ask, "What is the objective thing underlying it?" is to miss the point entirely.

We are faced here with a fundamental conflict of viewpoint. The following debate has been repeated, with minor modifications, hundreds of times:

A. "The subjective-objective distinction is the most primitive fact of our experience; to insist on both the reduction of the wave packet *and* completeness of ψ is to commit the most obvious self-contradiction."

B. "This situation is forced on us by fundamental limitations on the possibility of obtaining knowledge, expressed by the uncertainty principle. Because of it, the very ideas of 'subjective' and 'objective' must be regarded as metaphysical notions which have no place in science."

A. "You are saying, in effect, 'I can't measure it—therefore it doesn't exist.' This is no rational argument at all, but a form of mysticism that has no place in science."

B. "Let me try to explain once more. Since there is no conceivable way of obtaining more information about a system than is contained in the wavefunction ψ, any question about the 'objective' thing underlying it cannot be investigated by experiment. Far from being mysticism, it is the first principle of sound scientific reasoning that assertions which cannot be verified or refuted by experiment are empty statements. While they are not necessarily harmful, the point is that they cannot possibly be of any constructive use in science. By eliminating them from our discourse we avoid a danger of self-delusion and wasted effort. Your own statements show how very real that danger is."

A. "The fallacy of your argument is shown by many examples in the history of science. At the turn of the century, questions about properties of individual atoms could not be investigated by experiment, and there arose a school of 'energetics' led by Ostwald and Mach, who reasoned in exactly the way you are doing. Since the existing laws of mechanics and thermodynamics already accounted for everything that could be measured, the atom was held to be an unnecessary metaphysical notion. They did everything in their power to deprecate and discourage work in kinetic theory. Fortunately, they were not successful; Einstein saw that the theory made a new quantitative prediction which *could* be tested by experiments then possible. With Perrin's measurements on the Brownian motion, the school of energetics collapsed like the house of cards it was. There is an important lesson in this, which you have persistently refused to recognize; today you are committing exactly the same error of reasoning, and in the name of this mysticism you discourage all efforts in the direction which, historically, has always been the one which leads to progress. The verdict of history may place you beneath the persecutors of Galileo; you were in a position where you should have known better."

B. "I deny most emphatically and heatedly that I am trying to suppress free inquiry in *any* direction. You are free to pursue this line if you insist; I am convinced that your efforts will be wasted. On the other hand, you must understand that I cannot, in good conscience, encourage anyone to work in a field which I believe has no relevance to the real problems of science, or to give financial support to such work. Your historical allusions have no merit—the point which *you* persistently refuse to recognize is that, in the uncertainty principle, we are faced with an entirely new situation, which has no historical precedent."

They part company, each convinced that the other does not understand the situation at all, each convinced that the other is so blinded by a thoroughly discredited dogma that he is no longer interested in searching for the truth. And there the matter stands.

Is there any way out of this impasse? I think it is useless to argue whether the Copenhagen position is based on sound *reasoning*; there is no criterion, acceptable to both sides, which could settle this issue. The example of Ostwald and Mach suggests a better question: Is the reasoning of the Copenhagen school, whether good or bad, based on sound *facts*?

Now it is manifest that the uncertainty principle is not an experimental fact at all. It is an inference drawn from the assumed results of four or five thought-experiments, none of which has actually been performed. In its general form, found by H. P. Robertson, it asserts that if F and G are any two noncommuting observables, they cannot be measured simultaneously to greater accuracy than indicated by $\Delta F \Delta G \geq \frac{1}{2}|\langle[F,G]\rangle|$, where ΔF and ΔG are the rms deviations. This is a quantitative assertion, which can be checked by quantitative experiments; only technical difficulties have thus far prevented their actual performance.

Let us understand clearly what this implies. If anyone ever performs an experiment in which the observed rms fluctuations in repeated measurements are proved to give an uncertainty product less than this amount by even one percent, then the "hidden variables" *have* been observed, it is all over with the Copenhagen interpretation, and efforts like those of de Broglie and Bohm will become overnight the "hottest" area of theoretical physics. *The person*

who deprecates those efforts today in the name of the Copenhagen theory is claiming universal, absolute, and final validity for a principle which has never been subjected to experimental test.

The principle of parity conservation received many experimental checks before it was found that, while valid in many areas, it is not *universally* valid. If today the uncertainty principle had received a dozen experimental checks, it would be in about the same logical position as the parity law was before 1957.

de Broglie certainly cannot be accused of failing to appreciate the difficulties of his double solution theory; he gives such an exhaustive list of them as to make the existence of this book a little surprising. He rejects the closely related suggestions of Schrödinger and Einstein which, in this reviewer's opinion, offer a far more attractive, and probably the only, prospect of any return to the pre-Copenhagen picture of reality. We now know that Schrödinger's original theory, if it had been pursued further, could have given quantitatively correct accounts, by continuously operating causal mechanisms, of many facts which the Copenhagen school claims "cannot be understood in terms of classical concepts." The premature stifling of Schrödinger's theory may yet prove to have been a tragedy for science.

In any event, the work of de Broglie reported in this book, and more particularly that of Bohm reported elsewhere, have already accomplished one important thing in demolishing the claim that hidden variable theories were impossible *in principle*. Perhaps other claims of the Copenhagen school deserve the "profound re-examination" suggested by de Broglie. But they will not get it from those who have, in the past, jumped to the attack at any sign that the present theory is being questioned. Only fresh minds, free of dogmatic preconceptions, are capable of this. The responsibility rests on those of us who are now teaching quantum theory. If we, in textbooks or lectures, present the uncertainty principle as if it were an established fact not subject to further question, we may be perpetuating exactly the situation about which de Broglie warns us.

E. T. JAYNES
Washington University