LETTERS

Oppie's Colleagues Affirm His Leadership in Manhattan Project

As former members of the wartime Los Alamos laboratory, we were appalled by Lawrence Cranberg's letter (PHYSICS TODAY, September 1999, page 78), questioning J. Robert Oppenheimer's leadership.

Oppenheimer was a brilliant leader of Los Alamos. He had an unusually quick mind, understanding any new fact immediately and assimilating it in the overall picture of the project. At all times he was fully informed on all of the scientific developments, whether theoretical or experimental, in physics, chemistry, or metallurgy, that were relevant to the success of the project. He knew what was happening in the machine shops, and where Los Alamos was in terms of procuring whatever was needed. He was aware of both the latest successes and the most important unresolved questions. And he kept us all informed.

To keep the scientific staff current on the project's progress, Oppie established three levels of continuing communication. First was the governing board of about ten people who made the decisions on the scientific program. Second was the coordinating council of about 60 people, including group leaders and other senior scientists, where the participants reported their recent successes and ongoing problems. Often a person from a quite different part of the lab would make useful suggestions. And third, he established the general colloquium, open to about 300 people, including all the PhDs and a few others who were informed of the progress and prospects of the laboratory.

The result of this openness was that we all felt that we were part of the lab and that each of us was personally responsible for its success. The ability to foster this esprit, to get the very best from every member, is what makes a great leader of a large project, not the leader's individual contributions to the solution. Oppie made those as well.

But his greatest contribution was his insistence on this freedom of communication inside the laboratory. This was much against the wishes of General Leslie Groves, the overall project leader, who wanted information strictly compartmentalized. General Groves was a very difficult boss who was not very fond of scientists in general and Oppie in particular. Perhaps the best evidence that Oppie was, in fact, a very good leader of Los Alamos is that Groves kept him despite the difficulties in their personal and professional relationship.

Cranberg suggests that Los Alamos was merely needed to solve the engineering problems once the chain reaction was established. That is, in fact, what we believed when Los Alamos started work in March 1943. But it turned out not to be true. In the spring of 1944, one of the Los Alamos groups discovered that plutonium-240 has a strong tendency to fission spontaneously. This meant that a plutonium bomb would explode before it was fully assembled, and would then explode with only a small fraction of the design yield. This discovery was science, not engineering, and was not accidental. Oppie had established groups to investigate any phenomena that might prevent an atomic explosion. Spontaneous fission did raise a potential problem. Other groups did not find any troubles.

Because of this potential problem, we had to find a way to assemble the bomb very rapidly indeed. The way to do this was by implosion, which already had been suggested by Seth Neddermeyer in 1943. He had immediately been given a group to study it. Unfortunately, instead of assembling material, so far the group had only been able to shatter it.

A solution was offered by a British physicist, James Tuck, who had used explosive lenses to convert a divergent explosive wave to a plane wave. Oppenheimer immediately reorganized the laboratory. Famous physicists such as Luis Alvarez, Ed McMillan, and Bruno Rossi, and many less well-known scientists, were assigned to ensuring that implosion could yield a spherically symmetric assembly. And Oppie recruited the greatest scientific expert on explosives in America, George Kistiakowsky, to direct the work.

All of this is to answer positively Cranberg's statement "it is hard to say exactly what credit belongs to Oppenheimer."

Enrico Fermi was one of the great scientists of the 20th century. One of us, Hans Bethe, was Fermi's student for a year and has tried to follow his method of research ever since. Fermi and his small group achieved the first man-designed chain reaction in uranium on 2 December 1942. His German competitors were still far from this result in 1945. Before the war, Fermi and his group in Rome had made an exhaustive study of the action of neutrons on numerous nuclei, uncovering many principles that are now fundamental in nuclear physics. Fermi was the world master in inspiring small groups of ten or so scientists. He never wanted to lead a big laboratory.

Let Fermi and Oppenheimer each be remembered for their great achievements: Fermi as a great scientist, Oppenheimer as the leader of a great scientific laboratory.

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The Nitty Gritty on Compatible Families

The article by Robert Griffiths and Roland Omnès (PHYSICS TODAY, August 1999, page 26) is an attempt to provide an interpretation of quantum mechanics that eliminates the concept of measurement. It provides excellent reasons for getting rid of measurement. However, it also raises troubling questions.

As Griffiths and Omnès empha-

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size, the relation between probability and quantum mechanics is subtle. In the mathematical theory of probability, there is a given family of events. Each event has a probability. The probabilities satisfy the additivity property: For every pair of events A, B, the probability of the event A is the sum of the probability of the event (A and B) with the probability of the event (A and not B). The additivity property is necessary if the probabilities are to have a frequency interpretation.

There is a somewhat analogous structure in quantum mechanics, and it is natural to define a quantum event to be a projection operator. The quantum state assigns a probability to each quantum event. Commuting projection operators are compatible quantum events. The conjunction (A and B) of compatible quantum events is the operator product AB. The identity operator I that projects onto the entire Hilbert space corresponds to an event that is sure to happen. The negation (not B) is then $I - B$. For each family of compatible quantum events, the probabilities of the events in the family satisfy the additivity property. The consistent-histories theory deals with families of quantum events that need not be compatible. If the events in such a family satisfy a consistency condition relative to the quantum state, then again their probabilities obey the additivity property. Every compatible family of quantum events is a consistent-history family.

Suppose (as is usual in physics) that the physical meaning of probability is given by the frequency interpretation. As a precaution, however, consider that this interpretation may be relative to the consistent-history family. That is, given a consistent-history family, for each quantum event in the family there is a corresponding physical event to which the frequency interpretation applies.

In this spirit, consider the following premise concerning a system undergoing a certain physical process: The probabilities for each consistent-history family describe the frequencies at which physical events occur when the physical process occurs repeatedly. This premise is denied by an interpretation of quantum mechanics in which there are corresponding physical events only when a measurement is being performed on the system. In such interpretations, only the probabilities for quantum events in the consistent-history family selected for measurement describe the frequencies at which physical events occur. The premise could also be denied by an interpretation of quantum mechanics in which there are corresponding physical events only when a physicist chooses to reason about them. That would be selection by whim, rather than by measurement. However, it should hold for any interpretation in which there is no particular context that gives preference to one consistent-history family over another. The following argument shows that, under one additional and rather natural assumption, this premise leads to a contradiction.

The assumption links different compatible families: If one compatible family is contained in another compatible family, then the physical events in the smaller family occur precisely when the corresponding physical events in the larger family occur. One can imagine a theory in which this assumption is violated. For example, consider a system of two distinguishable spin $1/2$ particles. Say B is a quantum event associated with the first particle (a certain spin component is $1/2$), while C' is a quantum event associated with the other particle (some other spin component is $-1/2$). One can consider these spin components of the two particles together. Then the compatible family is generated by B, C'. Or one can single out the first particle and ignore the other. Then the compatible family is generated by B alone. It might happen in a particular realization that the physical event defined by (B and not C') for the two-particle family occurs, while the physical event defined by B for the one-particle family does not occur. Thus, without the assumption, the relation between quantum events (as mathematical objects) and physical events (to which the frequency interpretation applies) becomes complex.

Next, recall the system first introduced by John Bell (see the appendix of David Wick's book[1] for an elementary account). There are two distinguishable spin $1/2$ particles in a certain quantum state. There are quantum events A, B, C associated with the first particle (certain spin components have value $1/2$), and there are quantum events A', B', C' associated with the second particle (the corresponding spin components have value $-1/2$). Each quantum event associated with the first particle is compatible with each quantum event associated with the second particle. Each of A, B, C, A', B', C' have probability $1/2$. Also, each of (A and A'), (B and B'), (C and C') have probability $1/2$. (These probabilities imply that, with probability 1, the quantum events A, A' are equivalent, and similarly for the other corresponding pairs.) Finally, each of (A and not B'), (B and not C'), (C and not A') has probability $3/8$.

Imagine many repetitions of the physical situation. First, consider the family generated by B, C'. Consider a repetition in which the physical event defined by (B and not C') relative to this family occurs. In particular, the physical event relative to this family defined by B occurs. Next, consider the family generated by B alone. Then, by the assumption, the physical event defined by B for this family also occurs. In turn, consider the family generated by B, B'. Again, by the assumption, the physical event defined by B occurs. Hence, by the probability prediction, B' occurs. In the same way, consider the family generated by B' alone; again the assumption implies that the physical event defined by B' occurs. Finally, consider the family generated by A, B'. Again, by the assumption, the physical event defined by B' relative to this family occurs. In particular the physical event defined by (A and not B') relative to this family does not occur. The conclusion is that in no repetition is there a simultaneous occurrence of the physical event defined by (A and not B') and of the physical event defined by (B and not C'). In fact, there is never a simultaneous occurrence of two of the three physical events defined by (A and not B'), (B and not C'), (C and not A'). So the frequency of occurrence of at least one of these three physical events must be less than $3/8$. This conclusion contradicts the probability prediction of quantum mechanics.

Some proponents of the consistent-histories theory formulate "rules" of interpretation. Thus, Griffiths states, "A meaningful description of a (closed) quantum mechanical system, including its time development, must employ a single framework."[2] Similarly, Omnès says, "Every description of a physical system should be expressed in terms of properties belonging to a common consistent logic."[3] These rules are extraordinarily obscure. Apparently, different descriptions may use different consistent logics, but how are these descriptions related? The rules clearly limit the possibilities of description of a quantum system. Perhaps they could be invoked to claim that a multistage argument, each of whose individual stages is correct, is globally inadmissible. However, such a claim would cast more doubt on the rules than on the argument.

The rules remind us that there is no general notion of conjunction of quantum events. However, the argument presented above uses the conjunction of compatible quantum events, for which there is no problem. The argument does combine physical events, but only according to the following principle. Consider a sequence of repetitions of the physical situation. Suppose that, for each physical event, for each repetition there is a corresponding occurrence or nonoccurrence. Then, for each repetition, for each physical event there is a corresponding occurrence or nonoccurrence. In particular, for each repetition and each pair of physical events, there is or is not a simultaneous occurrence. This mathematical commonplace has nothing to do with quantum mechanics; it is inherent in the frequency interpretation of probability in any domain.

Nevertheless, it seems to be at the heart of the issue.[4]

References

1. D. Wick, *The Infamous Boundary: Seven Decades of Heresy in Quantum Physics*, Copernicus, Springer-Verlag, New York (1996).
2. R. B. Griffiths, Phys. Rev. A **54**, 2759 (1996).
3. R. Omnès, *The Interpretation of Quantum Mechanics*, Princeton U. P., Princeton, N. J. (1994).
4. R. B. Griffiths, Phys. Rev. A **57**, 1604 (1998); see p. 1615.

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Griffiths and Omnès reply: The issues raised in William Faris's letter require a technical response, and we apologize in advance to readers who may find it difficult to follow. Fortunately, we do not have to deal with consistency conditions; it will suffice to discuss probabilistic descriptions of a quantum system at a single instant of time.

Standard probability theory[1] requires a *sample space* of mutually exclusive possibilities or "points," an event algebra that, in the simplest case, is the collection of all subsets of points of the sample space, and a probability distribution (or measure). In the consistent-histories approach, the sample space of a quantum system is given by a decomposition of the identity operator: a collection of mutually orthogonal projectors (orthogonal projection operators) onto orthogonal subspaces of the quantum Hilbert space, which constitute a set of mutually exclusive properties of the quantum system. In the case of a spin-half particle, the two properties $S_w = 1/2$ and $S_w = -1/2$, where w is some direction in space, constitute a quantum sample space.

It is a characteristic of quantum physics that two sample spaces L and M can be mutually incompatible: There is no third sample space N whose event algebra includes all the projectors in both L and M. For example, the $S_x = \pm 1/2$ and $S_z = \pm 1/2$ sample spaces for a spin-half particle are incompatible. Incompatibility for quantum properties at one time arises only if some projector in L does not commute with some projector in M. Because the "operators" in classical mechanics commute with each other, there is no analog in classical physics of incompatible sample spaces, and there is never any difficulty in combining two probabilistic descriptions of the same classical system. As a consequence, physicists tend to get into the habit of talking about probabilities without paying attention to the precise nature of the sample space. But in quantum theory such carelessness leads to difficulties and paradoxes. For this reason, the consistent-histories approach contains a single-family or single-logic rule, which states (among other things) that a description of a quantum system at a single time, and the probabilistic reasoning that goes into constructing such a description, must be based on a single sample space. Since the corresponding rule is always satisfied when probability theory is used in classical physics, consistent-histories quantum theory represents a very conservative extension of ordinary probability theory into the quantum domain. In particular, once a sample space has been specified, all the apparatus of standard probability theory can be applied to the quantum case; and probabilities have their usual intuitive interpretation, in terms of ignorance or frequency or whatever one prefers. It is important to note that the sample space used in constructing a description is not determined by some law of nature. Instead, the choice is made on the basis of the physical question(s) one wants to address. A particular question can only be answered using a sample space in which it makes sense, and one can show that the (probabilistic) answer provided by quantum theory does not depend upon which sample space satisfying this criterion one uses. For further details on these matters, we refer the reader to our publications.[2]

Let us turn to the example considered in Faris's letter. It is the usual Einstein–Podolsky–Rosen situation as formulated by Bohm, with a pair of spin-half particles prepared initially in a singlet state. At some later time, A, B, and C are properties of the first particle in which the w component of spin angular momentum is positive, $S_w = +1/2$, for three choices of the direction w lying in a plane and separated from each other by 120°. Similarly, A', B', and C' are properties of the second particle in which the component of spin angular momentum is negative, $S_w = -1/2$ for the same three directions. If one thinks of these properties as projectors, A commutes with A', B', and C', and the same is true of B and C, since operators referring to one particle commute with operators referring to the other particle. On the other hand, A, B, and C do *not* commute with one another, and the same is true of A', B', and C'.

Faris constructs an argument whose conclusion is that there is never a simultaneous occurrence of two of the three events $X = (A$ and not $B')$, $Y = (B$ and not $C')$ and $Z = (C$ and not $A')$. But this conclusion makes no sense within consistent-histories quantum theory, because X, Y, and Z, regarded as projectors, do *not* commute with each other. Since they do not commute, there is no sample space that contains more than one of them, and talking about two of them occurring or not occurring simultaneously is meaningless. The place where Faris's argument goes astray, from a consistent-histories perspective, is at the very first point where he combines results involving noncommuting projectors. He starts by assuming (B and not C') and from this infers B. This is acceptable, since there is a sample space for the two spin system that contains (B and not C') as one of its elements, with B an element of the corresponding event algebra. However, the next step, the inference from

B to B', is problematic, because B' does not belong to the event algebra of the sample space used previously, as is obvious from the fact that it does not commute with C'. Consequently, either the step from B to B' is not allowed, or else one has to adopt a new sample space in which both B and B' make sense. But in the latter case it is necessary to abandon the earlier (B and not C'), as it cannot be a part of the new sample space. In either case, the argument cannot be completed. Chaining together arguments using mutually incompatible sample spaces is a common mistake in quantum reasoning, leading to a variety of quantum paradoxes. Readers may find it useful to consult reference 3 for detailed discussion of a similar example.

A possible way out of this conclusion might be the distinction that Faris makes in his letter, which is not very clear to us, between a quantum event and a physical event. He refers to X, Y, and Z as physical events, and it may be that Faris believes that one can sensibly speak of them occurring simultaneously despite the fact that the corresponding quantum projectors do not commute. One must certainly distinguish between physical events occurring in a laboratory and the mathematical objects, such as projectors, that represent them in the theorist's notebook. Still, insofar as quantum theory is a correct description of the world, it is unlikely that there are real events in the laboratory whose counterparts in the theory lack any meaning. To be sure, Faris has the right to develop his own theory using definitions and rules that are different from those we have developed for the consistent-histories approach. But then the contradiction that he has derived has to do with his own alternative proposal, and not with consistent-histories quantum theory as that has been defined up till now.

We do not think that the rules of consistent-histories quantum theory are at all obscure. Instead, confusion arises from importing classical ideas into quantum theory in a manner that is incompatible with the mathematics of Hilbert space. The consistent-histories rules, when they are taken seriously, prevent this sort of thing, and keep one from falling into the sort of contradiction that Faris is concerned about.

References

1. See, for example, W. Feller, *An Introduction to Probability Theory and Its Applications*, third edition (John Wiley & Sons, New York, 1968).
2. R. Omnès, *The Interpretation of Quantum Mechanics* (Princeton University Press, Princeton, 1994); *Understanding Quantum Mechanics* (Princeton University Press, Princeton, 1999). R. B. Griffiths, Phys. Rev. A **54**, 2759 (1996); **57**, 1604 (1998).
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Echegaray—Fiscal Scientist and More

This letter is in response to Lloyd Kannenberg's delightful article "Fiscal Physicists" (PHYSICS TODAY, December 1998, page 38; Letters, April 1999, page 15). I would like to add to Kannenberg's collection the name of the Spanish scientist José Echegaray Izaguirre (1832–1916), to whom the Bank of Spain dedicated the 1000-peseta banknote (approximately \$6) issued in 1971. The banknote, whose dimensions were 93 mm 153 mm, was in circulation until the beginning of the 1980s.

Although Echegaray may not have made any fundamental contribution to the advancement of physics worldwide, he played an essential role in the development of physics in Spain. Professor of mathematical physics at the University of Madrid, and now recognized as one of the best national mathematicians of the late 19th century, he introduced in Spain many of the ideas about physics and mathematics that were circulating in Europe. He also founded the Royal Spanish Society of Physics in 1903, and was its first president.

But his activities were not limited to this. Educated as a civil engineer, he was also an eminent economist and a supporter of free trade. His talent and knowledge enabled him to serve several terms as minister of finance; he was also elected to the House of Commons several times and later to the Senate. Echegaray improved the country's economy, and founded the Bank of Spain, which was—and is today—the national institution that oversees the economy and the national currency. The reverse of the banknote shows an illustration of the central building of the Bank of Spain, built while Echegaray was minister of finance.

Echegaray also was a writer; his works were an excellent expression of romanticism. In 1904 he was corecipient, with Frederic Mistral, of the Nobel Prize in Literature.

This extraordinary confluence of abilities would have been enough to gain him recognition, but his renown came at one of the most difficult times in Spain's history. In 1898 Spain had lost the war against the US and, as a consequence, had also lost the last of its former empire (Cuba, the Philippines, and smaller territories in the Pacific Ocean). These losses generated a feeling of frustration among the Spanish people, and the sense of being weaker than their neighboring European colonial powers. In this atmosphere, Echegaray became a focal point for Spanish nationalism.

I do not know of many cases like José Echegaray Izaguirre: outstanding mathematician, engineer, physicist, economist, politician, and writer. It would be nice if PHYSICS TODAY collected similar cases of physicists with expertise in such diverse intellectual pursuits.

In summary, Echegaray was a