QUANTUM PHASE AND QUANTUM PHASE OPERATORS: Some Physics and Some History

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Abstract

After reviewing the role of phase in quantum mechanics, I discuss, with the aid of a number of unpublished documents, the development of quantum phase operators in the 1960’s. Interwoven in the discussion are the critical physics questions of the field: Are there (unique) quantum phase operators and are there quantum systems which can determine their nature? I conclude with a critique of recent proposals which have shed new light on the problem.

1 Introduction

Quantum phase can be considered a main distinguishing feature between quantum and classical physics. All quantum interference phenomena are a priori dependent on it. Discrete eigenvalues can be viewed as a quantum phase condition for the Schrödinger equation.

At the core is the role of complex numbers in quantum mechanics. In situations such as classical electrodynamics, complex numbers can be a calculational tool, but are not a new and necessary aspect of the physics. In quantum mechanics, however, the role of complex numbers turned out to be necessary and crucial, even though this was not at first realized. In fact, when Schrödinger discovered the “coherent states” [1], he thought that the physics was only contained in the real part of his wave solutions. It was only later, when the interpretation of the wave function as a probability amplitude was understood, that it was realized that the complex phase has physical information.

However, observe that no experiment ever measures an imaginary number, and hence a “phase.” Rather, an imaginary number in the theory is interpreted in terms of a real physical process. The measured values in an experiment can be given in

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terms of phase, although the numbers themselves are in the form of a sine or a cosine of that phase. This point should be kept in mind in what follows.

The importance of quantum phase also is evident in gauge transformations and gauge theories. Weyl’s original idea was an exponential scale transformation. However, the observation, that by making the electromagnetic scale transformation imaginary one would obtain the Planck-Bohr quantization condition, led to the concept of gauge invariance in quantum mechanics. The full flowering of these ideas is in the modern, non-Abelian gauge theories of particle physics, but the concept also includes such quantum-mechanical marvels as the Aharanov-Bohm effect. The reader is directed to the works by Yang [2] and Dresden [3] for reviews of this physics and its history.

The idea of an explicit phase operator for quantum mechanics arose early in its development [4]. When the commutation relation ($\hbar = c = 1$)

$$[x, p] = i$$

is transformed to the second-quantized boson formalism, with

$$x = \frac{1}{\sqrt{2}}[a + a^\dagger], \quad p = \frac{1}{\sqrt{2i}}[a - a^\dagger],$$

it is natural to ask if a phase operator can be defined by the relationships

$$N = a^\dagger a, \quad a = \exp[i\phi_{op}]\sqrt{N},$$

where $N$ is the number operator. Dirac proposed this idea [4]. However, the associated $\phi_{op}$ is not Hermitian since

$$U = \exp(i\phi_{op}) = aN^{-1/2}$$

is not unitary:

$$UNU^\dagger = N + 1.$$  

In fact, even as Dirac [4] was proposing the existence of a phase operator, London, in two papers that were for the most part forgotten [5, 6], was observing that the operator $U$ is nonunitary. London pointed out that the matrix representations for $U$ and $U^{-1}$ were

$$U^{-1} = \begin{bmatrix} 0 & 0 & 0 & 0 & \ldots \\ 1 & 0 & 0 & 0 & \ldots \\ 0 & 1 & 0 & 0 & \ldots \\ 0 & 0 & 1 & 0 & \ldots \\ \vdots & \vdots & \vdots & \vdots & \ddots \end{bmatrix}, \quad U = \begin{bmatrix} 0 & 1 & 0 & 0 & \ldots \\ 0 & 0 & 1 & 0 & \ldots \\ 0 & 0 & 0 & 1 & \ldots \\ 0 & 0 & 0 & 0 & \ldots \\ \vdots & \vdots & \vdots & \vdots & \ddots \end{bmatrix}. \tag{6}$$

This means that

$$UU^{-1} = 1,$$  

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but
\[ U^{-1}U \neq 1 \]. \hspace{1cm} (8)
(Both Dirac and London used the opposite sign convention to ours.) In the first [7] and second [8] editions of his quantum mechanics text book, Dirac repeated London’s matrix argument and took it further. Dirac observed that the above operators yield the new commutation relation
\[ [\exp(i\phi_{op}), N] = \exp(i\phi_{op}), \] \hspace{1cm} (9)
or
\[ [N, \phi_{op}] = i. \] \hspace{1cm} (10)
This implies the associated uncertainty relation [10]
\[ \Delta N \Delta \phi \geq 1/2. \] \hspace{1cm} (11)
Observe that Eq. (11) is nonsense when \( \Delta N \) becomes so small that \( \Delta \phi \) must be greater than \( 2\pi \).

Even though Dirac dropped the phase operator discussion in the third edition of his text book [9], the argument of Eqs. (9)-(11) was picked up by Heitler [11, 12]. This led to the problem of Carruthers that we discuss below.

An ultimate explanation for the nonunitarity of \( \phi_{op} \) is that the number-state matrix representation of the diagonal number operator is bounded from below. The formal “resolution” of this problem is that, even though one cannot define a Hermitian phase operator on the harmonic oscillator Hilbert space, one can define Hermitian sine (S) and cosine (C) operators, whose bounded spectra yield meaningful uncertainty relations.

In the next three Sections, as I discuss the physics involved, I will reveal, from a personal viewpoint, some of the little-known history behind the discovery of the resolution. I will also emphasize that one aspect of the formal resolution which has troubled a number of people are the facts that the S and C operators do not commute and that the sum of their squares is not unity. This has partially inspired two new schools, that of Pegg and Barnett (PB) and that of Noh, Fourgères, and Mandel (NFM). In Sections 5 and 6 I will review the work of these schools, and how I view it in the overall context of the problem. I will close, with a discussion, in Sec. 7.

2 The discovery of quantum Sin and Cos operators

The blossoming of the “modern” era of this problem, is really due to Peter Carruthers. In the fall semester of 1962 (Sept. 1962 to Jan. 1993), Carruthers was giving a graduate course in advanced quantum mechanics (Physics 651) at Cornell University. Carruthers was aware of the Heitler discussion of Eqs. (9)-(11) [12], but because of the \( \Delta \phi \geq 2\pi \) argument given above, he did not believe it. In the best tradition of
Professors, Carruthers decided that a way to solve the problem might be to give it as part of a homework assignment. Then some student might solve it, not knowing how difficult the problem was. As the third problem in his third problem set, given out before Christmas [13], Carruthers produced the following [14]:

3. Phase Operator Suppose one defines the Hermitian operators $\phi_k$ and $\sqrt{N_k}$ by

$$a_k = \exp(-i\phi_k\sqrt{N_k}), \quad a_k^\dagger = \sqrt{N_k}\exp(i\phi_k).$$  \hspace{1cm} (1)

From the known commutation rules for the $a$’s find the value of $[N_k, \phi_k]$. Show that any photon state has

$$\Delta N_k \Delta \phi_k \geq 1$$  \hspace{1cm} (2)

Use the result and the defining (sic) equations (1) in the expansion of the vector potential, to discuss the classical limit.

Apparently no one has investigated whether the quantities $\phi_k$ and $\sqrt{N_k}$ “defined” by Eq. (1) really exist. A bonus will be given for an answer to this question.

For the record, the bonus turned out to be a single Budweiser Beer. (In Pete’s defense, he was not as rich then as he is now.)

Three students who were attending the course, Jonathan Glogower, Jack Sarfatt, and Leonard Susskind, started talking and/or working on the problem together. How much of each is a matter of disagreement, as we will see. Sarfatt later declared that at that time he was already interested in the problem [15]. Sarfatt also wrote a paper for Nuovo Cimento, received on 21 Sept. 1962 with a publication date of 1 March 1963 [16], in which he stated the problem. What is most interesting is that in this paper there is a Note added in proof, which reads [16]

Reference has been made to the difficulty in the definition of the phase operator in quantum mechanics. Recent work by L. Susskind, J. Glogower and J. Sarfatt shows that it is impossible to define a phase operator because of the existence of a lowest state for the number operator of the oscillator. Thus, the uncertainty relation $\Delta n \Delta \phi \geq 1$ is meaningless. . . .

Be this as it may, one year later Susskind and Glogower (SG) submitted a paper (received 13 May 1964) to a new and short-lived journal, Physics. The article was published in the first issue [17], and contained the solution we will come to below.

My own involvement started just at this time because I was looking for a new advisor. Hans Bethe had told me, “I do not feel qualified to be your advisor.” (Isn’t that a great line to be able to use?) Although Bethe really did use those words, the
meaning, of course, was not what seemingly might be implied. I had told Bethe that I wanted to do a thesis in particle theory. After my declaration, Bethe came to the conclusion that he did not have enough time to devote to both particle physics and also nuclear matter, and he had chosen to concentrate his research in nuclear matter.

Anyway, I decided to ask Pete and Pete decided to chase me off by giving me a problem. When I came back a couple of days later with what became Sec. IV of the first manuscript we wrote [18], Pete was stuck and I was set. Our work on this problem culminated in our 1968 review [19]. I refer the reader to this article for more details on the field up to that time.

Now, let us discuss the SG resolution.

To begin, SG rediscovered London’s result that $U$ is not unitary. [Compare London’s matrix representations, of what we call $N$ and $U$, in Eqs. (20) and (21) of his Ref. [6] with SG’s representations in their Eqs. (6) and (11) of Ref. [17].] SG then obtained their $C$ and $S$ operators. An intuitive way to do this is to be guided by the classical equations of motion [19]. One seeks solutions to the quantum equations

$$\dot{C} = (1/i)[C, H] = \omega S, \quad \dot{S} = (1/i)[S, H] = -\omega C,$$

or

$$[C, N] = iS, \quad [S, N] = -iC. \quad \quad (13)$$

Solutions are

$$C = \frac{1}{2} \left[ \frac{1}{(N+1)^{1/2}} a + \frac{1}{(N+1)^{1/2}} a^\dagger \right], \quad \quad (14)$$

$$S = \frac{1}{2i} \left[ \frac{1}{(N+1)^{1/2}} a - \frac{1}{(N+1)^{1/2}} a^\dagger \right]. \quad \quad (15)$$

However, $C$ and $S$ do not commute:

$$[C, S] = \frac{i}{2} P^0, \quad \quad (16)$$

where $P^0$ is the projection onto the ground state. Furthermore, the sums of their squares is not unity:

$$C^2 + S^2 = 1 - \frac{1}{2} P^0. \quad \quad (17)$$

The next step is to define phase-difference operators, since it is phase differences that are measured in quantum mechanics, not absolute phases. The phase-difference operators are what one would expect from classical trigonometry:

$$C_{12} = C_1 C_2 + S_1 S_2, \quad S_{12} = S_1 C_2 - S_2 C_1. \quad \quad (18)$$

As before, the phase-difference operators also do not commute among themselves,

$$[C_{12}, S_{12}] = \frac{i}{2} [P^0_1 - P^0_2]. \quad \quad (19)$$
even though they both commute with the total number operator:

\[ [C, N_1 + N_2] = [S, N_1 + N_2] = 0. \]  

(20)

Furthermore, once again the sum of the squares of the phase operators is not unity:

\[ C_{12}^2 + S_{12}^2 = 1 - \frac{1}{2} [\mathcal{P}_1^0 + \mathcal{P}_2^0]. \]  

(21)

Consult Refs. [17, 19] for further details.

After Carruthers’ and my review appeared [19], Sarfatt wrote the letter referred to earlier [15]. He wanted historical corrections to be published giving him credit. In the “Historical corrections” of his communication, given as our Ref. [15], he wrote:

\[ \ldots \text{In turn I ran across the fact that a Hermitian phase operator could not be defined in informal discussions with the late Dr. David Falcoff and some of his students at Brandeis during 1961-1962. Glogower was responsible for the ingenious mathematical solution of certain recursion relations, and Susskind did the bulk of the work on the proper form of the commutation relations for the C and S operators as well as the appropriate eigenfunctions. There is no question that Susskind completed the greater part of the final work on his own, but there is equally no question that the paper never would have been written were it not for my participation in the crucial initial stages when we were not even clear about the qualitative nature of the problem.} \]

Sarfatt also quoted from, and enclosed a copy of part of, a handwritten note from Susskind. It stated

\[ \ldots \text{Any way I feel bad about forgetting to acknowledge you. Glo and myself debated whether to put you as an author or Acknowledgement and in the scuffle I forgot. \ldots Lenny} \]

Carruthers wrote a kind letter to Sarfatt in return, gently pointing out that [20]

\[ \ldots \text{They (SG) did not acknowledge my aid either, although I spent a lot of time encouraging them and in reading the final MS! \ldots I'd like to remind you that in the fall of 1962 I gave a homework problem in which I asked for a discussion of the existence of } \phi. \text{ My curiosity on this point arose independently of your own, as I recall. (Also Louisell [21].) In any event, I think that my problem, or your remark, however perceptive and important towards motivating the solution, did not especially deserve reference. (We [19] did not intend to present a complete historical document.) \ldots} \]
Well, this article is a more, but not entirely, complete historical document, so I have related the above. (However, as you might expect, some of the things in my files are slightly more pugnacious than I have quoted.) In any event, eventually Sarfatt added an “i” to his name and went into Physics Consciousness [22].

Later, alternative phase operators were developed. I mention four of the more important schemes [23, 24, 25, 26]. The critical points always were if a Hermitian phase was possible or not, if alternative sine and cosine operators commuted among themselves, and if the sums of the squares of these operators was unity.

3 Are there discrete quantized-phase eigenvalues?

Before discussing the role of Louisell in this field [21], I want, and will need, to bring up another question. Can one obtain discrete eigenvalues of the $C$ and $S$ operators in physical systems? We know of one system where phase is quantized, that of quantized flux. But this question touches on the nature of the spectra of noncommuting observables.

The operators $x$ and $p$ do not commute. One can think of different physical systems in which both observables are discretely quantized: positions of atoms in a crystal for $x$ and Bragg diffracted particles for $p$. Leaving aside the question of time not really being an operator [19, 27], the Hamiltonian has discrete eigenvalues but the time is a continuously varying parameter. Time does not take on discrete, quantized eigenvalues, as far as we know.

Now the number operator has discrete eigenvalues. But what about the phase? In the early papers [17, 19] it was shown that the phase difference operator $S_{12}$, which commutes with the total number operator $N = N_1 + N_2$, has discrete, orthonormal eigenstates of the form

$$|\sin \phi_{Nr}\rangle = \left(\frac{2}{N+2}\right)^{1/2} \sum_{n=0}^{N} (-i)^n \sin \left[(n+1)(\phi_{Nr} + \pi/2)\right]|n, N-n\rangle,$$

with discrete eigenvalues $N$ and

$$\lambda_{Nr} = \sin \phi_{Nr}, \quad \phi_{Nr} = \left[\pi r/(N+2)\right] - (\pi/2), \quad r = 1, 2, \ldots, N+1.$$

for the operators $N$ and $S_{12}$. A similar set of eigenstates simultaneously diagonalize $N$ and $C_{12}$.

In Ref. [28], I described a gedanken experiment, where such a system could be realized. It was an idealized model of a Josephson junction, where the number of Josephson pair bosons was small and conserved. Then the Hamiltonian

$$H = \omega_1 N_1 + \omega_2 N_2 + VN_1 + ZC_{12}$$

could be used as a model to describe the Josephson effect. In particular, for low-$\langle N\rangle$ the DC Josephson effect ($\omega_1 = \omega_2$ and $V = 0$) would yield a quantized DC current proportional to $\sin[(n+1)\theta_{Nr}]$. 

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With low-number quantum systems now becoming commonplace, I wanted to raise this old question once more. It is one aspect of our more general question, “Are there unique and well-defined quantum phase operators?”

4 The work of Louisell

I now want to turn to the work of the late William Louisell. On 15 October 1963, a one page Physics Letters appeared, which had been received on 17 September [21]. In it Louisell took number matrix elements of Eq. (10), to show that they were undefined. However, on physical grounds he argued that the same difficulty would not ensue for periodic functions of $2\pi$, and then stated [21]:

We may then take $\cos \phi$ and $\sin \phi$ as Hermitian operators which satisfy

\[
[N, \cos \phi] = i \sin \phi, \quad [N, \sin \phi] = -i \cos \phi.
\]

With the addition of the associated uncertainty relations in his Eq. (8), that was it. The work was a beautiful piece of physical insight into what the solution must be like, but without the explicit demonstrations of the operators and their properties. However, for reasons we will come to, Louisell did not follow it up. Little work was directly due to this paper [29].

There matters might have stood, as far as our knowledge of Louisell’s work was concerned, except that during 1967 I submitted the quantized-phase paper mentioned in the last section. During part of the summer I was off to Mexico to (among other things) find the isolated mountain village where my parents had been born. So, I asked Carruthers to handle any correspondence on the paper for me.

While I was gone, Carruthers got a very negative rejection report from a referee who objected to SG not giving Louisell credit and to Carruthers and I for not giving Louisell credit in our work Refs. [19, 30]. Pete was quite angered and on July 21, 1967 sent a letter back to editor Sam Goudsmit, saying [31]:

...Why Nieto should take it on the nose for the sins of Susskind and Glogower is is beyond me but since it is there I will comment on it. As Susskind and Glogower were students at Cornell when they did their work, I can state that they were unaware of Louisell’s one-page Physics Letter ...Susskind and Glogower published their paper in Physics, a journal edited by P. W. Anderson, himself an expert on phase questions and a colleague of Dr. Louisell! Surely that was the time to call attention to inadequate citation of this literature.

Carruthers then went on to observe that, contrary to the claim, Louisell was cited in the first sentence in our paper, Ref. [30], and that, also, Louisell was cited in a different paper of my own, Ref. [32]. Best of all, Pete wrote:
Even more astonishing is the referee’s citation of our work (Ref. 12). (This referred to our review, the present Ref. [19].) He could not possibly have seen a copy of that work since copies were not available until after the date on your rejection letter. Nevertheless, our clairvoyant referee will find reference to Louisell’s work . . .

Carruthers simultaneously sent a copy of this letter to Louisell, along with a covering letter [33]. In it Pete wrote:

It recently came to my attention that some people think I (and my various collaborators) have not given you adequate credit for your contributions. . . . The matter came to my attention through a referee’s report concerning one of Nieto’s papers. (I am not suggesting that you are he—I have few doubts as to his identity!) . . .

Carruthers went on to discuss a disagreement he was having with another of Louisell’s colleagues at Bell Labs, Mel Lax, concerning a different paper of Carruthers’ [34]. He concluded with:

Please let me know whether the situation is real, or only a figment of my imagination.

In October, Louisell replied to Carruthers on University of Southern California stationary [35].

. . . As you may note, I am no longer at Bell Telephone Labs and have not seen Mel Lax for some time.

I was slightly surprised that the paper of Susskind and Glogower did not indicate they had seen my letter on phase variables. Even more surprising to me was the lack of knowledge of its existence by Phil Anderson, who was also not very far from me at Bell Labs. . . . I am sorry for any inconvenience caused you by the referee. I have no idea who the referee might be, but I am sure he must feel he was doing me a service.

At the time I considered the phase variable problem, I wrote a memorandum at Bell Labs involving quite a few detailed calculations. However, some of my mathematical assumptions were certainly not rigorous and after some criticism from people at Bell Labs, I decided not to publish most of this work. Frankly, I am convinced that even with lack of rigor, the results were correct. Unfortunately, in my move from Bell Labs I have lost all the copies of the Bell Lab memorandum on the phase variable and, therefore, can not send you a copy.

Essentially, I had a matrix representation for the \( \cos \Phi \) and \( \sin \Phi \) of finite order. I added extra elements in order to make the determinant for the eigenvalues a cyclic determinant. Then I proposed taking the limit as the number of elements went to infinity. My Bell Lab colleagues objected to this procedure strenuously. . . .
In rereading this letter some 25 years after the fact, the last two paragraphs struck
me as never before. It sounded as if Louisell might have done something very similar
to the Pegg-Barnett formalism I discuss in the next section, over 20 years earlier, only
to back off from it. I cringed, thinking of a motto I have in my office next to a nickel
I won from Frank Yang on a physics bet: “MORAL—Never forget! Just because
someone is smarter than you are, it doesn’t mean he’s right.”

Some digging turned up the fact that Louisell had written such a memorandum—
now get this—in 1961, before anyone else anywhere had published anything on “new”
phase operators [36]. The coauthor was John. P. Gordon.

Using the matrix representations for $U$ and $U^{-1}$ given in Eq. (6), they first
observed that Hermitian $\cos \phi$ and $\sin \phi$ operators could be defined by
\begin{equation}
2\cos \phi = U + U^{-1}, \quad 2i\sin \phi = U - U^{-1}.
\end{equation}

Then they made an “artificial proposal” for removing the difficulties related to Eq.
(8) and to connecting $U$ and $N$ with the $a$ and $a^\dagger$ or the $x$ and $p$ operators, first
disclaiming that [36]:

\[\ldots\text{although we make no claim as to its mathematical rigor.}\]

The proposal is to consider the matrices for $\cos \phi$ and $\sin \phi$ as finite
for calculation purposes and let them become infinite after the calculation
is complete. To make the matrices cyclic, we then add extra elements in
each as follows:

\[
\begin{align*}
\cos \phi &= \begin{bmatrix}
0 & \frac{1}{2} & 0 & 0 & \cdots & 0 & \frac{1}{2} \\
\frac{1}{2} & 0 & \frac{1}{2} & 0 & \cdots & 0 & 0 \\
0 & \frac{1}{2} & 0 & \frac{1}{2} & \cdots & 0 & 0 \\
0 & 0 & \frac{1}{2} & 0 & \cdots & 0 & 0 \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\
0 & 0 & 0 & 0 & \cdots & 0 & \frac{1}{2} \\
\frac{1}{2} & 0 & 0 & 0 & \cdots & 0 & 0
\end{bmatrix},

\sin \phi &= i\begin{bmatrix}
0 & -\frac{1}{2} & 0 & 0 & \cdots & 0 & \frac{1}{2} \\
\frac{1}{2} & 0 & -\frac{1}{2} & 0 & \cdots & 0 & 0 \\
0 & \frac{1}{2} & 0 & -\frac{1}{2} & \cdots & 0 & 0 \\
0 & 0 & \frac{1}{2} & 0 & \cdots & 0 & 0 \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots \\
0 & 0 & 0 & 0 & \cdots & 0 & \frac{1}{2} \\
-\frac{1}{2} & 0 & 0 & 0 & \cdots & 0 & 0
\end{bmatrix}.
\end{align*}
\]

That is, factors of $\pm\frac{1}{2}$ were added to the upper-right and lower-left elements of the
matrices in Eq. (26). These finite matrices then are cyclic, and have eigenvalues
$\cos \frac{2\pi p}{M}$ and $\sin \frac{2\pi p}{M}$, $p = 0, 1, 2, \ldots, (M - 1)$.

In his later, one page letter [21], Louisell did not include these more detailed
observations, which presaged the work of Pegg and Barnett.
5 The search for a Hermitian phase operator—the PB formalism

Phase states were defined in Refs. [17, 19]. Using delta-function normalization, they are

\[ |\theta\rangle = \sum_{n=0}^{\infty} \exp(in\theta) |n\rangle, \]

with resolution of the identity

\[ \int_0^{2\pi} d\theta |\theta\rangle\langle\theta| = 1. \]

At first sight it might appear that these could define a phase eigenstate and hence a Hermitian phase operator. However, as shown on p. 428 of Ref. [19], this would necessitate a number spectrum \(-\infty \leq n \leq +\infty\). (A point in line with Newton’s proposed resolution [26].) Once again, it is the one-sided nature of the spectrum, not its discreteness, which causes the problem.

A few years ago, however, Pegg and Barnett [37, 38, 39] asked if a way around this problem would be to consider a finite-dimensional phase operator. First consider the phase states with kroneker-delta normalization,

\[ |\theta\rangle = \lim_{s \to \infty} (s + 1)^{-1/2} \sum_{n=0}^{s} \exp(in\theta) |n\rangle. \]

Instead, PB propose a finite-dimensional reference phase state,

\[ |\theta_m\rangle_s = (s + 1)^{-1/2} \sum_{n=0}^{s} \exp(in\theta_m) |n\rangle. \]

In Eq. (30), \(\theta_m\) is

\[ \theta_m = \theta_0 + \frac{2m\pi}{s + 1}, \quad m = 0, 1, \ldots, s \]

where \(\theta_0\) is an arbitrary constant that labels the orthogonal set of basis states spanning this space. PB then define their phase operator as

\[ \hat{\phi}_s = \sum_{m=0}^{s} \theta_m |\theta_m\rangle_s \langle \theta_m|. \]

In the number basis, this operator is

\[ \hat{\phi}_s = \theta_0 + \frac{s\pi}{s + 1} + \frac{2\pi}{s + 1} \sum_{j \neq k}^{s} \exp[i(j - k)\theta_0] |j\rangle \langle k| \]

\[ \times \exp[i(j - k)2\pi/(s + 1)] - 1. \]

Finally, PB describe the matrix elements, \(M_{ab}\), of phase-dependent operators, \(O_s\), to be

\[ M_{ab} = \lim_{s \to \infty} \langle a|O_s|b\rangle. \]
With this they obtain a formulation which they propose as a representation of the quantum phase. One feature they like is that then there would be a direct quantum analogue of the classical variable.

There are two aspects of the PB formalism which have been widely discussed, concerning its mathematics and its physics. Let me start with the mathematics, since that is a well-defined, although often misunderstood, question. Further, it will lead to the physics.

The mathematical question is often stated as, “Is this operator Hermitian in the harmonic oscillator space?” The answer depends on what you mean by “Hermitian.” Unfortunately, most physicists, myself included, are usually unconcerned about differentiating “Hermitian” (or symmetric) from “self-adjoint.” In most physical situations the operators we deal with are either both or neither, and so the concepts have become confused as being synonymous. But they are not, as even careful, elementary text books note [40].

It is self-adjointness which is the critical physical property, since the spectral theorem depends on it. A Hermitian operator is not always self-adjoint nor does it necessarily have a self-adjoint extension. This last is a concept my good friends John Klauder and Dave Sharp never tire of trying to beat into my head. There are a number of enlightening discussions on this topic [41, 42, 43, 44, 45, 46].

That being said, the answer is “No.” The operators obtained in the PB formalism are not self-adjoint and have no self-adjoint extensions in the harmonic oscillator space – this last is an important caveat. This is similar to the problem of trying to define \( p_x \) on the whole line (self-adjoint), on a finite segment of the line (there are self-adjoint extensions), and on the half line (not self-adjoint and there are no self-adjoint extensions) [46].

However, PB declare that their operators are self-adjoint on the \((s+1)\)-dimensional space, so they can take expectation values and then let \( s \to \infty \). There we have the physics.

PB are in effect proposing a new mathematical extension for quantum electrodynamics. In the accepted formulation of quantum electrodynamics, as the standard discussions describe, the photon field \( \phi_0 \) is defined on the infinite-dimensional space. From the standard point of view anything which is not equivalent to this is not the photon.

As most everyone agrees, and as has been demonstrated explicitly by by Gantsog, Miranowicz, and Tanaś (GMT) [47], PB’s formulation is not equivalent to the infinite-dimensional space. If the standard point of view is correct, one has to be able to interchange the order of the infinite limit and the expectation value in Eq. (34). But these two operations do not commute. Further, if one first takes the infinite limit of the phase operator, meaning in the number basis one has

\[
\hat{\phi}_\infty = \theta_0 + \pi + \sum_{j \neq k} \frac{\exp[i(j - k)\theta_0]|j\rangle\langle k|}{i(j - k)},
\] (35)
then one obtains an operator which, as noted by GMT, has been discussed by a number of authors; e.g. [48] and [49]. This last work received little attention until the proposal of PB appeared.

Ultimately it remains an experimental question as to if this new mathematical formulation of phase is correct. But even if, as I do, one takes the conservative point of view, that standard, infinite-dimensional QED is correct, that still does not negate the physics of PB. Firstly, there is nothing wrong with proposing that “Hermiticity” (by the quotes, I mean self-adjointness) breaks down as long as there is no conflict with experiment and you realize that ultimately you have to deal with unitarity and understand what physics you are dealing with. Further, even if nature demands “Hermiticity” in every physical discussion, it does not mean that one cannot use a good non–“Hermitian” approximation. (Recall the spirit of Ref. [25].)

Many authors have found that the PB formalism is calculationally very useful in describing large-\(\langle N \rangle\) systems [50, 51]. Others have discussed the relationship between the SG and PB formalisms in certain measurement schemes [52, 53]. Accepting the standard QED formulation, I view the benefit of the PB formulation to be similar to that of the WKB approximation with respect to the Schrödinger equation. It is exact in the large-\(\langle N \rangle\) limit, it is calculationally useful, and, if it does break down, it breaks down only for small-\(\langle N \rangle\). (In this case, for states with nonzero \(\langle N \rangle_0\).)

The PB formalism is a useful and insightful development. The question remains if it is a totally correct description of nature. In the next section we discuss a new proposal which would give an answer of, “No,” to this and other phase proposals. It says there is no unique set of phase operators.

6 Does a unique quantum phase operator even exist (NFM)?

Since no theory, no matter how beautiful and formally correct it may be, can be accepted without experimental verification, we see from our previous discussion that it is in the low-\(\langle N \rangle\) limit where we lack sufficient understanding of phase operators, both experimentally and theoretically. Up until recently, there has been little experimental data in this regime [54], and certainly not enough to distinguish between various formalisms [55].

This situation has changed with the new experimental results, and their analyses, by Noh, Fougères, and Mandel [56, 57, 58].

I first heard about the NFM results from Mandel’s talk at the workshop we held in Santa Fe, in 1991 [59, 60]. I was both interested and perplexed about them, and remember telling myself that I had to look at them in more detail. I did a little, but not enough because, like many other people, I was too involved with what I was doing myself [61]. (I’ll return to that later.)

I have to thank Wolfgang Schleich for getting me to contribute to this issue,
because that is what forced me to look more seriously at the NFM results. Once I looked seriously, I was hooked. Their work is exciting and intriguing.

They have two experimental setups, which they describe as Scheme 1 and Scheme 2. It is important to see their third paper [58] because, although they analyze both schemes and give experimental results for Scheme 2 in their earlier papers, it is only in their third paper that they give experimental results for Scheme 1.

A diagram of Scheme 1 is shown in Fig. 1. In it, inputs 1 and 2 are combined with a 50:50 beam splitter (BS). Using the detectors $$D_3$$ and $$D_4$$ the area to measure the sine of the phase difference. If, however, a $$\lambda/4$$ shifter is inserted in one input beam, they are now able to measure the cosine of the phase difference, using the detectors now labeled $$D_5$$ and $$D_6$$. The quantum operators which they find to describe the situation are:

$$S_M = K_S \{i[a_2^\dagger a_1 - a_1^\dagger a_2]\} = K_S[N_4 - N_3], \quad (36)$$

$$C_M = K_C[a_2^\dagger a_1 + a_1^\dagger a_2] = K_C[N_5 + N_6]. \quad (37)$$

The $$K$$’s are constants that are to be determined. For large $$\langle N \rangle$$, the $$K$$’s become $$[2\langle N_i \rangle \langle N_j \rangle]^{-1/2}$$, $$(i, j) = (3, 4)$$ or $$(5, 6)$$. As with the SG formalism, $$S_M$$ and $$C_M$$ do not commute, so there is not a uniquely defined “quantum phase angle.” This is verified experimentally, as shown in Figs. 5 and 6 of Ref. [58]. (Before continuing, we note that the considerations of NFM do not start with fundamental phase operators, but rather with phase-difference operators.)

On the other hand, when NFM use the apparatus of Scheme 2, shown in Fig. 2, they find a well-defined phase difference. In this scheme, the inputs are on opposite ends of a four-arm interferometer. The sine and the cosine are then measured simultaneously at the two other corners of the interferometer, using detectors ($$D_3, D_4$$) and ($$D_5, D_6$$), respectively. To describe this situation, NFM derive quantum sine and cosine operators which commute with each other, thus yielding a unique quantum phase difference. Their experiments agree with this description [56, 57, 58]. Their conclusions are that there is no unique set of quantum phase operators and that the appropriate operators depend upon the measurement scheme.

NFM have made an insightful set of measurements and analyses. They may be correct in proposing that there is no unique set of well-defined phase operators. Even so, and without a complete resolution in hand, I am going to argue that, in fact, their measurements may suggest something else.

My first observation is that, since the simpler Scheme 1 finds a set of noncommuting $$S$$ and $$C$$ difference operators, it seems odd that the appropriate set of operators would commute in a more complicated scheme, unless something were modifying the quantum mechanics. (What happens if one of the two cosine arms and then one of the sine arms are blocked in turn? Would noncommuting phase measurements ensue?)

I next observe that Scheme 2 makes two measurements simultaneously.

What happens if you try to make simultaneous measurements of $$x$$ and $$p$$? I think it is agreed that, although you might measure something, it will not be $$\langle x \rangle$$ and $$\langle p \rangle$$.
particular, it will not be at the same time. Note that quantum commutation relations are equal-time commutation relations.

The measurements, in Scheme 2, of $S_M$ and $C_M$ are outside the light cone. They are not causally connected. The problem borders on the EPR paradox. Furthermore, that is why, at first glance, I am intrigued that Freyberger and Schleich [62] find agreement with the measurements of Scheme 2 using a Wigner-function analysis combined with Paul’s theory [24]. The Wigner function is nonlocal, i.e., acausal. The state preparation for the two measurements is different.

Clearly, the work of NFM has provided a very important new tool in our efforts to understand quantum phase.

7 Discussion

The last few years has seen a resurgence in efforts to understand quantum phase. As I hope is clear, in my opinion these efforts have been exceedingly fruitful. With experiments now able to penetrate the low-$\langle N \rangle$ limit as well as the high-$\langle N \rangle$ limit, we have reached the stage where theoretical ideas can be critically confronted by experiment.

We should also try to think of other types of experiments which might be useful. At first blush, neutron interferometry comes to mind [63, 64]. There you have something even newer about phase. Neutrons are fermions and hence rotations to the identity are $4\pi$ instead of $2\pi$ [65, 66, 67].

The combination of the facts that a) quantum phase was a direct outcome of the necessity of imaginary numbers in quantum mechanics, and b) fermions bring a new complication to phase, leads me to a final set of observations.

In the full fruition of quantum field theory, fermions are handled with Grassmann numbers and algebras. With our present understanding, it is not necessary to use Grassmann numbers (which are anticommuting numbers with a nilpotent fermionic pieces), even though it is an amusing exercise. However, if it turns out that there is a fundamental supersymmetry in nature (supersymmetry relates fundamental-particle boson and fermion partners), then there would be a necessity to introduce Grassmann numbers into quantum mechanics. This is something that I am very interested in [61], with stimulation coming from studies of supercoherent [68] and supersqueezed [69] states.

So, even though the first problem has not been solved, that of phase, I wish to alert you to the next one in line, that of Grassmann numbers.

This is all very exciting.

8 Acknowledgements

I have benefitted greatly, both recently and also over the years, from discussions, arguments, and fights with my many colleagues who have worked in this field. Many
of them, perhaps to their chagrin, are mentioned in the text and references.

This all ended up quite differently from what I thought it would when I agreed to Wolfgang Schleich’s request for a contribution. Actually, though, you once again have Peter Carruthers most to blame. If he had been smart enough to really chase me off almost 30 years ago, you wouldn’t have been bothered with this manuscript in the first place.

References


[13] Letter from Bruce Thomas to MMN, dated 10 June 1968. The problem set was not dated, and Bruce reconstructed the time sequence from his notes and files from the course.

[14] Carruthers found and mailed me a copy of the original spirit-duplicated problem set along with a covering letter dated 1 June (no year). From the contents of the letter, the year was 1968. In an email to me from Gene Golowich, dated 20 August 1992, the wording of the problem was verified.
Letter and “Historical corrections” by J. Sarfatt sent to E. U. Condon, editor of Rev. Mod. Phys., dated 20 May 1968, with copies sent to many individuals, including Carruthers, Louisell, Nieto, and Susskind. Also included was a copy of part of (not all of) an undated, handwritten letter from L. Susskind to J. Sarfatt.


L. Susskind and J. Glogower, Physics 1 (1964) 49.


P. Carruthers and M. M. Nieto, Rev. Mod. Phys. 40 (1968) 441.

Letter from P. Carruthers to J. Sarfatt, dated 6 June 1968.


H. Brunet, Phys. Lett. 10 (1964) 172; J. Harms and J. Lorigny, ibid. 173.


Letter from P. Carruthers to S. A. Goudsmit, dated 21 July 1967.


Figure captions

Figure 1. Scheme 1 of NFM, to separately measure the sine or the cosine of the phase difference between two light beams. Taken from [56].

Figure 2. Scheme 2 of NFM, to simultaneously measure the sine and the cosine of the phase difference between two light beams. Taken from [56].