**Correspondence with Journals**

*My paper,* "On the possibility of quantum field theory without renormalisation" *was submitted to Physics Letters B initially in May 1984. Here is the subsequent correspondence.*

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26 June 1984

This paper makes a very striking claim. It is important to check it carefully before publication. In my opinion, the author should take a well defined physical quantity, like the scattering amplitude to order $\lambda^2$ in Fig. 3, and compare his result with the usual Feynman integral. If the two integrals differ, the difference should be analysed. The usual result is known to agree with experiment to very high accuracy in QED.

Peter Landshoff, Department of Applied Mathematics and Theoretical Physics, University of Cambridge (on behalf of Physics Letters B)

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*I must have sent another letter, which I have not retained.*

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Is there or is there not a simple example of a scattering amplitude for which the author's integral can be compared with the usual Feynman integral (to some order)? If there is, I think that priority should be given to making the comparison, and nothing should be published until the result is known. If there is not, then it seems to me that the rules for the physical interpretation of the author's theory are not sufficiently well formulated to justify publication.

Therefore, I regret that I do not think that a paper, modified along the lines suggested by the author, would be suitable for Physics Letters.

Physics Letters B

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*I followed the referee's advice, and two years later was able to demonstrate that the results of Feynman graph analysis, at least up to tree level, could be reproduced. My investigations also convinced me that the interaction picture, upon which Feynman-Dyson perturbation theory is based, does not exist. In fact this was already known – Haag's Theorem. Hence the title of the paper:* "Does the interaction picture exist?" *The following correspondence applies to this paper.*

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4 June 1986. NB: Some corrections sent, 9 June 1986.

Dear Sir,

I would like to submit the enclosed paper for publication in Nuclear Physics B.

This work, which I have been doing for two years now, grew out of a study of arbitrary spin field theories, which formed the basis of my D.Phil. thesis when I was at Oxford. In fact, the vast bulk of the research I have done on this has been in the specific area of Quantum Electrodynamics, but I decided that the scalar field theory case was the best one to present because the principles are exactly the same, and the results entirely analogous: it is only that in Q.E.D. we have the complications of spin and the necessity of solving four spacelike commutators ($\{\psi, \psi\}$, $\{\psi, \overline{\psi}\}$, $[\psi, A]$ and $[A, A]$) rather than just one. I will, however, write a paper on this specific case in due course.

Most of the response I have had to my work has been very positive, but there have been some isolated cases of physicists dismissing it without seriously considering the arguments presented. Perhaps they have lived for too many years with the old methods to seriously entertain the notion that these methods can be improved upon. At any rate, I hope that this will not be your reaction. This *is* substantial work, and I can honestly state that most of my time on this is spent on checking the results.

I am acutely aware of the problem of reproducing the Lamb shift and anomalous magnetic moment to the required accuracy, but I refuse to make any claims until I have satisfied myself that I can properly recover a quantum mechanics style Schrödinger equation for the two-body system. There is, however, no doubt that the scattering theory works.

Yours Faithfully,

Dr. C. G. Oakley, Dept. of Physics, Hatfield Polytechnic, Herts., UK

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8 July 1986. *This did not come from the Journal, but is probably relevant.*

Dear Mr. Oakley,

Thank you for sending us your preprint "Does the interaction picture exist?" In fact, in 1955, R. Haag proved that it does not exist in any translationally invariant theory with vacuum polarization, and this includes any relativistic theory. A complete and rigorous proof was given by Hall and Wightman in Mat. Fys. Medd. Dansk Vid. Selskapet 31, 5 (1957). An accessible pedagogical account is to be found in the monograph I wrote with Wightman in 1962-4 called PCT, Spin and Statistics, and All That (Benjamin/Cummings, N.Y. 1964; second edition, 1979). This standard work can be found in any academic library, though it is now out of print, I believe. We call the theorem "Haag's theorem" and we conclude (p166): "Haag's theorem is very inconvenient; it means that the interaction picture exists only if there is no interaction". Your result is therefore not new.

Your method of proof is really not satisfactory. You treat the field as if it were a well defined operator-valued field. In fact even for the free field it is a tempered distribution. Your argument after eq(2), that a relationship cannot be true in one frame without being true in every other frame, is very dubious; for, the Lorentz transformations of the free theory would not do for the interacting theory, and so the covariance properties of $U(t) = e^{iHt}e^{-iH_0t}$, which contains both Hamiltonians, are not straightforward.

Finally, you seem to assume one of the main steps of the proof– that the vacuum is unitarily invariant (p15, §4). You cannot mean this literally– no state is or can be variant under all unitary transformations.

Hall and Wightman showed that the vacuum is invariant under $U(t)$, if it exists. This leads to a contradiction quite quickly, as in your paper. But it is certainly not a usual assumption. Indeed, if $\Psi_0$ is the free vacuum, $\lim_{t\to\pm\infty} U(t)\Psi_0$ is usually called the in(out); or dressed vacuum, and (if there is vacuum polarization) is quite different from $\Psi_0$.

There are aspects of your paper that indicate that you are isolated from research. For example, you work with the $\lambda\phi^3$ theory; but gives an energy unbounded from below. You do not refer to the known solution of $\lambda\phi^4$ in 1+1 and 1+2-dimensional space time (cf Quantum physics, Glimm and Jaffe, Springer). Though both your references are classics of their type, they do not have an impact on the deeper questions that you are dealing with.

Some of the arguments you give are sound, but well known e.g. most of your §3 is known as the Jost-Schroer theorem or the Federbush-Johnson theorem (1960). It is carefully described in our book.

Naturally I do not believe your argument against derivative couplings– you are dealing with distributions and much more care is needed. I am sorry to say that your paper is not publishable.

Your sincerely,

R.F. Streater, Dept. of Mathematics, King's College, London

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14 July 1986.

Dear Dr. Oakley,

The above paper has just returned from our referee with the enclosed report. In view of this, we very much regret being unable to publish you work in Nuclear Physics B.

*Referee's report:*

The non-existence of the interaction picture was shown by Haag thirty years ago. The author will find a simple proof e.g. in Streater and Wightman's "P.C.T., spin and statistics and all that", p. 166. The author should also be aware that the modern point of view of the problem is the following one: the Gell-Mann and Low formula, or the path-integral formula for the generating functional of Green functions gives only a heuristic guide for the derivation of Feynman rules. Perturbative unitarity and locality have to be checked on the renormalized theory. If these properties are proved (and this is the case for $\phi^4$, electrodynamics and Salam-Weinberg model), the renormalized theory can then be considered as physically correct.

I cannot recommend the publication of this paper which does not present any new result.

Nuclear Physics, B

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12 August 1986

Dear Professor Streater,

Thank-you for your letter of 8th July. I have considered your remarks carefully, and on account of these have substantially rewritten the paper that I sent to Nuclear Physics B, a copy of which I now enclose.

I was in fact aware that the non-existence of the interaction picture for an interacting field theory was proved a long time ago, that this is called Haag's theorem, and that a proof is contained in your book. The reason for emphasising this consequence of my work in the paper was simply that in the standard texts on field theory, i.e. Bjorken and Drell, and even Itzykson and Zuber, Haag's theorem is made rather light weather of, and, it seems, most "phenomenological" field theorists tend to ignore it completely. The fault has been corrected.

I have deleted the direct proofs of Haag's theorem in the modified paper, so the rest of the remarks on the first page of your letter no longer apply. Turning to the second page of your letter, my isolation from recent research is entirely voluntary, as I have never been able to make head or tail of the path integral approach. To substantiate an entirely emotional aversion there is the undoubted fact– conceded by even the most enthusiastic exponents– that this method is not "axiomatic", i.e. it does not derive from a set of assumptions such as the Wightman axioms.

As for section 3 of the paper– I may be wrong, but surely the arguments are only equivalent to the Federbush-Johnson/Jost-Schoer theorem if we take vacuum expectation values?

I am disappointed that you should dismiss the main thrust of the paper so readily in your last paragraph. You say that I should treat the fields as distributions, yet on the same page you mention solutions to $\lambda\phi^4$ theory when the concept of such a theory makes no sense in terms of distributions. If a rigorous treatment of interacting local field theory is possible then it is only after the removal of divergences– certainly this is the case for the path integral treatment of $\lambda\phi^4$, and it is the case for my own method. In an ideal world, of course, one could formulate a comprehensive field theory with complete rigour, entirely from a simple, elegant set of assumptions. I do not claim that the treatment I give comprises this: what I do claim is that it is a thousand times better than the usual $U$-matrix approach, based as it is on a non-existent interaction picture and requiring as it does a dirty scheme of infinite manipulations to make it work. I do not, therefore, feel that it is reasonable for you to dismiss the argument of §4 so lightly. Accepting the limitations of the method, the argument is sound. I have spent a lot of time checking the steps. In fact, although the argument leads one to local field equations on account of assumption (vi), if it led instead to non-local field equations with suitably smooth smearing of fields then a distribution-theoretical statement would have been possible. Having said all of this, I do not forget that ultimately we do need a field theory which is completely mathematically sound, and thereby permits a distribution theoretical statement. I am working on it, but there are also many other problems to be solved. If you do not believe the argument against derivative couplings then I suggest that you check the steps that lead to this conclusion. If, as seems more than evident, you are not prepared to give me the benefit of the doubt then please at least do me the courtesy of taking the necessary time and trouble to understand what I am trying to say. The high standards that you require of this work are certainly not met by the current path integral methods, so it is easy for me to conclude that your purpose is to defend the status quo against new ideas, good as well as bad, expending as little effort as possible in doing so. Of course, I would not want to think this, and would be happy to discuss my work with you at any mutually convenient time. With best wishes,

Yours Sincerely,

Dr. C. G. Oakley, Dept. of Physics, Hatfield Polytechnic, Herts., UK

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18 August 1986

Dear Dr. Oakley,

Thank you for your new version. I have the following comments.

1. Introduction, Line 1-8.

There is confusion here between the Feynman path integral method and the functional integrals used in Glimm and Jaffe. These are not the same. The latter is completely rigorous and I think that Glimm and Jaffe would have reason to complain if you called their book "intuitive", rather than logical. In any case, they do not use the functional-integral method to the "exclusion of all others". Indeed, their book uses every branch of functional analysis, real and complex analysis and algebra and graph theory.

2. P. 3. Your axiom (vi), multiplying fields at the same spacetime point, as in $\{\psi(x), \psi(x)\}_+ = c$ number, does not make sense for distributions. Of course $[\phi(x), \phi(x)]_-$ should be zero for any sensible definition. Free fields are distributions; they are not merely regarded as such.

3. Your idea of solving (15) as a power series in $\lambda$, as in (17), has been tried before. You should know of the famous paper of Epstein and Wightman (Ann. of Physics, about 1963); they actually evaluate what you call $\phi_1$. They come to the remarkable conclusion that $\phi_1(x)$ is not a local field! Their motivation was similar to yours– to evaluate the Wightman functions in perturbation theory instead of the time-ordered functions. The reason is, that $(p^2 - m^2)^{-1}$ is non-local, i.e. $\phi_1 = (p^2 - m^2)^{-1} : \phi_0^2 :$ is non local. Their work inspired Haag and Schroer to prove that if $\phi_0(x)$ is a free field then there is no local solution, $\phi_1(x)$ to

$$(\partial^2 + m^2)\phi_1(x) =: P(\phi_0(x)) :$$

where $P$ is any polynomial (degree $\geq 2$). This led to the full understanding, using Borchers classes (cf. Ch. 4 of our book).

To sum up: your series (17) is a sum of fields each of which (beyond $\phi_0$) violates (vi).

So short of hoping for some sort of accidental cancellation that depends on $\lambda$ taking a special value, it is hard to believe that the sum, if it exists, is local.

4. All the graphs you wrote down seem to be finite. But if you computed the time-ordered product instead you would be in trouble, since the field $\phi_1$ is not local (see (3)). So you can't compute $S$-matrix elements. See: O. Steinmann, Thesis ETH 1960.

5. The r.h.s. of (37) in general defines a non-local field. You need $M_r(\lambda)$ to $\to 0$ fast as a function of $r$ to get a localizable field (a generalized function of Jaffe class), and one needs the sum to be finite if $\Phi(x)$ is a free field to get a local field (= a Wick polynomial). Without Wick ordering, $\Phi(x)^r$ makes no sense, as it is a distribution. But derivatives are allowed e.g. currents are local.

In your letter you do not believe that $\lambda\phi^4$ is exactly solved. It has been, in 1+1 and 2+1 dimensions. The answer is a Wightman field. The sense in which the distribution $\phi$ is raised to the power 4 is also known and understood.

In 4 dimensions (=3+1) the same methods seem to show that $\lambda\phi^4$ in 4 dimensions, when renormalized, is trivial. So any new method is welcome. But to go beyond what is known, you should do more than is in your paper. Could you, e.g. show that all graphs are finite after Wick ordering?

Your proof that derivative couplings are not local is not rigorous: you have not proved the finiteness of (33), or mentioned domains of operators etc., convergence in powers of $\lambda$ and a host of other points.

Besides, some experts think that non-renormalizability and non-locality go together, so would smile at your claim that derivative couplings are ruled out by non-local-commutativity rather than non- renormalizability.

R.F. Streater, Dept. of Mathematics, King's College, London

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Dear Professor Streater,

Just for the record, here is the argument which shows that the first-order commutator in $\phi^3$ theory vanishes for spacelike intervals.

Order $\lambda$ contribution to spacelike commutator:

$$[\phi_0(x), \phi_1(x')] + [\phi_1(x), \phi_0(x')] = 0$$

$$\Rightarrow \quad \int_{-\infty}^{\infty} d\nu \big([\phi_0(p+\nu n), \phi_1(p'-\nu n)] + [\phi_1(p+\nu n, \phi_0(p'-\nu n)]\big) = 0$$

Consider

$$\int_{-\infty}^{\infty} d\nu [\phi_0(p+\nu n), \phi_1(p'-\nu n)]$$

shift $\nu \to \nu - (n \cdot p)$; define $r = p - (n.p)n$; $q = r + s + \xi n = p + p'$; $n \cdot s = n \cdot r = 0$; $\xi = n \cdot q = n \cdot (p + p')$

$$= \quad \int_{-\infty}^{\infty} d\nu [\phi_0(r+\nu n), \phi_1(s + (\xi - \nu)n)]$$

Now, $\phi_1(p) = (p^2 - m^2)^{-1} \int d^4p' \phi_0(p')\phi_0(p-p')$. therefore we have

$$\int_{-\infty}^{\infty} d\nu [\phi_0(r+\nu n), ((\xi-\nu)^2 - S^2)^{-1} \int d^4p' \phi_0(p')\phi_0(s + (\xi-\nu)n - p')]$$

$$= -2 \int_{-\infty}^{\infty} d\nu \int d^4p' . \epsilon(r_0 + \nu n_0)\delta((r+\nu n)^2 - m^2)\delta(q-p')\phi_0(p')((\xi-\nu)^2 - S^2)^{-1}$$

$$= -2 \int_{\infty}^{\infty} d\nu \; \epsilon(\nu)\delta(\nu^2 - R^2)((\xi-\nu)^2 - S^2)^{-1}\phi_0(q)$$

$$= -\frac{1}{R}\big[((\xi - R)^2 - S^2)^{-1} - ((\xi + R)^2 - S^2)^{-1}\big]\phi_0(q)$$

$$= \frac{-4\xi}{((\xi-R)^2 - S^2)((\xi+R)^2 - S^2)}\phi_0(q)$$

Where $S = \sqrt{m^2 - s^2}$ and $R = \sqrt{m^2 - r^2}$. Therefore the order $\lambda$ term is this minus the term obtained by exchanging $p$ and $p'$:

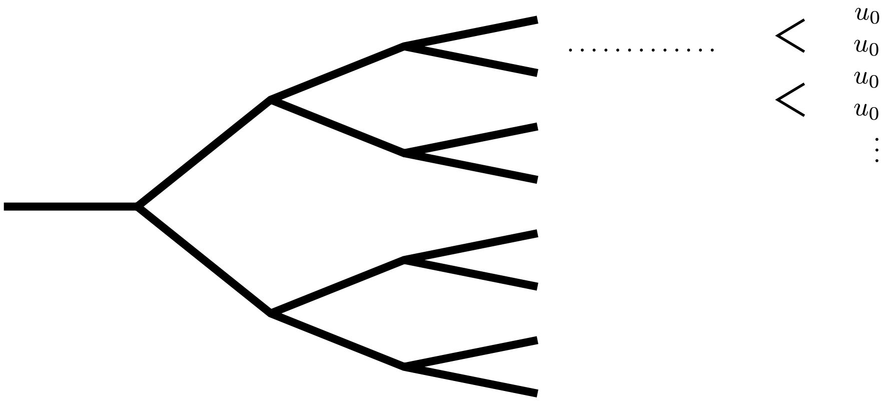
$$= -4\xi\phi_0(q)\big[\frac{1}{((\xi-R)^2 - S^2)((\xi+R)^2 - S^2)} - \frac{1}{((\xi-S)^2 - R^2)((\xi+S)^2 - R^2)}\big]$$

Regroup terms $\to 0$.

I find that Epstein and Wightman's work leads to a series which is inconsistent with the one I derive, i.e. $u_n$ as $n \to \infty$ does not become the true interacting field as I understand it. The recurrence relation

$$(\partial^2 + m^2)u_{n+1} = -\lambda u_n(x)^2$$

gives only the symmetrical tree contribution



Yours Sincerely,
Dr. C.G. Oakley, Dept. of Physics, Hatfield Polytechnic, Herts., UK

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24 September 1986.

Dear Dr. Oakley,

The revised version of the above paper has now been reviewed, and we have just received the enclosed second report of the referee.

In view of this, we regret having to maintain our decision not to publish the paper.

*Referee's report:*

The revised version of paper 9739 does not present any substantial improvement with respect to the original one. I'll add some remarks to my first report.

1. The author excludes derivative couplings by assuming the commutation relation:

$$[\phi(\mathbf{x},t),\dot{\phi}(\mathbf{x}',t)] = i\delta(\mathbf{x}-\mathbf{x}')$$

He is in fact assuming the result he wishes to prove, since $\dot{\phi} \neq \pi$ when there are derivative couplings.

2. After a long calculation and some uncontrolled approximations, the main achievement is to rediscover the formula for 2→2 scattering in the Born approximation!

3. Many claims in the paper are left unproven for example the claim that there are no infinities in the approach. It is not clear either whether the author's theory is unitary and local. The absence of the $m^2 \to m^2 - i\epsilon$ prescription makes me think that it is not. Finally there is no clear distinction in the paper between Green's functions and $S$-matrix elements.

In my opinion, this paper should not be published.

Nuclear Physics, B

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4 November 1986.

Dear Sir,

Thank-you for your letter of 24th September. As I understand it, the referee's main criticism is that my paper does not relate to the "modern point of view" of the problem of understanding Field Theory. On account of this, he says, the paper should not be published. The folly of this attitude need hardly be stressed, since we would not have Relativity if everyone thought like this. Neither would we have the path integral methods that he insists should be used: these certainly were not the "modern point of view" when Feynman was developing them in the 1940's.

However, this is not the point. The point is that if one is to take a truly axiomatic approach then one is forced to use operator-commutator methods rather than path integrals. A few years ago I did spend a great deal of time working of path integral techniques but came to the conclusion that these tools were not suitable for the purpose I required: it is not through ignorance that I avoid them.

For the other points: derivative couplings are indeed only excluded if we insist on commutators of the kind eq. (19). The accompanying paper is a modified version which stresses this point, and the enclosed calculations show that a simple choice of derivatively-coupled equations of motion with satisfy the appropriately modified commutators at least up to order $\lambda$. The claim that there may be no derivative couplings in theories involving vector particles is, however unaltered. This is because I have recently discovered that the set of spacelike (anti)commutators that we must solve are those involving $F_{\mu\nu}$, $\psi$ and $\overline{\psi}$ (the set involving $A_\mu$, $\dot{A}_\mu$, $\psi$ and $\overline{\psi}$ having no non-trivial solution). Since $F_{\mu\nu}$ is both co-ordinate and conjugate momentum, we are no longer free to choose the latter, and thus may not find a form which allows derivative couplings.

I am still working on the details, and have weakened the claims in the paper with regard to such theories until such time as I have all the details worked out.

Point (2) seems on the surface to be a subjective observation and, as such, not requiring any remark from me: however this is not the case. The pole domination approximation, far from being "uncontrolled" is in fact the approximation that is relevant for the kind of experiment being considered. In a scattering experiment the particles behave as though free for nearly all of the time: it is only for a very short time that $p^2 \neq m^2$ for any of them, and so considering only the zeroth order in a Laurent expansion around these values is much the most natural method.

I am disappointed that the referee should be unimpressed with the result, especially since no other calculation exists whereby the Born approximation 2→2 scalar amplitude is obtained from first principles in field theory, in a way that is consistent with Haag's theorem.

As for point (3): it is not possible for me to "prove" the absence of infinities any more than I have done already. When they arise, they appear in both sides of (38), and so, I imagine, can be cancelled. The logic is not irrefutable because assumption (vi) of the theory is not fully consistent with the other assumptions. However it is infinitely better than the usual removal of infinities by distinguishing between infinite "bare" parameters and finite "renormalised" parameters, which is just mathematical butchery.

As for locality, it is possible (?) that the referee is thinking of Epstein and Wightman's Ann. Phys. paper when he says that my theory is not local. In this paper it is claimed that a scalar $\Phi^3$ theory has field commutators that do not vanish for spacelike intervals. However, I have reason to believe that I am right and they are wrong, since they make the additional assumptions that the series defined by

$$(\partial^2 + m^2)u_0(x) = 0$$
$$(\partial^2 + m^2)u_{n+1}(x) = -gu_n(x)^2$$

will (a) converge on the true interacting field as $n$ becomes large, and (b) has vanishing spacelike commutators for intermediate values of $n$. I find neither of these to be true using the power series method, and my method only assumes expandability of the fields in the coupling constant.

The other remarks make me think that either the referee has not understood the motivation of the paper, or that he is not acting in good faith. "Unitarity", as understood by high energy physicists, requires the interaction picture. This paper attempts to be consistent with Haag's theorem which states that this picture does not exist. And even if this was a consideration, the results reproduce Feynman graph analysis, and so to accuse my theory of not being unitary is to accuse Feynman graph methods of the same. Equally, an $S$-matrix element has no relevance to this method, for the same reason.

Clearly, the referee would like to be able to dismiss this paper on simplistic grounds, the underlying logic no doubt being that if there was a way then someone would have discovered it earlier. What he maybe does not realise is that this is, in part, the fruit of a long and hard struggle, leading up many blind alleys before the final form of the theory took shape. Believe it or not, I am my own severest critic. There is nothing in the literature like it. Accordingly, just as I took time and trouble in working the method out, then if your journal calls itself world class– and not just a showcase for the latest in Superstring theory– then you owe it to me to take time and trouble in refereeing it. A full working day should be set aside for examining this paper, provided that the referee approaches it with an open mind (i.e. discarding the clutter of 40 years of physics). Only then would he be able to appreciate what this paper is about. If the demands on you referee's time are so great that this would not be possible, then I would be grateful if you could pass it on to someone else.

Yours Sincerely,

Dr. C. G. Oakley, Dept. of Physics, Hatfield Polytechnic, Herts., UK

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16 December 1986.

Dear Dr. Oakley,

The above paper has again been reviewed in the light of your letter and revised version of 4 November 1986, and we herewith enclose the report of the second referee.

In view of this, we regret having to maintain our earlier decision not to publish the paper.

*Referee's report:*

No relevant problems of contemporary QFT are considered in this paper. I recommend its rejection.

Nuclear Physics, B

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19 January 1987. *To the editor of Nuclear Physics B*

Dear Dr. Jones,

Thank you for your letter of 16 December 1986. Please note the new address for correspondence.

Your second referee's comment amounts to nothing more than short-sighted, ignorant stonewalling. Haag's theorem and the problem of infinities are unquestionably relevant problems of contemporary field theory and these are, in fact, the central considerations of the paper.

I understand from our telephone conversation today that the next stage, if I still want to get the paper published, is for a supervisory editor to review the file. I would like this done, but only if the paper will be sent to someone who will actually read it carefully, and check the steps in the mathematics, and preferably someone who is not an "axiomatic field theorist" since this group of people are too narrow in their approach. If this cannot be done, then I would be grateful if you could return the manuscript forthwith.

Yours Sincerely,

Dr. C. G. Oakley, University of Sussex, School of Mathematical and Physical Sciences.

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11 February 1987.

Dear Dr. Oakley,

The above file has been reviewed by a Supervisory Editor in the light of your letter of 19 January 1987, and we herewith enclose his report.

In view of this, we regret that our final decision is not to publish the paper.

The manuscript is being returned to you under a separate cover.

*Referee's report:*

In spite of its pompous language, this paper is, in fact, less rigorous, from the mathematical point of view, than ordinary perturbation theory which can be found in any good textbook or review article. I advise against publication.

Nuclear Physics, B

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13 February 1987. *To the editor of Nuclear Physics B*

Dear Mr. Jones,

The arrant stupidity of your last two referees cannot go unremarked. Their outlook is a symptom of disgraceful senility of so-called "fundamental" physics research. Your first referee was willing to discuss my paper by taking it point by point, but when he ran out of reasons not to publish it he then passed it on to these ignoramuses who block it on emotional, rather than scientific grounds. I would guess that the logic runs as follows:

"We have been working on this for thirty years and have not got an answer, so how could this unknown upstart get an answer?" Of course, to find the answer to this question you need only look at the history of science.

It is an absolute disgrace that fundamental research in theoretical physics is run by people who burned out decades ago and many of whom have even lost interest in the subject, using the precious commodity of intellectual freedom to play politics rather than moving on to give the younger person a chance. I am one of a growing body of people who think that fundamental change is the only solution. An academic position should <u>not</u> be a meal ticket for life, even for those who achieve spectacular results when they are young.

I shake the dust from my feet.

Yours Sincerely,

Chris Oakley, University of Sussex, School of Mathematical and Physical Sciences.

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*In the meantime I had sent out a letter to all the academic establishments in the U.K. that did quantum field theory offering to give a seminar. One of the replies was from Professor J.C. Taylor at the University of Cambridge:*

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26 January 1987.

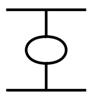
Dear Dr. Oakley,

Thank-you for your letter and preprint. I regret that I cannot invite you to talk to the HEP group here at present.

May I make a couple of observations on your paper?

The first is that the factor of $\frac{1}{2}$ discrepancy you mention in (54) is <u>serious</u>. Unless it can be resolved, there is something wrong.

Secondly, I recommend that you should concentrate on a <u>simple</u> example including closed loops and <u>not</u> the bound state problem. Why not take your $\phi^3$ model and do scattering to one higher order:



etc., to see if you can get the conventional answer (without mentioning divergent integrals). I hope that you don't find me impertinent, making these suggestions; but I think that this is the sort of thing most likely to persuade sceptical readers.

Yours Sincerely,

John C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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*Professor Taylor was an editor of the journal* Zeitschrift für Physik, *so as his letter of 26 January was not openly hostile, I decided to try this journal next:*

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17 February 1987.

Dear Professor Taylor,

I would like to submit the enclosed manuscript for publication in "Zeitschrift für Physik", C. I sent it initially to "Nuclear Physics", B who, it turned out, never had any intention of publishing it, although the first referee made a pretence at objective criticism at least initially. To save the referee time let me make clear now some points which the "Nuclear Physics" referee thought were reasons not to publish it:

1. The fact that this paper does not mesh with present "axiomatic" field theory does not necessarily mean that it is wrong.

2. "Unitarity" or the possible lack thereof is not a reason for the paper being wrong because "unitarity" means unitarity of the $S$-matrix, which is an interaction picture operator. The point (or at least one point) of the paper is to be consistent with Haag's theorem which states that this picture does not exist.

3. "Locality" or the possible lack thereof is similarly not a reason why the paper is wrong. Locality is maintained– as far as is possible– throughout.

4. All pathological divergences are removed by the procedure outlined at the end of §4. There are loops, but these are of propagators, and so will always converge.

I cannot understand why there is such an enormous, irrational resistance to my work. Ideas are the currency of scientific enquiry, and even if the ones contained in my paper are wrong, the issues raised are certainly worth considering. However this paper is not wrong. It is a self-consistent theory that is proposed, it is only its application to the real world that is unproved (at least apart from up to order $e^2$ scattering in Q.E.D.). As such, it is well worth publishing.

So, if you do not wish to see this in print, then please do me the courtesy of providing better reasons than the last journal did. If you are sceptical of the strong claims that are made– and I hope that you are– then at least make sure that the referee has fully understood the logic that leads to these. This may take a little time, since the approach is unconventional, but I can assure you that the effort will be well rewarded. The method is much cleaner than the conventional one. In fact, the last point is so important that I should repeat it:

IT IS NECESSARY FOR THE REFEREE TO TAKE TIME AND TROUBLE IN CONSIDERING THIS PAPER. IT IS NOT MERELY A VARIANT OF THE CONVENTIONAL METHOD, IT IS A RE-WORKING FROM FIRST PRINCIPLES.

Yours Sincerely,

Chris Oakley, University of Sussex, School of Mathematical and Physical Sciences.

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18 February 1987.

Dear Dr. Oakley,
Thank-you for sending your paper "New Methods for QFT" to Z. Phys. C.
I shall have it refereed in the normal manner.
Yours Sincerely,

J.C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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3rd April 1987.

Dear Dr. Oakley,

I enclose the referee's report on your paper submitted to Z. Phys. C. I am sorry that it has taken so long. As you see, he does not make a positive recommendation.

I will seek a second opinion.

*Referee's report:*

This paper represents a serious attempt to circumvent a clear theoretical problem (Haag's theorem) regarding the interaction picture in quantum field theory, and I wish to treat it fairly and without prejudice.

I would agree that the author seems to have described an approach, viable as far as it goes and has been claimed, for calculating amplitudes and cross sections for a field theory without use of an interaction picture. The resonance possibility, at first sight odd, seems to be necessary for getting results like (51) with formal resemblance to what conventional calculations give. I did not understand the significance, if any, of the $\lambda$ versus $\frac{1}{2}\lambda$ discrepancy mentioned at the bottom of page 21.

I believe that the interaction picture does more for me than simply allow calculations to be done. It is fundamental to my understanding of how physical particle states are described in quantum field theory, in association with some adiabatic hypothesis, which is no doubt related in some basic way to the relevance of Haag's theorem. The description of physical particle states is not addressed carefully in the paper under review. One's only view of amplitudes (it seems) is via the approximation procedure, whose uniqueness is not considered any more than is its relationship to any fundamental physical postulate. I would expect that even if avoidance of divergences survive to all orders in perturbation theory, renormalization of physical entities will still be called for. One might ask too how is physical mass and coupling defined.

I am not confident about what recommendation to make. Acceptance would seem to be a gamble that the paper is indeed the first step of the building of a new edifice. I am in some doubt as to whether enough has been done to justify such a recommendation. I would prefer to suggest the editor seek a second opinion.

*[Ends]*

Yours Sincerely,

J.C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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26 May 1987.

Dear Dr. Oakley,

I enclose a second referee's report on your paper submitted to Z. Phys. C. As you will see, this report makes strong criticisms, and I fear I cannot accept the paper. I am sorry.

*Referee's report:*

This paper should not be published. The research reported here does not make a significant contribution to the subject. The work is not very sophisticated and reveals some misunderstandings of the progress already made; as early as the first paragraph the author confuses the work in the book of Glimm and Jaffe with the "formal" methods of the path integral.

The "assumptions" of §2 are not adequate to deal with even the free quantized field: thus in (ii), $\Phi(x)\Omega$ is not a vector (of finite norm), so assumption (ii) is false for the free field. As for (iii), the author has omitted any relation between the Poincaré group and $\Phi(x)$. Also (vi) is meaningless unless the meaning of $\Phi(x)$, the field at a point, is clarified. Even though a fully rigorous interacting field theory in 4 space-time dimensions has not yet been achieved, there are results in 2 and 3 space-time dimensions which <u>are</u> fully rigorous. The author's formulation remains very nebulous also for these cases, so it is difficult to believe that he will succeed in 4 dimensions where others have failed. Indeed, it is more or less known that there is <u>no</u> scalar field theory obeying the spectral condition for $\lambda\Phi^3$, and there is no theory at all for $\lambda\Phi^4$.

The author's §3 touches on a result of 1960, due to Federbush and Johnson, and also Jost and Schroer. Neither work is referred to. The author's methods are not rigorous e.g. after equation (5) he concludes that a (generalized) function zero unless $p + p^1 = 0$ must have a factor $\delta(p + p^1)$ without mentioning why. A similar hasty conclusion leads him to say that, as $\phi(p) = 0$ unless $p^2 = m^2$ it has a factor of $\delta(p^2 - m^2)$ in which he rules out derivatives. These derivatives <u>can</u> be ruled out, as in the earlier proofs. So §3 is a less good treatment of a well-known result.

§4, the interacting case, follows another well-known idea, stemming from the Yang-Feldman equation of 1954. It is also known to fail. Apart from fine questions of convergence, of the series (17), it is known that $\phi_1(x)$ given in (18) is not a local field. This follows from the work of Epstein and Wightman, but also the general theory of Borchers classes, as explained in his ref [4].

The author's argument that the current must be a local function of a free field, with no derivatives, again uses the false idea (after eq. [30]) that any distribution vanishing unless $p + \Sigma p_i = 0$ must contain $\delta(p + \Sigma p_i)$ as a factor: he omits possible derivatives of $\delta$, and consequently is able to rule out the possibility of derivative coupling. But since, even for local currents the field $\phi$ is non-local anyway, his proof sits in a vacuum– it concerns a non-existent theory.

The author works out the three diagrams for $2 \to 2$ particles and comes out with the pole approximation. There are many ways to obtain these items as a first approximation, and success here does not mean that the treatment of the next order, with loops, will be free from divergences.

The section on electrodynamics contains no actual workings, and is a sort of advertisement.

In summary, the naïve methods and hasty arguments do not add up to a promising programme.

*[Ends]*

Yours Sincerely,

2 June 1987.

Dear Professor Taylor,

Thank you for the letter of 26th May. I was disappointed to hear that you have not taken a direct interest in my work on field theory. No doubt the logic is that since many papers which make strong claims have been wrong, it therefore follows that my paper, which makes strong claims, is also wrong. This is fact has been the attitude of almost all in the physics establishment, and has been the reason for all my applications for research fellowships drawing a blank. This is a pity, because I am quite sure that if you *had* studied my work carefully, and thought about it for a bit, then you would probably agree that the methods, as well as being more mathematically consistent, are also more natural than the text book ones. You also obviously do not want me to come to D.A.M.T.P. to speak on my work, since I expressed an interest to do so in January and it is now June and nothing has been arranged.

I believe that my methods *will* replace the text book ones– even thought there are still ample opportunities for it all to go wrong– it does at least provide a basis for consideration which is more consistent than the existing one. Your referee should note that the advertisement for Q.E.D. (done my way) can be substantiated by calculations. If he wishes it, I will send them to him.

Incidentally, I cannot condone the way in which I have been treated by the establishment. The characteristics of my approach which were responsible for my failure to get a research fellowship– namely my independence of mind, my unwillingness to do things just because everyone else was doing them, and my (relatively) thorough and critical approach to the mathematics– should have been regarded as necessary (if not sufficient) qualities for doing research rather than as reasons for being ejected. My view is that the whole system needs to be changed in order to encourage scepticism in young people rather than to stamp it out– which is what happens at the moment.

As you will have gathered, I cannot accept your conclusion that the paper I submitted to "Zeitschrift für Physik" should not be published. Turning to the referee's report, if I separate out the points which are either (i) ones which could be satisfied by a very minor rewriting of the paper; (ii) requirements that I should relate my work to present-day axiomatic field theory– which is impossible (as the referee well knows) since my method involves "starting from scratch", or (iii) his opinions, then I am left with the following:

1. The notion that $\phi_1(x)$ of (18) is a local field. The referee says, on the basis of the work of Epstein and Wightman, that it is not. This is erroneous. They find that $[\phi_1, \phi_1]$ does not vanish for spacelike intervals and conclude that $\phi_1$ is not local. In fact, they should have been looking at $[\phi_0, \phi_1] + [\phi_1, \phi_0]$, which is the correct order $\lambda$ contribution to the commutator $[\phi, \phi]$. This *does* vanish for spacelike intervals.

2. The possibility of allowing derivatives with respect to momenta in the delta functions. This is not, in fact, a possibility at all. My arguments, in each case, are ultimately based on the notion that $xf(x) = 0$ implies $f(x) = c.\delta(x)$. A derivative of a delta function will not do. This is easy to see. If we had

$$f(x) = c.\delta'(x) \quad \text{then we would need} \quad 0 = \int \xi(x)\, dx\; x.c.\delta'(x)$$

for *any* suitably smooth function $\xi$ which vanishes at the limits. This leads to

$$-\frac{d}{dx}\big(c.x.\xi(x)\big) = 0 \quad \text{at} \quad x = 0,$$

which is of the form of a constraint on $\xi(x)$ and is therefore unacceptable. Higher than first derivatives are ruled out in the same way.

I might also add that derivatives with respect to momenta in the delta functions would not amount to "derivative couplings" anyway– rather they would give (translation-invariance-violating) explicit appearances of the spacetime four vector in the configuration space equations of motion.

3. *all* infinities are removed by the argument of §4. This applies to *all* orders, not just order $\lambda^2$.

There is evidence that the referee is not acting in good faith. Firstly, because he ignores the fact (readily conceded by the first referee) that the formalism solves an important problem of field theory (consistency with Haag's theorem), and secondly because he repeatedly requires me to tailor the paper to the present day suppositions of axiomatic field theory when he is quite cognizant of the fact that this is not possible.

More generally, I am quite appalled that my work should provoke such irrational opposition– the fact that in three years of applications I have been shortlisted only once for a research fellowship; the fact that I have been unable to get anything into print, and the fact that only three institutions out of all the ones I wrote to were even willing to hear me talk on my work, shows an opposition to these new ideas which is far in excess of reasonable scientific scepticism. The curious thing is the Superstring theory, which has caught on so well, has far less scientific basis than my own work. Superstring theory has been arrived at by a process of elimination, i.e. nothing else seems to work. A direct assault on the problems of mathematical inconsistency in field theory, such as my own work, on the other hand never seems to be contemplated.

Yours Sincerely,

C. G. Oakley, University of Sussex, School of Mathematical and Physical Sciences.

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*Professor Taylor wrote to me on 9th June inviting me to give a seminar in July. The offer was never taken up. I sent him further minor corrections on 10th June. I was touched then to receive the following, indicating that he had spent time on my paper:*

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11 June 1987

Dear Dr. Oakley,

May I ask a question about your paper? After eqn. (2), it says that $r \cdot n = 0$ (and $q \cdot n = 0$). Does this condition continue to hold in all the vectors through to (28)? Does $q$ in (29) have to satisfy $q^2 = m^2$? Are there any conditions on the vectors $p_1 \cdots p_r$ in (30) to (36)?

Yours Sincerely,

J.C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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*I have not kept my reply, but I think that the questions were not too difficult to answer*

---

3 July 1987

Dear Dr. Oakley,

I enclose the second referee's report, which responds to your comments.

In view of the referee's reports, and the first referee's earlier, rather negative report, I cannot accept the paper.

If you wished, I would send everything to the Editor in Chief, and ask him to review the decision. But I won't do this unless you specifically request it.

*Referee's report:*

1. I am not erroneous in saying that $\phi_1(x)$ of (18) is not a local field. Indeed, $[\phi_1(x), \phi_1(y)] \neq 0$ if $x - y$ is space-like. The author admits this in his reply.

2. The author does say he is using the fact that if a distribution vanishes outside $p = 0$ then it is proportional to $\delta(p)$. This is a false conclusion.

If he intends to use the correct result, that $pT(p) = 0 \Rightarrow T(p) = K\delta(p)$, is in his reply, then (a) he should alter his text and (b) he should prove that $pT(p) = 0$, which he does not do.

3. He does not show that all infinities are removed, only claims it. This is not good enough.

In conclusion, do not publish this.

*[Ends]*

Yours Sincerely,

J.C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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28 July 1987.

Dear Professor Taylor,

Thank-you for your letter of 3 July. I conclude from this that you are unable to find good scientific reasons for not publishing my paper. The three points that the referee raises are easily answered:

1. The idea that $\phi_1$ defined by

$$(\partial^2 + m^2)\phi_1 = -\phi_0^2$$

satisfying $[\phi_1, \phi_1] = 0$ for spacelike intervals is a necessary condition for the field defined by

$$(\partial^2 + m^2)\phi = -\lambda\phi^2$$

to commute with itself for spacelike intervals is a mere supposition. On the other hand the requirement that $[\phi_0, \phi_1] + [\phi_1, \phi_0] = 0$ for spacelike intervals follows directly from a power series expansion in the coupling.

2. I will add the necessary footnote it the referee desires it.

3. It is simple to see how all infinities are removed by the procedure given in the paper. The procedure removes all infinities within the power series reduction of each interacting field. The only other possible infinities are loops of propagators between proliferator trees. The mass squared of the momentum in these is limited by the total four momentum entering the graph and so the integral is always over a finite phase space. Hence there are no divergences.

The referee has not come up with satisfactory scientific reasons for not publishing the paper, so I do not see that it is necessary to pass the file on to the Editor-in-chief.

Yours Sincerely,

C. G. Oakley, Theoretical Physics Division, Harwell Laboratory, Oxon., UK

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31 July 1987.

Dear Dr. Oakley,

Thank-you for your letter of 28 July. I will see if the referee wishes to comment on the points of physics which you raise.

Yours Sincerely,

J.C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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21 August 1987.

Dear Dr. Oakley,

I attach the second referee's third report on your paper "New methods in Quantum Field Theory", submitted to Z.Phys.C. on 18 February 1987.

I regret that I cannot accept the paper, and I do not feel justified in troubling the referees with any further correspondence. I am sorry.

*Referee's report:*

This paper should not be published.

a). The author does not prove that $pT(p) = 0$, and yet claims that $T = \delta(p)x$ a distribution. The author has had ample opportunity to rewrite this part, but has failed to do so.

b). The author now admits that $\phi_1(x)$ cannot be a local field; but then his Eq (32) exhibits $\phi_1(x)$ as having a local current. It is known that this implies that $\phi_1$ is local, as long as the formula (32) makes sense.

c). The author postulates (Axiom VI) that field commutators are $c$-numbers. This means that double commutators are zero, so his fields come into the class of "Lie fields" if they are local. These are known to give $S$-matrix $= 1$.

d). The finiteness of the theory is in question (apart from the convergence of e.g. the series (32). If the author can, as he claims, "easily" prove finiteness to all orders, he should do it. As it is, only tree graphs are evaluated; not very impressive!

e). It is known that $\lambda\phi^3$ is not bounded below, so any quantum field theory will not have a vacuum if it obeys the equation of the author.

*[Ends]*

Yours Sincerely,

J.C. Taylor, Department of Applied Mathematics and Theoretical Physics, University of Cambridge

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*I gave up on getting the scalar field theory paper published at this point, but had in the meantime sent out a preprint for the quantum electrodynamics case, entitled,* "Quantum electrodynamics without the interaction picture." *I sent a copy to Professor Taylor, who promptly forwarded it to the editor in chief, Prof. Dr. G. Kramer at Universität Hamburg Institut für Theoretische Physik. The first feedback I got from the preprint was from Professor Greenberg:*

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31 August 1987.

Dear Dr. Oakley

I have just come across your preprint "Quantum Electrodynamics without the Interaction Picture". Your approach seems to be similar to what I call the $N$-quantum approach, which is based on the Haag expansion which in turn goes back to work of Heisenberg done in the 1930's. I doubt that renormalization can be avoided, but the method does seem to have advantages, particularly in the treatment of bound states and symmetry breaking, I'm enclosing two recent articles on the method, from which you can trace the literature.

I would appreciate it if you could send me a copy of your article.

Sincerely yours,

O. W. Greenberg, University of Maryland Department of Physics and Astronomy.

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8 September 1987.

Dear Dr. Oakley,

Your paper has been sent to a referee. His report is enclosed. In view of this report, we very much regret that we cannot publish your paper in Z. Physik C.

*Referee's report:*

Motivated by his dissatisfaction with the text-book treatment of perturbative quantum field theory, the author proposes a new formulation of QED which tries to avoid infinite renormalization and the use of the interaction picture.

It seems that the author is not aware of the fact that these problems have been extensively discussed in the past. There exists a "finite" formulation of conventional QED by R.A. Brandt and K. Symanzik (cf. also the work of K. Zimmermann). It is also well understood how to avoid the interaction picture (cf. e.g. the work of J. Glimm and A. Jaffe). So there is no real need for further discussions on these problems.

Besides, already the starting point of the present investigation (relation 2.17) is questionable: there is strong evidence that these "canonical commutation relations" imply that the local fields are free (cf. the work of R.T. Powers and, more recently, of K. Baumann). That there are problems with the relation 2.17 was also realized by the author: he found that the mass of the photon in his model cannot be 0. This raises of course all kinds of questions (gauge invariance, uniqueness, etc.).

As to his treatment of infinities in his approach, the author simply "deletes all the infinite graphs which appear" (p. 19). The justification for this procedure is incomprehensible, and the resulting theory can hardly be local.

Finally it should be mentioned that the method of using the axiomatic constraints imposed on a quantum field theory for a recursive calculation of the higher order corrections to the quantum fields is also quite old and goes back to N.N. Bogoliubov. A thorough perturbative treatment of QED which is based on this idea was given by Ph. Blanchard and R. Seneor.

In view of the various points raised I cannot recommend publication of this paper.

*[Ends]*

Yours sincerely,

G. Kramer, Editor in Chief, Zeitschrift für Physik C

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*I have not kept my reply, but it looks as though it was just to ask for more details on the papers mentioned by the referee*

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21 October 1987.

Dear Dr. Oakley

Thank-you for your letter of 11 September.

A reference converning point (I) is

R. Brandt, Fortschritte der Physik 18 (1970) 249

Earlier references can be found in this paper. For your point (II) a reference is R. Glimm and A. Jaffe, Lectures, Les Houches 1970.

Earlier work is referenced there.

I hope that from these two references you can trace the earlier literature related to the topic of youir paper.

Yours sincerely,

G. Kramer, Editor in Chief, Zeitschrift für Physik C

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27 October 1987.

Dear Professor Kramer,

Thank-you for your letter. I did, in fact find most of the references that the referee was alluding to in his report. None of the papers do anything like as much as is claimed for them in the report. For example, the paper by Blanchard and Seneor is not based on the methods used in my paper, it merely tries to apologise for Feynman-Dyson perturbation theory. What the referee writes is nonsensical. In the context of Q.E.D.

which actually has a calculational framework, neither the problem of Haag's theorem violation, nor that of infinities have been solved. My work definitely solves the former, and is therefore worth publishing.

However, my experience tells me that, more than likely, no amount of petitioning on my part is going to alter your decision, so I will not waste time and energy in doing so. Suffice it to say that you have behaved in a predictably dull and ignorant way, and when– as will certainly happen– it is realised that my work has substance to it after all, you can add your names to the long list of those who did not help me when help was most needed.

Please return the manuscript.

Yours sincerely,

Chris Oakley, Theoretical Physics Division, Harwell Laboratory, Oxon., UK

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*I sent the scalar field theory paper to Physical Review D about this time. They obviously had heard about me, judging from this correspondence:*

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19 November 1987.

Dear Dr. Oakley,

The scope of Physical Review D is restricted to elementary particle physics and astrophysics. It is important to keep in this niche in order to maintain the vitality of the journal and to keep its readership. In general, Physical Review D does not publish papers which are purely of a mathematical nature. Although this rule has been applied somewhat laxly in the past, it is now enforced more fully. It is in view of this general framework that your manuscript, "New methods for quantum field theory" DL3609 is considered to be not suitable for publication in Physical Review D. We are returning it to you and suggest that you consider publishing it elsewhere.

Sincerely yours,

Lowell S. Brown, Editor, Physical Review D.

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24 November 1987.

Dear Dr. Brown,

Thank-you for your letter of 19th November. This paper is certainly unphysical in the sense that it is concerned with $\phi^3$ theory, for which there is no experimental evidence. However, this is the only sense since it goes to the extent of calculating cross sections. The same methods can be used for Q.E.D., and I have written a paper ('Q.E.D. without the interaction picture'– Sussex U. preprint, July 1987) which explains the approach in detail and shows how it reproduces Feynman graph analysis to a large extent. I note, however, that there are two papers in the October 15 'Physical Review D' which deal directly with scalar field theory and so am forced to conclude that whatever your objections to my paper are, they are certainly not this.

I would ask you, therefore to reconsider the case, and if you still do not wish to publish the paper in the journal, then at least provide me with reasons that I am more likely to understand.

Yours Sincerely,

Chris Oakley, Theoretical Physics Division, Harwell Laboratory, Oxon., UK

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11 December 1987.

Dear Dr. Oakley,

I am responding to your letter of November 24 concerning your manuscript "New methods for quantum field theory" DL3609.

As I explained to you in my previous letter, the scope of a journal must be limited in order to keep its readership. It was for this reason that your manuscript was considered to be not suitable for publication in Physical Review D. We are again returning the manuscript to you, and we must now consider the matter closed.

Sincerely yours,

Lowell S. Brown, Editor, Physical Review D.

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23 December 1987.

Dear Dr. Brown,

Clearly you are very anxious that my paper 'New methods for quantum field theory' should not be considered for publication in Physical Review. I am obviously in no position to argue with this, and so as soon as I receive a satisfactory answer to the point raised in my previous letter, I will stop pestering you. To remind you, this was as follows: the only sense in which the paper is unphysical is that it deals with scalar field theory. Since there are <u>two</u> papers on scalar field theory in the October 15 Phys. Rev. D, this cannot be the reason for you refusing to consider my paper. What is the reason, then?

Yours Sincerely,

Chris Oakley, Theoretical Physics Division, Harwell Laboratory, Oxon., UK

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22 January 1988.

Dear Dr. Oakley,

I am responding to your letter of December 23 concerning your manuscript "New methods for quantum field theory" DL3609.

As I explained to you in my previous letter, the scope of a journal must be limited in order to keep its readership. In particular, Physical Review D does not publish papers what are purely of a mathematical nature that deal with old and well-established theories. It was for this reason that your manuscript was considered to be not suitable for publication in Physical Review D, not that it dealt with scalar field theory. We are again returning the manuscript to you, and we must now truly consider the matter closed. Any further communication would be fruitless.

Sincerely yours,

Lowell S. Brown, Editor, Physical Review D.

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*Luckily, this was not the end of the story. Steve Barnett, a Quantum Optics specialist, who I shared an office with at Harwell, contacted his friend Stig Stenholm who was an editor for Physica Scripta. As Steve indicated that the paper was likely to get a friendly reception, I felt safe in re-wording the beginning and end to say* exactly *what I thought about Feynman-Dyson perturbation theory, i.e. that it is garbage and does not deserve to be called a theory at all. Here is the correspondence:*

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25 August 1989.

Dear Dr. Oakley,

Your MS on "Quantum Electrodynamics..." has been seen by two referees, whom I trust quite a bit, and both have taken a positive attitude to the publication. One only recommended acceptance; the other produced a report with some suggestions and questions. As these are all related to the presentation, I choose to interpret them as recommendations only; if you wish to consider them, the MS needs some rewriting. Consider them as comments by a knowledgeable reader, if based on misunderstandings, these may be avoidable. If you choose not to do anything, let me know and I will forward the paper without further delay. The paper has been around long enough that it deserves to get out. Let me know fast, by mail or electronic mail (STENHOLM@FINUHCB), and I can act immediately.

*Referee's report:*

The author builds QED on the basis of expansion of fields in powers of the coupling constant; higher order terms being obtained from the prescribed form of the commutator.

I have the following comments:

- It is not quite true that field theory is exactly in the same shape as it was in the late twenties. There are counter-terms procedures based on locality and unitarity (Bogoliubov and Shirkov) or on analyticity and unitarity (e.g. Vol. IV of Landau, Lifschitz course) and there have been attempts to formulate QFT in Heisenberg picture (reviewed in Bethe, Hoffman, Schweber, Vol. I or in Schweber's book).
- It is not clear to me, whether the procedure proposed by the author will lead to a theory satisfying the unitarity condition.

Recommendations

I recommend publication of this paper, provided that the author makes amendments in which

- previous work on the subject is mentioned and relationship of author's approach to the work briefly analysed
- the situation with fulfilment of unitarity in this approach is at least briefly discussed.

*[Ends]*

Sincerely yours,

Stig Stenholm, University of Helsinki, Institute for Theoretical Physics.

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*I elected to have the paper published in its original form. It appeared in Physica Scripta in early 1990, by which time I was working for a software company in London. It is now June 2006. I have not had substantial feedback and the paper has never been cited. To me, this is proof that even in a community that consider themselves among the cleverest in the world there is an inbuilt pathological conservatism. I believe that this can be blamed on the fact that the reins of power are held almost exclusively by old men. The paper that "proves" that there can be no local interacting fields in 3+1 dimensions actually does nothing of the sort. This paper: "The Inhomogeneous Wave Equation in Local Relativistic Quantum Field Theory", by A.S. Wightman and H. Epstein, Annals of Physics* **11***, 201-239 (1960) –* not 1963, by the way – *does in fact allow for the possibility that their suppositions may not be correct but the remainder of the "axiomatic" community seems to have discounted the possibility to the extent that they do not think that even re-examining the conclusions is a good use of time. For me, it was a bit like taking a ferry to visit someone and then being told upon arrival that you could not possibly be there because the ferry was made of steel and steel does not float. These old dogs obviously were not willing to learn any of my new tricks.*

*I would like the reader to note the consummate ease with which this dissenting voice (me) was silenced. I would like them also to be aware of the fact that – apart from developments that have not and may never have a connection to the real world (e.g. Supersymmetry) – the quantum field theory taught to graduate students now is substantially the same as that developed by Feynman in 1949, with all the problems that go with it. They seem to be content with this nasty mess, and they deserve no better.*