



Fermilab

Feb 27, 1985

Dear Prof. McDonald,

I've enclosed the Salam paper. It's interesting but probably irrelevant with respect to the standard KM model of CP violation. It relies upon the existence of a small vacuum expectation value of  $O(45 \text{ meV})$  to generate CP. The standard Higgs is of ~~an~~ order 175 GeV. Thus, if  $10^{16}$  gauss ( $\approx 10^{18}$  volts/cm) is an effect of order ~~the~~ 45 meV then the shift in the electron mass would be  $\sim \frac{45 \times 10^{-3}}{175} \approx .28 \times 10^{-3}$ . Of course we don't know the fermion mass generation mechanism and one should reexamine these ideas more closely.

Chris Quigg points out <sup>possible</sup> relevant work of order 4 yrs ago by Abe Cline and Rafuelshi (sp?). He claims there is a related Scientific American article by them. I didn't check this out.

I've enclosed my most recent preprint. Please feel free to call upon me if you have further questions.

Regards,  
Chris Hill

Princeton University

DEPARTMENT OF PHYSICS: JOSEPH HENRY LABORATORIES  
POST OFFICE BOX 708  
PRINCETON, NEW JERSEY 08544  
Telephone: 609 452-6608 Telex: 499-8512

March 5, 1985

Dr. Joseph Eberly  
Dept. of Physics and Astronomy  
Univ. of Rochester  
Rochester, NY 14627

Dear Dr. Eberly,

Enclosed is a paper summarizing some of my thoughts on the possible use of high powered lasers in elementary particle physics, which we discussed in a phone conversation in January. If this subject is of interest to the laser group at Rochester I would welcome the opportunity to visit and discuss the matter.

EBERLY

MAX-PLANCK-INSTITUT FÜR QUANTENOPTIK  
FORSCHUNGSGELÄNDE · D-8046 GARCHING b. München/Germany

DR. McDONALD -

I THINK I MENTIONED IN OUR TELEPHONE CONVERSATION THAT I WOULD BE IN GERMANY DURING MOST OF THIS YEAR. IN ROCHESTER YOU MIGHT HOWEVER CONTACT PROF. P.W. MILONNI WHO IS INTERESTED IN THE DAVIES-UNRUH EFFECT. YOU CAN WRITE TO HIM AT THE ADDRESS USED FOR ME ABOVE.

YOURS TRULY,  
J.H. EBERLY  
M.P.I. FÜR QUANTENOPTIK

The University of Washington

Dept. of Physics, FM-15  
Seattle, Washington 98195

Lowell S. Brown  
Professor of Physics  
[206] 543-8774

March 11, 1985

Professor Kirk McDonald  
Department of Physics  
Joseph Henry Laboratories  
P.O. Box 708  
Princeton, NJ 08544

Dear Professor McDonald:

Thank you for sending me your preprint, "Fundamental Physics During Violent Accelerations". I'm afraid I don't have too much to say in response to your letter inquiring about the electrodynamic equivalent of the Hawking effect, only that: (1) Your diagram has a loop corresponding to a virtual  $e^+e^-$  pair. I don't see how this corresponds to your semiclassical calculation since there are no such loops in the effective thermal Unruh radiation caused by the acceleration. (2) If the effect is right, shouldn't it appear in the synchrotron radiation by a very high energy electron? I would think that people have calculated this with QED.

Thanks again for the preprint.

Sincerely yours,



Lowell S. Brown

INSTITUTE FOR THEORETICAL PHYSICS

University of California  
Santa Barbara, California 93106

Telephone: (805)-961-4111

March 21, 1985

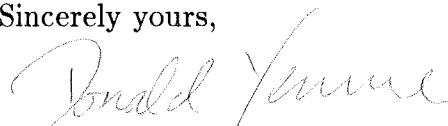
Dr. Kirk McDonald  
Department of Physics  
Joseph Henry Laboratories  
P.O. Box 708  
Princeton, NJ 08544

Dear Dr. McDonald:

Thank you for sending me a copy of your paper. I found it to be fascinating. Unfortunately, there is no easy way for me to answer your questions without a great deal of effort, which I cannot supply at this time. However, later this spring I'll be spending a portion of my sabbatical leave at SLAC; and I'll show your paper to Stan Brodsky, who has had some interest in strong field QED problems. If we have any interesting thoughts, I'll let you know.

Another experimentalist who has had a long time interest in the question of an electron in an extremely strong field is Tom Erber (Illinois Institute of Technology). He has been studying the bremsstrahlung produced when an electron is deflected by a strong magnetic field which is produced by an implosion. It may be worth while communicating with him.

Sincerely yours,



Donald Yennie

DY:cls



Professor Kirk McDonald

4/5/85

Princeton University

Physics Dept

P.O. Box 708

Princeton, N.J. 08544

Dear Professor McDonald

I just received your preprint "Fundamental Physics During Violent Accelerations" and also your note. I am very delighted to know that you have both an experimental and a theoretical interest in the physics of violent accelerations. Your idea of using the electric field of a laser to accelerate an electron sound very interesting. It is true that the vacuum starts already breaking down before the electron radiates more thermal radiation than bremsstrahlung. Whether

(2)

electron positron production will obscure thermal radiation scattering off the accelerated electron is not clear.

I have not yet read your preprint as carefully as I am going to. Thus I may have missed a key factor to make your scheme work, namely what is frequency of the laser? Will the frequency be low enough so that the accelerated electron can establish thermal equilibrium before the electric field reverses direction? In other words, is the inverse frequency of the laser long compared to the fermi-walker time, during which the electron ~~can~~ gets its velocity changed by an amount about equal to the velocity of light?

Let me emphasize in passing that an accelerated electron will see thermal radiation only if it has a horizon. Because such a horizon is absent relative to the circulating electron

considered by Bell and Leinaas, the (3)  
impression they create (namely that the Davies  
-Unruh temperature is being measured) is erroneous.

I agree with your assessment that quite a bit of theoretical work has to be done to add respectability, both theoretical and experimental, to the physics (quantum mechanics of a field) relative to accelerated observers. Although we all must be delighted that D&U. gave us their formula, especially to Unruh, who pointed out its physical relevance, we really are totally in the dark what is behind it. I must however say that I now have a much better understanding of what goes on than I did one year ago. I believe that once we understand the

framework behind the D-U formula  
 then it should not be too difficult to convincingly  
 by answer the physically very relevant  
 questions you mention in your note.  
 Please keep me informed about your  
 latest thinking about your thoughts  
 on the subject.

Best wishes,  
 Ulrich H. Gerlach

P.S. I am very much impressed by the word  
 processing system you are using. What is it?  
 Also, what [printer] kind of are you using?

P.P.S. 1 1/2 years ago I was talking with Richard  
 Gott (Astrophysics Dept) about the accelerated  
 observers and related topics. Say hello to him  
 if you happen to see him.





April 15, 1985

DEPARTMENT OF PHYSICS  
LOS ANGELES, CALIFORNIA 90024

Professor Kirk McDonald  
Department of Physics  
Princeton University  
Post Office Box 708  
Princeton, NJ 08544

Dear Professor McDonald:

Thank you very much for your letter of the 26 March and the accompanying preprint "Fundamental Physics During Violent Accelerations." Apparently we didn't meet at the UCLA Laser Workshop last January because you didn't attend --- that's a pity because meaningful discussions of non-linear quantum electro-dynamics are not quite letter size. Nevertheless, I'll try to make some helpful comments on your MS taking the topics in reverse order:

- (0) Non-linear Thomson Scattering (mass-shift and harmonics). These problems have been raked over most reliably and carefully by H. Mitter and his students (Becker & Mitter, J. Phys. A9, 2171 [1976], and many later papers; W. Holtmann, Tübingen Thesis, 1981). Building on the Volkov solutions, all these effects can be worked out in quantitative detail. See also Ritus, Lebedev #45, 1981.

Following Milburn's suggestions of 30 years ago, laser back scattering has been running routinely at SLAC to furnish 9 GeV polarized photons for hadron experiments. With higher intensities and tighter focusing it is in principle possible to reach the harmonic regime. But many independent --- albeit unpublished --- estimates show that the number of harmonic photons is much too feeble to be of any technical relevance. The conceptual interest in these types of QED tests is essentially nil. Try it out on Adler!

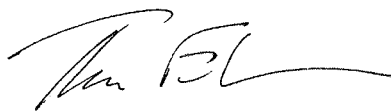
- (1) 'Stimulated' pair creation. This is really pair creation by photons traversing a background field. As you know, many careers have been built on wholesaling this process in pulsar physics. I'm an enthusiastic proponent of experiments of this kind, but the Adler's of this world consider that conventional pair creation in Coulomb fields is enough; so what's the point of substituting either laser fields or magnetic fields for Coulomb fields?

Again, many people have looked into the technical details of setting up an experiment of this kind. Even if one is very optimistic about the focusing and time synchronization, the signal-to-noise is too formidable for foreseeable laser devices. (But try 20 TeV  $e^-$  on 2 MG fields!)

- (2) Spontaneous pair creation. Yes, the old Klein 'paradox' lives on, but there isn't a prayer of demonstrating macroscopic field fragmentation with any device within grasp. The closest one can come is with transient 'super-critical' fields in heavy ion collisions. Greiner and many associates have raked this over in scrupulous detail, and perhaps the 'diving' effect has even been seen experimentally. Ironically, the more elaborate the calculations have become, the more conventional the underlying QED appears.
- (3) Light-light scattering. Another old story plagued by signal-to-noise problems in all experimental designs proposed to date for lasers. Since the box diagram is so intimately linked to vacuum polarization corrections --- these are quite large and confirmed in mu-mesic atoms --- Adler would again claim that this is a superfluous effort.
- (4) Unruh radiation. I quite agree with you that this is an exciting direction for research because there may be unresolved questions of principle involved. I don't think anybody yet even knows how the Casimir force between conducting plates would be affected. From this point of view any advance towards higher accelerations would be an interesting preliminary step.
- (5) Intense field effects in Vacuum Polarization. This topic isn't mentioned in the paper but appears in your letter. The most reliable work on an 'engineering' consequence of these effects is by Herold et al. (Phys. Rev. Lett. 54, No. 13, 1452-1455, 1 April 1985). Similar matters are discussed in Annals of Physics 102, No.2, 438-441, December 1976. —

In summary, what can really be said? Experimental QED has been quiescent for nearly two decades. All the excitement and fresh enthusiasm has flourished in the astrophysics community --- especially among those active in pulsar physics. All attempts to regain an experimental foothold for QED at front line accelerator facilities have failed. When experiments have been mounted, they are technically subordinated to device development (channelling radiation), or pathetic misadventures (attempts to detect magnetic vacuum birefringence). Laser physics may finally be the vehicle for restoring the connection. It is ironic that the right thing may finally be done for the wrong reason. All experimental and theoretical evidence available --- and this includes your paper --- indicates that high energy accelerators and megagauss targets are still the optimum route to new aspects of QED.

Best of luck,



T. Erber  
Visiting Professor



DEPARTMENT OF PHYSICS  
LOS ANGELES, CALIFORNIA 90024

May 22, 1985

Professor Kirk McDonald  
Department of Physics  
Princeton University  
Princeton, New Jersey, 08544

Dear Professor McDonald:

Thank you for your letter of 1 May. Let us take the points in order:

1. The electrical counterpart of the 'critical' field strength is

$$E_{cr} = \frac{m^2 c^3}{eh} \approx 1.323 \times 10^{18} \text{ Volt/meter}$$

You can easily check that fields of this magnitude enter in mu-mesic atomic spectra: in the K-shell

$$E_{\mu} (1s_{1/2}, Z) \approx 1.66 \times 10^{-2} Z^3 E_{cr} > E_{cr} \text{ for } Z \gtrsim 20.$$

2. QED experiments, in practise, depend not only on the ratio of (Field/Critical Field), but also on characteristic energies, times, volumes, etc. My thinking still inclines me to megagauss targets, but hopefully I have also made it clear that the first priority is to get some type QED work going again at a high energy machine. If this turns out to be some type of laser experiment... why not?!? I started out in megagauss physics by letting my research collaborators adjust the trigger logic on our nine 556 scopes, while I cleaned the mouse droppings from the cable raceways and saw to the final hook-up of the detonators on 12 kgms of 'comp-B' HE. The years have not dimmed my enthusiasm, and if all I can do is turn some screwdrivers on your set-up, I'd be happy to do just that!
3. My earlier letter contained explicit references only to the first and last work of Mitter and co-workers. The complete file is back at IIT. You can call Becker directly at University of New Mexico, and I'm sure he'd supply you with the entire bibliography.

Mitter and his group considered three interlinked problems: (i) The existence of an effective mass shift of an electron in a laser field --- this had been the object of extended but sterile polemics between Kibble, Fried, and others. (ii) 'Overtones' in Compton scattering --- this has also been done by Townes and others. (iii) What, if anything, is peculiarly quantum mechanical about the operation of 'free electron' lasers? I stand by my earlier recommendation that Mitter's discussions of these issues are sound and sensible.

4. The problem of absorbing a unique number of photons --- mentioned on p.2 of your 1 May letter --- is tied in with the uncertainty principle. This situation is discussed by Carruthers and Nieto in Rev. Mod. Phys. 40, 411 (1968).

5. Many people who set out to test QED haven't really thought through what is meant by a divergence between theory and experiment. Isn't it true that Bhabha scattering at 750 MeV disagrees with QED? But that hasn't implied the 'breakdown' of anything, has it? We still keep renormalizing away!

Best regards,

A handwritten signature in black ink, appearing to read 'T. Erber', with a long horizontal flourish extending to the right.

T. Erber  
Visiting Professor



5/23/85

Dear Prof. Mc Donald:

Many thanks for the copy of Nathan Myhrvold's Ph.D. thesis. I am having lots of fun going through. It also is very useful to me.

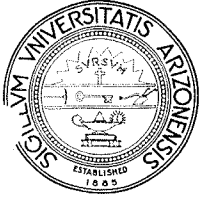
Are event horizon's necessary for the existence of Unruh's thermal ambience. Strictly speaking, yes; approximately speaking, no: only ("temporary") apparent horizons are necessary. Qualitatively one can argue that as a consequence a thermal ambience is impossible for a circularly accelerated observer. Roughly speaking his apparent horizon cannot be "big enough" to allow an approximation in terms of event horizons. ~~See~~ I have elaborated on this

a bit in chapter X of the enclosed manuscript.

Please let me know what your feelings are towards Hellmuth's ambience in the context of circular acceleration.

Sincerely

Ulrich H. Gerlach



THE UNIVERSITY OF ARIZONA  
TUCSON, ARIZONA 85721 USA

COLLEGE OF ARTS AND SCIENCES

FACULTY OF SCIENCE  
DEPARTMENT OF PHYSICS  
BUILDING #81  
(602) 621-6820

1 June 1985

Dr. Kirk T. McDonald  
Joseph Henry Laboratories  
Princeton University  
Princeton, New Jersey 08544

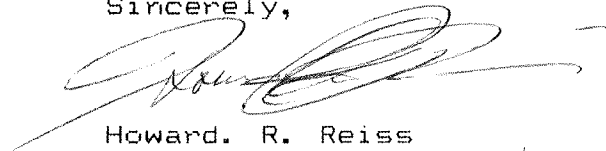
Dear Dr. McDonald:

I heard recently from Peter Milonni that you were interested in the effects of very intense laser fields on free electrons. He showed me a paper of yours entitled "Fundamental Physics during Violent Accelerations". I would appreciate having a copy of this paper.

I noticed that you referenced a paper of mine on the subject of photon-photon pair production. Enclosed are two later papers on this same subject that I wrote when it appeared to me that laboratory detection of the effect would become possible. I believe that such an experiment could be done now, though I am not aware that any plans to do so exist.

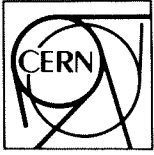
I maintain a strong interest in this subject, and I would like to hear from you about further developments.

Sincerely,



Howard. R. Reiss

Enclosures



ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE  
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

SIÈGE: GENÈVE, SUISSE

CERN  
CH-1211 GENÈVE 23  
SUISSE/SWITZERLAND

Téléphone: GENÈVE (022)  
Central/Exchange : 83 61 11  
Direct : 83

Votre référence  
Your reference

Notre référence  
Our reference

30 May 85

I managed to dig this up in a notebook full of papers on various Non-Linear Electrodynamics topics. I'll try to pick out anything useful and send it along. In the meantime, perhaps the enclosed notes might be useful (if for nothing else than some references).

I had forgotten how long ago I had worried about these things, and would ask you to please be so kind as to overlook both the trivialities and poor writing style of the attached.

Hopefully, this can be of some little assistance.

Best regards,

A handwritten signature in cursive script, appearing to read "Peter Denes".

Peter Denes





# University of Illinois at Chicago

College of Liberal Arts and Sciences  
DEPARTMENT OF PHYSICS

Post Office Box 4348  
Chicago, Illinois 60680  
(312) 996-3400

4868

15445

July 2, 1985

Professor Kirk McDonald  
Department of Physics  
Joseph Henry Laboratories  
P.O. Box 708  
Princeton, NJ 08544

Dear Professor McDonald:

Thank you for your letter of June 21, 1985 and the preprint entitled "Fundamental Physics During Violent Accelerations."

As far as the laser technology goes, the enclosed preprint of an article to be published in Science provides some information. For this, you can consult the last paragraph on page 3 and Fig. (3). Clearly the time is coming quite close for the experimental observation of some of the nonlinear effects that you have discussed, particularly, if accelerator technology is available.

Also enclosed is a recent Physical Review Letters which, in its last paragraph, considers intra-atomic pair production by the trident graph. All sorts of effects should become observable with the femtosecond sources that are currently being developed.

Very truly yours,

A handwritten signature in cursive script, appearing to read "Charles K. Rhodes".

Charles K. Rhodes  
Research Professor  
Department of Physics

ENCL: (Science, to be published; Phys. Rev. Lett. 54, 1490 (1985).)

CKR:ksb



July 31, 1985

DEPARTMENT OF PHYSICS  
LOS ANGELES, CALIFORNIA 90024

Professor Kirk McDonald  
Department of Physics  
Princeton University  
P.O. Box 708  
Princeton, NJ 08544

Dear Professor McDonald:

How are the QED adventures progressing? Any prospects for experiments?

I thought of you recently when one of our leading astrophysicists jangled my bell: He was thinking about strings on a truly cosmic scale --- say 30 light years long and  $10^{-19}$  cm in diameter. Through various schenanigans not worth repeating, these objects are supposed to generate highly non-uniform fields with intensities going up to  $10^{19}$ G. Of course he wanted a complete reading on all the associated QED: pair production, synchrotron radiation, etc. He wasn't happy when I couldn't (wouldn't?) supply the details!

At any rate, I have a very much lower level 'from-here-to-there' suggestion: During the 26-28 October '85, I'll be at Amherst, Mass. for a Meeting of the American Mathematical Society. It would be possible to stop by at Princeton on the way from Chicago; say 23-25 October. We could talk non-linear QED, and it might be amusing to renew old acquaintances with Treiman. If there is any interest, I'd be delighted to give a talk --- our current offerings include 'Order out of Chaos', 'Hysteresis', and of course 'Synchrotron Radiation'.

Regards,

T. Erber  
Visiting Professor

# ILLINOIS INSTITUTE OF TECHNOLOGY

Lewis College of Sciences and Letters  
Department of Physics

September 6, 1985

Professor Kirk Mc Donald  
Department of Physics  
P.O. Box 708  
Princeton, New Jersey 08544

Dear Professor Mc Donald:

It is nice to see that you are making progress in approaching SLAC with a specific proposal on QED. The 'Abstract' has a conservative tone that should encourage support. (Some typos are indicated on the enclosed copy.) You might find it helpful to read through 'Experimental Aspects of Synchrotron - Čerenkov Radiation' (Annals of Physics, 102, No. 2, 405 - 447, 1976): this article lays out all essentials with plenty of numbers supplied for purposes of experimental design. Probably the material in Sections 7 and 8 will interest you most.

Another useful reference is 'Quantum Electrodynamics and Channeling in Crystals' by J.C. Kimball and N. Cue (Physics Reports, 125, No. 2, 70-101, August 1985). This is the same theme over again but with crystals supplying the external fields. Note that we might be heading for a triad at SLAC:

1970 - megagauss targets

1983 - crystal field targets

198? - laser targets

It would be a very good idea to develop an overview of the prospects for strong field QED, and a talk at Princeton is a step in the right direction. Leave the 'stringers' to their own devices: but there are a number of other people who have had a continuing interest in these problems, know about our work, and who might want to attend the talk and join in the discussion --- particularly Ostriker from Astrophysics and Dyson from the Institute.

Unfortunately, the constraints of time look much more severe when seen from IIT rather than UCLA. I could reach Newark airport by the afternoon of Thursday the 24th of October, and barring bad weather, arrive at Princeton in time for dinner. The talk could be scheduled for Friday, 25 October. Friday evening I would have to be on the way to Amherst.

Best regards,



Thomas Erber



National Research Council  
Canada

Conseil national de recherches  
Canada

Division of Physics

Division de physique

18 September 1985

Dr. K. McDonald  
Joseph Henry Laboratory  
Princeton University  
Princeton, N.J. 08544

Dear Kirk:

I would like to formally extend an invitation to you to present a physics colloquium at N.R.C. I think there would be a lot of interest in your ideas on physics during violent acceleration. Last year we had Dr. Unhrah talk about physics in the vicinity of black holes. That some of the ideas that he discussed might be accessible in the laboratory is very exciting.

Physics colloquia at N.R.C. are attended by physicists of diverse backgrounds. Attendance is typically approximately 50 scientists. The talk therefore, should be aimed at a general physics audience.

N.R.C. will cover all of your expenses, including round trip economy air fare from Princeton. Perhaps I can telephone you in early October and set a firm date for your talk.

Kirk, I look forward to seeing you again and hearing about the further evolution of your ideas.

Yours truly

Paul Corkum  
Laser and Plasma Physics Section

PBC:ck

*Kirk, Please let me know about flight information. See you soon*

*Paul C*

PHYSICS COLLOQUIUM  
NATIONAL RESEARCH COUNCIL

K.T. McDonald  
Joseph Henry Laboratories  
Princeton University  
Princeton, N.J.

Fundamental Physics During Violent  
Acceleration

Physique fondamentale durant une  
accélération violente

The Hawking radiation of a black hole has an analogue in the radiation of an electron undergoing very large acceleration. The development of high peak power ultrashort pulse lasers will allow such giant accelerations to be achieved in the laboratory, and novel radiation effects to be explored. This prospect renders accessible a number of interesting phenomena due to vacuum polarization, such as pair creation by light, light by light scattering and vacuum Cherenkov radiation.

La radiation d'un électron à une très grande accélération est analogue à la radiation Hawking d'un trou noir. Le développement de lasers à impulsion ultra-courte à haute puissance de pointe va permettre la réalisation de telles accélérations géantes en laboratoire et l'exploration de nouveaux effets des radiations. Cette perspective rend accessible nombre de phénomènes intéressants dus à la polarisation du vide telles la création de paires par la lumière, la diffusion lumière-lumière et la radiation Cherenkov dans le vide.

Friday, December 6, 1985  
at 3:15 p.m.

Vendredi 6 décembre, 1985  
à 15 h 15.

Seminar Room  
Physics Building, M-36  
National Research Council  
Ottawa, Ontario

Salle de conférence  
Édifice de physique, M-36  
Conseil national de recherches  
Ottawa, Ontario

Coffee and doughnuts will be served  
at 3:00 p.m.

Du café et des beignés seront servis  
à 15 h.

CONFÉRENCE DE PHYSIQUE  
CONSEIL NATIONAL DE RECHERCHES

KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK

A-8010 Graz, Universitätsplatz 5  
Austria  
Prof. Dr. H. Mitter

Graz, den Nov. 7<sup>th</sup>, 19 85  
Telefon (0316) 380 - 52 25  
Telex 31662

┌ Prof. Kirk Mc Donald ─┐  
Dept. of Physics  
Joseph Henry Labs.  
Princeton University  
POB 708  
Princeton, N.J. 08544  
└ U S A ─┘

Dear colleague,

thank you very much for your letter from Oct. 23<sup>th</sup>. It seems, that high-intensity QED develops from a "field for insiders and lovers" into something accessible by experiment, and I appreciate this very much! In fact, I have been told by experimentalists long ago to watch out for (sub-) pico-second lasers, but this is hard for me to do, since I am a pure theoretician. - In the past few years I had almost no time to do physics, since I had to serve as a rector of my university. The university is both old (we had the 400<sup>th</sup> anniversary this year) and large (25000 students), so this was a "tremendous" job. My period of office ended on Oct. 1<sup>st</sup>; now I have to serve for another year as vice-rector and then I am back to normal again. Already now I have much more time for physics. So your letter arrived in the right moment: I am just trying to make up my mind, where I should start again. In addition, I have to look around, where I shall spend the sabbatical semester, to which I am entitled after Oct. 1<sup>st</sup>, 1986.

As far as physics is concerned, I would like to ask some questions. The first concerns the polarization of the pulse laser you have in mind. Is it possible to do the experiments with a circularly polarized beam? From the standpoint of the

theory this is to be preferred, since all formulae become shorter by orders of magnitude. More complicated processes cannot be calculated at all for linear or elliptic polarization. The reason for this is an additional symmetry, which is present for circular polarization and makes life much easier for theorists.

Another question concerns nonlinear effects as light-by-light scattering, photon splitting etc. If one treats the laser field as an external field  $A^{(e)}$  (as most people do, including Becker and myself), one has to consider an arbitrary number of laser quanta in any process. The vacuum polarization effects are then contained in the polarization current, which is the source term  $j_\nu$  in Maxwells equation

$$\partial^\mu (\partial_\mu A_\nu - \partial_\nu A_\mu) = j_\nu(A, A^e)$$

Here  $A$  refers to an em. field different from  $A^e$  ( $A \neq A^e$ , i.e. not the laser field).  $j$  depends both on this field and the external field  $A^{(e)}$ . The latter polarizes virtual pairs, the former feels this as a "probing" field. The current has an expansion in  $e$  (elementary charge)

$$j_\mu = e \pi_\mu + e^2 \pi_{\mu\nu} \cdot A^\nu + e^3 \pi_{\mu\nu\sigma} \cdot A^\nu \cdot A^\sigma + \dots$$

where each  $\pi = \hat{\pi}(A^e)$  has to be computed to all orders in  $A^e$  (i.e. without expanding in powers of the coupling to  $A^e$ : for  $A^e = A(\text{Laser})$  this expansion would correspond to a power series in  $\nu$ ). The first term  $\pi_\mu$  is zero for a laser field (but not for other external fields). This fact has been proved by Schwinger long ago and is sometimes misinterpreted: the refractive index  $N$  is not one (this would hold only for  $A^e$  itself). The next term was computed by Becker and myself for  $A^e = A(\text{Laser})$ . It amounts to a complex  $N$  caused by  $A^e$  and felt by  $A$ . Thus it describes scattering of light ( $A$ ) by laser light ( $A^e$ ) (including pair creation contained in the absorptive part  $\text{Im } N$ ), both with an arbitrary number of laser quanta involved: they appear summed up, cf. p. 1650 of our paper.

Only the next term  $\Pi_{\mu\nu\varrho}$  would describe fission or fusion of (non-laser) photons in presence of the laser field. For such a process one needs three photons. In the formula given above one of them is contained in  $A$  on the left-hand side, the other two on the right (the whole theory is of course symmetric).  $\Pi_{\mu\nu\varrho}$  has not been computed so far. This could perhaps be done - at least for circularly polarized  $A^e$  - but it has to be noted, that the term in the current contains an additional factor  $e$  in comparison with  $\Pi_{\mu\nu}$ . The rates for splitting/fusion are therefore smaller by a factor  $\alpha = e^2/hc$  in comparison with the - already very small - rates for scattering of light by (laser) light. Do you believe, that they can nevertheless be observed? Experimentally they could be distinguished by the dependence on the photon energies, but if the rate is too small one would never see the process. Before I (or somebody else) embark in the long calculation, a critical examination of the experimental situation should be done.

Processes, in which more than two laser quanta are scattered out, are contained in principle in  $\Pi_{\mu\nu}$ : one expects terms containing  $\delta(p-p'+2rk)$  ( $r=0,1,2,\dots$ ) in the propagator. For circular polarization we can have only  $r=0,1$  and no higher value due to the symmetry discussed in appendix 2 of our paper. This symmetry is not related to Furry (which leads essentially to the 2 in the  $\delta$ -function). For linear polarization one would expect also higher values of  $r$ , but the calculation of  $\Pi_{\mu\nu}$  cannot be done explicitly.

About the factor  $1/4$  I have to reflect. An error in the calculation is not probable, since we were as careful as possible, but one can of course never be sure.

Your remark about Čerenkov radiation is interesting. I remember, that Becker has thought about such problems. Since he is at present in the US (address: Dept. of Physics and



Astronomy (Inst. for Modern Optics), University of New Mexico, Albuquerque, NM 87131) you might contact him directly. He has continued to work on problems in intense em. fields. I hope you dont mind if I send him a copy of your letter - we were in good contact all the past years.

I hope, that this long letter is useful for you. Maybe I can visit you next year, since I shall spend my sabbatical certainly in the US. Do you think, that there is some chance at Princeton University (I would need little or no money from US sources)? To whom should I write? Thanks again for your letter!

Sincerely yours,

A handwritten signature in cursive script, appearing to read "K. J. Hillier". The signature is written in dark ink and is positioned below the typed name.

# STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address: Bin 81*  
SLAC, P.O. Box 4349  
Stanford, California 94305

November 25, 1985

Professor Kirk McDonald  
Professor of Physics  
Princeton University  
Post Office Box 708  
Princeton, New Jersey 08544

Dear Kirk:

I am responding to your letter of November 8, 1985, on non-linear laser-beam scattering using the SLC beam. The right people to talk to here would be Joe Murray, who designed the present laser back scattering beam line, and Dave Fryberger, who is supposed to know everything about our long term experimental program. In any preliminary discussions you have with people here, we should try and understand just what the competition is between kinds of experiments you propose and high energy physics operation of the SLC. The  $e^+e^-$  work is our highest priority and I discourage people from submitting proposals that would take a significant amount of running time away from that kind of work. In any event, start off with Murray and Fryberger and we'll see where we go from there.

Best Wishes,



Burton Richter, Director

nh

cc: Joe Murray, Dave Fryberger, Richard Taylor

Princeton University

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December 4, 1985

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
A-8010 Graz  
Austria

Dear Prof. Mitter,

Thank you for your reply of 7 Nov. to my inquiry. Before turning to some technical remarks, may I comment about the possibility of your visiting Princeton next year. This appears to be an excellent prospect from my point of view. I have discussed this with the Physics Department Chairman, Sam Treiman, whom you could write directly if you care to. Since you require no financial support from Princeton, the path is straightforward. You could be appointed a Visiting Fellow for the Fall semester, 1986, following a brief review by the senior faculty here. For this we need a *curriculum vita* and a publication list. Please indicate the status of your visa for the U.S.A. An appointment as Visiting Fellow regrettably includes no allowance for travel expenses, subsistence, or stipend.

I enclose a copy of a draft of a proposal for your appointment (which I have shown to Prof. Treiman). It is useful to submit the proposal soon, so there will be time to aid you with visa formalities, housing and travel arrangements...

Your presence here for a term would be very stimulating to the study of strong-field electrodynamics, as my theoretical colleagues spend all their time in 10 dimensions these days.

My technical comments are brief. While the generation of femtosecond laser pulses involves use of linear polarization, this can be switched to circular polarization with a quarter-wave plate prior to the last stages of amplification. It will clearly aid in the interpretation of the results to do so.

Regarding light-by-light scattering, are there two types to be distinguished? On p. 3 of your letter, I believe you refer to an external photon absorbing an even number of laser photons to form the 'scattered' final state. For external photons of GeV energy this is hardly distinguishable from 'no scattering.' The other form is for the external photon to absorb an odd number of laser photons, and then 'split' into 2 final state photons which

share the initial energy. It is calculations of the latter for 3, 5... laser photons which I believe to be of experimental interest.

In the optics literature I find reference to 'Čerenkov-like' radiation induced by travelling pulses of polarization density, themselves caused by a strong light pulse traversing a medium (Kleinman and Auston, IEEE QE-20, 964 (1964) and references therein). I presume this effect should have an analogue for external photons in a strong wave field—a 'shock-wave' variation of light-by-light scattering...

The reports from Darmstadt of anomalous  $e^+e^-$  production in heavy-ion collisions are quite intriguing. If this is a true strong-field effect, it should also be accessible via the laser technique..

I note that you have written about modifications to Møller scattering in a strong background field, which are most pronounced for very low energy electrons. This will be difficult to explore in the lab, because of the 'field-gradient' forces of Kibble. Roughly, an electron cannot penetrate into a high field region (from a region of zero field) unless its momentum is greater than  $m\nu/c$ , so for fields with  $\nu \sim 1$ , the electrons must have relativistic velocities or they will be deflected.

If you do visit us we could more comfortably discuss these and other issues. Please come!

Best wishes,

Kirk McDonald

# Los Alamos

Los Alamos National Laboratory  
Los Alamos, New Mexico 87545

DRP/FST, MS-E527

December 19, 1985

Mr. Kirk McDonald  
Princeton University  
P. O. Box 708  
Princeton, NJ 08544

Dear Mr. McDonald:

Enclosed is a copy of the Statement of Work for the Krypton Fluoride laser procurement that you requested. Should you wish to discuss this further, please call me at 505-667-5312.

Sincerely,



Charles Fenstermacher

CF:dh  
Enc: a/s

Cy: C. Fenstermacher  
CRMO, MS-A150 (2)



THE UNIVERSITY OF NEW MEXICO  
ALBUQUERQUE, NEW MEXICO 87131

December 18, 1985

Prof. Kirk McDonald  
Department of Physics: Joseph Henry Labs.  
Princeton University  
P. O. Box 708  
Princeton, N.J. 08544

Dear Prof. McDonald:

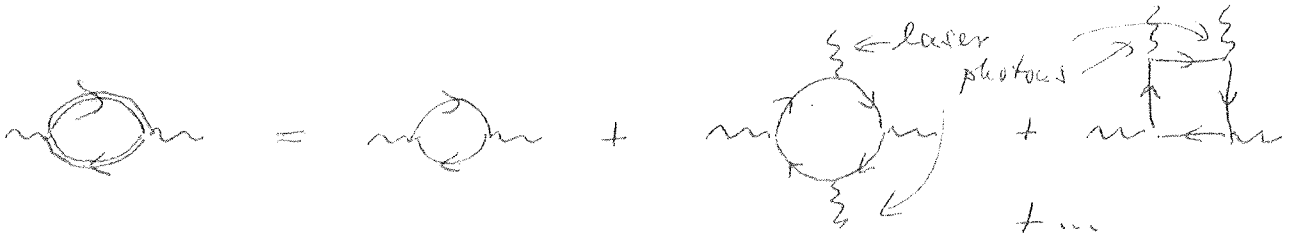
Thank you for your letter of December 3. I am very excited to learn that some of these intense field QED effects which so many people have spent so much time thinking about may finally be subject to experimental verification. This is particularly so since the efforts in Darmstadt with respect to the instability of the vacuum in strong Coulomb fields are obviously not getting anywhere.

The first effect which you mentioned you would address, viz. high-intensity Compton scattering, is actually the same on which the free-electron laser relies, the only difference being that in case of the FEL the kinematic conditions are such that everything is classical for all practical purposes while for your parameters the quantum recoil already becomes appreciable. Incidentally, the estimates of rates of harmonic emission which I gave to Tom Erber I drew from a paper of mine on free-electron lasers. As to the Breit-Wheeler effect, is it described by the diagram





(I like to use these in order clearly to identify a process. The double line represents the electron "dressed" by the laser field.)

To my mind, observation of the field induced refractive index of the vacuum which is essentially the real part of the diagram



Prof. Kirk McDonald  
December 18, 1985  
Page 2

would already be a clear demonstration of light-by-light scattering. Here, in the expansion, the lowest order term  has no real part if the

external (non-laser) photon is on the mass shell. So the leading contribution is from the fourth order diagram  with two laser photons and two non-

laser photons on the four corners. This is the lowest order light-by-light scattering diagram. It is largely responsible for our result (in the "vacuum polarization" paper) which you rewrote as

$$n \sim 1 + \frac{\alpha}{4\hbar} \left( \frac{E}{E_0} \right)^2$$

This essentially agrees with the analogous expansion for the refractive index in a constant crossed field ( $\vec{E} \perp \vec{B}$ ,  $|\vec{E}| = |\vec{B}|$ ) which is

$$n = 1 + \alpha \left( \frac{E}{E_0} \right)^2 \frac{11 \pm 3}{45\pi}$$

(V.I. Ritus, Zh. Eksp. Teor. Fiz. 57(1969) 2176; I am citing this from a review paper by Ritus, Trudy Academia Nauk USSR, Vol. 111 (1979) which would be really useful if it had been translated; maybe, it has been, by now). The  $11/45$  virtually agrees with our result while the splitting between the two helicity states ( $\pm 3/45\pi$ ) seems to be different in the constant crossed field case. I would not be surprised if the answer for the static electric or magnetic field were lower by a factor of four; in fact, either this or a factor of two is what I would expect comparing the case where  $|\vec{E}| = |\vec{B}|$  with  $|\vec{E}| = 0$ ,  $|\vec{B}| \neq 0$ , or  $|\vec{B}| = 0$ ,  $|\vec{E}| \neq 0$ .

The next class of phenomena is then associated with the diagram



of photon splitting whose rate is, as Mitter pointed out, smaller by the order of  $\alpha$  than vacuum polarization. If this would come within experimental reach someone will undoubtedly have to calculate it. It looks possible, but fairly horrendous.

I have not had the time yet to look at the Cerenkov related phenomena you were mentioning but I hope I will be able to do this soon.

Prof. Kirk McDonald  
December 19, 1985  
Page 3

If you have any further questions which you think I may be able to answer please let me know.

Yours sincerely,

A handwritten signature in cursive script that reads "W. Becker". The signature is written in dark ink and is positioned directly below the typed name.

Wilhelm Becker  
Research Assoc. Prof.

WB:jw



Princeton University

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February 12, 1986

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
A-8010 Graz  
Austria

Dear Prof. Mitter,

I am pleased to be able to report that your appointment as Visiting Fellow at Princeton has been approved. An official notification will also be sent you by Sam Treiman. As we did not know the exact dates of your sabbatical, we have made the appointment for the interval 1 Oct. 1986 - 1 Apr. 1987. Please advise us if this is appropriate.

Enclosed is a brochure summarizing the rental housing available through the university. Apparently it is typical for visitors to make direct contact with Ms. Coe Evans at the Housing Office, but if it would be more convenient for you, I could act as an intermediary. In that case please indicate to me more precisely what your needs might be.

An Assistant Professor here, Ian Affleck, shows an interest in the strong-field QED, and will be making some calculations with the work of Becker and yourself as a starting point. I hope that by the time of your visit there would be technical progress to report.

Best wishes,

Kirk McDonald

Princeton University

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March 5, 1986

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
A-8010 Graz  
Austria

Dear Prof. Mitter,

Could I trouble you to send a copy of a paper of yours which appeared in the proceedings of the 1975 Schladming conference. Our library is missing this particular volume (i.e. it was lost). Your paper would be useful to Ian Affleck who is now looking into strong-field QED.

We hope arrangements are proceeding for your sabbatical visit next Fall.

Best wishes,

Kirk McDonald

Princeton University

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March 7, 1986

Dr. Lester DeRaad  
LDR Co.  
5721 West Slauson  
Culver City, CA 90230

Dear Lester,

Tom Erber suggested I write you concerning my proposed experiment to demonstrate interference among Čerenkov, synchrotron and transtion radiation. I enclose a copy of the proposal which was submitted to Brookhaven Lab. A very similar proposal was also submitted to the Bates Lab in Boston, which is a better place to work as it has pure electron beams. We hope to collect data in May or June of this year.

A theoretical question remains as to the detailed effect of a geometry with a finite light collection path between two effectively infinite media of large dielectric constant. Perhaps you have some insight into this from your previous work on the matter.

Sincerely yours,

Kirk McDonald

# THE UNIVERSITY OF NEW SOUTH WALES

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TELEX AA26054 • TELEGRAPH: UNITECH, SYDNEY • TELEPHONE 663 0351  
EXTN.



PLEASE QUOTE

Professor Heinrich Hora  
Head, Department of Theoretical Physics

Professor Kirk T. McDonald  
Joseph Henry Laboratory  
Princeton University  
Princeton NJ 08544  
USA

7. March 1986

Dear Professor McDonald,

Though we were at the Malibu conference on laser accelerators in January you may not remember me as there was not enough opportunity for me to ask you for a discussion. I should mention to you that the generation of a high density plasma with oscillation energies of electrons and ions in the GeV range (which cannot be distinguished then by their mass and where apart from pair production a state of matter beyond the Tera-Kelvin range could be realized, is well under discussion since 12 years. This was simply done in the same way as the Livermore team under the late James Shearer considered this case (see the references 8,9,11,16 and 17 of enclosed paper). The topic there for space propulsion should not distract your attention, I only had to give my position on this question since my work was included in the discussions of the space research scientists since.

I am going to a two months stay at the University of Iowa (Department of Physics, VanAllen Hall, Iowa City, 52242) and would kindly like to ask you whether you are interested in discussion of the aspects of the extremely high intensity laser interaction.

It would be very keen to know whether you are interested at this stage of your research in a discussion of the problems and shall be pleased to hear from you.

Yours Sincerely

Heinrich Hora

545-294-385-2  
319-353-4343  
-3294

KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK

A-8010 Graz, Universitätsplatz 5  
Austria  
Prof. Mitter

Graz, den March 17<sup>th</sup>, 19<sup>86</sup>  
Telefon (0316) 380 - 52 25  
Telex 31662

┌ Prof. Kirk McDonald ┐  
Princeton University  
Physics Department  
P. O. Box 708  
Princeton, NJ 08544  
USA  
└ ┘

Dear Prof. McDonald,

thank you very much for your letters. I have sent you two reprints of my 1975 lecture. Some typographical errors have been corrected by hand. I am glad, that you have found a theorist at Princeton (Ian Affleck) to work on strong field QED. In fact I have started to look again on our old papers. In order to do useful work I would need some information on the technical side of the experiments you are planning.

First of all, I would like to know some (rough) data on the lasers you want to use. For all calculations one needs the wave length  $\lambda$  and the value of  $v^2$  expected during a pulse. In order to see, whether one can use a field of "infinite extent" to describe the laser wave, one should know also the duration of the pulses (eventually the repetition rate) and the approximate dimension of the cross section of the beam (or of the focal region, if you focus it).

Second, if you want to do experiments with particles in a laser field, one should know the values obtainable for the parameter  $\rho$  (see p. 401 of my paper). It has to be noted, that this parameter (with the rest mass of the electron in  $m$ ) controls also the dispersion properties of (non-laser) photons

$$\rho = \frac{2(p \cdot k)}{m^2}$$

$$\sim 4 \frac{\gamma \omega}{m}$$

$$p = (\gamma m, 0, 0, \gamma m v)$$
$$k = (\omega, 0, 0, \omega)$$
$$p \cdot k \sim 2 \gamma m \omega$$

./.

in the field, where  $\kappa$  comes in via the electron propagator in the vacuum polarization. From our paper on the latter it can be seen, that  $\rho$  should be around 5 in order to have a chance. For photon splitting one will need certainly similar values (if the process is observable at all; the rate is very small).

It seems also appropriate to accord theoretical work to be done in the next time. Therefore I would ask you or Affleck to indicate, which calculations you are planning. I can offer any advice and could start an independent calculation, if necessary.

With the same mail I have written to Ms. Evans (a copy of the letter is included) and also briefly to Sam Treiman. I thank you all very much for your trouble and am looking very much forward to my stay. With best regards, I am

Yours sincerely,



(H.Mitter)

KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK

A-8010 Graz, Universitätsplatz 5  
Austria  
Prof.H.Mitter

Graz, den March 17<sup>th</sup>, 19 86  
Telefon (0316) 380 - 52 25  
Telex 31662

┌ Ms. Coe Evans ┐  
Housing Department  
MacMillan Building  
Princeton University  
Princeton, N.J. 08544  
USA

L ┘

Dear Ms. Evans,

starting from October 1986 I shall spend a sabbatical semester as a Visiting Fellow at the Department of Physics of Princeton University. On recommendation of Prof.Sam Treiman I am writing to you with regard to an apartment. I would need a small furnished place (1 bedroom, kitchenette). I am married without children. Since my wife is working here, it is not yet sure, whether she can come along with me. Even if she can obtain a leave, we would be content with a small place. I would be interested in a relatively low rent, since I bring my salary from here. I shall stay for 6 months or a little less and shall arrive in the first days of October. The exact date of arrival will depend on air connections: I can start here only after Sept. 30th. Prof.Kirk McDonald from the Dept. of Physics could eventually act as an intermediary, if this is necessary. Thanking you in advance for your trouble, I am

Yours sincerely,



(H.Mitter)

Princeton University

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March 25, 1986

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
A-8010 Graz  
Austria

Dear Prof. Mitter,

We are pleased to receive word that you will definitely visit us for a term this Fall. Please let me know if I might be of service in arranging any details.

For your information as to my proposed strong-field QED experiments I enclose a few pages which were prepared for the funding agency. The laser will be built in 2 stages, but for each stage parameters  $\nu^2 \sim 0.1 - 1$  should be achieved. In the second stage of experiments, with a 50 GeV electron beam, the parameter  $\rho \sim 3$ . If these first experiments prove a success, I am confident that the technology will soon improve to yield  $\nu^2 \sim 100$  and/or  $\rho \sim 30$ .

Ian Affleck has already made an interesting first step in his calculations. He has shown that the technique of Brezin and Itzykson, Phys. Rev. D **3**, 618 (1971), may be applied to strong laser fields as well as static fields. This confirms that the index of refraction - 1 in a wave field has a maximum (dependent on the angle between the external photon and the laser wave vector) which is four times that for a static field of the same strength.

Best wishes,

Kirk McDonald



Princeton University

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March 25, 1986

Prof. Heinrich Hora  
Department of Physics  
Van Allen Hall  
Iowa State University  
Iowa City, IA 52242

Dear Prof. Hora,

Thank you for your letter of 7 March. I enclose a few pages describing my proposed experiments in strong-field QED, as submitted to our funding agency. The initial experiments do not go beyond the verifications of ideas which were discussed in the 1960's, but which have never been explored in the laboratory due to lack of technical resources. I certainly hope that a demonstration of these effects would lead to the opening of a frontier into nonlinear physics of a more exotic sort.

Best wishes,

Kirk McDonald

Princeton University

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July 24, 1986

Dr. Robert Woods  
U.S. Department of Energy  
High Energy Physics Program  
M.D. ER-221 GTN  
Washington, D.C. 20545

Dear Bob,

At the suggestion of Stew Smith, I am writing to advise you on the status of the efforts to begin a program of study in nonlinear quantum electrodynamics. Although the proposal has not yet gone through the peer review process, we hope that some funding will be available to begin work in 1987.

I preface the remarks on the new program with some comments on present work.

#### **The Čerenkov-Synchrotron Experiment at Bates Lab.**

Since your visit to Princeton we took data at Bates, and in only 24 hours of beam time showed very clear evidence for the interference effect between Čerenkov radiation and synchrotron radiation. A paper describing this is about to be submitted to Physical Review Letters, a draft of which is enclosed. I have never worked on an experiment which produced a new physics result in such a clean way in such a short time.

We would like to continue the experiment with a run at Bates in 1987 which would emphasize interference between transition radiation and Čerenkov radiation. Some improvement in the detector should be made for this. The level of funding required is about \$10k for equipment and operating expenses. (The 1986 experiment cost a total of \$30k according to our accounting.)

A suggestion has been made informally to me that the apparatus be eventually used for certain measurements of an applied nature. Thus this project may have a longer lifespan than originally anticipated.

#### **Nonlinear QED.**

A formal proposal to SLAC to begin the program to study nonlinear QED is more than half completed, and is now 50 pages. This document seeks to justify longer term interest in this physics, as well as the 'modest' first experiment.

The proposal was presented informally on July 11 to the collaboration to build the SLAC Accelerator Test Facility, at which the initial studies would be made. The reception was very positive, and some members of the ATF group expressed an interest to collaborate on the first nonlinear experiment. The parties are:

- Rick Fernow and Harold Kirk, BNL;
- Irving Bigio and Norman Kurnit, Los Alamos National Lab;
- Keith Bonin, Kirk McDonald and Pat Russell, Princeton U.

The contribution of Bigio and Kurnit is especially welcome as their expertise is laser physics, and they will be building the laser system for the ATF project. The front-end laser for the ATF is quite similar to that needed for the nonlinear scattering experiment, although improvements must be made in several parameters. We are working towards an understanding of how those improvements can be made as an incremental effort to that already underway at Los Alamos. Fernow and Kirk are the experts on the analysis spectrometer and beam line for the ATF, which play important roles in the nonlinear QED experiment.

It appears that funding restrictions are slowing the pace of the ATF project, whose completion is prerequisite for the first nonlinear QED experiment. Hence the funding for the latter should be distributed over a 2 year period. It also appears that some work on improving the quality of the ATF electron beam, and the synchronization between the laser system and the electron linac, should be considered part of the nonlinear QED project. Therefore I still estimate the cost of the experiment as about \$300k over a two year period. It would be highly desirable if \$100k could be available in 1987.

The configuration of the first nonlinear QED experiment results in an x-ray beam, which we believe could be made into the 'brightest' x-ray beam anywhere, albeit with poor duty cycle. Contact is being made with the synchrotron radiation user community to determine how this capability might be best developed. A separate proposal will be prepared, and additional funding would be needed, to establish the ATF as an x-ray source.

I would like to visit you to discuss these matters further after the nonlinear QED proposal is complete. Late August would be a good time. It might be appropriate to be joined by Bob Palmer, head of the ATF project, and Irving Bigio. We believe the physics interest in nonlinear QED adds to the vitality of the ATF facility, which itself deserves

To Dr. Robert Woods

July 24, 1986

Page 3

strong support in the immediate future.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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July 31, 1986

Dr. John Bell  
Theory Division  
CERN  
CH-1211 Geneve  
Switzerland

Dear Dr. Bell,

A preprint of yours arrived here, which would indicate that you have a continuing interest in the Unruh effect as related to synchrotron radiation. I enclose two papers which express my interest in similar topics. The short paper discusses a small experiment we recently completed which shows how Čerenkov and synchrotron radiation can be made to interfere. The longer document is an (unfinished) proposal to begin the study of nonlinear QED effects of electrons in strong laser fields. Section 2-7 speculates on the observability of the Unruh effect in electron-laser scattering. As for the incomplete polarization of electrons during synchrotron radiation, the Unruh radiation effect must have a lab-frame explanation. But in the latter case there seems to be almost no relevant literature.

Forgive me for troubling you a second time on these issues, which remain quite fascinating and are not beyond reach of experimental study.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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July 31, 1986

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
A-8010 Graz  
Austria

Dear Prof. Mitter,

Enclosed are two papers discussing my present research, which I hope will be of some interest to you. We were able to make a short run at the Bates accelerator in Boston which provided very clear evidence for the interference between Čerenkov and synchrotron radiation. The longer document is the (unfinished) proposal to study the nonlinear Compton effect at a new 50-MeV accelerator which will be built at Brookhaven Lab. Sections 2-4 through 2-6 concern issues you have worked on, and I would appreciate any advice on them.

Best wishes,

Kirk McDonald

Princeton University

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July 31, 1986

Prof. Thomas Erber  
Dept. of Physics  
University of California, Los Angeles  
Los Angeles, CA 90024

Dear Tom,

Enclosed is a draft of the proposal to study the nonlinear Compton effect as a first step in a program on nonlinear QED. The sections on the experiment aren't finished, but the review of theory is essentially complete. Section 2-6 comes closest to your past interests. Additional theoretical guidance would be welcome in sections 2-5 thru 2-7, if not elsewhere.

SLAC decided not to build the small accelerator at which the first experiment was to be done. The project was actually proposed by people at Brookhaven, and now they will very likely build it there in association with the National Synchrotron Light Source. In the short run this is probably advantageous for me.

In any case your comments would be appreciated.

Best wishes,

Kirk McDonald

Princeton University

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August 5, 1986

Dr. Norman B. Kurnit  
Group Chem-6  
MS J564  
Los Alamos National Laboratory  
Los Alamos, New Mexico 87545

Dear Norman

Enclosed is a note about synchronization, and some supporting documents I have gathered. One conclusion is that if Quantronix and Spectra Physics meet their stated performance there is little difference between them regarding synchronization. I believe the implementation of synchronization will be simpler for the Quantronix, but it appears that most of the cost is in the RF test instruments rather than the laser and RF components.

It remains my interest to have a system operating at 39.6666 MHz.

It will probably be best to defer my visit to LANL until after the Wisconsin conference.

I enclose 2 copies of a slightly updated version of the proposal. The main difference is the realization that the x-ray detector should include an x-ray spectrometer capable of high rates, rather than just a total absorption counter useful for only 1 x-ray per e-laser beam interaction.

Best wishes,

Kirk McDonald



Princeton University

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August 13, 1986

Prof. V.I. Ritus  
P.N. Lebedev Physics Institute  
Leninsky Prospect 53  
Moscow 117 924, USSR

Dear Prof. Ritus,

Enclosed please find a copy of a proposal for experimental studies of nonlinear electrodynamics. Papers by you and your colleagues have been very inspiring to me while planning this program. Technical considerations have led me to think about variations on the themes you have explored, as discussed in sections 2-4 to 2-7. Perhaps you would care to comment on some of these physics issues.

I also enclose a short paper on a recent experiment which shows that there is some novelty to be found even in 'ordinary' electrodynamics.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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August 13, 1986

J.J. Murray  
Bin 20  
Stanford Linear Accelerator Center  
P.O. Box 4349  
Stanford, CA 94305

Dear Joe,

Enclosed is a short paper describing our recent observation of interference between Čerenkov and synchrotron radiation. I understand that the conception for this arose out of experimental work you did with Tom Erber in 1970. I also enclose a nearly finished version of a proposal for studies of nonlinear QED in  $e$ -laser collisions. As you advised me in January, the initial work is to be done at a small linac, apparently to be built at BNL rather than SLAC. The longer range interest is still to bring the experiment to the C line. Some of the physics prospects that could be explored there are discussed in sections 2-4 to 2-7.

Sincerely yours,

Kirk McDonald

Princeton University

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August 13, 1986

Roger Gearhart  
Bin 20  
Stanford Linear Accelerator Center  
P.O. Box 4349  
Stanford, CA 94305

Dear Roger,

Enclosed is a short paper describing our recent observation of interference between Čerenkov and synchrotron radiation. I understand that the conception for this arose out of experimental work you did with Tom Erber in 1970.

Sincerely yours,

Kirk McDonald

KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK  
A-8010 Graz, Universitätsplatz 5  
Austria

Graz, den August 14<sup>th</sup> 1986  
Telefon (0316) 380 - 52 25  
Telex 31662

9-011-43-316

Prof. Dr. K. Mc Donald  
Physics Department  
Princeton University  
POB 708  
Princeton, N.J. 08544  
USA

NO MC  
316-302-143

Dear Prof. Mc Donald:

Thank you very much for your paper on Čerenkov/  
Synchrotron radiation and for the draft of your proposal. My friend  
and colleague H. Latal here enjoyed hearing, that the big efforts  
he was and is spending in close collaboration with Erber are not  
a waste of time. On the proposal I have some comments, which I  
shall send you separately, since I need a few days more for a  
good formulation. I have meanwhile calculated the corrections to  
nonlinear Compton scattering caused by the dispersion of the  
emitted photon (i.e. the analog of synchrotron-Čerenkov-radiation,  
if the magnetic field is replaced by the laser field in the vacuum;  
you call this vacuum Čerenkov effect on p.34 of your proposal). The  
results are valid in a domain of  $(N, q)$ , where the real part of the  
refractive indices of the vacuum dominates over the imaginary parts.  
I shall bring the results with me in October. Until then I have  
to do some checking. One has to be careful with respect to trivial  
errors (signs,  $\pi$ 's etc) and I have done the calculation alone.

With respect to my travel everything is o.k. from here.  
I have obtained also official leave from our ministry, my old  
(permanent) US visa has been transferred into my new passport by  
the US embassy. My wife will not be able to come along, since she  
does not want to lose her job. She will come over for a visit  
either for Christmas or in February. So I shall arrive in the  
first days of October: I shall write the exact date when I have  
brought the air ticket. Since the easiest way to go from New York  
to Princeton is probably by bus, I would be grateful, if you could

send me a brief information, when busses are leaving at the NY bus terminal. I know NY and will find my way from JFK airport to the bus terminal - I have done that before. I would also ask you to check with Ms. Evans, whether she has found some place for me to stay. Please write to me how things are developing.

Thanks again for your papers. With best regards I am

yours sincerely

A handwritten signature in cursive script, appearing to read 'H. Mitter'.

(H. Mitter)

KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK  
A-8010 Graz, Universitätsplatz 5  
Austria

Graz, den August 20<sup>th</sup>, 1986  
Telefon (0316) 380 - 52 25  
Telex 31662

Prof. Dr. K. Mc Donald  
Physics Department  
Princeton University  
POB 708  
Princeton, N.J. 08544  
USA

Dear Prof. Mc Donald:

Here are now my comments on your research proposal. My advice (if I can give one) would be to rely less on classical or semiclassical arguments and to use the quasi-level concept instead. This is a rather well established and old concept. It is not quite clear, who invented it: probably Ritus & Co, maybe Howard Reiss, certainly Zeldowich has merits, since he formulated it in a general way. In the first few pages of my comments I have tried to give an outline (at the risk of telling you nothing new), including the graphology. The remainder provides you with some graphs to contemplate. The only thing in your proposal which I find definitely wrong in your proposal is the graph on p.37 which does not contribute. I hope, that Unruh radiation does not cause too much Unruh' for you! x)

Please send (p)reprints of your papers to Ritus (lebedew Inst., Moskow). I know that he is interested in these problems. He has written a long review article in the Trudy FIAN, vol.111 (1979), unfortunately in russian, in which he discusses most processes for various external fields. The following article by Nikishow implements this to a book of 278 pages.

Hoping that my comments are helpful, I am

yours sincerely

*H. Lindley*

x) Unruhe is beside a name also a German word.  
It means unrest, restlessness, fidget, uneasiness,  
trouble, riot. It means also the balance of a  
mechanical watch. Maybe one of Mr. Unruh's

An electron in a plane wave field feels a potential, which is periodic. By analogy with solid-state physics one would expect, that the wave function is a product of an exponential function times a factor, which shares the period with the field. This is indeed the case, if we look on the Volkov solution. We have a close analogy with Bloch-type solutions in a solid. The difference is that the Bloch quasi-momentum becomes replaced by a four-vector  $\hat{p}$ : we have quasi-energies and quasi-momenta, since the potential is periodic in space and time. The quasi-levels are numbered by a (positive or negative) integer  $\ell$ :  $\hat{p} = \hat{p}(\ell)$  and are given by

$$\hat{p}_\mu = p_\mu - k_\mu \left( \ell - \frac{\kappa_*^2 - \kappa^2}{2(pk)} \right), \quad (\hat{p} + \ell k)^2 = \kappa_*^2, \quad \ell = 0, \pm 1, \pm 2, \dots$$

where  $p$  is the free momentum (outside of the wave),  $k$  is the wave vector of the e.m. wave and  $\kappa_* = m_* c/\hbar$  with an effective mass  $m_*$  depending on the details of the wave field (e.g. on its polarization), again in analogy to an electron moving in a solid. The wave function is then a sum

$$\psi(x) = \sum_{\ell=-\infty}^{+\infty} A_\ell \exp(-ix\hat{p})$$

where  $A_\ell$  depends on the spin and momentum of the particle.

All processes taking place in presence of the wave field can be characterized by the corresponding transitions between quasi-levels (e.g. by the difference of the corresponding  $\ell$ 's).

The quasi-level concept is both relativistic and quantum-mechanical. Its most important limit is the underlying assumption, that the e.m. wave field is an "external field", which is not influenced by the presence of particles (electrons, non-laser photons, atoms etc.). Since the integer  $\ell$  corresponds to the number of photons taken from or given to the external field, conclusions from the concept for very large  $\ell$  should not be drawn. "Very large" means in this context macroscopic numbers comparable with the number of laser photons present in the beam described by the field. Since this number depends on the intensity, the number  $\ell(\text{Max})$  up to which one may trust the concept can become small for low intensities. This is accounted for by the theory automatically: higher multi-laser-photon transitions come out small at low in-

The concept is not limited to a wave of infinite extent, since the Volkov solution holds also for finite pulses. The effective mass depends on space-time in this case (there are analog situations in solid state physics).

Lack of periodicity does not cause troubles, if the particles feel a periodic field over a domain large enough (measured by their own scale).

Some traces of the concept remain even valid for spherical waves (see Phys.Rev.A31 (1985)2030), but this is not interesting in the present context.

If one wants to make analogies with particles in other external fields, one may do so and take the quasi-levels as analogs of the Bohr levels in a Coulomb field, to the Landau levels in a constant magnetic field, to corresponding (quasi-momentum) levels in the wiggler field of a free-electron laser (the analogy is particularly close in this case, since the wiggler field is periodic in space) etc. The difference is of course, that in these cases the fields are constant in time, so that energy is a conserved quantity and not "quasi".

The advantage of the concept over all classical or semiclassical ones is, that it is both exact and simple. It is a result of standard QED and should be plausible to everyone, who knows Feynman graphs: one has only to replace the wave function resp. propagator of free electrons by the corresponding expressions in presence of the field. This makes calculations longer, but this need not worry an experimentalist. If no internal electron lines are present, even the calculation is not too hard. In particular the kinematics (conservation of quasi-momenta) gives all relevant formulae for frequency-shifts etc. For probabilities (cross sections) one needs of course also the wave functions (i.e. the  $A_\ell$ , which contain Bessel functions and  $\gamma$ -matrices). For most of the possible processes results are already available. One should, however, also write down the diagrams for those, where no results exist, since it is at least possible to make crude orders-of-magnitude estimates from diagrams (e.g. powers of  $\alpha$ ).



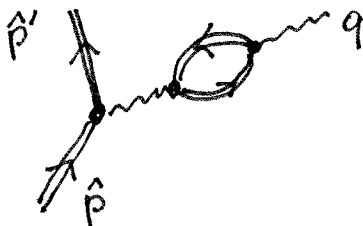
I would not consider it too relevant to look very much on how much of the concept is "classical". After all, one would not do that in making a proposal for a test of QED outside the high-intensity domain. Remember, that the classical limit of QED has never been very well established (see e.g. the review by Birula in the Schladming lectures 1977: it is worthwhile reading it). It is very hard to do that in general, since the  $\hbar$  appearing in the photon momenta cannot be put equal to zero without doing harm to the whole theory.

In order to illustrate the concept I shall discuss some processes you may want to consider in terms of graphs. Nonlinear Compton scattering is described by



From  $\hat{p}_\mu = \hat{p}'_\mu + q_\mu$  you obtain the formula for the frequency-shift by simple algebra, if the formulae given above for quasi-momenta are used - with the mass-shell condition given above both for  $\hat{p}$  and  $\hat{p}'$  and  $q^2 = 0$ . The integer in the condition is given by the difference of the level numbers. The intensity-factor enters via the effective mass. For the probability one needs of course the full wave function (as for the Klein-Nishina-formula). If one wants to stress the analogy with other external fields one should call the process rather "Bremsstrahlung in the (laser) wave field": it describes the radiation the electrons emit on their (circular or figure 8 or whatever) trajectory in the laser field. - By the way: the analogous algebraic calculation for the FEL wiggler gives the correct formula for the frequency of the FEL; the probability gives a correct quantum theory for the FEL (including electron spin). - The graphs for other simple processes are contained in my Schladming lecture.

For vacuum Cerenkov radiation the graph is



This is clearly a correction of (relative) order  $\alpha$  to the former process. From our paper on vacuum polarization one sees, what has to be done: the photon wave function in the final state has to be replaced by our  $\tilde{A}_\mu(p)$  from (6.1). In principle one has to consider both possible excitations plus their mixing. Since the mixing was shown to be small, one may neglect it. For the probability one has then to calculate the cross section for polarized outgoing photons, since there are two modes with opposite helicity and different dispersion law (different "Cerenkov condition"). The frequency shift (including Cerenkov) is obtained as above algebraically replacing  $q^2 = 0$  by  $q^2 = \frac{\omega^2}{c^2}(1-n^2)$  where  $n$  is the corresponding refractive index calculated in our paper.  $n = n(\nu, \rho)$  with  $\rho = \frac{2|qk|}{k^2}$  and complex in general, since there is absorption. Absorption means, that actually a pair is created. This trident-type process is described by the crossed graph to Møller scattering in presence of the laser field (and it will show a resonance structure as the Møller process). \*)



In a domain, for which the real part dominates ( $\rho \lesssim 0.6$ , see p.1652 of our paper) we can neglect the imaginary part. This (including the probability) is what I am calculating at present. The result will - as stated above - be a small ( $\sim \alpha$ ) correction to nonlinear Compton scattering. If observable, it will probe vacuum polarization, but one should be sceptical!

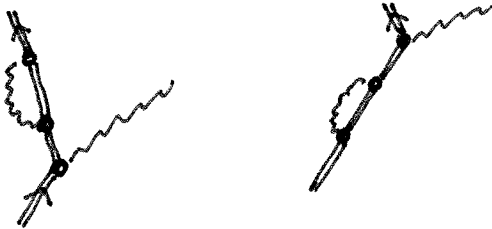
The difference with Erber et al. is of course, that  $n$  is provided by a medium in their case and is therefore an input for them. There would be also two (complex)  $n$ 's for the vacuum in a magnetic field, which have been computed. They are, however, very small for laboratory magnetic fields and can be safely neglected, unless one does the experiment on a pulsar. The paper of Steve Adler (Ann.Phys. 67 (1971) 599) contains everything on them, including remarks on the errors in the older literature. This paper has, by the way, motivated us to consider the analogous problem for a laser field, at least without photon splitting.

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\*) Without laser the process is kinematically forbidden. It is a true multi-laser-photon effect.

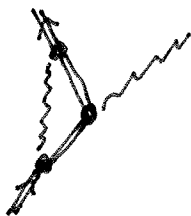
The pair creation process should, by the way, be considered. I am not aware of any calculation, but I have not followed the literature in the past years. When absorption is large ( $\rho$  high enough) the process could be more important than the Cerenkov effect.

Other radiative corrections would result including self-energy insertions in the electron lines

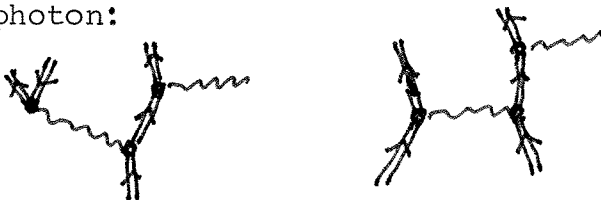


Since we have calculated the self-energy (J.Phys.A 12, 2171, 1976) the result to be expected is clear: the effective mass is changed by a small correction  $\sim \alpha$ , which is complex and means, that the levels are shifted (real part) and obtain a small width (imaginary part). This is probably not measurable. The imaginary part-decaying quasi-levels-would mean, that one considers a process in which four electrons/positrons take part (see graphs below).

A correction, which has not been considered so far (to my knowledge) is the vertex insertion



which is another correction  $\sim \alpha$  to the nonlinear Compton effect. In ordinary QED the vertex insertion gives the fluctuation contribution to the Lamb shift. So one could say here, that the electron in the laser field probes the e.m. vacuum fluctuations via this term. Cutting an internal electron line one would obtain again processes with four electrons/positrons, either trident processes or Møller scattering with emission/absorption of a non-laser photon:



Altogether the contributions of vacuum-polarization-, self-energy- and vertex insertions would form the analog of the Lambshift for the quasi-levels.

On Unruh radiation I must simply confess, that I cannot understand, what you mean by the analog to this outside gravitation theory. The graph on p.37 of your proposal gives a vanishing contribution, if the external field is a laser field, since the ring diagram with one extra corner

$$\text{Diagram} \quad (= \text{Diagram} + x \text{Diagram} + \text{Diagram} + \dots, \quad x (= \text{laser field}))$$

The diagram shows a series of Feynman diagrams. On the left is a ring diagram with a wavy line extending from the right side. This is followed by an equals sign and a series of terms: a ring diagram with a wavy line, plus a term 'x' multiplied by a ring diagram with a wavy line and a small circle on the right side, plus a term 'x' multiplied by a ring diagram with a wavy line and two small circles on the right side, plus an ellipsis. To the right of the ellipsis is a note: 'x (= laser field)'.

can be shown to vanish exactly in this case (this has been proved by Schwinger in 1951, see refs. 9, 11, 14 of my Schladming lecture). The diagram does not vanish for other external fields (e.g. for a Coulomb field).

Let me add some remarks on vacuum polarization. Here I would claim, that we think to have done a little more than just to provide a starting point. In our paper you can find the polarization tensor and its eigenvectors as well as the refractive indices in formulae (i.e. in terms of integrals) for arbitrary values of the two parameters  $\nu$  (your  $\eta$ ) and  $\rho$  (essentially your  $k$  p.29). From these data you obtain all relevant information on the scattering of light by laser-light (including absorption i.e. pair creation, but without fusion or fission of non-laser photons) with little-if any-calculation (see below). It is true, that we have given numbers only for a few values of  $\nu$  and  $\rho$  and the curves on p. 1653 refer only to one value of  $\nu$ . Any other curves can be obtained from our formulae, however, by evaluating the integrals on a computer. This is a kind of slavery-job for somebody familiar with programming. We have indicated on p.1651 below, how one may proceed. I have tried to find a student who spends some time on that for some payment (we have a little money left from our last research contract, but it is not very much). If you have better opportunities, I shall be glad to provide all necessary material.

Let me indicate, how to extract information. This is done by standard dispersion relation methods (in the "old" literal sense of the word, i.e. Kramers-Kronig resp. Bohr-Peierls-Placzek). The transition amplitude from a state with one non-laser photon ( $p$ ) to another state with one non-laser photon ( $p'$ ) in presence of an arbitrary number of laser quanta (arbitrary intensity  $\nu$ ) would be obtained multiplying our  $\hat{D}_{\mu\nu}^{\nu}(p,p')$ , equ. (5.9) with the polarization vectors of the incoming resp. outgoing photon. "Incoming resp. outgoing" means before resp. after entering the laser field: else an S-matrix (as well as cross-sections) cannot be defined and one can only speak of propagation in a (dispersive and absorptive) medium. Anyway, the amplitude is - up to trivial factors - given by the corresponding expressions  $K^{(i)}$  and  $\Delta$  of section 5. Note, that this amplitude is the analog of the Delbrück scattering amplitude with the Coulomb field replaced by the laser wave field (to arbitrary order in  $\nu$ ). It gives therefore all you may want to know on the nonlinear nature of QED except photon splitting. ~~The~~ amplitude is complex, of course, the imaginary part means absorption (i.e. pair creation). According to the optical theorem the Im part for forward scattering gives the total cross-section. This can be rephrased as  $\sigma(\text{tot.}) \sim \omega \text{Im } n(\omega)$  where  $n$  is the corresponding refractive index (which we have calculated, see sect. 6). In a region with little absorption this is essentially the scattering cross-section of light by laser-light. When absorption is important, one has to separate things. Anyway, it is of course also possible to obtain the differential scattering cross section (square of the amplitude times kinematical factors) in terms of the  $K$ 's and  $\Delta$ . The only trouble is to worry about the correct factors of  $2\pi$ ,  $c$  etc. - "theoretical theorists" like me don't like this kind of calculation, you may blaim me for that.

On photon splitting I am still sceptic, since it is clear from the corresponding graph



that the corresponding rates are smaller by a factor  $\alpha$  relative to scattering/absorption of light by laser light. By the way, the theorem you mention on p. 30 (that  $n$  can be only 1 or 3 for circular polarization) is an exact statement following from a simple symmetry (you should mention that). It can be proved by repeating what we have done for  $\langle 0 | T A_\mu A_\nu | 0 \rangle$  in appendix 2 of our paper for  $\langle 0 | T A_\mu A_\nu A_\rho | 0 \rangle$ . Corresponding theorems would hold for higher diagrams with more (non-laser) photon lines attached. The theorem does of course not mean, that you can compute the process restricting your attention to the two diagrams



$n = 1, 3$  refers only to the net number of laser photons involved. The two diagrams would only give the lowest nonzero contributions in an expansion in powers of  $\nu$ , which is not a good approximation (except for low intensities).

Princeton University

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September 2, 1986

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
A-8010 Graz  
Austria

Dear Prof. Mitter,

Thank you for your recent letter advising us of your impending arrival. We have a student-operated limosine service which can pick you up at Kennedy Airport. This will be much more straightforward than the use of public transportation. Of course we need to know your flight number and date to arrange this.

I talked to Coe Evans in the housing department. She said that you never really formally applied for housing, but that something would be arranged. This will likely be a small furnished apartment, presently occupied but being vacated about October 1. She also would like to know exactly when you arrive in case there is some conflict.

Thank you also for the comments on the proposal. I am pleased that you have had a chance to begin new work on the strong-field QED issues, so there will clearly be much to discuss after your arrival. Despite some recent political upheavels the scenario for the first experiment, nonlinear Thomson scattering, is now highly plausible. To continue this effort into true studies of QED will require persistence, and an increased awareness in the high-energy community of the interest in the physics issues. I am sure that your visit will contribute greatly to these matters.

Best wishes,

Kirk McDonald

Princeton University

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September 3, 1986

Dr. Norman B. Kurnit  
Group Chem-6  
MS J564  
Los Alamos National Laboratory  
Los Alamos, New Mexico 87545

Dear Norman,

Enclosed are 2 copies of the 'short' version of the proposal. I have sent copies of the present draft version to Claudio Pelligrini at BNL and to our DOE contract officer.

Best wishes,

Kirk McDonald



Princeton University

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September 4, 1986

Dr. Nicholas P. Samios  
Director  
Brookhaven National Laboratory  
Upton, Long Island, NY 11973

Dear Nick,

Enclosed is a draft copy of the proposal to study nonlinear Thomson scattering at the Accelerator Test Facility, which I have also sent to Claudio Pelligrini. The physics of this experiment ties in nicely with the application of the facility as an x-ray source.

I hope the path for the construction of the facility is smoothing out now, and welcome the opportunity to be of any service during that process.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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September 4, 1986

Dr. Claudio Pelligrini  
National Synchrotron Light Source  
Brookhaven National Laboratory  
Upton, Long Island, NY 11973

Dear Claudio,

Enclosed is a draft copy of the proposal to study nonlinear Thomson scattering at the Accelerator Test Facility. Also enclosed is a longer document which places the first experiment in the context of a larger program to study nonlinear QED effects.

I will visit BNL on Friday, Sept. 12 to continue our discussion of these matters.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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September 4, 1986

Dr. Robert Woods  
U.S. Department of Energy  
High Energy Physics Program  
M.D. ER-221 GTN  
Washington, D.C. 20545

Dear Bob,

This letter summarizes my request for Apparatus and Operating funds for 1987.

**1. \$100k for the nonlinear Thomson scattering experiment at BNL.**

Enclosed is a draft of the proposal to perform this experiment at the Accelerator Test Facility, now to be built at BNL. Conversations with Knotek, Palmer, Pelligrini, Samios and Sutter indicate that it is quite realistic that the ATF project will be well underway in 1987. My proposal to study nonlinear electrodynamics via the scattered x-rays ties in closely with the interest of the NSLS in the ATF as an x-ray source.

In the small collaboration to do this experiment the BNL people have primary responsibility for the electron beam, the LANL people will build the CO<sub>2</sub> laser system, and Princeton will provide the x-ray detector and laser-linac interaction region. The design of the experiment is not finalized, but we can readily identify apparatus costs of \$77k in the instrumentation that Princeton should provide (see attached list). I am asking for an additional \$23k to cover operating costs and contingency,

We at Princeton are also collaborators on the laser-grating experiment, and the additional \$23k would also serve to support our contribution to that, which is however not precisely identified at this time.

Also enclosed is a longer document which places the BNL experiment in the context of an extended program to study nonlinear QED. Requests for funding for this will be made in the future.

**2. \$25k for additional experimentation at Bates Lab.**

Our experiment to study the synchrotron-Čerenkov effect at Bates this year was highly successful. We would like to follow up with 2 new studies.

- a. Interference of transition radiation with Čerenkov and synchrotron radiation.

The predicted interference effect is more subtle than that which we recently found. A new photon detector and electron beam monitor will be required to permit running in the main electron beam at Bates.

- b. Synchrotron radiation in a short magnet.

So long as an electron traverses a uniform magnetic field of length greater than  $1700 \text{ cm/B(Gauss)}$  the radiated spectrum is very close to that of synchrotron radiation. But in a short magnet significant alteration of the spectrum can be expected. With some modification to our apparatus we can study this at Bates Lab, more easily than could be done at SLAC.

Interest in this phenomenon arises because the results are readily applied to the alteration of the beamstrahlung spectrum resulting from collisions of very short electron and positron bunches. The effect of beamstrahlung will be a major limiting factor to the utility of very high energy colliders. Evidence for suppression of beamstrahlung in very short bunches would be welcome.

Sincerely yours,

Kirk McDonald  
Professor of Physics

September 4, 1986  
Kirk T. McDonald  
Princeton University

**Apparatus Request for the Nonlinear Thomson Scattering Experiment**

1. Ortec GLP-50/10 Ge(Li) detector .....	\$15k
2. Readout electronics for item 1 .....	\$5k
3. Movable Pb slit and controller .....	\$3k
4. Movable stand for Ortec detector and Pb slit .....	\$5k
5. Pyrolytic graphite crystal .....	\$5k
6. Rotating stage for the crystal .....	\$3k
7. Vacuum chamber for the crystal spectrometer (Be windows) .....	\$10k
8. $f3$ mirrors for the laser focus .....	\$15k
9. Quadrant detector .....	\$3k
10. $x-y-z$ stage for mirrors and quadrant detector .....	\$8k
11. Vacuum chamber at the final focus .....	\$5k
<b>Total apparatus request .....</b>	<b>\$77k</b>

Princeton University

DEPARTMENT OF PHYSICS: JOSEPH HENRY LABORATORIES  
POST OFFICE BOX 708  
PRINCETON, NEW JERSEY 08544  
Telephone: 609 452-6608      Telecopier: 609 452-6218  
Telex: 499-8512

September 9, 1986

Dr. Philip Bucksbaum  
AT&T Bell Laboratories  
600 Mountain Avenue  
Murray Hill, NJ 07974

Dear Phil,

Thank you for sending me the several articles on your strong-field ionization experiments. You state that the ponderomotive force is well understood, but from the comments below you will see that this is not the case for me. I enclose an expanded version of the document I sent over a year ago, which indicates how, I hope, we will begin experiments on free electrons in strong fields. The use of highly relativistic electrons does not address the interesting physics you are pursuing, however.

1.

To me, good evidence that atoms have been subjected to strong wave fields is the suppression of the lowest-lying continuum levels. There should be suppression of more and more levels as the wave intensity increases. So I am somewhat surprised that your latest paper says you don't see this effect.

One ingredient in the argument is that the ac Stark shift of the ground state is negligible. I take your word for this.

The next is the notion that if an electron is ionized to a continuum level while inside an intense laser beam, then the quiver energy,  $e^2 E^2 / 4m\omega^2$ , is part of the ionization energy itself, and not due to subsequent interaction with the laser beam. [You might wish to point out that this expression holds for linear polarization; for circular polarization there is a 2 rather than a 4 in the denominator.] So independent of how the electron escapes from the laser beam it took more energy, *i.e.*, more laser photons, to ionize it if the electric field,  $E$ , is large enough. As you point out, intensity  $I \sim 10^{13}$  Watts/cm<sup>2</sup> corresponds to a quiver energy equal to the photon energy of a 1- $\mu$ m laser, so suppression should be seen for  $I$  slightly greater than this.

It would be nice to be able to trust this argument as it could be used to confirm that strong fields really were in effect. Can you be sure that you have detected ionization electrons which originated in the core of the laser beam, and not just the edges?...

2.

You note that at low intensities the ejected electrons line up with the polarization vector of the laser beam. This result is not intuitively obvious to me, but it does not seem consistent with the possibility that the higher continuum levels are populated via a cascade process. If a continuum electron absorbs a laser photon, it must absorb both its energy and its momentum, so that its motion would tend to line up with the wave vector rather than the polarization vector of the photon. Of course, a free electron cannot absorb a laser photon and stay on the mass shell. But an electron inside a laser beam has a spectrum of quasi-energies and quasi-momenta which accommodate photon absorption and emission.....

This leads to the next issue.

3.

What happens to the quiver energy as the 'free' electron leaves the laser beam?

This question seems more clear-cut than the related question, how does an electron take on its quiver energy if it enters a laser beam from a weak-field region? Perhaps my confusion on the latter makes me worry about the former.

An argument is that as the electron leaves the strong-field region, no matter in which direction, the gradient force,  $\sim -\nabla I$ , acts to increase the kinetic energy of the average motion of the electron, while its oscillatory motion dies out. Numerically, the initial quiver energy is supposed to equal the gain in the kinetic energy of the average motion. If so, lab measurements of kinetic energy really tell us about the quiver energy at the moment of ionization.

But I am still bothered about the second question. Kibble argues that if an electron enters a laser beam from the side its total energy does not change, with an exchange of initial kinetic energy of average motion for the final quiver energy. But if the electron enters the wave head-on, it gains both quiver energy and kinetic energy of its average motion. This behaviour does not seem consistent with that under a conservative force. Physically, I understand the second case better, both from a classical and from a photon point of view. My limited understanding is summarized in sections 2-1b and 2-1c of the enclosed paper (little different on this issue from the discussion in my previous paper).

The question may involve the relation between the ponderomotive force and radiation pressure. Classically the latter arises when the oscillations of the electron are not in phase with those of the laser beam, so that a net  $\mathbf{v} \times \mathbf{B}$  force exists along the wave vector of the laser. In terms of photons, this corresponds to absorption of laser photons without re-emission, which certainly raises the invariant mass of the electron, leading to Kibble's mass shift when the laser beam overtakes the electron. But if the electron enters the laser beam from the side, Kibble claims the mass shift occurs with no net energy change, *i.e.*,

no net absorption of laser photons. Why the net  $\mathbf{v} \times \mathbf{B}$  force should vanish in this case is not clear to me. Likewise, if the electron takes momentum but not energy out of the laser beam, it did not interact with the wave photons, but rather some spectrum of virtual photons which apparently must accompany any actual pulse of light....

4.

Suppose in your experiment an electron is ionized into a state of 1-eV kinetic energy of its average motion. Then its velocity is  $v/c \sim 2 \times 10^{-4}$ , or  $v \sim 6 \times 10^6$  cm/sec. To traverse the beam radius of  $15 \mu\text{m}$  then takes 250 psec. As your laser pulse is 100 psec long, it will have run past the electron before the latter can move sideways out of the beam. Hence the lab case is somewhere between the two limiting cases discussed by Kibble, and I am not completely clear on what to conclude. You see no level shifts, which is quite interesting, if it is clear that  $I > 10^{13}$  Watts/cm<sup>2</sup> applies.

It is quite plausible that the ponderomotive force alters the observed angular distribution of the ionized electrons in your experiment. This effect should certainly be more prominent for the lower-lying levels, as you observe. But the entire argument would be much more compelling for me if some of the loose ends were tied down.

5..

I feel it is a bit of an exaggeration of Kibble to say that the Kaptiza-Dirac effect is due to the ponderomotive force (which in fact he doesn't quite say). The Kapitza-Dirac effect involves an electron and exactly two real photons from two laser beams, while the ponderomotive force arises in a single beam and involves an unclear number of virtual as well as real photons.

I'm not sure my comments are of much help, but if you have the patience I'd be glad to discuss things further.

Sincerely yours,

Kirk McDonald



Princeton University

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September 18, 1986

Malcolm Perry  
DAMPT  
Silver St.  
Cambridge, CB39EW, England

Dear Malcolm,

I was advised by a Micheal Danos that one or more students at Cambridge may have new insights into the Hawking-Unruh effect. If any of these have been committed to paper I would appreciate a copy.

Best wishes,

Kirk McDonald

Princeton University

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September 18, 1986

Dr. Michael Danos  
Center for Radiation Research  
National Bureau of Standards  
Gaithersburg, MD 20899

Dear Michael,

Enclosed is a version of my proposal for experiments in nonlinear QED. As I mentioned there are several issues on which your advice would be welcome.

- pp. 9-11. A clearer physical picture of the field-gradient force would be useful. My quick argument suggests that the total energy of an electron is not constant while it experiences this force, but Kibble says that it is in certain circumstances. Some lab work involving this is underway at Bell (Murray Hill) by Phil Bucksbaum and others.
- p. 29 ff. Light-by-light scattering in a strong laser field involves multiple laser photons. Both a detailed calculation and a first-order impression as to the differences from 'ordinary' light-by-light scattering are needed.
- p. 32 ff. How seriously can simple arguments as to vacuum-polarization Čerenkov radiation be taken?
- p. 34 ff. What is the rôle of Unruh radiation in the scheme of things?

I appreciated the insights you showed during our conversation, and anticipate your thoughts on the above matters would be highly relevant.

Best wishes,

Kirk McDonald

Princeton University

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September 18, 1986

Prof. William Unruh  
Dept. of Physics  
University of British Columbia  
Vancouver BC V6T 2A6  
Canada

Dear Prof. Unruh,

Ian Affleck mentioned that he spoke to you recently about our interest in strong-field QED. This arose following the hypothesis that the thermal bath felt by an accelerating electron might lead to detectable effects in a laboratory scattering experiment. Enclosed is a long document outlining some of the steps necessary before such an experiment could be done, with a sketch of the possible effect in section 2-7.

The status of this conjecture is somewhat uncertain to me, and the political prospects of carrying out the lab work would be greatly enhanced if the effect had broader recognition in the community. An important development would be theoretical clarification of the rôle of acceleration radiation in  $e$ -laser scattering.

Clearly any comments you could make would be most welcome.

Sincerely yours,

Kirk McDonald  
Professor of Physics

KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK

A-8010 Graz, Universitätsplatz 5  
Austria  
Prof. Dr. H. Mitter

Graz, den Sept. 19, 19 86  
Telefon (0316) 380 - 52 25  
Telex 31662

Prof. Dr. Kirk McDonald  
Princeton University  
Physics Dept.  
**PO** Box 709  
Princeton, N.J. 08544  
USA

Dear Professor Mc Donald,

Thank you very much for your letter from Sep. 2<sup>th</sup>. I shall arrive on Oct. 1<sup>st</sup> in New York, JFK airport, at 18.50 local time with flight No. RJ 263 from Vienna.

Thanks also a lot for arranging my housing problem. I had written to Ms. Evans on March 17<sup>th</sup> on these matters. Maybe the letter got lost. - If there is any overlap problem I can stay for a few days in a hotel, of course. Since this letter may take too long time I shall send you a telex on Monday.

Thanks again and best regards

Yours sincerely



(H. Mitter)

UNIVERSITY OF CAMBRIDGE

Department of Applied Mathematics and Theoretical Physics  
Silver Street, Cambridge CB3 9EW

Telephone: Cambridge (0223) ~~331645~~  
337899

Sept 27, 1986

Dear Kirk,

I do not know Mr Danos  
is referring to. There are no students working on  
the Hawking Effect here. The only possibility  
is that he was talking about some recent  
work of Gary Gibbons, a preprint exists  
entitled "The Elliptic Interpretation of Black  
Holes." I am sending you a copy  
of that under a separate cover.

Best wishes

Malcolm Perry,

АКАДЕМИЯ НАУК СССР  
ФИЗИЧЕСКИЙ ИНСТИТУТ имени П.Н. ЛЕБЕДЕВА

117924, Москва, Ленинский проспект, 53

Телефон: 135-22-50  
Для телеграмм: Москва, В-333, ФИАН

ACADEMY OF SCIENCES OF THE USSR  
P.N. LEBEDEV PHYSICAL INSTITUTE

117924, Moscow, Leninsky prospect, 53

Telephone: 135-22-50  
Cables: Moscow, V-333 FIAN  
Telex: 411479 NEOD SU

Prof. K.T. McDonald  
Princeton University  
Physics Department  
P.O. Box 708  
Princeton, NJ 08544, USA

24 September 1986

Dear Prof. McDonald,

We are very grateful to you for sending the paper containing a thought stimulating review and proposals for experimental studies of nonlinear quantum electrodynamics, - the field in which we are working for many years. Naturally, we would be very glad if such experiments succeed. Certainly, it would be an interesting and new physics.

Our papers and some proposals were collected in 111 volume of Trudy FIAN "Quantum electrodynamics of phenomena in intense field", M.: Nauka, 1979, which later was published in English in USA. Probably, you get acquainted with this volume. If it is not so we can send you a copy but only in Russian.

We consider the radiation of a uniformly accelerated electron and its mass shift as interesting and principal problems. As to Unruh radiation we sceptically regard (as probably you do) the possibility of its justification in the framework of QED. Besides, <sup>what</sup> is understood as Unruh effect is sensitive to pure field (electrical) invariant, which is not changed at increasing of energy of an electron.

Our further works in nonlinear QED was recently summed up in 168 volume of Trudy FIAN "Problems of intensive field quantum electrodynamics", M.: Nauka, 1986, which was sent to you a few days ago. Unfortunately, this volume is not yet translated in English.

With deep respect

*V.I. Ritus* V.I. Ritus

*A. I. Nikishov* A.I. Nikishov

THE UNIVERSITY OF ROCHESTER  
RIVER CAMPUS STATION  
ROCHESTER, NEW YORK 14627

DEPARTMENT OF PHYSICS  
AND ASTRONOMY

October 17, 1986


Professor K. McDonald  
Department of Physics  
Princeton University  
Princeton, New Jersey 08544

Dear Professor McDonald:

I am sending to you a translation of a letter from V.I. Ritus and A.I. Nikishov. This is a short message written one day before my departure from the Soviet Union. They will mail a more detailed letter to you somewhat later.

I appreciate your invitation for my visit to Princeton and hope we will have a useful discussion of non-linear QED problems and the perspectives of experimental studies in the field. I will notify you about the date of my visit immediately after the approval from the U.S. Department of State is received.

Truly yours,

  
N. Narozhny

NN/dp  
Enclosure



Dear Professor McDonald:

Thank you very much for sending to us a paper with a stimulating review of problems in non-linear QED with which we have been working for many years and the proposals for experimental studies of these problems. We would be happy if this experimental program was carried out. Surely we will find out many new and interesting phenomena.

Our work and some proposals were summarized at "Trudy FIAN" v. 111, 1979, entitled "Intense Field Quantum Electrodynamics". This issue was published in USA in English later. Maybe you are familiar with it. If not, we could send a copy to you, but unfortunately only Russian variant is in our possession.

Some words about Unruh radiation. We are skeptical, as well as apparently you are, about its substantiation in the frame of QED. Moreover this effect is very sensitive to a field invariant (electrical) which remains constant while the electron energy grows.

Our further activities in non-linear QED have been recently reviewed at "Trudy FIAN" v.168, 1986, "Problems in Intense Field QED". Unfortunately the English translation of this volume has not been done yet.

Sincerely yours,

V.J. Ritus  
A.J. Nikishov

# ILLINOIS INSTITUTE OF TECHNOLOGY

Lewis College of Sciences and Letters  
Department of Physics

October 16, 1986

Professor K. McDonald  
Joseph Henry Laboratories  
Princeton University  
P.O. Box 708  
Princeton, New Jersey 08544

Dear Kirk:

Following yesterday's phone conversation, I thought it would be useful to make a brief summary of topics. When we get together in a few weeks' time we should have some kind of agenda as a basis for discussion --- that way we can eliminate some topics ahead of time and avoid irritating dead-ends (megagauss physics?); on the other hand, I can bring relevant background material along.

1. I would emphatically suggest that we schedule a talk, under some kind of auspices, with the title

"Compulsory Order in Chaos: The Leading Loop and The Leading Branch"

This deals with basic, new results having wide implications --- including the quantum telegraph. The 'QT', in turn, is tied in with some of our common interest because of its basic significance for QED and multi-photon processes.

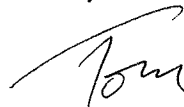
2. Short-range objectives - Starting from the premise that one swallow doesn't make a summer, it seems that further exploration of the basic features of synchrotron-Cerenkov radiation would be fruitful.
  - (a) The presently available equipment could be modified to demonstrate the existence of angular striations.
  - (b) As pointed out in the PRL paper, the initial S-C<sup>v</sup> experiment involved an 'interference' situation that was intuitively plausible. The next step is to exhibit unintuitive features of S-C; the suppression or extinction effects are striking-examples. The 'Fig.2' mailed to you some weeks ago shows that this should occur in He at densities comparable to those you currently have. The electron energy range is just right ( < 1 GeV ) Higher magnetic fields and higher photon energies ( < 100 eV ) would be involved. The experiment could be completely spoiled by intense scintillations in the VUV range --- and so some experimental 'look', however crude (without magnetic fields!) would be very helpful.

- (c) S-C radiation can be tuned so that it is very sensitive to variations in the index of refraction. There are countless situations in atomic physics where the index varies drastically --- hence, S-C radiation should be useful in atomic physics studies, especially in the X-ray region of the spectrum. Whether this is done through Cornell or NBS is irrelevant; ultimately it will be done, and we're in an ideal position to speed up the time scale.
- (d) It has been verified experimentally that indices of refraction are phenomenologically useful for X-rays even when the wavelengths are much smaller than the interparticle spacings. But somewhere this idealization must fail. Pushing S-C experiments to high energies is one way of exploring this situation -- see Section 7 of "Experimental Aspects...." (Annals of Physics, 102, pp.435-438, 1976).
- (e) You mentioned yesterday that interest in transition radiation and magnetic bremsstrahlung in inhomogeneous fields had waned. Add to this list gas scintillations, multi-photon processes, and modifications of the Bethe-Heitler radiation rates. All of these phenomena --- except coherent synchrotron radiation --- have been the subjects of previous experiments, but can and should be studied with your set-up. These needn't be mentioned in research proposals, and none will secure a round-trip to Stockholm. But I'm firmly of the opinion that we have an obligation to put reliable numbers into compilations of data -- in other words, that we put something back into physics, and not merely orient our careers to the approval or envy of colleagues!
3. Intermediate Objectives - Under this heading, I would put all of the items you mentioned in your pre-proposal on nonlinear QED. Presumably Mitter, Narozhny, etc. can help to polish the presentation. In this connection, I think it would be useful to keep an eye on parallel efforts, particularly the following:
- (a) Bremsstrahlung and pair-production in crystals. Despite the fact that there is a prevailing uneasiness about the approximations used by Cue and Kimball to motivate the 'crystal assisted' processes; the experimental results appear to be reliable and of basic significance. See, A. Belkacem et al., Nucl. Instr. Meth. B13, 9-14 (1986). The 'flaky' aspects of this work involve the idealizations concerning the existence of intense fields along atomic 'strings'. Related problems are bound to appear in estimating the effective field configurations at the focus of laser beams.
- (b) Processes in pulsar ambients. Still the best playground for QED theorists with a penchant for strong fields. Heimo Latal and his students continue to make excellent progress in this area.
- (c) Beamstrahlung, radiation reaction, quantum synchrotron radiation and other chimeras associated with the interaction point of linear colliders. Contact with SLAC and CERN on these issues should be helpful in advancing <sup>the</sup> overall cause of strong-field QED.
4. The really big picture! - What will be the historical significance of QED as seen in the year 2200? Is 'electroweak' a historic deepening of our understanding or merely a phenomenological graft? Will the study of non-linear QED ever lead to deeper insights concerning QED and/or its relation to hadron physics?

You and Mitter will have a few months to develop common points of view. But on the time scale of two or three days things are bound to be more fragmented --- the only way to counter this disorganization is to plan ahead. Let me know how your thinking develops.

In the meanwhile, best regards to Mitter. Tell him that the Chairman of the Topical Group on Few-Body Systems is Frank Levin, Dept. of physics, Brown U. 401-863-2291.

See you soon,

A handwritten signature in cursive script, appearing to read "Tom".

T. Erber

TE/tlb

# Princeton University

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Internet: <http://puhep1.princeton.edu/~mcdonald/>

October 1986

Dr. Gerard Mourou  
Laboratory for Laser Energetics  
250 East River Road  
Rochester, NY 14623

Dear Gerard,

I very much enjoyed the visit to your lab in September, and wish to arrange another visit in the near future, in which I would be accompanied by my colleague Keith Bonin. I will call you about this. As I mentioned to you at the OSA Conference, it would be very kind of you to provide us with some additional information on your dye laser system. Could you send me the following:

A copy of the thesis of Kafka, which discusses details of the dye amplifier configuration;

One or two 35mm slides of the cpm oscillator, and of the dye amplifier chain. I will be giving talks in which I wish to use your system as the example of a laser which might be useful to study non-linear quantum electrodynamic effects.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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October 28, 1986

Dr. Avraham Gover  
National Synchrotron Light Source  
Brookhaven National Laboratory  
Upton, Long Island, NY 11973

Avraham,

Enclosed are 3 papers: a long document which discusses possible future experiments in nonlinear QED using lasers; a proposal for a first experiment at BNL (which is essentially chapter 3 of the long paper); and a recent experiment we performed on the interference of Čerenkov and synchrotron radiation.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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October 28, 1986

Dr. Richard Heese  
National Synchrotron Light Source  
Brookhaven National Laboratory  
Upton, Long Island, NY 11973

Dear Richard,

Enclosed is a copy of the proposal to study nonlinear Thomson scattering at the Accelerator Test Facility. I will call next Wednesday to confirm my visit to BNL on Thursday, Nov. 6.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

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November 11, 1986

Prof. Adrian Melissinos  
Department of Physics  
University of Rochester  
Rochester, NY 14627

Dear Adrian,

Thank you again for inviting me to Rochester. Keep the exotic projects going!

Enclosed are receipts for the airfare and airport parking, a total of \$129.

I also enclose a document sketching the possible development of the experimental program I discussed in my talk.

Best wishes,

Kirk McDonald



# Princeton University

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Internet: <http://puhep1.princeton.edu/~mcdonald/>

November 1986

Dr. Gerard Mourou  
Laboratory for Laser Energetics  
250 East River Road  
Rochester, NY 14623

Dear Gerard,

Thank you again for the hospitality of you and your group during our recent visit. Enclosed is your copy of Kafka's thesis, which we have duplicated. If you do have the photographers document your dye laser system I would like to have some representative slides. I could send you a purchase order to cover costs, if this is otherwise an impediment.

Again I was impressed by the hard sell of the slab laser. Perhaps we should think of an entry-level use of a slab-geometry amplifier, which could provide 100 mj, 100 ps pulses at 100 Hz without incredible amounts of development work. Such an amplifier could then pump an additional stage in the dye amplifier to yield  $> 1$  mj in the femtosecond dye pulses at 100 Hz. I believe a 'slab' of cross section only  $6 \times 6$  mm<sup>2</sup> would be sufficient, which is modest compared to most discussions.....

Sincerely yours,

Kirk McDonald  
Professor of Physics

# STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

*Mail Address: Bin 68*  
SLAC, P.O.Box 4349  
Stanford, California, 94305

October 29, 1986

Professor Kirk McDonald  
Department of Physics  
Princeton University  
Princeton, New Jersey 08540

Dear Kirk,

The Stanford Linear Accelerator Center would like to add one additional tenure member in experimental high energy physics to its faculty. In view of the distribution of age and experience of the present members of the faculty (list attached), we would prefer to make an addition at the Associate Professor level carrying regular tenure at the level which might be the first tenure position of the individual.

We would like to ask you to nominate possible candidates at the point of their careers corresponding to the above description. A member of the tenure faculty at SLAC is expected to take independent leadership and responsibility for major experimental activities, either in association with existing groups or, if feasible, with independent support. Your views on the merits of all qualified individuals that you wish to suggest would be appreciated; statements of comparison with appropriate members of the SLAC staff would be very useful to us in this search.

I believe you are acquainted with SLAC's history and current programs. If you feel additional information is needed to respond to this request I would be very happy to explain both the position in question and the work of SLAC in further detail.

With many thanks for your help.

Sincerely yours,



Wolfgang K. H. Panofsky  
Chairman, Search Committee

Encl.

# Princeton University

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November 14, 1986

Prof. W.K.H. Panofsky  
Bin 68  
Stanford Linear Accelerator Center  
P.O. Box 4349  
Stanford, CA 94305

Dear Pief,

Should SLAC desire to strengthen the Mark II or SLD efforts with a senior appointment, the Search Committee is likely better aware than I of the relevant prospects. If, however, SLAC will again be able to support some diversity of effort following the impending success of the SLC project, may I take the occasion to present my own case.

I have become quite interested in a program of study of nonlinear QED via the combination of ultrashort-pulse laser technology with high-energy physics. In practice only a beam of the quality of the SLC would be suitable for this. The program would need about 1 pulse per second, and correspondingly, I suspect, about 1 percent of the resources available for experimental support at SLAC. If such a prospect were deemed interesting, but considered more viable with a senior person on-site, I would welcome the opportunity to relocate at SLAC (or Stanford U., where much pertinent work takes place at the Ginzton Lab).

Enclosed is a document sketching a vision of the physics program, which is however not yet fully focused on the experiments which could be pursued at SLAC.

Sincerely yours,

Kirk McDonald

**Princeton University**

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Telephone: 609 452-6608      Telecopier: 609 452-6218  
Telex: 499-3512

December 4, 1986

Dr. Howard R. Reiss  
Department of Physics  
American University  
Washington, D.C. 20016

Dear Howard,

Enclosed are two documents which show the state of my efforts on nonlinear QED. The Brookhaven proposal is taken quite seriously at that lab, but the severe funding crunch in 1987 does not yet permit a clear view as to how the work will actually proceed. The true QED experiments would have to take place at SLAC, following the success of the BNL experiment on one hand, and a willingness of SLAC to support such physics with the SLC beam technology.

As I mentioned on the telephone, it would be interesting if you could pay a visit to Princeton before the end of January 1987, when Heinz Mitter leaves. Possibly you could make a presentation to the atomic physics seminar. Let me know soon about your schedule, if you can fit a visit in.

Sincerely yours,

Kirk McDonald

# Princeton University

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December 22, 1986

Ms. Homaira Akbari  
E-632, M.S. 221  
Fermi National Accelerator Laboratory  
Batavia, IL 60510

Dear Homaira,

Following our telephone conversation I would like to pursue the possibility that you might join us at Princeton in a post-doctoral position.

Enclosed are two documents which sketch my interests in applying picosecond laser technology to elementary particle physics. The immediate prospect is the study of nonlinear Compton scattering, to be conducted at BNL. This work would be associated with a larger effort to verify a scheme for the laser acceleration of particles, based on the idea of Palmer, *Particle Accelerators* **11**, 81 (1980).

I would like to meet you when I am in Chicago on Jan. 26-27, 1987. On that Monday I give a talk at the U. of Chicago on a recent experiment (P.R.L. 57, 2264 (1986)), and I could come out to Fermilab on Tuesday.

It would also be good to have you speak at our high energy physics seminar on your thesis work. Open dates will be after Feb. 1, and I or Jon Bakken will contact you about this.

Sincerely yours,

Kirk McDonald  
Professor of Physics

Princeton University

DEPARTMENT OF PHYSICS JOSEPH P. KELLOGG LABORATORIES  
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Telex: 309 3512

January 7, 1987

Prof. Nikolai Zhuravov  
Department of Physics  
University of Rochester  
Rochester, NY 14627

Dear Nick,

I am enclosing some papers which might be of interest to you and your colleagues.

#### I. The Unruh Effect.

The paper by Bell and the Thesis of Myhrvold treat various aspects of Unruh's idea that an accelerating system is subject to quantum fluctuations which can be characterized by a temperature. Myhrvold notes in his thesis how your work on the radiation of an electron in a constant electric field supports this notion. That is, the spectrum of the radiation on the reverse motion of the electron is to give a Boltzmann distribution corresponding to  $T = \hbar a / 2\pi c$ . Bell notes that the effect of quantum fluctuations in synchrotron radiation on the circulating electron beam can also be described by a temperature.

As you may know I am interested in whether the radiation spectrum itself can be said to have a piece which can be identified as a Planck spectrum (in the rest frame) in addition to the dominant classical spectrum. A demonstration of either the constant electric or constant magnetic field would be of interest.

#### II. Beta decay in strong fields.

You give some consideration to this in your earlier papers. The problem has been addressed in recently by Howard Reiss, who just visited us at Princeton. Apparently some of the key remarks in his work. But the possibility which caught my attention is as follows. The electron in beta decay should be created in a Volkov state, which has more energy than a free electron of the same longitudinal momentum. This might reduce the phase space for the decay, and in extreme cases cause the rate to vanish!

My calculation of pion decay does not show such an effect, but the initial pion is also in the strong field, and so has a Volkov wave function which likely compensates the phase space. The situation might be quite different for neutron decay, or a nuclear beta

To Prof. Nikolai Narozhnyi

January 7, 1987

Page 2

decay where the strong field will have little effect on the nuclei. Any alteration of phase space might be highly useful in experimental studies of the neutrino-mass question.

As a step along these lines, H. Mitter is going to calculate muon decay in a strong field, but this may not be sufficient to settle the issue.

I am sending this packet in a hurry as I am leaving for the Utah Conference, and will not be back until Jan. 18. We enjoyed your visit very much, and hope we can continue to be in contact.

Best wishes,

Kick McDonald

DEPARTMENT OF PHYSICS  
LOS ANGELES, CALIFORNIA 90024

13 January 1987

Professor K. McDonald  
Joseph Henry Laboratories  
Princeton University  
P.O. Box 708  
Princeton, N.J. 08544

Dear Kirk:

Belated 'happy new year!' and two small items:

1. Discussed your factor of "4" with Tom Ypsilantis --- he's quite convinced that 2 out of the 4 originate from the manufacturers' fuessing of quantum efficiency figures --- supposedly a relevant reference is: Nucl. Instr. & Meth. 126, p.13 (1975), article by Baillon, Ypsilantis, et al. Tom liked your PRL article.
2. Local coordinates: phone 213 - 825 - 1619 / 4570 UCLA  
213 - 474 - 5962 home

Best



DEPARTMENT OF PHYSICS  
LOS ANGELES, CALIFORNIA 90024

7 March 1947

Dear Kirk:

In the absence of any further news from the East, am completing a paper on iteration & plasticity; next Quarter hopefully less teaching, and I'll write up the material on the Quantum Telegraph. By the summer, perhaps the 'leading loop & leading branch'.

Hope all goes smoothly,

Best  
Tom E.

Encl. # rebate

Princeton University

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April 28, 1987

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
Universitätsplatz 5  
A-8010 Graz  
Austria

Dear Heinz,

Enclosed is a copy of Leonid Kruglyak's senior thesis. I believe all the essential results were available when you were still here. I gather that Ian Affleck and Leonid will write up a paper for publication.

In talking with Leonid today we noted that there are 2 other 6-photon processes he did not calculate: 1 laser photon in + 4 external photons out; and 2 laser photons in + 3 external photons out. The final-state phase space for these is likely reduced compared to the case of 2 photons out, but there will be less suppression in the matrix element. Perhaps Leonid will work this out to complete the story for publication.

Ian and Leonid still think the full calculation is rather formidable, and I suspect they won't pursue it. If it is clear that the case of 3 laser photons in + 2 external photons out is suppressed as found in the low-frequency limit, then the case is indeed somewhat doubtful. I have the impression they did not carry the calculation far enough that numerical methods are the next step. Perhaps you could advise me on your view of a possible continuation.

I've just had a conversation with Alan Chodas of Yale who has a theory that there is a sort of 'confining phase' to QED in a strong background field. The conjecture is that there is a new family of positronium-like states which can exist in a very strong field, and might have been produced in the heavy-ion collisions at Darmstadt. He should be sending a preprint sketching this, which I will pass along to you. Certainly the possibility is intriguing....

Best wishes,

Kirk McDonald

# Princeton University

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July 7, 1987

Prof. Nikolai Narozhny  
Moscow Physics Engineering Institute  
Kashirskoe Shosse 31  
Moscow 115409  
USSR

Dear Nick,

During your visit I may have mentioned my interest in light-by-light scattering between an intense laser beam and high-energy photons. An interesting question is whether multiphoton effects are important, at least if the center-of-mass energy is near pair-creation threshold. I enclose a paper by Affleck who used the Euler-Heisenberg (low frequency) approximation to show that scattering via 3 laser photons is unimportant whenever this approximation is valid.

I gather that the a full calculation is a rather formidable challenge. Perhaps though you have some colleagues who would find the physics interesting enough to take up the effort. If the rate for scattering via 3 laser photons were sizable above pair threshold it would be quite exciting from the experimental side, as some chance remains to measure it. [If someone were interested I could send a longer document by Kruglyak detailing what he and Affleck did.]

Limited funding has become available for the facility at Brookhaven Lab at which the nonlinear Compton effect could be studied. I understand that I.F. Ginzburg and colleagues at Novosibirsk have a similar interest, but I have not been in direct contact with them yet.

I'm not sure whether you received the hastily assembled packet of papers that I sent to Rochester in January. I enclose a later note which summarizes my impression of the most accessible aspects of the Hawking-Unruh temperature.

Best wishes,

Kirk McDonald

**Princeton University**

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July 7, 1987

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
Universitätsplatz 5  
A-8010 Graz  
Austria

Dear Heinz,

Enclosed are 2 papers: the writeup of Affleck for publication, and the conjecture of Chodas on QED phase transitions. The feeling here seems to be that the phase transitions are of a highly speculative nature.

Have you anything to report on the vacuum Čerenkov effect? Do you share Ian's somewhat pessimistic view of the outcome of a full calculation of light-by-light scattering via 3 laser photons? If the 3 laser photon process actually becomes comparable to the 1 laser photon case above pair creation threshold, serious attention should be given....

Have a good holiday in Italy (perhaps you are even there now)!

Best wishes,

Kirk McDonald



own original report alone: the meeting is a school. We would of course like to have a lecture on a subject from experimental particle physics. Usually the focus of the meeting is on theory, but it is also tradition to include experimental subjects (please look on the previous notes, which appeared as supplementa to Acta Physica Austriaca - e.g. vol.XXV/1983. Since 1986 we have switched to Springer Heidelberg). A review on experiments/results before the next accelerator generation is a possibility, if this review is focussed on "interesting" topics. But may be you can propose something else.

If you dont want to lecture, you are of course welcome too, but then we cannot pay very much. In any case, we would like to have you in Graz at least for some days, either before or after the meeting.

In connection with Schladming I would like to ask you for another favour. It is my intention to invite other speakers from Princeton too. We must have a lecture on the recent issue of the Eötvös experiment. My first choice is of course Val Fitch. I write to him with the same mail. It were nice, if you could ask him, whether he can come (under the same conditions as above). If he cannot come, I shall try to hire de Rujula (I prefer Fitch, but why not a show from time to time). Another candidate on our list is Peebles. We need a lecture on Large scale structures, for which he is clearly the best possible choice (I prefer the more sceptic attitude to too much optimism). Perhaps you could ask him too, whether he could come. I write to him too.

In order to write on physics I need a few weeks more: we are still fighting with this stupid integral. I hope to hear from you sooner. If you want to bring your family, this is of course possible. There is a Ski-Kindergarten, which is the right thing for Alex. Owie is probably too small for skiing, but Nancy could take him out on a sled, there are many possibilities for that.

With best regards, also to your family (and from Marlis)

I am Yours sincerely



## Princeton University

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July 20, 1987

Prof. H. Mitter  
Institut für Theoretische Physik der Universität Graz  
Universitätsplatz 5  
A-8010 Graz  
Austria

Dear Heinz,

I have now received your letter regarding the Schladming Conference, which I would indeed like to attend. It would no doubt be very educational for me to prepare some lectures on techniques of experimental particle physics as applied to astrophysics. I am not really an expert on this topic, and so would like learn more than the audience—a splendid opportunity for me if acceptable to you.

Possibly you will have some of the experts on specific detection methods lecturing also, in which case I might review other methods. A quick list of possible subtopics:

Solar neutrinos/ supernova neutrinos

X-rays and high energy gamma rays from point sources

Very high energy charged particles = cosmic rays..Centauro events...

Monopoles, axions...

The sprit of the lectures would be to indicate what kind of measurements can be made, with some review of present successes, and of future prospects (for the measurements, as opposed to the theory or interpretation). Please advise me if such a scheme would fit within the scope of the Conference. You mention a lecture of '4 hours.' I presume this is not all in one sitting! What is the typical format?

I have not been able to contact Fitch, but Peebles indicates that the timing of the Conference conflicts with his teaching. Indeed this is the usual problem for us at Princeton, as the Conference is just after the beginning of the Spring term.

To Prof. H. Mitter

July 20, 1987

Page 2

I've tried sending you a Bitnet message, but even if it gets through I'm not sure you can reply, as we have no direct connection to Bitnet.

Best wishes,

Kirk McDonald



KARL-FRANZENS-UNIVERSITÄT GRAZ  
INSTITUT FÜR THEORETISCHE PHYSIK

A-8010 Graz, Universitätsplatz 5  
Austria

Prof. Dr. H. Mitter

Graz, den 29.7. 1987  
Telefon (0316) 380 - 52 25  
Telex 31662

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Prof. Dr. Kirk McDonald  
Dept. of Physics  
Princeton University  
PO Box 708  
Princeton, N.J. 08544  
USA

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Dear Kirk,

thanks for your letter from July 20<sup>th</sup>. the proposition for your lecture is indeed excellent - in fact I have had exactly such a review in mind, but did not dare to propose so much work to anybody (including you). It is, of course, true that "the best way to learn something new is to announce a lecture on it" (this is a statement by Heisenberg, which I remember verbatim). Perhaps it is a good idea to enter the field of "astroparticle" physics now - I intend to do this too on the occasion of the Schladming school.

The normal schedule of lectures at the school is one hour (60') per speaker per day. We leave 15' after the lecture for discussions and another 15' intermission before the next lecture (coffee etc. is available). So we have two lectures in the morning (8.30 - 9.30, 10-11) and two in the afternoon (4-5, 5.30-6.30). Speakers may give their personal preference (morning resp. afternoon session, first resp. second half of the school) and we try to arrange a schedule. We try to avoid two lectures on the same subject at one day (unless the speaker insists on that). You will obtain a leaflet with corresponding questions later on this year.

With respect to possible overlaps there is not very much danger. I have invited Stodolsky from MPI Munich to speak on his "dark matter detectors", but I have no answer from him so far (he should return from the US these days). ~~But~~ he has written, that he cannot come, so I shall try to hire one of the two guys he proposed. We shall have a lecture on black holes (theory) by Hajiček from Bern. I have invited Straumann from Zürich (a good

physics or black holes (observations). We must have lectures on the early universe: invited is Linde from Moskow, a big expert; lets hope that he comes. Otherwise I shall hire J. Ellis from CERN. On cosmic strings I had invited Kibble, who works on that (too!). Since he cannot come, I wrote to his collaborator N.Turok. A lecture on Monte-Carlo-Regge Calculus in gravity theory will be given by Berg from Hamburg. Other subjects are still open.

By the way: Börner's lecture from Schladming 83 (Acta Physica Austriaca Suppl. XXV) is still a good introduction to the standard model. He has written a book on that meanwhile, which will come out soon. Eventually I shall invite him again, I have only hesitated, since he was a speaker already twice.

With respect to the communication problem: I have so far not received your Bitnet message. If you are connected to another research net, let me know, which one. It is possible to set up also a communication with most other nets. A reason for this is, that I would eventually need the file, which Gloria has typed before I left (the text entitled H.Mitter, February 11, 1987). When we have finished the numerical work, we could make corrections here and finish the text (we can write in Tech here). This would save retyping the text. Files can be transferred easily by electronic mail back and forth. So one should look for a way to do that. By the way: I have successfully communicated with Reiss over Bitnet - he came here in June on a touristic trip with his children and rapid communication was necessary in order that he could arrange his schedule.

Next time I shall write on results. With best regards, also to your family,

Yours truly



LEHRSTUHL PROF. DR. H. SALECKER  
SEKTION PHYSIK DER UNIVERSITÄT MÜNCHEN  
8 MÜNCHEN 2, THERESIENSTR. 37

Professor Kirk T. McDonald  
Joseph Henry Laboratories  
Princeton University

TELEFON 23 94 / 43 73  
March 29, 1988

Princeton, New Jersey 08544, USA

Dear Professor McDonald,

with much interest I have read the work of your collaborators J. Affleck and L. Kruglyak about "Photon-Splitting in a Plane-Wave Field" in Phys. Rev. Lett. I have also worked with my collaborators on non-linear effects of QED. Since these papers appeared in European journals you may not know them. Another paper (Sakurayama, Anders and Salecker) which can test non-linear effects at high energies including  $Z_0$ -exchange according to fig. 6 of Ringhofer and Salecker will appear soon.

Yours sincerely,



H. Salecker

2 copies

1 reprint

# STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

SLAC, Bin 80  
P. O. Box 4349  
Stanford, CA 94309  
(415) 926-3300

May 20, 1988

Professor Kirk McDonald  
Physics Department  
Princeton University  
P.O. Box 708  
Princeton, NJ 08544

Dear Kirk:

Thanks for your letter. I just saw it and Burt's reply. We have discussed various techniques at various times, initiated by various persons. Implementing any single idea is never trivial because of the interferences it causes. Adding a laser beam to the present setup is a rather big deal.

The laser backscatter scheme has been suggested by several persons. It is indeed technically feasible, but not readily a solution to all our needs, because it occupies space very dear to the physics ... the vertex region ... which is prime real estate.

Detection of "backscattered" photons from laser or "beamstrahlung" photons from beam-beam interactions can be done by photon counting and/or backscattered electrons in a spectrometer after a bend. Which of these works best is yet to be determined, but depends mostly on the nature of backgrounds.

One thing that should be thought about ... when the laser wavelength is long relative to spot sizes, the Compton scattering becomes coherent over a number of electrons ... greatly increasing the rate. Have you considered that? I would like to understand what effects show up in that limit. The SLC specifies a spot size around  $1.8\mu$ , and a length of a few millimeters. Future linear colliders specify spots that are substantially smaller. Laser wavelengths will be longer than bunch cross sections, so backscattering phenomena could be interesting as a tool.

May 20, 1988

Thanks for your present and any future thoughts. I enjoyed seeing you at Baltimore.

Sincerely yours,

A handwritten signature in cursive script, appearing to read "Charles".

Charles Y. Prescott  
Associate Director  
Research Division

CYP:lg

cc: B. Richter

Princeton University

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May 25, 1988

Prof. Charles Prescott  
Associate Director  
Stanford Linear Accelerator Center  
P.O. Box 4349  
Stanford, CA 94305

Dear Charles,

Thanks for your reply regarding the laser diagnostic. It is clearly incompatible with a physics detector, and would be most useful only if the situation were somewhat critical...

Regarding coherent scattering, my view is that this is unlikely. The argument: I believe it makes most sense to analyze things in the rest frame of the electron, where the frequency of light is (largely) unchanged by the Compton scatter. In this frame the laser wavelength is rather short, and the electron bunch rather long, compared to the lab frame. Example:  $E_e = 50$  GeV, so  $\gamma = 10^5$ . Then if the laser wavelength is  $1 \mu\text{m}$ , its apparent wavelength is  $10^{-11}$  m in the electron rest frame. Meanwhile, if the electron bunch length is 1 mm in the lab it is 100-m long in the electron rest frame. If the bunch contains  $10^{11}$  electrons, the average spacing is  $10^{-9}$  m in the electron rest frame, or about 1 per 100 wavelengths. To me a strong coherent effect seems unlikely.

On the other hand, it is possible to build small lasers now that are so intense that an electron has essentially 100% probability of scattering just using the incoherent cross section. It is such a laser I would hope to bring to an external beam at SLAC to study strong-field QED....

Sincerely yours,



Kirk McDonald

# STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

SLAC, Bin 80  
P. O. Box 4349  
Stanford, CA 94309  
(415) 926-3300

May 17, 1988

Professor Kirk McDonald  
Physics Department  
Princeton University  
P. O. Box 708  
Princeton, NJ 08544

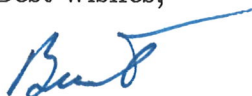
Dear Kirk:

Thanks for your letter of May 12, 1988 on measuring the beam size at the SLC. It is indeed a tough problem. We now do our size measurements with a wire scanner (carbon filament) which is okay at present intensities and sizes but which will burn up as soon as we get to a couple  $\times 10^{10}$  particles and a beam size of around two microns.

The laser idea is an interesting one, and I will pass your note on to the people working on the SLC beam-beam crossing problems. The detector, however, is a very tough problem, and I don't think your simple lead-glass block will work. Synchrotron radiation from the beam-beam interaction (if one wanted to use your system as a "live" monitor of beam size) has a critical energy in the hundred MeV region, and a total energy radiated of thousands of TeV per beam crossing. I think the Compton backscattering could be used with something like a pair spectrometer which would detect particles only above several GeV.

In any event, thanks for the interest.

Best wishes,



Burton Richter

BR:k

cc: C. Prescott  
W. Kozanecki

June 28, 1990

Professor K. McDonald  
Department of Physics  
Princeton University  
Jadwin Hall, P.O. Box 708  
Princeton, NJ 08544

Dear Professor McDonald:

I understand from Professor N. B. Narozhny that you would be interested to participate in regular research contacts between American and Soviet physicists in the field of nonlinear QED. If so, we would be pleased to include you among the listed participants in a proposal that will go to the NSF from the University of Rochester later this summer. Other participants will include Professors A. C. Melissinos, C. R. Stroud and myself.

If you would like to participate, please send me a very short description of your interests, including a few citations to the relevant literature (yours or other papers in journals or lab reports or whatever). "Very short" means no more than 1 page doubled-spaced.

Your direct benefits from a successful proposal will be the chance to visit Narozhny, et al., in Moscow (probably once in 3 years) and to have short visits by Soviet workers to Princeton supported on a modest per diem basis. Indirect benefits would come from increased general awareness of activities here and in Moscow, I guess.

Of course, you could easily write your own proposal to the NSF, but my experience has been that a project involving a number of investigators at more than one institution has a greater chance of surviving, and the NSF tends to recognize this. The proposal that we will put together will emphasize two main areas: (1) very strong field atomic and electron physics, and (2) laser-induced effects of atomic motion and dipole coherence. It will involve about 6 senior participants from 3 institutions.

Please let me know if you have any questions, or simply send me a one-page write-up and if that raises any questions here I'll get back to you.

Yours truly,



J. H. Eberly

JHE:js



1/15/91

Dear Kirk

I don't recall if I told you that our proposal at CERN (it was for the thermal photon stuff) was not approved. There is some wild interest in the non-linear stuff but no formal discussion took place.

On second thought, I feel that CERN is not the right place. If one wants to do higher harmonic generation, your expt at BNL is as good as any. The interesting thing - at high energy - is to do the tridents  $ex \rightarrow e e^+ e^-$ , but these are hopelessly difficult to get out of the circular machine - for instance CLIC would be nice for that.

Bausch and Lomb Building  
University of Rochester  
Rochester, New York 14627-0011  
(716) 275-4351  
Telefax: (716) 275-8527

So I fell back on the W experiment which seems

Tough - but worthwhile - in the context  
of the SLD since there is no other  
way to compete with LEP.

If you get me know if you have any  
comments and of course if you have  
an interest in it, it would strengthen  
the proposal very much. I am working  
through Baltay & Brendenbach to  
see if they like it and then it can  
be an SLD proposal.

If this gets turned down as well,  
my laser will be free and I  
wonder whether I could be of help  
at the ATF but of course this is  
your experiment and your decision.

Cheers

Adrian

Best regards to Pat

# Princeton University

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Telephone: 609 258-6608      Telecopier: 609 258-6360  
Bitnet: MCDONALD@FNAL      HEPnet: PUPHEP::MCDONALD

January 22, 1991

Prof. Adrian Melissinos  
Department of Physics  
University of Rochester  
Rochester, NY 14627

Dear Adrian,

Thanks for sending me your proposal to study  $\gamma e \rightarrow W\nu$  at SLD. I must admit that I never convinced myself that the rates were high enough that SLAC could beat LEP II to the study of the  $\gamma WW$  vertex. Also, the study is cleaner if polarized beams are available, which is another question mark. As I recall, the rates for  $ee \rightarrow eW\nu$  are similar to those for photoproduction, but with much less perturbation on the setup at SLAC; of course, without polarized beams it is hard to observe the angular asymmetry in this case.

I enclose a couple of draft writeups that I never completed. I got worried about the background from  $\gamma e \rightarrow Z^0 e$ , and a grad. student, Steve Naculich, now at Johns Hopkins (Bitnet: naculich@casa.pha.jhu.edu) made a calculation of this for polarized beams. I believe the result was somewhat unfavorable in that the use of polarized beams did not provide a strong suppression of the  $Z$  production (although I don't remember this too well).

On a related front, I was amused to hear that SLAC is actively seeking new collaborators to work on the final-focus test beam - for machine studies. This is the beam that could also be used for light-by-light scattering by adding a laser system. There have been some recent papers about QED phase transitions in strong fields. In principle the study of  $\gamma\gamma \rightarrow e^+e^-$  might be sensitive to this, and would certainly be much cleaner than uranium-uranium collisions. So I keep in the back of my mind the possibility of making a proposal for this....

Best wishes,

Kirk McDonald



# UNIVERSITY OF CENTRAL FLORIDA

CREOL--CENTER FOR RESEARCH IN ELECTRO-OPTICS AND LASERS

3267 Progress Drive, Suite B, Orlando, Florida 32826 • Phone (407) 658-6800 • FAX (407) 658-3939

February 12, 1993

Professor Kirk McDonald  
Princeton University  
Department of Physics  
P O Box 708  
Princeton, NJ 08544

Dear Professor McDonald:

Please find enclosed my paper "Low energy ..." which is related to high intensity laser-electron interaction. When this paper was first submitted to PRL the referee mentioned your name in connection with an experiment at SLAC related to this subject.

Here in CREOL I am the only one doing this type of calculations, so I work a little isolated. A longer version of the paper is presently being prepared. I would welcome any comment that you may have regarding my work.

Sincerely,

A handwritten signature in blue ink that reads 'Eduardo Ugaz'.

Eduardo Ugaz

**FAX Transmission**

---

**From:** Dr. Frédéric V. Hartemann UCD, Department of Applied Science

---

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**Questions?** Call (510) 423 7705 # 07276 LLNL, PO Box 808, L-794  
Fax (510) 422 2514 Livermore, CA 94550

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**To:** Dr. Kirk T. McDonald Phone : (609) 258-6608**Company:** Physics Dpt., Princeton U. Fax : (609) 258-6360**Address:** Princeton, NJ

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**Date:** January 25, 1995**Time:** 10:19 PMPages: 2 (including this one)

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Dear Dr. McDonald,

I am currently writing a paper entitled "Nonlinear Interaction of a Point-Like Charged Particle with a Classical Electromagnetic Field", and I would like to reference one of your papers (see next page). Could you give me the exact reference of this paper, and the references of any other work you might have done on this subject ?

I thank you in advance for your consideration.

Very sincerely,

  
Dr. F.V. Hartemann

## Princeton University

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January 26, 1995

Dr. Frédéric V. Hartemann  
Department of Applied Science  
University of California, Davis

Dear Dr. Hartemann,

I reference the paper you inquired about as *Fundamental Physics During Violent Acceleration*, in *Laser Acceleration of Particles*, C. Joshi and T. Katsouleas, eds., AIP Conference Proceedings No. 130, 23 (1985).

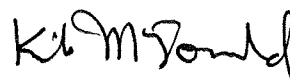
I later wrote a longer version, but it is referencable only as a preprint: *Proposal for Experimental Studies of Nonlinear Quantum Electrodynamics*, DOE/ER/3072-38 (Sept. 2, 1986).

These thoughts have led to an experiment currently taking data at SLAC, called E-144 in their nomenclature. The proposal could be referenced as J.G. Heinrich *et al.*, *Proposal for a Study of QED at Critical Field Strength in Intense Laser-High Energy Electron Collisions at the Stanford Linear Accelerator Center*, submitted to SLAC (Oct. 20, 1991).

A version of the proposal appeared as a conference contribution: C. Bamber *et al.*, *Study of QED at Critical Field Strength*, in *Workshop on Beam-Beam and Beam-Radiation Interactions: High Intensity and Nonlinear Effects*, ed. by C. Pellegrini, T. Katsouleas and J. Rosenzweig (World Scientific, Singapore, 1992), p. 127.

We have not yet published any results from the experiment, but we have clearly seen non-linear effects: a mixture of nonlinear Compton scattering and multiple Compton scattering. More work is in progress.

Sincerely yours,



Kirk McDonald  
Professor of Physics

From: SMTP%"melikian@jerewan1.yerphi.am" 9-MAR-1997 06:44:42.67  
To: mcdonald@puphed.Princeton.EDU  
CC:  
From: j: from Yerevan, Amatuni

Dear Dr. Kirk T. Macdonald,

Some time ago I informed you that a group from Yerevan Physics Institute was preparing a proposal to construct an installation for obtaining quasimonochromatic and polarized gamma quanta, using Compton backscattering of photons from existing 400 J CO<sub>2</sub> laser on electrons with the energy up to 4,5 GeV from Yerevan synchrotron. Your kind and quick reply to my message was encouraging enough for us and I am glad to express our gratitude for it.

Now proposal is presented for grant to International Science and Technology Center (ISTC) and has a registration number: Proposal A-169. I am sending you along with this message an Abstract of this proposal (latex text) and I would like to ask your support for this proposal addressed to ISTC. The main goal of ISTC, at least as they declared it, is the establishing and strengthening of International collaborations in all possible form. So in your Reference on our Proposal-169 it will be essential to ISTC to know how actual is the project, is it realizable, is it possible to establish a scientific exchanges between our and your groups, including exchange of the visiting scientist, organization of common Workshops in course of the project realization, devoted to discussion of the obtained results and plans for future work, construction and improvement of some part of installation (laser pulse shortening device, optical resonators, tagging system, polarimeters, etc.). Your group will not bear the financial obligation along of this kind of collaboration. The highest degree of collaboration is just a participation of you or somebody from your group in our project, and we certainly will welcome such a participation with great satisfaction. Please sent your Reference to:

Dr. Karen S. Bunyatov  
Project Office  
International Science and Technology Center  
Luganskaya Ulitsa, 9  
115 516 Moscow, Russia.

e-mail: bunyatov@moscow.istc.ru  
Fax: (7-095) 321 47 44  
Tel: (7-095) 321 45 33

Thank you in advance for help and collaboration.  
With best wishes, sincerely yours

A. Amatuni

==== RFC 822 Headers =====

Return-Path: melikian@jerewan1.yerphi.am

Received: by puphep.princeton.edu (UCX V4.1-12, OpenVMS V7.0 VAX);  
Sun, 9 Mar 1997 06:44:41 -0500

Received: from jerewan1.YerPhI.AM by viper.princeton.edu via ESMTMP (951211.SGI.8  
for <mcdonald@puphed.Princeton.EDU> id GAA17825; Sun, 9 Mar 1997 06:46:5

Received: by jerewan1.YerPhI.AM (940816.SGI.8.6.9/940406.SGI)  
id OAA25943; Sun, 9 Mar 1997 14:48:46 +0400

Date: Sun, 9 Mar 1997 14:48:46 +0400 (MSK)

From: "Robert A. Melikian" <melikian@jerewan1.YerPhI.AM>

To: mcdonald@puphed.Princeton.EDU

Subject: from Yerevan, Amatuni

Message-ID: <Pine.SGI.3.91.970309131440.25151C-100000@jerewan1.YerPhI.AM>

MIME-Version: 1.0

Content-Type: TEXT/PLAIN; charset=US-ASCII

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September 29, 1997

Dr. S.J. Brodsky  
Bin 81  
SLAC  
P.O. Box 4349  
Stanford, CA 94305

Dear Stan,

There are some issues of principle related to electron interactions in strong field that continue to trouble me, and I think a number of other experimentalists in this field. I talked a bit about this with Sam Treiman, who was not previously familiar with the topic, but allowed that there may indeed be an issue. If you could enlighten me it would be greatly appreciated. A clearer understanding would have impact on current discussions of laser acceleration, among other phenomenon.

The crispest version of the puzzle may be as follows:

An electron-positron pair is produced in the collision of a high-energy photon,  $\omega$ , with a photon,  $\omega_0$ , from a strong electromagnetic wave (laser). What is the threshold laboratory energy of the positron (or electron)?

In a strong field, the quantum states of electrons and positrons are described by Volkov solutions to the Dirac equation. See Sec. 101 of Quantum Electrodynamics by Berestetskii, Lifshitz and Pitaevskii (Landau & Lifshitz, Vol. 4). These states can be characterized by a quasi(4)momentum, which I write as

$$q = p + \frac{m^2 \eta^2}{2(p \cdot k)} k,$$

where  $p$  is the 4-momentum of the electron in the absence of the wave,  $k$  is the wave 4-momentum, and

$$\eta = \frac{eE}{m\omega_0 c} = \frac{e\sqrt{A^2}}{mc^2},$$

is a dimensionless (classical) measure of the field strength  $E$  of the wave.

The invariant mass associated with quasimomentum  $q$  is

$$\bar{m}^2 = q^2 = m^2(1 + \eta^2).$$



I interpret the quantum analysis as requiring conservation of quasimomentum rather than ordinary momentum for processes that occur inside the wave.

In this view, an electron-positron pair has minimum energy  $2\bar{m}$ , not  $2m$  in the pair rest frame, which is inside the wave.

For the (practical) case that  $\omega \gg \omega_0$  (and two photons collide head on), the laboratory energy of the pair is approximately  $\omega$ , and the energy of the electron (or positron) is  $\omega/2$  at threshold.

But to produce the pair we must have

$$s = 4\omega\omega_0 \geq (2\bar{m})^2,$$

and hence

$$\omega \geq \frac{\bar{m}^2}{\omega_0} = \frac{m^2(1 + \eta^2)}{\omega_0}.$$

I also interpret the quasimomentum  $q$  as reverting to ordinary momentum  $p$  when the electron leaves the wave – and is observed in some laboratory device. The ordinary momentum is then given by

$$p = q - \frac{m^2\eta^2}{2(q \cdot k)}k,$$

[noting that  $(p \cdot k) = (q \cdot k)$ ],

For a wave with  $\omega_0 \ll m$ , as holds for optical waves, and for particles moving against the wave, the difference between the ordinary energy,  $p_0$ , and the quasienergy,  $q_0$ , is extremely slight. Hence the threshold laboratory energy of an electron or positron from pair creation would be  $m^2(1 + \eta^2)/2\omega_0$ .

Many people appear to be uncomfortable with the use of the quasimomentum in the above way. They prefer to think that the electron and positron could be produced at rest with mass  $m$  (in the pair rest frame) and that subsequently they interact with the wave until they are ‘dressed’ with quasimomentum  $q$ . Then when they leave the wave they become ‘undressed’ and end in a state that could be calculated as if the wave were weak.

In this view, one finds  $m^2/2\omega_0$  as the threshold electron energy from pair creation.

That is, the threshold electron energy in pair creation is predicted to be larger in the analysis that includes quasimomentum than in one that does not! Equivalently, the threshold cm energy is higher when one uses the quasimomentum.

In the above I have perhaps presented the argument as if the quasimomentum view must be right. Doubt may enter when one considers that the motion of electrons in a wave has a classical limit. For example, an electron inside a (circularly polarized) wave has a classical transverse velocity of  $\eta c/\sqrt{1 + \eta^2} \approx c$ .

A classical interpretation of the analysis using quasimomentum is that the electron (and positron) is created with transverse velocity  $\approx c$ .

Many people find this unintuitive, and prefer to think that the quantum process creates the electron-positron pair with zero transverse velocity (during time scale  $\approx \lambda_{\text{Compton}}/c$ ), after which a classical interaction with the wave builds up a relativistic transverse velocity on a time scale of the period of the wave.

Does the theory clearly decide among these two points of view?

Sincerely yours,



Kirk McDonald

P.S. There are various subtleties. One is that once  $\eta$  exceeds one, the number of wave (laser) photons involved in the pair creation need not be one as assumed above. Indeed, for  $\eta \gg 1$  the mean number of laser photons is roughly  $\eta^3$ , and the threshold energy is not precisely defined, but is of order  $m^2/\eta\omega_0 \rightarrow 0$ . That is, in the large- $\eta$  limit it becomes extremely easy to produce a pair. Still, a different threshold energy results when one ignores quasimomentum.

Another subtlety is that if the above example is analyzed in the rest frame of the electron-positron pair, then in this frame the energy of a laser photon is of order  $m$ , and so the period of the laser appears to be of order  $\lambda_{\text{Compton}}/c$ . In this case the time required for the laser field to accelerate the electron to the speed of light is of the same order as the time to create the pairs. So maybe the two viewpoints aren't really distinct in this example.

I have other examples in which the 'quantum-only' view gives different results than the 'quantum + classical' view, but perhaps these can wait pending your thoughts on the subject thus far.