Princeton University

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December, 1985

Prof. Ernest Moniz
Director
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Prof. Moniz,

Enclosed is a short proposal to study an interesting radiation effect at the Bates linac. The interference between synchrotron radiation and Čerenkov radiation, although never before observed, should be readily demonstrated in a small experiement which could run in a parasitic mode.

I spoke on the telephone with Bill Lobar and Jay Franz, who were encouraging that the experiment might be run by obtaining a parisitic beam via scattering off a wire at the first focus of the external beam. Ed Booth of Boston U. gave me some advice as the the practicalities of running this way. I understand the earliest that some beam time might become available is March-April 1986.

If you consider that Bates might be able to run the experiment I would like to visit the lab to get a better understanding of its operation. Please advise me as to your interest in this project.

Sincerely yours,

Kirk McDonald
Professor of Physics

Princeton University

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December, 1985

Dr. Robert Palmer
Associate Director for High Energy Physics
Brookhaven National Laboratory
Upton, Long Island, NY 11973

Dear Bob,

Enclosed please find copies of a proposal for a small experiment to observe the interference between synchrotron radiation and Čerenkov radiation at the AGS. Don Lazaurs has advised me that it might be possible to run this in the LESB (B4) during the May-June 1986 slow beam period.

I have also submitted a very similar proposal to the Bates accelerator. They may be able to accomodate me in a 700 MeV parasitic electron beam in spring 1986. If so it is probably advantageous to conduct the experiment there rather than at BNL. However I believe a good job could be done in the B4 line, and request your consideration for that.

To mount the experiment in spring 1986 it would be useful to have determined which laboratory will be used by February-March 1986. Can I get an indication from Brookhaven on that time scale? I will of course keep you informed as to the status of the proposal to Bates Lab.

Sincerely yours,

Kirk McDonald
Professor of Physics

Princeton University

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December, 1985

Dr. Donald Lazarus
AGS Divsion
Brookhaven National Laboratory
Upton, Long Island, NY 11973

Dear Don,

Enclosed please find copies of a proposal for a small experiment to observe the interference between synchrotron radiation and Čerenkov radiation at the AGS. Both Neil Baggett and Ted Kycia advised me that I should submit this formally to Bob Palmer, and I have done so. I trust that Bob will contact you when he has a chance to consider the proposal.

Thank you again for your help is assessing the feasibility of the experiment in the B4 line.

Sincerely yours,

Kirk McDonald
Professor of Physics

BROOKHAVEN NATIONAL LABORATORY
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Upton, Long Island, New York 11973

(516) 282
FTS 666 3830

Office of the Director

December 23, 1985

Prof. Kirk McDonald
Department of Physics
Princeton University
P.O. Box 708
Princeton, New Jersey 08544

Dear Kirk:

Thank you for your proposal to look for synchrotron-Cerenkov interference. It is an interesting idea, and I would like to have it presented to the AGS Program Committee. They deserve some fun out of life, and the interaction may be useful to you. The next APC meeting is scheduled for March 6-7, which should be soon enough for your purposes. If the proposal is approved, you may still be able to run in the May-June period. Presumably the set-up will be fairly straightforward, and the impact on the AGS resources appears to be small.

The present document seems adequate as a proposal. However, it will not be sent to the Committee until January 23, the deadline for the March meeting. Therefore if you do have any revisions or additions, they can be included if they reach us by that date.

I look forward to seeing you in March.

Yours sincerely,

Neil Baggett for RBP

Robert B. Palmer

/ke

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December 27, 1985

Prof. Thomas Erber
Dept. of Physics
Illinois Institute of Technology
Chicago, Illinois 60616

Dear Tom,

Enclosed is a short note summarizing my calculations on transition radiation and scintillation in He gas. I hope we can make an actual measurement of the transtion radiation effect. The scintillation background should be no problem.

Best wishes,

Kirk McDonald

Kirk McDonald

Princeton University

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December 27, 1985

Dr. Xavier Maruyama
Center Rad. Res.
National Bureau of Standards
Gaithersburg, MD 20899

Dear Xavier,

Follwing your remarks at Bates I have looked into the effect of transition radiation. It is small, but perhaps we can actually measure it! Enclosed is a short note summarizing my calculations on transition radiation and scintillation in He gas. The scintillation background should be no problem.

Best wishes,

Kirk McDonald

Princeton University

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December 30, 1985

Dr. Robert Palmer
Associate Director for High Energy Physics
Brookhaven National Laboratory
Upton, Long Island, NY 11973

Dear Bob,

Thank you for your letter of 23 Dec. concerning my proposal to observe the synchrotron-Čerenkov effect. I would be pleased to make a presentation to the BNL PAC in March 1986.

It now appears extremely likely that I will actually do the experiment at Bates Lab in the Spring. However I would like to preserve the chance to do it at BNL if the Bates option falls through due to unforeseen circumstances. Please advise me if it is still appropriate to address the BNL PAC under these conditions.

Enclosed is a short writeup which indicates how we should also be able to measure the interference between Čerenkov radiation and transition radiation as a bonus.

Sincerely yours,

Kirk McDonald

Princeton University

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January 2, 1986

Prof. Ernest Moniz
Director
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Ernest,

Enclosed is a note which considers the effect of transition radiation on the synchrotron-Čerenkov experiment. With a little effort we should be able to observe transition radiation, which should occur at a rate somewhat below the synchrotron radiation caused by 80 gauss. Very close to Čerenkov threshold there is an interference effect, which we would probably be the first to observe.

So this potential 'background' only adds to the interest of the experiment! I look forward to taking data at Bates sometime this Spring.

Sincerely yours,

Kirk McDonald

Massachusetts Institute of Technology
Laboratory for Nuclear Science
BATES LINEAR ACCELERATOR CENTER

Post Office Box 846
Middleton, MA, USA 01949

617-245-6600
Telex 92 14 73

January 3, 1986

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

Dear Professor McDonald:

Thank you for visiting Bates for discussions about your proposed measurement of synchrotron-Cerenkov radiation. We will be able to support your experiment as a parasitic run in the South Hall. A run late in the Spring will likely be possible. Please keep us posted on your readiness to perform the measurement. Jay Flanz and Lyman Stinson will be able to answer questions you may have about the Laboratory and to make the necessary preparations.

Sincerely yours,

Ernest J. Moniz
Director
Professor of Physics

EJM/sad

BROOKHAVEN NATIONAL LABORATORY
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Upton, Long Island, New York 11973

(516) 282 / FTS 666 3830

Office of the Director

January 8, 1986

Prof. Kirk McDonald
Princeton University
Department of Physics
P.O. Box 708
Princeton, New Jersey 08544

Dear Kirk:

As we discussed, I would indeed like to have you present your synchroton-Cerenkov proposal to the AGS program committee in March, even though you may end up doing the experiment at Bates. Please send 40 copies to my office at Brookhaven by January 23 so that we can distribute them in the usual way and put your presentation on the agenda. It would be helpful if you could include a one-page summary as the second page of the document, immediately following the cover page. You will hear from Neil Baggett in early February regarding the detailed arrangements.

Yours Sincerely,

RB Palmer /NVB

Robert B. Palmer

RBP/mal

Princeton University

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January 14, 1986

Dr. Jay Flanz
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Jay,

Thank you for joining the synchrotron-Čerenkov experiment. I'm sure we will have fun doing it!

Enclosed is a note in response to some comments made during my visit to Bates. With a little extra effort we ought to see interference between Čerenkov radiation and transition radiation! This is possible because the electron beam must pass thru the light-collection mirror if we are to see the synchrotron-Čerenkov interference effect at angles $\sim 1/\gamma$ to the beam. In this case we cannot avoid transition radiation at the mirror surface.

The theory of Čerenkov-transition interference predicts a logarithmic singularity right at the Čerenkov threshold. This is a narrow peak in practice, which will be smeared out by finite resolution in the pressure and temperature measurements, by the variation with wavelength of the index of the He gas, and by the finite energy spread of the electrons.

How narrow in energy could we make the electron beam? As total electron rate is probably not a problem, it is worth throwing away the tails of the energy spectrum to obtain the most monochromatic beam possible.

For the success of the experiment, support from Bates will be useful in (at least) 3 areas:

- Good quality parasitic beam;
- Operational support for the Čerenkov counter;
- Fast electronics.

I enclose some pages describing the latter two categories, which I have also sent to Lyman Stinson. Let me know about the plausibility of these items after you've had a chance to review them. I'll be happy to visit Bates again to discuss things whenever it seems appropriate.

Sincerely yours,

Kirk McDonald

Kirk McDonald

Princeton University

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January 14, 1986

Lyman Stinson
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Lyman,

Thank you again for discussing possible support by Bates Lab for the synchrotron-Čerenkov experiment during my visit in December.

Enclosed are some pages summarizing my current thinking on these support items. If we could borrow the electronics from the Bates pool, while we are at Bates, this would be quite useful.

I am presuming the experiment will be set up in the C line, so that a fairly tall stand for the Čerenkov counter can be built by us.

Jay Flanz has joined the experiment, and I have sent him a copy of the enclosed notes.

I will of course be happy to discuss any of these items over the phone, or in another visit to Bates Lab.

Sincerely yours,

Kirk McDonald

Kirk McDonald

ILLINOIS INSTITUTE OF TECHNOLOGY

Technology Center
Chicago, IL 60616

Physics Department

January 17, 1986

Professor Kirk McDonald
Department of Physics
P.O. Box 708
Princeton, N.J. 08544

Dear Kirk:

This is the season of my birthday, and the two proposals you sent were among my favorite presents.

I've looked at the design of the experiments; here are some remarks that might be helpful:

1. Enclosed are two references (Bhiday et al. 1958; Burkhardt et al. 1980) that are reasonably well documented and show the threshold behavior of Čerenkov counters. There are many other references, but none worth mentioning.

2. The basic idea of the experiment is to look at radiation from high energy electrons in gases in the presence of a (weak) magnetic field. The 'old' concept of this situation is to regard it as a superposition of synchrotron radiation and Čerenkov radiation (S + Č); the new idea that is to be tested is that there is only a single radiation process, 'synchrotron-Čerenkov' radiation (SČ). There are two essential differences between S+Č and SČ that are relevant in the proposed set-up: (i) Čerenkov radiation has a definite threshold (phase transition); in particular, this implies that below a critical gas pressure Č is switched off, except for weak diffractive effects. In contrast, SČ radiation has no sharp threshold; the controlling Airy functions have no abrupt variations in the vicinity of the Č transition. In practise, this distinction can be demonstrated by

arranging the parameters so that the S spectrum peaks below the visible. Effectively, in the optical region of the spectrum, the Č threshold then marks the upper end of a spectral gap; the S+Č description then indicates a step-function behavior at the Č threshold. This sharp spectral veriation does not occur in SČ radiation; hence a dramatic contrast between S+Č and SČ radiation below the Č threshold. A numerical illustration is given in Appl. Phys. Lett. 35, 753,1979. The enclosed calculations show that with 700 MeV electrons in He this contrast will not be so conspicuous; the differences in radiation intensities will have to be substantiated by careful calibration. (ii) Another difference between SČ and S+Č radiation originates from the oscillatory behavior of the Airy functions. Č above threshold has a linear variation with pressure. SČ radiation has oscillations: these are damped or smoothed when one integrates over angles, i.e. $P_1(x)$ doesn't oscillate as much as $[Ai(x)]^2$, and integrating over wide spectral ranges smoothens things ever more. The initial Bates set-up should show this non-monotonic behavior--- in future editions it would be instructive to make angle-resolved measurements of SČ radiation; the Airy striations would then be even more conspicuous.

Comments on the Enclosed Computations

3. Scintillations in He (pp.1-4). The estimates show that a 700 MeV electron in He at STP will generate about 0.5 photons per cm in the visible region of the spectrum. With a 0.06 solid angle reduction factor, and another 0.03 due to operating at or below 0.03 Atm, the net scintillation rate should be of the order of 0.001--- therefore not a problem. However, the scintillation rate in the vacuum ultra-violet is higher by a factor of 14. If experimental extensions to the UV are considered, this enhanced scintillation rate may be a problem.

4. Coulomb bremsstrahlung rates (p.5). In He at STP the photon yield in the visible portion of the spectrum is of the order of 10^{-6} photons/cm for each high energy electron. This source of background is totally insignificant.

5. Angular Distribution of SČ Radiation (pp.6-8). In the proposed experiment, for HE pressures at the Čerenkov threshold, $P_T \sim 7.62\times10^{-3}$ Atm, the angular spread of the radiation should be of the order of 2mrad. In fact, the angular widths of all three radiation processes S, Č, and SČ, are of the order of a few milliradians.

6. Synchrotron Radiation (pp. 10-11). The total rate in the visible region of the spectrum is 0.007 photons per electron radiated over 2 meters in an 80G field. This is not too different from the expected SČ rate in the vicinity of the Č threshold. This computation ignores phototube efficiencies and relies on a mean value approximation of the 'Kappa' function for the synchrotron spectrum.

A more clear-cut comparison emerges if we look at the rates per unit length per unit frequency. Then for S we obtain 2.6×10^{-5} (bottom of p.10), and for SČ (The P_1 polarization component) 6.4×10^{-5} (at P_T, p.13); so the ratio of the rates is circa 2.5, and possibly higher if polarized light can be extracted from the set-up. These ratios are more conservative than those indicated on the graphs.

7. Synchrotron-Čerenkov Radiation (pp.12-14). This is mainly an exercise in checking for typographical errors and verifying consistency in the notation of various published formulas. Everything turns out to be OK. As before, the pressure at the Čerenkov threshold is 7.62×10^{-3} Atm, this corresponds to $P_1(0)=.388$. The first maximum occurs at $P_1(.6)=.443$ or 1.1×10^{-2} Atm. The first minimum is at P_1 $(1.8)=.295$ with a pressure of 1.8×10^{-2} Atm. Everything is consistent with the proposed experimental design. In the pressure range $\frac{1}{2}P_T$ to P_T we expect an enhancement of roughly 2.5

in the ratio of SČ to S+Č radiation. If there are no unpleasant surprises from background sources, this should make for a convincing demonstration!

8. Transition Radiation (pp.15-17). A vulnerable point in the proposed experimental design is the low radiation rate---for the SČ process, about $6x10^{-5}$ optical photons per centimeter per electron. Consequently, even optical transition radiation from high energy electrons may be significant. The computations indicate that about 0.003 optical phtons will be emitted into a 4 mrad cone at each dielectric-vacuum interface; about 0.013 photons will be radiated into a 30^{o}cone. The corresponding transition or formation zone is circa 1 meter ---so one can't really think of this emission as being localized at an interface. A. Bodek <u>et al</u>. discovered this (too late!) in their 1.5m threshold counter(UR 837, COO-3065-345,January 1983).

9. Magnetic 'Transition' Radiation. Undulator and wiggler inserts in storage rings perturb the synchrotron spectrum. It was first shown by Schott (1912, Chapt 8 and Appendix B of his book) that these perturbations can be described by modulating the synchrotron spectrum by the Fourier transform of the wiggler structure. But for sufficiently abrupt variations of the magnetic field, this perturbation picture must break down --- the exponentially damped synchrotron spectrum then is deformed into a power law spectrum resembling Coulomb bremsstrahlung.

The precise meaning of 'abrupt' is that the magnetic field varies appreciably over a distance of the order of the coherence (=formation) length for synchrotron radiation. For relativistic electrons this coherence lenght is given by

$$L(cm) \simeq 1.7x10^{3}/H(G).$$

For 80G fields this is a conveniently <u>large</u> distance, circa 21cm. This length scale is very much larger for protons, and is the basis for Coïsson's discovery of 'edge' radiation (Optics Commun. <u>22</u>, 135-137,August 1977).

Since 21cm is roughly comparable to the 94cm formation length of transition radiation (p.17), one might expect interference or 'synergism' between synchrotron and transition radiation. In any case, transverse field variations $0 \lesssim H \lesssim 100G$ over 5cm lengths (easily done with strip-lead, etc.) should lead to drastic changes in the synchrotron spectrum.

It is encouraging to hear that matters are progressing with Bates. The very best of luck with the experiment.

Best regards,

Tom

T. Erber

BROOKHAVEN NATIONAL LABORATORY
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Upton, Long Island, New York 11973

(516) 282 / FTS 666 3830

Office of the Director

January 27, 1986

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

Re: AGS Proposal #827

Dear Professor McDonald:

This is to acknowledge receipt of your proposal, "Proposal to Measure the Synchrotron-Čerenkov Effect." This proposal will be distributed soon to the members of the AGS Program Committee.

Sincerely,

Laura Zaharatos

Mrs. Laura Zaharatos

lz

Princeton University

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February 6, 1986

Dr. Jay Flanz
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Jay,

Enclosed are copies of the sheets I have submitted in repsonse to a request from Bill Lobar. Perhaps you could let me know if they need modification.

Work here is underway on the Čerenkov detector, but it is optimistic that it will be complete by April 1. So I have indicated that May 1 is a more realistic running date for the experiment.

I hope all is well with your family.

Best wishes,

Kirk McDonald

Princeton University

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February 6, 1986

Beam Requests, c/o W. Lobar
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Bill,

Enclosed are the beam request sheets for the synchrotron-Čerenkov radiation experiment. If clarification is needed please call me, or get in touch with Jay Flanz, who has joined the experiment.

Sincerely yours,

Kirk McDonald

2/27/86

D.J.'s Comments on K. T. McDonald's Proposal

1) This is interesting (not fundamental) piece of work and it should be done.

2) It best be done on Electron Machine (Cornell) to avoid unnecessary complications with p, π, e^+, etc. [As an example from my personal experience: I calibrated a Cerenkov counter at Cornell although at that time at ZGS working accelerator. See D. Yovanovitch et al NIM 94, 477 (1971)]

3) There is a whole spectrum of pratical pointers on how to do this measurement Mr. McDonald can get by reading our article in Zeit. Phys. C 18, 289, (1983). These have to do with integrating light with and without shutters etc.....

4) There is an effect due to infinite length of radiator (Cerenkov counter L=5m) which comes into play at these low light yield which has to be taken into account.

5) Transition radiation comes from both boundaries: 1) entrance window + 2) mirror, hence there is a factor of 2 missing.

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Office of the Director

March 8, 1986

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

Dear Kirk:

I am pleased to inform you that your Proposal #827, "Proposal to Measure the Synchrotron-Cerenkov Effect," is approved for 100 hours upon the recommendation of the AGS Program Committee. Most members of the Committee found the proposed experiment to be cute, even though the physics import was not clear. We assume that you will in fact be able to do the experiment at the Bates electron linac, which will certainly have advantages and is probably the best place to do this experiment. Nevertheless, we wanted to let you know of our interest in the measurement. Should the Bates experiment not work out, you will be welcome here; in that case, please contact Don Lazarus.

In any case, good luck with it, and please keep us informed as to your progress.

Yours sincerely,

Bob

Robert B. Palmer

/lz
cc: APC Members
D. Lazarus

Princeton University

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March 13, 1986

Prof. Thomas Erber
Dept. of Physics
Illinois Institute of Technology
Chicago, Illinois 60616

Dear Tom,

Bates Lab asked me to prepare a short note on the synchrotron-Čerenkov experiment for their 1985 annual report; here it is.

Best wishes,

Kirk McDonald

Princeton University

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March 13, 1986

Gary Nixon
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Gary,

Enclosed is a short writeup on our upcoming experiment at Bates. It would be easy for me to reformat the paper, as it is on the word processor. I could also provide photographs of the figures.

Sincerely yours,

Kirk McDonald
Professor of Physics

Princeton University

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March 13, 1986

Dr. Jay Flanz
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Jay,

Gary Nixon asked me to send him a short writeup describing our experiment, a copy of which is enclosed. It includes a suggestion that we might also be able to see an extra component of synchrotron radiation in the fringe field of the magnet.

Work on the detector is moving rapidly at last. The tank has been welded together, and the first magnet coil is being wound today. May 1 now seems like a very solid date for transporting the apparatus to Bates.

Best wishes,

Kirk McDonald

Princeton University

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April 2, 1986

Dr. Robert Palmer
Associate Director for High Energy Physics
Brookhaven National Laboratory
Upton, Long Island, NY 11973

Dear Bob,

This is in regard to my BNL proposal #827. Apparently I am on the schedule at Bates Lab as a parasitic user for 7 days beginning June 11, 1986. Given this commitment from Bates it is unlikely that I would wish to participate in the May-June run at BNL. I will keep you informed if any difficulties arise which alter this conclusion.

Sincerely yours,

Kirk McDonald

FROM TOM ERBER, MARCH 1986

during a distance $\Delta R < R$, then the characteristic frequency of the radiation is $\sim \gamma^2 c/\Delta R$, compared to $\sim \gamma^2 c/R$ for ordinary synchrotron radiation.[13] This effect has been observed for the radiation of 200-GeV protons in the fringe field of 1 Tesla magnets.[14,15] It turns out that for 700-MeV electrons and our magnet geometry, an enhancement of optical synchrotron radiation is be be expected for fields of less than 50 gauss. In our experiment with such low fields, this effect competes with the transition radiation, but we may be able to observe it.

$$\Delta R < R/\gamma \simeq \lambda_c \frac{H_{cr}}{H} \sim \frac{1.7 \times 10^{3}\ \text{cm}}{H(\text{Gauss})}$$

the criterion $\Delta R < R$ would include wigglers etc. and this gets back to ordinary (Fourier) modulation of synchrotron spectrum.

I like the write-up! Hope you continue to create a favorable climate. Best of luck with hardware — the enclosed shows the lengths to which we intended to go to suppress background.

Tom

UNIVERSITY OF CALIFORNIA, LOS ANGELES — UCLA

BERKELEY · DAVIS · IRVINE · LOS ANGELES · RIVERSIDE · SAN DIEGO · SAN FRANCISCO — SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

July 10, 1986

Professor K.T. McDonald
Joseph Henry Laboratories
Princeton University
Princeton, N.J. 08544

Dear Kirk:

Thanks very much for sending a copy of your preprint announcing the experimental discovery of synchrotron-Čerenkov radiation. The manuscript is concise and nicely done: it conveys a sense of the basic issues and lays out the essential components of the experiment.

There are a number of things you might want to consider in polishing the text --- as usual, these remarks are furnished "...on deep background..." as they used to say in the Kissinger days.

1. Typographical slips - enclosed are copies of the MS with several blemishes marked in red.

2. References - yes, quote the original sources!

   Čerenkov radiation: no doubt here experiment preceded theory; many prior sightings of the 'light' were misinterpreted as secondary effects accompanying ionizing radiation. A. Sommerfeld in Göttinger Nachrichten, p.201 et seq., 1905, worked out the dynamics of 'super-light' electrons --- if you insert an index of refraction in his results, you get formally correct expressions for the Čerenkov radiation rate.

   Synchrotron radiation: here theory was well in advance of experiment. All the correct relativistic results were worked out by Schott in his Adams prize essay 1912 (see enclosed references). Low energy electrons radiating in weak magnetic fields were used in magnetrons, later klystrons, during the 30's. G.N. Watson worked out the crucial asymptotic Bessel function results at Schott's instigation (1917-1919). Iwanenko first connected Schott's work with the parameters technically accessible with accelerator technology circa 1943. The priority of these papers is acknowledged by Schwinger in his 1949 paper. JS's only original contribution is the discussion of first quantum corrections in Proc. Nat. Acad. Sci. 1954.

3. Why not start paragraph two with "Some approximate..." rather than "The approximate..."

4. Add the sentence "The small momentum transfers to the field and the medium are essentially unobservable", at the appropriate spot in paragraph two.

5. Last paragraph on p.3, why not replace "simultaneous" with "synergic"? I hate buzz words too, but here it conveys the thought with greater precision.

6. Last line, first column p.4, continue the sentence as follows: "...refraction of gases in the UV and X-ray regions of the spectrum.[13] The present observations also suggest that less intuitive consequences such as the suppression of radiation in the soft X-ray range may be accessible to experiment.[8]"

7. A general observation: The phrase "...never more than one detected photon per electron..." is linked with the fact that the mean free path for photon emission at 50G is circa 3300 cm. Note that your field varies sharply on this length scale but that the predicted features of S-Ĉ radiation are observed experimentally. Hence, the non-trivial conclusion that the mfp is <u>not</u> the relevant length scale. So we presume ...unlike Coïsson... that α (mfp) ≈ 22 cm is what it takes! A sharp field variation on a 10 cm scale should definitely (a) turn the exponential synchrotron shoulder into a power law decrease; (b) diminish the S-Ĉ oscillations. Shorter path lengths (≈ 30 cm) should make angular structure accessible to measurement.

Best regards,

Tom

T. Erber
Visiting Professor

PS: I presume that heating effects limit the attainable field — but with no gas that's irrelevant. Running a couple of seconds at 200 G would be instructive. I worry about the normalization --- could there be some $\sqrt{4\pi}$ that's poisoned all the standard trees? The structure at the origin of Fig. 1 is intriguing!

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July 17, 1986

Dr. Xavier Maruyama
Center Rad. Res.
Building 245, Room B108
National Bureau of Standards
Gaithersburg, MD 20899

Dear Xavier,

Thank you for the invitation to talk at N.B.S. about our results on the synchrotron-Čerenkov effect. I await confirmation of the exact date, but Sept. 11 is good for me.

Enclosed is a draft of a paper summarizing our first efforts.

Best wishes,

Kirk McDonald

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July 18, 1986

Dr. Jay Flanz
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Jay,

Enclosed is a revised draft of the paper. For me the biggest question mark is the value of the magnetic field–your Rush probe suggested it might be 3% lower than the values stated. We still have a discrepancy in the absolute normalization of a factor of 4 or so. Half of this is suspected to be due to properties of the RCA 8854 phototube....

If you feel it would be helpful, I could fly up to Bates to assist in the magnetic measurements.

Best wishes,

Kirk McDonald

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July 21, 1986

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

Dear Tom,

Thank you for your comments on the draft of the Čerenkov-synchrotron paper. Most of them are included in the revised text in some form. Enclosed is a copy of the paper as it stands now. It has not yet been submitted to P.R.L.

As we discussed, I would welcome contact with people interested in applications of the Čerenkov-synchrotron effect, especially any which might make use of the apparatus, suitably modified.

Best wishes,

Kirk McDonald

UNITED STATES DEPARTMENT OF COMMERCE
National Bureau of Standards
Gaithersburg, Maryland 20899

August 8, 1986

Professor Kirk McDonald
Dept. of Physics
Joseph Henry Laboratories
Princeton University
Post Office Box 708
Princeton, New Jersey 08544

Dear Kirk:

Thank you for accepting our invitation to give a seminar to the Center for Radiation Research at the National Bureau of Standards. September 11 has been reserved. We have a coffee social at 10:45 and the talk begins at 11:00. Enclosed is a subway map. The Amtrack train arrives at Union Station and the metro station for NBS is Shady Grove. There is a shuttle on the west side of Shady Grove Station to NBS leaving at 9:15, 9:45 and 10:15 which comes to the main building. Our building is 245, which can be reached by obtaining an on grounds shuttle. You can obtain the shuttle by asking the receiptionist at the desk in the main building. However, if you call me at work (921-2505) or home (468-0613), when you get to Union Station, I will come to Shady Grove to pick you up. The trip from Union Station to Shady Grove takes about 45 minutes. I will call you earlier the week of September 11th to make final plans.

I was very impressed with your preprint. It gets to the point quickly and even I can understand it. For your information, enclosed is the paper on microwave Cerenkov radiation, which is a more mature observation continuing the work of your reference 22.

Yours truly,

Xavier

Xavier K. Maruyama
Nuclear Physics Group
Center for Radiation Research

Enclosures

Princeton University

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August 12, 1986

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

Dear Tom,

Enclosed is a copy of the Čerenkov-synchrotron paper as submitted to P.R.L. I also enclose copies of the final figures, which have somewhat improved fits, after small corrections to the beam energy, magnetic field strength, and pressure scale.

Best wishes,

Kirk McDonald

Princeton University

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August 12, 1986

Dr. Jay Flanz
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Jay,

Enclosed is a copy of the paper as submitted to P.R.L. I believe all your suggestions were incorporated in some manner.

Best wishes,

Kirk McDonald

Princeton University

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August 12, 1986

Editors
Physical Review Letters
P.O. Box 1000
Ridge, NY 11961-2701

Gentlemen:

Enclosed please find the manuscript 'Observation of Interference between Čerenkov and Synchrotron Radiation,' by K.D. Bonin *et al.*, which we wish to submit for publication in Physical Review Letters.

The experimental results show that two well-known phenomena share a more common origin than is usually perceived. This work will be accessible to a broader audience than is typical for an experiment in high energy physics. We have found that most people respond to the result with initial surprise followed by a rapid appreciation of an interesting physical situation.

We would like the paper, if published, to appear in the *Elementary Particles and Fields* section of P.R.L. Although the phenomena can be treated by a classical analysis, the experimental measurements were made on individual electrons and photons, using the techniques of elementary particle physics.

A coppyright transfer form is also enclosed. Please address communications regarding the manuscript to me.

Sincerely yours,

Kirk McDonald
Professor of Physics

Princeton University

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Telephone: 609 258-6608 Telecopier: 609 258-6360
E-mail: mcdonald@puphep.princeton.edu
Internet: http://puhep1.princeton.edu/∼mcdonald/

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

Princeton University

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Internet: http://puhep1.princeton.edu/∼mcdonald/

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

UNIVERSITY OF CALIFORNIA, LOS ANGELES — UCLA

BERKELEY · DAVIS · IRVINE · LOS ANGELES · RIVERSIDE · SAN DIEGO · SAN FRANCISCO — SANTA BARBARA · SANTA CRUZ

THE UNIVERSITY OF CALIFORNIA · LET THERE BE LIGHT · 1868

DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

16 August 1986

Dear Kirk:

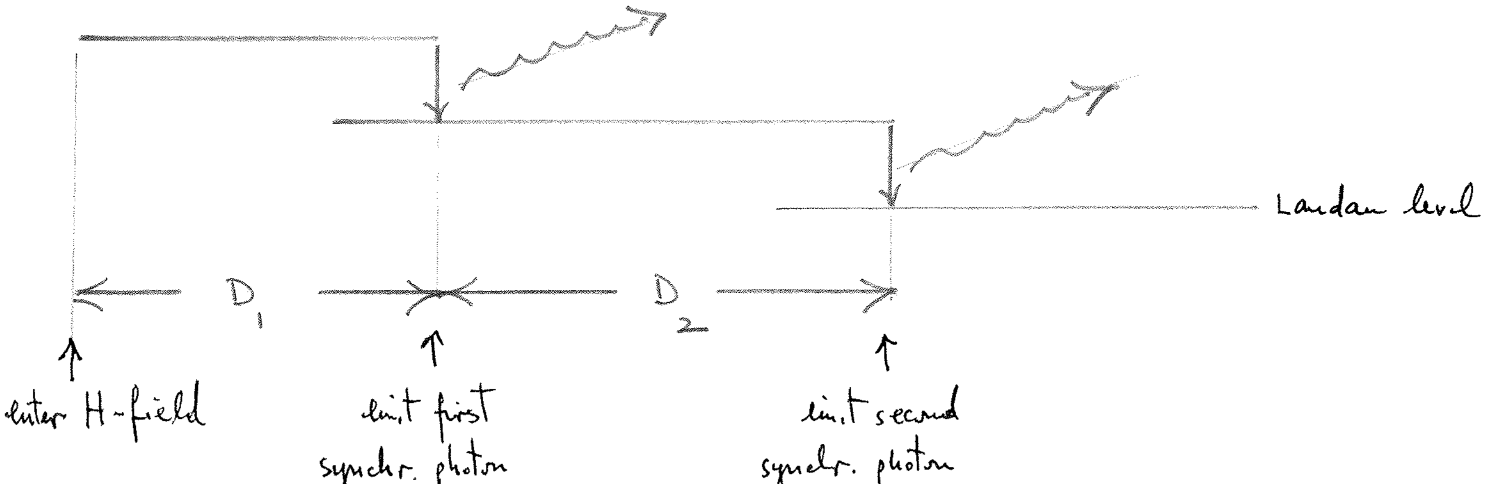
I have access to two secretaries — both saturated with last minute typing for me before return to IIT; so you'll have to put up with scribble by hand:

1) Thanks for sending penultimate version of MS — hope you have smooth sailing through PRL refereeing process.

If, and when, there's another edition of this experiment, it would be nice to track down the factor of "4". I don't really know of any high energy QED experiment with absolute calibration --- and am open minded enough to entertain the possibility that between J.J. Thomson and present day some of the standard trees could have become contaminated; say, misunderstandings of the magnitude of "e" in Heaviside units, etc. A special virtue of your set-up (& our SLAC proposals of eldritch times) is that radiation from single electrons is detected! Need I say more?

3) As a by-product of some current work on 'quantum telegraphs' my curiosity

about double photon emission rates was fixed up. Consider the following simple diagram:



D = total distance in H-field under phototube surveillance (~3.3 m)

L = characteristic distance to emit a photon (usually infra-red) ~ 33 meters for H ~ 50 G.
$\sim \frac{\lambda_c}{\alpha} \frac{B_{cr}}{B}$

Probab. for emitting n photons in distance D $= \left(\frac{D}{L}\right)^n \frac{e^{-D/L}}{n!}$ (Poisson statistics!)

Cumulative probability that 2 photons are emitted in distance D

$$= 1 - \left(1 + \frac{D}{L}\right) e^{-D/L} \underset{\text{if } D \ll L}{\simeq} \frac{1}{2}\left(\frac{D}{L}\right)^2 \sim \frac{1}{2}\left(\frac{3.3}{33}\right)^2 \sim \frac{1}{200}$$

Distance (or time) between successive photon emissions has an exponential probab. density function $= \frac{1}{L} e^{-D_2/L}$. So when 2 photons are emitted in D $(D_1 + D_2 \leq D)$ the time between photons is exponentially distributed --- favoring short times!

A short run at higher field values and with higher energies (shift peak of synch. towards visible) should help settle this question. If I have a chance to talk at Princeton about the 'telegraph' you'll appreciate full significance.

Best regards Tom

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September 3, 1986

Dr. Tom Himel
Bin 95
Stanford Linear Accelerator Center
P.O. Box 4349
Stanford, CA 94305

Dear Tom,

I understand that you had some conversations with Tom Erber on Thursday and Friday regarding beamstrahlung. He has been urging me to follow up some work I did on synchrotron radiation at Bates Lab with an experiment involving a short magnet. I believe this is indeed closely related to the alteration of the beamstrahlung spectrum for short bunches, as we briefly discussed.

I enclose a copy of the paper on the synchrotron-Čerenkov effect, which includes reference 14 to the short-magnet case. If we want to study the latter via optical radiation, some practical considerations are:

- The formation length can be written

$$L \sim R/\gamma = \lambda_c B_{\rm cr}/B = 1702[\text{cm}]/B[\text{Gauss}],$$

  using $B_{\rm cr} = 4.41 \times 10^{13}$ Gauss. Thus if we want L to be longer than the length of laboratory magnets, we must have $B < 100$ Gauss.

- The characteristic energy of the synchrotron radiation is

$$\hbar\omega \sim \gamma^2 mc^2 B/B_{\rm cr} = 1.16 \times 10^{-8}[\text{eV}]\gamma^2 B[\text{Gauss}].$$

  Hence if we use a field of $B = 100$ Gauss, and desire the radiation to peak around 1 eV, we must use electrons with $\gamma = 1000$, *i.e.*, 500 MeV.

These are pretty much the parameters of our effort at Bates Lab, chosen for slightly different reasons. But it is natural, and now perhaps more urgent that we continue and make the short-magnet study. Of course one could use higher energy beams such as at SLAC, and measure x-ray spectra, calling on the resources of SREL.

In any case, after prompting by you and Tom Erber it is likely we make an effort to study this in the near future.

Sincerely yours,

Kirk McDonald

ILLINOIS INSTITUTE OF TECHNOLOGY
Lewis College of Sciences and Letters
Department of Physics

September 25, 1986

Professor Boris W. Batterman
Director - CHESS
Wilson Laboratory
Cornell University
Ithaca, New York 14853-8001

Dear Professor Batterman:

The enclosed preprint by Kirk McDonald and coworkers describes a basic advance in synchrotron radiation. A second edition of this experiment is scheduled to run at Bates sometime during February - March '87. Since this work could have a fundamental influence on the way synchrotron radiation is exploited in materials research, it might be a good idea if someone from CHESS were to observe or participate in the experiment.

The basic physics issue is the following: Synchrotron radiation research ---as it is now carried out at all the major installations --- is essentially a two-step process: the light is generated in the accelerator and subsequently interacts with the sample to be studied. In synchrotron - Čerenkov radiation the 'light bulb' and the sample are combined --- the properties of the sample directly control the nature of the emitted radiation. Among numerous areas of application one of the most promising is the study of dispersive properties of tenuous media in the X-ray region of the spectrum. Current techniques can detect deviations from the free electron dispersion of the order of 10^{-6} in the keV region of the spectrum; synchrotron-Čerenkov radiation can extend this sensitivity to 1 part in 10^{9}. A detailed discussion is given by T. M. Rynne ["Synchroton-Cerenkov Radiation in the Vicinity of an Atomic Absorption Edge", Jr. of Appl. Phys. 52, No. 11, 6471-6481, Nov. 1981]. This technique is ideally suited for X-ray laser research.

Professor McDonald's primary interest is in high energy physics, but he is willing to bequeath his equipment for S-C research (see his enclosed letter). Needless to say, I want to take an active part in continuing the work, but IIT's resources are currently at a marginal level. In any event, life is too short for continuing 'demonstration' experiments, and what would really be useful is to check on the feasibility of the S-C resonance experiments with a 'third edition' at Bates, and then to adapt the procedure for a general purpose program of CHESS.

Perhaps a seminar on S-C radiation would be the most efficient way of acquainting people with this circle of ideas. Either McDonald or I would welcome the chance to visit Cornell to explore the possibilities.

Sincerely

T. Erber
Professor of Physics and Mathematics

encl:

cc: M. E. Fisher
K. T. McDonald

EXPERIMENTAL STUDY OF SYNCHROTRON-CERENKOV RADIATION

SLAC Proposal E-126

ADDENDUM: February 1978

1. Synchrotron-Cerenkov Radiation Adapted for Particle Discrimination (π/e) and Energy Determination ($0.3\ \mathrm{GeV} \gtrsim E \lesssim 10\ \mathrm{GeV}$).

2. Improvement of Krypton Experiment (D) with Channel Cut Silicon Crystal Monochromators.

Note that Eq. (1.5b) implies that for fields in the tens of kilogauss range we will have electrons producing a reasonable photon signal. For example an electron in a 20 kG field traversing a length of 1m will produce roughly 12 photons. If another particle of a different mass were to pass through the system the total number of synchrotron photons that could be produced would be

$$\frac{dN^S}{dL}\ (\text{photons/meter}) \simeq 0.6\ H(\text{kG})\ \frac{m(\text{electron})}{m(\text{particle})}\ .$$

Thus for particles other than electrons we will not see any signal except in those very rare cases where the photon statistics allow for a multi-photon signal. In other words synchrotron radiation itself can be used in cases where electrons must be discriminated against other singly charged particles. Information concerning the energy of the electrons can then be obtained by exploiting the characteristics of $G(\xi)$; specifically the threshold behavior at $\xi \sim 0.3$. We note first that in practical units (1.4) is equivalent to

$$\xi = 29.43\ \frac{\omega_p(\text{eV})}{E(\text{GeV})\ H(\text{kG})}\ ; \qquad (1.7a)$$

and therefore

$$E(\text{GeV}) \lessapprox 98.1\ \ \omega_p(\text{eV})/H(\text{kG}) \qquad (1.7b)$$

is the (adjustable) electron energy threshold below which S-C emission is completely quenched!

Let's consider the special case where we wish to reject electrons of energy less than .3 GeV. To obtain a reasonable signal for a 1 meter path length we shall choose a magnetic field value of about 20 kG. Under these conditions we find that the value for the synchrotron frequency is

$$\omega_c(\text{keV}) = 6.65 \times 10^{-2}\ E^2(\text{GeV})\ H(\text{kG}) = .12\ \text{keV}\ . \tag{1.8}$$

Since the principal portion of the Synchrotron-Cerenkov spectrum lies below this value it is necessary that we choose a medium whose absorptivity over a meter of material in the relevant spectral range is small. Helium at densities slightly less than STP fulfills these criteria. Using condition (1.7b) we find that the plasma frequency that meets the threshold condition is

$$\omega_p = 6.116 \times 10^{-2}\ \text{eV}\ ; \tag{1.9}$$

the corresponding density of helium is 9.01×10^{-6} gms/cc, or roughly one-twentieth the density at STP. Figure 2 shows the amount of photons produced for varying densities of helium as a function of the electron energy. We see that as the density decreases the "threshold" energy diminishes. Figure 3 shows the characteristics of synchrotron-Cerenkov radiation as a function of changing magnetic field. Clearly an energy discrimination is feasible. The threshold becomes sharper as the magnetic field intensities are increased. A simple variant of this arrangement suitable for $E \sim 10$ GeV discrimination is shown on Fig. 4.

2. Improvement of Krypton Experiment (D) with Channel Cut Silicon Crystal Monochromators.

The resolution of the experimental configuration shown in Fig. 0 of the blueprints can be dramatically improved by the introduction of a channel cut monolithic silicon crystal; see Fig. 5. This crystal monochromator would be mounted directly in front of the Si(Li) detector with slits being inserted to define the resolution of the instrument. From Bragg's law the resolution is given by

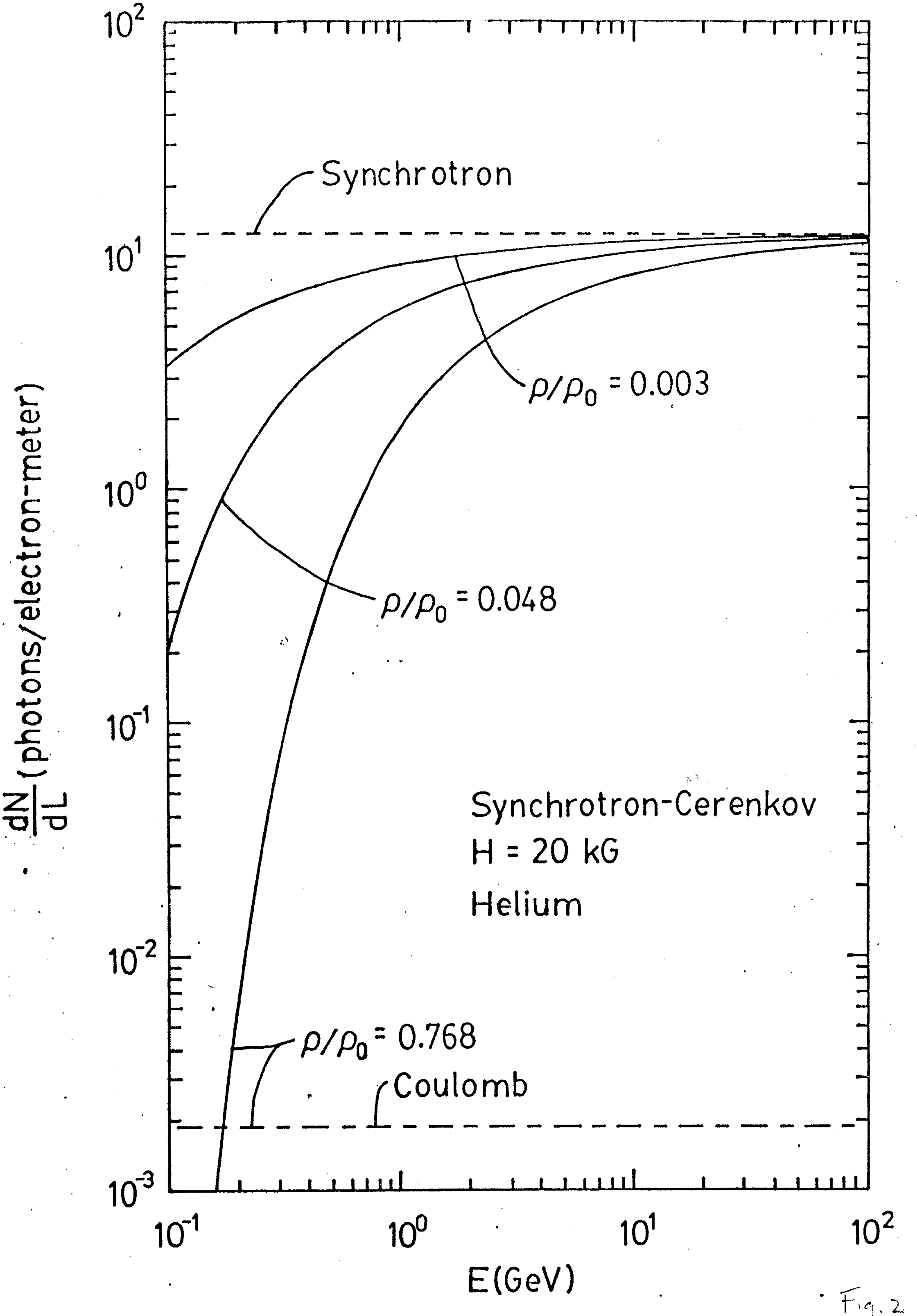
Synchrotron
ρ/ρ0 = 0.003
ρ/ρ0 = 0.048
Synchrotron-Cerenkov
H = 20 kG
Helium
ρ/ρ0 = 0.768
Coulomb
dN/dL (photons/electron-meter)
E(GeV)
10^-3
10^-2
10^-1
10^0
10^1
10^2

Fig. 2

Princeton University

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October 13, 1986

Dr. Robert Saunders
Center for Radiation Research
National Bureau of Standards
Gaithersburg, MD 20899

Dear Bob,

I believe I spoke with you a week or two ago about the possible use of a spectroradiometer at the N.B.S. electron linac. As we are interested in sensitivity into the infrared, the enclosed product information from Epitaxx might be of interest. On comparison with specs for PbS cells, the InGaAs cell did not seem an order of magnitude better, however.

Best wishes,

Kirk McDonald
Professor of Physics

Science and Engineering Research Council
Rutherford Appleton Laboratory

Chilton, DIDCOT, Oxon
OX11 0QX
Tel Abingdon (0235) 21900 Ext 6128
Direct Line (0235)
Telex 83159 RUTHLB G
Fax (0235)44 5848

Dr K D Bonin
Joseph Henry Laboratories
Princeton University
Princeton
NJ 08544
USA

29 October 1986

Dear Dr Bonin and Colleagues

DOE/ER/3072-37

I was very interested in your report on interference between Cherenkov and Synchrotron radiation, and your comments on the interference between Cherenkov and transition radiation. Enclosed are some short papers on this topic, including one describing a measurement done on the 29 MeV microtron at University College. Maybe of course you are aware of this, but after 20 years or so some things get buried!

Yours sincerely

John Lawson

J D Lawson

Enc

Princeton University

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November 14, 1986

Dr. J.D. Lawson
Rutherford Appleton Laboratory
Chilton, Didcot, Oxon OX11 0QX
England

Dear John,

Thank you for your letter regarding our recent paper. From your work and that of others it is clear that the intimate relation between Čerenkov and transition radiation is well understood by knowledgeable people. This latter category probably does not include ourselves–the logarithmic singularity at Čerenkov threshold referred to in our paper will not exist for a finite-length detector.

We had considered the merits of performing a small experiment exploring the angular distribution of radiation very close to Čerenkov threshold, but the excellent work of Aitken *et al.* (which we were not aware of) and Wartski *et al.*, J. Appl. Phys. **46**, 3644 (1975), render this somewhat redundant. Still, I enjoy working with these phenomena, and am presently comtemplating the pursuit of a suggestion of Ginzburg (Sov. Phys. Uspekhi **15**, 184 (1972)) that a Čerenkov effect can arise when the writing speed of an electron beam in an oscilloscope or streak camera exceeds that of light. This occurs due to the coherent superposition of the transition radiation from the individual electrons, and would provide another illustration of the points you have made in your several papers.

Sincerely yours,

Kirk McDonald
Professor of Physics

ILLINOIS INSTITUTE OF TECHNOLOGY
Lewis College of Sciences and Letters
Department of Physics

December 12, 1986

Professor Kirk McDonald
Department of Physics - Jadwin Hall
Princeton University
P. O. Box 708
Princeton, New Jersey 08544

Dear Kirk:

I've looked over again your remarks on 'superluminal' sources, and still think it is an intriguing idea. Possibly some further thought should be devoted to the physical nature of the interface. If it is a 'perfect' conductor, it seems that the effective source would be a dipole (see R. L. Liboff and N. J. Maresca, Z. Naturforsch. 29a 1418-1424, 1974). With a dielectric, it might be possible to simulate the motion of an electric monopole.

You might find it interesting to reread section 3.2 of "Unified": the set-up you're contemplating should be amenable to displaying 'harmonic-Čerenkov' radiation. Formula (3.32b) for the angular distribution, as well as the total power (3.33), can all be extended to the range $n\beta > 1$ (this is your case) by following the procedures outlined on the top of p.60. The nature of the line spectrum is discussed on pp.60-61. The suppression of Čerenkov radiation from sources with form factors is mentioned on p.37 and discussed in detail in reference [40].

Mitter was our house guest during the following weekend, and we had several long conversations concerning your projected laser experiments.

As you can see from the enclosures, Batterman did reply, and I thought a further letter might be helpful.

An offer from UCLA was waiting in the mailbox after the eastern trip. The whole matter was settled on a time scale of 17 minutes: IIT has approved a leave, so you can reach me at UCLA beginning 5 January 1987. I plan to stay in Los Angeles until the end of August.

Last and least, enclosed are photocopies of travel expenses:

Air Fare	$424.07
Car Rental	198.43
Hotel	490.53
	1113.03

IIT Center Chicago, Illinois 60616 (312) 567-3375

Let's forget the rest, and split the expenses three ways between Princeton, Bell Labs, and "other sources".

Happy Holidays!

Tom

T. Erber

TE:tlb

Enc.

P. S. The Aitken et al. article was very interesting.

CHESS

Cornell High Energy Synchrotron Source

CORNELL UNIVERSITY

ITHACA, N. Y. 14853-8001

Address reply to Wilson Laboratory

Telex # 937478
Tel. 607-255-7163

November 12, 1986

Professor Thomas Erber
Department of Physics
University of California, Los Angeles
Los Angeles, CA 90024

Dear Professor Erber:

Thank you very much for your letter of September 25 discussing the connection between Cerenkov radiation and synchrotron radiation. I looked through the pre-print you sent and discussed this with some of our staff and accelerator personnel at Wilson Laboratory.

The physics of the interference between the two is very interesting to me and I am sure will have a wide range of interest in the scientific community. However, it is not clear to me how I can be of any help. For one, we do not have any raw beam available to us. The only beam that was ever taken out of the machine was from the synchrotron for test purposes and that is only a 250 Mev electron beam operated at 1 Hz. This would put us in a position inferior to what you have available at the Bates linear accelerator.

If I have missed an important point in my analysis of the situation, I would appreciate hearing from you.

Very truly yours

B. W. Batterman, Director
Walter S. Carpenter, Jr.
Professor of Engineering

BWB/pe

Princeton University

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Internet: http://puhep1.princeton.edu/∼mcdonald/

1987

W. Lobar
Bates Linear Accelerator Center
P.O. Box 846
Middleton, MA 01949

Dear Bill,

Enclosed is a writeup for the 1986 Bates Annual Report describing our small experiment. The laser printer could easily reformat the article if desired.

The results of the experiment were published in Phys. Rev. Lett. **57**, 2264 (1986).

This experiemnt is complete. However, we would like to perform a followup experiment, on a similarly modest scale. The proposal for this has not been submitted to Bates yet.

I have given seminars on this work at

National Bureau of Standards, Sept. 11, 1986;

U. of Rochester, Nov. 10, 1986;

Princeton U., Dec. 10, 1986;

U. of Chicago, Jan. 26, 1987.

I contributed an abstract, and gave a talk to the 1987 Meeting of the APS Division of Particles and Field, Salt Lake City, Jan. 14-17, 1987.

Would it be possible for you to send me a copy of the 1986 Annual Report when it becomes available, and also the 1985 Report?

Sincerely yours,

Kirk McDonald

Princeton University

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1987

Dr. Jay Flanz
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Dear Jay,

Enclosed is a copy of the writeup I sent to Bill Lobar for the Annual Report. At the end it hints towards a followup experiment, a sketch of which I will try to commit to paper in the 'near futrure.'

Best wishes,

Kirk McDonald