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CERN/SPSC/74-68/I 64
May 27, 1974

Professor Pierre Lehmann
Chairman of the SPSC
CERN
1211 Geneva 23, Switzerland

Dear Professor Lehmann:

I am writing this as a Letter of Intent for an SPS experiment on Very Large Angle pp Elastic Scattering.

We originally proposed this experiment at NAL and it was approved for several years. During this time, NAL built the Proton West Lab to our specifications and we assembled 4 magnets and designed the electronic detection system. I am enclosing a copy of the NAL proposal and the correspondence with NAL staff members concerning this proposal. In 1973 the approval was withdrawn for various reasons. We have loaned the 7 meter septum (R_1) to NAL and are using the L_1 septum in our present ZGS experiment.

We would like the SPS Committee to consider the possibility of our doing this experiment at the SPS. The SPS proposal would be quite similar to the enclosed proposal.

The experiment might be done in the proton beam stub planned for the North area. We could provide the two septum magnets (L_1 and R_1) and possibly the two steering magnets (L_2 and R_2) which are now at Argonne. Hopefully CERN could provide the momentum analyzing magnets L_3 (a standard 2m PS magnet) and R_3 (several 6 to 8 meter SPS magnets). We could bring the electronic detection system in our trailer to CERN along with the magnets.

The experiment would be a collaboration between our group and one or more European groups. Our group consists of L.G. Ratner, J.B. Roberts, K.M. Terwilliger and about 5 postdoctoral fellows and students, with me serving as spokeman. Before submitting a formal proposal, we would confirm arrangements for the collaboration.

We are now working at the ZGS on polarized beam-polarized target experiments. However, we could finish these by 1978 to coincide with the opening of the north area.

During a recent visit to CERN, I discussed this experiment with several people including Drs. Allaby and Foa. I expect to visit CERN again in June or July for further discussions. If the committee has any questions, please write to me.

Sincerely yours,

A.D. Krisch
Professor of Physics

200 GeV PROTON PROTON ELASTIC SCATTERING AT HIGH TRANSVERSE MOMENTUM

This is a proposal to study p-p elastic scattering at the highest possible $P_\perp^2$ at NAL, using a CH_2 or H_2 target placed directly in the extracted beam and a double arm spectrometer. We expect to be able to set an upper limit at the level

$$\frac{d\sigma/dt}{d\sigma/dt)_{t=0}} \approx 10^{-14}$$

This would be sufficient to determine if there are exactly three regions in the p-p interaction with considerable precision.

L. G. Ratner
Argonne National Laboratory

A. D. Krisch, J. B. Roberts, K. M. Terwilliger
University of Michigan

June 5, 1970

Correspondent: A. D. Krisch, Randall Laboratory of Physics
The University of Michigan, Ann Arbor,
Michigan 48104

II. Physics Justification:

This experiment would measure the proton proton elastic scattering cross section at the highest possible $P_\perp^2$. Previous experiments at CERN[1], BNL[2], ANL[3], and LRL[4] have measured out to 90° at the highest available energies. It is generally true that these four accelerators have been used more or less to their limits for this measurement. Similar experiments are not presently possible at Serpukhov because of the lack of a slow extracted beam and of long straight sections in the ring itself and they are not possible at the CERN ISR because the interaction rate is down by at least 10^6 relative to NAL.

There is at present no fundamental theory which has been successful in explaining the dependence of the proton proton elastic scattering cross section on momentum and angle. Perhaps this is because the measurements have been made with such small errors over a cross section range of 10^{-11} or 10^{-12}. Thus these measurements may well be one of the most stringent tests of any theory of strong interactions.

There have instead been many parameterizations and phenomenological fits to the data. One such fit proposed in 1967 consists of plotting the differential cross section $\frac{d\sigma^\dagger}{dt}$ [5], against the quantity $\beta^2P_\perp^2$ where β is the c.m. velocity. This variable is suggested by an optical model with an interaction region which is a Lorentz contracted sphere. The † in $\frac{d\sigma^\dagger}{dt}$ indicates that some attempt[5] was made to consider the effects of particle identity in proton proton scattering near 90°. This plot is shown in Fig. 1 which contains all data above 3 GeV available up to 1970.

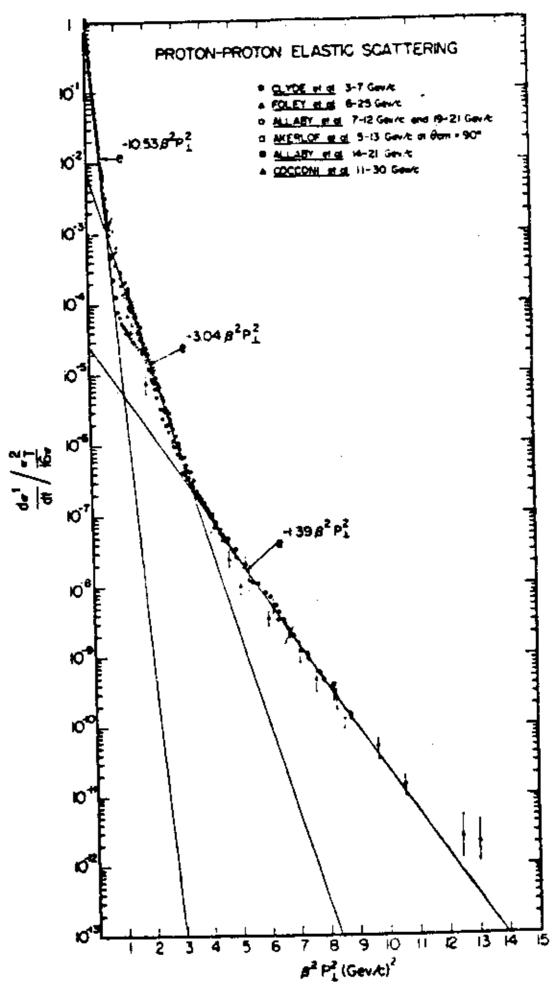
The most dominant feature of the cross section is the existence of three remarkably separate regions. In the 1st and 3rd regions all energy dependence or "shrinkage" appears to be removed so far, but in the 2nd region there is still some sort of energy dependence, which is not understood. These three regions have been interpreted as evidence for:

a. Three spatial regions in the p-p interaction of radii .9f, .5f and .33f.

b. Single, double, and triple scattering as in the Glauber model of proton-deuteron scattering.

c. The opening of new production channels; specifically: region 1 - pion production; region 2 - strange particle production; region 3 - baryon antibaryon pair production.

The advocates of the multiple scattering model point out that there should also be quadruple scattering and thus a fourth region and point to the last two BNL points which lie well above the line. Unfortunately these points have such large errors that they don't settle this question. Other theorists especially Cerelus, Martin and Kinoshita[6] have pointed out that if the cross section continues to drop as fast as



$$\frac{d\sigma}{dt} \sim e^{-1.48^2 P_\perp^2} \qquad (1)$$

then for fixed angle this is essentially an e^{-s} dependence which raises some problems concerning the analyticity of the scattering amplitude. If however there were a 4th region and then a 5th region and so on, then there would be no problem.

However the physics justification for this experiment is independent of any particular model or fit. It is clearly important to study the behavior of strong interactions at the highest $P_\perp^2$ possible. A violent probe such as this must give insight into the structure of strong interactions.

III. Experimental Arrangement

We propose to measure the cross section by placing a CH_2 or liquid H_2 target directly in the extracted beam. The two scattered protons will each be detected by one arm of a double arm spectrometer.

The cross section $\frac{d\sigma}{d\Omega}$ is determined from the equation

$$\text{Events} = I_0 N_T \frac{d\sigma}{d\Omega} \Delta\Omega \qquad (2)$$

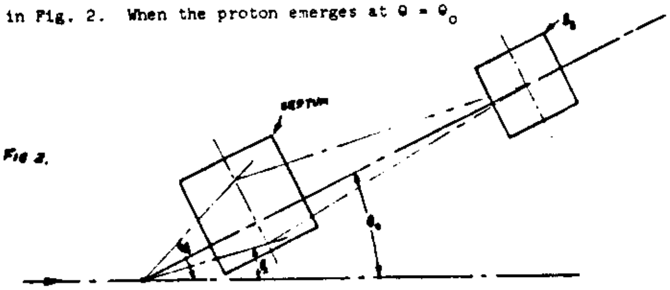
where N_T is the number of target particles/cm^2. The quantity I_0 is the incident beam intensity which can be determined by a radiochemical analysis of the CH_2 target looking for the spallation reaction

$$p + C^{12} \rightarrow Be^{7*} \rightarrow .48 \text{ MeV } \gamma\text{-ray} \qquad (3)$$

The Be^{7*} nucleus decays with a 77.5 day mean life which is very convenient for counting and rechecking.

The number of events will be determined by the coincidences between the two arms of the double arm spectrometer. Each spectrometer consists of magnets for angle and momentum analysis and scintillation counters to detect the protons and define the solid angle $\Delta\Omega$. An important part of each spectrometer is the septum magnet placed near the target. This acts as a steering magnet and allows protons scattered at various angles to be steered into the spectrometer without physically moving any magnets or counters. The basic concept is shown in Fig. 2. When the proton emerges at $\theta = \theta_0$

Fig 2.



then the septum and B_2 are turned off and the proton goes right down the center of the spectrometer. If however $\theta_1 > \theta_0$ then the septum is set to bend inward and B_2 is set so that it steers the proton along the central axis of the spectrometer. Similarly if $\theta_2 < \theta_0$ then the polarities of the septum and B_2 are reversed so that the proton is bent outward and into

the spectrometer. This technique which has been used on several experiments[3,7] allows protons scattered over a wide range of angles to be detected with a fixed spectrometer by merely varying the magnet currents.

After emerging from the B_2 magnet in a narrow cone the protons in each spectrometer are then bent vertically up as shown in Fig. 3. This provides the momentum analysis and also gets the protons up out of the tunnel and to ground level where they can be detected by counters with low singles rates. As shown in Fig. 3 all magnets can be contained in a normal main ring section of the EPB tunnel except for the magnets on the large angle side which we propose to place in an additional side section of main ring tunnel ~40 feet long and coming out at an angle of 450 milliradians. We would also require two pipes tunneling up 17 feet from beam height to ground level (one of 2 foot diameter and 130 feet long at an angle of 130 millirad and the other of 1 foot diameter and 700 feet long at an angle of 25 millirad). We would also require the main ring tunnel section of the EPB to be long enough downstream of our target to accommodate our high momentum septum magnets (~100 feet). These modifications of the main EPB tunnel are not free but we believe not excessively expensive since they utilize the main ring tunnel modules.

The CH_2 or H_2 target will be placed downstream of the EPB magnets in a tunnel section of the EPB. Thus all the radiation will go forward into the dirt shielding surrounding

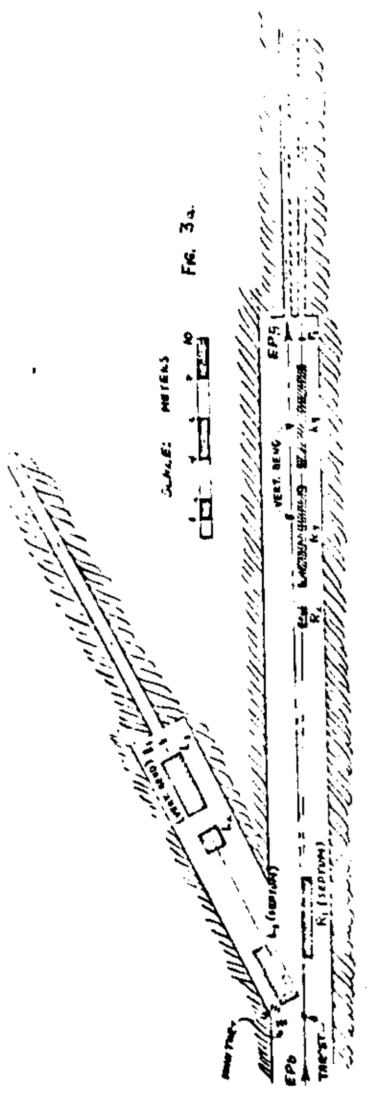


Fig. 3a

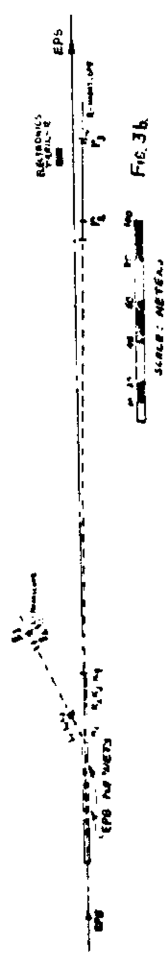


Fig. 3b

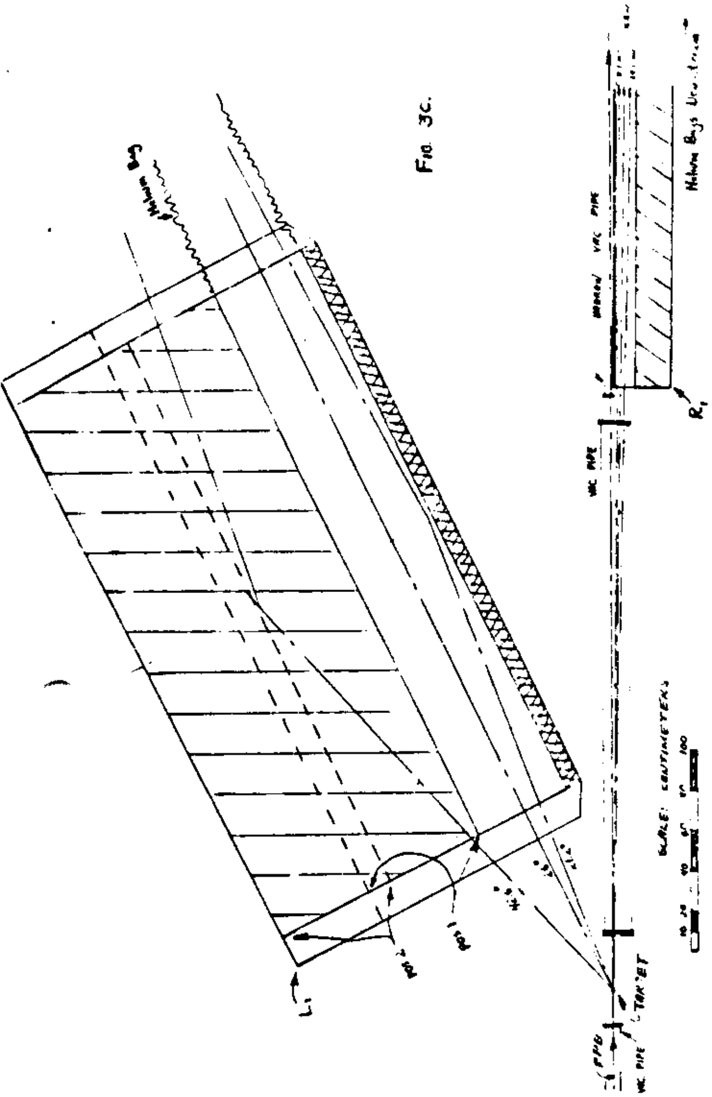


Fig. 3C.

a pipe section of the EPB, and will not cause radiation damage to any active elements of the EPB tunnel. We would prefer a 1 or 2 cm long CH_2 target (1 cm x 1 cm cross section). This gives reliable monitoring via the Be^{7*} reaction and the high radiation problems are easier to handle than with liquid H_2 which might boil excessively causing a change in the density. The main problem with CH_2 is it scatters the beam more and could cause some problems downstream in controlling the beam. We think that with a 2 cm target which has 4% of a collision length and radiation length these problems are not excessive. If they are judged excessive we would then use a 4 cm H_2 target (1% collision length and ½% radiation length) but it would then be much more important to have the beam defocused as much as possible at the point where it hits our target. If we use CH_2 targets we would have a remotely controlled wheel with perhaps 30 CH_2 targets on it so that no target would receive sufficient radiation to lose more than a few percent of its hydrogen.

We plan to cover the $P_\perp^2$ range from approximately $P_\perp^2 = 4 \rightarrow 20$ $(GeV/C)^2$. It is necessary to have magnets of sufficient bending power to steer and momentum analyze the protons at both extremes of this range. A kinematics table is shown for 200 GeV/C proton proton elastic scattering in Table 1. As shown in Fig. 3 the two central angles for the two spectrometers were chosen to be $\theta_o = 26^\circ$ and $\theta_o = .96^\circ$. We can then

KINEMATICS FOR P-P ELASTIC COLLISION AT 200 BEV/C
S= 377.?67
BETA= 0.9953
GAMMA= 10.3497

TABLE 1

$P_\perp^2 (GeV/c)^2$ (PT) 2	θ_{cm} THETA C-M	θ^R (degrees) THETA LAB	P^R (GeV/c) P LAB	BETA LAB	J^R J*	THETA C-M	θ^L (degrees) THETA LAB	P^L (GeV/c) P LAB	BETA LAB	J^L J*
0.021	0.00	0.000	0.000	0.0000	0.000	180.00	0.000	0.000	0.0000	[illegible]
0.028	1.00	0.248	199.985	[illegible]	429.263	181.00	84.641	0.169	0.1776	[illegible]
0.114	2.00	0.097	199.940	[illegible]	429.070	182.00	79.761	0.343	0.3431	[illegible]
0.256	3.00	0.145	199.864	[illegible]	427.745	183.00	74.828	0.524	0.4876	[illegible]
0.454	4.00	0.193	199.758	[illegible]	427.293	184.00	71.132	0.717	[illegible]	[illegible]
0.729	5.00	0.242	199.621	[illegible]	426.711	185.00	65.686	0.924	[illegible]	[illegible]
1.02?	6.00	0.291	199.455	[illegible]	426.0?1	186.00	61.528	1.149	0.7746	[illegible]
1.387	7.00	0.339	199.258	[illegible]	425.163	187.00	57.57?	1.394	0.8295	[illegible]
1.5??	8.00	0.387	199.032	[illegible]	424.199	188.00	54.11?	1.660	0.87??	[illegible]
2.285	9.00	0.436	198.775	[illegible]	423.1?7	189.00	50.84?	1.95?	0.9?11	[illegible]
2.815	10.00	0.484	198.488	[illegible]	421.891	190.00	47.944	2.264	0.9233	[illegible]
3.40?	11.00	0.533	198.172	[illegible]	421.552	191.00	45.173	2.?03	0.94??	[illegible]
4.237	12.00	0.582	197.825	[illegible]	419.?83	192.00	42.59?	2.?69	0.953?	[illegible]
4.726	13.00	0.631	197.449	[illegible]	417.497	193.00	4?.304	3.361	0.9632	[illegible]
5.465	14.00	0.680	197.044	[illegible]	415.797	194.00	3?.2?4	3.780	0.9725	[illegible]
6.256	15.00	0.729	196.6?9	[illegible]	413.959	195.00	36.28?	4.227	0.9762	[illegible]
7.095	16.00	0.778	196.144	[illegible]	412.0?8	196.00	34.513	4.701	0.9?37	[illegible]
7.983	17.00	0.827	195.651	[illegible]	409.945	197.00	32.?97	5.243	0.9?41	[illegible]
8.918	18.00	0.877	195.129	[illegible]	407.764	198.00	31.369	5.733	0.9?69	[illegible]
9.898	19.00	0.926	194.578	[illegible]	405.469	199.00	3?.?39	6.291	0.9?91	[illegible]
10.924	20.00	0.976	193.998	[illegible]	403.06?	200.00	28.725	6.877	0.9?08	[illegible]
11.993	21.00	1.026	193.389	[illegible]	400.544	201.00	27.538	7.491	0.9922	[illegible]
13.175	22.00	1.076	192.753	[illegible]	397.917	202.00	26.434	8.132	0.9934	[illegible]
14.257	23.00	1.126	192.088	[illegible]	395.184	203.00	25.497	[illegible]	0.9944	[illegible]
15.449	24.00	1.177	191.395	[illegible]	392.345	204.00	24.448	9.497	0.9952	[illegible]
16.679	25.00	1.227	190.675	[illegible]	389.4?4	205.00	23.552	10.221	0.9958	[illegible]
17.946	26.00	1.278	189.927	[illegible]	386.363	206.00	22.713	10.971	0.9964	[illegible]
19.248	27.00	1.329	189.152	[illegible]	383.224	207.00	21.926	11.749	0.9968	[illegible]
20.593	28.00	1.3??	188.3??	[illegible]	379.??8	208.00	21.186	12.?54	0.9972	[illegible]
21.9??	29.00	1.432	187.521	[illegible]	376.6??	209.00	20.499	13.?35	0.99?6	[illegible]
23.347	30.00	1.483	186.665	[illegible]	373.239	210.00	19.832	14.242	0.9978	[illegible]

calculate the necessary field integrals in the two septums for the two extreme cases.

$P_\perp^2$ $(GeV/C)^2$	P_{Lab} (GeV/C)	θ_{Lab} (degrees)	$\theta-\theta_o$ (degrees)	$P(\theta-\theta_o)$ GeV/C degrees	1.33 $P(\theta-\theta_o)$ GeV/C degrees	$\int B.d\ell$ KG-meters
20	12.55	21.2	-4.8	60.2	80	47
	188.4	1.38	.42	79.1	105	62
4	2.97	42.6	16.6	49.3	66	39
	198.2	.58	-.38	75.3	100	59

The factor 1.33 comes from the fact that the distance from the second magnet to the septum is 3 times the distance from the target to the septum. Thus we see that with two 16 kilogauss septum magnets of 3 meters (L_1) and 4 meters (R_1) we can steer all protons into our spectrometer for this entire $P_\perp^2$ range. The L_2 and R_2 magnets need only be 1 meter long since they only bend by 1/4 the angle of L_1 and R_1.

The L_3 and R_3-R_4 magnets then bend the particles vertically for momentum analysis. These must have enough $\int B.d\ell$ to handle the maximum momentum on each side.

Magnet	P_{Max}	Vertical Bend (ϕ)	$P_{Max}\,\phi$	$\int B.d\ell$
	GeV/C	degrees	GeV/C-degrees	KG-meters
R_3-R_4	198.2	1.43	284	166
L_3	12.55	7.5	94	55

Thus we require R_3-R_4 to each be a 5-meter magnet of 17 kilogauss and L_3 to be a 3-meter magnet of 18.5 kilogauss. All magnets will be described in more detail in Sect. IV.

We next discuss the question of resolution in Θ and P. We will define the solid angle ($\Delta\Omega$) on the low momentum side (L) since the Jacobian is so much larger on this side. The high momentum side (R) will then be overmatched to accept a larger solid angle. The defining ℓ_3-counter will be about 2 ft. x 2 ft. at 200 feet from the target so that $\Delta\Omega^L_{Lab}$ will be 10^{-4} steradians. On the other (R) side the final counter r_3 will be about 10 inches x 10 inches at 1000 feet from the target for an overmatched $\Delta\Omega_{Lab} = 7\ 10^{-7}$ steradians. The matched $\Delta\Omega_{Lab}$ varies between $(\Delta\Omega^R_{Lab})_{matched} = \frac{J_L}{J_R}\Delta\Omega^L_{Lab} = .32\ 10^{-7} \rightarrow 4.5\ 10^{-7}$ steradians.

We will probably use 10 x 10 hodoscopes of scintillation counters on each side to improve the resolution. This would give:

$$\Delta\Theta^L_{Lab} \approx 1 \text{ mr} \qquad \Delta\Theta^R_{Lab} \approx .07 \text{ mr} \qquad (4)$$

$$\Delta P/P)^L \approx \pm .4\% \qquad \Delta P/P)^R \approx \pm .15\% \qquad (5)$$

We feel that this resolution would be sufficient to discriminate against inelastic events and events from carbon in the CH_2 target. This can be tested by taking runs with a carbon target replacing the CH_2 target. In a similar experiment at ANL[3] an upper limit of 0.1% was set on events of this type.

We next calculate the estimated counting rate at various values of $P_\perp^2$. We estimate the value of the cross section $X \equiv \frac{d\sigma/dt}{d\sigma/dt)_{\Theta=0}}$ from Fig. 1. We assume an intensity of:

$$I_o = 1\ 10^{13} \text{ protons/sec} = 3.6\ 10^{16} \text{ protons/hour} \qquad (6)$$

The center of mass solid angle is given by:

$$\Delta\Omega_{cm} = J^L \Delta\Omega^L_{Lab} = 10^{-4}\ J^L \qquad (7)$$

The number of target particles/cm^2 is given by:

$$N_T = N_o \rho\ t \qquad (8)$$

where N_o (Avogadro's Number) is $6.02\ 10^{23}$, ρ is the density of hydrogen protons in CH_2 = .13 and t is the target length which we take as 2 cm. Then we get

$$N_T = (6.02\ 10^{23})(.13)(2) = 1.6\ 10^{23}\ \frac{\text{protons}}{\text{cm}^2} \qquad (9)$$

Similarly if we note that $d\sigma/dt)_{\Theta=0} \approx 10^{-25}$ then we get that

$$\frac{d\sigma}{d\Omega})_{cm} = \frac{P^2}{\pi}\ \frac{d\sigma}{dt} = \frac{P^2}{\pi}\ \frac{d\sigma}{dt})_{\Theta=0}\ X \qquad (10)$$

$$= \frac{100}{\pi}\ 10^{-25}\ X$$

$$= 3\ 10^{-24}\ X$$

These numbers all go into the equation for the number of events/hour.

$$\text{Events/hour} = I_o\ N_T\ \frac{d\sigma}{d\Omega})_{cm}\ \Delta\Omega_{cm}$$

$$= (3.6\ 10^{16})(1.6\ 10^{23})(3\ 10^{-24} X)(10^{-4}\ J^L) \qquad (11)$$

$$= 2\ 10^{12}\ J^L X$$

For various values of $P_\perp^2$ we tabulate J^L and X and then the counting rate is:

$P_\perp^2$	J^L	X	Events/hour	Events/day
4	.13	10^{-7}	$2\ 10^4$	$5\ 10^5$
10	.5	$2\ 10^{-11}$	20	500
12	.8	$3\ 10^{-12}$	5	125
15	1.0	$10^{-12} \rightarrow 2\ 10^{-14}$	$2 \rightarrow .04$	$50 \rightarrow 1$
20	1.8	$2\ 10^{-13} \rightarrow 10^{-16}$	$.4 \rightarrow 2\ 10^{-4}$	$10 \rightarrow .005$

Clearly our maximum $P_\perp^2$ depends on whether or not the cross section breaks again. However we can set a limit on the minimum measurable cross section. If we call the minimum upper limit a rate of one event per day then we get a level of approximately 10^{-14} below the forward cross section.

In the range from $P_\perp^2 = 4 \rightarrow 20$ (GeV/C)^2 we would make approximately 30 measurements with spacing and statistics that increase with increasing $P_\perp^2$. We would average about two days of running at each point for a total of 2 months of data running at $I_o = 10^{13}$/sec. Obviously most of the points in the range $P_\perp^2 = 4 \rightarrow 10$ could be run with considerably less intensity and a thinner target. Clearly this experiment can run simultaneously with the main target station downstream since it only depletes the beam by a few % and runs at 200 GeV/c.[8]

We will be ready to start taking data in the Fall of 1972.

We expect several more young scientists at the student and postdoc level to join this experiment around Fall of 1971.

IV. APPARATUS:

In this experiment there are four types of equipment that will be required:

1. Detection counters and electronics.
2. Magnets, power supplies, and vacuum pipes.
3. Targets.
4. Changes in the EPB tunnel.

We will discuss them separately.

1. Detection equipment:

We expect to provide essentially all detectors and electronics equipment. A major fraction of this equipment will be used on an experiment at the CERN ISR starting July 1971. We expect that experiment to have finished by Spring 1972, and will return the equipment to Chicago well before Fall of 1972.

In the unlikely event that the ISR schedule is substantially delayed, we would duplicate all the specialized items and possibly borrow standard scalers and logic from PREP or SHELF at ANL.

The detection equipment is quite simple, consisting only of scintillation counters and logic circuitry. The hodoscopes will probably not require a computer.

2. Magnets etc.:

We require a total of seven magnets and seven power supplies which are listed in table 2. Four of these magnets, L_2, L_3, R_3, and R_4 can probably be standard NAL beam magnets. We could certainly change our parameters a little to conform to the NAL standards when they become firm.

The other three magnets, L_1, R_1 and R_2 are all septums. We think that they would be useful for later experiments and we would hope that NAL would pay for them. We are again prepared to modify them somewhat if that would make them more generally useful. We are also prepared to contribute to the design of these magnets, if that is agreeable to NAL. We roughly estimate the total cost of L_1, R_1 and R_2 at \$100,000. The R_1 magnet might become too radioactive to be useful for future experiments.

If NAL does not consider such septums useful we could request additional funding from the AEC to build them ourselves. However, we are not very enthusiastic about this approach.

We think the power supplies are fairly standard and could be provided by NAL.

The modifications to the EPB vacuum pipe and the helium bags for the length of the two spectrometers would hopefully be provided by NAL.

Table 2 Magnets

MAGNET	GAP height cm	GAP width cm	GAP length	OVERALL height	OVERALL width	OVERALL length	MAx B_0 Kilogauss	COMMENTS
L_1	5	50	3	1.10	1.15	3.3	16	8 cm septum
L_2	≥ 12	≥ 20	1	Any Standard Magnet			12	
L_3	≥ 16	≥ 30	3	.8	1	3.75	18.5	Possibly standard magnet
R_1	2	10	4	.45	.30	4.3	16	1.5 cm septum
R_2	5	5	1	.2	.2	1.3	16	5 cm septum
R_3-R_4	≥ 5	≥ 10	5	.3	.6	5.4	17	Probably standard magnets

MAGNET	EST. WEIGHT	EST. COST	EST. MAX. CURRENT	EST. MAX. VOLTAGE VOLTS	EST. POWER KILOWATTS
L_1	55	\$55,000	900	400	360
L_2	-	-	-	-	< 100
L_3	30	(\$50,000) possibly std.	1000	360	360
R_1	6	\$30,000	800	175	140
R_2	~ 1	\$15,000	-	-	< 100
R_3-R_4	9	(\$35,000) prob. std.	1000	425	425 each

3. Targets.

In the event that we use CH_2 targets we would provide these targets and the target changing mechanism. These targets would be quite radioactive so we would expect to work in close communication with the NAL radiation safety group.

The radiochemical analysis would probably be done by the radiochemistry group at Argonne. We have worked closely with this group in the past.

In the event that CH_2 is rejected and we use a liquid hydrogen target, then we would certainly need a helium refrigerated target. This would keep the temperature around 17° or $18^\circ K$ and minimize boiling. This target could be built either at NAL or possibly by the ANL liquid hydrogen group which presently has several helium refrigerator units.

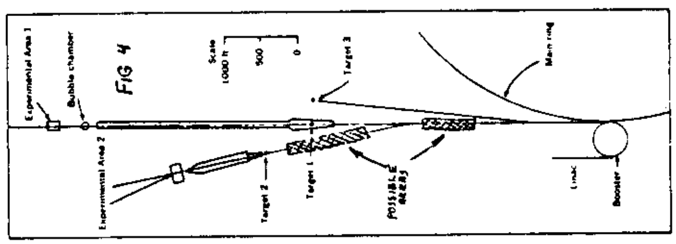
For the reasons mentioned in Sect. III we strongly prefer the CH_2 target.

4. Changes in the EPB Tunnel

As we mentioned in Sect. III we require some modifications to the EPB tunnel. We cannot list the exact modifications required since we do not have final plans for the EPB tunnel. We would work closely with NAL to find the area where our experiment could be installed with the minimum difficulty.

The experiment would probably fit best into one of the two general areas shown on Fig. 4.

82 • JUNE 1970 • PHYSICS TODAY



The exact position would depend on avoiding interference with roads, buildings, and other obstacles, and the planned positions of the beam magnets along the EPB. The dimensions of our experiment are shown in Figs. 3a and 3b. They could of course be modified somewhat and the high momentum proton could come out on the left instead of the right.

In general the target should probably be placed immediately downstream of a set of the EPB quadrupoles as shown in Fig. 3b. We believe that the present EPB plan is to have some main ring modules (10 foot diameter) for these EPB magnets separated by beam pipes (~ 1 feet diameter) about 500 feet long. By placing our target immediately downstream of these quadrupoles we could utilize the ~ 500 feet of earth shielding to protect the downstream EPB magnets from radiation produced in our target.

As mentioned in Section III, we would require an additional

~100 feet of main ring modules beyond the end of the EPB magnets and about 40 more feet of modules coming out at an angle of 26° as shown in Fig. 3. Thus we would require an additional 140 feet of main ring modules. One way to estimate the cost of this is to note that the main ring of circumference 20614 feet was estimated (1968 Design Report, 16-6) to cost ~$16.6 million. This would give:

$$\text{Cost} = \frac{140}{20614} \times \$16.6 \text{ Million} \approx \$113,000$$

The true cost might well be higher than this and would have to be estimated by NAL.

As seen from Table 2 our maximum DC power use would be about 1.9 megawatts. All of this would occur in the 140 foot tunnel section since there are no magnets outside of this area. The cost of this power will have to be estimated by NAL.

Our only other requirements are:

a. Small patches of blacktop on which to place our scintillators and electronics trailer.

b. Perhaps 10 KW of AC to our trailer which is fitted with a 440/110 transformer.

c. Tents to cover our scintillators which we might provide, if necessary.

REFERENCES

1. J. V. Allaby et al, Phys. Letters 23, 389, (1966); 25B, 156, (1967); 27B, 49 (1968).
2. G. Cocconi et al, Phys. Rev. 138, B165 (1965).
3. C. W. Akerlof et al, Phys. Rev. 159, 1138 (1967).
4. C. M. Ankenbrandt et al, Phys. Rev. 170, 1223 (1968).
5. A. D. Krisch, Phys. Rev. Letters 19, 1149 (1967). It has been assumed that $\frac{d\sigma}{dt}(90°) = 2\frac{d\sigma^{\dagger}}{dt}(90°)$ in the third region and that $\frac{d\sigma}{dt} = \frac{d\sigma^{\dagger}}{dt}$ in the first and second region.
6. F. Cerulus and A. Martin, Phys. Letters 8, 80 (1964); T. Kinoshita, Phys. Rev. Letters 12, 257.
7. J. L. Day et al, Phys. Rev. Letters 23, 1055 (1969); L. G. Ratner et al, Phys. Rev. 166, 1353 (1968).
8. We could also make measurements at lower momenta if the accelerator happened to be running at lower momentum.

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

September 9, 1970

Ref: 6

Dr. A. D. Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan

Dear Al:

We have reviewed your proposal, "200 GeV Proton Elastic Scattering at High Transverse Momentum" (our No. 6). I have concluded that the experiment should be performed, but that we need to plan a suitable location for it and for other experiments requiring similar bombardments. It is our feeling at the Laboratory that we might develop a transmission-target area as a part of Experimental Area 3. This would be an appropriate facility in which to perform your experiment. I suggest that you set up a meeting with Jim Sanford to discuss these questions.

Sincerely,

Robert Rathbun Wilson

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

December 18, 1970

Dr. A. D. Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Dear Alan:

I am writing with regard to your proposed experiment for the study of large angle p-p scattering (our Proposal #6). This proposal, along with several others which could use the Proton Laboratory that we intend to construct, was discussed at the recent Program Advisory Committee meeting. It was felt that the technique and apparatus proposed was excessively lengthy and complicated for the experiment that you are proposing. In particular the spectrometer arms are very long, and the requirements of magnet aperture and length made this a difficult experiment to envision for early running at the accelerator. Would you consider doing this experiment with higher resolution detectors coupled with a more modest magnet configuration near the target? I suspect that you have considered this question, and it would be well for us to understand the reasons behind your proposed choice of design.

I am deferring action on your proposal until we have resolved this question to our mutual satisfaction. Please feel free to get in touch with the Laboratory staff or with me at any time.

Sincerely,

R. R. Wilson

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

March 8, 1971

Professor Alan Krisch
Department of Physics
University of Michigan
Ann Arbor, Michigan 48104

Dear Alan:

Your proposal (6) for a measurement of p-p elastic scattering cross sections at large momentum transfers was discussed at the recent meeting of our Program Advisory Committee. On the basis of that discussion, I have decided to approve your experiment.

This approval pertains to the scientific interest of the experiment and to its technical feasibility. The next step toward the scheduling of the experiment is for us to examine its requirements for money, time and people. In order to take this next step, you must prepare a draft of the agreement that you have already been discussing with Jim Sanford.

A further reservation that I have in the particular case of your experiment is one that pertains to the particular technique that you have chosen to propose. I am still concerned about the large physical size of your apparatus and layout and about the rather high cost that appears to be associated with the experiment. If a new proposal should be submitted to carry out the experiment in a more economical way, I would welcome it and would expect to review the whole situation at that time and perhaps reverse this decision and substitute the new experiment if substantially better than yours. In other words, I consider your experiment a sitting duck for some young hero to shoot at. Perhaps you will take a shot at it yourself.

Sincerely,

R. R. Wilson

cc: K.Terwilliger

22 March 1971

Professor R.R. WILSON
Director
National Accelerator Laboratory
P.O. Box 500
BATAVIA
Illinois 60510

Dear Bob :

QUACK !

Sincerely yours,

A.D. Krisch

cc: K.M. Terwilliger

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

March 9, 1972

Professor A. D. Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Dear Alan:

By this time you have received R. Wilson's February 8, 1972 letter on the progress with the accelerator and the initial research plans. Since then we have achieved 200 GeV, and are now installing extraction equipment and preparing to connect the water cooling to the magnets. While this work is going on I am making the detailed arrangements for the initial experiments. Although your group's experiment is not among the first experiments, I want to convey to you what information I can about our experimental plans. I hope to be able to work out an Agreement for your experiment in a few months, but I understand that you need as much information as I can provide in the meantime. I believe that the following points are important to you:

1. You are the fourth major experiment in line for setup in the PW beam in the Proton Laboratory. Maybe we can install your beam equipment earlier, but that depends upon how feasible and practical it is to fit among the other experiments.

2. I cannot predict the running schedule at this time. I will have more to say about that after the first experiment (#63) has started in the beam line. I would not expect that you would be taking data before fall of 1973.

3. We have built the experimental pits according to the plans we had for your setup. When you come here next you can see what the Proton Laboratory area looks like.

I will expect to confirm these plans after the March 31st meeting of the Program Advisory Committee. We can then draw up a draft Agreement for discussion.

Sincerely,

James R. Sanford

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

July 11, 1972

Ref. #6

Professor Alan Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Dear Alan:

As some of us have suspected for some time, a new experimental group has stepped into the p-p scattering, large momentum transfer area and has done so with a proposed technique which differs significantly from the one that you have traditionally used and that you have proposed for Experiment #6. I am sure that this is no great surprise to you, nor will it be a surprise that the new group is led by Jay Orear. A copy of his recently submitted proposal is enclosed herewith for your information.

The situation was discussed at the recent meeting of our Program Advisory Committee, and as a result we have decided to hold a joint meeting of your group and that of J. Orear for the purpose of clarifying in our minds, and perhaps also in Jay's and yours, the relative merits and relative costs of the two techniques that have now been proposed by your separate groups. We also expect that some members of our Program Advisory Committee will be present. Needless to say, we shall be searching for the best way available for making a good set of measurements, consistent with our limited means. At the same time, we shall neither forget nor ignore your expressed prior interest, experience, and involvement in this kind of work.

I suggest that in view of the present state of our schedule, we should think of a time sometime early in November for the meeting in question. This should give both groups ample time to prepare. We shall be in touch with you in October to set a definite date.

Quack, Quack!

Bob

R. R. Wilson

Enclosure - Proposal #177

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

October 24, 1972

Professor A. D. Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Professor J. Orear
Laboratory of Nuclear Studies
Cornell University
Ithaca, New York 14850

Dear Alan and Jay:

I am writing to confirm arrangements that have been made for a meeting we should like to hold with the groups proposing experiment #6 and #177. November 3 is the date that we have set for that meeting. Each of you is welcome to bring collaborators with you, but we should prefer to limit that number to about three for each of your groups.

The purpose of the meeting will be to give you an opportunity to review for us and for some of our Program Advisory Committee members who will be present, the experimental methods that you propose to use for the study of proton-proton scattering at very large values of momentum transfer. The purpose of those reviews will be to give us an opportunity to reconsider the merits of the experimental technique that has been proposed by Krisch for use in experiment #6. Of course, in evaluating the two experiments, we shall be mindful of the early expression of interest and of the approval that has already been granted to the Krisch group. At the same time, we have no intention of using laboratory time, money and resources in carrying out an experiment in an inefficient or expensive way.

We intend to convene the meeting at 10 a.m. We shall first ask Jay Orear to review the experiment which he has proposed to do, placing emphasis on his choice of experimental techniques and on comparison of the capability of those techniques with the ones proposed by Krisch for experiment #6.

Following Jay's presentation, we shall ask Alan Krisch to review the experiment for which he has been approved, presumably responding to the arguments advanced by Orear for the use of a somewhat different experimental technique. We intend that the entire meeting be carried out in an informal manner with ample opportunity for questions, comments and criticisms from both of the involved groups as well as from members of our Program Advisory Committee and from members of our own staff.

We look forward to meeting with you on November 3. I suggest that you call Ned Goldwasser if you have some questions.

Sincerely,

Bob

R. R. Wilson

cc: O. Chamberlain
J. Cronin
J. D. Jackson
W. Willis

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

November 10, 1972

Effective Nov 6, 1972
call my direct at
312-840-3211
NAL main number is
312-840-3000

Professor Alan Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Professor Jay Orear
Laboratory of Nuclear Studies
Cornell University
Ithaca, New York 14850

Dear Alan and Jay:

I thank you each for appearing at our meeting of November 3 to make a case for your own large angle p-p scattering experiment, and to criticize constructively the capabilities of the other's method.

As you know, we have felt from the start that although technique of experiment #6 enjoyed all the benefits of conservatism, it also suffered from the handicap of being, possibly, too conservative. Krisch's group has had much experience with such experiments, using the same technique, to great advantage. Nevertheless, new techniques are now available, and it may be advantageous to use them for this kind of experiment. We were therefore very pleased when Orear's group came forth proposing to do so. We had hoped that the interesting physics proposed in these experiments might be completed at an earlier time and with a more modest demand for high energy protons.

In the Orear proposal he claims to be able to accomplish those goals. We are therefore inclined to accept his proposal. At the same time, we respect the claim represented by Krisch's early proposal and approval. For two years he has had our encouragement to come forth with a modified and improved technique for accomplishing the physics you both have proposed. He chose not to, presumably because he does not believe alternate techniques to be advantageous, or even feasible. Therefore, at our recent meeting, we gave the Krisch group the opportunity to convince us that the alternate method that has now been proposed by Orear would not work. At that meeting, Krisch put forth some strong arguments and calculations indicating that the singles rates and accidental rate to which Orear's equipment would be subject would preclude the successful performance of the experiment. However, because of the complexity of the situation, it was not possible for Orear to respond immediately.

Therefore, as a result of Friday's meeting, we are expecting to receive documentation of Krisch's calculations and arguments. He is also to provide Orear with access to them and to data from which he has concluded that Experiment #177 would not be feasible, as proposed. We expect Orear to defend, in writing, his claim that his detectors will work under the conditions that can reasonably be expected to prevail.

As soon as we have received submissions from both of you, we shall review and evaluate your work and will reach some kind of a decision. That decision might be to design a simple experiment which we felt would test the different claims that are under contention. It is our hope to be able to resolve this matter by January 1973.

Sincerely,

Bob

R. R. Wilson

THE UNIVERSITY OF MICHIGAN
ANN ARBOR

November 6, 1972

Professor R.R. Wilson, Director
National Accelerator Laboratory
P.O. Box 500
Batavia, Illinois 60510

Dear Bob:

This letter is a written summary of the main points I raised during the meeting of Friday November 3, 1972.

The main difference between E-6 and E-177 is that E-177 has a factor of ~100 more solid angle and therefore requires much less beam. This large $\Delta\Omega$ is obtained by placing their detectors inside the proton-W pit, while our detectors are up at ground level behind 100 to 550 feet of earth shielding.

We believe that their detectors will not operate in the pit with their proposed beam of $4\ 10^{11}$ protons/pulse striking a 4" H_2 target. For their t = 4→10 spectrometer we estimate that there will be at least $2\ 10^7$ particles/pulse passing through their fast arm and about 10^6 particles/pulse passing through their slow (large angle) arm. Assuming a 1/3 second effective spill, the single arm rates will be 60 mc and 3 mc. The singles in each detector will be even higher. We do not believe their detectors can operate under such conditions.

Moreover for this t = 4→10 spectrometer the number of accidental coincidences between the two arms will be

$$N = 2\ (\text{resolving time})(\text{fast rate})(\text{slow rate})(\text{spill time})$$
$$= 2(3\times10^{-9}\ \text{sec})(60\times10^{6}\ 1/\text{sec})(3\times10^{6}\ 1/\text{sec})(1/3\ \text{sec})$$
$$= 3.6\times10^{5}\ \text{accidentals/pulse}$$

This is more than 10^{10} larger than their only estimate of 1/week which was made for their t = 10→20 spectrometer.

Our estimate for the slow arm is based on data from our ZGS inclusive experiments where the angles and momenta were similar to those in the E-177 slow arm.

1. We calculated for the ZGS experiment the measured number of particles per interacting proton per $\Delta\Omega \frac{\Delta P}{P}$ for S_{123} or S_{45} coincidences.

2. We then calculated for E-177 their number of interacting protons ($4\ 10^{11} \times .025 = 10^{10}$/pulse) and the phase space bite for their t = 4→10 spectrometer.

$$\Delta\Omega \frac{\Delta P}{P} = (2.6\ 10^{-3})(.15) \approx 4\ 10^{-4}$$

3. We then calculated the E-177 rate assuming that $\frac{dN}{d\Omega dP/P}$ is similar for the two experiments. This assumption is supported by the fact that we observed little variation in the S_{123} rates under widely varying conditions in three different ZGS spectrometers.

Our estimate for the fast arm is based on the inclusive cross sections measured at the ISR, PS, and ZGS which seem to support scaling. We considered only those protons in the range P = 50→70 GeV/c, θ = 7→9 mrad, $\Delta\phi\sin\theta$ = 1.6 mrad. This range is accepted by the t = 4→10 spectrometer in the E-177 proposal, but is only a tiny fraction of the total range they accept (P = 50→200 GeV/c, θ = 7→16 mrad, $\Delta\phi\sin\theta$ = 1.6 mrad). They may stop some of the particles in the 7→9 mrad range with more collimators, but they cannot stop all of them and the collimators will scatter-in some additional particles from the ~ 4→7 mrad range. Moreover they cannot stop the particles in the 10→16 mrad range because this is their elastic acceptance range. Our calculations indicate that, of the 10^{10}/pulse inelastically scattered protons, 10^{-2} lie in the range P = 50→70 GeV/c. We find that 10^{-2} of these fall in the $\Delta\Omega = 3.6\ 10^{-2}$ sr range θ = 7→9 mrad - $\Delta\phi\sin\theta$ = 1.6 mrad, since most of the protons come out in a cone of angle

$$\theta = \frac{\langle P_\perp \rangle}{P} \approx \frac{.5}{60} \approx 8\ \text{mrad}$$

which has a solid angle $\Delta\Omega = \pi(8\ \text{mrad})^2 \approx 2\ 10^{-4}$ sr.

We give more details of these calculations in the attachments. I am sorry that they are not in an easier to understand form, but it was difficult to prepare this letter in time for your program committee meeting next week. However I spent several hours discussing these calculations with Professor Chamberlain who is quite expert in this area. I would also be glad to answer any questions by telephone (313-764-4443) and would be willing to visit NAL again on or before your program committee meeting.

In their proposal E-177 claimed that their experiment was superior to E-6 in 9 ways. We believe that they are wrong in all claims except for the larger solid angle. Our reply to each claim is given below.

1. We can also run at 400 GeV using our same exit ports by deflecting the incident protons with a 20 foot magnet placed just upstream of the target and adding two additional magnets to R_3 and R_4 to maintain the 25 mrad vertical bend.

2. We can also extend our t range in a similar way.

3. In our 90° p-p elastic experiment at the ZGS our point to point reproducibility was about 2%.

4. E-177 claims their superior resolution gave them a factor of 100 improvement over our signal to inelastic background ratio. In fact our momentum resolution is about a factor of 4 better than theirs in each arm, so that our rejection of inelastic background should be superior to theirs. They apparently ignored the 4" target length in calculating their resolution and only included the 1 mm wire resolution.

5. They claim they ask for a factor of 100 less intensity. This is even approximately true only for the high t part of the experiment where we require $2\ 10^{13}$ compared to $4\ 10^{11}$ a factor of 50. For 2/3 of our run we require $5\ 10^{11}$/pulse to $3\ 10^{12}$/pulse, a factor of 1.25 to 7.5 more.

6. Their solid angle is indeed much larger than ours by about a factor of 100.

7. In our 90° ZGS experiment we showed that the inelastic background was less than 0.1% by taking carbon target background runs. Based on this, calculations show that our inelastic background should be well below 1% with 5x5 hodoscopes in each arm. Moreover since our momentum resolution is a factor of 4 better than E-177's, their claim is difficult to understand.

8. As shown in the attached table our maximum power is 675 KW for the range t = 5→16 not 1900 KW as claimed. In comparing this with E-177's claim of 455 KW it should be recalled that they forgot to consider the dependence of ∫B·dl on transverse position for the 120" magnets and may require more magnets to give the bending they require. We further feel that their idea of putting an iron pipe inside one of these magnets to create a "septum" with a field free region for the $4\ 10^{11}$ protons to pass through is unwise. Our $R_1 R_2$ L_1 and L_2 magnets already exist.

9. The additional excavation we require is ~200 feet of 30" diameter pipe and about 550 of 12" diameter pipe: this is somewhat less than 1/4 mile. Assuming an average cost of \$20/foot this costs \$15,000, which is rather small compared with what either our group or NAL has already spent on E-6. Moreover E-177 does not fit into the pit that exists but fits either in a hypothetical 22' pit or in the area where Walker et al plan to run. They must move the pit or shorten their slow spectrometer which would further increase their $\Delta\Omega \frac{\Delta P}{P}$ and thus their accidentals.

In summary we feel, as we did in 1970, that E-6 is the only reliable and economic technique yet proposed for measuring cross sections in the 10^{-39} cm²/sr range. We feel that both arms of E-177 will be swamped with high rates and no minor changes can alter this: as their detectors are in a pit where about $3\ 10^{10}$ charged particles/pulse are produced and 10^{-5} to 10^{-3} of these particles ($3\ 10^5 \rightarrow 3\ 10^7$) will get into their detectors in spite of their best shielding efforts. The only economical way to alleviate this problem is to put the detectors up at ground level behind a lot of earth.

I understand your concern that we require a great deal of beam. However when NAL reaches design intensity of $4\ 10^{13}$/pulse our requirements may seem less excessive:

1 month	$5\ 10^{11}$/pulse
1 month	$3\ 10^{12}$/pulse
1 month	$2\ 10^{13}$/pulse

If the highest intensity is a problem we can drop our last two points in the 10^{-39} cm²/sr range and limit our experiment to the 10^{-38}cm²/sr range. However we cannot start running until January 1974 and we all hope that NAL will be close to design intensity by that time.

Sincerely yours,

A.D. Krisch

ADK:aa
cc: Professor O. Chamberlain
Professor J.W. Cronin
Professor J.D. Jackson
E-177 Group

NATIONAL ACCELERATOR LABORATORY
P.O. BOX 500
BATAVIA ILLINOIS 60510
TELEPHONE 312 231 6600
DIRECTORS OFFICE

November 27, 1972

Ref: #6

Professor Alan Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Dear Alan:

The critique that you submitted with regard to Jay Orear's proposed new experiment, and Jay's response to your criticisms have both been received and studied by our Program Advisory Committee. At their recent meeting they discussed the relative advantages and disadvantages of your experiment and Orear's. As a result of that discussion and of the presentations made by you and Jay, it appears to be very likely that the long- standing approval you have had for experiment #6 will be cancelled. The reasons remain those stated in my letter of March 9, 1971.

Orear has not yet convinced us of the feasibility of the experiment using his proposed technique. Nevertheless, the spirit of his approach is one that we hoped someone would work out and adopt for the exploration of this interesting region of p-p scattering. You may expect to hear from me once more as soon as we have resolved, to our satisfaction, the credibility of Jay Orear's confidence in his method. It is our expectation that he will be able to work out satisfactory solutions to the problems that you and we have posed and that he will be able to do the experiment at NAL.

You have indicated that you have already committed some funds to the preparation of the experiment. I would suggest that you get in touch with Jim Sanford about the possibility of our giving you some relief in that regard. We might, for example, purchase from you the magnets that you have specially constructed.

Sincerely,

R. R. Wilson

THE UNIVERSITY OF MICHIGAN
ANN ARBOR
THE HARRISON M. RANDALL LABORATORY OF PHYSICS
TEL. No. 313-764-4437

January 26, 1973

Dr. E.G. Pewitt
H.E.F. Division
Argonne National Laboratory
Argonne, Illinois 60439

Dr. J.L. Sanford
National Accelerator Laboratory
P.O. Box 500
Batavia, Illinois 60510

Dear Gail and Jim:

I am writing this letter to make sure that all people in the Chicago area are aware of the existence of the four magnets we have assembled for our N.A.L. Experiment 6, as it now appears unlikely that we will run before 1974.

The magnets are:

Magnet	GAP (inches) H	W	L	OVERALL (inches) H	W	L	MAX B_o Kilogauss	
L1	2	20	47	61	57	59	17	CERN 2" Septum 26 tons
L2	4.3	13	18	67	75	48	10	MICH Cloud Chamber 15 tons
R1	0.8	6	157	23	18	168	14	0.6 inch Septum 9 tons
R2	3	5	36	18	27	48	14	Tinkerbelle 2 tons

We expect to be using the CERN Septum for our Polarized Beam experiment at the ZGS throughout 1973. If you have any interest in any of the magnets please contact me.

Sincerely yours,

A.D. Krisch

ADK/all
cc: B. Hildebrand

NATIONAL ACCELERATOR LABORATORY
P.O. BOX 500
BATAVIA ILLINOIS 60510
TELEPHONE 312 231 6600
DIRECTORS OFFICE

June 1, 1973

Professor Alan Krisch
University of Michigan
Physics Department
Ann Arbor, Michigan 48104

Dear Alan:

Due to my misplacing your January letter on the magnets, I neglected to write to you about how we can help defray some of the costs you incurred in producing these magnets. As I remember you manufactured two septum magnets, but I gather that one is tied up in the ZGS experiment. Perhaps it would make sense for NAL to pay for these two magnets. Let me invite you to make an offer to us so that we can settle these matters to our mutual satisfaction.

Sincerely,

James R. Sanford

JRS:sjb

cc: E. G. Pewitt at ANL

THE UNIVERSITY OF MICHIGAN
ANN ARBOR
THE HARRISON M. RANDALL LABORATORY OF PHYSICS
TEL. No.

June 4, 1973

Dr. J.R. Sanford
National Accelerator Laboratory
P.O. Box 500
Batavia, Illinois 60510

Dear Jim:

Thank you for your kind letter of June 1, 1973 offering to reimburse us for the two septum magnets.

One of the septum magnets was in fact constructed for our ISR experiment and is now being prepared for our ZGS experiment. So it would seem that NAL has no obligation in regard to that magnet. This is the L_1 magnet in our NAL experiment.

The R_2 magnet was borrowed from CEA at negligible cost and is suitable for use at Argonne.

The R_1 septum magnet was indeed constructed specifically for our NAL experiment and its long narrow aperture makes it suitable only for experiments in the multi-hundred GeV range.

In addition the L_2 C-Magnet was removed from the sub-basement of Randall Lab and reconstructed at Argonne specifically for our NAL experiment at a cost of about $5000. It has not yet been fully reconstructed or tested because of anticipated delays in our NAL experiment.

It may be appropriate for NAL to reimburse us for these last two magnets. However I thought that it might be more appropriate to delay the question of reimbursement until NAL makes a clear decision on the approval or disapproval of our NAL experiment No. 6 which we remain very enthusiastic about. Do you anticipate that NAL will be able to reach a decision soon?

I am looking forward to hearing from you.

Sincerely yours,

A.D. Krisch
Professor of Physics

ADK/all

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

June 13, 1973

Professor Alan Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Dear Alan:

During the past several months we have been engaged in
discussions with Jay Orear and with some of the members of
our Program Advisory Committee in an effort to pin down an
adequate set of criteria by which a feasibility test for Orear's
experiment could be judged. It has not been easy to do that
to everyone's satisfaction, but we are closing in on a set
of conditions. We hope to be able to run a preliminary test
within the next few months.

In my letter of 11/27/72, I indicated that the most probable
position of your experiment would be a disapproval. I am
taking that step and hereby withdraw my previous approval
of your experiment (E6).

I do this with a certain combination of regret and embarrass-
ment. Your early enthusiasm to explore p-p scattering at large
values of t has been most useful for us in making our plans.
The shape of the Proton Lab has in part been determined by
your work. Thus we owe you our gratitude for your interest
and involvement. In the event that Jay fails to obtain clean
and convincing results in his test, we may come back to you
on our knees.

Well,

Quack . . . . Smack!

R. R. Wilson

THE UNIVERSITY OF MICHIGAN
ANN ARBOR

THE HARRISON M. RANDALL LABORATORY OF PHYSICS

July 12, 1973

Professor R. R. Wilson, Director
National Accelerator Laboratory
P.O. Box 500
Batavia, Illinois 60510

Dear Bob:

I am writing in reply to the recent letters I received from you and Jim Sanford in which you withdrew the approval for our experiment E-6 and Jim kindly offered to buy some of the magnets we have prepared for this experiment. I apologize for my delay in answering because we have been fairly busy with our polarized beam work at Argonne.

We have considered both your letter and Jim's offer and I decided that it would be inappropriate to sell the magnets at this time since we still hope to do this exciting experiment. I continue to feel that your decision to withdraw our approval in favor of Orear's proposed technique is unwise and that NAL will eventually come to realize this. I believe, that Orear's experiment will work only if he modifies it until it becomes so similar to our experiment that even you will agree that the situation has become inappropriate.

In preparation for your proposed visit to us when this occurs we will purchase a new rug for our trailer. I feel that both your age and stature make it inappropriate for you to injure your knees.

In the meantime we will be most pleased to loan the R_1 and L_2 magnets to NAL for any use you see fit, until we have some need for them. These magnets are at Argonne and can be obtained by having someone contact Larry Ratner at Argonne. It would seem appropriate for NAL to pay for shipping them to NAL.

Best regards,

A. D. Krisch

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 231-6600
DIRECTORS OFFICE

July 3, 1973

Professor Alan D. Krisch
Harrison M. Randall Laboratory
University of Michigan
Ann Arbor, Michigan 48104

Dear Al:

Now that the Director has made a decision about your experiment, I should see about purchasing from you the two magnets that you mentioned in your letter of June 4, 1973. I believe that they are:

R_1 Septum Magnet
L_2 C-Magnet

Could you please establish the construction costs for R_1, and I guess that $5,000 is the L_2 cost. If you will confirm these numbers, I will ask the Proton Section to prepare a purchase requisition.

Sincerely,

James R. Sanford

JRS:jp

cc: J. Peoples
J. Campbell

NATIONAL ACCELERATOR LABORATORY

PO BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 231-6600

August 28, 1973

University of Michigan
Experiment #177
Lazarus G. Ratner
Argonne National Laboratory
9700 South Cass Avenue
Accelerator Division
Argonne, Illinois 60439

Subject: Borrow of One R1 Septum Magnet

Gentlemen:

As per agreement between NAL and University of Michigan, the University of Michigan agrees to loan one R1 Septum Magnet (presently located at Argonne Laboratory) to National Accelerator Laboratory. The period of this loan will be from September 1, 1973 to January 1, 1975.

National Accelerator Laboratory agrees to pay all transportation costs incurred in the handling and movement from Argonne National Laboratory to National Accelerator Laboratory. Upon termination of this loan agreement National Accelerator Laboratory will return this magnet to the location specified by the University of Michigan at our expense. The magnet will be returned to the University of Michigan in the condition it was accepted other than normal wear and tear. Any modification to this magnet must be approved prior to making such modifications by the University of Michigan.

Very truly yours,

NATIONAL ACCELERATOR LABORATORY

8/28/73
James R. Sanford
National Accelerator Laboratory

Lazarus G. Ratner
University of Michigan