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A Personal History of Nucleon Polarization Experiments

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A Personal History of Nucleon Polarization Experiments

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ABSTRACT

The history of nucleon scattering experiments is reviewed, starting with the observation of large proton polarizations in scattering from light elements such as carbon, and ending with the acceleration of polarized proton beams in high-energy synchrotrons. Special mention is made about significant contributions made by C. L. Oxley, L. Wolfenstein, R. D. Tripp, T. Ypsilantis, A. Abragam, M. Borghini, T. Niinikoski, Froissart, Stora, A. D. Krisch, and L. G. Ratner.

It is a pleasure to be here, even if my topic today indicates that I am now old enough to be more of an authority on the history of polarization experiments than on current polarization work.

I believe the first inkling I had that polarization phenomena were going to be a source of great interest and enjoyment was at the January 1953 (3rd) Rochester Conference. I believe that at that time the work at Rochester was not thought to be ready for presentation at the Conference, but C. L. Oxley showed me around the apparatus. The remark that he made that sticks in my mind was: "If our results are right, then the polarization in scattering on carbon is really quite high."

That was the remark that led me to believe that the asymmetries they were observing at Rochester were most likely real and meaningful. I then stopped worrying, I think, about whether their observed symmetries were somehow false, and started taking them at face value.

By May 1953, the important Letter to the Editor of the Physical Review was submitted by Oxley, Cartwright, Rouvina, Baskin, Klein, Ring, and Skillman. It told the whole story: how to generate a polarized proton beam by scattering cyclotron protons from an internal target; how to detect the polarization by observing a left-right asymmetry in the scattering at a second target; and how to check for false asymmetries. Fig. 1 shows a plan view of their arrangement for making a polarized beam, a method we all followed. Fig. 2 of Oxley, Cartwright and Rouvina shows their compact scattering chamber, which could be rotated about the axis of the polarized beam.

Looking back at that work leaves me with the feeling there was only one small weakness shown: they emphasized scattering off hydrogen too much, as if they hadn't noticed and digested the fact that the polarization in carbon scattering, being higher, gives a better statistical handle on the answers. In later work, scattering from a carbon target induced the beam polarization, scattering from a second carbon target determined the beam polarization, and the polarized beam could then be used to study p-p scattering. In contrast, this earliest paper shows too much effort went into the attempt to use hydrogen targets at both first and second scatterings. Because the asymmetries were then smaller the results lost much of their statistical significance. And this was further compounded by the need, in most cases, to make a CH_2-C subtraction.

I remember that at that time I had only the simplest concept of the polarization process. If spin-up protons prefer to scatter to the left, then among the left-scattered protons there will be an excess of spin-up protons so that at a second target there will be an excess of protons scattered to the left. Our thinking at that point did not include the possibility of spin-flip phenomena.

In that first report from Rochester, Oxley et al. reported asymmetries from carbon and hydrogen 1st and 2nd targets. Their largest values were:*

e(C,H) = 0.10 ± 0.01

e(H,H) = 0.050 ± 0.025 (only a two-standard deviation effect)

e(C,C) = 0.25 ± 0.04

Beam polarization from carbon = P_C = 0.50 ± 0.04

Beam polarization from hydrogen = P_H = 0.21 ± 0.03.

(I have tried to re-express the results in modern terms.

$$\text{Asymmetry} = e = \frac{I_L - I_R}{I_L + I_R}.\ )$$

By the end of October (1953) the same group had submitted their big paper. This gave the whole story in more detail. It was clearly a great success [Oxley, Cartwright, and Rouvina, Phys. Rev. *93*, 806 (1954)].

The logical framework for these experiments had been laid down by Lincoln Wolfenstein [Phys. Rev. *75*, 1664 (1949) and Phys. Rev. *76*, 541 (1949)], but there was no prediction that appreciable polarization values could be expected in proton-carbon or

*In e (C,H), e is the asymmetry, C stands for a carbon first target, H for a hydrogen second target. I_L stands for the intensity of left scattering (at the second target).

proton-proton scattering. There was a suggestion that some polarization might be observable in proton-neutron scattering.

A paper by Wolfenstein and Ashkin [Phys. Rev. *85*, 947 (1952)] seems to be the first that utilizes modern notation and the density matrix.

In the summer of 1953 Emilio Segrè and I were at the Brookhaven Laboratory. The Cosmotron was just coming into operation. It was my first experience with beams at energies above 1 GeV.

When Segrè and I returned to Berkeley we were delighted to find that, in our absence, Tom Ypsilantis and Clyde Wiegand had succeeded in making a polarized proton beam based on an internal target of carbon. This got us in a position to do meaningful experiments.

Soon there were reports from a number of groups. By the end of January (1954) the polarization experiments had been much talked about at the Rochester Conference and we in Berkeley (Chamberlain, Segrè, Tripp, Wiegand and Ypsilantis) had submitted a Letter to the Editor. We had asymmetries as high as 0.39 ± 0.04.

At this point Segrè took the important step of persuading Lincoln Wolfenstein to spend the summer of 1954 in Berkeley. He was soon going to contribute some new ideas.

At the same time Marshall, Marshall, and de Carvalho submitted a letter. They said they had a beam polarization of 0.60† [Phys. Rev. *93*, 1431 (1954).]

When Emilio Segrè was returning to Berkeley after the January 1954 Rochester Conference he stopped over in Chicago, as he frequently did, to talk to Fermi. Segrè was pointing out to Fermi that we really didn't know the sign of the polarization. (After a left scattering at one target the beam would show more left than right scattering at

†An earlier report by the Marshalls had indicated that they observed no asymmetry at their second target. Segrè tells an amusing story: Leona Marshall asked him why it was that the Berkeley people saw an asymmetry and in Chicago they found none. Well, he said, you must be scattering at 36 degrees — at that angle we observe no polarization. He was quite right. He is justifiably proud of that guess.

the second target, but we did not know whether the once-scattered beam contained an excess of spin-up protons or spin-down protons.) Fermi said one ought to be able to figure out the sign. He went to the blackboard and asked Segrè to take notes. Fermi tried the Born approximation, using the spin-orbit coupling of the shell model, and found the Born approximation predicted zero polarization. Then he tried inserting an imaginary part in the potential, choosing the sign of the imaginary part so that it represented absorption. This was enough to give a non-zero polarization. It said that left scattered protons (i.e. protons that make a slight left turn at the first target) are predominantly spin-up. Fermi very soon submitted a paper to Il Nuovo Cimento [Nuovo Cimento *10*, 407 (1954)]. It was received on February 22 (1954). It was one of the last papers Fermi wrote, for he died in November of that year. The sign was finally determined experimentally by the Marshalls, who connected the high energy scattering phenomena to scattering in the few-MeV region where known nuclear resonances allowed reliable prediction of the sign of the effect.

During the summer of 1954 Wolfenstein gave us an important set of lectures in which we learned more about what polarization is and the elements of how to represent a partially polarized beam using the density matrix. The things he taught us were, for the most part, contained in the paper by Wolfenstein and Ashkin [Phys. Rev. *85*, 947 (1952)].

During that summer Wolfenstein asked us the crucial question: Is there any possibility of doing a triple scattering experiment? The idea was that the first scattering (probably on a carbon target) was to provide a polarized beam that would be incident on the second target. The second target, possibly liquid hydrogen, was to provide the scattering process, such as proton-proton elastic scattering, that was under investigation. The third target, probably carbon, was to determine the polarization of the twice-scattered particles. We would hope to find out how the polarization had been changed by the scattering process at the second target.

It seemed to me that a triple-scattering process would give unacceptably low counting rates, but Bob Tripp and Tom Ypsilantis proceeded to isolate themselves from the rest of the group for a long period; I think it was at least two weeks. During that time they almost refused to talk to the rest of us. At the end of this period they emerged with the claim that we could do a triple scattering experiment if we were patient and had enough running time.

On the basis, I think, of that much encouragement Wolfenstein submitted his paper: "Possible Triple Scattering Experiments." It was received September 7 (1954) and was published as Phys. Rev. *96*, 1654 (1954).

At that time two problems confronted us that we were not sure how to deal with. Our cyclotron runs had never been longer than 8 hours. But we needed at least two weeks of running to get meaningful triple-scattering results. The second item was that the 184-inch Berkeley cyclotron was about to be turned off for conversion to a 700 MeV machine (from 300 MeV). Tom Ypsilantis and I discussed the situation and decided that we should get our equipment fully ready just in case the improvement program for the cyclotron might be delayed. We did get everything ready and – just one day before the shutdown was to occur -- the engineers announced that the parts were not near enough to being completed in the shops so the shutdown would be delayed.

We were ecstatic. We were the only ones ready to run at the cyclotron. We put in our equipment and had the first-ever 3-week run. Fig. 3 shows the equipment we used. It was not substantially changed from the design brought forth by Ypsilantis and Tripp. This same geometry (surrounding the third target) served us well though a number of triple-scattering experiments.

Fig. 4 shows the beam profile of the twice-scattered beam as determined with two different absorber thicknesses. The shift seen in the figure reflects the fact that the twice-scattered beam has a higher average energy on one side than on the other. Care must be used to keep this from causing an error in the asymmetry measurement at the third target. Fig. 5 shows the profile for the beam as we used it.

We measured D (as defined by Wolfenstein), which can be defined through either of the following two statements:

$$D = \frac{LL + RL - LR - RR}{(LL + RL + LR + RR)P_1P_3}$$

in which the first L in LL stands for a left scattering at target 2 and the second L means a left scattering at the third target. P_1 and P_3 are the polarization at first and third targets. Thus LL is the counting rate of intensity for left-left scattering at 2nd and 3rd targets. Alternatively, D may be defined by saying the left-right asymmetry at the third target is

$$e_{3n} = P_3 \frac{(P_2 + DP_1)}{1 + P_1P_2}$$ Experimental values are shown in Fig. 6.

We also measured R (as defined by Wolfenstein). For that we used a beam polarized in the up direction, but the reaction plane is vertical. Thus the second scattering is either up (U) or down (D). Then the final (third) scattering is either left or right. The parameter R can be defined by

$$R = \frac{UL - UR - DL + DR}{UL + UR + DL + DR} \frac{1}{P_1P_3}$$

or

$$I_2\langle\sigma\rangle_2 \cdot s = I_0[A\langle\sigma\rangle_1 \cdot k_2 + R\langle\sigma\rangle_1 \cdot n_2 \times k_2]$$

Results appear in Fig. 7.

A measurement of the parameter A_W (A as defined by Wolfenstein) requires that the incoming beam at target 2 be longitudinally polarized, and this is arranged by upward magnetic bending of the beam before it impinges on target 2. The geometry of the measurement is shown on Fig. 8, taken from a paper by Simmons [Phys. Rev. 104, 416 (1956)]. Fig. 9 shows the experimental results for A_W. This triple-scattering parameter should not be confused with the analyzing power, now called A.

During much of 1953 and all of 1954 we were working on both the polarization experiments described here and the antiproton search. Often I was working on polarization by day and the antiproton search by night. In the fall of 1955 we announced we had found antiprotons. In the spring of 1956 Segrè and I went to a conference in Dubna. At that meeting we discussed the antiproton, but also presented a first cut at a phase-shift analysis of p-p scattering at 310 MeV. We did not have just one unique solution for the phase shift. Instead we had several solutions, but we thought we were very close to a unique solution.

We submitted the big paper on p-p amplitude analysis in the fall of 1956 [Phys. Rev. *105*, 288 (1957)], giving it the deceptively simple title of "Experiments with 315-MeV Polarized Protons: Proton-Proton and Proton-Neutron Scattering." Based, as it was, on three kinds of triple-scattering experiments, besides the simpler double scattering, it was a big work, one to be proud of. Fig. 10 shows the matching of the phase shift solution to the requirements of the reaction $p + p \rightarrow \pi^{+}d$. Solutions 5, 7, and 8 fail this test.

During the fall of 1959 I got a suggestion from Dick Wilson. He said, "Now that you have that Prize you have an opportunity to take a little longer-range view of your research program. We all know that a polarized proton target can be made – why don't you make one?"

I'm not sure Dick Wilson's remark was the crucial input, but it certainly was encouragement. Up to that point the best target material we knew of was irradiated polyethylene (or CH_2 of some kind). We argued that there was a class of experiments that could really only be done by utilizing a polarized target. I started by planning a magnet with uniform field, in the meantime keeping Carson Jeffries informed of my needs and my activities. Carson Jeffries, like Prof. Abragam, was one of the world's experts on spin relaxation processes at low temperatures.

One day Jeffries stopped me in the hall to say that he thought he had a key to a polarized target -- neodymium ions in LMN (lanthanum magnesium nitrate) (with lots of water of crystalization) looked like the material to use.

Within a few days he had tried out a small sample and it had worked well. Soon we were getting his expert advice on how to grow large (1-inch) crystals of LMN (Nd).

In 1962 Abragam and his collaboration [Abragam, Borghini, Catillon, Coustham, Roubeau, and Thirion, Phys. Lett. *2*, 310 (1962)] published a report of what I believe to be the first polarized-target experiment. They used a similar target material, but a very small target. It was a few cubic millimeters in volume. It was an impressive experiment in which they measured C_{NN} (A_{NN} in modern notation) for proton-proton scattering at 15 MeV.

The following year we made our first report [Chamberlain, Jeffries, Schultz, Shapiro and Van Rossum, Phys. Lett *7*, 293 (1963)] on work done with the polarized target. We had made a polarized proton target that was about one cubic inch in size, in the form of a one-inch cube.

We were apparently the first people to make a target with dimensions larger than the half wavelength of the microwaves used to transfer electron polarization to the protons. There ensued an argument over whether this was a significant contribution to the field. Prof. Abragam was asked why he hadn't made a big target if one could be made. "Well," he said, "Nobody asked me for a bigger target." To me it was a happy merging of classroom teaching and working in the lab. I had been teaching my class about many kinds of modes of electromagnetic oscillation in a conducting box.

I would like to show you our first and simplest arrangement for counting pion-proton scattering events from the polarized target. It is shown in Fig. 11. Apart from beam-monitoring equipment it required just 5 scaling circuits, for one counter on the pion side was placed in coincidence with each of 5 counters on the proton side. The scattering on free protons (the polarized protons) revealed itself as a peak in counting rate near the middle counter of the five.

Soon we were doing a number of experiments, with equipment like that shown in Fig. 12. Figure 13 shows the kinematic peak due to scattering on free hydrogen. (Fig.

13 relates to proton-proton scattering.)

As I conclude I wish to point out a few developments that I believe to be quite important, though I did not take part in or observe them closely. The first thing I would mention is the frozen-spin polarized proton target developed by Borghini and Niinikoski. They showed [see Nucl. Instrum. Methods *105*, 215 (1972)] that if the target material is cold enough, the target, once it is polarized, can remain polarized for months, even in a fairly weak field of 0.6 tesla. The frozen-spin target has the advantage that its magnetic field need not be very uniform, hence a very open coil geometry can be used, one in which it is easy for particles to leave the target in many different directions without having to penetrate the conductor of the coil that generates the magnetic field.

Along with this goes Niinikoski's observation of the importance of using a powerful dilution refrigerator, capable of handling the power dissipation during the polarization process and able to achieve the low temperatures needed for frozen-spin operation. [See Nucl. Instrum. Methods *97*, 95 (1971).]

Another important development was the work of Froissart and Stora [Nucl. Instrum. Methods *7*, 297 (1960)] who analyzed the depolarization mechanisms in circular accelerators. They enumerated the obstacles to be faced -- intrinsic resonance and imperfection resonances -- if polarized particles are to be injected into a synchroton and accelerated in a manner that preserves the polarization.

Lastly I will mention Alan Krisch and his colleague Larry Ratner, who showed that the depolarizing resonances can actually be overcome. Krisch, especially, has energetically pursued the acceleration of polarized protons, first at the Argonne Laboratory and now to 16 GeV at the Brookhaven National Laboratory. Fig. 14 shows the rising spin precession frequency that occurs as the beam energy increases during acceleration. When the vertical tune parameter matches the spin frequency the beam may be depolarized. If, as shown in the right half of the figure, the vertical tune ν can be abruptly changed, then the resonance may be passed through very rapidly, with very little loss of polarization. The tune change is accomplished by pulsing special quadrupoles that are built to produce a rapid rate of rise in the quadrupole field.

In summary, I want to list the people that I feel are the heroes -- the people who have given our work a special push:

C. L. Oxley: He showed that polarization in scattering can be larger than expected, and he showed how to proceed.

Lincoln Wolfenstein: He laid out the original theory and he asked the crucial question about triple-scattering experiments.

Bob Tripp and Tom Ypsilantis: They found a workable way really to do a triple-scattering experiment.

Abragam and Borghini and their colleagues: They made the first polarized target and performed the first experiment with it.

Borghini and Niinikoski: They introduced the frozen-spin target and showed the advantages of powerful refrigerators that keep the target extra cold.

Froissart and Stora: They analyzed the depolarizing resonances in a synchrotron.

Krisch and Ratner: They showed that depolarizing resonances can be overcome if one really works on the problems -- to 16 GeV so far.

Thank you for your attention. I hope this is only the beginning and that we shall sooner or later have highly polarized proton beams to the highest energies. And polarized antiproton beams also.

It has been a pleasure to have this opportunity to cite some of the people who, in my opinion, made especially noteworthy contributions to our work in spin physics. In retrospect I see I have expressed a rather personal view. Certainly I have drawn too many examples from the Berkeley work, even when similar work was going on at other laboratories. I hope that, for all its weaknesses, this review will prove enjoyable and possibly instructive to some of you.

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FIGURE CAPTIONS

Fig. 1. Experimental arrangement of Oxley, Cartwright and Rouvina. The system, utilizing an internal cyclotron target, was widely copied.

Fig. 2. Compact second-scattering geometry, rotatable about the beam axis.

Fig. 3. Plan view showing the second and third targets in the apparatus used to measure the Wolfenstein parameter D for proton-proton elastic scattering. The polarized beam is incident from the left.

Fig. 4. Beam profiles of the twice-scattered beam. The two curves were obtained with different amounts of absorber between counters 3 and 4, located as shown in the previous figure. The fact that the centroids of the two curves are different reflects the fact that the average energy of the twice-scattered beam can be different on the left and right sides of the beam.

Fig. 5. Beam profile of the twice-scattered beam. It was necessary to avoid using excessive absorber thicknesses to have a stable beam centerline.

Fig. 6. The depolarization parameter D in p-p scattering at 310 MeV. The curve shows the values associated with a certain set of phase shifts, the set originally called solution 4.

Fig. 7. The rotation parameter R for p-p scattering at 310 MeV. The curve shows solution 4.

Fig. 8. Perspective drawing of the A_W experiment geometry (not to scale). The circles labeled 2 and 3 represent the hydrogen target and the analyzing target respectively. The first scattering inside the cyclotron is not shown. The plane labeled π_1 is the vertical plane containing the deflected beam, while the planes π_2 and π_3 are, in order, the planes of second and third scattering. The planes π_1 and π_2 are perpendicular, as are the planes π_2 and π_3. The vector n_2 lies in the vertical plane. The vector H represents the horizontal magnetic field.

Fig. 9. Experimental results for the parameter A_{W}.

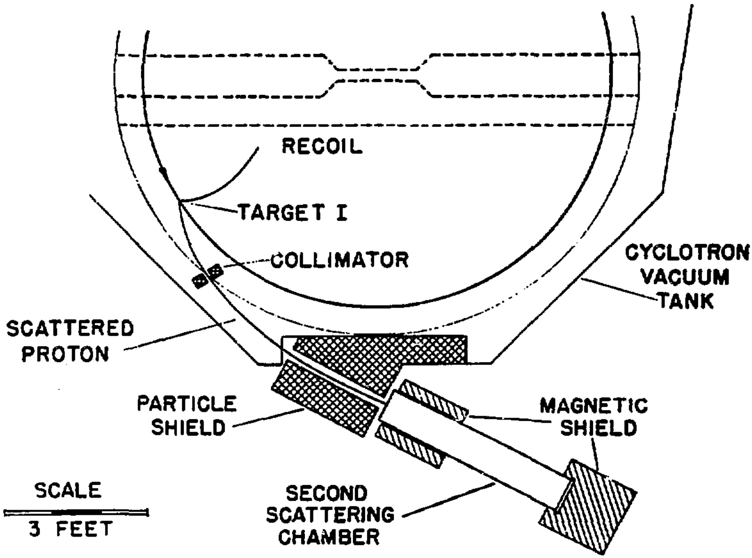
Fig. 10. The distorted crosses show the ranges of phase angles τ_0 and τ_1 that are consistent with the experimental results in the reaction $p + p \rightarrow \pi^+ + d$. The numbered dots indicate values for various phase-shift descriptions of p-p scattering, some of which could be discarded on the basis of this figure.

Fig. 11. Elevation view of the first and simplest apparatus for measuring the analyzing power A.

Fig. 12. A more useful geometry in which a range of scattering angles could be covered with a single setting. This apparatus was used in measuring A for proton-proton scattering.

Fig. 13. Counts in the "down" counters D1 througb D10 that are in coincidence with the "up" counter U6, shown in the previous figure. The center of the hydrogen peak falls in counter D_4, where about 80 percent of the counts are due to elastic p-p scattering.

Fig. 14. Rising spin precession frequency as it occurs during the acceleration process. Left: depolarization occurs as the beam passes somewhat slowly through the resonance. Right: pulsed quadrupoles allow a very rapid passage through the resonance condition, with little depolarization.



XBL 8412-5478

Fig. 1

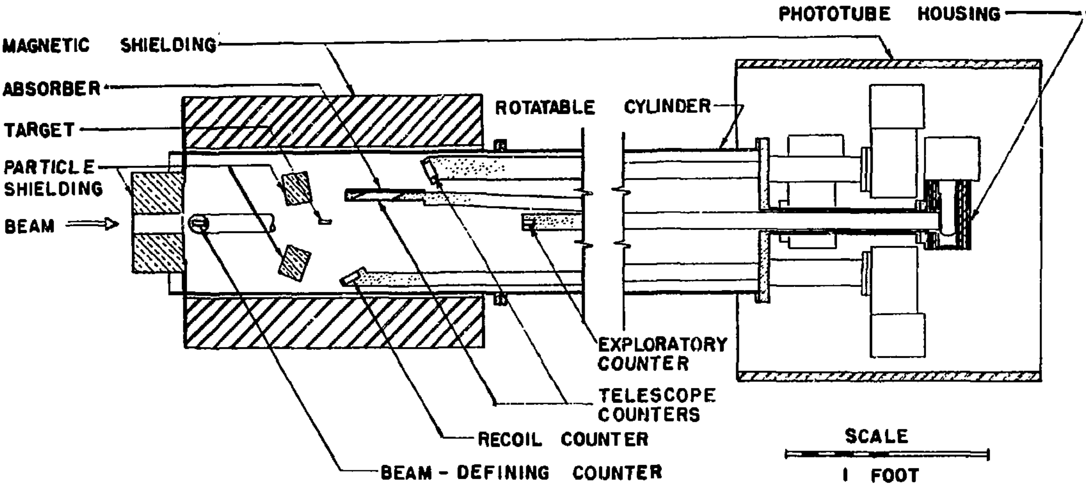


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Fig. 2

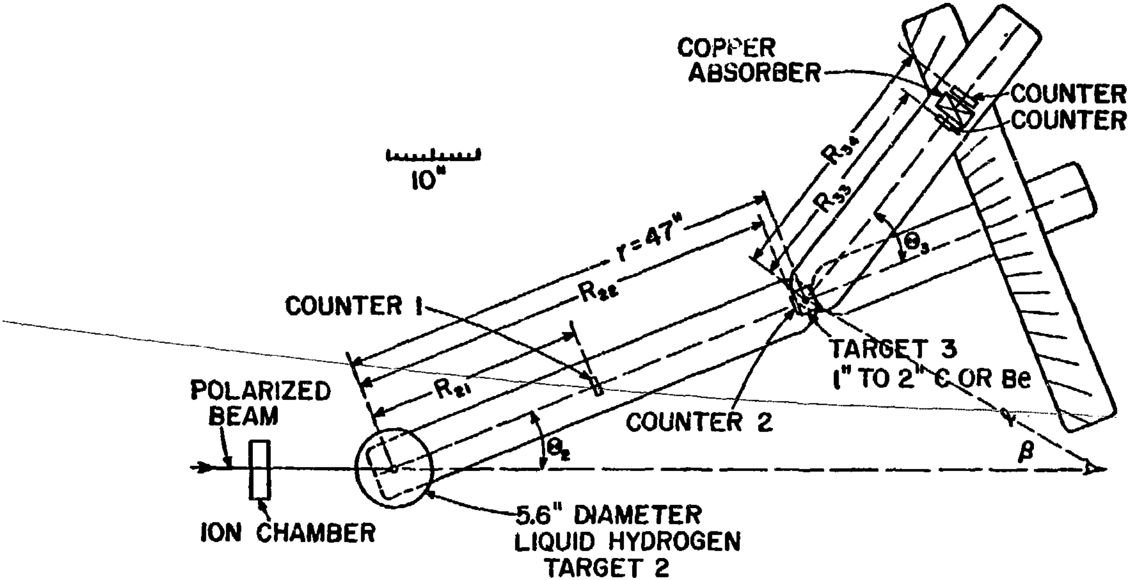

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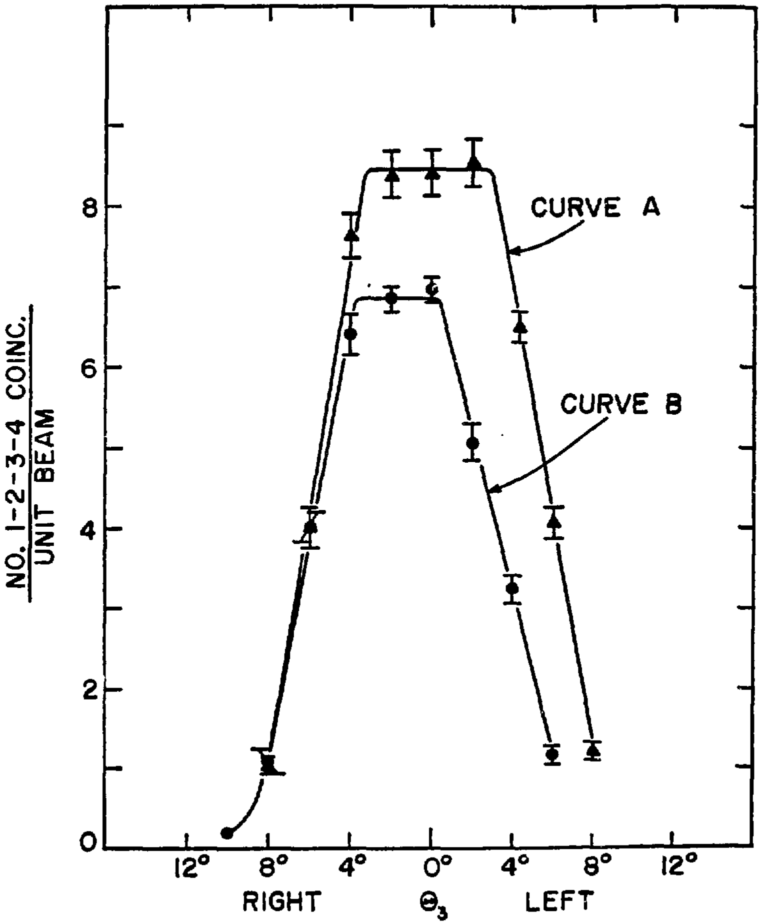
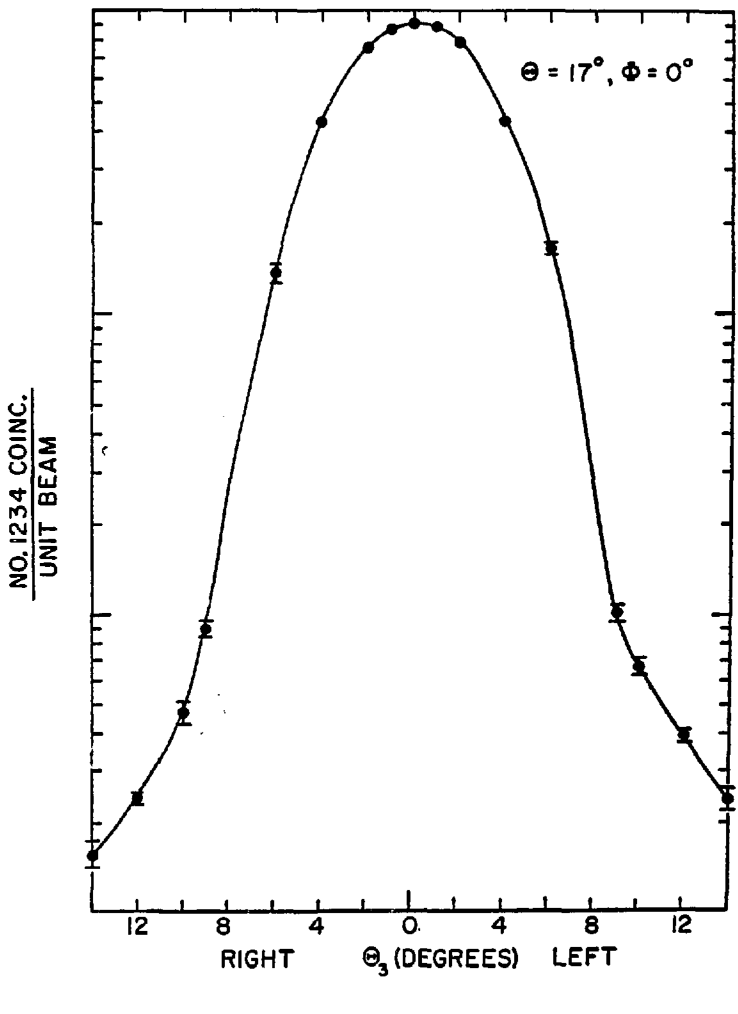
Fig. 3



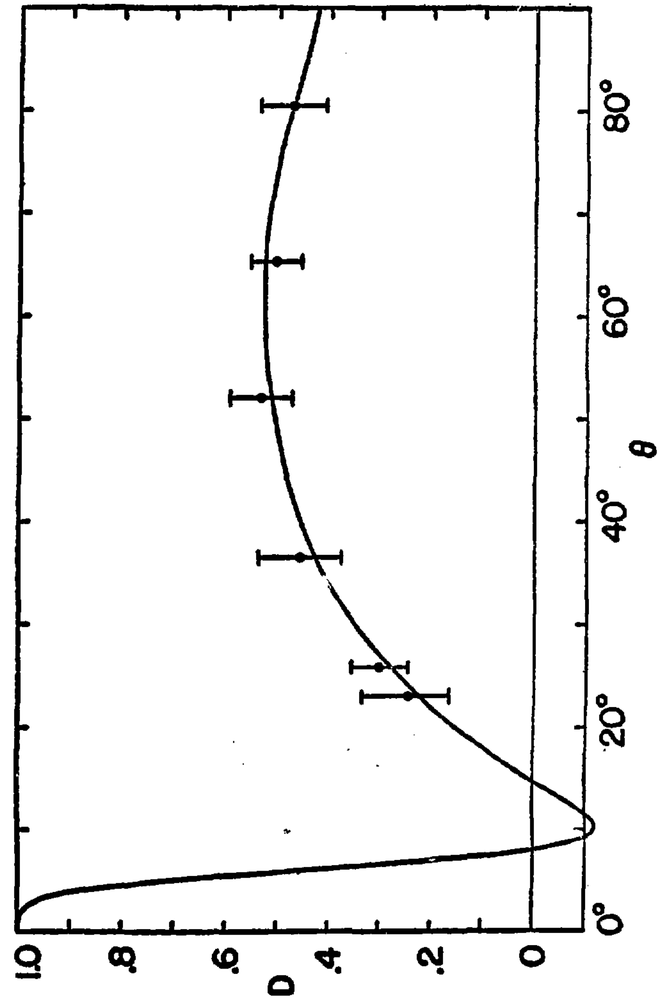
XBL 8412-5481

Fig. 4



XBL 8412-5482

Fig. 5



XBL 8412-5483

Fig. 6

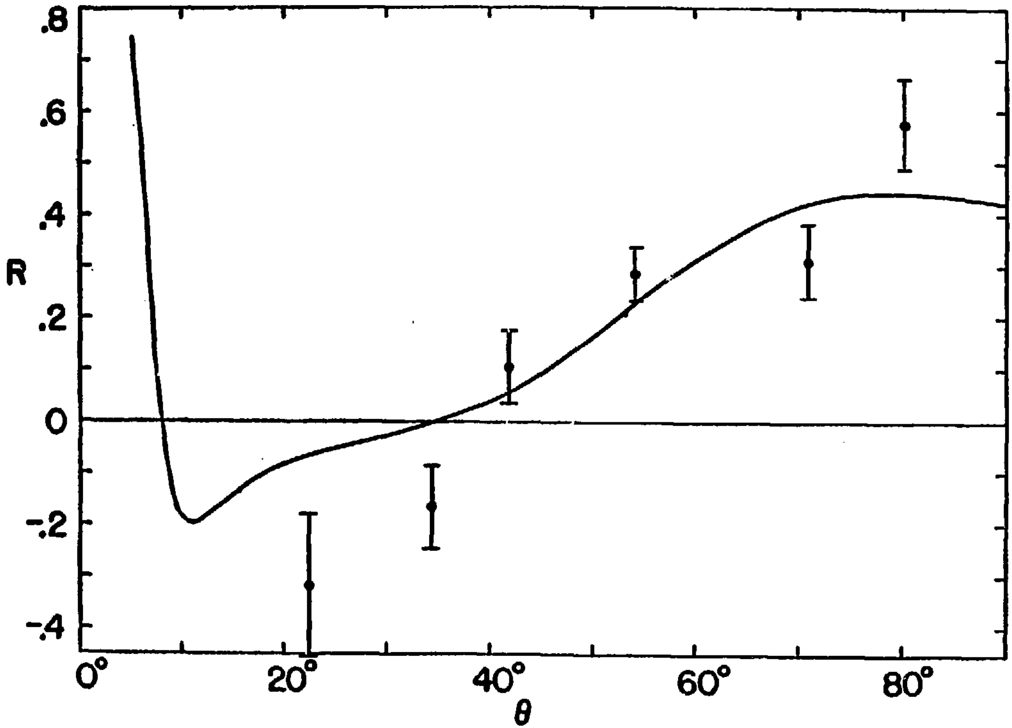


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Fig. 7

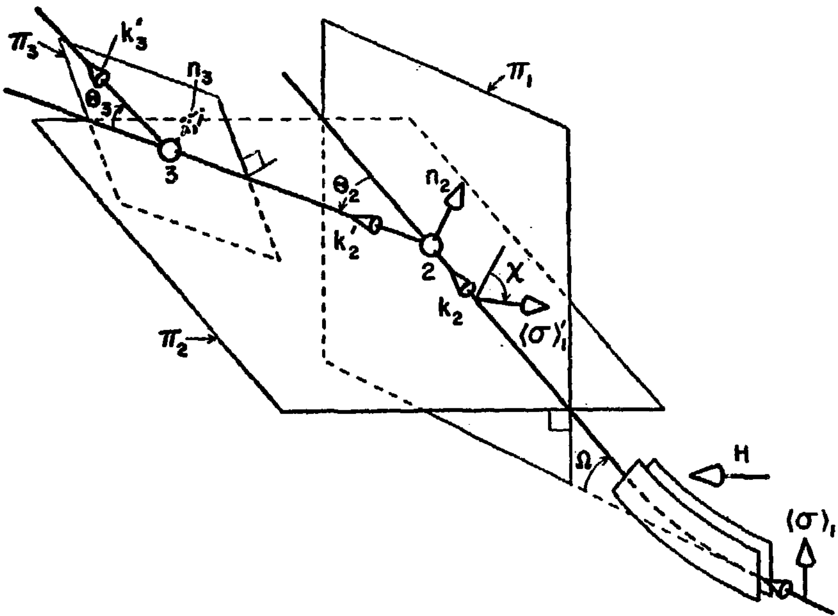

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Fig. 8

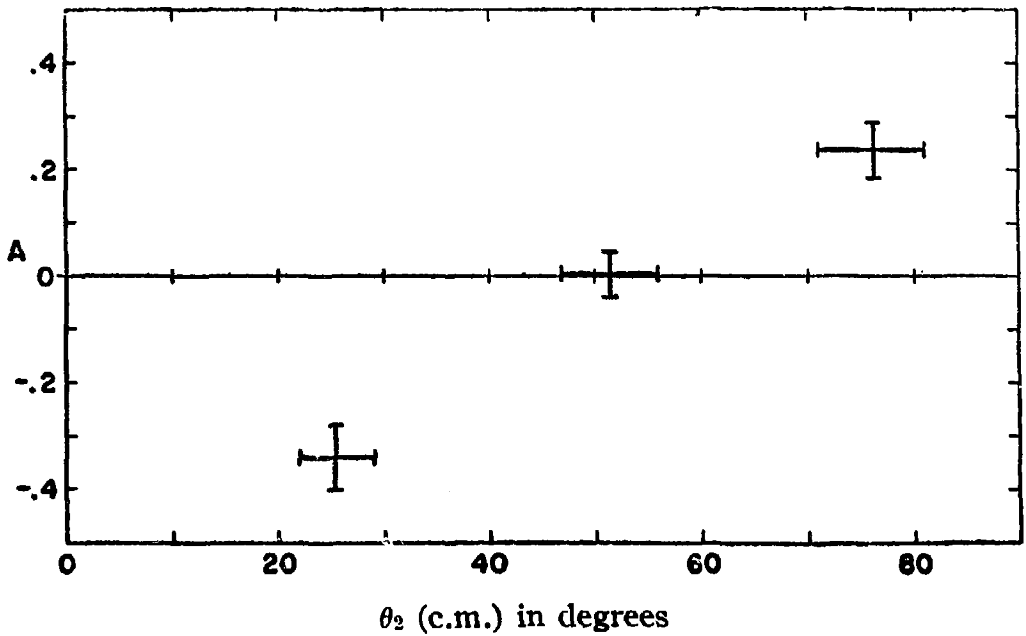

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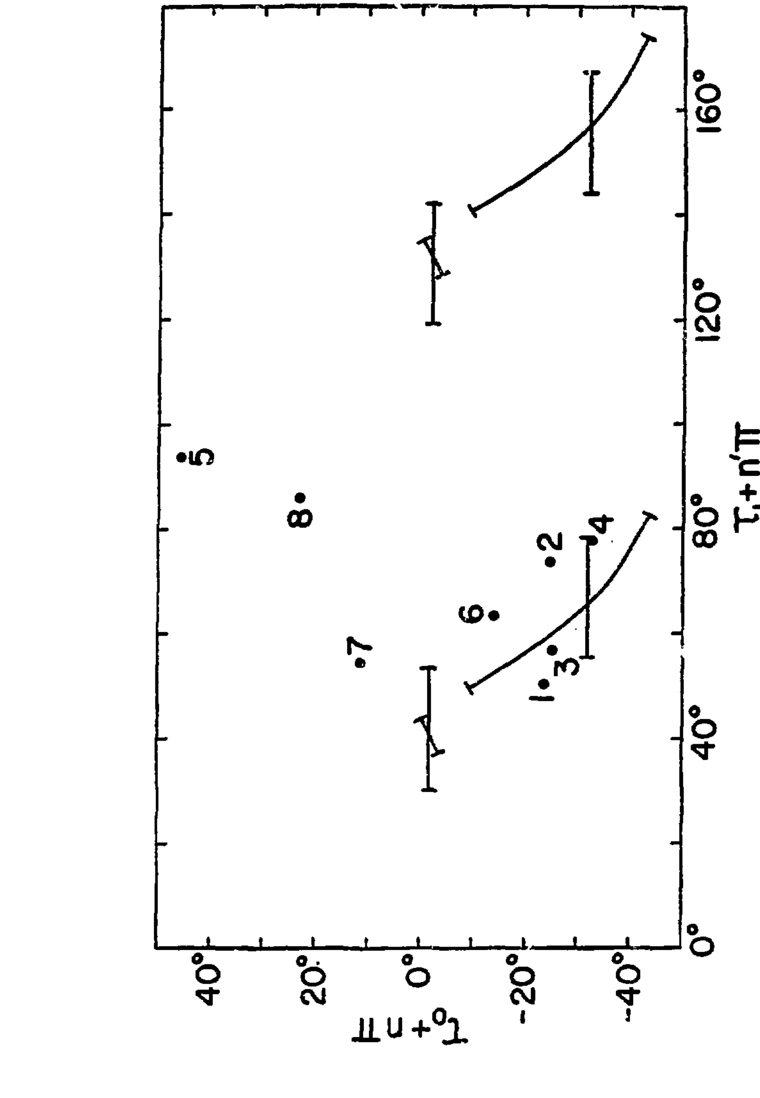
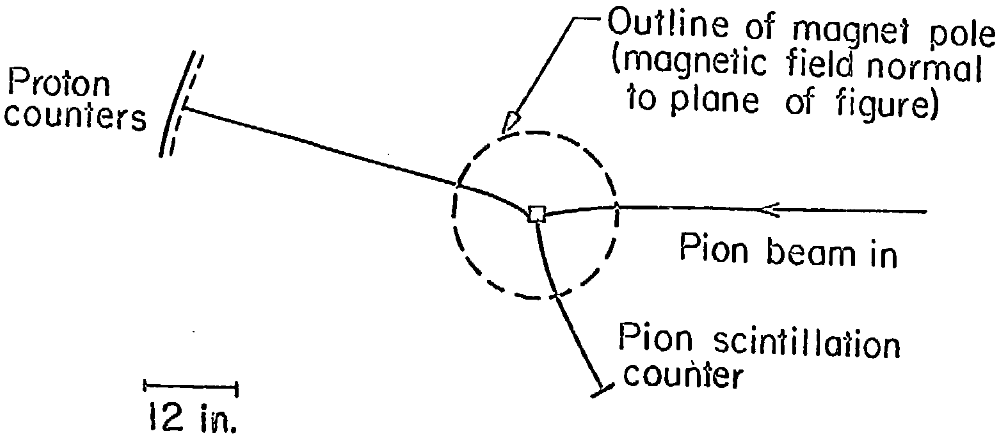
Fig. 9



XBL 8412-5487

Fig. 10



MU-30598A

Fig. 11

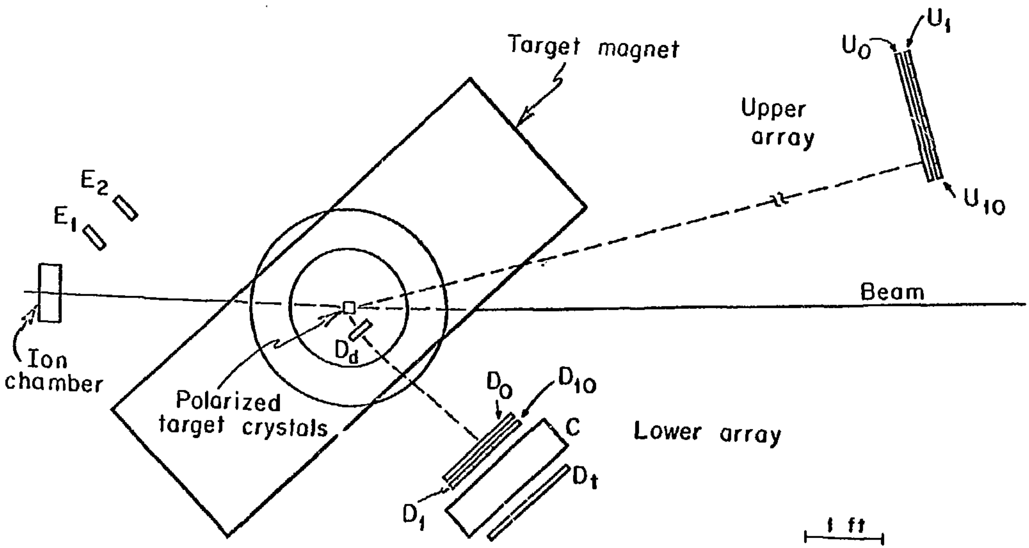


Fig. 12

MU-35856

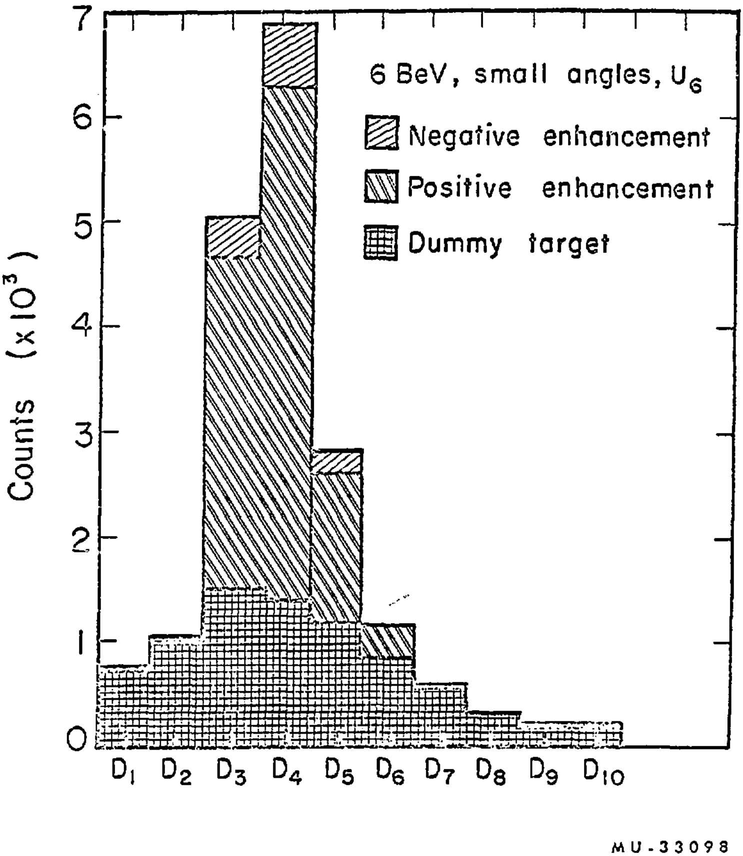
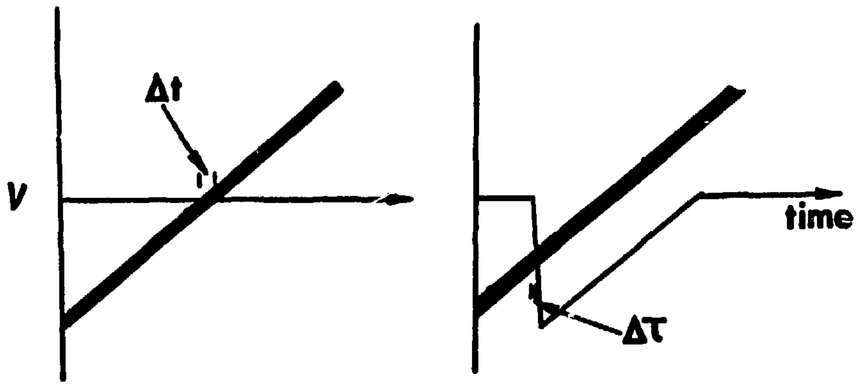

7
6
5
4
3
2
1
0
Counts (x10³)
6 BeV, small angles, U6
Negative enhancement
Positive enhancement
Dummy target
D1
D2
D3
D4
D5
D6
D7
D8
D9
D10
MU-33098

Fig. 13



XBL 8412-5477

Fig. 14