$\overline{P}$ PRODUCTION CHARACTERISTICS AND COLLECTION SYSTEMS

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I would like to report on the work of the target group.[1] Most of these ideas are old, but it is interesting to review them.

First I should say that the amount of data that exists on antiproton production is still too small. We really need to measure this. It has not been very fashionable for experimenters at Fermilab or the SPS to make measurements of low-energy antiproton production with high-energy beams. It has simply not been interesting, but I think it is now. I think it is extremely important. Nevertheless, I think we have a good phenomenological feeling for the relevant parameters in the production. The invariant cross section, as far as one can tell, probably peaks at $x = 0$, which means that there is probably a very broad maximum in the cross section for producing antiprotons of momentum such that $x = 0$ for a given laboratory energy.

The second thing is that there seem to be two nuclear effects that one can observe: (1) There is very little evidence for absorption of the antiprotons inside the nucleus itself.[2] This is not an understood physical phenomenon, but it doesn't seem to hurt in going from low-Z materials, low-A materials, to high-A materials. (2) On the other hand, there is evidence that if you are near threshold for production of antiprotons (you can find this from the work at CERN, Deckers et al.) that the cross section may be a factor of two larger per nucleon and heavy nuclei. So there is a gain by going to heavy nuclei, aside from just making targets that are shorter. Again, this should be measured.

The Production Cross Section Variation with Energy

For example, the existing data that are relevant to Fermilab for the Fermilab scheme using GeV/c antiprotons follows the production curve in Fig. 1. It seems to have a broad maximum as far as one can tell and continues flat up to very high energies. If these data are compared with the predictions of the Stanford-Wang formula, the results are plotted in Fig. 2. The group tried to understand to what extent these phenomenological models give the same general features. The cross sections probably follow a rule of thumb something like this. As a function of laboratory energy, the cross section for 3 GeV/c, which is roughly the momentum to be used at CERN, has a sharp threshold. This is the point of $x = 0$ where the cross section has roughly reached its maximum or the knee in the curve and probably goes up slowly after that. The results at 6 GeV/c follow Fig. 1 and probably higher momenta like 20 GeV/c will reach a larger asymptotic value than 6. A. Kernan has parameterized the $x = 0$ data as shown in Fig. 3.

As can be seen, a very high energy machine would continue to gain in cross section given the same acceptance of the collector. However, the gain is relatively smaller the larger the energy, as seen from Fig. 3. One might have naively thought that if you go to very high energy, you get many more lower energy particles, but that appears not to be true. The factor between 6 GeV/c and 3 GeV/c is between 5 and 10. Thus the Fermilab scheme antiproton production cross section and the CERN scheme anti-proton production cross sections are quite different. The acceptance of the CERN collector must be larger if the same number of $\overline{p}$ are to be collected/hour.

The Production Cross-Section Variation with Transverse Momenta

Another characteristic of the production is the $p_\perp$ distribution, which as everybody knows, has a fall-off something like $e^{-6p_\perp}$. Let me show you two examples of that. At 6 GeV/c $\overline{p}$ momentum, the angular distribution is plotted in Fig. 4.[4] There is a cut-off near 300 MeV/c. There will be little gain by building devices that collect all the way up to high $p_\perp$ because the yield of antiprotons is decreasing. The point is that you do not gain in trying to collect $p_\perp$ of 1 GeV/c because there is not much yield beyond that, as far as we can tell. Figure 5 shows the differential cross section for 9 GeV/c produced by 200 GeV/c incident protons.[4] It is expected that the characteristic will be the same in the two cases.

From these distributions, we can get an idea of the ideal $\overline{p}$ yields that can be obtained from these $\overline{p}$ collector systems. We work backwards and compare the ideal yield and compare what has been calculated through a realistic focusing system and targetry and find out how close we have come to the ideal. We can write

$$\frac{N_{\overline{p}}}{N_p} = \frac{1}{\sigma_a} E \frac{d^3\sigma}{dp^3} \frac{dp_{\parallel}}{E} \pi dp_\perp^2 ,$$

where σ_a is the absorption cross section, $dp/E = \delta p/p$ the momentum bite and $dp_\perp^2$ the transverse momentum bite.

For an ideal transverse momentum collection, $\pi dp_\perp^2 \sim \pi(0.3)^2$ which gives

$$N_{\overline{p}}/N_{p_{\text{ideal}}} = \frac{\pi(0.09)}{\sigma_a} \frac{Ed^3\sigma}{dp^3} \frac{\delta p}{p} .$$

We can estimate the ideal yields as[5]

$$N_{\overline{p}}/N_p = 1.1 \times 10^{-5} \text{ CERN}$$
(~ 2.2 with Fermi momentum effects)
$= 2.8 \times 10^{-5}$ Fermilab
$= 1.5 \times 10^{-2}$ and $\delta p/p_{\text{Fermilab}}$
$= 0.3 \times 10^{-2}$.

The $N_{\bar{p}}/N_p$ values for CERN and Fermilab are within a factor of two. Even though we started with a factor of 5-10 in cross sections, the larger momentum acceptance of the CERN machine has made up for that.

We caution, however, that these estimates are still for hydrogen. If the Fermi momentum effect is included, going to a heavier target at CERN will get another factor of 2.[5] But there is not much to be gained beyond that. The ideal collection yield of those two schemes will be roughly the same. The momentum acceptance offsets the change in cross section. At CERN it is necessary to take a larger momentum bite because the cross section is smaller.[5]

We can estimate the ideal yield of $\bar{p}$/sec at the two machines assuming

$$\langle N_p \rangle = 10^{13}/\text{sec} \quad \text{Fermilab}$$

$$\langle N_p \rangle \quad \text{CERN}$$

$$\langle N_{\bar{p}} \rangle_{\text{Ideal}} = 2.8\times10^{8}\ \bar{p}/\text{sec} = 1.0\times10^{12}/\text{hour} \quad \text{Fermilab}$$

$$\langle N_{\bar{p}} \rangle_{\text{Ideal}} = 7\times10^{7}\ \bar{p}/\text{sec} = 2.5\times10^{12}/\text{hour} \quad \text{CERN}$$

If these ideal yields could be reached, the number of $\bar{p}$ collected would be 10^{12} $\bar{p}$/hour (Fermilab) and 2.5×10^{11} $\bar{p}$/hour (CERN). Obviously such intense sources would lead to the possibility of high-luminosity $\bar{p}p$ storage rings.

The point of the discussion is that the ideal yields are large. Now when you go through the document of the CERN scheme or go through the numbers we come up with at Fermilab you find the actual calculated yields into our devices are considerably lower than this.[5-8]

Compare these ideal yields with the expected values of "realistic" targets and collection system at Fermilab and CERN.

$$\langle N_{\bar{p}} \rangle_{\text{Real}} = 1.2\times10^{10}/\text{hour} \quad \text{Fermilab}$$

$$\langle N_{\bar{p}} \rangle_{\text{Real}} = 3.6\times10^{10}/\text{hour} \quad \text{CERN}$$

In the first case, the ideal yield is 83 times larger and in the second case the ideal yield is 7 times larger. An interesting question is "where is the missing factor?" Part of it is almost certainly the transverse momentum acceptance of the Fermilab Booster, but a large factor seems to come from the effects of finite-size targets.

Target Efficiency and Depth of Focus of the Beam Transport

Consider a point target and, in a simple-minded way, assume that the average angular production angle of the antiproton is 30 milliradians, and the spot size is 0.1 mm. Then the emittance of the "beam" should be $3\pi\times10^{-6}$ mrad. In principle, the design acceptance of the Booster at Fermilab for 200-MeV protons is 4π versus $2\pi\times10^{-6}$.[7,8] In principle, the system would thus collect almost all the antiprotons that are available into the Booster, if you can make the spot size small enough. The problem is doing this in practice. practice.

We find empirically that $N_{\bar{p}}/N_p$ for the Fermilab system, which is the one I know best, is 2×10^{-7} compared to the theory which gives 2×10^{-5} so there is a factor of 83 between what is possible and what we expect to realize.[6,8] Some of that factor of 83 is due to the acceptance of the real Booster. We should put in the real Booster size, which is 2.65π by $1.3\pi\times10^{-6}$. There is a factor of 3 to 5 just in Booster acceptance, but there is still a factor of 20 to 30 left, which we could have gotten in principle with a point target, but which we do not get in practice. That it is sort of illustrated by calculations that George Chadwick has been doing for a tungsten target, focusing down the spot to 0.2 by 0.1 mm (Fig. 6).[6] As a function of target length, the yield into the Booster is shown in Fig. 6.[6] The target-length effect is an enormous effect. As a function of target length, there is little gain after 4 or 5 cm. (Four or five cm is 1/3 of an absorption length.) So the yield does not even peak out at one absorption length; it peaks out at 1/3 of an absorption length. We are really not using the protons wisely, let alone collecting all the antiprotons. So it seems that there is some gain to be made if we can find a way of reducing the effects of long targets. The group discussed the possibility of a better depth-of-focus system and we were very fortunate that Roy Blumberg was here, because he has been thinking about such systems for some time.[9]

It is a simple idea, at any given point, such as point B in the target, the emittance may look like the presentation in Fig. 7. But at point A at the end of the target, the emittance will be larger. So when all the points in the target are added, the phase space looks like that shown in Fig. 7. We want to match into a phase space that looks like a circle, but the finite length target blows up the emittance. One idea (I think it is probably the only credible one that I've heard so far) that might help increase the depth of focus is to use a pulsed wire.[9] This concept is just beginning. We haven't done nearly enough work on this, but it is at least very promising. The idea is to use a wire of length L and radius b with a current I through it. The field in the wire then increases like r, up to the surface, falls off like 1/r outside the surface. With a wire of 1 mm radius and 30,000 A the magnetic-field B at the surface is about 4T. Inside the wire, the focusing is like that of an ideal lens whose focal length varies with radius. We do not yet know how much of the focusing is done inside and outside the wire in the actual Monte Carlo calculations. But outside the wire, or at the surface, in order to trap particles of 300 MeV/c then BL should be about 1.6 T-m, for 300 MeV/c, a B of 4 T would give you 25 cm, so you would trap the antiprotons at a wavelength of 25 cm. In fact, a more careful calculation gives particles starting out with angles θ_o being bent straight by θ, and going straight, as given by the formula in Fig. 8.

Blumberg has done calculations (and I emphasize this is preliminary) for a target length of 35 cm and a radius of 1 mm or 0.3 mm. The angular distribution and yield versus current for the calculations are shown in Figs. 9 and 10. There is a peak in the yield as shown in Fig. 10.[9,10] The Sanford-Wang formula was used in this estimate; it is probably not entirely correct, but probably not so far off either. With no current in the wire, the yield is shown in Fig. 9. With current in the wire and viewed at the end of the wire, the particles are pushed down in angular distribution, so that the number within 20 mrad is considerably larger than it was for the zero-current case. Of course, the size of the beam is somewhat larger and we do not yet know how large it is. As a function of current, the yield peaks at 30 kA; it continues to increase, then it falls off and never increases again. This is roughly a quarter of a wavelength for the system. The particles have crossed over again at larger currents. In fact, some of them are always crossing over, because they start at different parts of the target, and therefore apparently the yield does not increase again because the particles have gone back through the wire and are being absorbed. Note that it is not necessary to go to extremely high currents to get a large effect. It is actually very simple to see how this works. In the limit that the phase space is blown up by a finite target length, but all the particles are trapped, the phase space is rotated as illustrated in Fig. 7. The magnetic field simply rotates the phase space and in principle could keep it as small as a point-like phase space.

What increases in yield might be expected over the present target systems?[7] Consider the Fermilab case, where target heating is not likely to be important. If a two absorption length target can be used, compared to one-quarter length that is used at present, an increased yield by some factor, perhaps 3 to 4, would result, so the better depth of focus immediately allows the use of a longer wire. The yield increases up to the optimal absorption length, which is perhaps two. There is probably an increased brightness or increased yield in the angular region accepted by the Booster. That could perhaps give a factor of four, but we do not know that factor yet and additional calculations are essential. There is also the possibility, according to the calculations of Blumberg, that longer wires will result in the collection of antiprotons from secondary interactions. It isn't clear how much this would give, but Blumberg believes it may give you as much as a factor of 1.5 to 2. It is probably again something that will have to be measured. If we are allowed to multiply all these factors (which we are probably not) a factor of 18 increase would result.

What is the future of the target studies? First, we need more calculations; I think we should also make a wire to test, but I think the conclusion is that better targets and focusing systems might give a factor of yield of 4 to 20, and that is certainly worth pursuing. This could lead to increased luminosity of a factor of ten in $\bar{p}p$ storage rings.

References

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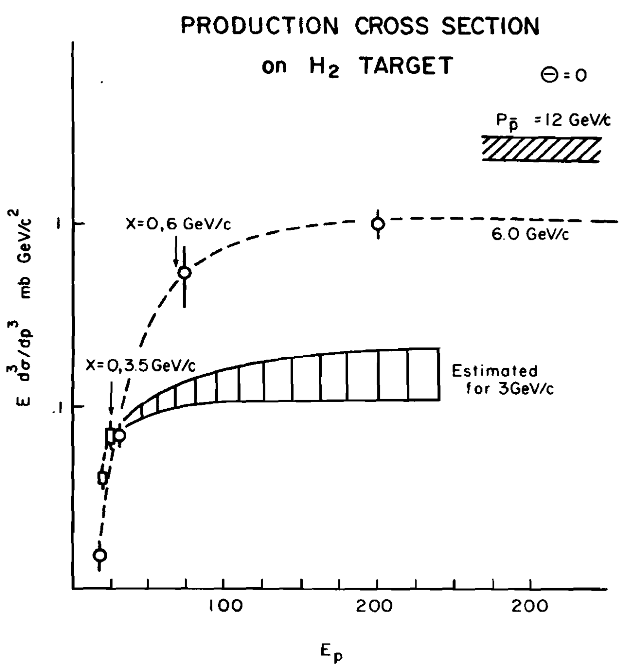

Figure 1

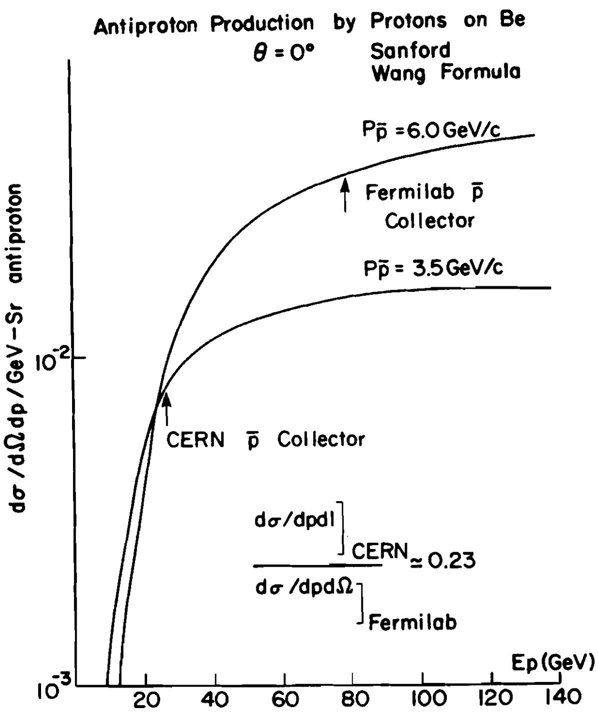

Figure 2

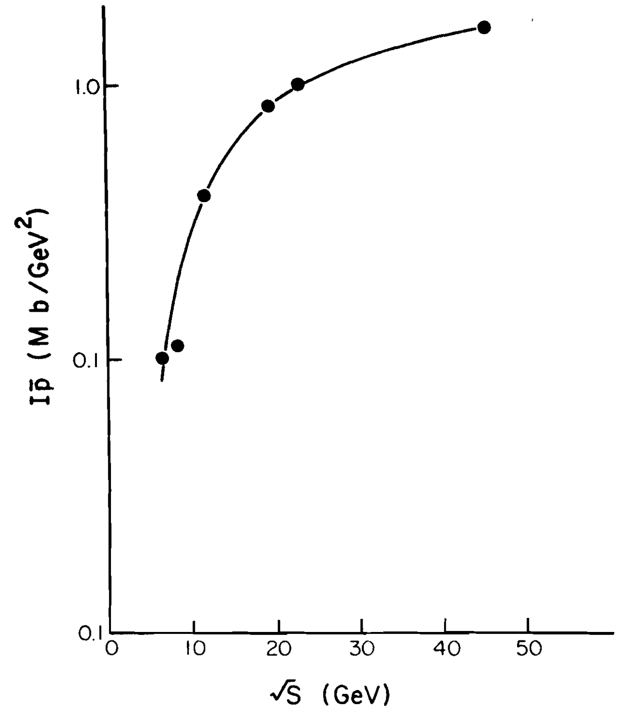

Figure 3

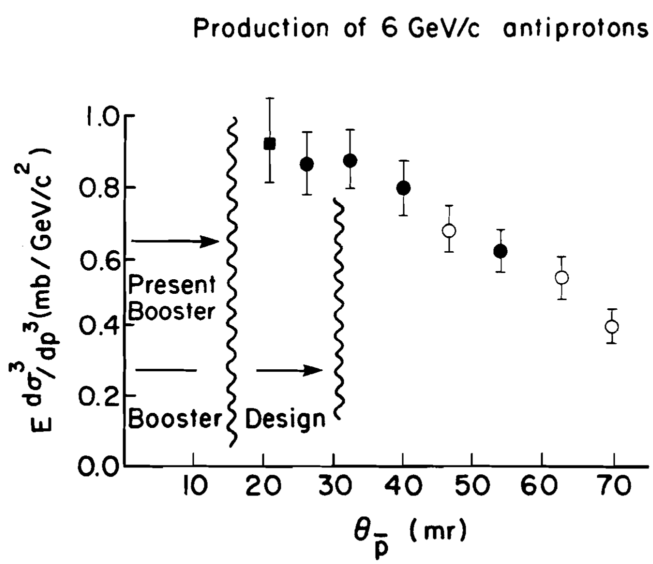

Figure 4

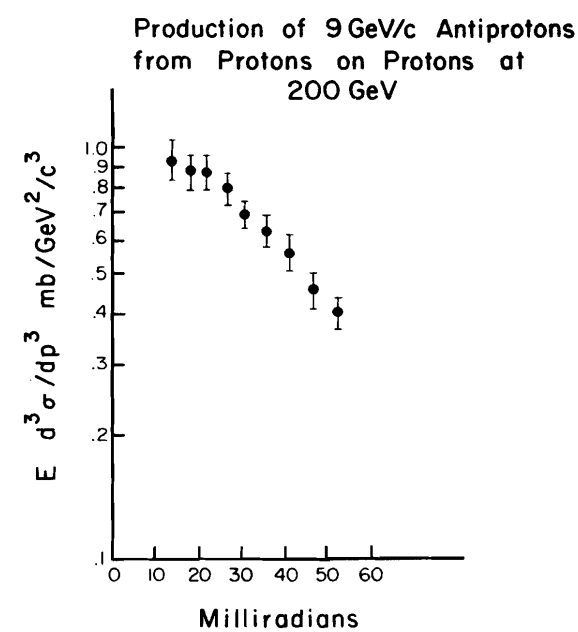

Figure 5

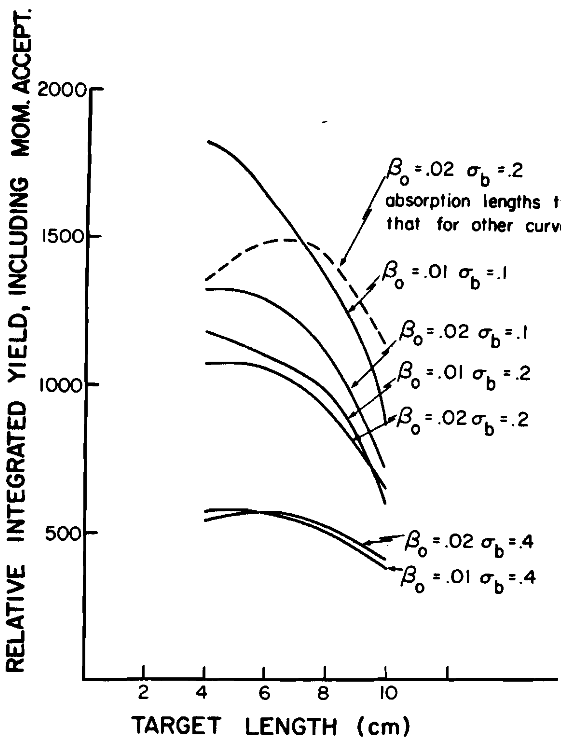

Figure 6

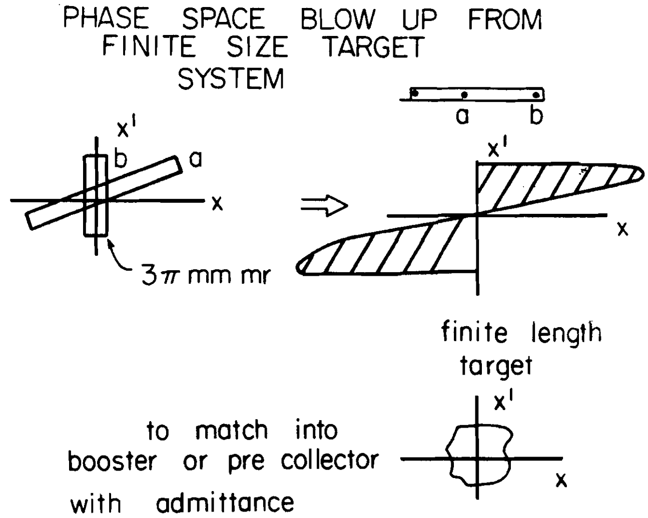

Figure 7

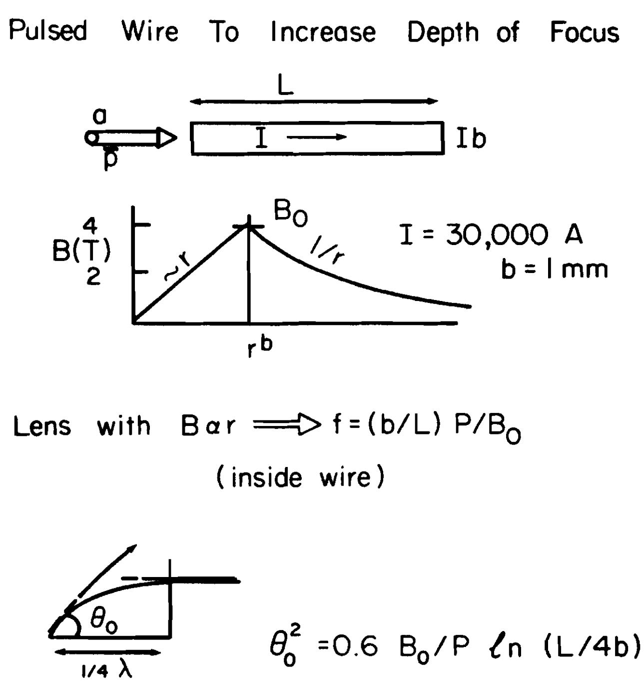

Figure 8

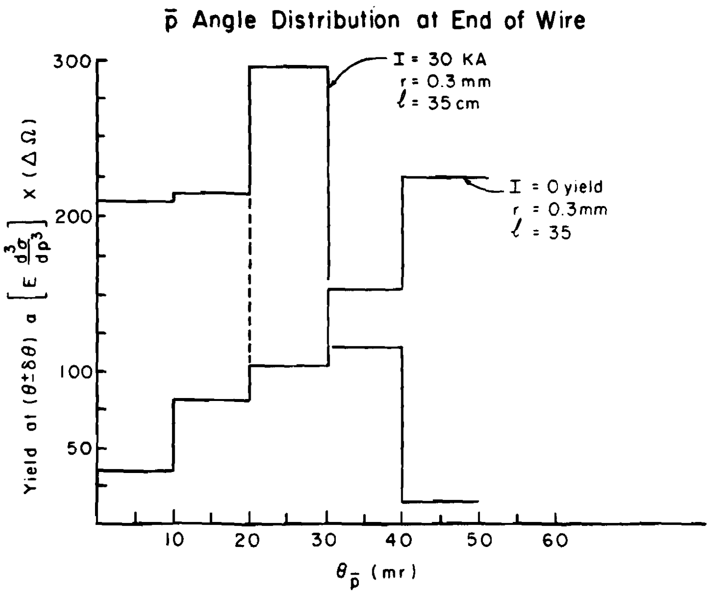

Figure 9

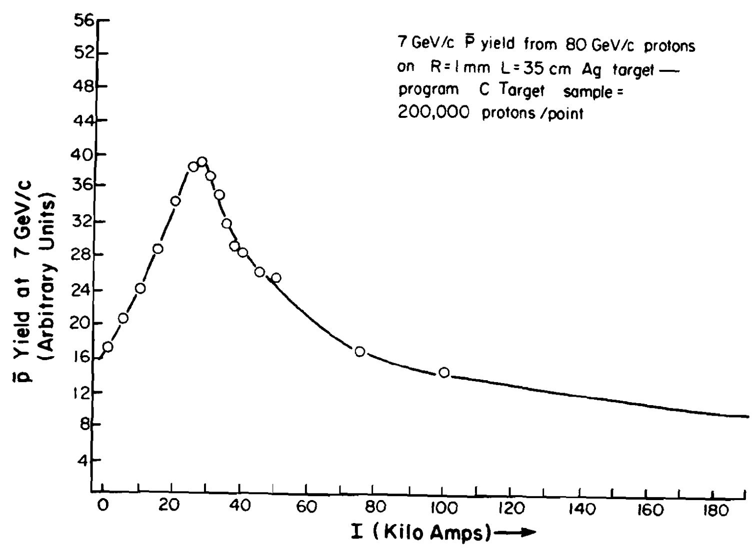

Figure 10