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Hard collisions of spinning protons: Past, present and future

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Abstract. There will be a review of the history of polarized proton beams, and a discussion of the unexpected and still unexplained large transverse spin effects found in several high-energy proton-proton spin experiments at the ZGS, AGS and Fermilab. Next, there will be a discussion of present and possible future experiments on the violent elastic collisions of polarized protons at IHEP-Protvino's 70 GeV U-70 accelerator in Russia and the new high-intensity 50 GeV J-PARC facility being built at Tokai in Japan.

PACS. 13.88.+e Polarization in interactions and scattering – 13.85.Dz Elastic scattering – 41.75.Ak Positive-ion beams

I will first discuss the violent elastic collisions of unpolarized protons. Figure 1 shows the cross-section for proton-proton elastic scattering plotted against a scaled P_t^2-variable that was proposed in 1963 [1] and 1967 [2]. This plot is from updates by Peter Hansen and me [3,4]. Notice that at small P_t^2 the cross-section drops off with a slope of about $10\,(\mathrm{GeV}/c)^{-2}$. Fourier-transforming this slope gives the size and shape of the proton-proton interaction in the diffraction peak; it is a Gaussian with a radius of about 1 fermi. At medium P_t^2 there is a component with a slope of about $3\,(\mathrm{GeV}/c)^{-2}$; however, this component disappears rapidly with increasing energy; at lab energies of a few TeV, it has totally disappeared. Thus, one can see a sharp destructive interference between the small-P_t^2 diffraction peak and the large-P_t^2 hard-scattering component. Since the diffraction peak is mostly *diffractive*, its amplitude must be mostly imaginary, as has been experimentally verified. Thus, the sharp destructive interference implies that the large-P_t^2 component is also mostly imaginary; thus, it is probably mostly *diffractive*. This large-P_t^2 component is probably the elastic *diffractive* scattering due to the *direct* interactions of the proton's constituents; its slope of about $1.5\,(\mathrm{GeV}/c)^{-2}$ implies that these *direct* interactions occur within a Gaussian-shaped region of radius about 0.3 fermi.

Since the medium-P_t^2 component disappears at high energy, it is probably the *direct* elastic scattering of the two protons. This view is supported by the experimental fact that proton-proton elastic scattering is the only exclusive process that still can be precisely measured at TeV energies. To understand this, note that *direct* elastic scattering and all other exclusive processes must compete with each other for the total p-p cross-section, which is less than 100 millibarns. At TeV energies, there are certainly more than 10^5 exclusive channels in this competition; thus, each channel has an average cross-section of less than 1 microbarn. Moreover, since the medium-P_t^2 elastic component does not interfere strongly with either the large-P_t^2 or small-P_t^2 components, its amplitude is probably real. Also note that the large-P_t^2 component intersects the cross-section axis at about 10^{-5} below the small-P_t^2 *diffractive* component.

An earlier version of fig. 1 got me started in the spin business. In 1966, we carefully measured p-p elastic scattering at the ZGS at exactly 90°_{cm} from 5 to 12 GeV [5]; the sharp slope-change, shown by the stars, was apparently the first direct evidence for constituents in the proton. Dividing these 90°_{cm} p-p elastic cross-sections by 4 (due to the protons' particle identity) made all then existing proton-proton elastic data, above a few GeV, fit on a single curve [2]. During a 1968 visit to Ann Arbor, Robert Serber informed me that, in dividing the 90°_{cm} points by 4, I had made an assumption about the ratio of the spin singlet and triplet p-p elastic-scattering amplitudes. I recall being astounded and saying that I knew nothing about spin and certainly had not measured the spin of either proton. He said with a smile that both statements might be true; nevertheless, my nice fit required this assumption. Professor Serber, as usual, spoke quietly; however, as a student, I had learned that he was almost always right. Thus, I looked for data on proton-proton elastic scattering, above a few GeV, in the singlet and triplet spin states. I found that none existed and decided to try to polarize the protons in the ZGS.

At the 1969 New York APS Meeting, I learned that EG&G was the representative for a new polarized proton ion source made by ANAC in New Zealand. I dis-

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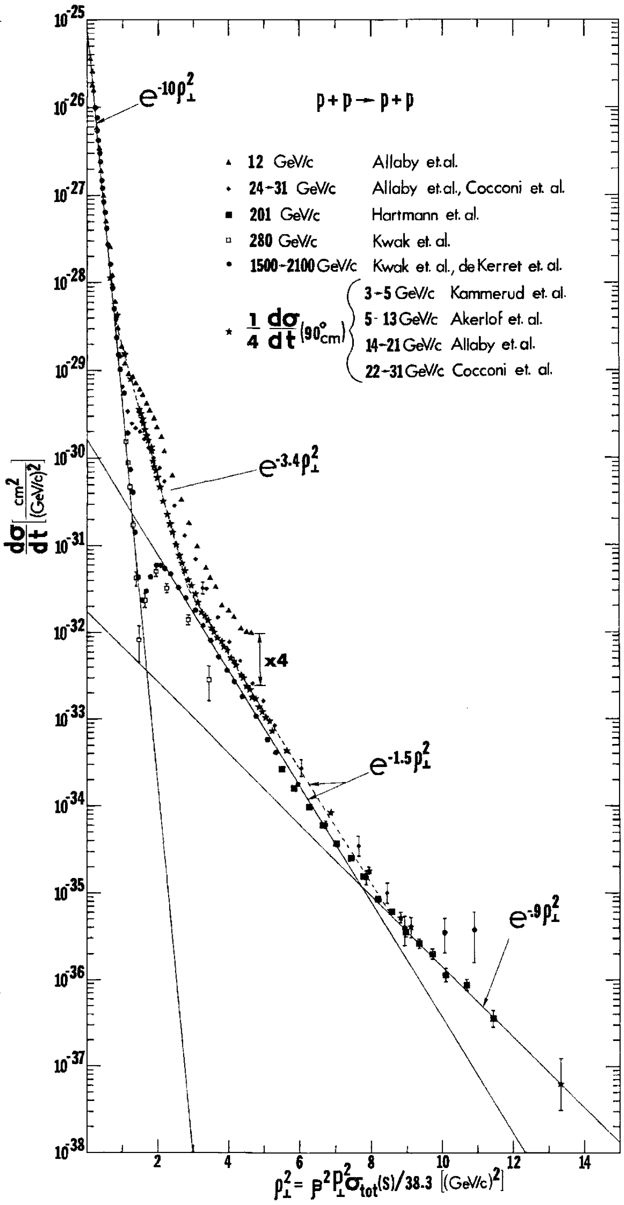

Fig. 1. Proton-proton elastic cross-sections plotted against the scaled P_t^2-variable. The 12 GeV/c points of Allaby *et al.* were not corrected for 90°_{cm} particle identity effects.

cussed this with my long-time colleague, Larry Ratner, and then with Bruce Cork, Argonne's Associate Director, and Robert Duffield, Argonne's Director. They apparently decided it was a good idea; Duffield soon hired me as a consultant to Argonne at $100 per month. In 1973, after a lot of hard work by many people, the ZGS accelerated the world's first high-energy polarized proton beam.

One needed some hardware to overcome both intrinsic and imperfection depolarizing resonances. Fortunately, both types of resonances were fairly weak at the 12 GeV ZGS, which was the highest-energy weak-focusing accelerator ever built. All higher-energy accelerators wisely use strong focusing, which makes the depolarizing resonances much stronger. If we had first tried to accelerate polarized protons at a strong-focusing accelerator, such as the AGS, we probably would have failed and abandoned the polarized proton beam business. Fortunately, it worked at the weak-focusing ZGS, and experiments [6] soon showed that the p-p total cross-section had significant spin dependence; this surprised many people, including me.

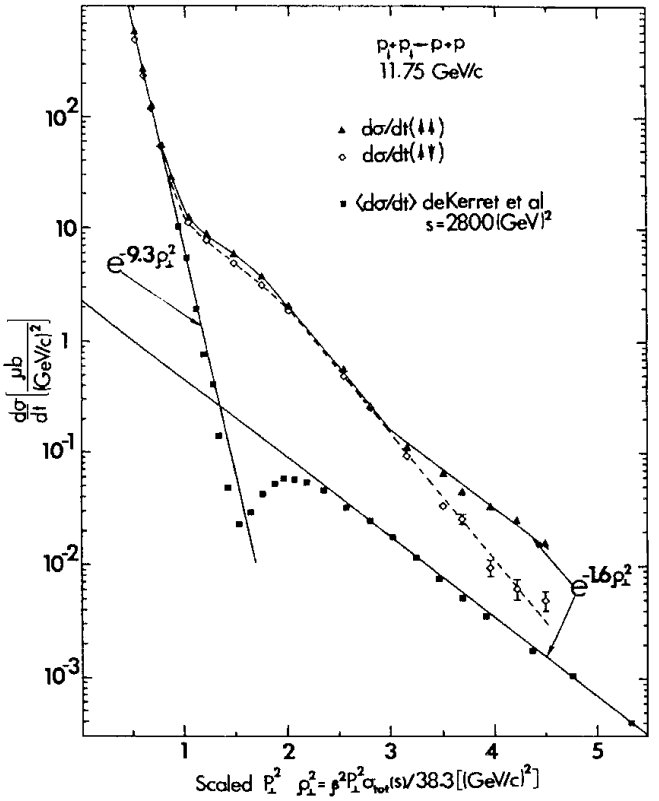

Fig. 2. The proton-proton elastic cross-section near 12 GeV in pure initial spin states is plotted against the scaled P_t^2-variable.

Figure 2 shows our perhaps most important result [7] from the ZGS polarized proton beam. The 12 GeV proton-proton elastic cross-section in pure initial spin states is plotted against the scaled P_t^2-variable; in the diffraction peak the spin-parallel and spin-antiparallel cross-sections are essentially equal to each other and to the unpolarized data from the CERN ISR at $s = 2800\,\mathrm{GeV}^2$; thus, in small-angle *diffractive* scattering, the protons in different spin states (and at different energies) all have about the same cross-section. The medium-P_t^2 component, which still exists near 12 GeV, has only a small spin dependence; again note that it has totally disappeared at $2800\,\mathrm{GeV}^2$. However, the behavior of the large-P_t^2 hard-scattering component was a great surprise. When the protons' spins are parallel, they seem to have exactly the same behavior as the much higher-energy unpolarized ISR data; however, when their spins are antiparallel their cross-section drops with the medium-P_t^2 component's steeper slope. When this data first appeared in 1977 and 1978, people were totally astounded; most had thought that spin effects would disappear at high energies. In the following years, many theoretical papers tried to explain this unexpected behav-

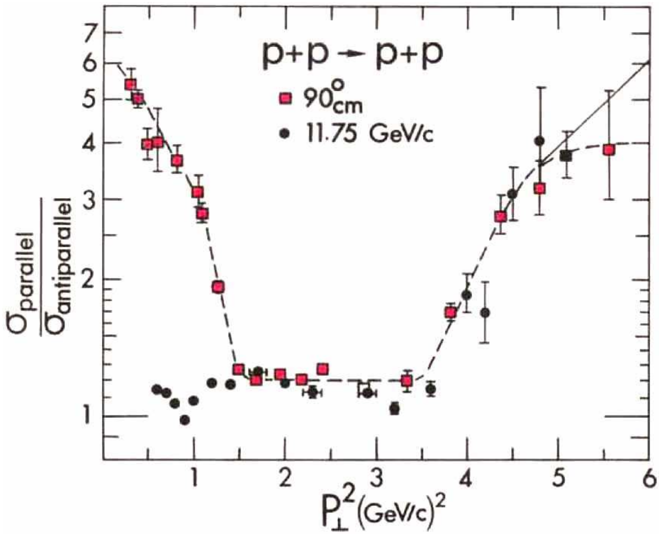

Fig. 3. The measured spins-parallel/spins-antiparallel cross-section ratio ($\sigma_{\uparrow\uparrow}/\sigma_{\uparrow\downarrow}$) is plotted against P_t^2.

ior; none were fully successful. In particular, the theory that is now called QCD, has been unable to deal with this data; Glashow once called this experiment "the thorn in the side of QCD". In his summary talk at Blois 2005, Stan Brodsky called this result "one of the unsolved mysteries of Hadronic Physics".

I learned something important from questions during two seminars about this result. Two distinguished physicists, Professor Weisskopf at CERN and then Professor Bethe at Copenhagen a week later, asked the same question, apparently independently. Each said that our big spin effect at large P_t^2 was quite interesting; but at 12 GeV, the spins-parallel/spins-antiparallel ratio was only big near 90°_{cm}, where particle identity was important for p-p scattering. They asked: how could one be sure that our large spin effect was due to hard-scattering at large P_t^2, rather than particle identity near 90°_{cm}? One would be foolish to ignore the comments of two such distinguished theorists, which were similar to Professor Serber's comment 10 years earlier.

However, it seemed that their question could not be answered theoretically; thus, we tried to answer it experimentally with a second ZGS experiment, which varied P_t^2 by holding the p-p scattering angle fixed at exactly 90°_{cm}, while varying the energy of the proton beam. This 90°_{cm} p-p elastic fixed-angle data [8] is plotted against P_t^2 in fig. 3, along with the fixed-energy data of fig. 2. There are large differences at small P_t^2, where the 90°_{cm} data are at very low energy; however, above P_t^2 of about $1.5\,(\text{GeV}/c)^2$, the two sets of data fall right on top of each other. The point at $P_t^2 = 2.5\,(\text{GeV}/c)^2$, where the ratio is near 1, is just as much at 90°_{cm}, as the $5\,(\text{GeV}/c)^2$ point, where the ratio is 4. This data apparently convinced Professors Bethe and Weisskopf that the large spin effect was not due to 90°_{cm} particle identity and was a large-P_t^2 hard-scattering effect.

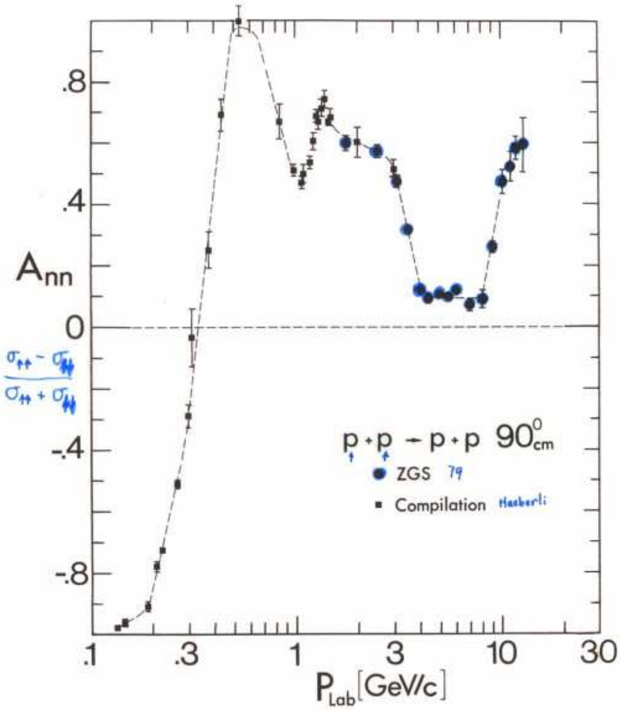

Fig. 4. $A_{nn} \equiv (\sigma_{\uparrow\uparrow} - \sigma_{\uparrow\downarrow})/(\sigma_{\uparrow\uparrow} + \sigma_{\uparrow\downarrow})$ is plotted against P_{Lab}.

Figure 4 shows A_{nn} for p-p elastic scattering [4] plotted against the lab momentum, P_{Lab}; it includes the ZGS data from fig. 3 [8] plus some lower-energy data obtained from Willy Haeberli who is an expert on low-energy p-p spin experiments. At the lowest momentum (near $T = 10\,\text{MeV}$) A_{nn} is very close to -1; thus, two protons with parallel spins can never scatter at 90°_{cm}. Next A_{nn} climbs rapidly to $+1$; then protons with antiparallel spins can never scatter at 90°_{cm}. Then at medium energy, there are some oscillations that were once thought to be due to dibaryon resonances, but are probably due to the onset of N^*-resonance production. In the ZGS region, A_{nn} first drops rapidly; it is next small and constant over a large range; it then rises rapidly to 0.6. These huge and sharp oscillations of A_{nn} are quite impressive.

I now turn to money and politics. In 1972 the AEC had agreed to shut down the ZGS in 1975 to get funding for PEP at SLAC. When the unique ZGS polarized beam started operating in 1973, the wisdom of this decision was questioned; AEC then set up a committee which extended ZGS operations through 1977. A second committee was set up in 1976; it extended operations of the unique ZGS polarized beam until 1979 [9]. Henry Bohm, the President of AUA (which operated Argonne), asked ERDA (was AEC) to set up a third committee to again extend ZGS running. But OMB objected, so there was no third committee; however, this effort had some benefit. When James Kane, of ERDA, responded negatively to Bohm, his justification for this was that it might now be possible to accelerate polarized protons in a strong-focusing accelerator, such as the AGS; he copied me on this letter.

We had also started interacting with Ernest Courant and others at Brookhaven about polarizing the AGS: first at a 1977 Workshop in Ann Arbor [10] and then at a 1978 Workshop at Brookhaven [11]. When he learned of Kane's letter, Brookhaven's Associate Director, Ronald Rau, asked for a copy of it. With this letter, he convinced William Wallenmeyer, the long-time Director of High Energy Physics at AEC, ERDA and DoE, to provide about $8 Million to Brookhaven, and about $2 Million split between Michigan, Argonne, Rice and Yale, for the challenging project of accelerating polarized protons in the strong-focusing AGS, and later in the 400 GeV ISABELLE collider, which was canceled, but then reborn as RHIC.

It was far more difficult to accelerate polarized protons in the strong-focusing AGS than in the weak-focusing ZGS. The strong-focusing principal, invented by Courant, Livingston and Snyder [12], made possible all modern large circular accelerators by using alternating quadrupole magnetic fields to strongly focus the beam and thus keep it small. Unfortunately, these strong quadrupole fields were very good at depolarizing protons. To accelerate polarized protons to 22 GeV at the AGS, one had to overcome 45 strong depolarizing resonances. This required: building lots of challenging hardware; significantly upgrading the AGS controls; and spending lots of time individually overcoming the 45 depolarizing resonances. Michigan built the 12 ferrite quadrupole magnets that were installed in the AGS to overcome its 6 intrinsic resonances by rapidly jumping the AGS's vertical betatron tune through each resonance. Brookhaven was building their 12 power supplies; each power supply had to provide 1500 amps at 15000 volts (about 22 MW) during each quadrupole's 1.6 μs rise time. Overcoming the many imperfection depolarizing resonances (occurring every 520 MeV) required programming the AGS's 96 small correction dipole magnets to form a horizontal B-field wave of 4 oscillations at the instant when the proton energy passed through $G\gamma = 4$; then, about 20 ms later in the AGS cycle, when $G\gamma$ was 5, the 96 magnets had to form a horizontal B-field wave with 5 oscillations, etc. ($G = 1.79285$ is the proton's anomalous magnetic moment, while $\gamma = E/m$.).

After all this hardware was installed, an even larger problem was tuning the AGS. In 1988, when we accelerated polarized protons to 22 GeV, we needed 7 weeks of exclusive use of the AGS; this was difficult and expensive. Once a week, Nicholas Samios, Brookhaven's Director, would visit the AGS Control Room to politely ask me how long the tuning would continue and to remind us that it was costing $1 million a week. Moreover, it was soon clear that, except for Larry Ratner (then at Brookhaven) and me, no one could tune through these many resonances; thus, for some weeks, Larry and I worked 12-hour shifts 7 days each week. Larry was older than me; after 5 weeks he collapsed. While I was younger than Larry, I thought it unwise to try to work 24-hour shifts every day. Thus, I asked our Postdoc, Thomas Roser, who until then had worked mostly on polarized targets and scattering experiments, if he wanted to learn accelerator physics in a hands-on way for 12 hours every day. Thomas apparently learned well; he now leads Brookhaven's Accelerator-Collider Division.

One benefit from this difficult 7-week period [13] was learning that our method of individually overcoming each resonance, which had worked so well at the ZGS, might work at the AGS, but would not be practical at higher-energy accelerators. This lesson helped to launch our Siberian-snake programs at IUCF [14] and then SSC [15,16].

In the 1980s, a new proton collider, the SSC, was being planned; it was to have two 20 TeV proton rings each about 80 km in circumference. Owen Chamberlain and Ernest Courant encouraged me to form a collaboration to insure that polarized protons would be possible in this new Collider. We first organized a 1985 workshop in Ann Arbor, with Kent Terwilliger. This workshop [15] concluded that it should be possible to accelerate and maintain the polarization of 20 TeV protons in the SSC, but only if the new Siberian-snake concept of Derbenev and Kondratenko [17] really worked; otherwise, it would be totally impractical. Recall that it took 49 days to correct the 45 depolarizing resonances at the AGS —about one a day. Each 20 TeV SSC ring would have about 36000 depolarizing resonances to correct. These higher-energy resonances would be much stronger and harder to correct; but even at one per day, this would require about 100 years of tuning for each ring. The workshop also concluded that one must prove experimentally that the *too-good-to-be-true* Siberian snakes really worked; otherwise, there would be no approval to install the 26 Siberian snakes needed in each SSC ring.

Fortunately, Indiana's IUCF was then building a new 500 MeV synchrotron Cooler Ring. Some of us workshop participants then collaborated with Robert Pollock and others at IUCF to build and test the world's first Siberian snake in the Cooler Ring. We brought experience with synchrotrons and high-energy polarized beams, while the IUCF people brought experience with low-energy polarized beams and the CE-01 detector, which was our polarimeter. In 1989, we demonstrated that a Siberian snake could easily overcome a strong imperfection depolarizing resonance [14]. For 13 years we continued these experiments and learned many things about spin-manipulating polarized beams; after the Cooler Ring shut down in 2002, this program was continued at the 3 GeV COSY in Juelich.

In 1990 we formed the SPIN Collaboration and submitted Expression of Interest EOI001 to SSC: to accelerate and store polarized protons at 20 TeV, and to study spin effects in 20 TeV p-p collisions. It was submitted a week before the deadline, which made it SSC EOI001 [16]. Thus, we made the first presentation to the SSC PAC before a huge audience that included many newspaper reporters and TV cameras. Perhaps partly because of this publicity, we were soon *partly* approved by SSC Director Roy Schwitters. By *partly* I mean that he decided to add 26 empty spaces for Siberian snakes in each SSC Ring; each space was about 20 m long, which added about 0.5 km in each Ring. Unfortunately, the SSC was canceled around 1993, before it was finished, but after $2.5 billion was spent. Nevertheless, our detailed studies of the behavior and spin-manipulation of polarized protons at IUCF and COSY helped in developing polarized beams around the world: Brookhaven now has 250 GeV polarized protons in each RHIC ring [18]; perhaps someday CERN's 7 TeV LHC might have polarized protons.

We eventually accelerated polarized protons to 22 GeV in the AGS [13] and obtained some A_{nn} data [19] well above the ZGS energy of 12 GeV; but we never had enough beam time to get precise A_{nn} data at high-P_t^2. However, during tune-up runs for the A_{nn} experiment, we used the unpolarized AGS proton beam to test our polarized proton target and double-arm magnetic spectrometer by measuring A_n in 28 GeV proton-proton elastic scattering; this data resulted in an interesting surprise. Despite QCD's inability to explain the big A_{nn} from the ZGS, our QCD friends had made a firm prediction that the one-spin A_n must go to 0; moreover, this prediction would become more firm at higher energies and in more violent collisions. But above $P_t^2 = 3\,(\mathrm{GeV}/c)^2$, A_n instead began to deviate from 0 and was quite large at $P_t^2 = 6\,(\mathrm{GeV}/c)^2$. This led to more controversy [20]; some QCD supporters said that our A_n data must be wrong.

Experimenters take such accusations seriously. Thus, we started preparing an experiment that could study A_n at high-P_t^2 with better precision. Our spectrometer worked well, but we could only use about 0.1% of the AGS beam intensity, because a higher-intensity beam would heat our Polarized Proton Target (PPT) and depolarize it. Thus, we started building a new PPT that could operate with 20 times more beam intensity; this required ^{4}He evaporation cooling at 1 K, which has much more cooling power than our earlier ^{3}He evaporation PPT at 0.5 K. However, to maintain a target polarization near 50% at 1 K, the PPT model required increasing the B-field from 2.5 to 5 tesla. Thus, we ordered a high-quality 5 T superconducting magnet from Oxford Instruments, with a B-field uniformity of a few 10^{-5} over the PPT's 3 cm diameter volume. We also obtained a Varian 20 W at 140 GHz Extended Interaction Oscillator; it was apparently the highest-power 140 GHz microwave source available. As the PPT assembly started in 1989, we worried that, if the PPT model was wrong, the polarization might be only 10%; instead, we were very lucky; it was 96% [21].

Moreover, the target polarization averaged 85% for a 3-month-long run with high-intensity AGS beam in early 1990. As shown in fig. 5, this let us precisely measure A_n at even larger P_t^2. When these precise new data were published [22], some theorists seemed quite unhappy; they still believed the QCD prediction that A_n must go to 0, but they now refused to state at what P_t^2 or energy this prediction would become valid. They also now said that QCD might not work for elastic scattering, which they now considered less fundamental than inelastic scattering, where they said QCD should work. Thus, one result of our experiments was to make both elastic-scattering experiments and spin experiments unpopular in some circles.

Other experimenters started doing inclusive polarization experiments, especially at Fermilab. Figure 6 shows

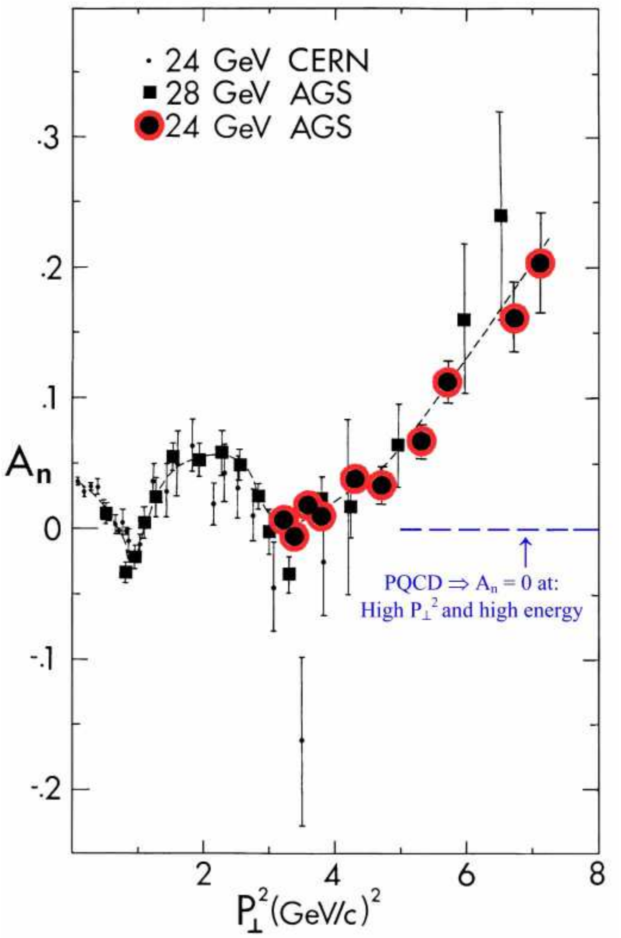

Fig. 5. $A_n \equiv (\sigma_\uparrow - \sigma_\downarrow)/(\sigma_\uparrow + \sigma_\downarrow)$ is plotted against P_t^2 for p-p elastic scattering.

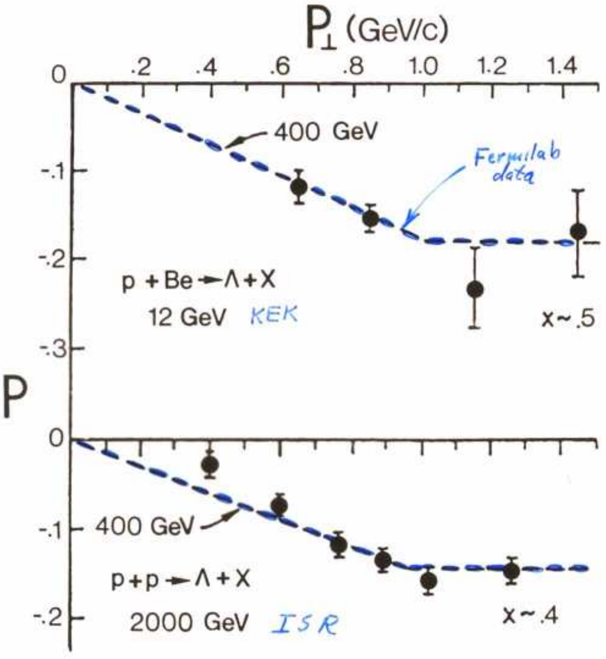

Fig. 6. The inclusive Λ polarization is plotted against P_t.

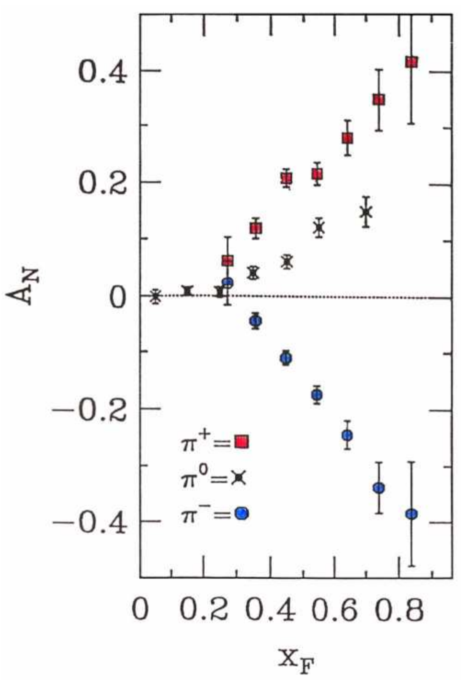

Fig. 7. A_n for inclusive π-meson production is plotted *vs.* X_F.

the 400 GeV inclusive hyperon polarization experiments from the 1970s and 1980s, led by Pondrum, Devlin, Heller and Bunce [23]; it clearly shows a small polarization at small P_t and a larger polarization at larger P_t. Moreover, their data is consistent with 12 GeV data from the KEK PS and with 2000 GeV data from the CERN ISR. These data do not support the QCD prediction that inelastic spin effects disappear at high energy or high P_t^2.

Another group at Fermilab, led by Yokosawa, developed a polarized secondary beam using the polarized protons from polarized hyperon decay. The beam's intensity was only about 10^5 per second, but its polarization was about 50% and its energy was about 200 GeV. They obtained some nice A_n data on inclusive π-meson production [24], which are shown in fig. 7. The A_n values for π^+ and π^- mesons are both large but with opposite signs, while A_n for the π^0 data is 50% smaller and is positive. These 200 GeV data provide little support for QCD.

We tried to measure spin effects in very-high-energy p-p scattering at UNK, which IHEP-Protvino started building around 1986; IHEP and Michigan signed the NEPTUN-A Agreement in 1989. Michigan's main contribution was a 12 tesla at 0.16 K Ultra-cold Spin-polarized Jet. UNK's circumference would be 21 km with 3 rings: a 400 GeV warm ring and two 3 TeV superconducting rings; its injector was IHEP's existing 70 GeV accelerator, U-70. By 1998 the UNK tunnel and about 80% of its 2200 warm magnets were finished, and 70 GeV protons were transferred into its tunnel with 99% efficiency. However, progress became slower each year due to financial problems; in 1998 Russia's MINATOM placed UNK on long-term standby.

IHEP Director, A.A. Logunov, had earlier suggested moving our experiment to IHEP's existing 70 GeV U-70 accelerator. By March 2002 the resulting SPIN@U-70 Experiment on 70 GeV p-p elastic scattering at high P_t^2 was fully installed, except for our detectors and Polarized Proton Target (PPT). However, just before our 4 tons of detectors, electronics and computers were to be shipped, the US Government suspended the US-Russian Peaceful Use of Atomic Energy Agreement started by President Eisenhower in 1953. Nevertheless, DoE asked us to send the shipment, since under the terms of the PUOAE Agreement, the experiment could be done exactly as planned. DoE faxed us a copy of the Agreement; thus, we sent the shipment; it arrived at Moscow airport on March 11, 2002. However, Russian Customs impounded it for 8 months before returning it to Michigan.

Despite this problem, we remain friends with our IHEP colleagues and there have been four SPIN@U-70 test runs using Russian detectors and an unpolarized target; we participated in the November 2001 and April 2002 runs. We hope that the US-Russian PUOAE Agreement is soon restarted so that the SPIN@U-70 experiment can continue and measure A_n at P_t^2 near $12\,(\mathrm{GeV}/c)^{-2}$.

However, over 4 years have now passed. If this international problem, involving the Iranian reactor, continues much longer, we may try to do a similar experiment at Japan's new very-high-intensity 50 GeV proton accelerator, J-PARC, which should run in 2008. If J-PARC can accelerate polarized protons to 50 GeV, then one could study the large and mysterious elastic spin effects in both A_n and A_{nn} for the first time in decades [25].

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