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.

To introduce the violent collisions of spinning protons, Fig. 1 shows the proton-proton elastic scattering cross section plotted against a scaled $P_t^2$-variable that was proposed in 1963 and 1967; this plot is from updates by Peter Hansen and me. Notice that at small $P_t^2$ the cross-section drops off with a slope of about 10 $(\text{GeV}/c)^{-2}$. Fourier transforming this slope gives the shape and size 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 $(\text{GeV}/c)^{-2}$; 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 $(\text{GeV}/c)^{-2}$ 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 about $10^{-5}$ below the small-$P_t^2$ diffractive component.

Figure 1: Proton-proton elastic cross-sections plotted against the scaled $P_t^2$ variable. The 12 GeV points of Allaby et al. were not corrected for $90_{\text{cm}}$ particle identity effects.

Figure 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 3: The measured spins-parallel/spins-antiparallel ratio is plotted against $P_t^2$. 

Figure 4: The proton-proton elastic cross-sections plotted against the scaled $P_t^2$ variable. The 12 GeV points of Allaby et al. were not corrected for $90_{\text{cm}}$ particle identity effects.
An earlier version of Fig. 1 got me started in the spin business. In 1966, we had carefully measured p-p elastic scattering at the ZGS at exactly 90° cm from 5-12 GeV [8]. 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. Prof. 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 discussed 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 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; moreover, early experiments showed that the p-p total cross-section had significant spin dependence; this greatly surprised many people, including me.

Figure 2 shows our perhaps most important result 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 $s = 2800 \text{ GeV}^2$ unpolarized cross-sections from the CERN ISR; 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 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 years following, many theoretical papers tried to explain this unexpected behavior; 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 this meeting, 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, Prof. Weisskopf at CERN and then Prof. 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 our 12 GeV energy, 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 we be sure that our large spin effect was due to hard-scattering at large-$P_t^2$,
rather than particle identity near \(90^\circ_{cm}\)? One would be foolish to ignore the comments of two such distinguished theorists, which were similar to Prof. 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 experiment at the ZGS. We varied \(P_t^2\) by holding the p-p scattering angle fixed at exactly \(90^\circ_{cm}\), while varying the energy of the proton beam. This \(90^\circ_{cm}\) 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 \(90^\circ_{cm}\) 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 \(90^\circ_{cm}\), as the 5 \((\text{GeV}/c)^2\) point, where the ratio is 4. After seeing this data, Profs. Bethe and Weisskopf each agreed that the large spin effect is not due to \(90^\circ_{cm}\) particle identity; thus, it is almost certainly a large-\(P_t^2\) hard-scattering effect.

Figure 4 shows \(A_{nn}\) for p-p elastic scattering\(^8\) plotted against the lab momentum, \(P_{Lab}\); it includes the ZGS data from Fig. 3 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 \(T = 10\) MeV) \(A_{nn}\) is very close to \(-1\); thus, two protons with parallel spins can never scatter at \(90^\circ_{cm}\). Next \(A_{nn}\) climbs rapidly to \(+1\); then protons with antiparallel spins can never scatter at \(90^\circ_{cm}\). 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 from OMB for PEP at SLAC. When the 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\(^9\). I later asked Henry Bohm, the President of AUA (which operated Argonne), to ask ERDA (was AEC) to set up a third committee to perhaps again extend ZGS operations. But OMB objected strongly, and there was no third committee; however, this effort had some benefit. When ERDA official, James Kane, responded negatively to Bohm, he said that one justification was that it might no longer be impossible to accelerate polarized protons in a strong focusing accelerator like the AGS; he copied me on his letter.

We had also started interacting with Ernest Courant and others at Brookhaven about polarizing the AGS: first at a Workshop in Ann Arbor in 1977\(^10\), and then at a Workshop at Brookhaven in 1978\(^11\). When I mentioned Kane’s letter to Brookhaven’s Associate Director, Ronald Rau, he politely asked if he could have a copy of it. With this letter, he convinced William Wallenmeyer, the longtime 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 perhaps later in the 400 GeV ISABELLE collider).

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 the 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 15,000 Volts.
(about 22 MW) during each quadrupole’s 1.6-microsec risetime. 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 msec later in the AGS cycle, when \( G\gamma = 5 \), 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 1978, 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 only on our polarized targets and scattering experiment, if he wanted to learn accelerator physics in a hands-on way for 12 hours every day. Apparently, he learned well; Roser is now head of Accelerator and Collider Operations at Brookhaven.

One benefit from this difficult 7-week period was learning that our method of individually overcoming each resonance, which had worked so well at the ZGS, might somehow work at the AGS, but certainly would be impractical at higher energy accelerators. This lesson helped to launch our Siberian snake programs at IUCF and then SSC.

In the 1980’s, 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 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 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 36,000 depolarizing resonance to correct; the higher energy resonances would be much stronger and harder to correct, but even at one day each, this would require about 100 years of tuning time 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. For 13 years we continued these experiments and learned many things about spin-manipulating polarized beams; this program now continues at the 3 GeV COSY in Julich.

In 1990 we formed the SPIN Collaboration and submitted Expression of Interest EIO001 to SSC: to accelerate and store polarized protons at 20 TeV; and to study spin effects in 20 TeV p-p collisions. This proposal was submitted a week before the deadline, which made it SSC EIO001; 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,
thus, this was an extra 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 200 GeV polarized protons in the RHIC collider; perhaps someday the 7 TeV LHC at CERN might have polarized protons.

We eventually accelerated polarized protons to 22 GeV in the AGS and obtained some data on $A_{nn}$ 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 a pleasant 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; this prediction would become more firm at higher energies and in more violent collisions. But above $P_t^2 = 3 \text{ (GeV/c)}^2$, $A_n$ instead began to deviate from 0 and was quite large at $P_t^2 = 6 \text{ (GeV/c)}^2$. This led to more controversy; 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 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, I worried that, if the PPT model was wrong, we might have only perhaps 10% polarization. Instead, the polarization was much larger than predicted; it was 96%.

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 this precise new data was published, 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 elastic scattering experiments and spin experiments unpopular in some circles.

But other experimenters started doing inclusive polarization experiments, especially at Fermilab. Figure 6 shows a 400 GeV inclusive hyperon polarization experiment from the 1980’s, led by Pondrum, Devlin, Heller and Bunce; 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. All this data does 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 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 $\pi$ meson production, which is shown in Fig. 7. The $A_n$ values for $\pi^+$ and $\pi^-$ mesons are both large but with opposite signs, while $A_n$ for the $\pi^0$ data is 50% smaller and is positive. This 200 GeV data provides 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
Figure 4: $A_{nn}$ is plotted against $P_{Lab}$.

Figure 5: $A_n$ is plotted against $P_t^2$.

Figure 6: The $\Lambda$ polarization is plotted against $P_t^2$.

Figure 7: $A_n$ is plotted against $X_F$ for inclusive $\pi$-meson production.
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; thus, in 1998 Russia’s MINATOM placed UNK on long-term standby status.

IHEP Director, A. A. Logunov, had earlier suggested moving our experiment to the existing U-70. 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; Russian Customs impounded it for 8 months before returning it to Michigan.

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 (GeV/c)^{-2}. But if this unusual problem continues, we may try to do a similar experiment at 50 GeV at Japan’s new very-high-intensity J-PARC. I will end here; thank you.

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