INELASTIC SUM RULES

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Abstract

The history and present status of several sum rules for deep-inelastic lepton scattering are reviewed, with particular attention to the discovery of scaling, partons, quarks and QCD. Two outstanding issues are then discussed in more detail: the singlet (Ellis-Jaffe) nucleon spin sum rule and the Drell-Hearn-Gerasimov-Iddings sum rule.

1   Introduction

It is a great pleasure to be here today to honour Sid Drell, and a great honour for me to have been invited to speak. I first heard of Sid in October 1964 when, as a beginning 21-year-old graduate student, I went to see my supervisor Dick Dalitz for the first time. Dick told me that he would like me to learn to calculate as quickly as possible, adding *'There is a new book by Bjorken and Drell: I have not seen it yet myself, but those characters would write a good book'*. He was absolutely right, and it was my bible for many years to come.

I first met Sid in September 1970 when I arrived here from CERN as a postdoc. I had the office next to his (which is the office occupied today by David Leith). Sid operated an open-door policy, and everyone was allowed to gather in his office, attracted by his loud voice, and participate in any discussion that happened to be going on. All of us postdocs and students learned a great deal of physics from those discussions, and we were also able to participate vicariously in the non-secret parts of Sid's Washington life.

Towards the end of my two years at SLAC, Sid went away for a short sabbatical and he and Harriet kindly allowed us to move into their house. This experience gave me a lot of insight into the Drell family and into their three characteristically named cats — Harvard, Princeton and Yale.

Subsequently, our paths crossed many times, for example when Sid came on sabbatical to Oxford. Most recently our world lines became intertangled through the HEPAP Panel (the so-called Drell Panel) that Sid chaired in 1994, which helped start what I hope is the dawn of a new age of global collaboration in particle physics. On behalf of all those who are involved with the LHC, in over 40 countries including the 19 Member States and the USA, I would like to take this opportunity to thank Sid for the statesman-like report that his Panel produced.

The first part of this talk will comprise a historical overview, together with a comparison with recent data, of various sum rules, with emphasis on the crucial role that they played in the discovery of the scaling phenomena, partons, quarks and eventually QCD. The bona fide sum rules are built into the parton distributions that are used nowadays to

fit the data, so we can anticipate straightaway from the consistency of the fits that these sum rules all work.

In the second part of my talk, I shall deal with two outstanding issues: the singlet (Ellis–Jaffe) nucleon spin sum rule and the Drell–Hearn–Gerasimov–Iddings sum rule, and the connection between these sum rules. Except in the final section, there will be little explicit mention of Sid's work in this talk (it is discussed more extensively in the preceding talk by Bob Jaffe and the following talk by Tung-Mo Yan), but his virtual presence pervades what I have to say, and — I believe — is strongly implicit in everything that has come from SLAC, experimental as well as theoretical.

2   Historical Overview

I would like to start by picking up on some remarks of Bob Jaffe about the climate in the mid-1960s. At that time, electron and photon interactions were a minority interest, and it was widely believed that the way ahead was to study proton collisions. Indeed, Sid's realization that SLAC would be a good source of pion beams was an important factor in legitimizing the construction of SLAC in the eyes of many people. There was almost no interest in inelastic lepton scattering, and only one paper was submitted on this subject at the 1967 SLAC Electron–Photon Conference. As Bob Jaffe has recalled, the focus was on nuclear democracy and bootstraps, and quarks were generally considered to be (at best) a heuristic tool. Chew's statement, in 1961, that *'I believe the conventional association of fields with strongly interacting particles to be empty. I do not have firm convictions about leptons or photons ... field theory ..., like an old soldier, is destined not to die but just to fade away.'* was, I believe, widely accepted.

My story begins in 1965 with the Adler sum rule [1] (although it could have started in 1964 with the Adler–Weissberger relation, between g_A and $\sigma_{\pi N}$, which can be derived from the Adler sum rule by taking the limit $q^2 \to 0$ and using Adler's PCAC forward neutrino theorem). In modern notation, which was not used at the time, the Adler sum rule reads:

$$\int_0^1 \left(F_2^{\nu n}(x, q^2) - F_2^{\nu p}(x, q^2) \right) \frac{dx}{x} = 2 \ . \tag{1}$$

This sum rule can be, and was, also written in the form:

$$\lim_{E \to \infty} \frac{d\sigma^{\nu n}}{dq^2} - \frac{d\sigma^{\nu p}}{dq^2} = \frac{G^2}{\pi} \ . \tag{2}$$

I will return to the parton interpretation of these results (in terms of the difference of the number of isospin-up and isospin-down constituents in the nucleon) later, but I would like to emphasize that it is *not* a parton sum rule, and is indeed exact for all q^2 (it can be derived by integrating the component W_{00} of the deep inelastic scattering tensor $W_{\mu\nu}$ over q_0, taking the limit $|\vec{p}| \to \infty$ and assuming that this limit can be exchanged with the integral, or alternatively by other manipulations which depend on assuming an unsubtracted dispersion relation for the structure function W_2). The forms (1) and (2) are highly suggestive of scaling and of scattering off point-like constituents in the nucleon, as was — uniquely — recognized by Bjorken.

Bj's first contribution to the subject was in his classic 1966 paper [2], to which Bob Jaffe has referred, in which — among many other things — he derived the polarization sum rule:

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$$\int \left(g_1^p(x, q^2) - g_1^n(x, q^2) \right) dx = \frac{1}{6} \left| \frac{g_A}{g_V} \right| \left(1 - \frac{\alpha_s}{\pi} \right) , \tag{3}$$

where I have included the leading-order QCD correction (which obviously was not known at the time, but is now known up to and including order of α_s^3). I will discuss the parton interpretation and derivation of this equation later. It was derived at the time by Bj by taking the 'Bj limit' $q_0 \rightarrow i\infty$ followed by the limit $|\vec{p}| \rightarrow \infty$. This procedure was later used by Bj to 'derive' scaling. Assuming scaling, it leads to the generic Cornwall–Norton sum rules [3], of which one example is:

$$2 \int F_1^\pm \frac{d\omega}{\omega^{n+2}} = \lim_{P_0 \rightarrow \infty} i \left(\frac{i}{2P_0} \right)^{n+1} \int \langle p| \left[\frac{\partial^n J^+}{\partial t^n}(\vec{x}, 0), \, J^-(0) \right] |p\rangle d^3x \tag{4}$$

and similar sum rules for the other structure functions. These generic sum rules were used to derive most of the specific sum rules discussed below.

In his 1966 paper, Bj dismissed his sum rule for g_1 as *'worthless'*, but it has now been tested to great accuracy. The latest SMC analysis [4] of the Bj sum rule, carried out at $q^2 = 5$ GeV^2 and including next-to-leading-order QCD corrections, implies:

$$\frac{g_A}{g_V} = 1.15 \pm 0.03^{+0.07}_{-0.06}{}^{+0.14}_{-0.04} ,$$

where the last error is theoretical, compared to the Particle Data Group value of 1.2601 ± 0.0025.

From his *'worthless'* sum rule Bj salvaged an inequality for unpolarized scattering which led to the conclusion that *'inelastic scattering is large ... comparable to scattering off point-like charges'*. In his 1966 paper, Bj used the same techniques to derive the fact that the electron–positron annihilation cross section should scale, and stated that

'The idea that the total hadronic yield from colliding $[e^+e^-]$ beams should be approximately the same as the $\mu^+\mu^-$ yield is folklore[33]. *33) B. Richter (private communication).'* Pretty select folk (perhaps only Bj and Richter!), and I believe that the prevalent view was that the cross section should vanish rapidly, damped by form factors. In the same paper, Bj also argued that the total neutrino cross section should rise linearly with energy.

1966 also witnessed the publication of the Drell–Hearn sum rule [5]. It had in fact been discovered independently by Gerasimov and published [6] in Russian in 1965, but the English translation only appeared in 1966. It also appears in a 1965 Physical Review paper by Iddings on the nucleon polarizability contribution to the hyperfine structure of hydrogen [7], but Iddings did not suggest testing the sum rule or discuss its implications. I will return to this sum rule at the end of this talk.

The most important contribution in 1967 was, in my opinion, made by Bj at the SLAC Electron–Photon Conference [8] (see also his Varenna Summer School lectures [9]). In his talk, Bj developed a description of the Adler sum rule in terms of incoherent scattering off point-like constituents. After quoting Eq. (2) above, Bj stated that *'This result would also be true were the nucleon a point-like object, because the derivation is a general derivation. Therefore the difference of these two cross sections is a point-like cross section, and it is big'*. He goes on to suggest an interpretation, as follows: *'We assume that the nucleon is built of some kind of point-like constituents which could be seen if you could really look at it instantaneously in time ... If we go to very large energy and large q^2 ... we can expect that the scattering will be incoherent from these point-like constituents.*

Suppose ... these point-like constituents had isospin one-half ... what the sum rule says is simply $[N \uparrow] - [N \downarrow] = 1$ *for any configuration of constituents in the proton. This gives a very simple-minded picture of this process which may look a little better if you really look at it, say, in the centre-of-mass of the lepton and the incoming photon. In this frame the proton is ... contracted into a very thin pancake and the lepton scatters essentially instantaneously in time from it in the high energy limit. Furthermore the proper motion of any of the constituents inside the hadron is slowed down by time deletion. Provided one doesn't observe too carefully the final energy of the lepton to avoid trouble with the uncertainty principle, this process looks qualitatively like a good measurement of the instantaneous distribution of matter or charge inside the nucleon'.*

Bj then went on to apply similar ideas to the electromagnetic cross section, reaching the conclusion that it also should be big and point-like. It is clear from these quotations that Bj already had the essential ideas of the parton model in 1967, although he did not make any explicit statement about scaling.

Bj's 'derivation' of scaling was worked out in 1968, although not published [10] until 1969. In his paper Bj says that *'A more physical approach into what is going on is, without question, needed'*. Thus Bj had partons without scaling in 1967, and apparently scaling without partons in 1968! Surely he must have put these ideas together, even if he did not tell the world. I have put the word 'derivation' in inverted commas above, because it depended on formal manipulations, which, although they appear to be valid in any renormalizable field theory, do not in fact completely survive the effects of renormalization, as will be discussed below.

The first inelastic scattering results from SLAC were presented by Panofsky [11] at the Vienna conference in 1968. Panofsky reported that these cross sections *'are very large and decrease much more slowly with momentum transfer than the elastic scattering cross sections and the cross sections of the specific resonance states ... therefore theoretical speculations are focused on the possibility that these data might give evidence on the behaviour of point-like, charged structures within the nucleon ... The apparent success of the parametrization of the cross sections in the variable ν/q^2 in addition to the large cross section itself is at least indicative that point-like interactions are becoming involved'.* Panofsky also discussed the experimental status of a sum rule derived by 'Godfrey' who treated the proton in a non-relativistic point quark model. This was in fact a reference to a paper [12] by Gottfried, published in 1967 before Bj's talk at the SLAC conference, to which I now return.

Gottfried noted that in the *'breathtakingly crude'* naïve three-quark model the second term in the following equation vanishes for the proton (it also vanishes for the neutron, but neutrons are not mentioned):

$$\sum_{i,j} Q_i Q_j \equiv \sum_i Q_i^2 + \sum_{i \neq j} Q_i Q_j \; . \tag{5}$$

Thus for any charge-weighted, flavour-independent, one-body operator all correlations vanish, and therefore using the closure approximation the following sum rule can be derived:

$$\int_{\nu_0} W_2^{ep}(\nu, q^2) d\nu = 1 - \frac{G_E^2 - q^2 G_M^2 / 4m^2}{1 - q^2/4m^2} \; , \tag{6}$$

where ν_0 is the inelastic threshold (the methods used to derive this sum rule are those that have long been used to derive sum rules in atomic and nuclear physics, for example the sum rule [13] derived in 1955 by Drell and Schwarz). After observing that this sum

rule appears to be oversaturated in photoproduction (we now know that the integral is actually infinite in the deep inelastic region), Gottfried asked whether it was *'idiotic'*, and stated that if, on the contrary there is some truth in it, one would want a *'derivation that a well-educated person could believe'*.

In his talk at the 1967 SLAC conference Bj quoted Gottfried's paper and stated that diffractive contributions should presumably be excluded from the integral, which could be done by taking the difference between protons and neutrons, leading to the following result, in modern notation:

$$\int \left(F_2^{ep}(x, q^2) - F_2^{en}(x, q^2) \right) \frac{dx}{x} = \frac{1}{3} \ . \tag{7}$$

This result, which is generally known as the Gottfried sum rule, is not respected by the data which give the value [14] 0.235 ± 0.026. In parton notation, the left-hand side can be written

$$\frac{1}{3}(n_u + n_{\bar{u}} - n_d - n_{\bar{d}}) = \frac{1}{3} + \frac{2}{3}(n_{\bar{u}} - n_{\bar{d}}) \ , \tag{8}$$

where the second expression follows using isospin conservation. The fact that the data give a number less than one-third implies that $n_{\bar{d}} > n_{\bar{u}}$ which is not implausible if we note that i) the proton can virtually dissociate into $\pi^+ n$ in which they are more anti-d's than anti-u's (as well of course into $\pi^0 p$ in which the numbers are equal), and ii) one might expect the production of up anti-up pairs in the nucleon to be suppressed relative to the production of down anti-down pairs as a result of the Pauli principle. Although not exact, the Gottfried–Bj sum rule is very interesting as it was the first result to give information about quark/parton charges.

In 1969 the parton model emerged from Bj and Feynman's notebooks, and Bjorken and Paschos published [15] an explicit parton model, with model functions for the quark distributions, but no gluons. 1969 also witnessed the publication of the first of the series of papers [16] by Drell, Levy and Yan. They studied a canonical field theory of pions and nucleons, with a cut-off on the transverse momentum applied in the frame in which target particles are moving with infinite momentum. This model provided a very important laboratory for identifying those processes, other than deep inelastic scattering, to which parton ideas might apply. It led not only to the classic predictions for what is now known as the Drell–Yan process, but also to the identification of most of the other processes which we now know can be consistently described by perturbative QCD.

In 1969 Callan and Gross published the first paper [17] that used sum rules involving commutators of a time derivative of a current with another current [see Eq. (3) above] to make testable predictions depending on the model that had been used to derive the commutator. In particular, by studying the sum rules for moments of the structure function F_2 and F_1 they found that:

$$\begin{aligned} \frac{\sigma_L}{\sigma_T} &= 0 - \text{quark model} \ , \\ &= \infty - \text{algebra of fields} \ . \end{aligned} \tag{9}$$

This sum rule can easily be interpreted, using the parton model in the Breit frame for absorbing a virtual photon by a constituent, in terms of the ability of constituents of different spin to absorb photons with different helicities, but the parton interpretation followed the formal derivation of the sum rule. It is, of course, now known that σ_L/σ_T is small, except at very small x, and that the value given by QCD is not exactly 0 but of

order α_s due to scattering off quark–anti-quark pairs with non-zero transverse momentum produced from gluons.

In 1969 I got involved in the sum rule/parton business, which by then had become relatively simple since essentially all the pre-QCD techniques were well developed (use of the Bjorken limit, the infinite momentum frame, current commutators and commutators of currents with their time derivatives). David Gross arrived at CERN, where I was then a postdoc, on sabbatical in early 1969 and gave a talk on the Callan–Gross relation and other recent developments. I was then working with John Bell on shadowing of neutrino interactions in nuclei, and as an exercise to see whether I had understood the description in David's talk, I applied the Bjorken–Paschos model to neutrino structure functions. I showed the results to David, who became very interested when he realized, looking at my results, that there is an additional structure function (F_3) in neutrino interactions. He immediately suggested that we see whether we could use the formal methods then in vogue to derive any result for the moments of F_3. After taking the appropriate limits, we derived on the blackboard the sum rule [18]:

$$\int F_3^{\nu N} \frac{dx}{2x} = 3 \ . \tag{10}$$

We did not put the factor one-half on the left-hand side and therefore had six on the right. David's immediate reaction was that sum rules give numbers like 1 or 2, but not 6, and that we must therefore have made a mistake. After checking the algebra and not finding any obvious error, I decided to check the sum rule in the Bjorken–Paschos model. It was only after elaborately integrating over their explicit models for quark distribution (which were expressed in a complex way as sums over contributions from configurations with different numbers of quark–anti-quark pairs) that I realized that in fact the sum rule has to be true in any quark model since it simply states that:

$$n_q - n_{\bar{q}} = 3 \tag{11}$$

because of baryon conservation. This sum rule obviously provides a critical test of the quark model. There are important QCD corrections on the right-hand side, which have now been calculated to order α_s^3. Including these corrections, the final analysis of the CCFR data gives [19]:

$$\alpha_s(M_Z) = 0.114^{+0.005\ +0.007\ +0.004}_{-0.006\ -0.009\ -0.003}$$

where the last error is the result of an estimate of possible higher twist contributions, which may be conservatively large. This result is to be compared with the values of 0.120 ± 0.003, obtained from fits to precision electroweak data, and 0.119 ± 0.004 from fitting all the data.

It was pointed out during 1969 [20] that, although non-trivial interaction Hamiltonians were assumed, the formal manipulations used to derive scaling and the scaling-limit sum rules are all invalid in interacting field theory, and in particular are spoiled by logarithms of q^2. Of course we now know that in QCD these logarithms can be summed up to give corrections that vary as $\alpha_s \sim \ln(q^2)^{-1}$, or powers of $\ln(q^2)$ given by anomalous dimensions. At the time, many of us were prepared to dismiss the logarithms that appeared in perturbation theory on the basis that the data seemed to support the underlying picture and that nature must somehow behave in a smoother way than that predicted by all the field theories then known. A typical reaction is that in my paper with David Gross, in which we arrogantly state that '*In second-order perturbation theory all the limits … are*

infinite .... [and] the sum rules ... diverge. In the case of electron–nucleon scattering this is contradicted by experiment. Thus the real world is less divergent than pertubation theory indicates'!

In late 1969, I decided to look for all sum rules that could be derived in arbitrary quark-parton models (a relatively simple exercise), and then see if they could all be 'properly' derived using the formal techniques discussed above. The result was the following two relations [21]:

$$F_3^{\nu p} - F_3^{\nu n} = 12(F_1^{ep} - F_1^{en}) \tag{12}$$

$$F_2^{ep} + F_2^{en} \geq \frac{5}{18}(F_2^{\nu p} + F_2^{\nu n}) \tag{13}$$

where the second expression is an equality if there are no strange quarks. Both relations were 'derived' for all moments of the structure functions from equations like (3) in quark models with model interaction Hamiltonians.

Equation (13) is known to be rather well satisfied as an equality. I promoted it as a good test of the quark charges, which it is. However, it is equivalent to the statement that the cross section for absorbing virtual isoscalar photons is one-ninth of the cross section for absorbing virtual isovector photons. This was known to be approximately true for real photons and had been explained by vector-meson dominance, so that it could be, and was, argued that perhaps the 5/18 relation had nothing to do with quarks! Of course, the data were really telling us that photoproduction already suggested non-integral quark charges.

In the same paper I discussed the then relatively well-known fact that the experimental value of 0.18 for $\int F_2^{ep} dx$ looked rather big compared to the parton value $\Sigma Q_i^2 \langle x_i \rangle$, even with non-interval charges, and pointed out that any valence plus uniform sea model would give a value greater than 2/9. I then noted that this value could be '*easily reduced by adding a background of neutral partons (which could be responsible for binding the quarks)*', an idea which I recall was attacked, in a seminar that I gave at CERN, for being not in the spirit of the quark model!

By the end of 1969 it was known that $\sigma_L/\sigma_T = 0.2 \pm 0.2$, which was encouraging for those of us who were proponents of the quark model. It would be wrong to think, however, that the quark parton model was widely accepted at that time. A review talk [22] that I gave in 1970 describes many other ideas that were still on the market (diffractive models, Harari's model, generalized vector meson dominance, a Veneziano-like model, etc.). By the end of 1970, however, it was known that the neutron and proton structure functions were different, which laid certain ideas (diffractive models; Harari's model) to rest, but there was still no general consensus on the correct underlying picture.

The most important development for our field in 1971 was, of course, 't Hooft's proof that non-Abelian gauge theories are renormalizable. On the theoretical deep inelastic front, I published [23] the following sum rule, derived by formal manipulations involving the energy momentum tensor, for the fraction of the momentum (ϵ) of a high-energy nucleon carried by gluons:

$$\epsilon = 1 + \int \left(\frac{3}{4}[F_2^{\nu p} + F_2^{\nu n}] - \frac{9}{2}[F_2^{ep} + F_2^{en}] \right) dx \ . \tag{14}$$

At that time the available neutrino data could not be used to do better than give an experimental lower bound ($\epsilon \geq 0.52 \pm 0.38$), although it was rather obvious from the data that ϵ could not be zero. Today the latest parton fits to the data [24], including all QCD

corrections, give $\epsilon = 0.39$ at $q^2 = 2$, increasing to 0.44 at $q^2 = 20$, 0.47 at $q^2 = 2000$, and 0.48 at $q^2 = 200\,000$.

Bob Jaffe has already mentioned the 'light-cone algebra' of Gell-Mann and Fritzsch, which reproduced all the general results of the quark parton model in terms of expectation values of bilinear light-cone operators abstracted from the quark model [25], without admitting to the physical reality of the quark-parton picture. Murray Gell-Mann came to give a talk on this at SLAC, in the auditorium in which we are meeting today. I was in the audience and was getting excited because I knew (my paper was written but not published) that because he was using a free quark model, it must imply $\epsilon = 0$, which was not compatible with the data. At a certain point Murray announced that he was about to derive a sum rule using the energy momentum tensor. I stuck up my hand and said that I knew what the sum rule was going to be (the sum rule above with $\epsilon = 0$) and that it was contradicted by the data. To this, Murray replied *'You must be Llewellyn Smith, I'm pleased to meet you but you spell your name wrongly'*!

By that time the attention of Bj and others had switched to trying to anticipate the gross features of the final states in deep inelastic scattering, which had not then been studied[1], and other 'hard' processes, such as the production of particles with large transverse momentum in proton collisions. Study of deep inelastic scattering in field theory continued. Gribov and Lipatov's seminal summation of the leading powers of $\ln(q^2)$ in an Abelian gauge theory was published [26] in 1971, and their results were rederived using the operator product expansion and the renormalization group by Christ, Hasslacher and Mueller in a paper [27] that established the techniques used later in QCD.

Asymptotic freedom and QCD were the major theoretical developments of 1973. The event of the year (perhaps of the decade) experimentally was the discovery of neutral currents by Gargamelle. The same experiment also produced the first y distributions for charged current interactions [28], which were of course in line with quark-parton expectations, as was implicit in the 1972 result that $\sigma^\nu \approx 3\sigma^{\bar\nu}$.

The first QCD calculations of corrections to scaling and to sum rules appeared in 1974, bringing the end of the sum rule story in sight. This, however, was not at all clear at the time due to the 'high y anomaly' in neutrino y distributions measured at Fermilab (which of course turned out to be wrong), the observation of neutrino dimuon and trimuon events (the former later interpreted as charm production; the latter wrong), and above all the apparently smooth rise of $\sigma_{\bar e e}$ between 3 and 5 GeV found at SLAC (following earlier indications that the cross-section was unexpectedly large from CEA).

The SLAC $\bar e e$ data, which became public at the end of 1973, were the focus of attention at the 1974 London conference, in a session with 61 theoretical contributions. In his capacity of rapporteur B. Richter declared [29] that *'the data contradict both the simple quark–parton model and the Bjorken scaling hypothesis'*. Commenting on his own *'favourite models involving new lepton–hadron interactions'* he remarked that, *'struck ... by similar features seen in hadronic interactions'* on first seeing the data, he had suggested that a kind of *'no photon annihilation'* was involved and had found that he was in distinguished company (Pati and Salam). The discovery of the J/Ψ later in 1974 opened the way to understanding the $\bar e e$ data and a full vindication of the quark-parton

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[1] Nor had the gross features of pp collisions yet been established by the ISR, where experiments began in late 1971. It is often forgotten that the existence of a central rapidity plateau was not established pre-ISR, and was not universally anticipated. I remember long discussions on this subject in the SLAC cafeteria in 1971–72, during which David Ritson regularly asserted that the existence of a plateau had been disproved by cosmic ray data which showed that there are two fireballs!
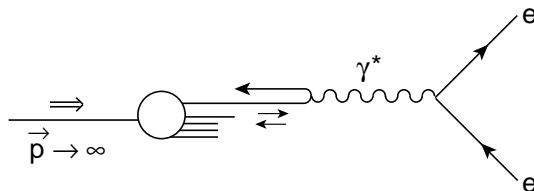
picture, although charmed particles were not found until 1976.

The Ellis–Jaffe sum rule, to which I will return in the next section, was published [30] in 1974. It should perhaps be called an ansatz, rather than a sum rule, as it depends on an explicit dynamical assumption. This assumption has turned out to be wrong, with the result that Ellis now calls it the Jaffe sum rule and vice versa.

This ends my brief history of sum rules. Although the interpretation of the J/Ψ as a $c\bar{c}$ bound state was not fully established in 1975, and the confusion caused by the high y anomaly persisted, the first indications of scaling violations in that year encouraged proponents of QCD (it is interesting to compare the plots of F_2 versus q^2 at fixed x shown at the 1975 SLAC conference [31], which had large errors and ranged up to q^2 of order 20 GeV^2, with the exquisitely accurate data now available [32], which go up to q^2 above $10\,000$ GeV^2 at large x). The following years witnessed very convincing tests of the QCD predictions, the development of perturbative QCD and the anticipated [33] manifestation of the gluon in three-jet events in $e\bar{e}$ annihilation.

3   Sum Rules for Spin-Dependent Lepton and Photon Scattering

The parton model describes deep inelastic scattering off polarized nucleons in terms of contributions from quarks with spins parallel ($\uparrow$) or antiparallel ($\downarrow$) to the spin of the parent nucleon in the infinite momentum frame:

The lowest moment of the structure function g_1 is related to the numbers of such quarks thus:

$$\int g_1(x)dx = \frac{1}{2}\sum Q_i^2 \left(n_i^\uparrow + n_{\bar{i}}^\uparrow - n_i^\downarrow - n_{\bar{i}}^\downarrow\right) \ . \tag{15}$$

It is convenient to write

$$Q_i^2 = \frac{2}{9} + \frac{1}{3}I_3^i + \frac{1}{6}Y^i$$

where c, t and b quarks are neglected here and in the rest of this section. Noting that in the parton picture

$$\langle\lambda = 1/2, p|\bar{\psi}I_3\gamma_\mu(1\mp\gamma_5)\psi|\lambda = 1/2, p\rangle = 2p_\mu \sum I_3^i \left(n_i^{\downarrow\uparrow} - n_{\bar{i}}^{\uparrow\downarrow}\right) \tag{16}$$

while

$$\langle 1/2, p|\bar{\psi}I_3\gamma_\mu\gamma_5\psi|1/2, p\rangle = 2p_\mu \left(-\frac{g_A}{2g_V}\right) \ , \tag{17}$$

the Bjorken sum rule, Eq. (3), can immediately be derived.

To make predictions for the proton and neutron separately, it is necessary to use data from hyperon decays (the F/D ratio) to obtain the Y_i term and make an additional assumption to predict the singlet piece. Ellis and Jaffe [30] assumed that

$$n_s^\uparrow + n_{\bar{s}}^\uparrow - n_s^\downarrow - n_{\bar{s}}^\downarrow = 0 \tag{18}$$

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on the grounds that there are presumably few strange quarks in the proton and they are unlikely to be polarized. In the parton model, this gives

$$\int g_i^p dx = 0.186 \pm 0.004$$

in serious contradiction with the observed value [34] of $0.121 \pm 0.003 \pm 0.005 \pm 0.017$ at $q^2 = 5 \text{ GeV}^2$ (the disagreement persists when QCD corrections are included).

An equivalent form of the Ellis–Jaffe prediction is

$$\Delta\Sigma = \sum_i \left(n_i^\uparrow + n_{\bar{i}}^\uparrow - n_i^\downarrow - n_{\bar{i}}^\downarrow \right) = 0.58 \pm 0.03 \tag{19}$$

compared to the experimental value [34] of $0.19 \pm 0.05 \pm 0.04$. The conclusion is that, on the basis of the naïve parton model, strange quarks carry a substantial fraction (-15%) of the polarization while most (70%) is not in quarks at all. Actually the fact that the Ellis–Jaffe ansatz does not give the naïve result $\Delta\Sigma = 1$ already leads to one or both of these conclusions without any input from deep inelastic scattering. This also reminds us that the success of the Bj sum rule, which relates deep inelastic data to the measured g_A/g_V, not to the naïve three-quark model value or to some simple number derived by counting, is highly non-trivial.

An immediate question is whether the naïve predictions survive renormalization in QCD. Physically, it should be noted that the parton model derivation assumes that the component of the struck quark's spin along $\vec{p}$ is identical to its helicity. This is a safe assumption in a super-renormalizable theory in which the integral over the quark's transverse momentum $\vec{k}_T$ converges. In a merely renormalizable theory, however, it may not be safe to let $\vec{p} \to \infty$ before integrating over $\vec{k}_T$. Consequently $|\vec{k}_T|/|\vec{p}|$ cannot necessarily be assumed small and the spin-equals-helicity assumption may not be correct.

I shall consider this from the point of view of the operator product expansion[2] in order to facilitate comparison with the rest of the literature. In the case of the non-singlet (Bjorken) sum rule, the twist-two spin-one operator, which controls the twist-two contribution to the Bjorken integral, is the axial isospin current. This current is conserved as $m_q \to 0$ and needs no special renormalization of its own (it is rendered finite by the standard QCD vertex and wave function renormalizations). The Bjorken sum rule and the associated parton picture therefore survive renormalization (albeit with the well-known correction factor $C^{NS}(q^2) = 1 - \frac{\alpha_s}{\pi} \cdots$ on the right-hand side of the sum rule).

In the singlet case the twist-two spin-one operator built from quark fields formally has the form '$\bar{\psi}\gamma_\mu\gamma_5\psi$', where the inverted commas indicate that in this case the divergences implicit in multiplying $\bar{\psi}(0)$ by $\psi(0)$ are not removed by the standard QCD renormalizations. Indeed, because of the anomaly, this Wilson operator needs a special renormalization of its own and the results depend on the renormalization scheme chosen.

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[2] Use of the operator product expansion makes it necessary to adopt a renormalization prescription for the relevant Wilson operators, one of which is afflicted by the famous anomaly in this case. The equivalent procedure of factorizing all the relevant Feynman diagrams into 'hard' (mass singularity free) and 'soft' parts likewise requires the adoption of a factorization prescription. In the diagrammatic approach, however: i) it is more apparent how to choose a prescription that allows a simple interpretation for the soft parts, which are normally identified with the quark and gluon distributions (I particularly like a prescription [35] which relates these distributions to Fourier transforms along the light-cone of quark and gluon correlation functions smeared over distances $(q^2)^{-1/2}$ transverse to the light-cone), and ii) the anomaly is never directly encountered (the amplitudes for deep inelastic scattering are all of course anomaly free).

It would seem natural to choose a scheme that respects QCD gauge invariance e.g. $\overline{MS}$. There being no gauge-invariant, twist-two, spin-one gluonic operator, the lowest moment of g_1 is controlled by the matrix elements of the gauge invariant (gi) Wilson operator (or 'current' — but this term tempts the naïve to think that it can be manipulated like a conserved current) that I denote $[\bar{\psi}\gamma_\mu\gamma_5\psi]_{gi}$. The divergence of this operator is given by [36]:

$$\partial^\mu[\bar{\psi}\gamma_\mu\gamma_5\psi]_{gi} = \frac{\alpha_s}{2\pi}F \cdot \tilde{F} + 0(m_q) \tag{20}$$

where F is the QCD field tensor and $\tilde{F}$ its dual. Because the gauge invariant 'current' has a 'hard' divergence, its matrix elements depend on the renormalization scale, which it is convenient to take to be q^2. The quantity $\Delta\Sigma$ introduced above, which enters the naïve parton sum rule, is therefore replaced by $C(q^2)\Delta\Sigma(q^2)$ where

- $C(q^2)$ is the coefficient function in the operator product expansion (= hard scattering amplitude in the language of factorized diagrams) which, as in the non-singlet case, produces the familiar correction factor $1 - (\alpha_s/\pi)\cdots$ at finite q^2;
- $\Delta\Sigma(q^2)$ is given by

$$\langle p|[\bar{\psi}\gamma_\mu\gamma_5\psi]_{gi}|p\rangle = 2p_\mu\Delta\Sigma .$$

The q^2 dependence of $\Delta\Sigma$, which arises because q^2 is chosen as the renormalization scale, is actually quite mild. It tends to decrease $\Delta\Sigma$ as q^2 decreases, thereby making the difference between the measured and the Ellis–Jaffe values bigger. A recent fit [34] gives $\Delta\Sigma(1) = 0.19 \pm 0.05 \pm 0.04$. However, given the need for renormalization, and that therefore the spin projection on the z axis is not equal to the helicity, it is not obvious that $\Delta\Sigma$ should have a simple quark model interpretation.

A number of authors [37] have advocated writing

$$[\bar{\psi}\gamma_\mu\gamma_5\psi]_{gi} = [\bar{\psi}\gamma_\mu\gamma_5\psi]_s + K_\mu \tag{21}$$

where K_μ is the gauge-dependent quantity

$$K_\mu = \frac{\alpha_s}{8\pi}\epsilon^{\mu\alpha\beta\gamma}A_\alpha^a\left(F_{\beta\gamma}^a - \frac{g}{3}f_{abc}A_\beta^b A_\gamma^c\right) . \tag{22}$$

As discovered long ago by Adler [36] and Bardeen [38] , the gauge-dependent current $J_\mu^s = [\bar{\psi}\gamma_\mu\gamma_5\psi]_s$ is conserved and is the current corresponding to the chiral symmetry (hence the label s) that QCD exhibits in this limit. Advocates of introducing this separation say that K_μ is 'obviously' to be identified with the gluon contribution. They then note that the quantity $\Delta\Sigma^q$ defined by

$$\langle p|J_\mu^s|p\rangle = 2p_\mu\Delta\Sigma^q \tag{23}$$

is q^2 independent, due to the fact that the symmetry current is conserved as $m_q \to 0$, and claim that this is 'necessary for a quark model interpretation'.

This leads to the relation

$$\Delta\Sigma(q^2) = \Delta\Sigma^q - \frac{\alpha_s}{2\pi}\Delta g(q^2) \tag{24}$$

where $\Delta\Sigma^q$ and Δg are claimed to be the contributions of quarks and gluons to the proton's spin (the gluonic term appears to vanish asymptotically, but this is not so since Δg varies as α_s^{-1}, as is necessary to reconcile the q^2 dependence of $\Delta\Sigma(q^2)$ and $\Delta\Sigma^q$).

A fit using this so-called 'Adler–Bardeen scheme' gives [34]

$$\Delta\Sigma^q = 0.38 \pm 0.03 \pm 0.05 \ ,$$

which does not help much in practice in interpreting quantities measured in deep inelastic scattering in a simple quark picture. Personally, however, I am not at all convinced that $\Delta\Sigma^q$ should have a simple interpretation. It is true that the divergence that appears to be responsible for spoiling the simple parton picture resides in K_μ, and that K_μ generates the two-gluon contribution in the t-channel view of $\langle p|[\bar{\psi}\gamma_\mu\gamma_5\psi]_{gi}|p\rangle$. However, $\langle p|J_\mu^s|p\rangle$ still contains multigluon contributions that are not present in a naïve quark picture, and the gauge-dependent quantities J_μ^s and K_μ are peculiar (unphysical?) objects, both of which necessarily couple to the unphysical 'ghost' U(1) Goldstone boson. Furthermore, I have yet to be convinced that the 'AB scheme' used to fit the data treats all the moments of the polarized quark and gluon distributions consistently[3].

It may be an illusion to expect that the deep inelastic data should have a simple interpretation in terms of the naïve (non field theoretical) quark model in the singlet case in which the divergences of field theory really matter. In any case, more experimental information on the structure of polarized nucleons is clearly needed, from experiments interpreted with consistently defined and used quark and gluon distributions. In this respect, the forthcoming COMPASS experiment at CERN, which will measure the polarization asymmetry in events in which charm is produced (a signal that gluons are involved), and the study of large p_T production of jets and photons in polarized pp collisions at RHIC will be particularly interesting.

New data will soon also cast light on the interesting question of the way in which the Drell–Hearn–Gerasimov–Iddings (DHGI) sum rule is (presumably) satisfied and the connection between polarization asymmetries in photoproduction and deep inelastic scattering. To derive the DHGI sum rule, consider the amplitude for forward elastic scattering of a photon of energy ω and initial/final polarization ϵ/ϵ' from a nucleon, which may be written:

$$T(\omega) = \vec{\epsilon}'^* \cdot \vec{\epsilon} f(\omega) + i\vec{\sigma} \cdot (\vec{\epsilon}'^* \times \vec{\epsilon}) g(\omega) \ . \tag{25}$$

The imaginary part of the $g(\omega)$ is proportional to:

$$\Delta\sigma = \sigma_{\gamma N}^{1/2} - \sigma_{\gamma N}^{3/2} \tag{26}$$

where $\sigma_{\gamma N}^{3/2}$ and $\sigma_{\gamma N}^{1/2}$ are respectively the total γN cross sections for the cases with total initial spins $\pm 3/2$ and $\pm 1/2$ along the photon direction. DHGI noted that, on one hand, $m^2 g(\omega)/(2\alpha\omega)$ is determined exactly by the anomalous magnetic moment (κ) of the target (proton or neutron) for $\omega \to 0$, with corrections of order ω^2, as a result of the celebrated low energy theorem, which depends only on Lorentz and gauge invariance. On the other hand, assuming that $g(\omega)$ satisfies an unsubtracted dispersion relation, as expected on the basis of standard considerations, this same quantity may be related to an integral over

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[3] The fits deal with the distributions, not their moments. The 'AB scheme' would appear to treat the lowest ($n = 0$) moment of the structure functions in a special way which seems to destroy the analyticity in n that is necessary to construct distributions from moments. Distributions can be defined directly as Fourier transforms along the light-cone of suitably regulated bilocal operators (see the previous footnote): from this point of view, the 'AB scheme' and the introduction of the subtractively renormalized quantity K_μ look very unnatural.

$\Delta\sigma$, which can be expanded around $\omega = 0$ in powers of ω^2. Equating the two expressions gives the DHGI sum rule:

$$-\frac{\kappa^2}{4} = \frac{m^2}{8\pi^2\alpha} \int \frac{\Delta\sigma d\omega}{\omega} \tag{27}$$

which, as noted by D–H, is *'of interest because of its experimental as well as theoretical implications'*.

A large part of the original interest of meeting what D–H called the *'formidable challenge'* of testing their sum rule experimentally, which has still not been done, was to test the assumption of an unsubtracted dispersion relation which (explicitly or implicitly) is also needed to derive the Adler sum rule. The sum rule is now generally expected to be true, and interest has focused on the very non-trivial questions of how it is satisfied and how the polarization and deep inelastic asymmetries match up as q^2 is varied. I deal with these questions in turn before finishing with some interesting results that can be derived by applying DHGI to other processes.

Inga Karliner, in a thesis [39] written at SLAC under the supervision of Fred Gilman which singles out Sid Drell for special thanks, constructed the contribution of $\gamma N \to \pi N$ to the DHGI integral using the results of phase shift analyses of unpolarized data; she also included an estimate of the contribution of $\gamma N \to \pi\pi N$. Her analysis, which went up to 1.2 GeV, has been extended to 1.7 GeV by Workman and Arndt [40] (the changes are small). The results (given without errors) are:

	p	n	$p+n$	$p-n$
WA	-257	-189	-446	-68
D HGI value	-204.5	-232.8	-437.3	-28.5

Although inelastic channels are known to be important in photoproduction, the amount by which pion photoproduction fails to saturate the sum rule is surprising, especially for $p-n$, where relatively rapid convergence of the integral is expected, given the (accidental?) success for $p+n$.

It will be very interesting to learn from experiments that are just starting at Jefferson Lab with CEBAF what additional channels contribute, including possibly $K\Lambda^*$, $K\Sigma^*$ etc. if the failure of the Ellis–Jaffe sum rule is due to substantial contributions from polarized strange quarks, as well as $\rho N, \epsilon N, \omega N$ etc. These channels are included in a calculation by Burkert and Li [41] who estimated contributions from all s channel resonances up to and including the $F_{37}(1950)$ using a mixture of experimental input and quark model predictions. They found for the DHGI integral:
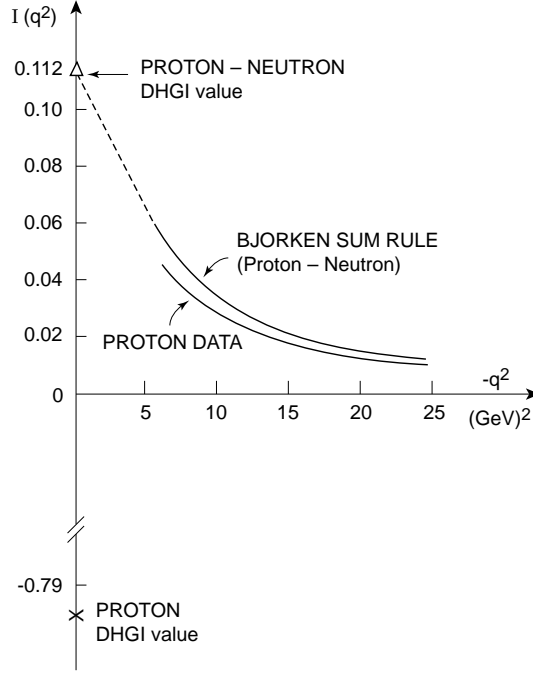
	p	n	$p+n$	$p-n$
BL	-203	-125	-328	-78

The failure to reproduce the DHGI prediction (due to non-resonant contributions? a failure of the generally successful quark model? inconsistent use of the quark model and data?) heightens the interest in the forthcoming CEBAF experiments.

These experiments will also explore the very interesting intermediate q^2 region. If we define

$$I(q^2) = \frac{2m^2}{|q^2|} \int g_1(x, q^2) dx , \tag{28}$$

then the right-hand side of the DHGI sum rule is equal to $I(0)$. Whereas for $p-n$ it is easy to match the large q^2 (Bjorken sum rule) value of $I(q^2)$ onto the DHGI prediction for $q^2 = 0$, the situation is very different for the proton, as shown in the following figure [42], where the break in the negative I scale should be noted.

DHGI requires (the appropriately weighted integral of) $\Delta\sigma$ for the proton to be negative for $q^2 = 0$, whereas it is positive in the deep inelastic region. $\Delta\sigma$ is negative for photoproduction of the dominant resonances $[\Delta, D_{13}(1520), F_{15}(1690)]$ as was explained long ago in the quark model [43]. The Δ is produced by an M1 transition, implying $\sigma^{3/2} = 3\sigma^{1/2}$. In the harmonic oscillator quark model, the helicity $1/2$ amplitudes for photoproducing the next two resonances are given by

$$A^{\gamma p}(D_{13}) \sim |\vec{q}_{cm}|^2 - \alpha^2/g$$

$$A^{\gamma p}(F_{15}) \sim |\vec{q}_{cm}|^2 - 2\alpha^2/g$$

(29)

where α is the harmonic oscillator constant, and g is the quark g factor. It turns out that $|\vec{q}_{cm}|^2_{F_{15}} \simeq 2|\vec{q}_{cm}|^2_{D_{13}}$ so both amplitudes can, 'accidentally', be — and for reasonable values of α and g are — small, in agreement with the data.

However, $|\vec{q}_{cm}|^2$ varies rapidly with q^2 and, as pointed out in 1972 by Close and Gilman [44], it is likely that $\Delta\sigma$ rapidly changes sign as q^2 increases, in conformity with the deep inelastic result. Quark model calculations by Burkert and Li [41] predict a very complex q^2 behaviour (initial increase of $\Delta\sigma$, due to the contribution of the Δ, followed by a rapid decrease due to the contributions of other resonances), which does not seem to be consistent with predictions based on chiral perturbation theory. It will be very interesting to see the CEBAF data.

Finally, interesting theoretical conclusions can be derived by applying DHGI to other targets. In terms of g factors, the sum rule reads

$$- (g - 2)^2 = \frac{m^2}{8\pi\alpha} \int \frac{d\omega}{\omega} \Delta\sigma \ .$$

(30)

In pure QED, $\Delta\sigma$ is of order α^2 (e.g. for an electron target the lowest-order process is $\gamma e \to \gamma e$). It follows that

$$g - 2 = 0 + 0(\alpha)$$

(31)

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i.e. consistency requires that $g = 2$ to lowest order in QED, a result which also follows from other considerations such as requiring good high-energy behaviour (this argument could only fail if the DHGI integral required a subtraction, which however would introduce a new arbitrary constant and spoil renormalizability). Plugging (31) back into (30) then gives:

$$\int \frac{d\omega}{\omega} \Delta\sigma_{QED} = 0(\alpha^3) \tag{32}$$

i.e. the integral of the order α^2 contribution to $\Delta\sigma$ must vanish, as pointed out by Altarelli, Cabbibo and Maiani [45]. This highly non-trivial constraint, which provides a check of order α^2 calculations, was generalized by Brodsky and Schmidt [46] using a loop counting argument to:

$$\int \frac{d\omega}{\omega} \Delta\sigma_{\gamma a \rightarrow bc}^{tree} = 0 \ . \tag{33}$$

This also provides non-trivial checks on calculations, and a potential diagnostic for new physics e.g. by checking whether the sum rule is satisfied by cross-sections such as $\gamma e \rightarrow W\nu$ that could be measured at a linear collider.

Brodsky and Drell have pointed out [47] that if a lepton (L) had substructure which could be photo-excited above a threshold m^* there would be an additional order α term:

$$\int_{m^*} \Delta\sigma_{\gamma L \rightarrow x} \frac{d\omega}{\omega} \tag{34}$$

on the right-hand side of (32). It follows that:

$$g - 2 = 0 + 0(\alpha^2) + 0\left(\frac{m_L}{m^*}\right) \ .$$

Brodsky and Drell's paper is mainly devoted to showing how this interesting relation arises in explicit models. On the one hand, it allows us to interpret possible deviations from the prediction of QED for $g - 2$ in terms of a mass scale (m^*) for substructure. On the other hand, it assures us that $g = 2$ would emerge naturally in any composite model in which m^* is extremely large, although no one knows how to make a model in which the mass of a lepton could be very small compared to the mass of its constituents.

4   Concluding Remark

It is pleasing to end with this nice result of Sid's. His paper with Stan Brodsky nicely illustrates the pragmatic and realistic style of Sid's many contributions to our field. General arguments are used, but only when they can be substantiated and illustrated using specific models, and relevance to possible experiments is paramount. I would like to end by thanking Sid for what he has given to our field, and to me personally, not only in terms of physics, but also by promoting this style of doing physics as the long-time leader of the outstanding SLAC theory group.

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