25 YEARS WITH THE POMERON

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Abstract: This is a report on my 25 years activity in understanding of the Pomeron structure. Since I was involved in the Pomeron business moreless from the beginning, I hope, that this report shows the development of the main ideas in their historical perspective from the first enthusiastic attempts to find a simple solution to understanding of the complexity and difficulty of the problem.

In other words, this is a story about a young guy who wanted to understand everything in high energy interaction, who did his best but who is still in the beginning, but who has not lost his temper and considers the Pomeron structure as the beautiful and difficult problem, which deserves his time and efforts to be solved.

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1 Instead of introduction.

I started to be involved in the Pomeron problem in early 70’s not because I felt that this was an interesting problem for me but rather because everybody around worked with this problem.

It was a heroic time in our department (theory department in St. Petersburg (Leningrad) Nuclear Physics Institute) when we worked as one team under the leadership of Prof. Gribov. He was for us, young guys, not only a dominant leader but a respectable teacher. I took seriously his words: “Genya, it seems to me that you are a smart guy. I am sure, you will be able to do something more reasonable than this quark stuff.” So I decided to try and, frankly speaking, behind my decision there was another reason. I felt that I could not develop my calculation skill, doing the additive quark model. However, I must admit, very soon I got a deep interest in the Pomeron problem, so deep that I decide to present here all my ups and downs in the attempts to attack this problem.

2 Reggeon Calculus.

My first remark is that in 70’s to find the high energy asymptotic was a highly priority job. During the last five years I have traveled a lot around the globe and I have found that it is very difficult to explain for young physicist why it was so. However, 25 years ago the common believe was that the analytical property of the scattering amplitude together with its asymptotic will give us the complete and selfconsistent theory for the strong interaction. The formula of our hope was very simple:

“Analyticity, Unitarity and Crossing + Asymptotic = Theory of Everything” (1)

It is clear that to find the second part of the above formula was a great challenge for theorists. The main ingredients of our attempt to approach the asymptotic behaviour of the scattering amplitude were and are the Reggeons which came up as the solution of the first puzzle: the contradiction between the experimental observation of the hadrons (resonances) with spin ($j$) higher than 1 and the steep energy behaviour of the scattering amplitude ($A$) due to exchange such resonances.

Indeed, the exchange of a hadron with spin $j$ leads to $A \propto s^j$, where $s$ is the invariant energy of the reaction. For $j > 1 A$ exceeds the Froissart boundary [1] which has been proven from unitarity, analyticity and crossing symmetry [2]. The solution to this puzzle
was to assume that all resonance with certain quantum numbers could be described by one function of the resonance mass \( t \) (trajectory \( \alpha(t) \)). At \( t = M_R^2 \), where \( M_R \) is the mass of resonance with spin \( j \), \( \alpha(t = M_R^2) = j \). In the scattering kinematic region \( t < 0 \) and \( \alpha(t) \) should be smaller than 1. In this case \( s^{\alpha(t)} \) does not exceed the Froissart boundary. The concept of Reggeons is still the only solution to the first puzzle that have been found during 25 years of the development of our theoretical approach.

However, it turns out that experimental trajectories give the value of \( \alpha(t = 0) \) which is well below 1. It means that, if the asymptotic is defined by only Reggeon contribution, the total cross section shall fall down as a power of energy. Therefore, we got the second puzzle: in the Reggeon approach the total cross section should decrease as a function of energy while experimentally it is constant at high energy if not rising. The only way out of this puzzle was to assume that there is a new Reggeon (Pomeron) with the trajectory \( \alpha_P(t = 0) = 1 \).

It turns out that even this strong assumption cannot give us a simple solution for the asymptotic \([3]\). The basic idea was that we can build the effective theory for high energy interaction assuming the Pomeron and taking into account the interaction of the Pomerons with colliding particle and between them. V.N. Gribov found the Lagrangian for such effective theory \([3]\) and started the approach which was called Gribov Reggeon Technique or Gribov Reggeon Calculus. A bit later A.A. Migdal and V.N. Gribov made the first attempt to solve this theory \([4]\).

I entered to the game just at this stage and Gribov, Migdal and me in Refs.\([5]\) tried to find the answer to the following question: “What could be a feedback to the high energy behaviour of the exchange of normal Reggeons with \( \alpha(0) < 1 \) (so called secondary trajectories or Reggeons) from their interactions with the Pomeron?” It was nice time to remember because we understood a lot and found a beautiful form of the Lagrangian for the interaction of the secondary Reggeons with the Pomeron, especially for so called fermion Reggeons. In one particular case, when the secondary trajectory does not depend on \( t \), we gave the exact solution to the problem \([5]\) which has been used in the solid state physics for more reliable physical applications. However, the physical result of this study for me was rather destructive, because we found that even sufficiently weak interaction between the Pomerons led to strong effect for the secondary Reggeons changing crucially their power behaviour at high energy. On the other hand, experimentally, in all reactions where we have only Reggeon exchange the cross section has a beautiful power behaviour. As far as I know it is still an open problem and more microscopic approach such as QCD have not led to a solution of this puzzle. I consider this problem as a great challenge for QCD and for all theorists who got involved in this high energy business.

The Reggeon Calculus is an effective theory for high energy interaction in which the transversal and longitudinal degrees of freedom are treated in a different way. By now,
we have understood that this a general property for any theory at high energy. V.A. Kudryavtzev, A.A. Schipakin and me in Refs.[6] gave the first example how this separation of transversal and longitudinal degrees of freedom simplify the treatment of the particles with high spin at high energy. We generalized the Reggeon Calculus for the case of particles with spin and found a number of predictions in Reggeon approach for the polarization at elastic and inelastic processes. Since only the general properties of the Reggeon Calculus have been used, our way to take into account the spin of particles at high energy is still alive and widely used in the phenomenology of high energy interaction.

In the beginning of this section I have mentioned that the Reggeon Calculus was not a pure theoretical invention but rather some way of compromise between the general properties of our amplitude and the existing experimental data. We considered as a very important job to check our prediction with the experimental data to get a feedback to enrich our theoretical approach. To do this we created in 1972 KHOLERA ( KHOz,LEvin, Ryskin and Asimov ) collaboration. We had basically two goals: (i) to check how well (or bad ) we could describe the current experimental data using everything that we had learned theoretically and (ii) to provide a community service since our experimentalist had started to work at CERN on elastic high energy scattering.

We did our best [7] and learned several lessons:

1. We are able to model to experimental rise of the total cross section, assuming that the Pomeron has still intercept $\alpha(0) = 1$ if we include the Pomeron interactions;

2. The diffraction dissociation process gives more restrictable information on the asymptotic behaviour of the theory than the total or/and elastic cross section;

3. We can provide the description of all processes assuming the additive quark model and applying the Reggeon analysis to quark - quark scattering amplitude.

We summarized our understanding in the lectures at our Winter School [8] which were translated to English. I have learned also a personal lesson which was and is my guide: the experimental data cannot specify a theory for us but can be used only to verify our theoretical picture for high energy interaction.

In recent years Gotsman, Maor and me in Refs.[9] have repeated the KHOLERA approach with new assumption on the Pomeron structure, namely with the Pomeron intercept $\alpha_P(0) = 1 + \epsilon$ which naturally follows from the QCD approach [10]. We derived the same conclusions as have been mentioned above, but , in some sense, with the opposite sign: the intercept of the Pomeron bigger than one does not mean a violation of the Froissart boundary if the interaction between Pomerons has been taken into account.
All such phenomenological approaches incorporate a lot of the model assumptions and can be discussed with a lot of reservations, but they are the best illustration to the point that we have to understand better the Pomeron structure in theory on more microscopic level than in the Reggeon Calculus, to develop the selfconsistent effective theory for the high energy interaction.

3 Multiperipheral Model.

The first attack on the microscopic structure of the Pomeron was undertaken by us, M.G. Ryskin and me, in the framework of so called multiperipheral model. The idea of this approach is very simple. The Pomeron has been introduced to describe the behaviour of the total cross section with energy. Let us try to study the total cross section itself to understand its energy behaviour using the optical theorem, namely

\[ \sigma_{\text{tot}} = \sum_{1}^{\infty} \int |A(2 \rightarrow n)|^2 d\Phi_n, \]

where \( M(2 \rightarrow n) \) is the amplitude of the production of \( n \) particles in the final state with the phase space \( d\Phi_n \).

At first sight we need to solve even a more complicated problem, namely, to find the amplitude for multiparticle production, but a hope was that we would need only general properties of such an amplitude to understand the principle feature of the energy behaviour of the total cross section. Indeed, even before we started to approach this problem Amati,Fubini and Stangellini [11] as well as Ter-Martirosian [12] have found that eq. (1) leads to power energy behaviour \( \sigma \propto s^{\alpha_P(0) - 1} \) if we assume that all produced particles have average transverse momenta which do not depend on the value of energy (so called multiperipheral kinematic). However, the first attempt to evaluate the value of \( \alpha_P - 1 \) was discouraging and led to \( \alpha_P - 1 \approx -0.7 \).

Our first question that we asked ourselves starting this project was to understand what more detail properties of hadron production we needed to build the total cross section which did not depend on energy (or better to say \( \alpha_P - 1 \ll 1 \)). The second question was to understand what really we were calculating: the Pomeron or the whole mess of the correct asymptotic behaviour which included the complicated Pomeron interaction. Our approach was based on two main ideas:

1. The Pomeron interaction is small and can be neglected at sufficiently high energies, where \( G_{3P} \ln s < 1 \) (\( G_{3P} \) is triple Pomeron coupling here). The experimental data supported this assumption since they gave \( G_{3P} \approx 0.1 \);
2. For the amplitude of \( n \)-particle production we can use the Veneziano model \([13]\), which was (and is) an example of the solution to our first equation. In this model the hadron interaction was described as creation and decays of the resonances in such a way that the sum over all resonances reproduced the correct Reggeon asymptotic with the only one shortcoming: there was no the Pomeron in this model. However, the last point it was just that we needed to avoid the double counting in our calculation.

As a result of calculations we found \([14]\) that:

1. In a natural fashion we are able to get the constant total cross section at high energy;

2. The process of the multiparticle production has two stages: (i) the production of resonance with small fraction of the direct produced pions and (ii) the decay of resonances, which gives the final distribution of pions;

3. The typical distances in the first stage of the production process (resonance production) are rather small (typical transverse momenta are large) of the order \( \frac{1}{0.5 - 1 GeV} \). The small observed transverse momentum of pion (\( p_{\pi t} \propto \frac{1}{m_{\pi}} \)) is the result of the sequent resonance decays;

4. The cross section of hadron production with large transverse momentum (\( p_t \)) has scaling behaviour, namely, \( \sigma = \frac{1}{p_t} F(\frac{2p_t}{\sqrt{s}}) \);

5. The Pomeron has broader multiplicity distribution than the Poisson one for the produced pions due to the resonance decays;

6. The rapidity correlation between pions with like-charge (for example, for \( \pi^-\pi^- \)) has an exponential like fall down at large values of the difference in rapidity (\( \Delta y = y_1 - y_2 \))

\[
R(y_1, y_2) = \frac{\frac{d\sigma}{dy_1} dy_2}{\frac{d\sigma}{dy_1} dy_2} - 1 = R(0) e^{-|\Delta y|/L} 
\]

with the correlation length \( L \approx 1 \).

All these properties had not been known at that time but were confirmed experimentally later on.
4 Multiparticle Production.

The beauty of the Pomeron approach to high energy interaction was and, I believe, still is the possibility to describe in the unique pattern both the elastic or/and semielastic processes and the processes of the multiparticle production. The theoretical basis of this description is the Abramovsky, Gribov and Kancheli cutting rules (AGK) which recover the interrelation between both kind of processes if we know the Pomeron structure or in other words if we know for what kind of the multiparticle production processes is responsible the Pomeron exchange.

Having in hand a sufficiently reliable model for the Pomeron structure, the next natural step for us, M.G.Ryskin and me, was to check how the Pomeron interaction would reveal itself in the processes of multiparticle production. We did this in a number of papers (see Refs.[16]) and our results could be summarized as follows:

1. The Pomeron interaction lead to long range rapidity correlation in the multiparticle process and the correlation function could be given in the form:

\[
R(y_1, y_2) = \frac{d\sigma}{\sigma dy_1 dy_2} - 1 = R(0) e^{-|\Delta y|/L} + 2 \frac{\sigma_2}{\sigma_{tot}},
\]

where \(\sigma_2\) is the contribution to the total cross section the exchange of two Pomerons;

2. In the semi-inclusive processes where we measure the number of produced particle the long range part of the correlation is negligible;

3. The Pomeron interaction gives the sizable part of Bose-Einstein correlation which is the most dominant contribution for BE correlations in hadron-nucleus collisions (see Ref.[17]);

4. The Pomeron interaction leads the the KNO scaling in the multiplicity distributions which will be broken out only at the ultra high energies.

Summarizing our efforts to understand the Pomeron structure in the framework of the multiperipheral model, I think, that we demonstrated the possibility of such an approach and ability of this approach, taking into account the Pomeron interaction, to describe available at that time experimental data. We consider as the shortcoming of our approach the sufficient complexity of it and the lack of the theoretical basis. However, two results which came as outcome of our approach we took with us for future investigations: the two stage of the processes of multiparticle production and rather small distances essential for these processes.
Partons, time - space structure of interaction and the death of the Pomeron approach.

Looking around for a theory which could help us to understand the scattering amplitude at sufficiently small distances we naturally found ourselves among supporters and activists of the parton model suggested by Feynman[18], Bjorken[19] and Gribov[20]. This model proposed to look at a fast hadron as a system of noninteracting point-like particle-partons. These partons interact in normal way but with unknown (at that time) Lagrangian while the distribution of partons in a hadron has a definite value of the transverse momentum. At that time, such an approach looked for us more general that the specific field theory and more suitable to match with our multiperipheral approach for the Pomeron.

Naturally, what we did first we translated our multiperipheral result into parton language and found even more argument for the hypothesis that the mean transverse momentum ($q_t$) of partons large enough: $q_t > 1 \text{GeV}$ [21].

The second, what we did, was to reconsider our multiperipheral result for the high $p_t$ hadron production in the parton model. We found [22] that in the parton model this cross section can be written through the cross section of interaction of a parton from one hadron (say 1) with a parton from another (say 2) and through the convolution of the two parton densities or probabilities to find partons 1 and 2 in the hadrons 1 and 2, respectively. The power behaviour of the cross section is determined by the parton - parton cross section at angle about 90 degree. We found also that in the parton approach unlike the multiperipheral one each parton should produce the jet of hadrons with specific properties that have been studied by us.

The third, we found a simple interpretation of the AGK cutting rules in the framework of the parton model [23], which gave us an understanding that the AGK cutting rules is a general property of any field theory.

The parton model for us was a way to clarify the space - time picture of the hadron interaction at high energy [24]. Especially important for us was paper [25] in which we developed the space-time picture for hadron nucleus interaction, understood the physical meaning of the Glauber approach in the parton model and found the solution to the hadron-nucleus interaction in the parton approach. Unfortunately, this paper was done at the very end of the parton era and it passed unnoticed by high energy community, but I consider this paper as the best of mine on the parton approach and the ideas and methods, developed in this paper I have used in the new attack on the Pomeron structure in QCD.
Just during this period we, M.G. Ryskin and me, realized that the Reggeon Calculus and the Pomeron approach is deadly sick. We did not publish anything on this subject, throwing in the waste-basin all our efforts to save this approach, but this fight was our way to QCD. Actually, the death of the attempt to build the effective theory starting from the Pomeron as the Regge pole has a definite date - 1975. In 1975 McCoy and T.T. Wu [26] and Matinyan and Sedrokyan [27] showed that in the wide class of the field theories the exchange of the two Pomeron did not give the correct asymptotic at high energy. We realized very quickly that the new diagrams which have been suggested in Refs. [26] [27] have the correct space-time structure in the parton approach and therefore, the effective theory based on the Pomeron contribution and on the interaction between Pomerons has ceased to exist. We have to start from the beginning, looking for new ideas for the effective theory at high energy.

6 DIS at low \( x \) ( 20 years ago ).

Fortunately for us, L.V. Gribov, M.G. Ryskin and me, Prof. Gribov asked us a question which triggered our thinking in a right direction. The question was: “what happened with high energy asymptotic in the deep inelastic scattering (DIS) where we know the evolution equation and, therefore, we have a solid theoretical basis.” It took several years to find the answer. Let me list here what we found [28] [29] [30] [31]:

1. The DGLAP evolution equations [32] give correct asymptotic for the deep inelastic structure function \( F_2(x, Q^2) \), where \( Q^2 \) is the virtuality of photon and \( x \) is Bjorken variable, the energy of collision is equal \( s = Q^2 \) for all values of \( x \) such as \( \ln \frac{1}{x} \ll \ln Q^2 \). This asymptotic leads to increase of \( F_2 \) mainly due to the growth of the gluon density \( xG(x, Q^2) \) in a target

\[
xG(x, Q^2) \propto \exp\left\{\sqrt{\frac{4N_c\alpha_S}{\pi}} \ln \frac{Q^2}{Q_0^2} \ln \frac{1}{x}\right\}.
\] (3)

2. In the region of \( \ln \frac{1}{x} \approx \ln^2 Q^2 \) we have to take into account the correction to the DGLAP equation, mainly, related to so called leading log(1/x) approximation of perturbative QCD (LL(1/x)A). The LL(1/x)A have been studied by L.N. Lipatov and his collaborators [33]. We found that we can use for the asymptotic in our kinematic region the BFKL equation, but with one important additional ingredient: the running QCD coupling constant. We solve this equation within the accuracy that we needed ( in so called semiclassical approximation ). The result is that the gluon density is still rising in the region of low \( x \) even more rapidly than in the DGLAP evolution.
3. The increase of the gluon density leads to a new problem in deeply inelastic scattering, namely the violation of s-channel unitarity, the requirement that the total cross section for virtual-photon absorption be smaller than the size of a hadron

$$\sigma(\gamma^* N) \ll \pi R_h^2.$$  \hfill (4)

Using even the DGLAP result (see eq. (3)) one can see that the unitarity constraint will be violated at at $x < x_{cr}$, where $\ln \frac{1}{x_{cr}} = c \ln^2 Q^2$ and $c$ is well defined constant. The resolution of this problem cannot be in the confinement phenomena. We must look for the origin and solution of this problem within perturbative QCD.

4. We understood what happens in the region of small $x$ by examining the parton distribution in the transverse plane (see Fig.1). Our probe feels those partons with size $r_p \sim \frac{1}{Q}$. At $x \approx 1$ a few parton are distributed in the hadron disc. If we choose $Q$
such that \( r_p^2 \ll R_h^2 \) then the distance between partons in the transverse plane is much larger than their size, and we can neglect the interaction between partons. The only essential process is the emission of partons, which has been taken into account in QCD evolution. As \( x \) decreases for fixed \( Q^2 \), the number of partons increases. and at value of \( x = x_{cr} \), partons start to populate the whole hadron disc densely. For \( x < x_{cr} \) the partons overlap spatially and begin to interact throughout the disc. For such small \( x \) values, the processes of recombination and annihilation of partons should be as essential as their emission. However, neither process is incorporated into any evolution equation. What happens in the kinematic region \( x < x_{cr} \) is anybody’s guess. We suggested that parton density saturates, i.e. the parton density is constant in this domain.

5. To take interaction and recombination of partons into account we must identify a new small parameter that lets us estimate the accuracy of our calculation. We found this parameter:

\[
\kappa = xG(x, Q^2) \cdot \frac{\sigma(GG)}{\pi R_h^2} = \frac{N_c \alpha_S \pi}{2 Q^2 R_h^2} xG(x, Q^2),
\]

where \( \sigma \) is the cross section of gluon - gluon interaction and \( R_h \) is the size of a hadron. The numerical factor in eq. (5) was evaluated by Mueller and Qiu [34]. This parameter \( \kappa \) is the probability of a gluon recombination during the cascade. The unitarity constraint can be rewritten in the form

\[
\kappa \leq 1.
\]

We rewrote the amplitude of DIS as a perturbation series in this parameter which we resummed. The equation that we obtained can be easily understood by considering the structure of the QCD cascade in a fast hadron. Two processes occur inside the cascade:

Emission (1 \( \rightarrow \) 2) with probability \( \propto \alpha_S \rho \);

Annihilation (2 \( \rightarrow \) 1) with probability \( \propto \alpha_S^2 r_p^2 \rho^2 \propto \frac{\alpha_S^2}{\xi^2} \rho^2 \);

where \( \rho \) is the density of gluons (\( \rho = \frac{xG(x, Q^2)}{\pi R_h^2} \)). The number of partons in a phase space cell (\( \Delta \Phi = \Delta \ln(1/x) \Delta \ln Q^2 \)) increases due to emission and decreases due to annihilation. Thus the balance equation reads:

\[
\frac{\partial^2 \rho}{\partial \ln \frac{1}{x} \partial \ln Q^2} = \frac{N_c \alpha_S \pi}{\rho} - \frac{\alpha_S^2 \gamma}{Q^2} \rho^2,
\]

or in terms of the gluon structure function:

\[
\frac{\partial^2 xG(x, Q^2)}{\partial \ln \frac{1}{x} \partial \ln Q^2} = \frac{N_c \alpha_S \pi}{xG(x, Q^2)} - \frac{\alpha_S^2 \gamma}{\pi Q^2 R_h^2} \frac{(xG(x, Q^2))^2}{xG(x, Q^2)}.
\]

This is so-called the GLR equation. The factor \( \gamma \) has been calculated by Mueller and Qiu and it is equal [34] \( \gamma = \frac{N_c \pi}{4} \).
6. We found the semiclassical solution to the GLR equation. It turns out that this equation has a critical line:

\[
\ln \frac{1}{x_{cr}} = \frac{b}{32N_c} \ln^2 \left( \frac{Q^2}{\Lambda^2} \right),
\]  

(9)

where \( \alpha_s(Q^2) = \frac{4\pi}{b \ln(Q^2/\Lambda^2)} \). For \( x > x_{cr} \), it suffices to find the solution of the linear DGLAP equation with the new boundary condition on the critical line. For \( x < x_{cr} \) we have a separate system of trajectories and solution does not depend on the solution for \( x > x_{cr} \). We found that the GLR equation is not the right tool to solve the problem in this region.

7. We realized that the critical line gives a new scale for the value of the typical transverse momentum in the parton cascade, namely

\[
q_t^2 = q_0^2(x) \big|_{x \to 0} \to \Lambda^2 e^{\sqrt{\frac{3\pi c}{b}} \ln \frac{1}{x}}.
\]

(10)

This new scale plays a role of the infrared cutoff in all inclusive processes and leads to a number of prediction. The most important from them is the fact that the main contribution to the production processes gives the minijet production. We have studied these predictions in our paper during the past decade[35].

7 DIS at low x (during the past 5 years).

During the last 5 years I have tried to understand better the relation between our approach and Wilson Operator Product Expansion. It turns out (see Ref.[36]) that all high twist operators become important just in the kinematic region near to the critical line. We can reformulate the problem of the low x asymptotic as the problem to find the anomalous dimension for high twist operators. In Ref. [36] we found the anomalous dimension for twist four operator. We realized also that we made a mistake in our estimates of the contribution of the diagrams that killed the Pomeron approach (see also the paper of J.Bartels who did this first [37]). The next step of my approach to the problem was to find the anomalous dimension for all high twist operators and to obtain a new evolution equation that replace the GLR one. Eric Laenen and me found the solution to these two problems in our papers [38][39] as well as the solution to the new evolution equation. However, this new progress did not change the qualitative picture that I have described in the previous section.

We also developed the approach how to take into account the new scale for the typical transverse momentum in our usual approach to hard processes based on factorization theorem [40]. We (M.G.Ryskin, A.G.Shuvaev, Yu.M. Shabelsky and me) proposed so called
the transverse momentum factorization \[41\] simultaneously with S.Catani,M, Ciafoloni and F.Hauptmann and J.Collins and R.K. Ellis \[42\]. In this approach we introduce the unintegrated gluon structure function and the cross section of the interaction between partons off mass shell. Using these new ingredients we were able to write the convolution formulae in the analogous way with the usual approach. We proved that both of these new values can be calculated using the evolution equation at least in the leading log approximation of perturbative QCD.

Part of my activity was devoted to better understanding of the evolution equations in the region of small \(x\). The main questions that we approached were the interrelations between the DGLAP and BFKL evolutions \[43\] \[44\], the estimates for higher order corrections \[44\], the evolution equations for the diffractive dissociation processes \[45\] and the nonperturbative contribution for the BFKL equation stems from the infrared and ultraviolet renormalons \[46\].

8 DIS at low \(x\) (now ).

1. My contribution:

The intensive experimental study of the low \(x\) behaviour of the deep inelastic structure functions at HERA rises a number of questions to the theorists. Two of them, namely, where is the BFKL Pomeron and where is the SC, are under my close investigation. We are only in the beginning and what has been done is the new evolution equation which is able to describe the region of \(x < x_{cr}\) \[47\]. For fixed \(\alpha_s\) this equation can be written in terms of \(\kappa\) (see eq. (5) and has a form:

\[
\frac{\partial^2 \kappa(x, Q^2)}{\partial \ln \frac{1}{x} \partial \ln Q^2} + \frac{\partial \kappa}{\partial \ln \frac{1}{x}} = \frac{N_c \alpha_s}{\pi} \{C + \ln \kappa + E_1(\kappa)\},
\]

where \(C\) is the Euler constant and \(E_1\) is the exponential integral. We solve this equation in semiclassical approximation and showed that the result gives weaker shadowing corrections than the GLR equation. I am going to work on this problem in the nearest future and my main idea to resolve the difficulties that I have pointed out is to prove that the BFKL Pomeron is hidden under sufficiently strong SC while the SC theirselves were taken into account as the initial condition for the evolution equations. Much work is needed to clear up the situation.

2. Two different theoretical approaches:

I would like to mention here that actually we have two theoretical approach to the high parton density QCD. To understand these two approaches we have to look at the picture
of a high energy interaction in the parton model (see Fig. 2). In the parton approach the fast hadron decays into point-like particles (partons) long before (typical time $\tau \propto \frac{E}{\mu^2}$) the interaction with the target. However, during this time $\tau$, all partons are in the coherent state which can be described by means of a wave function. The interaction of the slowest ("wee") parton with the target completely destroys the coherence of the partonic wave function. The total cross section of such an interaction is equal to

$$\sigma_{\text{tot}} = N \times \sigma_0$$

(12)

where

- $N =$ flux (renormalized ?!) of "wee" partons;
- $\sigma_0 =$ the cross section of the interaction of one "wee" parton with the target.

One can see directly from Fig. 1 that the number of "wee" partons is rather large and it is equal to

$$N \propto e^{\langle n \rangle} = \frac{1}{x \omega_0} \text{ with } \langle n \rangle = \omega_0 \ln(1/x) ; \omega_0 = C\alpha_S .$$

(13)

We have to renormalize the flux of "wee" partons, since the total cross section is the number of interactions and if one has several "wee" partons with the same momenta they only give rise to one interaction.

If $N \approx 1$, we expect that the renormalization of the flux will be small, and we use an approach with the following typical ingredients:

- Parton Approach;
- Shadowing Corrections;
- Glauber Approach;
- Reggeon-like Technique;
- AGK cutting rules.
However, when $N \gg 1$, we have to change our approach completely from the parton cascade to one based on semiclassical field approach, since due to the uncertainty principle $\Delta N \Delta \phi \approx 1$, we can consider the phase as a small parameter. Therefore, in this kinematic region our magic words are:

- Semi-classical gluon fields; • Wiezsäcker-Williams approximation; • Effective Lagrangian for hdQCD; • Renormalization Wilson group Approach.

It is clear, that for $N \approx 1$ the most natural way is to approach the hdQCD looking for corrections to the perturbative parton cascade. In this approach the pQCD evolution has been naturally included, and it aims to describe the transition region. The key problem is to penetrate into the hdQCD region where $\kappa$ is large. Let us call this approach “pQCD motivated approach”. Namely, in this approach we obtained eq. (11) which we consider as a correct tool to evaluate a high parton density effects in DIS.

For $N \gg 1$, the most natural way of doing is to use the effective Lagrangian approach, and remarkable progress has been achieved both in writing of the explicit form of this effective Lagrangian, and in understanding physics behind it [48]. The key problem for this approach was to find a correspondence with pQCD. This problem has been solved [49].

Fig.3 shows the current situation on the frontier line in the offensive on hdQCD.

Much more work has to be done before we will be able to find a solid theoretical description of the kinematic region of hdQCD. The “hot” problem is to find a matching between two different approaches. The first step has been done in this direction: in Ref. [50] was proven that the effective Lagrangian approach gives the GLR equation for the limit of sufficiently small parton densities. However, the interrelation between eq. (11) and the equation suggested in Ref. [51] is still unclear.

9 Conclusion.

Not only me but many people are working on the problem of the Pomeron structure. My main idea was and is to look on this problem going from inside of the hadron or in other words from small distances. Going in this direction we always feel the support of perturbative QCD which allows us to check our imagination and to foresee the direction of each new step in our attempts to solve the Pomeron problem. What is the Pomeron is still an open question and it is a challenge for all physicists who like to solve a difficult problem. The school of high energy phenomenology and the perturbative calculation created by Prof. Gribov in the theory department of St.Petersburg (Leninhgrad) Nuclear
Physics Institute and pushed forward by my generation of physicists made this department well known through all the world. The problem of the Pomeron structure, even has not been solved yet, gives a good training for learning of most of the secrets of the perturbative QCD approach and for the improving of the calculation skill. It is a window to all difficult problems of QCD and any field theory approach. I am firmly believe that the new ideas are needed to make a progress. I ask a young theorist to look back on my life with the Pomeron and admit that it was not so bad, at least, it was and is rather interesting life without any boring situation, with many ups and downs, but always with some perspective for further progress.

10 My several words about Prof. Gribov.

Of course, I was a lucky guy having such a great teacher as Prof. Gribov, who was one of the most outstanding physicists in this century. I was lucky twice, since I had a three year experience of doing physics with him. Formally speaking, we were doing several papers on the Reggeon - Pomeron interactions. Really, we tried to get a picture of high energy interaction based on the most advanced theory of that time. It was a hard work and I
learned several lessons which I follow during my life in physics and which I want to share with you.

1. **Physics - first.** He always tried to understand the result without mathematics, he did not trust the formal derivation of anything, he wanted to understand what physics were used in the calculation. The best for him was to obtain a result without calculation. Each day of working with him we started with the question: “What is the small parameter in our approach?” He called this process to create a picture. He was really unhappy when he could not create such a picture, in which everything should be selfconsistent and should not contradict any theoretical result or/and any experimental data.

Let me illustrate my point telling you one story with me. It was a long ago at that time when we thought that there were only Reggeons and nothing except Reggeons. I found a paper with the data of Lindenbaum where was a dip in $t$-dependence in some reaction. I was young and remembered everything that was thought in the university. I came to Prof. Gribov, showed him the data and told him that from my point of view it was very natural to have such a dip because of diffraction. After that I asked a naive question: “Why we cannot describe such a simple physical phenomena in the Reggeon approach.” He answered me something. I forgot what. However, he understood that he failed to convince me. After that, during the next two years, he was always coming back to my question and, finally, when he understood that shadowing corrections or exchange of Pomerons were unavoidable ingredients of our theory he was happy to explain me, how we should describe diffraction in the Pomeron approach.

He could not live without selfconsistent picture. He spent the past two decades in a some sort of isolation only because he could not find a physical picture for such an important physical phenomena as confinement of quarks and gluons. Unfortunately, he went away before explaining us what picture he has finally found. Unfortunately, it is almost impossible to understand everything from his paper. Could ancient Greek, the smartest one, understand one of our paper translated to the perfect Greek language of his time? I think, he could not. I feel as this Greek talking with Prof. Gribov. You have to live with physical problem, to nurse it and to think about the problem as he did to understand his picture.

2. **Mathematics is only a tool.** The knowledge of mathematics was requited but there was no a slight respect to it. Mathematics is only a tool like a hammer to nail something.

Once more, a story with me. It was an equation which even we did not put on the paper. He gave a solution of it asking me to check if everything is correct. I took a
couple of sheets of paper covered by formulae. To my surprise, I immediately found
that the first line was wrong. My lord, the second also turns out to be incorrect, the
third, the fourth.... So everything was wrong but the answer was absolutely correct!
Next day, I was very proud and came with a pile of papers with correct solution.
The most instructive for me was his reaction. He said: “Genya, why you spent so
much time recovering how to get this solution. Why you did not check, that this
is a solution, directly substituting my formula into equation?” For him it did not
matter how you got the solution of physical problem. The only thing, that counted,
was to get a solution.

3. Physics is the experimental science. Prof. Gribov was proud being theorist because a
thorist has a privilege to know all experiments and to create out of them a physical
picture. He was always saying that theory costs nothing if it contradicts a single
experimental fact.

He was upset when the first data from Srpukhov accelerator came showing that
there was a large difference between $K^+p$ and $K^-p$ total cross sections. He sent
me to ITEP to discuss with Prof. Okun’ and Prof. Ter-Martirosian these new data
which seemed to contradict the Pomeranchuk theorem. He was unhappy until the
example was built that the Pomeranchuk theorem could be violated without any
harm for theory.

4. Two duties. Prof. Gribov was a man who enjoyed a freedom of thinking and we had
to follow only two rules that restricted our freedom. Any member of our department
has only two duties:

(a) To attend any seminar in our department or in other places that you visit;
(b) To answer any question that an experimentalist ask you.

Our seminars were the place where we worked. They lasted as much time as we
needed to understand a problem. Sometimes we needed several days to do this.
Prof. Gribov was the leader, he understood better and attacked a poor speaker
first. A speaker had to have the guts to stay against these attacks and, in spite of
all obstacles, he had to try to defend himself. The tactics was very simple: try to
formulate during the first ten minutes the physical problem that you solved and the
result. If Prof. Gribov would understand your problem, he would help you to find a
solution to this problem. Of course, he would not listen to you for the first one hour
since he would try to solve a problem by himself. Sometimes, he did this and after
that you were a lucky guy ,if his solution coincided with yours. If not, he would try
to convince you that his solution was better. Sometimes, he would not been able
to find a solution. In this case, you were in a good situation. You could tell Prof. Gribov something new and got his full respect.

5. *The full responsibility.* You are responsible for any quoted result as it is your own. Presenting your talk in the seminar, you have to answer any question on the subject of your talk, especially, if you referred to some results of others. You cannot tell that this theorem was proven by somebody. You have to present the proof which will be scrutinized together with your ability to understand your subject. The rules were simple - no publication without presenting your paper in our seminar.

Unfortunately, the culture of such working seminars is passing away. Outside Russia I met only in two places the same atmosphere as was in Gribov’s seminars: in our Tel Aviv and in Minnesota Universities, but even there the seminar is restricted by one hour.

I grew up as a physicist under strong influence of Prof. Gribov. I am afraid that for the rest of my life I will look at physics with his eyes. We lost him too earlier to cope with this fact. I hope, that he will live in us. This is an idea behind this report on my own way to approach his beloved problem - the Pomeron.

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**References**

[1] M. Froissart: *Phys. Rev* **123** (1961) 1053.

[2] A. Martin: *“Scattering Theory: Unitarity, Analyticity and Crossing.”*, Lectures Notes in Physics, Springer - Verlag, 1969.
[3] V.N. Gribov: *Sov. Phys. JETP* 53 (1967) 654.

[4] V.N. Gribov and A.A. Migdal: *Sov. Phys. JETP* 55 (1968) 1498; *Sov. J. Nucl. Phys.* 8 (1968) 1002.

[5] V.N. Gribov, E.M. Levin and A.A. Migdal: *Sov. J. Nucl. Phys.* 11 (1970) 673; 12 (1970) 173; *Sov. Phys. JETP* 32 (1970) 1158.

[6] V.A. Kudryavtzev, E.M. Levin and A.A. Schipakin: *Sov. J. Nucl. Phys.* 9 (1969) 1274; 10 (1969) 1748; 11 (1970) 858.

V.A. Kudryavtzev and E.M. Levin: *Sov. J. Nucl. Phys.* 18 (1973) 451.

[7] Ya.I. Azimov, E.M. Levin, M.G. Ryskin and V.A. Khoze: *Nucl. Phys.* B89 (1975) 508; *Sov. J. Nucl. Phys.* 21 (1975) 413; 23 (1976) 853.

Ya.I. Azimov, E.M. Levin, M.G. Ryskin, M.I. Strikman and V.A. Khoze: *Sov. Phys. JETP Lett.* 23 (1976) 121.

E.M. Levin, M.G. Ryskin, M.I. Strikman and G.G. Takhtamyshev: *Nucl. Phys.* B123 (1977) 1020.

[8] Ya.I. Azimov, E.M. Levin, M.G. Ryskin and V.A. Khoze: IX Winter Leningrad School, v.II, p.5, 1974.

[9] E. Gotsman, E.M. Levin and U. Maor: *Z. Phys* C57 (1993) 667; *Phys. Lett.* B309 (1993) 109; B347 (1995) 424; *Phys. Rev.* D49 (1994) R4321.

[10] F. Low: *Phys. Rev.* D12 (1975) 163; S. Nussinov: *Phys. Rev. Lett.* 34 (1975) 1286, *Phys. Rev.* D14 (1976) 244.

[11] D. Amati, S. Fubini and A. Stangellini: *Nuovo Cim.* 26 (1962) 826.

[12] M. Baker and K.A. Ter-Martirosyan: *Phys. Rep.* 28C (1976) 3 and references therein.

[13] G. Veneziano: *Nuovo Cim.* 57A (1968) 190; *Phys. Rep.* 9 (1974) 199.

[14] E.M. Levin and M.G. Ryskin: *Phys. Lett.* B41 (1972) 681; *Sov. Phys. JETP Lett.* 16 (1972) 495; 17 (1973) 669; *Sov. J. Nucl. Phys.* 17 (1973) 388; 18 (1973) 431, 1108.

[15] V.A. Abramovsky, V.N. Gribov and O.V. Kancheli: *Sov. J. Nucl. Phys.* 18 (1973) 308.

[16] E.M. Levin and M.G. Ryskin: *Sov. Phys. JETP Lett.* 17 (1973) 669; 18 (1973) 654; *Sov. J. Nucl. Phys.* 19 (1974) 389, 669, 904; 20 (1974) 519; 21 (1975) 352, 1072, 1281; 22 (1975) 428; 23 (1976) 423; 24 (1976) 640.
[17] A.Capella, A. Krzywicki and E.M. Levin: *Phys. Rev. D*44 (1991) 704.

[18] R.P. Feyman: *Phys. Rev. Lett.* 23 (1969) 1415; “Photon-Hadron Interaction” N.Y. Benjamin, 1972.

[19] J.D. Bjorken: “Proceedings of the Int. Symposium on Electron and Photon Interaction at High Energy” p.281, Cornell, 1971 and references therein.

[20] V.N. Gribov: *Sov.J.Nucl.Phys* 9 (1969) 640; “Proc. VII LNPI Winter School” v.II,p.5, Leningrad 1973.

[21] E.M. Levin and M.G. Ryskin: *Sov. Phys. JETP* 69 (1975) 412.

[22] E.M. Levin and M.G. Ryskin: *Sov.J.Nucl.Phys.* 19 (1974) 519, 22 (1975) 428.

[23] E.M. Levin and M.G. Ryskin: *Sov.J.Nucl.Phys.* 25 (1977) 349.

[24] E.M. Levin and M.G. Ryskin: *Sov.J.Nucl.Phys.* 27 (1978) 790,29 1311.

[25] E.M. Levin and M.G. Ryskin: *Sov.J.Nucl.Phys.* 31 (1980) 429.

[26] B.M. McCoy and T.T. Wu: *Phys. Rev.* D 12 (1975) 546,577.

[27] S.G. Matinyan and A.G. Sedrokyan: *Sov.Phys.JETP lett.* 23 (1976) 588, 24 (1976) 240, *Sov.J.Nucl.Phys.* 24 (1976) 844.

[28] L.V. Gribov, E.M. Levin and M.G. Ryskin: *Sov.Phys.JETP* 80 (1981) 185, *Phys. Lett.* B100 (1981) 173, B101 (1981) 185, B121 (1983)65, *Nucl.Phys.* B188 (1981) 555, *Sov.J.Nucl.Phys.* 35 (1981) 1278.

[29] E.M. Levin and M.G. Ryskin: *Phys. Rep.* 100 (1983) 1.

[30] E.M. Levin and M.G. Ryskin: *Phys. Rep.* 189 (1990) 267.

[31] E. Laenen and E. Levin: *Ann. Rev. Nucl. Part.Sci* 44 (1994) 199.

[32] V.N. Gribov and L.N. Lipatov: *Sov. J. Nucl. Phys.* 15 (1972) 438; L.N. Lipatov: *Yad. Fiz.* 20 (1974) 181; G. Altarelli and G. Parisi: *Nucl. Phys. B* 126 (1977) 298; Yu.L.Dokshitzer: *Sov.Phys. JETP* 46 (1977) 641.

[33] E.A. Kuraev, L.N. Lipatov and V.S. Fadin: *Sov. Phys. JETP* 45 (1977) 199; Ya.Ya. Balitskii and L.V. Lipatov: *Sov. J. Nucl. Phys.* 28 (1978) 822; L.N. Lipatov: *Sov. Phys. JETP* 63 (1986) 904.

[34] A.H. Mueller and J. Qiu: *Nucl. Phys.* B268 (1986) 427.
[35] E.M. Levin and M.G. Ryskin: *Sov. J. Nucl. Phys.* **45** (1987) 234,*47* (1988) 1398,*50* (1989) 881,*53* (1991) 653, *Nucl. Phys.* **B304** (1989) 805.

[36] E.M. Levin, M.G. Ryskin and A.G. Shuvaev: *Nucl. Phys.* **B387** (1992) 589.

[37] J.Bartels: *Phys. Lett.* **B298** (1993) 204, *Z. Phys.* **C60** (1993) 471.

[38] E.Laenen, E. Levin and A.G. Shuvaev: *Nucl. Phys.* **B419** (1994) 139.

[39] E. Laenen and E. Levin: *Nucl. Phys.* **B451** (1995) 207.

[40] J.C.Collins, D.E. Soper and G. Sterman: *Nucl. Phys.* **B308** (1988) 833.

[41] E.M. Levin, M.G. Ryskin, Yu.M. Shabelsky and A.G. Suvaev: *Sov. J. Nucl. Phys.* **53** (1991) 653.

[42] S.Catani, M.Ciafaloni and F.Hauptmann: *Phys. Lett.* **B242** (1990) 97,*Nucl. Phys.* **B366** (1991) 135; J. C. Collins and R.K. Ellis: *Nucl. Phys.* **B360** (1991) 3.

[43] G. Marchesini, E.M. Levin, M.G.Ryskin and B.R. Webber: *Nucl. Phys.* **B357** (1991) 167.

[44] R.K. Ellis, E.M. Levin and Z. Kunszt: *Nucl. Phys.* **B420** (1994) 517.

[45] E.M. Levin and M. Wuesthoff: *Phys. Rev.* **D50** (1994) 4306.

[46] E. Levin: *Nucl. Phys.* **B453** (1995) 303.

[47] A.L. Ayala, M.B. Gay Ducati and E.M. Levin: *Nucl. Phys.* **B511** (1998) 355,*B493* (1997) 305.

[48] L. McLerran and R. Venugopalan: *Phys. Rev.* **D49** (1994) 2233,3352,*D50** (1994) 2225,*D53** (1996) 458; J. Jalilian-Marian, A. Kovner, A. Leonidov and H. Weigert: hep - ph/9701284,*hep-ph/9706377*; J. Jalilian-Marian, A. Kovner, L. McLerran and H. Weigert: *Phys. Rev.* **D55** (1997) 5414; Yu. Kovchegov: *Phys. Rev.* **D54** (1996) 5463; Yu. Kovchegov and A.H. Mueller: *hep-ph/9802440*.

[49] J. Jalilian-Marian, A. Kovner, A. Leonidov and H. Weigert: *hep - ph/9701284,* *hep-ph/9706377*.

[50] J. Jalilian-Marian, A. Kovner, A. Leonidov and H. Weigert: *hep-ph/9807462*.