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I describe through a few anecdotes, Gribov's work on the high energy limit of strong interactions.

1. Introduction

When I was a graduate student in the early 70's, there was much interest in the physics of strong interactions. Among the topics of central interest were: How are cross sections computed in strongly interacting theories? How are particles produced? How does this depend on the energy and species of colliding particles? These were the issues which drove many of my generation into theoretical particle physics.

The directions research took in trying to answer these questions were unexpected. For example, the dual resonance Veneziano model was proposed to explain basic features of high energy interactions [1]. One of the basic results of the Veneziano model was a beautiful and symmetric formula for cross sections of strongly interacting particles. It arose from a duality of the scattering amplitude under interchange of resonances in the s and t channels. The spectrum of these resonances, and the formula for the scattering amplitude could be derived by postulating a set of operators with a Virasoro algebra [1]. These operators had a simple interpretation in terms of the dynamics of a relativistic string. The origins of string theory arose from this attempt to explain measured properties of strong interactions [2].

Modern day string theory has largely evolved away from strong interactions, and is now thought of as an attempt to understand as yet unmeasured properties of gravity at extremely short distance scales. It has become a mathematical structure which survives more by its own arcane beauty than by its relationship with the world as we know it.

On the other hand, in the early 70's QCD had not arrived as a full fledged theory with the potential to describe strong interactions. It arose largely from combinations of attempts to understand masses of baryons and mesons, features of scattering of electrons from nucleons, and the mathematical beauty of Yang-Mills theory [3]. The combination of Bjorken's beautiful and seminal interpretation of electron-nucleon scattering in terms of underlying point-like degrees of freedom [4], quarks and gluons, and Gell-Mann's deep insight into hadron spectroscopy [5], together with 't Hooft and Veltman's development of techniques which made Yang-Mills theories computable [6], led in the mid-seventies to a revolution in our understanding of the nature of strong interactions. They arose from an underlying Yang-Mills theory of quarks and gluons. Early in this development came Gribov, Bjorken, and Feynman's development of pictures of the space-time evolution of high energy scattering, and their interpretation of it in terms of underlying degrees of freedom [4, 7, 8]. Another significant contribution was Dokshitzer, Gribov Lipatov, Altarelli and Parisi's determination of the evolution of the quark structure function in deep inelastic scattering as the resolution scale of the virtual photon exchanged between target and electron varied [9–11].

Ultimately, the computation of the dependence of the coupling constant of Yang-Mills theory upon distance scale by Gross, Wilczek, and Politzer, fundamentally changed the way in which we could understand strong interactions, and allowed us to compute a class of phenomena where the interaction strength of QCD is small [12, 13]. As a result of being able to compute some class of phenomena in QCD, a hierarchy of classes of problems began to evolve. The "interesting class" was defined to be that which you could reliably compute. This class represented very special processes which occur only at very short distance scales where the coupling constant of Yang-Mills theory is very weak. The "uninteresting" class included the overwhelming majority of phenomena of strong interaction physics: Cross sections, particle production, the quark and gluon distributions inside of nucleons, and the spectrum of baryons and mesons, represent a few examples.

As time has proceeded, QCD has come to be described as a "solved theory", or as "uninteresting" or labeled with other put-downs, implicit or direct. This is because it contains the sector of "interesting" physics that now has been solved; we understand the fundamental interactions in the QCD Hamiltonian. Many people earlier interested in QCD moved on to supersymmetric theories, possibly testable at the LHC. After supersymmetry, there was supergravity and superstrings, and with each theoretical advance we moved further away from observable phenomena and ideas which can be tested against experiment.

Meanwhile, the "uninteresting" problems remained largely not yet understood.

It is the purpose of this short paper to qualitatively explain Gribov's contributions to this "uninteresting" area of theoretical physics. Gribov was involved in deep, seminal and important developments in this understanding. Many of Gribov's contributions arise from deep physical insights, but his insights developed not simply from some mystical vision but also from doing many hard computations. He no doubt traveled many wrong paths, turned back, found another wrong path – yet ultimately found a correct path and proceeded towards deeper understanding. One difference between a creative person and a fool is that while both make many mistakes in reasoning, a creative person understands he or she has made a mistake. Sometimes these "mistakes" become great new ideas.

In this paper, I hope to convince the reader that there is something of fundamental importance to be learned from the class of "uninteresting" problems of particle and nuclear physics , and to introduce some more modern ways of posing, and perhaps understanding, the problems that Gribov was the among the first to conceptualize.

This paper is not about Gribov's contributions to the understanding of "interesting phenomena" in QCD. That work is perhaps better known, and was essentially the understanding of how quark distribution functions evolve as the resolution scale of the virtual photon changed in deep inelastic scattering. It has had a profound influence on our understanding of QCD. The work which I will describe is related to this and provides some of the intellectual basis for it. It has had a deep and lasting influence on the thinking of me and my colleagues.

I also want to tell a few anecdotes about Gribov. These anecdotes I think can convey some of his vitality and life force. Gribov was a rare man. Those of us fortunate enough to have met him knew his intensity and his strong sense of right and wrong. For Gribov, there was good physics, and nonsense – very little else between. He was a man of intense energy, emotion, and curiosity. He was filled with a passion for physics that seemed to dominate the way he related to others, and directed the way in which he saw the world around him. I never saw a Gribov relaxed or satisfied with himself.

I learned that with my Russian colleagues their deepest compliments were expressed simply, rather than with the use of extravagant adjectives.

One does not use the phrase "a real man" lightly. I greatly admired Volodya, and was not surprised to hear him called a Real Physicist, perhaps the highest complement you can give to a colleague.

2. The Pomeron and the Reggeon

The Pomeron is the collective excitation of QCD responsible for controlling the high energy limit of strong interactions. It is a single entity, which makes it universal and of fundamental interest. In modern language, it is thought of as a composite excitation made of gluons [14]. (This description of the Pomeron and what follows has been somewhat oversimplified in order to make it understandable to non-experts.) In the early days of strong interactions, it was abstracted from theoretical analysis of partial wave sums from scattering amplitudes. It was discovered that if one replaced the sum over angular momentum by a dispersion integral, then one could analytically continue into the complex angular momentum plane. Poles in the complex plane corresponded to Reggeons, and gave a characteristic power law dependence of the cross section on energy. There was a contribution which gave the leading high power law dependence in energy, and this was the Pomeron, named after the great Russian theorist Pomeranchuk. One can prove that the Pomeron, if it is indeed a simple pole, will lead to a constant asymptotic dependence of the cross section upon energy. Because of this, and the Froissart bound that cross sections can asymptotically rise at most as $\log^2(E)$, the Pomeron is the leading Regge pole. This led people to conjecture that cross sections asymptotically become constant.

In the early 70's, the first results from the Serpukhov accelerator at Protvino near Moscow came out for the pp cross section. It surprised nearly everyone, showing that cross sections were rising, not asymptotically going to a constant. This led to a conjecture that there was a bare Pomeron, which was a simple Regge pole which gave a positive power law dependence on the energy. This bare Pomeron was only a lowest first order approximation to the theory, and was presumably corrected in higher orders in the strength of Pomeron interactions, and these corrections would, hopefully, makes for a fully interacting theory which had cross sections growing with at most a $\log^2(E)$. Thus was the birth of Gribov's Reggeon calculus [15].

I first heard of Gribov as a graduate student at the University of Washington in Seattle. My advisor, Marshall Baker, took a year's sabbatical at ITEP in Moscow where he began collaborating with Karen Ter-Martirosian. When Marshall returned from Moscow he insisted with excitement that

I learn everything possible about the Reggeon calculus. The first thing he wanted me to do was to produce a proof of the Abramovsky-Gribov-Kancheli cutting rules [16]. These were a set of relations between the various imaginary parts of multi-Pomeron exchange diagrams. They follow from rather general conditions on the analyticity properties of scattering amplitudes (as I learned after a year of intense work).

Along the way to understanding these cutting rules, I studied Gribov's space-time picture of high energy interactions, a picture developed by Gribov in the late 60's and early 70's. As far as I can tell it was developed at about the same time as the parton model which was developed by Feynman and Bjorken, and independently of it. I basically learned about this area twice in my life, once from Gribov's papers and once later from listening to Bj. At the time I did not draw a strong connection between the two arguments since Gribov's arguments were dressed in the language of Feynman diagrams involving the exchange of ladders of mesons, and Bjorken's arguments were in terms of classical concepts laced with just the right amount of quantum mechanics.

These related approaches had a very strong effect on me. They led me to my interest in high energy nucleus-nucleus collisions, and to my interest in using these collisions as a tool to make and study new forms of matter. The ideas are at the very core of the problem I have spent the last ten years working on, the nature of matter which controls the high energy limit of QCD, the Color Glass Condensate. The glassy nature of this matter follows directly from time-dilation arguments originated in the work of Gribov, Bjorken and Feynman.

Gribov's Reggeon calculus was a field theoretical attempt to try to sum up multi-Pomeron interactions. It turns out that the basic interaction strength between three Pomerons, the triple Regge vertex, is imaginary. This lead to an amusing field theory, which is basically non-relativistic many-body theory with a complex Hamiltonian. Much work was done in trying to solve such a theory, but the ideas were being developed at the same time that the J/Ψ was discovered. This discovery, when combined with scaling seen in deep inelastic scattering as described by Bjorken, with the discovery that one can compute in non-abelian gauge theories by 't Hooft and Veltman, and with the discovery of asymptotic freedom by Gross, Politzer and Wilczek, led the entire community away from this problem to the fundamental issue of whether or not QCD is the correct theory of nature.

QCD studies drifted away from trying to understand high energy particle interactions. Interesting phenomena became defined to be either those phenomena which take place at high energy AND short distances, or the non-perturbative problems of confinement and mass generation. However, among the Russian community – particularly in the St. Peterburg and ITEP institutes where Gribov's work has been very influential – interest in high energy interactions thrived. In recent years, it has acquired some new dimension, since it appears that the high energy limit where there are very many Pomerons being simultaneously exchanged is also the limit where the gluon density becomes large. This is the Color Glass Condensate which I alluded to before, and now wish to sketch showing how it is built using concepts found in Gribov's work [17]:

- Color: Gluons are colored.
- Glass: According to arguments offered by Bjorken, Gribov and Feynman, during the time that fast moving particles sweep by a target, their wavefunction does not evolve. This is due to time dilation of the fast moving particles. Glasses are systems which one expects to change on natural time scales as does a liquid, but the time evolution is slowed down, and over very long times compared to natural time scales, the glass does not evolve.
- Condensate: Gribov argued that at very high energies, the growing total cross section could be understood by multiple Pomeron exchange. We now understand Pomerons as composite quasi-particles made of gluons. When the density of Pomerons becomes large so does the density of gluons. The density of gluons becomes so high that they condense, and are highly coherent.

The Color Glass Condensate is, I believe, the solution to the problem posed by the Reggeon calculus. The universality of this matter follows from the universality of Pomeron interactions.

3. Meeting Gribov

The first time I met a physicist named Gribov, I met Leonya, not Volodya. This was during my first visit to Russia in 1984. It was a black time then, and many physicists were boycotting going to Russia because of its mistreatment of Sakharov, and its bad human rights record. I had an opportunity to get to know several Russian physicists in Finland a year before, and they encouraged me to visit. I talked things over with my good friend Keijo Kajantie, who had managed to maintain good relations with

Russian colleagues, and decided to go. It was a good decision. I began many lifelong friendships as a result of the visit, and discovered that with remarkably few exceptions the Russian theoretical community showed little liking for the policies of their government. I remember Leonya as a tall, dark haired young man of great intensity. He had just finished an epic work with Genya Levin and Misha Ryskin on deep inelastic scattering. This work has proven to be something close to the Bible of deep inelastic scattering for the past 20 years [18].

I think Leonya would have become a major figure in our field had he lived. Unfortunately, while on a hiking trip with Mitya and Misha Diakonov, he fell through a crevasse in the Pamirs.

I suspect the pain of this loss caused a certain alienation of Volodya from the St. Petersburg school which he had built. Gribov had left for Moscow several years before, but had maintained strong ties with the St. Petersburg group. Contributing to that alienation, perhaps, was the growing tension between various groups of people. People who work together all their lives can develop somewhat strained personal relations. This happens in all groups but it is very painful to watch in a group which is your creation. Of course, such dynamics is quite complicated, and the growing tension might also have been due to the fact that Gribov with his strong personality was not present to provide cohesiveness for the group.

About four years later, I met Volodya Gribov at Fermilab, where I was on the permanent staff. This was the time of Perestroika. Many scientists who had earlier been forbidden to travel – a class that included just about all the active physicists in the Soviet Union – were suddenly able to travel like ordinary human beings. At Fermilab, there was a perpetual delegation of visiting experimentalists. They had their local command structure complete with a nachalnik who was in charge. I came to know the nachalniks quite well, and they all seemed to be decent people. In fact one of my nachalnik friends used the first available opportunity to remain in the US. (The only demonstration of his power that I ever saw him exercise occurred after I drove him down from Independence Pass near Aspen where we had been fishing. I learned that he became so frightened of my driving that he secretly forbade the members of his delegation to ride with me in the mountains.)

However benign the supervision of the local nachalnik might in fact be, Bjorken and I decided it would be more pleasant for Volodya to live off site. Bj formally put on record that there was no housing available on site at the time of Volodya's visit, and arranged that he would be put up in my house.

I went to the airport to pick up our visitor. Bj was going to meet me there, but had not yet appeared when the plane arrived and passengers began to disembark. I had never met Gribov, but was confident I'd be able to identify him. I can usually spot physicists. They have their own iconoclastic way of dressing, often have a dazed look, and seldom is their hair well combed. However, I saw no one that seemed likely to be Gribov. I searched the gate area for someone who looked lost, but without success.

I was getting worried – could he have missed his plane? Just then Bj walked up with Volodya. Apparently the latter had walked briskly out of the gate expecting to meet us at the luggage carrousel, and Bj had run into him en route. Volodya was wearing a suit and tie, and was impeccably groomed. How was I to recognize such a man as a physicist?

Volodya was a heavy smoker. The first morning in our house when he came downstairs for breakfast, he lit up a cigarette. We began talking and he lit another, having not finished the first. By the time breakfast was started he had three cigarettes going at once. He drank very strong coffee, and a lot of it. I should add that Volodya had no need of the stimulants: he was naturally hyper. In conversation, his baud rate was incredible. He believed in understanding everything under discussion in complete and utter detail. When he was pursuing an idea, he could not be deterred.

At this time, he was interested in seeing if confinement could be explained not by linear forces but by screening of color charge [19]. My thesis had been on vacuum polarization to all orders in the strength of an external field, so we had something in common to talk about. Yet somehow we never connected on what should have been a fruitful topic. Perhaps it was because I was at that time interested in electroweak baryon number violation, a process mediated by topological excitations. I tried to explain this interest to Volodya, but his only reaction was that topologically excitations, instantons and their ilk, were all absolute rubbish. I liked Volodya and I think he liked me. It still puzzles me that we never successfully found common intellectual ground.

To give an idea of another perspective on the impression Gribov made during his trip to Fermilab, let me quote from what my wife recorded about that visit:

"When Gribov was visiting Fermilab, we managed to arrange to have him stay in our house, allowing him greater freedom. I was entranced by his energy, and the passionate joy he had in the many things that interested him. What I remember most vividly was an evening when we all went with

Gribov and Bjorken to Chicago so he could hear a blues singer he knew would be singing at a jazz club there. (I'm accustomed to having Russian friends be more familiar with American and English literature than most American readers, but Gribov was also impressively knowledgeable about other aspects of our culture!) The singer was indeed wonderful, although the band was amplified to a level that threatened permanent hearing damage. Still, Gribov's pleasure in the music was contagious, and we stayed on. We finally left, and found that outside the club the music coming through the walls was at last at comfortable hearing level. Gribov strained happily to hear every last note, almost dancing with enthusiasm as we made our way back to the car."

Years later, I had moved to the University of Minnesota. I was able successfully to hire a number of good Russian theoretical physicists there. This was late in the Gorbachev years when it was apparent that Russian science was going to suffer badly in the new economic environment. We were lucky at Minnesota because we acted quickly and got really good people. In particle physics, there is Arkady Vainshtein, Misha Voloshin and Misha Shifman. We hired a number of excellent condensed matter people as well.

We had resources for visitors, and we invited Volodya and Julia Nyiri. Julia, a Hungarian, was Volodya's second wife. At this time, Volodya's affiliation was in Budapest. Julia's presence at Minnesota seemed to effect a subtle change in him that was pleasing to observe. The energy level was still as before, yet combined now with occasional overtones of something almost like relaxation.

During these visits I was developing my interest in the Color Glass Condensate, and I very much hoped to explain the ideas to Volodya. I remember Arkady Vainshtein tried very hard to help us to discuss physics successfully this time – he virtually shut us together in my office for long periods. Yet once again, in spite of our mutual wish to do so, we could not connect. This time some of our difficulty may have been due to a technical disagreement. Gribov could not accept the ideas that a classical coherent field could have anything to do with a quantum process like high energy scattering. But part of the difficulty probably stemmed from our difference in style. Volodya was very rigorous in his standards; he wanted well-developed mathematics. At that time, many of the basic ideas of what I was working on were in place, but mathematical structure of the sort he expected was still the the future – it took us several more years to develop it. That I could be comfortable working within such an imprecise framework, excited by ideas still nebulous by Gribov's standards, may well have struck him as uncomfortably close to bizarre. I regret we could not find a way to reach across that divide and connect – that I never managed to share successfully the work I was doing with a physicist I liked so well, a physicist whose own work had contributed to concepts on which I was then enthusiastically building.

We never had another chance. Gribov died not too long after the second of his visits to Minnesota.

4. Summary

Obviously I could not know Gribov as well as his lifelong colleagues did. As I have confessed, our attempts to discuss physics together were frustratingly unsuccessful. I nevertheless felt his influence throughout my career, and valued him highly both as a physicist and a human being. I hope this account makes that admiration clear, and adds to the record of the depth of his accomplishments and influence.

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