MC 0572

BOX 10-10 FOLDER 2

Russia [1973-1975, 1977]

OK

Lesson of 3 Years in Moscow: U.S. Is Sometimes Blinded by Suspicion

By Peter Osnos

MOSCOW (WP).—After three years of living in Moscow, it would be relatively easy in a farewell piece such as this to tell you all that is wrong with the Soviet Union, to rehearse once again the inefficiencies and inequities of the Soviet system. But if Americans know anything about the Soviet Union, we probably know what is bad about it.

Instead I would like to suggest something of what I think is wrong with us.

That may seem an odd way of rounding out a tour as a Moscow correspondent, but my point is precisely that we have tended over the years to dwell so much on the very real shortcomings of the Soviet Union that we bear a deep hostility toward this remarkable and confounding country—which doesn't do us any particular good and, in a nuclear age, could lead to catastrophe.

We recognize a great deal in the United States that is evil—crime, racism, poverty, injustice—and yet we don't conclude from such glaring faults that we are all bad. It is by the same token a mistake to conclude that because there is so much in the Soviet Union that we find repugnant—the lack of free expression, the self-righteousness and hypocrisy of the ideology—that the whole system is rotten.

As seen from here though, Americans are so suspicious of Soviet political motives that, aside from the ballet and making weapons, they don't think Russians are good for much.

An Old Suspicion

The phenomenon is hardly a new one. Twenty years ago, Harold Berman, then as now a scholar of Soviet law at Harvard, wrote a memorable essay, which he called "The Devil and Soviet Russia." In that era of Sputnik supremacy in space, when Americans were suddenly alarmed about the successes of Soviet science, Mr. Berman's contention was that we had become so fixated on the evils of Communism that we were not prepared for its achievements.

If the Soviet Union was really as bad as we imagined it, he wrote, "with 20 million prisoners in Siberian labor camps, workers ground down by management, every 10th person an informer, people afraid to talk about anything," then we in the West should have nothing to worry about: "Such a system could not survive a single major crisis."

In fact, said Mr. Berman, "The Soviet system which has been created is quite different. It is a working totalitarianism . . . it [illegible] of achieving the very goals it has set for itself: Economic security, political power and technological progress—by the very means it proclaims: absolute subservience to party discipline and the party line."

The professor was right. For all the backwardness in some rural areas and a general living standard that is still far below that in the West, the Soviet Union today is unquestionably one of history's imperial giants. The Kremlin now presides over the world's second largest economy, the biggest in terms of critical energy output. It has a mighty military machine and dominates an alliance that the Pentagon would have us believe is stronger in many respects than our own. And Moscow today wields formidable political influence on events in every corner of the globe.

Considering that this is a country that, as every Russian will tell you, was ravaged by revolutions, invasions and terror for most of the century, the record is certainly impressive. That much in recent years has come to be officially recognized in the United States—at least it was in the previous administration.

"The issue of how to deal with the Soviet Union has been a central feature of American policy for three decades," Henry Kissinger declared in a major pronouncement on the subject in February, 1976. "What is new today is the culmination of 30 years of postwar growth of Soviet industrial, technological and military power. No American policy caused this; American policy could have prevented it . . .

"Coping with the implications of this emerging superpower," he added, "has become our central security problem."

One Solution

Mr. Kissinger's solution was détente. (He wasn't the first to come up with the idea, but he was the one to get it implemented.) Détente, as the French writer André Fontaine neatly put it, was not the same as peace or else it would have been called peace. It was an arrangement whereby a combination of political, military, technical and commercial agreements were reached for the expressed purpose of preventing the sort of confrontation that would end in mutual annihilation. For a time, roughly between the summers of 1972 and 1975, the process was working. To borrow from Chairman Mao, a hundred flowers bloomed.

I have watched détente unravel since then, to the point where virtually all that is left is a batch of yellowing declarations [illegible] meaningless in a crisis, and the Strategic Arms Limitation Talks that are a lot further from success than any reasonable person would want them to be.

The Kremlin reviles President Carter, caling him a "demagogue" in his domestic policy who supports "absurd and wild concoctions" about Soviet abuses of human rights and who seeks "unilateral advantage" for the United States in the arms talks. Mr. Carter says people shouldn't get rattled every time Leonid Brezhnev sneezes.

Two-way trade is stagnant and hardly anyone here holds out much hope for improvement, let alone the billions that were once talked about. Cultural and scientific contacts are mostly cosmetic. For the first time in years, a U.S. diplomat and a U.S. journalist have been expelled.

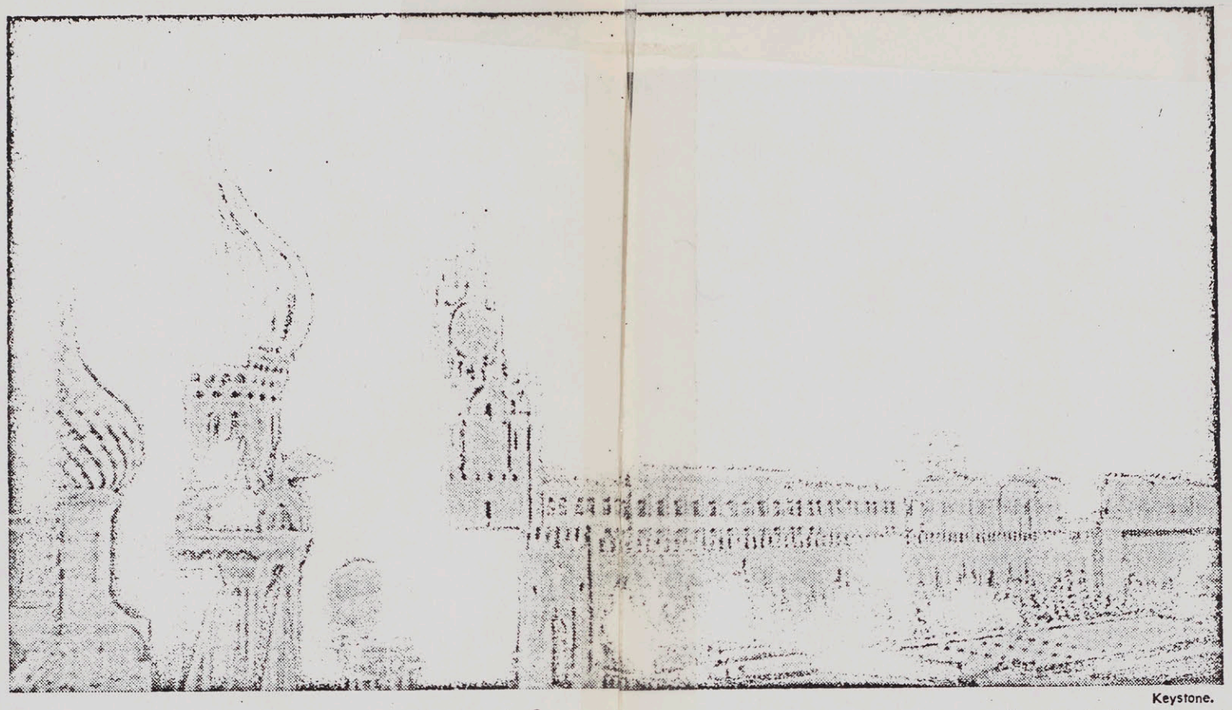
Some Explanations

Where have all the flowers gone?

There are, of course, a multitude of explanations for what went wrong. On my list are:

• Détente was oversold by Richard Nixon in an effort to distract attention from Watergate and then disillusionment set in.

• A powerful alliance of security-minded conservatives and human rights liberals in the United States whipsawed Mr. Kissinger [illegible]



Keystone.

• The Russians, being Russians, pressed for advantages in places like Angola (where they succeeded) and Portugal (where they did not), thereby cutting the ground out from under those in Washington who contended that Moscow would act responsibly.

• Military-industrial lobbies in both countries continued to pursue their vested interests in expanded outlays for defense.

I leave it to geopolitical pundits to assess the strategic implications of issues like the latter two listed. The arguments I want to stress are more the matters of attitude. It was unfortunately, I believe, U.S. antagonism to détente, those endless debates over one and two-way streets, whether we were duped in this deal or that, which was instrumental in détente's eventual collapse.

We have so deeply ingrained an aversion to godless Bolshevism going back for as long as the Communists have been around that we seem incapable of accepting that the Russians can ever do anything positive, except for the occasional talent or goodwill of individuals.

As Mr. Berman said two decades ago, the fact that this is a system we do not like does not mean that it is totally bereft of virtues.

"It is a false conception of evil," he wrote, "which assumes that men who believe in evil doctrines—such as doctrines of world revolution or the dictatorship of the proletariat—cannot at the same time work to accomplish great humanitarian benefits... for example, under the leadership of the Communist party of the Soviet Union, the number of doctors in the Soviet Union increased from about 20,000 in 1917 to about 300,000 in 1957... and under the same leadership, illiteracy declined from over 50 per cent to less than 5 per cent."

The Kremlin's firm and often harsh control has made it possible to mobilize the resources for transforming places like Kazakhstan in Central Asia, Daghestan in the Caucasus Mountains and Yakutia in northeastern Siberia from the wilds they were merely two generations ago, remote lands of nomads and exiles, into modern societies. After all, vast areas of the Soviet Union were totally undeveloped at the time of the revolution. But today, for instance, Yakutsk, the capital of Yakutia, has a population of 150,000, high-rise apartment buildings, theaters, a university—all that in winter temperatures that average more than 40 degrees below zero.

Yes, the Soviet Union is an empire run from Moscow and out-

"Did not Cromwell, the great restorer of English liberties, treat the Irish with barbaric cruelty? Did not Americans who fought for the inalienable rights of 'all men' at the same time buy and sell slaves?"

Turning the reasoning around a bit: Is it not conceivable that the same Soviet leadership that so severely restricts free expression at home and seeks ever greater influence abroad might genuinely want to improve its people's lives, might genuinely want a measure of mutually beneficial cooperation with the West, might genuinely be committed to preventing a nuclear holocaust?

The way it has looked to me from here, Americans, more often than not, say no.

Finding the Flaws

Partly we may be negative because Kremlin ideology is so infuriatingly bumptious, demanding credit that is not deserved and asserting achievements that have not been attained. Because the Russians so aggressively insist that they are perfect, we instinctively want to counter with their flaws.

[illegible] that no one who tried to leave [illegible] would be harassed.

It was obvious to Americans that those assurances were false. Hundreds of Jews lost their jobs, some were drafted into the army and others were jailed.

So instead of concentrating on how much the door had been opened—in 1973 about 35,000 Jews left—we focused on how closed it still was. Finally the Russians got fed up with the controversy, claiming that their humanism was not appreciated. The rate of Jewish emigration, at least, is down by more than half.

But sometimes it has struck me that our suspicions were exaggerated. Take the case of the 1975 Apollo-Soyuz mission, when Soviet and U.S. spaceships linked briefly in orbit. An article by a space expert, published on The Washington Post's editorial page, reamed the exercise, comparing it to the 1972 grain deal in which the Russians suckered U.S. traders. His contention was that the Russians were benefitting by access to our advanced technology while the United States got nothing.

The way it looked from here, the United States was getting valuable first-hand exposure to the Soviet space program and examining its intricacies and shortcomings, which we found to be many. But even more importantly, the mission was occasion for a tremendous outpouring of goodwill towards the United States.

U.S. Condescension

Many ordinary Russians were emotional. I listened carefully as they watched the blast-offs on television, clustered at store windows, in offices and homes. Invariably the comments centered on the excitement of such cooperation and how it might mean the countries would get along easier.

We are also condescending about some things the Russians do well. The example of literature has fascinated me. It is a very rare American who could name any contemporary Soviet author besides Alexander Solzhenitsyn, and he is better known for his political dissent than his novels.

Yet there is a very active literary life here. People like Yuri Trifonov, Valentin Rasputin, Vasily Belov, Alexander Vampilov, Vasily Shukshin, Chinghiz Aitmatov and Fazil Iskander are greatly admired by the intelligentsia, and they write with style and insight—even if they do battle the censors that are behind desks and in their heads.

[illegible] hole in what was once truly an Iron Curtain. Always lurking somewhere, crude and vicious, are the men from the KGB security police.

A Tough Question

But the KGB is not everything.

Well then, you may fairly ask, how do I think we can make our attitude toward the Soviet Union less reflexively hostile?

That is a very tough question for which I have no all-encompassing answer. We should try, in keeping ourselves informed about what is happening here, to separate the real advances in Soviet economic and social life from the ideologically inspired claims—pro and con. We should try, of course, to continue expanding contacts in scientific and cultural fields that slowly grind down barriers to understanding. We should be, perhaps, more skeptical of what dissidents say because with a cause to plead they cast matters in the most apocalyptic light.

Changes obviously can come about. Remember how menacing the Red Chinese seemed only a few years ago? Then came Richard Nixon's trip to Peking in 1972.

So far this has been a terrible year for Soviet-U.S. relations. A freeze like that, it seems to me, encourages just those repressive influences in the system that we find most abhorrent. The current crackdown on dissidents, the most extensive in this decade, would be harder for the Kremlin to undertake if Moscow's vested interest in good relations with Washington were greater.

Tactical 'Mistakes'

Lev Kopelev, a wonderful man, a writer, now 65, who spent a decade in Stalin's prison camps and has been harassed again in recent years for his outspoken defense of human rights, put the situation so eloquently in an interview not long ago that I would like to repeat it.

"I sympathize with your President Carter in his support of human rights," Mr. Kopelev said. "I think that he is a good and sincere man. There is at last a politician who puts together politics and morals. But I think that in his tactics, especially with our country, he makes mistakes.

"He is too straightforward, too direct. He doesn't understand the

Russia

Columbia University in the City of New York | New York, N.Y. 10027

DEPARTMENT OF MATHEMATICS Mathematics Building

Scientists Committee for Tverdokhlebov
c/o Lipman Bers

Dear Colleague:

We are enclosing, for your information, a small pamphlet about the Moscow physicist Tverdokhlebov. We hope that after reading it you will be willing to sign the enclosed petition and to ask other colleagues to sign it. If you need more copies of the pamphlet or of the petition, please let us know.

Thank you in advance for your cooperation. We should act fast since Tverdokhlebov may be tried during the summer.

Sincerely yours,

Lipman Bers

Lipman Bers
for the Committee

enclosures

To: The Presidium of the Supreme Soviet
The Kremlin, Moscow, USSR

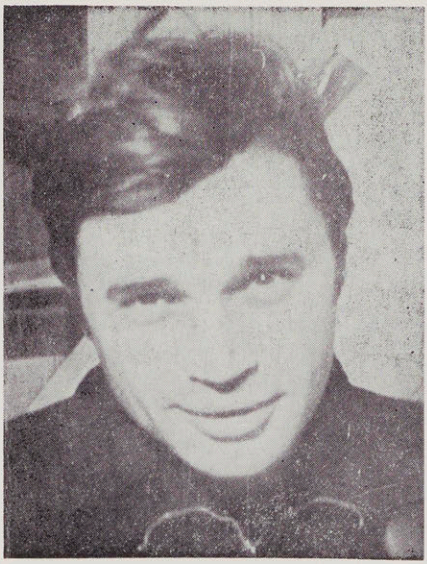
THE UNDERSIGNED SCIENTISTS REQUEST THE PRESIDIUM OF THE SUPREME SOVIET TO ORDER THAT THE MOSCOW PHYSICIST ANDREI TVERDOKHLEBOV BE FREED.

TO PUNISH HIM FOR HAVING OPENLY DEFENDED AND HELPED THOSE WHOM HE CONSIDERED VICTIMS OF PERSECUTIONS WOULD BE A GRAVE INJUSTICE AND WOULD HARM THE CAUSE OF INTERNATIONAL SCIENTIFIC COOPERATION.

NAME (please print)	Institution or address	Signature

Please sign and mail to:

SCIENTISTS COMMITTEE FOR TVERDOKHLEBOV
% Prof. Lipman Bers
Department of Mathematics
Columbia University
New York, N.Y. 10027



IN DEFENSE OF ANDREI TVERDOKHLEBOV

KHRONIKA PRESS
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In Defense of

ANDREI TVERDOKHLEBOV

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INTRODUCTION

This collection of data about the physicist, Andrei Tverdokhlebov, was compiled by his friend, Valery Chalidze. In 1970, these two, along with Andrei Sakharov, formed the Moscow Human Rights Committee. Today, Andrei Tverdokhlebov is in jail under investigation for allegedly having slandered the Soviet regime.

It is lamentable that the Soviet authorities jail a scientist whose only "crime" was to openly defend those whom he considered victims of injustice (including support of the informal seminars of Azbel and Voronel). In speaking out, Tverdokhlebov risked his career and personal liberty. We, who run no comparable risks dare do no less. In defending the rights of our colleague we also aim to protect the moral climate which makes international scientific cooperation possible.

The undersigned have formed a Scientists Committee for Tverdokhlebov. We are urging other scientists to join us. Write to any one of us and, more important, wire or write to the Presidium of the Supreme Soviet (Moscow, Kremlin), urging that Tverdokhlebov be freed.

Lipman Bers
Dep't of Mathematics, Columbia University, New York, N.Y. 10027

Hans Bethe
Dep't of Physics, Cornell University, Ithaca, N.Y. 14850

Owen Chamberlain
Dep't of Physics, University of California, Berkeley, Calif. 94720

Marvin Goldberger
Dep't of Physics, Princeton University, Princeton, N. J. 08540

Mark Kac
Dep't of Mathematics, Rockefeller University, New York, N.Y. 10021

ANDREI TVERDOKHLEBOV ARRESTED

Andrei Tverdokhlebov, a physicist and one of the founders of the Moscow Human Rights Committee, was arrested in Moscow on April 18. Since the autumn of 1974, Tverdokhlebov has been secretary of the Amnesty International Group in the USSR.

At the same time the Ukrainian writer Mikola Rudenko, also a member of the Amnesty International Group in the USSR, was arrested in Kiev. Rudenko was released; but it has been reported that before his release Rudenko was made to sign an undertaking not to leave Kiev.

Simultaneously, searches were conducted at the home of the Moscow physicist, Valentin Turchin, chairman of the Amnesty International Group in the USSR and Vladimir Albrekht, a member of that Group. Documents concerning the activity of Amnesty International were seized during these searches.

Observers regard as very unusual the fact that Tverdokhlebov's arrest was reported by the Soviet press agency Novosti. According to that report, Tverdokhlebov has been charged with disseminating libels defaming the Soviet system.

On April 19 Martin Ennals, Secretary General of Amnesty International, sent a telegram to Leonid Brezhnev, General Secretary of the CC CPSU, protesting the persecution of Sergei Kovalev, Andrei Tverdokhlebov, and Mikola Rudenko, members of the Amnesty International Group in the USSR.
There have been many protests against the arrest of Tverdokhlebov. Those speaking out in defense of Tverdokhlebov in the Soviet Union include: the writers Lydia Chukovskaya, Vladimir Kornilov, Vladimir Voinovich, and Lev Kopelev; the scientists Andrei Sakharov, Igor Shafarevich, Alexander Lunts, and Vladimir Slepak; the Sinologist: Vitaly Rubin; and others. Tatyana Khodorovich and Malva Landa have also issued statements.

Academician Andrei Sakharov issued the following statement on April 18:

"Andrei Tverdokhlebov (Moscow), the secretary of the Amnesty International group in the USSR, and the writer Mikola Rudenko (Kiev), a member of the group, have been arrested. At the same time searches were conducted at the homes of Valentin Turchin, chairman of the Amnesty International group in the USSR, and Vladimir Albrekht, a member

of the group. During the searches all documents pertaining to the activity of Amnesty International were confiscated. Sergei Kovalev, a member of the group, had been arrested earlier.

These actions, directed against Amnesty International by the state security organs, are a challenge to world public opinion. They strike at legality and at those humanitarian and democratic principles which have been consistently championed by Amnesty International and the members of its group in the USSR.

The activities of Amnesty International enjoy sincere respect and support throughout the world. This makes the persecution of its members in our country even more disturbing.

Decisive and open action by the world community is needed.

April 18, 1975

ANDREI SAKHAROV"

American scientists sent the following cable to Nikolai Podgorny on April 21:

WE UNDERSIGNED MEMBERS OF THE MATHEMATICAL AND PHYSICS SECTION OF THE NATIONAL ACADEMY OF SCIENCES ARE GRAVELY CONCERNED ABOUT THE ARREST OF THEORETICAL PHYSICIST ANDREI N TVERDOKHLEBOV AND APPEAL FOR HIS IMMEDIATE RELEASE

L. Bers, R. Bott, O. Chamberlain, G. Chew, H. Feshbach, V. Fitch, M. Goldberger, J. Hopfield, M. Kac, N. Levinson, M. Morse, P. Smith, D. Spencer, E. Stein, J. Tate, S. Treiman, G. Uhlenbeck, V. Weisskopf, E. Wigner, J. Zacharias, O. Zariski

A similar telegram was sent by:
L.V. Ahlfors; R.H. Bing; P.R. Garabedian; D. Heescher; C. Herring; J. Keller; P.D. Lax; L. Lederman; C. Levinthal; J. Moser; D. Mumford; L. Nirenberg M. Ruderman. Other Western scientists sent similar protests.

Khronika Press circulated Valery Chalidze's appeal in defense of Tverdokhlebov and Kovalev:

"AN APPEAL TO AMERICAN SCIENTISTS

The physicist Andrei Tverdokhlebov has been arrested. This qualified scientist, together with Andrei Sakharov and myself, founded the Moscow Human Rights Committee in

1970. He has engaged in research on the legal system of the USSR and in analysis of the human rights problems in that country. After the physicist Boris Zuckerman and the mathematician Alexander Yesenin-Volpin left the USSR and after I was deprived of Soviet citizenship, Tverdokhlebov was the only remaining representative of this analytic trend in the Soviet human rights movement. Now the Soviet regime has completely suppressed this trend — evidence that they consider serious, politically unbiased study of the Soviet legal system no less dangerous than resounding protests.

Tverdokhlebov is the secretary of the Amnesty International Soviet group which was recognized in 1974. The humanitarian and apolitical character of Amnesty International's activity is well-known. The authorities' behavior suggests that their current repressions are aimed specifically at this group: searches have been conducted at the homes of several group members; the biologist Sergei Kovalev was arrested last December; the archives of the group have been confiscated.

I ask American scientists to note that these repressions involve truly serious scientists who, despite their public activity and pressure from the regime, have continued their scientific work.

Tverdokhlebov and Kovalev have already been arrested, but several more scientists are threatened: in the first place Valentin Turchin, president of the Amnesty International Soviet group, and, as earlier, Andrei Sakharov. Will the regime continue its repressions against those scientists who are unwilling to renounce the freedom of thought which they find essential? That depends on whether the international scientific community can defend their Russian colleagues, on whether Western scientists can gain the release of Tverdokhlebov and Kovalev.

Experience demonstrates that the Soviet regime still takes into account the opinions of western scientists — scientific contacts are currently of crucial importance for the Soviet Union. I hope that scientists will be inspired by the memory of the principled, effective intervention by Dr. Philip Handler, president of the US National Academy of Sciences, in defense of Andrei Sakharov.

The regime has imposed absolute ideological control on many groups of the Soviet population. But one should remember that scientists are usually unable to accept, because of the nature of their profession and their cast of mind, forcible restrictions on the free exchange of information. Therefore, the regime will continue its fight against the free thinking of

scientists, and persecuted Soviet scientists have no defense other than to hope for the support of the international scientific community.

Many scientists have already sent telegrams protesting the arrest of Tverdokhlebov. Not only protests, not only appeals to humanitarian principles are important at this moment. I believe that the American scientific community possesses the strength to demand the release or, as a minimum, the exile abroad of the arrested scientists Andrei Tverdokhlebov and Sergei Kovalev.

April 20, 1975

VALERY CHALIDZE"

ANDREI NIKOLAYEVICH TVERDOKHLEBOV

Andrei Tverdokhlebov was born in Moscow in 1940. His father, Nikolai Tverdokhlebov, was deputy Minister of Culture in the 1950's, and later a Soviet diplomat in the Federal Republic of Germany.

Scientific Career:

Andrei Tverdokhlebov graduated from the Physics Faculty of Moscow University. He did post-graduate work at the Dubno Institute of Nuclear Research completing the course in theoretical physics. He served as an editor of the Abstracts of Theoretical Physics published by the All-Union Institute of Scientific and Technical Information. He was engaged in research on elementary particles and electrodynamics, and took courses in advanced mathematics at Moscow University.

On February 14, 1972, Tverdokhlebov's appointment to the All-Union Institute was terminated because of his public activities. Tverdokhlebov was working, prior to his arrest, on problems of mechanical vibration at the experimental laboratory for concrete in Moscow.

Scientific Publications (incomplete):

1. Tverdokhlebov, A.N., and Kopeliovich, V.B., "Electromagnetic T-odd Correlation in the break-ups [illegible]." *Yadernaya Fizika* (Nuclear Physics), August,1968, v. 8, n. 2. English translation: Soviet Journal of Nuclear Physics, Feb. 1969, v. 8, n. 2.

2. Tverdokhlebov, A.N. "Asymptotic Lower Bound for the scattering-amplitude Phase in the T-Plane." *JETR Letters*, 9-327, 1969.

3. Tverdokhlebov, A.N., and Shuster, A.L. "A Sphere in an Arbitrary Quasistatic Electric or Magnetic Field." *Zh. Tekh. Fez.* (Journal of Technical Physics) n. 42, 1972. English translation: Sov. Phys. — Tech. Phys., 17, 1427-32, March 1973.

4. Tverdokhlebov, A.N., and Shuster, A.L. "Electromagnetic levitation of a liquid metal drop-plet with strong skin effect (surface shape and internal pressure)", *Zh.Tekh. Fiz.*, 44, 2265-2271, (November 1974). English translation: Sov. Phys. — Tech. Phys., vol. 19, No. 11, 1399-1402, May 1975.

5. Tverdokhlebov, A.N., and Shuster, A.L. "A Liquid Metallic Droplet with a Large Coefficient of Surface Tension in an Axially-symmetric Electromagnetic Field with Strong Skin Effect", *Zh. Tekh. Fiz.*, 44, 2438, (November 1974.)

Activities in Defense of Human Rights:

For more than five years Andrei Tverdokhlebov has played a major role in the effort to define and defend the civil rights of Soviet citizens. Tverdokhlebov was a founding member of the Moscow Human Rights Committee, a founding member of GROUP-73 (an association devoted to assistance to political prisoners), and secretary of the first Amnesty International Group in the USSR (registered by the International Secretariat, London, in September, 1974).

Tverdokhlebov has analyzed the conflicts between statutes and practices of Soviet law and generally accepted international standards. He has intervened on behalf of many Soviet citizens who have been prosecuted for exercising their rights as proclaimed by the Universal Declaration of Human Rights. Tverdokhlebov has been a most persistent and effective champion of the humane treatment of prisoners — he edited four numbers of the samizdat journal *Amnesty International* which contains material on the situation and protection of prisoners.

Human Rights Publications:

Many of Andrei Tverdokhlebov's statements on human rights are collected in the book *Andrei Tverdokhlebov — v zashchitu prav cheloveka* (Andrei Tverdokhlebov — In Defense of Human Rights), ed. Valery Chalidze, Khronika Press, New York, 1975.

English translations of statements by Andrei Tverdokhlebov are contained in *A Chronicle of Human Rights in the*

USSR, Nos. 1, 2, 4, 7, 8, 9, 11-12, 14.

Chronology:

1969: Article defending Sakharov's "Thoughts on Progress, Coexistence and Intellectual Freedon." Tverdokhlebov's article was published in the samizdat journal *Social Problems*, No.1.

November 4, 1970: Tverdokhlebov, together with Sakharov and Chalidze, founded the Human Rights Committee. Tverdokhlebov participated actively in the work of the Committee. In 1972 he submitted a report on the so-called Anti-Parasite Legislation, the decree used to exile Joseph Brodsky, Andrei Amalrik and other dissenters "For evading socially useful work." Tverdokhlebov resigned from the Committee on December 29, 1972 for personal reasons.

December 27, 1970: Co-signed letter to Podgorny protesting the death sentences of Kuznetsov and Dymshits (in the Leningrad hijacking trial).

May 20, 1971: Supported Chalidze's statement "On the Persecution of Jewish Repatriates."

June 8, 1971: Letter to the Soviet Red Cross proposing humanitarian aid to families of prisoners of conscience.

August 12, 1971: Co-signed letter asking a pardon for the religious writer Anatoly Levitin-Krasnov.

November, December 1971: Joined appeals protesting violations of legality in the case of Vladimir Bukovsky.

January 10, 1973: Letter to the editor of *Vechernaya Moskva* about those convicted for anti-Soviet agitation.

1973: Tverdokhlebov edited four numbers of the samizdat journal *Amnesty International* (the first two together with V. Arkhangelsky). This journal includes material from the organization Amnesty International, international legal documents, and other material pertaining to the situation and protection of prisoners.

March 1973: Protested violations of legality in the investigation of Sergei Myuge for anti-Soviet agitation (Myuge was later permitted to emigrate).

August 27, 1973: Tverdokhlebov's apartment was searched by the KGB in connection with Case #24 (the *Chronicle of Current Events*). Archives of the Human Rights Committee,

legal literature and United Nations documents were confiscated.

September 1, 1973: Tverdokhlebov was one of the four founding members of GROUP-73, established to assist prisoners of conscience and their families. In May, 1974 GROUP-73 affiliated with The International Federation for Human Rights (Paris).

September 13, 1973: Tverdokhlebov sent a letter to the editor of *Literaturnaya gazeta* explaining the nature of Amnesty International's activity.

October 11, 1973: *The New Scientist* (London) published Tverdokhlebov's letter on Leonid Plyushch, involuntarily confined in a psychiatric hospital.

October 28, 1973: Letter to the director of the Dnepropetrovsk Special Hospital in defense of Leonid Plyushch,

December 5, 1973: Detained by police and prevented from participating in the traditional Pushkin Square demonstration by members of the human rights movement.

January 16, 1974: Letter (with A. Voronel) protesting denial of an exit visa to Alexander Galich (Galich has since emigrated).

March 14, 1974: Protest against the unlawful prosecutions of Evangelical Baptists.

March 17, 1974: Appeal for a pardon for Gabriel Superfin. Additional appeals for Superfin on March 18 (with Maria Slonim) and May 27 (with Andrei Sakharov).

May 28, 1974: Appeal in defense of Sergei Pirogov and Victor Nekipelov and also defending the legality of a *Chronicle of Current Events* (with Andrei Sakharov and Vladimir Albrekht). Reprinted: Index v. 3, no. 3, London, 1974 p. 87.

September 1974: Amnesty International recognized the first Amnesty Group in the USSR. Valentin Turchin is president and Andrei Tverdokhlebov secretary of the Group.

November 30, 1974: Statement in defense of Vladimir Osipov.

November-December 1974: Tverdokhlebov's apartment was searched on November 27 and again on December 23 in connection with the case against the *Chronicle of the Lithuanian Catholic Church*. Tverdokhlebov was interrogated in connection with this case on December 23, 24, 25.

January 1975: Interrogated in connection with the case involving Vladimir Osipov and the samizdat journal *Veche*. Article: "Two Searches and Four Interrogations."

April 18, 1975: Arrested and taken to Lefortove Prison.

LETTER FROM PROFESSOR LEON LIPSON

CONCERNING THE JURIDICAL WORK OF ANDREI TVERDOKHLEBOV

Professor Leon Lipson, Townsend Professor of Law, Yale University, addressed the following letter to Valery Chalidze:

June 11, 1975

You have asked me for my opinion of the juridical work done by Mr. Andrei Tverdokhlebov, so far as I have been made acquainted with it. My appraisal is based chiefly on his writings collected by you and published by Khronika Press in the booklet "In Defense of Human Rights". Of particular interest, I think, are his memorandum on the anti-"parasite" decrees; his work on the conditions under which criminal offenders are confined; his writing in aid of the families of certain political prisoners; and (to a lesser degree) his work in compiling documentation useful to Amnesty International

On the evidence of these writings Mr. Tverdokhlebov seems to me to approach the study of legal problems in a sober and scholarly spirit. His use of sources is not profuse but is exact and scruplous. His analysis is frequently minute and perceptive. He pays the Soviet legal system the compliment of taking it seriously — more seriously, in fact, than some of its own officials appear to take it. His report on parasitism raises questions of considerable depth and importance, not treated adequately in published Soviet literature on the subject. He has also, I think, contributed useful ideas to the subject of voluntary associations under Soviet law.

I hope we shall see more from his pen, and I regret that the Soviet government has chosen so repressive a method of enlarging his juridical experience.

Yours,

Leon Lipson

TO: THE PRESIDIUM OF THE SUPREME SOVIET OF THE USSR

The physicist Andrei Tverdokhlebov — a distinguished advocate of the protection of human rights — has been arrested in your country. Insofar as we can judge this case, Tverdokhlebov has been arrested because of his legal statements in defense of civil rights, because of his analysis of Soviet statutes, and because of his participation in the creation of associations concerned with human rights questions — activities which were clearly intended for the benefit of your people.

We ascribe particular significance to Tverdokhlebov's part in the creation of unofficial but completely legitimate associations for the defense of rights: the Human Rights Committee; GROUP-73; and the Amnesty International Group in the USSR. These associations have become affiliated with international non-governmental organizations in consultative status with the United Nations and their activity conforms with the purposes of the United Nations Charter and the Universal Declaration of Human Rights. The prosecution of one of the founders of these associations may create an unfortunate impression — that the Soviet government is hampering the exercise of the established human right of lawful association and thereby ignoring its international legal obligation to support the protection of human rights. This impression distresses us, and we ask you to exercise your constitutional authority in order to secure Tverdokhlebov's release from detention.

Peace and security cannot be realized in the absence of positive guarantees of human rights in all countries. Minimum guarantees of rights have been developed in international law; these are universal standards, and their non-observance cannot be justified by appeals to state sovereignty in internal affairs or to national customs.

We appeal to you to support international efforts to protect human rights everywhere and to secure the release of Andrei Tverdokhlebov.

The Tverdokhlebov Defense Committee of The International League for the Rights of Man
Patricia Barnes, Chairman, **Valery Chalidze, Bryant George, Edward Kline.**

For the International League for the Rights of Man:
Roger Baldwin, Honorary President
John Carey, Past Chairman
Samuel Dash, Director
Harrison Salisbury, Director
Jerome Shestack, Chairman

ANDREI TVERDOKHLEBOV (born 1940) is a Moscow physicist and a participant in the human rights movement in the USSR.

Tverdokhlebov was a founding member of the Moscow Human Rights Committee in 1970.

Tverdokhlebov published the samizdat journal *International Amnesty* in 1972 and 1973.

Tverdokhlebov was a founding member in 1973 of GROUP-73, an association concerned with assistance to prisoners of conscience. He also has served as secretary of the International Amnesty Group in the USSR.

Andrei Tverdokhlebov was arrested in Moscow on April 18, 1975.

HARVARD UNIVERSITY

DEPARTMENT OF PHYSICS

LYMAN LABORATORY OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02138

August 5, 1975

Professor I. V. Chuvilo
I. T. E. P.
Cheremushkinskaya 89
117 259 Moscow, U.S.S.R.

Dear Professor Chuvilo:

I am writing this letter as Acting Coordinator of the USA-USSR collaborations in the Fundamental Properties of Matter to enlist your help in clarifying the matter described in the letter of July 18, 1975 from Dr. Rolland P. Johnson to Dr. Victor Yarba of which I enclose a copy. Our help has been requested by Professor E. L. Goldwasser of Fermilab.

It is especially unfortunate that this matter has now cast a shadow on the success of Dr. Johnson's efforts at Serpukhov which were cited at our June 30 meeting in your Institute by Dr. Yarba as constituting a particularly successful USA-USSR collaboration at his laboratory. It thus seems very desirable that the complaint of Dr. Johnson be given both thorough and speedy consideration. It seems appropriate that we as coordinators make sure that all necessary steps are taken for this matter to be discussed among all of the concerned scientists with the hope that a satisfactory solution will be found.

Let me take this opportunity to thank you and your associates for your hospitality last month. I am looking forward to again some day being addressed by my new Georgian name.

Thank you very much in advance for your help.

With best personal regards,

Sincerely,

Karl Strauch

Karl Strauch

Copy to: W. Weisskopf
E. Goldwasser
J. Coleman

rec'd 7/30 MR2

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

July 18, 1975

Dr. Victor Yarba
Institute for High Energy Physics
P.O. Box 35
Serpukhov, Moscow District
U.S.S.R.

Dear Victor,

I had hoped to discuss some things with you at CERN last week, and so I was sorry to hear that you had cancelled your visit there. Hopefully, I can describe the situation to you in this letter and you can judge the best course of action.

My complaint concerns my right to sign publications of results from the NICE (Neutral IHEP - CERN Experiment). As you know, I worked with Prokoshkin's group to develop and exploit the large γ-detector used in the NICE experiment. I worked almost 14 months as a guest in your laboratory, supported by the Lawrence Berkeley Laboratory under an AEC-SCUAE agreement between Professors Macmillan and Logunov. This period, from May 1972 through July 1973, was spent helping to design, construct, and debug the γ-detector, the electronics and the computer-CAMAC system. During this time, I also wrote the first shower recognition and event reconstruction programs. I believe this work was helpful to subsequent work done at Karlsruhe and Pisa.

Later in November and December 1973, IHEP paid for my return to Serpukhov to help on the data acquisition. I took some data back to Berkeley after this run and worked on problems of event reconstruction and hadron shower development for two more months. I requested more data to continue work but communication was bad and I was forced to stop. A lack of travel funds then prohibited me from returning to IHEP.

After a struggle, I finally received travel funds from NSF in February to come to IHEP, but my application for a visa was not accepted. As you know, the visa was granted in June and my stay at IHEP coincided with the last run of the NICE experiment.

The goals of the NICE experiment, when I decided to join it, were the study of $\pi^- + P \to \pi^o n$, $\eta^o n$, $X^o n$ and $\pi^o\pi^o n$. In particular, we were searching for high mass states decaying into $\pi^o\pi^o$. Other multiphoton events taken with the same trigger were also to be studied.

Consequently, I was somewhat surprised when I saw the first published physics results of the experiment in Physics Letters B, May, 1975. For this publication, I was neither consulted, listed as an author, nor mentioned. I assumed that this might have been an oversight, but in fact I found it was quite deliberate.

In my last trip to IHEP, I discussed the $\pi^o\pi^o$ data and started working on several problems regarding the determination of the characteristics of the spin 4 state found at ~ 2.0 GeV.

Prokoshkin told me that I would not be allowed to sign the paper announcing the discovery of the new resonance. He gave me many reasons for this decision, ranging from the fact that he felt he had been treated badly at Brookhaven some 10 years ago to the fact that I wasn't at IHEP when the data were actually taken which were used in the analysis. Needless to say, I believe that none of his reasons were valid. Furthermore, I have trouble even guessing the real reasons for his statements and actions.

Prokoshkin also stated that he had discussed with other members of the NICE group whether I should have the right of authorship, and that everyone agreed I should not. In my case, he added, his opinion was sufficient.

I was unable to find one other collaborator who had agreed with Prokoshkin to omit my name. Heinz Muller, the leader of the Karlsruhe group, for example, said that he is willing to write a letter to you or anyone stating that my contribution to the experiment was sufficiently great that there should be no question as to my right of authorship. Both Mannelli (the leader of the Pisa group) and Muller told me that because I had worked with Prokoshkin's Russian group, there was nothing they could do to change the author list if Prokoshkin would not agree.

I have included in this letter copies of telex exchanges between Mannelli and myself and Muller and myself, which took place after I had discussed the matter with Prokoshkin. Subsequent discussions with Mannelli and Muller convinced me that Prokoshkin's decision to delete me from the experiment was deliberate, firm, and his alone.

At the very least, I consider this whole business to be a matter of bad faith on Prokoshkin's part. At worst, I worry that there may be some political overtones. I hope that there is no stigma in having an American's name on a paper announcing the discovery of an important new particle.

I am particularly sad that this probably means the end to what was otherwise a very enjoyable collaboration for me. In particular, I found the Russian group to be first-rate, dedicated physicists.

Without Prokoshkin's active support, my work in Russia is not possible, of course. Living expenses, travel costs and even visa authorizations are entirely dependent on him. Hopefully, something can be done to change this situation, at least to the extent that any future single-person exchanges can be made with some provision for the completion of the experiment.

With kindest regards,

Rolland P. Johnson

1975 PEP Summer Study

Lawrence Berkeley Laboratory
University of California
Berkeley, California 94720
Tel. (415) 843-2740

August 5, 1975

Professor E. L. Goldwasser
Fermi National Accelerator Laboratory
P. O. Box 500
Batavia, Illinois 60510

Dear Ned:

I enclose a copy of my letter to Chuvilo concerning the complaint of Dr. Rolland Johnson. After receiving your letter of July 22, I had a long talk with Rolland trying to understand how the resonance work which does not bear his name was carried out. As I understand it, the data was taken at Serpukhov after he had left and the analysis was mainly done at Karlsruhe using, however, programs for which he was to a large extent responsible. Thus, whether or not his name should have been included in the resonance publication depends to a large extent on what is considered to be a reasonable "decay time" for such inclusion when a member has effectively left a group. And this is a number which will vary from group to group, and from individual to individual.

I have little doubt that under present usages in the U.S., Rolland's name would have been included by most groups. However, it does not seem to be a completely straightforward case and I can understand a more old-fashioned point of view. Of course, we can only guess at what motivated Prokoslikin's decision and I suspect that he had other considerations in mind. However, the fact that there is this subjective factor seems to me to make it difficult to make a very strong case out of this very unfortunate experience.

One lesson to be learned from Rolland's experience is that U. S. approval for any long-term visits for work in a USSR laboratory should be given only if sufficient arrangements can be made for a few follow-up visits to exploit the work done and participate in the analysis. I think it is important to point this out to ERDA even more forcefully than we have done in the past and to suggest that this subject be discussed fully in each case before any new approvals are given to prevent misunderstandings. Do you agree?

With best regards,

Karl

Karl

Copy to: V. Weisskopf
J. Coleman

Russia

NORTHEASTERN UNIVERSITY

BOSTON, MASSACHUSETTS 02115

DEPARTMENT OF PHYSICS

December 11, 1974

Professors Karl Strauch and
Victor Weisskopf

Gentlemen:

This is one of the rare occasions in which I can use this salutation and mean it.

Enclosed is a section of the proposal submitted to NSF by the high energy group at Northeastern University in June of this year. The enclosed section deals with the proposal for an experiment on rho omega interference at VEPP 2' in Novosibirsk. This proposal is relatively complete except for detailed design of the detector. The detector design was completed this summer and is now being written up. I expect to submit a new proposal, including the more detailed detector design, to the NSF Division for International Programs (or some name very close to this) within approximately one month.

When the proposal was submitted to the NSF as part of our entire research effort essentially no funds were allowed for it (approximately $4,000 of budget was allowed for detector development). My purpose in submitting it to the International Division at NSF is that it may receive, as an isolated proposal, the attention which I believe it deserves.

At the present time my understanding is that Sidorov is interested in the experiment, and the peripheral benefits of a collaboration, but he insists that an amount of cash or equipment of value exceeding $100,000 be left at his laboratory, and that the type of equipment be that which he is short of. What he would really like is $100,000 payment for beam time. This of course violates western constraints. It may be possible within our usual stipulations to satisfy Sidorov. This could be done by leaving the detector which has approximately $100,000 worth of fast scintillator and fast phototubes. Sidorov is in short supply of both of these and unable to master the mysteries of manufacturing them.

As things stand now the left horn of my dilemma is that Sidorov wants payment for beam time and the right horn is that we cannot supply it within our present mores. What I would like to get in the way of funding is a rather low level grant which will permit me to continue detector design and

development, and permit an occasional visit to the Siberian laboratory. I believe that time will cure the other problem and that, if we can keep the rho omega experiment alive on this low level budget, it will eventually be scheduled to run at Novosibirsk. This is my naive view of the world and I leave it on the table for you to chop up.

I am of course more than willing to proceed at high speed (and high budget) toward the completion of this experiment.

My only other comment is that I would very much appreciate it if you gentlemen would keep me apprised of any negotiations with the Russians, a fortiore if the negotiations ~~follow~~ bear on my proposed experiment.

Best personal regards,

Roy

Roy Weinstein

RW/bjc
Enclosure

From N.U. proposal to NSF, June 1974

C. Rho Omega Phase

We propose to measure the ρ and ω phases relative to well understood QED terms in a colliding beams experiment. This will provide a measure, with no nucleus present, of the relative ρω "production" phase, β, which has now been measured in several experiments with a nucleus present. As outlined in this section there are reasonable grounds to suspect that β is non-zero, contrary to well founded theoretical expectation. The theoretical expectations are sufficiently well founded so that if β is indeed non zero, we believe a colliding beams experiment (i.e. one with no nucleus present) is needed to provide acceptable proof.

Our group has been interested in the (ρ,ω) interference problem for about eight years, and was the first to measure the branching ratio for $\rho \rightarrow \mu^+ + \mu^-$,[7] the first to recognize the important of the ρ,ω interference problem in the lepton channel,[19] and the first to measure the ρ,ω production phase[24,27].

At the same time as we measure β, we plan to perform an independent experiment aimed at measuring the branching ratio $\rho^o \rightarrow \pi^o \gamma$. This experiment will also be discussed below.

We will review in this section the state of the experimental problem and attempt to show the important theoretical problems involved in the ρω phase. We will also discuss the experimental design and budget.

1) Theoretically Expected Phases

Consider, for example, the diagrams shown in Fig. 1 for the reaction $\pi^+ p \rightarrow \pi^+ \pi^- \Delta^{++}$

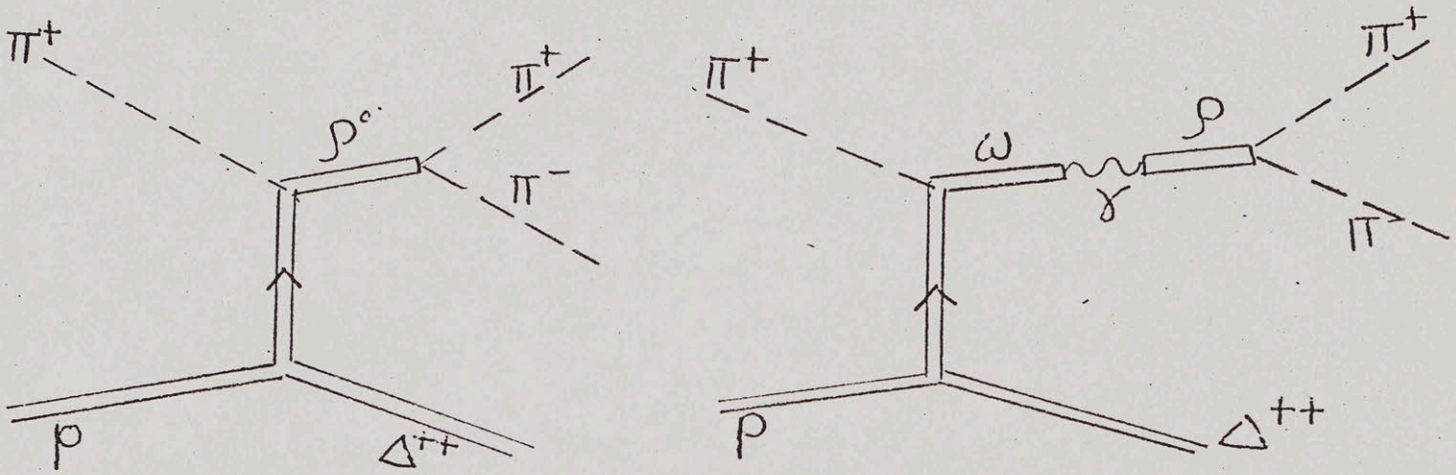


Figure 1: Example of ρ and ω Diagrams in Strong Production

In such a reaction the mass spectrum is usually assumed to have the form

$$\frac{dN}{dm} \alpha \left| A_\rho t_\rho(m) \sqrt{B_{\rho\pi\pi}} + \xi (A_\omega e^{i\beta}) \, t_\omega(m) \left(\sqrt{B_{\omega\pi\pi}} e^{i\beta'} \right) \right|^2 \qquad (1)$$

where $t_v(m)$ is a Breit-Wigner amplitude such as

$$t_v(m) = \frac{m_v \Gamma_v}{(m_v^2 - m^2) - i m_v \Gamma_v} \quad ,$$

the A's stand for production amplitudes, the B's are branching ratios to the final state and ξ is a coherence factor between 0 and 1 which measures the fraction of coherent ρω production in the process. The A's in eq. (1) are real and any relative phase between A_ρ and A_ω is subsumed in

$\beta \equiv$ ρω relative production phase.

The decay amplitudes have been treated the same way with

$$\beta' \equiv \rho\omega \text{ relative } \underline{\text{decay}} \text{ phase}$$

Usually eq. (1) is written in terms of a single phase angle

$$\varphi \equiv \beta + \beta' = \underline{\text{total}}\ \rho\omega \text{ phase}$$

It is this angle which the experiments determine; they do not separate β and β'.

We shall now briefly review what happens to the decay and production phases in both strong and electromagnetic interactions. We shall see that the decay phase, β', is readily predictable in either case, but that the production phase, β, is readily predictable only for electromagnetic interactions. It therefore is in electromagnetic studies that the phases may be tested with minimal ambiguities.

a) The $\rho\omega$ Decay Phase

i) $\pi\pi$ decay

If the ω decays into the $\pi\pi$ final state via a virtual ρ, as shown in Fig. 1, and none of the decay vertices contain phases (or any vertex phases cancel) then the decay phase is determined by the propagator of the virtual ρ, and is the phase angle of the ρ Breit-Wigner amplitude evaluated at the ω mass.

$$\beta' = \arg\,(t_\rho(m_\omega)) = \mathrm{Tan}^{-1}\ \frac{m_\rho \Gamma_\rho}{m_\rho^2 - m_\omega^2} = \beta_{\text{Fermi-Watson}}$$

$$= 106^\circ \pm 5^\circ \quad (\text{For } \pi\pi \text{ decay}) \tag{2}$$

It is usually assumed by theorists that the vertex phases are zero.[1] This is a consequence of time reversal, as we shall discuss in Sec. 3, below. Hence the decay phase given by Eq. 2 is considered to be as theoretically reliable as is time reversal, and the vector dominance model which leads to it.

ii) lepton decay

In leptonic decay modes, the portion of Fig. 1 showing ω decay into ππ via a virtual ρ is replaced by Fig. 2.

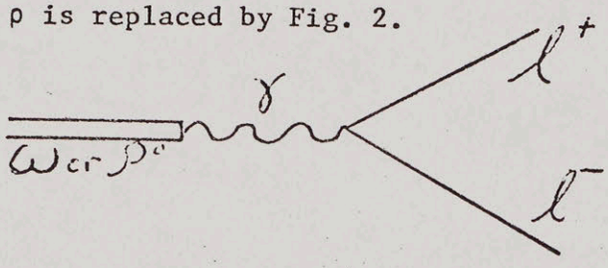


Fig. 2 Leptonic Decay

The virtual ρ propagator is absent in this diagram, and the ρ^o decays by the same process as the ω. As a result the expected relative ρω phase is

$$\beta' = 0 \text{ (leptonic decay)} \qquad (3)$$

b. The ρω Production Phase

i) Strong Production

Different exchanges are required to produce the ρ and ω, so that the relative production phase varies widely depending upon the particular reaction. Almost any shape may be observed in the mass spectrum, from a dip to a peak at the ω mass. In many cases the shape of the effect in one reaction can be predicted if the shape in another is known. Also, in strong production the coherence factor ξ may be quite small, and usually depends upon the reaction being studied, and upon the energy. In addition, there may be large coherent and/or incoherent backgrounds which depend upon the reaction and the energy. Thus each strong reaction introduces its own parameters into the overall picture. Goldhaber[2] takes the view that ρω interference, with its "known" parameters, should be used as a probe of strong interaction dynamics, and this has been done by, e.g., Wicklund et al[3]. It is our aim, however, to probe the basic phase assumptions which go into ρω interference, and this does not appear to be presently feasible utilizing strong interactions.

ii) Electromagnetic Production

It falls to the conceptually simpler electromagnetic interactions to provide experimental situations in which the $\rho\omega$ phases may be determined in a more interpretable way. In electromagnetic production of $\rho\omega$, the diagram of Fig. 2 appears, reversed, on the production side of the diagram. The production phase, β, is expected to be zero for the same reasons outlined above for the leptonic decay. In the case of $\pi\pi$ decay, where the decay phase is expected to be 106^{o}, the total (observed) phase is also expected to be 106^{o}. In the case of electromagnetic production followed by leptonic decay, terms as in Fig. 2 appear both on the production and decay side of the diagram. In these cases the production and decay phases are both expected to be zero, and consequently the total phase is expected to be zero.

There is reason to assume that some corrections to VMD exist. According to "strict" VMD the intermediate state in $\rho\omega$ mixing is a photon, as in Fig. 1, and the branching ratio, $B_{\omega\pi\pi}$, is determined by the two (γ,V) vertices. The theoretical value for $B_{\omega\pi\pi}$ determined in this way follows from measured ρ and ω coupling to photons and is $B_{\omega\pi\pi} = 0.07\%$. Observed values obtained by fitting theory of the form of Eq. 1 to experimental data, are approximately $B_{\omega\pi\pi} = 1.5\%$. This is a factor of 20 discrepancy!

In order to obtain this larger branching ratio from the theory, other $\rho\omega$ mixing schemes have been proposed, as discussed by Goldhaber[2], who gives a listing of pertinent theoretical references. If other significant $\rho\omega$ diagrams exist, one must also admit, for example, the possibility of a relative $\pi\pi$ decay phase other than that given in Eq. 2. This "excess" phase is generally predicted to be small.

We must, therefore, keep in mind that although the EM diagrams appear to be on a firm theoretical footing, there already exists one experimental result which the theory would describe as "anomalous". An alternative explanation of the anomalously large observed $B_{\omega\pi\pi}$ is that the traditional model incorporated in Eq. 1 may be incorrect. We shall discuss this possibility in Section 4.

2. A Review of Electromagnetic Production Experiments

We will now consider four electromagnetic interactions, of which the last one is our proposed experiment:

$\gamma N \rightarrow \pi\pi N$ (photoproduction of π pairs from nuclei or hydrogen)

$e\ e \rightarrow \pi\pi$ (production of π pairs in colliding beams)

$\gamma N \rightarrow \ell\ell N$ (photoproduction of lepton pairs from nuclei)

$e\ e \rightarrow \mu\mu$ (production of lepton pairs in colliding beams)

Figure 3 summarizes the relevant diagrams in each case. Figure 4 outlines (very schematically) the theoretical interpreation in terms of eq. (1); in this figure $g_V = \frac{em_V^2}{2\gamma_V}$ stands for a photon-vector meson coupling constant. The other symbols have been defined previously, except that the "A" used here explicitly factors out the (γ,V) coupling. We have used here $m_\omega \approx m_\rho$; otherwise the appropriate m should multiple each Γ.

a) $\gamma N \rightarrow \pi\pi N$ and $e^+e^- \rightarrow \pi\pi$

Table 1 lists the published phase and $B_{\omega\pi\pi}$ obtained in the first 2 reactions (i.e. those with $\pi\pi$ final states.)

The reaction $\gamma N \rightarrow \pi\pi N$ is shown at the top of Figure 4. The ω term is multiplied (Fig. 4) by a factor 0.16 which reduces the expected interference effect to a mere kink in the ρ distribution at the ω mass. The

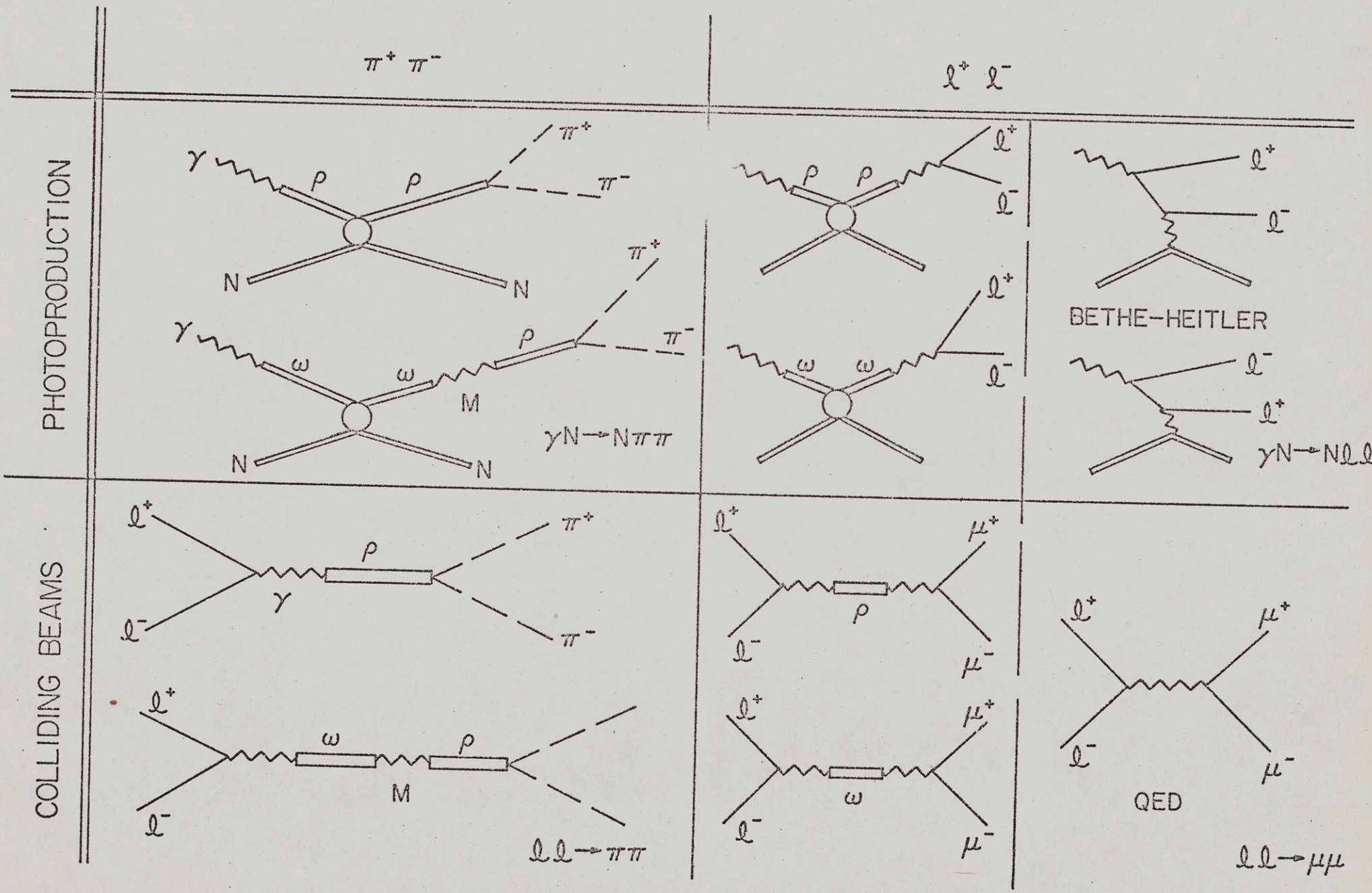


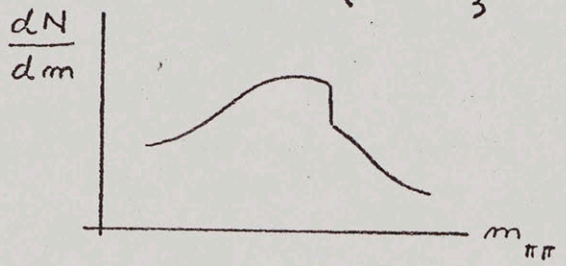
Fig. 3: $\rho\omega$ INTERFERENCE DIAGRAMS IN EM INDUCED REACTIONS

$\gamma N \to \pi\pi N$
$e e \to \pi\pi$

$$\left| \frac{g_\rho}{\sqrt{\Gamma_\rho}} A_\rho t_\rho \sqrt{B_{\rho\pi\pi}} + \frac{g_\omega}{\sqrt{\Gamma_\omega}} A_\omega e^{i\varphi} t_\omega \sqrt{B_{\omega\pi\pi}} \right|^2$$

$$= \frac{g_\rho^2}{\Gamma_\rho} \left| t_\rho + \sqrt{\frac{\Gamma_\rho}{\Gamma_\omega}} \frac{g_\omega}{g_\rho} \frac{A_\omega}{A_\rho} \sqrt{\frac{B_{\omega\pi\pi}}{B_{\rho\pi\pi}}} e^{i\varphi} t_\omega \right|^2$$

$$\left(3.8 \times \tfrac{1}{3} \times 1 \times 0.12\right) = 0.16$$

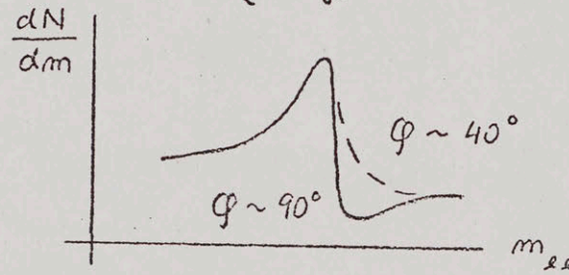


g_ρ (MEASURED BY YIELD)
$B_{\omega\pi\pi}$ (SIZE OF KINK)
φ (SHAPE OF KINK)

$\gamma N \to e e N$
$\gamma N \to \mu\mu N$
(CHARGE-SYMMETRIC)

$$\left| BH + \frac{g_\rho}{\sqrt{\Gamma_\rho}} A_\rho t_\rho \frac{g_\rho}{\sqrt{\Gamma_\rho}} + \frac{g_\omega}{\sqrt{\Gamma_\omega}} A_\omega e^{i\varphi} t_\omega \frac{g_\omega}{\sqrt{\Gamma_\omega}} \right|^2$$

$$= \left(\frac{g_\rho^2}{\Gamma_\rho}\right)^2 \left\{ \left| \frac{\Gamma_\rho BH}{g_\rho^2} \right|^2 + \left| t_\rho + \underbrace{\frac{A_\omega \Gamma_\rho}{A_\rho \Gamma_\omega} \left(\frac{g_\omega}{g_\rho}\right)^2}_{1.6} e^{i\varphi} t_\omega \right|^2 \right\}$$

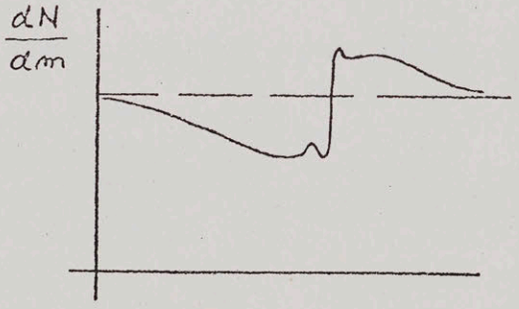


g_ρ (FIT PARAMETER)
$(g_\omega/g_\rho)^2$ (SIZE OF INTERF.)
φ (SHAPE OF INTERF.)

$e e \to \mu\mu$

$$\left| -\frac{\alpha}{3} + \frac{g_\rho}{\sqrt{\Gamma_\rho}} t_\rho \frac{g_\rho}{\sqrt{\Gamma_\rho}} e^{i\varphi_{\rho QED}} + \frac{g_\omega}{\sqrt{\Gamma_\omega}} t_\omega \frac{g_\omega}{\sqrt{\Gamma_\omega}} e^{i\varphi_{\omega QED}} \right|^2$$

$$= \underbrace{\frac{\alpha^2}{9}}_{95\%} + O(g_\rho^2 g_\omega^2) - \underbrace{2\left(\frac{\alpha}{3}\right) \mathrm{Re}\left(\frac{g_\rho^2}{\Gamma_\rho} t_\rho e^{i\varphi_{\rho QED}} + \frac{g_\omega^2}{\Gamma_\omega} t_\omega e^{i\varphi_{\omega QED}} \right)}_{5\%}$$



g_ρ, $\varphi_{\rho QED}$
g_ω, $\varphi_{\omega QED}$

FIGURE 4: QUALITATIVE STRUCTURE OF TRADITIONAL THEORY OF E.M. INDUCED $\rho\omega$ REACTIONS

GROUP REFERENCE TECHNIQUE	REACTION EXP'TL PARAMETERS	RESULTS
Daresbury, Biggs et al. (4) mag. pair spect. brems. beam	$\gamma C \to \pi\pi C$ $k_{MAX} = 4.6$ GeV $p_{SPECT} = 2.1$ GeV $8° \to 10.5°$	$\varphi = \beta_{F\omega} \pm 5°$ $B_{\omega\pi\pi} = 0.8^{+.28}_{-.22}$ % (USED $(g_\rho/g_\omega)^2 = 7$)
DESY (6) Alvensleben et al. mag. pair spect. brems. beam	$\gamma \begin{pmatrix} p \\ C \\ Pb \end{pmatrix} \to \pi\pi \begin{pmatrix} p \\ C \\ Pb \end{pmatrix}$ $k_{MAX} = 7.4$ GeV	$\varphi = \beta_{F\omega} \pm 15°$ $B_{\omega\pi\pi} = 1.22 \pm 0.3$ % (USED $(g_\rho/g_\omega)^2 = 9.4$)
SLAC Moffeit et al. (5) H_2 Bubble chamb. monochrom. 's	$\gamma p \to \pi\pi p$ $k_\gamma = 2.8$ & 4.7 GeV (RESULTS ADDED)	$\varphi = \beta_{F\omega} - (\sim 9°)$ $B_{\omega\pi\pi} = 1.3^{+1.2}_{-0.9}$ %
Orsay (7) Benaksas et al. coll. beams cylindrical det.	$e^+e^- \to \pi^+\pi^-$	$\varphi = \beta_{F\omega} - (10 \pm 16)°$ $B_{\omega\pi\pi} = 3.6 \pm 1.9$ % $B_{ee} = (4.2 \pm 0.4) \times 10^{-5}$ (NOVOSIBIRSK DATA (8) INCLUDED IN THEIR FIT)

TABLE 1 : Summary of Electromagnetically Induced $\rho\omega$ Experiments; Decay via Pion Channel

total yield of ρ's in this experiment, rather insensitive to the interference effect, is a direct measure of g_ρ. The size of the interference is a measure of $B_{\omega\pi\pi}$. Finally, the characteristic shape of the interference is a measure of the total $\rho\omega$ phase, φ, and indicates $\varphi \tilde{=} \beta_{FW}$.

This reaction has been studied experimentally by three groups, using a variety of nuclear targets, and obtaining good statistics. The results given in Table 1 indicate good agreement with the theoretical models discussed above, except, again, for the anomalously large value of $B_{\omega\pi\pi}$. The result most important here is that φ appears to be very close to the value given in Eq. 2 (probably within 10^o). This means, if the models discussed in the literature are correct, that the sum of any relative production phase and "excess" decay phase is near zero. This observation holds for widely differing target nuclei (as well as for "no nucleus" as discussed in the next paragraph).

We next consider colliding beams results for the experiment $e^+e^- \rightarrow \pi\pi$ as fitted by Benaksas et al[7] to colliding beams data taken at Orsay[7] and Novosibirsk.[8] The colliding beams experiment $ee \rightarrow \pi\pi$ differs from the photoproduction experiments only in the absence of a nuclear diffractive vertex. The fact that the measured values of φ in the colliding beams experiments agrees with the photoproduction experiments may be interpreted as a measurement of the relative $\rho\omega$ phase introduced at the nucleus, consistent with 0^o. This was the value assumed by most theorists prior to the experiments.

We conclude from the $\pi\pi$ data that overall phases, φ, appear to be very close to their expected values, and that the phase at the nuclear vertex is near zero, but that the observed branching ratio, $B_{\omega\pi\pi}$, is strikingly high. We might be tempted to conclude that the production and decay phases are

separately understood, but the experiments reviewed in the next section lend considerable doubt to such a conclusion.

b) Experiments On $\gamma N \to \ell^+\ell^- N$

The systematics of experiments measuring $\gamma N \to eeN$ are quite different. The ρ and ω diagrams each interfere with the Bethe-Heitler diagrams. In a charge-symmetric experiment (final state charges unknown, or ignored) these cross terms cancel, and BH terms act only as a background which is approximately half the total rate in favorable cases, and which can be calculated and subtracted. The coefficient of the ω Breit-Wigner is about ten times that in $\gamma N \to \pi\pi N$ because ω branching into $\pi\pi$ is not involved. Instead, a ratio of EM couplings has replaced $B_{\omega\pi\pi}$ in the final state, and the interference is considerably stronger. In fact, the ρ and ω amplitudes are approximately equal at the mass of the ω. This is the basis of the argument of Greenhut, Weinstein and Parsons[27] who suggested that the $\rho\omega$ phase could be measured by two techniques, one an investigation of the total, or integral, yield in the $\rho\omega$ region, as performed by Rothwell et al[24], and one a measurement of shape, or differential yield, as later performed by Biggs et al[9] and Alvensleben et al[10]. The integral yield experiment is possible because of the strong interference effect.

As noted in Sec. 1b, the value of φ expected theoretically, under the usual assumptions of no relative production phase in diffractive processes, and no relative phases at the vector meson/photon vertex, is $\varphi \tilde{=} 0^\circ$ rather than $\varphi \tilde{=} 106^\circ$.

The first determination of φ, following the method of Greenut et al was done by Rothwell et al[24] on the reaction $\gamma c \to \mu\mu c$. This muon experiment

had poor mass resolution so the ρ resonance is seen, but the presumed interference structure is not resolved. The interference is strong enough however to appreciably increase the total area under the ρ bump if $\varphi \tilde{=} 0^{o}$. Thus, if one naively calculates a "branching ratio" $B'_{\rho ee}$ based on the area of the bump, and compares it with $B_{\rho ee}$ deduced from $ee \rightarrow \pi\pi$, where the interference is two orders of magnitude weaker, one expects a "ratio of branching ratios" to be, for $\varphi = 0^{o}$.

$$R \equiv \frac{B'_{\rho ee}}{B_{\rho ee}} \tilde{=} 1.8$$

rather than R = 1. Generalizing this argument, Greenhut et al[27] plotted the expected value of R as a function of the assumed φ.

This technique does not provide the satisfying explicit observation of interference structure in the mass spectrum. However it has the advantage of being relatively insensitive to calibration of the mass scale. As we shall see below, when considering subsequent differential mass measurements, this is a useful attribute.

Rothwell et al[24] concluded that $\varphi = 100 \pm 35^{o}$ or 305 ± 35^{o}. The ambiguity in angle is inherent in the integral technique. (Since the time that the measurements of Rothwell et al were published, the value of $B_{\rho ee}$ determined by colliding beams has decreased markedly, thus reducing the phase value one would obtain from this experiment by reanalysis.)

Next, two experiments were performed on $e^{+}e^{-}$ final states using high resolution spectrometers.

The ratio g_{ω}/g_{ρ} from these experiments is not of paramount interest because more accurate determinations based on $\omega \rightarrow 3\pi$ measurements generally confirm the SU(3) prediction $g_{\omega}/g_{\rho} = 1/3$.

	GROUP REFERENCE TECHNIQUE	REACTION EXP'TL PARAMETERS	RESULTS
CHARGE - SYMMETRIC	Daresbury, Biggs et al. (10) mag. pair spect. brems. beam	$\gamma C \to e e C$ $k_{max} = 4.1$ GeV $p_{SPECT} = 1.802$ GeV $11.2^\circ \to 13.3^\circ$	$\varphi = 100^\circ \pm {}^{38^\circ}_{30^\circ}$ $(g_\rho/g_\omega)^2 = 7 \pm 3$ $B_{pee} = (4.9 \pm {}^{1.2}_{1.5}) \times 10^{-5}$ $m_\omega = 782.5 \pm 3$ MeV (USED $m_\rho, \Gamma_\rho, \Gamma_\omega =$ 770, 140, 12.6 MeV)
	DESY (11) Alvensleben et al. mag. pair spect. brems. beam	$\gamma Be \to e e Be$ $k_{max} = 7.00$ GeV $p_{SPECT} = 2.560$ GeV $7.5^\circ \to 8.8^\circ$	$\varphi = 41^\circ \pm 20^\circ$ $(g_\rho/g_\omega)^2 = 9.4 \pm {}^{2.6}_{1.6}$ (USED $B_{pee} = (6.5 \pm .5) \times 10^{-5}$ $m_\rho, \Gamma_\rho, m_\omega, \Gamma_\omega =$ 765, 130, 783.7, 12.7 MeV)
	CEA (24) Rothwell et al.	$\gamma C \to \mu\mu C$ $k_{max} = 6$ GeV $2.237 < E_\rho < 2.785$ $5.2^\circ \to 11.8^\circ$	$B'_{pee} = (8.2 \pm 1.6) \times 10^{-5}$ $\to \varphi = 22^\circ \pm 60^\circ$ (SEE TEXT)
	Daresbury Biggs et al. (12)	$\gamma C \to e e C$ (ASYMMETRIC)	$\varphi_{\rho QED} = 15 \pm 6^\circ$ ($\varphi_{\rho\omega} \cong 100^\circ \pm 20^\circ$)
	CEA Earles et al. (28)	$eC \to \mu\mu eC$	$\varphi_{\rho QED} = 16 \pm 22^\circ$

TABLE 2: Summary of Electromagnetically Induced $\rho\omega$ Experiments;

Decay via Lepton Channel

With respect to φ, the two $\gamma N \rightarrow ee\,N$ experiments disagree as Table 2 shows. The Daresbury group[9] obtained a value of $\varphi = 100^{o}\ {}^{+38^{o}}_{-30^{o}}$, entirely consistent with the value of Rothwell et al. Their data do not require the dip above the ω mass characteristic of a total phase near 0^{o}. The authors point out that a fit with $\varphi = 0^{o}$ is ruled out because the fitted ω mass is then low by 10 MeV. In other words, the large phase could be accounted for by an absolute mass error of about 1% which they feel is ruled out. (It is in this sense that the source of errors in the integral and differential techniques are independent.) Biggs et al also confirmed their large φ in a second measurement on asymmetric electron pairs. The DESY group obtained a result of $\varphi = 41^{o} \pm 20^{o}$, in an experiment very much like the earlier Daresbury experiment. This discrepancy in results remains unresolved.

The idea[10], that it might be due to target or energy dependent effects seems untenable in view of the insensitivity of the $\gamma N \rightarrow \pi\pi N$ results to these parameters. Also, such an effect would almost certainly have to be nuclear, and Greenhut and Weinstein[33] have shown theoretically that the maximum nuclear effect is 40^{o}. But more important, the agreement between φ measured by $\gamma N \rightarrow N\pi\pi$ and $e^{+}e^{-} \rightarrow \pi\pi$ shows the nuclear effect to be very small ($< 25^{o}$) and entirely consistent with zero.

(c) Summary

From EM production experiments with $\pi\pi$ final states (1) the observed phases appear to agree with the theory of Eq. 1, but (2) an anomolously large value of $B_{\omega\pi\pi}$ is observed. If this is due to higher order diagrams these must be large, but yet they appear to have no effect on φ. In addition (3) the agreement of phases determined by $\gamma N \rightarrow \pi\pi N$, and $e^{+}e^{-} \rightarrow \pi\pi$, appears to show that the phase introduced at the nuclear vertex is near zero.

In EM production experiments with lepton pair final states the observed phases do not appear to be consistent with zero. Each experiment separately disagrees with 0^o phase, by three, or at least 2, standard deviations, and at least four separate experiments exist. Due to experimental disagreements we can only conclude that φ differs from zero by 3 or 2 standard deviations and may be as large as 100^o. If these large production phases are as measured in lepton final states, it is very difficult to understand why the experiments with pion final states agree with theory.

The EM experiments with leptonic decay appear to be in disagreement with those with $\pi\pi$ final states. It is not our purpose here to theoretically speculate on the source of the disagreement (although we have spent some research effort doing so). We note instead that there are several possible sources of trouble, among which are:

i) The $\pi\pi$ experiments, which are in apparent agreement with theory, do not of course separate β and β'. It is possible that the agreement with theory is fortiutous and due to, e.g., a cancellation of production phases and "excess" phases. The ρ^o propagator (Eq. 2) may also be anomalous.

ii) The assumed diagrams may be insufficient. Vertex structures, second order terms, etc. may be quite different in the $\pi\pi$ and leptonic experiments.

iii) The effects of unitarity, not imposed on Eq. 1, may be large. This is discussed in Section 4.

iv) The VMD model is not valid.

v) Time reversal may be violated.

vi) The leptonic experiments may be wrong.

vii) The pionic experiments may be wrong.

3) The Problem, and Comments Upon The Proposed Experiment

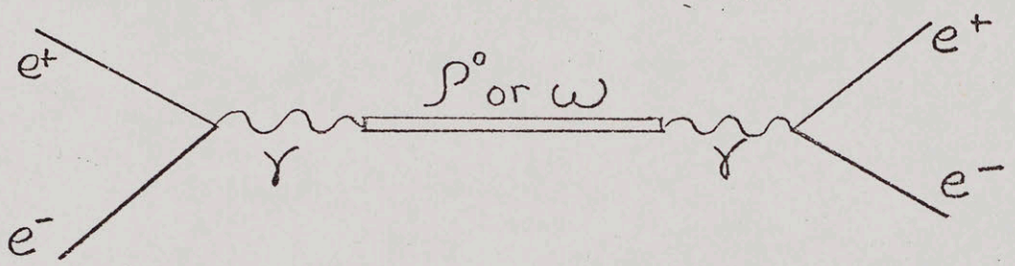
a) Importance of Problem

We believe for reasons outlined above that there exists a reasonable suspicion that the experiments on $\rho\omega$ phase are not adequately described by the theory. The problem exhibits itself in the experiments outlined in Sec 2b. We do not believe that these experiments absolutely confirm a shortcoming in the theory, but we do believe that they raise a suspicion serious enough to merit further investigations, and call, if possible, for a definitive experiment. The problem, as pointed out in Sec 2c, lies in the phase results of the class of experiments with leptonic final states, and is also indicated by the very high value of $B_{\omega\pi\pi}$.

If a phase problem exists, it is likely that it involves very fundamental assumptions of the theory. The straightforward reason for this is that theory has failed to reconcile the leptonic and pionic results. Some theorists feel sufficiently strongly about their basic assumptions to assume that the leptonic experiments must be wrong.(1) This is of course one possibility. There are several ways to express the leptonic results to make evident the basic nature of the problem, and we will follow in this section a short intiutive path which leads to the conclusion that time reversal may be breaking down. We do not favor this interpretation, and other possibilities abound, one of which we outline in the next section. We mention the possiblity of time reversal breakdown (a) because the suggestion appears repeatedly in the literature, and (b) becuase it is the most bizzar example of the seriousness of this problem.

Consider the diagrams of colliding beams formation of lepton pairs via a ρ or ω intermediate state, shown in Fig. 3, and assume the final state is an electron pair. Since there is experimental evidence that the nuclear vertex introduces no phase shift in the experiments on $\pi\pi$ final states, and since the

same nuclear vertex is involved in lepton final states, we may conclude that the large phase angles measured in photoproduction of lepton pairs does not occur at the nuclear vertex. Once this assumption is made we may predict that the phase measurement in the colliding beams experiment (not as yet performed) will be the same as in the photoproduction experiment, since the same vertices are involved. Thus we predict a phase of, say, 100°. But the colliding beams diagram is symmetric. Any phase



which appears on the left should appear with reversed sign on the right, and we conclude that the total phase must be zero for each diagram (ρ and ω) separately. The relative phase must then also be zero. If the experiments are correct, and the diagrams shown are the only important ones, then the vertex phases, if any, are not complex conjugates, and time reversal is incorrect.

There are of course other possibilities, but for all of them we conclude that this phase problem is very basic, and an additional experiment is merited.

b) The Colliding Beams Experiment

The experiment we propose is the colliding beams experiment

$$e^+ e^- \rightarrow \mu^+ \mu^-$$

This experiment removes questions of the nuclear vertex, and therefore eliminates one source of anomalous phase. Also, if a non zero phase in the leptonic experiment is observed, and is to be believed by the theoretical

community, it should be as clean a measurement as possible, and for this reason also should not contain a nucleus. The diagrams for this experiment are in Fig. 3. In this experiment, the square of the ρ and ω amplitudes are very small. The QED "background" strongly dominates the squared sum of amplitudes (Fig. 3). The ρ and ω amplitudes are visible via their interference with the Bethe-Heitler amplitude. In other words, one is beating the ρ and ω separately against the fundamental QED process, thus measuring g_ρ, $\varphi_{\rho\ QED}$ and g_ω, $\varphi_{\omega\ QED}$ independently of each other and of the $\rho\omega$ interference effect. The $\rho\omega$ phase is obtained by subtraction. Even a measurement of the ω phase alone might settle the problem. The ρ QED phase has been measured. Three experiments indicate that it is quite small, the experiments by Biggs et al[11], Alvensleben et al[10], and a measurement of electroproduction of muon pairs by Earles et al[28]. Taken together with the large $\varphi_{\rho\omega}$ in the lepton final state experiments, this would indicate a value for $\varphi_{\omega,QED}$ of 40° to 100°.

The result of the proposed experiment is expected to be a broad ρ-QED interference pattern with a narrow ω-QED pattern superposed at the ω mass; Fig. 5 (note suppressed zero) is a quantitative prediction using the unitary model discussed below. The resolution obtainable is that of the colliding beams themselves.

4) The Unitary Model

Recently Earles and Srivastava (ES) have proposed[132] a model for $\rho\omega$ interference which obeys unitarity. The old models, which give rise to Eq. 1, have long been known to violate unitarity, but the effects of unitarity have been expected to be very small. One of the ES findings is that the effects may be large, and indeed may account for the anomalous value of $B_{\omega\pi\pi}$.

Fig 5 $e^+e^- \to \mu\mu$

(Same theory and parameters as Fig 6, $\delta_3 = 0$)

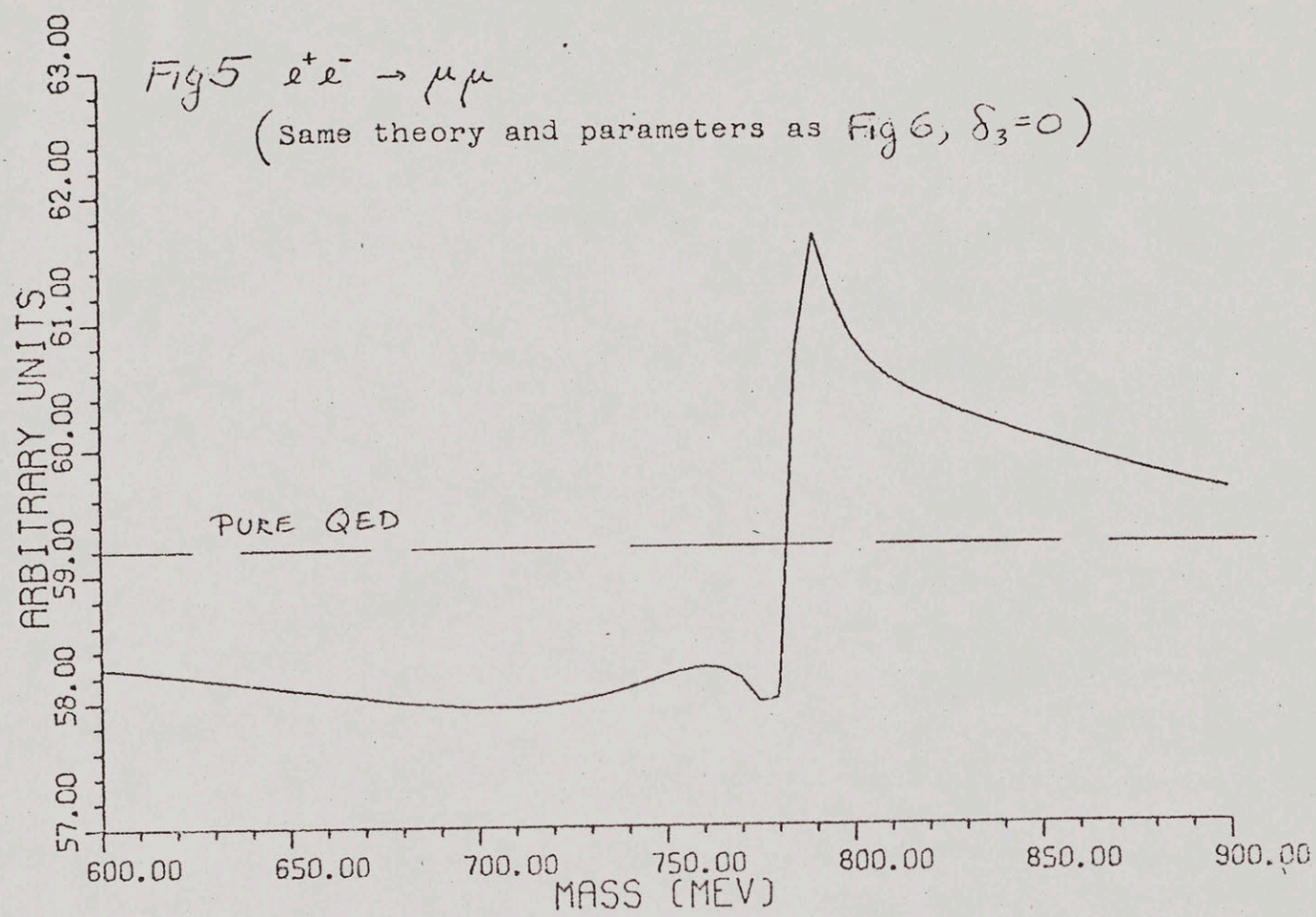
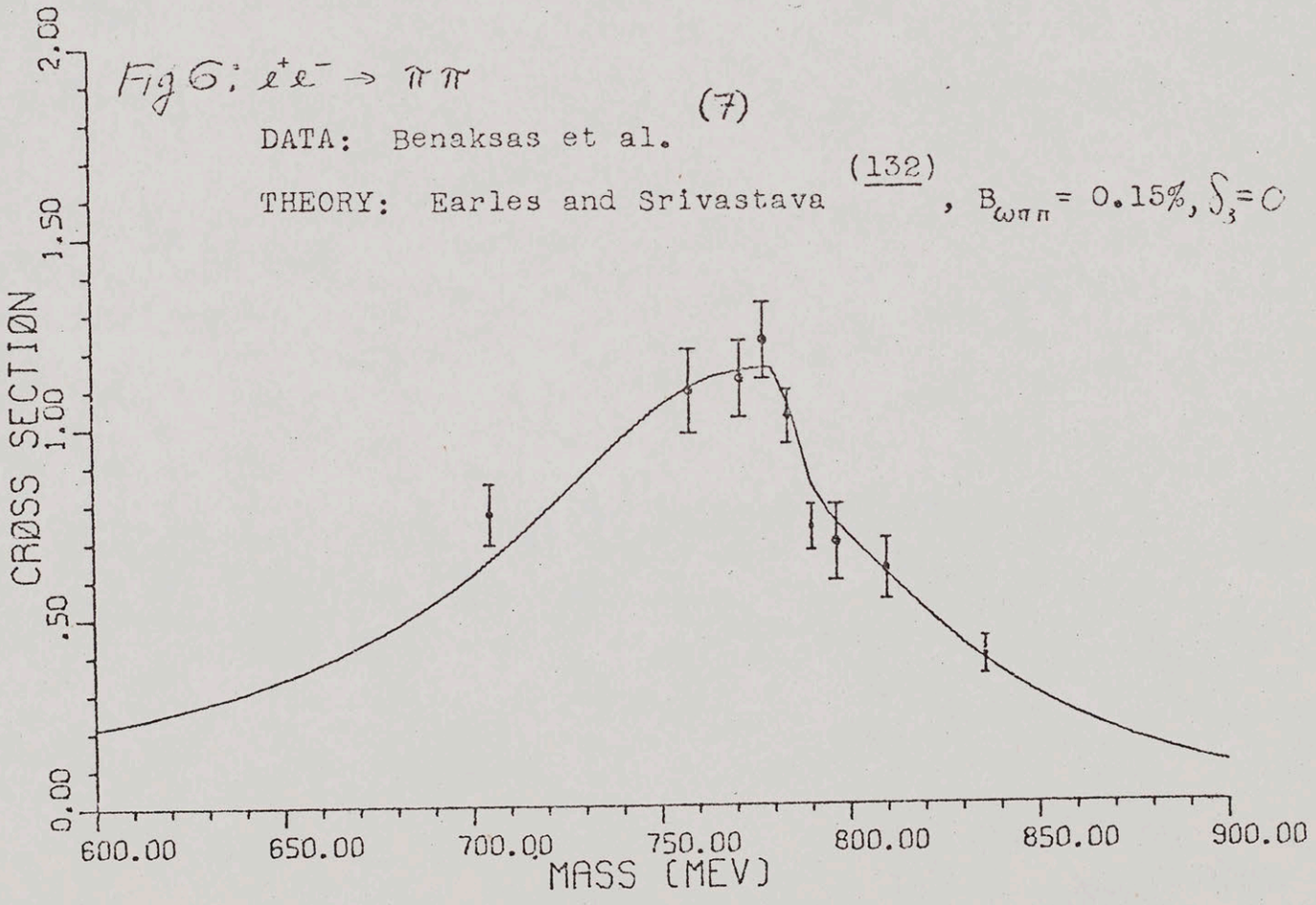


Fig 6: $e^+e^- \to \pi\pi$

(7)

DATA: Benaksas et al.

THEORY: Earles and Srivastava (132), $B_{\omega\pi\pi} = 0.15\%$, $\delta_3 = 0$



The ES model explicitly conserves unitarity and time reversal. There are several parameters of the theory which are fixed by the requirement that the theory describe a pure ρ and a pure ω when no common decay modes exist. There is one arbitrary function of m, which we will refer to as δ_3, which exists when common decay modes do exist.

Using the specific, but ad hoc form, $\delta_3 = 0$ Earles and Srivastava obtained the results shown in Fig. 6, for the $\pi\pi$ channel. In essence what they propose is that there is no anomalous branching ratio. Rather the anomalie appears as a result of fitting an incorrect theory. The large terms in their model, caused by unitarity conservation, make $B_{\omega\pi\pi}$ appear larger. Using the ES model, they fit the data with values of $B_{\omega\pi\pi}$ varying from 0.07% to 0.15%, with $\delta_3 = 0$. These results are fairly consistent with VMD. There are fundamental problems with some forms of δ_3, such as $\delta_3 = 0$. Essentially, the form of δ_3 reflects the effects of detailed dynamics. In particular, setting $\delta_3 = 0$ means that one of the resonances is no longer a pole. The points of importance here are (a) the ES theory conserves unitarity and time reversal and fits the data, with $\delta_3 = 0$, by violating a standard but less basic theoretical assumption; (b) the specific form used for δ_3 may cause a violation of other assumptions of dynamics, not as fundamental as unitarity and time reversal. (c) With some forms of δ_3 we can obtain interference shapes in the $\mu\mu$ channel which would, in terms of Eq. 1, be interpreted as a phase of near 90^o.

Fig 5 shows a result of the ES theory applied by us to the colliding beams experiment, using $\delta_3 = 0$, as in Fig. 6. If these results were interpreted by the theory of Eq. 1, the deduced phase would be 0^o. We have also used other *ad hoc* forms of δ_3, and managed to get phases of the order of 90^o. These assumed forms of δ_3 gave higher phase angle results in the $\mu\mu$ channel,

but had large consequences in the $\pi\pi$ channel. It appears evident, but is not yet proven, that there exists a class of δ_3 functions which can meet requirements of the data in both the $\pi\pi$ and lepton final states. If this speculation is correct, it means that the anomalously large phase observed in experiments on the lepton final state need not signal time reversal or unitarity breakdown, but may signal the inapplicability of some lesser assumption of meson dynamics, as measured by the presence and detailed form of δ_3.

5. The Proposed Experiment

The VEPP2' colliding beams machine has a design luminosity of $10^{31}/cm^2$ sec. During our visit of August 1974, the machine was turned on for the first time, and had the luminosity expected at turn on -- about $10^{28}/cm^2$ sec. Since that time L has been steadily increased, and in May 1974 was above $10^{29}/cm^2$ sec. It appears very probable that a luminosity of $10^{30}/cm^2$ sec will be achieved during 1975.

At this luminosity the experiment will take less than two months. We will take 15 data points with 1% statistics. This will determine $\varphi_{\omega,QED}$ and $\varphi_{\rho QED}$ to about $\pm 10^{\circ}$.

Expected data rates are shown in Table 3. This table should clarify our need to do the experiment at Novosibirsk. (For example, at Orsay the experiment would take about 5 years.)

We are still actively considering more than one possible detector system, but it now appears likely that we will settle on a range detector which will cover 1/3 of the total solid angle.

The detector is shown in Figure 7. Data are collected by an on-line PDP11/45. About 13 radiation lengths of lead take out a large background of electrons and reduce the most sinister background - π pairs - by a factor of

	$10^{29}/sec^2 cm$	$10^{30}/sec^2 cm$	(VEPP 2' DESIGN) $10^{31}/sec^2 cm$
$\pi^+ \pi^-$	5000/day 250/hour 4/min	50,000/day 2500/hour 40/min 0.65/sec	25,000/hour 400/min 6.5/sec
$\mu^+ \mu^-$	300/day 15/hour	3,000/day 150/hour	30,000/day 1500/hour 25/min .4/sec
Time for 10,000 $\mu\mu$ events	30 days	3 days	7 hours
Time for 15, 1% points	1.25 year	1½ months	5 days

TABLE 3: Estimated $\pi\ \pi$ and $\mu\ \mu$ Rates in a $\frac{4\pi}{3}$ Detector at Various Luminosities

about 3. The apparatus is triggered on approximately back-to-back pairs as determined by a coarse scintillation hodoscope. Proportional and/or spark chambers permit the off-line selection of exactly back-to-back pairs originating in the interaction region. The main experimental problem is separating back-to-back muons from the pions which are about 20 times more numerous, and vary more rapidly than the muon signal in exactly the most critical region. Clearly this background must not only be dealt with cautiously, but must be measured carefully. The π pair rejection is done by a combination of techniques. The main rejection is by means of range measurement. Nuclear absorption is also used, as noted above, as is nuclear scattering in the absorber. Finally, we plan to use observation of the fast decay $\pi^+ \rightarrow \mu^+ + \nu$ as an anti signal. This latter is probably not necessary, but is an added surity.

The present design calls for a tank of liquid scintillator subdivided into "counters" by Teflon sheets. Fig. 7 shows pion and muon peaks at various incident angles and beam energies, with widths calculated from range straggling. A cut, for example, halfway between the pion and muon peaks will yield a rejection factor better than 10^4 per side, and at the same time a negligible loss of muons. In practice, processes such as electron knock-ons, and in flight $\pi \rightarrow \mu\ \nu$ decay will contribute a smooth but negligible background under the muon peak; these rates have been calculated or estimated and are not expected to be serious.

6. <u>The Decay $\rho^o \rightarrow \pi^o + \gamma$</u>

A second experiment has been proposed to Novosibirsk. It involves the reaction $e^+ + e^- \rightarrow \pi^o + \gamma$. This experiment would be done at the same time as the $\mu\mu$ experiment. The diagrams of interest are shown in Figure 8.

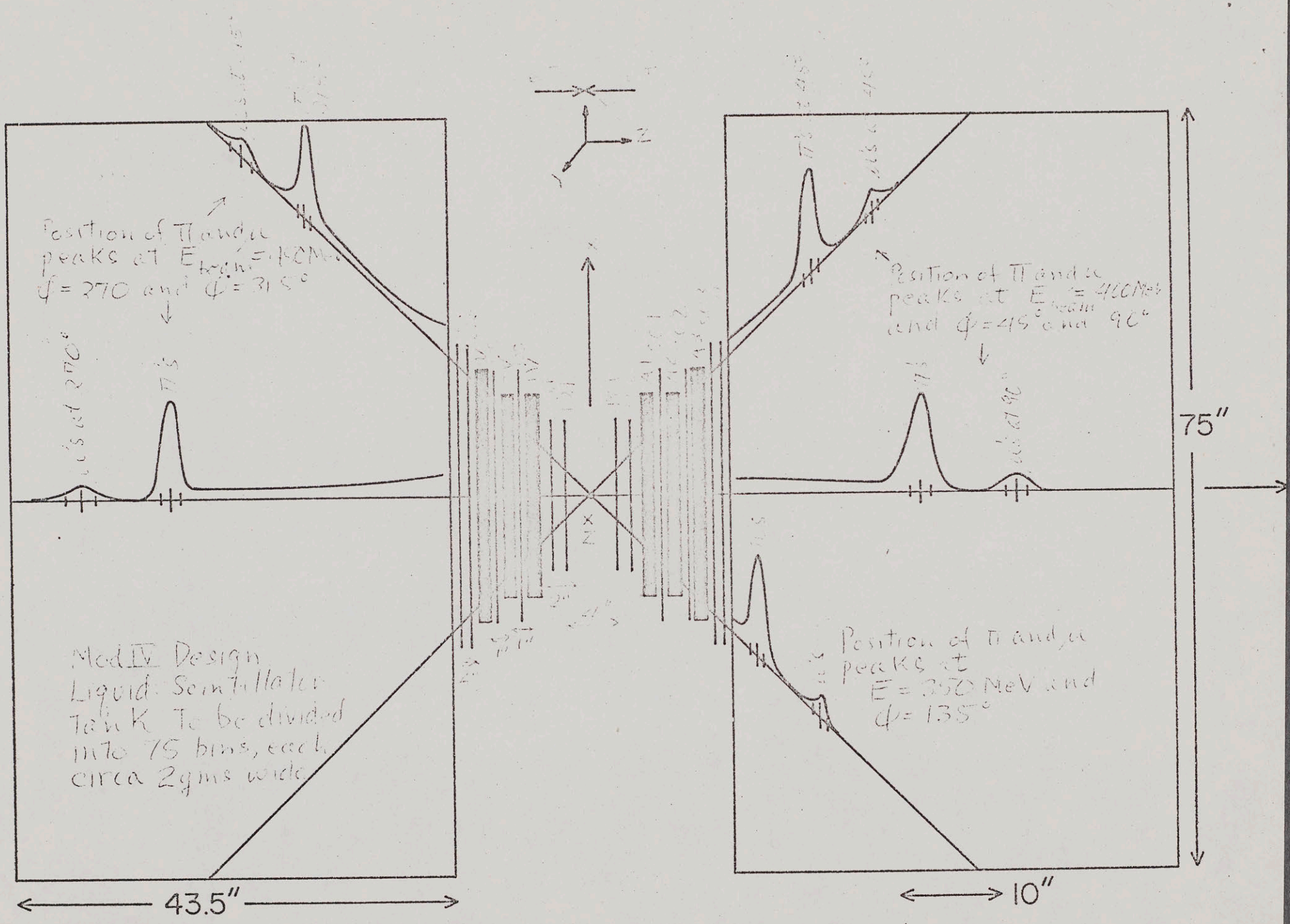
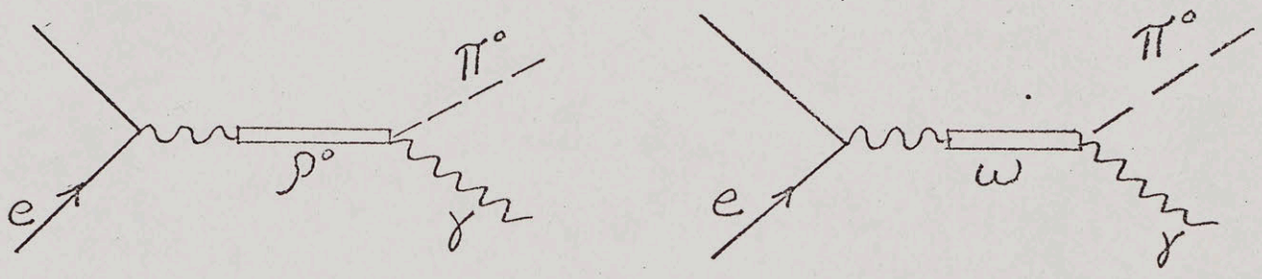


Fig. 7: MUON PAIR DETECTOR FOR PROPOSED EXPERIMENT

Figure 8



The purpose of this experiment is to measure the branching ratio of $\rho \rightarrow \pi^{o}\gamma$. This branching ratio is of considerable interest not only phenomenologically, but also because its magnitude has been predicted by a zero parameter current algebra theory, and is also given by SU_3 arguments. The branching ratio is expected to be of the order of 0.1% which makes it a very difficult quantity to observe. However, the interference of the small ρ term and the large ω term is expected to distort the ω resonance, introducing an asymmetry. The asymmetry is functionally dependent upon the magnitude of the ρ branching. Design studies are still in progress, but the magnitude of the effect appears promising.

7. The Collaboration and Funding

This experiment is planned as a collaboration between our group, and a group at VEPP2' directed by V. Sidorov. The use of VEPP2' is a necessity, as noted in the previous section, due to its uniquely high luminosity in the required energy region.

There has been an exchange of correspondence for about 1½ years and two of us, Weinstein and Earles, have visited the laboratory where VEPP2' was just being turned on. The conditions of the collaboration agreed to thus far include about four members of NU in the collaboration, and four to six members of the Novosibirsk group. Our group will supply most of the detector equipment, an on line PDP 11/45 with peripherals, and required programming.

Negotiations are presently going on concerning what equipment is to be left at VEPP2', and the US and USSR desires have not yet been reconciled. The US-USSR Joint Committee on Cooperation, established as a result of Article 5 of the June 21, 1973 Nixon-Brezhnev agreement, has considered this proposed collaboration via the February 1974 meeting of specialists. The proposed collaboration appears to have been very favorably discussed. Hopefully, the mechanisms set up by this group, to facilitate collaborative efforts of just this nature, will speed the final agreement on the collaboration. But we must certainly assume that more than the usual political and logistic problems attend this experiment.

As a reflection of these uncertanties, the budget has been divided into two parts. The largest of these is being separately submitted to the NSF Office of International Programs. This portion contains all items uniquely needed for the large logistic and equipment costs of the $\rho\omega$ experiment. A smaller portion of the budget, described below, is contained in this proposal. If we are successful in settling our US-USSR collaboration this smaller budget will also be used for the $\rho\omega$ experiment. If not, it is this portion of the budget which will permit us to pursue our traditional QED interests, for example via the Double Electron Pair Experiment described in Sec. IIE.

In particular, our PDP9 is totally tied up at NAL, and is fully programmed for the NAL work. We propose to assemble a PDP11 facility in a portable data trailer. We expect that this will be used at Novosibirsk but if it is not, it will be used

a) At the Bates Linac for a low energy measurement of Double Electron Pairs (DEPP)

b) At the Cornell Synchrotron for a high energy measurement of DEPP

c) At the Bates Linac for calibration of the efficiency of the neutron detector of NAL exp 51A.

and/or, d) On either or both of the BNL experiments described in Sec IIE.

At present our ability to pursue any QED type experiments is severely hampered by the lack of such a computer.

The present budget also includes that portion of travel budgets which will be needed for US experiments (by those of us who would be involved in the $\rho\omega$ experiment) if the Novosibirsk experiment is blocked.

Aside from these items only a relatively small amount is included in the present proposal budget for prototype detector development for the $\rho\omega$ experiment. Except for this small item the budget of this proposal contains only items which would be needed for other experiments if the $\rho\omega$ collaboration is not completed.

REFERENCES ($\rho\omega$)*

1. See for example R. G. Sachs et al., Phys. Rev. D5, 590 (1972).
2. G. Goldhaber, Experimental Meson Spectroscopy Conference 1970, U. of Pa., Columbia Univ. Press, pg 59.
3. Wicklund et al, "K(890) production and $\rho\omega$ interference from 3 to 6 GeV/c" IV International Conference on Meson Spectroscopy, Northeastern University, April 1974.
4. Biggs et al., Phys. Rev. Lett. 24, 1201 (1970).
5. Moffeit et al., Nucl. Phys. B29, 349 (1971).
6. Alvensleben et al, Phys. Rev. Lett. 27, 888 (1971).
7. Binaksas et al., Phys. Lett. 39B, 289 (1972).
8. Auslander et al., Soviet Journal of Nuclear Physics, 9, 69 (1969).
9. Biggs et al., Phys. Rev. Lett. 24, 1197 (1970).
10. Alvensleben et al., Phys. Rev. Lett. 25, 1373 (1970).
11. Biggs et al., Phys. Rev. Lett. 27, 1157 (1971).

*Underlined references in the text refer to group publications and are listed in Section VI.

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UNION CARBIDE
POST OFFICE BOX X
OAK RIDGE, TENNESSEE 37830

January 27, 1975

Professor Victor Weisskopf
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Vicki:

I am enclosing a copy of the Bogolubov letter which Herman Postma sent to you in December.

With best regards.

Sincerely yours,

Alex

Alexander Zucker
Associate Director

AZ:c

Enclosure

OAK RIDGE NATIONAL LABORATORY
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POST OFFICE BOX X
OAK RIDGE, TENNESSEE 37830

Office of the Director

December 18, 1974

Professor Victor Weisskopf
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professor Weisskopf:

Enclosed is a draft of a letter I propose to send to Bogolubov as we continue to negotiate toward an exchange of US-Soviet scientists in the field of heavy element studies.

The Dubna Scientific Council meets in January and it would be good if they could act on this business at that time. I would like, therefore, to ask you for a review of this letter at the earliest opportunity.

Sincerely yours,

Signed – Herman Postma
Herman Postma

HP:c

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DRAFT

Office of the Director

December 18, 1974

Dr. N. Bogolubov, Director
Joint Institute for Nuclear Research
Head Post Office
P. O. Box 79
Moscow, U. S. S. R.

Dear Dr. Bogolubov:

During his visit to the Soviet Union in October, Dr. John Teem initiated discussions concerning a possible US-USSR collaboration in the field of heavy element synthesis as part of the scientific exchange in fundamental properties of matter. In November, Dr. Alexander Zucker of this Laboratory and Professor G. N. Flerov of the Joint Institute for Nuclear Research developed these ideas in more detail. In a meeting at Dubna on November 21 between Dr. Zucker and Drs. Shimane, Shcherbakov, Flerov, Kaun, Oganesyan, and Shvanev, this matter was discussed more formally; the minutes of this meeting show that the parties present agreed on the value and importance of the scientific collaboration. As an outgrowth of these discussions between Soviet and U. S. scientists, we are now ready to propose a tentative collaborative research plan for your comments.

The exchange would take place in two phases. During the first phase, beginning in the Spring or Summer of 1975, two Soviet scientists would visit the Oak Ridge National Laboratory and, at approximately the same time, two United States scientists would visit the Joint Institute for Nuclear Research. Each of these visits would last about four weeks. The purpose of these visits would be to familiarize the Soviet-US groups with each other's equipment, and to carry out preliminary experiments of mutual interest. We suggest that an appropriate experiment for these two visits concern the alpha decay and spontaneous fission branching of $^{255}104$. This nuclide was first made at the JINR by bombarding a lead target with a titanium beam. This nuclide could be further studied at your Institute by means of your recently developed alpha recoil time-of-flight mass identifying spectrometer. At

the Oak Ridge National Laboratory, we would bombard ^{243}Cm or ^{244}Cm with ^{16}O to produce the isotope $^{255}104$ and study its properties with our X-ray alpha coincidence technique.

In the course of this work, Soviet scientists would gain familiarity with the alpha X-ray apparatus, and U. S. scientists would become familiar with the experimental arrangement and procedures at the Joint Institute for Nuclear Research. Such experience will be valuable when the main phase of the exchange takes place about a year later.

The principal effort of the exchange would be in the second phase which would begin in the Spring of 1976. This part of the collaboration would involve a visit of about six-months' duration by approximately four American scientists from the Oak Ridge National Laboratory to the Joint Institute for Nuclear Research. The U. S. scientists would bring with them targets of heavy elements such as ^{248}Cm, ^{249}Bk, ^{249}Cf. They would also bring with them a fairly complete system of experimental apparatus including detectors, electronics, pumps, a small computer, and other specialized hardware, to be used for the alpha X-ray coincidence method of identifying elements. The equipment would be mounted in a trailer for transport to Dubna and for use there. The trailer would also contain electrical gear necessary to convert the 50 cycle 220 volt power to 60 cycle 110 volts required by the experimental equipment. By assembling the ORNL apparatus here and testing it with lighter mass ions from the Oak Ridge cyclotron, the collaborative experiments with the Soviet scientists could commence in Dubna with minimum delay after the trailer arrives.

We propose for your consideration a set of three reactions for study in the US-USSR collaboration at JINR. These reactions are appropriate for the very intense ^{22}Ne beams available at Dubna, and for our heavy element targets. They are:

1) $^{248}_{96}Cm + ^{22}_{10}Ne \rightarrow ^{265}106 + 5n$

2) $^{249}_{97}Bk + ^{22}_{10}Ne \rightarrow ^{267,266}107 + (4n,5n)$

3) $^{249}_{98}Cf + ^{22}_{10}Ne \rightarrow ^{267}108 + 4n$

These reactions are of particular interest because, in addition to making new isotopes and as yet undiscovered elements, they allow

the investigation of spontaneous fission systematics in the heaviest element region which is so important for evaluating the possibility of superheavy element stability. After these experiments are completed, the ^{248}Cm target could be bombarded with ^{48}Ca, with a view to producing and identifying element 114 in the reaction

$$^{248}_{96}Cm + ^{48}_{20}Ca \longrightarrow ^{292}_{116} + 4n \downarrow \alpha \; ^{288}_{114}$$

We estimate that the four experiments together will require a minimum of thirty 24-hour days of actual beam on target at a useable intensity during the six months. However, more beam time may be required if the processes to be studied turn out to have low cross sections or unfavorable branching ratios, or if some unforeseen difficulties arise.

All experiments would be carried out jointly by U. S. and Soviet scientists, and oral or written publications would also be under joint authorship, requiring approval by the principal investigators involved in the research.

During the visits the Soviet scientists in Oak Ridge and the U. S. scientists in Dubna would be accorded all the rights and privileges that usually accrue to scientists of equal rank and experience. For its part, the Oak Ridge National Laboratory will provide housing and living expenses of $20 per day for each of the two visitors during their stay here. The Oak Ridge National Laboratory will also defray the cost of purchase and construction of our experimental equipment and the expenses associated with transporting it by trailer to Dubna. We would request that the U.S.S.R. provide air transportation between New York and Moscow for the scientists involved in the one-month visits; and for the scientists, their families, and a reasonable amount of household luggage for the six-month visit. Living expenses as well as those associated with housing for American scientists in Dubna will be defrayed by us. However, we propose that JINR arrange for suitable housing. For the six-month visit to Dubna, we request that American scientists be allowed to bring a reasonable number of automobiles, and that access to gasoline as well as appropriate documents be issued by the U.S.S.R. including permanent visas for travel between Dubna and Moscow.

We present these views for your consideration and we look forward to your comments. Before the proposed visits can take place, no doubt an official protocol will have to be completed between the U. S. and the U.S.S.R. We expect, however, that the protocol will be based on conclusions reached in the present exchange of letters.

Sincerely yours,

Herman Postma
Director

HP:c

cc: O. L. Keller
P. H. Stelson
K. Strauch
J. M. Teem
V. Weisskopf
A. Zucker

A CHRONOLOGICAL HISTORY OF ELEMENT 104

(Torts and Retorts)

June 1964 - G. N. Flerov and S. I. Polykanov

First report of production of a ~0.3 sec. spontaneous fission activity produced in $^{242}Pu(^{22}Ne,4n)$ reaction (E_{Ne} 113-115 MeV) with $\sigma \sim 2 \times 10^{-34}$ cm^2. Report to Congress on Nuclear Physics, Paris (June, 1964).

October 1964 - G. N. Flerov, Yu. Ts. Oganesyan, Yu. V. Lobanov, V. I. Kuznetsov, V. A. Druin, V. P. Perelygin, K. A. Gavrilov, S. P. Tret'yakova and V. M. Plotko. "Synthesis and Physical Identification of the Isotope with Mass Number 260 of Element 104." Formally reported discovery of $^{260}104$. Spontaneous fission activity with $t_{1/2} = 0.3 \pm 0.1$ sec. produced via $^{242}Pu(^{22}Ne,4n)$ (E_{Ne} = 113-115 MeV) using belt catcher, mica track detectors and using internal beam of U-300 cyclotron at Dubna. $\sigma_{max} \sim 1.3 \times 10^{-34}$ cm^2. Measured excitation function in range 108-124 MeV.

Atomnaya Energiya 17, 310 (1964) [trans. Soviet J. At. Energy 17, 1046 (1964)]. Also published in Phys. Lett. 13, 73 (1964).

1966 - I. Zvara et al.

"Chemical Properties of Element 104." Using gas phase chloride volatility techniques recorded 12 atoms of $^{260}104$ using track detectors. Follows Hf in passing thru the apparatus and not actinides. They therefore claim element 104 is member of Group IV of periodic system. Experiments done on internal beam of U-300. Half life results agree with 0.3 ± 0.1 sec. I. Zvara, Yu. T. Chuburkov, R. Tsaletka, T. S. Zvarova, M. R. Shalaevski and B. V. Shilov, Atomnaya Energiya 21, 83 (1966) (Publ. in Aug. 196[illegible] [trans. Soviet J. At. Energy 21, 709 (1966)], [also trans. Nucl Energy 21, 60[illegible] (1967)].

[illegible] 1966 - G. N. Flerov

Names element 104 Kurchatovium in honor of Igor Vasil'yevich Kurchatov at meeting of Scientific Council of Joint Institute for Nuclear Research held on 7/6/66. Claim based on excitation function and on Zvara's chemistry.

Nov. 1967 - G. N. Flerov and V. I. Kuznetsov. "The Heaviest Atom"

Report on 104 experiments, excitation function measurements and chemistry, and document naming element 104 Kurchatovium on 7/6/66.

G. N. Flerov and V. I. Kuznetsov, Priroda, No. 11, Moscow, Nov., 1967, pp. 35-44.

Feb. 1968 - I. Zvara, Yu. T. Chuburkov, R. Caletka, M. R. Shalaevsky

JINR P7-3783. Further experiments on gas phase chloride chemistry of 104 using internal beam of U-300. This report was finally published in 1969 in Radiokhimiya 11, 163 (1969).

April 19, 1968 - E. D. Donets and V. A. Shchegdov

"An Attempt to Observe the Alpha Decay of ^{260}Ku." Reported attempts to observe α decay of ^{260}104 without success in ^{242}Pu(^{22}Ne,4n) reaction by observing granddaughter ^{252}Fm. Argues that 5n reaction leading to ^{259}104 should have 5X bigger cross section. Also could not observe ^{242}Pu(^{22}Ne,α4n)^{256}No reaction ($\sigma < \sim 2 \times 10^{-34}$ cm^2). Suggests that 0.3 sec. activity observed by Flerov in 1964 and by Zvara in 1966 was really ^{259}104 and not ^{260}104 because estimated cross section for production of ^{260}104 is $<1 \times 10^{-33}$ cm^2 based on σ limits for ^{242}Pu(^{22}Ne,α4n)^{256}102.

Joint Institute for Nuclear Research Dubna Report, JINR P7-3835, April 19, 1968, Dubna, USSR. Could not find formal publication in literature.

April 1968 - I. Zvara

Reports on 104 gas phase chemical experiments using external beam of U-300 at San Francisco ACS meeting. Observed 14 atoms of 104.

Jan. 1969 - A. Ghiorso, M. Nurmia, J. Harris

"Search for a 0.3 sec Spontaneous Fission Activity in Element 104." Report negative results in attempting to produce $^{260}104$ in $^{253}Es(^{10}B,3n)$, $^{253}Es(^{11}B,4n)$ reactions. Set limit of ∿8 nb for $^{253}Es(^{11}B,4n)^{260}104$ (0.3 sec.) at ∿60 MeV and expected ∿17 nb. UCRL-18714, Jan. 1969. Nucl. Chem. Ann. Report for 1968, UCRL 18714, p. 61.

Jan. 1969 - A. Ghiorso, M. Nurmia, J. Harris, K. Eskola and P. Eskola

First report of production of $^{257}104$ produced in $^{249}Cf(^{12}C,4n)$ reaction. Also $^{259}104$ from $^{249}Cf(^{13}C,3n)^{259}104$. Did daughter recoil expts. Tentative production of 11-msec $^{258}104$ in $^{249}Cf(^{12}C,3n)^{258}104$ and $^{249}Cf(^{13}C,4n)^{258}104$. Activities not produced in $^{249}Cf + ^{14}N$ or $+^{11}B$ nor in ^{246}Cm or $^{248}Cm + ^{12}C$ and ^{13}C. Called tentative assignment of Z = 104. UCRL-18711, Jan. 1969. Nucl. Chem. Prog. Rept.

April 15, 1969 - A. Ghiorso

Official report of discovery of $^{257}104$, $^{258}104$ and $^{259}104$ at Minneapolis ACS meeting (Mendeleev Centennial). Also reported negative results in search for $^{260}104$. A.E.C. Press Release dated 4/15/69 No. M-87.

April 30, 1969 - A. Ghiorso, M. Nurmia, J. Harris, K. Eskola, and P. Eskola

Date of release of UCRL-18819 entitled "Positive Identification of Two Alpha Particle Emitting Isotopes of Element 104." Identification of $^{257}104$, $^{258}104$ and $^{259}104$. Formal publication Phys. Rev. Lett. 22, 1317 (1969).

June 25, 1969 - I. Zvara, Yu. T. Chuburkov, V. Z. Belov, G. V. Buklanov, B. B. Zakhvataev, T. S. Zvarova, O. D. Maslov, R. Caletka, M. R. Shalaevsky, Dubna Preprint D7-4542. Finally published in JINC 32, 1885 (1970). "Experiments on Chemistry of Element 104 - Kurchatovium. V. Adsorption of Kurchatovium Chloride from the Gas Stream on Surfaces of Glass and Potassium Chloride." Reports gas phase chemistry of 104 using external beam of U-300. Observed ∿50-60 events which according to them

"imply the half life of somewhat less than one second" in agreement with original 1964 value of ~0.3 sec. by Flerov et al. Also mentions that mass assignment of A = 260 could possibly be different.

July 15, 1969 - V. A. Druin

"Synthesis of Transmendelevium Elements in Nuclear Reactions Induced by Heavy Ions." Reports on 102 and 104 work at Dubna and compares to Berkeley 102 and 104 work. Casts doubt on Berkeley 104 work because of similarity of $^{257}104$ alpha spectrum to that due to Pb target impurities. Tells of 104 chemical work of Zvara et al. Suggests that 0.3 sec. activity could possibly be attributed "partially or completely due to either the decay of $^{261}104$ or $^{259}104$" or to both. V. A. Druin in "Nuclear Reactions Induced by Heavy Ions" edited by R. Boch and W. R. Hering, North-Holland Publ. Co., Amsterdam, 1970, p. 657.

July 15, 1969 - M. Nurmia

"Investigations of Transuranium Elements at Berkeley." Reports on Berkeley 102, 103, $^{257}104$, $^{258}104$, $^{259}104$ work and negative results for $^{260}104$ in $^{248}Cm + ^{16}O$ and ^{18}O and in $^{253}Es + ^{10}B$ and ^{11}B. M. Nurmia in "Nuclear Reactions Induced by Heavy Ions," edited by R. Boch and W. R. Hering, North-Holland Publ. Co., Amsterdam, 1970, p. 666.

November 4, 1969 - G. N. Akapiev, V. A. Druin, V. I. Rud and Sun Tsin Yan

"On the Role of α-Radioactive Background in Investigating α-Decay of Element 104." Preprint attacking Berkeley work on 104 isotopes because of activities produced from Pb impurities in target material. Suggest that Berkeley work is in error. Finally published in Yadernaiya Fizika? JINR-P7-4772, Dubna preprint.

November 1969 - Yu. Ts. Oganesyan, Yu. V. Lobanov, S. P. Tretyakova, Yu. A. Lazarev, I. V. Kolesov, K. A. Gavrilov, V. M. Plotko and Yu. V. Plubogarinov.

Polub.

"Identification of the Elements 102 and 104 by Means of the Collimation Method."

Measured $^{260}104$ by "collimation method" and showed was compound nucleus reaction. Also remeasured $t_{1/2}$ to be 0.1 ± 0.05 sec. and claims that the 1964 experiments led to a longer half life because of the "intense neutron beam inside the accelerating chamber. This fact must lead to an increase of the half life." Also claim that the 0.1 sec. fission activity "with half life of 0.1 sec. is undergone by the elements $^{259}104$ or $^{260}104$" and not $^{261}104$. Since the $t_{1/2}$ for $^{259}104$ is 3 sec as measured by Berkeley workers, the observed $t_{1/2}$ of 0.1 sec. is claimed not to be $^{259}104$. JINR Preprint P7-4797, Dubna, 1969. Finally published in Atomnaya Energiya 28, 393 (1970), [trans. Soviet J. At. Energy, 28, 502 (1970)].

November 17, 1969 - A. Ghiorso

"The Berkeley Hilac Heaviest Element Research Program." At Robert A. Welch Foundation Conference on The Transuranium Elements - The Mendeleev Centennial, Houston, Texas. Describes in detail the production of $^{257}104$, $^{258}104$, $^{259}104$ and first reports discovery of ∿70 sec. $^{261}104$ produced in $^{248}Cm(^{18}O,5n)$ reaction. Also reports "First Aqueous Chemistry of Element 104" as done by R. J. Silva et al. Ghiorso suggests on basis of excitation function calculations that Soviet SF activity must be A = 260 and less likely A = 259. Reports negative results for ∿0.1 sec. activity of A = 260 as produced in $^{246}Cm + ^{18}O$, $^{248}Cm + ^{16}O$. First proposal of name "Rutherfordium" for element 104. Says "1 α worth 10,000 fissions" in search for new elements. A Ghiorso in "Proceedings of the Robert A. Welch Foundation Conference on Chemical Research. XIII. The Transuranium Elements - The Mendeleev Centennial," W. O. Milligan, editor, Houston, Texas, 1970, p. 107.

November 17, 1969 - I. Zvara

"Transmendelevium and Superheavy Elements in Laboratory and Nature." At Robert A. Welch Foundation Conference on the Transuranium Elements - The Mendeleev Centennial, Houston, Texas. Recapitulation of Dubna 104 experiments including revised $t_{1/2}$ of 0.1 sec. for $^{260}104$. Says mass assignment of 1964 work is 260 $\pm$ 1 from excitation functions. Also says his 1968 work (JINR P7-3783) and published in Radiokhimiya 11, 163 (1969) supports "retention time of the order of one second." Now he says "that we deal with the Kurchatovium activity (or activities) with the half life of about 1 sec. or more with tails of the decay curve of the 0.1 sec. Kurchatovium activity." Also reports on remeasurement of $t_{1/2}$ for $^{260}104$ as derived from collimation experiments (JINR P7-4797) and also says there is a longer lived component in the decay curve with much lower cross section than the 0.1 sec. fission activity.

I. Zvara in "Proceedings of the Robert A. Welch Foundation Conference on Chemical Research XIII, The Transuranium Elements - The Mendeleev Centennial," W. O. Milligan, editor, Houston, Texas, 1970, p. 153.

March 1970 - A. Ghiorso, M. Nurmia, K. Eskola and P. Eskola, "^{261}Rf; New Isotope of Element 104." Report discovery of $^{261}104$ - UCRL-19565. Finally published in Phys. Lett. 32B, 95 (1970).

April 1970 - G. N. Flerov

"Synthesis and Search for Heavy Transuranium Elements." Casts doubt on Berkeley 104 work because of Pb target impurities and because half lives do notcome out correctly. Suggests that Berkeley repeat the 1969 work under cleaner conditions. Finally published in Atomnaya Energiya 28, 302 (1970), [trans. Soviet J. At. Energy 28, 390 (1970)].

August 1970 - R. Silva, J. Harris, M. Nurmia, K. Eskola and A. Ghiorso

"Chemical Separation of Rutherfordium." Report chemical separation of Rutherfordium using ∿70 sec. $^{261}104$. Finally published in J. Inorg. Nucl. Chem. Lett., 6, 871 (1970).

September 15, 1970 - A. Ghiorso, M. Nurmia, J. Harris, K. Eskola and P. Eskola

"In Defense of the Berkeley Work Concerning....."

Defend original 104 work at Berkeley and show that Pb impurities are no problem and that half lives are correct. Repeated the 1969 experiments with more detectors and with better statistics. Explained isomerism in $^{250}Fm + ^{254}No$. UCRL-19974. Finally published in Nature, 229, 603 (1971).

February 11, 1971 - Yu. Ts. Oganesian and I. Zvara

"Spontaneous Fission of Isotopes of Kurchatovium and Nielsbohrium"

Reviewed history of 104 and now claim to have produced 4.5 ± 1.5 $^{259}104$ in addition to 0.1 sec $^{260}104$ and measured SF branching of $^{259}104$ to be ∿20%. Zvara claims to have done his chemical experiments with ∿4 sec $^{259}104$ since he repeated the experiments in late 1970. ^{259}Ku was identified by excitation function and by "collimation method." "Spontaneous Fission of Isotopes of Kurchatovium and Nilsborium," G. N. Flerov, et al. in Proceedings of the International Conference on Heavy Ion Physics, Dubna, USSR, Feb. 11, 1971, JINR Report P7-5769, p. 125; also I. Zvara; ibid., p. 145.

April 1971 - M. Nurmia

Talk at APS Meeting, Washington, D. C. UCRL-20497 entitled "Heavy Element Research at the Berkeley Hilac." Tells about competition with Dubna group and repeats "one α particle is worth maybe 100, maybe 10,000 fissions." Says lower limit to $t_{1/2}(SF)$ is 500 sec for $^{261}104$. From curve, estimated $t_{1/2}(SF)$ for $^{260}104$ = few μsec. Also talks about trends in Q_α and $t_{1/2}(SF)$ values. Talk is also condensed in LBL-666, p. 42 (1972), "Nucl. Chem. Ann. Rept. for 1971," May 1972.

July 6, 1971 - I. Zvara, V. Z. Belov, L. P. Chelnokov, V. P. Domanov, M. Hussonois, Yu. S. Korotkin, V. A. Schegolev and M. R. Shalayevsky. "Chemical Separation of Kurchatovium." Claims all chemistry done by them previously was done with ^{259}Ku, not ^{260}Ku since ^{260}Ku has half-life of 0.1 sec. Since Flerov, Lazarev, Lobanov and Oganesyan reported 4.5 ± 1.5 sec ^{259}Ku at 1971 Dubna Heavy Ion Conference from ^{242}Pu(^{22}Ne,5n) and since 0.1 sec ^{260}Ku would not have made it thru column, they must have been working with ^{259}Ku. Reasons for ^{259}Ku (1) $E_{^{22}Ne}$ = 119 Mev, i.e., close to σ_{max} for (^{22}Ne,5n); (2) No exponential decrease in tracks as is expected for 0.1 sec activity; (3) Increasing $E_{^{22}Ne}$ above 125 MeV decreases yield of "eka hafnium" and is consistent for excitation curve of ^{259}Ku. Says no reason to call 104 Rutherfordium since 1964 experiments were done with ^{260}Ku, both done before 1969 Berkeley experiments. "There is no ground to use the name rutherfordium proposed by them." Inorg. Nucl. Chem. Lett. 7, 1109 (1971).

July 28, 1971 - A. Ghiorso, M. Nurmia, K. Eskola and P. Eskola "Comments on 'Chemical Separation of Kurchatovium'". Do not agree with Zvara's July 6, 1971 claim to chemistry on ^{259}104. (1) Claims branching limit for SF <20% and is not likely to be more than 10% which perhaps could not account for the fission events observed by Soviets. Expected $t_{1/2}$(SF) of ^{259}Rf to be "orders of magnitude"longer than ^{261}Rf which is >500 sec for $t_{1/2}$(SF). (2) Says Dubna results give no information on half life and claim the 16 fissions observed by them could be breakthru of ^{256}Md-^{256}Fm since ~2% of activity passes column anyway. (3) Claim Soviets should have seen 0.1 sec activity decaying "in flight" in chromatographic column.

Says "We believe that these comments raise some valid questions as to whether or not element 104 (kurchatovium-Ku) was chemically isolated and identified." Inorg. Nucl. Chem. Lett. 7, 1117 (1971).

~Jan. 1973 - V. V. Stantso "Brackets in the Mendeleev Periodic Table. What is Behind Them." Chemistry and Life, No. 1, 1973, pp. 3-12. Reviews 102-105 experiments from Soviet point of view.

March 23, 1973 - V. A. Druin, Yu. S. Korotkin, Yu. P. Kharitonov, V. I. Krashonkin, Yu. V. Lobanov, D. M. Nadkarin, S. P. Tretyakova. "On Nonobservation of the Spontaneously Fissioning Activity of Kurchatovium-259 by the Berkeley Group." Report measurement of SF branching of ^{259}Ku as produced in ^{246}Cm(^{18}O,5n) reaction using conveyer belt and glass detectors. Observed 31 SF events with $t_{1/2} = 3.2 \pm 0.8$ sec. They say this $t_{1/2}$ is in agreement with work reported by them at Dubna Heavy Ion Conference in 1971 (see JINR Report P7-5769, p. 125). Using cross section relationships they claim SF branching is ~7%. Also say that based on their branching, the chemical experiments of Zvara are well explained in terms of yield if he did experiments with ^{259}104. JINR Report E7-7023, March 23, 1973 (submitted to Atomnaya Energiya). Also in V.A. Druin, "Synthesis of Transuranium Elements and the Study of Their Radioactive and Chemical Properties" Finnish Summer School in Nuclear Physics, 1973. Report NP-19936

April 25, 1973 - C. E. Bemis, R. J. Silva, D. C. Hensley, O. L. Keller, Jr., J. R. Tarrant, L. D. Hunt, P. F. Dittner, R. L. Hahn and C. D. Goodman. "An X-Ray Identification of Element 104." Report conclusive identification of element 104 by observing Z = 102 (No) K x rays in coincidence with alpha particles from decay of ~4-5 sec ^{257}104. Work confirms original 1969 work of LBL for ^{257}104. Phys. Rev. Lett. 31, 647 (1973) (Sept. 3 issue)

May 1974 - P. F. Dittner, C. E. Bemis, Jr., R. L. Ferguson, D. C. Hensley, F. Plasil and F. Pleasonton. "Properties of $^{259}104$." Chem. Div. Ann. Prog. Rep., May 20, 1974; ORNL-4976, p. 39.

Measured SF branching decay of 3.0 ± 1.3 sec $^{259}104$ as 10 ± 7%.

Produced $^{259}104$ in $^{249}Cf(^{13}C,3n)$ reaction at 86.5 MeV.

June 1974 - G. N. Flerov

"Search for Superheavy Elements". Presented at "Third International Conference on Reactions Between Complex Nuclei", Nashville, Tenn.; to be published in Proceedings of Third International Conference on Reactions Between Complex Nuclei, Vol. II, North Holland, ∿1974-75.

Reports the production of ^{256}Ku ($t_{1/2}$ ∿ 5 msec) in ^{208}Pb (^{50}Ti,2n) reaction (spontaneous fission activity) and also production of $^{255}104$ ($t_{1/2}$ ∿ 4 sec S. Fission) in ^{207}Pb (^{50}Ti,2n). Report also that $^{255}104$ may be ∿50% alpha decay.

Did not see $^{254}104$, i.e., $t_{1/2}$ < 3 msec. All expts. done using rotating target and stationary dielectric track detectors.

Aug. 9, 1974 - Yu. Ts. Oganessian, A. S. Iljinov, A. G. Demin and S. P. Tretyakova "Experiments on the Production of Fermium Neutron-Deficient Isotopes and New Possibilities of Synthesizing Elements with Z > 100".

JINR-Report D7-8194 (submitted to Nuclear Physics).

Detailed reports on $^{254,255,256}104$ fission activities produced in $^{206,207,208}Pb$ with ^{50}Ti ions. Also reports on 106.

Aug. 26, 1974 - Yu. Ts. Oganessian, A. G. Demin, A. S. Iljinov, S. P. Tretyakova, A. A. Pleve, Yu. E. Penionzhkevich, M. P. Ivanov, and Yu. P. Tretyakov. JINR D7-8224.

"Experiments on the Synthesis of Neutron Deficient Kurchatovium Isotopes in Reactions Induced by ^{50}Ti Ions".

(submitted to Atomnaya Energie and Nuclear Physics).

Reports again on $^{255}104$ and $^{256}104$ in $^{207,208}Pb$ + ^{50}Ti.

ПАМЯТНАЯ ЗАПИСКА

Во время пребывания в Объединенном институте ядерных исследований (2I ноября I974 года) директора Физического Департамента Национальной Лаборатории в Окридже (США) проф.А.Цукера в Лаборатории ядерных реакций и дирекции Института состоялось обсуждение вопросов возможного сотрудничества в области синтеза и исследования тяжелых и сверхтяжелых элементов.

Эта встреча состоялась по инициативе американской стороны, и в ней от ОИЯИ приняли участие:

Вице-директор ОИЯИ - Ч.ШИМАНЕ

Ученый секретарь - Ю.А.ЩЕРБАКОВ

Директор Лаборатории ядерных реакций - Г.Н.ФЛЕРОВ

Зам.директора Лаборатории ядерных реакций - К-Г.КАУН

Начальник отдела исследования тяжелых ядер - Ю.Ц.ОГАНЕСЯН

Начальник Международного отдела ОИЯИ - В.С.ШВАНЕВ

Было отмечено, что на основе достижений, которые имеются в Национальной Лаборатории в Окридже и в Лаборатории ядерных реакций ОИЯИ возникла возможность проведения совместных экспериментов и получения качественно-новых результатов в области синтеза и свойств тяжелых ядер.

Принимая во внимание уникальные мишени, которыми располагает Лаборатория в Окридже и имеющиеся мощные пучки тяжелых ионов в Дубне, а также целый ряд прецизионных устройств, разработанных в этих двух лабораториях, придставители обеих сторон пришли к заключению, что постановка подобных экспериментов возможно уже в I976 году. Для осуществления этого сотрудничества представляется целесообразным в течение I975/6 года командировать по одному-двум физикам из Окриджа в Дубну и из Дубны в Окридж

for one month

для подготовки аппаратуры и участия в экспериментах по данной программе. Предполагается, что в 1976 году совместные эксперименты в Дубне займут около 6 мес. с участием 3-4 американских ученых, которые прибудут в Дубну с семьями на этот срок.

Совместные эксперименты предусматривают использование аппаратуры, разработанной в Окридже и Дубне, которая будет установлена на пучках тяжелых ионов циклотрона У-300. Детальная программа эксперимента и участие в совместных работах как дубненских физиков, так и физиков из Окриджа может быть определена в ближайшее время путем обмена письмами между дирекциями Национальной лаборатории в Окридже и ОИЯИ, содержащими конкретные предложения по физической программе.

Финансовые и организационные вопросы предлагается обсудить также путем обмена письмами.

Настоящая записка является началом дальнейших переговоров о сотрудничестве.

Обе стороны выражают надежду, что предполагаемое сотрудничество будет полезным и взаимовыгодным.

Protocol will be required

Talk in protocol about U.S. & Soviet Physicists (not Dubna)

UNIVERSITY OF CALIFORNIA, SANTA BARBARA

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO

SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF PHYSICS
SANTA BARBARA, CALIFORNIA 93106

12 April 1974

R

Professor V.F. Weisskopf
Department of Physics
Massachusetts Institute of Technology
Cambridge, Mass. 02139

Dear Vicki:

I understand you are becoming coordinator of this messy business of collaborations with the Soviets. After two and a half years of discussions we finally had a breakthrough, which is spelled out in the enclosed letter from Cherenkov. I have now spent a couple of weeks trying to get NAL and the AEC to talk to each other about how to proceed, and my enclosed letter explains the results.

I will keep you informed, and I do hope your presence on the scene will make this all much simpler!

Best wishes,

Dave

David O. Caldwell

DOC:jjf
enclosures

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312-840-3000
DIRECTORS OFFICE

Russia

September 27, 1974

Mr. Donald L. Bray, Area Manager
Batavia Area Office
U.S. Atomic Energy Commission
P.O. Box 2000
Batavia, Illinois 60510

Dear Mr. Bray:

In response to your request of August 23 for additional information concerning the proposed US-USSR Collaboration in Theoretical Physics the following information is submitted.

We would hope that the entire group would arrive at the Fermilab in September, 1975 and would stay for a period of six months to one year. During the period of their stay B. W. Lee and H. D. I. Abarbanel will act as the official Laboratory hosts. The full names, approximate age, and area of theoretical physics to be worked on are as follows:

Name	Approx. Age	Area of Theoretical Physics
Eugene M. Levin	34	High Energy Diffraction Scattering and Phenomenology of Production Processes
Lev N. Lipatov	34	High Energy Behavior of Gauge Field Theories and Properties of Renormalizable Field Theory
Aloysha B. Kaidalov	34	Phenomenology of High Energy Hadron Reactions
Lev B. Okun	44	Theory of Weak Interactions
Alexander A. Migdal	29	Invariance and Renormalization in Quantum Field Theory
Alexander M. Polyakov	29	Renormalization Group Applications and Confinement of Quarks in Classical and Quantum Field Theories

We hope the above information will expedite your approval for this collaboration.

Sincerely,

Donald R. Getz

Donald R. Getz

cc: H. Kinney

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 840-3000

Russia

September 9, 1974

Dr. A. M. Petrosyants
Chairman, USSR State Committee
on Utilization of Atomic Energy
Moscow, USSR

Dear Dr. Petrosyants:

In a series of conversations during the past year, Professor Darrell Drickey of the University of California at Los Angeles has been discussing with Dr. Edouord N. Tsyganov the possibility of a collaboration between Dr. Tsyganov and his colleagues on the one hand and Dr. Drickey and his colleagues on the other hand for the performance of a pion-electron scattering experiment designed as an extension of their previous work at Serpukhov. The experiment would be carried out at the Fermi National Accelerator Laboratory, and the purpose would be to measure the pion form factor by probing it at higher energies than have been used before.

The specialists whose names have been discussed are: Woitek Gajewsky, a Polish citizen; Ioan X. Ioan, a Rumanian citizen; Anotole A. Kuznetsov, a citizen of the USSR, and Edouord N. Tsyganov, another citizen of the USSR. I believe that Dr. Drickey has already written directly to them.

I would now like to ask your help in arranging the visit of these four scientists to the Fermi Laboratory for a period of about a year, starting in December 1974. Such an arrangement could be similar to those that have been applied to other collaborations with specialists from Dubna. Their wives and children would be invited to accompany them. Salaries and travel expenses would be paid by their home laboratory. Housing would be provided, at no charge, by Fermilab as would emergency medical care. Other living expenses in the United States would be paid by the visitors.

All four of these men have previously worked, as experimenters, in the United States. Each brings valuable experience to the experiment which is the next natural step in the exploration of the pion form factor.

I am most enthusiastic about the prospects for this new collaboration. I hope that you will join me in encouraging this new phase of the experiment. It is a natural sequel to

bcc: D. Drickey
W. Wallenmeyer

the previous collaborative experiment for which the Drickey group went to Serpukhov for a year and could even be considered a continuation of that work. I therefore trust that plans can be made for these specialists to arrive in the United States before the experiment actually gets underway. I look forward to hearing from you in the near future.

Sincerely,

R. R. Wilson

cc: N. N. Bogolubov, Dubna
D. L. Bray

FERMI
NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 840-3000

August 16, 1974

Professor Victor Weisskopf
Massachusetts Inst. Tech.
Department of Physics
Cambridge, Massachusetts 02139

Dear Viki,

I returned last week from my three and a half weeks in the Soviet Union. My trip was interesting in that I saw many places and visited many laboratories that I had not previously seen. I visited several of the Laboratories of the Lebedev Institute in Moscow, I also visited Dubna and Serpukhov and the Leningrad Institute of Nuclear Physics at Gatchina. Finally I visited Budker's Institute in Novosibirsk.

Relative to my last visit to the Soviet Union ten years ago, at the external level the changes that I observed in the Soviet Union to me were less dramatic than the changes I observed, for example, in Geneva during that same period. Nevertheless, of course, I did detect the level of economic change, the obvious growth in cities like Moscow and also obvious improvements in the level of their technology especially in regards to their computers. I also detected only very slightly the degeneration in terms of freedom of movement of the Soviet physicists themselves. But as you had indicated to me, in London, that is an area that you probably are in a much better position to observe than I am.

I did ask, as we had discussed I might do, a number of Soviet physicists their feelings as to the most appropriate way for western scientists to affect their freedom of motion. I unfortunately found and was able to discuss this question in an appropriate manner primarily with physicists in good standing in the Soviet government. I did not get a chance to discuss this matter with any of the obvious dissidents, and in only one or two cases was I able to discuss the question with people who were clearly themselves restricted more today than they were a few years ago. In every case, the essence of the response I got was that we should not inhibit our trips to the Soviet Union as a mechanism of applying pressure.

The general feeling is that any such pressure would certainly play into the hands of the right-wing elements in the government and would have just the opposite effect to that which we might think we could accomplish by such procedures.

I might add that I did try to see one of the dissidents, Alexander Voronel,while I was in Moscow. I had met his son during the last day of the London Conference and at the son's request had carried in a translation from Russian to English of an article that Voronel had submitted to the Reviews of Modern Physics. When I was in Moscow, I did go to visit his apartment on two different occasions during one afternoon and failed to find Voronel in his apartment. I did leave the manuscript outside the apartment and hope that he received it. I was disappointed not to have been able to meet the man.

Sorry that I have nothing more positive to report on the matter of easing scientific intercourse between the eastern European countries and ourselves.

Here at Fermilab things seem to be continuing to improve, although it's clear the budgetary problem which we are beginning to see seriously now will start complicating matters to some extent. By and large though, I am pleased with the general evolution of the laboratory.

Sincerely yours,

A. E. Brenner

AEB:cp

CC: Victor Weisskopf-CERN Office

January 28, 1975

J. Ballam, Chairman
Organizing Committee
1975 International Symposium
on Lepton and Photon Interactions
Stanford Linear Accelerator Center
P.O. Box 4349
Stanford, California 94305

Dear Joe:

Thank you for sending me your correspondence with Bogolubov, Chuvilo and Markov. I am not surprised by the reaction you received. It is all along the lines of previous experience. Dubna acts independently of the rest and they act relatively quick. The Soviet authorities do not act at all. We had the same experience in regard to the New Orleans Conference except that the Soviets finally did send us a list after all.

Please keep me informed about future developments.

With best regards,

Sincerely yours,

V. F. Weisskopf

VFW:dle

Sponsored jointly by the International Union of Pure and Applied Physics, the U.S. Atomic Energy Commission, the National Science Foundation, and the Stanford Linear Accelerator Center

1975 International Symposium on Lepton and Photon Interactions at High Energies

August 21-27, 1975

Stanford University

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M. L. Perl
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Jan. 2, 1975

Professor V. F. Weiskopf
Department of Physics
Massachusetts Institute of Technology
Cambridge, Mass. 02139

Dear Viki:

Enclosed is a response from Bogolubov to my request that he, Chuvilo and Markov form a committee to select the Soviet delegates to the 1975 Lepton-Photon Symposium.

As you can see, he does not seem willing to be a part of any committee, but rather considers himself to be the head of an International Laboratory, somewhat independent from the rest of Soviet high energy physics. However, I consider it a good sign that he did respond, almost within the requested time, and did name some delegates.

I still have not heard from Chuvilo or Markov.

We have decided to give the Soviets a quota of 25--down somewhat from their previous quotas. This was based on their attendance at the Cornell and Bonn Symposia, which was 10 and 6, respectively. If we would augment this by the fractional increase in total delegates for this Symposium over the other two, this would come to 12 and 8. Thus the number 25 is at least a factor two times their previous attendance.

Regards,

J. Ballam, Chairman
Organizing Committee

JB:hm

cc: W.K.H. Panofsky

Please address all correspondence to: Symposium Secretary
Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, California 94305

SLAC Telephone
(415) 854-3300

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON, D.C. 20545

JAN 15 1975

A.S. Friedman, Director, Division of International Programs

VISIT TO FERMILAB BY L.N. SHTARKOV, USSR NATIONAL

The Division of Physical Research recommends approval of the proposed invitation to Dr. Lollii N. Shtarkov to visit FERMILAB during early 1975 to participate in Experiment No. 177 on large angle proton-proton elastic scattering.

This collaborative high energy physics experiment will be carried out under the terms of the US-USSR Agreement, and the participation of Dr. Shtarkov will enhance the program of collaboration and also contribute to the potential success of the experiment.

D. R. Miller

D.R. Miller, Acting Director
Division of Physical Research

Sheinshopf

A. S. Friedman, Dir., DIP
ATTN: Bill Hill, Chief
East-West Affairs Branch

FERMILAB EXPERIMENT NO. 177 - PROPOSED INCLUSION OF DR. LOLLII N. SHTARKOV

This is in reference to conversations you previously had with our office on December 20 and 23, 1974 concerning the above subject. We have enclosed for your review a request to add one more collaborator, Dr. Lollii N. Shtarkov, from the USSR to Experiment No. 177 at Fermilab.

Donald L. Bray
Area Manager

BAO:MHB

Enclosure:
As stated

cc: CH Security, w/encl

bcc: Loren Adams, w/encl
Roy Lang, w/encl

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

Directors Office

January 2, 1975

Mr. Donald L. Bray, Area Manager
Batavia Area Office
U. S. Atomic Energy Commission
P. O. Box 2000
Batavia, Illinois 60510

Dear Mr. Bray:

Through recent correspondence from Professor Jay Orear, of Cornell University, we have been informed that he would like to add one more collaborator from the USSR to the two who were previously approved to participate in his experiment at Fermilab. The two approved collaborators are Pavel S. Baranov, and Sergey W. Rusakov. The Third physicist is Dr. Lollii N. Shtarkov. I am enclosing a brief resume of Dr. Shtarkov's background. I hope we can receive an early approval for this additional participant in the experiment in question.

Sincerely,

Edwin L. Goldwasser

Edwin L. Goldwasser

Enclosure

bcc: W. Wallenmeyer ✓

Dr. LOLLII N. SHTARKOV

A BRIEF PERSONAL HISTORY

Lollii N. Shtarkov is a physisist-experimentator experienced in the field of elementary particle and high energy physics.

He has worked on photodesintegration of Deuteron, elastic scattering of photons on protons, radiative decays of vector mesons, production of eta-meson and so on.

The research work of him has been connected with electronic counter methods and espesially with use of computers for data processing.

Education: Graduated from the Moscow University in 1950.
Specialyty: Physics.
A Doctor (A Candidat in the USSR) of Science: since 1961.
Office: Lebedev Physical Institute, Moscow, USSR.
Positinn: A senior research worker, On staff.
Number of publications: 32.

Full address: Dr. Lollii N. Shtarkov,
Lebedev Physical Institute
Photomesonic Laboratory,
Leninsky Prospect 53,
MOSCOW,
USSR.

Dr. LOLLII N. SHTARKOV

A LIST OF THE MAIN PUBLICATIONS.

The full list of publications includes 32 titles. Here are referenced only the main publications. For the Russian publications references include the English transcription of the magazin's Russian name and a short translation of the title. In all cases only the first author is referenced.

1. Photodesintegration of Deuteron at 50 - 150 Mev,
   Aleksandrov et all, JETF 33, 614, 1957.
2. Photodesintegration of Deuteron at Intermediate Energies,
   Shtarkov, Thesis for Doctor Degree, 1961.
3. Elastic Scattering of Photons on Hydrogen at 247 Mev,
   Baranov et all JETF 41, 1713, 1961.
4. Photodesintegration of Deuteron,
   Shtarkov, Proceedings of FIAN XXII, 155,1964
5. Photon elastic Scattering on Protons
   Baranov et all, Jadernaja Phisika 3, 1083, 1966.
6. Data Analysis for Photon Elastic Scattering on Protons,
   Baranov et all Jadernaja Phisika 5, 1221, 1967.
7. Observations of Omega Decays into e^+e^-,
   Azimov et all, Proc. of XIII Int. Conf. on H.E.Physics 313, 1966.
8. Decays of Rho- and Omega- Mesons into e^+e^-,
   Khatchaturian et all, Phys. Lett. 24B, 349, 1967.
9. E^+E^- - Decays of Vector Mesons,
   Azimov et all, Jadernaja Physika 6, 515, 1967.
10. A Search for Decays of X^o into Two Gammas,
    Azimov et all, Proc. of XIV Int. Conf. on H.E.Physics, N 772, 1968.
11. An Observation of Decays of Phi-Meson into E^+E^-,
    Astvatsaturov et all, Phys. Lett. 27B, 45, 1968.
12. Eta-Meson Production in Pi-P Collisions at 4 Gev/c,
    Hladky et all, Phys. Lett. 31B, 475, 1970.
13. Eta-Meson Production in Pi-P Collisions at 7.2 Gev/c,
    Adamovitch et all, FIAN Correspondenses on Physics, N 5, 1972.
14. New Experimental Data on Proton Polarizabilyties,
    Baranov et all, FIAN Preprint N 97, 1974.

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

Directors Office

January 2, 1975

Professor David Caldwell
Physics Department
University of California
Santa Barbara, California 93106

Dear Dave:

I find that in our correspondence, there is no written record of some of the arrangements which we expect to follow in connection with the Fermilab visits of the Russian physicists who will collaborate on your experiment. The exchange arrangement requires that housing for the Russians be provided free of charge. It is our intention, when possible, to provide that housing on-site at Fermilab. In the case of your collaborators, arrangements for such housing have already been made. However, it is our policy that the cost of housing for visiting Users shall be borne by the Users. It therefore is also our policy that when visiting Users bring Russian collaborators, the cost of the Russians' housing will be paid by the U. S. User Group.

I believe that we have discussed this matter in telephone conversations, but I thought it was important to get it in writing so that there could be no possible misunderstanding as we proceed toward the implementation of the collaborative arrangements.

Sincerely,

Ned

Edwin L. Goldwasser

bcc: W. Wallenmeyer ✓

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

Directors Office

January 2, 1975

Professor J. Orear
Cornell University
Laboratory of Nuclear Studies
Ithaca, New York 14850

Dear Jay:

I have received your letter of December 24th requesting permission for Dr. Lollii N. Shtarkov to join your other two Soviet collaborators as a participant in your Fermilab experiment. I note that Dale Corson, presumably at your request, opened the question of a third collaborator in the experiment as long ago as February 1, 1974. It is indeed unfortunate that you did not obtain the required approval from us before, rather than after you sought this additional participant.

I have today requested AEC approval for the participation of Dr. Shtarkov. I shall let you know as soon as we receive word of a decision. In the meantime, I would like to take this occasion to be sure that there is a clear understanding of certain of the conditions under which this participation will be managed.

The general arrangement for Russian participation requires that housing, in the vicinity of Fermilab, be provided free to the Russian visitors. Without exception, to this date, we have been able to provide on-site housing for these visitors, and it will be our intention to continue to provide such housing as long as that remains the Russians' preference and as long as the housing can be made available.

In your case, the late addition of a third Russian collaborator may place an extra burden on our housing facilities. That depends, to some extent, on the housing requirements of the particular individuals who are coming to work with you. We must know all of the details of their needs as soon as possible. In addition, it is not our policy to provide free housing for visiting Users. When an explicit USSR-Fermilab collaboration is negotiated, we, as the home users, provide the free housing for the Russian visitors. When a university group, or a group from some other laboratory fills the role

bcc: W. Wallenmeyer ✓

Professor J. Orear
Cornell University

January 2, 1975

of U. S. collaborator with Russian visitors, it is our policy to bill that U. S. group for the cost of the housing occupied by the Russians. I know that we have discussed this previously. I am placing it in writing here, for the record, so that there may be no misunderstandings.

Sincerely,

Ned

Edwin L. Goldwasser

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON, D.C. 20545

JUN 14 1974

Professor I. V. Chuvilo, Director
Institute of Theoretical and
Experimental Physics
Cheremushkinskaya ulitsa, 89
Moscow, USSR

Dear Professor Chuvilo:

I would like to take this opportunity to communicate with you regarding the request from Academician Markov for Professor V. A. Lobashev to visit the Anderson Meson Physics Facility (LAMPF) at Los Alamos for one month.

We consider that Professor Lobashev's stay at LAMPF should be carried out within the framework of our Atomic Energy Agreement of June 21, 1973. The Director of LAMPF, Dr. Louis Rosen, has already communicated to Academician Markov his personal satisfaction at the prospect of Dr. Lobashev's extended visit at LAMPF. Recognizing that LAMPF is a U.S. installation unique in the world in the field of medium energy science, that a visit by Dr. Lobashev would be of considerable value to Soviet scientists concerned with building a similar facility in the USSR, we propose that, as reciprocity for Dr. Lobashev's stay at LAMPF, Dr. Robert Penneman of the Los Alamos Scientific Laboratory visit the Scientific Research Institute of Atomic Reactors at Dimitrovgrad for an extended period, primarily for the purpose of studying transuranium element production and separation and for participating in research studies at the Institute. Dr. Penneman is well-qualified in the field of heavy element chemistry and we consider that his visit at Dimitrovgrad working with Soviet scientists would be of mutual benefit in this very important field of research.

I would appreciate hearing from you regarding this matter as soon as possible in view of the fact that Professor Lobashev wishes to come to the Meson Physics Facility at Los Alamos in the very near future.

Sincerely,

/s/ J. M. Teem

J. M. Teem

cc: Academician M. A. Markov,
Academy of Sciences of the USSR
Professor V. F. Weisskopf, MIT
Professor K. Strauch, Harvard Univ.

February 24, 1975

Professor Henry D.I. Abarbanel
Department of Physics
Lauritsen Laboratory of High Energy Physics
California Institute of Technology
Pasadena, California 91109

Dear Henry:

Thanks for sending me a copy of your letter to David Pines. I agree very much with your proposal of financing short-term visits for theorists with the Soviet Union. I heartily support this proposal.

The only objection I would have to your letter is the leaving out of senior scientists like Gribov, Joffe, Okun and Ter-Martirysyan. It would be very wrong in my opinion to give in to the fact that they haven't come in the past. In fact, that is what the Russians want us to do, but I think it will be very much counter-productive. The leaving out of the American "old guard" is done for good reasons, however I deplore the underlying facts.

With best regards,

Sincerely yours,

V. F. Weisskopf

VFW:dle

cc: D. Pines

CALIFORNIA INSTITUTE OF TECHNOLOGY

CHARLES C. LAURITSEN LABORATORY OF HIGH ENERGY PHYSICS
PASADENA, CALIFORNIA 91109

February 18, 1975

Viki:
David asked me to send a copy of this to you.
Cheers,
Henry

Professor D. Pines
Department of Physics
University of Illinois
Urbana, Illinois 61801

Dear David:

It was a pleasure indeed to speak to you last Monday about US-USSR exchange programs for theoretical physicists. As you requested, I am writing to recall my ancient and mostly deceased plan for sending approximately ten US physicists to the USSR on short visits and to provide you with the names of several American and Soviet theorists in high energy physics who would make exchanges very worthwhile.

First, my proposal of Fall, 1972 to the Office of Science and Technology asked for $25,000 to finance approximately ten short term visits by American particle theorists to prominent Soviet laboratories. Short term means three to four weeks. I argued then and still believe that even in this brief encounter the nature of theoretical physics allows very useful interactions to occur. The 25K was to be spent over two years or so, and I hoped that the OST or the National Academy would administer the project. The total funds were calculated on the basis of travel, basic living expenses in the USSR, and some salary support. I have not endeavored to up date those figures, but if they were now low by 10-20%, I wouldn't be surprised. My experience in Moscow and Leningrad last summer has made me even more enthusiastic about the high scientific value of this small-time (financially) kind of operation. The mutual rewards to the physicists on both sides are remarkably bountiful.

Second, you requested the names of American theorists who might be interested in visiting the USSR on this basis and Soviets who might be able to come here and be very welcome. Here they are

R. L. Sugar, UC Santa Barbara
J. L. Rosner, U. Minnesota
H. D. I. Abarbanel, FNAL
C. de Tar, MIT
R. Dashen, Institute for Advanced Study
F. Gilman, SLAC
M. Baker, U. of Washington
J. Bronzan, Rutgers University
M. Einhorn, FNAL

Cont'd..

and Soviets

E. M. Levin, Leningrad
L. N. Lipatov, Leningrad
A. A. Anselm, Leningrad
M. Dyakanov, Leningrad
A. B. Kaidalov, ITEP, Moscow
V. Zacharov, ITEP, Moscow
A. A. Migdal, Landau Institute
A. M. Polyakov, Landau Institute
O. V. Kancheli, Tblisi
V. A. Abramovskii, Tblisi.

You will note that I left off the "senior" people: Gribov, Joffe, Okun and Ter-Martirysyan since, probably, they cannot come. Similarly I left off the American old guard since they have never on their own or in response to my urging shown an interest in going.

I hope this will be helpful. If I may be of more assistance in general or in nitty gritty, I'd be delighted.

The best to you and Suzy.

Sincerely,

Henry

Henry D. I. Abarbanel

HDIA/ams

Examples of Requests for
FERMILAB Staff Members
to Visit Laboratories in the USSR

Dr. Ernest Malamud has already made one visit to colleagues in Dubna in connection with analysis of data from their initial hydrogen gas jet experiment and is being asked to return there in connection with preparation for their next proposed experiment, using helium. In addition, the Russians in that collaboration have asked that two or three of our physicists, engineers and/or technicians should spend several months at Dubna, working with the Russians on the development of a helium gas jet target.

On a somewhat parallel plane, Dr. Frank Nezrick and other coworkers in the approved collaboration with Serpukhov and ITEP for an experiment involving a neutrino bombardment of our 15 foot bubble chamber, have been asked by their Soviet colleagues to make regular visits to their laboratories. Such visits are technically desirable, because the Russian scientists are preparing and will be carrying out film scanning, measuring and analysis procedures which must be compatible with ones that will be in use in this country. The people who will be working on those procedures in the Soviet Union will be many more, in number, than the few who will be spending some time at the Fermilab. Three Soviet physicists have already been in this country, two of them for a period of a year. They strongly feel that visits are now required by some of our people to their laboratories.

Of a somewhat different category, Dr. H.D.I. Abarbanel, a bright young theorist on our staff, works in an area which is closely related to the one in which Dr. V. Gribov of Ioffe Physical-Technical Institute has been playing a leading role. Dr. Gribov has apparently been denied access to the United States on several occasions in the past. In a somewhat unprecedented gesture, he recently wrote to Abarbanel inviting him to pay a visit to his institute. Abarbanel is eager to pick up that unusual overture.

Of a still different kind is an invitation received by Dr. Alfred Brenner, head of computer activities at the Fermilab. Some time ago he was host, for several months, to a Dr. Y. V. Stupin, who came to the United States under the auspices of the two National Academies of Science. Dr. Brenner has now received an invitation from Dr. Stupin to return his visit and to come to Moscow. Brenner is eager to make that trip and to visit other laboratories in the Soviet Union.

Finally, for several years, now, both Dr. Wilson and I have been receiving enthusiastic invitations to come to the Soviet Union. Of course they would like to have us report, in a general way, on the status and plans for the Fermi National Accelerator laboratory, but I also believe that they are eager to demonstrate their good will by extending their warm hospitality to us, in reciprocation for ours, in having them here at Fermilab. For a number of reasons, both Bob and I have been somewhat reluctant to accept these invitations. So far, the availability or unavailability of financial support has not been the key factor in our decisions to go or not to go. However it could very well become an overriding factor, were either of us to decide that the time was ripe for a visit.

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 840-3211
DIRECTORS OFFICE

January 29, 1974

Dr. A. Friedman
Division of International Programs
U.S. Atomic Energy Commission
Washington, D.C. 20545

Dear ~~Dr. Friedman:~~ Abe

We have found one recurring problem associated with the USA-USSR collaborations that have been arranged for the performance of high energy physics experiments at NAL.

In the Protocol, the "sending country" is uniformly responsible for the support of its scientists traveling to the "receiving country". By mutual agreement in subsequent Annexes, an exception has been made to those general provisions so that we have provided free housing and emergency medical care for Soviet scientists working at our Laboratory. That arrangement is quite satisfactory, and, in fact, is more logical and easier to implement than the arrangement that was originally proposed in the Protocol. However, it does cost us real dollars.

Those dollars are not a serious concern to us, but there is another expense that is. That one is the cost of travel when scientists on the U.S. side of the collaboration are required to visit their colleagues in the USSR for the purposes of planning an experiment or the analysis of the results. As things now stand, such travel must be paid by us and therefore accommodated within our Laboratory's foreign travel budget. But that budget is already inadequate to support the other normal foreign travel requirements of the Laboratory. It cannot possibly accommodate the "reverse travel" which is often deemed necessary by both sides of these collaborations.

We would like to suggest that in new negotiations some new definitions might be made. The laboratory at which any given experiment is performed could be called the "host laboratory" for that experiment. The laboratory which is sending scientists to collaborate in experiments might be identified as the "guest laboratory" for that experiment. Then, recognizing the fact that in the course of such an experiment the host laboratory incurs many costs which are not balanced by the presently defined fiscal responsibilities of the guest laboratory, it might be arranged that whenever it is mutually agreed

that visits by scientists from the host country, to laboratories in the guest country, are required for the conduct of the experiment, expenses of those visits (overseas travel and living costs) will be born by the guest country.

Do you feel that such an arrangement might be negotiated in the future? Do you feel that it might be negotiated, retroactively, in connection with Annex II? For that experiment, the USSR participants apparently want our physicists to visit their laboratories for consultation regarding the development and implementation of analysis procedures.

I look forward to learning your opinion about these suggestions.

Sincerely,

Ned

Edwin L. Goldwasser

cc: D. L. Bray

File "Russia"

NATIONAL ACCELERATOR LABORATORY

P.O. BOX 500
BATAVIA, ILLINOIS 60510
TELEPHONE 312 840-3211
DIRECTORS OFFICE

This is a John Teem - Friedman worry ——— should come up in the US-USSR Joint Coordinating Committee Mtg scheduled in late fall.

June 3, 1974

Dr. John M. Teem
Director
Division of Physical Research
U.S. Atomic Energy Commission
Washington, D.C. 20545

Dear John:

I am writing to express a serious concern regarding the implementation of the Nixon-Brezhnev Agreement, insofar as it pertains to high energy physics activities. If the Agreement is intended to stimulate an increasing rate of exchange of people between the USA and the USSR, incremental foreign travel support will be required. Yet I see no evidence that such support is materializing. I am particularly interested in this problem as it pertains to collaborations at this Laboratory.

When physicists from the Soviet Union collaborate with U.S. physicists on experiments at FERMILAB, it is frequently advantageous, either in the preparatory stages of the experiment or in the analysis stage, for one or more of the U.S. physicists to travel to the Soviet Union in order to assure a certain degree of coherence in the preparation of equipment and in analysis procedures. The desirability of such interchanges has given rise to a number of requests for FERMILAB staff members to visit laboratories in the Soviet Union. Examples of such visits are listed on the enclosure.

Given the highly restricted travel budgets under which we all operate these days, it is next to impossible to contemplate the number of visits that would be entailed in the above list. In fact, that amount of travel to the Soviet Union would have more than consumed our entire foreign travel budget for FY 74!

I can see two possible approaches to a solution to this problem. The first one would simply be to provide the funds which are necessary for the travel which will be inherent in any successful implementation of the Nixon-Brezhnev Agreement. This could be accomplished, for example, by developing a new source of foreign travel funds for this specific purpose, or it could be done by ruling that all such travel would be exempted from the normal foreign travel budget restrictions.

An entirely different approach has occurred to me and was suggested in my letter of January 29, 1974 to Abe Friedman. I

realize that the proposed procedure represents a departure from the present practice under which the sending country pays for transportation of its people and, in the normal case, their expenses while abroad. In that letter, a copy of which is enclosed, I suggested that we might make an attempt to recognize, with our Soviet colleagues, the real-life asymmetry of the present situation. A unique research tool is located at the FERMILAB, and it is that tool which constitutes a special interest to scientists in the Soviet Union. While working at this Laboratory, they do receive a substantial number of considerations and services, all of which we like to give, but all of which nonetheless represent a significant burden on our people and on our funds. The cost of their housing, which we provide free of charge, is but the tip of the iceberg. Much more than that is involved.

It therefore has seemed to me that both sides might well take cognizance of that fact, and when, as a part of the exchange, visits by U.S. collaborators to the Soviet Union are mutually deemed necessary, the Russians might graciously make the gesture not only of offering to pay all expenses within the Soviet Union, but also of providing a roundtrip air ticket, presumably on Aeroflot.

I have suggested such a possibility, informally, in conversations with some of our Russian visitors. They appear to understand the suggestion and to see its merits. If you feel that it is a bad idea, I shall stop suggesting it, even informally. However if you think it worth pursuing, I might become a little more aggressive in making the suggestion. On the other hand, you might prefer to move toward another solution of a funding of foreign travel.

I shall appreciate any comments or suggestions that you might care to offer.

Sincerely,

Ed

Edwin L. Goldwasser

cc: V. Weisskopf
A. Friedman

UNIVERSITY OF CALIFORNIA, LOS ANGELES

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DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

R

April 18, 1974

Professor V. F. Weisskopf
Massachusetts Institute of
Technology
Department of Physics
Cambridge, Massachusetts 02139

Dear Vicki:

I received the enclosed telex on April 11 and regard it as the first response to the matter we took up last fall that culminated in Larson's letter to Petrosyants. We at UCLA are delighted that Tsyganov is finally coming here and hope very much that this will lead to final resolution of the pi-e experiment.

Best regards,

Darrell

Darrell Drickey,
Associate Professor of
Physics

DD:jt
Enclosure

TO K LANIUS, VICE DIRECTOR
JINR

FROM DARRELL DRICKEY

APRIL 16, 1974

I AM DELIGHTED TO OFFICIALLY INVITE DR D E N TSYGANOV AND DR M F LIHACHEV TO VISIT UCLA FOR THE PERIOD OF MAY 7, 1974 TO JUNE 5, 1974 AND HAVE TAKEN STEPS TO INSURE ISSUANCE OF THEIR VISAS AT THE U S EMBASSY IN MOSCOW.

WE ARE EAGERLY LOOKING FORWARD TO THEIR VISIT.

SINCERELY
PROFESSOR D DRICKEY

Friedl

NON-NEGOTIABLE RECEIPT

HARVARD TRUST COMPANY
HARVARD SQUARE
CAMBRIDGE, MASSACHUSETTS

FOREIGN REMITTANCE

Date April 12, 1974

198

Received from Physics Emigrant Fund account

the sum of One Hundred Fifty Three and26/100 --- U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of One Hundred Fifty Dollars U.S.
AMOUNT IN WORDS

to be sent by ☐ regular mail ☒ air mail ☐ cable

to Bank for Foreign Trade of the U.S.S.R.
Moscow, for payment to:
PAYEE'S NAME
Eugene Morisovich Yakir Physicist; Wife: Rima
STREET AND NUMBER
Profsoyusnaya Street 100
Building 5, Apartment 35
CITY OR TOWN
PROVINCE OR DISTRICT
Moscow, USSR
COUNTRY

Message (if any)

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		150.00	3.00		.26	153.26

This transaction is subject to the terms and conditions stated on the reverse hereof.

By M. Izard
AUTHORIZED SIGNATURE

D1035

Non-Negotiable Receipt

Harvard Trust Company
Harvard Square
Cambridge, Massachusetts

Foreign Remittance

Morse

137

Date April 23, 1974

Received from Dr. David Frisch
(account)

the sum of --- One hundred fifty-three and 26/100 U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of --- One hundred fifty Dollars U.S. ------
AMOUNT IN WORDS

to be sent by ☐ regular mail ☒ air mail ☐ cable

to DR. BRONISLAV LYNER Metallurgist; Wife: Irene
MOSCOW 117342
PAYEE'S NAME
PROFSOYUZNAYA 87, Corp. 4, KV. 26 2 small children
STREET AND NUMBER
U.S.S.R.
CITY OR TOWN
(Pay in Rouble Certificates) (Please obtain receipt
PROVINCE OR DISTRICT
COUNTRY

Message (if any)

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		$150.00	$3.00	.26	---	$153.26

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By [signature]
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Levinson

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HARVARD SQUARE
CAMBRIDGE, MASSACHUSETTS

FOREIGN REMITTANCE

Date May 16, 1974

215

Received from David Frisch (Acct.)

the sum of ----------One Hundred fifty-three and 26/100. Dollars
AMOUNT IN WORDS

to cover a remittance of --------One hundred fifty Dollars U.S.----------
AMOUNT IN WORDS

to be sent by ☐ regular mail x☐ air mail ☐ cable

to Dimitry Ram (spelled Romm Yoo) wife: Bella Palatnils
PAYEE'S NAME
U.S.S.R.
Moscow 117 437 2 children
STREET AND NUMBER CITY OR TOWN
Miklukho-Maklaya STR. 30
Korpus 5, Apt. 242
PROVINCE OR DISTRICT COUNTRY

(Pay in Rouble Certificates)

Message (if any) "PLEASE OBTAIN RECEIPT"

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		150.00	3.00	.26		$153.26

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By C. Chin
AUTHORIZED SIGNATURE

D1035

Frisch

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HARVARD TRUST COMPANY
HARVARD SQUARE
CAMBRIDGE, MASSACHUSETTS

FOREIGN REMITTANCE

Date April 24, 1974

209

Received from Dr. David Frisch
(account)

the sum of -- One hundred fifty-three and 26/100 U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of ---- One hundred fifty Dollars U.S. --------
AMOUNT IN WORDS

to be sent by ☐ regular mail xxx☒ air mail ☐ cable

to Dr. Vladimir Oliker Math. Physics; Wife: Elena
Leningrad U.S.S.R
PAYEE'S NAME
Baseinaya 105, Corp. 1, Apt. 158 Small Daughter
STREET AND NUMBER CITY OR TOWN
(Pay in Rouble Certificates)

PROVINCE OR DISTRICT PLEASE OBTAIN RECEIPT COUNTRY

Message (if any)

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		$150.00	$3.00	.26	--	$153.26

This transaction is subject to the terms and conditions stated on the reverse hereof.

By [signature]
AUTHORIZED SIGNATURE

D1035

Frisch

NON-NEGOTIABLE RECEIPT

HARVARD TRUST COMPANY
HARVARD SQUARE
CAMBRIDGE, MASSACHUSETTS

FOREIGN REMITTANCE

208

Date April 24, 1974

Received from Dr. David Frisch (account)

the sum of ---One hundred fifty-three and 26/100 U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of ----- One hundred fifty Dollars U.S. ------
AMOUNT IN WORDS

to be sent by ☐ regular mail ☒ air mail ☐ cable

to Dr. Boris Rubenstein Wife: Natasha, philologist
Leningrad U.S.S.R PAYEE'S NAME
Gavrskaya 11, Apt. 81 2 children
STREET AND NUMBER CITY OR TOWN

(Pay in Rouble Certificates)

PROVINCE OR DISTRICT COUNTRY

PLEASE OBTAIN RECEIPTS

Message (if any)

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		$150.00	$3.00	.26	--	$153.26

This transaction is subject to the terms and conditions stated on the reverse hereof.

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By Samuel Burbage
AUTHORIZED SIGNATURE

Non-Negotiable
Receipt

Harvard Trust Company
Harvard Square
Cambridge, Massachusetts

Morse

Foreign
Remittance

Date May 16, 1974

216

Received from David Frisch
(Acct)

the sum of --------One Hundred fifty-three and 26/100 U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of --------One Hundred fifty Dollars U.S.---------
AMOUNT IN WORDS

to be sent by ☐ regular mail ☒ air mail ☐ cable

to Samuel Pesakhovich Gegsberg
U.S.S.R.
PAYEE'S NAME

Leningrad ~~XXXXXXX~~ K 17 Prospect Teresa Marissa 102
STREET AND NUMBER CITY OR TOWN

~~XXXXXXXXXXXX~~
~~XX~~ Korpus 1, Apt. 32 (?. ↑ said to be name of French revolu-
PROVINCE OR DISTRICT COUNTRY

(Pay in Rouble certificates)

Message (if any) tionary leader)

"PLEASE OBTAIN RECEIPT"

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		150.00	3.00	.26		$153.26

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By _____
AUTHORIZED SIGNATURE

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Demo

NON-NEGOTIABLE RECEIPT

HARVARD TRUST COMPANY
HARVARD SQUARE
CAMBRIDGE, MASSACHUSETTS

FOREIGN REMITTANCE

Date ~~May~~ June 3, 1974

247

Received from David Frisch Acct.

the sum of ------One hundred fifty-three and 26/100------ U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of ------One hundred fifty and 00/100 U.S. Dollars------
AMOUNT IN WORDS

to be sent by ☐ regular mail ☒ air mail ☐ cable

to Dr. Zigun Lev
U.S.S.R.
PAYEE'S NAME
Leningrad,
CITY OR TOWN
STREET AND NUMBER
Prospekt Metallistov
Dom. 18, KV.9
PROVINCE OR DISTRICT (Pay in Rouble Certificates) COUNTRY

Message (if any)

PLEASE OBTAIN RECEIPT

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		150.00	3.00	.26		$153.26

This transaction is subject to the terms and conditions stated on the reverse hereof.

By C Ellen
AUTHORIZED SIGNATURE

D1035

Elias

NON-NEGOTIABLE RECEIPT

HARVARD TRUST COMPANY
HARVARD SQUARE
CAMBRIDGE, MASSACHUSETTS

FOREIGN REMITTANCE

246

Date June 3, 1974

Received from David Frisch , Acct.

the sum of One hundred fifty-three and 26/100 U. S. Dollars
AMOUNT IN WORDS

to cover a remittance of One hundred fifty and 00/100 U.S. Dollars
AMOUNT IN WORDS

to be sent by ☐ regular mail ☒ air mail ☐ cable

to Dr. Alexander Luntz Cybernetician; Wife: Ludmilla, geologist
PAYEE'S NAME
U.S.S.R.
Moscow, W-335
STREET AND NUMBER — CITY OR TOWN
Garibaldi Str. 15
Corpus 2, Ap. 76
PROVINCE OR DISTRICT — COUNTRY

Message (if any) (Pay in Rouble Certificates)

PLEASE OBTAIN RECEIPT

Conversion Rate	Amount To Be Remitted	U. S. Dollar Amount	Commission	Air Mail Cost	Cable Cost	Total Amount Received
		150.00	3.00	.26		$153.26

This transaction is subject to the terms and conditions stated on the reverse hereof.

By [signature]
AUTHORIZED SIGNATURE

D1035

Russia

UNIVERSITY of PENNSYLVANIA

PHILADELPHIA 19104

May 7, 1974

The College

DEPARTMENT OF PHYSICS

Professor Victor F. Weisskopf
Massachusetts Institute of Technology
Department of Physics
Cambridge, Massachusetts 02139

Dear Vicky:

In reply to your letter of May 2 I can only say that Marx telegramed us on April 15 that he had been waiting six weeks for an American visa. We called the A.E.C., who informed us that on April 16 the State Department had recommended to the American Embassy in Budapest that Marx's visa be granted. We do not know the reasons for the delay but Marx in fact was able to get here by April 22 in time for the Washington Meeting of the A.P.S.

I am sorry that you could not attend our Conference, which in fact was quite exciting and successful.

Sincerely yours,

Sidney A. Bludman

Sidney A. Bludman

SAB/dh

UNIVERSITY OF CALIFORNIA, LOS ANGELES

Russia

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DEPARTMENT OF PHYSICS
LOS ANGELES, CALIFORNIA 90024

September 24, 1973

CONFIDENTIAL

Professor V. Weiskopf
Massachusetts Institute of
Technology
Department of Physics
Cambridge, Massachusetts 02139

Dear Vicki:

I write this letter to you to ask for your help in the Russian collaboration. I use a letter rather than the telephone because I feel it is finally time that the full story be known.

The problem is the following: Shepard has just returned from Russia, again without agreement on publication. The Russians have agreed to complete their analysis by October 1. However, the only method to find final agreement is to have the Russian group leader, E. N. Tsyganov, come to UCLA for discussions. I emphasize that we are dangerously close to an impass and that it is possible--perhaps even probable--that the pi-e experiment will never be published. Such an action would be a tragedy for both the scientific and social experiment. Technically, there are few problems. Shepard and I are convinced that there is no substantial difference between the UCLA and Dubna analysis and that in a normal collaboration these differences would easily be resolved. To understand Tsyganov's actions, one must understand the whole history of the collaboration, a story I now think it safe to tell.

Tsyganov is a complicated man--thoroughly Russian and thoroughly loyal. His difficulties came about as a result of his efforts to push the first US-USSR collaboration. In Spring, 1970, Friedman, Panofsky, Fields, and I tried to negotiate an agreement on pi-e scattering but were unsuccessful because Serpukhov wanted too much for the American entry there. After the formal discussions had ended in failure, Tsyganov and I tried to resurrect the experiment. It was at this time that Baldin said about Logunov, "We now know what he is, we now must establish the price." We did so in informal conversations; his price for American entry was one million dollars of equipment in the form of advanced electronics. Tsyganov refused to accept this view, and after I left Russia for UCLA, went directly to Petrosyants either personally or by letter (I am not sure which). Simultaneously, I persuaded our AEC to send a letter from Seaborg urging that we proceed under an ad hoc agreement.

So far, Tsyganov had done nothing illegal under Soviet law. Ultimately (in June), Petrosyants drafted a favorable reply to Seaborg. I know of this letter because Tsyganov told me of it in a communication smuggled out to CERN by Pier Innocenti. Tsyganov then obtained a copy of the letter and transmitted it to me via telex and I promptly sent it to Washington. The official letter did not arrive until a month or two later; I am not sure of the exact date. Tsyganov and I then carried on a number of telex communications, many of which discussed the politics of the proposed agreement. Under Soviet law this was an illegal use of channels of international communication and, after the problem came to a head, was important enough that Breshnev issued a warning prohibiting such use. Tsyganov's problems came about because Panofsky and I gave a copy of these telexes, not knowing that they were illegal, to Ermalov and Mukhin when they were at SLAC in July or early August, 1970. Mukhin did nothing, but Ermalov took the telex copies to Serpukhov where Tsyganov was accused of passing state secrets to me. The Dubna telex was then removed from private hands and placed under KGB control. Tsyganov underwent an intense questioning period including such questions as whether I had been asking about rocket installations and other military matters and whether or not I had offered him money for this information. Prokoshkin, Logunov, and Ermalov were especially vehement in their accusations. All of this was unknown to me when I arrived at Protvino in August, 1970 and negotiated a Dubna-UCLA-Serpukhov protocol covering the experiment. I learned part of it when Tsyganov and I were in Dubna the following week where we were under intense surveillance. As an example, Tsyganov was required to report our conversations daily. At one point, Tsyganov and I estimated the probability that he would not be sent to Siberia as 4%. I returned to the U. S. after the Kiev conference, where Tsyganov was originally a delegate but was forbidden to attend, and went to Serpukhov with our group in mid-September. None of the agreed-upon preparations for our experiment had been undertaken. Tsyganov was not allowed to make any decision on the experiment. In fact, there were daily communications from Serpukhov to Dubna demanding them to name a new group leader (Kuznetsov was discussed). Baldin resolutely refused these demands and only after several months was Tsyganov's name finally associated with the experiment. (It was a big day for Tsyganov and me when a Serpukhov schedule finally listed the experiment under Drickey-Tsyganov.)

As time went on, things slowly became better although even at the end of the pi-e experiment Prokoshkin told me that Tsyganov would never do another experiment at Serpukhov. The ultimate impact of these troubles is that Tsyganov's career has been destroyed and that he is forbidden to travel. However, time heals all wounds, the experiment was a success, and now Tsyganov has only a small threshold to overcome and be re-instated as a practicing physicist. This threshold can be broached if he is allowed to come here to negotiate the final results and I ask your help in obtaining a high-level invitation for him to do so.

I have refrained in the past from telling this story because of the damage it could do to Tsyganov. Now I believe the inverse is true, and it can only help him. Most of my group do not know all of this story although they realize that something major took place. I have told parts of it also to Pief and to Wallenmeyer, but I feel that you are the one person in a unique position to understand it all.

I would like an invitation from the highest level--from Chairman Ray if possible, and probably issued directly to the State Committee. These discussions should take place in December. Ticho and I issued an invitation last December when Tsyganov telexed me forbidding any further talks on our experiment. (I was scheduled to talk at the USC winter APS meetings but sat in the audience and did not do so.) There has been no reply to this invitation, so that it might be possible to simply re-issue it. It would be a tragedy if this collaboration, undertaken at so much effort on both our sides, were ultimately to fail.

Sincerely,

Darrell

Darrell Drickey,
Associate Professor of Physics

DD:jt

Crandall Card
25% COTTON FIBER

TRIP REPORT OF

PAUL F. SHEPARD

(Soviet Union and Poland)

August and September, 1973

I. Traveler: Paul F. Shepard, Adjunct Assistant Professor of Physics, University of California at Los Angeles, AEC contract # AEC AT (11-1) Gen 10, Proj. 17.

II. Purpose of travel:

1) Discussions with E. N. Tsyganov from the Joint Institute for Nuclear Research (JINR), Dubna, USSR to try to reach agreement for publication on the results of a joint American-Soviet collaborative pi-e experiment conducted at Serpukhov during 1970-71.

2) Discussions with the Filmless Detector Group from the Institute of Nuclear Physics, Cracow, Poland about

a) the analysis of π-π scattering data tkaen at Serpukhov as a background to the π-e experiment mentioned above; and

b) informal and unofficial discussions of possible future collaborative experiments at NAL and SLAC.

III. Itinerary and Highlights

A. Soviet visit

1) August 21-22. Leave Los Angeles and travel to Dubna, USSR.

2) August 23-28. Detailed discussions with Tsyganov covering all phases of the Russian and American analysis of the π-e experiment.

3) August 29. Conversation with John Ward at the American Embassy concerning some of the problems with the collaboration.

4) August 30. Meeting with Baldin, Savin, and Tsyganov to summarize the results of our talks.

5) August 31. Meeting with K. Lanius, Novak, Likhochov, Savin, and Tsyganov again to summarize the results of our talks.

6) September 1-2. Final discussions with Tsyganov.

B. Polish visit

1) September 3. Travel from Dubna to Cracow, Poland.

2) September 4-6. Informal discussions about basis for future collaboration, the present π-π analysis, and the status of the π-e experiment.

3) September 7-9. Revised proposal for NSF special foreign currency funds.

4) September 10. Public seminars about UCLA's recent results at the SPEAR facility at SLAC followed by private conversations with Professor Miensowicz concerning the preparation of the NSF proposal for future collaboration, the status of the π-π analysis at Cracow, and the difficulties connected with the π-e collaboration with the Soviet Union.

5) September 11. Traveled to Warsaw with Michel Turala and Bogdan Niczyporuk. We spoke briefly with Professor Zalinski about our future plans and present difficulties with the π-e collaboration.

6) September 12. Meeting at the American Embassy with Mr. Greenberg (Scientific Attache), Mr. Wiechowski (NSF special foreign currency program officer), Turala and Niczyporuk to make general inquiries about the appropriateness of this NSF proposal, and specific inquiries concerning international travel and other budget categories.

7) September 13. Returned to Los Angeles.

IV. Narrative

A. Soviet Visit

The basic purpose of the trip to the Soviet Union was to find a basis for the completion of the work on the joint American-Soviet π-e scattering collaboration in the near future. This experiment was started in September, 1970. The American analysis of the data was finished in October, 1972. It was expected last October that the experiment would finish very shortly. However, the leader of the group from Dubna, E. N. Tsyganov, began a new and extensive analysis of the data. We were not aware that such a decision had been taken until

December, 1972 when discussions by telex with Tsyganov became somewhat acrimonious. Tsyganov recently informed us that his analysis was nearly finished, and my trip was undertaken to find out if there was a basis for discussions leading to the completion of the experiment and the publication of the results.

To understand how this situation developed and the tenor of some of the conversations during the trip, I shall review briefly some of the history of the experiment in the last two years. At the completion of the experimental data taking in July, 1971, it was informally agreed between the American group and the Dubna group that the analysis of the experiment would be done in the United States for the reason that the computing facilities at Dubna were insufficient for the analysis of such a large experiment. To this end, invitations were sent to Tsyganov and others to come to the United States and participate in the analysis. Two Polish members of the Dubna group (Turala and Niczyporuk) came immediately, but Tsyganov was not allowed to come. An "official" refusal indicating commitments to other experiments was eventually received at UCLA. This was untrue as Tsyganov has worked fulltime on the π-e collaboration since its inception. As a result of this official lack of cooperation on the part of the Soviet Committee on Atomic Energy, some revision of the plan of analysis was necessary. The bulk of the analysis would be done at UCLA, but Tsyganov would check some portion of the analysis at Dubna. To cooperate in this plan, we made computer time available at UCLA for the running of analysis programs, written at Dubna by Tsyganov's group. These programs were carried to UCLA and executed on American computers by the

Polish members of the Dubna group. By March, 1972, UCLA had preliminary results from about 25% of the data, and it was agreed after consultations between Darrell Drickey and Tsyganov in Dubna to present these preliminary results at the APS meeting in Washington, D. C. in April, 1972. It was also understood that preliminary results from the UCLA analysis on the entire sample of data would be available by summer, that the partial Dubna analysis would finish then; and, therefore, a meeting in Dubna to discuss these results should take place at the end of the summer. Darrell Drickey and I went to Dubna in August, 1972 for this purpose. As a result of discrepancies which became apparent between the UCLA and Dubna analysis, it was agreed that more work was necessary, but that final results could be expected by early fall, 1972. In fact, we did resolve all these difficulties by October, 1972.

Unfortunately, Tsyganov had by this time decided to undertake a new and extensive analysis of the entire data sample. This was made possible by the acquisition of a CDC 6200 computer at Dubna. We did not agree with this plan nor did the Polish members of the collaboration. Nonetheless, this extensive new analysis was undertaken. It was clear from its beginning that no meaningful discussions about a joint publication were possible until Tsyganov had finished this new analysis. Accordingly, when Tsyganov indicated that his analysis was nearly finished and that a visit would be useful, I went to Dubna to see where the situation stood with respect to finishing the project.

It was clear from the beginning of our conversations that although Tsyganov's analysis was close to finishing, it was in fact not yet finished. Consequently, agreement on final results was not possible. Instead, a plan to review all phases of both analyses was carried out with the intention of setting a set of deadlines for the completion of the project. Tsyganov and I carried out this general review of the project between August 23 and 29. All of the numerous corrections to the experimental data were considered and the attenuation corrections for pions in the spectrometer were revised based on recent high energy π^- - nucleus data from Serpukhov. I believe that this review of the experimental corrections was the most beneficial result of the discussions in Dubna.

Prior to beginning my discussions with Tsyganov, we met briefly with Savin, the head of Tsyganov's division. He indicated that the laboratory was anxious to finish the project and have the results published. I agreed. I subsequently found out that Savin had been asked by Bogolubov, the director of the JINR, to participate in our discussions. Savin, out of deference to Tsyganov and myself, wisely chose not to do so. However, he clearly felt the need to express the laboratory's anxiety about any further delays in completing the project. Since the delays cannot be attributed to the American work, I could see that Tsyganov was in a difficult position with regard to his superiors in this matter. Although I was also anxious to get Tsyganov to expedite the work at Dubna, it was not my desire to create a still more difficult position for him with respect to the scientific directorship at Dubna. Consequently, I tried to proceed with circumspection.

By the end of my visit to Dubna, the pion form factors for all the important data samples had been calculated. I personally made these calculations on the new CDC 6200 computer at Dubna using Tsyganov's new results and the experimental corrections which we had reviewed earlier in my visit. Despite this success in expediting their analysis and contrary to our understanding of the situation as late as last spring, it was clear that there would have to be one more discussion in order to finish the project. I emphasized two points in this respect. First, there should only be one more discussion and that if this discussion cannot produce a result, then the experiment must be regarded as having yielded no result. Second, the discussion cannot take place until all analysis at Dubna is finished, and ought to occur by December, 1973. To this end, Tsyganov agreed that it should be possible for his group to finish the analysis by October 1, 1973, and to send their results to UCLA by November 1, 1973.

This general plan for finishing the project was discussed with the directorship of the Dubna laboratory at two meetings. The first meeting was with Professor Baldin, the director of the high energy physics laboratory at the JINR, Savin, Tsyganov, and myself; the second was with Dr. Karl Lanius, vice-director of the JINR, Novak, Likhochov (vice-directors of the high energy physics laboratory), Savin, Tsyganov, and myself. Everybody agreed to the idea of finishing the analysis by October with final discussions to be held by December, 1973.

The question of where these last discussions should occur was considered. I argued that since the delays in completing the experiment resulted from the lengthy Dubna analysis, the most propitious place for a final meeting would be the United States. In actual fact, the situation is very complex. Had Tsyganov been able to come to the United States in the first place, probably none of these difficulties would have occurred. The fact that Tsyganov was denied permission to come to the United States in connection with our collaboration has sharply curtailed his future professional opportunities. It has created a situation where a lengthy and repetitive anaylsis at Dubna was at least partially a condition for professional survival. Under these circumstances, it is hard to escape the conclusion that a concluding discussion with Tsyganov in the United States is necessary to restore a reasonable balance to the collaboration.

When it became clear to us in December, 1972 that a new analysis was beginning in Dubna, we tried again to invite Tsyganov to UCLA. That invitation is still extant, and was never answered by Tsyganov or Baldin. In my conversation with Baldin, I strongly criticized this behavior. Baldin expressed his apologies to Professor Ticho and Professor Drickey, but it was clear that no answer can be expected at present. This invitation could be revived, and would receive official support from Dubna. However, it would undoubtedly require a high-level request for cooperation in this matter from the AEC in order to move the Soviet Committee on Atomic Energy. I was advised informally by the embassy that such a request for cooperation with regard to completing our project would be appropriate under the present agreements with the USSR, and, in fact, would be a useful test of good faith.

As a summary of my discussions with Tsyganov and others at Dubna, I wrote a brief page for internal use by the laboratory which is repeated below. Tsyganov wrote a Russian paraphrase omitting the comments about the place for final discussions.

SUMMARY OF DISCUSSIONS ON
pi-e EXPERIMENT
August 22 -- September 3

Tsyganov and I have reviewed both the UCLA and Dubna analysis of the pi-e experiment. We have reached tentative agreement on all important corrections to the experiment, but a final agreement for publication is not possible until the Dubna analysis is completely finished. I believe that there can be no more than one more meeting to reach agreement on the results of the experiment. Such a meeting should not take place until all work is completed at Dubna. Tsyganov believes he can complete the Dubna analysis by October 1. All the results of the Dubna analysis should be sent to and be in our hands at UCLA by November 1, 1973. I regard this as a prerequisite for any final discussion. These results should include the form factors and all corrections for the three important data samples (April, July, July no C).

Since the delay in completing the experiment has resulted principally from the lengthy but extensive Dubna analysis, the most propitious place for a final meeting is at UCLA. To this end, I think the best plan is for Tsyganov to come to UCLA for final discussions when the analysis at Dubna is finished. Should this prove to be impossible, we would request that JINR pay the travel of one or two Americans to Dubna for the final discussions. In any event, it is advisable that final discussions take place before December, 1973 at the latest, since it will be impossible to carry on any additional pi-e analysis at UCLA beyond that time.

UNITED STATES
ATOMIC ENERGY COMMISSION
DIVISION OF PHYSICAL RESEARCH
WASHINGTON, D.C. 20545

June 5, 1974

For your information.

H. L. Kinney
Asst. Dir. for ADA
Div. of Phys. Res.

Russia

MAY 3 1 1974

B. D. Hill, Chief, East-West Affairs Branch, Division of International Programs

TRAVEL RESTRICTIONS ON SOVIET EXPERIMENTERS AT NATIONAL ACCELERATOR LABORATORY (NAL)

This is in response to your memorandum of May 1, Travel Restrictions on Soviet Experimenters at NAL. The Division of Physical Research supports fully the request of the Batavia Area Office to relax the present restrictions for Soviet experimenters at NAL to visit the Argonne National Laboratory. As the Batavia Office indicated, the need for such visits is primarily for computer runs on the Argonne 360/195 and the need often cannot be forecast two weeks in advance. NAL Soviet attendance at ANL physics seminars and NAL Soviet use of reference material from the Argonne library would also be enhanced by easing the present restrictions. It is our understanding that easing of these restrictions poses no significant additional problems for the Chicago Operations Office.

Original Signed by
H. L. Kinney

John M. Teem, Director
Division of Physical Research

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TO PROFESSOR V.F. WEISSKOPF
CAMBRIDGE USA

DEAR PROFESSOR WEISSKOPF I SEND YOU AS YOU HAVE REQUESTED THE YEAR OF YOUR ELECTION AS A FOREIGN MEMBER OF THE USSR ACADEMY OF SCIENCES 1976 BEST WISHES VICTOR A MATVEEV DIRECTOR OF THE INSTITUTE FOR NUCLEAR RESEARCH OF THE USSR AC SCI MOSCOW
BEST WISHES VICTOR MATVEEV

Also Oct 76 → Political

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Now I would like to say a few words about my own conversations with Academy members. When I saw Markov the first time, I told him that I intended to visit Sakharov when I passed through Moscow. He said, in a friendly tone, that this is my private business and, naturally, I am free to visit anybody I wished. A few days later, Logunov told me privately that the Academy intended to elect me as foreign member at its next session which took place on May 31. A few days later I was requested by Markov and Logunov to see the Secretary of the Academy and I was asked officially whether I would accept such an election. I said I would, that it would be a great honor, and also a help in respect to collaboration between East and West. The next day, however, I asked Logunov and Markov for a confidential conversation. I said to them that I was most pleased by this election, but I would like to draw their attention upon one fact -- I am worried about the case of Sakharov. If the Academy would expel Sakharov, I would be forced to resign and I would like them to be aware of this. If this statement of mine would perhaps induce them to change their mind and decide not to offer me membership, I would by no means be offended, and I would understand the situation. They were seemingly not surprised by what I said and answered me in a rather light-handed and not at all offended way. [I did tell Markov a few days earlier that I intended to visit Sakharov when I passed through Moscow.] They said there is no intention to expel Sakharov from the Academy -- and what I do in case such things would happen is my own business and I am free to do what I want.

Later on I visited Piotr Kapitza (the old man), who already knew that they intended to elect me and read to me the summary of my activities as it was communicated to all members for a vote. I told Kapitza about my conversation with Logunov and Markov in respect to my possible resignation if Sakharov were expelled. Kapitza was glad that I did so and told me that he considers such a possibility as highly improbable, because they need a 2/3 secret vote in order to expel a member, and he doesn't think it would ever get 2/3 in the case of Sakharov.

I also wrote a letter to Logunov in which I mentioned that the feelings between CERN and DUBNA and the Soviet Union are somewhat tense and that the present directorate is not too sympathetic to collaboration with the Soviets. I said that an extended visit of Gribov to the Theoretical Division at CERN could make an enormous difference in the attitude of the CERN directorate towards collaboration and would make it much easier for me to help improving the situation. I urged him to do his utmost to make such a visit possible. I really believe that it would make a difference and I am curious whether Logunov will follow my advice or not. I also told the same thing to Yarba. Indeed I gave the letter directly to Yarba so that it doesn't go through the administrative apparatus of the Academy but will go directly into the hands of Logunov.

Coming back to Kapitza -- I asked him about his opinion as to the general situation in respect to Jewish scientists who want to emigrate. He told me the situation is by far not as bad as the Western press made it. Many people are allowed to emigrate. In fact a large number of artists, musicians, and painters have emigrated. This fact was also told to Wilson, by Alichanian. Kapitza says there are a few cases in which permission for emigration has not been granted yet, like the case of Levitch (he is convinced however that the Levitch case will be solved soon). So he thinks the West exaggerates the situation -- I hope he is right but I am not sure. Kapitza is most sympathetic towards Sakharov -- he refers to Sakharov as a saint. He said to me that, when I see him, I should tell him he should do more physics since this would make it easier for all of us to help him.

On my last day I did visit Sakharov and had a long conversation with him. It was very warm and human. There are few facts I can report except that he has difficulties with his apartment; they did not give him the right of residence in Moscow and the Academy doesn't lift a finger to help him. The children of his wife are still badly off -- nothing much has changed in that situation. His daughter is without job, his son-in-law has a job but not a very satisfactory one.

I also saw Engene Feinberg and Eugene Lifschitz when I was in Moscow. Both have given me slightly different versions of the situation. Feinberg is in general an optimist and says that things are getting somewhat better. Anti-semitism still exists, and is partly caused by the envy of other people -- envy that some Jews have the right to emigrate whereas the non-Jews cannot. There seems to be still difficulties for Jewish kids to be accepted at the university, a fact that Kapitza denied by saying that 3% of the students at Moscow University are Jews. Feinberg and Lifschitz told me, it is extremely hard for Jews to enter the Moscow University. Those two statements may not be, by the way, contradictory. It is also extremely difficult for young Jews to get the kind of job they want. Lifschitz is more pessimistic; he says the situation is deteriorating and the Jews have a more and more difficult time, and, in general, freedom and civil rights are diminishing and things get slowly worse. I don't know who is right. Lifschitz is known to me in the past as a man who always has a tendency of seeing things darker than they are. However, there is no denying that Lifschitz has now gotten the permission for travel abroad which he considers a fluctuation. But Feinberg and I consider it as a sign that things are getting slightly better. I heard three views on the Jewish question from three people who talked to me openly and frankly: Kapitza, Feinberg and Lifschitz. Their opinions range all the way from "not so bad" to "very bad".