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PREFACE

The travel reports of Messrs. Clarke and Roberts, although written independently, are submitted jointly because they supplement each other, and the authors feel that this formal combination will be useful to the reader.

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TRIP REPORT

J. F. Clarke

Visit to the International Symposium on Closed Confinement Systems held at Dubna, U.S.S.R., Sept. 28-Oct. 3, 1969; Kurchatov Institute, Moscow, Oct. 6-7; Culham Laboratory, England, Oct. 10, 1969; and, CEN, Fontenay-aux-Roses, France, Oct. 9, 1969

DUBNA SYMPOSIUM ON CLOSED CONFINEMENT SYSTEMS

September 29-October 3, 1969

Summary of the Meeting and a Visit to the Kurchatov Institute

Since it was only a year since the Novosibirsk meeting one expected little in the way of dramatic advances at this conference. This expectation was confirmed but the meeting had a significance in spite of this, since it allowed an evaluation of the state of knowledge concerning toroidal systems and moreover a measure of the time derivative of this knowledge. The meeting concentrated to a large extent on the two main toroidal systems, stellarators and TOKAMAKS. As at Novosibirsk the U.S. had little to contribute to either of these topics. In the case of stellarators the vigorous programs in Great Britain, Germany, and the U.S.S.R. were concerned with investigations on non-Bohm losses and their relation to the newly formulated theory of classical losses by the Russian theorists Galeev and Sagdeev. To be sure the situation is in a state of flux but the impression is one of movement. Losses are still larger than classical but are smaller than those predicted by Bohm diffusion. The U.S. stellarator program was represented by a report on the Livermore stellarator which is closing down for lack of staff and the Princeton Model C which is being converted into an imitation of the Russian T-3 TOKAMAK. The Livermore work was more in tune with the tone of the meeting in that it was concerned with classical loss processes while the Princeton contribution consisted of an attempt to explain the reported long confinement times in TOKAMAKS and some of the non-U.S. stellarators on the basis of two-temperature velocity distributions observed in anomalous Model C stellarator operation.

The success of the Russian TOKAMAK program and the confirmation of their previously reported temperature measurements by the British laser scattering team was the high point of the meeting. The laser results showed unambiguously that in the T-3 device 99% of the plasma electrons

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have a single-temperature Maxwellian velocity distribution. There may be a small hot group of electrons but it is not detectable within the accuracy of any of the measurements. Thus it is clear that the Russians have produced an extremely interesting plasma. The unanswered question is why does their device perform so well and stellarators so poorly. The qualitative reason advanced by Academician Artsimovich is that stellarators have a delicate magnetic structure which is easily upset by small defects in the external windings used to create it. TOKAMAKS, he argues, have their rotational transform and shear generated by internal plasma currents which are less sensitive to perturbations. These arguments appear rather mystical in nature but they should be considered in the context of several interesting papers presented at the conference. Meade, from the University of Wisconsin, reported on the serious plasma losses from the Wisconsin multipole due to magnetic imperfections which produced asymmetries in the confining field. Yoshikawa,of Princeton, reported on plasma losses in the spherator occurring at the asymmetries represented by the supports of the internal ring, which produced part of the confining field. Grieger, of Garching, presented a paper which included data on enhanced particle containment when a specific perturbation field was introduced. He attributed this improvement to the fortuitous correction of an existing defect in the magnetic structure. Several stellarator papers reported that in general confinement was dependent on rotational transform but not on magnetic shear. Since large shear can eliminate most instabilities long blamed for Bohm diffusion whereas rotational transform is related only to field structure it appears that Artsimovich's surmise about the delicate nature of the stellarator's magnetic structure may be founded in fact.

If this is the case it will be worthwhile to attempt to make the applied magnetic fields in a TOKAMAK as ideal and symmetric as possible.

If the present TOKAMAKS are so superior to present stellarators by virtue of their slightly superior symmetry then one can hope for further improvement by constructing a truly symmetric device. This was one of the basic design requirements for the ORMAK device, and Artsimovich commented favorably on this aspect of the device. Razumova, who is in charge of the TM-3 device, was also pleased at the prospect of a direct comparison of the ORMAK-I results with her work and felt that this would provide a test

of the importance of the high symmetry.

During our discussions in the two days spent visiting the Kurchatov Institute we discovered that they were unaware of the method which we are using to eliminate the large perturbation caused by gaps in the stabilizing copper torus. Their new experiments are probably too far along to incorporate this correction which they agreed could be of importance in the anomalous plasma behavior at high densities. Thus in at least one respect the U.S. TOKAMAK program at Oak Ridge is in advance of the Russian work.

The scaling laws introduced at Novosibirsk which allow one to project the Russian TOKAMAK results to the thermonuclear regime have been somewhat modified since that meeting. The energy confinement time is now reported to scale

$$\tau_E \alpha \quad a^2 \, H_\theta$$

where a is the plasma radius and H_θ is the poloidal field produced by the plasma current. Since this field is related to the applied toroidal field H_θ by the Kruskal-Shafranov limit this can be rewritten as

$$\tau_E \alpha \quad \frac{a^2 H_\theta}{qA}$$

where A is the aspect ratio, i.e., the torus radius divided by the plasma radius and q is the stability safety factor. Thus long energy confinement time still depends on large applied fields and low aspect ratio although to a smaller degree than previously expected. The Russians are clearly following this dependence in the construction of their new lower aspect ratio experiment, T-6, and in the rebuilding of TM-3 to allow a 50% increase in field. In this respect they showed great interest in the design of the Oak Ridge TOKAMAK, ORMAK, which incorporates these features along with much greater diagnostic access than their design.

Table I shows the predictions of this scaling law for the Russian and American TOKAMAKS now under construction, based on the past T-3 results.

Table 1

	T-3	New TM-3	T-4	T-6	ORMAK I	ORMAK II	Stellarator Conversion
B_θ (kG)	35.0	50.0	55.0	20.0	50.0	50.0	55.0
A = R/a	6.7	5.0	5.9	3.0	5.0	2.3	7.7
a cm	15.0	8.0	17.0	25.0	8.0	15.0	13.0
τ_E(Msec)	20.0	11.0	47.0	73.0	11.0	84.0	21.0
$T_e + T_i$(keV)	1.5	1.8	2.7	2.0	1.8	6.0	2.0

It became clear in the discussions following the various TOKAMAK papers that there was no good theoretical prediction as to the limiting plasma pressure which can be achieved in a TOKAMAK. If this pressure is too low economic considerations would make a TOKAMAK reactor very unlikely. Experimentally the plasma pressure in stable operation is found to be less than the poloidal magnetic pressure which is probably too low for reactor operation. Mercier, of France, suggested that the anomalous destruction of the plasma above a certain critical density could be related to the excess plasma pressure observed at early times in the discharge. This is clearly one of the most important areas for future investigation in TOKAMAKS.

There were two theoretical calculations presented which attempted to model the behavior of the TOKAMAK plasma on the basis of the Galeev-Sagdeev-Kovrizhnikh theory of classical losses. One of these calculations was by the Russians, Denstrovsky and Kostomarov, and the other by the French under Mercier. The Russian work was hard to evaluate critically. They appear to have used an incorrect current distribution but their final results were that they could reproduce the behavior of the T-3 experiment at high densities on the basis of purely classical processes. The work of Mercier et al. is easier to evaluate critically since I visited him at Fontenay-aux-Roses after the meeting to discuss it. He treated the charge exchange losses incorrectly by neglecting ionization and underestimated the radiation losses by neglecting impurity radiation which the Russians claim accounts for half of their energy loss. However, these effects tend to balance one another and his results again came close to reproducing the steady-state

T-3 data at high density on the basis of classical processes. There is an anomaly when he attempts to reproduce the dynamics of the plasma as the skin effect tends to concentrate the current on the outside of the plasma column. Some non-classical resistance is necessary to reproduce the observed current penetration. This indicates that the plasma behavior cannot be entirely classical. Experimentally the energy balance measurements which the Russians use to bolster their contention of purely classical processes is uncertain within a factor of five. Much more precise measurements are needed on the heating process of the electrons and ions in TOKAMAKS, but the close relation of these processes to classical effects is encouraging.

A program on feedback stabilization to control plasma instabilities in TOKAMAKS is being initiated by a number of capable people in the Golovin division at Kurchatov. This may be important if one discovers that the plasma pressure limit is due to MHD instabilities. In addition there are two turbulent heating experiments at Kurchatov which offer the possibility of increased energy input into TOKAMAKS. The results of these rather small-scale devices will be of great interest since the ohmic heating in the present TOKAMAKS has almost reached the limit of its usefulness.

In summary the meeting revealed that there are many basic mysteries connected with the operation of existing TOKAMAKS. The only certainty is that the plasma is hot and dense and that the Russian staff is of high caliber. Much more basic work is necessary in the U.S. as well as Russia before these mysteries are solved and they must be solved if one is to hope to scale the encouraging results to the reactor regime. The participation of the U.S. in the TOKAMAK effort is necessary if it is to proceed rapidly simply because our technological base allows us to utilize new techniques more rapidly than the Russians. In addition, the usefulness of competition and cross fertilization cannot be over estimated. The new dynamism in stellarator research in the U.S.S.R., West Germany, and Great Britain can in a real sense be attributed to this competition and cross fertilization.

Visit to Fontenay-aux-Roses (France)and Culham Laboratory (England)

These visits were decided on at the Dubna meeting when it became apparent that the microwave techniques in use at Culham were not familiar to the American company with which we were dealing and that the French theoretical effort was the most important theoretical work relative to TOKAMAKS in the west. As it turned out the Culham 2-mm techniques were developed under a half-million pound contract by the Royal Radar Establishment. Hamberger and Sharp claim to have the best millimeter wave technique in the world, and what I saw was impressive. It consisted in clever application of quasi-optical techniques to reduce the large losses in this frequency range. They were reluctant to part with detailed data on these techniques on the principle that it had cost them a lot of money to obtain. They promised to check with Dr. Pease, the Director of Culham, about the possibility of selling the information or components not available in this country.

At Culham I also visited their translation officer, Mr. Ronson, to inquire why we didn't receive the Culham translations. He apologized but seemed nettled by the lack of cooperation received in the past with regard to the sharing of translations between himself and the U.S. Our translations of Kurchatov internal reports always seem to appear later than the Culham translations, which is a real disadvantage in our work. It appears that a better system could be worked out than whatever now exists.

The theoretical group at Fontenay is currently doing some extremely relevant work on the stability and energy balance of TOKAMAK plasmas. When one performs experiments on a system as complex as a TOKAMAK one sorely needs a good theoretical picture of the interactions which can take place. Mercier's group is just now beginning to attempt to include all of the known effects which determine the plasma temperature, such as diffusion, heat conductions, radiation, and charge exchange. The results of this work will not only make the experimental results more intelligible but if the postulates are confirmed can lead to predictions of behavior in the thermonuclear regime. They seemed eager to cooperate with us in the future when the ORMAK is operational.

Political and Social Observations

In the year since Novosibirsk an improvement in the standard of living, at least in Moscow, has been apparent. This is reflected in the number of miniskirts and fashionable outfits to be seen on the street. At Novosibirsk the few well-dressed women were rather scornfully pointed out as fashion-house girls and their number was low. From a rather futile effort to obtain some matching replacement clothing for a colleague whose luggage disappeared this was understandable. This is apparently changing.

On the political level quite a few of the scientists gathered at Novosibirsk last year were eager to discuss Soviet and American politics. There may have been some selection process operative at Dubna, but the mood was very much one of let's stick to business and ignore the seamier topic of international relations. The Russians were extremely friendly and open about their work, as they were at Novosibirsk, but the extracurricular conversations were either extremely abstract technical discussions or of very personal family type exchanges.

At Moscow airport my luggage was searched quite thoroughly by a polite but firm customs official. He became excited by the Kurchatov reports which had been given to me by Dr. Razumova. The Red Army was called and took away my passport and three or four reports. The search continued and more papers uncovered. Eventually my passport was returned and I was asked to sign a paper which I was told indicated that I agreed to the confiscation of the documents. I asked why they were being seized and was told that since I had not paid for them I could not take them out of the U.S.S.R. I produced my conference registration fee receipt and was able to retrieve the conference abstracts. My plane was about to leave. I signed the document. The customs man and I parted on good terms. Three hours later Dr. Roberts left and was not bothered. The papers confiscated were also obtained by others and no harm was done.

While the above was being written I received a letter at ORNL from Dr. Razumova containing two of the preprints which were taken at the airport. Her letter stated that she knew I wanted them and made no mention of the airport incident. There was no arrangement for her to forward these reports, as she gave me copies at Kurchatov. I feel an apology has been extended.

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REPORT OF FOREIGN TRAVEL

Michael Roberts

I. INTRODUCTION

This report covers the author's attendance and participation in the week-long International Symposium on Closed Confinement Systems held at Dubna, U.S.S.R. Sept. 29-Oct. 3, 1969, as well as visits to Kurchatov Institute, Moscow on October 6 and 7, and the Culham Laboratory, England, on October 10, 1969. The main purpose of this trip was to learn as much as possible about the design, construction, and operation of the TOKAMAK experimental devices in use at Kurchatov Institute. The ORNL ORMAK program is a major U.S. effort to provide a useful TOKAMAK plasma for fusion studies. The author also presented two technical papers (not on the original program but to be included in the published proceedings) concerned with ORNL work both in TOKAMAKS and in multipoles--the latter work performed in collaboration with M. Lubin at the University of Rochester. A number of questions about TOKAMAKS, unanswered before Dubna, which are of direct interest to the ORNL ORMAK design, were answered fully during the time. These questions dealt with methods of gas breakdown, plasma production, magnetic field generation, diagnostics, and general construction details. It should be emphasized that we,as visiting scientists,were treated with the utmost patience by the Soviet scientists who answered the same questions over and over again from representatives of each country and each laboratory. As a result of the openness of the Soviet staff I feel my visit was completely worthwhile and has already had a healthy influence on the ORNL program.

The trip itself was physically exhausting because of the various factors involved in long-distance foreign travel. One full day's rest prior to the conference would have been helpful and should be an integral part of foreign travel. As usual, sink stoppers and toilet tissue are very handy. In general, this second trip to the U.S.S.R. was less of a mental strain since we knew of the various rules and customs that must be complied with for efficient service and travel.

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II. DUBNA CONFERENCE

A. Multipoles. The two sessions of direct interest to me were those concerned with multipoles and TOKAMAKS. As yet, there is essentially no multipole research conducted in the U.S.S.R. The results of the U.S. efforts indicated that asymmetries of one kind or another, i.e., geometrical, magnetic, or plasma production, were the major obstacles to better confinement in internal ring devices. Whereas, a year ago, at the IAEA Novosibirsk, U.S.S.R. conference internal ring devices apparently were in disarray, it now seems that solid achievements have been made. T. Ohkawa, at Gulf-General Atomic, has built a large, low-magnetic field, d.c. octupole in which the ring supports are relatively very small and confinement times of 300 Bohm times are achieved. At the University of Wisconsin, D. Meade has investigated the apparent connection between magnetic field errors and localized plasma disturbances. S. Yoshikawa, at Princeton University, reported improved confinement in a reduced neutral pressure spherator limited primarily by the ring supports. M. Lubin, at the University of Rochester, with ORNL help, has measured an interesting shift in confinement dependence upon magnetic field ($\tau \sim B^2$) above a certain critical field strength. This ORNL-Univ. of Rochester paper received a number of useful questions, all from U.S. participants.

B. TOKAMAKS. The two papers presented by Ivanov and Robinson summed up the present state of plasma parameters and diagnostics on T-3 in a clear fashion. Unfortunately, the succeeding presentations on detailed work on TM-3 were not very helpful to me so, as a result, I will concentrate on the two T-3 papers.

Ivanov presented the compilation of ion temperature measurements on T-3, some of which is shown in Fig. 1. Through the use of charge exchange (Fig. 1a) the ion temperature is determined to be greater than 500 eV (500 eV was reported at the meeting, the figure showing 400 eV was taken from the recently published paper by Ivanov, et al.) in the usual operating regimes. The low energy charge-exchange flux is thought to be caused by particle interaction with the high neutral density at the periphery of the plasma column. The doppler broadening of H_α for transverse T_i agrees reasonably well with the charge exchange measurement for longitudinal T_i.

The neutron measurements were made with BF_3 counters showing reproducibility within a statistical spread. During the pulse, 30 counts are received, indicating a total of 10^6 neutrons in 4π steradians per pulse. With B shielding, the yield was reduced 100-fold, indicating that the yield is of neutrons, not x rays. It was stated that the neutrons are generated symmetrically in azimuth, i.e., not just at the limiter; in addition, the probability of wall generation is said to be small.

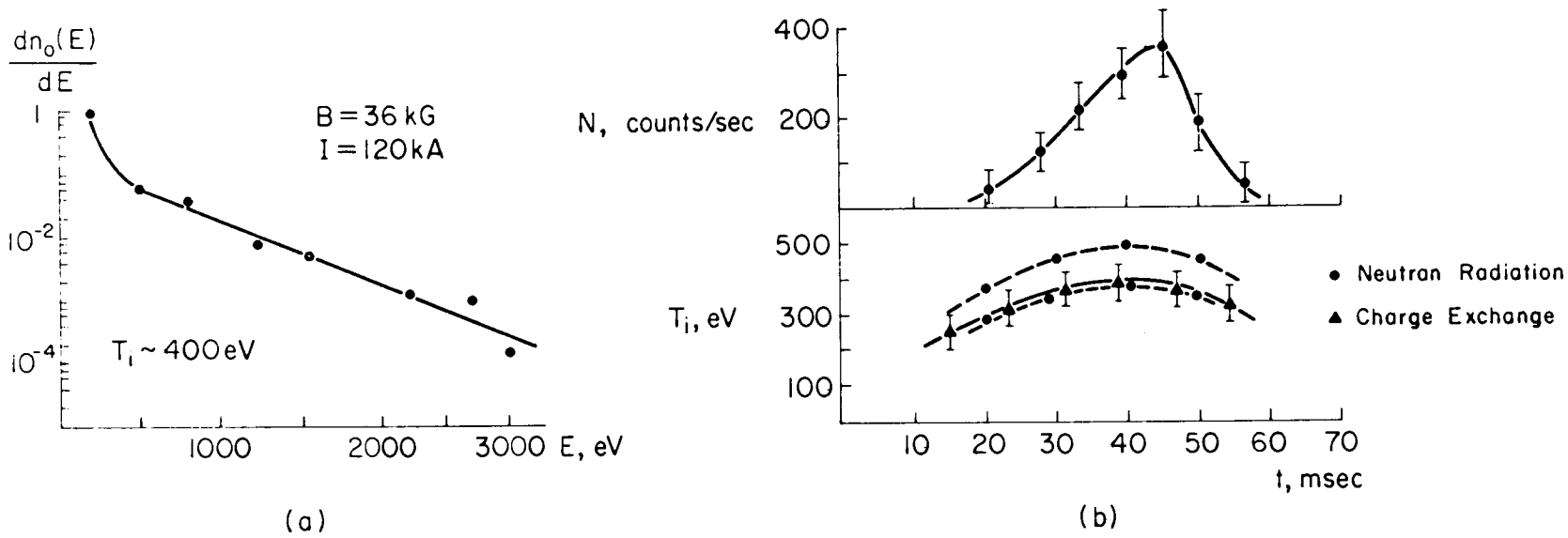
Figure 1b shows the temporal behavior of the density, neutron yield and ion temperature. The ion temperature is shown from both the charge exchange experiment and a calculation from the neutron yield data using two radial temperature profiles that bracket the experimental results. The measured T_i appears to apply to the main body of the plasmas of radius 12 cm. Ivanov said that one-half the neutrons are associated with the Maxwellian distribution.

One of the major questions in the TOKAMAK understanding has to do with the ion heating and energy loss. It was felt that the energy input could be divided into three parts: 20% of the energy input goes into Coulomb heating of the ions, 40% into radiation and presumably 40% to loss by the electron channel. As a result, energy lifetimes are variable over a small factor, 3 or 4, depending upon the assumptions made. Above a density of 6×10^{13}/cc there is column breakup.

Robinson's paper presented a near textbook laser scattering experiment with encouraging results. A copy of the text with figures is available for closer scrutiny so I will only sketch the end results. Both the raw data and analyzed data were shown, indicating a clear Maxwellian distribution of energy with an electron temperature of 900 eV for one particular discharge (Fig. 2a) and a range of T_e from hundreds of eV to 1100 eV. Seven energy channels (out of ten) were used in the determination since the first channel was jammed with primary light and channels 5 and 6 were jammed with H_α light. A radial profile showed T_e constant over the bulk of the column (Fig. 2b) falling near the limiter position. Temporal density measurements agree with microwave density measurements and also had the same shape as the temporal T_e shape. These measurements represent the properties of at least 90% of the electrons and apparently less than 1% of the electrons can have energies different than the 1-keV temperature. All these measurements were characterized by an apparent lack of ambiguity.

TOKAMAK T-3 PARAMETERS

ORNL DWG.-12576



Determination of T_i by Charge Exchange

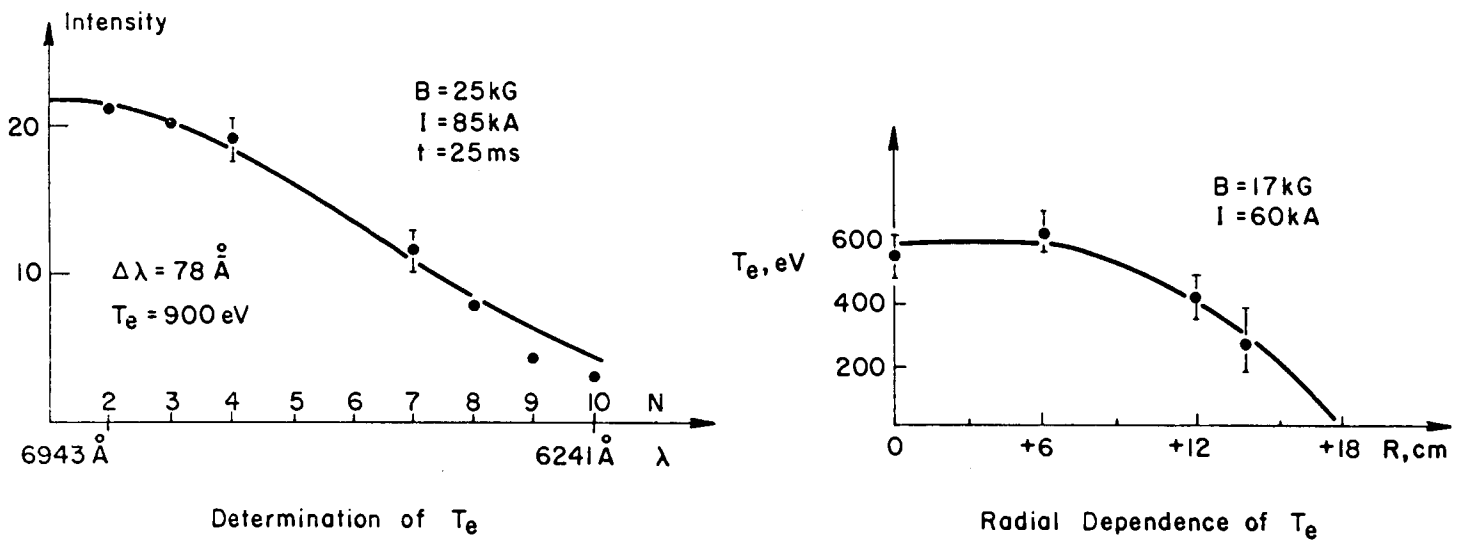
Temporal Behavior of N and T_i

T_i Measurements by Ivanov et al. Using Charge Exchange and Neuton Yield. Dubna, 1969 (JETP Letters 10 1969)

Figure 1

TOKAMAK T-3 PARAMETERS

ORNL DWG.-6912575



T_e Measurements by Robinson, Peacock, et al. Dubna, 1969
Using Thomson scattering with a Ruby laser.

Figure 2

C. Meeting Place. The meeting was held in a large lecture room located in a secure complex about a ten-minute walk from the Dubna Hotel. We were allowed into the complex upon presentation of our passes; the walls were distinguished by being high stone walls with cleared, raked areas inside, having a many-stranded electrified fence. Sessions ran from 9 to 5 or 6 daily.

Along the few streets on which we had time to walk, we could see the usual (for Moscow and Novosibirsk, at least) new-looking apartment houses and some shops. On closer examination, the buildings are generally constructed in a manner that would be called shoddy in this country. There is an interesting mixture of dress on the streets, from reasonably modern, conservative Western clothes to, presumably, more traditional Russian dress.

Just as on our previous trip and in Moscow, we had apparent full freedom of movement inside the cities.

III. LABORATORY VISITS

A. Kurchatov Institute. Our visit to the TOKAMAK group spanned one and a half days and included detailed discussions with most of the principal scientists involved with the experimental work. The most useful discussions were with Ivanov on T-3, Razumova and Bobrovsky on TM-3, and Mukhavatov on T-6. Before describing the details of the experiments a general comment on the magnetics of TOKAMAKS is in order. We, at ORNL, have placed considerable emphasis on proper magnetics design in the ORMAK device. This emphasis has resulted both from my involvement in the ORNL toroidal multipole work and from the work of Kerst at the University of Wisconsin. Before Dubna, we could not understand the position of the TOKAMAK scientists with respect to magnetics. From the available literature, it appeared that none of the TOKAMAK devices incorporated any schemes to correct magnetic field errors that occur in a toroidal induction device with conducting walls. In discussions with the Soviets, it became clear that they recognized the existence of these magnetic errors but simply had no understanding of what to do about them. Both the ORNL and University of Wisconsin delegations explained the principles of magnetic corrections to the Soviets who appeared to be reasonably interested in

these techniques. It is not clear why more sophisticated magnetics are not available in Kurchatov. Perhaps it comes from being preoccupied with the difficult task of achieving their present successes on TOKAMAK or perhaps there are no real scientific discussions about the magnetics difficulties.

I was permitted to bring my camera into the laboratory but not permitted to use it. It turned out that some times cameras are allowed and some times not.

1. T-3 Machine. The T-3 device is the oldest, biggest, most poorly designed and most successful machine in the TOKAMAK family. I spoke primarily with Ivanov about the machine and only very slightly with Strelkov, Mirnov, and Gorbunov.

a) Mechanical and Vacuum. T-3 is sturdily built with heavy coils. I believe there is no vibration of the machine with the present coils although there was vibration with the earlier sets of coils. Robinson said the vibration problem with the scattering measurement stemmed from the laser vibrating when the main generators were energizing the coils. This difficulty was overcome by the simple technical but difficult logistics solution of a long concrete beam resting on firm spots in the floor.

Unfortunately the toroidal coil shapes necessary for TOKAMAK cannot be obtained (by the Kurchatov group) with hollow conductors, thereby removing the possibility of easy cooling of the coils. Consequently, they must wait twelve minutes between each 35-kG run to allow the cooling on the coil case to cool the coils.

The access to the machine is very limited, being a set of small (inch-size) ports in each of the two gaps located on the diameter perpendicular to the plane of the iron core. It is my understanding that the stainless steel vacuum liner is welded in place as the last assembly operation; this is a difficult weld and apparently is giving them trouble, another reason to go to T-4. These two gaps are also used for pumping. No longer are there Hg diffusion pumps on the system; ion pumps seem to be in evidence. The two gaps in the vertical plane parallel to (and through) the iron core are used for right-angle pumping of the space between

the liner and copper shell. These accesses are not available for diagnostics. A pulsed gas valve is used to allow hydrogen from a palladium leak source to back fill the chamber. Robinson mentioned that the pulsed gas valve is not exactly reproducible in its effect.

b) Magnetic and Electric. In terms of symmetry, each of the three magnetic fields, toroidal (B_θ), poloidal (B_ϕ), and vertical ($B_\perp$) leave much to be desired.

i. Toroidal field. The 16 B_θ coils are separated by 4-inch gaps. Ivanov gave us a detailed drawing of the T-3 coils so that now PPPL and ORNL have the information to calculate the exact field profiles. The on-axis B_θ ripple is about 1%, increasing significantly to about 10% toward the plasma edge. The 36-kG field is generated by 7 kA in the coils fed from a flywheel generator. We heard that at present only half the field is available because of a generator fault. The stray fields from the coil feeds are reduced by having the opposite polarity wires adjacent to each other. There is a 300-Hz ripple on the field but apparently this is unimportant as I have a note that something, presumably plasma behavior, is not strongly dependent upon B_θ varying less than 1%. Finally, the flat, circular plate at the top of the center iron pole was said to have been a non-useful attempt at a flux screen for the driving flux. It is now used as a platform for cables.

ii. Poloidal field. It is with the poloidal field that much of our concern has been attached and we were anxious to understand, in detail, the Russians' approach to this problem. On T-3, the primary plasma driving coils consist of two heavy coils on the center iron pole, above and below the chamber, and four small coils spaced on the four horizontal members near the corners. Ivanov said the center pole piece has a 2-mm air gap and although there is a few-turn back bias winding on the core, it has not been used. The core has two windows and is made of stacked laminations heavily bolted together. Inside the coils lies the eight-part copper shell

that carries the image currents arising from the plasma currents. This shell has the useful azimuthal cuts along the major circumferences (two concentric circles in the equatorial plane) and four cuts along minor circumferences. Two of the latter cuts are under the core and they are of negligible width but without the special precautions necessary to make an electrical joint. The other two cuts are at the diagnostic ports (along the diameter perpendicular to the core axis) and are each 10 cm wide.

I believe that their poloidal magnetic field geometry works in the following way. The eddy currents in the copper shell must be on both the inside and outside of the shell. This arrangement of currents primarily excludes flux from the shell itself and allows flux to fill the space between the plasma ring and the primary wires in a manner determined by the total magnetic circuitry. Unfortunately, the iron core is involved in this circuit which implies that as the plasma current is varied and the core flux density approaches saturation, the flux patterns will change considerably. This has specific application to the uncompensated gaps where the errors can change magnitude and direction; it is at these gaps that all the diagnostics are located. By virtue of the nonlinear magnetic circuit created by insufficient winding techniques it was necessary for the Soviets to make empirical determinations of the coil placement. We feel that our scheme of removing the iron from the circuit, by arranging judicious current paths, will aid in removing possible difficulties introduced by non-symmetric magnetic fields. Another possible consequence of this non-linear arrangement is the necessity of working at low flux densities (in the iron core) in order to avoid these very difficulties. Ivanov (and Artsimovich in Utrecht to G. G. Kelley) mentioned that the cores are run nowhere near saturation. With the iron utilized so poorly it is not possible to operate low-aspect ratio, high-field devices as we are attempting to do.

The primary coil has 80 turns and connections for a variation in turns ratio. The power supply is made up of two capacitor banks, the first 4 - 6 kV and the second 1 kV. The connections bringing the plasma driving current into the machine are widely spaced and well insulated from each other. Again, stray field from this mass of buss bars can be a difficulty.

iii. Transverse field. It came as a surprise that the small perpendicular field ($B_\perp$) appears to play such an important role in establishing the desired plasma conditions. This detailed information has probably saved us a considerable amount of time and concern. We understood beforehand the necessity of eliminating small stray fields as much as possible. But, in addition, Ivanov told us that the temporal profile of $B_\perp$ must also be programmed properly. There is, of course, the possibility that this information applies solely to the peculiar field geometry on T-3.

In any case, the requisite behavior of $B_\perp$ is the following: with the set of four external (to the copper shell) wires, a 100 g vertical d.c. field is established. Then, using the internal set of four wires, an 80 g opposing $B_\perp$ is pulsed on and the initiation of the discharge is made to coincide with the minimum field near 20 g. The vertical field is then returned to the original 100 g value in a time comparable with the buildup of the discharge. $\dot{I}(t)$ during this time is a programmed variable, although Ivanov made mention of no specifics saying only that the plasma regime is strongly dependent upon programming. (The TM-3 experience with this field apparently is that the programming of $B_\perp$ is less critical there.)

The maximum d.c. field producible through the 3/4-inch diameter external wires is 150 g. If the $B_\perp$ at initiation is greater than 30 g, the Russians cannot get a gas breakdown. When $B_\perp$ is programmed improperly, apparently runaways hit the inside of the limiter. Usually the limiter is only touched by the plasma on the outside.

T-3 is limited to four vertical field wires inside, even though Ivanov says this non-uniform field disturbs the plasma column; there is no room for more wires.

The actual placement of the inner set of $B_\perp$ wires is such that there are large areas having no pulsed $B_\perp$. The $B_\perp$ wires are in four sections corresponding to the copper shell quarters and end far from the edge of the shell. Ivanov said the gap in azimuth between the ends of the wires is about equal to the chamber diameter, an enormous error. They can only hope this error is not important but have no way of fixing it. In our scheme, this error is very small. The Russians do embed the crossover turns, connecting the various coils, in the copper shell, thereby minimizing that error. I have a not fully understandable note that the (8 kV) vertical field windings are in series with the main coil, presumably this refers to the external set of wires.

c) Miscellaneous

i. Limiter. The T-3 limiter is formed from two concentric wire circles wrapped together with thin (10-20 mil) Mo-plated stainless steel. This limiter lasts for over a year in the machine although it did appear as if the spot furtherest from the central axis did melt and reform from the plasma bombardment.

ii. Gas breakdown. Breakdown is accomplished in a simple, straightforward manner in all the TOKAMAKS. On T-3 only 40 volts are available per turn so that simple electric field breakdown is a chancy operation. An electron gun is used to produce a 200-300 eV beam that permits the electric field to break down the gas easily and reliably. This gun is placed in the shadow of the limiter and apparently works whether or not acceleration is applied to the beam. The dirtiness of the system appears to determine the ease of breaking down the gas.

iii. Laser measurement. The British scientists used a single wall, steel screen room and a center-tapped

electrostatically shielded isolation transformer to supply power inside the room to make the laser scattering measurements. Robinson mentioned a few of the difficulties encountered in the experiments. Since, in their scheme, any mirror or prism can act as a secondary source, the nearest (to the input window) piece of optics along the primary beam path is located 4 m below the machine. The high-power prisms also get pocked randomly, presumably because of undetectable internal flaws. The beam exit is a 1-m vertical pipe coated internally with absorbing black material ending in a Brewster window going to the beam dump.

d) Future. The time schedule for T-3 is supposed to be something on this order. The British have priority on its use until about the end of November. Then the Russians will work with it for a short period, maybe one or two months, then T-3 is to be taken apart and replaced with T-4. T-4 has a 90-cm diameter central iron core rather than the 100-cm T-3 core and a 58-cm diameter bore instead of the 62-cm T-3 bore. T-4 also is to have 55 kG rather than the 40 kG of T-3. Realistic estimates (from Mukhavatov) are for operation of T-4 about mid-1970. All we saw of T-4 was the coil arrangement without any wiring in evidence. It does seem clear that T-4 cannot have the improvements in magnetic structure that we have and that we discussed with the Russians.

2. TM-3. The TM-3 device is the newer, smaller, better-designed and less interesting of the two operating TOKAMAKS. I spoke with Razumova and Bobrovskii in detail about some of the magnetics of the device. When asked about magnetics, the TM-3 people relied primarily upon Mukhavatov's work on T-5 and T-6. TM-3 was built about five years ago but has the same type of magnetics in general as T-3.

The current feeds for the 24 toroidal field coils are nicely symmetrized to the extent that the connections between coils (and the adjacent return buss) are all double runs of buss. In fact, it was necessary to put vertical spacers in the iron core to allow for the extra room taken up by the two-fold current paths. The 2-cm thick copper shell is split along the

two extreme major circumferences (i.e., cuts are in and out) forming identical upper and lower halves of the torus. Our cuts are on the mean circumference, making an inner and an outer piece, i.e., cuts are up and down, and I asked Bobrovskii about the difference in the cut location. He again referred me to Mukhavatov who had done some sort of comparison experiment between the two schemes showing that their method gave better confinement. John Clarke and I decided on the following possible explanation with which Mukhavatov agreed. In the case in which the gap is properly compensated and eddy currents at the gap are truly radial it makes no difference to an expanding plasma ring where the azimuthal cut is. [If the plasma is undergoing a screw motion, the placement of the cut will determine the (minor) azimuthal angle at which the image field is distorted.] In the uncompensated gap, however, eddy currents are non-radial but presumably are symmetric about the hozizontal midplane. If this assumption is true, then an in-out cut as the Russians have would be on the symmetry plane and the non-radial gap currents would be unaffected. The up-down cut, on the other hand, would be a break in the current path, causing a further distortion of the gap field. One concludes, then, that there exists the possibility that distortion of the gap error field causes a worsening of the confinement properties.

I was given the impression that the TM-3 coils are to be improved shortly.

3. T-6. The T-6 device is the newest, nicest, best-designed and non-operating TOKAMAK. Mukhavatov is in charge of it and I spoke with him at length about magnetics in general. T-6 is nearly complete and is expected to be operational by January 1970.

a) Mechanical and Vacuum. T-6 has a number of innovations, primarily made possible because of the low toroidal field of 18 kG. In an attempt to bring the stabilizing shell closer to the plasma, the vacuum liner (bellows) has been put outside the copper shell which is gold-plated on the inside next to the plasma. The aspect ratio of this device is about three, the lowest yet in the TOKAMAK family. A nice touch is achieved by providing cutouts in two of the 32 field coils to allow tangential access ports, the first on a TOKAMAK. The coils have a cross section of 2 in. x 6 in., with a 2-ft. bore and are touching at the inner circumference.

[redacted]

The vacuum system consists of an aspirator to get to 20 torr, an adsorption pump, a turbomolecular pump (250 liters/sec), and an ion pump for holding at night when the turbomolecular pump is turned off. I believe I was told that the inner liner can be run between 77 ^{o}K and probably + 200 ^{o}C. Mukhavatov said that when the chamber was clean it was tough to start the breakdown; impurities made it easy. On T-6 he thought that possibly the field structure was good enough that there would be no problem in breaking down the gas without an electron gun. He plans for no preionization. Between 50 and 100 volts per turn will be available for $E_{toroidal}$.

b) Magnetics. There is a single window core on T-6 with 12-mil air gaps. The Russians have to work hard to get this small a gap as the factories apparently won't supply it this way. So, my understanding was that the TOKAMAK crew at Kurchatov has to re-work the core faces. The pole threading the torus is cruciformed (circular cross section) of 15-mil laminations with paper insulation between each 1/2-in. step. The various power leads are fed coaxially to cut down stray fields. There are four combined back-bias and plasma driving coils located horizontally at each of the four corner air gaps. These are the proper places to concentrate the back-bias mmf but again the poloidal field coils leave much to be desired. With the system of copper inside the vacuum bellows it will be a difficult job, if possible at all, to bring the eddy currents out around the pole through the bellows.

The perpendicular field is energized in series with the poloidal field, apparently with an external Helmholtz coil, as I recall.

Mukhavatov worked on the predecessor to T-6, i.e., T-5, on which he performed some symmetry experiments. He said that when 4 of the 32 coils were not energized (in two pairs of two) the q_{min} went up to 20 - 40% indicating a reduction in plasma performance. This is with T_e = 100 eV, a major radius of 62.5 cm and a minor radius of 25 cm. There didn't, however, appear to be much change between having one coil out and two separated coils out. These statements are not fully clear in my notes and did not,

[redacted]

at the meeting time, appear to represent well defined tests of symmetry since many other errors were present while the B_θ symmetry was being changed.

4. TB-1. This newest, smallest, most different machine is being constructed for the purpose of trying turbulent heating in a TOKAMAK. It is at present a double-window iron core and a 1/2-in. thick copper torus in two equal up-down halves with minor diameter about 5 inches and major diameter about 20 inches.

5. Moscow. Our visit to the secure Kurchatov Institute started off with a half-hour to one-hour wait in a cold bus outside the gates until the second bus (from the other hotel) arrived. After that we were given a choice of visiting any of the three divisions in fusion work there, and we were embarrassed for Golovin as nearly everybody wanted to see TOKAMAK and hardly anyone wanted to see his work. We had complete access to the entire TOKAMAK experimental area; the rest of the building was separated by doors with pushbutton combination locks.

John and I developed sufficient confidence to use the subway system to advantage and visited the Soviet economic achievement exhibition, particularly the atomic energy exhibit, with the up-to-date models of the fusion devices which we could take pictures of. Some of the subways were approximately 300 feet underground, behind blast doors, and all were immaculate.

B. CULHAM LABORATORY. My visit to Culham was unfortunately only a few hours long as I contracted some short-lived virus that sent me to the Culham infirmary for the afternoon. A word of thanks is certainly due the Culham clinic staff for their kind attention to me during my brief illness. We (John Clarke and I) spoke with Sidney Hamberger and Les Sharp about two topics, microwave diagnostic systems and the symmetric magnetic field production scheme Hamberger uses in his turbulent heating device.

Here I was able to use my camera and did take some pictures of both the apparatus and the microwave hardware. Hamberger's device produces a moderate toroidal magnetic field using the technique we adopted, i.e., a bottom mounted, coaxial feed , toroidal transformer. With his low fields, construction is not difficult but his experience with high current electrical joints is interesting. He fastens the 5-ft. circles together

with 96 brass bolts that carry the current across the joint whether he wanted them to or not; i.e., he felt that in a bolted joint, unless one went to some length the bolt would carry the current. Each of the mating circles is slotted in 96 places to allow individual adjustment of each joint. He also said his engineers were against using indium as a joint sealant on the grounds of their experience; no details were available at the time.

With regard to shielding electromagnetic signals in his experiment, Hamberger mentioned two helpful techniques. One, the low-frequency shielding with a center-tapped, well-grounded power supply to operate equipment inside the screen room, and second, the high-frequency shielding with optical coupling through the screen room for trigger signals.

We talked to Sharp and Hamberger about the difficulties of high-sensitivity, high-frequency microwave diagnostics. In our device, we had been confronted with the problems of working at 2-mm wavelength components. These small pieces are costly and lossy, combining to make an undesirable circumstance. Both the Culham and Kurchatov people get around both these problems by using overmoded waveguide, i.e., 3-cm wavelength guide to pass a multimode version of the 2-mm wave. Sharp told us that Culham had spent about 1 million dollars in developing, through the Royal Radar Establishment, techniques for construction of all size waveguide components. Hamberger thought that Culham might be interested in selling to the U.S. these specialized waveguide components, namely, the difficult tapered transition pieces from 2-mm to 3-cm waveguide. They use quasi-optical techniques, such as mirrors in the waveguide, to handle the high-frequency waves. We were also able to appreciate that, under the proper circumstances, the accuracy required in assembling these components need not be so high.

IV. GENERAL REMARKS

It is my opinion that the formal information transfer at the Dubna meeting as well as at the IAEA Novosibirsk (1968) meeting was small because the presentations were on the whole so poor. This remark applies primarily to the Russian authors but also to some non-Russians as well. Most of the Russians spoke at such a rapid rate that the interpreter's version was hardly comprehensible. Their slides were flashed on and off the screen in a few seconds, it seemed, and the slides themselves were

not legible, both from poorness of planning and execution. The content of many talks, too, was not well conceived and arranged and the occasional well-prepared talk was a joy to listen to. I would strongly suggest that some guidelines for oral presentation beyond the time limit be proposed for future international meetings. It seems to me to be terribly unfortunate that so little information seems to be transferred at the formal meetings which, after all, account for most of the time spent.

V. CONCLUSION

The Dubna conference was interesting and useful because I was able to participate by presenting two papers, get a comprehensive survey of the field, and also appreciate in detail a few relevant papers directly concerned with my present work. Formal information transfer at the two international meetings I've attended has been poor and steps should be taken to improve it. It was during the meeting that we got to meet some of the TOKAMAK people, at least formally, so that during the following week it was easier to approach them and make useful contact. The visit to Kurchatov made the entire trip worthwhile in real terms. We were able to find the answers to many specific questions on the TOKAMAK magnetics (they honestly are poor), gas breakdown (straightforward electron beam), operation ($B_\perp$ is critical) and many other detailed points. This information has caused and permitted some important changes in our design. There was apparently free flow of information between both sides, and I believe it is in our best interest as well as general scientific interest to maintain the contact with the Russian scientists. We look forward to their possible visit in 1971 at the time of the next international fusion conference.