rhodesiensis. Or, by those who regard these two fossils as specifically different from modern man, they may be designated as merely varieties of *Homo rhodesiensis*, the name originally applied to Rhodesian Man. Whatever the preference, either of these classifications would invalidate the terminology suggested by Drennan.

The taxonomic views of Singer (*1*) with respect to Saldanha Man are not entirely clear. It is evident, however, that he would not separate him from Rhodesian Man, for he states that the Saldanha discovery "confirms that the Rhodesian skull is no isolated, abnormal or pathological type of primitive man." This also appears to be the opinion of Sir Wilfrid LeGros Clark (5), who states that the Saldanha skullcap is "almost a replica of that of Rhodesian Man" and that its discovery is "important because it confirms the evidence of Rhodesian Man that there was a rather aberrant type of *Homo* in Africa at the end of the Pleistocene, and because it suggests that this type may have been in existence there over a considerable period of time."

An extensive stone industry, characterized by hand axes and other implements of an Acheulian type, and a large fossil vertebrate fauna were associated with the Saldanha skull. From the archeologic, paleontologic, and geologic evidence, as well as from results secured by the fluorine-dating method, it appears likely that Saldanha Man can be assigned to the early part of the Upper Pleistocene (*1*), at a time probably prior to the last glaciation in Europe (*5*). The skeletal remains of Rhodesian Man, on the other hand, were associated with a different culture. These artifacts, which include quartz flakes, round bolas-stones, and bone points and gouges, are in the Levalloisian tradition (*6*). Chemical studies (analyses of lead and zinc content) indicate that the human and other animal remains found at Broken Hill are approximately contemporaneous (*7*). The total evidence, while short of being conclusive, assigns Rhodesian Man to the Upper Pleistocene (*6, 8*) at a time that is probably equivalent to the Upper Paleolithic of Europe (*7*). If so, he would appear to be more recent than Saldanha Man.

As is noted in the preceding paragraph, the site of the Saldanha discovery is characterized by an extensive stone culture. In addition, two so-called "crude bone implements" were recovered. Interpreted as "fossilised bone chisels made by prehistoric man from the metacarpal bones of horse" (*9*), they have been regarded as particularly significant in being the first such implements found in association with the older South African cultures. These bone "chisels" have recently been studied by Singer (*10*). Actually they are equid metatarsals, of which the distal extremities have been fragmented. However, they are not identical in form, as was originally supposed, and, what is the most significant, they clearly exhibit furrows such as would be made by the teeth of carnivores. Bones of other fossil mammals from the same site also were found to exhibit manifestations of tooth marks or fragmentation which produced bizarre shapes resembling chisels, cleavers, and the like. Recent bones from a cave in Fish Hoek, which could not have been inhabited or frequented in recent times by man, show similar evidence of mutilation by the teeth of carnivores. Since various fossil Carnivora occur at the Saldanha site, Singer concludes that "there can be no doubt" that the so-called "bone chisels" or implements thought to have been made by man are actually only bone fragments originally chewed by carnivores and then subjected to weathering.

This study of Singer's (*10*) has implications that extend beyond any interpretation of the cultural capacities of Saldanha Man. It indicates the need for a careful assessment of the reality of other early, supposed bone tools, such as those of the so-called "osteodontokeratic culture" recently attributed by Dart (*11*) to the fossil Australopithecinae of Makapansgat—those early Pleistocene "man-apes" of South Africa.

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Mathematics in the Soviet Union

A. J. Lohwater

I had been living in Helsinki, Finland, for almost a year as a Guggenheim and Fulbright scholar when, early in June 1956, I was invited to give a mathematical address at the third Congress of Soviet Mathematicians to be held in Moscow beginning 25 June 1956.

Since the congress was the first such convention in more than 20 years, the organization committee decided, at almost the last minute, to invite about 40 foreign mathematicians, whose research was of current interest to Soviet mathematicians. All expenses for the foreign mathematicians, except travel, were to be borne by the Soviet Academy of Sciences; my travel expenses were paid by the Guggenheim Memorial Foundation. Norman Steenrod of Princeton University was the only other delegate from the United States; it is perhaps amusing to note that, until the congress in Moscow, we had not met each other.

General Characteristics of the Congress

The meeting was similar in character to a national meeting or convention of any one of our scientific societies; there were lectures to the general membership of the congress, special sections for research papers in the various specialties of mathematics, a business meeting of their mathematical society, scheduled entertainment of various sorts, and, of course, a banquet at the close of the con-

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gress. The foreign guests were given comfortable quarters in the Leningradskaya Hotel, a modern deluxe hotel catering mostly to guests of the state, while most of the Soviet members of the congress were put up in vacant student dormitories at the new Moscow State University, where the congress took place.

Following a reception for the foreign guests at the Academy of Sciences building, the congress was opened with ceremonies at the new buildings of the Moscow State University. After a welcoming address by Academician I. M. Vinogradov, each foreign country was introduced, through one of its delegates, and the warmth of the ovation given to the American delegation, both before and after it extended greetings from colleagues in the United States, made it appear, at that time, that we were attending the birth of a new era in cultural and scientific relations between the two countries.

Opening session of the congress. The American delegate extends greetings from his colleagues in the United States. For most Soviet mathematicians present, this was their first opportunity to see and hear an American mathematician. [Photo by Soviet newsman]

When the formal part of the opening ceremonies was over and the relations became more personal, the warmth of their welcome made itself felt. The Western mathematical literature is quite easily available in the Soviet Union, particularly in the larger mathematical centers, so that the names of many American research workers are well known there, just as we are acquainted with the publications of most of the more important Soviet mathematicians. We were continually asked about our American colleagues: what sort of people were they, for example, and with what sort of problems were they presently concerned. The Soviet mathematicians seemed to be particularly delighted to learn something personal about American mathematicians, something which made the Americans more human and more than merely names on research articles. Some of the queries indicated just how long certain Russians had been out of touch with the West; for example, I was asked what R. C. Paley was working on these days. (Paley, whose best known work is his book on Fourier integrals with N. Wiener, died in 1933). Many of the older Soviet mathematicians had been acquainted with various American mathematicians in the 1930's and naturally inquired about them; I was given many messages and letters to send to them as well as letters to relatives from whom they had heard nothing in years.

For me, one of the most important aspects of the congress was the opportunity to "talk shop" with my Russian colleagues whose papers I had read. This sort of intercourse forms the heart of all national and regional mathematical meetings in America, and I must remark that the facilities there to encourage this exchange of ideas were among the best that I have ever encountered. In the halls of the university outside the rooms in which research papers were being presented were tables and chairs, and a huge commons room was set aside for the use of members of the congress; a wide variety of free refreshments was available at all hours.

The conduct of the section meetings for contributed papers gave some insight into Soviet mathematics and indicated that the Russian mathematician is not very different from his American counterpart. Following each research paper, the meeting was thrown open to discussion, and the spirit of the mathematical discussions was very much the same as it is at American meetings. If a listener believed that a result was trivial or already in the literature—Soviet or Western—there were no inhibitions about bringing this belief to the attention of the speaker and audience, even if the speaker was an exalted member of the Soviet academy. It seemed to me that the free spirit of inquiry would have ridiculed out of existence any attempt to base some mathematical conclusion on political ideology; later conversations with Soviet colleagues brought out the amused remarks that such attempts had been made at various times but that the reactions of their colleagues had served only to accelerate the passage of such results together with the sponsors into mathematical oblivion. To be sure, since the Russian literature is that which is most available to the mathematicians of the Soviet Union, and since many new results are discovered simultaneously here and there, it is natural that, in many instances, the Western results are for a period unknown there and, consequently, are not referred to.

There are mathematicians in the Soviet Union, too, who attempt to further their own positions by a studied ignorance of the Western literature and by an obsequious nationalism regarding scientific priority; such scientists have their counterparts in all countries. I did not encounter this sort of nationalistic obsession among those mathematicians whom we judge to be of the first rank, and I think that the following episode illustrates the point which I have made. A mathematician who, in my judgment, has not made contributions of any significance, was giving a survey of results in the theory of univalent analytic functions. In the course of this survey he was attributing most of the progress to I. E. Bazilievich and the late G. M. Goluzin, who, to be sure, have achieved stature of the first rank for themselves for their contributions to this field. Both Cartwright of Cambridge University, for whom I was providing a running account of the talk, and I were extremely uncomfortable by the almost pointed avoidance of the spectacularly powerful results which had been achieved recently by W. K. Hayman, who is one of England's greatest mathematicians, and, at the end of the talk, we were about to point out this omission, when Bazilievich himself rose and delivered a 20-minute outline of Hayman's results and methods concerning the coefficient problem for univalent functions.

Quality of Technical Education

Despite the fact that I personally cannot understand the appointment of one or two mathematicians to the Soviet Academy of Sciences, I believe that Soviet mathematics has been particularly impenetrable to the political and authoritarian dogma which has seriously compromised certain fields such as biology. Indeed, great concern was expressed to me by dozens of mathematicians about the training of Soviet engineering stu-

dents. Acting on the assumption that the performance of a class of engineering students in their first-year mathematics course is a fairly reliable gage of the technical promise of the new group, I attempted to learn whether the reported increase in the number of Soviet engineering students had been accompanied by a corresponding decrease in the quality of their technical education. In America, of course, we have suffered certain growing pains since World War II, both in the public schools and in the colleges, and have witnessed a frightening drop in standards, both in the material taught in the schools and colleges and in the admission requirements in all but a handful of our colleges. Many university presidents and administrators have used the reported increase in the number of Soviet engineering graduates as a pretext to assert that, for patriotic and other reasons, we must enlarge existing university facilities to accommodate excessive numbers of students. I have seen it happen all too often that, in order to fill the new dormitories, the universities must resort to lowering admission requirements and soliciting students in the high schools.

I therefore took every possible opportunity to discuss with Soviet mathematicians the teaching of mathematics in the Soviet Union. I was particularly interested in the views of those who could compare the situation of today with that of 10, or even 20, years ago. Each of the professors to whom I spoke—and I discussed the problem with professors from about a dozen large technical institutes as well as from the large centers in Moscow and Leningrad—was alarmed by the calibre of student which the reduced admission standards had forced him to confront, especially in the last 10 years which have constituted the period of most intensive growth. I had a very interesting conversation with the head of the mathematics faculty in one of the largest engineering schools in Russia; he showed me the final examination given this past year to first-year students. In deploring the decline of standards, he asserted that one of the objects of the Ministry of Higher Education was the propaganda value of the mass production of technicians. It was not denied, however, that all of these technicians will be useful in the industrialization of the Soviet Union.

Although the demand for technically trained personnel has bloated the technical schools with its resultant effects on Soviet standards, it must be pointed out that the Ministry of Higher Education has been keenly aware of the effect on students of superior intellectual capacity of being presented an intellectual training geared down to the level of students who are either not capable or not interested in it, a problem which we are only now coming to recognize.

Banquet at end of the congress. Menshov (U.S.S.R.) (center) tells anecdote to Lohwater as G. Kurepa (Yugoslavia) tries to listen. On far left is the delegate from the People's Republic of China. [Photo by Soviet newsman]

Of all the differences in American and Soviet education, that which impressed me most, I found, was the appreciation there of the really gifted student, who is sifted out and not committed to the stultifying process of the huge educational production line. Such a student comes early under the influence of the greatest scholars in his field, and he forms with his teachers a relationship similar to that in the old German universities. The Soviet Union is producing several exceptionally powerful young men; for example, in my field (complex variables), a young man in his twenties, A. A. Goldberg, one of several excellent students of L. I. Volkoviskii of Molotov State University, has given a remarkable solution of a famous problem of Nevanlinna concerning the defective values of a meromorphic function. I felt that Steenrod had the same impressions concerning the training of research workers in the field of topology; in particular, he was deeply impressed by the work of the young topologist, M. M. Postnikov.

Steps toward an Exchange Program

On the evening of 27 June many of the foreign delegates were guests at a reception held by V. P. Elyutin, the Soviet Minister of Higher Education. There were about 100 people present, including many members of the Soviet academy from fields other than mathematics. During the last hour of the reception I was asked to join a group of people consisting of Elyutin and several members of the Soviet academy for the purpose of formal toasts before some Soviet reporters and photographers.

As an individual, Elyutin was quite friendly; however, as a politician, he seemed more immune to the cordial spirit displayed by everyone else we encountered at the congress, and he seemed to feel that he had an obligation to his press to confront me with the shortcomings of American foreign policy. The nature of his first question—whether I had seen the highly publicized Iron Curtain on my train ride down from Helsinki—prompted me to abandon the use of Russian during the interview and to make formal use of an interpreter so that no misunderstandings might arise. Following this line of questioning, he asked me how long it had taken me to obtain a visa at the Soviet embassy in Helsinki and whether it had been necessary for me to submit to the humiliation of being fingerprinted. We discussed the concept of fingerprinting in great detail, from its widespread use in America as a means of identification of criminals and government employees to the general revulsion throughout Europe against its use as a requirement for an American visa. When this topic had been exhausted, Elyutin expressed the hope that our visit, and that of the American physicists, marked the beginning of a fruitful exchange of scientists. He remarked that a limited student-exchange program was being contemplated, to commence in the fall of 1956, but that, unless the State Department rescinded its fingerprinting requirement, he would announce the cancellation of the exchange program before the first of September. (On my return to the United States in August, I read of the collapse of the contemplated exchange of the students.)

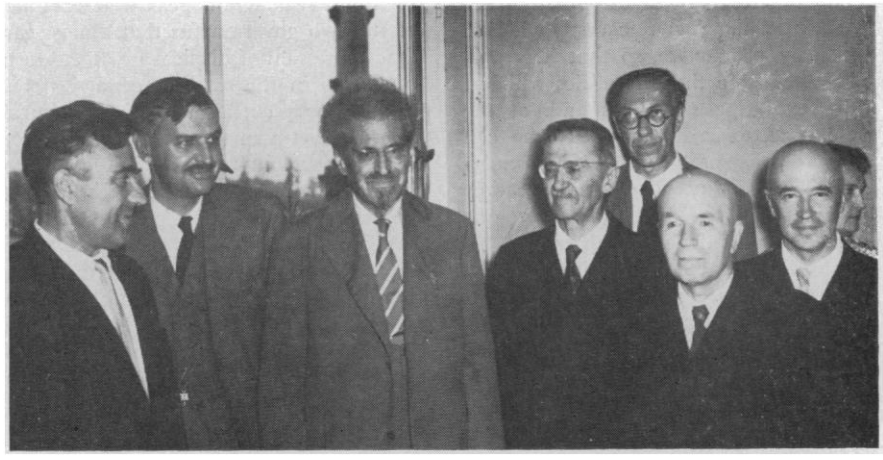
On 29 June I was guest of honor at a luncheon held by Academician M. A. Lavrentiev at the Praga Restaurant in Moscow; the purpose of the luncheon was to discuss details of the possible fu-

ture exchange of mathematicians between the United States and the Soviet Union. In addition to Lavrentiev there were present Academician A. I. Markushievich and L. I. Volkoviskii, P. P. Kufarev, and P. P. Belinskii.

A friendly and encouraging atmosphere prevailed, partly because all of us have conducted research in the same fields and were well acquainted with the work of one another, and partly because of a genuine desire on both sides to initiate a period of increased intercourse between the mathematicians of both countries. It was agreed at the outset by all of us that an unfortunate set of relations prevails between the U.S.S.R. and America and that the hopes of our discussion would not be furthered if we were to spend our time attempting to fix the blame for the political situation. Instead, we agreed to define those problems whose solutions would contribute most to our common goal, namely, the earliest possible successful exchange of our mathematicians.

I was careful to make it clear to them that I had with me no authority to speak officially on behalf of any American organization, a fact which was known to them at the time of my arrival in Moscow. We then reviewed our conceptions of the problems to be encountered in issuing invitations. On the Soviet side, the problems are simpler, because the decision of the Soviet Academy of Sciences to invite an American scientist implies the approval of the Soviet Government, so that a formal invitation from the academy would be sufficient for an American to obtain a Soviet visa. The problems are considerably more complicated on our side. We must first persuade some institution—a university, scientific society, or philanthropic society—to provide the funds in order to substantiate such an invitation. With this part of the problem, we do not expect insurmountable difficulties. The difficult part of the problem appears when the institution sponsoring the invitation seeks to persuade the State Department to issue a visa so that the foreign scholar may enter the United States. It was almost impossible, I pointed out, for me to describe adequately the difficulties to be encountered with this second part of the problem. I mentioned that one of our principal difficulties with this part of the problem was trying to discern some sort of consistent policy. They asserted that they were as well aware of this facet of the problem as I and that the fingerprinting requirement was only one of the barriers to be overcome.

As a possible means of overcoming a part of this problem, it was suggested that each side proceed, although it was not to be expected that both be successful simultaneously in the effort to extend invitations. If, as seemed probable, the Soviet academy were the first to obtain official approval to extend an invitation, it should proceed to do so, for the effect would be to stimulate the efforts being made on our side.



At the reception given by V. P. Elyutin (Soviet Minister of Higher Education). Left to right: Elyutin, Lohwater (U.S.A.), F. Severi (Italy), A. Denjoy (France), and M. A. Lavrentiev, I. M. Vinogradov, and I. G. Petrovskii. Petrovskii is rector of the Moscow State University. [Photo by Soviet newsman]

We then discussed what could be expected in the matter of expenses for the visiting scholars. I was told that the Soviet academy would either provide or pay for transportation to America and return for whomever we invite but that all other expenses inside America should be provided by us. Conversely, an American scholar who is invited to the Soviet Union will be expected to get there at his own expense, but that, once there, he will be provided with everything else. I remark, parenthetically, that if the level of hospitality shown to Steenrod and me during our visit is an indication of what our next man is to expect, let us hope that he has a strong constitution!

The nature of the activities of the scholars under the exchange was then brought up. It was asserted that, as things stood then in the academy, it would not be convenient for an academician to visit for more than a month or two, a remark which had been made to me at Elyutin's reception by other officials of the academy. Thus, in its first stages, the exchange is anticipated to be no more than a series of lectures at the research level, rather than in the nature of visiting professorships or research study grants which could come later, if and when the scope of the exchange program is enlarged. They mentioned the names of several American mathematicians in whose work they were interested; I presume that these will be among the first to be invited.

It appears to me from the discussion at this formal meeting, as well as from other conversations, that we are expected to invite, if only in our first invitation, someone of academy stature, and I feel convinced of the need for observing this protocol. In mathematics, to be sure, a large part of the research is produced by relatively young scientists, and, from our observations there, the situation in the Soviet Union is no exception to this rule. Now it is natural for us to want to hear from those mathematicians who are at the height of their creative periods; on the other hand, their assertions to the contrary, the young do not have a mo-

Luncheon held at Praga Restaurant to discuss details of exchange of mathematicians. Left to right: A. I. Markushievich, L. I. Volkoviskii, P. P. Kufarev, Lohwater, P. P. Belinskii, and M. A. Lavrentiev.

nopoly on creativity, and there are many members of the Soviet academy who are extremely productive, so that it is possible to observe the formalities which I judge are expected of us and, at the same time, to invite someone who can offer us considerable scientific stimulation. A possibility which might satisfy both points of view is that we invite one or more academicians together with an equal number of their gifted young mathematicians.

Since the congress last summer, the upheavals in eastern Europe have changed drastically not only many of our conceptions of Soviet-American relations but also the Soviet conceptions of these relations. It does not speak well of either of the two governments that almost the first expression of displeasure at the turn of events in the satellite countries was the summary curtailment of all cultural relations between the two countries. For if it is our long-range intention not to live in perpetual hostility to the Soviet Union—and the logical consequences of the hypothesis of perpetual hostility are obvious—then it appears to me that each side has struck at the best means of implementing its long-range intention. It was my impression that the longing of Soviet scientists and other scholars for more contact with their Western colleagues is the most important fissure which has appeared in the Iron Curtain, in the sense that the proper exploitation of this fissure can lead to pressures for change inside the Soviet Union by a group of people of considerable prestige and influence: the sort of pressures which we have asserted would bring about those changes for which we have been hoping. In my view there exists an urgent necessity to replace and to enlarge the basis for the scientific and scholarly contacts at all levels, but at the very least, there exists the urgent necessity of examining and discussing our objectives in the general field of the cultural and scientific relations between the United States and the Soviet Union.

L. M. Terman, Pioneer in Mental Measurement

When Lewis M. Terman died, near the end of his 80th year, he was working on the manuscript of volume 5 of *Genetic Studies of Genius* and was simultaneously planning the next 3 years' research on his group of 1500 "gifted children." No other facts could mark the man so well. From 1903, when he arrived at Clark University to begin graduate work, until his death, his career was a continuous sequence of research and writing, broken now and then by illness and accident but never interrupted in its main course.

At the turn of the century, the idea of measuring human abilities was little more than a dream. To be sure, E. H. Weber had measured sensory thresholds long before, and the "new psychology" of the 1880's and 1890's had isolated such simple behaviors as discrimination and reaction time. But a man's performances in these molecular processes, it was quickly apparent, were quite unrelated to his performances as a thinking, reasoning, judging, creating human being.

A new approach was in the making, however. In Paris, Binet had discovered how to distinguish bright and dull pupils with a test composed of difficult "school-type" questions. At Teachers College, Columbia University, Edward L. Thorndike had developed tests for measuring school achievement. These efforts caught Terman's imagination at the very beginning of his graduate work. He had had sufficient experience as a school teacher himself to know of the great differences among children in their capacities for learning. He knew, too, from his own early experiences in an intellectually arid farm county in Indiana, that high talent is likely to be wasted for want of recognition and encouragement. He thought that such wastage was shameful, a tragic loss to society of its most valuable resource. He conceived of social progress as dependent on how rapidly, and with what economy, intellectual giants—"geniuses" as he later called them—could reach their maximum development and produce their great ideas. He devoted his career to developing methods of measuring intellectual ability and to discovering the qualities of those who are most gifted. In a century that has concerned itself strongly with the remedying of evils—illness, poverty, inequity—and in a science that seems, perforce, to have had to orient itself too often toward the care and understanding of the weak and inept, Terman turned resolutely toward the positive side of man's existence. As a student of the intellect, his interest in feeblemindedness was perfunctory, his zeal for the study of genius, burning.

The half-century since Terman finished his doctoral training is almost coincident with the history of mental testing. So is Lewis Terman. From the first, there was something provocative and exciting to him in the very idea of measuring complex psychological qualities. His doctoral dissertation was a comparison of seven bright and seven dull schoolboys. He gave each child a battery of more than 40 hours of individual tests, probably the most overwhelming test-assault inflicted on any child up to that time. Nothing much came of the study, but it did give Terman a chance to try his hand at making up tests. He loved it, and he went right on loving it to the end of his life. He built test after test for 40 years, all of them good (technically) and nearly all of them useful.

His first major venture was the revision of Binet's intelligence test. He began this task when he went to Stanford University as an assistant professor of education, in 1910. The Stanford-Binet test, as he called it, became at once the standard intelligence test for use in schools and clinics throughout the United States. His selection of a score for the test—the IQ—proved immediately popular. As a simple measure of the influence of Terman on modern society, try to imagine eliminating the IQ from our language!

Although Terman received his doctor's degree in both psychology and pedagogy, he was perhaps more closely associated with the latter than with the former in his early years at Stanford University. He was in the School of Education. The department of psychology was tiny, experimental in the Wundtian sense, and, with respect to research, essentially moribund. When he published *The Measurement of Intelligence,* in