

RESEARCH ON RESEARCH AND SOME PROBLEMS OF RESEARCH BUREAUCRACY

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by

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Ideo-centrism in the sociology of science

The sociology of science was born and reared as a child of the sociology of knowledge which represents one of the most sublime and least down-to-earth branches of sociological study.

These lofty origins and family traditions of our discipline are still reflected in the attitude of many eminent sociologists of science. Thus, Bernard Barber characterizes the sociology of science as "one part of the sociology of knowledge ... the part that specializes in defining the nature of scientific ideas and in describing their relations both to other kinds of ideas /e.g. ideological, philosophical, aesthetic, religious/ and to various institutional and personality factors".^{1/} In other words, scientific ideas and their various spiritual, social or person-bound relationships ought to be regarded as the central subject matter of the sociology of science.

Those who share this ideo-centric interpretation of the subject matter of our discipline, show a distinct preference for the investigation of such noble and high-minded problems of the sociology of science as the cultural contexts of science, the interrelationship between various idea systems /in the Parsonian sense/, patterns of serendipity in scientific discovery, the symbolic and instrumental uses of scientific knowledge, and the like. On the other hand, they have relatively little interest left for the sociological study of the shadier sides of scientific life, such as the dirty business of actually doing research and the still dirtier business of getting research done and funded.

Barber's very valuable and well-conceived article on the sociology of knowledge in the International Encyclopedia of the Social Sciences, which we have quoted above, represents a prototypical expression of the ideo-centric position and ^{was} many of its consequences. We may learn a lot from it about the social nature of science, about cultural differentiation as a favorable factor in the development of science, about science as a social problem, and so forth. However, research

1/ Bernard Barber, The Sociology of Science, in: International Encyclopedia of the Social Sciences, New York: 1968, Vol. 14. p.92.

as a scientific activity is only casually mentioned in this discourse. Everything turns around scientific discovery which is the ideal outcome of certain creative processes among which we may count also successful research activities. But even the broadest treatment of scientific discoveries cannot replace the study of research as such which is of paramount interest to the sociology of science -- even if unsuccessful and given up in despair or because of the lack of means. Still, one must read half through the 8 quarto-pages Barber wrote about the main topics of the sociology of science till even the word "research" occurs. Not a single reference can be found to the professional researcher as an important subject to be investigated by the sociology of science. In the fairly long section on the motivations and rewards for scientists not the slightest allusion is being made to the fact that nowadays a huge number of people get paid, and sometimes even quite well paid, for doing research and that money may perhaps play some role as a motivation and reward for people involved in scientific research.

A colleague from Mars, who read Barber's article in order to prepare himself for a study tour in the terrestrial scientific community, might be caught by surprise when ^{he} learns after his arrival on Earth that research costs money on our planet, that we have research budgets and research contracts, research institutes and research firms /governmental, academic and industrial, profit and non-profit/, research directors and research staffs, nay, even a well-developed research bureaucracy. As Barber studiously avoided to discuss such lowly and insignificant ingredients of terrestrial scientific life in this review of the subject matter of the sociology of science, how could the good man from Mars know about them? We can understand his surprise when he looked around after the arrival of his spaceship at Cap Canaveral.

Jokes aside, I have naturally the greatest respect for the truly broad-based scholarship and the outstanding achievements of Bernard Barber as a sociologist of science. I am also sure that he is personally as much aware of the relevance of the material and operational aspects of research to the sociology of science as anybody else. Besides, there are quite obvious limitations imposed on an author who has to present the gist of a scientific discipline on the few pages reserved for it in a fairly general purpose encyclopedia.

I singled out Barber's article merely because it presents in a very clear and compact form a typical conception of the sociology of science that is still rather prevalent in America but much less so in Western Europe. It has nearly vanished in that part of the world from which I happen to come.

The root of the ideo-centric position is very expressed in Barber's definition of the sociology of science where he states that "the sociology of science is one part of the sociology of knowledge".^{2/} This statement implies that the field of the sociology of science lies wholly the wider and more comprehensive field of the sociology of knowledge.

Now, it is true and we have even stressed it ourselves that -- historically speaking -- the sociology of science was born and reared as a child of the sociology of knowledge. What Barber and the ideo-centrists fail to see and to acknowledge, is the fact that the sociology of science has since long reached adulthood and has outgrown by far the limits of the sociology of knowledge. We might even say that today the center of gravity of the specific field of studies in the sociology of science lies outside the domain of the sociology of knowledge. In other words, the sociology of science has become a self-sustaining discipline the field of which is overlapping to some extent that of the sociology of knowledge. But it has also many disciplinary concerns of its own and is investigating a huge array of matters that have little or nothing to do with the sociology of knowledge. To maintain that sociology of science forms simply part of the sociology of knowledge is about as accurate as to say that meteorology as a discipline forms simply part of the kinetic theory of gases.

The importance of research on research

If we take a world-wide view of recent developments in the field of the sociology of science, it becomes soon obvious that an ever growing and as of now already preponderant part of the studies in this field are not primarily concerned with scientific knowledge as such, nor

^{2/} Barber, loc. cit.

with the institutional relations of science in general, but rather with the socio-economic process by which the bulk of new scientific knowledge is being produced and is getting applied in our days, and with the material and organizational foundations of this very same process.

Briefly, research on research -- especially research on the objective socio-economic, organizational and operational aspects of research institutionalized and professionalized activity -- has become very central to the sociology of science in our days. It is hardly an overstatement to say that it was research on research that made the greatest contribution to the recent development of our discipline.

Research on research touches upon many problems which have little if anything to do with the sociology of knowledge, however generously we may define the boundaries of this venerable branch of sociological scholarship. To name only a few examples from our little parable about the visitor from Mars: research contracts, research firms, industrial research institutes, research directors, research staffs and research bureaucracies are certainly not topics of direct interest to the sociology of knowledge. They are, however, of most immediate concern to research on research and the whole sociology of science.

We have already referred to the curious fact that Barber's encyclopedic article on the sociology of science makes little use of the word "research" and avoids to discuss, partly even to mention, such matters as the existence of professional researchers and research establishments, the role of research as a branch of governmental, academic, industrial employment and military service, as a sector of public and private investment and -- last but not least -- as a business enterprise. From the ideo-centric position, research appears to be interesting mainly as an intellectual process producing scientific ideas and discoveries.

It is certainly true that the sociology of knowledge and the sociology of science have an overlapping interest in the study of this intellectual process. However, the intellectual aspect of research activity is by no means the only one that the contemporary sociologist of science has to investigate. There is no good reason to attribute some general priority to it.

And here we come to speak on the most important omission in Barber's encyclopedic review: - an omission that is highly characteristic of the bias introduced by ideo-centrism, especially by its American variety,

In the late thirties John D. Bernal's pioneering work, "The Social Function of Science", opened up a new page in the sociology of science.^{3/} When we speak of his trail-blazing achievements, we have not so much his somewhat naive though still original applications of some marxist principles to the study of the past and present of the scientific establishment on our mind. We refer here to Bernal rather as a founder of empirical research on research and of the data-based study of organizational networks, material embodiments and operational mechanisms of contemporary science.

Bernal was the first to collect, collate and evaluate a huge **body** of national and international data and statistics on the number and distribution of researchers and ^{of} other kinds of scientific manpower in various branches of science /giving even attention to their distribution by sex/, on governmental, industrial and war research expenditure, on quantifiable outputs of research activity, such as publications and economies affected in industry as a result of research, on various national patterns in the organization of science and on the hierarchy of research institutes and research staffs. Bernal's appendices attached to his great book are still indispensable data sources for research on organized research and on the state of the scientific establishment before World War II; his tables provide a base for most important time-series data used in contemporary studies on the development of "big science". Vannevar Bush's famous Report on a Program for Postwar Scientific Research to the President of the United States, better known under its book-title "Science: The Endless Frontier", uses Bernal's data collection as a starting point for calculations and projections which are of high interest to the sociology of research.^{4/}

As a matter of fact, John D. Bernal and Vannevar Bush can be regarded as the founding fathers of modern quantitative research on research in Britain and in the United States, respectively. Barber makes no reference to their work and does not even list "The Social Function of Science" and "Science: The Endless Frontier" in the

3/ J.D. Bernal, The Social Function of Science, London: 1939.

4/ Vannevar Bush, Science: The Endless Frontier, Washington, D.C.:1945.

bibliography of his encyclopedic article on the sociology of science.

Different trends in European and American
social studies of science

Bernal's initiative in the field of "Science of Science", as he preferred to call it, exerted a lasting influence on the development of the sociology of science in Britain and in the whole of Europe, the socialist countries included. He is still one of the most quoted authors in European literature on the sociology of science. The Science of Science Foundation was formally inaugurated in London in October 1964 on the occasion of the twenty-fifth anniversary of the publication of his classic book. In quite a number of countries much of the activity in the domain of the sociology of knowledge is being displayed under the flag of "Science of Science" /"Naukovedenie" in the Soviet Union, "Naukoznanstwo" in Poland, etc./ The heart of this science of science is, of course, research on research.

In the United States things worked out in a different way. Not too many sociologists of science followed in the footsteps of Vannevar Bush and certainly few of them cared to read Bernal. Probably due to the prevalent ideo-centric conception of the sociology of science, the empirical investigation of the hard facts and material bases of contemporary science organization, science policy and research practice was left to a great extent to political scientists, economists, management scientists and last but not least to historians of science who had their own good reasons to study these topics. As a consequence, American sociology of science has to thank for many of the sociologically most relevant and empirically best validated insights into the workings of contemporary science to scholars who never belonged to the august guild of the professional sociologists of science but did the job of these sociologists just as a subsidiary occupation in the pursuit of their own professional tasks, like Derek de Solla Price, the historian of science, to name only one prominent example. Price's ingenious and fruitful efforts to develop measures for the growth of science and research activity repaid so to say to our discipline the debt which the history of science owed to the sociology of science for Robert

Merton's masterly sociological studies on 17th century science.

We may add here that in a very refined and subline form the ideo-centric bias seems to affect even Merton's thinking. On the occasion of the death of Paul Lazarsfeld, the New York Times reported a few eulogistic words which Merton said about this recently deceased great pioneer of mathematical methods and models in sociology: "He /Lazarsfeld/ used numbers in a humanistic way. He was not a technician. He used numbers to arrive at ideas." Had our deceased friend heard this remark, he might well have returned with an impish smile: "Do you know how many ideas it takes to arrive to numbers? And by the way, what's so wrong with technicians?"

The problem with ideo-centrism in the sociology of science is of course not that it directs the sociologist towards the study of scientific ideas and ideational processes in science. These are, naturally, all-important subjects of investigation. The problem is that ideo-centrism, especially in its more extreme forms, tends to draw away the sociologist of science from concerns with non-spiritual aspects of scientific life. It tends to narrow down the field of the sociology of science so that the material demands of society on science, the investments of society in the scientific establishment, the bed-and-board questions of research activity, the hard realities of national and industrial research policies, etc. become matters of peripheral interest sociological investigation and are left to a great extent to the care of other disciplines.

Lessons of an international symposium and some peculiarities of the American sociology of science

At this point I am reminded of an important international symposium organized in 1967 by the Science of Science Foundation in London. It was probably the last major scientific meeting in which John Bernal took an active part. Decision-making processes in national science policy and in research policy at the industrial level, the evaluation of research productivity and of the scientific potential of given research organizations were among the main topics to be discussed.

One should think that such topics are of a major interest to

sociologists of science and indeed there were a number of sociologists among the participants of the symposium. However, of the three scholars who came from the United States and presented magisterial papers at the meeting, none was a sociologist. Russel Ackoff, Director of the Management Science Center at the University of Pennsylvania, demonstrated a new operations research approach to problems of national science policy and set forth a comprehensive conceptual model of socio-economic inputs and outputs of science and technology. William D. Carey, Assistant Director of the Bureau of Budgets in Washington, D.C., analyzed the decision-making process in United States Government in respect to the planning and administering of national science policies and research programs. Alvin Weinberg, Director of the Oak Ridge National Laboratory, reviewed a number of such novel problems of the sociology of science as the two-fold role of the scientist as a researcher and an administrator, the rivalry between the academic and the "mission-oriented" governmental or industrial laboratory, the administrative constraints put on free basic research, and so forth.^{5/}

It was Raymond Aron who summed up the discussions of the four-day meeting. Speaking as "an observer of the social drama of the symposium", he referred to a "fundamental difference between the way science policy functions /a/ inside the Western world, between France, the United Kingdom and the United States, /b/ between the Western world and the Eastern countries, and /c/ between the industrialized nations and the non-industrialized or poorly industrialized nations". He made also some very keen observations on the different ways in which political and socio-economic problems affecting science and research are being approached by scholarly investigators in the United States and elsewhere.^{6/}

During the intermission that followed Raymond Aron's summary, a number of scholars who attended the symposium gathered around Aron for a chat. As far as I can remember, C. Freeman of the University of Sussex, H. Krauch from Heidelberg, A. Rahman from New Delhi, R. Cardon from

5/ Cf. Anthony de Reuck, Maurice Goldsmith and Julie Knight /eds./, Decision Making in National Science Policy: A Ciba Foundation and Science of Science Foundation Symposium. London: 1968.

6/ Raymond Aron, Applying First Principles, in: de Reuck - Goldsmith - Knight /eds./, op. cit., pp. 282-292.

Buenos Aires, G. Dobrov from Kiev and myself were among those participating in this conversation. It became soon apparent that almost all of us were somewhat puzzled by the fact that no American sociologist happened to join us in this rather timely international meeting concerned with the study of socio-economic and policy problems of contemporary science and research. We were naturally most impressed by the admirable presentations of such outstanding American scholars as Ackoff, Carey and Alvin Weinberg whose papers contained many considerations of highest relevance to the sociology of science and who seemed to be involved themselves very strongly in research on research. But where ^{remained} the American sociologists of science? None of us had a ready explanation to this question.

Then and there I began to suspect that there may exist an influential trend in the American sociology of science which concentrates the attention of American sociologists too much on questions concerning the social context of scientific ideas and reduces their sensitivity to the bread and butter problems of science in a world where the decision-making about the tasks of the scientist, including the research tasks he has to undertake, has slipped out to a very great extent from his hand.

I had subsequently the great privilege to work for a number of years as a researcher and an academic teacher in the United States. Needless to say that this experience - rare for a Hungarian sociologist - reinforced my deep respect for the work of my American colleagues and for the monumental contribution of American scholarship to the sociology of science. Nevertheless, I found my suspicion confirmed to a considerable extent by personal contacts and discussions I had with many members of the American sociological community.

As I think, the prevalence of ideo-centric tendencies and interests sets the American sociology of science somewhat apart from the sociology of science in other parts of the world. There is nothing inherently wrong about this separateness. It takes all kinds to be a sociologist of science and why shouldn't there be an American variant of this species? However, the feeling of being apart and being different from the rest, should certainly not lead to self-sufficiency or to the delusion of being alone in the mastery of a whole field of knowledge and of having achieved ^{alone} everything worth mentioning in it.

Reading American books and papers on the sociology of science, one

might sometimes get the impression that this whole discipline must have been invented in America and does not even exist anywhere else. Even very illustrious American colleagues may produce occasionally paradigmatic examples of such national solipsism in their writings.

As you may have guessed by now from my quotations, I happen to have in my library a copy of the huge International -- repeat: International -- Encyclopedia of Sciences, published by an American publisher. Now, believe it or not, in this 17-volume encyclopedia neither the 25-item bibliography on the subject of the sociology of science, nor the 27-item bibliography on the subject of scientific communications and the 22-item bibliography on scientists, their career mobility, their incentives, their productivity, etc. list a single book or research paper written by anybody else but an American author, or at worst -- in a few cases -- by a scholar from abroad who has spent much time in American academia and has become accepted by it. All these bibliographies form part of encyclopedic articles authored by such eminent American sociologists of science as Bernard Barber, Warren Hagstrom, Norman Kaplan and Norman Storer. The works of all of them are so much appreciated and quoted by sociologists in other parts of the world that they simply must have known about the fact that some little sociological research on science, on scientific communications, on the incentives and the productivity of scientists, and the like, is being done outside of America, sometimes maybe not even completely without appreciable results...

Naturally, it was not some conscious prejudice or a lack of information about non-American social studies on science that resulted in the onesidedness of the bibliographies presented by these highly competent and knowledgeable authors. We could have chosen many other examples from American sociological literature to demonstrate similar symptoms of a certain self-centeredness and self-sufficiency that can be observed in some sectors of American sociology.

We may leave it open to what extent positive factors, like the great riches and the huge volume of autochthonous American sociological research and literary production, or negative factors, like the lack of interest in learning foreign languages and in reading scientific literature published in other languages in English, might have played

a role in the development of a fairly widespread indifference among American sociologists of science towards the work of their colleagues abroad. This indifference resulted occasionally even in the neglect of some new and rather challenging problems which have been much studied by non-American sociologists of science but did not fit well in- to the traditional American conception of what the sociology of science is all about.

By going so far in my criticism, I have perhaps taken an unfair advantage of the rights of a guest. My American hosts may now turn the tables and ask me the pointed question: Why don't you sweep before your own door? Is there nothing to say about failings and weaknesses manifesting themselves in the work of Eastern European sociologists of science? To this I can only answer that there is certainly a lot to sweep before our own door and we are trying hard to tidy up. Therefore, we would welcome if you helped us by any substantive criticism of our work. However, there is one fault we have never committed, even in spite of the relative smallness of our "door". We have never failed to take notice of the research done and the progress achieved by sociologists of science working abroad, in the United States or elsewhere. Neither we, nor our Western European colleagues for that matter, can be accused of having neglected to take cognizance of the achievements of American sociologists of science, or of having shown an indifference towards their work. Nothing hurts more the chances of international cooperation in a field of scientific endeavor than indifference and neglect. This is the main reason why I brought up this whole topic.

Fairness prompts me to say that it was one of the great men of American sociology of science who recognized among the firsts the urgent need for more and better international cooperation in our discipline. It was Robert Merton who called to life the Research Committee on the Sociology of Science in the framework of the International Sociological Association and became ^{also} its first President.

I must ^{also} acknowledge at this point that in recent years a most welcome change has begun to assert itself in the American sociology of science in respect to just those tendencies on which most of my criticism was directed.

It suffices to take a look at the Program of the First Annual Meeting of the newly founded Society for Social Studies of Science /"43"/ to see how the popularity of down-to-earth research on research has grown among American sociologists of science. Topics like the comparison of the organization and performance of research units, initiative-taking in research policy, etc., were in the past not too often discussed in general meetings of sociologists of science in the United States. It is also good to see that comparative sociological studies are now being undertaken in cooperation between American and European centers involved in research on research. In spite of the strong development of cross-national comparative social research during the last twenty-five years, international or cross-national projects were very rare in the field of the sociology of science. Finally, the fact that the Society for Social Studies in Science included in this Program several scientific sessions co-sponsored by 4S and the ISA Research Committee on the Sociology of Science, and granted hospitality to a number of scholars coming from countries as far away as Eastern Europe and the Soviet Union -- well, this certainly demonstrates an increased interest of our American colleagues in the international perspectives of our discipline and in the cooperation with sociologists of science belonging to different parts of the world -- an interest which we fully reciprocate.

The creation of the Society for Social Studies of Science may well have beneficial effects also on American sociology of science as such. We have commented earlier on a certain trend among American sociologists of science to devote too little attention to the hard facts of contemporary scientific life, to the material growth and the increasing material needs of science, to science policies and actual research practices. We criticized that our American colleagues tend to leave the study of such topics to a great extent to duly interested political scientists, economists, management scientists and last but not least to historians of science. Now all this crowd of duly interested American political scientists, economists, management scientists and historians of science is right here, together with the American sociologist of science, in this newly founded Society for Social Studies of Science. This togetherness and the increased interaction it involves will surely widen the professional outlook of all scholars who take an active interest in this "interdiscipline" called social studies of science.

Considerations on R & D and research bureaucracy

May I now come to speak on a subject which has as yet been too little investigated by sociologists of science all over the world although it seems to pose very considerable problems in contemporary scientific life. What I have in mind is the development and the role of research bureaucracy.

Bureaucracy as such is of course a phenomenon to which sociologists have turned much attention since Max Weber's times. There is also a wealth of sociological literature on various types of bureaucratic organizations and quite an amount of research has been done on the bureaucratization of governmental, municipal and industrial administrations.

Although much of what has been found out about bureaucracy in general is also applicable research bureaucracy, the latter still presents a special case worth to be investigated by sociologists of science. Research bureaucracy seems to have some rather interesting features of its own and doubtlessly exerts a considerable influence on the destinies of science in our times.

The rise of research bureaucracy coincided with the upswing of what we call nowadays "R & D", that is "Research and Development". By its conventional meaning, this composite term encompasses less than it says. It does not refer to all activities performed in the methodical quest for new scientific knowledge and for its transsubstantiation into productive practice. The capital letter "R" denotes only research performed in organizations established /at least in part/ for the purpose of doing research. /Something very similar can be said also about the denotation of the capital letter "D" which stands for development. However, we do not want to dwell on this particular question./

Although numerous scattered examples of formal organizations created explicitly for the performance of research could be given from earlier times, there is no doubt that the extensive development of the network of organizations of this kind began after the turn of the century and took really massive proportions only around the time of World War II.

In 1940 the United States spent an amount corresponding to 1/10 of a per cent of its GNP on research and development; in the subsequent thirty years a thirtyfold increase of this percentage was to be observed. A similar process took place in most industrially developed countries at about the same time or with a delay of a few years.

This was the period when R & D came of age, together with "big science" -- and with research bureaucracy. Several per cents of the GNP are not a child's play to be left to scientists to fool around with.

As a matter of fact, the whole notion of R & D was one created by research bureaucracy. It has always been and still is most unusual among scientists to discriminate between research done in organizations established for that purpose and research done elsewhere, maybe as a sideline, or even privately, in one's own studio, and without any financial incentives. Jacob Schmookler and many other have drawn attention to the fact that all estimates of the volume of present-day research based on current R & D statistics tend to be on the low side because R & D statistics cover by definition only institutionally organized research. However much the importance of R & D may have increased, it is not easy to foresee a time when all research will be done within the framework of research organizations. Some branches of scholarship stick rather strongly to old-fashioned standards of "little science" and are blooming quite well also outside of the R & D establishment.

In Hungary, where R & D statistics on a national scale reach back to 1953 and are being published yearly by the Central Office of Statistics, great efforts have been made to extend the scope of data collection in order to cover as much as possible ☐ the total research activity going on the country. It might be presumed that such an extension of R & D statistics can be done more easily in a socialist country in which almost all material resources of research, not to speak of all scientific institutions and research establishments, are under more or less direct governmental control. Still, we estimate that by whatever standards we may measure the volume of research activities in Hungary, our R & D statistics could hardly encompass more than 80 % of their effective

total volume.^{7/}

It is a great pity that under such circumstances the most bountiful statistical data sources on contemporary research, namely the R & D statistics of the various countries, can be only of limited service to the sociology of science which is concerned with all research, be it performed within or outside of organizations established for that purpose. However, our problems do not end here.

As we have mentioned, the whole notion of R & D was created by research bureaucracy. It should not be regarded as a scientific notion. Its extension is best described by the action radius of research bureaucracy: it reaches as far as the authority of research bureaucracy which has powers only over institutionally organized research though its influence may reach much farther.

It ought to be also taken into account that almost all categories used in R & D statistics and in the administrative regulations and practices of those who have established them, are defined according to bureaucratic exigencies and are thus not based on scientific considerations.

Although terms like "fundamental and applied sciences" had some currency and perhaps also some meaning in Ostwald's times, no scientist spoke of the triad of "basic research" - "applied research" - "development" before the advent of R & D. The same applies to terms like "scientists and engineers", "Scientific manpower", "full-time equivalent of the research personnel", etc. -- terms which encumber the headings of R & D statistics and form part and parcel of the strange dialect of contemporary grand-scale research planning and organization.

^{7/} I have discussed this and other problems of the R & D concept and of current R & D statistics in much detail in my book, Az ipari kutatás tervezése és szervezése, Veszprém: 1964. An English translation of this book has been brought out by the National Science Foundation: Sándor Szalai, Planning and Organizing Industrial Research, Washington, D.C.: 1967, NSF TT 67-58003 /mimeographed/.

It is quite interesting for a sociologist of science to look up the bureaucratic definitions of such terms /e.g., what is to be regarded as "basic research"? who is to be ranged among "scientists and engineers"? who belongs to the "scientific manpower"?/ in the technical notes attached to R & D statistics or in current regulations issued by various administrative authorities in the field of research. To the best of our knowledge, as yet no major systematic study has been made of the question as to how all these terms and categories relate to the real structure and process of scientific research.

The correspondence between bureaucratically defined categories and reality may be weak but this does not mean that bureaucratic thinking and practices do not have a considerable impact on reality. We are advocating an intensive study of research bureaucracy by sociologists of science just because we think that research bureaucracy has a very strong influence on the ways in which research is being done and science is developing in contemporary society.

Research bureaucracy has to be taken very seriously. It is quite easy and even pleasurable to make fun out of some of its attitudes and practices. However, as sociologists of science, we should leave no doubt about it that we do not adhere to the popular pejorative and teasing usage of the word "bureaucracy" when we speak of the bureaucracy of research.

Although research bureaucracy does not conform in all respects to the Weberian ideal type of a bureaucracy, it represents still a very respectable and quite modern approach to the professional administration of research. Surely, the immense progress of science and technology which has taken place during the last decades could not have come about without the help of research bureaucracy. "Big science" would be unthinkable without its help.

Admittedly, social scientists have some reason to bemoan that so many administrators in the top echelons of research bureaucracy are so to say reversed mandarins. In ancient China officials qualified through humanistic learning rather than technical proficiency. In contemporary research bureaucracy the opposite seems to be the case. But that may change in time, especially as social science is becoming technically more proficient too. On the other hand, it should not be forgotten that

as a rule even the most strongly reversed mandarins of research bureaucracy do not overlook completely the needs of the social sciences. Even though the whole R & D system was very much tailored to take care of the material needs of research in the natural and engineering sciences, social research benefited too from the new order of magnitude of the centrally administered material support granted to scientific research.

As far as research institutes are concerned, present-day research bureaucracy seems to be composed of at least three functionally different segments.^{8/}

/1/ Any research institute of some size, be it an "independent" research institute, or a research unit within an academic department, a governmentally supported or industrial research laboratory, etc. has to have a professional administrative staff that takes care of the extrinsic needs of organized research work and performs certain duties in the organization of all activities within the institute. This administrative staff does not do research, nor does it assist the researchers directly in their research activity. However, by managing the research establishment, by handling finances, personnel procurements, publications, by maintaining contacts with superior or coordinate administrations or clients, by providing all sorts of administrative services, etc., its expertise becomes indispensable in the planning and implementation of the substantive research activity carried out in the given institute. Thus, the administrative staff often exerts a rather powerful influence even on scientific decision-making with regard to the research to be performed. The assignment

^{8/} We cannot dwell here on the partly very different structure of professional research administration encountered in industrial firms having research sections built into their productive apparatus, in establishments performing research only as a sideline to other duties, in foundations rendering financial support to research, etc. Some of our considerations may apply however.

of priorities to various possible research projects, the selection of specific topics or methods of research, and many other scientific decisions of this kind might have important implications with regard to instrumentation, personnel, finances, etc., which the scientist himself may not even be able to assess. Thus, the internal administrative staff or research institutes represents the first segment of the research bureaucracy involved in the institutes' work.

/2/ Depending on the size of a research institute and some other factors, there exists usually a more or less extensive administrative hierarchy of the scientists working there. The ladder may lead up, for instance, from senior researchers in charge of small research groups or single projects to heads of sections, heads of departments, and finally up to the research director or the institute director himself. At every step upwards on this ladder more and more managerial duties and responsibilities are weighing on the shoulder of the scientist -- an increasing part of his work consists in directing and supervising the research of other and a decreasing part of it consists in doing research himself. Thus, the internal administrative hierarchy of the scientists working in research institutes represents the second segment of the research bureaucracy involved in the institutes' work.

/3/ Under present conditions, most research institutes are not really independent in administrative respects. They form part of a complex or network of institutions which has a central administration of its own. If there are numerous research establishments within this complex or network, or if research activity is a major concern of its central administration, then this superior administrative authority usually puts a special administrative staff in charge of the "central" planning, organization, management and financing of research activity in the establishments under its control. Several levels of such ever more "central" research administrations might be stacked over each other and their top may reach up to the highest levels of general governmental administration. The specialized staff of such "central" research administrations which are superimposed on the internal administration of research institutes represents the third and perhaps most powerful segment of the research bureaucracy involved in the institutes' work.

Due to the lack of sufficient research on research bureaucracies, we have very little detailed information about the sizes in which they come, about their structural characteristics, their impact on the shaping of research policies and programs. There are surely different types of research bureaucracies but nobody has as yet attempted to survey them. Still, every scientist doing research nowadays can attest on the basis of his own experience that research bureaucracies do exist and that they can be very powerful indeed.

We do not even know how many people are involved in the administration of research in various branches of science and how their number relates to that of the scientists performing research, or of the total personnel employed in research establishments. It is perhaps not by chance that the categorization of "manpower" in current R & D statistics makes it so difficult to extract information of this kind from their huge body of data. After all, it is research bureaucracy itself that produces these R & D statistics.

Thanks to the courtesy of Department of Personnel in the Hungarian Academy of Sciences, we were able to get re-processed the personnel statistics of the total personnel employed in the 38 full-fledged research institutes the Academy.

We divided this total personnel into four categories suitable for the purposes of our investigation.

/a/ research scientists -- scientists, normally with a university degree, employed for performing research in the institute;

/b/ assistant research personnel -- engineers, technicians and other specially trained or skilled personnel, e.g. laboratory assistants, documentalists, programmers, etc., assisting the scientists in their research work;

NB. /a/ and /b/ together constitute the research personnel.

/c/ administrative personnel -- officials, clerks, secretaries stb. performing administrative tasks in the research institute;

/d/ auxiliary institute personnel -- lower qualified office workers and blue-collar workers needed for the servicing and upkeep of the research institute.

The number of the total personnel employed by all 38 full-fledged research institutes of the Academy amounted -- as of 31 December 1975 -- to 7093.

It is not necessary to characterize here one by one all these research institutes of the Hungarian Academy of Sciences, especially as a very up-to-date little book published in English lists and reviews all of them and gives much factual information about their main features.^{9/}

It suffices to state that the network of the research institutes of the Hungarian Academy of Science is directed towards the performance of "basic" and "applied" research in a vast array of

- /a/ the physical sciences /understood as comprising mathematics and the natural sciences, with the exclusion of the life sciences/,
- /b/ the life sciences /understood as including also the agricultural sciences/, and
- /c/ the social sciences /understood as including also philosophy and the humanities/.

The so-called engineering sciences, which form in principle part of the physical sciences, are represented only by a few institutes in this network. However, even these few institutes are performing mostly very high-level technical research.

On the whole, the network of research institutes maintained by the Hungarian Academy of Sciences is in many ways similar to that maintained by the Academies of other Eastern European countries. The work of the institutes is being carried out in the main by the permanent by employed full-time staff.

There are naturally even in Hungary many research establishments which are very different from the type of Academy research institutes we have just described. Still, a network of 38 research institutes with a total of 7093 employees, does not represent such a small object that it would not be worthwhile to study it as a separate entity. We thought that its study might provide, in spite of all our caveats and reservations,

^{9/} Research Institutes, Institutions and Enterprises of the Hungarian Academy of Sciences, Budapest: 1975.

at least some useful hints about what may be expected elsewhere and under different circumstances.

Anyway, we have nothing better to offer here than a few observations based on our re-processed personnel statistics.

Thus, we calculated the numerical proportions of the administrative personnel to

- /A/ the staff of research scientists;
- /B/ the research personnel /research scientists + assistant research personnel/;
- /C/ the total institute personnel /research scientists + assistant research personnel + administrative personnel + auxiliary institute personnel/.

We can present a brief summary of our calculations in the following little table:

	Number of administrative personnel		
	per 100 research scientists	per 100 research personnel	as a percentage of total institute personnel
	/A/	/B/	/C/
a/ Physical sciences /14 institutes with 4744 employees/	74	40	21
b/ Life sciences /8 institutes with 1316 employees/	70	33	17
c/ Social sciences /16 institutes with 1033 employees/	29	23	17
Total /a+b+c/ /38 institutes with 7093 employees/	62	36	20

As far as the network of the Hungarian Academy of Sciences are concerned, ^{this table} demonstrates unequivocally, that the numerical ratio of the administrative staff to the staff of research scientists and to the whole research personnel is, on the average, highest in physical science

institutes, somewhat lower in life science institutes and very much lower in social science institutes.

. Why is research activity in the physical sciences in need of so much administration? In spite of the oddities and the unrepresentativeness of our small "sample" of physical science institutes, we have some reasons to venture the hypothesis that a similar situation as we have found might prevail in many other research establishments and networks of research institutes.

The physical sciences are very strongly engaged in the field of "big science". Thus we may put forward here the conjecture that "big science" tends to go hand in hand with big research bureaucracy. For instance, in the Central Research Institute for Physics of the Hungarian Academy of Research which is heavily involved in such typical "big science" undertakings as particle and nuclear physics, reactor physics, solid state research, computer development, the number of administrative personnel per 100 research scientists reaches the astoundingly high value of 132, the number of administrative personnel per 100 research personnel /including research scientists and assistance/ the high figure of 64. Compare that with 35 and 19 in the good old Astronomical Observatory, or with 14 and 9 in the Institute of Sociology.

If our data give any hint of what is going on in a wider sphere of research, then it seems to us that research bureaucracy flourishes best where some kind of "bigness" is present -- big science, big investments in the research plant, big research contracts, big commercial value of the research output, and the like. /Some Parkinsonian effects might play a role in this attraction of research bureaucracy to bigness./

We have not yet spoken about the third column of our little table. It shows that there is surprisingly little difference among our physical science, life science and social science research institutes in respect to the numerical proportions of the administrative personnel to the total institute personnel. This is, at least in part, the consequence of some general norms of staffing to which all government-financed research institutes in Hungary have to adhere. However, this means also that wherever the administrative personnel occupies a larger place than usual

in such institutes, the growth of the research bureaucracy goes at the expense of the availability of institute personnel for other essential services.

Naturally, the whole network of the research institutes of the Hungarian Academy of Sciences has also a central administration within the Secretariat of the Academy. This central administrative staff numbers about 160. That amounts to roughly 11 per cent of the administrative staff of the total "peripheral" administrative staff the 38 research institutes.

In addition, about one fourth or one fifth of the research scientists working in the institutes are so highly placed in the hierarchy that they must by necessity pick up a sizeable load of managerial and general administrative tasks. However, it would be difficult to give a reliable estimate of the so-called "full-time equivalent" of this managerial activity of the scientists without much further study. Therefore, we do not take into account this factor in the following brief calculation.

Out of 7093 persons employed in the research institute network of the Hungarian Academy of Sciences 1409 are involved in research administration. Another 160 persons are active in the central administration of the research network within the Academy itself. It takes thus an administrative staff of 1569 persons to administer the research done by 2277 research scientists who are employed in this network. This is a ratio of 2 : 3 between the staff administering research and the staff performing it.

If this ratio is of any general validity, then it shows that we still have not quite reached the stage in the development of research bureaucracy where as many research administrators may be needed as there are researchers around.

However, even the presently observed ratio of 2 : 3 may serve as a warning to sociologists of science that research bureaucracy is a topic which deserves much more attention than it has received in the past.

That is all we wanted to demonstrate here.