BIG SCIENCE AND GRADUATE EDUCATION

by

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The division of basic research into "big science" and "little science" has become popular nomenclature. I confess that I dislike the terms greatly, first because they imply a discontinuity while in fact there is a continuum, and second, because while they do describe a difference in method of doing science, there is, however, no difference in scientific motivation.

What one usually means by "little science" is research carried out in the traditional academic pattern; that is, research supervised by a professor, assisted by graduate students, a very small number of technicians, and supported by some central shop facilities, access to a computing center, etc.

One calls "big science" scientific research where investigators generally operate in a group and where in effect some segment of industry is mobilized in support of the work. This may occur either by large purchases or subcontracts, or by setting up establishments which are of the large size needed to support this type of work and which are partially industrial in character.

There is, however, much so-called "little science" which has in fact now adopted many of the features associated with big science; this is brought on by the increasing realization in some University departments that there frequently

* I should like to express my appreciation to Mr. Omar Snyder for collecting and tabulating the career data of Stanford and the University of California at Berkeley Ph.D. students in Physics and to the academic authorities of both institutions for permitting him access to the material.

appears to be a "critical size" of staff required to be competitive in a given field. The inter-disciplinary laboratories for materials science, a substantial part of which is devoted to solid state physics, are a manifestation of this tendency. Even theoretical physicists generally object to being "isolated" and prefer to work where there is an active and productive group of fellow theorists. In short, some of the much quoted, so-called "evils" of "big science" as related to scholarly work such as multiple authorship of papers, increased specialization, etc. apply to some extent to much of recent "small science" also. Research budgets of investigators in "little science" in universities are not too dissimilar to budgets of university scientists participating in the work of national centers using large research tools.

However large one views the difference between "big science" and "little science," the fact remains that these terms describe the extremes in the range of methods each specifically suited to particular areas of basic research. In my discussion I will restrict myself to "basic academic science"---that is, work which in general is initiated by academic people; i.e., investigators who also have a joint interest in education and the advance of knowledge.

In recent times elementary particle physics has been the main example in this category generally described as "big science." However, other fields of academic interest (astronomy, radio astronomy, nuclear structure physics, and even solid state physics) are making a transition toward the methods associated with big science in many cases. It is in connection with this gradual transition that my dislike for the terms "Big Science/Little Science" originates. Much of science still described as "little" is not really as little as all that. If one looks in detail at the single investigators' laboratories one finds that his instrumentation is much more extensive than it used to be, that the services available to him on a

university campus are much more lavish than in the past and that the access he has to funds for travel, consultants, or guest lecturers, taking leaves of absence, and the like, have also generally become much greater. Therefore, nostalgically identifying little science with the "good old days" is simply misleading. Even more misleading is the implication which one can read into many public statements on the subject identifying little science with "virtue" and big science with "vice," as far as the relationship of research to education is concerned.

The common theme to most of this discussion is that "little" and "big" science do not exist as such; the terms as I use them represent only extremes of a continuous range of the methods of experimental science.

It has become apparent recently in many areas of research that some activities which have been carried out in the "little science" framework would gain substantially in their effectiveness if they made a transition toward big science. However, this change is being resisted by investigators in the field because carrying out their work more in the big science manner may involve large personal inconvenience and also may be considered by the investigators' colleagues as a sacrifice of academic values. Let me illustrate this problem by the crisis now being faced in nuclear structure physics.

Until relatively recent times nuclear structure physics had been carried out with instruments located at single universities or at single research institutions where these tools were generally used only by the investigators in residence at these institutions. All research during what one might call the "Golden Age" of nuclear structure physics, before the war, was carried out in this manner. Nuclear physics was then concerned both with what is now classified as "nuclear structure physics" and what is now called "elementary particle physics." The only elementary particles accessible to laboratory methods at that time were neutrons,

protons and electrons: Since the maximum energy accessible pre-war for elementary particle physics was in the nuclear binding energy range, the methods of both fields were largely identical. Since the war, elementary particle physics has moved into a higher energy region where the tools have become so large that economic necessity has demanded that access to the accelerators be shared. On the other hand, nuclear structure physics remained in its original energy range and the instruments generally remained as single institution devices. Even this, however, is changing as a result of new technology: Nuclear structure physicists now use electrostatic generators and variable energy cyclotrons which cost several million dollars. Moreover, the operating costs associated with electrostatic generators have so surprised the supporting agencies that administrators have become reluctant to authorize too many of these "big" pieces of equipment for this field which still likes to be considered as "little" science. In addition, during the last few years there has been a revolution in particle detection which permits a combination of high-energy resolution and high data rates. This in turn has led to the adaptation of powerful means of on-line computation to handle the vast volume of information generated. To this technological change there has been added the recognition that high-energy accelerators in the more than several hundred MeV range are powerful tools for low-energy nuclear structure physics. As an example, high-energy linear electron accelerators are productive instruments in carrying out nuclear structure physics at low-energy excitations but at high transfers of momentum to the targets under investigation. The Atomic Energy Commission has invented the term "Intermediate Energy Physics" to describe research programs with high energy accelerators in the 50 MeV to 1000 BeV range; from the point of view of scientific purpose this region is however not a separate endeavor but largely describes work in nuclear structure physics carried out with

a certain class of tools. In short we have here a concrete, but by no means unique, example where the methods of "big science," for scientific and technical reasons, may offer overriding advantages over the traditional approach.

In spite of these developments most nuclear structure physicists, when convened into committees and asked to forecast the future for their science, tend to project a world in which each university has an almost identical machine in its basement populated by the professor and his graduate students, all doing fairly similar kinds of physics, although at an expanded rate and using the new techniques when possible. When one talks to the committee members separately as to why this was their forecast they are quick to acknowledge the power of the new methods which can in fact be practiced more productively in the big science mode, but they are apt to say that they are not willing to follow the path of iniquity trod by the high-energy physicist; more bluntly, the concern to avoid the methods identified with big science has led many an investigator in nuclear structure physics to actually limit the range, if not the effectiveness, of his research. Moreover, this attitude has led to grave problems for government program administrators who are faced with a large number of relatively similar proposals for multimillion dollar installations at a large number of universities. All these proposals contain somewhere a phrase like: "The proposed installation is an ideal establishment for the training of graduate students." These remarks should not reflect on the quality of the research or the educational value of much of the individual nuclear structure physics work in this country; the remarks do, however, identify an over-all dilemma.

The roots of this dilemma are clear: The prevalent opinion appears to be that big science is a relatively "bad" vehicle for the training of graduate students while small science is "good"; therefore, in the interest of education the investigator is willing to limit his research interests by sticking to small science methods,

and the government is expected to make similar sacrifices in terms of its management of research funds. It is therefore of considerable importance to examine critically the validity of the premise, namely, that graduate students' training is indeed poor if the student is being directed in his dissertation in a "big science" laboratory.

The fact that advanced research and graduate education are inseparable is taken as axiomatic by almost all writers on the subject and might even be taken as the definition of education leading to a Ph.D. The reasons for this conclusion are, however, rarely examined critically.

Let me give five general reasons why a graduate student should receive primary research experience as part of his Ph.D. program.

1. His acquaintance with and direct participation in an exciting and significant piece of original research will motivate him further towards original research work;
2. He will learn techniques which only exist in research involving genuinely new questions and he will learn judgment on the choice of research problems and of proper tools and methods;
3. He will work with his professor and his co-workers who, hopefully, will teach him creative approaches to research and increase his education beyond classroom learning;
4. Close association with graduate students maintains the vitality of the research faculty by exposing them to searching unbiased questioning;
5. In original research the student can be assigned individual responsibility for a given piece of work and thus he will develop his resourcefulness in dealing with difficult research situations

and he will learn to take personal responsibility for the solution of a scholarly problem.

In the light of these criteria let us examine the extent to which these objectives are met in the span between the methods of little science and big science. During the last decade the primary example of academic so-called big science has been elementary particle physics: In this examination I will therefore restrict myself almost entirely to using elementary particle physics as an example of big science.

Clearly the first four reasons for research involvement of a Ph.D. candidate given above apply just as well to any valid research in whatever "mode" it is carried out. The first criterion mentioned above, namely, having the student participate in an exciting piece of primary research, brings immediately into focus the paradox on which I elaborated above in connection with nuclear structure physics. If the investigator chooses to limit the problems he can attack, and is even willing to limit their fundamental significance in order to remain in "little science," does he not at the same time deny at least to some students the opportunity to participate in truly exciting and significant research? Here is therefore an instance where the tables are turned: The emphasis on the virtues of little science may actually work out to the detriment of graduate education. Conversely, the strongest argument against big science as being a proper adjunct to graduate education lies in the fifth category, i.e., the value of individual research responsibility. It is becoming increasingly difficult to select topics for Ph.D. theses in big science, such as in high-energy physics, in which one can give a graduate student complete individual responsibility for an important piece of research. If the requirement for the Ph.D. included the condition that his thesis be publishable

In a reputable journal as an article of which the candidate be the sole author, then few degrees in high-energy physics would be given. The reason is that, purely physically, it is often impossible for a single scientist, even a professor, let alone a graduate student, to take full responsibility for a major high-energy physics experiment. In addition, mistakes, caused by inexperience, would be very expensive when made in a large high-energy physics experiment; moreover, pressure for running time on large facilities is so high that it is hard to justify devoting these facilities to student practice.

Some of these problems are, of course, not new and also not restricted to big science. There have been many cases where a professor has developed an elaborate piece of research equipment such as an X-ray spectrometer, molecular beam apparatus or similar instrument in his laboratory over many years and where he would not dream of giving a student full responsibility in carrying out experiments on this apparatus without extensive supervision. However, it is clear that such a situation is more frequent and thus more serious as we move toward big science. As a result there is apt to be a larger fraction of post-Ph.D. relative to pre-Ph.D. contributors, for example, participating in a single, high-energy physics experiment. A few years ago I observed the ratio of pre-Ph.D. to post-Ph.D. physicists at the Radiation Laboratory at the University of California, who were working on the 184-inch cyclotron operating near 600 MeV and on the 6 BeV Bevatron. The ratio was twice as large on the lower energy machine. This is not a surprising result. Since scientific results in big science require collaboration among many workers, the fraction of the responsibility which can devolve on a single student will therefore be smaller. This partial responsibility is, however, often quite as educational as total responsibility for an experiment requiring a lesser effort; the senior investigator

will generally attempt to delegate as complete a responsibility for a "sub-experiment" to the student as is compatible with the success of the complete undertaking.

The question remains whether this method of work gives a student a worse education or a better education. Clearly it will give him a different education; namely, an education through which he learns both the purely scientific aspects of very important work as well as how to collaborate efficiently with other people. On the other hand, he clearly suffers from being unable to carry out his work along a path governed by his own conclusions, but he is able to contribute extensively through original ideas.

We would, therefore, conclude that from the point of view of the fifth point in the list of criteria given above, graduate education in big science might by and large be inferior to education in little science, but from the point of view of the other four it might in many cases be superior, particularly if the student is permitted to participate in a piece of truly exciting and important work.

Big science, by its very nature, usually involves sharing of unique facilities by several investigators. This in turn results in the need for an acceptable decision making process to govern access to the common equipment. As long as the supply of shared facilities adequately satisfies the demand (as has been the case in astronomy until relatively recently), there are few problems; if the demand by qualified scientists overloads the "shared" facilities, the "social problems" can become severe. I will not discuss this complex problem here in detail; however, it is clear that the necessary process by which an academic investigator has to justify repeatedly a proposed experiment to a committee or an administrator is a departure from academic

tradition: In past University practice, a professor is appointed after careful examination of his qualifications and of the facilities available to him; after appointment, his work (in principle) has to be "justified" only to posterity, and not to any review committees. Again this departure from academic tradition as applying to research opportunity has always been a fact in experimental science, be it big or little. Support of any experimental work is only given for a limited interval so that even the "little" scientist has to "justify" his work repeatedly, usually to a committee of his peers. Again we are meeting here a continuum rather than a distinction between little and big science.

This departure from academic tradition is another facet which may interfere to some extent with the involvement of graduate students in big science research: The professor, in effect, has to ask the student to share with him the risk of being given or denied access to the accelerator, telescope, oceanographic ship, rocket or whatever the shared facility may be. The more adventuresome and strongly motivated student will gladly share this risk; after all, any original research is risky to a graduate student in terms of the time required to complete his thesis. From local experience, I see little evidence that a creative student is affected in his choice by the problem of shared facilities; nor as shown below, are there any data which indicate that the time to complete the Ph.D. degree (time from B.S. to Ph.D.) is significantly different for a thesis in elementary particle physics relative to the other physics sub-fields.

There have been developments in recent years which have been a general consequence of the growth of the content of science, and also of the magnitude of specific scientific experimental enterprises and the increasing variety of tools. Increased specialization both of subject and method has been the unavoidable result. During the first part of the century we have seen the increasing division

of physicists into experimentalists and theorists. With the death of Fermi we lost one of the few remaining members of the physics community who might validly be considered to be a leading contributor to both experimental and theoretical physics. We are now generating a third branch of physicists whom, for lack of better words, I would like to call "data reducers." These are people who, on the whole, have received a better education in theory than most experimentalists, but who are neither creative theoretical physicists nor experienced designers of complete experiments. Such workers start their research, for example, by taking pictures in a bubble chamber exposed to a high-energy particle beam; they are not involved in the design or operation of the bubble chamber or the accelerator. They then subject these pictures to analysis, usually with extensive use of computers, and then draw physical conclusions. As the result of this work they learn a great deal about modern data processing techniques, in addition to becoming conversant with current problems in modern physical thought. However, when students who received their Ph.D. as "data reducers" continue their academic careers at smaller institutions or engage in research where they would have to design experimental apparatus "from scratch," then in general they have difficulties since their experience has been severely limited. The fraction of graduate students who receive their Ph.D.'s in high-energy physics as "data reducers" is increasing. Again, we should not conclude that this form of education is better or worse than the education received either as experimentalists or as theorists; it is just education in a different branch of the science. Many students who received their Ph.D. experience in any of the three areas have become excellent creative physicists. I would attribute the increasing number of degrees given to "data

reducers" as a manifestation of increasing specialization rather than as a symptom of degeneration of scientific education.

The ever-increasing amount of knowledge and variety of techniques, accompanied by the limited capacity of the human brain, produces serious problems to education, particularly graduate education. How to deal with this problem effectively has been the subject of much discourse but this is a question all its own and is not uniquely connected with the interaction of big science and graduate education.

Again, the phenomenon of the "data reducer" is not new. In astronomy the student has been operating in this mode for decades; he certainly does not design nor build telescopes and in general does not devise original methods of observation. Even in "little" laboratory physics a student has frequently received his education without having had the experience of designing his apparatus. At the turn of the century the sign of a reputable physics department was to have a 21-foot-radius diffraction grating in the basement for spectroscopic research. A graduate student would frequently receive his Ph.D. thesis by mounting a specific element in an arc source, exposing photographic plates on the focal circle of the grating, and then by spending the bulk of his research time on measuring and analyzing the spectral lines on the photographic plate; there are many other examples of this kind of thing in the past. This type of thesis was probably not judged too valuable, even at that time, but many highly prominent physicists received their graduate education in this way. The only new circumstance applying, for example, to high-energy physics is that now a larger fraction of students is completing thesis work as "data reducers" than used to be the case.

We have thus identified many factors through which education in "big" science involves different circumstances than education of the graduate student in "little"

science, but we are unable to draw a conclusion which education is "better" or "worse," since we do not know how to weigh the various criteria given above. In principle, one would like to be able to obtain such a value judgment by observing students in their future careers who have been educated either in the little science or big science mode, and try to ascertain which ones are better prepared for future productive careers in science. To do this objectively is at best a very large task and at worst a hopeless one since defining what is meant by a "successful" or "productive" career is in itself difficult. I have made a somewhat sketchy attempt to examine how the future career of a physicist is affected, depending whether he received his Ph.D. research experience in big or little physics. For this purpose Mr. Omar Snyder of this laboratory examined the present professional positions held by Ph.D. alumni of the Physics Departments of Stanford University and the University of California at Berkeley. Although this is clearly an oversimplification, we identify "elementary particle physics" with "big science," while we designate the other subdisciplines as "little science"; under this assumption Mr. Snyder's data is summarized in the following table (Table I).

Data are separately given for Ph.D.'s in "little" and "big" physics. The not-too-surprising conclusion from this very limited study is that as far as preparation for continued productivity in academic research is concerned, it is very difficult to conclude whether little and big science education is inferior or superior. About the same fraction of big science and little science Ph.D.'s go into University teaching; a somewhat larger fraction of the "big" science students remains in basic vs. applied work than that of the "little" science students; this is however more likely caused by the fact that the particular "big" science in this study --elementary particle physics--is extremely basic while the fields in the

"little" science category include solid state and atomic and molecular physics, which are also of direct applied interest.

The most striking conclusion from more detailed examination of the input data is that students choosing a particular professor as a thesis supervisor are more apt to adopt a particular career. In short, both in little and big science, leadership of the research professor has a great deal more influence on the student's career, than does the style in which the student undertakes his thesis research.

We thus reach the gratifying conclusion that the individual qualities of the graduate student and professor greatly overrides in importance the "mode" in which the research is carried out.

The study also tabulates the time between the Bachelor and Ph.D. degree for students engaged in the various subfields of physics at the two institutions. Table III tabulates the number of students in each subfield who take a given number of years to attain their Ph.D. degrees. The harmonic mean of the time interval is computed and shown in the table. As noted previously no significant difference in time interval is evident.

In the preceding sections we have identified differences in the opportunities for graduate students working in fields between little and big science without being able to arrive at a relative value judgment. We therefore would conclude that education in this range of modes can be highly valuable to the student and should be considered an essential asset to the country; thus, the criteria for support of scientific research in the various fields should be based primarily on the value of the science; educational criteria based on the mode of carrying out the research have little or no validity.

The question has been raised frequently whether education through big science is not "too expensive" and whether for that reason the government should bias its program choices in favor of little science. This is, of course, not a fair

comparison since the questioner implies that the entire cost of the research should be charged to education; it is not surprising that if one compares, for example, the cost of space research, which has a very small academic base and involves very little participation of graduate students, with the cost of theoretical physics, the cost per graduate student in space research will appear to be vastly higher. One can simply not expect that there is any reason for such cost ratios to be uniform. Nevertheless, the variation in cost per student educated, even if computed by this simple-minded method, is much less than is commonly thought. This is particularly true if one relates the total cost to the Federal Government in supporting basic research in a given area to the number of Ph.D. students produced. Such data is shown in Table II for the field of physics, taken from the Report of the Physics Survey Committee of the National Academy of Sciences. This table contains among other entries fiscal year 1963 costs in the major branches of physics in terms of total Federal dollars. The different specialties range from "big science" (elementary particle physics) to small laboratory science such as atomic and molecular physics. The table also includes the number of Ph.D.'s produced per year in each of these fields.

If one forms the quotient between total Federal dollars and the number of Ph.D.'s produced one finds that in terms of total Federal dollars the ratio varies by a surprisingly small factor, being about a one million dollar per Ph.D. trained in elementary particle physics, astrophysics, solid state physics and in plasma physics, with about one half that amount in nuclear structure physics (not including nuclear structure physics carried out at higher energy machines): The lowest

ratio is for atomic and molecular physics at a value near one tenth of one million dollars per Ph.D. produced.*

The reasons for this relatively small variation in the top group are, of course, complex. The cost of a given experiment in solid state physics which might involve production of a Ph.D. thesis is, of course, much smaller than that of an experiment in high-energy physics. On the other hand the Federal Government supports much work in basic research in solid state physics not involving student participation at all, while in elementary particle physics the entire field is academically motivated, and student involvement is generally encouraged throughout. To say it in different words--in some fields of big science such as elementary particle physics, all branches of the field ranging from instrumental development to theoretical work are involved with academic life and thus with students, while in other fields, the most notable example being plasma physics, much of the work is being carried out in non-academic laboratories; I will not comment here whether this is or is not a desirable situation. Clearly fields of basic research which do not have a strong academic base of their own must draw on scientific manpower of other fields and thus become consumers rather than producers of talent. On the other hand, largely academic fields such as high-energy physics, nuclear structure physics, and atomic and molecular physics, are net producers rather than consumers of scientific talent, whether they are carried out in the big science or little science mode.

Maintaining academic involvement of all fields of basic science, whether carried out in the big or little science mode, is clearly of overriding importance,

* Atomic and Molecular Physics is in a special situation; after a period of relative stagnation it has during the last years become the most rapidly growing sub-field of Physics, both as the result of new techniques and of the need for the data for applied purposes. As the result the methods in this field will now evolve rapidly and probably there will be consequent increase in cost per student.

both for the vitality of the science itself as well as for making it possible for each of the sciences to produce people of advanced training in their field. We cannot afford to be so inflexible as to require the form of graduate education to remain as it was during the past century, in the face of rapidly changing methods of research. By so doing we would prevent important segments of science from contributing to education, with a consequent loss both to the science and to education. We have seen above that the value of graduate education ranging from "little" to "big" science, can be very great--depending mainly on the inspiration of the scientific problem and of the thesis supervisor, and much less on the circumstances under which the thesis work is carried out.

Present field of professional activity of Physicists who received their Ph.D. 's at Stanford University and University of California (Berkeley) from 1956 to 1965.

Present field of professional activity of Physicists who received their Ph.D. 's at Stanford University and University of California (Berkeley) from 1956 to 1965.

TABLE II

Distribution of scientific workers, Ph. D's participating, Ph. D's produced and fiscal 1963 funds expended over the various subfields of Physics. From report of the Physics Survey Committee of the National Academy of Sciences, March 1966 (the "Pake Report").

PHYSICS SUBFIELD	Total No. in Subfield	Ph. D's in Subfield	Ph. D's Produced 1963	Total Funds* 1963	Federal Funds* 1963	Federal Funds to Universities*	Total Funds/ Ph.D. Prod.*	Federal Funds/ Ph. D. Prod.*	Federal Funds to Universities/ Ph. D. Prod.*
Astrophysics, etc.	1180	590	50	59	49	25	1.2	1.0	0.50
Atom. & Mol. Physics	1260	620	118	17	15	13	0.14	0.12	0.11
Elem. Part. Physics	1630	950	110	125	125	100	1.2	1.2	0.91
Nuclear Physics	3200	1540	154	69	69	36	0.45	0.45	0.23
Plasma Physics	800	400	35	50	43	8	1.4	1.25	0.23
Solid State Physics	7080	3260	226	173	95	36	0.73	0.42	0.16

* In Millions of Dollars

TABLE III.

Table of the number of students in various subfields of physics studying at Stanford University and U.C., Berkeley shown in relation to the time interval between the B.S. and the Ph.D. degree

Sub-field Time in Yrs.	Astrophysics Space, Etc.	Atomic & Molecular	Elementary Particle	Nuclear Structure	Plasma Physics	Solid States	Classical Physics	Applied Physics	Total
2			1						1
3		1	5	1		1			8
4	3	13	24	8	1	7	2	1	59
5	2	23	47	19	2	10	5	5	113
6	2	11	46	16	2	9	4	4	94
7	4	19	28	12	6	6	4	3	82
8	2	10	23	9	1	4	3	2	54
9		7	10	11	2	6	5	2	43
10	1	4	11	3		2	1		22
11	1	3	8	3		1	2	1	19
12		4	5	2				2	13
13		1	5	1					7
14			3	2					5
15	1		1	2				1	5
16		1	1	3					5
17		1	1	2					4
18+		1	3	1			1		6
Total no. of Students	16	99	222	95	14	46	27	21	540
Harmonic Mean of Time Inter- val (Yrs)	6.24	6.18	6.01	6.64	6.40	5.82	6.76	6.68	6.25