Reflections of a Dean
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University of Missouri-Columbia
1982
To Joel H. Hildebrand, my first inspiration, and Florence without whose insistence and encouragement I would not have persevered.
Acknowledgments

Lloyd Berry, when Dean of the Graduate Faculty of the University of Missouri, suggested the writing of this volume and furnished clerical assistance in the preparation of the first draft. After his death, Dean Andrew C. Minor and Dean Richard Wallace encouraged the completion of the manuscript and furnished further assistance. Without the encouragement of these two friends, the work would never have been undertaken or finished.

Marjorie McKinney served as editor for the first draft and undertook the formidable task of taking what started as informal dictated notes from memory and converted them into the final manuscript. After Marjorie’s death, I was most fortunate in acquiring the assistance of Natalia Nadraga who refined the manuscript and brought it to its completion.

To all of the above and the many friends who have commented on various details or assisted in checking records, I am most grateful. To these, I extend my deepest thanks.

H. E. B.
Columbia, Missouri
1982
"We cannot be said to reflect upon any external object (i.e. graduate work) except so far as that object has been perceived, and its image become part and parcel of our intellectual furniture." —Sir W. Hamilton

The preservation of a record of graduate work during the twenty-eight-and-a-half years that I was dean undoubtedly was the major objective of the late Lloyd Berry, Dean of the Graduate School of the University of Missouri, when he asked me to record my observations. To be faithful to this charge, I have tried to act as a witness. Some of the validity of my observations would doubtless be lost if I acted also as advocate and judge, if I indulged in praise and blame. Where I may have deviated from this narrow path my intent is to describe the attitudes of a dean rather than to pass judgment as an author.

To qualify as a witness, I have attempted to restrict my comments to what I observed rather than to hearsay or what has been completely outside my range of experience. As a result, many outstanding achievements and many important individuals are hardly mentioned. Such omissions I hope will be understood and forgiven. Many schools, departments, and individuals achieved great success with little or no assistance or contact with the graduate office or with me. Comment on these could only be inadequate.

The title "reflections" suggest another quite different meaning. When a doctor strikes a person's knee with a hammer, the resulting kick is a good reflex, but a reaction that has little value, except to indicate that the patient is reacting normally. Just so when I mention the reaction of a faculty member or myself to some "blow," I am recording what may have little meaning other than to give a little idea of the magnitude of the "blow."

Another implication of the word reflection involves the modification of the observation by the process of thinking, or at the least emphasis created by the mere process of selection.

A phenomenon is not a phenomenon until it is an observed phenomenon.—John Wheeler
I justify including this aspect of reflection in the hope that it may be considered part of the record of how a dean, and perhaps many deans, reacted to certain situations or problems.

Perhaps the most important “reflection” by the author is analogous to the reflection in an old glass window in a barn, perhaps a mile from the observer, of the brilliant setting sun. When I became dean, I was thirty-seven years old and immediately found myself in the presence of the deans and presidents of our largest and most distinguished universities. My appropriate posture seemed to be that of a student. I listened carefully. Now more than forty years later, I owe it to the reader and to those who gave much thought to graduate education to reflect their wisdom with as little distortion as possible.

Chapter titles should help the reader find topics of special interest. The first few chapters are largely of local interest, while the later ones deal with state and national issues. To a considerable extent, the narrative follows the course of history, the early chapters dealing with conditions in the first and second quarter of this century. Some of these situations are much dated. However, an examination of the program of a meeting of graduate deans suggests that the questions have changed little, only the answers are new.
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I.

Orientation of a New Graduate Dean

The following narrative was written at the request of the late Lloyd Berry, dean of the Graduate School (1972-1977), in order to give some of the history of the Graduate School of the University of Missouri. The period covered is from February 1938 to August 1966.

Although friends have assisted in reconstructing some of the events, most of what I’ve recorded is based from my experience as a graduate dean. I shall not attempt to give a history in the ordinary sense, using the kind of material that is found in the minutes of meetings, or published in bulletins, or in the press; rather, I shall place more emphasis on the individuals, on faculty members and their attitudes, and on the attitude of the University as a whole. I shall include efforts that failed—the kind of information that usually does not appear in written reports—since it is my belief that failures or near misses can help us to learn, as well as successes. I use the first-person personal pronoun simply for convenience and in order to distinguish between my statements and those of others.

I shall also use the masculine pronouns to avoid the awkwardness of he/she, his/her, and him/her. My first contact with the Graduate School was with Dean William J. Robbins, who had been acting president of the University for a year. I suspect he had hopes that he would become president when Frederick A. Middlebush (Middlebush had also served as acting president for a year) was chosen. Robbins began to look for other pastures and, in the middle of the year, was appointed director of the Botanical Gardens in New York City. Hence, my appointment as dean was made without much fanfare.

In February of 1938, I was associate professor of chemistry, having arrived on the campus in the fall of 1936; I remained associate professor for a time while dean of the Graduate School. Middlebush explained to me that it probably would be more diplomatic for me not to be advanced to the rank of full professor, since faculty members...
might feel that this was too rapid an advancement. He also suggested that I talk with Robbins and find out what my duties as dean would be. I made an appointment and went over to Robbins’s office, which was on the south side of the first floor of Jesse Hall, about halfway between the center and the west entrances. Here I found a small office with a secretary, Miss Jessie Dierks, and from this a door to a larger office, from which Dean Robbins operated. He was sitting at a table—no desk—and was reading a proof of his latest research paper. There was little other material on his desk; apparently the responsibilities of a dean of the Graduate School did not weigh heavily. I sat down and asked him what the job would entail. He was not a very communicative individual; he smiled and said, rather briefly, “Well, what happens is that the student gets his form for, say, the master’s degree, and takes it to his adviser, and together they fill out his course program. He will bring this over to the Graduate School and, as graduate dean, you will sign it,” and with a rather casual smile, he concluded, “That’s about it.”

I was quite aware that his was an overly brief description of my duties as graduate dean, but he did not seem interested in saying much more and turned back to his manuscript as I left. He did make one or two other remarks, however. He said, “You won’t have to worry very much about money matters because you don’t have any. Your salary and the salary of the secretary are not paid from your budget and consequently you will be expected to get along on $600 a year. The cost of The Graduate Bulletin was met by publishing it every other year.” Robbins commented that he had saved up money for two years in order to get a little costumer on which to hang coats and hats when visitors came into his office.

For several years, the office got along on little more than $600. I should add, however, that during the summertime I was able to acquire the help of a student to do typing, which was made necessary by the large enrollment in the School of Education. Not long after assuming my duties, it seemed desirable to have more space, and we moved into the office that was just to the east of the south-central entrance to Jesse Hall. In addition, after about the first year I found it necessary to perform one of the most unpleasant tasks of any administrator, namely to replace a staff member whom I found to be agreeable, pleasant, and most loyal, which made it all the more difficult to displace her. Dannelle Sheley, who had been assistant to the secretary in the president’s office, asked for the job and proved to be the most delightful person I have ever worked with. For many years, she was the most cordial, friendly, and happy person one can imagine. She made every one of the graduate students her personal charge. She decorated the office with flowers, and had Christmas
celebrations for the students by providing candies and cookies and a little Christmas tree. She was happy with the office and its convenience—it was her home.

All good things come to an end, however, and one day a woman came into the office, a person Dannelle knew well—Mrs. Woods, I think was her name—with a tape measure and began to measure the wall space in our office. Dannelle was worried and horrified when she found that Mrs. Woods was planning to move in—the first notice that I or she had received that we were being moved. This summary action was typical of that period when much of the responsibility for decision-making fell on the shoulders of Leslie Cowan, who was the business manager in the University. I say this advisedly because he told the president what to do on more than one occasion. He had served under several presidents, controlled the purse strings, so when the president decided he wanted a new rug in his office it was Leslie Cowan who told him he couldn’t have it. It was Leslie Cowan who had decided to move the Alumni Office into our office and move us to the west of the main entrance on the south side of Jesse Hall.

In order to placate Dannelle to some extent, Cowan came in and asked her how she would like to have a rug—a nice rug—in her office. When it came, she was proud of it. I came over to the office one Sunday afternoon and there she was, in her stocking feet, working and just enjoying this lovely rug. On the question of rugs, I’ll digress again. I realize that some of this may seem rather ridiculous but it gives some feeling for the way in which this University operated. After we had been in this office for a number of years, the staff of the Business Office (Cowan’s center of operations) decided they wanted it, so we were moved upstairs into what had been a lecture room for the history department—this space is still (1981) occupied by the Graduate School, just to the east of the balcony in Jesse Hall. We did not have much money, but we did partition off the end of this lecture room to make a good-sized office for the graduate dean and a large room for two secretaries, Mrs. Vera W. Ward who had charge of doctoral programs, and Mrs. Sarah Crane who had charge of the masters’ programs. We put a row of file cabinets across the north end of this room to make space for a typist who did my dictation. We also had the vault to the south of this room where we kept many of our supplies and records and eventually acquired the two rooms farther east. All this leads to the story of the carpet. One day, one of the workmen looked at the carpet we had brought up from downstairs and since it was too small for my office, said, “Why don’t you get a new carpet?” I explained the difficulties. Whereupon the workman said, “Well, I know where there is a good carpet that is in storage over in Storeroom A, and I think you can get
I asked him where it came from and he said it had come from the dean's office in the College of Agriculture at the time the dean moved into the new building at Hitt and Rollins. Apparently, by this time more money was available, at least for the College of Agriculture. The dean not only had a nice-looking office but also a brand new carpet. So the question arose, what to do with the carpet in his old office, which was in Mumford Hall. It was quite obvious that only a dean and the president should have a carpet; the chairman of no department would be allowed to have a carpet in his office. Rather than leaving the carpet in place, Buildings and Grounds took it up and stored it. The chairman of the Department of Animal Husbandry, who moved into that vacated space was not to be treated preferentially to any other departmental chairman. There was even enough carpet for the conference room, which was at the far east end of our operation. At this time, I also laid claim to the room on the south side of the hall where Dean Berry had his office. There was a rumor that this area would go to the Business Office. Well, so much for a brief description of the physical aspects of the Graduate School.

What did I find when I came to the Graduate School? Especially, what did I find with regard to research and stimulation given to it by the University? The minutes of the Research Council state that "The first allotment for the Graduate School for research" was in May 1931 in the amount of $3,000. This was promptly divided up among seven members of the faculty. (We may assume that the depression of 1929 was responsible for there being no further allotments in the years following.) One of Dean Robbins's last achievements was to set up the Research Council. This body was a small committee appointed by the president. We had a budget of $5,000. We made grants to members of the faculty to support their research—a tremendous step forward as far as the faculty was concerned. I remember many members of the faculty telling me that they were eager to do research but there was no money available. After the council was established and we had a small budget, one of these men asked for $300, which he received promptly, but as far as I know he never asked for any additional help. I began to suspect that some faculty members were happy to have excuses for not doing research rather than being as eager to do it as would appear from their conversations.

In addition to the above grants for research, the administration supported the University of Missouri Studies. This quarterly publication was administered by a committee of the faculty for the sole purpose of publishing manuscripts that normally could not be published in journals because they were too long or were of only local interest.

When I accepted the position as dean of the Graduate Faculty, I
hoped to continue with my teaching of chemistry and with my research. I still have the letter I wrote to President Middlebush, in which I expressed the hope that I should feel free to resign if I found that the responsibilities of the dean's office interfered seriously with my efforts to be a chemist. He said he was happy to agree with my reservation, though I suspect he knew deep down in his heart that I would become sufficiently involved in the matters of the Graduate School that I would have difficulty in withdrawing. For many years, I went to the office only after 12:00 M. I say 12:00 M. because, if I wanted to see the president, I usually had to make an appointment for some time between 12:00 M. and 1:00 P.M. It was his custom to go home for lunch at one o'clock, then undress and take a nap for, I suppose, half an hour or an hour, then come back to his office much refreshed. He suggested that I also adopt the habit of taking a nap at noon, and this I did, although mine was usually ten to fifteen minutes and did not quite involve the elaborate ceremony of going to bed. I mention this because I think it illustrates a little bit of the relaxed pace of the period in which we were living. For our president to work through the noon hour was perhaps a little unusual and was certainly inconvenient for those who had to make their appointments to see him. But to be able to take off an hour or more in the middle of the day was an indication that the pressures on the president of the University were not great.

Perhaps I should not seem to criticize the president, because his relaxed attitude toward the concerns of his office was typical of most of the University officers. Many of the full professors retired to the Faculty Club, which was situated in the basement of Tate Hall, at around three in the afternoon, where they played bridge, chess, or billiards. A graduate student showed up at, say, four o'clock, put on a white, freshly laundered jacket, and prepared tea and cookies, which he served to members of the faculty. The atmosphere was relaxed and pleasant. The chairman of one of our departments remarked to us when he finally retired, "Well, Henry, it's been a nice life." By that he meant the life as a full professor had been comfortable and pleasant and not too strenuous.

Turning to the question of the attitude toward scholarship and research on the campus, I observed among members of the faculty a mild form of hero worship for great men among their predecessors. I won't try to mention them all. Many of them had gone on to distinguished careers at older, longer established universities. Thorstein Veblen's was the name I heard most frequently. My first impression was that the faculty, although not especially active in research, cherished the reputation of distinguished members, particularly those who had left the campus. As I became more informed,
however, I learned that many faculty members were quite active in research and were doing work of high quality. They did not seem to be a well integrated part of the total university program, but certainly their activities were viewed with interest and even enthusiasm.

While on this general question of attitude toward scholarship, I should mention that there was a Harvard Club on the campus, composed of members of the faculty who had received their doctorates from Harvard University. Some of them claimed that the University of Missouri was “the Harvard of the Middle West.” Not having received a degree from Harvard, I was not eligible for membership in the usual sense but having taught there for ten years I was invited to the first luncheon of the club to take place after my arrival. As far as I know, there was never another luncheon, and the organization quietly faded away.

Who were the people who were actively engaged in research in 1936 and earlier? Far the most conspicuous was Lewis J. Stadler, who was working in genetics and had an active program of research. Associated with him were several other people of distinction. Barbara McClintock later left the University and acquired a distinguished reputation elsewhere, as did Fred M. Uber, who worked in the Physics Building and was a member of the physics staff but spent half of his time, and perhaps all of his research efforts, working on the radiation of corn pollen in order to produce genetic changes. I am told had Stadler been working on fruit flies instead of corn, his work might well have received the Nobel Prize in genetics rather than Mueller of Indiana. Corn requires a good part of a year to mature a crop while fruit flies propagate much more rapidly. Some of Stadler’s difficulties developed from his having to work in the Department of Field Crops and to compete for funds with more immediately practical research that was being carried on by the College of Agriculture.

Stadler had a corn plot just south of Schlundt Hall, which was attacked one morning by the Buildings and Grounds Department. Stadler came to me in haste to see if I could help him save his corn crop. It represented all or most of the seed that he had saved and developed during years of experimentation. With no concern and no warning, the Buildings and Grounds Department was marching in to take over the plot and tear up this precious crop. This kind of high-handedness was not uncommon in the University, the attitude being that Leslie Cowan and the Buildings and Grounds Department were preeminent, the faculty members and their needs were tolerated, and the faculty’s offices and laboratories were not theirs; they belonged to the University. Of course, there is merit in this point of view, but it was hard to adjust to a situation in which at any
moment you might find window washers or painters coming into your office without warning and moving books and apparatus. On the other hand, the Business Office did most of its work very well. I never missed a monthly check nor do I know of anyone else who has.

Stadler brought to the University national and international recognition. He was president of the national scientific Society of Sigma Xi at one time. Most faculty recognized him as our most distinguished investigator. There were others, however: In geology, E. B. Branson and M. G. Mehl did outstanding work in a specialized field of conodont research. They published extensively in this area. Branson traveled widely—Alaska and Russia, for two instances. He helped to establish a summer camp for geologists at Lander, Wyoming, and typical of the times, he, Mehl, and Walter D. Keller, and Raymond E. Peck did much of the construction of that summer camp with their own hands. The camp added greatly to the University's graduate program because graduate students received their field training at Lander.

Albert G. Hogan, in the Department of Agricultural Chemistry, had an active group of students working with him. His own library and the journals he was able to get from the University library made his collection invaluable to graduate students. He held a weekly seminar and created an atmosphere of research that was of top quality. He himself was working on vitamins and was successful in isolating for the first time one in the B-complex.

Turning from science to English, the man who impressed me the most was Robert L. Ramsay, who was pursuing ongoing research of place names in the state of Missouri. His students produced eighteen or twenty master's theses in this area. These theses traced the origins of the names of towns, rivers, and important buildings of the state. Ramsay maintained that he could reconstruct the entire history of Missouri, if it were ever lost, if he had the place names for reference. People thought rather carefully before they selected a name for their town, and their selection was significant. He also insisted that study of these names was one of the best possible training programs for graduate students who would be teaching in high schools in the state, because they would learn so much of the history of the state of Missouri from studying place names. Ramsay received a sizable grant on several occasions to consolidate these place names. He produced a catalog that summarized the work of many years and many theses; its value as a reference continues.

Turning to medicine: Max M. Ellis was perhaps the most conspicuous member of the faculty as far as research was concerned. He had a grant from the Fisheries Department of the federal government, and at one time, I think, was the man in control of fresh
water standards throughout the United States, as far as the federal government was concerned, in maintaining quality for marine life. He had a number of graduate students working with him at all times. He was a remarkable man, rather autocratic, but a man with high standards who inspired many of his students.

In zoology, Mary Jane Guthrie maintained an active program of studying bats in the caves of Missouri. Through her own work and that of her graduate students, she acquired an outstanding reputation as an investigator.

In chemistry, Herman Schlundt was an unusual man who in middle life developed a new interest after a sabbatical year at the Cavendish laboratory in England in the field of radioactivity. He had an active group of students working with him, many of whom received the doctoral degree. These students were so interested in the program and so inspired by Schlundt that they came back in the summers from other areas and formed a group of eight or ten investigators to continue their research in Schlundt Hall. Schlundt also had a "factory" for separating and concentrating radium salts. Part of the work was carried out in the basement of the old Chemistry Building, and the later steps in purification were carried out on the second floor of Schweitzer Hall. At one time, several of our doctoral graduates continued work in the field of radioactivity with the Radium Corporation of America. William J. Byler, who recently established two professorships at the University, was one of Schlundt's students and Schlundt's graduate assistant in 1936.

Schlundt was a remarkable man and an inspiring teacher. He would take his graduate students in his own car and drive to St. Louis to the monthly meetings of the American Chemical Society. He had the reputation of being a fast driver, which belied his generally placid appearance, but perhaps this behavior expressed a driving force beneath the calm surface that accounted for much of his activity. He went to Yellowstone National Park to sample the waters of the geysers and springs and to test them for radioactivity. When I came to Missouri, his electrosopes were still in Room 21. At one time, I was told, he was considered the principal authority in the United States in standardizing radioactivity, and the Bureau of Standards in Washington sent samples to Columbia for him to analyze. Another example of his devotion and commitment to research was that not only did he use his own money to pay for travel to go to meetings but also he used his own money for equipment. To say that he spent $150 for an electroscope does not seem like much at present, but when full professors were receiving $4,000 a year, $150 was a generous outlay.

In addition to his commitment to research and his enthusiasm for training graduate students, Schlundt had the reputation of being the
best lecturer here. He had the ability in a remarkable way to create enthusiasm in his students. For many years after I came to Missouri, I kept hearing about the wonders of Schlundt's lectures. It was his practice to bring a student up to the lecture table, and together they would talk about something or other that they were trying to understand, and together they would try to develop an appropriate experiment. When they would do this experiment on the lecture table, the person who was most astonished and seemingly overwhelmed by surprise would be Schlundt. He would convey to the whole class the emotions of the investigator who first made the discovery. Schlundt put on this act year after year and students enjoyed it, knowing that it was a show, and it was a great show. He traveled all over the state with a committee that accredited high school chemistry and had a great influence on the quality of work at that level throughout Missouri.

I am giving much attention to chemistry because this is the area in which I am most familiar. Probably similar tales could be told about other departments, but I don't know about them. I am relating the tales about the Department of Chemistry in the hope that they give the flavor for the institution as a whole.

C. W. Turner, in the field of dairy husbandry, was to become internationally known through his work at Missouri. I'm not sure that his reputation was fully established in 1936, but he was an important faculty member who was doing basic work in milk secretion. Samuel Brody, in the same department, was doing work in the energetics of food metabolism and animal growth, which led to the publication of an important monograph.

The sciences were the subjects of more active research than the other areas at the University in part because of the federal funds coming to a land grant institution. In history, however, Charles F. Mullett was regularly publishing articles and probably was better known for his publications than anyone else in his department. The subjects for his research ranged from plagues in medieval England to the analysis of the political implications of the Mother Goose rhymes. All of this research led to publication in journals or in the University Studies series. Prof. Henry M. Belden, who had been chairman of the Department of English, became chairman of the Studies Committee after he retired, and he himself published a two-volume monograph of the ballads of Missouri. This collection was a monumental effort, in which he enlisted the help of many residents of Missouri, former students, and students in course. The collection continues its life as a source of information for folk music.

The record of the Department of Botany suggests a significant trend in our research. Harold W. Rickett was here at the same time as
Robbins, and eventually both men went to New York. While he was here, he published *The Wild Flowers of Missouri*. The reason I mention this is that Rickett was a first-class botanist, but his research was seen to be of little practical importance in the state of Missouri.

There was some feeling that the land grant college’s primary responsibilities were to the farmers of Missouri rather than to science as a whole. I remember it came to me as a shock when Prof. William C. Etheridge, chairman of the Department of Field Crops and one of the most influential members of the College of Agriculture, invited me to go with him one afternoon to see some of the work the members of the department were doing. He was proud to show a field that had been planted according to a double crop system involving lespedeza. To cap his demonstration, he said, “Now the best thing about lespedeza is the fact that you can’t grow it in Iowa because it is too cold in the winter and you can’t grow it in Arkansas because it’s too hot in the summer, and so we have developed something here for the farmers of Missouri, which enables them to more than compete with their neighbors and will not help anyone else.” I think it was the idea that this development would not help anyone else that bothered me and made me wonder to what extent we were a national university.

Perhaps these comments are enough to give some indication of the areas in which significant work was being done at this University. Many people, of course, will be left out of this report whose work was highly significant, but I am recording what I observed at the time and what remains in my memory.

Now, what about the University’s policy concerning research? The University believes that its function is primarily to teach. The Graduate School is referred to at University convocations often with a comment, “Graduate work is the frosting on the cake.” This statement could be interpreted in a variety of ways, but my feeling was that it was only a gesture toward graduate work. After all, the frosting is not the main reason for having a cake and can even be omitted without any serious consequences. This led me to the following justification of the Graduate School: If teaching is the main function of the University, then it is possible to justify research to a large extent as part of the teaching program—the part dealing with the teaching of graduate students. Everyone is willing to admit that the dissertation is the major part of the doctoral program and therefore we can justify the faculty’s doing research themselves in order to be adequate in helping students in the sciences to learn how to do research. I say “in the sciences” because to a large extent, it seemed to me that in the humanities and social sciences the graduate students saw little of the professor while they were doing their
research. Typically, the student chose a topic and went off to the library and worked for a year or two, occasionally consulting his adviser, and then came back to the adviser with a manuscript that would be carefully scrutinized and criticized. He would frequently rewrite his manuscript after consulting with his adviser, but the role of the faculty member's own research was not important to the student.

Research is part of the professional training of graduate students who are preparing to do research. Summertime is often the best time for many faculty members to do research, because the teaching program is less demanding. It was, therefore, part of the effort of the Research Council to give financial help for research during the summer. But this effort went contrary to the point of view of the president. I remember the astonishment with which President Middlebush said to me, "You mean to say you have to pay faculty to do research in the summer?" The implication was that a faculty member had an annual salary and if he really had a deep interest in research he would be most happy to use his summer freedom from class schedules for that purpose. In other words, the faculty member should contribute his summertime to research without any sort of recompense.

The deans of the College of Agriculture occupied a key position with respect to research because most of the doctoral dissertations were in agriculture. I say this in spite of the fact that the School of Education gave more degrees. Many of their dissertations were at the masters' level and even the doctoral studies were concerned with solutions of problems in different school systems rather than with basic contributions to the philosophy or psychology of education. In fact, I can remember the difficulty that I had in securing an agreement with the School of Education that every master's candidate should have on his program at least one course in the psychology of education and one course in the philosophy of education. To be sure, putting a course on the program is no guarantee as to what the student learns, but it seemed to me an important step in the right direction. This was one detail that I watched rather carefully, since I personally signed every program for the master's degree. Frequently, I found that this requirement was ignored.

In the early days of the College of Agriculture, there was considerable interest in basic research. Later, the emphasis came more and more to bear on applied research and especially on problems that were related to Missouri. When Sam B. Shirky, a devoted member of the University staff, became director of research in the College of Agriculture, I had long talks with him on this topic
but was never able to influence him in regard to the importance of basic research in his college. This reluctance on his part continued in spite of the fact that when the Land Grant Act was passed the principle was recognized that there should be three quite distinct functions performed in land grant colleges. One was the teaching of students, a second was fundamental research, and the third was the extension program, which would carry the knowledge to the farmers. There was bitter debate at the time the land grant system was set up, there being many who, like Shirky, would have eliminated the basic research. As far as the early history of the movement was concerned, however, basic research stayed in the picture.

At the time I came to the University of Missouri, President Middlebush was just taking office. I think he had a feeling that Missouri had lost ground with regard to scholarship and something needed to be done to upgrade the faculty. He traveled around the country in search of new faculty. I remember his calling me to his hotel, the Copley Plaza in Boston, to be interviewed, since he was trying to see personally the people he might add to the staff. At that time, I had been teaching and doing research at Harvard for ten years. Other people who were added at this time were Newell S. Gingrich from MIT, who was a promising young man in physics; Leonard M. Blumenthal, a mathematician, who had been at the Institute for Advanced Study at Princeton; and Fred Uber, a physicist, who was brought in to support Stadler's work in genetics.

Middlebush made a conscious effort to bring in from other institutions young men with some research experience. When I came to Missouri, I had been using a piece of equipment that had cost $2,000. Middlebush said he thought money could be found to purchase this equipment. My colleagues in chemistry told me this was the first time that they could remember that any such sum of money had been spent on a faculty member's research.

But then the war began in 1941, and this crisis stopped much academic research for a period of five or more years. No one knows just how much further along we might have been without such obstacles. During the war, many faculty members left the campus to serve with the military or to do war-related research; others devoted their time on campus to such research. I spent only two or three hours a day in the Graduate Office, since I was supervising "confidential or secret work" in Schlundt Hall. There were many other members of the faculty doing the same thing.

One of my assignments from President Middlebush was to survey the faculty to ascertain what the University could contribute to the war effort. At that time, we were scared to death of Hitler. We had no idea whether or not England could hold out, and the attitude toward
the war was that we were gripped in a terrible emergency that required our maximum effort if we were to survive. In retrospect, it is a little hard to believe this, but there is much evidence that it was a conviction of many people. For example, a plant at Weldon Springs was established with equipment sufficient to produce all the TNT required for the war in case England was overrun and could no longer manufacture any high explosives. This indicates how serious the threat was. Similarly, there was a time when the submarine threat was so great it was an open question whether or not we could continue to send men and supplies to Europe. Again, when rubber supplies were cut off there was a serious question whether or not materials could be transported without rubber tires. These problems were all solved in what looks, in retrospect, like conventional ways. At the time, it seemed that the scientific community was barely able to meet the challenge. The rubber problem was solved just in the nick of time.

Many of the faculty were drawn away to do research on problems vital to the war effort. The enrollment in the Graduate School dropped from 577 in 1939 to 142 in 1944. So much for the early history of the Graduate School.
II.

Stimulating Research

After the war, when most of the faculty returned to peacetime occupations, the Graduate Office tried to stimulate research and to improve its quality. These efforts are not listed in their order of importance but simply as they occur to me. One of the simplest means used by the Graduate Office was to issue weekly announcements of the seminars that were being conducted in many departments to facilitate a faculty member in attending seminars in departments other than his own. This effort was met with only moderate success. Out of courtesy, some departments sent us their notices; others were definitely opposed to having their seminars announced. Even A. G. Hogan, in agricultural chemistry, who had perhaps one of the best seminars, felt that it was a personal experience with his students, and he did not welcome people from outside the department. Similarly, Addison Gulick, in physiological chemistry, frequently held his seminars at his home in the evening—an arrangement that was not conducive to visitors' attendance. Nevertheless, for a number of years, the Graduate Office distributed every week an announcement of the speakers and the topics for seminars where these were open to the public.

Another effort to publicize research was a listing of the research publication of the faculty in the back of the Graduate Bulletin. This was done ostensibly to enable prospective graduate students to get some idea of the research that was being conducted at the University and to help them find the advisers who were working in the fields in which they were interested. In addition, it gave publicity to the productivity of faculty members as witnessed by the fact that many were eager to list short book reviews and mimeographed material as part of their publication record. It was a start, and I think a rather successful one.

Another effort of the graduate dean was to stimulate the development of the Faculty Club. This organization had been active in 1936, when I first knew it. Perhaps a hundred members attended its meetings every Saturday night. Usually, some scholarly topic was discussed, and then the meeting would break up into small groups of
a dozen or so for discussion of specialized interests. It was a time when new faculty could meet the deans and other administrators. The president at times came to talk to the faculty informally, and research people exchanged ideas. However, the Faculty Club fell upon difficult days. First, the Law School preempted the club rooms; this dismayed many members of the faculty and made them feel indignant. We had understood that the will had specified that the lower floor of Tate Hall should be a faculty lounge. Without doubt, the Law School needed the space, and the Faculty Club suffered a setback from which it never recovered. We then moved to Lathrop Hall, a resolution of the problem that was unsatisfactory, and, as a result, the Faculty Club deteriorated. From Lathrop Hall, we went to Reed Hall, where the University redecorated our quarters, but it was in the basement with only small windows. It was not attractive. We did have a good little periodical room and some faculty used it, but nowhere nearly as many as in Tate Hall, which was much more pleasant and nearer the center of activity. From there, we moved to the present location, again a basement room, in Gentry Hall.

It was my hope that the Faculty Club would make a significant contribution to the quality of the University as such organizations have at Harvard, Chicago, and California. In recent years, Prof. Ray Lansford gave a great deal of time in efforts to develop a faculty club and especially to construct a building. However, the administration gave little support. Neither the president nor the Board of Curators apparently felt that such an organization is an important part of the life of a university.

One of the big problems in the Graduate School was getting attendance at meetings of the Graduate Faculty. The attendance would be only a few dozen in most cases—perhaps an average of thirty-five. When the only business to be transacted was the voting of degrees, there would sometimes be one or two people present. For this reason, the decision was made to organize a faculty council with two people from each department (elected by the faculty) who would bring to the council the needs of each department. At the same time, it was hoped to attract individuals with broad enough interests who would consider the welfare of the University as a whole. Incidentally, this problem is one of the greatest in the University: to find faculty members for committees who will see the needs of the University as a whole and not view themselves as representatives of a special clientele, which may be small and which may have interests that conflict with the broad interests of the University.

This effort was only partially successful. The council was not given legislative authority. The Graduate Faculty, in the University bylaws, had this authority. It seemed well for it to retain legislative
responsibility for all graduate work. Consequently, the council was set up as an advisory body for discussion, for recommendation, and for study of Graduate School problems. This had much merit because there was no fear that some motion would be presented, a vote taken, and a commitment made that would be injurious to some particular department. It gave a free-wheeling feeling to council meetings, for anything could be discussed frankly, with no concern that a final decision was about to be made. The council itself decided that no formal action would be taken when a problem was presented for the first time. This delay would give the departments the time and opportunity to discuss their own involvement.

In some ways, the council worked effectively. It made recommendations to the Policy Committee of the Graduate School, which was a committee that was appointed by the president and consisted of about ten members. The Policy Committee would consider the implications and administrative difficulties, and if the matter was in the committee’s jurisdiction, it would take appropriate action. On the other hand, if the matter seemed of fundamental importance in the graduate program, it would be taken to the Graduate Faculty for final action. Although the council had no legislative authority, its major recommendations were invariably passed by the Graduate Faculty because by the time the recommendation went that far it had been thoroughly discussed in all departments concerned and conflicts had been well ironed out.

One serious deficiency in the operation of the council resulted from the fact that the faculty continued to feel that its prime responsibility was teaching undergraduates and that graduate work and research were the “frosting on the cake” and did not need to be taken seriously. Consequently the departments did not send their most distinguished or most experienced faculty members to represent them on the council. They treated it frequently as a way in which young men could be initiated into the workings of the University and would begin to learn what the problems are. This practice might be acceptable if but one or two inexperienced individuals are involved, but when a council is composed largely of people who are trying to learn the operations of the University, it cannot be an effective body. Furthermore, as is well known, a new member of a university faculty tends to try to transform the institution in which he is newly employed into the mold of the one where he received his degree. Thus, men from Harvard wanted to make us like Harvard, men from Michigan State wanted to make us like Michigan, and so on. However, formation of the Graduate Council was the first step in making the faculty strongly aware of graduate work. While we are speaking of this problem, it should be mentioned that almost
invariably at meetings of the Committee of Deans, regulations and procedures would be presented that completely ignored the fact that we had graduate students on the campus, many of whom were married. Therefore, the role of the graduate dean more often than not was to remind the rest of the University that we had a Graduate School, with mature students.

A word about the deans’ meetings, which may be interesting. These were started in the late forties primarily by Frank F. Stephens, who was associate dean of the College of Arts and Science. The deans and the president met regularly at the Daniel Boone Tavern for lunch. The group, which included the registrar and Vice-President Cowan, numbered ten or twelve persons. Here was a chance to get really meaningful discussion between divisions. Later on, when the meetings became three times as large, the atmosphere was completely changed.

The next effort to develop graduate work was the formation of the Graduate Student Club. This effort had to be repeated annually because, with the turnover of graduate students, continuity was difficult to maintain. This was long before the era when students expected to make major decisions administratively in the University and to serve on all committees. There was no thought of putting them on the Policy Committee of the Graduate School, especially since it frequently considered student petitions. The Graduate Student Club, therefore, usually started off with a fair number of its membership attending fall meetings. The dean of the Graduate School would often address the group.

The first effort to improve facilities for research was not successful, although it helped in recruiting faculty for many years. This was the purchase of an electron microscope. The electron microscope extends the range of magnification far beyond that of the best microscope using visible light and seemed to be an ideal instrument to have on the campus to stimulate research in biological and physical sciences. A number of faculty members felt this to be true, and I certainly agreed with them. So Missouri had one of the first electron microscopes in the United States. RCA made a dozen or eighteen of these, and we had one of them. The cost was about $20,000, and it was operated by Ray T. Dufford in the Department of Physics for a while. Later, we employed a man part-time to operate it because the early model required considerable attention. It was a large machine with several pumps and had electronic equipment that required much care. Curiously, this acquisition was a failure—because no one used it. I say no one, although Daniel Mazia in zoology used it on some protein work involving monomolecular films. C. Edmund Marshall used it to examine colloidal soil particles,
and Ralph L. Scorah studied metal surfaces by taking pictures of replicas of metal surfaces. But by and large, the faculty was not interested in the instrument or soon satisfied their interest by one or two observations. It would have been far cheaper to go to some other institution for observations. So it seemed as though the greatest value of the electron microscope was in exhibiting it to prospective or new faculty members to show that Missouri was up-to-date in its equipment and ready to support research. I well remember Lewis Stadler commenting that it is a mistake to buy equipment and then expect faculty to use it. What you need to do is "to buy" the faculty and then expect that they will make demands for equipment, which you then try to satisfy. On the whole, it might be said that the electron microscope was a mistake, which Cowan could not forget.

By contrast, our liquid air operation was successful. Liquid air is essential for many types of research in chemistry and physics, and when Earl A. Long came to the campus, he and I built much of the equipment, with Earl doing most of the work. We set up a small Rix compressor in the basement of Schlundt Hall and, for many years, supplied the chemistry, physics, and other departments with liquid air at nominal cost. It had been not only expensive but also awkward to have liquid air shipped in from Saint Louis, especially since we could never be quite sure when it would be needed. The demand was not great enough to justify keeping a large supply on hand, and the transportation frequently involved loss of liquid air and sometimes injury to the containers.

The faculty took the initiative in developing a glassblowing service. This effort started out on a small scale, employing a graduate student in chemistry (David Lyon) who had received his first instruction in glassblowing from me. Lyon turned out to be a far better glassblower than I and was able to assist the faculty in many ways. He went to California, where he became a professor of chemistry. The glassblowing shop grew and eventually had a full-time employee and became an important part of our research services.

In 1937, we had no machine shop or instrument shop for research. Gus Tornsjo, whose workshop was located in Storeroom A near the Power Plant, built prosthetic devices for the Missouri Crippled Children's Service. When Gus wasn't busy, it was possible to get him to assist in the construction or repair of mechanical equipment for graduate faculty doing research. Eventually, John Snell was installed in the Physics Building (then Stewart Hall) and Prof. Newell Gingrich took on the administrative responsibility. Later, the shop was moved to a location near the Power Plant and still later to Research Park.

The Graduate School assisted research in other ways, such as
providing microfilm readers and dictaphones that staff members could take to the library for note taking. Our funds were so limited that we felt we could not afford to assign a dictaphone permanently to each person who might want one, and therefore a storeroom in the Graduate Office housed a number of pieces of equipment for loan.

Another effort that was quite successful was the support of faculty members with research appointments. The idea came from a faculty member, Lloyd Thomas, and it illustrates the fact that a graduate dean can often be most effective if he keeps his ears open and finds out what the faculty needs rather than handing down decisions. Thomas’s original idea was that faculty members who would have to seek employment in the summer because they were receiving low salaries as assistant professors could under this program receive several hundred dollars, essentially as a fellowship. This money was to be given without restrictions and therefore was not taxable by the Internal Revenue Service. These grants greatly assisted many faculty members. One of the difficulties in carrying out the program was that the committee that administered these summer research fellowships consisted of senior members of the staff. It wasn’t long before the full professors were asking and receiving help during the summer, to the detriment of the assistant professors who needed the money more.

Still another research support was travel money to national meetings of scholarly societies. This money was not administered by the graduate dean but by the deans of the divisions, probably for two reasons. One, the administration felt that the Graduate School in some ways was kept purer if it were separated from contamination by dollars. Also, the divisional deans enjoyed the responsibility of giving this money to departments. The travel money for research customarily paid for transportation but not for the hotel, room, or meals.

Another effort the Graduate School supported strongly and helped to put into effect was the granting of sabbatical leave on full pay for one semester. The difficulty with sabbatical leave on half pay was that salaries were so low that faculty members could not afford to take off a year to do research. Sabbatical leave on full pay was therefore set up with the stipulation that the recipient would have not only the semester free from his University duties but also the summer before or after, which would give roughly nine months for research. But in accord with the general attitude of the University, the faculty member was supposed to give the summer without recompense. Eventually, this impediment was eliminated, and it was possible to give a summer research appointment following a sabbatical leave in order to make it financially more attractive.
All of the above activities were more or less under the eye of the Research Council, which made the final decisions on all grants. The council was composed largely of faculty, but included one or two deans. Another project of the Graduate School was the establishment of a distinguished faculty award by the alumni, to recognize outstanding research achievement. This effort required several years of negotiation. President Emeritus Middlebush, in charge of alumni gifts, supported this idea, and finally it became a reality. However, administration of the fund was given to other members of the administration. As a result, the award ceased to be an award for achievement in research but rather for general service to the University. This was a loss as far as stimulating research was concerned.

The greatest achievement of the Graduate Office during these three decades was in connection with Title IV of the National Defense Education Act (NDEA) program. The provisions of this federal program offered an extraordinary opportunity for federal support when state funds were meager. Here I should point out that at no time did the University go directly to the legislature for support for the Graduate School. When Title IV was first established, the faculty was encouraged to submit applications for grants. A great deal of effort by both faculty and the Graduate Office was spent in drawing up the best possible requests. Zoology was outstandingly successful; Warren R. Fleming and Clinton H. Conaway were successful in obtaining the award of eight fellowships the first year. It was one of the largest grants in the United States. These fellowships were the best ones we had in the Graduate School: $2,500 went to each student each year for three years. In addition, $2,500 went to the University to help support the training of graduate students. To improve the quality of graduate work, it was often necessary to bring in additional staff or to add equipment.

The following year (1959), I was invited as graduate dean to go to Washington, D.C., to administer the award of the first fellowships and to plan the program for the following year. This assignment seemed an ideal way to learn how to make the best use of the program and perhaps to make some contribution to the development of graduate work nationally. The experience was useful to the University of Missouri. For several years, Missouri had approximately as many fellowships as any institution in the United States. Where previously we had less than a dozen fellowships, we now had fifty to a hundred.

In order to make this program effective, President Elmer Ellis allowed the overhead, as the $2,500 to the University was called, to be administered by the Graduate School, and we spent a great deal of
time approving budgets for the individual departments. We did not assume that the new programs created by the departments required precisely $2,500 for each fellow enrolled. In this way, we could lump together the government subsidies to buy, for example, an expensive X-ray camera for geology or to hire a new staff member in engineering or industrial arts or in some other field. Then, for several years, that department might not receive funds. It was not always easy to persuade a department that it should not have full use of the funds that came with its students, but on the whole, I think, we set up an effective operation.

One measure of judging its effectiveness was that quite a number of departmental chairmen (around six) eventually came from among new staff members who were brought in under this program. Often the dean of a college would supply most of the salary—perhaps all of the standard salary—for a man of a given rank and then the NDEA program would add a few thousand dollars to make it attractive enough to bring in a top-notch individual. This was a tremendous boost to the morale of the faculty and also to our graduate program.

I add here that for many years, in fact from the beginning of the Graduate School, there was no out-of-state tuition charged to graduate students. This was equivalent to granting a fellowship automatically to every out-of-state student. As a result, we attracted many students who would not have come here otherwise. It was our fear at one time that we might suffer seriously if this assistance were withdrawn. However, the results in the School of Journalism of charging out-of-state tuition are not as serious as we feared. Perhaps after an institution once is established at a fairly high level of graduate work there is not so much danger of losing students; its momentum seems to carry it on.

An advantage of this freedom from tuition was that we felt a little freer to dismiss students when their work was not of high quality. As graduate dean, I personally looked at the grade record of every graduate student at the end of each semester and wrote letters either warning or expressing "dismay"—if that is the proper word—at the quality of work and asking for a conference, if the student wished one, in order to discover what the trouble might be. If the below-standard work was repeated a second semester, the student was dismissed. This was an alternative to the idea of having a grade point average required for the admission of students.

There are basically two philosophies in graduate schools with regard to quality of work: One is that you accept only students with good records and in this way hope to have good graduate students; the other is that you are fairly free in admitting students but watch their records and are careful to dismiss them when they are not
performing satisfactorily. The war experience tended to reinforce the second point of view, since there were many students who had a good time and low grades during their undergraduate work but were able to get down to serious work in graduate school. Clearly, several years of maturing and the realization of the importance of an advanced degree are often more significant than an undergraduate record.

To continue with discussion of the efforts on the part of the Graduate School to encourage and stimulate research, the next item probably should be computers. Some of the incentive for this effort came from the School of Engineering, where there was a considerable demand. Dean Huber Croft urged strongly that the Graduate School investigate this need, which we did. For some time, we did not have the funds available for a computer, but we did manage to make arrangements with the State Farm Insurance Co. It had an IBM 650, which was a first-class computer at the time. Charles E. Beedle was vice-president of the company, and he was most cooperative, allowing us to use it after work hours. This arrangement continued for a few years while we were trying to decide what equipment we wanted and were searching for the money. The University was not prepared to or did not have funds to put into computer equipment. Finally, we went to the National Science Foundation for assistance and found that they were unwilling to commit themselves to an operating budget for a computer but they would match funds with us to buy equipment. This put us in a real bind because the equipment that seemed to be the most widely used and perhaps the most satisfactory was made by International Business Machines Corporation, and the policy of this company was not to sell but rather to lease computing equipment. All of the IBM equipment used by John B. Combs was leased. The equipment used by Combs was primarily in connection with registration and the Business Office. When it came to research computations, although we used some of his equipment, it seemed evident that we needed a different type. After long discussion and a careful study by a committee, we finally chose the Datatron 205 as the one we would like to buy. The National Science Foundation matched our funds; the total cost was $75,000.

Next was the question of where to put the computer. We wanted a central location and space enough to make the equipment readily available. A new building for the School of Business and Public Administration was under construction at this time, and therefore we decided this would be the best place. We could plan the building with a substantial floor at the ground level where the foundation would carry the weight. We found a graduate student who was competent to operate it; that we could not afford to employ a full-time technician.
gives some idea of how limited our budget was at that time.

This facility served well for several years. Our basic plan was to have one computing center for the entire campus, including engineering, medicine, and agriculture, and that the Business Office would have its own computing equipment, which would be different in character.

Much support for research came from the College of Agriculture. Sam Shirky contributed to the development of research in many ways. He never lost the spirit of team play, which he displayed as an undergraduate. He was a member of the basketball team at a time when one man did all the free throws. This was Sam for our team, and he has a remarkable record. When he became director of research, he helped develop services that were available to the entire research staff. The spectrographic laboratory, financed by money from testing of fertilizer, was an outstanding contribution. High-quality men acted as directors of this service, the last being Edward E. Pickett. He and his assistants made important contributions to the research of many of the faculty. Without the support of Shirky, all of this would have been impossible.

When the matter of computers arose, Shirky again was of tremendous help. He had obtained a computer for the College of Agriculture, but supported strongly the idea of a more powerful machine for the entire University.

Printing and publication became a responsibility of Shirky's that he managed with great success. In many ways, this contributed to the research effort of the faculty.

Homer Thomas served on the Library Committee for many years, much of the time as chairman. It would be hard to overestimate the contribution he made to building up our book collection. With little or no financial support from the University, he managed to make trips abroad and to set up procedures with dealers that greatly facilitated our purchasing and especially in completing our sets of learned journals. His meticulous records on our gaps in sets were invaluable. The dedication and time involved were great and the results have been impressive.

The general facility for the support of research that should be mentioned here is the reactor. The background for this equipment is interesting in showing how the legislature and the University respond to a situation created by a scientific discovery. The motivation on the part of the University at Columbia and also on the campus at Rolla was to become up-to-date and involved in the latest type of research. A related anecdote concerns Harry A. Curtis, who was dean of the College of Engineering (1938-1948). He took the Department of Chemistry to task because it included in the
fourth-year course of theoretical chemistry some discussion of the structure of atoms and the composition of the nucleus. Curtis’s comment was that engineers don’t want to know anything about atoms or the nucleus of atoms; we ought to be teaching them things they could use, such as the nature of surface tension. Only a few years later—ten years, perhaps—after Curtis had left the University we had the Department of Nuclear Engineering in the College of Engineering. This instance shows the need for caution in predicting the future of scientific research and what is going to be important. The basic sciences are inclined to stress those facts and theories that are of broad general importance. On the contrary, the applied areas, such as engineering and parts of agriculture and of medicine, are more interested in immediate results. Probably a balance of the two creates a stronger University than to have either one dominate the picture.

The administration at Rolla was rather aggressive at about this point and secured support from the Atomic Energy Commission for a modest-sized reactor. This was quite a blow to the College of Engineering at Columbia. It seemed to indicate that it was not progressive and could not compete with the program at Rolla. There had long been rivalry between these two engineering schools within the University. So it was natural that Dean Croft should become active in trying to get a much larger reactor at Columbia than was at Rolla. A committee was formed to study the problem, with the dean of the Graduate School and Newell Gingrich of the Department of Physics as members. Gingrich was in a particularly favorable position to give advice since he had spent a sabbatical year at MIT, where they had a reactor. It was evident that a reactor was going to be an expensive machine, not only to build but also to operate. Gingrich and I met in 1959 when he was in Boston and I was in Washington, D.C., to discuss the need to add faculty in many departments if we were to have a reactor properly used. The emphasis on cost was an unwelcome factor as far as Dean Croft was concerned, and he was rather indifferent, it seemed to Gingrich and me, to this aspect of the problem. It was not long until both Gingrich and I were dropped from the committee.

The rest, of course, is well known. The reactor was built. It is interesting that the legislature, which we so often accuse of being unwilling to support higher education in the state, seized upon this opportunity as a way of making the University of Missouri distinguished. The question whether or not the whole project was desirable is difficult to answer. When Bryce Crawford of the University of Minnesota came to Columbia to evaluate some of our scientific work and especially the department of Chemistry, his comment was,
“Don’t you think you might sell the reactor?” I suspect he was simply indicating that we had a white elephant that would never be adequately supported and would not make a significant contribution to graduate work and fundamental research on campus. We did hire new staff: in physics Horace R. Danner; in chemistry David E. Troutner; and others with the reactor in mind.

The decision for a department to offer a Ph.D. program is basically an administrative responsibility. A department that wants to grant the Ph.D. degree can hardly be impartial in its assessment of its own qualifications. Therefore, the Graduate School established a procedure whereby the University brought in a consultant to help in this delicate matter. We asked the Council of Graduate Schools to nominate a suitable individual. Their consultant was to study the situation for two or three days and was to write a report indicating the strengths and weaknesses and what was needed in order to give an adequate doctoral degree. Then the whole report was to be presented to the president, since significant amounts of money were usually involved, not only for new staff, but frequently for equipment and often a sizable amount for the library. This procedure was laborious. However, our departments went along with it, often utilizing the visitor as a special pleader for their interests. When this was the case, departments were sometimes recommended strongly for the doctoral programs whether or not they were qualified. On the other hand, sometimes a visitor would indicate a totally inadequate program with too few students and too few jobs available to make it worthwhile, and yet the final approval sometimes went through for such a department. When the recommendation from the consultant was negative, it was not uncommon for the dean of the division to go directly to the president and to receive approval for the program. The outcome was a bit demoralizing for the operation of the Graduate School.

The situation just described illustrates the basic difference between the role of the president of the University and the role of the dean of the Graduate School. The former, in his efforts to secure funds, must have confidence in his institution. His task is easier if he can say and believe that his departments are of high quality. The latter, on the other hand, is keenly aware of the need to worry continually about quality and to take every step possible to improve the quality of those departments giving the Ph.D. degree. The president hears the evaluations of departmental chairmen and especially of deans of divisions, all of whom are anxious to present a picture of successful achievement on their part; the dean of the Graduate School hears the comments of individual faculty members and students not only in his own institution but also in other
universities that he may visit. These evaluations may be quite different from those which come to the president.

The president is optimistic and convinced that the efforts he has made have been successful—that the institution is now much better than most people realize and that in another ten years its reputation will be greatly improved. The dean is pessimistic and frustrated as he sees one after another of the best research faculty members leave for other institutions. This attitude reflects what he hears from the faculty and from the chairmen of some departments. Both points of view have their merits, but to harmonize them is not easy. It is an unusual president who will remark, as did Robert Hutchins when he was president of the University of Chicago, that "the University of Chicago is not a very good university but it happens to be the best in the United States."

To continue with the contrasts, the president is pleased to have the evidence of healthy growth furnished by new Ph.D. programs, new departments, and new schools. The graduate dean is dismayed at the creation of new demands for faculty, space, equipment, and library acquisitions when even our best departments are suffering for lack of such support.

Adequate support for a department may mean to a president fairness with respect to other departments with the resources available to the institution, approval of a department by an accrediting agency, and the presence of an adequate number of graduate students. On the other hand, the graduate dean may believe that "fairness" is not a relevant criterion. Even Harvard, to quote President James B. Conant, does not have the resources to be superior in every field. He believed that we must select the areas for graduate work, pointing out that Rice Institute achieved distinction when it had but three or four strong departments giving the Ph.D. degree. As for accrediting, the graduate dean will feel that this is little more than a floor below which we should not sink—a license to operate but not a definition of the ceiling to which we should aspire. As for the number of students being a measure of the prestige of a department, the graduate dean recognizes that students sometimes seek the degree that can be attained with the least effort rather than the one that will represent the greatest achievement.

By 1955, it was evident that graduate work would prosper throughout the country and that if an institution were to take advantage of federal funds to be distributed it would be well to strengthen its program as much as possible. After returning from a year in Washington, D.C., and working with the Title IV of the NDEA, I had a long conversation with President Ellis. It was my contention that if we wanted to take advantage of the opportunities
Title IV offered, the University of Missouri should make its graduate program the top priority for several years, that even with such an effort we would at best barely reach our goals. His response was that the University was much better than I realized and that time would justify the belief that we had departments that were superior to those in many well-recognized eastern universities. As a result of this kind of reasoning, the position of the graduate dean became increasingly untenable in any attempt to limit the number of Ph.D. programs on the campus. The only result of trying was friction between the Graduate Office and the departments in question. Ultimately, the department achieved its objective. Therefore, gradually the gates were opened, and more and more departments were authorized to grant the doctoral degree.

To conclude the discussion of the organization and operation of the Graduate School, a word might be said with regard to the Policy Committee, which was appointed by the president and was intended to advise the dean of the Graduate School and to act on graduate students' petitions, which if granted would violate various rules and regulations. Such petitions, if presented to the faculty as a whole, would have been time-consuming for the faculty and would have involved great delay for the student. The Policy Committee eventually became a difficult committee to work with because the president often selected its members on the basis of recommendations from divisional deans. Sometimes members of the Policy Committee selected in this way felt no particular loyalty to the Graduate School or its dean but were merely promoting the interests of their divisions. It was hard to get a committee that would view problems from the standpoint of the University as a whole. From the standpoint of administration, it would seem that one would have a more smoothly functioning committee if the dean of the Graduate School, who acted as chairman, were allowed to make the appointments with recommendations and suggestions from the president and the various deans.
III.

Faculty Participation

The bylaws of the University assign to the Graduate Faculty the responsibility to establish degree requirements and the rules and regulations of the Graduate School and to the dean of the Graduate Faculty and his staff the responsibility to administer these regulations. In practice, there was some overlapping of these two functions, and, as a result, faculty members were able to participate in the administrative procedures.

In order to obtain much input from the faculty, and at the same time conserve their time, the procedure we followed was to distribute to members of the Graduate Council the agenda for each upcoming meeting of the Policy Committee. This agenda contained names of people who were petitioning and thereby gave members of the faculty an idea of what sort of requests were being made. Later, they would learn what actions had been taken. It was the privilege of faculty members in the council to initiate discussion and suggest changes the Graduate Faculty as a whole might initiate. The basic idea was that no action by the Policy Committee was necessarily final; if there were difficulties or objections of which the committee was unaware, then the following month the question could be considered again and perhaps contrary action taken. In my opinion, this was an efficient way of operating the graduate program. Only those members of the faculty who were concerned about the problems needed to spend time on them, but there was an adequate mechanism for any faculty member to talk with members of the Policy Committee or with the dean either before or after action had been taken. As Dean M. F. Miller of the College of Agriculture once said, “It’s a lot better to have the dean take action promptly without wasting the time of departmental meetings or faculty meetings, and then feel free to change the action in case it turns out that hardships or undesirable results are a consequence.”

Many of the problems the Graduate School encountered were discovered or solved by faculty members as individuals, and the
development of the graduate program rested upon this broad base of cooperation. Some of the men and the issues in which they were involved included Frederick B. Mumford, who was dean of the College of Agriculture in 1911. According to him, one of the problems that was worked on by the Department of Animal Husbandry was what sort of shelter to give cattle in the wintertime in Missouri. The experiment consisted of having three groups of cattle: one was given good housing; another was given only enough shelter to ward off snow and rain; and a third was given no shelter at all, being allowed to fend for themselves and to seek shelter under trees and shrubs. The outcome was that the unsheltered cattle fared better than either of the other two groups, perhaps in part because the sanitation problems in barns might have fostered disease. However, Dean Mumford laughingly told me, “It wasn’t possible to publish a bulletin describing the results of this experiment because the experiment station was so new and the results of the experiment so contrary to the opinion of most farmers that they were sure that the farmers in the state would say, ‘Well, this shows how crazy those people are up there at the University. They don’t understand what agriculture’s all about.’” Although the results were conclusive from the experiment, farmers were not informed of the findings. Mumford was a forceful administrator. Dean Winterton C. Curtis used to enjoy describing the College of Agriculture as not a democratic organization. When Mumford did not appear at a general faculty meeting, the members of the College of Agriculture didn’t know how to vote because they hadn’t been instructed by their dean.

Winterton C. Curtis was important not only in the development of the Graduate School but also in the University as a whole. Curtis came from the Johns Hopkins University around the turn of the century and was committed to building up the University in quality in every way possible. When he discovered Lewis Stadler, he wholeheartedly supported him. He succeeded in persuading the president to supply the funds to bring Uber into the Department of Physics to operate a spectrometer that was capable of irradiating pollen grain with ultra-violet light of variable wave lengths in order to artificially produce mutations. Curtis was also largely responsible for seeing that there was a good geneticist in the Department of Zoology and another in the Department of Botany. Stadler himself was in the Department of Field Crops and did not wish to set up a department of his own because he was quite willing to have Etheridge do all the bookkeeping.

The building that now houses genetics, Curtis Hall, has an interesting history. The Department of Chemistry had been having difficulty for many years because it was housed inadequately. In the
first place, its buildings were scattered over the campus. The old Chemistry Building, now the Museum of Art History and Archaeology, was in poor shape. The ventilation was entirely inadequate—in fact, many students threatened to sue the University because they felt their health had been injured by working in basement rooms with small windows and with no exhaust fan; basement floors of were rough boards, in some places missing, so that only the dirt floor was underfoot. Analytical chemistry was located in Schweitzer Hall on the White Campus and was isolated from the rest of the department. Prof. Gerald E. Breckenridge carried on practically all of the teaching in qualitative and quantitative analysis, sometimes with the help of one postdoctoral appointee. Physical and general chemistry were in Schlundt Hall, not too far from Schweitzer but still far enough to interfere with communication, especially when faculty time was heavily involved in teaching and in laboratory supervision. After World War II, the department was so crowded that the Schlundt Annex was added. In recent years, a new chemistry building has been added. When I came to Columbia in 1936, Leslie Cowan told me I was the most fortunate person on the faculty because chemistry was at the top of the list for a new building and Professor Schlundt was nearing retirement, so I had every opportunity to step into chairmanship of the department. Decades elapsed before chemistry had a new building partly because there were other needs for new buildings, such as art, music, psychology, home economics, veterinary medicine, and social work. The University erected a host of new buildings, and chemistry still remained at the top of the list, but that favorable position apparently was meaningless. Finally, the legislature voted money for the Chemistry Building, but before it could be started, Curtis, then dean of the College of Arts and Science, discovered that there was an opportunity to get matching funds—from the Rockefeller Foundation. So without any discussion with the Department of Chemistry, as far as I know, he went directly to the president, and, as a result, the money was used to build a new genetics building. For many years, it bore no name for fear the legislature might wonder why the money had not been spent for the designated purpose, and also it would be hard to explain why chemistry was back for building when money had already been appropriated for one. What should the Department of Chemistry do in this situation? I think there was little comment or criticism. We had all admired Stadler and his work though we were deeply disappointed that we were forced to continue with almost impossible arrangements, as far as administration of the department was concerned. The chairman had to walk many blocks almost every day in order to have contact with the members of the department.
Two members of the faculty suggested improvements in our grading of students. This seemingly minor issue is the kind that faculties often spend a large amount of time considering. When I came to the Graduate Office, we had two grades, Pass and Fail. It was in my first or second year as dean that two faculty members came to me to protest our system and to complain that it was quite inadequate. One of these was Harry Gunnison Brown, who was one of our most distinguished teachers and scholars in the field of economics. Brown told me of an incident in which two of the best students in his department had applied for graduate fellowships at Yale University (the school from which Brown himself had received his degree). Neither of the applicants received a graduate fellowship. Brown was incensed, and at his first opportunity asked a faculty member from Yale why they had turned down our applicants. Their comment was, "Well, there was nothing distinguished about the records of these students; their grades were just Pass, and there was no way of discovering whether they were really outstanding." The fact that Brown had described them as the best students he had had in some twenty-five years of teaching apparently did not carry much weight.

At that time, I think most faculty members believed that letters of recommendation had to be taken with a good deal of salt and were much inferior in significance to the judgment expressed in grades awarded by a large number of faculty members when not under pressure. I served for several years on the committee for the National Academy of Sciences in evaluating applicants for the National Science Foundation awards. Here it was true that letters of recommendation were frequently a laughingstock because it was so transparent that they were written under pressure to help students who needed fellowships. On the other hand, grades were given a great deal of attention because they represented the judgments of a larger number of people over a considerable span of time, long enough to show the student's continued interest. Finally, pressure on the faculty member to give high grades was not as great then as it became later.

The second criticism came from Prof. John Rufi, who was the most meticulous of any faculty member I have known in keeping records of his students and in securing placement for them after they had earned degrees in the College of Education. I remember Rufi taking me into his office and proudly showing me the files that he had for each of his students. When a former student received a promotion and moved from one high school to another, it was usually on the recommendation of Professor Rufi, who was responsible for the program for training principals. Sometimes as many as five other
former students would profit by this one move, someone moving into the vacancy created by the first move and a third going into the second's and so on, so that a whole chain of events was started as a result of one recommendation by Rufi.

In order to do this, Rufi had to know a great deal about his students, and he did, for he kept careful records. He came to me and said that the time and labor involved were burdensome because twice a year, in June and February, he went the rounds with his records of the registration of each of his advisees—I suppose he had fifty or more—and consulted with the teacher in the course to get the ranking of that student in each course. Thus, he had a fairly accurate measure of his students; he would not be recommending an inadequate student for a position he could not creditably fill.

Rufi's contention was that the Graduate School needed a better system of grading students in order to reduce the work of obtaining information. I appointed a committee that consisted of Harry Gunnison Brown and Leonard M. Blumenthal to study the problem and to make recommendations. Up till this time, the undergraduate school had an E, S, M, I, and F system developed by Prof. Max Meyer of the Department of Psychology. Meyer had developed this system in order to get away from a skewed type of grading in which A's and B's were given to almost all students. Meyer's system proposed that on an average over a period of years, something like 25 percent would be A and B, about 50 percent C, and about 25 percent D and F. This system was used in our undergraduate schools. It was confusing to anyone off the campus because no institutions in other states used this system. Many of the state schools in Missouri, however, did adopt it because many of their students planned to come to the University of Missouri.

We decided that for the Graduate School we would use A, B, and C. Our first thought was that we would have only a pass and a high honor grade. It was realized, then, that some students might want to take courses outside of their major; under the pass-fail system or with an A and B system, there would be difficulties because a botany student with few previous mathematics courses would not be held to the same standard as a mathematics major. As a result, professors had a double standard for grading. Graduate students in a given department would be graded in one way, but the same grades would be given for less achievement to a graduate student from another department. To avoid this confusion, we decided that a C grade would be the appropriate grade to give to a student who was not doing as good work as might be expected of a major but still adequate for a graduate student working in a field outside of his major. This decision made it possible for the unusual student who was doing
outstanding work in an outside department to receive an A or B grade. It was also realized that sometimes a graduate student who had an inferior undergraduate major might not be able to meet our standards. The C grade was adopted for these two reasons: to take care of the student who was taking courses outside of his major field and to take care of the student with inadequate preparation. We limited the number of C grades that could be counted toward a master's degree to 20 percent. The A grade was originally intended for the outstanding work of a student who was performing far above average.

It was sometimes suggested that an A grade should go to the student who was doing work of such quality that you would be happy to recommend that student for a position on the faculty at the University of Missouri after he received his degree. Like all good things, the grading system was modified by the faculty in actual operation. Sometimes misunderstood, new faculty members would try to use the letters in the same way that they had been used in the institution from which they came. Constant effort was required to keep the system reasonably consistent. Every year or so, we would send to all faculty members a list of grades given by them and by all members of the graduate faculty in order to show if and to what extent they were out of line in their assignment of grades. It was hoped that when a faculty member saw that he was using the grades in an entirely different way than other members he would change his procedure. Wide variations existed. In some departments, only A grades were given, while in other departments there would be an abnormally large number of C grades. All these factors had to be considered when looking at a student's record and deciding whether or not he should be eliminated from the Graduate School. After some thirty to forty years, the grading trend again is leaning toward Pass and Fail.

Another, perhaps trivial, matter is the kind of thing faculty can debate at great length: course numbering. When it became apparent that the numbering system we were using was cumbersome, we undertook to devise a new and better system. To show how important this matter is to some faculty members, I was told that it would be impossible to get a system that we proposed for the Columbia campus to be adopted at Rolla because a certain distinguished professor of history, who was influential at Rolla, gave a course in American history and the number for which was 1776. Any system that would not allow him to keep that number would be opposed by him and therefore not adopted.

What did we have before we made the change? We had courses from 0-99 that were primarily undergraduate and courses from
100-199 that were upperclass and in part graduate courses; not all of these upperclass courses carried graduate credit. Those that did carry graduate credits had an asterisk after the number, which led to no end of confusion. The asterisk was frequently forgotten or not noticed and so a graduate student would take courses, recommended by an adviser, that had not been approved for graduate credit. A student might fail to qualify for a degree even though he had fulfilled the requirements. After discussion with other divisions, it was agreed that numbers 0-99 and 100-199 would be adequate to take care of our undergraduate needs. We then decided that courses for graduate students would be numbered 300-499. It was expected that graduate students on occasion would take courses in the lower series without graduate credit and, similarly, undergraduates if well prepared might take graduate courses. The numbers in the 400 series were courses that were strictly for graduate students with no undergraduates admitted except on rare occasion, on petition. It was thought that the quality of graduate work would be improved if all of the students in some course had an undergraduate major and were serious enough about their work to be working for an advanced degree. On the other hand, the courses in the 300 series might be taken by upperclassmen, and, therefore, it was expected there would be both undergraduate and graduate students in these courses. Finally, the courses in the 200 series were upperclass courses that usually were required for majors. A student taking a course outside of his field might expect to get graduate credit for a limited amount of 200 course work. This made it easier to advise students, look at the program, and see whether or not it was truly advanced work or simply a continuation of beginning work.

To illustrate the problems we were encountering: students came back to the campus after earning a bachelor’s degree and, with change of interest, took a series of courses open to freshmen. For these courses, they hoped to get graduate credit. In fact, some faculty members would assign a graduate student to a problem course, say 350 (we saved this number for a problem course), and then the adviser would assign as the problem the taking of a freshman course; the student, thus, was getting credit at the 300 level for freshman work. The argument was that this was the most valuable thing the student could do. He had, for instance, never had a freshman course in chemistry; he wanted to take graduate work in dietetics and obviously needed some chemistry. These were some of the problems we tried to solve by course numbering.

The most important factor in a distinguished graduate program is the quality of its research. This problem was attacked after we had created the Graduate Council, because this body made it possible to
reach conclusions and to make statements to the faculty that would have been quite unacceptable if they had come from any administrator. Prof. C. Edmund Marshall (at that time in the Department of Soils) was a highly respected member of the faculty. He undertook the task of making a thorough study of the research productivity of our faculty and of comparing it with other schools of comparable standing. This “Marshall study” showed that we were falling far behind the achievement of many other schools and that our research activity, although in some departments strong, in the institution as a whole was deplorably weak in comparison with other universities. A few years later, Joseph E. Flynn, of the School of Medicine, brought the report up-to-date. His conclusion was the same as Marshall’s. We decided the first step in upgrading our graduate work was to create a more selective Graduate Faculty. The Graduate Faculty in the past had consisted of those members of the faculty who gave any courses taken by graduate students. As a result, when I came as an associate professor in chemistry with the assignment to handle the beginning course, I gave no course taken by graduate students and was not a member of the Graduate Faculty although I had doctoral candidates working with me. The first time I attended a Graduate Faculty meeting was when I presided over the meeting as dean a year and a half after I came to the Columbia campus. One of the mistakes I made early in my administration was to include in the Graduate Faculty all of those who were involved in teaching graduate students, both at the master’s and doctor’s levels. This seemed desirable for a number of years when we were putting little emphasis on the Ph.D. program and on research. Most of our graduate students were master’s candidates. But along in the fifties, when it appeared that we needed to place more emphasis on research, it seemed desirable to change the nature of the Graduate Faculty. We decided that the best way to do this was to limit the Graduate Faculty to those members of the staff who were actively engaged in research. Thus, many advisers of master’s candidates would not be included in the Graduate Faculty; the master’s degree was well established and seldom created problems for the Graduate Faculty. The important problems had to do with candidates for the doctor’s degree. The most significant reason for limiting the Graduate Faculty to those active in research was to insure that the advising of Ph.D. candidates would be by members of the Graduate Faculty. This limitation was important because it ensured that a doctoral candidate would have an adviser who himself was at least competent in and interested in research. Heretofore, being a doctoral adviser seemed to be a matter of pride for many faculty members who did no research themselves. In some cases, the student did not receive the best training. A second reason, and more
important, was that we were trying to stimulate the faculty to do research by giving them recognition and honor in making them members of the Graduate Faculty when they had shown productivity in research. The question, of course, arose as to how to get this measure accepted by the faculty as a whole and then how to select the Graduate Faculty. Individuals that had to vote on the new regulations would be in part eliminated from the Graduate Faculty if they were adopted. Another difficulty arose when the chairman of a department did not have a doctor’s degree and had done little if any research, at least of a basic nature. There were a number of these situations and they were not easily handled. For instance, Chancellor John Schwada stated that he must be on the Graduate Faculty even though he had no intention of having any doctoral candidates. The problem of trying to convince faculty members that membership in the Graduate Faculty was not simply an honor but had a functional meaning was more difficult when the chancellor himself did not agree with this point of view. It would have been much easier to persuade faculty members that we were trying to describe a function if we could have said, “Not even the chancellor is a member of the Graduate Faculty because he is not able to function as a student adviser.”

The only way it seemed possible to make this transition was to have a grandfather clause, which included in the Graduate Faculty all those who in previous years had trained doctoral candidates. Even though some were left out who had had only master’s candidates, the new definition was adopted without undue opposition. The next question was how to select the Graduate Faculty. This was done by a committee of the faculty itself—a committee of fifteen to twenty members representing as many fields as possible. The faculty member to be considered was to be nominated by the chairman of his department. We even allowed individuals to apply on their own initiative, in the fear there might, in some instances, be a chairman who did not wish competition within his department. At any rate, the applicant was to submit copies of his publications, which this faculty committee would review. We broke up into subgroups so that the people recommended in science would have their publications reviewed by scientists, and those in humanities by members in this general area. Then, we met as a full committee to avoid wide divergence in standards. In the opinion of many, this method worked successfully. There were some individuals who were unhappy when they were not approved for membership, and some committee members who disliked being given the responsibility of making hard decisions. One full professor asked to be relieved of membership on the committee because he felt it was not his responsibility to pass on a
faculty member from some other department; further, he thought it was the responsibility of deans of divisions to make such decisions. To me, this stand was unprofessional; it relegated the faculty to the status of paid employees with little or no responsibility for one of the most significant functions of the institution. Such comments cannot be made to the faculty member involved; one can only hope that there would be many individuals with different points of view. The grandfather clause, after a period of five to ten years, was blanketing in a smaller and smaller percentage of the faculty, and it appeared that in another five to ten years we would have a Graduate Faculty that had been carefully screened and that consisted of truly productive scholars. Did it work? In one sense yes, and in another sense no. I know of some members of the faculty who would move heaven and earth to get publications during their first three or four years, be nominated, and elected to the Graduate Faculty, only to immediately leave their research laboratories and go into offices and insist that they never expected to do any further research. It is only by reading between the lines that one can tell whether or not a man’s publications are designed to further his own advancement or indicate his deep-seated interest to research. We never considered dropping individuals from the Graduate Faculty after a period of inactivity. This probably would have come naturally after another five or ten years. At the beginning, there was no point in it because the new people being admitted would not have had a long enough period to demonstrate inactivity.

Another issue was the meaning of the Doctor of Education degree. It was established in 1935 at the University of Missouri during the administration of Dean Theo Irion of the School of Education, who was a gentle man, a discerning person, and a true scholar. Irion recommended the Doctor of Education degree with a specific set of criteria that differentiated it from the Ph.D. degree. In the field of education, many of those who wanted advanced training wished to be able to do a better job in their school districts as principal or superintendent. Often there were problems that had to be solved of a local nature involving legal, financial, and administrative difficulties. The Doctor of Education degree was designed to serve these people. It was not a research degree. It was intended to help men in responsible positions to solve specific problems, for example, how to consolidate schools in a certain county. It made no pretense of contributing to “scholarship.” This is a significant function in anyone’s book, and it seemed to be an honest way of serving the education community. A second feature in line with the objective of the degree was the absence of the requirement of a foreign language. Since knowledge of a foreign language was primarily to aid scholars in reading
research papers, there would be relatively little point in requiring a foreign language for the Doctor of Education degree. Doubtless, many graduate students took the Doctor of Education degree, not because they wanted that degree, but because they didn't want to fulfill the foreign language requirement. Later, we found that many faculty members failed to make the distinction between the two degrees and insisted that the thesis for the Doctor of Education in no way differed from the dissertation for the Ph.D. The College of Education offered both degrees. The distinction is hard to define and difficult to maintain. It was clear in the mind of Irion, but I doubt if it was clear in the mind of any of his successors.

Another issue concerned the areas of study. Anyone interested in graduate work is well aware of the fact that a graduate student frequently must have considerable knowledge outside of his own department. Sometimes, this is so extensive that a departmental degree is not appropriate. The first case to come to my attention in the Graduate Office was related to graduate work in genetics. Here we had Stadler in the Department of Field Crops; we had good geneticists in zoology, botany, and physics and later on in fields such as poultry husbandry, and yet the student had to receive his degree in the department in which his adviser was listed. So Stadler's students were getting degrees in field crops even though their concern could hardly have had less to do with field crops. It seemed quite evident that it would be desirable not to confine degrees to departments but to give degrees in the area in which the student had done his work. The first area we approved and announced in the Graduate School was the area of genetics. Others are to be found in later issues of the Graduate Bulletin. The idea seemed a good one and especially promising from the standpoint of strengthening our program, so that a student could take advantage of study with faculty members in several different departments in obtaining an advanced degree. In this effort, I had complete cooperation from President Elmer Ellis, who was aware of the expense of trying to set up separate doctoral programs in, for example, physiology in two or three different departments.

Physiology was the first additional area in which we attempted to give an area degree. It was not easy to do this because of departmental loyalties and the idea that the department may not report as many degrees that were conferred. We had excellent support from Prof. Dallas K. Meyer and were able to announce physiology as a general area of study, including veterinary medicine, physiology in the Medical School, and physiology in other departments such as zoology. It took a matter of years to get the idea adopted widely and to get it functioning with reasonable smooth-
ness. At the beginning, there was so little coherence among the investigators in various fields that it seemed desirable for the dean of the Graduate School to act as chairman of the physiology area. Later, procedures were developed to a point at which faculty members could take over. This procedure was not imposed by the graduate dean but was a recognition of the fact that the position could not be filled acceptably by any faculty member. As soon as the consensus was developed as to the nature of the area, then the dean appropriately dropped out. Other areas followed, but perhaps with less success. Anatomy was given in veterinary medicine as well as in medicine; pathology was taught in several departments. In both these disciplines, the difficulty in overcoming departmental loyalties was considerable. Development of an area of statistics was a complete failure. The courses in statistics were given in six or more different departments, and frequently the beginning course in statistics was numbered by the department as a strictly graduate course and therefore for graduate credit. Such action violated the basic tenet of the Graduate School, namely, that graduate courses should not be beginning courses in an area. When, for example, students in botany, or in economics, go into the Department of Agricultural Economics to take a course in statistics it is pretty hard to convince the adviser that they should not be given graduate credit.

From the standpoint of the dean, probably the greatest obstacle in developing high-quality graduate work was the feeling on the part of many students and faculty that an advanced degree should be given to reward a student solely on the basis of his effort—one year for a master’s degree and three years for a doctor’s degree. From this assumption, a student who worked diligently and did what he was told for the given period of time should get an advanced degree even though much or perhaps all of his work had been undergraduate in character.

The most striking case of this sort that I remember was a student from California who wanted a degree in nutrition. She apparently was a school supervisor who was responsible for nutrition in a large number of schools. She wanted to be competent in this area and to have an advanced degree to demonstrate her competence. The courses she wanted were a first course in physiology, and a first course in chemistry, and a first course in soils. Her whole program would have been made up of courses in which most or all the other students were freshmen or sophomores. I tried to persuade her that this program was not appropriate for an advanced degree. I told her what she was planning was admirable and that we would be glad to help her in any way we could, but we did not consider that granting an advanced degree would be appropriate, since the word advanced
should mean that a student has gone beyond the beginning course in at least one subject. (By the time she came to the campus, there was a stack of correspondence several inches thick divided between my office and Prof. William Albrecht’s office in the College of Agriculture. He was chairman of the Department of Soils and a man much interested in diversification of course work for graduate students.) Finally, she accepted her cruel fate and came. When she left at the end of her year’s work, she said that it was tremendous; she had just what she wanted and was happy that we had insisted that she take these courses and not confine herself to home economics or to some other department that would have had its own requirements for a specialized degree.
IV.

The High Quality of Graduate Programs

The efforts of the Graduate Office were first directed toward establishing and maintaining a distinguished staff. President Middlebush had announced that every new staff member expected to direct graduate work was to be interviewed by the graduate dean as well as the department chairman and the dean of the division. Some chairmen ignored this announcement; others in a perfunctory way would bring a candidate to introduce him and then would give him perhaps ten minutes to talk with me but frequently not even ask afterward what I thought of him. I remember only one clear-cut instance of the graduate dean’s recommendation being rejected. The College of Engineering wanted a director of research to stimulate such work in the college and was not satisfied with a director of research for the University as a whole. But the man that they brought seemed to me totally inadequate in his training and experience. He did not have a doctor’s degree and had little or no research experience. Nevertheless, he was employed and, for a few years, was on the campus.

One of the difficulties in building up staff was the fact that in some—but not all—departments there was strong resistance to bringing in men of experience who might be in the top salary bracket in the department. In one department, where considerable effort was made to build up the staff, several of the full professors insisted that they would object to anyone being brought in at a salary equal to or higher than their own. This attitude was not characteristic of the University as a whole, as shown by the fact that for a period of several years Stadler, who devoted his efforts almost exclusively to research, received a salary that was exceeded by only the president, or in some years the president and one dean. I never heard any complaint by either faculty or deans. I believe many were proud of the fact that this seemed to indicate that the University of Missouri placed scholarly work above administrative responsibilities.
When we gave approval for a department to give the doctoral degree, we made an effort to see that there would be more than one adviser available. We tried to assure that at least three members of the faculty were qualified to direct the doctoral dissertations. The objective was that a student should have not only the opportunity to select from a variety of research areas in doing his thesis but also that there should be several members of the department giving graduate courses who had research interests and experience. This is particularly important in the sciences, where a student needs much more careful attention in his day-to-day research than is true in English or history, where it is more likely that after having had a good course in bibliography he can be allowed to work for long periods of time by himself in the library. One factor in maintaining the quality of graduate research was to combat the common attitude that if there was anything that needed to be done in the way of investigation that we could get a couple of students to do it and give them the doctoral degree for their research. For example, when the state legislature wanted information, it was assumed that students would do the work. This seemed to me an unfair way to treat graduate students, who were trying to get the best possible training for the degree. As an example of the opposite, Stadler, who had high standards of research and also of training of graduate students, employed graduate students to do many of the necessary tasks, such as pollinating corn in his genetics program. During the first year or so of the student’s graduate work, he received no credit for this type of activity but was paid enough to support himself as a graduate student while doing course work. Similarly, when A. G. Hogan required graduate students to mix feed for experimental animals, the student was rewarded with cash rather than graduate credit. But there is a great tendency in many departments to feel that if a professor has a research problem, anything the student does to help should receive credit toward a degree. It was defended on the ground that his thesis would not be approved until he had done fairly independent work by himself. But, in practice, when a student had acquired thirty to forty hours of research credit there was great pressure to say that this work was appropriate for a thesis.

Providing good facilities in the way of equipment and services was another way to make graduate research as meaningful as possible. Still another factor in the graduate student’s program is the time that he has for his course work and research. We tried to limit the amount of course work in order to allow a student adequate time for each course; sixteen hours was the limit, but full residence credit was given for twelve hours of course work in order to discourage the simple amassing of credit hours. If a student were employed as a
half-time teaching assistant, we expected that he would have less
time for his graduate work. The general assumption, as far as the
regulations of the Graduate School were concerned, was that a
student could take more than a nominal 100 percent load but not
more than 125 percent. In other words, if he were employed for
half-time teaching, he might expect advancement toward his degree
to the extent of three quarters of a normal year.

This probably was the most controversial regulation in the
Graduate School. It frequently brought departments into conflict
with the routine operation of the Graduate Office. In geology, in
particular, the staff insisted that if a student who was taking twelve
hours of work taught half-time, he should be credited with a full
year’s work in the Graduate School even though the program could
be described as one-half years of effort in one year. The contention in
the Graduate Office was that a student has only so much time; if he
spends half his time on twelve hours of work it should not be thought
of as equivalent to full-time on the same twelve hours of work. In
some departments, it was stated that twelve hours was the maximum
that any student should ever expect to take. No objection was raised
to this, but to say that a student could make the same progress
whether or not half of his time was spent in reading themes or
correcting papers seemed implausible.

Frequently, a student would sign up for research credit even
though he had no intention of doing any at all. The idea was that if
his schedule would permit a few hours of research credit, he would
be accumulating credit for the degree and at some later time he would
do the research. This example is indicative of the sympathetic attitude
that was held by faculty members for their students. The official
record sometimes became meaningless. Thus, a man would try to
work full-time in a gas station and would still obtain credit for
full-time work in the Graduate School. This was particularly true
right after the war when GI benefits were given, but only to a student
doing full-time work. Under these circumstances, I would find a
student failing all of his course work.

There is little that a graduate dean can do to affect the quality of
course work, since this depends entirely on the instructor. However,
the nature of the students in a course also has an effect, and therefore
the stipulation was made that a certain number of courses must be
taken by a graduate student in classes where there were no
undergraduates. For similar reasons, we attempted to eliminate
students who were failing courses, since a considerable number of
failing students in a small seminar or discussion are bound to have an
influence on what the group can achieve. As to our policy of
admitting students rather freely to the Graduate School, we found
that some students who showed little promise before admittance turned out to be good. Our practice was to admit somewhat freely but to eliminate them rather strictly.

Another effort to improve or to maintain quality for graduate degrees was to have regular general seminars in departments and to encourage students not only to attend these but also to make it easy for them to attend seminars in other departments that were doing related work. When we approved a new doctoral program, one of the questions we always asked was, "Are there seminars covering current research in the field?" This effort was sometimes ineffective because a department might establish a seminar before offering a Ph.D. degree and then eliminate it shortly after receiving approval. The word seminar is often used, especially in the sciences, to mean a weekly meeting probably attended by the entire staff and student body to consider current research. In every first-class institution that I know, such seminars are an inevitable part of the training of graduate students.

Still another effort to improve the quality of the graduate training was to pay part of the expenses of students to attend national meetings. We were not able to do this until we had the NDEA funds, which we used to improve the quality of graduate work. We did not pay travel expenses for beginning graduate students but did assure every graduate student during his last year of work on the doctor's degree that most of his expenses to a national meeting would be paid. Attendance at such a meeting gave the student a chance to appreciate the research that was being done in other institutions and also the opportunity to make contacts with faculty from other universities and benefit from the services of an employment agency when such a service was part of a national meeting.

World War II had a significant effect upon the Graduate School. President Middlebush asked me to make a survey of the entire campus to see how each department could best contribute to the war effort. Many members of the staff stopped their research and the training of graduate students. When I started the survey of departments, I had the feeling that chemistry was the major department as far as service to the war effort might be concerned, since chemical warfare had been conspicuous during the last years of World War I. There was a great deal of fear that chemical warfare would again be used in World War II and would be a dominant factor. Mussolini had used chemical warfare in Ethiopia and I believe in North Africa. There was good evidence that Germany and Italy were both prepared for chemical warfare, and the United States was making desperate efforts to keep up with them. When I came to survey other departments, however, I found to my surprise that almost every
department considered its contribution as the most important for the
war effort. For example, the entomologists recognized that no army
can be effective if it is contaminated with lice and other parasites. The
people in the College of Veterinary Medicine insisted that their work
was vital in maintaining a strong army because veterinarians
inspected all the meat and food in the military installations. I believe
it was Napoleon who said, "An army marches on its stomach." The
Department of Classical Languages insisted that its contribution was
the most necessary of any the University could make because its
faculty were frequently involved in decoding enemy messages. Every
department had its contribution to make although some were not
called upon heavily by Washington. The graduate enrollment
dropped from 577 in 1939 to 142 in 1944. Both students and faculty
were drafted for the services. The graduate program was a major war
casualty. What research we did tended to be war-oriented and clearly
of less significance in the training of students than it had been in
peacetime. In addition to the strains on graduate work caused directly
by the war, the University was striving to make education available to
as many students as possible so we moved from the semester system
to the quarter system. This change placed quite a burden on the
faculty, since it meant that for a year or so considerable effort was
necessary in restructuring courses.
A graduate school of high quality can exist only in an institution with strong departments. Genetics was our best example. Lewis Stadler brought a number of outstanding people to the University faculty. The ones I remember most clearly are Barbara McClintock and Alexander C. Faberge. Later, Barbara became a member of the National Academy of Sciences. Ernest R. Sears was also a member of the National Academy of Sciences. Success in building this department was a result of two factors. First of all, Stadler had a research program that was fundamental in character, that is, it was not applied research. His stature was so great, both nationally and internationally, that others wanted to work with him. Second, Stadler was a good judge of research and so knew who were outstanding investigators. In addition to selecting good people in genetics, he gave them a great deal of freedom and adequate praise for their achievements. He himself was a quiet person. I never heard him raise his voice, although there were certainly areas in which he had strong feelings. As a result of his broad interests, he encountered some difficulties. At one time, Stadler was selected by the National Academy of Sciences to represent the United States at an international conference in genetics.

Since he was to be a representative of the United States, out of courtesy, the State Department in Washington volunteered to supply him with his passport, so he would not have the customary $10 fee. This is where the trouble started. If the State Department were to issue a passport, apparently it was necessary to examine the credentials of the person. It was at this time that they discovered that Stadler had aided Spanish refugees out of Spain by giving them money. In addition, they found that he subscribed to The Daily Worker, among periodicals of many points of view. Stadler could have gotten the passport himself without any question, but the delay was for so long that he was unable to attend the conference.

Physics was one of our promising departments. Gingrich made
every effort to build up the department, which had a reputation for giving good course work and for having a scholarly attitude. Men like Herbert M. Reese and Oscar M. Stewart were highly respected teachers, but the department had published little research. Gingrich was successful in bringing Conrad Herring, Dale Corson, and Bernard Goodman to the department as instructors.

At this time, there were more physicists than jobs; Herring was hired at a salary even lower than the norm for that time. However, when President Middlebush was attending meetings of the Association of American Universities, two different people, one from Princeton, Luther P. Eisenhart, and one from California School of Technology, Richard C. Tolman, told him how fortunate we were to have Herring on the staff. These comments startled Middlebush. As a result, Herring was given a salary increase of a couple of hundred dollars before he ever appeared on the campus. Herring eventually left the campus and a few years later was singled out as one of the most promising young physicists in the United States. Corson also left and attained the position of chancellor of Cornell University. George Vineyard was equally distinguished, but also left the campus after a rather brief period.

I think in all three of these cases we were unwilling to put forth the effort to hold these men. In the case of Vineyard, the situation was aggravated by the requirement that he serve on the Student Discipline Committee. I say required because it was pretty difficult to refuse an appointment of this sort and this one came at just the time of our ridiculous, infamous "panty raids." Many students were involved, and the committee felt it necessary to interview each one and decide upon punishment. So Vineyard, starting on a professional career and trying to become a physicist, found that he had to spend hours and hours talking with students about the panty raids. The administration, for its part, felt that every faculty member ought to do his share in whatever needed to be done.

Albert S. Eisenstein was another outstanding member of the physics staff, a man who had gotten his doctor's degree with Gingrich, had gone to Cornell University for a few years, and then was brought back. Eisenstein died of a heart attack, when quite young, cutting short a potentially brilliant career.

Bernard Goodman was another outstanding man in physics whom we lost. Few men had his depth of understanding of the theory of modern physics. Had there been a little more appreciation of his ability and a little more respect for his concerns about the development of the University, he probably would have stayed. This lack of concern on the part of the administration for the ideas of the faculty is probably the basis for much of the attitude of the faculty.
toward the administration, and such attitudes lead to the formation of teachers’ unions.

Chemistry is the department I know best, so I will give a little more information about it than it would receive, if this were a balanced discussion of the University. Some of the factors that affected chemistry, however, help us to understand what was happening in other departments. First, chemistry was a large and important department in the Graduate School for reasons quite unrelated to the quality of the program. This arose from the fact that the chemical industry flourished right after World War I, and the number of chemists in industry was ten times the number in teaching. This difference is in striking contrast to many areas of study for which there were no such industrial outlets. As a consequence, there were plenty of jobs for graduate students in chemistry, and classes were large on the undergraduate level. An additional factor was that chemistry was required in agriculture, in medicine, and in engineering. This combination of plenty of teaching to be done in the beginning courses plus the availability of jobs for Ph.D.’s meant that we had a thriving operation in most graduate schools with large enrollments in chemistry. When I came to the University of Missouri, the number of majors in chemistry was larger than in any other department in the College of Arts and Science. The number of doctoral degrees was equaled only by those in journalism and in education. Our effort to recruit staff in chemistry was not as easy as in physics because there were many industrial jobs available and the University was unwilling to pay a larger salary simply because of the competition. When we tried to get men at the level of assistant professor instead of instructor, the answer in general was no. We considered many highly qualified persons at that time because I had a good deal of contact with chemistry departments in other institutions and was able on a personal basis to get recommendations. Earl Long of the University of California came here shortly after I became dean of the Graduate School and took over some of my teaching responsibilities. Also Glen T. Seaborg, who eventually became a Nobel Prize winner and served as chairman of the Atomic Energy Commission, was recommended to us. Charles Price at Illinois, who was an outstanding organic chemist, came to the campus for an interview. Oscar Rice, who was at Harvard and eventually went to the University of North Carolina, also was a man we wanted. These are some of the people who either came here or whom we considered for appointment. In almost every case, the president was unwilling to step out of line as far as salary was concerned or to make any firm promise regarding research support. Long was the only one whom we acquired. During the war, he went to the University of Chicago

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and then to Los Alamos. I think we could have gotten him back to Missouri if we had been willing to make a little effort, but this was not forthcoming. I remember Middlebush commenting after the war, when physics salaries were going up, that perhaps we ought to abandon graduate work in physics if we were going to have to pay such outrageous salaries.

Geology was one of the major departments in the Graduate School. It had been built up largely through the energy and devotion of several members of the staff, including Branson and Mehl. Walter Keller was also an important force for progress. He was always interested in developing quality. He served as chairman of the department for many years. As an outstanding investigator, he brought recognition to the department and inspired graduate students.

As oil geology boomed, there were demands for geologists, which helped the expansion of graduate work in this area. One explanation for the growth of the Department of Geology was that it was thought by students to be an easier science than physics or chemistry; also, it was located close to the School of Education whose students took it to meet their science requirement. I remember that we were having a series of meetings in which the person in charge of the beginning course in each department indicated what was involved conceptually in the course. Branson explained, for the Department of Geology, that one of the concepts that used to come up in the beginning course in geology was the concept of dip and strike, which designated the slope (the dip) and strike (the direction of the slope) for an outcropping of rock. Branson stated that this concept was rather difficult for freshmen to understand—in fact, even some of his graduate students had trouble with it—so they decided to eliminate it from the course for beginning students.

By contrast, the number of concepts involved in chemistry is very great. In fact, there is difficulty in limiting the beginning course to a number of concepts that can be covered in any reasonable length of time. In this same series of meetings, I remember Mary Guthrie criticizing chemistry offerings because she said we tried to give too much in the way of basic concepts, atoms, molecules, valence, solutions, and so on.

Psychology was a small department in 1936. Housed on the fourth floor of Jesse Hall in quite inadequate rooms, there were but four professors. Arthur Melton, the chairman, was an outstanding investigator and leader, and Henry N. Peters, Frederick J. Courts, and Fred McKinney were also active members. How did the Department of Psychology succeed in moving out of its inadequate quarters twenty years later into McAlester Hall, where it is situated now?
Much effort was spent in presenting its needs to the dean of the College of Arts and Science. The feeling among some was that their mimeograph machine must be going steadily, which presented the needs of the department. The real stroke of genius was to get psychology accepted as one of the natural sciences in the requirements for the bachelor's degree. This was achieved not so much for educational reasons, in my opinion, but to get larger classes so that there could be graduate assistants and a significant graduate program. Many thought psychology, as an experimental science, hardly ranked with zoology, botany, chemistry, or physics, since the subject was so complicated that meaningful experiments for freshmen were pretty difficult to come by. Why was it adopted as an alternative for a science course? The dean of the College of Arts and Science at that time was W. C. Curtis who was a zoologist and was afraid that if he opposed it, it would look as though he was trying to eliminate some of the competition for the students. The result was that it went through the faculty without any opposition, and Curtis once told me he thought it was up to the faculty, not the dean, to raise the objection. The question still remains, Who should take such a responsibility?

In some ways, the Department of Industrial Arts has been the most remarkable department in the Graduate School. One would not expect to create a great graduate school starting with industrial arts as an area of study. Nevertheless, of all our departments, this one was the most strikingly successful. The department taught the teachers who were to instruct in the industrial arts. Why was it so successful? Basically, it succeeded because of the work of one man, H. H. London, a man of tremendous energy, enthusiasm, and ambition. It was extraordinary to see what he accomplished. At one time, the University of Missouri gave more than half of the doctors' degrees granted in the United States in this area.

How did London do it? First, he took a personal interest in every student. I was invited each summer to their chicken fry, which was held at the Cosmopolitan Park in Columbia. Students put on the party. They were mostly mature men with wives, I suppose thirty to forty years of age. London introduced more than one hundred of his students by name, without using a note. He named the school the student came from and added some little personal item. London's accomplishment is an extraordinary example of what one man can do.

When the National Defense Education Act (NDEA) program was being developed, it seemed we had something good and ought to profit by it, so we made funds available to bring in a second and, if needed, a third outstanding man in the field of industrial education.
The Museum of Art History and Archaeology owes its development to one or two persons. When I came to the campus, Prof. William Gwatkin was chairman of the Department of Classical Languages. He taught Latin and was depressed by the fact that the number of students studying classical languages was shrinking every year. He saw no way of improving the situation. When Saul Weinberg came to the campus, we acquired a young man who was enthusiastic and aggressive in shaping the department’s offerings to changing circumstances. He had attended the classical school in Greece for a time and had carried out archaeological investigations in the Near East. He gave courses in the classics in translations, and the department began to grow. He and Homer Thomas combined to produce rapid expansion of the department, and as a result of their efforts and of Gladys Davidson Weinberg’s, we now have the distinguished Museum of Art History and Archaeology. There are other departments that might be mentioned but these are the ones that stand out strongly in my memory. It was evident that the University of Missouri did not have the resources to have twenty or thirty outstanding departments giving the doctor’s degree, and, therefore, one principle I attempted to establish was that we should concentrate our efforts in two or three areas where we already had strength. Rice University is a good example of an institution that was granting doctors’ degrees in only a few departments—chemistry, physics, geology, and mathematics. Rice became so distinguished in these four related departments that it became a member of the Association of American Universities. By contrast, the University of Missouri was granting the doctors’ degrees in twenty or thirty fields, and we were not distinguished at the highest level in any. The NDEA program was designed to improve the quality of graduate education. It seemed essential, if we were going to benefit from the NDEA program, that we focus our efforts. Also, if the University were going to obtain national recognition, we could do so only by limiting our objectives. It was difficult to get any agreement on this principle, and so the Graduate School took the lead in discussion with the deans of divisions. We envisioned clusters of departments, one to be constituted of chemistry, physics, and mathematics, another of political science and history, and I think a third was to be a grouping of the biological sciences. The reason for this selection was that we already had established distinction in these areas and they served the professional schools of medicine, agriculture, and engineering, which was not true of history and political science.

Resistance and resentment on the part of the faculty, however, were considerable. In one field, the staff insisted that their department was as good or better than any other on the campus, and that it
was totally indefensible to state arbitrarily that any department should not aspire to become the strongest in the University. We indicated that every department should try to improve, but we needed to choose certain areas and to give the strongest support there. I was not in the Graduate School long after we embarked upon this program. It started in the early sixties and I retired as dean of the Graduate School in 1966. Shortly after my retirement, the faculty in botany and biology were able to persuade Chancellor Schwada that biology should have priority. The principle was not entirely ignored, but changing goals leads to ineffectiveness.
VI.

Statewide Graduate Education

In 1936, the campus at Rolla did not have a chancellor but rather a dean, who ranked in our administrative hierarchy as equivalent to the dean of the College of Engineering or of the College of Agriculture on the Columbia campus. Dean Curtis Wilson was brought in with this clear understanding, and cooperated fully in recognizing the role of Rolla. However, the faculty at Rolla always felt that it was discriminated against. They felt that their salaries were lower and that they were not quite appreciated. They knew that their training of faculty was much less than of the faculty at Columbia—few of them held the doctor's degree. This feeling created hostilities and some backbiting. My effort in the Graduate School was to include the Graduate Faculty at Rolla on absolutely equal terms with those at Columbia. Members of the Rolla staff came to meetings on the Columbia campus, and we went to Rolla at times. The director of graduate studies at Rolla attended our Policy Committee meetings regularly and was a loyal supporter of all of my efforts. I have always been grateful to Wouter Bosch for his support. He found difficulties, however, in carrying his faculty along with him. Once, in a meeting of the Graduate Council, a representative from Rolla proposed that we strengthen graduate work at Rolla and also improve the faculty there by enrolling members of that faculty in doctoral programs at Rolla. Now, it had been a regulation for a long time in most Graduate Schools that we would not give doctoral degrees to our own staff members. It was not easy to enforce this rule and frequently a member of the staff would be demoted from full professor to instructor in order to qualify for graduate courses; immediately following the award of his degree—perhaps within a week—he would be promoted to his former professional level. We were doing it on the Columbia campus, so why not at Rolla on a larger scale? We could develop a large graduate school because we would have many faculty members enrolled as graduate students and at the same time we would be improving the quality of our faculty by enabling a large
number of them to earn doctoral degrees.

This proposal rather shocked the members of the Graduate Council. David Pinkney of the Department of History presided as chairman of the council, and it was interesting to observe his reaction. He paused a minute and then without showing the slightest bit of emotion or surprise said, "Let's have a show of hands as to how many members feel that this is a proposal that should be discussed. All those in favor hold up your hands." One hand went up—the man from Rolla had voted. He looked around the room and saw forty or fifty people not showing their hands. With remarkable good nature, he said, 'This just goes to show how long it takes for a good idea to be accepted.' Nothing more was said about the proposal. I mention this incident because it indicates something of the nature of the problem at Rolla and that faculty’s desire to move as rapidly as possible into graduate work and presumably independence of the campus at Columbia.

Through all of my years in the Graduate Office, the graduate work at the Ph.D. level at Rolla was handled through my office. Dissertations were read by staff members at Rolla and also at Columbia, and frequently members at Rolla read dissertations prepared by faculty at Columbia. It was a considerable task to administer graduate work at a distance because it meant members of the faculty at Columbia traveled to Rolla for final examinations—time-consuming and sometimes a bit frustrating because of the emotions that are involved in case there was adverse criticism of the student’s dissertation.

A word or two with regard to the quality of work at Rolla. There were some investigators who would qualify for positions on any first-class university staff. Martin E. Straumanis and a few others were, in my opinion, quite outstanding. A major difficulty arose when Rolla tried to move rapidly into graduate work and employed men with the Ph.D. degree but with no teaching or research experience. Since these were obviously the men in the department best qualified to direct doctoral dissertations, there was considerable unhappiness that our regulations required them to demonstrate a continuing interest in research before allowing them to have advisees. This practice was followed at Columbia, and although there were some departments that circumvented this restriction by a senior member formally acting as adviser while a new (but unqualified) man was in effect the real adviser, still the regulation on the whole accomplished much. It prevented the all-too-common situation in which a man gets his degree and then considers he has reached the peak of his achievement and from then on does no research at all.

My initiation to the state colleges, of which there are five—at Cape
Girardeau, Springfield, Warrensburg, Maryville, and Kirksville—came during the first few months of my deanship in 1938. The question came up of establishing graduate work at these state institutions. The colleges were pushing for it and bringing pressure on the legislature to authorize it. Since T. W. H. Irion, dean of the School of Education, was in the hospital, President Middlebush called me in a hurry one day and requested that I visit Irion and discuss with him the best strategy for blocking this activity on the part of the state colleges. I have a feeling that the motivation was not so much what was in the best interest of students as it was the fear that state colleges would walk away with graduate education, at least in the field of professional education. If they would combine their political strengths, they would influence a much larger number of legislators than Columbia. As I remember, the action of the state colleges was not successful at that time, but the question was still open as to how students, especially students in professional education, could take advanced degrees without coming to Columbia.

The key figure in this whole discussion was President Roy Ellis of the college at Springfield, for whom I had the highest respect. He had earned a degree from Harvard, and he had been president at Springfield for many years. He had a good sense of proportion and a keen awareness of the importance of professional education. Also, he was a leader among the state college presidents and the one with whom I worked most closely as we developed a cooperative program.

I believe the motivation behind this program we created was good. It is true that there are always mixed motives in matters of such complexity, but the welfare of the students ranked high both from the standpoint of Roy Ellis and also from my perspective in the Graduate School. The idea was that many courses were being given by competent teachers on the campus at Springfield, which could be considered to be advanced and to be part of a program for the master’s degree. Therefore, it was to the interest of the students for us to devise some mechanism by which students could spend part of their programs for the master’s degree on the state college campus. It would be to the interest of the state colleges to have a cooperative program, since it would stimulate more advanced work on the campus and perhaps bring some distinction to members of the faculty who gave these graduate courses. In Ellis’s words, it would free the state college from major concerns with regard to graduate work if the Columbia campus would take over the problem of administration and would enable the college to do a better job of teaching on the undergraduate level. This area would be the one in which there would be the largest number of students.
Ellis stayed with this idea throughout his entire administration. We had nothing but the friendliest cooperation in relations with him and it seemed at the time that this program gave promise of providing a desirable organization of graduate work throughout the state. From the standpoint of the Columbia campus, there would be some advantage in that we were rendering a service to students in the state and also that individual departments might profit in the matter of numbers by having students begin their work at Springfield but with a Columbia campus adviser—that is, begin the work at Springfield and then after eight hours or more come up to Columbia and finish the program for the master’s degree. The plan would thus have some value as a recruiting mechanism. On the other hand, the commitment of time and effort was considerable. All the courses that were approved for graduate credit at Springfield were referred to departments on the Columbia campus for approval.

This often meant that members of the Columbia staff visited Springfield to talk with members of that staff and to investigate the equipment and the library to see whether or not facilities were adequate. It also meant a considerable investment of time in registration; thus, we would drive to Springfield—usually Dean Loran G. Townsend and myself, including almost always two or three and sometimes four or five members of the faculty. Two cars would go, and we would stay over night and carry on the registration. Dean Townsend took a large share of the responsibility in this cooperative program, since a vast majority of the students were in the field of education. Graduate credit, however, was given increasingly in subject matter departments for students who might come to the Columbia campus to take a master’s degree in some subject matter area. Some of these courses were taken as part of a program for an education degree where the candidate wanted to teach in some particular subject matter area.

This program continued for quite a number of years. It worked so well at Springfield that after a year or two, on the initiative of administrators at Cape Girardeau and at Maryville, similar programs were developed. Our relations were cordial at Maryville and in general this was true at Cape Girardeau when W. W. Parker was president. When Mark F. Skully became president, relations were still reasonably cordial, but Skully was of a temperament that made conflict more likely. Kirksville, on the other hand, expressed no interest in the cooperative program of the University. When Warren C. Lovinger became president at Warrensburg, I wrote him, describing the kind of cooperation we had with Springfield and indicating that if he wished to develop a cooperative program we would be glad to assist. As far as I can remember, I never received a reply. I mention
these two schools to emphasize the fact that at no time was there any pressure on any state college to cooperate. We wanted to help them and their students, and presumably they wanted to help their students and were happy to secure the assistance of faculty and administrators at the University of Missouri with regard to what was good graduate work. I had hoped that the program would achieve the following: lead to a higher quality of graduate work throughout the state; eliminate competition; and place the University of Missouri in a position of having a good influence on education. In one state college, the president, a man with ambition and a considerable amount of drive, I think, was unhappy not to have complete control and authority over everything that was done on his campus. I suspect President Elmer Ellis respected his wishes and also felt that the chief administrator of a college should in no way be subordinate to any other institution or to administrators of inferior rank, or to faculty members on any other campus. So his institution terminated its cooperation.
VII.

National Aspects

I consider the Association of American Universities to be the most important of the associations of graduate deans. It was founded around the turn of the century by a group of college presidents who met for some quite different purpose, which I was never able to discover. When I was secretary of the association, I wanted to learn some of the association’s early history and wrote to President Emeritus E. A. Birge (former president of the University of Wisconsin) who was one of the original founders of the association. I asked Birge if he knew for what purpose the presidents had gathered at the time that they decided to form the association. His reply was that he was over ninety; he was sorry that he could not remember why the group had gotten together. He had found that after an operation he had at the age of ninety for appendicitis his memory was not as good as it had been—at least for facts outside his field of zoology.

We don’t know why the association was founded at that particular meeting, but we do know the reason for its founding. The German universities were training the majority of American professors who received the doctoral degrees. Any scientist without a German degree was suspect. The German universities wanted to be selective about whom they admitted from the United States. They wanted to know what four-year colleges were doing respectable work. This desire was finally communicated to the presidents of the major universities in this country; the Association of American Universities was founded with the understanding that it would supply to German universities a list of names of American institutions that were considered to be adequate at the undergraduate level. How would the association know that they were adequate? Obviously, by inspection, visiting, and examining the records of the graduates of these schools. Thus, the accrediting operation of the Association of American Universities started. It was quite selective; to be on this list was a considerable triumph for any school.
The influence of this accrediting operation was not that the schools and the presidents of the various four-year colleges felt so much admiration and respect for the large universities that were inspecting them but rather the fact that the American Association of University Women (AAUW), which was striving to maintain high standards for its membership, decided that it would allow the organization of chapters only at institutions that were on the list of the AAU. This is an interesting example of a by-product completely dominating the main issue. Accreditation of schools in order that students could do graduate work was almost lost sight of, and accrediting of a school in order that an AAUW chapter could be established became the primary factor in giving significance to the list.

What did this accrediting amount to? How significant was it? From my experience, it involved deans of considerable academic experience. I say this because in my time almost every dean was active in research in his own field. He had come up through the departmental program and had become a dean after he had served as associate and full professor. Senior members of the academic profession, sometimes two at a time, visited a school and gave it a thorough going-over for two or three days. Very often serious problems were discovered, and presidents were happy to have the assistance of these outside visitors in accomplishing what they were unable to achieve due to local contrary pressures. This is often the case with accrediting; the local administration can use the findings as a weapon or as a tool in order to get things done that it is powerless to do without outside help. Accreditation is also misused sometimes by departments that are able to wheedle out of the administration extra concessions that may not be to the best interest of the institution as a whole.

To show the value of accrediting to the schools, I received a hundred or more letters at the time AAU gave up accrediting: letters from presidents urging us to continue and emphasizing the value of the accrediting program in maintaining quality of their offerings. It did more than this, however. It put the fear of God in the hearts of some college administrators who were playing fast and loose with some of the details of their operations.

As far as I know, the following has never been put in print, and it is possible that it should not be put down even in this record, but it is so old now that I believe the institution involved would not consider it a disaster for these statements to be made. The difficulty occurred at the College of William and Mary, one of our oldest and most prestigious institutions, an institution from which many deans in the association had received degrees. The college could not have been a
member of the Association of American Universities because it did not have a large enough graduate program. The AAU visiting team found that when a certain student wanted an appointment to West Point, being short in some of the requirements—notably physics—the registrar simply added to his record credit for a course in physics that the student had never taken. In some way or other, the visitor to the institution discovered this fact. It was such a flagrant misuse of student records that the committee recommended that the accrediting be removed from William and Mary. When this came up in meeting for the approval by the AAU as a whole, it created somewhat of a furor. Deans wanted to know why, and of course the committee was reluctant to expose the institution to the criticism that would be bound to occur if the facts were known. Finally, it was necessary to present the details in the meeting, and then the deans who held degrees from William and Mary admitted that there was nothing to do but remove accreditation.

This action is just one example of higher education, when properly structured, being able to police its own members. It would be difficult to imagine any federal agency acting as effectively as AAU did to keep the standards high and to maintain not only the integrity of records but also the quality of higher education.

It is surprising how many practices would surface in this accrediting procedure. I remember Yankton College, in South Dakota, which I visited at one time. There, salaries were all kept secret. A new member of the staff had little notion of what was to happen to him when he signed up to teach there. His salary as an instructor was perhaps $1,800, which at that time was as high as most institutions in the country paid. It was customary for a Ph.D. to start out as an instructor rather than as an assistant professor. Little did the man know that if he stayed at Yankton and became a full professor, that after twenty-five years his salary might be as high as $2,100 or $2,200. Not only were salaries low, but, in addition, the college practically required every faculty member to contribute to the college endowment fund—not a token gift, but an amount that would be substantial—as I recall, of the order of $100. This was reported as a donation from alumni and faculty to demonstrate to accrediting agencies how loyally supportive its graduates were of the college.

There were many other practices among colleges that were equally questionable, such as a school investing its endowment in the buildings of the institution or even of fraternities and frequently borrowing from endowment funds to meet operative expenses, all

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1*Journal of Proceedings and Addresses of the Forty-third Annual Conference*, held at the University of Nebraska, October 30, 31, and November 1, 1941, published by the University of Chicago Press, Illinois.
the while reporting the financial soundness of the institution by indicating the size of the endowment fund. Sometimes the practices that were discovered had to do with instruction more directly, such as one person giving ten or fifteen courses in a range of fields such as sociology, political science, and economics. Many other practices that were uncovered by the accrediting committee, too numerous to mention, were the subject of consultations between the committee and the college administration; more often than not, corrective action was taken. Often, the president was not aware of what was going on in his own institution.

The question next comes up: Why did the Association of American Universities discontinue its accrediting operation? The answer reveals a curious situation. The committee on accreditation had always been somewhat under fire from members of the association, since it was felt that AAU should be doing something more than scrutinizing undergraduate education, even though this was no financial burden. The school being visited paid the entire cost, which included the travel expense of the visitors and a modest honorarium of $50 or $100 a day. Perhaps part of the objection to the accrediting procedure came from institutions that had been treated critically by the accrediting committee, and they in turn might have raised complaints with the presidents in the AAU. For example, a church-affiliated institution dismissed a member of the faculty who had been on the staff for a number of years. It was discovered by the administration of the college that he was a member of the Unitarian Church. The fact that he had not made this membership known to other members of the faculty or to the student body was not significant as compared to the fact that he did not belong to what was called an evangelical denomination.

After long discussion within the association of whether or not AAU should discontinue accreditation, it was finally decided by a mail ballot (so that presidents would be involved) that the process of accrediting was valuable in higher education and should be continued. Since the burden had become considerable, it was decided that the dues for the association should be increased in order to make possible the hiring of help, so this valuable work would not depend on the spare time of the deans. This was the point at which I became the chairman of the committee. On the basis of this careful study and the almost unanimous support of AAU, the office was moved to the Columbia campus and Prof. William H. Lichte became the paid executive. After just a few years, the program was terminated. I suspect the person who could fill in this part of our story would be the dean from Northwestern University whose role in the reorganization of the Association of American Universities I never quite
I understood. I suspect this was arranged by the presidents of the member institutions. The deans of graduate schools who were on equal footing in the association with the presidents were never, I believe, consulted. At any rate, Dean Arthur R. Tebut took, I believe, a year off from his duties at Northwestern and traveled around the country talking with administrators—primarily the presidents, I suspect—with regard to what they wanted of the association. By the time we had our reorganization meeting, which was in Philadelphia with the University of Pennsylvania as host, the die had been cast. The presidents were united concerning matters they believed important, and discussion, as a result, was limited. The deans were somewhat dismayed to find that the accrediting operation was to be discontinued and that their organization would no longer be called the Association of American Universities. Dean James S. Wilson, from the University of Virginia, commented it was the first time he had ever known of a child giving birth to a parent. The "Association of American Universities" was the name taken over by the presidents for a parent organization with membership consisting only of presidents. The deans then became institutional representatives in the "Association of Graduate Schools in the Association of American Universities."

I am spending time in discussing this reorganization, not because of its importance to the association itself, but rather because of its significance for graduate work and especially for the role of the presidents. A second factor, which I think is probably relevant but not of prime importance, was that the North Central Association of Colleges and Schools was becoming more and more important in the field of accrediting. It was felt that the regional accrediting agencies should be given full responsibility for the quality of education in their areas and that there was duplication, which undoubtedly was true, in the AAU being in this business. It should be pointed out, however, that the North Central Association was essentially a self-regulatory activity in which institutions accredited themselves. A certain amount of senatorial courtesy was inevitable, so there was not the objectivity that was possible when AAU was involved in the accreditation of institutions that were not members of the association.

Another factor that I am sure influenced the attitudes of many presidents toward AAU was the increase in dues required to support the accrediting function. President E. B. Fred at the University of Wisconsin, who had previously served as dean of the Graduate School, made a study of the total amount of money the University had to pay for all the various accreditations. It was large. Without doubt, this payment was a big factor in the minds of many presidents. I think the main reason the presidents wished to organize
independently of the graduate deans, however, had to do with the concern of the presidents regarding the intrusion of federal agencies into the operation of universities. It was felt that there should be some spokesman for higher education in the United States and that the presidents of AAU as a group ought to be able to wield considerable influence. Political influence on a national scale would be involved, and obviously this was an area in which graduate deans would not be appropriate spokesmen. I believe the presidents who were particularly interested in this aspect of the reorganization were led by James B. Conant of Harvard University, although I think Middlebush of the University of Missouri—who I believe by this time was on the advisory board of the National Science Foundation—also felt strongly on this point. Furthermore, the presidents felt more at ease talking with each other than they did talking with the deans. Confidential meetings involving presidents only were a welcome relief from the isolation a president experiences in his own institution. These meetings provided a place for him to relax and talk freely about his problems.

One of the major problems of institutions in this period was rising budgets and especially the high cost of medical schools. Some of the presidents felt that here was a mechanism whereby they would be able to exert more authority. The medical budgets were often a sizable proportion, perhaps 20 percent to 40 percent of the total budget of an institution, and this cost was a matter of great concern to the presidents. It was a time when, I believe, one or two medical schools had to close because they could not be financed.

Closely connected with this tremendous demand for money for medical schools was the fact that the medical schools enjoyed a kind of autonomy that was irritating to the presidents. Medical schools were able to go directly to national agencies and foundations and secure support that often involved matching funds or at least commitments by the parent institution. The reorganized AAU provided a means, it seemed to the presidents, to take positive action against such demands in addition to isolating themselves from pressures brought by the graduate deans. It was the first step, according to President Middlebush, in a move to organize universities nationally in an Association of American Universities composed of only the presidents. Subdivisions of the universities such as the graduate schools, the medical schools, the colleges of arts and sciences, and the business schools would have no independent associations but would be under the direct supervision and direction of the presidents. The professionalization of social work and of business administration was viewed with suspicion. Some presidents felt that they were losing control of their own institutions.
Curiously, nothing was ever done about extending the AAU organization to include schools other than the graduate schools. Doubtless, it would have been difficult to force medical school deans to organize and to subordinate their organization to the Association of American Universities. In addition, many medical schools would have been left out, since a medical school could qualify for membership only if it were in a university whose Graduate School could qualify for membership. In my opinion, it was not a well thought-out proposal. It was based largely on frustration and dismay at the direction in which higher education was moving.

Still another factor that gave rise to enthusiasm of presidents to separate from the graduate deans was the anomalous situations that occurred at meetings attended by presidents and deans together. In such a meeting, a question came to a vote. A president if present usually insisted that the graduate deans cast the vote for their institutions. Usually, the question at issue had a great deal to do with graduate work and the dean was more deeply involved than the president.

Certain embarrassing situations arose, however, when the president and the dean disagreed on some issue. President Henry M. Wriston of Brown University was the most vehement in his comments. The graduate dean at Brown was G. D. Richardson, a mathematician, who had been dean for twenty years or more and was influential in the association. At one point, Wriston voted on one side of a question and Richardson on the other. Wriston was quoted to me as saying that it was preposterous that a dean of an institution should vote against his president.

I felt the same embarrassment when the time came to vote for the discontinuance of the accrediting program. I was unsure of Middlebush’s opinion. I was reasonably certain that he would go along with the other presidents and would agree to have it discontinued, although he had never talked much to me about it. Middlebush was not one to express his view readily or to speak frankly with his own administration. When the question came up for vote, I was serving as secretary of the meeting and started to call roll. I assumed that Middlebush was going to vote, but when it came to Missouri he smiled and whimsically said, “The graduate dean will cast the vote for Missouri.” I paused for several moments, wondering what my obligation might be. Should I be loyal to what I thought Middlebush wished, though not certain in my mind, or should I vote on the issue in the way I felt strongly was to the best interest of higher education? One doesn’t have long to think under such circumstances—perhaps three or four seconds. I may have made a mistake, but I voted to continue with the accrediting function. The vote was not important as
far as the final issue was concerned because the vote to discontinue accrediting was overwhelming. Afterward, when I commented on this to Middlebush with some trepidation, he said, "Well, I knew how you were going to vote, Henry."

I think the most important part of this whole reorganization was not any of the factors I have mentioned, because the presidents did not go ahead with their political activity. It is probably too much to expect any continuity of purpose in an organization that meets once a year and has such independent individuals as university presidents as its membership. I believe AAU has had little if any influence on national politics. Certainly, the reorganization had no influence on medical schools, or on education, or on business schools, which were carrying on their own national organizations. It did, however, increase the isolation of presidents from graduate work. This was a trend that had been under way for a long time, since financial problems and medical schools were increasingly occupying the attention of presidents.

However, in 1900, the president of a major university had as his major concern the doctoral degree program and the quality of his faculty. The first meeting was attended by presidents, and the concern of the association from the beginning was with graduate work. In fact, there is a wealth of comment and discussion regarding graduate programs in the early proceedings of the AAU. When one considers that for fifty years or more the speakers were those most competent in the largest and most prestigious universities—deans and presidents—it becomes evident that if we want to know what the thinking of our predecessors has been this is the best place to find it. Increasingly through the years, however, the presidents were absent from the national meetings. A record for the presidents' attendance was established at Missouri in 1939 when the University held its centennial celebration, and President Middlebush made a great effort to get all of the presidents to attend. As I recall, between thirty-five and forty presidents attended, out of a membership of a little more than forty. President Middlebush entertained them royally. He was something of a hunter and he gave a banquet at his home, the president's house on campus, at which quail was served—all of which he had shot himself.

With reorganization of the association, substance was given to the statement that the graduate deans knew more about the problems of the Graduate School than the presidents and the answers should be given largely by them rather than by the presidents. The reorganization was natural but an unfortunate step in graduate education, since the presidents in the past had given much thought and much support to graduate work. The position of the graduate dean in many
institutions was precarious, since the Graduate School had no budget and was completely at the mercy of any aggressive divisional dean who might want to ignore the whole graduate program. To be effective, the graduate dean needed the president's support. In some institutions, such as Indiana University, professional schools simply withdrew from the Graduate School and granted their own doctoral degrees. In other institutions, like Michigan State University, the Graduate School was essentially dissolved and the dean became simply a record keeper.
VIII.

Graduate Work in the United States

This brings us to the nature of graduate work throughout the United States. It seemed that we might learn something from studying what was done in each others' institutions, and Dean William N. Lacy of the California Institute of Technology undertook the task of canvassing graduate schools and finding out how they operated. The Association of Graduate Schools has the large scroll Lacy produced to summarize his findings.

Briefly, Lacy found that graduate work could be carried out with a tremendous variety of administrative procedures. In some institutions, there was practically no graduate organization. This was true at The Johns Hopkins University, where there may have been some office for keeping records but there was no graduate dean. At Commencement time, the president of the institution would look around and spy the oldest member of the faculty and suggest that he present the candidates for the doctor's degree. There was no official who had this responsibility as part of his duties—surely, the extreme informality. At the University of Kansas, John N. Nelson held a unique position. He was a member of the Department of English before becoming graduate dean and gradually became more and more important in the university until he was on the finance committee and was able to have a considerable influence on the distribution of funds. This role gave him an influence that most graduate deans did not enjoy. The attitude of the faculty toward a graduate dean in general is a certain amount of courtesy and respect but primarily sympathy for the fact that he has no money and that his suggestions can easily be ignored not only by the administration but also by the chairmen of departments and by the faculty. The president of the University of Missouri used to encourage me by saying, "Well, what you accomplish you will accomplish by persuasion, and after all, this is the most important way to have a permanent influence." I suspect he was correct, but sometimes one
becomes a bit frustrated by finding that persuasion is ineffective and that things are going in the opposite direction from what you had hoped.

On the other hand, as far as I know, the University of California had the most extensive organization of the Graduate School, and I remember William R. Dennis, professor of philosophy and dean of the Graduate School at California at Berkeley, saying that he or his office approved of every appointment in the university, even across the bay at the Medical School and including the appointment of graduate assistants. This statement does not mean that he scrutinized qualifications in every case, but if anything turned up that was worthy of scrutiny he certainly had the mechanism for exerting pressure. Berkeley had had the experience on more than one occasion of having to take a vigorous stand in order to develop or to maintain the quality of its graduate work.

Case Histories

One case history happened before I became a graduate student there. The Department of Chemistry had deteriorated, as far as graduate work was concerned, to the point where (as far as the faculty were concerned) it was primarily a place for consulting chemists to hang their hats. I am exaggerating because there were some industrious and distinguished professors, but nevertheless a large number of the faculty were inactive in research but were doing a great deal of consulting. Edward O’Neil, professor during that period, went to W. W. Campbell, an astronomer who was president of the university, and told him that the only way the situation could be improved would be to clean house, bring in a new head, and start over. Campbell followed O’Neil’s advice; the man who was chosen to lead the new regime was G. N. Lewis who turned out to be one of the most profound and influential chemists in the United States. He had been at MIT, had been in government work in Puerto Rico for a few years, and had done outstanding work in chemistry. Lewis, who had his degree from Harvard, was brought to Berkeley with the understanding that he would have complete freedom. In fact, California became one of the two institutions in the country that had a school of chemistry, not merely a department of chemistry, with Lewis as dean. The other institution that had a school of chemistry was the University of Minnesota. The first thing that Lewis did was to fire all the faculty. He then looked over the group and hired back, I think, two. One was Edward O’Neil, who had been responsible for the reorganization. O’Neil was within a year or two of retirement and was given the assignment of teaching the History of Chemistry. This
gave him a comfortable berth, but no conspicuous responsibility for research. The other man Lewis hired back was Walter C. Blasdell in analytical chemistry, who had been a thorough, careful teacher and also had done sound research. All the new people were outstanding chemists from many universities. My adviser, Joel H. Hildebrand, came from the University of Pennsylvania—a vigorous and active investigator. He had many graduate students and published several important monographs.

Another institution Lewis created was the departmental seminar, at which the entire staff of perhaps twenty or twenty-five faculty once a week, on Monday evening, would sit around a table in the center of a large room, flanked by chairs around the wall on a slightly raised platform where the graduate students sat. There were always two papers, one usually given by a faculty member or an advanced graduate student. When this paper was completed—which sometimes seemed to be of no importance or of little interest to the group—there would be complete silence. Lewis would sit back in his chair, blow a few smoke rings toward the ceiling, and then come out with an interesting and profound analysis of the problem, usually far better than had been presented by the original author.

Usually, this part of the seminar would take about forty minutes. Then, after the formal paper had been presented, Lewis, with a most benevolent, fatherly attitude and pleasant smile, would let his eyes rove over the group of graduate students sitting in a single row next to the wall. He would pick someone and say, for example, "Bent, how would you like to describe what you are doing in your research?" Of course, we all knew what was coming, but we didn't know who would be the one. A fear and trembling on the part of the graduate students was evident, but to some extent we were prepared because we knew that the ax might fall at any seminar. So the trembling graduate student would in five or ten minutes describe his research. Frequently, he would receive some highly significant help from either Lewis or another member of the faculty. I mention this because it is a kind of experience that is invaluable, one of the kinds of learning that has to be experienced in order to be appreciated. The prospect of talking to all of the graduate students in the department and to all of the professors was something that kept one on his toes.

When a visitor in town wanted to find out what was going on at the University of California in the Department of Chemistry, he could contact a graduate student and find him well informed. I remember being a guide on a number of such occasions and taking the visitor through all of the research laboratories and being able in a few sentences to describe what the focus of the effort was in each laboratory. If the visitor wanted more detail, he could talk to the
faculty member or to the student who was at work in that laboratory.

I have described in some detail the atmosphere in chemistry at Berkeley, because not only was it without doubt the strongest department in chemistry in the United States but also it produced a number of Nobel Prize winners and outstanding chemists. When I went to Berkeley from Northwestern University, I had a master's degree and two years of graduate work in the field of organic chemistry. I was shifting to physical chemistry at Berkeley and so naturally had to take course work. Following the type of education I had been accustomed to at Oberlin and Northwestern, I made some inquiries and signed up for three courses. I also had my research and a teaching assistance. I was then told that I should go to George E. Gibson, a faculty member who was adviser for graduate students. He looked over my program and said, "I think you have too many courses. Why don't you cut out one of them? We at Berkeley feel that you will learn more from the graduate students and their research than you will from any course work." I had never heard that statement before, and I have never forgotten it. Then, he said, "Why don't you go and talk with G. N. Lewis? He likes to see each new graduate student." So I went to see Lewis with my program. By this time, only two formal courses besides my research remained: One was a junior-senior course in electricity and magnetism given in the Department of Physics. Lewis looked over my program and remarked, "Are you any good at studying a subject on your own without taking a course?" I had never been asked this question before, and I am sure I blushed and stumbled in my answer, for obviously I had never tried it. "Why don't you drop electricity and magnetism, which is an undergraduate course?" Thus, I was left with just one course, a course in thermodynamics to which I devoted all my efforts.

A senior professor named Axel R. Olsen came into my laboratory while I was standing nervously waiting for my final oral examination. He asked me if there was anything I didn’t know. I thought his remark was simply facetious and probably showed it in my response. "No," he said, "I am serious. Is there some area in chemistry in which you feel that you are almost completely ignorant?" I said, "Yes, there is. I never had a course in phase rule, and I have never studied the phase rule seriously." "All right," he said, "I will see that you are not asked any questions in that area on your final examination." It was obvious that they were trying to find out what I knew in chemistry and were not particularly interested in embarrassing me or proving that I was ignorant.

One other interesting point about the Department of Chemistry at Berkeley: Every graduate student was expected to take several
courses in the Department of Physics. Physics was also a strong department at Berkeley. The faculty in physics were rather piqued by the fact that they had to do most of the teaching of the graduate students in chemistry, while the faculty in chemistry spent their time in research. This is a far cry from the practice in many departments in some schools, which will not allow their students to take a course outside their area for fear they will lose registrations. The difference in point of view is the difference between black and white. At Missouri, this has been one of our problems. We could not get adequate breadth of training in the programs of many of our graduate students.

This has been a digression, but it leads to one other item in the reorganization of the Association of American Universities. This had to do with accrediting of chemistry departments by the American Chemical Society. I want to go into this in some detail because it was relevant to the reorganization of the AAU, and it is also significant in indicating some of the problems institutions face in this whole area of accrediting.

During the depression of 1929 and in the following period, a good many chemists were out of work. Frank Whitmore, at one time chairman of the Department of Chemistry at Northwestern and later dean of physics and chemistry at Pennsylvania State University, was a man of personal charm and of great sympathy for graduate students. When he became aware of the number of chemists who were out of work, he thought something ought to be done about the situation. He investigated and discovered that most of the chemists without jobs had majored in chemistry in four-year colleges and had a completely inadequate background for professional work. They had perhaps had three or four courses, but they had no advanced courses and no significant laboratory work outside of their beginning courses. These were the first to be laid off. It seemed to him that we could eliminate much of this hardship if we let students know when they had completed enough chemistry in their undergraduate programs to be on their way professionally in the field of chemistry. This was not to say that all students majoring in chemistry should necessarily be prepared to go on professionally. Obviously, there were many students who majored in chemistry with the intent of going on in medicine and to expect them to do everything that might be necessary professionally in chemistry would be ridiculous.

Whitmore was influential in setting up a committee on professional training. It was not called an accrediting committee; it did not accredit departments but it simply published a list of those departments that had been examined and found to have the ability to give at the undergraduate level an adequate professional start in the field of
chemistry. It was recognized that such departments would likely be training two types of students—some who simply would be majors in chemistry and would not be certified by the department and others who would be certified by the society.

The American Chemical Society agreed to advance certified individuals to full membership in the society a year earlier than would be the case if they were not from "accredited" schools. I use the word accredited here in quotation marks because that is the word frequently applied to the certifying schools. A student without training in a department on the approved list of the American Chemical Society (ACS) could become a member of the society after work for the master's degree or a couple of years in industry, as I remember. But he had no other privations; he had access to journals at reduced rates just as anyone else. Furthermore, the ACS made no charge for its investigation of institutions. The ACS never visited an institution without being invited by its president. So here was a situation where the president had to take the initiative by issuing an invitation. He paid nothing for the visit; in fact, the visitors were warned not to accept hospitality from the host institution. When I made some of these visits, we were allowed to have one luncheon as a guest while on the campus for a period of two or three days, but we were essentially forbidden to accept any elaborate kind of entertainment. Nevertheless, the ACS was the object of violent criticism by many universities and by many people outside of chemistry in the arts colleges for the following reasons. On the part of the faculty from other departments, it was a matter of envy because chemistry was booming; there were many undergraduate students; and consequently much financial support in the form of teaching assistantships. Adverse criticism is one of the penalties for success, especially when some succeed while others equally competent are powerless to imitate the process that led to such success. The objections of the president were even more pointed. The ACS seemed to say, "You are not being honest with your students if you say they have a major in chemistry when they have had no organized course in the theory of chemistry, or have inadequate laboratories and equipment." What does the president do under these circumstances? Well, he worries, feels deeply concerned, takes money from English or classics, probably feels resentment toward ACS, adds a room or two to the chemistry building, and hires another chemistry professor. I remember when this whole issue came to the front at one of the AAU meetings, and President Wallace W. Atwood of Clark University discussed in vigorous terms the evils perpetrated by ACS. It so happened that at that time W. A. Noyes, Jr., was dean of the Graduate School at the University of Rochester and was also
president of ACS. Noyes knew perfectly well that most of the things that Atwood was criticizing were untrue: ACS didn’t force itself on any institution, and it did not cost the institution anything as far as the visit was concerned. But he probably also knew that the effects of ACS on Clark University were significant since it was not a large institution and was having great difficulty financing graduate work. He had much sympathy from other presidents.

Accrediting of chemistry departments was growing rapidly. The presidents were afraid that every department would soon have its own accrediting committee. In some institutions, the problem became serious. In the field of social work, there were two competing organizations that were accrediting social work, and they couldn’t get together. At one time, the head of the program had to be a dean. This is the kind of thing ACS never did. ACS was concerned with the quality of instruction and what it did for the students and how we could help students know whether or not they were getting adequate training and how to be protected during economic depression. In the field of social work, on the other hand, the accrediting became a mechanism for raising the prestige of the department, for securing better salaries and administrative positions, and for a host of other gains.

As a result of all this activity, an organization was formed to accredit accrediting agencies, of which Reuben G. Gustavson, chancellor of the University of Nebraska, was made head. It was hoped that the formation of this agency with the chancellor as the head would give the presidents control of the whole accrediting process that heretofore had the responsibility to only separate disciplines.

To summarize, the AAU was recognized. With the withdrawal of the presidents, their involvement in graduate work became more tenuous. The graduate deans’ difficulties increased because it was not easy for them to bring their problems to the presidents or for the presidents to become involved. It was a turning point in graduate education with some unfortunate results. Essentially, the presidents said, “Take your graduate work and go off and do the best you can with it; we will simply see that you don’t get out of hand.”

Awarding of Fellowships

Another example of the chaotic nature of graduate work in the forties involved the recruitment of students. At that time, graduate schools were small, and institutions were anxious to get more graduate students. Since the enrollments were small, additional students could be accommodated with almost no cost to the
institution, and therefore great effort was expended by departments to interest good graduate students and to sign them up early in order to be sure of them. Chairmen of departments were going out earlier and earlier to sign up students. In the beginning of the senior year, a student would be committed, often by early January, even before the grades were in for the first semester of the senior year. An added difficulty existed because California had its own calendar. It started the fall session in mid-August.

So even in a small group like AAU there was brisk competition and unwillingness to cooperate. One of the causes of this lack of cooperation was the absence of authority for the graduate deans. It meant nothing for a graduate dean to tell a department that it could not award a teaching assistantship in January. Even at California, where the assistantship had to be approved by the dean, nothing could prevent a department from making early promises. After years of discussion and observation of the evils of this cutthroat competition, the AAU came to the conclusion that the best thing to do was for all institutions to offer awards at the same time. The first of April was selected, and students were asked to make their decisions by the fifteenth of April. This was to the advantage of the student because he would have his best possible offer before him when he had to decide.

This agreement included only AAU institutions. What about all the other graduate schools? This question posed a significant problem. There was no Council of Graduate Schools to help. I was in the midst of the discussion because I had been chairman of the Graduate Division of the land grant colleges and was active in AAU. It seemed possible that if we could get all of the AAU institutions and all of the land grant colleges to agree, we would have a nucleus large enough to induce the cooperation of other schools, partly out of a sense of reasonableness and partly because they might want to eventually become AAU members.

How did we proceed? We graduate deans could not lay down any regulation and require the faculty or administration of any institution to abide by it. Departmental chairmen are too numerous and independent, and undercover promises are impossible to control. Therefore we did not regulate institutions but rather defined what would be ethical. What we did was to say that it would be considered by academic institutions as quite ethical for a student who had expressed an interest in an institution to delay until April 15 the making of a final decision regarding acceptance of an appointment. This would mean that if he had been approached by a third-rate school in January he would be completely free to express his interest and hope that he might receive a fellowship. He could even make
such a statement and indicate his willingness to accept the offer, but he would still be free ethically to resign this award and accept another up until April 15. After that date, he would be bound by his promise if he were to consider himself ethical and could be free to resign only with the approval of the school in which he had received the award. Thus, the solution to the problem was a statement of what a student could do and not what the university should do.¹

This proposal was taken to a meeting of the AAU and was readily adopted because the association had already agreed upon April 15 as the date of decision. Next, I remember presenting it to a meeting of the governing body of the land grant colleges, which consisted mostly of college presidents. These in turn gave their approval. Next, we circulated to other institutions granting graduate degrees our statement as to what we were approving and asked them if they would like to cooperate in this effort—universities like the University of Buffalo, for example, which were neither land grant nor members of AAU. We asked all institutions to agree to send out this statement and the names of institutions conforming, with every appointment for a fellowship. Thousands of copies of this statement were sent out every year, which meant that graduate students were well informed. It was therefore difficult for an institution to circumvent the agreement when the graduate student had already been told that over two hundred institutions in the United States had reached this agreement and that the institution offering him a fellowship had also officially agreed to the April 15 date. Now in 1981, we may have reached a happy ending to this program. The difficulties have almost ceased to worry graduate students and deans; the statements of agreement are no longer sent out with offers of fellowships I am told; and April 15 has now become a part of accepted folklore. It may be worth noting, however, that with a shortage of good candidates for fellowships in the future the dragon we had thought to be slain may come to life. If it does, the resolution may well be called into use again as without doubt no one has ever even thought of revoking it.

Cooperation among universities in graduate education has been an important feature of higher education for many decades and goes back to well-established procedures. Before commenting on two recent developments, we should pay tribute to some of these time-honored practices without going into any detail with regard to their value and their difficulties. The loaning of library books between universities, the travel of scholars to universities for lectures or sometimes for extended periods of time, the transfer of credit toward advanced degrees, the accrediting of institutions, the serving

¹See Appendix
as consultants, the aid to national organizations and the federal
government in the awarding of grants and scholarships, and finally
the exchange of information that has led to the formation of national
organizations such as Association of Graduate Schools and the
Council of Graduate Schools are well known. The above list is far
from complete, but it indicates the role of cooperation in higher
education.

When cooperation involves large costs in time and money and the
partners are unequal in what they can contribute to the cooperation,
it becomes difficult to justify the expense. Ralph Parker, long the
librarian of the University of Missouri, told me of a letter he received
from the University of California indicating that the cost of loaning
books to universities had become so great that they were compelled
to limit this activity and therefore the University of Missouri would
no longer have the privilege of borrowing from them. Ralph replied
that after checking our records he found that California borrowed
more from us than we borrowed from them. As a result, California
decided to continue the exchange of books with us.

The need for cooperation became increasingly evident from 1960
to 1965 when the federal government was making large contributions
to graduate education. Duplication of expensive equipment in
universities, when not used to capacity, is not in the best interest of
universities or of the granting agency.

The Big Eight universities formalized a process whereby graduate
students could spend time at another institution in that group, when
suitable arrangements could be made, for the use of equipment. This
seemed to many of us an ideal area in which the Council of Graduate
Schools could function. At the last meeting of the council, which I
attended in 1965, I outlined a procedure that might be followed to
safeguard the operation and made a motion that the council approve
the granting of “passports” for graduate students that, with proper
signatures, would permit easy transfer of students from one
university to another. This motion was passed and referred to the
executive committee.

As far as I know, nothing ever came of this effort. Perhaps the
need for such procedures was not sufficiently great, the time spent in
building up adequate support for the idea had not been available, and
finally there was fear on the part of some representatives of the best
equipped schools that the burden for them would become too great.
IX.

The Council of Graduate Schools

The founding of the Council of Graduate Schools resulted from efforts of the Association of Graduate Schools in the AAU. What were the reasons for this organization? How did it function during its first year when I was president? I think the prime motive for forming the council came from personal difficulties of graduate deans. There were too many organizations of graduate deans. Of the regional associations, the Midwest association was the largest. I believe it included institutions all the way from Arizona to New England. There was the New England Association, which was by comparison a small operation dominated by Harvard, Yale, and Brown. It was almost a family affair of graduate schools in which personal relations of the deans were significant. Then there was a middle states association in the eastern part of the United States, which was a fairly effective operation. The Southern Association of Graduate Schools was quite large and well organized and did much to raise the quality of graduate work in the South, at a time when Southern institutions were struggling to become comparable in quality with graduate schools farther north. There was a Rocky Mountain group of institutions that had their own association, and finally, on the Pacific an organization of graduate schools. The AAU was national in scope but a small group, a little over forty of the most prestigious institutions in the United States, including not only state and private institutions in about equal numbers but also the Canadian universities, McGill University and the University of Toronto. It was also a fairly chummy group in which everybody got to know everyone else quite well and exchange of information was easy.

Finally, we had the Association of Land Grant Colleges, which eventually joined with the Association of State Universities and became the combined association. Within the Association of Land Grant Colleges, there was a division for graduate work. This association was dominated by the colleges of agriculture, which cherished their central role and were able to subordinate the role of the division of graduate work by allowing a representative from that
division to be present in the councils of the association but without the privilege to vote. I think the division of graduate work was alone in this humiliating status. The division was to have a president to represent the graduate deans in the councils of the association, and we could have a graduate dean as observer, but he was not permitted to talk. The engineering schools had full status, because the Land Grant Act specifically mentioned them.

All of these associations had their annual meetings and frequently had excellent overlapping programs. It was customary for a graduate dean in a land grant school to go to at least three of these national meetings: the AAU, the Land Grant Association, and his own regional association. The burden of attending all three was considerable, since several days were involved in each meeting, and the expense was a factor. Sometimes a speaker would appear at two meetings, giving almost the same address. Such repetition was wasteful of time and money and was the first reason for eliminating some of the meetings. Furthermore, it was felt in the fifties that the federal government was getting into graduate education, that this would be dangerous, and that something must be done to prevent mistakes at the federal level. The federal Office of Education had heretofore been involved primarily with high school and Graduate School instruction, but now there was great fear that this office, dominated by the school administrators, would undertake to set the rules for graduate education.

It became apparent that there was need for a voice in national affairs representing all of graduate education. The question arose, Should the new association represent all of graduate work, or should there be a requirement for membership based on the number of students and the quality of work? The decision was finally made that well over 90 percent of all graduate work should be represented and that this could be done without any serious compromising of standards. It was felt that the stronger institutions would help raise the quality of work of the weaker ones, and therefore we could accept every institution that conferred a sizable number of doctoral degrees. As I remember, over a period of five years there had to be fifteen doctoral degrees granted by an institution, and the degrees had to be offered in at least three departments.

I was not present at the original meeting in Princeton that led to the formation of CGS. Robert H. Bruce (with whom I discussed this matter briefly) of the University of Wyoming was there. Hugh S. Taylor called the meeting and financed it with foundation money. At this point, it was decided not to form a federation of the regional associations but to supplant them by a single organization. I believe some of the regional associations eventually disbanded. I made a
motion at an AAU meeting that the AAU be abolished in order to show our confidence in the new organization and also to be sure the strongest universities would be members. My motion was defeated. Peter Elder of Harvard remarked that he could not vote for hara-kiri for the AAU.

The name, *Council of Graduate Schools in the United States*, was designed to eliminate the Canadian universities since the purpose of the council was to speak for higher education in the political sense in Washington. These two Canadian institutions felt that it would be quite improper for them to be even nominally associated with this process. I was the one who suggested the name and I have always questioned my wisdom in doing so because I dislike long names, but it seemed a bit arrogant to call ourselves the American Council of Graduate Schools and thereby ignore what is North and South of the United States.

I found being president of the Council of Graduate Schools during the first year, in addition to the graduate deanship and my teaching of chemistry, a heavy load. Fortunately, I had tremendous help from Father Robert J. Henley, who was elected secretary and helped greatly during the first year. Evaluation of credentials and voting on new members involved much correspondence. At our first meeting about 125 institutions were represented.

The major effort of that first year was to find a permanent president, to decide on his functions, his salary, and his location. We were operating on a shoestring, since the universities had not given us money. My feeling was that the presidents were not enthusiastic about the new organization. I am quite sure this was true for Missouri. Here we were, having recently been thrown out of the AAU, organizing independently, in much the same way as the medical schools or the business schools.

I made many efforts to get a president. One of the men whom we tried hard to get was Alexander Heard, who after being dean at the University of North Carolina became president at Vanderbilt University. We proposed a salary of $25,000 a year for the president, expecting this amount would attract a top-quality individual, but evidently it was not sufficient to compete with the presidency at Vanderbilt, although it seemed to be a tremendous salary at the time. Only a few deans, I suspect, even approached this salary. One of my few achievements as president of the council was the acceptance by Gus Arlt who was then dean of the University of California at Los Angeles, of this presidency. He was nearing retirement and had a lovely home in California. It was quite a sacrifice in many ways for Gus and Mrs. Arlt to move to Washington, but he accepted the responsibility and gave a prestige and a wisdom to the operation that
could not be surpassed.

I had several conferences with him on trips to Washington, and I remember particularly the time when the National Humanities Foundation was being established. I don’t know who claims the credit for this. I suppose it is much like the NDEA Title IV. When I went to Washington, it seemed everybody to whom I was introduced in the Office of Education confessed to me privately that he almost alone had been responsible for this legislation. I suspect that a great many others claim and probably deserve much credit for the establishment of the National Humanities Foundation. However, it was Gus Arlt who, at the critical point, gave it what it needed. A senator called him on a Saturday said that on the following Monday there was going to be a bill presented to do something for the humanities comparable to what the National Science Foundation did for the sciences. Gus, after seeing the bill, told the senator it was impossible as it was written. The senator responded, “Then you write a bill yourself.” So Gus Arlt, who I suspect had no experience in this field, spent the weekend writing a bill that was accepted and approved by the Senate with almost no change.

He worked hard to establish the foundation, and I believe was modest himself in claiming credit for this achievement. If the Council of Graduate Schools had done nothing else, I think this achievement justified all the effort that went into its establishment. The foundation corrected to some extent the imbalance in the efforts of the federal government to support scholarship. Aid for the sciences was natural as a result of two disastrous wars. We barely survived as a result of technical developments that enabled us to overcome our enemies. But as Vannevar Bush, dean at MIT and a founder of the National Science Foundation, remarked, “The major function of science from a practical standpoint is to relieve society of the burden of work and permit enough leisure time so the humanities can flourish.” This was a point of view that I think most science deans and doubtless science professors would find quite acceptable, but it might be a surprise to those in the humanities and social studies who viewed with suspicion all work in science because of the relatively poor support in their own fields.

This first year of CGS for me not only involved finding Gus Arlt and persuading him to take the presidency but also going to Washington and securing the necessary quarters on Massachusetts Avenue right near the Association of Land Grant Colleges where he could function effectively. I never thought I would be involved in the real estate business and signing up for office space in Washington, D.C. It was with a profound sense of relief that I gave up the presidency of the council and saw it taken over by Arlt.
X.

In Retrospect

This chapter is devoted to a brief overview of the two major concerns of graduate schools: research and intellectual development of graduate students. These are interdependent and each would be unsuccessful without the other. The first, however, is focused objectively on securing new understanding while the second is focused on the development of the individual student.

The proceedings of the Association of American Universities and of the Association of Graduate Schools over a period of eighty years contain many addresses on the above topics given by faculty members and administrators of our most distinguished universities. These are accessible with the aid of an index compiled by Margaret Dickenson in 1957, but unfortunately few, if any, graduate schools have complete sets and these are more than likely to be found in the storage with little used volumes. We still have the same problems though some answers have changed.

The administration of graduate schools as it relates to the development of students varies greatly between two extremes. On the one hand, there is the institution with a long tradition of excellent graduate work, a distinguished faculty, and carefully selected graduate students. Perhaps the greatest possible contribution of the administrators of the Graduate School is to stay out of the way. I have already pointed out that at one time The John Hopkins University had no graduate dean. I am informed that the most recent dean at Harvard accepted the position with the understanding that he would have but one function, namely to serve on the committee that considers faculty for promotion and tenure.

At the other extreme are institutions that go to great pains to "protect" the quality of the Ph.D. degree. The faculty establishes "requirements" for the degree, which become numerous and often threaten the freedom that is so necessary to a graduate student. Graduate schools are often criticized for insisting on trivial details of graduate work rather than emphasizing great achievement. It is quite
true that no regulations can assure a high quality of work, and it is also true that many trivial details may largely destroy what might have been a successful experience. On the other hand, a highly significant achievement is often the sum of a large number of what might be called trivial details. Most faculty members consider the following essential for a high-quality graduate program:

1. A competent, research-oriented faculty;
2. A considerable number of full-time, good students in each field;
3. Frequent lectures by visiting scholars;
4. A research library;
5. For experimental sciences, research laboratories, instrumentation, and technical assistance;
6. A student program that assures taking advantage of the offerings of the university;
7. A schedule that enables one to complete degree work in a reasonable number of years.

All of the above factors may be favorable for good graduate work without giving any guarantee that the result for a given student is success. The student may not make use of the opportunities because he fails to understand their significance for his own development or he avoids them because of his financial problems, especially if he is married and has a family. The Title IV Fellowships of the NDEA program administered by the Office of Education tried to solve this problem by granting stipends that were large enough to make outside work unnecessary. The student was required to agree that he would not supplement his income by teaching. In addition, the fellowship program provided support for three years for an individual in order that he would not be pressed to hurry his degree work.

The following comments at an AGS meeting have been helpful to me in evaluating our procedures and requirements:

Richard C. Tolman of the California Institute of Technology remarked that there are but two requirements for the Ph.D. degree: first, mastery of a body of organized knowledge, and, second, a contribution to that body of knowledge through research. The first rules out a degree in certain fields, for example, in which we may have social problems, but there is not yet an organized body of knowledge. Tolman's criteria do not solve the problem posed by a competent student who comes to a university with a thesis completed in an industrial laboratory.

K. T. Compton, president of the Massachusetts Institute of Technology, remarked that many students come from industry where
they have completed excellent research with superb equipment and then want a degree. MIT decided that its degree will be given for work and training for which it is responsible.

Paul Hudson of the Ohio State University suggested that a thesis should not be considered primarily as a research report but rather as a record of a student’s growth while beginning research and having failures as well as success. What he does as a result of his failures may be the most important part of his thesis.

Finally, no set of rules and regulations can be criticized as being too rigid, if the administration makes proper exceptions. I remember one graduate student who failed to complete an undergraduate major in chemistry because he was so busy taking advanced courses in physics and mathematics. Another student, for what seemed good and sufficient reasons, had to have his thesis bound on the right side instead of the customary left side. A third graduate student found that a twenty-four-hour day with the customary breaks for meals and sleep did not permit an effective work schedule on his thesis so he went on a thirty-six-hour day.

Changes in the Role of Research

Probably no one would challenge the statement that the most important single element in the program for the Ph.D. degree is research and the thesis. The radical changes that have taken place in university research in the last fifty years are, therefore, of great importance in any discussion of graduate education.

In an earlier section of this narrative, I have given some idea of the attitude toward research in several universities in the first quarter of this century. One or two additional comments may serve to highlight the changes.

When Frank Whitmore, chairman of the Department of Chemistry at Northwestern University, gave advice to graduating students, he remarked that they might find it hard to break away from the research they had done for theses that had been initiated by their advisers. He suggested that possibly a good way to proceed would be to take any good textbook of chemistry, an organic book for an organic chemistry, open it at random, read the first sentence, and then say, “I don’t believe that.” Then go into the laboratory to discover the truth. He went on to say that you might well fail to make your point but in the process you would probably discover a number of unexpected facts that you could not explain and you would be off on a research effort.

G. N. Lewis, dean of chemistry at the University of California, gave advice to students starting research not to read the literature
until after they had done some experiments. In other words, let nature speak first and be sure you hear what she says by not having the more or less prejudiced comments of other chemists ringing in your ears. Hugh Taylor, chairman of the Department of Chemistry at Princeton University, advised students to do what he called "A damn fool experiment" by which he meant a simple and casual experiment to see what nature had to say before spending a lot of time planning what some text or theory might suggest.

Going still farther back in time to the founding of land grant institutions, there was considerable debate on what their functions should be. The upshot of the discussion was that there should be three distinct activities: the training of students, the discovery of new knowledge that might be useful to farmers, and the communication of these practical results to the farmers.

The foregoing comments suggest a fresh, exciting, and stimulating attitude toward research with emphasis on listening to what nature has to say and trying to understand what can happen. The concept of exploiting such knowledge was not entirely absent, but it was certainly not dominant in the minds of investigators.

The practical and commercial value of research may have received its first official blessing with the inclusion of research in the programs of the land grant colleges. It was hoped that basic research would produce discoveries of value to farmers. After World War I, the chemical industry in the United States became aware of the importance of research. The du Pont Company was one of the leaders in creating a research budget and even went so far as to finance graduate fellowships in fields not related to their special interests except that they were in the general area of chemistry. When Wallace Carothers was acquired by du Pont, he was given almost complete freedom to work in any field in which he was interested. His discovery of nylon was eloquent testimony that this was a good investment.

More immediate and practical results were expected as the Chemical Warfare Service expanded its activities with the addition of new and more effective agents. Oppenheimer pointed out that the development of the atomic bomb did not constitute more basic science but the shaking of a tree of knowledge to obtain the fruit rather than planting new trees.

Meanwhile, the Office of Naval Research (and later the National Science Foundation) was making sustained efforts to maintain our progress in basic scientific research. But it was Lyndon Johnson who announced that in the future we would make sure that research would be directed toward goals that would be of practical value to society. While all of these changes were taking place, the role of
research in universities was changing dramatically. Instead of research being a financial burden on a university, it increasingly was able to find support from federal agencies that would not only pay all of the usual costs of the research, including the salaries of investigators, but also pay enough overhead to support a significant fraction of the university budget.

It is not surprising, therefore, to find that the presidents who only a few decades earlier had withdrawn from graduate school activities and formed a new association now became much interested in that phase of graduate work that is central to the earning of the doctoral degree. Thus in 1978 appears a report entitled “Research Universities and the National Interest—A Report from Fifteen University Presidents.” This report gives much attention to the financial aspects of research. In the writing of the report, several academic individuals were involved including a dean of business administration, but not one dean of a graduate school. Perhaps if a graduate dean had been involved the sentence on page 16 that “basic scientific research as a whole is not profitable in the ordinary commercial sense” would not have been included but instead the importance of research training in creating scholars who could be problem solvers would have been emphasized. The emphasis on the economic benefits from research has led to the conclusion in the “Presidents’ Report” that an investment in research is not too wise, and hence the impact of this report may undercut the federal support for graduate work. At the same time, the desire on the part of universities to obtain government contracts for applied research has put universities in competition with industrial laboratories that in turn might be expected to reduce the demand for university graduates. In other words, faculties doing research and using graduate students at relatively low salaries are in competition with the product of the graduate schools. This again is a loss to our graduate schools.

At the same time that these changes have been taking place, the sciences have become more mature and experimental methods more sophisticated. As Seaborg has pointed out, the investigator now increasingly uses “black boxes” to obtain his data. In other words, his understanding of many details of his research is less complete. The same effect may result from the use of computers and data processing equipment. This is not to urge a return to the “good old days” when the investigator perhaps had a better understanding of the influence of some variable on his results, but it does suggest a different kind of appeal on the part of a research problem. Similarly, large equipment requiring team work and specialization changes the attitude of the investigator and involves him less in the experiment under way.
If the salary of a member of the staff of a university is a measure of the importance the institution attaches to the individual’s work, then it is evident that an ambitious staff member should make every effort to move up the ladder from his research and teaching into an administrative position. Similarly, the recognition that comes to a staff member when he succeeds in bringing a grant to the institution may, and sometimes, does, lead him to give up basic research and to turn to some immediately practical problem.

Where will all of the above factors and many more take us? The purpose of this narrative is not to give answers but rather to state facts and to raise questions. Much will depend on our leaders. Even the worst future, as it might seem to some, may have a bright side. I remember Kirk Bryan of the Geology Department at Harvard remarking that it might be well to experience the kind of poverty that would make necessary again the wearing of our academic gowns to cover our rags and our hoods to carry our lunches. We would then be sure that only those who were genuinely interested were in the profession.
Appendix

COUNCIL OF GRADUATE SCHOOLS IN THE UNITED STATES

Resolution Regarding Graduate Scholars, Fellows, Trainees, and Assistants

Acceptance of an offer of financial aid (such as a graduate scholarship, fellowship, traineeship, or assistantship) for the next academic year by an actual or prospective graduate student completes an agreement which both student and graduate school expect to honor. In those instances in which the student accepts the offer before April 15 and subsequently desires to withdraw, the student may submit in writing a resignation of the appointment at any time through April 15. However, an acceptance given or left in force after April 15 commits the student not to accept another offer without first obtaining a written release from the institution to which a commitment has been made. Similarly, an offer by an institution after April 15 is conditional on presentation by the student of the written release from any previously accepted offer. It is further agreed by the institutions and organizations subscribing to the above Resolution that a copy of this Resolution should accompany every scholarship, fellowship, traineeship, and assistantship offer.

The following list includes those institutions which have indicated their support of the above resolution as of April 1, 1976.

Adelphi University
Alfred University
Arizona State University
Arkansas State University
Atlanta University
Auburn University
Ball State University
Baylor College of Medicine
Baylor University
Boston College
Bowling Green State University
Bradley University
Brandeis University
Bridgewater State College
Brigham Young University
Brown University
Bryn Mawr College
California Institute of Technology
California State University at Fullerton
California State University at Hayward
California State University at Long Beach
California State University at Los Angeles
California State Polytechnic University, Pomona
California State University, Chico
California State University, Sacramento
Case Western Reserve University
Catholic University of America
Central Missouri State University
Central Washington State College
City University of New York
Clark University
Clemson University
College of Saint Rose
College of William & Mary
Colorado State University
Columbia University
Connecticut College
Cornell University
Creighton University
Dartmouth College
DePaul University
Drake University
Duke University
Duquesne University
Eastern Illinois University
Eastern Kentucky University
Eastern Washington State College
East Texas State University
Emory University
Federal City College
Florida Atlantic University
Florida State University
George Peabody College for Teachers
Georgetown University
Georgia Institute of Technology
Georgia Southern College
Hahnemann Medical College and Hospital of Philadelphia
Harvard University
Hofstra University
Idaho State University
Illinois Institute of Technology
Illinois State University
Indiana State University
Indiana University of Pennsylvania
Iowa State University
John Carroll University
Johns Hopkins University
Kansas State College of Pittsburg
Kansas State University
Kent State University
Lamar University
Lehigh University
Loma Linda University
Louisiana State University
Louisiana Tech University
Loyola Marymount University
Loyola University of Chicago
Marquette University
Marshall University
Medical College of Wisconsin
Medical University of South Carolina
Memphis State University
Miami University
Michigan State University
Michigan Technological University
Mississippi College
Mississippi State University
Montana State University
Montclair State College
New Jersey Institute of Technology
New Mexico Institute of Mining and Technology
New Mexico State University
New School for Social Research
Niagara University
North Carolina State University at Raleigh
North Dakota State University
North Texas State University
Northeastern University
Northern Illinois University
Northwestern University
Nova University
Oakland University
Ohio State University
Ohio University
Oklahoma State University
Old Dominion University
Oregon State University
 Pace University
Pan American University
Pennsylvania State University
Pepperdine University
Polytechnic Institute of New York
Princeton University
Purdue University
Rensselaer Polytechnic Institute
Rice University
Rockefeller University
Rutgers, The State University of New Jersey
St. John’s University
Saint Louis University
San Diego State University
San Francisco State University
Sangamon State University
San Jose State University
Seton Hall University
South Dakota State University
Southern Illinois University at Edwardsville
Southern Methodist University
Southwest Texas State University
Stanford University
State University of New York at Albany
State University of New York at Binghamton
State University of New York at Buffalo
State University of New York at Stony Brook
Stephen F. Austin State University
Stetson University
Stevens Institute of Technology
Temple University
Tennessee State University
Texas A & M University
Texas Christian University
Tulane University
Tuskegee Institute
Vanderbilt University
Villanova University
Virginia Commonwealth University
Virginia Polytechnic Institute & State University
Wagner College
Wake Forest University
Washington State University
Washington University
Wayne State University
Western Illinois University
Western Kentucky University
Western Michigan University
Wichita State University
William Paterson College of New Jersey
Winthrop College
Wright State University
Xavier University
Yale University
Youngstown State University
University of Akron
University of Alabama
University of Alabama in Birmingham
University of Arizona
University of Arkansas
University of Bridgeport
University of California, Berkeley
University of California, Irvine
University of California, Los Angeles
University of California, Riverside
University of California, San Diego
University of California, Santa Barbara
University of Chicago
University of Colorado
University of Connecticut
University of Dayton
University of Delaware
University of Detroit
University of Florida
University of Hawaii
University of Houston
University of Illinois at Chicago Circle
University of Illinois Medical Center, Chicago
University of Iowa
University of Kansas
University of Louisville
University of Lowell
University of Maine
University of Maryland
University of Massachusetts at Amherst
University of Michigan
University of Minnesota
University of Mississippi
University of Missouri at Columbia
University of Missouri at Kansas City
University of Missouri, Rolla
University of Missouri at St. Louis
University of Montana
University of Nebraska, Lincoln
University of Nebraska at Omaha
University of Nevada, Las Vegas
University of Nevada, Reno
University of New Mexico
University of New Orleans
University of North Carolina at Chapel Hill
University of North Carolina at Charlotte
University of North Carolina at Greensboro
University of North Dakota
University of Northern Colorado
University of Northern Iowa
University of Notre Dame
University of Oklahoma
University of Oregon
University of the Pacific
University of Pennsylvania
University of Pittsburgh
University of Rhode Island
University of Richmond
University of Scranton
University of South Florida
University of Southern California
University of Southern Mississippi

University of Tennessee at Chattanooga
University of Tennessee, Knoxville
University of Texas, Austin
University of Texas Graduate School of Biomedical Sciences at Galveston
University of Texas Health Science Center at San Antonio
University of Texas at San Antonio
University of Toledo
University of Tulsa
University of Utah
University of Virginia
University of Washington
University of Wisconsin, Madison
University of Wisconsin, Milwaukee
University of Wisconsin, Oshkosh
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BOOKS MAY BE RECALLED BEFORE THEIR DUE DATES

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