The Making of an Institute

The MBI at UCLA: 1960–1978

Richard E. Dickerson        January 2009
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1960 – 1978

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INTRODUCTION

One year short of half a century ago, a committee of UCLA faculty made an important proposal: the University should have a "Laboratory (or Institute) of Molecular and Cellular Biology". The idea simmered for three years, but really got underway in 1963 when Paul Boyer was recruited from the University of Minnesota faculty. After nearly two years of planning he finally assumed the post of the first Director of a newly formed Molecular Biology Institute, and his leadership over the following sixteen years led to a concept, a faculty, a graduate program and finally an Institute building. For years it was known simply as the "Molecular Biology Institute Laboratory", but in 1998 it was renamed "Boyer Hall" in recognition both of Paul's tireless work in building the MBI, and his award of a Nobel Prize in Chemistry.

This is the story of how all this Institute-building came about. It has been gleaned from the voluminous files of the MBI office and those of Paul himself, and conversations with various players in the drama. I had the privilege of serving as the second Director in 1983, Arnie Berk followed in 1994, and Steve Clarke in 2001. We all (hopefully) did our best. But none of us had the formative influence that Paul exerted in the period covered by this book.

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1. THE CONCEPT OF MOLECULAR BIOLOGY

When scientists turned their attention to the chemistry of living organisms in the mid-1800's they initially called it, logically enough, "organic chemistry". But after this term had been hijacked by the carbon crew a new term became necessary. In the late 1800's the label "biochemistry" appeared, and this sufficed for more than half a century.

By the late 1930's another step forward was about to be taken. Biochemistry traditionally dealt with the chemical reactions that occurred within living organisms. When it became possible to think of working out the large-scale structures from which living cells were built, and the genetic origin of both these reactions and these structures, the issue became more than one of simple chemical processes. The issues that ultimately defined molecular biology were twofold:

(a) What are proteins, and how do they work?
(b) What are genes, and how do they work?

We now know that globular proteins are folded polypeptide chains, and genes are folded DNA chains, but neither piece of information was available when molecular biology began. And the issues "How do they work?" were a complete mystery.

In the 1930's Hermann Mark in Vienna and William T. Astbury in Leeds, England, began to examine fibrous proteins with x-rays and J. D. Bernal in London started looking at crystals of globular proteins, aided by his graduate students Dorothy Hodgkin and Max Perutz.

At some point during this era someone coined the term "molecular biology" to describe this new structure-oriented discipline. Warren Weaver was the director for Natural Sciences at the Rockefeller Foundation, and in the Annual Report of the Foundation for 1938 he wrote that: ".....gradually there is coming into being a new branch of science—molecular biology—which is beginning to uncover many secrets concerning the ultimate units of the living cell" (1, 2).

William Astbury, the fiber x-ray diffractionist at Leeds, also used the term "molecular biology" and put it in print for the first time in a review article in the 1939 volume of the Annual Review of Biochemistry (3). The term is found only twice in his article: once in the opening sentence: "Biology is fast becoming a molecular science..." and later: "To the molecular biologist there can be no question but that the most thrilling discovery of the century is that of the nature of the tobacco mosaic virus; it is but a nucleoprotein." Astbury cites 31 literature references dated 1938 and none dated 1939, so it is probable that his review was written early in 1939 at the very latest.

So who first invented the term "molecular biology": was it Warren Weaver, William Astbury, or someone else? After the passage of seventy years, trying to find this out is as frustrating as trying to hatch dinosaur eggs. Max Perutz has told me that he thinks Astbury was the probable author. A good part of Astbury's research funding came from the Rockefeller Foundation and Weaver
would have been his administrator. It seems more likely to me that the administrator picked up the terminology from one of his grantees, than that Astbury would first have learned of the expression from an internal Rockefeller Foundation report which was not widely circulated and was little known to the scientific world at large.

Linus Pauling, the master of the first person singular, has also entered the fray. He told Horace Judson (1) that, "Warren Weaver’s usage was quite significant. The Rockefeller Foundation had started supporting my work about 1932, I believe it was, or 1933.....This was largely Warren Weaver’s idea, that the time had come when a more basic attack ought to be made on the problem of life, in the field of biology and medicine. They put up a large amount of money for our work in Pasadena, several million dollars over a period of years, and I think at that time I may have used the term 'molecular biology' too." (emphasis added). If Pauling indeed used the term, it never got into print.

But all questions of authorship aside, it is important to realize that the term "molecular biology" did not move rapidly into general use. In 1915 the 25-year old William Lawrence Bragg had shared the Nobel Prize for Physics with his father, William Henry Bragg, for their pioneering work in x-ray crystallography. In 1947 W. L. Bragg sought and obtained funds from the Medical Research Council of Great Britain to set up a laboratory at Cambridge for the x-ray crystal structure analysis of proteins with Max Perutz as the principal investigator. A decade later it was known as the "Medical Research Council Unit for Molecular Biology" (or more colloquially the "MRC Laboratory"), but its original name was the "Medical Research Council Unit for the Study of Molecular Structure of Biological Systems".

In 1959 John Kendrew, Perutz’s colleague and collaborator at Cambridge, founded the Journal of Molecular Biology. As an indication of how uncertain people still were about the term, Kendrew joked at the time that he worried about the legitimacy of the title "molecular biology" for his journal. But the Japanese were using the term, he said, so it must be valid. (As an interesting bit of trivia, Kendrew had wanted a full page size for his new journal, 8.5" by 11", as found in many other scientific publications. But the publishers demurred; such a large page size would make the journal too thin, which is bad psychology. They insisted on reducing the page size to 6.5" by 10", thereby increasing the number of pages and guaranteeing a journal of respectable thickness. John was not pleased. Of course you know the outcome: within a decade the JMB was occupying several running feet of new library shelf annually, and the publishers finally elected the larger page size.) Kendrew’s journal probably was the greatest single factor in the growing acceptance of the term "molecular biology".

"Molecular biology" as a term really became widespread after the anno mirabilis of 1962 when Cambridge had four Nobel Prize winners: Perutz and Kendrew in chemistry, Crick and Watson in medicine. But three years prior to that a committee of life science faculty at UCLA had decided that it was high time for us to found a program and an Institute of Molecular Biology here. That is the story of this book.
2. THE CALL FOR A MOLECULAR BIOLOGY INSTITUTE

In 1959 John Green, Professor of Anatomy at UCLA, put together an informal committee to look into the idea of creating a "Laboratory (or Institute) of Molecular and Cellular Biology". The ad hoc committee consisted of:

John D. Green   Anatomy
Karl C. Hamner    Botany
George A. Bartholomew, Jr.  Zoology
Thomas W. James    Zoology
Ralph W. McKee    Physiological Chemistry
Louis J. Zeldis    Pathology
Joseph F. Ross  Nuclear Medicine & Radiation Biology
Justin Stein  Cancer Research Institute

After meeting for several months, on 29 January 1960 they wrote a memo to Chancellor Vern Knudsen which amounted to a Magna Carta for the eventual MBI, and which is shown in Figure 2.1. As they stated there:

"The proposal is for a Laboratory (or Institute) of Molecular and Cellular Biology, and would be composed of members of the large number of departments which are engaged in this kind of activity. Its primary objective would be to set up an organization to coordinate existing teaching and research activities in these fields..."

They emphasized that they were only recommending the establishment of such an institute, and did not necessarily consider themselves as constituting the organizing committee:

"Since this has met with an enthusiastic endorsement from a large group of faculty, we are asking you to consider appointing a chairman and a formal committee to investigate the advantages of such a plan, leading perhaps to a laboratory or an institute along the lines suggested."

As a potential leader for this effort they suggested either Wilfried Mommaerts (Physiology and Medicine) or Fritiof Sjöstrand (Zoology). They proposed the creation of a central laboratory for this research (which did occur in 1976), and a formal alliance with UC Santa Barbara, UC Riverside and UC San Diego (which did not occur).

The proposal met with a favorable reception and the committee continued planning. In their minutes of 23 March 1960 they said:

"There was some discussion as to the nature of Institutes and their desirability, and it was suggested that this proposal should at first be modest and consist of no more than a seminar program jointly financed by the departments concerned. At some later date a research and teaching program could be started and perhaps funds sought for building purposes."
UNIVERSITY OF CALIFORNIA MEDICAL CENTER
Los Angeles 24, California

Department of Anatomy
School of Medicine

January 29, 1960

Chancellor Vern O. Knudsen
Administration Building
Campus

Dear Chancellor Knudsen:

For some months an informal group, originating as an ad hoc committee in the Medical Center, but quickly expanding campus-wide, has been discussing the possibility of coordinating teaching and research in cellular biology. Since this has met with an enthusiastic endorsement from a large group of faculty, we are asking you to consider appointing a chairman and a formal committee to investigate the advantages of such a plan, leading perhaps to a laboratory or an institute along the lines suggested.

If you wish, our group would be most happy to discuss this with you or any time you designate. The proposal is for a "Laboratory (or Institute) of Molecular and Cellular Biology" and would be composed of members of the large number of departments which are engaged in this kind of activity. Its primary objective would be to set up an organization to coordinate existing teaching and research activities in these fields, provide a basis for joint research grant applications and establish a coordinated group which might take advantage of any financial opportunity to establish a permanent home.

The scientific justifications for such a laboratory or institute seem to us to be:

1. A more efficient teaching program might be established by coordination and combination of activities and avoidance of duplication.

2. The establishment of a postgraduate training program.

3. As widely diverse departments as those of Agriculture and Human Anatomy make use of precisely the same types of expensive equipment in the study of cells, a more efficient use of items such as electron microscopes, ultracentrifuges, spectrophotometers, etc., could be arranged.

4. A close relationship could be established between various basic and applied scientific departments.

5. A strong position would be gained in seeking research grants and, in particular, for general support grants.

Figure 2.1. Initial letter from 1960 calling for the formation of a Molecular Biology Institute--page 1.
6. A more efficient use of personnel might be achieved.

Our group has repeatedly stressed the desirability of some leadership in this direction, and among names which have been mentioned as desirable people to spearhead this activity are those of Dr. Wilfried Mommaerts (Departments of Physiology and Medicine) and Dr. Frithiof Sjostrand (Department of Zoology). We believe that there is sufficient academic support that an active individual capable of devoting a large part of his time to this aim is needed. We believe that the breadth of interest may be indicated by the large number of people from diverse departments who have expressed enthusiastic backing. In many departments practically the whole staff has shown interest, and the list of persons signing this letter indicates only the breadth and not at all the total extent of support offered.

We hope that eventually a central laboratory for this kind of work might be established and, should this come about, the suggestion has been made that our activities could extend to the various campuses of the southern section including Santa Barbara, Riverside and particularly La Jolla and the Scripps Institution. In this way they would parallel closely those of Professor Mazia and his group in the northern section, and to a less extent those of the Geophysics Institute.

Sincerely yours,

For the ad hoc committee:

John D. Green, Professor of Anatomy  Louis J. Zeldis, Professor of Pathology
(Informal Chairman)

Ralph W. McKee, Professor of Physiological Chemistry  Karl C. Hamner, Professor of Botany

George A. Bartholomew, Jr., Professor of Zoology  Thomas W. James, Assistant Professor of Zoology

Joseph F. Ross, Director Nuclear Medicine and Radiation Biology  Justin Stein, Director, Cancer Research Institute

Figure 2.1. Initial letter from 1960 calling for the formation of a Molecular Biology Institute--page 2.
At least a decade might elapse before full fruition. The present concept is that the Institute would be a group with which interested persons might affiliate to pursue problems of mutual interest in the field of cellular and molecular biology.”

The prediction of "at least a decade" for construction of a separate building turned out to be overly optimistic. Their memo was written in 1960; the present Molecular Biology Institute building (now Boyer Hall) was completed in 1976.

Discussions continued, and on 23 January 1961 Vice Chancellor Foster Sherwood appointed the requested committee, consisting of:

<table>
<thead>
<tr>
<th>Name</th>
<th>Department</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wilfried Mommaerts (Chairman)</td>
<td>Physiology &amp; Medicine</td>
</tr>
<tr>
<td>Dan Atkinson</td>
<td>Chemistry</td>
</tr>
<tr>
<td>R. W. McKee</td>
<td>Physiological Chemistry</td>
</tr>
<tr>
<td>J. B. Biale</td>
<td>Plant Biochemistry</td>
</tr>
<tr>
<td>M. J. Pickett</td>
<td>Bacteriology</td>
</tr>
<tr>
<td>Fritiof Sjöstrand</td>
<td>Zoology</td>
</tr>
</tbody>
</table>

The Mommaerts committee met regularly during the subsequent year, and in February 1962 issued a detailed, nineteen-page "Report of the UCLA Committee on Cell Research and Molecular Biology". It was received favorably on campus and in statewide committees, and the final version went to the Vice Chancellor on 8 June 1962.

From the outset they saw two roles for the institute: research and training:

"The two main goals of the proposed institute are (1) to facilitate research in modern biology by bringing together workers with backgrounds and competencies in the various crucial areas of molecular biology and (2) to contribute, at the graduate and post-doctoral levels, to the type of broad education and training that will be needed by the molecular biologists of the future......We visualize that the Institute will be an interdepartmental unit of teaching and research.....”

They proposed that what they called the "core programs" be located in a new central building, but that many investigators housed elsewhere also be included as members. This core was to consist of "six major scientific programs on various aspects of cell research and molecular biology":

- Ultrastructure of cells and tissues
- Molecular physiology of cellular activity, specifically muscle contraction
- Physics and chemistry of macromolecules
- Molecular approach to genetics
- Regulation and energetics of cell metabolism
- Fundamental study of nerve cell activity
In connection with a core building, they even went so far as to give estimates of the square footage necessary for various Divisions of the proposed Institute:

<table>
<thead>
<tr>
<th>Division</th>
<th>sq. ft.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Electron microscopy (developed from Sjöstrand’s program)</td>
<td>12,000</td>
</tr>
<tr>
<td>Macromolecular structure &amp; x-ray crystallography</td>
<td>5,000</td>
</tr>
<tr>
<td>Molecular genetics</td>
<td>5,000</td>
</tr>
<tr>
<td>Cell metabolism, regulation &amp; energetics</td>
<td>5,500</td>
</tr>
<tr>
<td>Molecular physiology (developed from Mommaerts’ program)</td>
<td>6,500</td>
</tr>
<tr>
<td>Molecular physiology and biochemistry of the brain</td>
<td>5,000</td>
</tr>
<tr>
<td>Selected programs from various departments</td>
<td>5,000</td>
</tr>
<tr>
<td>Department of Plant Biochemistry (then housed in Physics!)</td>
<td>12,000</td>
</tr>
<tr>
<td>Machine &amp; electronic shops, seminar rooms, administrative offices</td>
<td>5,000</td>
</tr>
<tr>
<td><strong>Total:</strong></td>
<td><strong>61,000</strong></td>
</tr>
</tbody>
</table>

(The MBI building as constructed in 1976 contained a little less than 68,000 assignable square feet of space.) It is curious that Plant Biochemistry was then housed in the Physics Building. In the end, that Department did not become a part of the central Institute building.

They estimated the total cost of building and furnishings to be $5.5 million, and suggested NSF, NIH and other federal agencies as sources of funds. As to where this new central building might be located:

“We have no final recommendation regarding the site of this building, feeling that this must be considered in the light of other aspects of campus planning. As we have indicated in the preliminary report, the two preferred sites are (a) A connecting building with underpass joining the Medical Center with the Life Sciences building, or (b) A structure on the site of the present greenhouses, which can develop connections towards the buildings of Botany, Chemistry and the Life Sciences.”

Neither of these options was followed up. Later planning involved two bizarre sites that were never implemented, and a scheme to connect Life Sciences and Chemistry that was frustrated by budgetary problems but which led to our present building.

They ended their report with a call to arms:

“May we repeat, in summary, that the field of Molecular Biology is in a stage of explosive growth, that we have an excellent opportunity to assume a position of national leadership, but that, if we do not act decisively, UCLA might find itself left out of one of the most crucial scientific developments during the next decades.”
3. GETTING STARTED AND FINDING A DIRECTOR

In the expectation that plans for a new molecular biology program would be approved, recruitment of faculty was begun. In the spring of 1963 two people who would play a key role in development of the MBI were persuaded to join us: Paul Boyer from the University of Minnesota, and Emil Smith from the University of Utah. Boyer joined the new Division of Biochemistry within the Chemistry Department in the College of Liberal Arts, while Smith became Chair of the Biological Chemistry Department in the Medical School. In Paul's words at the Fall, 1987 MBI Retreat at the Lake Arrowhead Conference Center:

“We were both surprised, several months after we arrived, to learn that there were plans for a Molecular Biology Institute and building.....The UCLA campus was in a rapid growth phase--buildings were coming up like mushrooms all around the place. In fact, I moved into a new wing of the Chemistry building, now Young Hall, when I came in 1963. It looked like there was no problem at all with the building program. One was promised for the Molecular Biology Institute, so we tried to get some capable outside scientist to be its Director. We saw it as an opportunity to meet a need for UCLA, but were unsuccessful in recruiting a director when we tried to show the space that would be available for the new Institute; it amounted then to only a few rooms in the basement of the Life Science Building. I need not tell those of you acquainted with the basement of the Life Science Building that it doesn’t sell very well.”

The concept of a molecular biology institute grew in support that year, and Dean Franklin P. Rolfe set up an Advisory Committee:

Wilfried Mommaerts (Chairman)
Paul D. Boyer
Theodore H. Bullock
Sydney C. Rittenberg
Fritiof S. Sjostrand
Emil L. Smith
Samuel G. Wildman

One of the first jobs of the committee was to select a Director for the new Institute. Rolf’s memo stated: “Although the Director has not yet been chosen (and partly because he has not been chosen), it appears desirable to have this Committee in being to advise on all matters affecting the Institute.” When Mommaerts went on leave that September, Emil Smith stepped in as Chair of the MBI Advisory Committee.

Paul Boyer gradually moved into a leadership role in development of the new Institute. In November 1964, he wrote and submitted to the Advisory Committee four memoranda, prepared after preliminary discussions with Emil Smith:
1. Memorandum on the Role of a Molecular Biology Institute.
2. Memorandum Regarding the Present Status and Organizational Needs for a Molecular Biology Institute at UCLA.
3. Memorandum Regarding Establishment of Courses in Molecular Biology and Biology at UCLA.
4. Suggested Operational Procedures and Functions of a Molecular Biology Institute at UCLA.

Memorandum #2 gives us the date of approval in principle of a Molecular Biology Institute: "[The 1962 Mommaerts committee’s recommendation] served for subsequent approval by the Faculty, by Chancellors Sherwood and Murphy, and by the Regents for the establishment of an Institute."

Initial attempts to find a Director were unsuccessful. In memorandum #2 Paul stated:

"The limitations of the present bases for a possible institute has been a principal stumbling block in our attempts to attract top-level personnel to the directorship. Both written and unwritten reasons for declination of three individuals should serve to alert the Molecular Biology Committee and the Administration to the unsatisfactory nature of plans at their present stage of development."

In response to a query, Paul has informed me that Albert Lehninger and Phillip Handler were the people who declined the Directorship, and attempts to recruit Boris Magasanik and Dieter Soll to provide early stature and possible leadership were not successful.

In memorandum #4 Paul emphasized the vital importance of faculty recruitment:

"Nothing is as important to the development of the Molecular Biology Institute at UCLA as the recruitment of outstanding staff. Provision of adequate degree programs, course programs, and academic environment are essential to the attraction of staff of high caliber. Recruitment can only proceed through the full cooperation of departments. FTE’s need to be provided by the administration through the Institute to the departments and from the departments to the Institute. Active departmental participation is required in nomination of suitable candidates for membership in the Institute from present staff and in recruitment of new staff members of the Institute."

He also raised the issue of space and a new laboratory building:

"A considerable deterrent to the development of an Institute is the long time period which must elapse before adequate new building facilities will be available. In the interim, the Institute will have to depend heavily on the respective participating departments. If the departments wish a viable Institute, considerable expansion in staff over a period of the next
four to six years is essential. Space will need to be made available, perhaps crowding some of the present operations. Many appointments will need to be postponed until facilities are completed. When the Institute facilities become available, the departments could look forward to a welcome release of their space."

There is no point in going after major "over-scale" appointments, he commented, as long as we have no building. But in any event we should recruit younger people:

"In addition, we are less likely to freeze our operation and areas of interest at the present time if staffing is done with younger, promising staff. Such a staffing may insure having a molecular biology which will achieve its most important status ten years hence."

An important principle applies when setting up a charitable fund-raising committee: Select a Chairman who is rich enough to fund the project on his own. Then if the fund-raising campaign drags along without success, the time may come when this Chairman says, "Oh, never mind. It's a good cause. I'll fund it myself." This seems to have been the story with Paul Boyer. He was not initially recruited to UCLA specifically to become Director of the new MBI, and was not even the Chair of the MBI Advisory Committee that conducted the search for a Director. But it is obvious from the memoranda mentioned above that his mind was moving in that direction. On 4 December 1964 he wrote to Vice Chancellor Sherwood:

"I appreciate the opportunity to discuss academic and administrative needs for a viable Molecular Biology Institute with you next Wednesday morning, as there is an outstanding opportunity in this area at UCLA. My views of the needs have been brought into considerably sharper focus by the possibility of my assuming the Directorship. As you are likely aware, I have expressed to the Molecular Biology Committee the view that the present plans and formulation of an Institute are not sufficiently attractive for recruitment of a director or for development of the type of Institute you would like.......At the best, development of an Institute of the type you want will be difficult. Personally, I would not want to leave my present highly-valued academic position to assume the Directorship unless conditions appeared extremely favorable."

When you talk to a Vice Chancellor in this tone of voice you obviously feel that you are in control.

In a 10 December memo following the meeting with the Vice Chancellor, Paul referred to their "frank and cordial discussion", which usually means politeness but not necessarily agreement. He listed nine tough requirements, including:
a. Twenty to thirty new FTE's over the next five years.
b. FTE's to be made available to Departments through the Institute and its Director.
c. Faculty appointments to be made at all levels, junior through senior.
d. “The administration recognizes that most of the FTE’s cannot be filled until a new building is completed and assurance is given that FTE’s will be made available as space develops.”
e. Details of provision of recruitment and support funding to the MBI.

Boyer also asked for firm commitments regarding an institute building:

“The location between Chemistry and the Life Sciences building is suitable and approved for a Molecular Biology Institute building. A final but very important item concerns the projected time for completion of a building. Occupancy in the fall of 1970 leaves a potentially disastrous hiatus between the initial momentum and enthusiasm for an Institute and its fruition. Whatever assurances could be forthcoming to reduce this time would be a strong additional incentive to my assuming the Directorship.”

If all of these points are agreed to, said Paul, “I would be in a position to accept the Directorship and move ahead enthusiastically with plans.” In retrospect, I wonder how Paul would have reacted had he known that bitter battles with state and federal funding agencies loomed ahead, and that the potentially "disastrous hiatus" until a building was ready in 1970 would in fact extend to 1976? Vice Chancellor Sherwood’s reply on 18 December said, “I am asking Vice Chancellor Young by copy of this memorandum to give serious consideration to the possibility of moving the construction of the Institute space up in the five year building program.” It didn't happen!

Negotiations continued. Finally, on 2 March 1965 Paul sent a memo to Emil Smith and the other members of the Molecular Biology Advisory Committee that he had accepted Vice-Chancellor Sherwood’s offer of the Directorship, and on 29 March the Vice-Chancellor finalized the appointment. A few weeks later Boyer requested that the name of the organization on letterheads, catalogue listing, etc. be officially the "Molecular Biology Institute", and not other terms that had formerly been used interchangeably: "Institute for Cell Research and Molecular Biology", "Institute for Molecular Biology", or "Institute of Molecular Biology". Sherwood concurred.
4. BUILDING AN INSTITUTE

Paul Boyer's first public act as the new Director in 1965 was to write and issue a widely-distributed statement entitled "Molecular Biology Institute, University of California, Los Angeles: ORGANIZATION, PLANS AND PROSPECTS, 1965". Under "Facilities", he stated:

"Initially, staff members will be housed in facilities of present departments throughout the campus. This includes laboratory and office space in the recently completed construction in the Life Sciences Building. Plans call for beginning construction of a Molecular Biology Institute Building in about three years......This is part of the projected building program of UCLA, to be placed before the legislature for funding in the near future.....Additional support by extramural funds is anticipated, resulting in a building approaching 100,000 net square feet of usable space.....A splendid physical location is available which will place the Institute building between the Chemistry and Life Sciences building, closely adjacent to other physical and medical sciences.....[T]he facilities should provide an architecturally and aesthetically pleasing environment, sharing other advantages of the UCLA campus located in an excellent residential area in the foothills of the Santa Monica Mountains. The amiable and relatively smog-free climate, and the outstanding cultural and intellectual opportunities of a major campus should not detract from productivity of staff."

The final building was completed in 1976 and comprised a little less than 68,000 square feet of assignable space. I don't know how to take that remark about "amiable and relatively smog-free climate"; is this public relations, or humor, or sarcasm? Probably a mixture of all three.

Five months after assuming the Directorship, Boyer issued a 4-page report entitled "Annual Newsletter No. 1". These annual newsletters have continued to the present day with a certain amount of augmentation: the 2007 edition contains 177 pages.

In Newsletter No. 1 Paul announced his first faculty recruitment: Verne Schumaker to arrive in late 1965 as an Associate Professor of Molecular Biology in Chemistry. Paul quoted a recommender whose words I cannot resist repeating: "Behind a pleasant, soft-spoken and polite outward appearance, probably achieved by careful self-discipline, lies a very forceful, determined and deeply thoughtful individual." Paul also made the enigmatic statement that "As many of you are aware, some two or three temporary American members of a relatively obscure British Laboratory visited the campus last spring and considered joining the Zoology and Bacteriology Departments as members of the Institute. I am obliged to report that they will start their professorial careers elsewhere." Relatively obscure? In retrospect it would be interesting to know just who these three candidates were. Emil Smith, who spoke to them in England afterwards, reported that they found the UCLA academic program highly attractive, but the salary offers too low by $3,800.
Negotiations with Bob Langridge also were going well, and Boyer expressed the hope that he would accept our offer. (He didn't.)

The most enigmatic, and fascinating, statement in Newsletter No. 1 concerns plans for construction of the MBI building, formally designated as Life Sciences Unit 3:

"Some discussions with the staff of Vice Chancellor Young's office have led to what appears to be a firm suggestion that the Molecular Biology Building cut across the court north of, and parallel to, the present Life Sciences Building. This offers easy connection to the vivarium area of the Life Sciences and possibly to the Chemistry Building."

If implemented, this would have been a long, probably low building running east/west across the bottom end of the South Campus Court; a twin companion of the present Life Sciences. No plans or sketches of this proposal apparently still exist. Paul has no recollection of such documents having existed, and thinks that the idea may only have been tossed around in general discussions. But it is interesting how different the South Campus area would have been, had this idea been pursued.

The MBI now was in business. New faculty were being recruited. A committee of Smith, Sjostrand, Wildman, Mommaerts and Rosenberg drafted a Molecular Biology Ph.D. proposal which the Graduate Council approved, and the first class of graduate students arrived in the fall of 1966.

The issue of a MBI building remained. In May 1966 Boyer issued a position paper, "The Purpose, Specifications, and Needs in Life Science Unit 3". In it he declared a strong position regarding assignment of space within the planned MBI building: "The location of personnel and the use of the facilities within the building are not to be on a departmental basis, but on a related function basis." That is, investigators with related problems or methods would be grouped together and share facilities in the new MBI building, regardless of the individual departments they might have come from. There would be no "Biological Chemistry floor", or "M & I floor".

In our present MBI building, or Boyer Hall as it is now known, floors 2 and above have three faculty office/laboratory suites on each side of a central north/south corridor. Suites to the east of this corridor are deeper than those to the west. East-side suites have an extra secretarial office between the faculty office and the corridor, and their two large laboratories have additional student and postdoctoral quarters facing the outside of the building. West-side suites have neither of these features. I was given to understand years ago that the architects originally planned these as suites for junior faculty on the west side, and senior faculty on the east. But this would be difficult to manage. What would happen when a junior faculty member achieved tenure? Would he be moved across the hall to the east, and if so, where? What would become of the senior faculty member whose space he took over? Would promotion to tenure have to be coupled with retirement plans? Most floors today have adjusted for
this space inequity by converting the three shallow suites on the west side into two suites of roughly the same area as east side suites.

These plans have deep roots. In Boyer's May 1966 position paper he stated, "All staff and faculty offices are to have exterior windows. Two-thirds of the offices are to be arranged so that entry is only through a secretarial office. These offices are also to have separate access to adjoining laboratories. Other offices may have direct hall entrances." One wonders how the two-thirds/one-third layout would have been achieved. In the ancient Babylonian Epic of Gilgamesh, Gilgamesh is described as the world's mightiest King, two-thirds God and one-third human. One can only speculate about the genetic basis for that ratio, but the two-thirds "East" and one-third "West" arrangement of the proposed MBI laboratory suites is equally puzzling. In the end the ratio (for the MBI, not Gilgamesh) was 50:50.

MBI Newsletter No. 2, in July 1966, reported rapid progress in faculty recruiting:

Verne Schumaker Assoc. Professor of MB in Chemistry ("our lead-off man")
John Fessler Assoc. Professor of MB in Zoology
Felix Wettstein Assoc. Professor of MB in Medical Microbiology & Immunology
Dan Ray Asst. Professor of MB in Zoology
Bruce Howard Asst. Professor of MB in Biological Chemistry
Michael Konrad Asst. Professor of MB in Chemistry
Park Nobel Asst. Professor in MB in Botany and Plant Biochemistry

Note that, in agreement with the philosophy about hiring younger staff that Paul proposed in his 1964 memorandum #4, all seven of these appointments were at the Assistant or Associate Professor levels. Schumaker was already on site, Fessler, Ray, Konrad and Howard were due in the fall of 1966, and Wettstein and Nobel were expected some time during the following year.

Advances in building plans also were set out in the 1966 Newsletter:

"The original Project Planning Guide called for a construction of Life Sciences Unit 3 of about 120,000 net square feet of space. It became evident, however, that the views of our campus and those of the Statewide administration on justifications for postdoctoral and research personnel, and other items which the Building Committee only partly understands, were not the same. An overall result of these discussions has been concurrence and directions from Vice Chancellor Young's office through George Larke, to go ahead with plans for a building of a total of 90,000 net square feet of assignable space. Of this, 45,000 is to come from State supported funds, and the rest of it is to be requested from Federal agencies."

"A most welcome development resulting from the administrative actions on this campus was the listing in the University Bulletin of proposed capital improvements for 1967-68 as approved by the Regents in their May 20th meeting. Those listed at UCLA, in order of priority are:
Marymount School acquisition  
North Campus Library, Unit 2  
Service Yard expansion  
Hospital and Clinics Unit 1, and alterations, step 2  
College Library alterations, step 2  
Engineering, Unit 4, and  
Life Sciences, Unit 3.

_We are at least on the ladder._

The comment that the number one priority for UCLA was acquisition of the Marymount School property is interesting, since it never happened. Marymount High School is still there, on the north side of Sunset Boulevard. Boyer rather optimistically expressed the hope that detailed construction plans could be completed in 1967, Federal building funds could be found that same year, and ground broken in 1968. But he concluded with an ironic comment, "Perhaps Vietnam, rather than the excellence of our overall planning, may be a deciding feature."

Regarding publications, "_The Institute invites its Members and Associates, at their discretion, to list the Institute as a source of their publication._" An informative feature of this and future Newsletters is a list, for each MBI member, of publications during that year that specified the MBI as at least one of its sources. The box score for 1967 was: Boyer (5 publications), Harary (5), Romig (1), Schumaker (2), Smith (2), Wildman (2), Zabin (1) and Zamenhof (2). These numbers alone mean nothing, but together they suggest that the MBI was beginning to be considered a research entity in its own right.
5. INSTITUTING A BUILDING

The ground rules mentioned earlier clearly specified that the State would only supply half the cost of a new building, and that the other half had to come from Federal sources, probably NSF or USPHS. In July 1966 Boyer traveled to Washington DC to consult with both the NSF and USPHS about funding. NSF told him that their earlier budget of 28 million dollars for major programs had been cut to 15 million, of which only a fraction was earmarked for projects of 1 million dollars or more. Both NSF and USPHS suggested that the same proposal be submitted simultaneously to the two agencies on an either/or basis.

NSF had a curious caveat concerning MBI building plans. “One question was raised about a seminar room space for 90 people. This may be too much, and would resemble a classroom to the NSF. One suggestion that emerged was provision for means to separate the space into two smaller rooms by a movable partition.” So NSF wasn’t in the business of subsidizing anything that looked like a classroom? God forbid that the National Science Foundation should support something like science education! And is this the reason that today we have dual seminar rooms, 159/173, with a folding partition between them?

The USPHS suggested that an application for construction funds be submitted by 1 March 1967, with Council review in June. If the program was approved, then UCLA could go to the State Legislature in 1968 with federal funds in hand, and request matching State funds. NSF suggested waiting for the following Council review in September. “In any event, it looks plausible for us to aim for review by both the NSF and USPHS by mid-fall of 1967.”

The architectural firm of Honnold and Rex in Los Angeles was appointed as executive architects on 22 August. The reply by their Design Architect Sam Carson on 2 September 1966 was respectful to the point of being obsequious:

“It is with the greatest respect for you and your colleagues that I read and studied it.”

“As a research scientist you are most aware of the need for uninhibited thought.”

“We as architects in our desire to provide a more meaningful architecture, and a greater sense of reality, often proceed on uncharted courses.”

“The Molecular Biology Institute will be functioning long after I am dead, and it will be functioning long after you have discovered the secrets of life.”

“It is imperative that we not limit our thoughts by cluttering our minds with personal experiences and reference to existing equipment as criteria.”

But after all this hype, Carson made a specific proposal for the MBI building that was rejected by Boyer and his committee:
"As to the point of whether the building works better vertically or horizontally, I at this point am not prepared to say. I can, however, see a facility with high speed computer elevator service, single man lifts, automatic discharging dumbwaiter system, a communication system combining T.V. and telephone, a pneumatic type [tube?] system, and I am sure that as there are many research tools which are yet to be developed and are beyond even your imagination there are architectural devices which I cannot conceive and if were [if were?] to be suggested to me, I would laugh and scoff."

One would hope that Carson was a better architect than he was a writer. Honnold and Rex’s proposal was for a tall tower in the middle of the south campus plaza just south of the present Bombshelter, that would counterbalance the lower buildings surrounding it. The 17-story Factor Building that houses the Jonsson Cancer Center did not then exist.

In a 6 September letter to James Westphall in the UCLA Architect and Engineers Office, Paul ruled out the tower:

"Ease of communication of staff and students to the Chemistry and Life Sciences buildings, including Botany, cannot be overstressed. This is one of the principal reasons why a tower will not serve as well as a 5 or 6-story building. Graduate students and staff from at least basement, 1st, 2nd and 3rd floors (and perhaps higher floors for the more vigorous) should be able to easily leave the building.....to Life Sciences or Chemistry without an intervening elevator or other restriction."

In a draft memorandum on a 15 September meeting of the Building Committee for Life Sciences Unit 3 the various possibilities presented by Honnold and Rex were outlined:

"1) An about 14 story structure located in the main court just north of the main entrance to Life Sciences.” (This is what Honnold and Rex proposed but the committee rejected.)

"2) An about 10-13 story structure located north of the Vivarium wing of Life Sciences with connection at all existing levels in Life Sciences.”

"3) A 6 to 8 story structure located at the east side of the court north of the Life Sciences Auditorium, with a wing extended north along the court and possibly connecting with Chemistry.”

"4) A division of the construction to locate part in the area between Chemistry and Life Sciences and part as extension of Life Sciences Unit 2.”

"After considerable discussion, concurrence was reached that alternative 3 was preferable, and the recommendation was made to Mr. Westphall that he advise the Executive Architects, Honnold and Rex, to develop schematic documents accordingly. It was further agreed that if State funds alone should be available, the construction was to be located
just north of and with connection(s) to the present Life Sciences Building.”

In fact, when later budget cuts forced the reduction of the MBI building from four towers to three, it was the connection to Life Sciences that was given up, while that with Chemistry remained. In the final notes for the same meeting matters were put even more plainly. A high rise structure north of Life Sciences “did not meet the requirements of the program as far as the Building Committee was concerned.” Moreover, “A possible site north and west of the existing Vivarium was discarded in favor of a site extending northward between the existing Life Sciences Auditorium to the Chemistry Building.”

Relations with Honnold and Rex continued to be rocky. On 21 December 1966 Paul reported to the Building Committee about a meeting between the architectural firm on one hand, and Boyer and representatives of the UCLA Architects and Engineers Office on the other. Honnold and Rex were asked to scrap plans for a high-rise tower and begin again. “In the ensuing discussion it became sufficiently clear that the present building did not sufficiently meet our program needs to warrant moving ahead. The decision was thus reached to abandon the present design, and to ask the architects to start again.....Unless we are to set the whole project back a year, we need our building applications ready for the NSF and USPHS for a March 1 [1967] deadline.”

Word of the new research institute finally reached the press. On 10 November 1966 a UCLA press release entitled "Exploring the Mystery of Life" led to two short articles in that day’s issue of the Los Angeles Times: "Molecular Biology Unit Established at UCLA" and the page 2 preview "Biological Institute Being Set Up at UCLA". Nothing was said that wasn't obvious, and there were no illustrations. Nevertheless, it marks a first in external MBI publicity.
6. BUILDING IN PROGRESS

Honnold and Rex went back to the drawing board at the turn of 1967, and came up with a proposal that can be regarded as the direct ancestor of the building that we now occupy. A cross section is shown in Figure 6.1, and a model built for the Regent’s examination is in Figure 6.2. The photograph of this model appeared in the 21 April 1967 issue of the Los Angeles Times. The proposed building was based on a north/south line of four 6-story towers, abutting against Young Hall (Chemistry) to the north, and the auditorium wing of the Life Sciences building to the south. This auditorium was demolished in 2003 to make room for a new high-rise, the Orthopaedic Hospital Research Center or simply the Luck Building.

Figure 6.1. Cross section plan of the original four-tower MBI Laboratory
Figure 6.2. Model of the original four-tower design for the MBI Laboratory. From the Los Angeles Times, 21 April 1967.
In this 4-tower proposal the north tower, or tower 1, was to be connected to Young Hall by ramps at several levels. Elevators were to be located between towers 2 and 3, and the south tower 4 was to abut against the Life Sciences auditorium. Open archways on the ground level of tower 4 would provide access from the south court to Charles E. Young Drive (then known simply as Circle Drive). Connecting structures between towers 1 and 2, and 3 and 4, also would be open at the ground floor level. Connections between tower 4 and the Life Sciences building would involve a tunnel under the auditorium connecting the MBI basement with the first floor of Life Sciences. (Because Life Sciences was built on a slope, its floor 2 was at ground level facing the south court.) The third floor of the MBI would also be connected directly to Life Sciences by a pedestrian bridge crossing over the top of the auditorium, rather like the core of our present nanotechnology building. Ultimately, the tunnel was built but the skyway bridge was not, in large part because budgetary limitations forced the abandonment of the south tower.

The model in Figure 6.2 shows Life Sciences at the lower right, with its box-like one-story auditorium abutting against the south tower of the MBI. The elevated walkway between buildings is not represented in this model. Young Hall also is visible just to the left of the north tower.

The accompanying article in the Los Angeles Times said that the Regents had approved a budget of $7 million for construction of the MBI building. Regent Edwin Pauley opposed the project because he considered the architects' design to be out of character for the campus. Chancellor Franklin Murphy replied: "All of us are very much for that building. We think it would be an attractive addition to what is now a very pedestrian part of the campus." And Regent Dorothy Chandler urged the Board of Regents not to second guess architects hired to plan buildings. So much for you, Pauley!

But along with Regents' approval did not come the funds necessary for preparing working drawings of the proposed building. The 1 July target date for submitting identical applications to NSF and USPHS was missed. In the third annual newsletter, in June 1967, Paul said glumly, "Our next target date is November 1, but this now means that the earliest we can possibly hope to start construction will be in the summer of 1969. Wish us well." In retrospect it is a good thing that no one realized, then, that groundbreaking would not actually take place until 18 September 1974!

Other matters in 1967 were going along more cheerfully than the building program. An interdepartmental Molecular Biology Ph.D. program had begun the year before under a committee composed of:

Paul Boyer (Chair) Chemistry
Verne Schumaker Chemistry
Irving Zabin Biological Chemistry
Dan Ray Zoology
Albert Barber Zoology
Eugene Barber Bacteriology
Samuel Wildman Botanical Sciences
Three graduate students had been admitted then, and ten more were scheduled for the fall of 1967. New faculty members Park Nobel and Felix Wettstein, mentioned in last year's Newsletter, arrived on the scene. And two current UCLA faculty members requested and were granted appointments as Associates of the Institute: Fred Eiserling from Bacteriology and Dave Fahrney from Chemistry. Recruitment of MBI faculty was internal as well as external. An active seminar program brought in talented speakers such as William Lipscomb of Harvard and Robert Kretsinger from the IMB in Geneva.

Identical building fund applications went off to NSF and USPHS in late 1967 as expected. Proposed costs for a building with 90,000 assignable square feet were as follows:

<table>
<thead>
<tr>
<th>Description</th>
<th>Amount</th>
</tr>
</thead>
<tbody>
<tr>
<td>Construction, exterior elements, fixed equipment, fees &amp; misc.</td>
<td>$8,037,000</td>
</tr>
<tr>
<td>Movable equipment</td>
<td>1,580,000</td>
</tr>
<tr>
<td>Nonqualifying costs (no federal participation)</td>
<td>513,000</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>$10,130,000</strong></td>
</tr>
</tbody>
</table>

The hope was that the state could provide half of this sum, and federal sources the other half, or $5,065,000. Site visits from both federal agencies were carried out that fall, but the results ultimately were disappointing.

On 19 April 1968 Boyer was in Washington to discuss issues with Robert Hendrickson, the USPHS administrator. UCLA had proposed a building with 90,000 square feet of assignable space, 45,000 to come from state funds and 45,000 from federal. Hendrickson said that the review Council had recommended approval, but support for only 22,000 square feet and a total dollar limitation of $1 million! Even this would have to wait until the 1970 fiscal year. In Boyer's words,

"[Hendrickson] opened by saying that he was surprised that the council had acted as severely as they did in making the budgetary cut. He felt that we were caught in a peculiar set of circumstances, where budget limitations were severe and expectations grim, together with a very negative reaction for another large application from the California system and UCLA. Typical comments, which I jotted down on a sheet as he read them from his notes at the meeting were as follows:

We have put a lot of money into UCLA already.
As far as I am concerned this is a geographic decision.
Their request represents a large percent of the budget we have to work with.
For this type of money I would like to see more tie-in with the medical school.

Dr. Hendrickson informed me that the USPHS had had requests since 1958 for about $12,700,000 in projects at UCLA, and it paid $8,700,000. At the present time there are also other large applications from the California system. How these fared I do not know nor did I ask."
Hendrickson questioned whether it had been a wise move to ask for total funding from a single agency, and whether 100% of the proposed research was really health-related and hence appropriate for the USPHS. He also queried whether UCLA would have the reputation necessary to attract first-class colleagues from outside. (At this point Paul mentions that the latest acquisitions by the MBI were David Eisenberg and Winston Salser, who had turned down offers from Princeton, Chicago and elsewhere.)

News from the NSF was equally depressing. With only $10 million in building program funds for the entire country, NSF could hardly be expected to give two or three million to UCLA. So Boyer wrote Chancellor Franklin Murphy to ask whether it might be useful and realistic for the MBI to approach some individual private donor or donors in the Los Angeles area. He had discussed this earlier with Sherman Mellinkoff, Dean of Medicine, who agreed to give the matter consideration. (Mellinkoff ultimately introduced Paul to Albert Parvin, whose financial contribution got the MBI building plans moving.)

In Newsletter No. 4 (June 1968) Paul related all of the above trials, and concluded:

"It thus appears likely that we cannot hope that these agencies can furnish about four million dollars in matching funds necessary to allow construction of Life Sciences Unit 3 with a net of 90,000 usable square feet. As a possible means of still keeping alive hopes for starting construction in 1969, requests will be made to the U.S. Office of Education, and explorations will be instituted into possible private support. The U.S. Office of Education will grant funds up to a maximum of 1/3 of the state funds. Thus their participation would mean a reduction in the size of the building."

But on the brighter side, two new faculty colleagues arrived in the fall of 1968: Winston Salser as an Assistant Professor of Molecular Biology in Zoology and David Eisenberg as an Assistant Professor of Molecular Biology in Chemistry. Salser did undergraduate research with Barry Commoner at Washington U., graduate studies with Cyrus Levinthal at MIT, and postdoctoral work at the IMB in Geneva. Eisenberg worked with John Edsall as an undergraduate at Harvard, went to Oxford as a Rhodes Scholar and earned his Ph.D. with Coulson, and followed this with two postdoctoral venues: Walter Kauzmann at Princeton and my own laboratory at Caltech. His appointment in the MBI was part of a deliberate plan to move into the area of macromolecular structure analysis.
7. Give no Quarter? Well, Why Not?

On 19 February 1969 Boyer sent a discouraging report to Vice Chancellor Young. NIH funds for health-related research facilities during the current fiscal year had been cut from $20 million to $0. Another possible source was the Higher Education Facilities Act of HEFA. But they had a backlog of $120 million in approved grants, with only $14 million on hand to spend. Prospects at NSF were equally bleak; no new construction grants were being made.

By 7 March things were even worse. Boyer wrote again to Young:

“I am sharply concerned about the apparent lack of appreciation by the Statewide Administration of our need for Life Sciences Unit 3.....As is well-recognized on our campus, a principal purpose of Life Sciences Unit 3 is to provide for a grossly underdeveloped need in molecular biology.....Almost every university of comparable size in the country, and many universities of lesser size and stature, have separate biochemistry or molecular biology departments of considerable scope.....As we are painfully aware, the possibility of matching Federal support for our building is not bright. Thus we must attempt to salvage as much of a building program as possible to meet the most pressing needs. The alternative of cutting off our rapidly expanding Ph.D. program in molecular biology and of denying the impact of this program on undergraduate biology is grim indeed.....Your Building Committee and the Molecular Biology Institute staff have discussed the minimal needs for viability, and an approximate estimate at this time is for about 60,000 assignable square feet. If it may be of value to your planning, we could examine this more closely with various departments concerned so as to give you a better estimate of such a minimal figure.”

In the face of all of these difficulties, both state and federal, the MBI building lost its south tower. The Building Committee began considering how to go from 90,000 square feet to three-quarters or even half this size. Figure 7.1 from Paul’s files lists thirteen different plans for a reduction, along with estimates of the remaining square footage in each case:
- Deletion of the 6th floor.
- Deletion of the 6th floor and parts of the 5th and 4th.
- Elimination of the south tower.
- Elimination of the north tower.
- Elimination of both towers.
- Various combinations of elimination of towers and floors.

Figure 7.2 contains the various cost estimates for these plans, along with possible breakdowns of costs between state and federal sources. Finally, Figure 7.3 is Boyer’s sketch of a hypothetical floor plan of a building relieved of both its north and south towers. This plan would have only four faculty suites per floor, rather than the eight of the original 4-tower design or the six of today’s building. Room boundaries are only tentative; but note that already the design calls for
### POSSIBLE PLANS FOR REDUCTION IN BUILDING SIZE OF LIFE SCIENCES UNIT 3

<table>
<thead>
<tr>
<th>Plan</th>
<th>Deletions</th>
<th>Approx. Net a.s.f.</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Removal of 6th floor.</td>
<td>75,000</td>
</tr>
<tr>
<td>2</td>
<td>Removal of 6th floor and 5th floor of north and south towers.</td>
<td>68,500</td>
</tr>
<tr>
<td>3</td>
<td>Removal of 6th floor and 4th plus 5th floors of north and south towers.</td>
<td>62,500</td>
</tr>
<tr>
<td>4</td>
<td>Removal of entire south tower (floors 2-6 inc.).</td>
<td>74,000</td>
</tr>
<tr>
<td>5</td>
<td>Removal of entire north tower (floors 1-6 inc.).</td>
<td>71,000</td>
</tr>
<tr>
<td>6</td>
<td>Removal of 6th floor and south tower.</td>
<td>63,000</td>
</tr>
<tr>
<td>7</td>
<td>Removal of 6th floor and north tower.</td>
<td>60,000</td>
</tr>
<tr>
<td>8</td>
<td>Removal of north and south towers (Should be considered only if center towers can be shifted so that long dimension runs north and south).</td>
<td>52,000</td>
</tr>
<tr>
<td>9</td>
<td>Removal of north and south towers, shifting of other towers, and addition of a 15 ft. length to each of two laboratory sections.</td>
<td>61,000</td>
</tr>
<tr>
<td>10</td>
<td>As 9, but removal of 6th floor from office-conference section at each end.</td>
<td>58,500</td>
</tr>
<tr>
<td>11</td>
<td>Removal of north and south towers, shifting center towers so that long dimension runs north and south, addition of 10' to each center tower, and removal of 6th floor from north and south office-conference areas.</td>
<td>55,500</td>
</tr>
<tr>
<td>12</td>
<td>Removal of 6th floor, all of south tower, 5th floor of north tower, and 5th floor office-conference areas. (allows alternate of adding south tower, 3 floors to give about 66,500 asf, and, in addition, of 5th floor office-conference areas to give about 68,500 asf.</td>
<td>57,000</td>
</tr>
<tr>
<td>13</td>
<td>Removal of floors 5 and 6, and 4th floor of north and south lab towers. Addition of 1,000 asf in first floor and basement.</td>
<td>55,500</td>
</tr>
</tbody>
</table>

Figure 7.1. Cost-cutting alterations to the original four-tower model.
<table>
<thead>
<tr>
<th>Alternative</th>
<th>Funds</th>
<th>Total</th>
<th>Possible Plan</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>State</td>
<td>NSF</td>
<td>Other</td>
</tr>
<tr>
<td>1</td>
<td>4,200,000</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>4,200,000</td>
<td>1,000,000</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>4,200,000</td>
<td>1,000,000</td>
<td>150,000</td>
</tr>
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<td>4</td>
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<td>300,000</td>
</tr>
<tr>
<td>5</td>
<td>4,200,000</td>
<td>1,000,000</td>
<td>890,000</td>
</tr>
<tr>
<td>6</td>
<td>4,200,000</td>
<td>1,000,000</td>
<td>1,400,000</td>
</tr>
<tr>
<td>7</td>
<td>4,200,000</td>
<td>1,000,000</td>
<td>1,800,000</td>
</tr>
</tbody>
</table>

Figure 7.2. Possible funding schemes for the reduced MBI building.
Figure 7.3. Sketch by Paul Boyer of a possible redesign with only four laboratories per floor.
suites to the west (bottom) of the central corridor to be shallower than those to the east (top).

Boyer withdrew the 1968 request, which the NIH had first chopped down to 22,000 sq ft, then approved but not funded. In August 1969 he submitted another independent "Application to U. S. Public Health Service for construction of Life Science Unit 3, Molecular Biology Research Facility, UCLA". This request now was not for 90,000 sq ft of assignable space but for three-quarters of it, or 67,500 sq ft. “Floor plans adapted to a building of reduced size from that previously approved by the USPHS will be sent separately.” The south tower was gone. Selection of this tower for elimination was dictated primarily by the fact that physical connections with Life Science (the aerial walkway and the tunnel) were intrinsically more tenuous than those with Young Hall. The reduced budget for the three-tower building now was 73% of that for four towers:

<table>
<thead>
<tr>
<th>Construction, exterior elements, fixed equipment, fees &amp; misc.</th>
<th>$6,282,000</th>
</tr>
</thead>
<tbody>
<tr>
<td>Movable equipment</td>
<td>1,113,000</td>
</tr>
<tr>
<td>Total</td>
<td>$7,395,000</td>
</tr>
</tbody>
</table>

It was hoped that the state would still finance 45,000 sq ft of space, and that the smaller residual figure of 22,500 sq ft could be covered by federal sources. The new 1969 USPHS application contained a request for $1,500,000 of this.

In Paul’s 1987 talk at the Arrowhead MBI retreat, he describes an interesting alternative to the new grant request:

"Along the way we entered another application to USPHS and withdrew the previous one.....About that time the UCLA administration asked if, in order to get an Institute, would you like to remodel the old Botany Building? The Building Committee backed me up, and our decision was no. “We’ll go for broke.” We wanted and needed something more.”

"This application fared better than the previous one--it was approved with satisfying priority, but construction funds in USPHS had vanished. There was an important change in building plans by this time that is reflected in the building today. Previously, we had a building with four modular components. This was reduced to three, a decrease in 25% of the total building program. That's why there is space now between the Molecular Biology Institute and the Life Sciences Building. I mention this so that if some of you students or faculty acquire unexpected wealth, you can donate money for a nice new wing that might even bear your name."

There's a curious anticipation in that last remark. While I was Director of the MBI in the early 90's, the thought occurred that it would be appropriate to recognize Paul’s massive contributions by altering the name of the institute (and by inference the building) to the "Boyer Institute of Molecular Biology". When I discussed this with Fred Eiserling, then a Dean, he told me, "Dick, they don’t name buildings after faculty; they name them after donors!" This principle remained true
for a time, but was abandoned when Paul shared the Nobel Prize in Chemistry for 1997. The building became "Boyer Hall" soon thereafter.

In MBI Newsletter No. 6 in June 1970, Paul gave a cautious evaluation of the building situation:

"A time of contrasts: One view from the Director’s office shows apprehension and decline. Bond authorizations for health sciences are defeated. The rift between the University and the community grows. Only about 11% of the approved grants recommended to the National Institutes of General Medical Sciences are funded. Both USPHS and NSF stop funding basic science building construction, although total construction in the country reaches an all time high....."

"Largely because of the continued support of our UCLA administration, the beginning of construction of the Biological Sciences Building to house MBI would likely have become a reality in the fall of 1971, if both Propositions 1 and 7 on the recent ballot had passed. There might still remain some prospects of such an enheartening occasion, but our plans must include potential delay....."

"To a considerable extent, our prospects have been kept viable because of the generous offer of Mr. Albert Parvin to commit about $1,000,000 toward our building needs, provided construction was started as scheduled. We are indebted to Dean Sherman Mellinkoff for making Mr. Parvin aware of our plans and needs. Whether we can retain this support is uncertain....."

Paul mentions in passing that UCLA is not alone in feeling a building-fund crunch. Caltech has had private funds for a new Biology Building for some time, but cannot secure the necessary matching federal funds. Harvard is struggling to finance a new molecular biology building without external funds. "Clearly we are not alone....."

The 1970 Newsletter announced that Fred Fox and Philip Thornber would join us as new faculty that fall: Fox as an Associate Professor of MB in Bacteriology and Thornber as Assistant professor of MB in Botanical Sciences. While the MBI waited for the ultimately unsuccessful outcome of the 1969 USPHS grant application, plans were made for occupancy of the building, should it come to pass. Boyer circulated a memo listing the following nineteen MBI members as tentative occupants of the new building:

Barber  Fessler  Nierlich  Schumaker  
Boyer  Fox  Nobel  Thornber  
Brunk  Halpern  Ray  Wettstein  
Eisenberg  Konrad  Romig  Zabin  
Eiserling  Laties  Salser  

*Guidelines are that total space assignments should not include more than about 1000 sq ft for an Assistant, 2000 for an Associate and
3000 for a Professor’s group.....If only the state funded portion of the building is constructed, considerable restrictions below this will likely be necessary. The building design provides different modular blocks of laboratory space of approximately 2000 sq ft on the east side next to offices, and 1500 sq ft on the west side next to offices.”
8. BUILD IT? YES, I CANCER!

In early 1970 the University finally came through with funds for working drawings for the three-tower building, but not for actual construction. With both USPHS and NSF effectively out of the picture, Boyer turned to a new strategy that ultimately proved successful. He had discussed with Dean Mellinkoff of the Medical School the possibility of finding support from private donors, and Mellinkoff placed him in contact with a local millionaire, Albert Parvin. Parvin had made a fortune in real estate and in the design and furnishing of commercial buildings. He was interested both in cancer research and laboratory design, and after discussions offered to donate $1 million toward the new MBI facility, providing that other funds could be found and the project could move forward quickly.

One arm of the USPHS still had a funded building program: the National Cancer Institute. Richard Nixon had declared a "War on Cancer" in his State of the Union address of January 1971. This led to his signing the National Cancer Act on December 23, and created a pool of funds for new cancer centers. NCI would support up to 75% of building fund requests, if the other 25% came from non-federal sources. A large fraction of the research that MBI members carried out was indeed significant for an understanding of the biological aspects of cancer, so it would be quite legitimate to apply for NCI funding for at least part of the building.

Using the Parvin donation as leverage, in November 1971 Paul applied to the NCI for $3,151,875 in funding for "Cancer Facility, Life Sciences Unit 3". A 34,250 sq ft Parvin Cancer Research Laboratory was proposed, which would share the new building with a more general molecular biology facility of similar size to be funded by the State of California. Combined support for the building would be as follows:

Parvin Cancer Research Laboratory:
- Albert Parvin Foundation $1,000,000
- Other private funds 75,125
- National Cancer Institute 3,151,875
  Total: $4,227,000

Additional Molecular Biology Laboratories:
- State of California $5,728,000
  Grand Total: $9,955,000

Paul's arguments were persuasive. To NCI he said: We can produce much with these cancer facilities, and can provide the 25% non-federal component as your ground rules require. To the University and the State he said: We have an opportunity of bringing four and a quarter million dollars to the campus, if and only if, the State can provide a parallel contribution of five and three quarter million. UCLA will end up with a world-class molecular biology center at a cost to the State of only 58 cents on the dollar.
The battle was not quite over. On 17 April 1972, the Director of the Construction Program at NCI wrote Boyer:

"The National Cancer Advisory Board at its March 1972 meeting recommended approval of your construction grant application at a funding level of $3,139,537. We regret, however, that the relative priority for your application in competition for funds available to us precludes funding at the present time. We will keep your application under active consideration until March 1973, should additional funds become available to support your project."

Eight days later, Boyer and Associate Dean Fred Rasmussen from the UCLA medical school were in Bethesda, MD, conferring with NCI staff and again making the case for funding. They were told that the total funds available for building programs in that year's NCI budget were only $11 million. That sum was to be divided among the top five applicants, none of which were from California. The UCLA MBI application was among the second five. NCI had requested an additional $40 million to cover currently approved but unfunded applications, and the matter was then before Congress. Closure finally came on 30 May, when the NCI wrote words that ought to be engraved on a brass plaque and mounted in the corridor:

"The National Cancer Advisory Board, at a special meeting on May 23, 1972 recommended immediate funding of your construction grant application at the requested level from a supplemental budget recently made available to the National Cancer Institute."

The battle was won, thanks to a helpful Medical School Dean, a generous donor, and Director with a keen sense of strategy!

The NCI application contained a detailed roster of current MBI faculty and their recent publications. At this turning point it is interesting to examine that list to see who constituted the pre-MBI building "old guard", and who among this group are still around. Table 8.1 is derived from that roster. "Members" were those with the closest ties to the Institute, and "Associate Members" were more loosely affiliated colleagues, or in some cases, new or junior faculty who in time would move up to Member status. In each of these two categories, I have listed people chronologically according to the year in which they first came to UCLA in a tenure track position. External appointments to the MBI began with Verne Schumaker in 1965, along with a change in appointment style. Emil Smith was "Professor of Biological Chemistry", but Verne Schumaker was "Professor of Molecular Biology in Chemistry". This title, "of Molecular Biology in [X]", was used consistently for all new appointments of Members. The distinction may seem minor today, but at the time it helped to identify the MBI as a research institution in its own right.
### Table 8.1. MBI Membership Roster as of November 1971

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Title</th>
<th>Department</th>
</tr>
</thead>
<tbody>
<tr>
<td>1950</td>
<td>Samuel G. Wildman</td>
<td>Professor</td>
<td>Botany</td>
</tr>
<tr>
<td>1951</td>
<td>Wilfried Mommaerts</td>
<td>Professor</td>
<td>Physiology &amp; Medicine</td>
</tr>
<tr>
<td>1952</td>
<td><strong>Irving Zabin</strong></td>
<td><strong>Professor</strong></td>
<td><strong>Biological Chemistry</strong></td>
</tr>
<tr>
<td>1957</td>
<td>William R. Romig</td>
<td>Professor</td>
<td>Bacteriology</td>
</tr>
<tr>
<td>1958</td>
<td>Isaac M. Harary</td>
<td>Professor</td>
<td>Biophysics &amp; Biol. Chemistry</td>
</tr>
<tr>
<td>1959</td>
<td>Fritiof S. Sjostrand</td>
<td><strong>Professor</strong></td>
<td>Zoology</td>
</tr>
<tr>
<td>1963</td>
<td>Paul D. Boyer</td>
<td><strong>Professor of Chemistry</strong></td>
<td>Biochemistry</td>
</tr>
<tr>
<td>1963</td>
<td>Emil L. Smith</td>
<td><strong>Professor</strong></td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1964</td>
<td>Stephen Zamenhof</td>
<td>Professor</td>
<td>Microbial Genetics</td>
</tr>
<tr>
<td>1965</td>
<td><strong>Verne N. Schumaker</strong></td>
<td><strong>Professor of MB</strong></td>
<td>Chemistry</td>
</tr>
<tr>
<td>1966</td>
<td>John H. Fessler</td>
<td><strong>Professor of MB</strong></td>
<td>Zoology</td>
</tr>
<tr>
<td>1966</td>
<td>Michael W. Konrad</td>
<td>Assistant Prof. of MB</td>
<td>Chemistry</td>
</tr>
<tr>
<td>1966</td>
<td><strong>Dan S. Ray</strong></td>
<td><strong>Associate Prof. of MB</strong></td>
<td>Zoology</td>
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<tr>
<td>1967</td>
<td>Park S. Nobel</td>
<td>Associate Prof. of MB</td>
<td>Botanical Sciences</td>
</tr>
<tr>
<td>1967</td>
<td>Felix O. Wettstein</td>
<td>Associate Prof. of MB</td>
<td>Microbiology &amp; Immunology</td>
</tr>
<tr>
<td>1968</td>
<td>David S. Eisenberg</td>
<td><strong>Associate Prof. of MB</strong></td>
<td>Chemistry</td>
</tr>
<tr>
<td>1968</td>
<td>Winston A. Salser</td>
<td>Associate Prof. of MB</td>
<td>Zoology</td>
</tr>
<tr>
<td>1970</td>
<td>C. Fred Fox</td>
<td><strong>Associate Prof. of MB</strong></td>
<td>Bacteriology</td>
</tr>
<tr>
<td>1970</td>
<td>J. Philip Thornber</td>
<td>Assistant Prof. of MB</td>
<td>Botanical Sciences</td>
</tr>
<tr>
<td>1970</td>
<td>John M. Jordan</td>
<td>Assistant Prof. of MB</td>
<td>Chemistry</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Title</th>
<th>Department</th>
</tr>
</thead>
<tbody>
<tr>
<td>1949</td>
<td>Clara M. Szego</td>
<td>Professor</td>
<td>Zoology</td>
</tr>
<tr>
<td>1952</td>
<td>Daniel E. Atkinson</td>
<td><strong>Professor of Chemistry</strong></td>
<td>Biochemistry</td>
</tr>
<tr>
<td>1955</td>
<td>Thomas W. James</td>
<td>Professor</td>
<td>Zoology</td>
</tr>
<tr>
<td>1958</td>
<td>Albert A. Barber</td>
<td>Professor</td>
<td>Zoology</td>
</tr>
<tr>
<td>1959</td>
<td>George G. Lattes</td>
<td>Professor</td>
<td>Plant Physiology</td>
</tr>
<tr>
<td>1964</td>
<td>Alexander Glazer</td>
<td>Professor</td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1964</td>
<td>Patrice D. Zamenhof</td>
<td>Associate Professor</td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1965</td>
<td>Richard M. Halpern</td>
<td>Associate Professor</td>
<td>Medicine</td>
</tr>
<tr>
<td>1965</td>
<td><strong>Donald P. Nierlich</strong></td>
<td><strong>Associate Professor</strong></td>
<td>Bacteriology</td>
</tr>
<tr>
<td>1966</td>
<td>Frederick A. Eiserling</td>
<td>Assistant Professor</td>
<td>Bacteriology</td>
</tr>
<tr>
<td>1966</td>
<td>Bruce Howard</td>
<td>Assistant Professor</td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1967</td>
<td>Dohn G. Glitz</td>
<td>Associate Professor</td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1968</td>
<td><strong>Clifford F. Brunk</strong></td>
<td><strong>Assistant Professor</strong></td>
<td>Zoology</td>
</tr>
<tr>
<td>1968</td>
<td>George J. Popjak</td>
<td>Professor</td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1968</td>
<td>Larry Simpson</td>
<td>Assistant Professor</td>
<td>Zoology</td>
</tr>
<tr>
<td>1969</td>
<td>Harvey Herschman</td>
<td>Assistant Professor</td>
<td>Biol. Chemistry</td>
</tr>
<tr>
<td>1970</td>
<td>William R. Clark</td>
<td>Assistant Professor</td>
<td>Zoology</td>
</tr>
</tbody>
</table>
Key:
Arr. = Date of first arrival at UCLA in a tenure-track appointment
MB = Molecular Biology
**Bold face** = Still associated with the MBI in July 2008. (E) = Emeritus

This 1971 roster contained 20 Members and 17 Associates. I was amazed to find that of those 37 people, 17 are still around! Appendix 2, "Voices of the Pioneers", has brief recollections by many of these, plus others who joined just before or just after the building was completed.
9. **JUST DO IT!**

Molecular Biology Newsletter No. 8, in June 1972, sounded a justifiable note of triumph. "Much of the present Newsletter will be devoted to the building plans", it stated. Along with a description of the building and the struggle to obtain it were several very clear plans and cross-sections, included here as Figures 9.1–9.5. Figure 9.1 locates the MBI building on south campus, with its two towers to the north next to Chemistry, and the third tower to the south. Note the "ghost" of the missing fourth tower, shown in outline. That shows how close the MBI would have come to the Life Science auditorium, had it been built as originally planned. Figure 9.2 is a plan of the ground floor, and 9.3 is a typical plan of upper, laboratory floors. Note that in each case the outline of the ghost tower is added; its disappearance was still mourned. Figure 9.4 shows several possible connections to Young Hall. Those eventually used were the one drawn between MBI floor 4 and Chemistry floor 5 (here "floor F"), and a second one not drawn between MBI floor 2 and Chemistry floor 3 (here "floor D"). To the bottom right is the sole connection with Life Sciences, a tunnel from the MBI basement to Life Science floor 1 (also the basement). As a path of communication between the two buildings, the tunnel was a flop. But the two walkways to Young Hall truly integrated the two buildings. Figure 9.5 is a west/east cross section that only illustrates the still-inexplicable difference in size between west and east suites.

The overall appearance of this final 3-tower building is shown in the architects' model in Figure 9.6. Compare this with its 4-tower predecessor in Figure 6.2. The style of the outer wall faces has changed, and the building seems more vertical, as architect Harry Shimabukuro intended. The triple arches at the foot of each tower are gone, and emphasis is on one central entrance between towers 2 and 3.

A schedule for progress was outlined in the 1972 Newsletter:

"The architectural firm of Honnold, Reibsamen and Rex is currently proceeding with the working drawings. In late fall, a contract is to be let for the site preparation. We may at that stage have appropriate ground-breaking ceremonies. A little formal recognition of our welcome developments appears appropriate.

"Bids for construction are to be let in late 1972 or early 1973. The usual schedule for a building of this type at the University calls for occupancy about two years later. We hope this schedule can be improved, and would very much like to have the facilities available for the fall of 1974. The winter or spring of 1975 is more likely."
Figure 9.1. Revised plans for a three-tower MB I Laboratory (1)
Figure 9.2. Revised plans for a three-tower MB I Laboratory (2)
Figure 9.3. Revised plans for a three-tower MB I Laboratory (3)
Figure 9.4. Revised plans for a three-tower MB I Laboratory (4)
Figure 9.5. Revised plans for a three-tower MB I Laboratory (5)
Figure 9.6. Los Angeles Times report of building plans, with model.
Figure 9.7. The disastrous financial shortfall.
Planning for the building moved on slowly. In the June 1973 Newsletter No. 9, Paul reported:

"If the University’s principal function were to produce buildings rapidly in response to established funding and dire needs, its status would be critical. At the time of our last annual Newsletter, we fully anticipated that bricks and mortar would be showing by now. The ease with which new financial palaces are completed in Westwood stands in sharp contrast to our progress. But in fairness, our building is more complicated. Delays have been occasioned by revised earthquake standards, and such considerations as modified biohazard facilities and adequate meeting of various environmental clearances. Plans must be checked at various levels, including the University Architect’s Office in Berkeley, various Statewide offices only vaguely known to the MBI Director, the National Cancer Institute, and last but not least, our own Building Committee and Staff. And as recently as last month, the Director learned that a final clearance, by no means guaranteed, was necessary from the State Public Works Board. It was a distinct pleasure to hear final approval granted at the Public Works Board meeting of June 6 in Sacramento.

"Advertisement for bids has been issued. Bids are to be opened July 18. Indications from our architects, Honnold, Reibsamen and Rex, are that there is considerable interest in the building."

In the 1972 Newsletter Paul mentioned that there was nothing to report regarding successful new faculty searches, and attributed this to the stagnation of progress in obtaining the new MBI building. In 1973 he announced with pleasure the appointment of five new Associate Members:

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
<th>Title</th>
<th>Department</th>
</tr>
</thead>
<tbody>
<tr>
<td>1956</td>
<td>Marcel Baluda</td>
<td>Professor</td>
<td>Viral Oncology</td>
</tr>
<tr>
<td>1958</td>
<td>Roberts Smith</td>
<td>Professor of Chemistry</td>
<td>Biochemistry</td>
</tr>
<tr>
<td>1966</td>
<td>John Collier</td>
<td>Associate Professor</td>
<td>Bacteriology</td>
</tr>
<tr>
<td>1968</td>
<td>David Sigman</td>
<td>Associate Professor</td>
<td>Biological Chemistry</td>
</tr>
<tr>
<td>1972</td>
<td>Randolph Wall</td>
<td>Assistant Professor</td>
<td>Micro. &amp; Immunology</td>
</tr>
</tbody>
</table>

Wall had just arrived on campus from Columbia University; the other four were previous UCLA faculty who had elected to join the Institute. (As in Table 8.1, the date before each name marks the first arrival at UCLA on a tenure-track appointment.) Bill Clark and Fred Eiserling moved from Associate Member to Member; Park Nobel from Member to Associate at his own request because his research interests changed.

As noted earlier, the total funds anticipated for the building in the 1971 NCI application were $9,955,000: $4,227,000 from Parvin and NCI, and $5,728,000 to be requested from the State. But when the dust settled in 1973 the actual funds available were only $7,798,000: $4,227,000 from Parvin/NCI, and $3,571,000 from the State. There's a saying that "In this world, no good deed ever goes unpunished", and this certainly seems to apply to the fight for MBI building funds. A call for bids was issued to nine contractors, and six responded. But the lowest bid, from Gust K. Newberg Construction Company, was for $9,741,000, or
$1,943,000 more than the Institute possessed for building. In his 1987 Arrowhead talk Paul referred to 17 July 1973, the day the bids were opened, as "one of the roughest days in my existence." At the bottom of the Bid Summary sheet, Paul wrote, "1,943,000 Over. We’re Dead!" (Figure 9.7)

The low-bid contractor, Gust Newberg Co., was willing to negotiate. But there was a legal problem:

"[Newberg Construction] said that there were ways we could cut the cost of that building and make it less expensive if we could renegotiate the bid. Legally, you are not supposed to renegotiate with a single bidder but must go through the time consuming process of redesign, notifications and rebidding. The Legal Counsel of the University advised against the renegotiation without reopening competitive bidding. We were pleased that Vice-Chancellor William Young and Chancellor Murphy supported a request for renegotiation to the Regents of the University. The Regents listened to the whole case and said they would take the chance of any legal problems; go ahead and having the building redesigned."

Costcutting eliminated items such as one of the three elevators, some of the equipment funds, and waterproofing between floors. (That waterproofing would have been greatly appreciated during the earthquake of 1994, and would have saved FEMA some money!) The State came up with another $700,000 to save the project, and the firm bid was signed in late August of 1973. In Paul’s words, "It was only about 10 days later that the Arabs shut off the oil spigot and we had the first Arab oil crisis. If this had happened before the bid finalization, the contractor would never have put in a bid at that price."
CHAPTER 10. SUCCESS AT LAST!

Boyer wasted no time getting the project under way after the money was in hand and the bid accepted. A ground-breaking ceremony was held on Tuesday, 18 September 1973, at the building site. Chancellor Charles Young, Albert Parvin, Palmer Saunders from the NCI, and Paul Boyer as Director, all had a go at the soil with the "Golden Shovel" (Figures 10.1-2). A group of MBI graduate students then helped Paul lay the "official cornerstone" (Figure 10.3). Representatives of the UCLA Architects & Engineers Office and the architectural firm Honnold, Reisamen & Rex unveiled a model of the building and made brief comments on its design and function. There were short talks by campus administrators, by Saunders and Parvin, two MBI faculty members (Eisenberg and Fox) and two students. Figure 10.4 shows part of the audience; from left to right in the front row are Mrs. and Professor Emil Smith, Albert Parvin, Palmer Saunders and Chancellor Charles Young. After all this the entire audience then adjourned for champagne.

After the September 1973 groundbreaking, things went well until the evening of 6 November. An earthmoving tractor broke a water main in the excavation site and created a fourteen foot deep swimming pool before the water supply could be turned off (Figure 10.5). As the Daily Bruin reported two days later,

"A spokesman for the physical plant said the water shut-off had affected the Med Center, but that no building was without water. He added the building was to be at reduced water pressure yesterday afternoon. He further reported that the physiology and botany buildings were without water for a three to five hour period starting at about 9 pm Tuesday. One effect of the reduced water pressure was that toilet valves of the ninth and tenth floors of the Med Center operated erratically. However, a spokesman for the hospital maintenance department said yesterday morning the hospital was at 50 per cent water volume in the whole complex."

The price of progress!

But the swimming pool was drained, and construction of the steel frame of the building resumed. Figure 10.6, taken in December 1973, shows the now even larger (and dry) excavation site. By May 1974 the foundation had been laid and the building commenced to rise. Figure 10.7 is a view northward with Young Hall visible in the background.

In November 1974, Boyer was host to Mr. and Mrs. Parvin, who came to see what their million dollar gift was accomplishing. Figure 10.8 shows the three in front of the construction site sign, and in Figure 10.9 Paul is pointing out features of the framework of girders. This again is a view toward the north, with Young Hall behind.
Figure 10.1 (see text for identifications)

Figure 10.2.
Water pipeline break fills area to 14 feet

Tractor turns construction site into muddy pool

The construction site for the new Life Science complex was temporarily turned into a somewhat muddy swimming pool, when at 7:30 pm Tuesday night a construction loader machine broke a main water pipeline. The water was not shut off until 9:10 pm and by then the excavation site was 14 feet under water.

Yesterday morning, pumps had lowered the water level to around 10 feet. Workers estimated that about 24 more hours would be needed before all the water would be cleared out. They added that some two days would be needed before work could continue because of the time needed to clear out the mud.

A spokesman for the physical plant said the water shut-off had affected the Med Center, but that no building was without water. He added the building was to be at reduced water pressure yesterday afternoon.

He further reported that the physiology and botany buildings were without water for a three to five hour period starting at about 9 pm Tuesday. One effect of the reduced water pressure was that toilet valves of the ninth and tenth floors of the Med Center operated erratically.

However, a spokesman for the hospital maintenance department said yesterday morning the hospital was at "50 per cent water volume in the whole complex." All departments in the med school were appraised of the situation, because in order to keep the hospital on full pressure, water had to be diverted from the med school.

As a result, air conditioners, ice cube makers and toilets were affected. According to the spokesman, the insufficient water pressure kept the toilets constantly flushing. Any complaints? "Quite a few," the spokesman said ruefully.

Figure 10.5. Frustration Lake
Figure 10.6. Excavation site for the MBI laboratory, December 1973
Figure 10.7. MBI construction from the back, looking north toward Young Hall. May 1974.
Figure 10.8. Paul Boyer with Phyllis and Albert Parvin at the construction site. Nov. 1974
Figure 10.9. Paul Boyer showing construction to Phyllis Parvin, Lyda Boyer and Albert Parvin, Nov. 1974. Looking north with Young Hall behind.
The dream that Paul Boyer, Emil Smith and their colleagues had had in 1964 had finally come to pass. In Newsletter No. 10, September 1974, Boyer reported:

"As of the preparation of this Newsletter, construction cranes and the steel frame of the building rise prominently at the site. Judging from the mass of concrete and steel placed in the foundation and used in the framework, the building should be firm. If at earthquake time most of Los Angeles is leveled, the Molecular Biology Institute building should still stand. The building is now scheduled to be available for occupancy in December of 1975. Whether this date can be held firm in view of delays caused by strikes is at present uncertain, but even early 1976 for occupancy is a fine prospect."

Paul's comment about earthquakes was justified. In 1994 the Northridge earthquake (6.9 on the Richter scale) shook the UCLA campus severely. MBI towers 1 & 2, the central service tower, and the south tower were designed to twist and bend independently rather than being locked together in a larger unit that would have a tendency to crack under stress. You can see the earthquake "hinges" on any floor, just north and south of the elevators; strips of flooring that actually bridge over the space between towers. They worked beautifully in the 1994 quake. But the shearing of one tower against another ruptured power and water lines, and water poured down through floors and ceilings from the 7th floor to the 1st. (Perhaps the deleted waterproofing originally planned between floors might have prevented this.) Major water damage occurred mainly in the east half of the building, and those whose laboratories were on the west side considered themselves fortunate. But after seeing the way that FEMA and university sources repaired the damage, some of this relief turned to envy! The east half ended looking better than it ever had.

Figures 10.10 – 10.12 were taken in January 1975. The first is a view due east from the other side of the South Campus Plaza, roughly where the Nanotechnology Building now stands. The central elevator service tower has its concrete sheath, the girders of tower 2 are at left, and the beginning of the framework of the south tower or tower 3 can be seen above the trees at right. Figure 10.11, taken a little later that month, is a view south down the Plaza, with Young Hall to the left, the girders of the growing MBI building beyond it, and Life Sciences in the background. The south tower framework now is farther along. In Figure 10.12, looking due west toward the back of the building, the service tower is enclosed in concrete while the three laboratory towers still are only frameworks of girders. But by April, in Figure 10.13, the concrete sheathing of all three towers is complete.

The 1975 Newsletter was confident and cheerful:

"With the addition of the vertical glass windows for the laboratories, offices, and conference rooms, a handsome, sculptured building has emerged. But more important, each collision of research personnel in the overcrowded laboratories in Biology, Bacteriology, Chemistry, Biological Chemistry, and Medical Microbiology has been
Figure 10.10. View east across the Bombshelter plaza, January 1975.
Figure 10.11. View south showing Young Hall, the MBI framework, and Life Sciences. January 1975.
Figure 10.12. View of the back of the building, looking west. January 1975.

Figure 10.13. Similar view of the back of the building, as of April 1975.
cushioned by the knowledge that the building will soon be finished. Fortunately, the construction has been proceeding well the last several months, and it appears now that the building will be ready for occupancy during the 1976 spring quarter break...."

Twenty-two current MBI members were scheduled to move into the new building, which would have room for thirty faculty research suites, six per floor on floors 2 – 6.

<table>
<thead>
<tr>
<th>Staff Member</th>
<th>Affiliation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marcel Baluda</td>
<td>Professor Microbiology and Immunology</td>
</tr>
<tr>
<td>Paul Boyer</td>
<td>Professor Chemistry and Director of MBI</td>
</tr>
<tr>
<td>William Clark</td>
<td>Associate Professor Biology</td>
</tr>
<tr>
<td>David Eisenberg</td>
<td>Associate Professor Chemistry</td>
</tr>
<tr>
<td>Frederick Eiserling</td>
<td>Professor Bacteriology</td>
</tr>
<tr>
<td>George Fareed</td>
<td>Associate Professor Microbiology and Immunology</td>
</tr>
<tr>
<td>John Fessler</td>
<td>Professor Biology</td>
</tr>
<tr>
<td>C. Fred Fox</td>
<td>Professor Bacteriology</td>
</tr>
<tr>
<td>Jay Gralla</td>
<td>Assistant Professor Chemistry</td>
</tr>
<tr>
<td>Michael Grunstein</td>
<td>Assistant Professor Biology</td>
</tr>
<tr>
<td>John Jordan</td>
<td>Assistant Professor Chemistry</td>
</tr>
<tr>
<td>Harumi Kasamatsu</td>
<td>Associate Professor Biology</td>
</tr>
<tr>
<td>Thomas Kornberg</td>
<td>Assistant Professor Chemistry</td>
</tr>
<tr>
<td>Judith Lengyel</td>
<td>Assistant Professor Biology</td>
</tr>
<tr>
<td>Dan Ray</td>
<td>Professor Biology</td>
</tr>
<tr>
<td>Emil Reisler</td>
<td>Assistant Professor Chemistry</td>
</tr>
<tr>
<td>Winston Salser</td>
<td>Associate Professor Biology</td>
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<tr>
<td>William Wickner</td>
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Nine new MBI faculty members were to arrive in 1976 or 77: George Fareed, Jay Gralla, Michael Grunstein, Harumi Kasamatsu, Thomas Kornberg, Judith Lengyel, Harold Martinson, Emil Reisler and William Wickner.

On 25 May 1976, Boyer circulated the memo shown in Figure 10.14 that must have represented an enormous sense of achievement and closure. Entitled: "The Building is Ours", it began: "We have finally received authorization to issue keys to staff for the use of the new building." The building was essentially finished. MBI 159 would house its first meeting on 28 May, to talk about the new building. An open house for faculty, staff and students was scheduled for 18 June, and the formal dedication on 5 November.

Staff began occupying the building in June, and by 1 September 24 professors and their research groups had moved in, including six of the nine new faculty mentioned above. (Kasamatsu, Kornberg and Reisler had not yet arrived.) The story from the Santa Monica Outlook of 28 October (Figure 10.15) pictured the completed building. Compare this with the models in Figures 9.6
TO: Members and Associates of the Molecular Biology Institute  
FROM: Paul D. Boyer  
RE: The Building is Ours

We have finally received authorization to issue keys to staff for the use of the new building. Some acoustic floor coverings are still to be installed in the reading and seminar rooms, and some other minor items completed. A few workmen are still around, but essentially the rate limiting step on initiation of building use is now ours.

In order to present and discuss procedures and policies for use of the new building, a meeting will be held Friday noon, May 28th in the main seminar room, new building (we may have to search for chairs). Bag lunches will be in order. Coffee will be on hand.

A general dedication and open house is planned for the fall (tentatively Friday, November 6th). This will be particularly for UCLA staff and local friends and potential friends of the Institute. In the spring of 1977 a symposium is planned for recognition of the launching of the Farvin Cancer Research Laboratories.

But it also seems appropriate that the Institute family take a bit of recognition of our good fortune this spring. Tentatively there is scheduled for Friday, June 18th in late afternoon and early evening, an open house specifically and only for faculty, support staff, and graduate students associated with MBI. The building should be a resource for all Institute members, including those whose main facilities are elsewhere. We could have brief tours of the building, some discussion about how we may best make use of our resource, and a bit of light refreshment and relaxation.

PDB: ssb

Figure 10.14. The Call of Triumph!
New UCLA building houses Molecular Biology Institute.

Biology Institute Dedication Nov. 5

UCLA Scientists To Study Basic Life

The new six-story UCLA Molecular Biology Institute will be dedicated and introduced to the public beginning with tours of the building at 2 p.m. Friday, Nov. 3. The building's laboratories and offices will house scientists studying life at its most fundamental level. Their research holds promise for the prevention or cure of hundreds of diseases in humans, plants and animals, including cancer and the degenerative diseases of aging.

Formal ceremonies will start at 4 p.m. in front of the building, which is located on the east side of the Court of Sciences. UCLA Chancellor Charles E. Young and Prof. Paul D. Boyer, director of the institute, will give brief talks.

The principal address will be given by Dr. DeWitt Stetten Jr., deputy director for Science of the National Institutes of Health, who will discuss "Science vs. Technology, Research vs. Development."

Containing 67,000 square feet of working space, the building was constructed at a cost of $9.3 million. Funds were raised through a $1 million gift from the Albert Parvin Foundation and support from the National Cancer Institute and the University of California Board of Regents.

Basic studies in the biology of cancer will be conducted in the half of the building named the Parvin Cancer Research Laboratories. The other half will be devoted to fundamental studies in molecular biology.

"Whenever we become ill, whether it's a tumor, a headache, or a breakdown in kidney tissue, our sickness is basically on the molecular level," said Dr. Boyer. "By concentrating on this basis, we foresee vital contributions to the entire field of clinical medicine."

Figure 10.15. The Santa Monica Outlook gets it right, 28 Oct. 1976.
Figure 10.16. Moving Day
and 6.2. Figure 10.16 shows our Director moving essential laboratory materials from Young Hall to the new MBI.

The formal dedication of the MBI building occurred on schedule on 5 November 1976. The program is shown in Figure 10.17. A reception was held afterwards, and it was followed later by a dinner for 50 guests. In Figure 10.18 Paul presents an award of merit to the architect of the building, Harry Shimabukuro, and in Figure 10.19 he gives a similar award to Donna Bryan, who helped bring about the building and then spent more than thirty years as Building Manager until her retirement in 2006.

Paul thought that it might be appropriate to christen the new building by breaking a bottle of champagne. If the MBI building were an ocean liner one would smash the bottle against its prow. A good idea in principle, Paul reasoned, but there must be something more appropriate for a molecular biology laboratory than simply swinging the bottle by hand. Something that involved DNA, perhaps. He asked Dave Sigman to come up with a mechanism, and Figures 10.20 and 10.21 display the result. Dave rigged up a bottle at the end of a long cord (Fig. 10.20), and then held it away from the wall of the building by means of a string containing a short segment of DNA. The "trigger" is shown in Figure 10.21. At the appropriate moment, Parvin applied a dose of enzyme to the DNA segment, the segment dissolved, and the champagne bottle swung down, smashed against the side of the building, and officially baptised it. Rumor has it that Dave insured the ceremony against failure by also providing a small amount of acid in case the enzyme failed to work, but this has never been proven. The 29 November 1976 issue of the UCLA Weekly has the "official" account:

**SPEEDY CHEMICAL REACTION OUTWITS PHOTOGS**

"....Albert Parvin, who donated $1 million toward the cost of the building, applied the initial activator drops. The philanthropist did his part so effectively that UCLA Chancellor Charles E. Young barely had a chance to follow with an enzyme solution before the bottle smashed against the wall, leaving photographers with their focuses down."

A particularly attractive photograph of the finished MBI laboratory is shown in Figure 10.22. A decade of thought, planning, persuasion and simple hard work had come to fruition. The Molecular Biology Institute had come into being, and evolved as a research framework for scientific colleagues and a training ground for a new generation of fledgling molecular biologists. When the Molecular Biology Institute building officially became Boyer Hall in 1998 to honor Paul Boyer as a Nobel Laureate, this was only a small recognition of his immense formative influence in bringing about the Institute.

Narratives frequently are concluded with the words "The End". In this case the terminology is inappropriate. This report of the rise of an institution will end with the words:

**THE BEGINNING**
The Director and Faculty of the Molecular Biology Institute cordially invite you to attend the Dedication of the
MOLECULAR BIOLOGY INSTITUTE
Friday, November 5, 1976
Open House - Building Tours 2-4 p.m.
Dedication Ceremonies
Court of Sciences 4-5 p.m.

Welcome to Guests .................. Dr. Paul D. Boyer, Director
Statement from President Saxon
Introduction of Dr. Stetten ............ Dr. Emil L. Smith, Chairman
M.B.I. Advisory Committee
Dedication Address .................. Dr. Dewitt Stetten
Deputy Director for Science
National Institutes of Health

“Science vs. Technology
Research vs. Development”

Presentation of M.B.I. Awards of Merit ....... Dr. Boyer
Molecular Biological Dedication of the Building ....
Mr. Albert Parvin
Chancellor Charles Young

Reception Following the Dedication .... M.B.I. Seminar Room

In case of rain ...... Dedication in room
W.G. Young 2250

Figure 10.17. The Molecular Biology Institute is dedicated at last.
Figure 10.18. Boyer presenting a certificate of merit to the MBI Laboratory architect, Harry Shimabukuro, at the dedication ceremony on 5 Nov. 1976.

Figure 10.19. Presenting a certificate of merit to the MBI Laboratory Manager, Donna Bryan, who took care of the building for the subsequent thirty years.
Figure 10.20. The ceremonial champagne bottle, to be smashed against the Laboratory at the dedication ceremony, with a little help from DNA.
Figure 10.21. Albert Parvin applying a dose of enzyme to the DNA fiber that constrained the champagne bottle. Paul Boyer at right, UCLA Chancellor Young at left. Note the constraining thread running up and to the left at the left side of the photo.
Figure 10.22 The Molecular Biology Institute, complete at last!
### Appendix 1: Faculty Publications Attributed to the MBI by Year

This table is intended only to present a general overall picture regarding faculty acquisition and research activity in the MBI. Data on numbers of papers are taken from annual MBI Newsletters, and regrettably those for 1976 and 1977 did not include lists of current faculty papers. Only those papers are included that appeared in appendices to these MBI Newsletters. Some of the faculty members, Associate Members in particular, have published papers attributed to their home Department rather than to the MBI, but these data are not easily available and have not been included here. The order of listing in this table is based primarily on the publication record rather than dates of arrival on campus or MBI affiliation.

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TOTALS: 20 | 33 | 39 | 64 | 61 | 78 | 66 | 96 | 129 | 121 | 143 | 158 | 189 | 169

HALF CAPS = Full Member in 1973.
*Italic* = Associate Member in 1973. Many of these later became full Members.
Arr MBI = Date of joining the MBI.
Arr UCLA = Date of first arrival at UCLA.
>68 = 1968 or before.

Early Newsletters listed publications from the previous July to current June. Later Newsletters reported the previous calendar year instead. But any given paper is only listed in one Newsletter.
Appendix 2: VOICES OF THE PIONEERS

To this point you have heard about the Institute, its Laboratory, and how all this came about. Now let us turn to the essentials of any world-famous Institute; the people who built it. Several of these pioneers—people who arrived between the founding of the MBI in 1965 and the fully housed Institute in 1978—have provided personal accounts of what it was like in those early days. Specific topics suggested were:

(1) How did you first hear of the MBI at UCLA, and what made you decide to join it rather than going elsewhere or simply remaining in an academic department at UCLA?
(2) What were interactions like within the MBI in the early years before the present building existed, when members were scattered in other buildings across the south campus? Did a true sense of camaraderie develop?
(3) What made you think that being a member of the MBI was intellectually and scientifically better than simply belonging to an academic department?
(4) What do you recall about the long and arduous process of designing, funding and constructing the MBI Laboratory? Did you have any active interaction in its planning and design?
(5) How did things change when you moved into the new building? Or if you did not move, how did life within the Institute change for you after the new building came into existence?
(6) What, in your opinion, has been the net impact of the existence of a Molecular Biology Institute on the UCLA campus? Where do you think the Institute should go from here?

No one person addressed all these topics, of course. But taken together, these accounts paint a vivid picture of the evolution of a vital institution. The individual accounts that follow are listed in the order in which their authors first joined the Molecular Biology Institute. I want to thank all contributors for taking the trouble to record their experiences for the next generation of MBI members.

Paul Boyer, 1963  William Clark, 1970
Emil L. Smith, 1963  Fred Fox, 1970
Verne Schumaker, 1965  Randolph Wall, 1972
Donald Nierlich, 1965  Jay Gralla, 1975
Fred Eiserling, 1966  Michael Grunstein, 1975
John Fessler, 1966  Harumi Kasamatsu, 1975
Dan Ray, 1966  James Lake, 1976
David Eisenberg, 1968  Emil Reisler, 1977
Cliffoed Brunk, 1968  Steven Clarke, 1978
Larry Simpson, 1968  Arnold Berk, 1979
PAUL BOYER, 1963

RECOLLECTIONS ABOUT THE LAUNCHING OF THE MOLECULAR BIOLOGY INSTITUTE
NOVEMBER 2008

This brief contribution will give an account of the past events leading to the establishment of the Molecular Biology Institute, the MBI. Since its establishment some forty years ago, the Institute has served an important role in providing an intellectual and physical environment for research in molecular biology and for recruiting outstanding faculty to UCLA. The most important asset of the Molecular Biology Institute is its faculty. Excellent faculty have made the MBI a fine Institute and contributed to making UCLA a great university. From my varied academic experience I have learned that to attain a productive research and teaching academic environment you must have highly qualified people. The best research entity devised is a professor and his research group of students and postdocs. They achieve greater productivity working, not alone, but in interaction with others. The MBI has achieved an association of exceptionally qualified people in an environment favoring open discussion and interaction. Cooperation with, rather than competition among, staff characterizes the academic environment at UCLA.

Although I ceased administrative responsibilities some twenty years ago, it has been my privilege to note the continued service and function of the Institute. The MBI has permeated the campus, a function promoted by the physical proximity of departments and faculty. The Interdepartmental PhD Program in Molecular Biology is a resounding success. The building associated with the MBI (now Boyer Hall) continues to provide a central location for Institute functions.

I’ll digress from local considerations to indulge in a bit of history that might give some better perspective about how academic institutions develop. When I arrived as a graduate student at the University of Wisconsin, many years before most of you were born, there was a new building just finished. I was one of the first to work in a new, well-designed laboratory, and I didn’t give a thought as to who planned that building, why it was there, what events set the stage for the expansion, and what had made the Department of Biochemistry at Wisconsin great at that time. I just took it for granted that there would be interesting seminars and conferences down the hall from my laboratory, where people like Fritz Lipmann and Severo Ochoa and other great biochemists of that time would speak. I now look back in retrospect with a feeling of respect and thanks for the people and the university there.

You may be interested in how the Molecular Biology Institute became an entity and some of the precursor rumblings along the way. The first breath of life for the MBI came nearly a half-century ago. It actually involved a professor of Anatomy whom I never met, John Green. He organized a small ad hoc committee, first in the medical School, and then branching into the College of Letters and Sciences. Professor Green wrote to the then Chancellor of the University, saying "For some months an informal group, originating as an ad hoc
committee in the Medical Center, but quickly expanding campus-wide, has been discussing the possibility of coordinating teaching and research in cellular biology." The letter continued with a proposal for a Laboratory or Institute of Molecular and Cellular Biology.

The next official item that I know of about the Institute came as a report from a formal committee headed by professor Wilfried Mommaerts of the Department of Physiology in the Medical School, dated February 1962. Their report gave a strong recommendation that an Institute of Molecular Biology be established at UCLA.

About these events, I knew nothing at the time. Indeed, at about that time I was making another trip to UCLA to see if I should move from Minnesota to Los Angeles. UCLA obtained two new biochemical faculty in the spring of 1963. Emil Smith, who had for a long time been at the University of Utah, joined as Chairman of the Biological Chemistry Department in the Medical School. I joined the Chemistry Department in the College of Letters and Sciences, in conjunction with the organization of an official Division of Biochemistry within the Department. We were both surprised, several months after we arrived, to learn that there were plans for a Molecular Biology Institute and a building. This had been approved by Chancellor Franklin Murphy as part of a rapid expansion of UCLA that was underway. The campus was in a rapid growth phase—buildings were coming up like mushroom all around the place. In fact when I came in 1963 I moved into a new wing of the Chemistry building, now Young Hall.

It looked like there was no problem at all with a building program. A building was promised for the Molecular Biology Institute, so we tried to recruit a capable outside scientist to be its Director. We saw it as an opportunity to meet a need for UCLA, but were unsuccessful in recruiting a Director when we tried to show the space that would initially be available for the new Institute; it amounted then to only a few rooms in the basement of the Life Science Building. I need not tell those acquainted with the basement of the Life Science Building that it doesn’t sell very well. It soon became evident that there might be some problems in attracting a Director and getting the Institute under way. I recall complaining a bit about the need for better staffing at UCLA in Molecular Biology. In one of these many meetings about the Institute, or somehow or other while I was making noises about unmet opportunities, everyone else somehow decided that actions would be appreciated more than words, and that maybe I should (somewhat by default) be Director of the Institute.

It was not until 1965, when my research was not progressing as well as I had hoped, that I decided that perhaps I could be more helpful to my profession by attempting to build an Institute. The first thing was to find if there was enough interest in a graduate training program in Molecular Biology, and if most of our best faculty in this area would want to participate. If not, it wasn’t worth doing. Thus my first tenuous exploration was to assess the support for a Ph.D. program. The response was gratifying, and an interdepartmental program was formulated and approved (the administrative requirements are by no means simple).
Unfortunately, prospects for a building for the Institute became disappointing. Construction grant requests to the NSF and USPHS were launched. Although approved with good priority after site visits, potential federal funds remained small and our requests remained unfunded. At this stage through an initiative by Deans Sherman Mellinkoff and Fred Rasmussen of the Medical School, I was introduced to Albert Parvin as a potential donor of a million dollars toward a building. As part of a “war on cancer” promoted by President Nixon, some potential federal support for buildings became available, with the requirement that at least one-fourth of the cost be provided by the recipient. Our request for federal funds to allow a four million dollar building was approved. But we deemed that this would be quite inadequate for the Institute program, and informed Vice Chancellor William Young, who was in charge of the UCLA building programs, of our problem. Chancellor Young and I made presentations of our needs to legislators in Sacramento. We essentially told the State that we would lose the four million dollars from Parvin and the National Cancer Institute if the State did not provide matching funds that would allow the project to go forward. It is fortunate that the State provided the needed additional four million.

One of the happiest memos of my career was the notice on 25 May 1976 to Members and Associates of the MBI, announcing that the long-sought building was finished and keys were ready for issuing. Only about thirty research groups could be accommodated within the new structure, but the building readily became a resource and a rallying point for all of our members, whether housed there or within their home academic Departments. Activities in molecular biology have permeated departments throughout the campus. The MBI continues to serve the information and intellectual needs of people from all over south campus who share an interest in understanding life at the molecular level.

**EMIL L. SMITH, 1963**
16 October 2008 interview, at age 97!

*What was your connection with Paul before you came to UCLA? You and Paul were both recruited by Sherman Mellinkoff. You mentioned before that Paul and Lyda traveled to Salt Lake City to confer with you. Can you give me some background?*

Paul and I had known each other for years. We had met at the Biochemistry Society. I was on the Board of Editors, or whatever they were, of the Annual Review of Biochemistry. And Paul was invited at that time, to attend the meeting as a contributor. He had worked with Murray Luck, who was the founder. Then I got a call one day (I was at Salt Lake): Would I mind rooming with Paul because rooms were very short? So I said, of course not. This must have been around 1949, 50, 51. I was an early member of the Annual Review. We became very friendly, you know; we lived together for three days. We had a lot of frank talks about things of interest. And over the years thereafter I would see Paul from time to time. We talked and always were very good friends.

In the early days of good biochemistry there was no great rivalry, at least in my view. I recognized no rivals. I took the attitude that if anyone wanted
any of my ideas, my views, they were welcome to them; I had plenty I couldn't work on. It was arrogant, but it was a good view; and I made many more friends than enemies that way.

On July 2, 1962 I was staying in Los Angeles in a motel near my parents. We were on our way back to Salt Lake City from a family wedding in San Diego. I received a telephone call at the motel. The secretary got on the line and said "Dr. Mellinkoff is the new Dean of the Medical School at UCLA and would like to talk to you." He came on and said the Chairmanship of Biochemistry is vacant (it was Physiological Chemistry at that time). Would I be interested? I said well I'll come and see you tomorrow, because I'm leaving the following day. His response was a startled "What?" His Secretary had called Salt Lake and found out that I was in Los Angeles, but forgot to tell Mellinkoff that I was here, which was an interesting mixup.

I came out to UCLA the following morning, and we spent all morning talking. He showed me around, but it was quite obvious that I was not interested because the space was totally inadequate. I told him that I was interested in the job, but that I was not ready to give up research and become a Chairman. For my research I wanted at least as much space as I had at the present time, which was for ten technicians, graduate students and fellows (which I stuck with, by the way, for all the rest of my life). So he said, "Give me a month or two and I'll see what I can do. This is my second day as Dean; I became Dean the day before". So I had to talk about it. John Pierce was a friend of mine, from Cambridge actually. He was a postdoc there when I went back there the first time after the war. He drove me back; he was interested; he was Acting Chairman at the time.

A day or two after I got back to Salt Lake, I had a telephone call from Paul, saying that he had just learned that I had been offered the Chairmanship in the Medical School, and that he had been offered a position of leadership in the Division of Biochemistry in the Chemistry Department. Could he come and talk to me? I said "Of course; bring your wife and have a barbecue dinner with us, and we'll talk." So they came to Salt Lake City. It was the first time we'd met Lyda, and the wives got along famously. We talked about the situation at UCLA, and I told him there were a number of things I didn't like.

First of all, there were two independent graduate programs, which was absolutely ridiculous as far as I was concerned. Apparently they had had some antagonisms before. What they were, I don't know and I don't care. I suspect that I know, but I really don't care.

And second of all, the space was inadequate. Until they could get the space straightened out, I was uninterested, even though I would like to move to Los Angeles, as my parents were still alive, and my only brother lived there. So, about three or four weeks later Mellinkoff called me to say that he thinks he has the space, and "Can you come back in early September and look at it, and we'll talk about it?" Some time around late August or early September, my wife and I went back. I found that Irv Zabin was giving up some space, John Pierce was giving up some space, and Fred Rasmussen in Bacteriology was willing to give
up a lab, for the time being, because at the moment they were planning to add four more stories to the Medical School.

Do you know the history of that? Originally they had planned a seven-story Medical School and hospital. The State did not come across with enough money. So the then-Dean Stafford Warren figured out that he had better hold onto the land, because if we didn't there'd be no room for later expansion. So they built a hospital and Medical School of three stories, but with foundations that would hold seven. They would add four more floors when the money became available.

They were hoping to add these floors a year or two years after I was due to come in 1962. So their plans were that Biochemistry would be assigned all of the third floor, which it had been sharing at that time with Bacteriology. Well, Microbiology, as they called it. They made a distinction between the Department of Bacteriology and the Medical School Department of Microbiology. They duplicated campus departments, in other words, which led to problems. Fred was the hero because he gave up his lab, which we could use for both the Tiselius electrophoresis apparatus and the ultracentrifuge. I needed both, at that time. There were no others throughout the campus. I said that I would share it with the whole Department if they wanted to, and they could use my technician who ran both of those. And that was the agreement.

At this same time Paul and I got hold of the then-Chairman of the Chemistry Department, and we talked about the duplication of graduate programs, and he saw no reason why this existed. The people who were in in Biochemistry, agreed. So eventually, when we both arrived, we set up a joint graduate program in Biochemistry, using a one-year Introduction to Biochemistry that they could take either in the Chemistry Department or in the Medical School.

Were there course duplications also?

There were duplications all the way down the line. So we now planned a one-year curriculum which was mandatory for all graduate students, using the best people in each field, which would vary from time to time, obviously, as the field changed. Paul and I pretty much decided this, and got the agreement of everybody else. We also got the agreement to change the name of our Department from Physiological Chemistry to Biological Chemistry, as in JBC, which was the Journal of Biological Chemistry. I was not in agreement that we were a subgroup of the Physiological Department. Long ago Biochemistry had become an independent discipline, where people learnt about things other than mammalian physiology. And that was agreed to.

In 1963 we both came. And both families immediately became very close friends. Esther and Lyda used to belong to the same tennis group.

So this was just before the Molecular Biology Institute effort began?
I gather there was a committee on Molecular Biology set up in 1961 that had involved various people in various departments, but I knew nothing of its existence when I arrived. I was never told it had been proposed; the campus was functioning independently of the Medical School, in effect. As with Bacteriology, for example.

Paul has also said that he was not aware of plans for this, so it must have been buried in the system somewhere.

It did exist, on paper, and in somebody’s eyes, but whose eyes, and who planned it, and who created the committee, I haven't the faintest idea. Fritiof Sjostrand was one of the people on that Committee, I later learned. Another person on that Committee was Willy Mommaerts. Now Willy had started out in Biochemistry, but switched to Physiology; I think because he was overwhelmed by what was happening in Biochemistry, and wasn't prepared. He switched to Physiology, although it was worthless quasi-biochemical. And he was the acting Chairman of Physiology at that time. He succeeded somebody who retired later.

So that's the beginning of it, in a sense, with Paul and me.

How did the two of you become aware of the plans, or the concept, of an Institute?

I first became aware of it, I think, through the then Vice-Chancellor, Foster Sherwood. And I was told that they were going to have some candidates for Director of Molecular Biology to come out and look at the situation. As far as I know the first one to be invited was Al Lehninger. Lehninger wasn’t really interested; it was just an ego trip. And Paul was delighted that he wasn’t interested. I knew Lehninger, but I was not particularly fond of him, by the way. So I was also delighted that he wasn’t interested. Later they invited Phil Handler, who was a close friend of mine, and a co-author on my book (White, Handler and Smith).

Phil came out to look us over, but he had two problems: One, he was very loyal to Duke. A second problem: He was already very much mixed up in national politics. Every time they gave science testimony on the Hill, Phil Handler was the guy who spoke for science. The Chairman of the subcommittee in the House was Emilio Daddario, who became a very close friend of Phil’s, and a friend of mine. The Chairman of the Health Committee in the House was first Jim Fogarty, and then it became Ted Kennedy. Phil used to write speeches for Ted Kennedy all the time on health. (All anonymous, of course.) And the third problem was that Phil’s wife had multiple sclerosis. Phil decided that it would be too hard for her to come out here and make all new friends, when what she was living with in Duke was a problem of her own, but she had a lot of correspondence with paid caregivers, and kept it up because she couldn’t do very much. So Phil turned it down. I gather he had a long interview with Foster Sherwood and the Chancellor. And the upshot of it was, Phil told me later, that he said: "Why the hell are you looking for anybody from the outside, when you have two perfectly good people here, who would be both extremely qualified to be Head of Molecular Biology? Specifically, Emil Smith, who has a background in Genetics as well as in Biochemistry, and Paul Boyer. Foster asked me whether
I was interested, and I said "No". I had too much to do in the Medical School, and I was too busy getting the Department settled, and revising all the teaching, and discouraging the people who couldn't teach from doing any teaching.

The real break came when they then approached Paul Boyer to be the Head of MBI. Paul decided to take it, so they dissolved the search committee. Paul created an advisory committee for the MBI, and he asked me to be Chairman of it. Then Paul and I served on a committee to reorganize the Biology Departments. We recommended, with the rest of the committee, that they unify Botany, Zoology and Bacteriology. There was no sense in having three separate Departments, since each was offering Genetics, each was offering Physiology; there was no difference. Bacteriology insisted on remaining separate, at least for a time. Whether they are still separate or not, I don't know. But Botany did merge with Zoology.

The new Biology Department was given a number of FTE's, with the agreement that a fair number of those FTE's were to be allotted to the MBI. How many of them, I don't know. New appointments to the Biochemistry Division also were going to be allocated to the MBI. Paul and I had an informal agreement about the Medical School, and we would see what came up. The problem was that there was no sense being Head of the MBI unless there were going to be appointments through the MBI. And most of them came through Biology.

Later on, for example, when the Institute existed, and Paul was trying to recruit Bill Wickner, Bill was an M.D. and wanted to be affiliated with the Medical School. Paul and I talked about it and we said, "Of course." And so his FTE came out of the College, as far as I know, but his appointment was in the Medical School, and he did teach in the Medical School course. He wanted to keep his hand in the Medical School. It was a shame that he left, but he wanted a more rural place to live: Dartmouth. He became Chairman at Dartmouth and is still there.

When you were Chair of Biological Chemistry and Paul was Head of the MBI, to what extent did you and he get together to plan recruitment as a unit? Did you actually plan recruitment together, or was it an independent process?

It was pretty much independent.

But most of the people whom you would hire would want to affiliate with the MBI, would they not?

I didn't have many new appointments after, well, let me say this: Space was very limited. We had certain things in view that I wanted to do when I came. I wanted more chemistry in our Department. I didn't want old-fashioned Physiology. I recruited Dave Sigman, for example, by simply getting on the telephone and calling Frank Westheimer. I said I wanted a good bioorganic chemist, who was interested in enzymes, and Frank said, "I have just the man for you; he's now on a postdoc with Elkin Blout. Talk with him." So I called Elkin
and I called Sigman, and he wanted to finish his postdoc with Elkin and his wife wanted to get her Ph.D. in Psychology. "O.K., I'll wait for you."

Alex Glaser was the only graduate student of my own I ever hired back. He went to Berkeley. He wanted to escape because he felt that if he stayed here and I retired he was going to be Chairman, and he didn't want to be Chairman in the Medical School because his interests had changed completely. He is now working completely on bluegreen algae, and didn't feel it was a fit subject for the Medical School.

**VERNE SCHUMAKER, 1965**

I arrived here in 1965 after being recruited by Professor Paul Boyer. His plans for the Molecular Biology Institute were very exciting. What was it like? Well, it was a very busy time for all of us, of course. I was the Associate Director, and that meant, first, being supportive of Paul Boyer and being involved in his ambitious plans for the Institute, and second, getting my research underway and at the same time trying to fit in smoothly with the Chemistry Department, when it was still called "Chemistry", and third, watching and helping to guide the growth of the graduate program.

Paul quickly recruited a basic staff: John Fessler, Zoology; Felix Wettstein, Medical Microbiology and Immunology; Dan Ray, Zoology; Bruce Howard, Biological Chemistry; Michael Konrad, Chemistry; Park Nobel, Botany and Plant Biochemistry. And Life Science Unit 3 (i.e. the MBI building) was now listed as item #7 on the building plan for 1967-1968. But the war in Vietnam was expensive, and a series of delays stalled the ground breaking for almost eight years.

Meanwhile, Winston Salser and David Eisenberg were brought to UCLA. I was especially pleased with David, who was a bright young crystallographer from Cal Tech where he worked with Richard Dickerson. David and I jointly organized a course, "Physical Chemistry of Biological Macromolecules", which we taught together for a number of years starting in 1969. It still is my favorite course, and as a matter of fact I will teach it in the coming Spring, 2009, forty years later.

We were filled with high hopes for the Institute, and we recruited well. But the Building didn't get finished until 1976, a delay of over 10 years if you started counting in 1965. Still, the MBI was a force on campus with its focus on excellence, the recruiting of the best scientists and teachers we could find, and a graduate program, started in the Fall of 1966, which provided a steady stream of students and yielded a list of achievements, that is, scientific papers in Molecular Biology.

Let me list the titles of the first three MBI dissertations, which appeared in 1970:
By 1976, the year of opening of the MBI Laboratory, we had 30 dissertation titles, which we felt constituted a very productive graduate program.

Efforts to fund the building were privately recognized by the donation of a million dollars by Alfred Parvin, which gave us the total of 10 million when you counted contributions from the state and federal governments. Ground breaking was in September 1973. Two months later, construction equipment broke a water main and the excavation filled with water. But this was soon repaired and construction proceeded smoothly. Two and a half years later the MBI was completed, and in November 1976 a formal dedication was provided. The MBI had arrived!

An amazing series of faculty were recruited. Let me list some of the names: Arnie Berk, James Bowie, Bill Clark, Steve Clarke, Robert Clubb, Richard Dickerson, David Eisenberg, Fred Eiserling, Juli Feigon, John and Lisa Fessler, Fred Fox, Jay Gralla, Michael Grunstein, James Lake, Jeffrey Miller, Sherie Morrison, Emil Reisler, Winston Salser, Verne Schumaker, Randy Wall, Todd Yeates. There are some I have forgotten, and some famous ones have moved away, for example Bill Wickner and Bill Clark. But it has been a splendid place to work.

In 1983 Paul Boyer retired after leading the MBI for eighteen productive years, to be succeeded by Richard Dickerson, who had been recruited to UCLA and the MBI two years earlier. Dickerson, in turn, was succeeded in 1994 by Arnie Berk, our third Director. In 2001 Steve Clarke became our fourth Director, in collaboration with Sabeeha Merchant as Vice Director.

But Paul had one more important contribution to make. In 1997 he received the Nobel Prize in Chemistry for his elegant studies elucidating the generation of ATP by the molecular rotator, mitochondrial ATP synthase. This Nobel Prize recognized the culmination of a vast amount of work by many scientists from the world over, and in part coincided with the formation of the Molecular Biology Institute.

So I think we had accomplished a great deal: We built and staffed the MBI with an excellent faculty who were productive scientists, we had a first class graduate program up and running, and we had become a world leader in research with a Nobel Prize. Paul Boyer's dream had become reality!
It was fall 1965, and we sat at a patio lunch table near where the MBI building one day would be, but then was still just a plan. There was Paul Boyer, explaining the virtues of forming the Institute: interaction of colleagues with interests in diverse subjects, but sharing common tools, thinking and goals; access to graduate students who were well prepared in chemistry, physics and problem solving; and mentioned then or not – a collegial group. Besides me, there were two or three others, members of the then Biochemistry Division of Chemistry.

I believe that we had all enquired about joining the Institute, and Paul was pitching it in his low-key but concise way. “What is a molecular biologist”? I asked. “It’s someone practicing biochemistry without a license.”* I attribute the quip to Dan Atkinson or Charlie West, not that I remember so clearly. “Am I a molecular biologist – I think of myself more as a bacterial physiologist?” I said. Paul assured me that I was a molecular biologist.

In January, I wrote to Paul asking for membership and attaching a brief description of my research. Shortly thereafter, I received a letter of invitation. “I am pleased to inform you that your application has received unanimous approval of the Members, and to welcome you as an Associate.” I believe the Associate was acknowledgement that I had not been recruited to UCLA by the Institute, but maybe it was a trial of sorts.

To explain why I questioned whether I was a molecular biologist is hard. I came to UCLA and the Department of Bacteriology in January of 1965; my recruitment was the end of chance events. Prior to that, I benefited by training

[*Editor’s Note: The brilliant but often bitter biochemist Erwin Chargaff, who in a sense refused to take the steps necessary to solve the structure of DNA, published Essays on Nucleic Acids in 1963. Chapter 11, entitled Amphisbaena, is a dialogue between "Old Chemist" and "Young Molecular Biologist". These are not identified, but their context makes it clear that they are Erwin Chargaff and James Watson respectively. The style is modeled after the discussion between Sagredo and Simplicio (Wise One and Simpleton) in Galileo’s 1632 Dialogue Concerning the Two Chief World Systems. In Chargaff’s debate, Old Chemist proclaims that "Molecular Biology is essentially the practice of biochemistry without a license." The Amphisbaena of the title is a two-headed monster in Greek mythology, a serpent with a head at each end. One wonders whether Chargaff meant his two-headed monster to represent the two strands of DNA, or Watson & Crick, or perhaps Chargaff vs. Watson. Chargaff used this "without a license" remark again on other occasions, including a letter to the journal Science. He surely was its original author; it embodies his particular style of intellectual sarcasm. A little farther on in the same dialogue there occurs the following exchange:

   OC: May I ask whether you mean that this beautiful scheme applies to meiosis as well as mitosis?
   YMB: I am not interested in diploids.
   OC: I wish your parents had felt the same way.

They just don’t make scientific repartee like they used to! —RED]
with many of the now iconic leaders of molecular biology. I had been well advised and very lucky, going from Santa Monica High School, to Caltech, to Harvard, to MIT, and then to Paris – and then to return.

At that time the community of “molecular biologists” was small, a few hundred people working on the most diverse systems, and in different academic departments. And while we junior researchers all thought at the time we knew where we were going, prior to my conversation with Paul, I didn’t quite have a name for it: biology, biochemistry, bacteriology, biophysics, biologie physico-chimie? But there is vision and a solidification that occurs in having the right name – even though different meanings of the term, molecular biology, are still held by different people. Most remarkably, in these 42 years since I became a member of the Institute, the excitement doesn’t seem to have diminished, with signaling, genetic and genomic networks and their molecular bases being much of the focus.

My life at UCLA was greatly enhanced by MBI membership. I have had wonderful interactions with its faculty, both personal and professional, including some joint publications. In many instances, members helped me or my students with procedures or materials. I recall particularly a week that I spent in Paul’s cold room, and made, with his technicians help, our first batch of bovine mitochondria. Such generosity was a not unusual. Four of the graduate students that I trained (Toivonen, Beiger, Davis and Okono) were in the MBI program. The weekly faculty seminars provided on-going stimulation, and from time to time, a wonderful opportunity for a critique of my own work.

And I enjoyed contributing to the joint effort. While the MBI building was going up, with Winston Salser, John Fessler, and Donna Bryan, I served on the committee to furnish and equip it. Once completed, I looked after the reading room and journal collection for a number of years.

And the building was a great asset. I taught a graduate nucleic acids course in its teaching-lab space with Larry Simpson in the late ‘70s and ‘80s. [I believe we were the first (probably in 1980) anywhere to teach a formal course that included DNA sequencing, which I learned on sabbatical leave in 1979 in Fred Sanger’s lab in Cambridge.] I used common facilities such as the fermentor, and later obtained nucleic acid primers and cell-culture medium from its support staff. And I enjoyed parties there and many an extended chat.

I’ll never know how it might have been if I had moved into the new building. I was once earmarked to go, but at the time the building was completed, I had moved into newly remodeled labs in the Life Sciences building, and it didn’t make sense to move again. But the Institute was much more than a building.
I joined the UCLA faculty in 1965, after doing both BA and PhD at UCLA. After my postdoc in Geneva in biophysics, and having spent time in Paris, Cambridge and Caltech in the early 60’s, I realized that UCLA was at that time a fairly mediocre player in the emerging fields of molecular, cell and developmental biology. Important changes were on the way when Paul Boyer arrived, and Sydney Rittenberg became chair of Microbiology. The most important were the new faculty hires and the concept of an interdepartmental institute and a doctoral program in molecular biology, and the administrative agreement to provide the Institute with faculty tenure-track appointments (FTE), to be made jointly with a teaching department. Although something similar had existed in neuroscience and the Brain Research Institute, this model had a major effect in transforming UCLA into a contender in modern biology. It’s important to note that dean Harlan Lewis was an important player in helping to facilitate these new faculty hires, as was his successor, dean Dennis O’Connor. They were the ones who worked with the MBI search committees and department chairs to make the excellent new faculty appointments happen.

Another major event in transforming the biological sciences at UCLA was the change at the NIH from departmentally-based PhD training grants to interdepartmental area training grants. This caused a huge upheaval and required collaboration across units many of which were unaware of each others existence in the College and the School of Medicine. That event required new alliances to be forged in both teaching and research. I recall that Paul Boyer asked me to be an Associate Director, and then informed me that I should be the first Program Director of the nascent Cell and Molecular Biology Training grant. If I had only known what a difficult and somewhat thankless task that was, I think I would have gratefully declined, but we managed to keep it going for 15 years before it was turned over to Steve Clarke.

The new building, now known as Boyer Hall, was another major factor in providing excellent physical facilities for interdepartmental research and training. David Eisenberg and I planned to occupy adjacent laboratories where we could begin a joint structural biology group that would use X-ray diffraction and electron microscopy to investigate molecular and cellular functions at the nanoscale level.

We also learned not to over-customize the new laboratories through a now amusing mistake in instrumentation. I had learned from Michael Reedy, a new faculty member in the Physiology Department interested in muscle structure, that we needed a very solid support for an optical diffractometer to create analog Fourier transforms of electron micrographs. The solution we agreed on was to have a crane deposit a 12-foot long, 8-inch diameter steel pipe weighing over 300 pounds inserted into a room that was still under construction. Several years later, as computer technology replaced this gargantuan instrument, Jim Lake was greatly relieved when the thing was cut up into pieces and removed from his lab.
The structure group became very successful, and with David Eisenberg and Richard Dickerson providing leadership, along with a number of wonderful new faculty recruitments, has been a mainstay of the concept of interdepartmental cooperation that serves UCLA well today.

JOHN FESSLER, 1966

Thank you, Dick, for all the work that you have done in laying a foundation for the history of MBI and its artwork. Thank you also for requesting a thumbnail sketch of why and how each of us came to join this enterprise.

I think that I told you that ever since high school days when I heard Astbury talk about “alpha” and “beta” folds of fibrous proteins, on a dreary, wet evening in war-time Yorkshire, I wanted to combine the views of chemistry and medicine to biological problems. Taking Astbury’s advice, “just follow your nose lad”, I came via Oxford, Massachusetts General Hospital, the British Medical Research Council and Caltech to Westwood, where I was impressed by Paul Boyer’s outline of the immediate future for MBI at UCLA. He kindly invited me for supper and upon entering his home I faced what I took to be an excellent picture by the nineteenth century French social commentator, Daumier. I knew there and then that this organized man was not only a fine biochemist but also a sound individual with a sense of beauty and social responsibility. I could trust and respect him, and I have done so ever since.

Like others, I was attracted by the prospect of helping to build up a new research organization. I share your interest in the MBI building, but that was a distant mirage in the future when I came. For balance, I concentrate here more on the human side of the development. What mattered was the enthusiasm with which good minds discussed not only the latest scientific news, but also how they impinged on the then current and future social problems. The nascent Interdepartmental Ph.D. Program in Molecular Biology was loosely designed to accommodate a rainbow of lively graduate students from diverse backgrounds. We had several informal evenings with outstanding visitors to discuss, with students and faculty together, some of the social implications of new scientific approaches. Linus Pauling said that the idea of doing something about the harmful side effects of nuclear bomb tests was not his but his wife’s, and he just thought it was important enough to do something about it. Sinsheimer worried about the potential dangers of using bacterial viruses for recombinant studies and became a key participant in the Asilomar Meeting on that topic, which I regard an outstanding example of scientific social responsibility. Bob Edgar talked about serious considerations of modern educational changes at the university level, then took up a lead position at the then starting UC campus at Santa Cruz. Way before the “Green Revolution” we had a bright incoming MBI student who rotated through our lab, liked our research, but insisted that her research training would have to prepare her for doing something about the degradation of the Owens Valley.

Bright independent people do not always agree and there was keen competition at times, but communal interest supervened. Lisa Fessler had the idea that molecular biology was now important enough to be taught at the
undergraduate level. So Dan Ray, Winston Salser, Cliff Brunk and I started a new course, Introduction to Molecular Biology, Biol 144, which emphasized asking questions and finding answers through experiments. It was fun and lively, with just the original literature as no textbook existed. Subsequently it calcified into a formal core requirement. For the MBI faculty Paul Boyer initiated a weekly noon seminar in a Young Hall conference room where we learned about the endeavors of our colleagues, sometimes with critical criticism rather than acclaim. Marcel Baluda had incomplete evidence for reverse transcription from RNA into DNA, but it was neither good enough for nucleic acid biochemists nor seemed to have much point for the lac operon/ bacteriophage enthusiasts. Our brave new world was partly limited by its own dogmas. But the insistence on reliable and unquestionable results was and is essential for what we are.

The MBI was not just built out of concrete and steel, it’s the human beings for whom it is a part of their transient existence in the shifting space of science that keep making and rebuilding it. First it was just an idea, then Lisa built it as a model out of gingerbread for a Christmas when Santa did not come through with funding. Eventually it sparkled forth in fun multi-colors, but Donna Brian sobered it to a respectable, bland white. We trust that the future will be more imaginative than just turning out the lights, automatically, in Boyer Hall.

DAN RAY, 1966

John Fessler, Mike Konrad and I arrived on campus in 1966 as new MBI recruits targeted for space in the new building. Verne Schumaker, who had been recruited to the Chemistry Department the previous year, would also be joining us. At the time I was being recruited I was excited about the scientific opportunity at UCLA, the development of an interdepartmental Institute devoted to molecular biology, and the prospect of having space in a new building. Visiting from Munich Germany in mid-winter no doubt also had an impact on my decision to come to UCLA. A 1A draft status while the Vietnam war was going on also impacted my decision to cut short my postdoctoral time in the Max Planck Institute for Biochemistry. (At that time university faculty were exempt from the draft.) As soon as the offer from UCLA was firm I quickly put together an RO1 application to NIH to continue my work on single stranded phage DNA replication. Fortunately, the times were good then and I was able to get my research program under way soon after arriving at UCLA. I was joined within the first year by one of the first MBI graduate students, Arleen Forsheit, an outstanding postdoc, Bertold Franke, and a most incredible undergraduate, Randy Schekman. When Randy left my lab he had two JMB papers and a Nature paper. Also between his junior and senior years he spent a summer at Harvard with David Denhardt and got a publication from that work as well. Wow, I have spent the past 40 years hoping to find another undergraduate like that. Randy was truly exceptional!

The departmental affiliation did not seem so important initially, since members of the Institute were to be housed together in new facilities. In retrospect my appointment in Zoology seems strange since I had never even taken a Zoology course and was trained in biophysics and biochemistry! In the
next year or so we were joined in the MBI by other new recruits: David Eisenberg, Park Nobel, Winston Salser and Felix Wettstein. After Winston Salser and Cliff Brunk joined John and me in the Zoology Department, the four of us instituted an undergraduate course entitled Introduction to Molecular Biology, which is still taught today in the Department of Molecular, Cell and Developmental Biology (MCDB). As each year brought new MBI recruits the various departments underwent significant changes, with Zoology and Plant Biology becoming a Department of Biology that ultimately evolved into MCDB and the Department of Ecology and Evolutionary Biology. As more molecular biologists joined the Institute I think most of us identified primarily with the MBI; departmental affiliation didn’t seem to be so important. Lively interactions between MBI members, participation in a new PhD program and involvement in recruiting new faculty kept spirits high in the early years even though we were housed in temporary space. Paul Boyer’s enthusiasm and drive was infectious!

In my initial excitement about being one of the early members of the MBI I failed to appreciate fully the distinction between planning to construct a new building and actually having the building funding in hand; not that it would have mattered. If Paul Boyer said that we were going to have a new building then we were going to have a new building. But federal funding for buildings was very limited at the time and there was a long, but ultimately successful, struggle to obtain the funding. If it could be done, we had complete confidence that Paul could do it! Only on September of 1973 was ground actually broken for the new MBI building! Little did I know in 1966 that it would be more than 10 years before I would finally have a new lab! In these early years John Fessler and I were housed in the Life Sciences building, partly in converted vivarium space, while waiting for the new MBI building to materialize. I still recall my first meeting with John. We had each requested a preparative ultracentrifuge as part of our startup package. Paul had assured each of us that we would have an ultracentrifuge and upon our arrival, there it was, an ultracentrifuge that John and I would happily share for many years!

Paul Boyer was out of town when we first got word of federal funding for the building. David Eisenberg, John Fessler and I decided to meet Paul at the airport upon his return to celebrate the news with him. We all went out to an ice cream parlor and ordered for Paul a 12 scoop sundae termed an “Earthquake”, or something like that. It was fantastic news!

Once construction had started on the new building our recruiting of outstanding new faculty became even easier. The months before actual occupancy of the new building were exciting times, planning who would go where. Having seen the wisdom of shared facilities in the Stanford Biochemistry Department, Mike Grunstein, Bill Wickner, Owen Witte, Arnie Berk and I agreed to share common facilities and equipment on the third floor and to have a weekly floor meeting in which students and postdocs would present their research. We were joined later on the third floor by Tom Kornberg to complete the “Stanford Mafia” as Bill used to refer to us. The sharing and weekly floor meetings still continue today even though the mix has changed with departure of Bill, Tom and Owen.
DAVID EISENBERG, 1968

No understanding of the early days of the MBI is possible without an appreciation of the remarkable character and leadership of Paul Boyer, and the magical transformations that these qualities wrought. It was in spring 1967 when I first heard Paul Boyer’s voice. I was then a postdoc in the lab of Dick Dickerson at Caltech, and someone told me that I had a call in the telephone booth down the hall. The voice introduced himself as Paul Boyer of UCLA, and told me that UCLA was thinking it was time to start a program in protein crystallography. He had received my name through Dickerson and would like to invite me for a seminar.

I explained all this to my wife Lucy that night. We had come from Princeton where I had done a previous postdoc, and Princeton just then was talking with me about coming back as an Assistant Professor. We both still thought of ourselves as being from back east. But I said to Lucy, “This Dr. Boyer sounds awfully nice.” She replied, “Then you should go over and see what he has to say.”

So I did. I met with Dr. Boyer and the then members of the Molecular Biology Institute, which at that time had no home. Dr. Boyer’s office was in Chemistry. He explained that he had earlier hoped to recruit Paul Sigler, or another senior protein crystallographer, but these scientists had indicated they were happy in their present settings, and so now he wanted to build with a junior appointment. I responded that I had not had a lot of experience in either protein crystallography or laboratory biochemistry, but I would like to try. Boyer told me that he thought I would find collaboration and help at UCLA to the extent I would like it. His straightforward, open manner gave me confidence that this would be an environment in which I might succeed.

Also in Chemistry, I met Verne Schumaker, whose warmth and enthusiasm for biophysical chemistry I immediately saw. In the old Life Sciences building, I met Dan Ray and John Fessler together in one of their labs. I remember asking them, “Is Dr. Boyer a person you can trust?” They both said, “Definitely!” and no truer assurance was ever given to me.

One matter we had to take on trust was that there would be a building for us to work in. At that time, Paul was submitting application after application to federal agencies for funding of the building. That was before the days when UCLA would put up a building by some sort of magical deficit financing. Each of these applications was the thickness of several dictionaries, all typed by hand. One after another was turned down, but somehow we just knew that Paul would come through with the building. At length he persuaded Las Vegas businessman Albert Parvin to donate $1,000,000 toward the building, and used this gift as a keystone, telling each of his other potential funding sources that if they would come through, the building could go up. The final missing contribution was from NIH.

At this crucial juncture, Paul traveled to NIH to make his persuasive pitch in person, and lo and behold, he called back to tell Lyda that this was it. Lyda,
the Rays, the Fesslers, and the Eisenbergs decided to surprise Paul with a champagne celebration at LAX when he disembarked from the plane. Those were the days when you could actually meet passengers coming out of planes, and you could actually take a wine bottle into the waiting room. We unfurled a huge banner that said “Welcome Home, Paul, Father of the MBI.”

As the passengers filed out of the plane, they were extremely interested in our celebrating group, and some waited to see who this important person Paul would turn out to be. But Lyda became worried. She said, “I don’t understand. Paul is always the first person out.” Finally the last person came out, and we were left holding the banner, and champagne glasses, looking silly. Just then Paul came up behind us, and asked “What’s all this about?

When Paul had arrived at Dulles airport, he found another departing plane that would arrive in LA a few minutes earlier than his scheduled flight. Never one to waste a minute, he jumped onto the earlier flight without time to call Lyda.

So our surprise having backfired, Lucy suggested we drive to Westwood for ice cream at the Swenson’s parlor. “Good,” said Paul, “I love ice cream.” There we dared Paul to order the largest of all the offerings, a massive concoction termed The Earthquake, consisting of a half dozen or more scoops of various flavors, plus sauces, nuts, and whipped cream, and my recollection is that Paul pretty well polished it off. That was a great day for the MBI, as so many days since then have been.

Also in those early MBI days, Paul together with the Biochemistry Division recruited John M. Jordan to the faculty. John was a biochemical virologist, and one of the first African-American tenure-track molecular biologists in the country. John was a dignified, gentle, hard working scholar and teacher, invariably seen late at night tending his animal cells and setting up his experiments. Not long after he moved his research group into the new building, John’s career was cut short by a paralytic stroke. Squarely confronting this tragedy, it was Paul Boyer who assumed responsibility for making sure that John received medical care and financial aid, and that his interests were taken care of.

During the period of Paul’s fundraising and building planning, he kept our spirits up by calling us to occasional meetings where we would discuss matters such as the Ph.D. program or the seminar series, and the type of scientists we would recruit when the new space would be available. We seemed to arrive at our decisions by a completely democratic process, but as I watched Paul operate over time, I realized that he had discussed each important matter with each of us separately ahead of time, and had gently implanted the seeds of the eventual decision. There were virtually no disagreements and generally good camaraderie. As one example of camaraderie, I recall Winston Salser’s early entrepreneurial actions, purchasing huge amounts of biomedical supplies from Vietnam war surplus, and carrying them around to the rest of us.

There also was camaraderie in teaching. Verne Schumaker started me off in teaching a biochemistry laboratory course with him. Watching Verne carry
out biochemical extractions in front of the class was watching a true master at work. Later Fred Eiserling and I taught a course on structural biology. That course eventually morphed into its current version taught by Todd Yeates, Juli Feigon, Rob Clubb and me.

In the planning of the building, Paul paid attention to every detail, befriending the architect, Harry Shimabukuro. Harry later married Paul’s technician Margaret, and Paul gave his other technician Donna Bryan as a gift to the rest of us as building manager. This was another of the “democratic” decisions that just seemed to emerge.

For the building dedication, Paul encouraged Dave Sigman to devise a biochemical christening. A bottle of champagne was suspended by a long strand of twine from the roof, and held away from the façade by a restraining line to the railing of a VIP reviewing platform. This platform was in front of the building, at the edge of the bomb shelter, and held a lectern, and seats for Paul, Mr. Parvin and Chancellor Young. The restraining strand of twine passed, by Paul’s design, through a small grommet, and was secured in place by a wad of DNA. Dave Sigman had prepared a solution of DNAase, which Mr. Parvin under the watchful eye of Chancellor Young squirted on the DNA with a syringe. Either the hydrolysis of the DNA, or maybe just the lubricating effect of the liquid, diminished the friction on the restraining twine, freeing the bottle to swing slowly through a great arc towards the building, smashing against the concrete side, evoking a roar of excitement from all of us well wishers. It was more Paul Boyer magic.

CLIFFORD BRUNK, 1968

I finished my doctoral studies in the summer of 1967 and was planning on postdoctoral studies at the Carlsberg Biological Institute in Copenhagen, Denmark. However, I had applied late for NIH post-doctoral support and would not receive word on funding until literally the time I was to leave. I knew Dan Ray quite well; we both worked in Phil Hanawalt’s group in the Biophysics Lab at Stanford. Dan was Phil’s second student and I was his third. During the summer of 1967, Dan invited me to give a seminar at UCLA and I accepted primarily to practice my presentation. After a number of years in the San Francisco Bay area I had little desire to relocate in Los Angeles. My wife and I literally tossed a coin to see if she would join me on the trip to UCLA, as the prospect of our coming to LA were deemed remote. The coin came down heads and she made the trip.

I found UCLA very different than my preconception, which was based largely on exposure to Anaheim. Dan had been recruited by Paul Boyer to the Molecular Biology Institute (MBI) and was very enthusiastic about the new Institute. I found the Zoology Department a congenial group of faculty, with a strong reputation, interest in protozoology and a desire to move to the molecular level. I actually accepted a faculty position in Zoology with the agreement that I could do at least one year of post-doctoral work before beginning on campus. The NIH funding came through and I went off to Copenhagen with a commitment to join the Zoology Department. In retrospect it is clear to me that
the MBI was a major factor in my decision. Although Zoology was committed to move toward molecular approaches and had hired Dan Ray and John Fessler, it was really the prospect of the MBI and the dynamic activity of Paul Boyer that made UCLA attractive.

The year in Copenhagen was an eventful one. My research progressed well, but that year was remarkable. I had been involved in the anti-Vietnam War movement as a graduate student and viewing the US 1967-68 from abroad was incredible. The Democratic convention in Chicago disturbed my Danish colleagues greatly and they became more sympathetic to my views. The assassination of Martin Luther King in the fall and the assassination of Robert F. Kennedy in the spring, along with the riots in Paris and the Russian invasion of Czechoslovakia, were unsettling. I had attended the Federation of European Biochemical Societies meeting in Prague a mere two weeks prior to the invasion. Even events in the University of California were troubling; the Regents fired UC president Clark Kerr, one of my heroes.

I returned to UCLA after a year in Copenhagen, although I had a second year of funding, as I was anxious to set up my own lab. There was a strong feeling of new possibilities, which largely centered on the MBI. There were frustrations with the delayed building plans, but new faculty were pouring in and Paul infused everything with confident optimism. It would be virtually impossible to overestimate the positive effect he had on the new faculty; great things not only seemed possible they seem enviable.

Within a year Dan Ray, John Fessler, Winston Salser and I initiated a course, Introduction to Molecular Biology, in which I participated for several decades. The course grew rapidly and course material increased so quickly that the major challenge each year was to judicially pick topics to be omitted so that new material could be accommodated.

Like many young faculty I never intended to stay at UCLA for 40 years. However, during those 40 years UCLA has grown immensely, so that it would be hard to find a better institution. The MBI has been a large part of that achievement of excellence. I have spent sabbatical years in Israel, Denmark, Canada, Germany and Spain. None of the Universities I have resided in have anything comparable to the MBI. Of particular value are the lunch seminars; they do a better job of keeping the membership abreast of each other’s research than anything I have seen anywhere else.

UCLA has been lucky to have Paul and the MBI. Those of us who affiliated with the Institute have been equally fortunate, and this has been so right from the very earliest times.

**Larry Simpson, 1968**

I came to UCLA mainly because it was the only University that offered me a position with a 2 year leave of absence to go on a postdoc in Europe. I actually thought I would not spend much time at UCLA since I had heard such horror stories about Los Angeles. Almost forty years later now, I realize that I had been
mistaken about Los Angeles, and that I found UCLA such a compatible and exciting place that I never really considered moving.

Initially I was a member of the Zoology Department, which morphed into Biology and then into Division I of Biology, etc. I had not been aware of the Molecular Biology Institute initiative prior to arriving on campus in September, 1968. When I learned of it, I was very excited since we had just discovered kinetoplast DNA minicircles and I planned to become more molecular in my research. I attended every Tuesday lunch seminar after the building was constructed and this exposed me to those people actually doing real molecular biology. I soon applied for membership and was accepted as an Associate Member. Truthfully after the building was constructed, I did contemplate moving into the new building but was never asked to do so and did not have the nerve to request this myself.

The new building greatly accelerated my interest in molecular biology, as did my closest colleagues and friends at UCLA who worked in the Institute and greatly influenced the direction of my research. People such as Mike Grunstein, Dan Ray, John Jordan and Winston Salser were especially helpful.

Over the years I watched the MBI become a preeminent institution and the catalyst for the diffusion of the modern molecular biological techniques of cloning, sequencing, etc. into every life science Department on campus and then into Medical School Departments. As this occurred, I observed an interesting phenomenon: Every Department wanted to change its name to have the word “Molecular” in the title. There were actually turf battles over Departmental names, as silly as this seems now. Upon thinking more about this phenomenon, I realized that this was general in science world wide and was a harbinger of the “Death of the .ologies” as I wrote in a short essay a few years ago. I think this essay may be of interest to readers of this history so I reproduce it below:

It is my sad duty to report on the death of one of academics’ most cherished institutions—the “ologies”. Way back before we knew so much and understood so little, Universities compartmentalized knowledge by the addition of the Greek logos, meaning the ‘study of’, ‘specialty in’ or ‘art of’ a given field to the end of the word. I did a Google search on “ologies” and the winner (at http://phrontister.info/sciences.html) had 633 different fields with this suffix. There is also an entire book on this subject called “Ologies and Isms: A Dictionary of Word Beginnings and Endings” by Michael Quinion for those who want to delve further. The purist should note that the “o” is superfluous, with the true suffix being “logy”, which had led to a few “alogies” as pretenders to the “logy” throne.

This compartmentalization was quite successful and led to the creation of many departments of “- logies”, each with a separate supposed set of goals that survive to this day in all major centers of higher learning. It also led to the creation of innumerable tenure-level jobs for Chairs, Directors, Deans and the like, each with their own fiefdom of administrators and budgets and also a few researchers or professors.
Generalizations and attacks on the status quo are dangerous and lead to shrill counter attacks by those affected, but I feel it is clear that the growth of modern science has led to the death of this venerable institution. Let me illustrate this in the field I am familiar with—modern biological research. Yes there are still Departments of Biology, Physiology and even Botany, Zoology and Protistology, but everyone in these Departments is doing the same thing and this has nothing to do with the “ology” that pays their salary. They are studying interesting problems from every point of view at every possible level. The Protistologist, say, is interested in the motility of a particular protist. He or she however is interested in not only the molecular mechanisms involved down to the level of the molecules and energetics, but also the comparative aspects of motility in other related protists and even metazoans, the evolution and origin of this mechanism (and the cell itself), the morphological aspects, the natural history of this phenomenon, and even the role of this phenomenon in eco-communities of cells, and from a selfish anthropomorphic point of view, the possible biomedical significance, including the immunological aspect and interaction with the host if they are parasites. Each aspect of this study could be called by a different “ology” and therefore the existing nomenclature fails to communicate the existing reality.

One sign of the death throes of the “ologies” is the desire of almost all University Departments in the Life Sciences to include the word, Molecular, in their title. At my University, UCLA, it began with the creation of an interdepartmental Institute of Molecular Biology almost 42 years ago. Then the powerful techniques of molecular biology diffused to the existing “ology” Departments and there was a recurring scramble to rename the existing Departments and to wage the turf battles with the Institute that resulted. Even clinical Departments in the UCLA School of Medicine began hiring basic researchers who used recombinant DNA techniques and the great name change race was on. The final result as of today is that the venerable Molecular Biology Institute is a place where one eats a free lunch on Tuesdays while listening to seminars, and every Department is the same with a heterogeneous group of people all using every possible research technique ranging from molecular to organismal to ecological to study basic problems in the life sciences and biomedicine. But they still advertise themselves as different, claim to have specific goals and still teach courses with different names, but this is like whistling into the winds of change.

Another sign of the death throes is the emergence of entirely new fields derived from amalgamation of existing fields and exhibiting new “emerging properties”, such as, for example, Systems Biology or Astrobiology and the Search for Extraterrestrial Life. I myself find that I utilize examples in my courses of, for example, the discovery of dark matter and dark energy in the universe from Astronomy as metaphors for the discovery of the World of Small RNAs in Biology.
I make no value judgment on this paradigm—changing change in academic organization, except to say that it makes life much more interesting but at the same time more confusing. Politicians, universities, students and even faculty like compartments and get disoriented when faced with the chaos of real life. But perhaps the death of the “ologies” may lead to a deeper appreciation of the true goals of modern science and academics—an answer to the really big questions of what life is, where it came from and how it works, who we are and where we came from, and what is existence and the nature of things.

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WILLIAM CLARK, 1970

I had been an undergraduate Zoology major at UCLA in the 1960s. The department at that time, as the name suggests, was classically oriented—anatomy, physiology, embryology—and of course “field studies”, before ecology became the fashion. I loved biology, but I could never quite get what they were driving at. The only class that piqued my interest, and in which I got an A, was Dick Siegel’s Genetics course. Jacob and Monod’s model had just broken out, and I got a glimpse of where biology might be headed. But my grades in biology were not good enough for graduate studies in Zoology at UCLA, and they declined to see more of me. Fortunately my grades in physics and chemistry were much better, and I got into grad school in Chemistry at the University of Illinois, and followed on with a doctorate in Biochemistry at the University of Washington.

All of this is by way of saying that when, at the end of a post-doc at the Weizmann, it never occurred to me to apply for a position at UCLA. But they had contacted my doctoral advisor, Bill Rutter, looking for job candidates, and the Biology department at UCLA ended up writing to me. I was putting together a job seminar tour at the time, and added UCLA on as a way to get a free ticket to LA to visit my mother, who lived across town.

I was almost certain I would not be interested in UCLA for a faculty position, but the UCLA I found some seven or eight years after leaving it bore little resemblance to the school that had rejected me as unsuited for their graduate Zoology program. The main factor in this change, of course, was the advent of the MBI in the intervening years. This was 1970, and there was no building yet, but it was clear that the MBI under Paul Boyer’s leadership was having a major impact on reshaping biology across a wide range of departments. I had already formed a vision of universities as a sort of giant sandbox to play in, and the notion of living one’s academic life within a single department held little appeal to me. The MBI weekly seminars, which were already in place—and one of which I attended during my visit—struck me as a great way to get into the sandbox.

So to my great surprise, with half a dozen job offers in my back pocket, in 1970 I ended up coming back to my undergraduate department at UCLA. Five
years later, with tenure under my belt, I was delighted to be invited to move into the just-completed building. I was pleased to be included with some of the best of our existing faculty, and to participate in the recruitment of exciting new young people too numerous to mention here. Unquestionably, the MBI was a major force promoting excellence in biological research at UCLA over the following decades, and I am proud to have played a role, however modest, in that.

FRED FOX, 1970

SUCCESS COMES EASY IN THE HOUSE PAUL BOYER BUILT

My first contact with Paul Boyer and the MBI was a visit to UCLA in early 1966 to explore the possibility of an appointment. I think it was in January, and vividly recall a faint scent of photochemical smog in the air and a welcoming visit to Paul’s home. I was favorably impressed by both UCLA and what Paul and his colleagues were creating, but I was on the brink of submitting my first grant to the NIH, and a timely decision could not be reached owing to the absence of a participating chair who held the key to space (laboratory, of course). As that deadline grew uncomfortably close, I had no choice but to conclude my negotiations with UCLA and accept an offer that supported submission of my grant.

Several years later I reinitiated contact with Paul when it became apparent that the west coast had become the most productive community in science in the area I found most appealing: the interface between mammalian cell biology and biochemistry. This time around negotiations moved rapidly and smoothly through effective communications between Paul and Sid Rittenberg who held the key to what would become my initial UCLA laboratory home. Ralph Martinez was particularly helpful in facilitating renovations and other components that enabled us to work productively the week after the van delivered our equipment and supplies from Chicago.

The environment for supporting interdisciplinary communication and research collaboration that Paul and his colleagues had established was truly impressive, and together with the ease of intercampus communication in California facilitated our achieving productive research collaborations at UCLA and other institutions. This environment also supported my instinct to establish a much-needed mechanism through which scientists in our field could communicate face to face and share ongoing research in a timely fashion.

Two years after arriving on campus, the first UCLA Symposium on Molecular and Cellular Biology was held at Squaw Valley. These meetings provided opportunities for many UCLA MBI faculty (and this was most of us in the life and biomedical sciences) to organize meetings that moved science forward in their fields. Several of these meetings were pivotal: the first workshop on cross species DNA transfer and expression (chaired by Herb Boyer and Stanley Cohen), the first major meetings on gene expression (Don Nierlich and Bill Rutter), plant biotechnology (Bob Goldberg), Acquired Immune Deficiency Syndrome (Mike Gottlieb and Jerry Groopman), and a host of
immunology meetings with Eli Sercarz as prime mover, to name but a few. The unhurried development of these meetings and what they accomplished for UCLA and beyond owe much to Paul’s appreciation of their value and his support through space in the MBI until meeting income was adequate to support their administration off campus. Today’s Keystone Symposia owes much to Paul Boyer.

The channels of communication that Paul had created also proved invaluable at the point in time when the NIH redesigned its institutional training grants around interdisciplinary science fields as opposed to teaching departmental lines of administration. I caught wind of this “change” during a visit to NIH and obtained a copy of the RFA, which had yet to be circulated. What good fortune: it was as if the RFA had been designed for us. In effect it had been. Paul’s had created a community that could be activated quickly and effectively to seize new opportunities, and we worked together seamlessly to become funded to fullest extent possible in every area of training identified in the RFA, and were the only campus in the country to achieve this.

The MBI building was an important element in the building of our community, but should be viewed as secondary to the spirit of community which Paul, and his supporting partner Lyda, built brick by brick. I am grateful for the opportunity to have shared in Paul’s magnificent venture at UCLA.

**RANDOLPH WALL, 1972**

I arrived at UCLA in October, 1972, direct from my postdoc with Jim Darnell at Columbia University. I decided to “go west” after a trip to a national meeting in San Francisco where I first saw the Pacific coast. My Department, Microbiology and Immunology, was chaired by John Fahey and was mostly located on the fourth floor of the Center for Health Sciences (CHS). Unfortunately, I discovered the first day at UCLA that my lab space was not up there and was still awaiting renovation. Happily, I was rescued by the generous offer of space in Marcel Baluda’s laboratory where I shared a lab bench with one of his PhD students, Ron Evans. When finally finished, my lab was two small rooms (and an adjacent closet of an office without windows) on the A-floor basement of the CHS just off the corridor that housed the hospital morgue. I successfully began my research there and counted the days until our move into the new building in the summer of 1976.

In our rush to get into the new labs on the fifth floor of the new building, we arrived well before the curtains and the California sun hitting the extensive windows turned the place into a hothouse every afternoon. Our interim solution was to cover the windows with silver mylar sheets which lent a space ship aura to the building, but allowed us to work decently clothed. Today, after more than 32 years in the building, I still admire the robust unadorned concrete and dark glass structure created by the MBI’s architect, Harry Shimabukura. Harry became a dear friend and also played a critical role in advancing our research when he converted two lab suites on the fifth and sixth floors into high level biocontainment labs (a.k.a., biohazard suites) which were federally required in the early phase of our mammalian recombinant DNA cloning work.
I had decided to apply molecular biology to study the immune system before leaving Columbia. UCLA’s strong programs and accomplished faculty in immunology (from my Department) and in molecular biology (via the Institute) proved to be important foundations for our early work cloning antibody genes and messenger RNAs. Since then, the Institute has continued to be a remarkable and stimulating place to do research. It is a fitting tribute that under Paul Boyer’s leadership and vision as Director, the MBI developed into one of the premiere Organized Research Units (ORU’s) in the entire University of California system. This excellence attracted a number of stellar young faculty and many outstanding PhD students to the MBI. I still marvel at the exceptional MBI (and M&I) graduate students and postdocs who chose to work with me over the years. Their work and creativity made the lab successful. The MBI also proved to be an ongoing and fertile source of important collaborations. I especially enjoyed working with David Sigman, using his chemical nucleases to probe DNA structures and protein factor interactions. I still recall with great fondness, receiving what was high praise from Dave when he would say, “not bad for someone who isn’t a chemist.”

**JAY GRALLA, 1975**

**MOVING INTO THE BRAND NEW MBI BUILDING**

Many of us were hired in the rush of excitement surrounding the staffing of the new MBI building. Most of the new hires were smarter than me – they waited until the building was finished before arriving at UCLA. So I was one of the few who arrived the year before the building opened. My temporary home was in Young Hall, on the fourth floor. Basically, this was in the midst of Organic Chemistry; my lab had lots of hood space, connections to gas lines, vacuum jets and other marginally useful accoutrements. There was little of the Biochemistry equipment needed for Molecular Biology available. This was in the days before kits and at first one couldn’t even buy restriction enzymes. I was the only biochemist who had ever cloned anything (well, it was just pieces of bacterial genes, but still) and so we had to reinvent the wheel for use at UCLA.

Things were cheaper in those days but money was tight, which wasn’t helped when common lab equipment periodically disappeared. We eventually tracked this down to grad students in the neighboring labs who were permanently borrowing our glassware, pipets and small equipment. When I complained to their young faculty advisor, he was apologetic but explained he had virtually no money. In fact, I gave him some advice about a small grant, and when he got it, the pilferage stopped. Needless to say we were anxious for the MBI to open.

By the beginning of summer I had accumulated a small group of students and we made ready for the move. Of course we moved everything ourselves, pushing carts over the Young Hall to MBI bridge, carrying boxes, moving our precious enzymes in buckets of dry ice. The group was small enough that nobody needed to fight over desks next to windows. After the move the 3 students and I we went off to celebrate at a Chinese restaurant (yes, beer was
involved) where a remarkable event occurred. After dinner my fortune cookie arrived and inside were 3 identical fortunes reading “you will make a great discovery.” I gave one to each student and the rest is history, so to speak.

Shortly after the move, we had one of those late season heat waves and then we paid for those big sunny windows. The air conditioning hadn’t been designed properly and we, and our precious equipment, began to swelter. My ever-resourceful students went off somewhere and came back with reflective sheeting, which they pasted on all the lab windows. This kept us and our equipment somewhat happier until the air problem was solved.

To say that the MBI was an improvement over the Organic Labs in Young Hall is like saying that a fine wine is better than Kool-Aid. This refers not only to the wonderful facilities but also to our new colleagues. Finally, the people we passed in the hallways were interested in and doing Biochemistry and Molecular Biology. One cannot overestimate how much easier it was to work when such casual contacts led to useful discussions, collaborations and progress. In addition to the excitement of the building filling up with amazing research groups the camaraderie of everyone quickly made the MBI much more than a place to do experiments and for this we were and still are very grateful.

MICHAEL GRUNSTEIN, 1975

I came to UCLA in the summer of 1975, having just completed my postdoctoral period at Stanford Medical School. I mention this since I met Bill Wickner there. He was with Arthur Kornberg and I was across the corridor with Dave Hogness in the Department of Biochemistry. We spoke briefly, he let me have a few units of bacterial RNA polymerase, and in talking to him I mentioned that I was invited to interview at the MBI. Bill had already committed to the MBI, spoke at length about Paul Boyer, and Bill’s coming here was an inducement to my coming here as well.

The person who recruited me was Winston Salser, in part as a result of my studies on histone gene regulation with Larry Kedes at Stanford and my developing a cloning technique (colony hybridization) in Hogness’ lab. Cloning had not yet come to UCLA. Consequently, when we first arrived we spent a month renting Winston Salser’s house in Pacific Palisades (Winston and his family were at Los Alamos where Winston was trying to purify terminal transferase, in quantities that required mixing with an outboard motor, for the upcoming cloning revolution). A few months later Judy and I bought a home in the Palisades and soon after noticed that across the street was a house for rent. I called Bill, I think he rented the house sight-unseen, and I pulled up the For Rent sign. Now our families were housed across the street from each other.

In coming to UCLA we tried to recapitulate some of the better features of the Stanford Medical School Department of Biochemistry. These were:

(a) Faculty of similar interests should be located in proximity. We decided to have labs on the third floor of the MBI building where Dan Ray and Fred Fox were already housed and we attracted at various times Tom
Kornberg, Katherine Calame, Arnie Berk, Owen Witte, Utpal Banerjee and Harvey Herschman to our floor.

(b) Sharing of equipment, large and small. All the groups on the floor had keys to the other labs.
(c) The faculty in the MBI should meet once each week to hear one of them discuss their work. Hence the Tuesday noon MBI meetings.
(d) A weekly floor meeting was used to get the students and post docs and the PIs to know each other. The floor meetings affected our work profoundly.
(e) A yearly Retreat (Stanford Biochemistry had Asilomar, we had Lake Arrowhead).
(f) We instituted a program to attract independent Research Fellows for a limited period of time. John Scott came from Stanford as an MBI fellow for several years.

The only thing that we did not copy shamelessly was the sharing of grants between labs on the 3rd floor.

Those first few years were especially exciting. Paul Boyer included all of us in decision making for the new MBI (hiring, equipment, even size of couch) and we felt that this was our institution. With the focus on Genetics on the third floor I was encouraged enough that I switched from studying histone gene regulation in sea urchin development (1975-1979) to histone protein function in yeast. My first two MBI students John Wallis and Mary Rykowski started the yeast work in the lab setting up yeast gene sequencing and gene replacement, after Hinnen, Hicks and Fink produced a paper (PNAS, 1978) describing the first successful attempts at yeast DNA transformation. John and Mary were fearless, setting up the yeast genetics and gene replacement system from scratch in a sea urchin development lab. Eventually, we got rid of the sea urchin tanks especially after one of them, a 150 gallon salt water tank, leaked onto Jim Lake’s lab on the 2nd floor below. We never looked back.

HARUMI KASAMATSU, 1975

I was not among the founding members of the inter-departmental research organization that eventually became MBI. Some time in 1975, I received an offer of an Associate Professorship from the MBI and Division I of then Biology Department at UCLA. At that time, I was a super-postdoc, senior researcher fellow in the laboratory of the late Jerry Vinograd of the Division of Biology at Caltech, and knew little of the founding scope and history of MBI. Larry Simpson actively recruited me to UCLA. I chose the offer from UCLA over another one I had received earlier from a renowned institution on the East Coast. The selection was rather straightforward. First, the MBI’s first graduate student, Arleen Forsheit, who completed her Ph.D. work with Dan Ray and who was then a postdoctoral fellow with Norman Davidson at Caltech, invited me to her house with Don Glitz and his wife. They spoke fondly of MBI and its program. Second, I compared the two offers at a very personal level. Accepting the other offer from an east coast university would have meant another long-distance commute. At the end of 1974 my husband, Takuji, had come back from one of the Max-Planck Institutes in Goettingen, then West Germany, and started to work in the Division of Biology at Caltech as a senior research fellow. Since we wanted to
avoid another separation, UCLA’s offer provided us with a perfect solution to maintain balance between work and family. Once my choice had been made, I felt very thankful for the given occasion. I fondly remember my first encounter with many MBI people who have become my close friends and colleagues over the years.

At the time when I was recruited in mid-1975, the MBI building was not completed, so I took a one-year leave of absence from UCLA to learn cloning procedures from Tom Maniatis, who has just moved from Cold Spring Harbor to Caltech. This period played indeed a critical role not only in securing outside funding but also in preparing experimental materials for a new project, “The morphogenesis of DNA tumor viruses,” which I launched upon joining MBI.

In 1971 while working in the Jerry Vinograd’s lab, I discovered a partially 3-stranded covalently closed circular mitochondrial DNA, the D-loop, as an intermediate of mitochondrial DNA during its replication. The finding stirred an excitement. After many experiments using various procedures (that are all obsolete now such as equilibrium CsCl centrifugations with an analytical Model-E, mille-curies of radioisotopes, and many electron microscope hours) and many blunt exchanges of words to prove or disprove the nature of 3-stranded DNA, the term of “D-loop DNA” was born at Caltech. Jerry was reluctant at first to admit the presence of a “heretic 3-stranded DNA,” but was eagerly waiting for printout of radioactivity counts, standing by the side of a scintillation counter. Plotting a profile of counts by hand on a graph, he was even unhappy when he found a few points not lying on a straight line. I understand from the late Giuseppe Attardi and Anne Chomyn that the field of mitochondrial DNA has been thriving. I valued very much an air of transparency in many laboratories at Caltech and I enjoyed daily exchanges and discussions with various people who belong to different laboratories. I owe much of what I enjoy in science to discussions with my mentors Jerry Vinograd and Norman Davidson and other faculty including Robert Sinsheimer, Giuseppe Attardi, Jean-Paul Revel and Max Delbrück.

I eventually arrived at UCLA in late 1976 when my lab in the new MBI building was ready. MBI occupants at that time were:

6th floor: Paul Boyer, the late David Sigman, the late John Jordan and George Fareed
5th floor: Marcel Baluda, Fred Fox, Randy Wall, Winston Salser and Alexander Kolin
4th floor: Verne Schumaker, Emil Reisler, Jay Gralla, John Fessler and Harumi Kasamatsu;
3rd floor: Dan Ray, Tom Kornberg, Bill Wickner and Michael Grunstein
2nd floor: David Eisenberg, Bob Sweet, Fred Eiserling, Jim Lake, Bill Clark, and the late Judith Lengyel.

George Fareed and Marcel Baluda were animal virologists in the building and were helpful to my research by letting me use their facilities, as were non-MBI residents Felix Wettstein and Jack Stevens. Irving Zabin, Audree Fowler,
Agda and Larry Simpson also provided access to their facilities. John Jordan, Lisa and John Fessler, Verne Schumaker and Marten Phillips in the MBI building helped me immeasurably when I arrived at MBI.

I recall that designing, funding and constructing my tissue culture room in 1976 within my allocated lab space on the 4th floor was expensive (the MBI provided the construction and modification costs as my startup package came with recruitment). I designed a small room to accommodate two tissue culture hoods, experimental benches, a refrigerator, a freezer, and a CO₂ incubator. The “waiting games” between the campus electricians, plumbers, and carpenters and me to place simple electrical plugs on walls, sinks and faucets, and bench/desks lasted a most exhausting 6 months. A hidden benefit was that during this waiting period, I trained myself to read architectural drawings. Without Donna Bryan’s assistance the completion of my tissue culture room would have been delayed much longer than it was. Having a pro-active building manager seems to be a necessity rather than a luxury.

There is no single good way to answer the question: “What made you think that being a member of the MBI was intellectually and scientifically better than simply belonging to an academic department?” Informal faculty research seminars/discussions on Tuesdays have been one of the most stimulating activities of the MBI, as are the cross-disciplinary education of graduate students and the MBI seminars held on Thursdays. Enjoying the program, I have taught the graduate courses. In addition, there were excellent cross-departmental seminar programs outside of MBI offered for animal virologists and immunologists. Making priorities was critical for me to select which seminar I would attend, since I worked at the bench in my lab. Many new scientific discoveries are pouring in: many assumptions to be erased and many aspirations must be rewritten. The MBI has proven to be a stimulating place for educating graduate students by providing them with the solid and basic scientific background.

JAMES LAKE, 1976

My wife, Laura, and I arrived in Los Angeles on Memorial Day in 1976; at least we thought we had. We drove across the country from New York City with our beloved Norwegian Elkhound, Baldr, in the backseat. The journey gave us a sense of how far we were moving. As we were leaving Victorville Memorial Day afternoon, we saw the much-anticipated “Los Angeles County Line” sign and called Fred Eiserling to let him know that we were in LA and would be soon be arriving in the Palisades to stay overnight with him. Fred said, “We’ll leave the light on and the door unlocked. We usually go to bed around midnight.” We thought that was a very odd response to our belief that we were just around the corner. Little did we appreciate that on Memorial Day, he was right. We got there about one in the morning.

It was very exciting to be coming to UCLA and the MBI. Dave Eisenberg and Fred Eiserling had worked hard to get the university to invest in an appointment in structural biology, and I was overjoyed to be the first hire.
Having the three of us together on the second floor was like a dream. We had high hopes, and I felt as if we were starting a new institution that, if we were lucky, could become a structural biology center, like the MRC in Cambridge.

When Laura and I got to the university the next day, one of the first people we got to know was Betsy Heidner in David Eisenberg’s lab. A few days later, my lab equipment arrived from New York and Betsy helped us with the moving in. She was then a senior postdoc, and in charge of moving Dave’s lab into the MBI. We were one of the first labs to move in, and Betsy explained how the university worked, what forms to fill out, etc. I quickly learned that a P39 was not a World War II airplane, and a lot of other information unique to UCLA. She explained to us that Donna Bryan had gotten around university requirements that all filing cabinets and bookshelves were required to be black, and purchased yellow, blue, orange, as well as black cabinets. Laura, Betsy, and I were pleased with Donna’s purchasing acumen. Color! What a concept! At first we reveled in the colors, but then we realized that Donna had decided that all the labs should have the same number of each of the colors, and we thought we could do better. Betsy, I recall, liked the blue and Laura and I liked the orange and yellow. So, late one night we decided to redecorate and switch cabinets between Dave’s lab and ours. That worked so well that a few nights later we decided to also switch cabinets between ours and one of the unassigned labs.

At first Donna was helpful and a good buddy, helping us move in. But after about six months or a year, she became furious because she had numbered all the cabinets; when we switched colors, we ruined her records. She found the discrepancies and went down to our labs to let us know. She then exchanged all of the rearranged cabinets and put our stuff on the floor. So long to the colors!

Over the years I’ve watched the MBI grow and prosper and have seen structural biology become very strong. My interests have changed and grown in other directions, but I still look back at those first few years and often think what a good decision I made to come to UCLA and the MBI. Maybe even some day I’ll get new cabinets.

EMIL REISLER, 1977

The interview at UCLA in 1975 changed my mind about staying on the East Coast and filled me with enthusiasm about the prospect of joining MBI. As with others who came to MBI in these early days, the most important factor in my decision was the vision of Paul Boyer for the future of the Institute, and his inspiring leadership in creating a vibrant research environment where ideas and collaborations flourish. The fact that part of Paul’s research program on ATPase enzymes focused at that time on myosin, the muscle protein on which I worked as well, was another attraction for me. The benefit of having one of the best enzymologists as a close-by colleague was rather obvious. Because of my interest in structural and biophysical understanding of the proteins I worked on, MBI – with the presence of Verne Schumaker, David Eisenberg, Fred Eiserling, Jim Lake, and Robert Sweet – was the ideal place for me to be.
I wasn’t as sure about that any more the day I arrived at UCLA in December 1976 and took possession of an empty laboratory, with all drawers empty, on the fourth floor of MBI, across from Verne Schumaker’s lab. To come from a fully equipped and well-oiled laboratory, although not my own, to an empty space – with only very modest set-up funds – was a trying experience. Fortunately, I was immensely helped and mentored in setting up my lab and starting my own research program in muscle proteins biochemistry and biophysics by Verne Schumaker. Not only did he open his lab and its equipment to the use of my group, but he also helped in decisions about purchasing, in the administrative aspects of academic life, and in my first undergraduate teaching assignment. Verne hosted me and my first co-workers in joint group meetings and provided intellectual stimulation in discussions about papers or the interpretation of analytical ultracentrifugation and light scattering results.

Verne’s invaluable assistance in starting my group did not end there. He managed to convince a new graduate student who was interested in immunochemistry to join the two of us on a collaborative project. To rise to the occasion I devised a new project involving immunoprobing of myosin light chains. Because of this project, which turned out quite well, this was the only time that I and my group owned and periodically visited a goat. After that experience we decided to partner with rabbits for making site-specific antibodies.

The other anchor of our research discussions, and frequently also preparation of muscle proteins, was Paul Boyer’s group, with its splendid postdoctoral fellows and visitors. After a short while, I learned also to benefit from and enjoy the always challenging, but stimulating discussions with Dave Sigman. All these interactions with my MBI colleagues enriched greatly my research program. Thus, after few months, with my lab no longer empty, I knew that MBI was indeed the best place for me.

STEVEN CLARKE, 1978

WITH A TRIBUTE TO DAVID SIGMAN, WHO JOINED THE MBI IN 1968 BUT DIED IN 2001

The Molecular Biology Institute was certainly crucial to my recruitment to UCLA. I interviewed in late 1977 on almost the one-year anniversary of the building’s dedication. Paul Boyer, David Sigman, Jay Gralla, Verne Schumaker, Bill Wickner, Fred Fox and others made a lasting impression on me – I felt like I was coming into a special place where great science would be done. The excitement of being in the MBI was obvious – I don’t think anyone said anything specific but the message was really clear.

I arrived in September of 1978 to space on the sixth floor across the hall from Paul Boyer and adjacent to David Sigman. Both of them could not have been more welcoming and turned out to be lifelong mentors, friends, and supporters.

David Sigman, who passed away in 2001 at the height of his career, was an important part of the MBI. He was one of the founding members of the Institute and exemplified for me what the MBI could do in terms of its interdepartmental and interdisciplinary missions. David’s own scientific interests
bridged the fields of organic chemistry, biochemistry, and molecular biology. At UCLA, David occupied a special niche – he was simply the guru for bioorganic chemistry. If one had a question on enzyme kinetics, David knew the answer – if it was on enzyme mechanism, David knew the answer. In his career he branched out from proteins to nucleic acids, from biochemistry to molecular biology to pharmacology and molecular medicine. Through his MBI connections, he collaborated with probably a dozen or more UCLA faculty in the biomedical community. David was as dedicated a mentor of younger (and older!) scientists both here and elsewhere as there could be. He was also a very large part of the collegial glue that held our biomedical community together. His support for his colleagues was bulldogged and unwavering. He was not afraid of going to battle for what was right. Perhaps the most visible tribute to this was his almost single-handed campaign in 1998 to convince UCLA to take the high road and name the MBI building in honor of its founding Director Paul Boyer instead of waiting for a "naming donor".

For my career, what David did was beyond any call of simple collegiality. He looked after me as an older brother would, helping me meet someone here, fending off trouble there. In my early days on campus, David would delight in showing me the ropes. Even though David was in a different department, he seemed to know everyone on south campus. More than once at lunch or walking on campus, David would introduce me to someone as "Steve, let me have you meet one of the members of your department". But sometimes things backfired. On one occasion early on I was using the balance in the lab just outside his office (I did take David seriously at his offer to use his lab – I'm not sure he realized how much myself and my students would be invading his space and monopolizing his equipment and time!) when he heard me talking outside and yelled out the open door: "Steve – what do you think about spin resonance and lipid structure?" As I breezed into his office, I said something like "that's the most overblown crock I've ever heard of" and there sitting on David's couch was a very distinguished looking visitor who I immediately recognized as the father of that field.

I treasured the time we spent together – his wit, his humor, his humanity. And his most colorful language – one never new what would come next. I had probably only been here a few months when David came into to my office to look at one of my Annual Reviews of Biochemistry – as I watched as he rifled through the pages trying to find what he wanted, I suggested he try the index. His snapped back "I went to Harvard too, and ended the sentence with a two word (or perhaps one word) description of a lower part of one's anatomy.

David not only felt responsible for my science, but for my exercise and competitive nature. He would get me to agree to Sunday 10K races, which I would forget about until he showed up outside my apartment at 7 am honking his horn. We ran for Soviet Jewry, for the Brentwood Chamber of Commerce, for the Westlake School. Mostly David would beat me (that gave him some pleasure), sometimes I would come in ahead (that was youth talking), and sometimes we would ran in together, just glad that we hadn't collapsed at the side of the road.
And David loved a party. Every time something nice happened to someone on the floor, it was the time for David to bring out the wine and cheese and celebrate in the conference room or in David’s office. And I certainly won’t forget David’s Friday afternoon "sherry hours" where I wooed my wife and participated in a thousand types of sane and inane discussions, punctuated by David’s "Quiz time" "What’s the atomic number of Bromine?" "You don't know that? And you call yourself a chemist?"

ARNOLD BERK, 1979

I am deeply grateful to Dick Dickerson for giving me a chance to write a few words about my time as Director of the MBI. I can also continue the story that Steve Clarke began about the achievements of our colleague David Sigman, who died altogether too early in 2001. I was privileged to serve as the third Director of the Molecular Biology Institute from 1994 to 2001. It is an obvious point that my greatest accomplishment as Director was to provide full support to David Sigman’s successful effort to rename the Molecular Biology Institute building in honor of Paul Boyer.

As I have mentioned to Paul on several occasions, all I had to do was sign where David asked me to sign. David Sigman is the one who worked tirelessly to make the building renaming happen. When first approached about renaming the building, the UCLA administration politely replied that the price for the application of an individual’s name to a building at UCLA was $20,000,000. David was not satisfied with that response. He very quickly gathered signatures on a petition signed by MBI Members and Associate Members and presented it to Provost Copenhaver, who had never before received a petition signed by nearly 150 faculty. The signatories included members of departments of virtually all of the physical and biological sciences at UCLA. Of course, it did not hurt that Paul had just won the Nobel Prize in Chemistry for his brilliant insight into the ability of proteins to form a rotating machine—a Eureka moment that came from following O^{18} atoms through the multi-protein ATP synthase. In the face of all this, the administration relented, especially since Paul had done 99% of the work required to raise the funding for the building in the first place.

However, the drama of the re-naming of Boyer Hall continued one more step. Credit for successfully navigating this final pitfall goes to Paul himself and to his skill as a diplomat. This was most appropriate since Paul’s diplomatic skill had served the MBI so well so many times earlier. This last chapter of the story arose because Chancellor Carnesale reneged on his promise to rename the building in honor of Paul. A few weeks before the date of a ceremony planned around the re-naming, the Chancellor claimed that he had agreed only to the naming of the Molecular Biology Institute in honor of Paul Boyer, not the MBI building.

Needless to say, David Sigman was beside himself, and everyone in the MBI who knew about the situation was furious. However, Paul calmly took the required action: He called the Chancellor’s bluff, and declined the offer to have the Institute named after him. This put the Chancellor in a very awkward situation since invitations to the renaming ceremony—a potential opportunity to
raise donations—had already been mailed out and several acceptances received! After hearing about Paul’s refusal to give the MBI the awkward name of *The Paul D. Boyer Institute of Molecular Biology*, Carnesale quickly agreed to change the name of the building instead, claiming that the whole situation had been a misunderstanding. (At least this is how I saw it. Others would no doubt have an alternative view of things, an aspect of human nature beautifully portrayed in Akira Kurosawa’s film *Rashomon*.)

My goal for the MBI during my tenure was to develop it into a conduit for communication between all molecular biologists in all departments at UCLA and affiliated hospitals. I was happy to support the tradition of the “free lunch” that Dick Dickerson began in order to increase attendance at our MBI Tuesday Faculty Noon Seminar series. My motivation was selfish since I love to hear these talks by my colleagues. I have done a number of collaborations and taken new directions in my research as a result of hearing these talks that cover the broad spectrum of biological sciences at UCLA. Consequently, it was particularly poignant to me that during struggles between academic units for limited resources, the MBI has been criticized for being little more than a “free lunch.” To me, these free lunches are one of the most important aspects of the MBI.

My second greatest accomplishment as MBI Director was to establish a committee with representatives from all of the ACCESS Program Affinity Groups to invite speakers for our Thursday 4 PM MBI/ACCESS seminar series. Before that, one faculty member had been asked to organize the seminars for each quarter. As a consequence, the breadth of topics covered was somewhat limited. The seminar committee has allowed experts on campus in each of the areas of the biomedical sciences to invite the most dynamic and significant speakers from the summer meetings they had recently attended. Again, my motivation was very selfish.

My third greatest accomplishment as MBI Director was to turn the job over to Steve Clarke and Sabeeha Merchant as Director and Associate Director. They are doing a terrific job of keeping this valuable UCLA asset going. I can tell you, the job means enduring multiple “up or down” reviews, since the MBI Organized Research Unit is reviewed on a five-year cycle, and the Interdepartmental Graduate Program is reviewed on an eight-year cycle. The Academic Senate Committee handling the last IDP review I was involved in, was horrified when I suggested reviewing the graduate program every five years rather than every eight years so that the two closely-linked entities could be reviewed together. Coordinating the two reviews proved to be beyond the abilities of UCLA administrators. And so, I join with everyone involved in biomedical research at UCLA in thanking Steve and Sabeeha for taking on the good fight.

Paul Boyer understood that an organization such as the MBI is impermanent. Cement and steel are likely to last much longer. Nonetheless, the research done at UCLA that was stimulated and enhanced by the Molecular Biology Institute will continue to have its influence even after the building is gone.
REFERENCES TO CHAPTER 1:


Also recommended: