6-1966

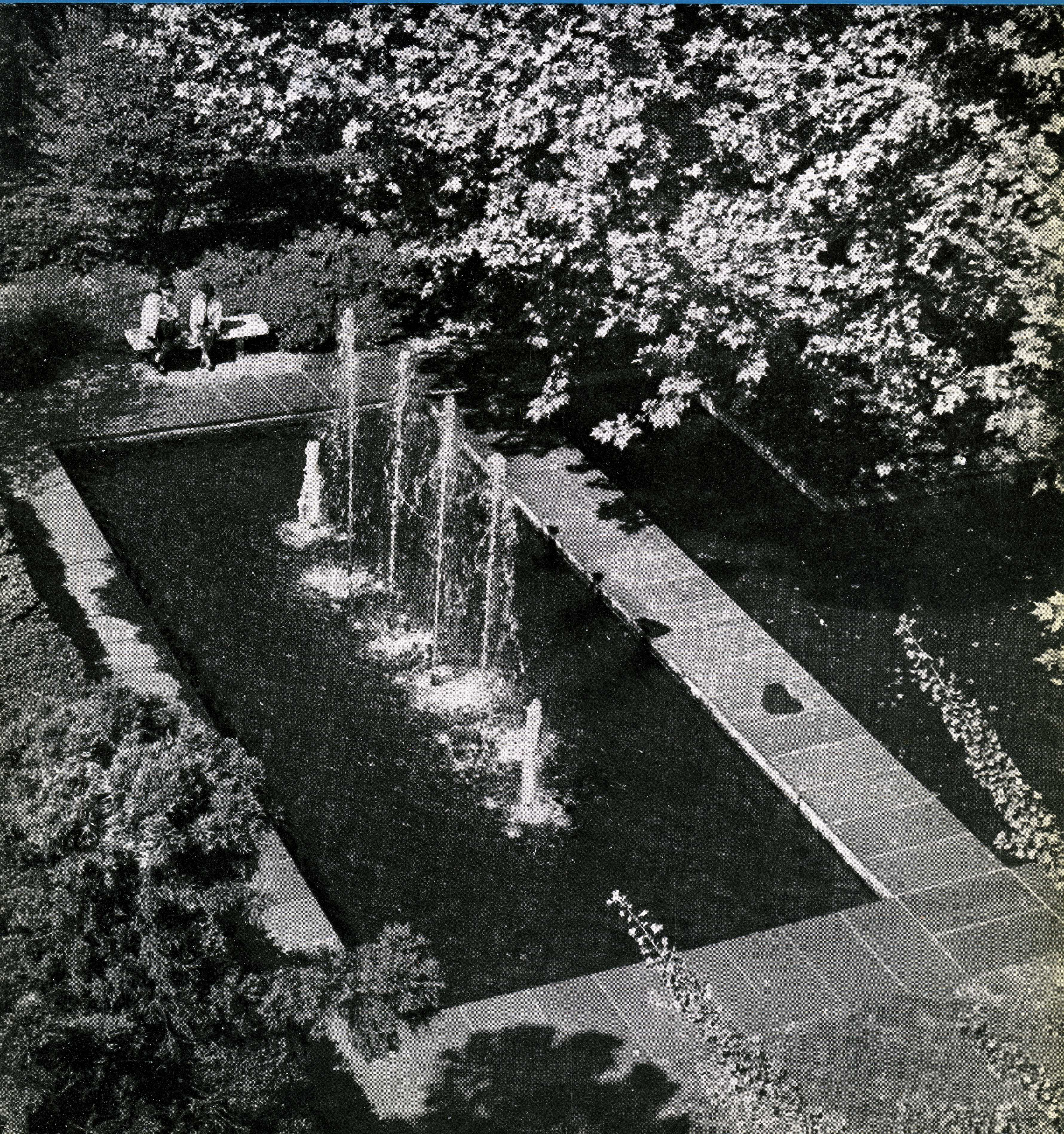
The Rockefeller University Review 1966, vol. 4, no. 3

The Rockefeller University

THE ROCKEFELLER UNIVERSITY

MAY • JUNE 1966

REVIEW



The Rockefeller University Review, May-June, 1966. The Review is issued bimonthly. This is volume 4 number 3. Published by The Rockefeller University, Sixty-sixth Street and York Avenue, New York, N. Y. 10021. Second-class postage paid at New York, New York. The price for a subscription for one year is three dollars, single copies sixty cents.

THE DESIGN OF BOOKS

FUNCTION AND FASHION

BY SIR FRANCIS MEYNELL

Sir Francis Meynell is best known for his scholarly editions of the classics in the tradition of Aldus of Venice and Pickering of London. Nearly every publisher and designer in the English-speaking world today has been encouraged and influenced by his inventive and uncompromising art. Founder and director of the Nonesuch Press, Typographic Advisor to Her Majesty's Stationery Office, Member of the Royal Mint Advisory Committee and of the Advisory Council of the Victoria and Albert Museum — Sir Francis has turned his attention of late to the production of children's books, The Nonesuch Cygnets, which are read with delight by budding bibliophiles here and abroad. Of his "Works of Shakespeare," T. E. Lawrence wrote, "the tact and grace of your editor have been surpassing. I think I like the size and shape and binding almost as well as the text.... Nobody will ever dare to produce the old type of edition now.... It means a permanent improvement in Shakespeare." Sir Francis is the father-in-law of Professor David P. C. Lloyd; the article which follows is adapted from a lecture which Sir Francis gave in Caspary Auditorium during a two-week stay at the University in March.

Printing has been called "ars artium omnium conservatrix." The preserver of all the arts — and dare I say it in this assembly, of the early sciences. Equally it must itself be *con*servative — at any rate in books for reading, not merely for the coffee-table.

For books are written, and are read, in the same way now as they were 500 years ago. They must bow to the same discipline of use. Since the uses of books are common to all the countries using the roman alphabet, the functional part of style will be a common, a general, discipline. Though in the aesthetic detail there can be an infinity of variations, printing in its successive generations can do no more than modify its own modifications. How static, or, as I should prefer to say, how *constant* is its ideology, may be judged from the fact that English printers still put the "new" letters W and J at the end of their alphabets in the compositor's type-case of letters; and have to find in the same type-case an odd empty position among the asterisks and signs for the small k — as though k-less Latin were still our prevailing language. In the words of a poet of another age:

I have heard many people say 'Give me the Ideas; it is no matter what Words you put them into'; and others say 'Give me the Design; it is no matter for the Execution'. These people know enough of Artifice, but Nothing of Art. Ideas cannot be given but in their minutely Appropriate Words, nor can a Design be made without its minutely appropriate Execution.

When William Blake wrote those words he had not in mind specifically the art of printing. His was a wider thesis — as wide as all art. But it is by no accident that it is so singularly appropriate to printing. Blake himself was long preoccupied, dissatisfied, experimental, in the craft which we are here to consider. "Ideas cannot be given but in their minutely Appropriate Words, nor can a Design be made without its minutely appropriate Execution." Here the author, the artist, and the printer alike have a fit expression of their creed.

What does appropriate execution mean in the case of a book? For I am going to talk about books — not so much because they are more important than other pieces of printing, but because we *read* books, and we *look* at most other printed things. Therefore a book *must* be legible, and *should* be "readable" (I will make the distinction later). But it may also deliberately add another to those basic qualities; it may make a claim for itself; it *may* mean the illumination, the enhancement, of the work which it embodies.

I propose to take these three points seriatim.

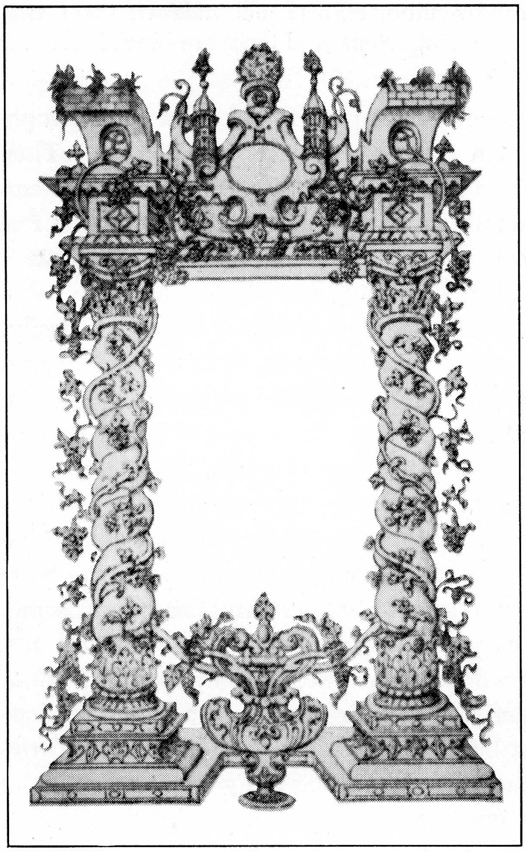
Legibility

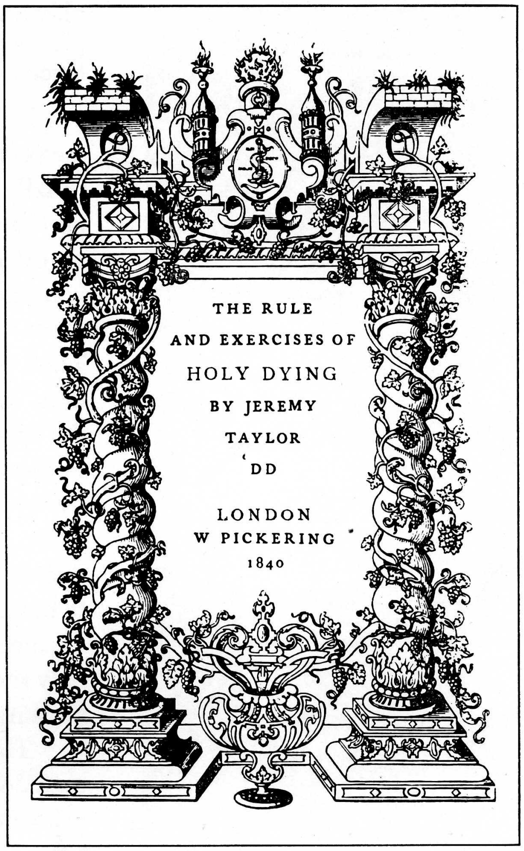
By legibility I mean a proper observance in all its infinite details of that principle of order and convention which is the basis of written communication.

I use the word "convention" deliberately. Let us remember that printing is wholly artificial — an artifact. The alphabet is a set of arbitrary conventions. Words are but meaningless sounds — till we give them sense. Six hundred years ago the sound "bus" meant nothing in English. Two hundred years ago the sound "bus" only meant "kiss." Now a means of transport adds a new meaning to the old sound; or do you, more literately, say omnibus? Three hundred years ago there were no such letter symbols as W or J. The shape of letters and the order of letters, the sound of letters — and the assembly of those letters into words, except for the comparatively few words which are onomatopoeic — are all purely conventional. We have agreed that they shall be thus and thus. It is the most perfect example, this of language and printing, of the capacity of men to unite in a common cause. The individualist for his safety and his sanity's sake could not stand out. Doubtless there were diehards who held to their private property in sound and symbol; but wide and large the men of one country adopted a common convention of sound: and wider and larger still, the men of all western countries adopted a common convention for signifying that sound, and of setting out the signs in a certain order. There is nothing inevitable in our habit of reading from left to right, and from top to bottom of a page: the Chinese have a different rule of the road. But we came to an agreement, a code. Legibility then is observing the code. Legibility is shape, is due precedence, is order, not merely in the forms of letters but in the very shape and organization of the whole book. This legibility is the bare necessity. It stands between us and meaninglessness. It stands between us and chaos.

But first a word of warning. Probably all the points that I am going to make will seem to you to be of the most trifling, details of the most uninteresting kind, fiddling little touches that no one but a printer-pedant would think of. "The Art of Printing" is a fine pretentious phrase; but the Greek for both "art" and "craft" was τεχνη, and until the nineteenth century "art" in English meant much the same as "craft" and "craft" was synonymous with "trade." Printing is the most technical, the most precise, and perhaps one of the most limited and limiting of the arts. It consists of fidgeting with minute pieces of metal, metal which is stubbornly precise in measurement and shape and has to be fitted into certain spaces. It usually seems not an art but a critical game of spillikins in which everything goes wrong.

Much of what is called "art" might well be called arrangement, and type or typographical decoration in the hands of someone who understands how to arrange it will prove as plastic as clay in the hands of a sculptor. The elasticity of type is achieved only by a careful consideration of very small points. Type, in short, has to be humored. First as regards its setting — for some "faces" read best without interlinear space, others with space as deep as the type itself. Next as regards inking and impression on the paper, or the "kiss" as they said in the eighteenth century.





I go back a little over a hundred years, to William Pickering. He called himself the English Disciple of Aldus; and in the Aldine fashion he made books handsome but also handy. He applied to books a classical tradition — like that of the brothers Adam in house decoration. On the left is the pencil drawing made for the border; on the right is the woodcut version as it was printed.

Legibility has other factors. I might be able to prove that the untrained eye is less fussed by one particular series of letters than it would be by another. I might be able to show mathematically that there are plainer distinguishing strokes between letter and letter in one alphabet than in another. A wholly new code, if we were learning to read, might be devised which was better than any we have. But our eyes have already formed their habits; they have been conditioned. The most legible type is not the new code that the laboratory-optician might invent, it is that specimen of the old code, the customary code, which has its essential letter-differences best distinguished. But this is only half the truth. The educated eye does not read letters so much as words. So that our legible type must have this other quality, letter must relate happily to letter to make the *legible* word unit. Now for some of my details.

Consider the serif, the horizontal stroke at the two feet of the capital "M," for example, or at the three feet of the small "m." These serifs occur in al-

HEREBEGINNETH THE TALES OF CANTERBURY AND FIRST THE PROLOGUE THEREOF

WHAN THAT Aprille with his shoures soote
The droghte of March hath perced to the roote,
And bathed every veyne in swich licour,
Of which vertu engendred is the flour;
Whan Zephirus eek with his swete breeth
Inspired hath in every holt and heeth
The tendre croppes, and the yonge sonne
Hath in the Ram his halfe cours yronne,
And smale foweles maken melodye,
That slepen al the nyght with open eye,
So priketh hem nature in hir corages;
Thanne longen folk to goon on pilgrimages,
And palmeres for to seken straunge strondes,
To ferne halwes, kowthe in sondry londes;
And specially, from every shires ende
Of Engelond, to Caunterbury they wende,
The hooly blisful martir for to seke,
That hem hath holpen whan that they were seeke.

BIFIL that in that seson on a day,
In Southwerk at the Tabard as I lay,
Redy to wenden on my pilgrymage
To Caunterbury with ful devout corage,
At nyght were come into that hostelrye
Wel nyne and twenty in a compaignye,
Of sondry folk, by aventure yfalle
In felaweshipe, and pilgrimes were they alle,
That toward Caunterbury wolden ryde.

William Morris fathered the English revival of printing, but that revival did not take the form, as he insisted it must, of a reversion to so-called handwork: it came from a conversion to better uses of the machine he detested. It was a Pyrrhic defeat. Even his own greatest work, this "Chaucer," harbors a partial defeat; it is magnificent, but it is not a book, if a book is something to read.

most all the letters of a normal alphabet. But lately reformers, and still more lately stuntsmen, have brought into dominant vogue a series of "sans-serif" types, or block letters.

In an advertisement headline or any text with very few words, the very crudeness of these letters will shout effectively. But not in continuous reading. For the serif is the connection between one letter and another, and so helps to make letters into words.

My admired American friend, the late Tom Cleland, once wrote:

Laymen call a certain style of letter 'block letters'... they simplify the traditional forms of type as you might simplify a man by cutting off his hands and feet. This is simplification for simpletons, and these are block letters for blockheads.

I have said nothing so far of what most people mean when they say a "nice legible type." They usually mean largeness of type. But just as type can be too small for reading, so it can be too large. For an extreme instance, a large poster is not legible if you stand at book-distance (that is half-arm's distance) from it. The type is too big to be read easily. Thus, if the word is the unit, then the "compound unit," to use a rather higgledy-piggledy term, is not the word but the line. There should be from eight to eleven words in a line of prose. If there are less, the eye will have to stop, turn, drop, and restart too often. If there are more, the eye will not take in the line as its "eyeful," it will pause part way, will have to balance to keep its bearing. And this is wearisome. In summary, not only the letter but the word, not only the word but the line, not only the line but the page itself, nay, the sequence of pages, must be legible. The book-designer who realizes that, and the craftsman who can fulfil his design, have obeyed the primary compulsion and at least the first law of beauty, the compulsion of fitness for use.

To continue with legibility: not the printed word merely but the paper on which it is printed may make or mar legibility. Here my first instance is very obvious. A dirty-coloured or greyish paper may assimilate the colour of the type too much. Or a so-called "art," or coated, paper may have so high a "shine" on it, and be so dazzlingly white as to make the paper more resplendent than the ink which is applied to it. Or a "laid" paper, that is, one with a sort of water-mark of parallel lines, may have so insistent a pattern of these ribs that they contest with the printing.

Everyone knows how in bad printing the ink fills up the little loop of the "e," or fails to print with an even degree of blackness from page to page. But bad presswork has more subtle faults than this. The type must not be so heavily impressed on the paper as to make the book a braille book. But it should not be wholly impressionless as it is with the photographic processes by which not a few books are printed today — a process which avoids the contact of metal types with paper. Remember that the classical and still standard letter faces, like Caslon, Baskerville, and

Bembo, are designed and cut with thinner strokes than they are meant to show; the designer has calculated on the spread of the paper under pressure.

In the addition of many new sizes of one design, modern procedure has not always been vigilant or conscientious. The practice has been to cut one series of designs pantographically in perhaps four different sizes. But this automatic reduction or enlargement must alter the character of the letter, and the balance of its thick and thin strokes. The eye demands a minimum thickness of the thin strokes and of the closed loops, and the pantographic reproduction, satisfactory in one size, may in another fall below those limits.

In designing the first Nonesuch Shakespeare I learned the importance of this point.

The type which, for various reasons irrelevant to this argument, I had decided to use has peculiarly large capitals. My text was the First Folio, with its prolific use of capitals. Every noun begins with a capital letter. When I saw the first trial pages I was dismayed to see a page studded with these letters. What in a page with a modern scarcity of capitals would have been pleasant and effective was, in this peculiar case, patchy and interruptive.

I therefore had all the capital letters cut over again. The height was reduced from supernormal to subnormal, pantographically. And unexpectedly they still "jumped" from the page. Whereas at first they were noticeable because they were too tall, now they were noticeable because they had become too narrow. It was necessary therefore to make new designs, not mere proportionate reductions of the old. The capitals were then properly harmonized with the small letters, and so true to the "colour" of the page.

My last demand for legibility will be this: that the spacing between words should be equal, and close. This demands patient skill on the part of the compositor, and intelligence on the part of the book-designer. You cannot set a large type to a narrow measure without involving excessive spaces between words *or* a constant splitting of words at the end of a line. Again, the careful compositor will make sure that there are not what he calls "rivers" in his page — the *over* spacing between words, almost one above the other in successive lines, which combines to make a streak of white down the page. Hold the page almost horizontal and they manifest themselves. It happened that the edition of *Don Quixote* which I designed for the Nonesuch Press was set up in my absence. When I had all the proofs before me I was appalled by these rivers. The whole of the 900 pages

"Adventures of the Black Girl in Her Search for God," by George Bernard Shaw, illustrated by John Farleigh, 1931: G.B.S., *protesting many times that he left the whole matter of this book's design — cover, endpapers, illustrations, text arrangement — to the illustrator, in fact scrutinized every detail, and had to be well-satisfied with them all. Many of the engravings have a vivid Shaw scribble as their inspiration. In nearly all he modified some detail: "In the garden-gate picture, if Voltaire had a small implement — a trowel or snipper or something — in his left hand, slightly raised, it would give him a perfect air of being taken by surprise in the act of gardening by the Black Girl's call. As it is, he looks as if they were old friends and had been talking there for years. Stage management again."* TOP *early stage of illustration.* BELOW *revised version after Shaw's comment.*

had to be over-run, line into line, to take up the slack of these excessive whites. These "rivers" are not merely unsightly; they catch the eye into their current.

This, then, is legibility in its more obvious forms. What of "readableness"?

Readableness

By this I want to convey that something more and that something less. The readable book invites you. It has charm, it has sympathy. It is co-ordinated. Its materials are related, type to paper, paper to size of book, size of book to subject. The readable book is the well-dressed woman. Richness of clothing is not the test, but fitness for the occasion. The book itself has many component parts, and these must accord. Title page, contents, chapters, notes, index, prefaces, and appendices – all these must have a closer relation with each other than the mere use of the same or harmonious types.

I have a debatable opinion to express here. Readableness may on a rare occasion be *more important* than easy legibility. I often print poems in italics, not only because they have a lighter and perhaps a more poetical and ornamental allusion or suggestiveness but also because I find I read poetry too quickly, and the very slight difficulty I have in reading a line of italics slows me up beneficently. I am sure that William Morris realized that his prose romances read better in his own type faces than they would in reprints, because his undoubtedly difficult letter-forms give something of the slow rhythm, something of the decoration, of his prose. Readableness does not require "period" printing, a stale and barren habit in any art or craft. But it does involve allusive, or sensitive, design of the whole book.

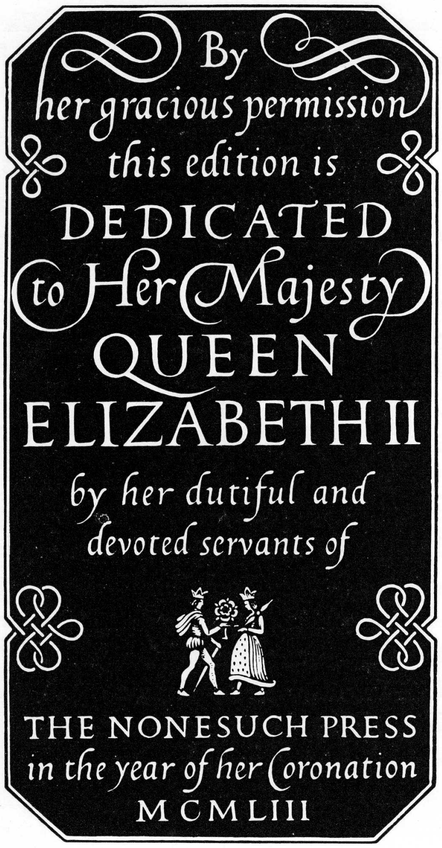
I must content myself with two examples only. The first shall be drawn not from printing but from unprinting, from the margins of a book. For paper also is part of the picture. Margins are not ornaments only, nor extravagances only, nor habits only. They have this purpose to serve: to "set off" the printed page, to divide it from the rest of the visible world. They are what a mount is to an etching, or a frame to a painting. And if you will, as you should, regard not a page but an "opening," i.e. two facing pages, as your picture unit, you will find that the habitual proportions of the book-margins conform closely to the picture-mounter's convention. But the printer has comfort as well as proportion to justify him. He makes his margins of a kind to allow ample room for a thumb-hold at the outsides or the bottom of the pages.

The contemporary school of miscellaneous printing in England – I don't know if this is true in your country – has chosen to rebel against this functional arrangement of margins. So it puts the greatest margin at the head, the smallest at the foot, and a larger margin in the "gutter" or inner side than on the "foredge" or outer side. Rebellions are healthy, but by their nature they cannot last. If they lose they are overtaken; if they win they become the Establishment – they are rebelled against. But at the moment much commercial printings in England, and a few books even, are so margined that, if one holds them to read, one's thumbs must obliterate the words at the bottom or the outer side of the pages. This obsession will not last long. Jan Tschichold, the great Swiss typographer, once the High Priest of the deformities, has recanted and found the old salvation.

Now I give an example of the necessity for coordination between the message and its mannerly reproduction according to polite expectation.

Mr. Dwiggins, the eminent American designer, once published a monograph on American dollar bills, postage stamps, and so on. He made a devastating argument against the designs in use. He showed what he called "spinach" everywhere. Spinach round the President's head; spinach round the figure of Columbia; spinach round the dollar sign. His case was overwhelming, until he came to show his alternative suggestions. The design for the five-dollar bill was charming, no spinach anywhere. But it did not look like a five-dollar bill, or like currency of any kind: it looked like the packet-wrapper of an "artistic" face-cream manufacturer.

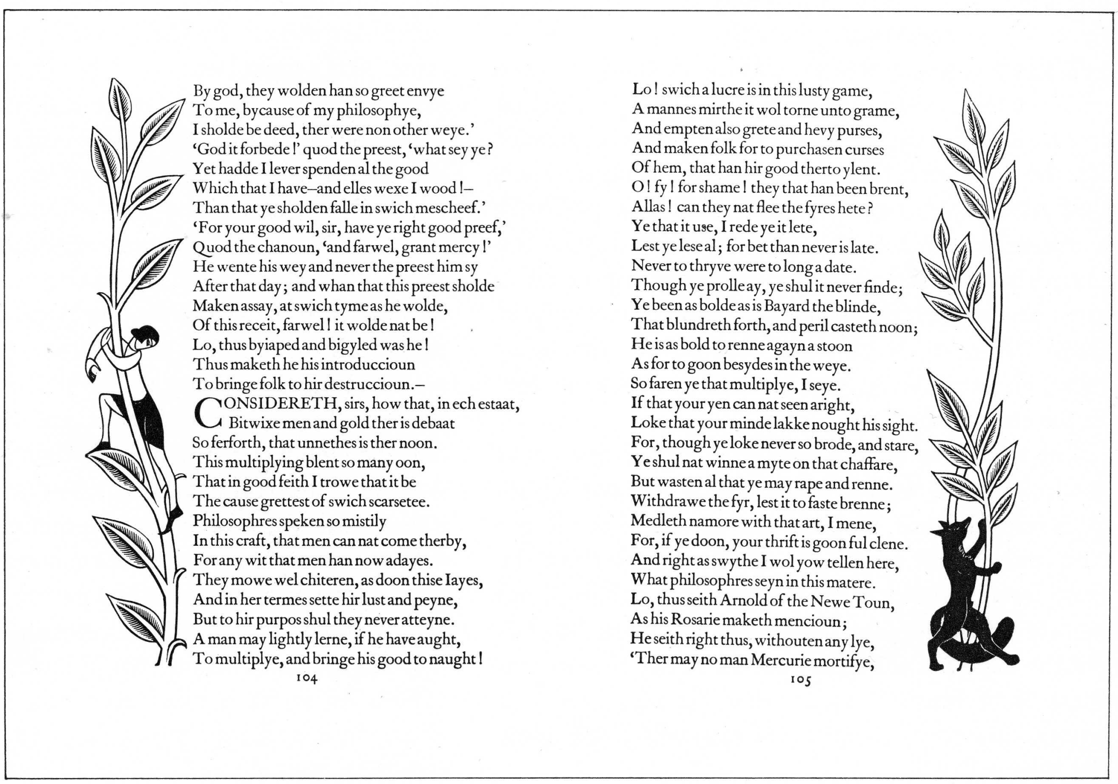
It had the wrong connotation. It was legible but not readable; it offended custom, and therefore confidence. You may say that the public habit should be changed. I only partly agree. Within the accepted mode a thing may be better or worse, according to our lights; within that mode everywhere, whatever it is, much is to be done. But to change all modes to one is to make life dull and characterless. Specialized and peculiar modes are aids to meaning, and give the pleasure of diversity.



By
her gracious permission
this edition is
DEDICATED
to Her Majesty
QUEEN
ELIZABETH II
by her dutiful and
devoted servants of

THE NONESUCH PRESS
in the year of her Coronation
MCMLIII

LEFT *The new "Nonesuch Shakespeare," 1953, dedication page: a wood engraving by Reynolds Stone. Why did I design a second Nonesuch Shakespeare? It was mainly a sociological opportunity or even compulsion. The first, in seven quite thick volumes, was issued in the days when people lived in larger apartments and houses, with plenty of bookshelf space. Thirty years later the scene has changed. Small apartments, narrow bookshelves; the people so conditioned should, I thought, have books so conditioned — but no less carefully designed. The overall dimensions of the first edition are 740 cubic inches; of the second 208 cubic inches.* BELOW *"The Canterbury Tales," with wood engravings by Eric Gill, Golden Cockerel Press, 1928-31: this seems to me one of the best-executed books of our time. The Caslon type has never looked younger; sparely beautiful, and with no mannerisms, it is shown here with benefit of perfect composition, perfect presswork, and perfect paper. Eric Gill's engravings serve their decorative purpose and keep their place in the margins; a condiment which you may desire but also may wish to leave on the side of your plate.*



By god, they wolden han so greet envye
To me, bycause of my philosophye,
I sholde be deed, ther were non other weye.'
'God it forbede!' quod the preest, 'what sey ye?
Yet hadde I lever spenden al the good
Which that I have–and elles wexe I wood!–
Than that ye sholden falle in swich mescheef.'
'For your good wil, sir, have ye right good preef,'
Quod the chanoun, 'and farwel, grant mercy!'
He wente his wey and never the preest him sy
After that day; and whan that this preest sholde
Maken assay, at swich tyme as he wolde,
Of this receit, farwel! it wolde nat be!
Lo, thus byiaped and bigyled was he!
Thus maketh he his introduccioun
To bringe folk to hir destruccioun.–
CONSIDERETH, sirs, how that, in ech estaat,
Bitwixe men and gold ther is debaat
So ferforth, that unnethes is ther noon.
This multiplying blent so many oon,
That in good feith I trowe that it be
The cause grettest of swich scarsetee.
Philosophres speken so mistily
In this craft, that men can nat come therby,
For any wit that men han now adayes.
They mowe wel chiteren, as doon thise Iayes,
And in her termes sette hir lust and peyne,
But to hir purpos shul they never atteyne.
A man may lightly lerne, if he have aught,
To multiplye, and bringe his good to naught!

104

Lo! swich a lucre is in this lusty game,
A mannes mirthe it wol torne unto grame,
And empten also grete and hevy purses,
And maken folk for to purchasen curses
Of hem, that han hir good therto ylent.
O! fy! for shame! they that han been brent,
Allas! can they nat flee the fyres hete?
Ye that it use, I rede ye it lete,
Lest ye lese al; for bet than never is late.
Never to thryve were to long a date.
Though ye prolle ay, ye shul it never finde;
Ye been as bolde as is Bayard the blinde,
That blundreth forth, and peril casteth noon;
He is as bold to renne agayn a stoon
As for to goon besydes in the weye.
So faren ye that multiplye, I seye.
If that your yen can nat seen aright,
Loke that your minde lakke nought his sight.
For, though ye loke never so brode, and stare,
Ye shul nat winne a myte on that chaffare,
But wasten al that ye may rape and renne.
Withdrawe the fyr, lest it to faste brenne;
Medleth namore with that art, I mene,
For, if ye doon, your thrift is goon ful clene.
And right as swythe I wol yow tellen here,
What philosophres seyn in this matere.
Lo, thus seith Arnold of the Newe Toun,
As his Rosarie maketh mencioun;
He seith right thus, withouten any lye,
'Ther may no man Mercurie mortifye,

105

I have talked about *legibility,* which is a bare necessity, and about *readableness,* which is good manners, invitation, overtone. The one for the eye; the other for the mind's eye. These will produce a legible book and a readable book, but they will not necessarily produce a beautiful book.

Illumination

And now for the much more, or rather, the very different — the inessential but greatly valuable: the *illumination* by printer or illustrator of the author's point. It seems to me that the decorator of a book, be he printer or artist-illustrator, has to approach his work as if he were an annotator, an editor. He must not, like a bad editor, abridge his author's meaning; he must not, like a pompous editor, write his notes so long that the text is obscured by his interventions; he must not, like an inefficient translator, alter his author's values. But if he can see his author's point, and even a trifle beyond it; if he can, by his choice and disposition of type, throw light and yet conceal his own lamp; if he can hang his curtains and group his actors, his letters, as does the invisible master-producer of a play, so that the dramatic significance is the clearer for his effects; if he can be magnificent and humble, evocative and silent at the same time; then he is making beauty as well as conveying it.

There was a time when the aesthetic teachers gave us the useful word "functionalism." They asked us to consider the use to which house or chair or book is to be put. More, they asked us to concentrate upon that use, to ignore everything else. The perfectly comfortable chair, they said, must inevitably be the perfectly beautiful chair; the perfectly legible book must be the perfectly beautiful book. It was our old friend "Fitness for purpose," but it was something more, and something less; more in the doctrine, less in the end-product.

But what was all the bother about? If our backs are to be fitted with suitable chairs, and our bodies with suitable beds, may not our eyes, and our finger tips, and all our apparatus of sensory pleasure, be considered as well? Is not the giving of delight, from ecstasy down to plain pleasure, itself a function? Is not the maker of this pleasure, the artist, the decorator, a "functionalist" no less than the plumber? It is a poor estimate of the needs of man which can allow the answer "no."

In books it has been customary to think of the qualities of high craftsmanship — readableness and illumination — as belonging only to what is called hand-printing. I wish to dispute both the thesis and the expression. There is not now, there never has been, any such thing as hand-printing. It is a contradiction in terms. When Gutenberg first cast letters he became the first mechanical mass-production man. He invented movable types to make more, cheaper, and quicker copies of precious works which until then could only be duplicated in manuscript at infinite labour. This is the typical object of machine production. His types were parts of a machine, his simple wooden press *was* the machine. All we have done is to elaborate. Gutenberg set by hand-power his mechanical letters — themselves made by what may fairly be called elementary machines, for tools like punches and matrices are at least hand-shakes with the machine. He printed his sheet by a machine of which the lever was worked by man. We print ours by a machine of which the levers or their substitute are worked by plain electricity or tape or computer. All are machines.

I want to hammer away at this because it disposes of so much nonsense about the art of hand-printing. Some people still insist on thinking of hand-work as something essentially "different." It is essentially the same. And a great book — great from the typographic point of view — may be produced more easily and more cheaply by the more elaborate mechanism of our day than by the less efficient mechanism of yesterday. No printer of the past had such rich materials to work with, or materials so controllable. How many types had Aldus? Not half-a-dozen. And William Morris? Half half-a-dozen. Our papers, our inks, our presswork are all richer in the same proportion. True, we also possess, we use, we must for most purposes use, less than our best of paper and ink. But a well-designed letter is no more costly than an ill-designed. And there are still occasions when paper and ink of a true quality can be used with modern machines to achieve beauty and moderate economy. That realization indeed was the guide-line for my own Nonesuch Press and one reason for its success. Limited editions had always been "hand" work, limited to 50 to 100 copies. Mine were machine work, *controlled* machine work, and the limit perhaps 1200 copies. That meant an appeal to a larger public at a lower price.

The Anatomical
Exercises of Dr. William
HARVEY
De Motu Cordis 1628:
De Circulatione Sanguinis 1649:
The first English text of 1653
now newly edited by
Geoffrey Keynes

Issued on the occasion of
the tercentenary celebration of the first
publication of the text of De Motu Cordis
THE NONESUCH PRESS
LONDON

One of the few Nonesuch Press books of direct scientific interest. Like its 1628 Latin predecessor it was printed in Holland and with seventeenth-century types—"allusive typography." The text is importantly revised by the collation of the English translations of 1653 and 1673.

CONSERVATRIX SCIENTIAE

There are occasions when eager or anxious editors make fresh editions out of little more than a newly discovered comma or a fresh interpretation of a parenthesis. But every now and then they discover gaps—wide gaps, strange gaps—in significant works, where pleasure and use and propriety alike require an amendment. This is true even of works which have a high and continued scientific interest. Sir Geoffrey Keynes, equally eminent as surgeon and editor, found such a gap in the treatment of Harvey's two treatises on the circulation of the blood. In more than a score of instances he found mistranslations of the original Latin in the English versions of 1653 and 1673—these in addition to many printer's errors. Here was the need for the Nonesuch *De Motu Cordis* of 1928. It restored also a much-improved version of the illustration which appeared in the Latin edition, strangely omitted from the English translations although references to it were retained. The purpose of the illustration was to show the function of the valves in the veins. Keynes practised at St. Bartholomew's Hospital, where Harvey made his discovery; and it was here, under the guidance of Keynes, that Stephen Gooden made the new drawing. Gaps in editions can be failures of assembly. To correct such a failure was the purpose of the Nonesuch edition of *The Writings of Gilbert White of Selborne.* On common ground with other editions in printing in full "The Natural History of Selborne," it went far beyond them in demonstrating the versatility of White's genius; for it gives much of "The Antiquities of Selborne," six years of "The Naturalist's Journal," and a selection of White's letters. Thus the features of this great man were assembled into a living portrait.

Science is not always solemn. In the 17th century it could be gay and gallant. Women, particularly if they were noble and beautiful, could be granted to have eager though untutored minds. There is no better example than Fontenelle's *The Plurality of Worlds,* which in its manner was almost a flirtation, though its purpose and theme adventured, three hundred years ago, into our space age. In the edition which I designed I made much use of typographical signs of the zodiac—an example, this, of allusive typography.

Lately it has become the habit of the publisher, not the printer, to design his books. This is held by some to be a retrograde step. In a sense that they do not mean — in a purely historic, not an aesthetic sense — this is of course true. The first printers were publishers. The first publishers were printers. That could be said not merely of the first but of many generations of book printers. Perhaps the division of functions was a misfortune, and it would have been better for printer and publisher to remain one. Perhaps. But the design of his wares is essentially a responsibility of the first instigator and ultimate seller of a book, the publisher. That he should today assume that responsibility seems to me to be altogether healthy.

"With 24 soldiers of lead I have conquered the world." That is the resounding claim made anonymously for the printer some three hundred years ago. Its date is to be surmised by the 24 soldiers against our 26 letters of the alphabet . . . as I have already reminded you, there was no W, no J 300 years ago. Perhaps the only other significant way in which types have changed is in the development of punctuation marks. Gutenberg had only one, an upright line meaning a full-stop. Until the end of the 18th century there were no quotation marks: the 1611 King

224 HENRY THE FOURTH · PART I

But yet a woman: and for secrecie,
I well Q1-3 No Lady closer. For I will beleeve
Thou wilt not utter what thou do'st not know,
will Q1-6 And so farre wilt I trust thee, gentle Kate.
La. How so farre?
Hot. Not an inch further. But harke you *Kate*,
Whither I go, thither shall you go too:
set forward, Q6 To day will I set forth, to morrow you.
Will this content you *Kate*?
La. It must of force. *Exeunt*

Scena Quarta.

(A tavern in East-cheape.)

Enter Prince and Poines.

Prin. Ned, prethee come out of that fat roome, & lend me thy hand to laugh a little.

Poines. Where hast bene *Hall*?

Prin. With three or foure Logger-heads, amongst 3. or fourescore Hogsheads. I have sounded the verie base string of humility. Sirra, I am sworn brother to a leash of Drawers, and can call them by their names, as *Tom*, *Dicke*, and *Francis*. They take it already upon their confidence, that though I be but Prince of Wales, yet I am the King of Curtesie: telling me flatly I am no proud Jack like *Falstaffe*, but a Corinthian, a lad of mettle, a good boy, and when I am King of England, I shall command al the good Laddes in East-cheape. They call drinking deepe, dying Scarlet; and when you breath in your watering, then they cry hem, and bid you play it off. To conclude, I am so good a proficient in one quarter of an houre, that I can drinke with any Tinker in his owne Language during my life. I tell thee *Ned*, thou has lost much

them all by their Q1-6 christen names, Q1-4 Christian names, Q5,6
their salvation, that Q1-6 [but] Q6
and tell me Q1-6
not proud Q4-6
boy (by the Lord so they call me) and when Q1-6
[then] Q1-6
I will tell Q6

ACT II. SCENE IV 225

honor, that thou wer't not with me in this action: but sweet *Ned*, to sweeten which name of *Ned*, I give thee this peniworth of Sugar, clapt even now into my hand by an under Skinker, one that never spake other English in his life, then *Eight shillings and six pence*, and, *You are welcome*: with this shril addition, *Anon, Anon sir, Score a Pint of Bastard in the Halfe Moone*, or so. But *Ned*, to drive away time till *Falstaffe* come, I prythee doe thou stand in some by-roome, while I question my puny Drawer, to what end hee gave me the Sugar, and do never leave calling *Francis*, that his Tale to me may be nothing but, Anon: step aside, and Ile shew thee a President.

away the time Q1-3
do thou never Q1-3
a present. Q1-6

Poines. Francis.

Prin. Thou art perfect.

Poin. Francis. (Prin.) Frances. Q1-3 (*Exit Poines.*)

Enter Drawer. [Enter Drawer.] Q6

Fran. Anon, anon sir; looke downe into the Pomgarnet, *Ralfe*. Pomgranet, Q6

Prince. Come hither *Francis*.

Fran. My Lord.

Prin. How long hast thou to serve, Francis?

Fran. Forsooth five yeares, and as much as to——

Poin. Francis. (*Within.*)

Fran. Anon, anon sir.

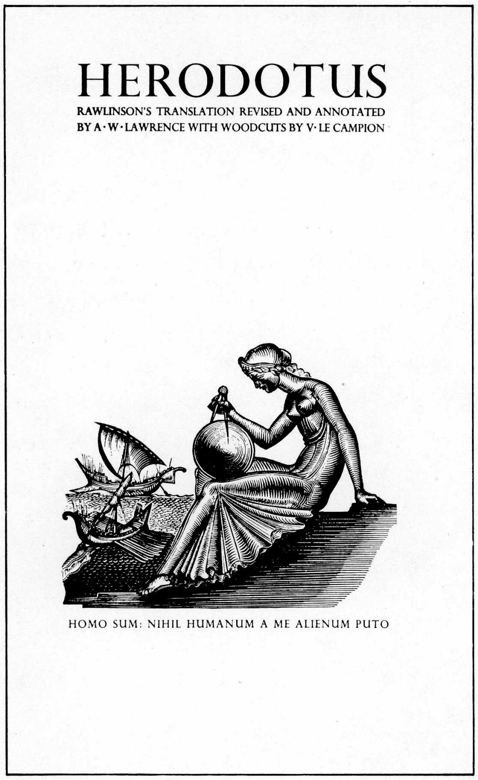
Prin. Five yeares: Berlady a long Lease for the clinking of Pewter. But Francis, darest thou be so valiant, as to play the coward with thy Indenture, & shew it a faire paire of heeles, and run from it? yeare, Q1,2 chincking Q6

Fran. O Lord sir, Ile be sworne upon all the Books in England, I could finde in my heart. [the] Q4,5 *heart—*

Poin. Francis. (*Within.*)

N III 15

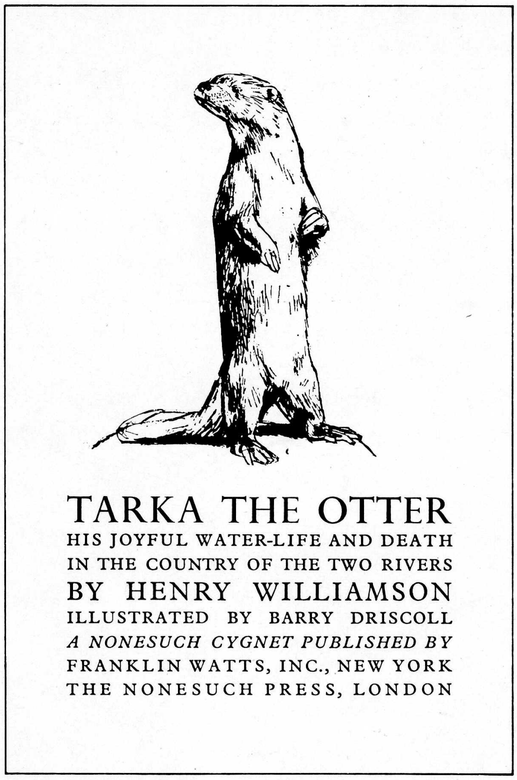
"The Works of Shakespeare," Nonesuch Press, 1929-33: Shakespeare became without intention part-designer of all editions of his works when he decided to write in iambic pentameters. This verse form set a length of line averaging eight words, with sometimes nine and seldom seven. From this beginning the book designer proceeds to choose a size of type which will give a line neither clumsy nor narrowly crowded; a height of page in proportion; and a character of type without oddity and legible even in the minute size needed for marginal notes.



HERODOTUS

RAWLINSON'S TRANSLATION REVISED AND ANNOTATED
BY A·W·LAWRENCE WITH WOODCUTS BY V·LE CAMPION

HOMO SUM: NIHIL HUMANUM A ME ALIENUM PUTO



TARKA THE OTTER
HIS JOYFUL WATER-LIFE AND DEATH
IN THE COUNTRY OF THE TWO RIVERS
BY HENRY WILLIAMSON
ILLUSTRATED BY BARRY DRISCOLL
A NONESUCH CYGNET PUBLISHED BY
FRANKLIN WATTS, INC., NEW YORK
THE NONESUCH PRESS, LONDON

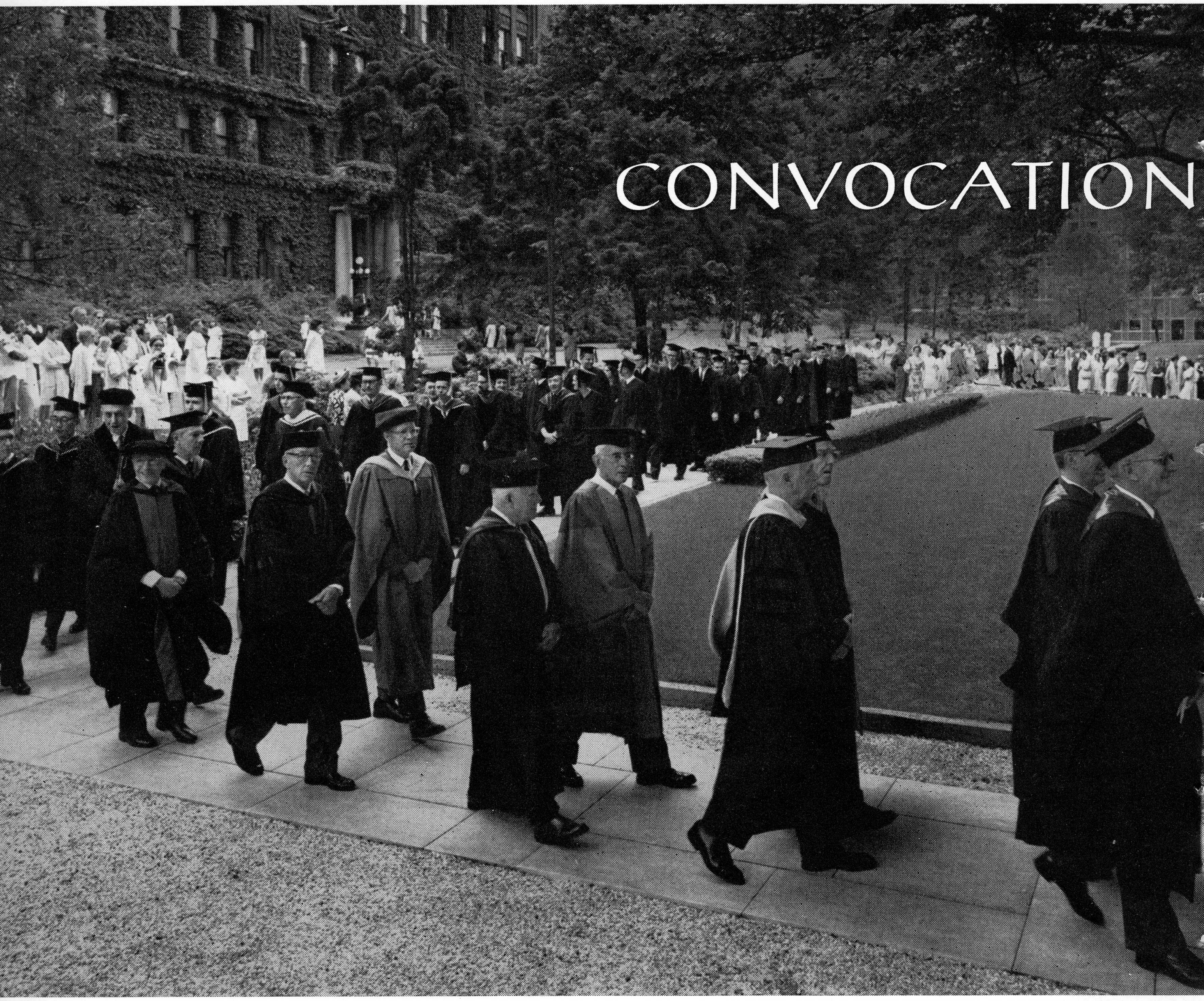
LEFT *Of all the many editions of this great work published in many languages, this is the most completely annotated – no less than 170,000 words – in the light of archaeological, topographical, anthropological, and historical discoveries. It was published by the Nonesuch Press in 1935, more than 2400 years after the birth of Herodotus.* RIGHT *This design aims to combine the normal symmetry of a title page with the excitement of the book's subject matter; one of a new series of "unlimited" editions of books for young people. The "Tarka" page measures 6¼ x 9¼ inches, the "Herodotus" page 7 x 11½ inches.*

James Bible has not one. It is fascinating to me to see that the vastly popular French magazine *Paris-Match* has reverted unknowingly to Gutenberg's single upright stroke to punctuate its headlines.

When one talks in general terms one is bound to be unevocative and (as you will have discovered) dull. I would like to narrow the statement to a very few individual books, and so try to make it more apparent. The accompanying illustrations, let me avow it now, are a harum-scarum lot that I happen to have. They are not representative. They have nothing of the greatest – I repeat, the greatest – of book artificers, your own Bruce Rogers.

They communicate nothing of Elmer Adler and Rockwell Kent, whose *Candide* for Random House was one of the pleasures of the nineteen-twenties.

"COMMUNICATION." You will be grateful that so far I spared you this word of our day. But perpend! There was a Professor of Egyptology at Oxford who, like many a don, was wont to use his bicycle. He was seen one day in The High with a flat tire. He was pumping up the front tire when a passer-by pointed out that it was the back tire that was flat. "What," said the Professor, "what, do they not communicate?" Printing is the surest means of communication – of the accurate communication of the scientist's fact and of the poet's imagination. I hope that you will feel that I have not been pumping up the wrong tire.


CONVOCATION

FOR CONFERRING DEGREES

On June 9, 1966 The Rockefeller University at its eighth annual convocation awarded twenty advanced degrees, including the first degrees granted by this young university in the ancient disciplines of mathematics and physics. President Bronk conferred the degree of Doctor of Philosophy on the eighteen graduate fellows whose work is described on the following pages; and the degree of Doctor of Science, honoris causa, *on Donald D. Van Slyke and Eugene L. Opie, in recognition of their scholarly achievements and the noble example they have set for the youthful graduates. The Reverend Reinhold Niebuhr, great philosopher and tolerant leader of social reform, delivered the Invocation and the Benediction.*

Rosemary Faulkner Bachvarova

A.B. RADCLIFFE COLLEGE

PRESENTED BY ALFRED EZRA MIRSKY

Several years ago I was surprised to find in our library Walther Flemming's book entitled *Cell Substance, the Nucleus and Cell Division,* published at Leipzig in 1882. I was delighted to see that it had belonged to Dr. Simon Flexner, who some years later was to organize The Rockefeller Institute. He had probably purchased it when he came to the Hopkins in 1889. Flemming was an outstanding figure in one of the most exciting and creative movements in the history of biology, a movement that reached its height in the 80's and 90's of the last century. Other outstanding figures in this great period were van Beneden, Hertwig, Strasburger, Driesch, Boveri, and Wilson.

For these biologists the study of heredity and development was one science. They discovered that chromosomes carry the materials of heredity and they first perceived that variations in the activities of the hereditary material could explain the development of a complex organism from a fertilized egg.

The end of the first great period of cell biology was marked by a growing separation of investigations on heredity and development—a separation that led to some unfortunately rigid attitudes in genetics and to a relatively uneventful period in embryology.

All this has been completely changed by the fusion of chemistry and biology that has occurred in recent years. Molecular biology has established ever more firmly the hereditary material in chromosomes as the center of operations in a cell, and surely not a rigid autonomous center but one readily responsive to its surroundings. This conception has revived developmental biology as it focuses again on the chromosomes. The problems posed by Flemming, Boveri, and Wilson are now approached on a molecular basis.

Rosemary Faulkner Bachvarova's work has been part of this broad movement. A fertilized frog egg rapidly divides and repeatedly subdivides to form a mass of some 30,000 cells. Then quite suddenly, as one looks through the microscope, the first signs appear that a differentiated organism is being formed. The older workers, of whom I have spoken, had good reason to suppose that at this dramatic moment there is a sudden increase in chromosomal activity. Molecular biologists can now identify the chemical processes that occur in chromosomal activity. Mrs. Bachvarova showed that the chemical events characteristic of chromosomal activity do in fact occur just before the first signs of differentiation are seen under the microscope.

I have described Rosemary Bachvarova's work as part of a broad movement. Broad as it is, a guide was needed, and the guide has been Dr. Eric Davidson. Only three years ago I stood here to present him for a degree. Now he is an Assistant Professor, and an expert guide.

William Edward Bowers

A.B. PRINCETON UNIVERSITY

PRESENTED BY CHRISTIAN RENÉ DE DUVE

William Bowers is my senior by a few months, at least as a member of The Rockefeller University. By the time I joined the University in the beginning of 1962, he had already written a thoughtful essay on lysosomes, a group of cell particles in which I happen to have some interest. This led us to join forces in a venture which, had he but known it at the time, was going to demand from Bill every one of the qualities which go to make a successful scientist and scholar today. First of all, he joined the laboratory at a time when it consisted of a handsome expanse of open space on the seventh floor of the South building. Perhaps one day in his future career, he will find it useful to have experienced at first hand the growing pains of a new research laboratory; but this valuable experience certainly did not facilitate the task that brings him on this podium today. In addition, when we discussed suitable problems for his thesis, Bill unhesitatingly chose the one which I described as most challenging, in spite of my warnings that he was more likely to crack his head on the problem than to solve it. Indeed, he almost cracked his head, but finally decided to crack the problem instead.

Actually, what he set out to do was quite simple and can be described in a few words. First take a few rat spleens and work out accurate and reliable techniques for the assay of about a dozen selected enzymes. Then take a few more spleens and work out a nice method for separating the fragile little organelles present in the spleen cells in about a dozen distinct fractions. Finally, get up bright and early in the morning, collect some more spleens, and just combine the two operations — that is, separate the twelve fractions and then measure each of the twelve enzymes on each of the twelve fractions. Experiments of this sort have been done before by a team of investigators assisted by about half a dozen technicians. How to do it on one's own and still keep healthy, even-tempered, and on good terms with one's wife is Bill Bowers' secret. All it took was patience, tenacity, rigorous accuracy, a meticulous attention to technical details, and endless hours of work.

However, the real challenge came later, when the twelve distributions turned out to be so complex as even to confuse the computer that handled the results. Technical excellence and a laborious disposition did not suffice any more. It needed creative insight, imagination, and a wide background of knowledge to guess at the order behind the apparent chaos and to devise the experiments that finally revealed the underlying pattern — a pattern which, once unraveled, turned out to be, like all scientific truth, simple and beautiful. Bill provided all these attributes and has rightly earned the certificate of responsible and creative scholarship that is being bestowed upon him today. It gives me particular pleasure, President Bronk, to present to you Mr. William Bowers as a worthy recipient of the Degree of Doctor of Philosophy.

Arthur Houghton Burr, Jr.

A.B. HAMILTON COLLEGE

PRESENTED BY DAVID CHARLES MAUZERALL

Arthur Burr has had a good variety of educational experiences. He obtained his bachelor's degree in chemistry, worked in Dr. Hartline's laboratory on vision, explored some photochemistry with me, and did his thesis on oxygen detection and photosynthesis. He also found time for camping, bird watching, and mountain climbing. When Jay couldn't go to the mountains, he brought the mountains here in a series of films. In this way many shared his love and interest in nature.

Jay's thesis concerns the production of oxygen by plants. This is the darkest of all the dimly lit areas of photosynthesis. Oxygen itself is an odd molecule. Somehow, its peculiar chemical binding allows the extreme potential reactivity of oxygen to be dampened. Catalysts are often required for it to react with organic compounds at room temperature. It is in this way that the sea of oxygen around us, instead of

converting us to the ashes that thermodynamics so drearily insists upon, provides for the controlled burning of foods to supply the chemical energy of most forms of life. We exist, it seems, on a succession of such fine accidents. The problem Jay sought to investigate is how plants, deprived of even a sentient soul by some theologies, magnificently reverse this burning, remaking as they do both oxygen and foods with the energy of sunlight.

The first step in this investigation required a sensitive method to determine oxygen. Jay developed a new method based on the light emitted in the reaction of oxygen with a molecule appropriately named luminol. The reaction is a kind of inverse of photosynthesis. The luminometer, as Jay calls the final apparatus, is not only extremely sensitive and specific for oxygen, but it responds linearly to over a millionfold range in changes of oxygen concentration. The extreme sensitivity of the method also makes it susceptible to leaks of the order of a couple of thimblefuls of oxygen per century. Jay's patience and the thoroughness of his work raised the idea from its birth as a vague concept through the troubled times of development and to its maturity as a useful apparatus.

Some years ago, Professor Granick made a large number of mutants of the green alga, Chlorella, for a very productive study of the biosynthesis of chlorophyll. With the luminometer, Arthur has found that many of these mutants have a surprisingly rich and varied photosynthetic ability—surprising, because these brown and yellow mutants hardly look like healthy green Chlorella. In one of these mutants, Jay has been able to show that the photosynthetic apparatus is stable and simply dilutes out at each cell division in the dark where no new photosynthetic apparatus is made. The combination of the sensitivity of the luminometer with the varied pigment and oxygen-evolving properties of the Chlorella mutants, promises to shed light on the darkness which surrounds the dazzling ability of plants to supply us with both foods and the oxygen to consume them.

Jay is leaving the Rockefeller not only with a degree but with a wife. Professor Trager has furnished our group with many valuable things: to Professor Granick, many fruitful discussions; to me, feathers of the Turaco bird; and now to our student, his daughter Carolyn. A more pleasant example of the traditional cooperation at the Rockefeller is hard to find.

As with a newborn child, so with a new method in science, and even more so with a new scientist, we hope for the new things to come and for the changes that will occur. It is with these hopes that I gladly present Arthur Burr to you, Dr. Bronk, for his Doctorate.

Charles Chapman Carter

B.A. POMONA COLLEGE

PRESENTED BY GEORGE EUGENE UHLENBECK

Charles Carter came to the, then, Rockefeller Institute in 1961 after one year of graduate study at the Massachusetts Institute of Technology. I always call him therefore our refugee from M.I.T. He objected to the rigid and prescribed course of study at that famous institution, and I suppose that the freedom of The Rockefeller University seemed like paradise to him. I think he later found out that this freedom is often difficult to bear, and that it requires the development of an internal discipline which may well be the most important lesson our University can give to a student.

Charles Carter started to work with the late Professor Berlin and then continued with me. His dissertation is on the Van der Waals theory of binary mixtures. I will not try to describe this theory! Let me only say, that when I proposed to Charles about two years ago to extend the work on the Van der Waals theory, which I had been doing together with Dr. Mark Kac and Dr. Per Hemmer, to the case of mixtures, I thought that it was an interesting and more or less straightforward problem so that it seemed an ideal topic for a thesis! I was wrong, because it turned out to be much more involved than I expected. Not only were the calculations at least an order of magnitude more complicated, but Charles had to go back to first principles and to think through again the delicate and ticklish question of the relation between thermodynamics and statistical mechanics. He found the proper statistical ensemble for the problem and the corresponding thermodynamical interpretation, bit through the long calculations, and obtained as a dividend a modification of the Ornstein-Zernike theory of the critical opal-

escence which may have interesting experimental consequences. The work is still not finished; in fact, there is still almost enough to do for a second thesis! But Charles has made a clear-cut and valuable contribution, and I hope and I believe that the hard work to get one's ideas straight — and all the factors two and minus signs in place — has helped him to acquire the internal discipline of which I spoke.

It is a pleasure to present Charles Carter to you for the first Ph.D. degree in physics of The Rockefeller University.

Paul Theodore Englund

A.B. HAMILTON COLLEGE

PRESENTED BY TE PIAO KING

When Paul Englund first came to this University, he had intended to pursue studies in the area of cell biology. However, it so happened that during his second year he took up studies with Dr. D. Yphantis on the physical chemistry of macromolecules. This exposure apparently influenced him to change his major field of interest to that of protein chemistry, and subsequently this area became the topic for his doctoral research.

Happily, at that time Dr. Craig had available a sample of a crystalline protein whose chemistry had yet to be extensively investigated. This protein called ficin is a proteolytic enzyme found abundantly in the latex of fig trees. It had been obtained in crystalline form in 1938 by Dr. A. Walti, who at one time had spent three years in the laboratory of the late Dr. P. A. Levene of this institution. In fact, it was Dr. Walti who had given Dr. Craig the crystalline preparation.

Paul soon learned from several exploratory experiments with the crystalline material, that a pure ficin could be isolated readily from the crude latex. This was an important step forward as he no longer had to depend on the valuable crystalline sample, which was obtained by Walti only after considerable trouble.

Paul then made several precise chemical studies of this pure ficin, and as a result of his persistent efforts, most of the molecular parameters of this enzyme are clearly established. Thus, a solid foundation is now laid for its use as another model to build an understanding of that intriguing question central to biochemistry, namely, how a protein functions as a catalyst. Indeed, he has performed many experiments expressly with this objective in mind.

Through these studies, Paul has succeeded not only in gathering knowledge for his own training, but has also made a contribution to our growing store of information. Thus, the age-old magic formula for the advancement of knowledge through continuous and persistent learning and re-learning, is being repeated, and is amply demonstrated today on this platform. Both Dr. Craig and I, as his research advisors, are personally pleased to see that Paul has now in his possession much of that formula. We wish him many more successful applications of it as he goes on this fall to begin new studies in that dynamic branch of biochemistry, namely, nucleic acids. These studies may be finally bringing Paul closer again to his original interest in cell biology.

Rachel Gillett Fruchter

B.A. UNIVERSITY OF OXFORD

PRESENTED BY ARTHUR MEADE CRESTFIELD

Rachel Fruchter came here from England in 1962, newly graduated from Oxford University with a degree in chemistry, and newly married to an American philosophy major and author, Norman Fruchter. Her interest in the variety of interactions that occur between large molecules in living systems led her to the laboratories of William Stein and Stanford Moore in order to learn something of the approaches to protein chemistry and enzyme action that were evolving there. As a beginning project, she undertook to study the chemical properties of a dimer of pancreatic ribonuclease. In this dimer, two molecules of the enzyme have combined without impairment of the enzymic activity. She explored the reactions of the dimer with a reagent that has an affinity for the active site of the enzyme. That study was demanding of time and patience, and she had ample opportunity to display the tenacity which is so important for a good investigator. As is commonplace in chemical reactions, there was a side reaction. This she chose to evaluate, and in the course of this evaluation happened upon the kind of unexpected discovery which occasionally provides excitement in a laboratory.

In very brief summary, Rachel has obtained convincing chemical evidence that — in the interaction which leads to the formation of dimer — two ribonuclease molecules come together in such a way that parts of each of the two active sites are supplied by each monomer unit. Each monomer supplies one of the two histidine residues needed for activity. This fact was established by a study of the inactivation of the dimer which is brought about by reaction with iodoacetic acid.

These findings are of significance in two very different areas of current research — enzymology and genetics. The ribonuclease dimers permit the study of the activity of an enzyme as a function of the orientation of two known amino acid residues. The dimers and their inactive derivatives are of interest to geneticists, for they provide a simple chemical analog to biological complementation — the phenomenon whereby active enzymes are created by the union of inactive monomer units.

Her research led further to the discovery that iodoacetamide, instead of iodoacetic acid, would react with only one of these two histidine residues at the active site of the enzyme. Of the many reagents tried by other workers, none react this specifically. These investigations, therefore, have enriched our knowledge of the unique architecture in the active site of the enzyme, ribonuclease.

Next year, Rachel plans to begin study of a different protein-protein interaction — that between antigen and antibody. She will join the laboratory of Rodney Porter at St. Mary's Hospital Medical School in London. Our best wishes accompany Rachel and Norman in their new enterprises.

President Bronk, it is my pleasure, on behalf of the Faculty of The Rockefeller University, to recommend to you Rachel Rachenda Gillett Fruchter for the award of the degree of Doctor of Philosophy.

James Douglas Jamieson

M.D. THE UNIVERSITY OF BRITISH COLUMBIA

PRESENTED BY GEORGE EMIL PALADE

James Douglas Jamieson came to us soon after graduating as a Doctor of Medicine from the University of British Columbia in Vancouver.

I suppose that he reached our campus still guided by the holistic approach, the preoccupation with a whole living unit that is part of medical education everywhere. He made some reductionist adjustments — since this seemed to be in the spirit of that time —

moved one or two notches down the scale from whole organisms to cells, and joined our department.

From the beginning he showed great skill, broad curiosity, and a willingness to embark in any reasonable project. As a result, his life among us became a chain of interesting and sometimes instructive experiences. Prompted by his curiosity and that of his many friends around the campus, he soon became our local authority in bacterial cytology. Later on he went through the trying experience of working on a project which could not be brought to its expected conclusion because of technical difficulties. Each turn in this first period did not lead to crisis or turmoil. It just produced another paper, a new crosspiece to a ladder that will come to its end sometime in the future. Each turn also added a few electron micrographs to the mural decoration of his laboratory corner.

But a few years ago, he left behind this period of gathering for posterity pieces of still unexplained information, and embarked on a complex project that demanded not only skill but also organization, endurance, and resourcefulness.

He started from a base provided by work previously carried out in our laboratory on the functional cycle of a glandular cell, the exocrine cell of the mammalian pancreas. But advisedly, he shifted from experiments on whole animals to experiments on tissue slices, whose cells, he ascertained, maintain intact their organization and continue to function normally for a few hours *in vitro*. This shift made possible short, well-defined pulse-labeling — with radioactive amino acids — of the proteins synthesized by the pancreatic cells for secretion. With such labeling, and by using in-parallel cell fractionation and autoradiography, he was able precisely to follow the secretory proteins as they moved in time from cell compartment to cell compartment. Here again innovation was needed, for a new fractionation scheme had to be worked out to isolate as many as possible of the suspected compartments. The main problem was to isolate a fraction of smooth microsomes representative of certain elements of the Golgi complex of the cell. He succeeded and established that secretory proteins are in transit through the corresponding elements at a definite time in the secretory cycle.

Cell fractions are notoriously exposed to contamination by other cell components than those assumed to be there. So towards the end of the project, while following the secretory proteins to their final station in the cell — the secretion granules — he ran into such a situation. At the beginning, it appeared as a serious discrepancy between his two sets of experimental data, one derived from cell fractions and the other from autoradiography. Here again, by imaginative departure from usual procedures, he solved the problem and identified the contaminating culprit, which was another element of the Golgi complex known as condensing vacuole. I suppose that this final piece of detective work gave James Jamieson great satisfaction. A good sleuth remains a good sleuth even when he deals mainly with intracellular granules.

The project he carried through led to a clear solution of an outstanding problem in cell biology: it demonstrated the role played by the Golgi complex in secretory cells. Moreover, it opened new and unexpected vistas on vast and varied intracellular transport operations that remain to be explored in the future. Finally, this project perfected the transformation of a promising student into a modern, resourceful, well-trained cell biologist, able to ask meaningful, far-reaching questions and able to use a whole array of techniques to solve them. I believe this is the type of research man present and future cell biology needs.

Therefore, Mr. President, I have the honor of presenting to you James Douglas Jamieson. By his knowledge, his skill, his proved ability as a scientist, he fully deserves the title of Doctor of Philosophy of our University.

Alexander Kessler

B.A. NEW YORK UNIVERSITY

M.D. COLUMBIA UNIVERSITY COLLEGE OF PHYSICIANS AND SURGEONS

PRESENTED BY RENÉ JULES DUBOS

Dr. Alexander Kessler came to us with a distinguished background in clinical medicine, acquired in famous medical schools and hospitals. Furthermore he had published several orthodox papers on the chemistry of collagen, as early as 1959.

Such extensive experience seemingly ruled out the need for additional scientific training. But in the

course of his medical activities, Dr. Kessler had noted that physicians and biologists tend to neglect the population problem or approach it from a narrow point of view. For this reason, he wanted to acquire new kinds of theoretical knowledge and skills, in order to study the complex phenomena of biological integration and disintegration that occur during life in large social groups. He was eager also to develop unorthodox experimental procedures for the study of population dynamics, under conditions of crowding similar to those prevailing in congested cities.

With creative insight, Dr. Kessler devised artificial enclosures in which several strains of mice were allowed to interbreed and to reproduce freely in several types of physical environments. In such environments that could be regarded as "melting pots" of the mouse world, the populations soon became as dense as the human populations that congregate at the corner of Broadway and 42nd Street on New Year's Eve.

Remarkably enough, the animals living permanently in such crowded environments did not suffer from infectious or physiological diseases, as one might have expected. But, unexpectedly and much more interestingly, they displayed a wide range of behavioral abnormalities, ranging from sexual aberrations, to aggressive behavior, or complete social unresponsiveness. In brief, the mixed and crowded mouse populations exhibited a kind of antisocial behavior analogous to that commonly observed in our own cities.

On the basis of this work, Dr. Kessler has been asked to help develop for the World Health Organization in Geneva a research unit concerned with the various aspects of human reproduction. He will bring to WHO a sophisticated knowledge of scientific medicine, a deep concern for the biology of human populations, and great skill in the development of experimental models for the study of sociomedical problems. We wish him success in his esoteric and challenging enterprise. But we hope also that this country will at last create research facilities where his unique experience can be applied to the study of populations – unquestionably the most urgent biological problem of our time.

Mr. President, I take great pride in presenting to you Dr. Alexander Kessler for the degree of Doctor of Philosophy.

Robert Myron Krug

A.B. HARVARD COLLEGE

PRESENTED BY IGOR TAMM

Robert Myron Krug came to us from Harvard where he had distinguished himself as a student of chemistry. At our University, Bob Krug strengthened his knowledge in those disciplines which would enable him to tackle specific chemical mechanisms. It was apparent to his teachers that Bob had an unusual ability to evaluate and understand the more subtle reasoning behind the established principles in such fields as organic chemistry and enzymology. Bob also deepened his knowledge of living systems, especially bacteria and viruses, and came to see the chemical problems in which he was interested in their proper biological frame. Such a frame was provided for Bob by the biology of animal viruses.

Bob used to the fullest the opportunities offered at Rockefeller for the free association of scholars. With the guidance of Doctors Lipmann and Nakamoto he investigated certain aspects of the mechanism of synthesis of cell proteins. At the same time he continued to be interested in the biosynthesis of animal viruses. An exciting event occurred in 1963 which attracted Bob's attention. Peter John Gomatos reported in his thesis lecture that a new kind of genetic material, a double-stranded ribonucleic acid, had been discovered in reovirus, a widely distributed virus of man and animals. The double-stranded ribonucleic acid of reovirus presented new challenges concerning the mechanisms whereby nucleic acids direct the synthesis of proteins and replicate themselves. Bob became Peter's companion in difficult new work while continuing to benefit from the counsel of Doctors Lipmann and Nakamoto.

The basic aim of the new research was to construct a plausible *in vitro* model system for the early events in the biosynthesis of reovirus. The ribonucleic acid from reovirus was used as a template in test tube systems for nucleic acid or protein synthesis. Two results were expected on theoretical grounds: first, that the double-stranded viral ribonucleic acid would not serve directly as a template in protein synthesis, and, second, that a complementary ribonucleic acid would be enzymatically made on the reovirus RNA template. Both these predictions were borne out by Bob's work. In addition, a number of

unexpected, but important findings were made. These were in every sense pioneering studies and represented the first steps toward demonstrating the mechanism whereby the genetic messages encoded in reovirus ribonucleic acid are read off.

The study of enzymatic processes in animal virus infection marks the beginning of a new chapter in animal virology. It is an area of far-reaching importance. Bob Krug is now one of the few investigators who are superbly prepared for significant work in this field. His clear thinking and his energy and tenacity have served him well in the past and I am sure will do so in the future. I am happy to note that Bob's productive association with Peter Gomatos is to continue, now at Sloan-Kettering Institute.

As I look back to the peaks of intense activity in Bob's vigorous career at Rockefeller, I can have only admiration for his charming wife, Marjorie, without whose patience and understanding Bob's work most certainly would have been less enjoyable and rewarding.

Mr. President, it gives me special pleasure to present to you a candidate for the degree of Doctor of Philosophy who was taught by an earlier son of this University.

Alan Roger Latham

B.S. HARVEY MUDD COLLEGE

PRESENTED BY HARRY BARKUS GRAY

Approximately three years ago I was called over from Columbia University by Professor Theodore Shedlovsky and introduced to Alan Latham. At that time Alan had just finished his second year as a Graduate Fellow of this University. His research interests were unusual, so unusual in fact that they did not match at all the interests of the other Graduate Fellows or, for that matter, of the faculty. But, as I quickly found out, they perfectly matched my own interests. It was clear from the very beginning that Alan and I would engage in a cooperative scientific venture. This was particularly fortunate for me because it drew me much closer to the Rockefeller community, and I have made many good friends here.

Alan's field is called physical inorganic chemistry. This is a field which was dead, absolutely dead, in the 1920's and 1930's, but after World War II was revived by the development of powerful new experimental and theoretical techniques — the so-called Renaissance of inorganic chemistry. Today physical inorganic chemistry is one of the most rapidly growing areas of physical science. Alan's special interest is in the electronic structures, the energy levels, and the spectroscopic properties of complex molecules which contain a centrally located transition metal in one of three major structural types. Of these, only two were completely understood when Alan started his work at this University. The third type, the so-called square planar system, was very poorly understood, and it baffled investigators for a number of years. I should confess that I was one of these investigators. Alan chose to study this problem, by using a number of stabilizing chelate ligands which would provide examples of the square planar system over a very wide range of transition metals. In his study he used copper, nickel, palladium, platinum, and gold. You can see this isn't cheap work. Since the different transition metals have slightly different energy levels themselves, they may be used, in a series of analogous molecules, as a probe to bring out the electronic structural secrets of the entire molecule. This basic approach was utilized by Alan Latham in his doctoral work to provide the first truly complete model of the electronic structure of the square planar systems. I found Alan Latham to be a man who knows what he wants, and he goes after it. He has long wanted to teach and do research in Alaska, and presently will be doing just that. At the very least he will be very close to the source of the precious metals. Rockefeller University can be proud of this representative in the field of physical inorganic chemistry, to which he is already an important contributor. President Bronk, it is with the greatest of pleasure that I present to you Alan Roger Latham for the degree of Doctor of Philosophy.

Harvey Franklin Lodish

A.B. KENYON COLLEGE

PRESENTED BY NORTON DAVID ZINDER

A few years ago a new graduate student trained in chemistry and mathematics arrived in my laboratory. He said that he would like to look around for a few days and talk to the people working in the lab. I do

not exactly recall, but I believe we never discussed the possibility of his doing graduate work with me. At that time, the now Dr. Stephen Cooper was busily engaged with his thesis research. Harvey and Steve were soon working together. Steve had finally found someone who could talk and argue in his own league. Harvey just stayed on. I consider his staying one of the wisest decisions that I ever made.

At that time our investigations of the small RNA-containing bacteriophages were just beginning. Since the essence of the thinking in molecular biology is a happy, perhaps naïve, optimism, that by careful dissection of the parts of small organisms the whole can be encompassed — thereby providing model systems for more complicated organisms — this kind of study of the phage suited Harvey's precise mind and cheerful personality. The method of dissection chosen was genetic. Mutants of the phage had been obtained which had lost their ability to grow either on certain bacterial hosts or when the usual infected cultures were shifted to temperatures slightly elevated from the normal. Hopefully, such mutants would scatter in all of the phage's genes which, because of its small size, could number at most five. The philosophy of experimentation was to determine why the mutants would not grow under the special conditions and try to infer the normal processes lacking.

On paper this is simple to do; however, following infection, the bacteria — in addition to making phage — continue to synthesize all of their bacterial components. This necessitated Harvey's developing special techniques to see the phage-specific components in the midst of an enormous mass of bacterial components. Needless to say, he accomplished this and classified the mutants into three fundamental classes. Three genes had become apparent: a gene determining the structure of a viral-synthesizing enzyme, a gene determining the structure of the virus coat protein, and a gene determining the proper assembly of the particle. Not content with classification of these genes, Harvey set out the rules by which they normally function.

Let us instance just one. Imagine a mutant which makes a temperature-sensitive viral RNA polymerase. Obviously at elevated temperatures it cannot make RNA, and since RNA is the template for protein synthesis, none of the normal viral specific elements appear.

However, what if the infection were started at the usual low temperatures and then at later times the temperature raised? What would happen then . . . ? Did the enzyme have a continuing role and just what intermediates in RNA replication would be first to be affected? In such manner each gene was probed and Harvey showed that there is a well-regulated temporal organization of the functioning of these genes. Some components are made early, others late. Some in vast amounts, others in lesser amounts, and it is the proper functioning in time that controls these processes. Ultimately, with the proper functioning of these genes, 20,000 faithful replicas of the infecting particle are produced from each infected bacterium. Great insight has been obtained in our understanding of gene function and interaction.

I would be remiss to mention only Harvey's scientific accomplishments, significant though they are. A laboratory is also in many ways a biological entity. It too requires a well-ordered temporal organization and intermingling of work, ideas, and personalities. In these last years, to the senior people, Harvey was a colleague, always ready to contribute of himself. To the junior people, he was a student and an inspiration. I know that next year we will all miss two sounds on the fourth floor of Theobald Smith Hall . . . the early morning whistling as Harvey comes cheerfully in to start a new day and the thump, thump, thump of a 5-gallon carboy of TCA rolling down the hall as Harvey gets set up for one of his incredibly complicated experiments. Dr. Bronk, it is a great pleasure for me to present to you Mr. H. Lodish for the award of his degree.

Garland Ross Marshall

B.S. CALIFORNIA INSTITUTE OF TECHNOLOGY

PRESENTED BY ROBERT BRUCE MERRIFIELD

Since the time of Wöhler in the early part of the last century a prime objective of the organic chemist has been to synthesize the complex compounds of Nature and in that way to understand them better. The peptides are a class of such naturally occurring compounds which play many important roles in living systems. The recognition of their biological importance served as a special incentive for major advances in their synthesis. Some of the significant early advances were made right here in this institu-

tion, particularly by Max Bergmann and his colleagues, while the most recent contributions have been those of Garland Marshall.

Shortly after entering the University, Garland became interested in some problems concerning the biological action of peptides. He recognized the need to learn more about the chemistry of these compounds and joined our laboratory for that purpose. With his typical enthusiasm for new ideas he quickly became engrossed in a new approach to their synthesis which was called Solid Phase Peptide Synthesis. Through a series of detailed studies Garland has been able to contribute significantly to the development and application of the principles of the technique.

With his new information at hand, he was then able to undertake the synthesis of the important hypertensive peptide, angiotensin. He showed that this hormone and certain of its derivatives could be made very efficiently, in high purity, in appreciably better yields than had been possible before, and possessing the full activity of the native peptide. This work has facilitated the preparation of other analogs of the hormone which will be useful in studies of its mechanism of action.

These investigations have led Garland into diverse new areas of study and in that way have served to broaden his knowledge and interests not only in biochemistry but in physical chemistry and inorganic chemistry and physiology as well. He has made good use of the opportunities at the University to learn in these areas.

The new synthetic approach is now being extended to much larger and more complex peptides with the hope that it will eventually become possible to synthesize proteins. The road is long and difficult, but the goal is important and some progress is being made. In fact, Garland now holds an unofficial record for the synthesis of the longest peptide chain of defined sequence. But records in themselves are of little importance unless they lead to new discoveries or open the way to new experiments. I think Garland's work will do both and I am confident that we will hear more of his achievements in the years ahead.

Dr. Bronk, it is with real personal pleasure that I present Garland Marshall for the degree of Doctor of Philosophy from The Rockefeller University.

Norman Robbins

A.B. COLUMBIA UNIVERSITY

M.D. HARVARD MEDICAL SCHOOL

PRESENTED BY PAUL ALFRED WEISS

The central nervous system has been compared to a telephone exchange. Messages from receptor stations travel through nerve cables to centers, where they are somehow processed and recombined into outgoing messages to muscles and glands. Incoming messages carry information about the body's physical and chemical environment; about pressures, sounds, shapes, colors, temperatures and scents and flavors. To operate effectively, such a system must be able to identify the content of the incoming messages unequivocally.

How to account for this discriminatory faculty has been an age-old puzzle. The fact that centers and periphery become connected only secondarily during later development, confronts us with a choice between two assumptions: either each nerve is somehow guided unfailingly to its exact predestination, or else communication between centers and periphery is a matter of some coded neural language.

This antithetical alternative lay at the root of Norman Robbins' thesis. Obviously, if orderly function depended on stereotyped connections, any crossing of wires would hopelessly confound the switchboard. Back in the last century, a noted physiologist, Du Bois-Reymond, phrased the issue thus: "If optic and acoustic nerves could be cross-connected, a thunder bolt would be seen as a flash, while lightning would be heard as a roar."

A fascinating thought, but at the time, no more than an idea. Norman Robbins decided to put it to a practical test. He chose the chemical sense of taste, which rests on the faculty of taste buds in the tongue to distinguish between different types of substances. From more than four decades of sensory electrophysiology, pioneered by Adrian and Zotterman and Bronk, we have learned that the chemical receptions of sense organs are transcribed to the nerves in a sort of electrical vocabulary. The question was: would this electric idiom of gustatory discrimination be lost or reappear after a nerve cross; after hitching the tongue onto a foreign, nongustatory, nerve source; for instance, skin nerves?

In a series of delicate experiments, bedeviled by

contrariness of animals and transplants, and even puckish gremlins, Norman Robbins succeeded in cheating frog tongues into accepting skin nerves as surrogates for their native taste nerves.

The combinations healed and worked. Responses to different substances, far from garbled, were well identifiable in the electric records from the supplanted nerves. In summary: sensory nerve fibers are quite promiscuous and connect readily with foreign sense organs; and having done so, they can transmit the distinctive idioms of their new end-organs.

Besides this answer to an age-old question, Robbins' work contains many further significant new data; for instance, on the life cycle of taste bud cells, which pass incessantly from reproduction through function to death. But quite apart from its tangible results, his work commends itself for the critical and disciplined manner of its execution and for the scholarly restraint shown in its interpretation. Norman Robbins has been more than explicit in pointing out the many facets of the problem left unresolved. It is indeed refreshing to watch a student view his contribution to science progress modestly in sound perspective, his eye on the wide-open future and its challenge, rather than nurturing the illusion of having been a party to the laying of the final capstone of knowledge bounded by a self-glorifying present.

On all counts, Norman Robbins, Doctor of Medicine, has qualified as a worthy member of the community of scientists and scholars.

I, therefore, take pleasure, Mr. President, in presenting him for his well-earned additional degree of Doctor of Philosophy.

David Domingo Sabatini

M.D. UNIVERSITY OF LITORAL SCHOOL OF MEDICINE

PRESENTED BY GEORGE EMIL PALADE

When David Domingo Sabatini came to us from the Medical School of the University of Buenos Aires in Argentina, he had already a name in science and his

innovations in fixation procedures for electron microscopy were keeping very busy and quite happy lots of histochemists around the world. Yet after spending a year as a postgraduate fellow in our laboratory, he came to the courageous decision to become a student again. That was nine years after graduating as a Doctor of Medicine and seven years after his first appointment as an assistant professor in Buenos Aires. I suppose that before arriving at that decision, he measured the distance to the crest of the advancing wave in his field of science, realized that he could overtake it with a few more years of study, and enrolled in our student program. This fortitude, this perseverance, this disregard for years, this readiness of accepting high challenge is not uncommon among men who grew up in enlightened but faraway provinces of their world. Many years ago such circumstances shaped a number of Spanish soldiers into the best princes of the Roman Empire. (In this case, I don't think we have to worry, politically; he doesn't yet have a resident visa.)

In our laboratory, David Sabatini chose to work on a problem that borders on cellular and molecular biology. It was postulated about ten years ago, as a result of work carried out by our group, that the ribosomes attached to the membrane of the endoplasmic reticulum synthesize proteins for export, and that upon completion these proteins are discharged across the membrane into the cisternal space, that is, into the cavities of the reticulum. Although this postulate soon became the basis for many of our current interpretations of cell organization and cell function, it remained over all these years no more than a hypothesis.

David Sabatini decided to put this hypothesis to test by investigating both its structural and functional aspects. He first established — by dissociating ribosomes still attached to membranes, and by correlated electron microscopical observations — that the site of attachment is on the large ribosomal subunit, the one that carries the newly synthesized protein molecule. He also established that ribosomes actively engaged in protein synthesis are more firmly attached to the subjacent membrane than those inactive. He then proceeded to demonstrate in a series of remarkable experiments carried out on isolated microsomes, that upon interruption of protein synthesis by Puromycin, the unfinished polypeptide is released from the ribosomes and preferentially discharged into the microsomal cavity, which in isolated microsomes is the equivalent of the cisternal space. This finding came at a time when a similar, though less clear-cut, demonstration of amylase transport from ribosomes to microsomal content was carried out in our laboratory by Redman *et al.* Together, these two pieces of work firmly established the existence of directed transport of protein newly synthesized for export from the attached ribosomes to the cisternal space, across the membrane of the endoplasmic reticulum. Moreover David Sabatini's experiments indicated that from the inception of its synthesis the polypeptide chain must be located in a channel or space within the large ribosomal subunit which is permanently or intermittently continuous with the cisternal space.

Needless to say we have all been excited about the results obtained in these experiments. They finally replace a hypothesis with facts and put a secure basis under so many of our current interpretations of cellular organization. In the present scientific climate in biology, in which so many monumental constructions rest on a few crucial hypotheses, the replacement of any hypothesis by fact should be a source of great satisfaction.

Finally, the findings mentioned indicated that full scale, unidirectional transport of proteins across the membrane of the endoplasmic reticulum takes place in many cells. This is a novelty in biological transport which will undoubtedly attract considerable attention in years to come.

What I said about David Sabatini's work should give you the measure of his worth as a scientist. I don't think that he needs other compliments except for a statement in good old words —

doctus est

dignus est intrare communitatem nostram

scientiae rerum vitae naturaeque dedicatam

Mr. President, I have the honor of presenting to you David Domingo Sabatini who, in our judgment, fully deserves the title of Doctor of Philosophy of our University.

Leonard Austin Sauer

B.S. CORNELL UNIVERSITY
M.D. THE UNIVERSITY OF ROCHESTER SCHOOL OF MEDICINE AND DENTISTRY

PRESENTED BY PHILIP SIEKEVITZ

We all know of the great advances made during the last two decades in the biological sciences, particularly that branch which broadly can be called cell biology. Much of the credit for this increase in knowledge of events happening at the cellular level is due to the putting into practice of what can be called a philosophy of "reductionism." In practice, this means that complexities which are either not understood or cannot be experimentally defined are simply ignored, and it is the remainder of the system, containing parameters which can be heuristically defined, which is used in the laboratory. One of the methods by which this reduction in complexity can be obtained is by using "models," either by setting up an abstract, a model system analogous to a cellular one, or by subtracting from the whole cellular physiological mechanism some parts of it which can be dealt with in the laboratory. It is this latter which Dr. Sauer has done.

For some 90 years in the case of the Pasteur effect, and some 35 years in the case of the Crabtree effect, biochemists and physiologists have puzzled over the explanation for these phenomena. And no wonder, for both effects describe the imposition of regulatory mechanisms upon cell metabolism, and it is these regulatory mechanisms, encompassing as they do the interplay of many enzymes and enzyme systems, which are the hardest to solve. Dr. Sauer has tried to come somewhat closer to solving this mystery – in this case the Crabtree effect – by using a model system, a mitochondrial fraction from ascites tumor cells, and observing the responses of this fraction under different metabolic states. Now the Pasteur effect is the dampening of glycolysis by oxidative phosphorylation, while the Crabtree effect is the reverse, the inhibitory effect on respiration by glycolyzable sugars. Dr. Sauer has found that the model mitochondrial system can reproduce this latter effect in about as good a manner as the whole cell. Since the mitochondria are not capable of complete glycolysis, this finding means that it is one of the early steps in glycolysis that is responsible for the respiratory inhibition. I do believe that Dr. Sauer has provided evidence that it is this early step – the hexokinase reaction, a reaction which takes place at the mitochondrial surface – which is responsible for the Crabtree effect, and he has also defined the necessary conditions for the occurrence of the effect. Thus the response of the cell to glucose is essentially the response of the mitochondrial system to changes in its metabolic pattern. Although there are still hiatuses in the story, Dr. Sauer has led us further along the road to the complete elucidation of one of the most puzzling aspects of cellular metabolism, the regulatory mechanism involved in this metabolism.

Thomas Walter Schleich

B.S. CORNELL UNIVERSITY

PRESENTED BY JACK GOLDSTEIN

SRNA or soluble ribonucleic acid, is a name generally used to describe a type of ribonucleic acid of relatively low molecular weight which functions as the primary translator of the genetic code. It does this (with the aid of various enzymes and co-factors) by selecting a certain amino acid, transporting it to the site of protein formation, the template, and then placing the amino acid at a specific location on the template.

Since there are usually about twenty amino acids which are found in naturally occurring proteins, it was thought at first that only the corresponding number of specific amino acid acceptor soluble ribonucleic acid molecules existed. It was later shown, however, that a much larger number of these molecules were present, in many cases, with several specifically interacting with the same amino acid. The powerful separating procedure, countercurrent distribution, developed by Professor Lyman Craig of this University, played a major role in establishing this.

At this point, while we were engaged in investigating these multiple amino acid acceptor SRNA's, Mr. Schleich joined us in this work. Employing a technique of molecular sieving known as gel filtration, Tom set about studying the size and shape of these molecules. As is more often the case than not, his initial results were totally unexpected. He found that a portion of these molecules had aggregated

and in so doing, lost their biological activity. Undismayed, he promptly found a way of de-aggregating them with restoration of biological function, but there was an unexplained observation here. Unfractionated SRNA, although it appeared to have aggregated molecules too, because of its similar gel filtration profile, did not respond to the same treatment.

The issue was joined, and after three years at hard labor, Tom had learned a great deal. He was able to show for the first time that unfractionated SRNA, as it is usually prepared, contains 25% of other RNA's; that these RNA's do not share the biological role of the amino acid acceptor RNA's, also differing from them in chemical composition; and finally, that one of these was identical with a ribonucleic acid previously only found associated with the ribosome, a cell particulate involved in protein biosynthesis.

These, then, are the bare bones of his scientific achievement; time does not allow for fitting muscle to this skeleton. But, during these years, I was given the unique privilege of participating in the development of a man as a scientist. At first, somewhat unsure, Tom's questions were timid. As time passed, however, his ability to stick with a difficult problem until it was solved strengthened him; his manner steadily gained assurance and our talks became spirited, at times even heated; but afterward, always assuaged by tall, cool glasses of beer.

Philip Seeman

B.SC., M.SC., M.D. McGILL UNIVERSITY

PRESENTED BY GEORGE EMIL PALADE

There are, in research, people who depend largely on contemporary findings for formulating their projects, or are attracted so much by the contemporary scene that they join, at least in part, the clamor about the questions, truly great or apparently great, that agitate their times.

But there is always a self-centered minority which refuses to be diverted from its own lines of work, and thereby keeps alive the rest of the body of science, while this or that stormy fashion sweeps the scene.

Philip Seeman belongs to this minority. While the rest of us kept worrying about ribosomes, microsomes, and other bodies of relatively recent vintage, he preferred to work on an old, reliable topic: the human red blood cell, regularly supplied by a trusted volunteer. More precisely, he studied the effects of a series of polar compounds, or detergents, upon the permeability of the red blood cell membrane to hemoglobin. He used hypotonic hemolysis — that is, the loss of hemoglobin in dilute salt solutions — as an assay system, and clearly showed that most of these compounds have a biphasic effect: at low concentrations, they increase the resistance of the red blood cells to hypotonic hemolysis, while at high concentrations, they decrease it. At high concentrations they are themselves hemolytic.

In a broad survey, which could be the envy of a whole, active department of pharmacology, he showed that this biphasic effect is produced by tranquilizers, antihistaminics, local anesthetics, steroids, alcohols, and detergents. Because of the nature of these compounds and of the lack of specificity involved, he postulated that the effect is localized to the cell membrane and obtained suggestive evidence that the area of the membrane increases by about 20% upon stabilization, that is, when the resistance to hemolysis is increased. Hence he assumed that stabilization results from the expansion of the cell membrane that follows the invasion of its lipid phase by the hydrophobic heads or tails of the polar molecules involved in the process. In a series of ingenious experiments, using methemoglobin as an intracellular pH indicator, he further showed that stabilization is mainly due to the protonated form of the detergent molecules tested, acting on the inner side of the membrane. He had resumed the work on the red cell membrane from way back, but now he was moving into the modern era by trying to explain his findings in terms of general molecular interactions within the cell membrane.

Once again, however, he went back into the past to reopen old discussions concerning the nature of anesthesia. He put in parallel stabilization of red blood cells and anesthesia of nerve cells and fibers, and arrived at the unifying assumption that they are different aspects of the same set of molecular events, i.e. the packing of the cell membrane by hydrophobic moieties of polar molecules. As a conclusion, he proposed that the red blood cell be used as a practical test object in developmental work on new anesthetics.

For a while we worried whether Philip Seeman, in his concentration on red blood cells and their membranes, was not missing something of the excitement of this century, and we also fretted a little about his getting excited over Overton's and Traube's work when neither one nor the other knew anything about the code and its punctuation. We soon discovered, however, that all those compounds with which he was working could be used, and some of them have already been used, in stabilizing or lysing any membrane, including the variety of intracellular membranes that are at present objects of great interest in cellular and subcellular biology.

This goes to say that it is hard to tell whether we did more for Philip Seeman than he did for us. In any case, here is a man of whom one could truly say that he has earned his degree not only by his brains and his diligence, but also by his blood.

Mr. President, I have the honor to recommend Philip Seeman as a deserving candidate for the title of Doctor of Philosophy of our University.

Daniel Wyler Stroock

A.B. HARVARD COLLEGE

PRESENTED BY MARK KAC

How an electric charge is distributed on a conductor and what field it produces was one of the central problems of mathematics during the past century.

More recently there has been a revival of interest in this classical problem primarily because of its connection with the theory of Brownian motion.

Named after an English botanist who first observed and described it in 1828, Brownian motion is the erratic motion of small particles suspended in a liquid and caused by the thermal motion of its molecules.

The theory of this phenomenon is, by necessity, statistical, i.e. it is based on the concepts of chance and probability.

And yet when one calculates the probability that a particle in Brownian motion will at some time, during its random journey, visit a prescribed portion of space, one finds that this probability is equal to the potential produced by an appropriate charge placed on a conductor molded so as to fill that portion of space.

Thus a link between caprices of chance and austerity of classical potential theory.

It is to the exploitation and to the extension of this remarkable link that Dan Stroock, a refugee from biophysics, devoted his talents and energy.

As a result he has made an original and significant contribution to mathematics and has acquired a right to ancestry, doubly noble, for it can trace its origins to the founding fathers of potential theory, Gauss and Dirichlet, and to the grand master of chance, Pierre Simon Marquis de Laplace.

For originality and zeal in research as well as for dedication to scholarship I take great pleasure, Mr. President, in presenting to you, Daniel W. Stroock, for an award of the degree of Doctor of Philosophy; may I also, Mr. President, be permitted to record that

when the act is done it will be the first degree conferred by this young University in the ancient discipline of Mathematics.

Donald Dexter Van Slyke

PRESENTED BY EDWARD LAWRIE TATUM

It is indeed a privilege and an honor to present Dr. Van Slyke for his honorary degree from The Rockefeller University. He certainly needs no introduction to many in this audience, but perhaps a brief summary of his career, to date, will still be of interest to all.

Donald Dexter Van Slyke was born in Pike, New York in 1883, and received his degree of Doctor of Philosophy in Chemistry from the University of Michigan in 1907. He came directly to The Rockefeller Institute to work with P. A. Levene on the chemistry of proteins and nucleic acids. He became one of its brightest stars, working here for 41 years, 27 years as Member.

On becoming Emeritus in 1948, Dr. Van Slyke then started his second professional career, at the Brookhaven National Laboratory, where he served as Assistant Director, and where he is still active.

Dr. Van Slyke's contributions to biochemistry and physiology have been many and varied. His classic studies on blood electrolytes and proteins have greatly illuminated our knowledge of kidney physiology and of the physiology and biochemistry of transport and exchange of gases, particularly CO_2.

In order to explore these phenomena, he developed many precise quantitative methods of microanalysis of body fluids and gases—novel, ingenious, rapid, and precise. Students and research workers for many generations have necessarily become familiar with and used one or another Van Slyke method and apparatus.

His contributions have justly brought him too many honors to list here. I will mention only the latest of these, the National Medal of Science, awarded earlier this year, "For classic studies of the chemistry of blood and of amino acid metabolism, and for the quantitative biochemical methodology underlying much of clinical medicine."

Dr. Bronk, I present to you our esteemed friend and colleague, Donald Dexter Van Slyke.

Eugene Lindsay Opie

PRESENTED BY DETLEV WULF BRONK

The obvious and sincere affection with which we have spoken of Donald Van Slyke will suggest to you, our graduates, that a pleasant and priceless quality of the scholarly career before you is the fabric of friendship that we weave during the course of our scientific endeavors. Generations are thus bound together. The scope of individuals' lives is widened. As I thought of that, I coveted for very personal and sentimental reasons the privilege to speak of another we would honor.

When our founder was inspired to create what has become this institution, he called upon William Welch of the Hopkins to mold the pattern. Welch had had a student, Simon Flexner. So Welch called Flexner from Pennsylvania to direct what Mr. Rockefeller had envisioned. Both Welch and Flexner had a student at the Hopkins whom they much admired. Thus young Eugene Opie became in 1904 a Senior Member of the first group to work in our first laboratories. As have so many others, Opie soon left this place to found another institution. There, in St. Louis, Opie had a youthful colleague by the name of Herbert Gasser.

After many years, Opie succeeded Flexner at Pennsylvania, and thence Flexner urged Opie to come to Cornell whither Opie lured Gasser. Not long after, Gasser succeeded Flexner at the Rockefeller. Then, thanks to this pattern of friendships, Gasser drew Opie back to us, 25 years ago, after an absence of 30 years.

The vast significance of 70 years of research cannot be encompassed by words. Nor have I the vision to perceive the work that Dr. Opie plans for years ahead. But this I would say of a friend I have long known and admired. He is an investigator who is more than a competent specialist; he is a scientist who has always been the scholar. He was a dean who was more than an administrator; he was an academic leader who used a university to guide new social forces. He has always been a teacher who relied on research as a means of learning and as a source of enduring knowledge. He is a true teacher, because he *had* to share with youth his discoveries in the laboratory, and because he is a warm human being and a selfless friend.

THE ROCKEFELLER UNIVERSITY

NEWS

Four Collisions

On May 18, Professor E. G. D. Cohen showed a brief film illustrating the possibility of four collisions between three perfectly smooth hard spheres moving without friction in space. The motion of hard spheres as models for molecules has interested physicists in connection with a problem in statistical mechanics, the branch of physics which seeks to explain macroscopically observed properties of matter in bulk in terms of the microscopic properties of the molecules.

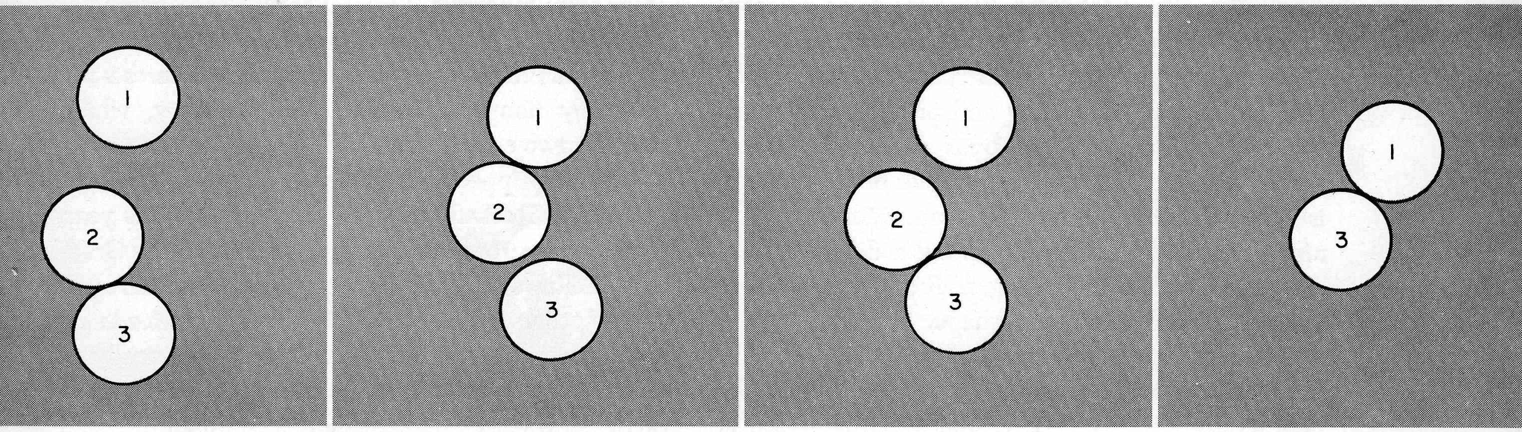
In particular, the possible sequences of collisions that can occur between these three hard spheres has relevance to the approach of a dense gas to thermal equilibrium. Contrary to the generally held belief that there would be a maximum number of three consecutive collisions, Mr. James Foch, Rockefeller Graduate Fellow, found that there could be four. Later another Fellow, Thomas J. Murphy, actually proved that four is the maximum number of collisions. Moreover, four collisions can be produced only by the general pattern of events shown in the film, either in that order or in the reverse order. The problem according to Dr. Cohen, is not only of interest in connection with statistical mechanics, but also has a bearing upon the famous three-body problem in pure mechanics. The film was produced by the Illustration Service under the direction of Richard Carter.

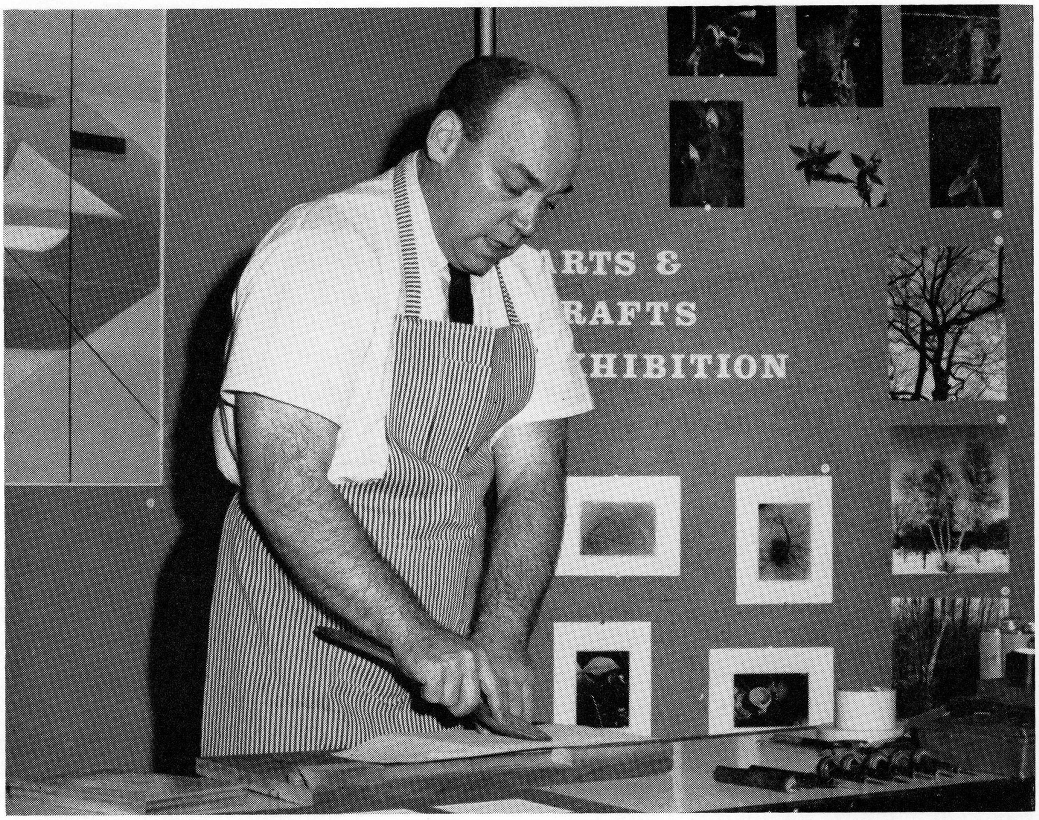
Arts and Crafts

The Fifth Biennial Arts and Crafts Exhibition was held during the last week in May in the Faculty and Students Club of Abby Aldrich Rockefeller. This year's exhibit was the largest and most varied to date, with forty-seven different contributors, including in quite equal proportions, faculty, students, and other University personnel. The entries, which numbered well over one hundred, included oils, water colors, drawings, sculpture, collages, ceramics, rubbings, needlework, and a handsome handcrafted fireplace in modern design which had been put together from scrap metal and was instantly coveted by almost every visitor. Among the contributors were Vice President McCarty (ceramics, charcoal), Mr. Martial Gousse of the Machine Shop (oils), Professor Rollin Hotchkiss (water color, charcoal), Technician Rose Sapelli (water colors), Graduate Fellow William Lowrance (photographs), and Mr. Hugo Nuñez of the Paint Shop (the fireplace). This listing, a random sample and in no way selective or definitive, conveys something of the breadth and vitality of the show and of the spontaneity and goodwill which radiated from almost every corner of the overflowing exhibit area.

Perhaps the man to whom this year's exhibit owes the most is energetic, outspoken J. Forest Vey, who has taught a weekly art class at the University for the last academic year. Mr. Vey, who also teaches at the

The four collisions of three spheres in space can be produced only in the order shown or in the reverse order





J. Forest Vey
Instructor in Art

Brooklyn Museum and at Hunter College, was brought to the Rockefeller by Professor Walther Goebel who has been one of Mr. Vey's most enthusiastic pupils. Fifty persons indicated an interest in Mr. Vey's course when it was first announced in the fall, but it was limited to eighteen on a first-come, first-serve basis. About one-fourth of the contributors to the Art Show were Mr. Vey's students. Mr. Vey, officially an Instructor on the faculty, speaks with admiration and great personal pride of the achievements of his class. During the week of the exhibition, Mr. Vey presented two demonstrations of wood block and linoleum block cutting and printing which are two of his many personal areas of interest. In his demonstrations, he showed examples of his own finished work including a color composition involving nine different blocks each printing a different form and color, with overprinting adding great varieties and subtleties of shades and shape.

Professor Goebel has long been a moving spirit in the artistic life of the University. As he recalls, the beginnings of the present Art Show can be found in a series of photographic exhibits held intermittently through the 1930's. Professors Duncan MacInnes and George Lavin were his colleagues in this enterprise although several others contributed to the show. The 1930's were a golden age in amateur photography owing to the recent advent of 35-millimeter film, but interest in photography as an avocation waned during the emergencies of World War II. Photography as a hobby has recently received a vigorous stimulus at the Rockefeller with the opening of a new laboratory and darkroom which is open to faculty and students alike, as long as their interests are primarily nonprofessional. The laboratory, which contains two enlargers and all the necessary accessories, was equipped with funds provided in a gift from Mr. David Rockefeller, Jr. Opened earlier this year, it was the source of enlargements exhibited by Graduate Fellows Ronald Carr and Phyllis Romanoff, among others. Its value will be greatly enhanced next year, according to Dr. Carr who is informally in charge of this activity, by a series of lectures and demonstrations on photographic technique.

The organization committee for this year's exhibition included Graduate Fellow Robert Barlow who received warm praise from his fellow committee members for his enthusiasm and hard work. Others on the Committee were Reynard Biemiller of the Press, Professor Goebel, and Anne Morris, occupational therapist of the Hospital.

¶

¶ Many distinguished scholars from foreign countries were in residence at the University for various times during May and June. These included a score of scientists and scientific administrators from India and Pakistan who participated in the Conference on Science in South Asia – jointly sponsored by the University and the New York State Department of Education – to be reported in a forthcoming *Review;* Professor Dorothy Crowfoot Hodgkin of Oxford University who delivered the final University Lecture of the academic year, on the crystal structure of insulin; Dr. Miroslav Holub of the Czechoslovak Academy of Sciences, Professor Shun-ichi Yamada of the University of Tokyo, Professor Pierre Desnuelle of the University of Marseille, and Dr. A. K. Covington of the University of Newcastle-upon-Tyne. And during the last of May, President and Mrs. Bronk gave a dinner in honor of the eight representatives of the Japan Science Council, who completed at Rockefeller University a two-week tour of United States universities and scientific institutions while official guests of the National Academy of Sciences and the United States Government.

¶ President Bronk was elected an Honorary Member of the Swiss Academy of Sciences at their annual meeting in Bern during May. His scientific research and his contributions to the furtherance of science throughout the world have previously been recognized by his election as Foreign Member of The Royal Society of London and the academies of science of France, Russia, Sweden, Denmark, and Brazil.

¶ The Zoological Society of France conferred its medal for distinguished achievement in zoology on Professor Theodosius Dobzhansky, and convened the Annual General Assembly in May under his honorary presidency. Dr. Dobzhansky also delivered the featured discourse on "The Evolution of Natural and Experimental Populations of Drosophila."

¶ The American Academy of Arts and Letters, the second-oldest learned society in the United States, elected three members of the Rockefeller faculty to Fellowship at its annual meeting in May: Professors Armin C. Braun, Maclyn McCarty, and Alfred E. Mirsky.

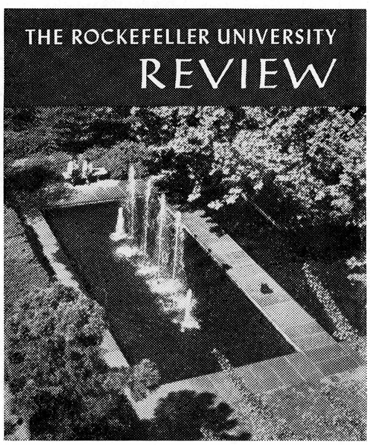
¶ Other recent elections of University faculty to academies and societies include Dr. Peyton Rous, Honorary Member of the Connecticut State Medical Society; Dr. Paul A. Weiss, Member of the Leopoldine German Academy of Researchers in Natural Sciences; and Dr. William Trager, Honorary Member of the *Assemblée du Groupement des Protistologues de Langue Française.*

¶ Six members of the Board of Trustees and faculty received honorary degrees during the June commencement season: David Rockefeller, Chairman of the Board of Trustees, Doctor of Laws, Williams College; Dr. Carl Pfaffmann, Doctor of Science, Bucknell University; Dr. Peyton Rous, Doctor of Medicine, Jefferson Medical College; Dr. René J. Dubos, Doctor of Laws, Colby College, where Dr. Dubos also gave the commencement address; Dr. Mark Kac, Doctor of Science, Case Institute of Technology; and Dr. Donald Griffin, Doctor of Science, Ripon College. At its annual commencement June 11, the University of Washington conferred on Dr. Neal E. Miller the Alumnus Summa Laude Dignatus Award given each year to the one "most distinguished University of Washington alumnus."

¶ At a special luncheon in New York City on June 7, Dr. Peyton Rous was presented with The Clement Cleveland Award given annually by The American Cancer Society New York City Division "for outstanding service in cancer control."

¶ President Bronk was one of the principal speakers at a convocation on "The University in America" in Los Angeles on May 10, sponsored by the Center for the Study of Democratic Institutions.

¶ Mayor John Lindsay was the honored guest at the Semiannual Meeting and dinner of the Health Research Council of the City of New York held in Abby on June 23.



THE COVER shows the gardens and fountains west of Caspary Hall on a summer day. Photograph by Don Young.

ACKNOWLEDGMENTS: PAGE 1 the printer's mark is a variation of the device by Stephen Gooden for the Nonesuch *Bible* Volume II. PAGES 3-5, 7 and 9-11 courtesy of the author and The Rockefeller University Illustration Service. PAGE 12 photograph by Joseph Barnell. PAGES 17 and 27 photographs by Heka. PAGES 23, 29 and 30 photographs by The Rockefeller University Illustration Service.