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OF

WALTER (JENNINGS) JONES

1865–1935

BY

WILLIAM MANSFIELD CLARK

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Walker Jones

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Those who knew Walter Jones use thread of gold for the warp of the tapestry that they weave on the loom of memory. They float the golden warp for the figures of the eloquent teacher, the keen investigator and the kind friend. Because the unique personality made extraordinary impressions, every weaver uses highly colored, homespun yarn for the wefts of his tapestry.

While we may draw the figure of the scientist from the written record, we have little more than recollections with which to sketch the man. Walter Jones seems never to have thought of retreat into the realms of his own recollections. He left among his effects, as records of his career, only an incomplete set of reprints, a list thereof, and three diplomas. Indeed, this adventurous warrior burned every bridge as he left it behind. It was his habit to tear up a letter while he still mused upon the content. He wrote few extensive letters and when at long last he had a secretary he was known to go to the typist of another department that he might dodge the carbon copy that his secretary would have filed. When he retired he thrust some of his own choice possessions into the hands of assistants who had carried on faithfully during his illness.

Fate conspired in this plotting of obscurity. At every place in Walter Jones's career circumstances or accidents prevented or destroyed the written record of his ordinary activities.

Since living the moment at its full was characteristic of the man, his biographer must not complain that Walter Jones left little with which to document events or to verify reports of his ideas. And yet we wish there were a substantial body of documentary evidence concerning the thoughts that were not constrained by the merciless rules of scientific publication. It would be interesting to have enough to discover some relation between the attitude of challenge that Jones bore in daily life and the part he played in science. It might be inspiring, could we discover the aspirations of the whole man in the eloquence of his lectures. And, I suspect that there would disappear misunder-

standing of tales that have become legendary, were it possible to resolve by documentary evidence some of the enigmatical parts of this character.

I venture to suggest that a good deal of the mischief in which he gaily indulged was his delicious way of telling people not to take too seriously the intensity of his feeling in matters that he had to take seriously. Certainly, the torrent of Walter Jones's conversation carried none of the wreckage of a reformer. It was not a muddy freshet; it roared perennially in a cañon cut deep in the stratified thought of his time.

So much conjecture would have to enter serious attempts to resolve some parts of this character that a biographer had best hold himself in check. Nevertheless, it seems permissible to present certain facts of the life in suggestive juxtaposition and with such comments as will be illuminating while recognized as tinged with the biographer's suppositions.

Walter Jones's forebears were Marylanders through several generations; the paternal ancestors being dwellers on the Eastern Shore of the Chesapeake Bay country, and the maternal on the Western Shore. For finding or checking the following information about them I am indebted to Miss Harriet Perkins Marine.

The fractionation of Walter's genes through the straight male line begins with Levin Jones [b. 1799, d. 1878] the father, and thence through Levin Jones the grandfather to William Jones, a first settler from Wales. Levin Jones (the son of William) married Nancy Jones [b. 1769] who was the daughter of Roger Jones and Elizabeth, his wife. Roger Jones was an ensign and captain in the War of 1776 and his brother Thomas was a colonel who, by family tradition, was an aide to General Washington.

The most picturesque of Walter's collateral ancestors who fought in the War of Independence was Colonel John Jones (son of Colonel Thomas). One of his most famous exploits was performed when a group of three British transports with troops put into the Little Choptank seeking refuge from a storm. The story is that the Colonel summoned his neighbors who took positions behind ice banked high on the shore by the storm. From these natural ramparts at daybreak they peppered the

vessels with shot and slugs from their duck and squirrel guns. Perhaps it was "36 hours" of peppering or perhaps it was an argumentative skill, so dominant as to turn up again in great-grandnephew Walter, that finally induced the British captain to surrender when once he had lost the first point of the argument by raising the white flag.

Through his great-grandmother, Nancy Jones, Walter Jones was fifth in descent from Justice Joim Jones [b. 1699, d. 1774] and his first wife, Sarah Woolford [b. about 1704, m. about 1726, d. after 1775]; sixth in descent from the first settler Thomas Jones of Somerset-Dorchester Counties and his wife, Martha Davis [b. 1670, m. about 1688], daughter of William Davis, who came to Maryland from Virginia in 1664. Sarah Woolford, the daughter of Colonel Roger Woolford [b. 1670, d. 1730] and Elizabeth Ennalls [b. about 1668], descended from the following first settlers of the Province of Maryland: her grandfathers, Roger Woolford [b. in Wales, d. 1701] and Bartholomew Ennalls [d. 1688] and her great grandfather, Levin Denwood [b. 1602, d. after 1665].

Thence came to Walter's family its pride in having descended from Welsh ancestry and from early settlers of Maryland and Virginia; a pride to which Walter was indifferent.

Several of the men on the male and distaff sides of this part of the ancestral tree were holders of large lands in Maryland and Virginia and active in the services of their local governments.

Levin Jones, Walter's father, left the Eastern Shore and settled in Baltimore as a ship chandler with his business at the Light Street Wharf. He had gained the sobriquet *Captain* by way of owning a small fleet of vessels. He was a substantial business man and owned considerable property. It seems appropriate to mention his nervous habit of shuffling coins while he spoke, for Walter's similar habit of tearing letters while he mused had consequences that we regret. The father died when Walter was thirteen years of age. His widow, who was twenty-two years younger, survived him twenty-eight years.

Walter's mother, Zeanette Jane Bohen [b. 1821, m. 1840, d. 1906] was the daughter of James Bohen [b. 1797, d. 1840]

and Sarah Ann West [b. about 1800, m. 1820, d. prior to her husband]. James Bohlen traced his ancestry [Bohn] to an ancient English family of noble estate. Sarah Ann West was the daughter of Benjamin [Ben] West and Annie Spencer and was related to the Wests, Spencers, Hopkinses and other prominent families of Prince George's and Anne Arundel Counties. Walter's mother, like her husband, was very active in the affairs of the Methodist Church in Baltimore. She was one of the founders of The Nursery and Child's Hospital and of The Home of the Aged of the Methodist Episcopal Church. She was one of the vice-presidents of the latter institution at the time of her death.

Walter Jones was born at Baltimore in the City's period of distress. The date was Friday, April 28, 1865. This was two weeks after the death of President Lincoln became the climax of the Civil War. That war's effect on Maryland is described by Mr. Gerald Johnson.¹

"Driven by internal dissension, drawn by affection in one direction and by interest in another, suspected and reviled by both sides, exposed to all the horrors of war without enjoying its fierce exaltation, sharing the dangers, the losses and the woes of both North and South, but never with any part in the triumphs of either, it [*Maryland*] was trampled under the feet of both contestants and emerged beaten and broken."

Walter Jones was too original to have imitated those of his generation who ruminated what their parents had taken in, but he must have lived on the war's aftermath. Other men have said that this storm-spoiled fodder sickened them; emotionally Walter Jones had a weak stomach. In a letter concerning the World War the ageing man did not write of issues, or of defeat, or of history; he wrote of confusion. Throughout life Walter Jones frequently gave the impression of one who felt the world to be out of joint.

The Civil War also occasioned the first of the obliterations of the records pertaining to the fateful Walter. The churches at that time were lax in keeping records. Several hid their books. Diligent search therein has failed to reveal any church

¹ *The Sun Papers of Baltimore*, by Gerald W. Johnson, Frank R. Kent, H. L. Mencken and Hamilton Owens.

record of Walter's birth and baptism. Indeed, it has failed to bring to light any record concerning the birth or baptism.

Could a record be found it might illuminate a curious matter. Walter was given the middle name *Jennings* for a physician and friend of the family. *Jennings* was used on his wedding invitation and in one autobiographical sketch. The initial *J* appears on the paper he published with Stone. But it is alleged that later he dropped the middle name because he had heard that it was not pronounced at his baptism. Consequently few of his later friends ever heard of *Jennings*. On being told of the middle name one of his friends declared testily, "His name was Walter Jones!" So be it. His work clearly distinguishes him from that brilliant and eccentric barrister of the same name who won a place in the history of Virginia.

The family life that was to be Walter's lot was that of well-to-do people. Both the father and mother owned considerable property. The family life seems also to have been that of people moderately well educated according to the common standards of the time. Thence must have come naturally to the boy those simpler graces encompassed in the larger meaning of *grammar*. Schooling alone hardly could have given the mature man his unerring case of expression, although it may have polished that handwriting which recipients of letters have likened to engravings.* It is evident that the boy was familiar with the better known classics of English literature and that his love for music had early nourishment. On the other hand, there seems to have been no close association of the family with any of those intellectual vocations or avocations that might have fixed a comfortably conventional attitude toward academic ideas. On the contrary, the ideas current in Walter's familiar environment were sufficiently undomesticated to have made him unconcernedly used to the appellative *eccentric*.

The evolution of the family has established that the youngest of a large family has the inalienable right to be mischievous. As the thirteenth child, Walter was placed, tentatively, in a very favorable position; as the last child his opportunity was assured.

*The tremulous execution of the signature under the portrait is of late date.

The temperament to seize it was determined by the genetic dice that turned up a strange group of characteristics—among them volatility. When Emerson remarked, "We boil at different degrees", it was not incumbent upon the essayist to add what his experiences as a chemist and as a moralist had taught, namely, that a boiling point is a function of restraining pressure. This principle seemed inapplicable to the spirit of Walter Jones. No occasion was so oppressed with dignity as to suppress his sense of humor; no personage so high that he would not dare to banter. In the light byplay of laboratory life Walter Jones was frequently up to something. His grandest opportunities for mischief arose from his conversational ability. He was a brilliant conversationalist and yet so forthright that he said of himself, "When I talk the loudest, I know the least". He could be frank to the verge of offense and yet he had a student say of him, "One always knew where one stood with Walter Jones and that was a great comfort". Of course, so forthright, frank and vigorous an ideologist would develop a reputation of a sort. The sort Walter Jones himself would indicate by twirling his finger about his head. Here was his grand opportunity. On occasion, this creature of opinion would be put into action to make the creators dance. On occasion, the creature was made a convenience. An undesired applicant for laboratory privileges was told, "You won't want to work here. I'm crazy."

The house where Walter was born was located on South Sharp Street, now called Hopkins Place, between Lombard and Redwood Streets.

There may be a significance more than geographical in the fact that a brother has described the particular location of the birthplace by its position relative to a Baptist church, an Episcopal church and a house "where a venerable Hiesite Quaker lived." Both parents were devout Methodists in a city rich in Roman Catholic traditions. From the fact that the mature man seldom, if ever, was tripped on a catch-question regarding the Bible, it is fair to assume that the early catechistic training was intense. Indeed, there is testimony of long Sunday school attendance and teaching. Some near relatives were addicted to strict religious observances. Both the father and the mother were promi-

ment in the affairs of their church and had the reputation of being generous to a fault in the giving of their services and their fortune to the work of the church.

In recalling this familiar background, it is important to realize that the boy was imbued with a faith which was part of a great movement, sometimes called the romantic rebellion against an earlier "age of reason". The boy was born to a generation of followers who found themselves resisting the devastation of a new age of reason. Among the many issues "Darwinism" was but one and "The Higher Criticism" one of the others. In its own peculiar way each of several issues seemed to concern something worth fighting for. The time was to come when the significance of these issues faded, either because some people had tiptoed around them or because others had risen to new enlightenment without succumbing to that evil aspect of tolerance, indifference. But let no one now, in hauteur, point merely to straight-laced practices of the stock from which Walter Jones came or attach *superficial* implications to the subject matter of Walter Jones's discussions. In all of his discussions there must have been more poignancy than this generation can feel.

There need be no hesitation in writing that Walter Jones made much of theological discussions. Whatever may have been his religious feeling at any time, his theological views were not so precious as to shield them from courteous biography. He proclaimed his views to conferees, to agents for chemical supplies, to his bank clerk, and to his friends among the clergy. Probably no one ever knew his deeper musings and probably no label would have been acceptable, but it was made plain that he could not reconcile the tenets of theology and science. From much testimony of his attitude I select one bit that seems to jibe best with the whole. It is a straightforward story told by one who listened to a long series of conversations. This was during Jones's correspondence with an eminent physicist who had publicly declared his own reconciliation. It is reported that Jones maintained the highest respect for his correspondent and earnestly looked to him for a constructive contribution. But answers came that seemed to Jones evasive and to be carrying logic into the

fog of wishful thinking. Then he gave vent to one of those tirades that too often led people to say "he baited his victim." Rather it was that Walter Jones, facing in his way the greatest problem of his age, saw the devil and threw his inkwell.

It goes without saying that tirades on religious subjects and a good deal of provocative banter is a combination likely to lead to grave misunderstanding. Therefore it is fortunate that Walter Jones worked among men whose instincts gave a natural respect to his peculiar need for freedom of thought and freedom of expression. Without this perfectly natural respect Walter Jones could not have done the scientific work that he did. This man of complete independence could not have endured freedom at the cost of condescension. There was no need. That grand gentleman, John Abel, who held the control of Walter Jones's early academic career, always kept first things first. He respected Walter Jones's scientific ability and in this atmosphere Walter Jones won his own way and fame.

As a boy Walter was active in sports. He hunted and was a very good tennis player. He is remembered by a nephew, of similar age, as "one of the best fancy ice skaters in Baltimore, the envy of the younger ones as he performed on Sumalt's ice pond." No one seems to understand why he suddenly ceased to be active out of doors, but there can be no doubt that he maintained his interest in sports. He told Professor Abel that he "went with the football crowd at Purdue" and throughout his life at Johns Hopkins he was an ardent lacrosse fan and a vigorous critic of the players not only in their contests but also in their routine practice. Some early habits may have determined the carriage of the tall, lank professor who was physically active and gracefully so.

The young man played the piano well enough to afford entertainment. The mature man collected records of classical music and knew them well. In music Baltimore offered him opportunities that he seldom missed and once mentioned to a class in this manner: "The usual recitation is scheduled for four o'clock tomorrow. Those of you who are uncivilized do not know that the Boston Symphony Orchestra is to give a special concert at that hour and so you will report here promptly. *I shall not be here!*"

Walter's early education was obtained partly in small private schools of his neighborhood and partly in public schools. Whatever these may have contributed was enlivened by the devotion of his sister Annie who followed his studies keenly and who in later life declared that she had attained a college education through Walter's eyes. This personal touch may well have exerted its potentially great effect. Tutorial habit exercised with a loving pupil can fix the photographic image. It can develop the high lights. That Walter's image of a lesson was clear is attested by college mates who heard him recite. The mature professor expected the same of his pupils. They could laugh at his extravagance while feeling the significance of his exclamation to a forgetful student, "Even the Baltimore street car conductors know the amino acids!"

In 1879 Walter entered the City College of Baltimore for its five-year course. The College has no record except that he completed the course creditably in the spring of 1884.

The following fall, Walter Jones registered in the Collegiate Department of The Johns Hopkins University, stating, "I wish to take a course which will be principally mathematics and Latin. I also wish to study chemistry, French and German." He took the courses then known as Group IV with chemistry and physics predominating in the last two years. With his progress in course his grades improved until he became a high standing student and won a University Scholarship for the year 1888-9. Granted the B. A. degree in 1888, Walter passed into graduate work in chemistry, taking minors in mineralogy and geology. Work for his dissertation was done under Professor Ira Remsen. He was granted the degree of Ph.D. in June, 1891.

Since few of the contemporary students recall anything especially worthy of note regarding Walter Jones, it may be inferred that he then exhibited what was characteristic of him in later life—supreme independence in going his own way.

On September 1, 1891, there occurred in St. Paul's Church, Ocean Grove, the marriage of Walter Jennings Jones and Grace Crary Clarke. Miss Clarke was the daughter of the Rev. and Mrs. George Clarke of Ocean Grove, New Jersey. To this seaside resort Dr. and Mrs. Jones went frequently on vacations.

Soon after his marriage, Doctor Jones took his bride to Springfield, Ohio, where he had been engaged as Acting Professor of Natural Science in Wittenberg College. The engagement there was for one year during which the professor of that subject, A. F. Linn, returned to Johns Hopkins to complete some investigations under Remsen. As was customary in those "good old days", Professor Jones "offered courses in chemistry, mineralogy, zoology, and botany. It is possible that he may have offered a course in crystallography."²

His first appointment terminated. Dr. and Mrs. Jones returned to Baltimore. There, August 13, 1892, was born their only child, Marion Eleanor.³

One may read between the lines of the following, taken from a letter concerning The American Society of Biological Chemists.

"The meeting of the Society during Christmas recess has always been a matter of regret to me, for this is the time of year that I have been accustomed to devote to home affairs since I was a child and it is very difficult indeed to break away from such a custom."

His granddaughter, Charlotte, made Walter Jones her devoted playmate.

In September, 1892, Doctor Jones went to Purdue University as Professor of Analytical Chemistry. Winthrop E. Stone, Professor of Chemistry, had been made vice-president of Purdue and already was taking over a good deal of the administrative routine for which he soon was to assume full responsibility as president. Yet, when Jones arrived, Stone drew him into an investigation on which the two men reported. Jones later contributed an article from Purdue on a problem related to a subject then lively at Hopkins. Dean Enders informs me that the records of Purdue University contain nothing regarding Walter Jones other than that here given. That Jones finally became dissatisfied at Purdue he told Professor Abel. At any rate Jones returned to Baltimore without a job and again taking up work under Remsen was made a Fellow by Courtesy for the year 1895-6.

² Letter from Dean Shatzer.

³ Marion married Gilbert A. Jarman of Baltimore.

The only enlightening information from Wittenberg and Purdue is that Walter Jones was then a vigorous teacher who used unique ways of stimulating student interest and who challenged students to questioning.

In March 1896 Professor John J. Abel took Jones to the medical school where he was appointed Assistant in Physiological Chemistry for the remainder of the academic year. There Walter Jones was to remain for the greater part of his career.

Since Walter Jones was to become the head of the Department of Physiological Chemistry in the Johns Hopkins School of Medicine, it is well to review briefly the early history of that department.

The original plan for the Medical Department⁴ of The Johns Hopkins University included the teaching of chemistry to students of medicine under the auspices of the established Department of Chemistry. Indeed, the first *Announcement* lists the head of that department, Ira Rensen, as Professor of Chemistry in the roster of the Medical Faculty. Rensen held this title until he became the second president of the university in 1901. Rensen had been one of the committee to consider the establishment of the Medical Department and he continued on its Advisory Board, first as an elected member and finally *ex officio* as President.

With the opening of the school in 1893 the original plan of teaching preclinical subjects was modified as the exigencies demanded. The teaching of physiological chemistry was entrusted to the professional school under an arrangement stated as follows in the first *Announcement* of 1893. "The instruction in Physiological Chemistry will be for the present under the charge of Dr. John J. Abel, Professor of Pharmacology, with the aid of an assistant."

It is important to note that physiological chemistry was given a unique position in the new school. The original intention to have medical students trained under the guidance of the Depart-

⁴ The name was changed during President Goodnow's administration to harmonize with *School of Hygiene and Public Health, School of Engineering, etc.*, and is now *The School of Medicine, The Johns Hopkins University*.

ment of Chemistry was a reflection of the purpose to train medical students as university students. The actual, initial placement of physiological chemistry in the Department of Pharmacology broke an historical relation that had made the subject the foster child of physiology. The entrusting of the teaching to Professor Abel gave an especially noteworthy character to the whole affair.

Professor Abel had come, directly and indirectly, under the influence of several German investigators, who, although trained in the general field of medicine, had acquired an expert's knowledge of chemistry and who devoted it to fundamental work. This influence, and Professor Abel's own keen appreciation of chemistry, made him determined to give the science its full due. He wished to avoid the limitations of the purely "analytic school" and the subordinate place of chemistry suggested by the term "chemical physiology." Abel's attitude is reflected by Jones's specification for an assistant that he sought in 1925, nearly thirty years later.

"We want a man who has a Ph.D. degree, who has teaching ability as well as research ability and who is well grounded in the fundamentals of chemistry. Of course, it is desirable that he should also have had a training in physiological chemistry and the biological sciences but he must be a chemist primarily."

While the burdens of organization and the financial catastrophe in the early years of the school inhibited the development of Abel's plans, he managed to create the plant out of which brilliant investigations were to blossom. It was at the beginning of this flowering that Walter Jones came.

When the school was opened in the autumn of 1893 the students, of course, had not reached pharmacology and Professor Abel, with his first assistant, Dr. Thomas B. Aldrich, could handle the introductory course in chemistry. Thereafter, Abel turned over the instruction in this subject to assistants. There was also a spatial separation of the work, chemistry being left for a time in the old Pathological Building and pharmacology going with anatomy to the newly built (1894) Women's Fund Memorial Building until chemistry and pharmacology were reassembled in the Physiology Building in 1898.

Thus, when Jones came to assist in the teaching of physiological chemistry, there was no autonomous department for that subject, although the subject was taught as a distinct discipline. At the same time the close relation between subjects taught under Abel's general supervision carried Jones into the teaching of toxicology.

The list of Jones's successive titles is instructive:

- 1896-8 Assistant in Physiological Chemistry.
- 1898-9 Assistant in Physiological Chemistry and Toxicology.
- 1899-1902 Associate in Physiological Chemistry and Toxicology.
- 1902-8 Associate Professor of Physiological Chemistry and Toxicology.

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- 1908 (Department of Physiological Chemistry established).
 - 1908-1923 Professor of Physiological Chemistry.
 - 1923-1927 DeLamar Professor of Physiological Chemistry.

When Jones was made full professor in 1908, the Department of Physiological Chemistry was created. The new title, conferred on Professor Jones in 1923, commemorates the School's benefactor Captain DeLamar. The choice of the Professorship of Physiological Chemistry to receive his name, as one of several means of commemoration, gives recognition to Captain DeLamar's acquaintance with one branch of chemistry and to the specific part of his will that refers to his interest in nutrition.

The order of arrival of those who assisted Professor Abel and were officially designated as Assistants in Physiological Chemistry were Thomas B. Aldrich (1893-1899), Edwin S. Faust (1895-6), Walter Jones (1896-1908) and Arthur S. Loevenhart (1905-1908.⁵) During parts of this period Abel had but one assistant assigned to physiological chemistry. Indeed, during most of his term, Jones alone handled the teaching of chemistry. When Jones became a professor he had but one assistant most of the time up to the end of the World War. They were:

- Arthur H. Koelker (1908-11)
- Eli Kennerly Marshall, Jr. (1911-14)

⁵ After 1908 Loevenhart stayed on for a time in the Department of Pharmacology.

D. Wright Wilson (1914-1922)
 Annabella E. Richards (substituting for Wilson 1918-19 during his leave on military service)

After the World War there came

Mary Van Rensselaer Buell (1921-1930)
 Marie E. Perkins (1921—)
 Lawrence Wesson (1922-25)
 William Hoffman (1922-27)
 Herbert O. Calvery (1925-27)

This roster does not include an occasional assistant not listed in Jones's own prospectus of the course.

The list is of interest itself, and also because its comparison with Jones's bibliography proves that, prior to the last period of his work, members of his staff seldom entered his field of research. Indeed he advised them not to do so.

After Doctor Jones had been with him a few years, Professor Abel advised him to study under Kossel. This advice was accepted but a financial difficulty stood in the way. Indeed there were times in Walter Jones's life when he had to be so absorbed in saving money that he feared he was becoming a "penny pincher". He related that when this fear was felt he had great satisfaction in throwing a penny into Jones Falls⁶ as he crossed the bridge on his way to work. Appreciating the financial difficulty, Professor Abel had an inspiration one day when a sheriff appeared with the organs of a woman suspected of having been poisoned. While Abel was loath to have his laboratory burdened with such cases, he arranged to have Jones take this case. When Jones had identified strychnine he went to the distant town with his evidence. He found the people in an uproar over some scandal and a trial impracticable to hold, so he returned disappointed. At last he testified and brought home the needed fee.

The visit to Germany was short, June to December, 1899; but within that period Jones accumulated the data for two papers and became so inspired by Kossel that he devoted himself thereafter exclusively to the study of nucleic acids.

⁶ The stream that runs through Baltimore.

It was Dr. P. A. Levene, the other American leader in this field, who welcomed him as Doctor Levene himself tells in the following letter.

"My memory pictures the arrival one day of a lean, tallish American of rather indefinite age, somewhat forlorn in a foreign land which he was visiting for the first time and, as I soon found out, having with him a wife and child. As my own sojourn in the city of Marburg had already had a history of a month or six weeks, I considered myself called upon to help Jones and family to find living quarters which was not a very difficult matter since the lady who sheltered an Englishman and myself, both of us working in Kossel's laboratory, was only too glad to make room for an American who could afford to bring a family with him all the way across the ocean and who, in her imagination, must be one of the American millionaires.

"The events that follow are rather vague in my memory and I am certain the fault is mine for Jones was not a man to permit a day to pass without leaving some impression on it. However, it so happens that besides Jones, two Englishmen, and one Italian, there were in the laboratory three Russians, a Frenchman, and myself and somehow, the Franco-Russian alliance is more vivid in my memory than that of the rest of the 'Internationale.'

"I remember clearly that as soon as Jones discovered that English was the dominating tongue of the Internationale—the Professor speaking English and the Russians being silent in every language but their own—he dropped his shyness and was ready for argument. Great enthusiasm and force of expression revealed themselves soon and, before long, I became a victim of them.

"Jones and myself shared a long laboratory bench but not too long to prevent occasional discussions. Jones was given the concrete problem of making derivatives of thymine. The work was progressing successfully and was destined to shape his principal interest in nucleic acids. My own problem was rather fantastic for I conceived the idea that vitellin must contain the chemical nucleus of nucleic acids since the nucleins developed during the growth of the embryo in the yolk. The problem being fantastic, and originating with myself, it naturally progressed poorly. Kossel was of much help to Jones and little to me. Jones became a devoted admirer of Kossel. I was more impressed by Hofmeister who, an enthusiast of the type of Jones, was certain that he held the key to the solution of the structure of the protein molecule. Jones was an arguer by nature; argument is the Russian sport par excellence. We had a lively argument and I was floored in the first though protracted bout. To save my ego, I blamed my defeat on the fact that Jones had the better command of English.

"So the friendship of Jones and myself began with an argument and continued in the same way for many years for we were warm friends regardless of our temporary scientific disagreements."

In the year of Jones's pilgrimage (1899) began the long series of papers on nucleic acid chemistry that constitute far the greater part of his contributions to science. These will be reviewed later.

Here we may dwell upon the sociology of his scientific research and teaching because it throws the light of one, somewhat isolated, case upon an important era of American physiological chemistry.

The views that Walter Jones held with respect to his dual rôle as an investigator and as a teacher, the adjustments that he made in relating a rapidly growing science to pedagogical problems, indeed, his whole attitude toward his position, present an enigma. Testimony conflicts and conflicting with that testimony which would make of Walter Jones a man interested only in nucleic acids stands one remarkable fact: the few letters that have come to light contain little of interest in regard to nucleic acids or similar scientific matters and much that bears upon his pedagogical problems. There is too little of this for one to dare a reconstruction of Walter Jones's views. If this sole body of documentary evidence is to be used, quotations must be placed against the background of his scientific career.

We may mention first the theoretical attitude toward chemistry in the new school. Perhaps there is no better way to do so than by reference to the addresses of Professor Welch,⁷ for, while Welch may have appropriated many of the ideas that he expressed, no one preserved better their perspective in the general scheme and the very fact of some appropriation made him recognized as a spokesman.

Considering the time (1894) at which he spoke, Welch displayed amazing perspicacity in the following remark.

"Physiological chemistry means much more than what is usually taught in our medical schools as medical chemistry, which includes little more than the chemical analysis of certain fluids of the body for diagnostic purposes."

⁷ William Henry Welch—Papers and Addresses, Vol. III.

Continuing, Welch put the seal of his approval on the following quotation from Hoppe-Seyler.

"I cannot understand how in the present day a physician can recognize, follow in their course, and suitably treat diseases of the stomach and alimentary tract, of the blood, liver, kidneys, and urinary passages, and the different forms of poisoning, how he can suitably regulate the diet in these and in constitutional diseases, without knowledge of the methods of physiological chemistry and of its decisions on questions offering themselves for solution and without practical training in their application."

These quotations express in fairly definite terms the attitude that led the authorities to give a carefully considered place to chemistry in the medical curriculum.

There was also considered the important factor of the student's preliminary, liberal education of which Welch wrote that ". . . it should not be taken with reference to any utilitarian purpose. . . . when studying chemistry it should be done with the object of learning the general principles."

In general the effort was made to keep the Medical Department more than nominally a part of the University; ". . . ideals of the university must inspire the whole life and activities of the medical department." These *ideals* were to permeate the clinical departments where medicine should be treated as "one of the natural sciences".

All this had a great deal to do with the relation of Walter Jones to his students. It set up a guiding syllogism the first parts of which were made visible in the School's catalogue.

1. "The medical art should rest upon a suitable preliminary education and upon a thorough training in the underlying medical sciences."

2. Chemistry was made an important member of the "underlying medical sciences" by imposing an unusual entrance requirement, and by making physiological chemistry a prerequisite to the study of clinical subjects.

The third part of the syllogism was not stated in the catalogue but there was clearly the implication that a body of thought, the initiation of which had been so carefully planned, would be

carried on to the final objective. Welch stated this in 1899 when, after having previously indicated the advantages of keeping the pre-clinical subjects basic and free from too many clinical applications, he said:

"The real aim of medical education should be the training of practitioners of medicine and surgery, and the benefits of thorough grounding in the fundamental medical sciences are to a large extent sacrificed if the student does not find in the latter two years of his undergraduate study well-conducted clinical courses which afford opportunity for the practical application of knowledge previously acquired in the laboratories."⁸

Whether Walter Jones thought out the position of his department in this educational scheme or merely slid into the policy that he followed is immaterial when set beside the fact that he did seek to train students in what he considered the fundamentals and did leave clinical applications to be taught where used. In principle, this was in harmony with the general scheme. In fact he applied, however imperfectly, that part of the declared syllogism for which he was responsible. Could it be completed? The answer depended upon an appreciation of events.

Welch realized that physiological chemistry had replaced what he called "medical chemistry." If we continue to discriminate, as Welch did to emphasize developments, we shall find that during Jones's career there developed need of further distinction within chemistry. While the older physiological chemistry arising in medical centers had played with the fringes of clinical subjects, its more substantial new contributions were bringing the basic science to those developments of its biological phases that became the foundation of *several* branches. Among these was *clinical chemistry*. Furthermore, as the several parts of biochemistry's application carried topic after topic out of the hands of the medical profession and into the hands of special groups, the evolving clinical chemistry became, theoretically, the particular concern of the medical profession.

Within the period of Jones's career new analytical tools had made possible sufficient knowledge of body components to provide several new categories of thought associating material

⁸ Welch: Papers and Addresses. Vol. III, p. 68.

changes with what is observed in disease. General principles of physical chemistry attained the power to deal with the interplay of multiple components as disease shifts equilibrium states throughout the body. There accumulated vast arrays of facts requiring sifting by clinical experience and, where possible, reduction to order by the logic of the basic science. Withal, a new perspective developed; sometimes the special science had to take courses of its own but perhaps as often it bore out the remark of Whitehead. "The paradox is now fully established that the utmost abstractions are the true weapons with which to control our thought of concrete fact." In all events, the broader knowledge had given scope to a type of thought known of old to clinicians but requiring discipline in new bodies of fact and logic. What Welch had decried as "medical chemistry" should have been relegated to special technical laboratories. Physiological chemistry remained essential, but, speaking broadly, it had expanded as a general subject, very unevenly and without any particular objective. Clinical chemistry was becoming a distinctive body of logic with attendant bodies of fact, not only centered on one objective but drawing its power from the broad concepts of the basic science and from techniques derived therefrom.

How much of this did Jones have in mind when, writing of the attitude of his department, he exclaimed, "Chemistry is looked upon here as the key to modern medicine."? It is reported that on occasion Jones would carry a topic from its academic beginnings to its decisive place in modern practice and that he was not above extravagant praise of the practical accomplishments of his science. This occasional jangling of the keys may have caught the attention of some students but such attractions were not needed by others. Jones made his science fascinating and it is testified that many a student was surprised to find the text so dull when the lecture had been so interesting. Here was stuff for educators to conjure with.

But the conjuring would remain lost motion so long as the new science retained only in theory the position envisaged by Welch and Abel and Jones. A student still could ask: Is a knowledge of chemistry *vital* to medical practice? It seemed a sufficient

answer that practitioners who declared their admiration of advances in chemistry forthwith would express regret at not having kept up.

Thus, Walter Jones was left to deal as best he could with that academic interest in chemistry which felt little pressure from the profession. He was led to write, "The younger men here usually take their chemistry as a perfunctory part of their medical education." As late as 1923 he complained, "Physiological Chemists are much more difficult to secure than either pharmacologists or pathologists for the reason that most students of physiological chemistry are headed toward medicine and do not stop in the former science long enough to become expert."

Had Jones taken more graduate students he could have counted on an established seriousness toward *general* biochemistry. But it would have been too much to have expected him to do so. He had too much to do almost single handed. Nor would this support of general biochemistry have met ultimately the basic cause of his complaint.

It is true that during many years the catalogue announced elementary and advanced courses in Physiological Chemistry for graduate students; but throughout the Medical School the intent of all "graduate" courses was to provide advanced training for physicians. It is not clear that any physician took Jones's offered graduate course. It is certain that only on very rare occasions was a graduate student accepted. The graduate students in West Baltimore felt that they were not wanted in the laboratory in East Baltimore and there are letters informing applicants that the department offered no training leading to advanced degrees.

Replying to Professor Henry Harper's inquiry of what he was doing for graduate students, Jones wrote in 1922: "Physiological chemistry . . . is treated exclusively as a medical subject and the entire resources of the Department are devoted to medicine. No attempt is made to train men for philosophical degrees." Thus Walter Jones was completely committed to the first parts of the syllogism framed for the training of medical students.

But also he had little inclination to be concerned with the concluding part. He had entered the school as an organic chemist with no particular interest in medical affairs, without specific

training in the evolving science of physiological chemistry, without recorded training even in biology, and he was valued initially and throughout his career as an investigator. Brutally honest with respect to his interests he nevertheless felt at home in the medical school not only because his teaching fitted the declared scheme but also because of one of the outstanding characteristics of university life at that period.

Americans had taken from an imported system only one of its better parts. This they had converted to an enthusiasm for research so furious as to have swept aside other parts of scholarship. The salutary effect of the transition from didactic teaching and from book learning to direct demonstration and observation in the laboratory needs no review. It was gloriously great and Jones gloried in being a part of it as an investigator in a new field. On the other hand the trend was so impetuous as to have obscured the simple fact that, by no stretch of the imagination, can any student reconstruct for himself a small part of what the experiments of others, now and in past ages, can give him as his scholarly heritage. In its new freedom American scientific scholarship retained no certainty of how to make research subserve the whole of scholarship. As Welch warned in 1888, the encouragement of research is not the primary conception of the true university ideal. Possessed of a native genius for organization Americans elaborated their organization of research and gave little heed to the consolidation and reorganization of the knowledge acquired. Occasionally pedagogy went wild in the lower schools for lack of adequate organization by the higher schools of the *knowledge* deserving emphasis and logical placement in the progression of training. In the higher schools datum piled on datum and theory went the way of the convenience of specialists.

Jones was distinctly a specialist and was honored as such. He also took infinite care to consolidate in his monograph the knowledge of his field and he took great pride in having done so.

For lack of consolidation the attainments of research in the field of *clinical chemistry* went as isolated contributions, not as parts of a whole; as new specializations in an over-specialized culture, not as a group of evidences from which could be drawn

principles that permeate. The methods of chemistry would be conceded their uses in medicine, but clinical chemistry, then unrecognized as a distinctive body of thought, would have to await leaders strong enough to overcome professional inertia.

Those who knew Walter Jones, the general situation, and the unique position given to chemistry by the designers of the School will feel the power of exceptionally restrained expression in the following note with which he returned a widely discussed report on the teaching of clinical subjects in America.

"I supposed, at first glance, that I would receive some information from the report on the teaching of physiological chemistry, but it deals with the subject only in so far as it is submerged in physiology."

Also there was accumulating too much to teach and there remained too little time in which to teach even a part of it. American biochemists were becoming a little noisy in their demands for longer introductory courses. Professor Jones could not be drawn into this. Not only did he accept (perhaps passively) the plan for shortened courses in all departments agreed upon at Hopkins shortly before his retirement; he also wrote sometime before as follows:

"I should say that the time devoted in the medical curriculum to physiological chemistry depends to a considerable extent upon what portion of this subject is taken up in other departments on the borderline. I think the tendency has been to teach clinical medicine in departments of physiological chemistry and this greatly lengthens the time allotted to the subject."⁹

The curious position in which Walter Jones found himself should now be evident. The vigor of his research fitted the temper of the time. Its quality brought encouragement, promotion and fame. Valued primarily for this he was given a pedagogical problem of the first magnitude involving the cultivation of such an appreciation of a developing science as would subserve professional ends that were in view but that were only vaguely recognized within the profession. Doubtless he was given this task with full recognition of his genius as a teacher.

⁹ Letter of February 16, 1921.

Yet to the historian of his time it could appear that students were then regarded as needing only exposure to original minds. The year was divided so as to give Jones ample opportunity for original research. But during the teaching period he handled large classes, at first single handed, then with one assistant. There is no evidence that he felt an incongruity between this and his complaint to Dr. Abraham Flexner of The General Education Board that medical students seem to have been ill prepared in college because they had "been crowded into large classes where they are expected to work under the poorest conditions. . . . They give excellent evidence of never having received the individual instruction which is so necessary in the . . . education of most men." It was set in the scheme of affairs that he should teach principles, not practices. There was provided no systematic way to build practice on the principles that he taught. He and many others were constructing the parts of a science that were to remain more or less unarticulated. The man himself had two major academic interests—nucleic acid chemistry and good lectures. He was somewhat indifferent to the rest. Still more indifferent to anything but research had been several of his preceptors and were several of his colleagues.

In summary it may be said that Walter Jones was not a student of medical education; he was an inspiring teacher. He was not a propagandist of an evolving science; he was a creator of one of its parts.

The conflicting opinions on the apportionment of his thought and effort to each phase may arise from the asking of questions that seem inappropriate when once the setting of his work is appreciated. Thus, it has been asked whether Walter Jones should not have created a school of physiological chemistry when the period of his career was favorable thereto. Since one yard stick used to measure a teacher is the stature of his pupils in his field this appears to be a fair question and a negative answer a detraction. But let us examine the facts.

If the reader cares to study the list of coauthors in Jones's bibliography he will find the following. The list includes the names of several members of the clinical departments. In the early years of the School the "preclinical" laboratories were cen-

ters of a good deal of research by members of the clinical staffs. Later, the introduction of the "full-time" system in clinical departments was accompanied by a more systematic organization of research in those departments and the participation of clinicians in the types of investigation that were influenced by the points of view in the basic sciences measurably declined. While the going was good Jones cooperated.

The greater part of the list of coauthors is made up of medical students. The list of these medical students, some of whom Jones literally snatched from the bench of the introductory course to ensconce in his own laboratory, proves him to have been a good picker and sympathetic with the School's ideal of association between teacher and student in the rôle of coseekers. Of the fourteen medical students and four graduate students who published with Jones, nine attained academic distinction by professorial rank. One became a Nobel prizeman. It is not intended to say that Jones was mainly responsible for the training of these individuals. For example, among the medical students Whipple and Winternitz owe far more to Welch. It is intended to say only that Jones selected wisely and contributed much to the training of those who made this remarkable record.

On the other hand when we look at the careers of the *medical* students who published with Jones we find that none is distinctly in the field of clinical chemistry. To be sure, several of those who worked with Jones as members of his staff, as visitors, or as graduate students and several of the medical students who took his elementary course carried on with distinction in one or another field of chemistry or with the tools thereof. What is now under discussion is the influence of close association with medical students as judged *only* by the record of joint publication. An examination of the circumstances already cited will not detract seriously from the esteem of Walter Jones as a teacher; it will reveal the weakness of an evolving system.

Under the circumstances the trend of Jones's intimate students is not difficult to see. They were well initiated by the introductory course in which Jones hewed to the line of the established syllogism. Then Jones gave them an invaluable introduction to the serious investigative method. Finally, they came to the

clinical departments. There clinical chemistry was merely a valued adjunct, not given a position commensurate with that of morphological sciences. There established sciences had their scholars. Within their fields both the universal experimental method and the knowledge culled from the trials of ages were united in one objective. Few students could have resisted this. None deviated from the organized path to seek training by paths not organized. Such, perhaps, is one perverse manifestation of the American genius for organization.

The testimony is almost universal that Walter Jones was a brilliant lecturer. It is not equally well appreciated that he took great pains to prepare the materials of his lectures and that he did not spare himself in developing their eloquent logic. In the closing paragraph of a letter to be quoted later Doctor Read writes *of those moments before a lecture in which a great emotional strain was evident*. Before that, however, there had been a long period at his favorite place before the balance where he worked out the flow sheet of what was to appear as a smoothly developed subject. Perhaps they were his eloquence, his gossipy reviews of controversies, his sharp wit that brought the advanced students back to the first-year class room; but it may be doubted that these alone induced them to come year after year. There was an intellectual interest. Robert P. Kennedy writes:

"Walter Jones's lectures, both in class room and in private conversation, were virile and impressive. No matter what sort of tirade his dissertation may have sounded like, his thoughts were logical, his expressions extremely accurate and if the listener were inclined to argue he was always worsted by a better piece of argumentative effort."

Professor Wright Wilson, while an assistant to Jones, noted the frequency of Jones's new approaches to old subjects and the dramatic quality of the lectures which were "made so interesting that students of upper classes often returned to hear him speak."

The vividness of certain impressions created during those lectures is illustrated, perhaps extravagantly, by the effect of his comparison of combustion in a mouse and a candle. A mouse and a candle were brought near to extinction under bell jars

and then revived in air—all in pantomime. A student today swears that the experiment was real! "And the wonderful thing is this", said Jones, "one has to light the candle but one doesn't have to light the mouse."

There were times in the early years when descriptive material loomed rather large in the course. Physiological chemistry was then largely descriptive. There were times in the later years when nucleic acids loomed rather large in the perspective. Emphasis here had the advantage of giving the students an insight into a subject in the making as presented by a maker and what they lost in general perspective was compensated, as many of them have attested. When others complain of such restrictions let them not forget the times.

In view of Walter Jones's training, his preoccupation with teaching and with highly specialized research, it is not surprising that his technical knowledge of the evolving applications of physical chemistry was limited. He was blatantly honest in all such matters. But I can testify that in the later years he had an appreciation of its developing importance. The following letter to Doctor Holt reveals both sympathy with one development and a characteristic regard for his responsibility to students headed toward the clinical uses of chemical thought.

"December 12, 1923.

"Dear Doctor Holt:

"After thinking over the matter that has been a subject of conversation between us and talking it over with a number of people I have come to the conclusion that the physical chemistry of proteins, as it now stands, is rather too special a matter for our second year students. Of course, an optional group of men can be gotten together for any course, as students will take your word if you say that the subject treated is of importance in medicine.

"I see no reason, however, why you should not give this optional course on your own accord and in connection with the Department of Pediatrics. I am in sympathy with it and would encourage it in every way except as concerns matters above stated.

"Very sincerely yours,

"Walter Jones."

The usual testimony is that Professor Jones was a stimulating teacher and not a soft one. One student will not forget the moment he poured the materials of an experiment down the sink, explaining, "It didn't work the way the book said." The Professor exploded, "My God, man, if you threw a brick out of the window and it went up instead of the way the books say, wouldn't you stick your head out?"

Occasionally, Jones was accused of being a bit too severe with stupid students. Another aspect is revealed in a letter regarding a candidate for an advanced degree who was subjected to an oral examination by Professor Jones. After stating that he was not satisfied with the candidate's knowledge, Jones added:

"The matter is a little annoying to me and I would not make a report of this kind if there was the slightest possibility of making any other. I suggest in Mr. —'s interest that you substitute me on the Committee by someone else and see if a disagreement among authorities cannot be produced."

Jones could be mischievously enigmatic. To Doctor Mendel, Editor of the *Journal of Biological Chemistry*, he wrote, when forwarding a paper:

"If you will read the last page of the long article you will concede the desirability of concealing not only its contents but even the fact that I am publishing any article at all."

When admitting a clearly explained mistake of his own that Wilson had noted, Jones bubbled over with an old jibe.

"Dear Doctor Wilson:

"If you have as much difficulty in calculating in dollars and cents as you have in reckoning normal solutions, I should think you would get along pretty well in dollars and cents. Your calculation is exactly right. I now see the reason. I made arithmetical mistakes all the way through. So everything along the line is now beautiful."

It is difficult to convey the nuances of Walter Jones's acts and utterances. He would press to the hilt the thrust of his argument. If the victim felt wounded he had still to discover that Jones's glee in the game made him unconscious of a personal aspect and in time he would discover that Walter Jones could

melt into the very essence of thoughtful kindness. Again it might be thought a typical Americanism when he bullied a student into a position where he could confer on him a great favor, or it might be thought sweetness of a soft kind when he dried the tears of a secretary whom someone had offended unjustly. Not so; with many an act or word of kindness would go some unique remark that devastated the occasion for the kindness shown. Many a witticism drags in the telling for the simple reason that few can reproduce the relations of circumstance and effect on the listener. Frequently, it was only when blood oozed that the listener realized there had been a rapier thrust.

The following dialogue is reported by Doctor Rowntree to have taken place when Osler returned to Baltimore and Rowntree led him to Jones's laboratory.

Osler Well, Doctor Jones, so you are still wasting your time playing with test tubes. One of these days the Grim Reaper will come and afterward people will say: "Didn't Walter Jones do this?" Later they will say: "Didn't Jones do this?" Finally they won't know who did this.

Jones I see you've become a pessimist since you left Hopkins. No, Doctor Osler, we are not forgotten. Only the other day someone asked me: "Didn't Hippocrates do so and so?"

Osler Hippocrates! He was one in a hundred million.

Jones Oh, well, there are others. Every week some one says, "Galen did that!"

Osler Ah, but Galen was one in a hundred million!

Jones By the way, Doctor Osler, how long is it since you left Baltimore?

Osler Five years.

Jones I'll swear I heard your name mentioned in those five years.

Walter Jones's early training developed an intense idealism; the later training turned its values to the service of a "tough-minded" realism. The incompatibles encountered on the way gave his conversational abilities their chief opportunity, but they seem to have had a very deep significance for the man himself. Unfortunately, we can judge this only from the impression of a profound intellectual struggle. It is hard for me to believe that

the constant recurrence of all sorts of profound questions in his conversations meant only the creation of an opportunity to talk, as some would have us believe. Nor can I see much in individual citations of the way topics were handled. In their isolation they often lack point. It is the impression of a constant hammering that seems to denote perpetual struggle. What would appear on the surface and without warning would be something of the following order. A colleague, bumped into while passing a doorway, is confronted.

Jones, opening and closing a jack-knife, "Tell me, What do you see me doing?" A facetious reply is stopped half-said. "Answer me! What do you see me doing?"

A, "Why, Doctor Jones, you opened the knife; then you closed it."

J, "Of course, and now tell me this. When I opened the knife, what became of the closedness? When I closed the knife, what became of the openness?"

A, "But, Doctor Jones, that's . . ."

J, "Of course it is; but that's Kantian philosophy!"

No one would claim that Walter Jones was a profound thinker. Everyone will admit that he hit with deadly aim the nonsense current in the common ideas of his time. They might be ideas of primordial creation or the square root of minus one that were the topics of discussion around the lunch table. It was not his object to expound but to take the ideas that were current and lash the nonsense unmercifully. To find the significance of the fact that the square root of minus one irked Walter Jones one must not think of him dealing with that quantity's logical arrival, its place in a broad theory of numbers, or its practical use, as in electrical theory. One must recall the verbal logic without attachment of extended meaning fed to most of us in the texts of our youth—that verbal nonsense which still permeates much elementary instruction today and bespeaks the failure of the universities to organize advanced learning in such a way as to make its reduced elements senseful. Of like significance is the story of Jones's argument with a physicist over the meaning of "dextro" and "laevo" with reference to the rotation of the plane of plane-polarized light. Since few advanced treatises record the historical change of convention and hardly any text states

any convention clearly, it may have been natural that one of the disputants followed the older and one the newer convention. Walter Jones clinched his argument by rushing to his polariscope with a solution of D(d) glucose [d(+)] glucose] and pointing to the direction in which he rotated the analyzer.

So, in all matters, Walter Jones demanded attachment of significance to words,—where possible, significance that could be made concrete and demonstrable. There at last he found his joyful satisfaction and his willingness to forego the delusive satisfactions of the philosophic WHY. Said he, "No question in biology that begins with *why* will ever be answered."

Yet he never questioned the satisfactions of others. Because his friends in all callings felt this, they would allow Walter Jones to hit his hardest. And hit he did to the consternation and discomfort of the unknowing.

In the fall of 1923, Doctor Jones wrote of "a remarkable weariness in my left leg after walking a few blocks." It soon became evident that he was suffering from thrombo-arteritis and in January he was unable to meet his classes after the first lecture. Thereafter he appeared at the laboratory only to discuss general matters. The unaccustomed restraint worried him no end and finally he admitted, "I have a case of 'nerves'". Then he abandoned work entirely. He retired in 1927 and recovered his health slowly and incompletely.

In 1921 tentative plans had been made for "a new building for physiological chemistry". The immediate necessity arose from the expanding work of the Departments of Physiology, Pharmacology and Physiological Chemistry and of the offices of administration, all housed in the Physiology Building. Chemistry was confined to the attic. The state of affairs there may be imagined from the fact that the hood available for students was so badly ventilated that adjustments of Kjeldahl digestions had to be made between rushing into and out of the room with hated breath. The purveyor of student supplies could hardly turn around in his coop. Every inch of space, including that under the eaves, was used, and two polariscopes were set up in a room so small that the doorway was in constant use as a means of turning around.

Therefore, it should have been with high hopes that Professor Jones accepted the chairmanship of a committee on plans for a new building. Nothing concrete was accomplished until after Jones's retirement. Then, after the completion of the "New Physiology Building" in 1929 there occurred a little incident that should be recorded.

After a long absence from his old department, Walter Jones appeared to lunch with the new staff. As of old he held the course of conversation, and long enough for a secret messenger to obtain flowers for the laboratory that had been reserved for the Professor Emeritus. Then he was led to his new quarters. There Walter Jones became for a moment his old self sending the faithful "Andy" on trips for glassware and reagents. Suddenly, he disappeared never to return. Later it was learned that there had come over him a discouraging sense of ground lost and of lack of energy where energy had known no bounds. Nor could he be induced to try again where once the sense of commanding superiority had been lost. He knew then that he had retired completely to pleasant places on the shore or in the mountains during summer months and that music and bridge must keep him diverted in the winter. For excitement, he played with the stock market or drove his car "two thousand miles a month" searching all the byways of Maryland's beautiful countryside.

Walter Jones died in Baltimore, February 28, 1935. Mrs. Jones died the following year, November 18, 1936.

Before we take up the record of his scientific work, let us revert to the happier days before the incapacitating illness as those days are described in the following letter from Dr. B. E. Read.

"Henry Lester Institute of Medical Research
"Shanghai, China
"May 25, '35

"Dear Professor Clark:

"I regret that foreign travel has kept me from storing old letters and that I have not preserved any of Walter's delightful compositions.

"It was upon Doctor Welch's recommendation that Professor Jones was willing to accept my unworthy self to work in his

laboratory in 1916 and 1917 for eighteen months' intensive study of nucleic acids and derivatives. I was privileged to enjoy the warmth of his friendship which for years afterwards he sustained by an occasional postcard or message through mutual friends.

"Jones's appreciation published in *Science*¹⁰ is an admirable summary of his rare qualities. I, for one, realized quite frequently that his warm personality could be scorching hot. In his contacts with his fellow men his mind acted as a refiner's furnace, from which the more timid shrank, by which the unaware were rudely surprised, and in which the more courageous found a degree of salvation.

"He had an intense love of his fellow men unappreciated by the victims of his assaults,—assaults upon everything small, weak or mean in men around him. At the conclusion of his speech at the New York dinner of the Federation in 1917 he confessed his own creed in the words:

"This above all: to thine own self be true,
And it must follow, as the night the day,
Thou canst not then be false to any man!"

"Walter Jones's contemporaries will witness to the high order of his intellect, thousands know of the brilliance of his classroom talks, a few, like myself, were privileged in a remarkable way to know his daily round of sound reason and extravagant wit. Whilst Walter Jones isolated himself from his fellows more than any scientific man I have known, intellectually he saw them and himself as one in the great struggle for the triumph of reason; and underneath his mocking laughter was a human sympathy of a very fine order.

"I had more courage than usual one day when I said to him, 'Professor Jones, I saw an unusual example of American spelling this morning.'

"Oh, what was it?"

"A negro fish shop on Monument Street had a notice of "CLAMPS" for sale.'

"Jones's reply was instantaneous, 'Oh, that's nothing. When I was in England they gave me lead in my plum pudding.'

"That kind of banter made guanylic acid not quite so sticky and the morning's work passed quickly and Wright Wilson, Sam Goldschmidt and Eddie Plass would gather round the ham sandwiches for a session of seasoned wit more carefully considered and narrated.

¹⁰ *Science*, March 20, 1935.

WALTER (JENNINGS) JONES—CLARK

"Before his brilliant lectures Jones would pace his private laboratory in agony like a mother praying for the life of her child. More intimate details would reveal the fact that in his synthesis of thought and feeling Walter Jones exhibited real genius.

"Sincerely yours,
"Bernard E. Read."

AFFILIATIONS WITH LEARNED SOCIETIES

The National Academy of Sciences. Elected member in 1918.
The American Physiological Society. Elected member in 1898.
The American Society of Biological Chemists.
Councilor in first group of officers, 1907, and in 1920 and 1921.
Treasurer 1910-12.
President 1915 and 1916.
Journal of Biological Chemistry: Editorial Committee, 1905 (vol. 1) to 1929 (vol. 84).
Society for Experimental Biology and Medicine.
Elected member 1905. Resigned 1921.
American Association for the Advancement of Science.
Elected member 1928, Fellow 1931.
Phi Beta Kappa. Elected member 1906.
Sigma Xi. Signed the petition for the establishment of the Johns Hopkins Chapter. No record of membership.

SCIENTIFIC WORK

Walter Jones's dissertation (1891)¹¹ describes the preparation and analyses of some new sulphonphthaleins. The work was part of a series of investigations being carried out in Remsen's laboratory and is of little importance beyond that extension of the record which is necessary in such cases.

As an associate of Winthrop E. Stone at Purdue, Jones was co-author of a paper (1893) regarding the digestibility of pentosans. In 1894 he published from Purdue a note regarding the dichlorides of orthosulphobenzoic acid. Others had reported that the reaction of phthalyl chloride with "hydrosulphide" yielded

¹¹ Published in the *American Chemical Journal* (1895).

$C_6H_4 \begin{matrix} CS \\ \langle \rangle O \\ C=O \end{matrix}$. Jones expected a similar compound on similar

treatment of a dichloride of orthosulphobenzoic acid but he ob-

tained $C_6H_4 \begin{matrix} CH_2 \\ \langle \rangle O \\ SO_2 \end{matrix}$ under various conditions. Apparently

Jones had hoped that he might obtain additional evidence of the isomers of the chloride of orthosulphobenzoic acid which had been under active investigation in Remsen's laboratory but no definite contribution to this subject appeared.

Jones's first paper from "the Laboratory of Physiological Chemistry of the Johns Hopkins University" was published with Thomas B. Aldrich as senior author and describes the isolation and identification of α -methyl-quinoline as a constituent in the secretion of the anal glands of *M. mephitica*. This was an addition to the list of nitrogenous secretory products.

Many years later W. Hoffman continued work in Jones's laboratory on the secretion of the anal glands of the skunk. Then people in neighboring laboratories recalled the earlier days and were made aware of the inadequacy of the hoods in the chemical laboratory. One bottle of skunk secretion, shipped from West Suffield, Connecticut, was broken in transit and was followed by a letter from the shipper saying:

"I sent one pint and box was proper but I have had H— from Express Co."

Jones next turned to a study of melanins, following an exploration previously made by Abel and Davis. Jones stated in the opening sentence of his first paper: "The name 'Melanin' is a generic term which is used to include all the dark brown or black animal pigments, whether formed in the body by normal processes, or under pathological conditions." The chemistry of these pigments is by no means clear today. Jones's work upon them was largely preparative. By successively destroying other constituents of black horse hair, he prepared material that, when further subjected to a caustic alkali melt, yielded so-called melaninic acid. This was subjected to elementary analysis and

various characterizing tests. A second paper with Auer described the results of its oxidative treatment.

Perhaps it is not inappropriate to speculate upon what might have happened had Jones not found his talents in the next subject to which he turned. Find himself he did in the study of nucleic acids. That he might have continued in one field is not an improbable premise in view of the fact that after his enthusiasm for the study of nucleic acids had been aroused he never published on any subject not directly or indirectly concerned with these substances. That he might have continued his interest in the melanins is not improbable for they furnished an abundant opportunity for exploratory work to which Jones seems to have been adapted. Had he continued the way he had been going, he might have retraced the weary path of preliminary exploration that has proved necessary in the study of nearly every important group of natural substances and that usually comes to a blind alley until the state of the theoretical science attains new power to break through. It is fortunate that he was attracted to the nucleic acids. Enough was known of certain of their constituents to provide a sound basis for systematic chemical work. So much was not known that he was provided an abundance of opportunities. Existing discrepancies and confusions called forth Jones's peculiar talent.

Before considering the specific contributions of Walter Jones to our knowledge of nucleic acids it may be well to outline the state of the subject when he entered the field. Brief as this outline must be, it may aid in recalling the nature of the problems that he attacked and in revealing some of the objectives of his investigations.

The study of nucleic acids begins with the pioneer and classic work of Friederich Miescher. As a student under the anatomist His, Miescher was inspired to look beyond the range of the microscope for the chemical dimensions of morphological peculiarities and he went to Hoppe-Seyler's laboratory with his vision. He saw an opportunity to get at nuclear material by digesting the more easily attacked parts of pus and by this and other methods he obtained powders rich in nuclear material and having chemical properties unlike anything known before. What

Miescher considered to be the characteristic constituent he called "nuclein". In 1869 Miescher submitted a paper on this material to Hoppe-Seyler, the founder and editor of the *Zeitschrift für physiologische Chemie*. Hoppe-Seyler held this paper for confirmation by his students and they soon separated "nuclein" from various sources. In the meantime Miescher was called to Basel and there recognized an opportunity presented by the Rhine salmon. During their ascent of the river the salmon develop their reproductive organs enormously and the males become a potentially huge source of nuclear material since the spermatic fluid, when expressed through the *vas deferens*, consists largely of a suspension of spermatozoa the bulk of which is of nuclear origin. Seizing the opportunity, Miescher prepared from this source samples of "nuclein" of definite and reproducible properties. He was led to believe himself in possession of a salt of a new organic acid and an organic base that he called "protamine". The base was later to be characterized as one of the simplest of the proteins while the organic acid was to become known as nucleic acid.

In 1899, when Jones began his work, a nucleic acid had been separated from the "nuclein" (or nucleoprotein) but "nuclein" rather than its resolved components was still the chief material used in various sorts of investigation.

We must leave much of the detail of Miescher's work to the reviews given by Jones and by Levene and Bass¹² and note that, among the numerous investigators who followed Miescher's pioneer work, the one who was to become Jones's mentor was Albrecht Kossel. Kossel provided Jones a fairly direct intellectual heritage of the field since he had worked under Hoppe-Seyler who in turn had received his start in the field from his painstaking and brilliant student Miescher.

It was Kossel who, in the period 1879 to 1886, first definitely identified purines among the products of the hydrolysis of "nuclein" and recognized their source in the part later to be called nucleic acid rather than in the protamine moiety. It was Kossel who first recognized a pyrimidine in the hydrolysate. With Altmann's preparation in 1889 of the non-protein component,

¹² Jones, *Nucleic Acids* (1920). Levene and Bass, *Nucleic Acids* (1931).

which he called nucleic acid, the material for a more rigid system of constitutional studies was provided. The protamines were to furnish Kossel and others one distinctive group of materials and the nucleic acids another. Jones was to work on the nucleic acids. Although he may have done work on the protamines, he never published it.

With the identification of purines and pyrimidines in the hydrolysates of nuclein and nucleic acid the story of the nucleic acids becomes linked with the story of uric acid which had been more than a century in the making. It is often said that uric acid had the attention of more leading chemists than any other compound. It was discovered in 1776 by Scheele, perhaps the greatest of discoverers in the field of chemistry. Its elementary analysis was made by Liebig, the founder of quantitative organic analysis, and a comprehensive study of its decomposition products was published by Chemistry's Damon and Pythias, Wöhler and Liebig, in 1838. No less a one than von Baeyer undertook and nearly completed one synthesis of uric acid and Emil Fischer completed not only this synthesis but also brought a comprehensive order to the structural relations of the purines as he did for other groups of compounds that are of the greatest bio-chemical importance. While these names claim the attention of every casual student of chemical history, the historian will find many another notable name associated with uric acid, with its close relatives in the purine group and with the next of kin, the pyrimidines. The labors of the great and lesser had provided a very substantial body of information directly applicable to the study of nucleic acids. Nevertheless, this information had to be adapted to the new situation, and while the definitive constitutional studies were still in the making.

In later years it was to be proved that the two better known nucleic acids contain residues of the following substances.

<i>Thymus Nucleic Acid</i>		<i>Yeast Nucleic Acid</i>
Phosphoric Acid		Phosphoric Acid
Adenine } (Purines)		{ Adenine
Guanine } (Pyrimidines)		{ Guanine
Cytosine }		{ Cytosine
Thymine }		{ Uracil
d-2-Desoxyribose (Carbohydrates)		d-Ribose

Between the beginning of Jones's study (1899) and the identification of d-ribose by Levene and Jacobs ten years passed. Desoxyribose was found after Jones's work was ended. (Levene and London, 1929). Jones himself was never to be concerned extensively with the carbohydrate moieties. Pyrimidines were known as a group and had been dealt with extensively in the study of uric acid; but the first pyrimidine to be isolated from nuclein was thymine, which Kossel and Neumann had found in 1893. The study of its constitution was under way in 1899. Indeed, it was a study contributory to this that Jones was drawn into when he went to Kossel's laboratory as will appear later. Purines, of course, were known as a group and those related to nucleic acids had been found in various natural products; but while the important adenine had been discovered by Kossel in 1885, it was not till 1897 that its relation to uric acid was definitely established by Fischer.

In 1899 it was not proved that the known constituents of a nucleic acid are linked in one molecule. An individual nucleic acid might contain but one purine. There was little vision of how components might be linked together in one nucleic acid. Nucleotides, with the order of linkage phosphate-sugar-purine (or pyrimidine) were unrecognized as pertinent to the studies of nucleic acids. Two nucleotides, inosinic acid (Liebig, 1847) and guanylic acid (Hammarsten, 1894) had been isolated from tissue, but their structures were unknown. Indeed Jones was to mention guanylic acid for several years as a nucleic acid. Nucleosides from sources other than nucleic acids had been known since 1885 but their uses in unravelling the structure of nucleic acids was not fully realized until the work of Levene and Jacobs in 1909 when they were recognized as representing the arrangement sugar-purine (or pyrimidine) in nucleic acid.

Thus a good deal of spade work remained to be done and the absence of those clear-cut constitutional definitions which determine the course of many sorts of investigation left the subject alive with puzzling problems. Perhaps it was because he had been plunged into the midst of these that Jones opened his monograph on the nucleic acids with the remark:

"The early development of nearly every scientific subject is marked by a set of conditions under which it is extremely difficult or even impossible to distinguish the important from the unessential, and unfortunately any misapprehensions which in consequence arise are likely to be so engrafted upon the nomenclature as to perpetuate themselves automatically."

The subject was outlined very dimly during the early days of Jones's work. He found a literature laden with discrepancies. He dealt with material that presented difficulties still felt. There is the unique constitutional complexity. Also it is difficult to prepare nucleic acids in purity sufficient for some of the refined uses of analysis and when this desired end is approached the materials do not lend themselves well to ordinary physicochemical tests for homogeneity, molecular weight, etc. Recollection of all this is essential to an appreciation of Jones's work.

Before a review of Jones's specific contributions, the polemic cloud that hangs over much of his writing must be explained and dispelled. In 1908 Jones said, ". . . the literature on the subject reveals a mass of contradictions, corrections and inconsistencies which it would seem almost impossible to reduce to any satisfactory scientific order." Jones was determined to bring order; he could not endure disorder. Consequently, we find that many of his papers have their immediate objectives in the resolution of discrepancies now forgotten. Were too much emphasis placed upon them, a review could miss the major objectives.

Levene and Bass say of the field in general: "It is singular that in the history of the chemistry of nucleic acids each new conclusion was reached by a path of disagreements, controversies, and errors, and that error often led to progress."

There is a tradition that Jones once ordered the course of a lecture by writing on the black board the names of contributors to the literature of nucleic acids; then crossed out the name of each after discussing his mistakes.

Be that as it may, it was inevitable that the shillalah of this forthright man would occasionally hit a hard head and elicit a reply. To dwell too long upon the resulting controversies, or to pick up each argument where Jones begins on smaller issues, would be to confuse the main issues. The only loss will be to

Hypoxanthine is formed by deamination of adenine. Xanthine is formed by deamination of guanine or by oxidation of hypoxanthine.

Reports of the occurrence of these four purines and in proportions that maintained no uniform relation had led Kossel to the supposition that there are four nucleic acids each containing the residue of one purine. On the other hand, Schmiedeberg had been led by his study of salmon nucleic acid to the assumption that adenine and guanine occur together in one nucleic acid.

One, who, like the writer, is not intimately acquainted with details of the technical procedures, will have difficulty in judging the extent to which the presence of mononucleotides in tissues confused the issue. Indeed, Jones was later to deal with one aspect of this. But, whatever may have been the extent of this source of confusion, there remained a distinct problem in the relation of these four purines to true nucleic acids. Jones drew the first of the students who were to publish with him on nucleic acids into an examination of this problem. The student was George H. Whipple.

It happened that Abel was then studying the pharmacologically active principle in the medulla of the suprarenal gland. The unused residues furnished Jones and Whipple a source of a nucleic acid that had been reported to contain a methyl purine and to be free of guanine,—unlike any of the nucleic acids from other sources. From their own examination of this material Jones and Whipple (1902) drew the conclusion that the nucleins of the suprarenal glands of sheep and beef are similar to that of the pancreas and that each yields adenine and guanine but no demonstrable amounts of other purines.

Thereafter Jones suspected that the reported xanthine and hypoxanthine might arise from deamination of the amino purines. First, it was advisable to check previous observations. Jones (1904) found that while guanine, adenine and thymine can be identified among the products of hydrolysis of thymus nucleic acid, if acid hydrolysis is used, autolysis of the gland results in the formation of xanthine and hypoxanthine. This was checked by studies of the spleen and suprarenal. Then tissues were

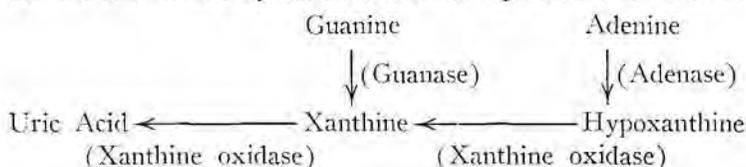
examined for deaminizing enzymes. Jones and Partridge (1904) reported guanase; Jones and Winternitz (1905), adenase.

The justification for inferring specific enzymes for the free purines was chiefly of two sorts; first, the conversion of added guanine to xanthine and the conversion of added adenine to hypoxanthine by infusions of certain tissues; second, failure of the one process and demonstration of the other by specific tissue infusions. Thus, a spleen infusion brought about an alteration of adenine to hypoxanthine, presumably by the action of an adenase, and thence to xanthine in the presence of air, presumably by the action of xanthine oxidase. On the other hand, guanine remained unaltered in this infusion. In contrast to this set of results an extract of pancreas completely changed added guanine to xanthine.

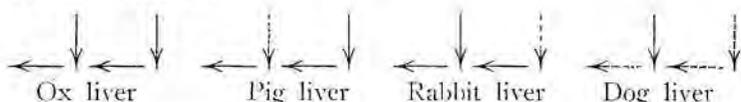
The failure of Jones to emphasize the species from which he obtained the spleen for his experiments led to some amusing and important consequences. Schittenhelm could not confirm Jones's report, but Schittenhelm had used ox spleen while Jones had used pig's spleen. When Jones discovered this he entered a controversy that determined the course of several of his subsequent investigations, for the sharp emphasis given to species and organ specificities led Jones and his students to extensive exploration.

The reader of Jones's papers will note that the initial controversy with Schittenhelm, beginning with the paper by Jones in 1905, is pursued in other directions. Indeed, the full flavor of it developed outside the printed page. Doubtless some of the cryptic remarks that appear in Jones's later papers and in his monograph lack clarity for the reason that their full meaning needed the expansive atmosphere of the lecture room.

The first findings in regard to the distribution of deaminizing enzymes are summarized by Jones in the following diagrams. Let the scheme of enzymatic activities be represented as follows:



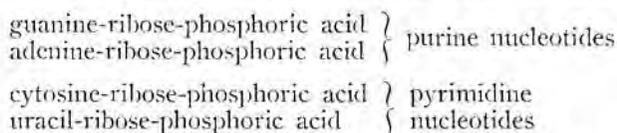
If a full line arrow represent the indicated enzymatic activity and a dotted arrow represent its absence, distribution may be shown as follows:



See Jones and Winternitz (1905), Jones (1905), Jones and Austrian (1906), Jones and Austrian (1907).

That the enzymes are formed successively during embryonic growth was shown by Jones and Austrian (1907), Jones and de Angula (1908) and by others, giving additional presumptive evidence of their individualities. This use of embryos has been followed in other biochemical studies.

Levene and Jacobs (1909-10) then demonstrated the order of linkage in inosinic acid to be: phosphoric acid—pentose—hypoxanthine. By acid hydrolysis they obtained a nucleoside. They discovered its pentose to be the hitherto unknown d-ribose (Levene and Jacobs, 1909-12). Then they extended their methods to yeast nucleic acids and obtained the four nucleosides: guanosine, adenosine, cytidine and uridine. There was now presumptive evidence that yeast nucleic acid is composed of the residues of four nucleotides in which the orders of linkage are:



There was at hand a constitutional basis upon which to erect a working hypothesis of the enzymatic degradation of yeast nucleic acid.

In the period that follows, Jones recognized that deamination of the purine may occur either while the purine is combined or after it is set free. Having previously reported that guanylic acid is unaffected by pig's pancreas, as judged by the failure of that tissue to catalyze the liberation and deamination of guanine, Jones reinvestigated the matter in 1911 and reported the liberation of phosphoric acid. Therefore, he corrects the interpretation of the previous findings. Guanosine (not guanylic acid)

is unaffected. On the other hand, adenosine is converted to inosine by pig's pancreas, that is, deamination now occurs while the purine is combined.

In the same year Amberg and Jones (1911) showed that although dog's liver forms hypoxanthine from yeast nucleic acid (which would admit of either of two routes) it does not form hypoxanthine from free adenine. Therefore, it was assumed that the liver contains the deaminizing agent, adenosinase, but not the other deaminizing agent, adenase. It also contains the nucleosidase which, if specific, is inosine "hydrolase", but not adenosine "hydrolase".

On the other hand, Amberg and Jones (1913) found that yeast is unable to deaminize adenine whether free or combined. Yeast also shows the specificity of its polynucleotidase by attacking yeast nucleic acid but not thymus nucleic acid.

In Jones's lectures he often emphasized that an enzymatic action upon a specific group may depend upon the mode of linkage of the group carrier with some other residue and he anticipated in some measure what was to be shown subsequently regarding phosphorylated metabolites in general.

The recasting of the scheme of enzymatic nucleic acid degradation, initiated by the constitutional investigations of Levene and Jacobs, was also dealt with specifically by Levene and Medigreccanu in 1911, and has since been further elaborated in directions that we need not follow.

The formulation of yeast nucleic acid as a tetranucleotide opened the possibility that a highly selective catalysis of its hydrolysis might result in the formation of dinucleotides or even a trinucleotide as well as mononucleotides. Jones and Richards (1914) believed they had found a dinucleotide resulting from the action of fresh pigs' pancreas on yeast nucleic acid. These "dinucleotides" were described further by Jones and Richards in the following year. The subject was to be followed further in those studies with unorganized hydrolytic agents that will be mentioned in another connection.

In the late period of his work Jones devoted less attention to enzymes. However, in 1920 he described a most remarkable experiment that indicated a thermostable agent in pigs' pancreas by the use of which yeast nucleic acid was split into its four

nucleotides and possibly into dinucleotides initially. (See also Jones, 1922.) Jones and Perkins (1923) returned to this as a means of studying constitution. Levene and Bass (1931, p. 312) state that in unpublished work Levene "was not successful in his attempts to repeat the experiments of Jones". There the remarkable observations on the thermostable agent seems to rest, but the repeated use of the procedure by Jones leaves no doubt that it deserves further study. After the comment on the thermostable agent was written, Jones's observation was confirmed by Dubos and Thompson. See *J. Biol. Chem.*, *127*, 501 (1938).

The series of enzymatic accelerations leading from nucleic acids to uric acid (in man) has an obvious bearing upon the problem of gout and, while this problem involves other phenomena, the clarification of the enzymatic processes in man is a necessary part of the resolution. The few articles in which Jones touches upon this are characteristically confined to items that can be discussed with available experimental data. This is the more remarkable because few other subjects had received more speculative treatments in medical circles. Osler, to be sure, was very cautious but it was not until the 8th edition (1912) of his *Practice of Medicine* that the discussion of the etiology of gout acquired such definiteness as chemistry had provided.

Miller and Jones (1909) examined the Brugsch-Schittenhelm theory with the developing technique of enzyme manipulation. The theory was to the effect that a disturbance in the chain of enzymatic degradation of nucleic acids leads to an accumulation of uric acid. Miller and Jones did find that the organs of a patient (dying of nephritis) who had had gout were unable to destroy uric acid, but Wicichowski had shown that the organs of a gout-free man also lacked uricase. This decisive finding was confirmed by Miller and Jones. Other findings, particularly the absence of adenase in human organs, seems to have suggested to Jones the possibility of tracing some other anomaly of purine metabolism that might have a bearing on gout. The suggestion led to no systematic work on gout.

Leonard and Jones (1909) recognized that the developing knowledge of purine metabolism might account for exogenous uric acid and took cognizance of the developing problem of the

endogenous origins. Burion and Schur had found that perfused muscle produces uric acid at the cost of its hypoxanthine. Observing that the voluntary muscles of the pig, dog and rabbit cannot convert adenine to hypoxanthine, Leonard and Jones assumed that this ruled out nucleic acid as the source of the hypoxanthine known to be present in such muscles and then suggested that this set of data contributes to our knowledge of the source of endogenous uric acid. Voegtlin and Jones (1910) later perfused surviving muscle and found no alteration of adenine.

The production of uric acid in disease is touched upon again by Rohdé and Jones (1910) who found no difference between the enzymatic actions of normal and diseased rat organs. In this same paper endogenous uric acid is again touched upon.

In the article *On the Threefold Physiological Origin of Uric Acid* Jones reviewed the enzymatic studies up to 1910 with particular reference to the subject of gout. He indicates that uric acid may arise from the degradation of ingested nucleic acid, from the hypoxanthine (or its precursor) of the muscle, or through a *de novo* synthesis of the purine ring. Of course, it had been known for a long time that the latter takes place, for instance in starving salmon. Ascoli and Izar in 1908 had given a particular slant to the rôle of uric acid synthesis by claiming that uric acid is destroyed in tissues under aerobic conditions and is resynthesized from the products under anaerobic conditions. Calvery (1927), working in Jones's laboratory, could not confirm the findings of Ascoli and Izar.

Mention already has been made of the fact that gout had long been a subject of wild speculation. In view of this and the fact that Jones was frankly an enthusiast on the subject of purine metabolism one cannot but admire the restraint that he exhibits in his occasional writings on gout, a subject that still remains obscure.

STUDIES BEARING MORE SPECIFICALLY ON CONSTITUTION

As already stated, Jones had contributed to the view that only the residues of adenine and guanine and not those of xanthine and hypoxanthine are native to nucleic acids. An apparent

anomaly was presented by the case of guanylic acid which is now known to be a mononucleotide but which, in 1908, was still called a nucleic acid (see Jones and Rowntree, 1908). The products formed by the hydrolysis of guanylic acid had been in dispute and even the existence of the substance had been denied.

Jones and Rowntree (1908) were able to separate the well-established thymonucleic acid from the guanylic acid of the pancreas as did Levene and Mandal (1908) at the same time. Jones and Rowntree established the wide distribution of guanylic acid. They found guanine to be the only purine liberated on hydrolysis. In the same year Jones compared the nucleic acids of the thymus, spleen and pancreas, utilizing in his experimental work the means he had devised for freeing the preparations from guanylic acid. He gave additional evidence of the identity of this nucleic acid. It is noteworthy that in this paper, where there is resolved the confusion in the literature introduced by contaminating guanylic acid, this substance is spoken of as a "nucleic acid (if it be properly so called)". When Jones returned from further excursion in the enzyme field to the study of constitution, he recognized the general advance in the knowledge of constitution made by Levene and Jacobs and particularly their contribution to our knowledge of guanylic acid and he saw that guanylic acid is a mononucleotide (Jones, 1911) and yeast nucleic acid presumably a tetranucleotide (Jones and Richards, 1914).

Readily accepting the initial and decisive clarification of constitution that Levene and Jacobs had produced, Jones now turned his talents to some isolations projected by the new concepts of structure. From these it was reasonable to suppose that a residue of guanylic acid should be a component of yeast nucleic acid and that suitable hydrolysis of yeast nucleic acid should yield this guanylic acid. Jones in 1912 reported its isolation from a hydrolysate. Jones and Richards, 1914, reported a more detailed study of this substance. It was not emphasized that the constituents of a guanylic acid can be joined in different ways. Consequently, Jones's study of properties was not adequate to prove conclusively that the material isolated from yeast nucleic acid is identical with naturally occurring guanylic acid.

Evidence of the identity was shown later by Levene. Nevertheless, a milestone had been set.

In attempts to hydrolyze a nucleic acid by a partial cleavage that would leave dinucleotides, Jones and Germann (1916) tried Levene and Jacobs's method which consisted of heating the nucleic acid with ammonia at high temperatures. Jones and Germann found reasons for preferring lower temperatures. This and other hydrolytic processes were examined in some detail; stepwise oxidation by permanganate was also tried. By the latter means Jones and Kennedy in 1919 apparently removed the cytosine, uracil and guanine nucleotide groups of yeast nucleic acid and then they isolated for the first time in crystalline form adenine mononucleotide. Another milestone had been set. This isolation of adenine nucleotide had a three-fold significance. From a technical point of view it was an achievement. It joined with the isolation of guanylic acid in confirming the projected polynucleotide structure of yeast nucleic acid. Lastly, this nucleotide was the first of the group of adenine nucleotides to be studied and became the prototype of those compounds that now are seen to be central to several catalytic processes of the cell. Adenine nucleotide was described independently in the same year by Thannhäuser.

Thus there were identified in 1912 and 1919 two purine nucleotides stemming from yeast nucleic acid and giving concrete evidence of the polynucleotide structure of nucleic acid deduced from Levene's work.

In 1914 Jones and Richards reported the hydrolysis of yeast nucleic acid under conditions such that there were obtained "dinucleotides". This, surely, is a theoretical possibility. Its experimental pursuit was to be reported in several papers (Jones and Richards, 1915, Jones and Germann, 1916, Germann, 1916, Jones and Read, 1917, Read and Tottingham, 1917). In reviewing this subject in the second edition of *Nucleic Acids*, Jones remarks "without a single exception, every modern investigator in this field has prepared from yeast nucleic acid a substance which he believed to be a chemical individual containing more than one nucleotide group." While an examination of Jones's own reports leaves a good deal to be desired in the appli-

cation of criteria for individuality, it must be granted that the evidence found in his papers seems adequate to have supported the continuance of research. Where Jones erred was in using the assumed dinucleotides as material for the study of new problems before solving the basic problem of homogeneity. This criticism is so easy to write! Yet with these substances rigid tests have proved very difficult to apply,—so difficult that one wonders what would appear if neglected methods were to be applied to some of the materials reported even recently to be definite compounds. When Levene demonstrated that *certain* preparations of “dinucleotides” could be resolved into fractions each consisting of mononucleotides, Jones accepted this evidence that he had made a mistake in that particular instance, (see Jones and Abt, 1920). Was he *entirely* wrong in general? He implies that he thought dinucleotides to be real in the famous paper on constitution published with Perkins in 1923.

Jones had now entered a period when his genius for clarification had turned from long standing discrepancies in the literature to problems of constitution that were in the making. Mistakes were inevitable but no review of them is adequate without the realization that Jones reported fearlessly what he found and fearlessly admitted mistakes that he was convinced were such.

In 1916 Jones gave a presidential address before the Society of Biological Chemists. In the part that was published he notes that yeast nucleic acid when submitted to 5 per cent sulfuric acid at 100° C. liberates phosphoric acid as if from two sources—the purine nucleotides that release their phosphoric acid residues rapidly and the pyrimidine nucleotides that release their phosphoric acid residues very slowly. In this brief paper there is no very satisfactory analysis of the experimentally observed curve relating phosphoric acid liberated to the time of heating but in a subsequent series of papers individual nucleotides were examined and the results bore out the conclusion that there is a distinct difference in the ease of dephosphorylation of the purine and pyrimidine nucleotides. More exact measurements on rates were to be made later by Levene and Yamagawa. The method was to prove very useful. There is some confusion in following Jones's application of the method because he used

it in dealing with what he supposed to be dinucleotides, and in arguing on modes of linkage. If the central theme be reconstructed with the elimination of these special arguments it reduces to the following. The rate of dephosphorylation of yeast nucleic acid indicates two steps, the first *corresponding to* the rapid dephosphorylation of *isolated* purine nucleotides and the second to the slow dephosphorylation of *isolated* pyrimidine nucleotides. Jones does not deal clearly with possible modifications of rate that might be assumed if some of the phosphoric acid residues, which are terminal in nucleotides, are linked in the nucleic acid itself, although he stated in his 1922 paper on the thermostable agent of pig's pancreas that he had given consideration to this. He leads one to infer that he considered his data proof of the terminal position of the phosphate residues in nucleic acids. This sometimes obscures appreciation of Jones's finding that equal quantities of phosphate are set free in each of the two distinct steps. This latter fact remains one of the outstanding pieces of presumptive evidence that there are equal proportions of pyrimidine and purine nucleotide residues in nucleic acid. In the absence of precise quantitative analytical methods the physico-chemical evidence takes the lead. The method when applied to the elucidation of the modes of linkage led Jones to suggestive but inherently weak arguments.

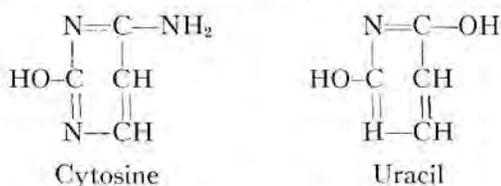
His next argument on structure utilizes some data on the number of free acid groups in yeast nucleic acid—data which Levene promptly criticized.

In 1923 (Jones and Perkins, 1923) Jones protested that he had never defended a formula for the mode of linkage of the nucleotides in yeast nucleic acid, having confined his efforts to "attacks on formulas proposed by others". "Now we change from the offensive to the defensive and propose the following formula." With respect to modes of linkage of nucleotides, with which alone Jones was then concerned, his formula differed from that of Levene (which is now copied in texts) by showing one ether linkage between carbohydrate and carbohydrate. I have established that the evidences in this paper were there *summarized* only after Jones had repeated the experiments again and again. Possible misinterpretations of the observations are

difficult to trace because in neither this nor other related papers are the protocols sufficiently detailed for this purpose. With these remarks I must leave detailed criticism of Jones's evidences to those more familiar with the field. None of the evidences which I happen to have seen for the modes of linkage of nucleotides in nucleic acid appeals to me as rigid proof or more than presumptive and since the evidence before me leaves contradictions that I would not presume to discuss without additional data it would be improper for me to defend or oppose Jones's conclusions.

Perhaps new evidence that certain nucleic acids are large polymers of nucleotides will force reconsideration of data that hitherto have been interpreted under the presumption of a tetranucleotide constitution.

In one of the last of Jones's publications he turns to the question of whether there is a parallelism between the deamination of purines and the deamination of pyrimidines during treatments of nucleic acids. The cytosine and uracil reported in yeast nucleic acid stand in the following relationship.



Jones and Perkins (1925), on hydrolysis of yeast nucleic acid with 1 per cent NaOH solution, failed to find the uracil nucleotide. They say: "In so far as this result is of value the conclusion is obvious that the oxypyrimidine derivatives (uracil, uracil nucleoside, and uracil nucleotide) are not referable to an oxypyrimidine group in nucleic acid but are secondary products formed during hydrolysis by deaminization of the corresponding cytosine derivatives or their precursors."

Levene and Bass (1931, p. 276) refer to this paper as reviving the trinucleotide theory of yeast nucleic acid. If so, it was by inference, for no statement to that effect occurs in the paper.

Later Jones and Calvery (1927) discovered that the "failure

of Jones and Perkins and of Calvery to isolate uracil nucleotide from the hydrolytic products of yeast nucleic acid was due to a loss of material". By use of ammonia hydrolysis they obtained an hydrolysate from which all of the expected nucleotides were isolated.

It should not be overlooked that the failure to find uracil had been a *fact* that stood not only against accepted views but also against a group of evidences to which Jones himself had contributed. The publication might be said to have been premature but it cannot be denied that it was courageous. The withdrawal replaced Jones in the rôle in which he was supreme—the resolver of discrepancies—this time one originating in his own work.

NOTES ON MISCELLANEOUS PARTS OF JONES'S WORK

There were four papers published with Gamgee on the optical activity of nucleic acid and nucleoproteins. These papers were published in 1903. They bear internal evidence of Gamgee's authorship but that Gamgee was the author this writer has no proof. Aside from an abundance of entertaining remarks the papers present the then interesting observations that the nucleoproteins are unlike most proteins other than hemoglobin in that they are dextrorotatory, that the specific rotation of nucleic acid differs from that of the nucleoprotein, and that the specific rotations vary with the acidity of the solutions. Later Jones (1908) was to use the identity of specific rotations under fixed conditions and the uniformity of their variations with changes of the acidity of the solutions as an argument that the nucleic acids of the thymus, spleen and pancreas are identical. The optical properties of solutions of nucleic acids were to be referred to occasionally as, for example, by Amberg and Jones (1911), but were not extensively used. The change of rotation with change of the solution, especially its acidity, was a very important observation.

Since Jones was associated with Kossel at a time when the separation and analytical determinations of purines and pyrimidines were developing and since most early methods of sepa-

rating nucleic acids were empirical and their improvements dependent upon accretions of experience, it is difficult to appraise the originality of some of Jones's contributions to preparation and analyses. These aspects are so seldom emphasized in Jones's papers as to lead one to believe that, while he contributed his part, he would not have claimed a large portion of credit. In a letter to C. A. Morrow he writes:

"I do not know whether or not the description given in my monograph for the preparation of guanine and adenine from yeast nucleic acid is original. I have never seen it described in just this form anywhere. But you have to take into consideration that the preparation of guanine and adenine is very much more difficult with animal nucleic acid than with plant nucleic acid and the earlier descriptions for the preparation of the two bases applied to the more difficult preparation from thymus nucleic acid."

The two papers on phosphorus determination (1916, 1923) may be regarded as indulgencies. It is often said of a good analyst that he can do better with a poor method than a poor analyst can do with a good method. If Jones preferred to dust a dried precipitate of ammonium magnesium phosphate from the filter paper and discard the paper rather than to ignite and convert the phosphate to pyrophosphate it was doubtless his privilege and it was evidently his joy to show a reliability consonant with his own requirements.

In the closing years of Jones's career he was entering a field for which he was eminently suited—the examination of tissues for new material related to the compounds of his earlier studies. Mention has already been made of his studies of the naturally occurring guanylic acid, and of his interest in inosinic acid. Had space permitted we would have discussed his occasional concern with "β-nucleoproteins," substances or mixtures some of which had yielded guanylic acid. In 1922 Jones and Perkins turned their attention to those nucleotides in animal tissues that differ from the nucleotides of "animal" nucleic acid in having the pentose of "plant" nucleic acid. They say: "We were formerly inclined to believe that the presence of plant nucleotides in animal tissues is caused by the plant food which the animal consumes. But the tentative and confessedly inadequate evidence upon

which this view was based has since been found erroneous." Jones and Perkins then recovered from the " β -nucleoprotein" of the pancreas not only guanine nucleotide but also cytosine and adenine nucleotides that bore every resemblance to the corresponding *ribo*-nucleotides from yeast nucleic acids. The inference was that a *ribo*-nucleic acid is native to the animal, a view later confirmed by Jorpes. Said Jones: "It thus seems more than probable that the distinction between animal and plant nucleic acid will in the future not be so definitely drawn." Up to that time the distinction had been drawn sharply.

The reviving spirit of exploration that radiated from Walter Jones was made evident by unpublished examinations of corn and wheat "germ flours" and tubercle bacilli, by the note of Shaffer, Folkoff and S. Bayne-Jones on the nucleic acids of bacteria, by Calvery's examination of tea leaves, etc. Important contributions from Jones's laboratory were Buell's isolation of an oxyadenine from blood and her evidence that inosinic acid of muscle can originate in the adenine nucleotide.

Throughout his career Walter Jones counseled his assistants to pursue their own problems; but of the later period it may be said that he could not hold them from enthusiastic participation in cooperative and independent researches within his field. Of course, it is impossible to say what might have come out of his laboratory had not his illness cut short his own work and finally altered his staff. Nevertheless, it is clear that hands were getting close to remarkable substances that it was given to others to discover. Let imagination play with the dream of Walter Jones's enthusiasm could he now see that the materials which his hands almost touched are linking hitherto unrelated realms of research—catalysis of phosphorylation, catalysis of oxidation-reduction, vitamins of the B group, also the structure of chromosomes. Again the pursuit of understanding for its own sake, from which Walter Jones would not deviate, bids fair to place in the hands of the physician more power than frontal attacks have yielded.

Jones's Monograph, *Nucleic Acids*, the first edition of which was published in 1914 and the second in 1920, remained for many years the only comprehensive review available in English. It dealt briefly with the historical background and set forth the

chemistry of the then known components of nucleic acids. Structures were discussed. Properly, a good deal of attention was given to the enzymatic transformations which Jones had studied extensively. The appendix gave invaluable directions for important preparations and there was a good bibliography.

Arthur Harden, reviewing the second edition in 1921, wrote "To the biochemist this book cannot fail to be of profound interest, alike for the importance of the matter and the lucidity of the exposition." With me Walter Jones left the impression that while he remained modest in his claims for the *monograph* he considered its consolidation of knowledge his duty as a scholar.

From time to time Jones contributed to various books and reviews.

Professor Abel had written several of the reviews of physiological chemistry for Gould's American Year-Book of Medicine and Surgery and from 1900 to 1905 Walter Jones and Reid Hunt together took over this laborious duty. These reviews were occasionally punctuated with spicy remarks. Examples:

"Morner's discovery of two isomeric forms of cystin as hydrolytic products of horn may furnish food for reflection to the artists who are accustomed to draw pictures of the proteid molecule without giving the sulphur atom any consideration."

"By a curious mixture of good chemic argument and unwarranted assumption, the authors (Nencki and Zaleski) arrive at an appalling structure formula for hemin."

"The discovery of an analytic method always marks an epoch in chemical development. . . . Fischer now introduces a method of separating the hydrolytic products of proteids. . . ." (Fischer's famous ester method described at length).

The yearly comments on the developing knowledge of epinephrine, in which Abel kept the lead, are especially interesting and historically important.

The following bibliography of Walter Jones's scientific work is, I believe, complete except possibly the list of minor notices. Its order conforms in sequence to the numbering discovered in Jones's own, incomplete set of reprints and it contains titles not found in Jones's list of his papers,—the only document pertaining to his scientific work that was found among his effects after his death.

BIBLIOGRAPHY OF WALTER (JENNINGS) JONES

Separates, Books, and Contributions to Books

- Walter Jones. Sulphon-phthaleins Derived from Ortho-sulpho-para-toluic Acid. Dissertation, Johns Hopkins University. Baltimore. 1891. Press of Isaac Friedenwald Co.
- Anonymous. Supplementary Notes to Laboratory Work in Chemical Toxicology for the Use of Students in the Johns Hopkins Medical School. Privately printed. (No date.) [Note: Professor J. J. Abel, in whose laboratory it was used, states that this was written by Walter Jones.]
- Walter Jones and D. Wright Wilson. A Laboratory Manual of Physiological Chemistry. Johns Hopkins University, Baltimore. (No date.) Printed at the Waverly Press, Baltimore.
- Walter Jones. Nucleic Acids, Their Chemical Properties and Physiological Conduct. First edition, 1914; second, 1920. Monographs on Biochemistry. Edited by R. H. A. Plimmer and F. G. Hopkins. Longmans, Green and Co.
- Reid Hunt and Walter Jones. Non-alkaloidal Organic Poisons. In A Text-Book of Legal Medicine and Toxicology. Edited by Frederick Peterson and Walter S. Haines. Vol. II, p. 532. W. B. Saunders and Co., Philadelphia, New York and London, 1904.
- Walter Jones and Reid Hunt. Physiological Chemistry. (Reviews.) In The American Year-book of Medicine and Surgery. Edited by George M. Gould. Published by W. B. Saunders.
- 1900 Volume *Medicine*, p. 562.
- 1901 Volume *Medicine*, p. 612.
- 1902 Volume *Medicine*, p. 648.
- 1903 Volume *Medicine*, p. 638.
- 1904 Volume *Medicine*, p. 601.
- 1905 Volume *Medicine*, p. 620.
- Walter Jones. Nucleic Acids. In Endocrinology and Metabolism. Edited by Lewellys F. Barker. Vol. 3, p. 134. Appleton and Co., 1922.

Journal Articles

- W. E. Stone and Walter J. Jones. The digestibility of the pentose carbohydrates. *Agri. Sci.*, 7, 6, 1893.
- Walter Jones. A reduction product of orthosulphobenzoic chloride. *Am. Chem. J.*, 16, 366, 1894.
- Walter Jones. Investigations on the sulphonphthaleins. VI. Sulphon-phthaleins derived from orthosulphoparatoluic acid. *Am. Chem. J.*, 17, 556, 1895.
- T. B. Aldrich and Walter Jones. α -Methyl-quinoline as a constituent of the secretion of the anal glands of *mephitis mephitica*. *J. Exptl. Med.*, 2, 439, 1897.

- Walter Jones. The chemistry of the melanins. *Am. J. Physiol.*, 2, 380, 1899.
- Walter Jones. Ueber das Thymin. *Ztschr. physiol. Chem.*, 29, 20, 1899.
- Walter Jones. Ueber die Darstellung des Thymins. *Ztschr. physiol. Chem.*, 29, 461, 1900.
- Walter Jones and J. Auer. On the oxidation of native pigments. *Am. J. Physiol.*, 5, 321, 1901.
- Walter Jones and G. H. Whipple. The nucleoproteid of the suprarenal gland. *Am. J. Physiol.*, 7, 423, 1902.
- A. Gamgee and Walter Jones. On the nucleoproteids of the pancreas, thymus and suprarenal glands, with especial reference to their optical activity. *Proc. Roy. Soc.*, 71, 385, 1903.
- A. Gamgee and Walter Jones. On the optical activity of the nucleic acid of the thymus gland. *Chem. News*, 87, 303; *Proc. Roy. Soc.*, 72, 100, 1903.
- A. Gamgee and Walter Jones. Ueber die Nucleoproteide des Pankreas, der Thymus und der Nebenniere, mit besonderer Berücksichtigung ihrer optischen Aktivität. *Beiträge zur chem. Physiol. [Hofmeister]* 4, 10, 1903.
- A. Gamgee and Walter Jones. On the nucleoproteids of the pancreas, thymus, and suprarenal gland, with especial reference to their optical activity. *Am. J. Physiol.*, 8, 447, 1903.
- Walter Jones. Ueber das Enzym der Thymusdrüse. *Ztschr. physiol. Chem.*, 41, 101, 1904.
- Walter Jones. Ueber die Selbstverdauung von Nucleoproteiden. *Ztschr. physiol. Chem.*, 42, 35, 1904.
- Walter Jones and C. L. Partridge. Ueber die Guanase. *Ztschr. physiol. Chem.*, 42, 343, 1904.
- Walter Jones and M. C. Winternitz. Ueber die Adenase. *Ztschr. physiol. Chem.*, 44, 1, 1905.
- Walter Jones. Ueber das Vorkommen der Guanase in der Rindermilz und ihr Fehlen in der Milz des Schweines. *Ztschr. physiol. Chem.*, 45, 84, 1905.
- Walter Jones and C. R. Austrian. Ueber die Verteilung der Fermente des Nucleinstoffwechsels. *Ztschr. physiol. Chem.*, 48, 110, 1906.
- Walter Jones and C. R. Austrian. On thymus nucleic acid. *J. Biol. Chem.*, 3, 1, 1907.
- Walter Jones and C. R. Austrian. On the nuclein ferments of embryos. *J. Biol. Chem.*, 3, 227, 1907.
- Walter Jones and L. G. Rowntree. On the guanylic acid of the spleen. *J. Biol. Chem.*, 4, 289, 1908.
- Walter Jones. On the identity of the nucleic acids of the thymus, spleen, and pancreas. *J. Biol. Chem.*, 5, 1, 1908.

- M. C. Winternitz and Walter Jones. Über den Nucleinstoffwechsel mit besonderer Berücksichtigung der Nucleinfermente in den menschlichen Organen. *Ztschr. physiol. Chem.*, 60, 180, 1909.
- M. C. Straughn and Walter Jones. The nuclein ferments of yeast. *J. Biol. Chem.*, 6, 245, 1909.
- V. N. Leonard and Walter Jones. On preformed hypoxanthin. *J. Biol. Chem.*, 6, 453, 1909.
- J. R. Miller and Walter Jones. Über die Fermente des Nucleinstoffwechsels bei der Gicht. *Ztschr. physiol. Chem.*, 61, 395, 1909.
- A. Rohdè and Walter Jones. The purin ferments of the rat. *J. Biol. Chem.*, 7, 237, 1910.
- Walter Jones. Über die Beziehung der aus wässerigen Organextrakten gewonnenen Nucleinfermente zu den physiologischen Vorgängen im lebenden Organismus. *Ztschr. physiol. Chem.*, 65, 383, 1910.
- C. Vöglin and Walter Jones. Über Adenase und ihre Beziehung zu der Entstehung von Hypoxanthin im Organismus. *Ztschr. physiol. Chem.*, 66, 250, 1910.
- Walter Jones. On the threefold physiological origin of uric acid. *Johns Hopkins Hospital Bull.*, 21, 240, 1910.
- G. DeF. Barnett and Walter Jones. On the recovery of adenine. *J. Biol. Chem.*, 9, 93, 1911.
- Walter Jones. Concerning nucleases. *J. Biol. Chem.*, 9, 129, 1911.
- Walter Jones. On the physiological agents which are concerned in the nuclein fermentation, with special reference to four independent desamidases. *J. Biol. Chem.*, 9, 169, 1911.
- S. Amberg and Walter Jones. On the application of the optical method to a study of the enzymatic decomposition of nucleic acids. *J. Biol. Chem.*, 10, 81, 1911.
- S. Amberg and Walter Jones. Über die bei der Spaltung der Nucleine in Betracht kommenden Fermente mit besonderer Berücksichtigung der Bildung von Hypoxanthin in der Abwesenheit von Adenase. *Ztschr. physiol. Chem.*, 73, 407, 1911.
- Walter Jones. On the formation of guanylic acid from yeast nucleic acid (preliminary communication). *J. Biol. Chem.*, 12, 31, 1912.
- S. Amberg and Walter Jones. The action of yeast on yeast nucleic acid. *J. Biol. Chem.*, 13, 441, 1913.
- Walter Jones and A. E. Richards. The partial enzymatic hydrolysis of yeast nucleic acid. *J. Biol. Chem.*, 17, 71, 1914.
- Walter Jones and A. E. Richards. Simpler nucleotides from yeast nucleic acid. *J. Biol. Chem.*, 20, 25, 1915.
- Walter Jones. An indirect method of determining pyrimidine groups in nucleotides. Presidential address, American Society of Biological Chemists. *J. Biol. Chem.*, 24, Proc. iii, 1916.
- Walter Jones. The admissibility of ammonium magnesium phosphate as a form in which to weigh phosphoric acid. *J. Biol. Chem.*, 25, 87, 1916.

- Walter Jones and H. C. Germaun. Hydrolysis of yeast nucleic acid with ammonia. *J. Biol. Chem.*, 25, 93, 1916.
- Walter Jones and B. E. Read. Adenine-uracil dinucleotide and the structure of yeast nucleic acid. *J. Biol. Chem.*, 29, 111, 1917.
- Walter Jones and B. E. Read. The mode of nucleotide linkage in yeast nucleic acid. *J. Biol. Chem.*, 29, 123, 1917.
- Walter Jones and B. E. Read. Uracil-cytosine dinucleotide. *J. Biol. Chem.*, 31, 39, 1917.
- Walter Jones and B. E. Read. The structure of the purine mononucleotides. *J. Biol. Chem.*, 31, 337, 1917.
- Walter Jones and R. P. Kennedy. Adenine mononucleotide. *J. Pharm. Exptl. Therap.*, 12, 253; 13, 45, 1918 and 1919.
- Walter Jones. The application of the Kjeldahl method to compounds of brucine, with reference to the brucine salt of a new nucleotide. *J. Pharm. Exptl. Therap.*, 13, 489, 1919.
- Walter Jones and A. F. Abt. The preparation of adenine nucleotide by hydrolysis of yeast nucleic acid with ammonia. *Am. J. Physiol.*, 50, 574, 1920.
- Walter Jones. The chemical constitution of adenine nucleotide and of yeast nucleic acid. *Am. J. Physiol.*, 52, 193, 1920.
- Walter Jones. The action of boiled pancreas extract on yeast nucleic acid. *Am. J. Physiol.*, 52, 203, 1920.
- Walter Jones. The thermostable active agent of pig's pancreas. *J. Biol. Chem.*, 50, 323, 1922.
- Walter Jones and C. Folkoff. The isolation of nucleic acid from tissues. *Johns Hopkins Hospital Bull.*, 33, 443, 1922.
- Walter Jones and M. E. Perkins. The preparation of nucleotides from yeast nucleic acid. *Johns Hopkins Hospital Bull.*, 34, 63, 1923.
- Walter Jones and M. E. Perkins. The gravimetric determination of organic phosphorus. *J. Biol. Chem.*, 55, 343, 1923.
- Walter Jones and M. E. Perkins. The nucleotides formed by the action of boiled pancreas on yeast nucleic acid. *J. Biol. Chem.*, 55, 557, 1923.
- Walter Jones and M. E. Perkins. The formation of nucleotides from yeast nucleic acid by the action of sodium hydroxide at room temperature. *J. Biol. Chem.*, 55, 567, 1923.
- Walter Jones and M. E. Perkins. The occurrence of plant nucleotides in animal tissues. *J. Biol. Chem.*, 62, 291, 1924.
- Walter Jones and M. E. Perkins. The nitrogenous groups of plant nucleic acid. *J. Biol. Chem.*, 62, 557, 1925.
- H. O. Calvery and Walter Jones. The nitrogenous groups of nucleic acid. *J. Biol. Chem.*, 73, 73, 1927.

Minor Notices

- Walter Jones. On the enzyme of the thymus. *Am. J. Physiol.*, 10, xxiv, 1903.

- Walter Jones. On the enzyme of the suprarenal gland. *Am. J. Physiol.*, 10, xxv, 1903.
- Walter Jones and C. R. Austrian. The occurrence of ferments in embryos. *J. Biol. Chem.*, 3, Proc. xxviii, 1907.
- Walter Jones and J. de Angulo. Studies in comparative physiological chemistry. *J. Biol. Chem.*, 6, Proc. xlv, 1909.
- Walter Jones and G. DeF. Barnett. On the recovery of adenine. *J. Biol. Chem.*, 9, Proc. xix, 1911.
- Walter Jones. On a specific nuclease from guanylic acid. *J. Biol. Chem.*, 9, Proc. xxviii, 1911.
- Walter Jones and Riley. [Mentioned by Jones in *Nucleic Acids*, 2nd ed., p. 43; but see Presidential Address, Jones, 1916.]
- Papers, by Jones's assistants, that pertain to his major subject of research, and that were published from his laboratory.*
- E. K. Marshall, Jr. On the self digestion of the thymus. *J. Biol. Chem.*, 16, 81, 1913.
- H. C. Germann. The partition of phosphorus in thymus nucleic acid. *J. Biol. Chem.*, 25, 189, 1916.
- B. E. Read. Guanine mononucleotide (Guanylic acid) and its preparation from yeast nucleic acid. *J. Biol. Chem.*, 31, 47, 1917.
- B. E. Read and W. E. Tottingham. Tritico nucleic acid. *J. Biol. Chem.*, 31, 295, 1917.
- D. W. Wilson. Studies in pyrimidine metabolism. *Proc. Soc. Exptl. Biol. Med.*, 17, 170, 1920; *J. Biol. Chem.*, 56, 215, 1923.
- W. S. Hoffman. The isolation of adenine nucleotide from blood. *Johns Hopkins Hospital Bull.*, 35, 417, 1924.
- W. S. Hoffman. The isolation of crystalline adenine nucleotide from blood. *J. Biol. Chem.*, 63, 675, 1925.
- H. O. Calvery. The preparation of adenine nucleotide from tea leaves. *J. Biol. Chem.*, 68, 593, 1926.
- M. V. Buell and M. E. Perkins. Crystalline guanine nucleotide. *J. Biol. Chem.*, 72, 21, 1927.
- H. O. Calvery. The action of sodium carbonate on yeast nucleic acid. *J. Biol. Chem.*, 72, 27, 1927.
- H. O. Calvery. The chemistry of tea leaves. II. The isolation of guanine nucleotide and cytosine nucleotide. *J. Biol. Chem.*, 72, 549, 1927.
- M. V. Buell and M. E. Perkins. Oxyadenine. *J. Biol. Chem.*, 72, 745, 1927.
- W. S. Hoffman. The micro determination of pentose in yeast nucleic acid and its derivatives. *J. Biol. Chem.*, 73, 15, 1927.
- H. O. Calvery. A note on the enzyme uricase. *J. Biol. Chem.*, 73, 77, 1927.
- H. O. Calvery and D. B. Rensen. The nucleotides of triticonucleic acid. *J. Biol. Chem.*, 73, 593, 1927.

WALTER (JENNINGS) JONES—CLARK

- M. V. Buell and M. E. Perkins. Adenine nucleotide content of blood with a micro analytical method for its determination. *J. Biol. Chem.*, 76, 95, 1928.
- H. O. Calvery. The isolation of four pentose nucleotides from chicken embryos. *J. Biol. Chem.*, 77, 489, 1928.
- M. V. Buell. On the origin of inosinic acid. *J. Biol. Chem.*, 85, 435, 1930.