Part I. The Making of a Physicist

Chapter 1. Bridgman as an Experimental Physicist

According to Gerald Holton, Bridgman published during his life "seven technical books and some 200 scientific papers, plus seven more books and about 60 papers on philosophy and the social study of science." [1] Although Bridgman was a productive writer outside technical physics and is more widely known for his contribution to the philosophy of science than for his experimental study, his main concern and activity were always in high-pressure physics. He spent most of the year in the laboratory and kept aside only the summer for writing on both technical and philosophical matters.

To date, however, not many historical studies have dealt with the details of Bridgman's scientific education and experimental work at Harvard University.[2] Albert Moyer has given notable comments on the philosophical aspect of Bridgman's college education, but left this physicist's training in science and mathematics untouched on. Two other important articles outlining Bridgman's experimental study, the biographical memoir of Bridgman by E. C. Kemble and F. Birch and a thematic chapter in Bridgman's biography by Maila Walter, provide us with knowledge of the development of his research, including its interaction with industrial technology. Nevertheless, none of these

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[1] Gerald Holton, "Percy W. Bridgman, Physicist and Philosopher," in *Einstein, History, and Other Passions* (New York: American Institute of Physics Press, 1995), pp. 221-227, p. 222.

[2] A biography of Bridgman by Maila L. Walter, *Science and Cultural Crisis: An Intellectual Biography of Percy Williams Bridgman (1882-1961)* (Stanford, California: Stanford University Press, 1990), is the richest source of information of Bridgman's life. Some other articles still remain valuable: Edwin C. Kemble and Francis Birch, "Percy Williams Bridgman, 1882-1961," *National Academy of Sciences of the United States, Biographical Memoirs,* 41 (1970), pp. 22-67; Albert E Moyer, "P. W. Bridgman's Operational Perspective on Physics: Part I: Origins and Development," *Studies in History and Philosophy of Science*, 22 (1991), pp. 231-258, and, "P. W. Bridgman's Operational Perspective on Physics: Part II: Refinements, Publication, and Reception," *Studies in History and Philosophy of Science*, 22 (1991), pp. 373-397; Gerald Holton, "Percy W. Bridgman, Physicist and Philosopher."

studies has discussed the relation between Bridgman's experimental study and his philosophical stance, operationalism.

In this chapter, I will describe Bridgman's education, his training as a physicist, and his experimental research in order to locate some of the origins of his philosophical reasoning. I will show that although Bridgman never clearly mentioned any connection between his experimental study and his philosophical writings, he unconsciously relied upon his experience in the laboratory while reflecting on the fundamentals of science. I will also try to detail the specific features of his scientific education and activity as compared with the general tendency of twentieth-century American physics, focusing especially on Bridgman's intellectual inclination.

1.1. Early Years, Private Life, and Antipathy toward Religion

1.1.1. Early Years

Percy Williams Bridgman was born in Cambridge, Massachusetts, on April 21, 1882. His father, Raymond Landon Bridgman, a writer and newspaper correspondent assigned to statehouse affairs, was known as a profoundly religious and idealistic person and a strong advocate of international law.[3] Bridgman's biographical memoir written by his former students tells that his mother Mary Ann Maria Williams was "conventional, sprightly, and competitive."[4] Both were, like their only son, born in New England. In Bridgman's recollection, his family "[f]inancially always felt cramped, but never suffered hardship."[5] As the young Bridgman had little money to spend, he

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[3] Some of Raymond L. Bridgman's books are: *The First Book of World Law: A Compilation of the International Conventions to Which the Principal Nations Are Signatory, with a Survey of Their Significance* (Boston: Ginn and Company, 1911) and *The Folly and Fallacy of Biennials* (Boston: Published by the Author, 1924).
[4] Kemble and Birch, "Percy Williams Bridgman, 1882-1961," p. 25.
[5] P. W. Bridgman, "Autobiographical remarks by P. W. Bridgman," undated, in ECKP,

enjoyed constant improvisation. Since his childhood, he had been interested in tools and making things.

Bridgman liked music, especially Beethoven, and sometimes resented later in his life not having been given piano lessons that his sister Florence enjoyed. While an undergraduate at Harvard, he happened to live in a room with a piano and had a chance to learn how to play it from his room-mate. One day, however, an upset freshman came up, saying that if they did not stop the banging he would report them to the proctor. Bridgman himself was the proctor then.[6]

When Bridgman was still very young, the family moved to Newton, Massachusetts. He attended the public school system of this city before entering Harvard College. A lifelong friend of Bridgman's, Robert Chandler, who would later serve as an educational missionary in China for the American Board of Foreign Missions, remembered that almost everyday they walked together two and a half miles to high school.[7] Another fellow high-school student Richard C. Tolman, a future physical chemist who also contributed much to the establishment of theoretical physics in the United States, kept a close friendship with Bridgman until Tolman's death in 1948.[8] Their later correspondence would involve heated discussions concerning each one's view of the fundamentals of science, dimensional analysis, relativity theory, and quantum mechanics.

At high school, Bridgman always liked the mathematical and scientific studies, but had "no violent dislikes." To him, his choice of physics as his specialty seemed natural. Still, he later recalled that it

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HUG (FP) 72.10.

[6] Jane Bridgman Koopman to E. C. Kemble, May 20, 1967, in ECKP, HUG (FP) 72.10.

[7] Robert E. Chandler, "A Deep and Rich Friendship," Expressions of Appreciation, PWBP, HUG 4234.24.

[8] Nathan and Ida Reingold, eds., *Science in America: A Documentary History, 1900-1939* (Chicago: University of Chicago Press, 1981), p. 419. J. R. Goodstein, "Tolman, Richard Chance," *Dictionary of Scientific Biography* (New York: Scribner's, 1976), vol. 13, pp. 429-430.

was probably "two very good teachers in the High School"[9] who made his choice of physics more inevitable.

In his high school years, Bridgman read several works of philosophers of science. In a letter to his friend written in 1936, though denying any conscious effect of them upon his later works, Bridgman admitted that very likely the foundations for his use of "concept" had been laid while he was at high school.[10] In his last year in high school, he read Karl Pearson's *Grammar of Science*[11] and the works of Ernst Mach, Henri Poincaré, William Clifford, and John Stallo, which were all he could pick up in the local public library. Still, he later recalled that by the time he started to write on the foundations of physics, he had forgotten almost everything in them. Apparently, Bridgman found Stallo most attractive among these philosophers. In the late 1950s, when the general editor of the Harvard University Press, Howard M. Jones, asked him to prepare a new edition of Stallo's *The Concepts and Theories of Modern Physics*,[12] Bridgman admitted to Jones that Stallo had had some real effect in "steering [him] into physics,"[13] although he did not know whether he would again be as enthusiastic. While editing a new version, Bridgman found some points he could not accept in Stallo's work.[14]

1.1.2. Personality and Family Life

In June 1900, Bridgman graduated from Newton High School.

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[9] P. W. Bridgman, "Autobiographical remarks by P. W. Bridgman."
[10] Bridgman to Bentley, Nov. 23, 1936, PWBP, HUG 4234.10.
[11] Karl Pearson, *The Grammar of Science*, 2nd ed., revised and enlarged (London: Adam and Charles Black, 1900).
[12] J. B. Stallo, edited by Percy W. Bridgman, *The Concepts and Theories of Modern Physics* (Cambridge, Massachusettes: The Belknap Press of Harvard University Press, 1960).
[13] Bridgman to H. M. Jones, Aug. 17, 1958, PWBP, HUG 4234.15. The new edition is, Stallo, *The Concepts and Theories of Modern Physics*, ed. P. W. Bridgman, 3rd ed. (Cambridge, Mass.: Harvard University Press, 1960).
[14] Bridgman to Jones, Sept. 9, 1958, PWBP, HUG 4234.15.

The same year he entered Harvard College and started his long academic life attached to that university. He received an A. B. *suma cum laude* in 1904, an M. A. in 1905, and a Ph. D. in 1908. The latter year he was appointed research fellow and joined the staff of the Harvard Physics Department. He became instructor in 1910, assistant professor in 1913, professor in 1919, Hollis Professor of Mathematics and Natural Philosophy in 1926, and Higgins University Professor in 1950. After a forty-six-year long university service, he retired in 1954 and was appointed Professor Emeritus.

After becoming a staff member of the Harvard Physics Department, Bridgman spent most of his time in experimental work in high pressure physics at his laboratory and only a small part in teaching. He did his writing and reading during summer vacations, which he usually spent in Randolph, New Hampshire. In sabbatical years, he mostly stayed abroad and wrote books. As his colleagues often did, he could have earned some money by giving several summer courses at other universities. Still, summer vacations seemed too precious to such an experimentalist as Bridgman to spend for extra income. In 1922, A. A. Michelson, an American experimental physicist known for his attempt to measure the earth's relative motion to the ether, invited Bridgman to give one graduate lecture course during the next summer at the University of Chicago, for which he was to receive $ 1,200, the amount corresponding to fifteen percent of his annual income. Bridgman politely declined the offer by writing, "My summers are definitely set aside for computation and writing up of the data that I obtain during the college year, and also for such reading as every one must do to keep up to date."[15]

Bridgman, however, could not peacefully enjoy summer vacation exclusively in writing and reading. Many scientists visited him at

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[15] Michelson to Bridgman, Nov. 20, 1922, PWBP, HUG 4234.8; Bridgman to Michelson, Nov. 26, 1922, PWBP, HUG 4234.8.

Randolph to discuss scientific matters. "In the evening," his daughter Jane wrote, "his callers might come—Leigh Page, J. Q. Stewart, Harvey Davis, or Dr. [G. N.] Lewis possibly—and the conversation would be interesting but over our heads."[16] Best-known for his presidency of Harvard and administration of the Manhattan Project, James B. Conant, whose family usually summered in Randolph, was also among the frequent visitors. With him, the discussion was not only about the progress of science, but about more worldly things, such as appointments or budgets of the Physics Department. They were close friends and colleagues. Conant had been an able chemist before he became President of Harvard and did experimental research with Bridgman. They co-authored a paper titled "Irreversible Transformations of Organic Compounds under High Pressure."[17] As the President of Harvard, Conant was enthusiastic over strengthening its science departments, like another chemist-President Charles Eliot. Being sensitive to the trend in the physical sciences, Conant urged the Department of Physics to renovate its research and faculty. Conant remained close to Bridgman and his family. In August 1961, when Bridgman, suffering from a bone cancer, committed suicide by shooting himself in the head, Conant received a phone call from the physicist's family and hurried to their summer house immediately after the event.[18]

Bridgman's comprehensive biography by Maila L. Walter revealed the details of his family life and personality, as well as some colorful episodes.[19] He was basically so honest as to expect always his words to be taken at their face value. His impatient logic sometimes made him look even blunt and bold; inside, however, he was shy and never

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[16] Jane Bridgman Koopman, "Personal Recollections," quoted in Walter, *Science and Cultural Crisis*, p. 21.
[17] P. W. Bridgman and J. B. Conant, "Irreversible Transformation of Organic Compounds under High Pressure," *Proceedings of the National Academy of Sciences of the United States*, 15 (1929), pp. 680-683.
[18] James Hershberg, *James B. Conant: Harvard to Hiroshima and the Making of the Nuclear Age* (Knopf: New York, 1993), p. 730.

liked public talks, including lecturing in the classroom. Physically, he was lean, not too tall, quite vigorous, and rarely sick. He neither smoked nor drank. He married Olive Ware in 1912, on which his laboratory note simply says, "getting married and writing paper."[20] They had a daughter, Jane, born in 1914, and a son, Robert, born in 1915.

Among various aspects of Bridgman's personality, his attitude toward religion is worth detailed description, since the episodes relating to it depict an important element of his character. His feelings toward religion stand almost incomprehensible when one takes into account the profoundly religious environment surrounding him. He rejected any kind of religion, while his parents, wife, and children were all churchgoers. His father Raymond, a deacon in the Congregational Church for many years, was religious enough to be concerned that his only son should become a professing Christian.[21] Bridgman's wife Olive also sometimes served as a Sunday school teacher.

According to a story Bridgman told to his daughter only once,[22] he decided not to commit himself to religion when he was sixteen years old. This decision involved a struggle. That summer, Bridgman's parents asked their son to join the church. After thinking over this matter during the season, Bridgman confessed to them that he could not do so since he could not believe in God. The minister told him that if he tried to believe he could then see that what he was believing was true. This backwards approach, however, was not acceptable to Bridgman. He continued to go to church only to please his parents, until a special prayer-meeting took place at the church, at which, completely to the shy boy's surprise, everyone tried to persuade him to join the religious

[19] Walter, *Science and Cultural Crisis*, pp. 13-22.
[20] Notebook IV, p. 137, May 16, 1912, PWBP, quoted in Walter, *Science and Cultural Crisis*, n. 2, p. 18.
[21] P. W. Bridgman, "Autobiographical remarks by P. W. Bridgman."
[22] Jane Koopman to Kemble, May 20, 1967, ECKP, HUG (FP) 72.10.

community. In fact, that was the sole purpose of the meeting. It is not difficult to imagine that this "play" aroused antipathy toward religion in him.

As to the reason Bridgman could not follow his parents' suggestion, he later explained in a letter to Scudder Klyce that it was "the method of superficial probability" that helped him make up his mind:

> I made the discovery that this method is necessary quite early in life, when in Sunday School I was confronted with various expert "proofs" of the historical validity of many of the things in the bible. I saw that if I did a thorough job at deciding for myself the validity of these claims my time would be spent on nothing else. I was not willing to give my life to this, so I merely rejected, without sufficiently logical argument, many of the claims of the religiously minded most of my elders, on the basis of inherent improbability, because of their failure to jibe with the external world as I found it.[23]

John C. Slater, who was Bridgman's doctoral student, described this unconventionally simple logic of Bridgman succinctly: "There is nothing devious or fuzzy about his thinking. If something is true and makes sense, that is the end of it."[24] Together with his preference for "the external world," this straightforwardness made an essential component of his way of reasoning.

Despite his inner struggle, Bridgman's independent decision did not cause any serious problem within his family. He respected his wife's religion and let their children go to church without any objection. Bridgman may have inherited this generosity from his father's attitude toward himself. Raymond Bridgman after all stopped exerting undue pressure and respected his son's decision. In 1924, he wrote his son that he affirmed that "every one must work out his own position for himself and that two lives will no more be alike in their religious

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[23] Bridgman to Klyce, Jan. 5, 1930, PWBP, HUG 4234.12.
[24] John C. Slater, "Presentation of Bingham Medal to P. W. Bridgman," *Journal of*

experience than the physical bodies and the intellectual gifts are alike."[25]

Outside the family, however, Bridgman sometimes looked almost intolerant of anything connected to religion. Harvard University had a rule that no classes should be held between 8:40 and 9:00 in the morning, so that classes would not conflict with the chapel service. Bridgman, however, often neglected this rule.[26] Moreover, he rejected to attend at least three symposia for the reasons related to his feelings toward religion. In 1948, when he was invited to write a paper for the Conference on Science, Philosophy and Religion, at first the reason for his rejection was that he did not have anything to contribute to the subject of the conference, "What Should Be the Goals of Education."[27] To the second invitation that asked for his attendance, Bridgman replied by revealing his feelings toward religion. "The reason," he wrote, "is my very definite feelings on the subject of religion."

> It seems to me that the world has now progressed to such a stage that religion as conventionally understood has nearly outlived its function. I believe that the salvation of society is to get away from conventional religion, not to return to it as so many people urge. Religion is on its way to becoming the great human vice. No one could recognize more than I that many of the factors which are included in what is ordinarily understood as religion are wholly admirable and stem from the deepest in humanity, but there is so much else that is inextricably mixed up with a foggy metaphysics and which seems to me palpably unture [*sic*], that I think we shall get ahead only by making a drastic break with the past and discarding the name religion in our endeavors for the future. And it does indeed seem to me that something pretty drastic is necessary. Although I have never attended any of the meetings of the Conference I have bought and read the printed reports, and have always been completely disappointed. It seems to me that you are getting

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Colloid Science, 7:3 (June 1952), pp. 199-202.

[25] Raymond Bridgman to Percy Williams Bridgman, Oct. 13, 1924, PWBP, HUG 4234.8.

[26] Moore to Lyman, April 6, 1921, PLDC, UA V 692.5.

[27] Bridgman to Finkelstein, Jan. 21, 1948, PWBP, HUG 4234.10.

> nowhere along the old lines, and I do not see how you can.
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> So long as the name Religion appears in the title of the Conference, I could not with self respect associate myself with it.
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> I hope you will accept these words in the spirit in which they are meant--it has not been easy for me to write them.[28]

Although he briefly mentioned his criticism of the reports of the conference, apparently the word "religion" stimulated him. The reason for him not to attend the conference seems to be only that its title contained this word.

Bridgman also reacted fiercely to individuals who had shown deep commitment to religion. In 1956, the American Academy of Arts and Sciences decided to appoint a new committee on the Unity of Learning, replacing the former committee on the Unity of Science on which Bridgman had long been serving. The Academy asked him to join the new committee. Bridgman's reply to John E. Burchard, the President of the American Academy of Arts and Sciences, was short but clear:

> I am, of course, glad to serve on your new committee on the Unity of Learning, unless you regard as a disqualification my feeling that theology is not a branch of learning at all and my skepticism as to the fruitfulness of having a theologian serve on the committee, even if he is my successor in a University Professorship.[29]

He did not miss the name of Paul Tillich, a celebrated theologian, in the list of the expected members of the committee.

The older Bridgman became, the stronger his antipathy toward religion grew. He did not accept the sponsorship for a symposium, if it came from a religious institution. In 1959, following Karl K. Darrow's suggestion, Henry Margenau asked Bridgman to take part in the Symposium on Philosophy of Physics, to be held in conjunction with the American Physical Society meeting at Marquette University in

[28] Bridgman to Finkelstein, May 17, 1948, PWBP, HUG 4234.10.

Milwaukee. This time Bridgman's target was a Catholic institution, Marquette University. He described all of his feelings toward Catholics:

> What I dislike most is its sponsorship by a Catholic institution and the heavy slanting of the questions toward the Catholic point of view. It has been my universal experience in the past that I can never make any headway in discussions with professional Catholics--they are too heavily committed as Catholics to the official point of view and will play the game only according to their own rules, which to me do not make sense. I am coming more and more to feel that the religious and in particular the theological point of view constitutes the cardinal intellectual vice of the human race. If I accepted your invitation I would do it because I hoped to convince some of my hearers of this, although of course I could not expect to touch the professionals themselves. For this reason I would not be willing to accept unless I could be sure that the proceedings would be printed and so reach a larger audience than those actually present. I can perfectly well see that you or the Fathers would not care for an acceptance made in this spirit.[30]

Margenau, however, was competent enough to persuade Bridgman. He explained that "men like yourself and [Philipp] Frank and [Erwin] Schrödinger are needed" in order to expel any pretense of Catholic domination of the symposium. He further flattered Bridgman:

> The spirit of your letter delights me and makes me think that the meeting will be successful indeed. It is true that you may not convert the men of Marquette to your point of view, but I know you will force them to depart from your lectures with increased admiration for your objectivity, your forcefulness and your willingness to speak to them.[31]

Margenau also promised to exert himself in every way to see the proceedings printed. Bridgman accepted his offer in the reply.

The logic supporting Bridgman's rejection of religion was simple and straight. This sort of simplicity and straightforwardness is

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[29] Bridgman to Burchard, July 17, 1956, PWBP, HUG 4234.10.
[30] Bridgman to Margenau, Jan. 8, 1959, PWBP, HUG 4234.15.

characteristic of most of his reflections, whether on science or on other issues. Bridgman never pretended that he understood or accepted what he actually could not, nor did he hesitate to express his honest feelings without paying much attention to others'.[31] In the following chapters, we will see that Bridgman held this same attitude in his experimental work, philosophical reflections, and political activities.

1.2. Harvard Education

1.2.1. Scientific and Mathematical Education

Bridgman spent eight years of his youth as a student at Harvard. When one focuses on his operationalism or experimental work, this period is often neglected or unexplored. Yet, as I will show, the student years played an important role in the development of his scientific thought. If one does not closely examine this period of Bridgman's life, many questions will remain unsolved: his education in mathematics, physics, and philosophy; the features of education and research at the Harvard Physics Department; the possible influence of pragmatism upon Bridgman through, for example, William James; the general intellectual milieu surrounding the young Bridgman. In the following, mainly by surveying the courses he took, I will try to explicate the starting point of his long-standing reflection on science.

As an undergraduate student, Bridgman completed twenty-three courses, while the normal requirement for graduation was then the completion of seventeen full courses.[32] Four full courses (two in mathematics and two in physics) out of five taken in his senior year were later used for the A. M. degree.[33] At the Graduate School, he took ten courses, 3.5 courses in mathematics and 6.5 in physics (Table

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[31] Margenau to Bridgman, Jan. 12, 1959, PWBP, HUG 4234.15.
[32] Kemble and Birch, *op. cit.*, "Percy Williams Bridgman."
[33] Kennedy to Kemble, July 26, 1962, ECKP, HUG (FP) 72.10.

1-2-1).

Notably, Bridgman's program of study put more stress on mathematics than on physics, especially at college, where he took more courses in mathematics than in physics (Table 1-1-1). The details of his mathematical courses are shown in Tables 1-1-2 and 1-2-2. The course on "Modern Methods in Geometry," which Bridgman took in 1901-02, included "Determinants and their simpler geometrical applications."[34] The courses on "Trigonometric Series" in 1903-1904 dealt with some typical linear partial differential equations, the Fourier transformation and its application to the conduction of heat and acoustics, and a treatment of spherical harmonics and Bessel's Functions.[35] By the time he received a Ph. D., Bridgman's training in mathematics had included: differential and integral calculus; linear differential equations; infinite series; the Fourier transformation; and some introductory knowledge of determinants. As for his grades as an undergraduate, Sargent Kennedy of Registrar's Office of Harvard University reported to Kemble that "[i]t is certainly a most distinguished record, culminating in a Summa in June, 1904."[36] Furthermore, he remarked that Bridgman's grades at the Graduate School were "of the highest distinction."[37] Later, when Bridgman discussed the foundations of relativity theory or quantum mechanics, he often remarked that he did not find any difficulty in their mathematics. As far as his knowledge of mathematics is concerned, we can see that he was prepared, at least, to the degree that he could comprehend what kind of mathematics the new theories adopted and then could acquire sufficient knowledge, if necessary, by reading appropriate textbooks.

The courses in physics, as well as some courses in mathematics,

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[34] *Official Register, Division of Mathematics: 1902-1903* (Cambridge, Mass: Harvard University, 1901), p. 20.
[35] *Official Register, Division of Mathematics: 1903-1904* (Cambridge, Mass: Harvard University, 1903), p. 27.
[36] Kennedy to Kemble, Sept. 4, 1962, ECKP, HUG (FP) 72.10.

covered most of the fields in classical mechanics and its applications, electrostatics, electromagnetism, electrodynamics, and thermodynamics (Table 1-1-3 and 1-2-3). Although the special theory of relativity, to which the American physicist started to pay attention roughly around 1910, was not taught while he was a student (Bridgman himself was to deliver the first course at Harvard whose material included relativity theory), there were some courses on the contemporary development in physics. For example, the syllabus of the course by G. W. Pierce on "Radiation" in 1902-1903 was as follows:

> I. Thermal Radiation.
> (a) Empirical Facts and Laws.
> The spectrum.
> Radiating, absorbing, and reflecting power.
> Selective radiation, absorption and reflection.
> Diathermanency of solid bodies.
> Diathermanency of solvents and salt-solutions.
> Diathermanency of gases with application to meteorology.
> Dependence of emission on temperature—Laws of Newton, Dulong and Petit, Weber, Stefan.
> (b) The Thermodynamics of Thermal Radiation.
> Absolutely black bodies—Wien and Lummer.
> The relation of emission to absorption—Kirchhoff.
> The normal pressure of light—Maxwell, Boltzmann.
> The dependence of radiation on temperature—Stefan, Boltzmann.
> The distribution of energy in the spectrum of hot bodies—Wien, Paschen.
> II. Luminescence.
> (a) Fluorescence.
> (b) Phosphorescence.
> (c) Electro-luminescence.
> (d) Roentgen Rays, Becquerel Rays, and Radioactive Bodies.[38]

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[37] Kennedy to Kemble, June 26, 1962, ECKP, HUG (FP) 72.10.
[38] *Official Register, Division of Physics: 1902-1903* (Cambridge, Mass: Harvard University, 1902), pp. 19-20.

Although this course did not take up Planck's theory which appeared very recently, it included important results of studies in radiation, such as the Stefan-Boltzmann Law and Wien's contribution.

Bridgman had a chance to acquire rudimentary knowledge of statistical mechanics. E. H. Hall's course on "the Theory of Probability and the Kinetic Theory of Gases" was an introductory course to the applications of statistical methods in physics, in which Hall lectured on the "distribution of velocities among the particles of a gas" following Maxwell's Law. Hall did not seem to discuss the work of Ludwig Boltzmann or Josiah Willard Gibbs.[39]

Comparing the courses that Bridgman took at the Mathematics Department and those at the Physics Department, one can see that most of the advanced mathematical courses in physics were given at the Mathematics Department ("Elements of Mechanics," "Dynamics of a Rigid Body," "Methods in Mathematical Physics," and "The Linear Differential Equations of Physics"), even though one of the instructors in charge, Benjamin Osgood Peirce, belonged to the Physics Department. The educational program of the Physics Department accentuated training in experimental work and did not require physics majors to acquire expertise in mathematics. Physics students interested in the theoretical or mathematical aspects of physics could find appropriate courses only among those given at the Department of Mathematics. Before attending those courses at the Mathematics Department, however, physics students somehow had to acquire mathematical knowledge necessary to understand these courses. Physics undergraduates like Bridgman, who was apparently more interested in the theoretical aspects of physics than the Physics Department required him to be, therefore took more hours in mathematics than in his major.

The lack of mathematical subjects in the curriculum may have

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[39] *Official Register, Division of Physics: 1904-1905* (Cambridge, Mass: Harvard University, 1904), pp. 19-20.

indicated a lack of necessary instructors who could teach these subjects. Clearly, the Harvard Physics Department lacked an interest in mathematical and theoretical fields. This inclination, along with its emphasis on experimental research, had been the Department's tradition since 1870, when John Trowbridge started to reform Harvard's education and research in physics.

Charles W. Eliot, who had been making an effort to reconstruct Harvard toward a modern research university since he became its President in 1869, assigned Trowbridge to modernize the Physics Department. Before Trowbridge, Joseph Lovering was in charge of education in physics at Harvard. Lovering, who used to be a student of divinity, "seems to have felt no more called upon to extend the domain of physics than as a preacher he would have felt obliged to add a chapter to the Bible,"[40] as his former student Edwin H. Hall recollected.

Trowbridge was a self-taught physicist and had never studied abroad, which was unusual for a professor at Harvard. Yet, encouraged and urged by Eliot, Trowbridge reformed the curriculum and personnel of the Department so that it could keep close contact with the development of physics in Europe. Trowbridge reconstructed the curriculum and emphasized experimental research in the courses. Theoretical or mathematical physics, however, was beyond his vision. In 1877-1878, Trowbridge gave two courses on mathematical physics, one of them on Thomson and Tait's *Elements of Natural Philosophy*[41] and the other on Maxwell's *Electricity and Magnetism,*[42] both of which he found to be "profoundly mathematical and proverbially difficult

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[40] Samuel Eliot Morison, *The Development of Harvard University: Since the Inauguration of President Eliot, 1869-1929* (Cambridge, Mass: Harvard University Press, 1930), p. 277.
[41] William Thomson and Peter Guthrie Tait, *Elements of Natural Philosophy* (Oxford: Clarendon Press, 1867).
[42] J. C. Maxwell, *A Treatise on Electricity and Magnetism* (2 vols., Oxford: Clarendon Press, 1873).

reading."[43] He gave them up the next year.

Furthermore, following Trowbridge's vision, the Department appointed younger physicists, E. H. Hall, Benjamin Osgood Peirce, Wallace Clement Sabine, Theodore Lyman, and William Duane, who were experimental physicists but knew mathematics enough to deliver courses on theoretical physics. Still, their research interests and therefore those of their students were mostly in experimental fields. Peirce had studied in Germany for three years and had the greatest mathematical ability among the staff members. Yet, he was concerned mainly with experimental research.[44] He gave mathematical courses both in the Mathematics and Physics Departments, but he considered them to be merely teaching loads. He spent the rest of his time on his experiment.[45] It was after World War I that the Physics Department started a program in theoretical research. Bridgman would take the lead in this change.

Between the 1870s and the 1910s, in fact, most of the physics departments in America neglected mathematical training and emphasized experimental work. As the historian of science John W. Servos has pointed out in his article on the mathematical preparation of American physical scientists, in 1910, "a one-year course in elementary calculus was sufficient to satisfy the requirements for a physics major at Yale, Harvard, Stanford, California, and Michigan."[46] Furthermore, most of the distinguished physicists in the United States in this period were experimentalists: A. A. Michelson, H. A. Rowland, R. A. Millikan, and E. H. Hall, to name but a few. Although J. W. Gibbs was an only exception, he was better-known and more appreciated in Europe and

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[43] Morison, *The Development of Harvard University*, p. 279.
[44] Benjamin Osgood Peirce, *Mathematical and Physical Papers, 1903-1913* (Cambridge, Mass.: Harvard University Press, 1926).
[45] Katherine Russell Sopka, *Quantum Physics in America: 1920-1935* (New York: Arno, 1980), p. 1.34.
[46] John W. Servos, "Mathematics and the Physical Sciences in America, 1880-1930," *Isis*, 77 (1986), p. 616.

did not leave any successors in the United States.[47]

Since one could train a professional physicist without advanced mathematics, the lack of mathematical knowledge was not uncommon among American physical scientists of Bridgman's generation. Servos has illustrated some examples of the prominent physicists with poor mathematical preparation.[48] Arthur L. Day (1869-1960) had earned a Ph. D. from Yale, taught at Yale for three years, and then trained at the Physikalische-technische Reichsanstalt in Berlin. Still, he failed a test consisting of elementary integration and differentiation of a logarithm and elliptic integrals, when applying for a position as chief physicist of the U. S. Geological Survey. Despite this fault, he was appointed to that position and went on to be the first director of the Geophysical Laboratory of the Carnegie Institution of Washington. Another example is a close friend of Bridgman, who happened to be as old as he was. Irving Langmuir (1881-1957), a chemist and Nobel Laureate, suffered from his mathematical weakness while attending the courses on electricity, mechanics and theoretical physics during his stay in Göttingen. His supervisor Walter Nernst finally advised Langmuir to change the research topic of his Ph. D. thesis. An insufficient training in mathematics was clear even among American scientists who had been exposed to the scientific education in Europe. Although the requirements in mathematics for physics students were more demanding in Europe, studying there did not always ensure a chance to acquire mathematical expertise.

At Harvard, Bridgman could fortunately find courses and instructors suitable for his interest in theoretical and mathematical topics. Apparently, however, he was not satisfied only with learning them. His first two published papers (in fact, a letter and a paper)

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[47] Sopka, *Quantum Physics in America,* p. 1.34.
[48] Servos, "Mathematics and the Physical Sciences in America, 1880-1930," pp. 618-622.

show his early interest in reflecting and writing on mathematical and theoretical issues, while two manuscripts he wrote for his courses suggest his concern with the foundations of physical concepts. While continued with his high-pressure experiments as a Ph. D. candidate, Bridgman was also enjoying playing with some abstract thoughts, as these papers show.

Bridgman's article, titled "The Electrostatic Field Surrounding Two Special Columnar Elements,"[49] was published in the *Proceedings of the American Academy of Arts and Sciences* in 1906. He wrote this article to present several diagrams showing the equipotential lines of force surrounding certain two dimensional distributions of electrostatic charge. Although Maxwell had already shown some figures in his *Electricity and Magnetism*, [50] Bridgman presented diagrams that Maxwell had not given. This paper, first presented by B. O. Peirce on November 8, 1905, was apparently written in connection with the courses on electromagnetism given by Peirce and was more graphical rather than mathematical. It is an elaborate work that strikes the reader with Bridgman's zeal for visualization, but it was not an essential contribution to the theory of electromagnetism.

Bridgman's letter[51] published in the *Philosophical Magazine* in 1908 pointed out an error concerning one term of Bessel's function he found in several books. His discussion is entirely mathematical, though its subject, Bessel's function, one of the most familiar functions to physicists, was closely connected to a practical problem in Bridgman's experimental research: "a numeral calculation of the torsion of a circular cylinder under shearing forces distributed arbitrarily over

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[49] Percy Williams Bridgman, "The Electrostatic Field Surrounding Two Special Columnar Elements," *Proceedings of the American Academy of Arts and Sciences,* 41 (1906), pp. 617-626.
[50] J. C. Maxwell, *A Treatise on Electricity and Magnetism* (2 vols., Oxford: Clarendon Press, 1873).
[51] P. W. Bridgman, "On a Certain Development in Bessel's Functions," *Philosophical Magazine*, 16 (1908), pp. 947-948.

the ends."[52] This short letter shows that Bridgman did not just trust formulas given in several handbooks, but painstakingly followed the computations involved in solving differential equations.

Bridgman's two unpublished essays, one on the medium in electrostatics and the other on the concepts of thermodynamics, deserve special attention in connection with the later development of his scientific thought. His two early published articles, mentioned above, illustrate his interest in mathematical and theoretical aspects of physics. They do not necessarily imply his concern with the foundations of physics. His two early manuscripts, on the other hand, reveal his fondness for reflection on fundamental problems of science.

The manuscript titled "The Role of the Medium in Electrostatics,"[53] has the author's handwritten comment on the front page: "Probably written in connection with Physics 9 or 10. 1905 or 1906. 25/4/61." From the date of this note, one can estimate that Bridgman read this manuscript while he was preparing the final draft of a monograph on the special theory of relativity, *A Sophisticate's Primer of Relativity*,[54] published posthumously in 1962. Physics 9 and 10 are the courses on "the Mathematical Theory of Electricity and Magnetism" given by B. O. Peirce. Bridgman took Physics 9 in 1904-1905 and Physics 10 in 1905-1906. Although Peirce, a distant relative to Charles Peirce, did not leave anything noteworthy in the philosophy of science by himself, he had kept a close friendship with Karl Pearson since his stay in Berlin.[55]

Bridgman's early essay on electrostatics describes part of his attitude toward the controversial notion "ether" before he became

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52 *Ibid.*, p. 947.
53 P. W. Bridgman, "The Role of the Medium in Electrostatics," handwritten manuscript, PWBP, HUG 4234.10.
54 Percy Williams Bridgman, *A Sophisticate's Primer of Relativity* (Middletown, Conn.: Wesleyan University Press, 1962).
55 C. Eisele, "Peirce, Benjamin Osgood, II," *Dictionary of Scientific Biography* (New York: Scribner's, 1974), vol. 10, pp. 481-482.

acquainted with the special theory of relativity. This essay owes much to W. H. Bragg's article published in the *Philosophical Magazine*[56] that discussed the necessity of an elastic medium in electrostatics. Although Bragg's discussion contained some mathematical explanation, Bridgman dealt with the topic rather qualitatively. In so doing he attempted to scrutinize the validity of the "elastic medium" model from a physical point of view. I will discuss the details of this manuscript in connection with his general attitude toward relativity theory (§4.1). Now I just point out that in this essay one can find an early form of his later inclination to emphasize the distinction between mathematical and physical concepts. For instance, he wrote:

> [F]or physical purposes a physical conception is greatly to be preferred to a mathematical one. It would seem as if this idea [of imperfect but physical analogy] might be introduced to the student, before he takes up any mathematics at all: the subsequent mathematical work will then seem motivated and less artificial.[57]

Bridgman kept his preference for physical concepts to mathematical models throughout his life.

Furthermore, in the seventeen-page essay on the foundation of some concepts of thermodynamics in his "Private Note Book" dated Jan. 14, 1907,[58] one can even find something similar to Bridgman's later stance, known as operationalism. Bridgman was seemingly stimulated by the courses on thermodynamics given by E. H. Hall in 1903-1904. Hall, best-known for the Hall Effect, was not just a hardworking experimentalist, but had profound knowledge of Ernst Mach's empiricism and William James's pragmatism. Moreover, over fifty

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[56] W. H. Bragg, "The 'Elastic Medium' Method of Treating Electrostatic Problems," *Philosophical Magazine*, 34 (1892), pp. 18-35.
[57] P. W. Bridgman, "The Role of the Medium in Electrostatics," pp. 11-12.
[58] P. W. Bridgman, "Private Note Book of P. W. Bridgman," pp. 17-34, PWBP, HUG 4234.65.26.

years later, while preparing a new edition of Stallo's *The Concepts and Theories of Modern Physics*,[59] Bridgman found the evidence that Hall had also enjoyed reading a copy of it kept at the Harvard Library.[60]

In his essay, Bridgman tried to show that the first and second laws of thermodynamics had its empirical basis on a directly measurable quantity, namely, temperature, not depending on an abstract concept, "heat."

> This is going to be an attempt to find out what thermodynamics is talking about, and what is really involved in the general deduction. The starting point is going to be the idea of temperature. This is thought to be the one fact that is most directly given by experience: the idea of quantity of heat which is sometimes made the starting point (by Prof. Hall e. g.) is conceived to be really a derived idea, it can mean nothing to an unsophisticated mind.[61]

Bridgman thought that just as in kinetics the idea of time plays the fundamental role as "given," the sense of temperature should play the fundamental role in thermodynamics. One can tell whether one of two bodies is hotter than the other, and if neither is hotter, then their temperatures are the same. Temperature is a physical quantity that can be measured directly, while heat is a derived idea dependent on the concepts of temperature and heat capacity. Bridgman attempted to construct thermodynamics on the basis of physical quantities that were directly measurable.

In the concluding remarks on the notion of heat, Bridgman presented his requirement of operational directness for physical concepts:

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[59] J. B. Stallo, ed. Percy Williams Bridgman, *The Concepts and Theories of Modern Physics* (Cambridge, Massachusetts: The Belknap Press of Harvard University Press, 1960).
[60] Bridgman to H. M. Jones, Sept. 9, 1958, PWBP, HUG 4234.15. Bridgman found Hall's handwriting in the copy in quoting a review by Oliver Lodge.
[61] P. W. Bridgman, "Private Note Book of P. W. Bridgman," p. 17.

> The two laws of thermodynamics have thus been stated without the use of the word heat or the idea of some substance that passing from one body to another produces change of temp[erature]. It would therefore be possible to develop the whole of thermodynamics in the same way. Hence anything more than has been found necessary here that is read into the expression "heat" is superfluous. "Heat" according to this view is merely a short way of expressing the idea of equivalent changes of temp. Of course since the idea of changes of temp. and temp itself are closely related, it is possible to start with either as known and deduce the properties of the other. But, as stated at the beginning, the temperature idea seems to be that which is given immediately in experience, and hence, in so far, the preferable one to start from.[62]

In his effort to comprehend physical theory, Bridgman preferred directly experiential concepts to "derived" ones. Although Bridgman would gradually recognize the significance of those derived concepts as he widened his scientific perspective, the distinction between direct and derived concepts would develop into the distinction between the primary and secondary quantities in his scrutiny of dimensional analysis (see Chapter 3). Together with his preference for physical concepts in the manuscript on the elastic medium, this essay shows us his operational and empirical inclination before he started serious reflections on relativity theory and dimensional analysis.

1.2.2. Early Interest in Philosophy

The young Bridgman's essays show that he possibly started his own attempts to reflect on the foundations of science during his student years, though they were yet to be precise, comprehensive, or sophisticated. One may want to explain his early empirical stand by emphasizing his inclination toward experimental research. Yet, more likely he enjoyed theorizing and scrutinizing the foundations of science before he immersed himself deep in experimental study. In 1928, Bridgman wrote to Arthur F. Bentley that he had once gone through "a stage of interest in philosophy" while in college, though he could only find that there were many things with which he could occupy himself with greater profit.[63] In 1936, writing more on the history of his "philosophizing" activity, Bridgman recollected how early he began his reflection:

> I don't believe you realize either how far back I started. I was always more or less interested in "philosophizing" and listened in an [*sic*] some of the courses while an undergraduate; one by [George] Santayana on philosophy of Nature, and one on [Richard] Dedekind I remember especially. I never could quite get what they were trying to do or driving at, but I remember perfectly definitely that I had very definite ideas as to what constituted sensible things to try to do. Once I started a little essay with the statement [that] my absolute starting point was the recognition that "things really exist". I cannot recapture now what I meant by that, and in fact the expression becomes more and more meaningless to me every time I shave, but I do remember that things like that did mean something to me once, and I can account for the action of most of my fellows only by assuming that statements like that still mean something to them.[64]

Bridgman did not detail the influence of those courses in philosophy, but he remembered that they stimulated him intellectually. Though Bridgman was unaware of it, his early belief that "things really exist" was to undergo several transformations in the course of development of his perspective on science (see Chapters 4 and 5).

As Santayana lectured on neither philosophy of nature nor Dedekind's theory, the course Bridgman actually audited was inferred to be either "Outlines of the History of Philosophy," "Philosophy of History.—Ideals of Society, Science, and Religion," given in 1902-1903

[62] P. W. Bridgman, "Private Note Book of P. W. Bridgman," pp. 33-34.
[63] Bridgman to Bentley, Jan. 1, 1928, HUG 4234.12.

and 1903-1904, or "Metaphysics.—The Fundamental Problems of Theoretical Philosophy. Realism and Idealism; Freedom, Teleology, and Theism," given in 1903-1904. Santayana, who was then an assistant professor and still under the influence of the pragmatism of his colleague William James, was in charge of these courses, at least jointly. The course on philosophy of nature ("The Philosophy of Nature, with especial reference to Man's place in Nature.—The Fundamental Conceptions of Science; the relation of Mind and Body; Evolution") was given every year while Bridgman was an undergraduate. In 1902-1903, William James shared in the teaching of this course. The textbooks for the latter included Bridgman's favorite from high school, Karl Pearson's *Grammar of Science*.[65] Bridgman, however, did not take any of these courses officially.

As Table 1-1-1 shows, Bridgman took a credit of only one course in philosophy while an undergraduate. This was the course on "General Introduction to Philosophy," given by Josiah Royce and Hugo Münsterberg in 1902-1903. It assigned two textbooks, William James's *Psychology* (briefer course)[66] and Jevons's *Lessons in Logic*.[67] James seems to have attracted Bridgman, since, as Albert Moyer has found, he ordered James's *The Varieties of Religious Experience*[68] in 1912.[69]

At least around Harvard, James was, in fact, the most favored

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64 Bridgman to Bentley, Sept. 21, 1936, PWBP, HUG 4234.10.
65 *Harvard University Catalogue: 1901-1902* (Cambridge, Mass.: Harvard University, 1901), pp. 370-372. *Harvard University Catalogue: 1902-1903* (Cambridge, Mass.: Harvard University, 1902), pp. 383-386. *Harvard University Catalogue: 1903-1904* (Cambridge, Mass.: Harvard University, 1903), pp. 415-418.
66 William James, *Psychology: Briefer Course* (London: Macmillan, 1892).
67 *Harvard University Catalogue: 1902-1903* (Cambridge, Mass.: Harvard University, 1902), pp. 383. Jevons's *Lessons in Logic* means, W. Stanley Jevons, recast by David J. Hill, *The Elements of Logic: A Text-book for Schools and Colleges: Being the Elementary Lessons in Logic* (New York and Chicago: Sheldon, 1883).
68 William James, *The Varieties of Religious Experience: A Study in Human Nature: Being the Gifford Lectures on Natural Religion Delivered at Edinburgh in 1901-1902* (New York and Bombay: Longmans, Green, 1902).
69 Moyer, *op. cit.*, "Bridgman's Operational Perspective: Part I," p. 247.

author among three pragmatists, James, C. S. Peirce, and John Dewey. On the occasion of a meeting in memory of Bridgman, James Conant recollected the intellectual atmosphere of his young days:

> To what extent William James' interpretation of the ideas of Peirce may have subtly influenced a young physicist in the early 1900's no one can safely say. Of direct connection there is no evidence as far as I can tell but one may be permitted to believe that the intellectual atmosphere which a man breathes when he is young may have influences of which he himself is quite unaware.[70]

In addition to this environment, Bridgman seems to have had a chance to have an actual contact with James and his works.

Bridgman did not confess any clear influence upon his thought from the works he read in his youth. Moreover, in his philosophical work, he seldom clarified how he formed the ideas described in them. However, from the examination of Bridgman's early interest in science and philosophy, I can point out several factors that may have turned his attention to the fundamental problems of science: His interest and training in mathematical subjects, exceptional for an American physics student of his generation; his early reflections on the foundations of physics; the influence of pragmatism and empiricism through the works of Mach, Poincaré, Stallo, Pearson, Clifford, and James; and the courses delivered by Hall, Peirce, Santayana, Royce, Münsterberg, and perhaps James. These can easily be overlooked if one just discusses his life as a professional scientist. To explicate how Bridgman's perspective developed further, one has to examine his scrutiny of dimensional analysis, relativity theory, and quantum mechanics. Yet, I can safely maintain that Bridgman constructed at least the pedestal of his view of science in his student years.

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[70] James B. Conant, "A Truly Extraordinary Man," in "Expressions of Appreciation," PWBP, HUG 4234. 25.

1.3. The Experimentalist Bridgman

Bridgman was an experimental physicist throughout his life. He had already established himself as a pioneer in high-pressure physics when he published his best-known book, *The Logic of Modern Physics*.[71] Since Bridgman did not admit any connection between his experimental work and his operational perspective, one may venture to think that he or she can discuss his operational perspective on science without paying any serious attention to his experimental research, as some historians have actually done. True, he scrutinized physical theory mainly from a general point of view, not especially from that of an experimentalist. Moreover, it is difficult to consider his perspective on science as showing the experimentalist's stand just because he was an experimentalist. Yet, before analyzing the development of Bridgman's scientific thought, one should naturally pay careful attention to his position as an experimental physicist in the 20th-century American community of physicists in the middle of enthusiasm over the rise of theoretical research. Then, it seems inevitable to examine how he started his career as an experimentalist, how successful his experimental research was, and what theoretical research meant to his experiment.

Bridgman's way of doing physics itself is an interesting issue for historians of science. Simple and small-sized as it might be, his style represented some aspects of well-wrought experimental research. The honors Bridgman received for his research in the period of "Big Science" testify to what sort of scientific results could survive frequent shifts in scientists' research interest in the twentieth century.

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[71] P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927).

1.3.1. Opening a New Field

Before World War I, research in physics at Harvard was almost exclusively experimental. Bridgman's graduate research was no exception. Although Bridgman liked mathematical and theoretical subjects, very likely he did not even think of doing theoretical graduate work. According to David L. Webster, who entered Harvard College in 1906 and stayed there as a graduate student and an instructor at the Physics Department until 1917, the equation "good physics = experimental physics" was "a very common belief at Harvard when I was a student there, and at that time I did not know of any university in this country where I could expect to find doubts about it."[72] Attracted by theoretical physics, Webster wanted to train as a theoretician, but was told "plainly and emphatically, that Harvard would never give a Ph. D. degree for any theoretical thesis."[73] In order to earn a doctorate, he had to place an experimental section into his theoretical thesis. To Webster's friend, Robert H. Kent, the Physics Department actually acted upon this prejudice: he wrote a thesis on an application of the virial theorem to a thermodynamic problem, and could not receive a Ph. D.[74]

In Bridgman's case, there seems to be no track of difficulty in choosing his field. From the beginning, he was too happy with his experimental research to think of other choices. In 1905, immediately after starting his graduate work, he hit upon an invention that produced high pressures no one else had reached before, and together with it, a vast and fertile unexplored field to which he was to devote a life-long effort. As Table 1-2-3 shows, Bridgman took a research course in "Light and Heat" by Wallace Sabine for four years at the Graduate School, which led Bridgman to his graduate study under the

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[72] Sopka, *Quantum Physics in America, 1920-1935,* p. 1.38.
[73] *Ibid.*
[74] *Ibid.*, pp. 1.38-1.39.

auspices of Sabine, a specialist in acoustics.[75] Despite Sabine's specialty, Bridgman started with optical phenomena and attempted to investigate "the effect of pressure upon the indices of refraction of liquids."[76] Soon after beginning his experiment, however, an accident happened that kept him away from optics:

> After my apparatus was constructed and some preliminary manipulations were made, there was an explosion—something very likely to happen with glass, which is most capricious. This destroyed an essential part of the apparatus, which had to be reordered from Europe; the United States had not at that time acquired its present degree of instrumental independence. In the interval of waiting for the replacement I tried to make other use of my apparatus for generating pressure. While designing a closure for a pressure vessel, so that it could be rapidly assembled or taken apart, I saw that the design hit upon did more than originally intended; the vessel automatically became tighter when pressure was increased, so that there was no reason why it should ever leak.[77]

Figure 1-1 illustrates the design for sealing he then invented. The force exerted by the piston is the same as the one exerted by the packing that is usually made of rubber. The hydrostatic pressure in the liquid is calculated by dividing this force by the annular packing area plus the cross-section of the free space. On the other hand, the pressure in the packing is calculated by dividing the same force by the annular packing area only. The pressure in the packing is therefore always greater than the pressure in the liquid and prevents the leaking. This principle, because of the unsupported packing, is called "the unsupported area principle," and now applied in many fields to prevent leaks.

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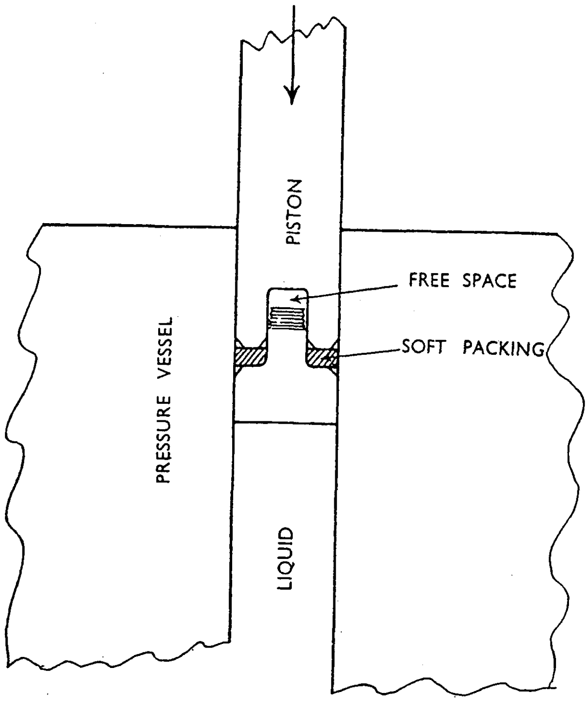
[75] Emily Thompson, "Dead Rooms and Live Wires: Harvard, Hollywood, and the Deconstruction of Architectural Acoustics, 1900-1930," *Isis*, 88 (1997), pp. 597-626.
[76] Kemble and Birch, "Percy Williams Bridgman, 1882-1961," p. 27.
[77] Percy Williams Bridgman, "Recent Work in the Field of High Pressures," *American*



Application to a piston of the principle by which the pressure in the packing is automatically maintained at a pressure greater by a fixed percentage than the pressure in the liquid. Leaks therefore cannot occur.

Figure 1-1. A piston using the principle of "unsupported area."
Source: P. W. Bridgman, "Some Results in the Field of High Pressure Physics," *Endeavour,* 10 (1951), p. 64.

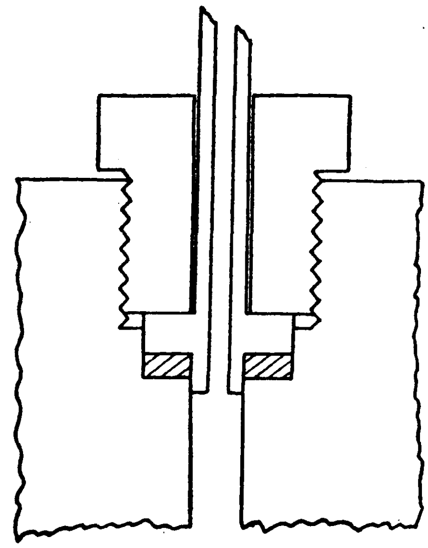


Figure 1-2. Amagat's type of packing, which leaks when the pressure gets as high as the initial pressure exerted by the screw.
Source: P. W. Bridgman, *The Physics of High Pressure* (New York: Dover, 1970), p. 81.

Scientist, 31 (1943), p. 3.

In the history of high-pressure experimentation, the prevention of leaks had been the most crucial concern.[78] Before Bridgman, the French physicist E. H. Amagat reached the highest pressures by the apparatus shown in Figure 1-2. Amagat's main interest was in the compressibility of gases and liquids. Although his method of packing brought a maximum pressure of 3,000 or sometimes 4,000 kg/cm^2, the packing shrank and caused leaks at higher pressures. Amagat also developed special methods for measuring the various physical properties under high pressures, until he abruptly stopped his experimental work in 1893. That year, the German chemist G. Tammann started a long series of experiments on the physical and chemical properties of dilute liquid under high pressures. As he did not make many improvements in high-pressure technique, the maximum pressure he reached remained around 3,000 kg/cm^2. Yet, he measured the properties of liquids systematically and extensively, especially in connection with the phenomena of solidification. In 1903, Tamman published the results of his research in a volume titled *Kristallisieren und Schmelzen*.[79]

Bridgman's invention in 1905 that prevented leaks automatically enabled scientists to reach any desired pressure, as far as the strength of the metal parts allowed. Very early in his experimental research, while following the melting curve of water, Bridgman had already reached up to 21,000 kg/cm^2. At this pressure, however, the apparatus could survive only a single application; it received a permanent stretch that made it useless for other experiment. For about three decades since the beginning of his experiment, the maximum pressure involved in most of his work remained 12,000

[78] For the history of high-pressure experiment, see, P. W. Bridgman, *The Physics of High Pressure* (London: G. Bell and Sons, 1931), pp. 1-29.

kg/cm². An apparatus without initial flaws could stand several hundred applications of pressure in this range without causing fracture.[80]

Later, when asked what the most important discovery in his life was, Bridgman answered that it was doubtless the discovery of a method of producing high hydrostatic pressure without leaks.[81] This innovation was as well the most important discovery in the history of high-pressure physics. Though having just begun his graduate research in optics, Bridgman realized the importance of his invention and quickly moved to an entirely new field.

> My intended optical experiment was therefore dropped; the laboratory wrote off the expense of the replacement part and of the apparatus already constructed, and the development of the new field was begun. I never returned to the original problem. This was a case where pertinacity of purpose would not have been good tactics.[82]

However, the maximum pressure attained in his graduate research by a screw compressor turned with a six-foot wrench remained 6,500 kg/cm², not much higher than those reached by other researchers.[83] His main concern in his graduate study was the establishment of a reliable pressure scale. The title of his Ph. D. dissertation, submitted in 1908, was "Mercury Resistance as a Pressure Gauge." The reviewers of Bridgman's dissertation were John Trowbridge, Wallace Sabine, G. W. Pierce, B. O. Peirce, H. W. Morse, H. N. Davis, and Theodore Lyman.

Even though Bridgman could attain drastically high pressures, this capacity would mean nothing to scientific knowledge if he did not present a reliable method of measurement that confirmed his

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[79] Gustav Tamman, *Kristallisieren und Schmelzen* (Barth: Leipzig, 1903).
[80] P. W. Bridgman, "Recent Work in the Field of High Pressures," p. 7.
[81] P. W. Bridgman, "Autobiographical remarks by P. W. Bridgman."
[82] Bridgman, "Recent Work in the Field of High Pressures," p. 4.
[83] P. W. Bridgman, "The Technique of High Pressure Experimenting," *Proceedings of*

achievement. Moreover, there was no guarantee that the scale previously used could work well under high pressures that no one had ever achieved. A reliable way of measurement is essential, especially when one is about to open an entirely new field. Bridgman was sure that he had opened a new field of physical research by his unexpected invention. The next thing he intended to do was to establish his high-pressure experimentation and to make the contemporary scientists accept it. For this purpose, Bridgman needed to construct a new type of gauge that could work under higher pressures than had been achieved before.

In 1909, Bridgman published the results of his graduate research in three successive papers in the *Proceedings of the American Academy of Arts and Sciences*.[84] In these three papers, though he had by then been fully aware of the significance of his invention, he mentioned almost nothing about it, gave no illustration of the design, and only discussed the construction of the gauges. Before the "unsupported area" seal broadened the range of reachable high-pressure, the free-piston gauges invented by Amagat had been used. Yet, the gauges could not work in the new domain. Bridgman thus adopted the Amagat type of gauges for the primary gauges that served to calibrate the secondary gauges that were used. Constructed on the basis of the pressure-dependence of the electrical resistance of mercury, the secondary gauges were used in the actual measurements. Bridgman had to alter the Amagat gauge for use under higher pressures than before and to improve the mercury resistance gauge for measurements in the new field. Furthermore, he needed to correct the empirical readings of both of the gauges. The reading of the free-piston gauge

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the American Academy of Arts and Sciences, 49 (1914), p. 629.

[84] P. W. Bridgman, "The Measurement of High Hydrostatic Pressure. I. A Simple Primary Gauge," *Proceedings of the American Academy of Arts and Sciences*, 44 (1909), pp. 201-217; "The Measurement of High Hydrostatic Pressure. II. A Secondary Mercury Resistance Gauge," *ibid.*, pp. 221-251; "An Experimental Determination of

involved some effects from distortion, stretching of the cylinder, and leaking of the liquid. The mercury resistance gauge was affected by impurities in the mercury, the compressive action of the pressure, and the amalgamation of the mercury and the steel vessel.

Having attained high pressures, Bridgman had to consider how much accuracy he should require for his measurement. No absolute standard for the accuracy was available for him. The accuracy was relative to the type of phenomena, of which almost nothing had been known before the measurements were actually carried out. Furthermore, the physicist could not predict what kind of measurements would later become significant enough to call for further refinement. Bridgman could have been content only with his invention of the new apparatus for reaching higher pressures than before and could have left the problem of measurement to others. However, he could not stand to imagine that at some later time all of his measurements would have to be done over again.[85]

In the process of refining the method of measurement, the physicist has to make a compromise somewhere; in Bridgman's case, "the nature was mostly permitted to take its course."

> A certain degree of accuracy was obtainable without too great effort with the pressure gauges adapted for the new domain, and this I accepted, hoping that it would be sufficient. Up to 12,000 [kg/cm^2] it proved to be possible to measure pressure easily with an accuracy of about 0.1 per cent. In justification of having rested content with 0.1 per cent, it may be said that at that time physical theory did not seem to demand even as accurate a knowledge as this of those phenomena that would be naturally studied at these pressures. The theory of liquids, for example, certainly was not in a position to demand such accuracy.[86]

As one of the standards, he counted on the accuracy demanded by

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Certain Compressibilities," *ibid.*, pp. 255-279.

[85] Bridgman, "Recent Work in the Field of High Pressures," p. 7.

contemporary physical theory.

Bridgman's later study was to supercede the results shown in the papers published in 1909: he would soon improve the primary gauge, change the mercury gauge to a manganin wire gauge, and revise the compressibilities of steel, mercury and glass reported in the third paper. Still, the 1909 papers built the foundations for his later work. The new packing device, though not mentioned in the papers, continued to be adopted in his later experiments. Through the work published in those papers, Bridgman established the basis for estimating and deciding the appropriate accuracy of measurement. Later, he tried many different materials for the containing vessel, the gauges, and the specimens, but he adopted the same principles of packing and measurement for the following twenty years.

In 1911, Bridgman reported pressures up to 20,000 kg/cm^2 or even higher.[87] Still reluctant to reveal the design of the sealing, he only left a short remark: "The magnitude of the fluid pressures mentioned here requires brief comment, because without a word of explanation it may seem so large as to cast discredit on the accuracy of all the data."[88] In 1908, the president of Watson-Stillman, a company dealing in hydraulic equipment wrote twice to him for the design of the packing, but did not receive any reply.[89] In 1912, John Johnston, a physicist at the Geophysical Laboratory, asked for the information of the sealing technique and received it from Bridgman, on the condition that Johnston should keep quiet and preserve Bridgman's letter.[90] Bridgman may have desired to complete his devices before disclosing them. Some suspected that he might have been thinking of patenting

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[86] *Ibid.*
[87] P. W. Bridgman, "The Action of Mercury on Steel at High Pressures," *Proceedings of the American Academy of Arts and Sciences*, 47 (1911), pp. 321-343.
[88] *Ibid.*, p. 330.
[89] Walter, *Science and Cultural Crisis*, pp. 38-39.
[90] *Ibid.*, p. 39.

the apparatus.[91] Bridgman's letter written in 1912 to Jerome Greene, General Manager at the Rockefeller Institute for Medical Research, yet suggests another possibility: "With regard to setting up an apparatus for you for the high pressure work, I have no objection at all to imparting the technique to others. The reason that I have not published it as yet is because of lack of time as much as anything else."[92] As Bridgman successively obtained new experimental results, he was too busy to write about the fundamental device.

Bridgman disclosed the details of packing, pistons, cylinders, gauges, and so forth as late as in 1914.[93] The sort of work Bridgman had been engaged in also became clear. He explained the necessary skills to drill the tubing from the solid rod as follows:

> The inside diameter of the tubing is 1/16 of an inch, and it is quite possible with a little practice to drill pieces at least 17 inches long. The drill should be cut on the end of a long piece of drill rod; it does not pay to try to braze a long shank onto a short drill.[...] The drill need not be expected to run more than 1/2 of an inch out of center on a piece 17 inches long. After getting the drill accurately started for two or three inches it will be found convenient to put the drill in a hand tool holder and force it in by hand.[...] I have found that it pays to carefully clean out the hole with a swab after drilling not more than 1/8 of an inch. It is easy, if all precautions are observed, to drill a hole 1/16 of an inch in diameter 17 inches long in from seven to eight hours.[94]

To complete appropriate parts for the experiments, the physicist himself had to work patiently. It took almost one day to finish only one piece of tube. And one piece of tube was not at all enough, especially for his

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[91] High-pressure physicists have rumored that Bridgman's application for a patent for the invention of this packing technique was turned down as the same method had already been adopted in the apparatus for mincing meat (Naoto Kawai, *Cho-koatsu no sekai* (The World under High Pressures) (Tokyo: Kodan-sha, 1977), p. 54).
[92] Walter, *Science and Cultural Crisis*, p. 39.
[93] P. W. Bridgman, "The Technique of High Pressure Experimenting," *Proceedings of the American Academy of Arts and Sciences*, 49 (1914), pp. 627-643.
[94] Bridgman, "The Technique of High Pressure Experimenting," p. 638.

type of experiments, where explosions caused by high pressures were almost everyday affairs. Each of the parts required several days of work. Through the eyes of students, Bridgman was a resilient lab-coated figure, either drilling holes in his clumsy apparatus or bending over the big lathe.[95] Naturally, he did not have enough time to write papers. Furthermore, it is easy to understand why Bridgman remained to be the first one to be mentioned in high-pressure physics even after he disclosed his devices; no one but the founder of the field could be so enthusiastic and patient over this traditional field that demanded dirty, hard, and sometimes dangerous work, in a period when new and attractive research fields were emerging one after another.

1.3.2. An Experimentalist's Life

Bridgman preferred to work by himself. Although an experimental assistant and a machinist served him with his experiments, he continued to work literally with his hands. Several students remembered Bridgman arriving at his laboratory by bicycle and starting his day by pumping up the pressure by hand. Edward Purcell guessed that he "took satisfaction in providing with his own muscle the ultimate work of compression, -PdV."[96]

During his long service at Harvard, he supervised only fourteen doctoral theses on high-pressure topics and several ones on other subjects.[97] Although it was rare to see more than three students in his laboratory at a time, he was unwilling to converse even with this small number of students. Gerald Holton, who wrote his Ph. D. dissertation under Bridgman's supervision, remembered that he was allowed to do

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[95] Edward M. Purcell, "The Teacher and Experimenter," in "Expressions of Appreciation," PWBP, HUG 4234. 25.
[96] *Ibid.*
[97] Kemble and Birch, "Percy Williams Bridgman," p. 32.

his thesis work with Bridgman as long as he would not bother his advisor too often. In fact, no one could bother him easily while he was taking data. In the fall of 1946, when Holton told him about the phone call that asked for an interview with Bridgman when the news that he won the Nobel Prize had just arrived, Bridgman continued at the pump and only said, "Tell them—I'll believe it—when I see it," without missing a stroke.[98]

At his laboratory, Bridgman did not care much about social niceties. Hatten S. Yoder Jr.[99] remembered that Bridgman allowed him only seven minutes when he, as a graduate student at MIT, made an appoint to visit Bridgman for advice on the design of high-pressure apparatus.[100] William Paul visited Bridgman's laboratory in 1952 with some suggestions about extending high-pressure techniques to the study of semiconductors. He happened to break open his head on a sharp piece of steel used as a shield. Upon seeing Paul's bloody hands and head, Bridgman only called his machinist, "Chase!" and came back to his measurement at the pump.[101] Students and colleagues not close enough to Bridgman were even scared of him.[102]

Paul was on the fortunate side. High-pressure experiments could cause disastrous accidents. Between October 13 and December 3, 1910, for instance, Bridgman recorded at least five explosions with "two lower cylinders being burst, two upper cylinders, and one connecting pipe."[103] In the most serious accident, which occurred on May 19, 1922, an engineering research fellow Atherton K. Dunbar was blown to

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[98] Holton, "Percy W. Bridgman," p. 223.
[99] For Hatten S. Yoder, Jr., see, E. F. Osborn, "Hatten S. Yoder, Jr.," in B. O. Mysen, ed., *Magmatic Processes: Physicochemical Principles (A volume in honor of Hatten S. Yoder, Jr.)* (University Park, Pennsylvania: The Geochemical Society, 1987), p. 1.
[100] Robert M. Hazen, *The New Alchemists: Breaking Through the Barriers of High Pressure* (New York: Times Books, 1993), p. 48.
[101] William Paul's untitled paper presented at Percy Williams Bridgman Symposium, April 23-24, 1982. HUG 4234.92.
[102] Hazen, *The New Alchemists*, p. 48.
[103] P. W. Bridgman, "Mercury, Liquid and Solid, under Pressure," *Proceedings of the American Academy of Arts and Sciences*, 47 (1911), p. 414.

pieces, and an assistant William Connell was killed instantly. Shortly after the event, a new research laboratory for high-pressure experiment was built and named after Dunbar.[104]

Some high-pressure experimentalists called their high-pressure cylinders "bombs" and covered them with shields. However, even after several accidents, Bridgman, who preferred simplicity of apparatus, did not like to prepare shields since he knew the weak point of an apparatus (usually a valve spindle) and could predict its trajectory. Those who visited Bridgman at his laboratory, if they were lucky, could see him jumping high into the air in the middle of the room in order to avoid the possible trajectory of his pressure-gauge stem. Furthermore, careful visitors might find the fine holes in the windows of Bridgman's laboratory that had been drilled by such high-speed projectiles.[105]

While working with his students, Bridgman always took the part he considered to be the most difficult, that is, the design and construction of the apparatus. John Slater, the theoretical physicist and one of his first thesis students, recalled, "[H]e would spend most of his time in the shop or laboratory personally making part of his equipment or designing the rest, leaving the taking of observations mostly to assistants."[106] Bridgman fully understood that the most crucial part for experiments was in the preparation of equipment, samples, and environment for measurements.

Despite the high pressures he reached, the devices Bridgman employed were so simple that they allowed him to enjoy making them by himself. He conducted most of his experiments with pistons, springs, levers, and falling weights, combined with direct-current

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[104] Robert M. Hazen, *The New Alchemists*, p. 57.

[105] David T. Griggs, "High-pressure Phenomena with Applications to Geophysics," in Louis N. Ridenour, ed., *Modern Physics for the Engineer* (New York, Toronto, London: McGraw-Hill, 1954), pp. 272-235, p. 277.

[106] J. C. Slater, "Presentation of Bingham Medal to P. W. Bridgman," p. 201. See also interview with Slater conducted by T. S. Kuhn and J. H. Van Vleck on October 3, 1963, AHQP.

electrical measurements of resistance and potential.[107] The electronic devices, even the simple vacuum tube, played no role in his work, while the electrical parts of the measuring system were mostly home-made. To read the scales, he used meter-long slide wires with reading glasses. In 1910, Bridgman reached 20,000 kg/cm^2 with these devices. By the mid 1930s, he succeeded in producing 50,000 kg/cm^2 with some improvement in the design of the sealing. Even when a maximum pressure rose to 400,000 kg/cm^2 or higher, the fundamental devices employed remained almost the same simple ones.

Bridgman's ingenious invention and hard work with simple devices, however, were not enough to bring success even to such inexpensive research as his. The development of industrial technology played a crucial part, too. The metal parts of the apparatus, especially the piston and the vessel, had to be strong enough to stand the pressures he intended to produce. Although he fortunately started his research at a time when the advances in industrial metallurgy were bringing about stronger steels, the search for suitable steels was not an easy task. Before Bridgman started his research, no way to test steels under such high pressures had been available. He therefore had to try almost all the possible steels. Conversely, steel companies considered his experiments to be precious chances to examine the strength of steels and were willing to help him.[108]

Every time Bridgman was about to reach higher pressures, he needed new steels. He reached the pressure of 20,000 kg/cm^2 after finding the Krupp nickel steel, which was soon replaced by an electric-furnace chrome-vanadium steel, used for most of the vessels and connecting tubes. No steel piston, however, could support as much as 50,000 kg/cm^2. He had to wait for General Electric's new steel, carboloy, formed by cementing a fine powder of tungsten carbide

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107 Kemble and Birch, "Percy Williams Bridgman, 1882-1961" p. 33.
108 Walter, *Science and Cultural Crisis*, n. 1, p. 36.

with cobalt. By adopting the pressure cylinder and piston made of carboloy, Bridgman made the step from 50,000 to 100,000.[109]

The new expensive steel was not easy to purchase. At first, the carboloy division of General Electric presented Bridgman pistons made of carboloy, which cost more than its weight in gold in the open market. These pistons were continually breaking in Bridgman's experiments. In a letter reporting the proposed expenditure of the Carnegie Grant for the year 1937, Bridgman wrote that he could not count on the company's courtesy indefinitely, having already used the carboloy equivalent to eight hundred dollars.[110] The other items, such as a hydraulic pump, a hydraulic multiplier, a small electric furnace, and expert assistance in X-ray analysis of minerals, cost seven to eight hundred dollars in total. The running expenses, including the one for renewing parts made of the best grades of steel, were between five hundred and one thousand dollars per year. Except for the annual salary of assistants, which was 2,400 dollars per person in 1934,[111] carboloy was the largest single item of expense for Bridgman.

Bridgman learned from the industrial and governmental engineers the experimental techniques for preparing appropriate samples including large single crystals and for dealing with the new types of steel. He sometimes asked them for the samples he needed. He therefore kept contact with such companies as General Electric, Westinghouse, Fansteel Products Company, and New England Electrical Works, as well as several steel companies.[112]

Having established techniques for producing high pressures and ways of measurement, Bridgman could apply them to various, or almost

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[109] P. W. Bridgman, "A General Survey of Certain Results in the Field of High Pressure Physics," in *Nobel Lectures: Physics, 1942-1962* (Amsterdam: Elsevier Publishing Company for the Nobel Foundation, 1964), pp. 53-70.
[110] Bridgman's report is included in Theodore Lyman's letter to Bridgman, Nov. 23, 1937, UA V 692.5.
[111] Keppel to Theodore Lyman, May 21, 1934, UA V 692.5.
[112] Walter, *Science and Cultural Crisis*, p. 50.

all, kinds of samples, in order to investigate mechanical, thermal, and electromagnetic properties of matter, including compressibility, electrical resistance, elastic constants, viscosity, and thermal conductivity. Some typical titles of his papers are, "The Effect of Pressure on the Viscosity of Forty-Three Pure Liquids,"[113] "Polymorphic transitions of 35 substances to 50,000 kg/cm^2," [114] and "Rough Compressions of 177 Substances to 100,000 kg/cm^2."[115] Bridgman's colleague at another department once remarked that "Bridgman assembles as much data in one paper as most physicists do in a lifetime."[116] By completing and applying only a series of techniques, he succeeded in discovering a variety of startling phenomena.

One of the most famous results was his discovery of "hot ice," a solid form of water at high temperatures, which Bridgman made while studying polymorphism under high pressures.[117] Edwin Hall reported on Bridgman's attitude toward his discovery of "hot ice":

> [T]he Boston *Herald* of December 14, 1928, published a photograph of Bridgman and some of his apparatus, with the heading *Machine That Can Boil Eggs In Ice Water* and a statement that with this machine 'potatoes can be baked at below zero temperature and ice manufactured that is red hot.' The last part of this statement is a reporter's version of the fact that the substance water may under great pressure exist in a solid condition, very different from that of ordinary ice, at temperatures much above that of common freezing. Bridgman proved this a long time ago, about 1910, but it did not occur to him to describe this new product as 'red hot ice,' and so the

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113 P. W. Bridgman, "The Effect of Pressure on the Viscosity of Forty-Three Pure Liquids," *Proceedings of the American Academy of Arts and Sciences*, 61 (1926), pp. 57-99.

114 P. W. Bridgman, "Polymorphic transitions of 35 substances to 50,000 kg/cm^2," *Proceedings of the American Academy of Arts and Sciences*, 72 (1937), pp. 45-136.

115 P. W. Bridgman, "Rough Compressions of 177 substances to 100,000 kg/cm^2 ," *Proceedings of the American Academy of Arts and Sciences*, 76 (1948), pp. 71-87.

116 Quoted in John H. Van Vleck, "Percy Williams Bridgman," *Year Book 1962, the American Philosophical Society* (1963), pp. 106-110, p. 107.

117 P. W. Bridgman, "Water, in the Liquid and Five Solid Forms, under Pressure," *Proceedings of the American Academy of Arts and Sciences*, 47 (1911), pp. 441-558.

public mind remained unexcited.[118]

By the time this *Boston Herald* news appeared, Bridgman had been known to general readers for his *Logic of Modern Physics*, published in 1927. Around the same time, however, the newspaper introduced him as a physicist who boiled eggs in ice water.

1.3.3. The Interaction between Experiment and Theory

While Bridgman was active in his research, theoreticians could not give theoretical explanations to many of the results he obtained. This situation did not change much even after the development of solid state physics.[119] In his attempt to understand his discoveries, Bridgman ventured, and sometimes felt required, to do theoretical work by himself.[120] But first, "he learned thermodynamics," as Slater explained, "in a sense because he wanted to get out of work."

> It is very hard to make thermal measurements at high pressure; and he wanted to know to what extent one can really be confident in calculating the results of hypothetical specific heat and latent heat experiments at high pressure, by using thermodynamics applied to other measurements that are easier to carry out. He found one really could be confident in these methods; but in the process he learned how to use thermodynamics, and has become one of the best experts in the field. Similarly his electromagnetic theory was vitalized by the uses he made of it in connection with electrical and magnetic measurements at high pressures.[121]

In a case like this, physical theory could play only an auxiliary role in

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118 Morison, *The Development of Harvard University*, p. 291.
119 John C. Slater, "P. W. Bridgman and High-Pressure Physics," *Science*, 148 (1965), pp. 805-806.
120 Some of the attempts were described in, P. W. Bridgman, "Theoretically Interesting Aspects of High Pressure Phenomena," *Reviews of Modern Physics*, 7 (1935), pp. 1-33.
121 John C. Slater, *op. cit.*, "Presentation of Bingham Medal to P. W. Bridgman," p. 201.

describing the results of experiments more completely. Nevertheless, Bridgman further attempted to construct theoretical schemes to explain some phenomena related to his experiments. When the argument remained within the realm of thermodynamics and electromagnetism, as in the cases of compressibility and polymorphism, his attempts seem to have worked to a certain extent. He could not, however, catch up with theoretical research involving the application of quantum mechanics, as in the case of the electron theory of metal (see §2.1).

After acquiring interesting results from the measurements of electrical resistance under high pressures and the effect of pressure on thermoelectric properties, Bridgman spent some time in speculating on thermal and electrical conductivities of metal and tried to explain them on his own scheme. He was one of a few Americans invited to the Fourth Solvay Conference on theories of electrical conduction, held in Brussels in 1924.[122] Through this theoretical study, Bridgman "did get a new point of view which had its elements of interest for a while," as he recalled in 1943, "but [it] was presently made obsolete by wave mechanics."[123] By then, the time when one physicist could be productive in both experimental and theoretical researches had almost passed.

With the development of theoretical study in solid state physics based on quantum mechanics, theoretical physicists succeeded in explaining at least a small part of Bridgman's experimental results. Ironically, however, the rapid progress of theoretical study after the advent of quantum mechanics convinced Bridgman that it became more and more difficult for him to combine theoretical and experimental productivity. Still, Bridgman continued his effort to keep up with the

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[122] Bridgman's report was published in 1927. P. W. Bridgman, "Rapport sur les phénomènes de conductibilité dans les métaux et leur explanation théorique," in *Conductibilité électrique des métaux et problèmes connexes (Report of the Fourth Solvay Congress, April 24-29, 1924)* (Paris: Gauthier-Villars, 1927), pp. 67-114.
[123] Bridgman, "Recent Work in the Field of High Pressures," p. 11.

contemporary development of theoretical research. Before the quantum physical theory of solid state appeared, he relied upon Max Born's theory of solids[124] in calculating the compressibilities of various samples, finding it fail to explain the volume-pressure relation.[125] In the late 1930s, applying the method for calculating the energies of the monovalent metals developed by Eugene Wigner and F. Seitz, John Bardeen succeeded in giving an accurate theoretical explanation of Bridgman's results for the alkali metals.[126] Bridgman welcomed Bardeen's success with enthusiasm and satisfaction.[127] He did not like his work to "degenerate into the hoarding of new data for their own sake."[128]

On the other hand, Bridgman was sure of the value of experimental research independent of theoretical concern. In 1959, looking back his experimental work, he wrote that his interest had been "almost entirely in discovering what new things were in fields hitherto unexplored."[129] Although Bridgman admitted that he had never made an experiment without having some kind of expectation of the result, he emphasized that "the interest of the experiment was not at all to verify the expectation." Experimental physicists' tactics, when dictated by the desire to play a significant role in theoretical study, can even prove to be ill advised. Bridgman experienced this situation when he tried to raise the accuracy of measurement of the effect of pressure on the thermal conductivity (see §2.1).[130]

While Bridgman immersed himself in high-pressure experiment,

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124 Max Born, *Atomtheorie des festen Zustandes (Dynamik der Kristallgitter)* (Leipzig and Berlin: Teubner, 1923).
125 Bridgman, "Recent Work in the Field of High Pressures," p. 24.
126 John Bardeen, "Compressibilities of the Alkali Metals," *Journal of Chemical Physics*, 6 (1938), pp. 372-378.
127 Edward M. Purcell, "The Teacher and Experimenter."
128 Bridgman, "Recent Work in the Field of High Pressures," p. 25.
129 P. W. Bridgman, *The Way Things Are* (Cambridge, Mass.: Harvard University Press), pp. 131-132.
130 Bridgman, "Recent Work in the Field of High Pressures," p. 11.

remarkable progress in theoretical physics took place outside his laboratory. His experimental research had little to do with the two significant products of the twentieth-century physics, relativity theory and quantum mechanics. In establishing the high-pressure experimentation that had started with an accidental invention of a better sealing, Bridgman had no specific physical theory in his mind to which his research would contribute. When he launched a series of measurements, his goal was to obtain knowledge of physical properties of matter under high pressures. Although Bridgman attempted to construct his own theoretical schemes, his main concern remained with expanding experimental research, not with forming theoretical frames.

Despite its apparently small contribution to contemporary theoretical research, Bridgman's high-pressure experiments received high recognition in and outside Harvard and the United States, which culminated in his receiving the Nobel Prize for 1946. Although one may suppose that the high evaluation of Bridgman's research was mainly due to its applications to industrial use, the contemporaries praised Bridgman's experiments almost exclusively for their purely scientific value. In fact, high-pressure physics did not turn out to be industrially useful while Bridgman was alive, producing few remarkable commercial products, except for the artificial diamond that he failed to synthesize at his laboratory despite several serious attempts.

The reputation of Bridgman's research had little to do with its possible industrial application or its expected contribution to the development of theoretical study. Bridgman's research consisted of two main parts: the establishment of high-pressure experimentation and the subsequent discovery of new phenomena under high pressures. Of these two, the former was more significant. When he hit upon a new idea of leak-proof packing, he did not know what he would discover at pressures higher than 3,000 kg/cm^2. It was possible that at high pressures he would find no particular phenomenon that would attract contemporary physicists' attention. However, when Bridgman came across a new idea, he immediately understood the significance of the invention and started to concentrate on establishing the high-pressure technique. He had had to wait for several years until he could submit his first report on various strange behaviors of matter under high pressures. It was not a specific theory, substance, or phenomenon that brought Bridgman into high-pressure physics. True, the discovery of marvels under high pressures enhanced his enthusiasm and contributed to high evaluation of his research; however, without accomplishing the trustful high-pressure experimentation, his discoveries could have been meaningless. Throughout his scientific career, he recognized the importance of broadening the range of reachable pressures and establishing reliable means of its measurement. Although Bridgman applied part of his talent to the preparation of samples, his main concern was with improving high-pressure experimentation. Having begun with the unsupported area principle, he went on to develop the "cascade" method and "Bridgman's anvil" to produce even higher pressures in the 1930s.

By making reachable and measurable pressures higher, Bridgman broadened the controllable range of pressures. In Bridgman's own expression, he expanded "the universe of operations"[131] in the realm of high pressures. Bridgman's experimental activity shows that the business of expanding the universe of operations in a particular realm, even though it had little to do with contemporary theoretical research or industrial interest, can occupy an entire scientific career.

[131] The phrase "the universe of operations" was originally the title of a critical review of Bridgman's *Nature of Modern Physics* (New York: Dover Publications, 1936) by William Marias Malisoff (Malisoff, "The Universe of Operations," *Philosophy of Science*, 3 (1936), pp. 360-364). Bridgman adopted it in *The Nature of Thermodynamics* (Cambridge, Massachusetts: Harvard University Press, 1941) (see §5.2).

1.3.4. Applications of Bridgman's Research

Bridgman's work did not directly lead to many remarkable applications as one may expect from the nature of his research. Few industrial products were closely connected with his research. Although some companies asked him for information on the compressibility of various substances and the calibrated pressure gauges, Bridgman was never busy with purely industrial concerns.[132] On some occasions, however, Bridgman and his expertise in high-pressure physics played a crucial part for specific industrial or military purposes.

Scientists, including amateur ones, had for a long time understood that high-pressure techniques were essential for the synthesis of diamonds. Bridgman recalled in 1955 that for over a quarter century an average of two or three people had visited him every year to offer to share the secret and the profit of making diamonds in return for his help with high-pressure techniques.[133] Bridgman was not aloof from the lure of synthesizing diamonds, either. David Griggs, who worked with Bridgman during the period when he raised working pressures from 20,000 to 100,000 kg/cm^2, noticed that each time he prepared a new apparatus, graphite was the first substance tried. Griggs observed that then "Bridgman would become secretive and brusque" and that "kibitzers were not welcome."[134] Bridgman applied 425,000 kg/cm^2 to graphite at room temperature and 70,000 kg/cm^2 at red heat (around 700 ℃). Trusting the calculated phase diagram published in 1938, Bridgman expected that he could produce stable diamonds at these pressures and temperatures; alas, he could find no transformation in these attempts.[135]

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132 Walter, *Science and Cultural Crisis*, p. 50-52.
133 P. W. Bridgman, "Synthetic Diamonds," *Scientific American*, 193 (1955), pp. 42-46, p. 42.
134 David T. Griggs, "High-pressure Phenomena with Applications to Geophysics," p. 282.
135 Bridgman, "Synthetic Diamonds," p. 45.

In 1941, seeking an independent source of industrial-grade diamond abrasives, three industrial companies, General Electric, Norton, and Carborundum persuaded Bridgman to enter into a 5-year agreement to build an appropriate apparatus and do tests.[136] Bridgman and the GE scientists knew that at atmospheric pressure diamonds begin to revert to graphite when heated to 1,500 ℃ in the absence of oxygen. Expecting that the reverse transition would occur at higher pressure and similar temperatures, they started a series of experiments with a thousand-ton press installed at the Harvard Geophysical Laboratory. They applied pressures from 30,000 to 45,000 kg/cm^2 to graphite alone or to graphite with tiny diamond seed crystals at temperatures above 2,000 ℃.[137] However, all the tests failed. As Bridgman soon became occupied with war work, his effort for synthesizing diamond lasted for less than two years instead of the five contemplated. In 1946, Bridgman and the three companies did not renew the five-year agreement. The Norton Company plant in Worcester took over the apparatus.

In 1954, GE announced their eventual success in synthesizing diamonds. The GE physicists established means to maintain pressures of more than 100,000 kg/cm^2 and temperatures of 2,500 ℃ for hours. Furthermore, they succeeded in finding and preparing a chemically favorable environment for crystallization. Although the high-pressure technique was essential for diamond synthesis, it turned out to be only the first step to success.

Bridgman was willing to apply his high-pressure technique to wartime problems too. In 1915, he wrote to A. G. Webster, a member of the Naval Advisory Board, about his idea of a method for increasing the yield point of artillery gun barrels by applying to the inside a

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[136] For details, see Walter, *Science and Cultural Crisis*, pp. 52-54.

[137] P. W. Bridgman, "An Experimental Contribution to the Problem of Diamond Synthesis," *Journal of Chemical Physics*, 17 (1946), pp. 692-698.

hydrostatic pressure high enough to strain the entire mass beyond the elastic limit.[138] The idea itself was not new; as Bridgman himself admitted, John Perry had suggested it before. Yet, only Bridgman had the technique that made it work. He had applied the same method to the construction of his pressure vessels. However, the idea, which would later be adopted widely, came too late for World War I.[139] In 1917, Bridgman started to work for another wartime research project, the detection of submarines by acoustical methods, and developed a sound insulating system.

During World War II, Bridgman studied plastic flow in steel in connection with the penetration of armor by projectiles.[140] Later, he published the result in *Studies in Large Plastic Flow and Fracture.*[141] For the Los Alamos Laboratory, he did secret work closely connected with his specialty, measurements of the compressibilities of uranium and plutonium.[142] In the last months of the development of atomic bombs, one of the main issues was how to deal with uranium and plutonium under high pressures. Data of their compressibilities were of a crucial importance. Although he never talked about this latter work in public, he published the results of these measurements[143] after he asked Cyril S. Smith, who served as the head of the project he was working for, to declassify the material.[144]

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138 Bridgman to A. G. Webster, Oct. 26, 1915, PWBP, HUG 4234. 8; Millikan to Bridgman, Sept. 15, 1917, PWBP, HUG 4234. 8.

139 Kemble and Birch, "Percy Williams Bridgman," p. 37.

140 Bridgman to N. Mott, Aug. 17, 1943, PWBP, HUG 4234.10.

141 P. W. Bridgman, *Studies in Large Plastic Flow and Fracture, with Special Emphasis on the Effects of Hydrostatic Pressure* (Cambridge, Mass.: Harvard University Press, 1964).

142 The file "Pressure on Plutonium," PWBP, HUG 4234.15; the file "Correspondence Los Alamos 1943-1945," PWBP, HUG 4234.17.

143 P. W. Bridgman, "Compression and the α-βtransition of plutonium," *Journal of Applied Physics*, 30 (1959), pp. 214-217.

144 Bridgman to Smith, May 14, 1954, PWBP, HUG 4234.10.

1.3.5. Prizes, Funds, and Job Offers

Bridgman's establishment of high-pressure experimentation and discovery of various physical properties of matter at high pressures received high recognition within the contemporary community of scientists. By 1930, the Harvard Physics Department had come to regard Bridgman as the most productive and most prominent experimentalist among the staff members. He would remain so until a younger Harvard physicist Edward Purcell, with Robert Pound and Henry Torrey, found nuclear magnetic resonance in 1945.[145] The list of national and international medals and prizes Bridgman received shows that his reputation was not local: the Rumford medal of the American Academy of Arts and Sciences (1917), the Cresson Medal of the Franklin Institute (1932), the Roozenboom Medal of the Netherlands Royal Academy (1933), the Comstock Prize of the National Academy of Sciences (1933), the Research Corporation of America Award (1937), the Nobel Prize for physics (1946), and the Bingham Medal of the Society of Rheology (1951).

The observations of Bridgman's teachers and colleagues tell more details of his reputation. As early as in 1907-1908, while he was completing his graduate research, his achievement already attracted the special attention of the Director of the Jefferson Physical Laboratory, John Trowbridge. In his report on the activity of the Physics Department submitted to the President of Harvard College, he praised Bridgman's research: "Among these researches are several of great practical importance: that of Mr. Bridgman is epoch making, for he has carried the work far beyond the celebrated investigation of Amagat."[146] In 1915, Theodore Lyman, Director of the Jefferson Laboratory of

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[145] For the discovery of nuclear magnetic resonance at Harvard, see, Mark Gerstein, "Purcell's Role in the Discovery of Nuclear Magnetic Resonance: Contingency versus Inevitability," *American Journal of Physics*, 62 (1994), pp. 596-601.

[146] *Report of the President and the Treasurer of Harvard College: 1907-08* (Cambridge,

Harvard, reported to President Lowell that Bridgman was the most prolific writer of scientific papers in the laboratory.[147] By the beginning of the 1920s, atomic physics had become the most popular field among both experimentalists and theoreticians. Nevertheless, Bridgman's work kept attracting attention. Frederick A. Saunders, the Chairman of the Physics Department, reported in 1934 to the Dean of the Faculty of Arts and Sciences on the activity of the Department, evaluating Bridgman's research.

> It would require a prophetic insight to enable one to predict which of these researches will prove to have the greatest ultimate value. It might be safe to guess that Professor Bridgman's research commands the most serious attention outside, and this can be said in spite of the fact that his field is not one that happens to be fashionable at the moment.[148]

The same year, Lyman numbered Bridgman "among the first half dozen scientific men of this country,"[149] in a letter asking for research money to pay the salary of Bridgman's assistant.

Outside Harvard, too, Bridgman's work received high recognition, as job offers to him from other institutions show. Although he never left Harvard until he retired in 1954, many other institutions offered him various positions. At least a few of them Bridgman seriously considered. In 1909, the first offer came to Bridgman, who was then a research fellow, from Arthur Day, Director of the Geophysical Laboratory of the Carnegie Institute of Washington.[150] Having spent many years at his alma mater, Bridgman was then considering the desirability of some change for broadening his perspective and

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Mass.: Harvard University, 1909), p. 254.

[147] *Report of the President and the Treasurer of Harvard College: 1914-15* (Cambridge, Mass.: Harvard University, 1916), p. 223.

[148] Saunders to K. Murdock, Dean of the Faculty of the Arts and Science of Harvard University, Oct. 15, 1934, DPCC, UA 691.10.

[149] Lyman to Henry James, April 14, 1934, PLDC, UA V 692.5.

[150] Day to Bridgman, April 30, 1909, PWBP, HUG 4234.8.

intimated this to Day during his visit to Cambridge. For theoretical study Day suggested that he go abroad; for research into the properties of matter under extreme pressures and temperatures, Day regarded the Carnegie Institute as having a better opportunity than anywhere else, either in America or abroad. In reply, Bridgman wrote that his "interest is almost entirely in experimental research and much less in teaching." "Neither," he went on, "is the prospect of theoretical study abroad inviting." However, Bridgman declined Day's offer. He doubted "whether in a laboratory dedicated to a special purpose the same freedom of choice of subjects would be possible [...] as in a University."[151]

Nevertheless, Day tried again in 1916 with a generous offer of a position, promising too favorable conditions for Bridgman, then an assistant professor at Harvard: four thousand dollars as an annual salary, two experienced assistants with doctorates, a shop with five instrument makers, and ample financial resources.[152]

The contemporary developments in geophysical research made Bridgman's work appear attractive to laboratory directors and department chairmen. Bridgman started to broaden the range of experimentally reachable pressures exactly when geophysicists began to feel the need for high-pressure techniques. In 1906, one year after Bridgman discovered his packing technique, Richard Oldham, an Irish geologist who directed the Geological Survey of India, discovered key data on the earth's core. Seismologists started to improve travel times for seismic waves, opening a new chapter in the study of the earth's internal structure and composition. In 1909, when Bridgman published his first papers on high-pressure physics, Andrija Mohorovičić published his discovery of evidence for the discontinuity of

[151] Bridgman to Day, May 7, 1909, quoted in Walter, *Science and Cultural Crisis*, p. 26.
[152] Day to Bridgman, Nov. 25, 1916, PWBP, HUG 4234.8.

seismic velocity. The mean pressure at this discontinuity, located about 35 km below the surface, was estimated at about 10,000 kg/cm^2. Around that time Bridgman was the only experimentalist who could offer techniques to reach this pressure in a laboratory. Seismic velocities could give information on values of compressibility and distributions of density. In order to study the implications of these densities and compressibilities in terms of minerals or rocks, geophysicists and geologists needed to conduct various kinds of experiments that all used Bridgman's high-pressure techniques.[153]

Bridgman, however, showed the same kind of doubt as before that the work at the Carnegie Laboratory "contains an inherent limitation in the matter of program"[154] and again turned down Day's offer. This time Day did not give up easily, sending Bridgman a long reply:

> I was indeed disappointed by your decision in the matter of which we have been speaking. If it is final it means a real sense of loss to us which is shared by Dr. Woodward and by several members of our staff who have repeatedly expressed the wish that you might join us now that an appropriate opening has occurred. But I think it also means more, it means a loss to scientific progress, under present conditions, amounting to at least one half of your possible productiveness. With two assistants of the quality I have indicated to you and a very unusual equipment both in shop and laboratory, there is certainly no question that you would with equal effort more than double your contributions to science. It is this situation that I cannot bring myself to understand and it is this situation also which offers the reason for this further communication, for I shall presently suggest an effort to avert it.
>
> You see I have had the utmost confidence in your conscientious fidelity to research, gained from a rather intimate knowledge of your work extending back to its beginning, and I simply cannot understand how you can sacrifice this proven loyalty to science, to your loyalty to the place of your birth and

[153] Later, Bridgman committed himself to geophysical study, and one of his students, Francis Birch, became a geophysicist after he earned his Ph. D. under Bridgman. Francis Birch, "Bridgman and Geophysics," MS, in Papers Presented at PWB Centenary Symposium, April 23-24, 1982, HUG 4234.92.

[154] Bridgman to Day, Sept. 15, 1916, PWBP, HUG 4234.8.

early training however firmly this latter be grounded. If I had suggested a transfer of your activities to the industrial world or to another university where you might have been overburdened with various distracting duties, which threatened your ideals, I could understand your hesitation, but I have offered, simply, to double your opportunity, as I see it, without the sacrifice of a single ideal or imposing a single countervailing burden. If this were an appropriate time or place I would even assert the thesis that *when other things are equal* a man is more competent outside his initial educational environment than he is in it.[...]

> [...]If then you are still in doubt about how to decide I will make this very unusual personal proposal, you are at liberty to show the offer to the acting head of your department or to President Lowell [of Harvard] and to ask his advice.[155]

Day was persistent. On November 25, he wrote two letters, one detailing the conditions of the position offered to Bridgman, and the other, apparently written after Day received Bridgman's declining reply, explaining that Bridgman would be better-off in Washington than in Cambridge. Bridgman finally decided to go down to Washington, D. C. to see the Geophysical Laboratory and was almost in a mood to accept the offer, until he came back to Cambridge and found Lyman with his own offer. In the final letter to Day, Bridgman wrote:

> When I returned to Cambridge Dr. Lyman was waiting for me with the promise of enough money, which he had raised during my absence, to obtain an additional assistant for at least five years. If it had not been for this new development I feel that I would probably have accepted your offer, in spite of the reasons for hesitancy of which I spoke to you so freely.[...] As I told you, I am sure that no one could have done more to persuade me than you have, and I deeply appreciate all the trouble you went to and the pleasure you gave me during my brief visit.[156]

Lyman, Director of the Jefferson Physical Laboratory at Harvard, was

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[155] Day to Bridgman, Nov. 25, 1916, PWBP, HUG 4234.8. Day wrote two letters this day. Another one, cited earlier, told Bridgman about the salary and the other conditions. Day wrote this present letter on the same day, immediately after he received Bridgman's declining reply.

[156] Bridgman to Day, Dec. 8, 1916, PWBP, HUG 4234.8.

desperate. Day's offer was not the only threat. Bridgman had received a similar offer from the University of Michigan the year before. Furthermore, the National Academy of Science, which had been providing a research fund for Bridgman, refused to do so in 1916. Lyman was afraid that Bridgman might be deliberating to move to an institution that offered him conditions with which he could continue high-pressure experiment. Lyman wrote the following letter to ask the President for research money for Bridgman:

> For a good many years past Dr. Bridgman has been provided with an assistant the funds being raised partly by me and coming partly from a fund from the National Academy. It now seems to me that the time has arrived when, in view of the extreme importance of Professor Bridgman's work, the University should contribute regularly to this object. The National Academy having this idea in view and being much pressed by other claimants, has refused its grant this year.
>
> We must remember that Bridgman is now regarded as one of the leading physicists in this country being far and away the best man of his age. He is constantly importuned by other institutions, I have already reported to you the matter of the University of Michigan and now one of the Bureaus in Washington is after him.
>
> Under the circumstances, it is our policy to do everything we can to make it comfortable for him here. The machinist costs $1100 a year. If the Corporation thinks this excessive, a grant of $500, which was the sum formerly received from the Academy, will help us a great deal.[157]

Lyman could secure the approval from the Harvard Corporation narrowly in time for Bridgman's return from Washington, D. C. and succeeded in keeping him at Harvard.

Another noteworthy offer came from Princeton University. In 1930, Princeton offered Bridgman a position of Karl T. Compton, who had just accepted to become President of MIT. By then, Bridgman had learnt how to take advantage of offers from outside to secure a more

[157] Lyman to Lowell, Nov. 18, 1916, PLDC, UA V 692.5.

favorable position at his own institute. Bridgman wrote about his attempt to his old friend Robert "Bobby" Chandler.

> Perhaps you have noticed in the papers that Compton has been appointed the new president of M.I.T. This is Karl Compton, from Princeton, not his better known but no more able brother, Arthur, of Chicago. [...] This appointment has reacted on me in two ways. In the first place, the Princeton people offered me Compton's job, which is technically that of Research Professor, with no regular teaching duties whatever. This was a thing that I had long wanted, although I had not the slightest desire to go to Princeton. I did what every other mucker does in the same position, and used the offer to get [what] I wanted here, namely an explicit statement from the authorities that henceforth my duties here would be research, with the understanding that I need do no teaching unles [*sic*] I damned pleased, and also to get a pleasant little increase of salary.[158]

His reputation outside helped him obtain a better position at Harvard. Clearly, the Harvard Physics Department was anxious to keep him. Throughout the interwar years, Bridgman remained to be one of a few physicists at Harvard who could attract attentions from outside and sustained its prestige.

1.3.6. Small Science

Except for Bridgman's high-pressure research and E. V. Appleton's work, which included the discovery of the Appleton layer as well as other investigations of the upper layer atmosphere, all the works for which the Nobel Prizes in Physics were awarded in the late 1930s and the 1940s were related to quantum physics. The old-fashioned style and small scale of Bridgman's research is noteworthy in its contrast with the dominant tendency of physics in the United States at that time, that of "Big Science," represented by the development of the cyclotron by E. O. Lawrence, the winner of the prize for 1939. In 1952, one of

Bridgman's graduate students, Slater described his former thesis advisor's independent way:

> In the first place, in these days of great cooperative research projects, he has remained an old-fashioned physicist [...]. He has never built up a great group of technicians and collaborators; even now his laboratory has much the same quiet, intimate atmosphere that it had 30 years ago, and it is still true that when you visit him he will show off the results of something he tried out a few days before. It is still possible to do good physics with relatively simple equipment; for, in spite of the great pressures he reaches, the high pressure apparatus is simplicity itself compared to some of the equipment of modern nuclear physics. It is even still possible to get Nobel prizes for this kind of work, and in fields other than nuclear physics.[159]

A small amount of money, a simple apparatus, over-all control of the laboratory, old-fashioned research topics, and hard manual work characterized Bridgman's research, which he enjoyed and was proud of.

Only on a few occasions, Bridgman expressed his feelings toward his own work or his experiments in general. One of such rare cases was his speech given at a dinner at the Harvard Club of Boston on January 11, 1947, to which he was invited in recognition of the recent award of his Nobel Prize.[160] In this speech, he pointed out some of the most important conditions of his research. The first was freedom of investigation, for which he had politically fought since the late 1930s (see Chapter 7). The second was "the smallness of its scale"[161] that enabled him to maintain the closest contact with the details of the work. Because of this smallness, he could also conserve the requisite amount of leisure to develop new methods and ideas by trying them with his own hands. Then, he compared his relatively unpopular field with

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[158] Bridgman to Bobby (Robert Chandler), Aug. 31, 1930, PWBP, HUG 4234.8.
[159] J. C. Slater, "Presentation of Bingham Medal to P. W. Bridgman," pp. 200-201.
[160] P. W. Bridgman, "Science and Freedom: Reflection of a Physicist," in *Reflection of a Physicist* (New York: Philosophical Library, 1955), pp. 431-440; originally in *Isis*, 37 (1947), pp. 128-131.

such large-scale competitive fields as nuclear physics, in which most physicists had to spend their time on "the purely engineering job," as "the slave of one of these instruments [such as the cyclotron] [...] driven by some one at the head who has the ideas,"[162] without enough time for reflection or rumination on the significance of their projects. In Bridgman's observation, even the physicist directing the team was likely to be overwhelmed with the administrative work of the large enterprise. His concluding remark was pessimistic:

> As I look to the future I am therefore troubled by two misgivings: that there will be less and less place for the small individual experimenter, and that the time of all of us will be increasingly commandeered by administrative mechanical details. In view of these misgivings I cannot help wondering as I look back on the past whether, if I were to start over again now, I could or would be able to do again what I have done.[163]

Bridgman, while admitting that it was inevitable and necessary, could not help being critical to the trend of "Big Science," advocating his old-fashioned way of doing physics.

1.3.7. Experiment, a Repeatable and Recognizable Activity

Bridgman many times publicly stated that his operational philosophy of science did not arise out of his experimental research; he recollected that it had come out of preparatory work for lectures on electrodynamics he started to deliver in 1914 and his essays on dimensional analysis.[164] Correspondingly, no historical study has hitherto attempted to find a path from Bridgman's experimental activity

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[161] Bridgman, *Reflection of a Physicist*, p. 432.
[162] *Ibid.*, p. 436.
[163] *Ibid.*, p. 440.
[164] For example, "Operational Analysis and the Nature of Some Physical Concepts," *Nature*, 166 (1950), pp. 91-93, p. 93.

to his operational perspective on physics. However, a careful examination of his essays would suggest some connection between his operationalism and his experimental research. When urged to present a definition of the term "operation," Bridgman occasionally hinted that "one must work in a laboratory in order to understand fully what operational analysis is." [165] Probably one can clarify the relation between his experimental research and philosophical reflection by examining Bridgman's definition of practice.

In 1928, reviewing Hugo Dingler's *Experiment,* [166] Bridgman summarized the book's main thesis thus: "the essence of experiment consists in discovering in experience certain recurring combinations or 'forms,' which are recognizable and which we can reproduce."[167] As there is no similar sentence in Dingler's book that corresponds exactly to Bridgman's summary, this expression to a large extent reflected the reviewer's own interpretation of the book. Although in this book review Bridgman did not clearly write whether or not he agreed with Dingler's definition of experiment, he would later repeat similar expressions when he had to define experiment or operation, without referring to its origin. Apparently, by reviewing Dingler's *Experiment,* Bridgman found an expression of the concept of experiment that appeared appropriate to him.

In 1932, for instance, in a letter to Bentley, Bridgman described what his daily activity was like, briefly hinting at the possible connection between his experimental research and his reflection on science.

> Your chapter on semantic analysis made me wonder a little how

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[165] Bridgman, "Operational Analysis and the Nature of Some Physical Concepts," p. 92.
[166] Hugo Dingler, *Das Experiment, sein Wesen und seine Geschichte* (Munchen: Ernst Reinhardt, 1928).
[167] P. W. Bridgman, review of *Das Experiment, Sein Wesen und Seine Geschichte, Physical Review,* 32 (1928), pp. 316-317.

> one would go to work to analyze the cerebrations of an experimentalist like myself in groping for the solution of some every-day problem in the laboratory. A good deal of this cerebration, I find by analysis of my own activity, is apparently divorced from any verbal element, but is almost entirely motor and visual in its character, with reactions when confronting any specific situation almost as definite and clean cut as the words of a conscious language. I[t] is, of course, impossible to communicate such reactions without language, [...] but I have a feeling that in some cases the gropings of our language might be better understood if some way could be devised for taking account of these motor reactions which often rise to full consciousness, and which seem to involve recognizable and repeatable elements.[168]

To Bridgman, experiment consisted of motor-visual, recognizable, repeatable activities. He required verbal communication to have the same clearness and definiteness, thus introducing operational analysis into philosophical scrutiny of science.

In 1953, Bridgman tried to define the concept of operation in a private letter. Admitting that he had never given a formal definition of operation, he showed one example of his own: "Operation is to be understood in the sense of any consciously directed and repeatable activity."[169] In *The Way Things Are*, published in 1959, he emphasized repeatability as a universal feature of experimenting: "we would not be interested in finding that in a particular experiment water freezes at 75 ℃ under 20,000 atmospheres unless it always freezes under these conditions." Bridgman used the terms experiment and operation interchangeably and gave almost the same definition to both. To him the ideal example of operation was his daily experiments at his laboratory that always led him to the same results once he had established the procedures directed for the specific goals.

Publishing one's results of experimental research means showing other scientists the experimental procedures with which they can reach

[168] Bridgman to Bentley, Nov. 28, 1932, PWBP, HUG 4234.10.

the same results that are presently published. Bridgman established high-pressure experimentation and showed other fellow physicists the means to reach the same directed results as his. For this purpose he elaborated on how to construct the high-pressure apparatus, how to measure high pressures and other physical quantities under high pressures, and how to prepare samples for measurements under high pressures.

As an experimentalist, Bridgman took it for granted that the same procedures would always lead to the same results, as far as one exactly followed the directed manual. At the laboratory, the same procedures should or are made to correspond uniquely to the same results. He was so ambitious as to believe that by defining scientific concepts with their corresponding operations he could eliminate the ambiguity of concepts and thereby introduce clarity and specificity into verbal discussion. In Chapter 4 and Chapter 5, I will discuss how he did this and whether he succeeded or not.

[169] Bridgman to Hart, May 27, 1953, PWBP, HUG 4234.10.

Chapter 2. The Establishment of Theoretical Physics at Harvard

Before the 1920s, American physicists' interest in research and teaching was almost exclusively experimental, in contrast with the new developments in theoretical physics in Europe, such as Max Planck's quantum hypothesis (1900) and Albert Einstein's relativity theory (1905 and 1915-1916). Historians have made several attempts to find plausible accounts for this "Baconian" tendency of American physicists. Some have emphasized Americans' preference for the practicality of experiment as opposed to abstract theory, suggesting possible connections with technological application.[1] They have also pointed out that American physicists before World War I widely assumed that the completion of electromagnetic theory by Maxwell had left nothing to study in theoretical physics.[2] Others have stressed the cultural factors of American scientists that directed them to experimental activities:[3] many of them considered the laboratory as a place to mold character through manual work which supposedly taught them such values as honesty, diligence, and perseverance.

It may be, however, not appropriate to exaggerate too much the idea of the experimental tradition of American science, since, by the end of 1920s, young physicists had started to create several centers of theoretical research, transforming the style of physics in the United

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[1] Richard Harrison Schryock, "American Indifference to Basic Science during the Nineteenth Century," *Archives Internationales d'Histoire des Sciences*, 28 (1948-1949), pp. 3-18. I. Bernard Cohen, "Some Reflections on the State of Science in America during the Nineteenth Century," *Proceeding of the National Academy of Sciences*, 45 (1959), pp. 666-677.
[2] Katherine Russell Sopka, *Quantum Physics in America: 1920-1935* (New York: Arno Press, 1980), p. 1.35.
[3] Robert H. Kargon, *The Rise of Robert Millikan: Portrait of a Life in American Science* (Ithaca, N. Y.: Cornell University Press, 1982), pp. 31-44. Larry Owens, "Pure and Sound Government: Laboratories, Playing Fields, and Gymnasia in the Late Nineteenth-Century Search for Order," *Isis*, 76 (1985), pp. 182-194.

States:[4] E. C. Kemble and J. C. Slater at Harvard, J. R. Oppenheimer at Berkeley, and J. H. Van Vleck at Wisconsin, to list but a few. Because physics, since its very beginning, has been an experimental science both in Europe and in America, the rise of theoretical physics seems to require more attention than the experimental tradition in America. Theoretical physics emerged as a local phenomenon in the German-speaking universities in the late nineteenth century. Even at the turn of the century, not many universities had professorships in this field. "Perhaps half a dozen in all of Europe,"[5] the historian of physics S. S. Schweber has estimated. The number was even smaller in the Unites States. When Bridgman started to examine the nature of physical theory in the mid-1910s, entirely theoretical research was still a novel phenomenon to many of American physicists.

American physicists changed their attitude toward theoretical research in the 1920s:[6] right after World War I, the leading physicists, most of them experimentalists at Harvard, Caltech, Princeton, Michigan, and Chicago, started a program to establish theoretical research, with financial support from the Guggenheim, Carnegie, and Rockefeller Foundations. This support enabled these universities to take necessary actions to expand their physics departments. The fellowship programs supported by these foundations were also indispensable for promoting theoretical programs, since they made it financially possible for young American physicists to study in Europe.[7] Finally, the influx

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[4] Sopka, *Quantum Physics in America*, p. 4. 71.
[5] S. S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States 1920-1950," *Historical Studies in the Physical Sciences*, 17 (1986), pp. 55-98, p. 69.
[6] Sopka, *Quantum Physics in America, 1920-1935.* Schweber, "The Empiricist Temper Regnant." Stanley Coben, "The Scientific Establishment and the Transmission of Quantum Mechanics to the United States 1919-1932," *American Historical Review*, 76 (1971), pp. 442-466.
[7] Alexi Assmus, "The Creation of Postdoctoral Fellowships and the Siting of American Scientific Research," *Minerva*, 31 (1993), pp. 151-183. Robert E. Kohler, *Partners in*

of European refugees resonated with American physicists' efforts to establish theoretical physics and enriched the faculties at departments of physics.

The transformation in the 1920s was a significant leap toward the maturation of American physics in general. As Forman, Heilbron and Weart have found,[8] at the turn of the century, the United States already had the largest group of academic physicists (some 215) among the countries in Western Europe and North America. They also enjoyed a higher average personal income than their European counterparts and experienced a larger rate of increase in the number of academic posts, personal income, and expenditures for laboratories between 1900 and 1910. However, their average productivity was smaller than that of European physicists. The rise of American physics during 1920s improved this situation.

During the 1920s, along with the establishment of theoretical physics and the growth of the physics community, the leading physics departments in the United States expanded.[9] The phrase of the president of the Rockefeller Foundation's International Education Board, "to make the peaks higher," clearly illustrated their policy to support only the elite universities.[10] Historians, too, have paid attention mainly to the success of these elite physics departments, leaving each

Science: Foundations and Natural Scientists 1900-1945 (Chicago and London: The University of Chicago Press, 1991). Roger L. Geiger, *To Advance Knowledge: the Growth of American Research Universities, 1900-1940* (New York and Oxford: Oxford University Press, 1986), pp. 160-173 and pp. 233-245.

[8] Paul Forman, J. L. Heilbron, and Spencer R. Weart, "Physics *circa* 1900: Personnel, Funding and Productivity of the Academic Establishments," *Historical Studies in the Physical Sciences*, 5 (1975), pp. 1-185.

[9] Spencer R. Weart, "The Physics Business in America, 1919-1940: A Statistical Reconnaissance," in Nathan Reingold, ed., *The Sciences in the American Context: New Perspectives* (Washington, D. C.: Smithsonian Institution Press, 1979), pp. 295-358.

[10] Coben, "The Scientific Establishment and the Transmission of Quantum Mechanics to the United States 1919-1932" and "Foundation Officials and Fellowships: Innovation in the Patronage of Science," *Minerva*, 14 (1976), pp. 225-240.

department's specific situation relatively unexplored.

Historians of physics in America have depicted the experiences of the first generation of American theoreticians—E. C. Kemble,[11] J. C. Slater,[12] J. H. Van Vleck,[13] and the group of molecular quantum physicists.[14] Their studies have revealed the details of these physicists' early scientific training, their experiences in Europe, and their effort to establish an American school of theoreticians. Some of them have pointed out the "Americanization"[15] of physics.

However, few historians have sufficiently analyzed older physicists' attitudes toward the rise of theoretical physics. Many historians of physics seem to have regarded that physicists outside Europe in the interwar period had no other choice than to accept the achievements of relativity theory and quantum physics and strive to establish theoretical research in their own countries. It has been pointed out that it is the old experimentalists' program that led and supported the birth of a new specialty in the United States. Yet, not much has been known concerning their motivations and strategies for beginning theoretical courses and producing theoretical physicists. Thus, several questions have remained unanswered: Why and how did experimental physicists

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11 Gerald Holton, "On the Hesitant Rise of Quantum Physics Research in the United States," in Stanley Goldberg and Roger H. Stuewer eds., *The Michelson Era in American Science 1870-1930* (New York: American Institute of Physics, 1988), pp. 177-205.

12 S. S. Schweber, "The Young John Clarke Slater and the Development of Quantum Chemistry," *Historical Studies in the Physical and Biological Sciences*, 20 (1990), pp. 339-406.

13 Frederick Hugh Fellows, *J. H. Van Vleck: the Early Life and Work of a Mathematical Physicist* (Ph. D. dissertation submitted to the University of Minnesota, 1985).

14 Alexi Josephine Assmus, *Molecular Structure and the Genesis of American Quantum Physics Community, 1916-1926* (Ph. D. dissertation submitted to Harvard University, 1990), and "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences*, 23 (1992), pp. 1-34.

15 Assmus, "The Americanization of Molecular Physics" and Schweber, "The Empiricist Temper Regnant." For a similar attempt in the history of quantum chemistry, see, Kostas Gavroglu and Ana Simoes, "The Americans, the Germans, and the Beginnings of Quantum Chemistry: The Confluence of Diverging Traditions," *Historical Studies in the Physical and Biological Sciences*, 25 (1994), pp. 47-110.

need to raise theoretical physicists? How could they establish a new specialty in their department? And what was their attitude in general toward the new type of physics?

In this chapter, in order to examine one of the aspects of Bridgman's reaction to the rise of theoretical physics, I will analyze his commitment to the institutional efforts for establishing theoretical research at Harvard. One can interpret his epistemological reflections on science to be a product of an experimentalist's struggle to seek the legitimacy of their activity at the face of theoretical dominancy. Analysis of Bridgman's program of promoting theoretical physics and raising theoreticians will show his disciplinary concern with theoretical matters and will clarify how and why some experimentalists tried to nurture theoretical research, while others persistently remained uncooperative. As will be shown, Bridgman was never hostile to the rise of theoretical research, though he sometimes mercilessly criticized contemporary physical theories. He even hoped to contribute to theoretical research through his philosophical reflections.

Bridgman's efforts at Harvard deserve detailed historical study, since he took the lead in Harvard's decision to open the first theoretical course in the United States with a full-time faculty member. Furthermore, three of the first American theoretical physicists, E. C. Kemble, J. C. Slater, and J. H. Van Vleck, trained at Harvard and attended several of Bridgman's courses. Two of them, Kemble and Slater, earned their Ph. D.'s under Bridgman's auspices. These Harvard theoreticians stimulated the later development of theoretical research in America by producing a large portion of the next generation. Quoting Sopka's work, Holton has estimated that "[b]etween 1922 and 1935, the twenty-six dissertations by Kemble's students, and by *their* students, represented about one-third of all theoretical physics dissertations completed during that period in all United States institutions."[16] By examining Bridgman's role in the establishment of theoretical physics at Harvard, one can estimate his position at the Physics Department and Harvard's position in the new trend of American physics.

In the following, I will examine the policy of the Harvard Physics Department to inaugurate theoretical courses and raise theoreticians, focusing mainly on Bridgman's teaching, research interest, and administrative effort. I will also discuss the training and careers of the first generation of theoreticians who worked closely to Bridgman at Harvard: Kemble, Slater, Van Vleck, and J. R. Oppenheimer.

2.1. Courses on Relativity and Quantum Physics at the Harvard Physics Department

2.1.1. Bridgman and Pierce on Relativity and Quantum Theory

Joining the staff of the Harvard Physics Department in 1908, Bridgman served as an instructor, partially in charge of an experimental course for undergraduate students, and gave one full summer course (five times a week for six weeks).[17] In 1912, he also started to give a half course for graduate students on elasticity,[18] which required some

[16] Holton, "The Hesitant Rise of Quantum Physics Research," p. 192.
[17] *Harvard University Catalogue: 1908-09* (Cambridge, Mass.: Harvard University, 1909), p. 401 and p. 443.
[18] *Harvard University Catalogue: 1912-13* (Cambridge, Mass.: Harvard University, 1912), p. 371.

mathematical preparation of students. His experimental course was on mechanics, sound, light, magnetism, and electricity. Since the summer courses in physics at Harvard were usually on elementary subjects, Bridgman's early classes were mainly on classical physics. Until around 1915, the other instructors' courses had not changed noticeably since Bridgman's college years (1900-1904). One finds, for instance, little mention of relativity theory in the description of the courses during this period. B. O. Peirce had been giving a course on "Applications of Vector Analysis to Problems in Electro-Magnetism. As illustrated in the works of Heaviside and Lorentz,"[19] which, judging from the title, probably discussed electrodynamics and may have introduced Lorentz's electron theory, another and earlier formulation of the special theory of relativity, grounded on an epistemological foundation different from Einstein's. The course on radiation by G. W. Pierce had not started to introduce Planck's quantum hypothesis. In 1909, Theodore Lyman started a course on "Radioactivity and Electric Conduction in Gases with special reference to the Modern Theories of the Constitution of Matter"[20] for both undergraduates and graduates. However, judging from its prerequisite, his course does not seem to have dealt with the new development in atomic physics. A half course for graduate students on "Modern Developments and Applications of the Electron Theory," starting in 1910,[21] which Bridgman would later take over, perhaps included some discussion on Lorentz's electron theory, but not its reformulation by Einstein. Some courses on mathematical physics given at the Mathematics Department were concerned with the

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[19] *Harvard University Catalogue: 1911-12* (Cambridge, Mass.: Harvard University, 1911), p. 372.
[20] *Harvard University Catalogue: 1909-10* (Cambridge, Mass.: Harvard University, 1910), p. 411.
[21] *Harvard University Catalogue: 1910-11* (Cambridge, Mass.: Harvard University, 1910), p. 383.

application of mathematics to classical physics, such as mechanics of the rigid body or electromagnetism.

In December 1913, the unexpected death of B. O. Peirce caused a change in the courses given at the Physics Department. Considered to have had the best mathematical skills among the staff members, Peirce had been giving the advanced courses on mechanics and electromagnetism, some at the Mathematics Department. On his death, it suddenly became necessary for the Physics Department to find instructors to take over his courses in mathematical physics. One choice was to invite another mathematical physicist, Max Mason, who was then a physics professor at the University of Wisconsin, for a joint professorship with the Mathematics Department. Mason, later to be known as the president of the Rockefeller Foundation, was born in 1877 and was among the few American mathematical physicists of his generation. He gave a course at the Harvard Mathematics Department in 1911-12.[22] Yet, his main interest was in classical physics.[23] The Physics Department did not favor this choice. Lyman, Director of the Jefferson Physical Laboratory, wrote to President Lowell:

> The Division of Mathematics is extremely anxious that the Division of Physics should join with them in recommending to the Corporation that Professor Max Mason be called here for a joint professorship. We gave the matter long and careful consideration and came to the conclusion that we did not wish to join with the mathematicians in this matter.
>
> It is the intention of this Division to attempt to take care of the work of the late Professor B. O. Peirce by means of the men already on the ground. To this end, Dr. Chaffee has been given Physics 3 and Professor P. W. Bridgman will give Physics 9 next

[22] *Harvard University Catalogue: 1911-12* (Cambridge, Mass.: Harvard University, 1911), p. 439.
[23] Warren Weaver, "Max Mason, October 26, 1877—March 22, 1961," *National Academy of Science of the United States of America, Biographical Memoirs*, 32 (1961), pp. 204-236.

> year and will also conduct a seminar course on the electron theory. Meanwhile, we are sending Dr. David Webster abroad on a Sheldon Fellowship to study mathematical Physics with the idea of introducing him into the Division on his return as an instructor. Of course no promises of any description have been made to Dr. Webster. I mention the matter now so that you may be aware of the plans of our Division which, of course, are always subject to the approval of the Corporation.[24]

Instead of calling Mason for a joint professorship with the Division of Mathematics, the Physics Department decided to assign Peirce's courses to its younger staff members, Chaffee, Bridgman, and Webster. Although Webster's plan to study in Europe seems to have been interrupted by World War I and his war work, after coming back to Harvard, he served as an instructor until he left for Stanford in 1917.[25] Chaffee's Physics 3 and Bridgman's Physics 9 were the introductory and advanced courses on electromagnetism.

Bridgman took over and up-dated his new mathematical courses, which turned out to be the first to introduce new developments in theoretical physics. In 1914, he started "Seminar on the Electron Theory" and "The Mathematical Theory of Electricity and Magnetism."[26] The next year, Bridgman renamed the former "The Electron Theory and Relativity."[27] About four decades later, when invited to talk in the symposium on operationalism, Bridgman recounted how this course stimulated his operational reflection: "[P]reparation for this [his operational method] in my own thinking went back at least to 1914, when the task of giving two advanced courses in electrodynamics was suddenly thrust upon me. Included in these courses was material

[24] Lyman to Lowell, April 4, 1914, PLDC, UA V 692.5.
[25] Sopka, *Quantum Physics in America,* p. A. 10.
[26] *Harvard University Catalogue: 1914-15* (Cambridge, Mass.: Harvard University, 1915), p. 379.
[27] *Harvard University Catalogue: 1915-16* (Cambridge, Mass.: Harvard University, 1916), p. 417.

from the restricted theory of relativity."[28] Though having studied the special theory of relativity before starting these courses, Bridgman did not have a chance to receive any intensive instruction on this theory. He prepared the material for his classes mostly through reading several monographs. As the historian A. Moyer has found, nine month after Peirce's death, Bridgman ordered five books from a bookseller in the Netherlands in preparation for the new courses: *Das relativitätsprinzip*,[29] a collection of papers by H. A. Lorentz, Einstein, and Hermann Minkowski; *Neue Probleme der theoretischen Physik*,[30] the lectures given by Wilhelm Wien at Columbia University in 1913; *The Theory of Electrons*[31] by H. Lorentz; the second edition of *Einführung in die Maxwell'sche Theorie der Elektricität*[32] by August Föppl as updated by M. Abraham; and *Die Theorie der Strahlung und der Quanten*[33] by Arnold Eucken.[34] Apparently Bridgman was sensitive to the new developments in physics and was enthusiastic over introducing the fresh materials into his courses.

As for atomic physics, Lyman launched in 1914 an advanced experimental course on radioactivity that included considerable laboratory work (eight hours a week).[35] More impressively, in 1915, G.

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[28] P. W. Bridgman, "The Present State of Operationalism," in Philipp Frank, ed., *Validation of Scientific Theories* (New York: Collier Books, 1961), p. 76.
[29] H. A. Lorentz, Hermann Minkowski und Albert Einstein, *Das relativitätsprinzip, eine sammlung von abhandlungen, mit anmerkungen von A. Sommerfeld und vorwort von G. Blumenthal* (Leipzig: B. G. Teubner, 1913).
[30] Wilhelm Wien, *Neue Probleme der theoretischen Physik* (Leipzig: B. G. Teubner, 1913).
[31] H. A. Lorentz, *The Theory of Electrons and Its Applications to the Phenomena of Light and Radiant Heat* (Leipzig: B. G. Teubner, 1909).
[32] August Föppl, *Einführung in die Maxwell'sche Theorie der Elektricität* (Leipzig, Berlin: B. G. Teubner, 1904).
[33] Arnold Eucken, *Die Theorie der Strahlung und der Quanten* (Halle: Wilhelm Knapp, 1914).
[34] A. Moyer, "P. W. Bridgman's Operational Perspective on Physics, Part I: Origins and Development," *Studies in History and Philosophy of Science*, 22 (1991), p. 250.
[35] *Harvard University Catalogue: 1914-15* (Cambridge, Mass.: Harvard University, 1915), p. 378.

W. Pierce changed the title of his course on radiation into "Radiation and the Quantum Theory"; this course included "Planck's Theory of Quanta, and Debye's derivation of Planck's Law" and used the translation of Planck's *Wärmestrahlung*[36] as its textbook. Pierce may have been the first instructor to introduce the quantum hypothesis to Harvard; at least his course was the first to bear the word "quantum" in its title. Following Pierce, Webster, who was back from war work, gave a course on his theoretical specialty, "X-ray phenomena" in 1916-17.[37] This course, however, did not appear in the catalogue until E. C. Kemble resumed it in 1919.[38]

Max Mason would not have been interested in introducing relativity theory and the quantum hypothesis into new courses. Though he was a theoretical physicist with better mathematical knowledge than his contemporary physicists, he was critical toward these new theories. Warren Weaver, who was Mason's student and colleague, wrote that Mason "actively disliked [quantum theory], and considered that it was so unpleasantly messy, so full of internal contradictions, and so clearly headed in a wrong direction, that he would have little or nothing to do with it."[39] He tried to teach a course in quantum theory once in 1914-15, "but this one trial quite clearly

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[36] *Official Register of the Division of Physical Sciences, Department of Physics with the courses in Astronomy, 1915-16* (Cambridge, Mass.: Harvard University, 1915), pp. 17f. The German original book was Max Planck, Wärmestrahlung (Leipzig: J. A. Barth, 1913). Morton Masius's English translation was *The Theory of Heat Radiation* (Philadelphia: P. Blakiston's Son and Company, 1914). The translator Masius, born in 1883, earned his doctorate in physical chemistry from the University of Leipzig in 1908, served as a fellow at Harvard for one year, and became a faculty member at Worcester Polytechnic Institute in 1909.

[37] *Announcement of the Courses of Instruction Offered by the Faculty of Arts and Sciences for the Academic Year 1916-17* (Cambridge, Mass.: Harvard University, 1916), p. 58.

[38] *Harvard University Catalogue: 1919-20* (Cambridge, Mass.: Harvard University, 1920), p. 385.

[39] Weaver, "Max Mason," p. 219.

finished him off."[40]

Clearly, experimentalists were more enthusiastic over introducing quantum theory and relativity than theoreticians. Though by then having established himself as a high-pressure experimentalist, Bridgman gave the courses on relativity and electromagnetism, subjects that had little to do with his research. G. W. Pierce, who had been giving the course on radiation and quantum theory, was actually an electronic experimentalist and engineer, best-known for his invention of oscillators. He was neither active in the field close to quantum physics nor seemingly young enough to take fresh interest in the new theoretical development. Born in 1872, he was then over forty years old.[41] At Harvard, these two middle-aged experimentalists started the courses on relativity and atomic physics. Apparently, it was not their research interest, but their mathematical skills, their interest in theoretical physics, and their sensitivity to new developments, that motivated them to start the new courses.

In fact, by World War I, the need to introduce new theoretical topics had become obvious to Bridgman. In 1908, the MIT physical chemist Gilbert N. Lewis, stimulated by Einstein's theory, published his attempt to revise the fundamental concepts of matter and energy.[42] The next year, Lewis further formulated a unique American contribution to relativity theory, "non-Newtonian Mechanics,"[43] with his graduate student Richard C. Tolman. The Harvard Physics Department asked Lewis to give lectures on relativity in 1910 in the form of colloquia.[44]

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[40] *Ibid.*
[41] E. L. Chaffee, E. C. Kemble, H. R. Mimno, and F. V. Hunt, "George Washington Pierce," Jan. 8, 1957, ECKP, HUG (FP), 72.10.
[42] G. N. Lewis, "A Revision of the Fundamental Laws of Matter and Energy," *Philosophical Magazine*, 16 (1908), pp. 707-717.
[43] G. N. Lewis and R. C. Tolman, "The Principle of Relativity and non Newtonian Mechanics," *Philosophical Magazine*, 18 (1909), pp. 510-523.
[44] Lyman to Lowell, Dec. 12, 1910, PLDC, UA V 692.5.

Bridgman was fortunate that two close friends of his, Lewis and Tolman, served as precious sources of information of the novel development in electromagnetism. Before Arthur Eddington's 1919 solar-eclipse observations, which eventually turned out to be an empirical confirmation of the prediction of the general theory of relativity, American physicists conducted little research connected with relativity theory. When Bridgman started to teach relativity in 1914, only one English monograph, written by a mathematician, was available on this subject.[45]

To quantum theory, American physicists started to pay serious, though unfavorable, attention in 1913, the year when Bohr announced his quantum theory of atomic structure. At the joint meeting of the American Physical Society and the American Association for the Advancement of Science held on December 31, 1912 at the Case School of Applied Science in Cleveland, Ohio, quantum theory was publicly reviewed for the first time before American physicists: R. A. Millikan, the retiring Vice-President and Chairman of Section B of the Association, gave an address titled "Atomic Theories of Radiation," in which he described five different quantum theories developed by Planck, Einstein, Thomson, and Bragg.[46] Wilhelm Wien, who was at Columbia University as Foreign Lecturer in the spring of 1913, presented more detailed and more mathematical discussions on quantum theory.[47] Furthermore, the following November, a symposium on quantum theory was held during the regular meeting of the American Physical Society at the Ryerson Physical Laboratory, in which five American physicists delivered talks on quantum theory.[48] Thus, as K. R. Sopka has revealed, "by about 1915, practically all the universities with graduate physics programs had either

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[45] Robert D. Carmichael, *The Theory of Relativity* (New York: John Wiley & Sons, Inc., 1913). Carmichael was Professor of Mathematics at the University of Illinois.
[46] R. A. Millikan, "Atomic Theories of Radiation," *Science*, 37 (1913), pp. 119-133.
[47] Sopka, *Quantum Physics in America*, p. 1. 47.
[48] *Ibid.*, p. 1.48.

recently introduced or expanded their discussion of [quantum theory] for their advanced students."[49]

The students' comments on the experimentalists' courses on theoretical materials were favorable. John C. Slater, who entered Harvard as a graduate student in 1920 and received a doctorate three years later, recounted Bridgman's courses on relativity thus: "It was very good, thorough training in special relativity and not very much general; but relativity theory of electromagnetic transformations and all that stuff, as well as the mechanics."[50] Bridgman's course apparently included at least an introductory part of the general theory of relativity. J. R. Oppenheimer, who as a chemistry undergraduate student took two of Bridgman's courses around 1924, one on thermodynamics and statistical mechanics, the other on electromagnetic theory, also praised Bridgman as a physics teacher: "I found Bridgman a wonderful teacher because he never really was quite reconciled to things being the way they were and he always thought them out; his exercises were a very good way to learn where the bones were in these two beautiful parts of physics."[51]

Bridgman later characterized himself as "not human enough to enjoy directing other humans."[52] In fact, when he had a chance, he asked the Department for a research professorship free from the teaching load, though he continued to teach some of his favorite topics. To those who attended his classes, "they were an unforgettable experience,"[53] but

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[49] *Ibid.*, p. 1.57.
[50] Interview with John Clarke Slater conducted by T. S. Kuhn and J. H. Van Vleck on October 3, 1963, AHQP.
[51] Interview with J. R. Oppenheimer conducted by T. S. Kuhn on November 18, 1963, AHQP.
[52] Robert E. Chandler, "A Deep and Rich Friendship," in "Expressions of Appreciation," PWBP, HUG 4234.25.
[53] Edwin C. Kemble and Francis Birch, "Percy Williams Bridgman, April 21, 1882—August 20, 1961," *National Academy of Sciences of the United States, Biographical Memoirs,* 41 (1970), pp. 22-67, p. 22.

"his classes were small"[54] as E. C. Kemble and F. Birch remembered. J. H. Van Vleck expressed his opinion toward Bridgman's courses in a more reserved way: "I think Bridgman was a very fine teacher for the better students."[55] Whether the students liked them or not, Bridgman's courses played a significant role in showing the mathematical treatment of classical physics and introducing the special theory of relativity to Harvard. Even after younger theoretical physicists started to teach, Bridgman continued to deliver his course on relativity and electromagnetism in alternate years until 1932.

Pierce's course was also important as the only course at Harvard that discussed Planck's hypothesis. Kemble, who attended his course during 1914-15, recalled: "Pierce, a man of the older generation, gave a good course that did justice to Planck's work but, in view of the inconsistency between it and Maxwell's theory, continued for years to search for a purely classical explanation of the phenomena on which quantum theory was based."[56] Pierce discussed Planck's theory in his course even before he changed its title from "Radiation" to "Radiation and the Quantum" in 1915. Like his colleagues in and outside Harvard, Pierce did not entirely trust quantum theory.[57] However, among the Harvard physicists, only Pierce had sufficient mathematical skills to teach the material. Except for Webster's course given during the second half of the academic year 1916-17, Pierce's course remained to be the only course on quantum theory until 1919, the year when Kemble started teaching. Even after that, Pierce continued to give this course until 1926. His courses played the same role in introducing quantum theory to Harvard as Bridgman's course played in the case of relativity theory.

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[54] *Ibid.*

[55] Interview with John H. Van Vleck, conducted by T. S. Kuhn, on October 2, 1963, AHQP.

[56] Kemble to Sopka, Fall, 1972, cited in Sopka, *Quantum Physics in America*, p. 1.56.

[57] Sopka, *Quantum Physics in America*, p. 1.58.

2.1.2. Bridgman's Attempts in Theoretical Research

Bridgman was not satisfied with only *teaching* theoretical materials. On November 21, 1919, six months after Arthur Eddington's solar-eclipse observations, Bridgman reported on the "Temperature Effect of Gravitation" with E. B. Wilson, a professor of MIT, at the meeting of the Physics Section of the American Academy of Arts and Sciences.[58] On March 24, 27, and 28, 1922, he arranged a series of conferences on relativity titled "Relativity and Gravitation," conducted by the MIT physics professor H. B. Phillips, at the American Academy of Arts and Sciences.[59] The following December, Bridgman himself gave a talk on the philosophical aspect of the special theory of relativity in a symposium on relativistic aspects of space and time, held at the Boston AAAS meeting.[60] This turned out to be the first occasion for Bridgman to mention the word "operation," which was to become a keystone of his scientific thought, in almost the same sense as he later used it. These addresses of Bridgman were reports on new developments in physics and a methodological reflection on relativity, not to be categorized as original theoretical research. Yet, they show his interest in scrutinizing the fundamental problems presented by theoretical research.

Bridgman started his methodological reflection on physics a few years after he took over the courses on electromagnetism. He began his scrutiny of theoretical physics with an attempt to clarify the validity of dimensional analysis, or, the "principle of similitude." The result first appeared as Bridgman's paper in the 1916 volume of *Physical*

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[58] Lyman and Wilson to Holden, Nov. 13, 1919, PLDC, UA V 692.5.
[59] Lyman to the Assistant Librarian of the American Academy of Arts and Sciences, March 11, 1922, and, Lyman to Birkhoff, March 11, 1922, PLDC, UA V 692.5.
[60] Moyer, "P. W. Bridgman's Operational Perspective on Physics, Part I: Origins and Development," p. 254.

Review,[61] and then as a 112-page long monograph titled *Dimensional Analysis*[62] in 1922. These show Bridgman's keen interest in the latest trends in physics. I will later discuss his scrutiny of dimensional analysis and relativity in detail, since it played a crucial role in forming Bridgman's operational perspective. Here, I only point out that his philosophical reflections on theoretical physics, culminating in *The Logic of Modern Physics*, reflected his concern as a researcher and teacher, not as a philosophical bystander. Bridgman seriously intended to contribute to the contemporary theoretical research through his philosophical work.

Furthermore, Bridgman attempted to formulate theoretical schemes to explain some of his experimental results. European physicists noticed his theoretical activity and invited him to the Fourth Solvay Conference whose main subject was electric conduction of metal. In a letter to Lyman, Bridgman expressed his confidence in his theoretical work:[63] "Not only would it give me a chance to see the European physicists and the European physicists a chance to see me, [...] but it would give me a chance to bring my work to their attention in a way that I have often felt desirable." One of Bridgman's contributions to discussion at the conference was his suggestion of the role of thermodynamics in approaching the problem of superconductivity, which, though not attracting attention of many participants, would prove to be an appropriate argument a decade later.[64] The Fourth Solvay Conference was not very fruitful itself, but it gave him an

[61] P. W. Bridgman, "Tolman's Principle of Similitude," *Physical Review*, 8 (1916), pp. 423-431.
[62] P. W. Bridgman, *Dimensional Analysis* (New Haven: Yale University Press, 1922).
[63] Bridgman to Lyman, July 25, 1923, PWBP, HUG 4234.8.
[64] Lillian Hoddeson, Helmut Schubert, Steve J. Heims, and Gordon Baym, "Collective Phenomena," in Lillian Hoddeson, Ernest Braun, Jürgen Teichmann, and Spencer Weart, eds., *Out of the Crystal Maze: Chapters from the History of Solid-State Physics* (New York, Oxford: Oxford University Press, 1992), pp. 489-616, p. 497.

invaluable chance to become acquainted with European physicists and to know their latest results. Edwin H. Hall, Bridgman's former teacher and colleague at the Harvard Physics Department who was also invited to the conference, was delighted to find their names surrounded by those of famous Europeans: "I observe that in the list of those invited to take part your name comes next that of [Niels] Bohr and mine next to that of Einstein. We seem to be in good company."[65] The lists of the invited physicists and the committee members also included, Leon Brillouin, Peter Debye, Paul Ehrenfest, Erwin Schrödinger, J. J. Thomson, Marie Curie, Paul Langevin, H. A. Lorentz, and Ernest Rutherford. During the 1920s and 1930s, many of these physicists gave lectures at Harvard, responding to the invitation of the Physics Department.

While interviewing K. C. Kemble,[66] T. S. Kuhn, who studied at Harvard and had a personal contact with Bridgman, recalled a story illustrating Bridgman's enthusiasm over quantum theory: "I've been told the story that when the first edition of Sommerfeld [his *Atombau und Spektrallinien*] came out, he [Bridgman] was in Germany, he immediately grabbed it and locked himself in the office and studied it carefully, and wouldn't let anyone else have it until he was through." Arnold Sommerfeld's *Atombau und Spektrallinien* was a widely read textbook of quantum theory. After its first edition[67] appeared in September 1919, the revised editions were published in 1920, 1922, 1924, and so forth. The first edition of *Atombau* took note of theoretical work of Kemble whose graduate thesis Bridgman had supervised. Yet, the story Kuhn told to Kemble sounds implausible, since Kemble told

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[65] E. Hall to P. W. Bridgman, July 24, 1923, PWBP, HUG 4234.8.
[66] Interview with E. C. Kemble, conducted by T. S. Kuhn and J. H. Van Vleck on October 1 and 2, 1963, AHQP.
[67] Arnold Sommerfeld, *Atombau und Spektrallinien* (Braunschweig: Friedrich Vieweg und Sohn, 1919).

Kuhn that he had never heard of it. Still, it shows how Bridgman's attitude toward quantum theory impressed his Harvard colleagues.

Bridgman may not have read Sommerfeld's first edition in the way Kuhn remembered, but he certainly obtained sufficient information through his personal correspondence with Sommerfeld. Their main concern was exchanging their opinions on the problem of metallic conduction. Sommerfeld sent Bridgman reprints of his papers on the electron theory of metals he read at the conference held at Como, Italy in the September of 1927 in celebration of the Volta Centenary.[68] Bridgman was invited to this conference, but could only send a paper on "Electrical Properties of Metal Crystals."[69]

It seemed, however, that it was difficult for Bridgman to devote himself to theoretical research, especially in quantum physics. Although he once attempted to invent his own electron theory of metals, he gave up on it when he realized the necessity of a good command of quantum theory for further development. Bridgman had enough mathematical knowledge to understand quantum mechanics, and he supervised Kemble's doctoral thesis which was highly, though not entirely, theoretical. Yet this seems to be the limit. To teach physical theory is one thing; to use it in theoretical research is quite another. In 1919, declining the invitation to attend the research committee on atomic structure organized by the National Research Council, Bridgman expressed his desire to concentrate on high-pressure experiment:

> I am not at all sure that I ought to attempt to serve at all. I have not any special work to offer in this field, and several members of the committee preeminently have. My own field of

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[68] Bridgman to Sommerfeld, Nov. 26, 1927, PWBP, HUG 4234.8.

[69] Majorana to Bridgman, Nov. 26, 1926, and Bridgman to Majorana, Dec. 14, 1926, PWBP, HUG 4234.8. Quirino Majorana was a professor at the Institute di Fisica A. Righi in Bologna, Italy and was organizing the Volta Conference at Como held in 1927.

work is definitely indicated, and my opportunities are so nearly unique, that I think there can be no question whatever that my activities should be confined to this field.[70]

Though interested in using high pressure research as a tool to attack the problems of atomic structure, he did not believe that he "personally would be justified in spending the time that would be necessary in preparing a formal report."[71]

Atomic structure was one of a few topics in atomic physics to which American scientists could contribute. Two close friends of Bridgman's, G. N. Lewis and Irving Langmuir, known for their classical, that is, non-quantum, atomic theories, were also in the list of the committee members.[72] However, since Bohr published his quantum theory of hydrogen atom in 1913, P. Epstein, A. Sommerfeld, Peter Debye, and M. Planck had published attempts to apply quantum theory to atomic and molecular structures. But for profound knowledge of quantum theory, it would have been impossible to keep up with the development of atomic physics.

Since quantum theory appeared, experimental physicists had often found difficulty in following contemporary theoretical research. In his 1922 letter to L. Silberstein, a theoretical researcher at Eastman Kodak in Rochester, Bridgman expressed his envy for the works of professional theoreticians:

> I am sending you under separate cover reprints describing in more detail my experiments on Ohm's law, and several other matters connected with the phenomena of conductivity. I have been trying to get around to this for some time, but wanted to have a chance to look over the papers which you so kindly sent me before doing so, and have been so busy trying to clean up my

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70 Bridgman to Bumstead, Dec. 14, 1919, PWBP, HUG 4234.8.
71 *Ibid.*
72 Bumstead to Bridgman, Dec. 5, 1919, PWBP, HUG 4234.8.

> experimental work at the end of the year that I have not hitherto had a chance. Now however, because of the serious accident in our laboratory yesterday, which you may have seen in the papers, I shall not be able to experiment for a number of days, and I have taken the first opportunity to look over your papers. I think I got considerable profit out of them, particularly from the one on the aspherical nucleus of hydrogen, but the chief feeling aroused by them was one of envy for the theoretical physicist; I find it almost impossible to devote most of my time to experimental work and still keep my theoretical tools sharp, and this limitation is a source of keen regret.[73]

He had not been able to have enough time to read Silberstein's papers until an accident in the laboratory prevented him from doing experiments. However, after reading them, he realized that it was difficult for experimentalists to keep up with theoretician's works.

In the early 1920s, when Bridgman could still brush up his knowledge of current theoretical studies, he believed that "non-radiating quantum orbits"[74] would guarantee his assumption of resistanceless paths of electrons within the atom. In 1925, admitting that no adequate theory of metallic conduction was available, Bridgman hoped that development of quantum theory would show new ways to approach this problem.[75] He had made an attempt to find the effects of pressure upon electric conduction and reached a very general scheme of conduction in which the atoms played an essential part. In the pre-quantum conduction theory, the role of atoms, which get in the way of the electrons and prevent them from moving about, remained secondary and negative. However, quantum theory showed that the electrons cannot drift about without the intervention of the atoms. Bridgman suggested a possibility that the atoms, lined up in certain

[73] Bridgman to Silberstein, May 20, 1922, PWBP, HUG 4234.8.
[74] P. W. Bridgman, "The Electrical Resistance of Metals," Physical Review, 17 (1921), pp. 161-194, p. 163.
[75] P. W. Bridgman, "Certain Aspects of High-Pressure Research," *Journal of the*

ways, pass on the electrons from one to another, or a possibility that there were tracks between the intricate "mazes" of quantum orbits within the atom, along which the conduction electron may travel. To him it seemed natural to suppose that these tracks are connected with quantum conditions and with high quantum numbers. Until the middle of the 1920s, he had been able to expect that he would understand new results of theoretical study and their implications for conduction theory.

Yet, after quantum mechanics appeared in 1925-26, Bridgman could no longer anticipate being active in theoretical research. For active experimentalists like Bridgman, it took a prohibitingly long time even to learn the mathematics used in the physical theory. In 1928, explaining the topic of the conference on theoretical physics to a Harvard graduate student James H. Bartlett, Jr., Bridgman expressed his feelings toward the mathematics required to understand quantum physics:

> Professor Kemble and Slater are running the Thursday afternoon conference again this year, the subject being applications of mathematical group theory to wave mechanics, starting with several talks by Dr. Brenner of the Mathematics Department on group theory. I shall not try to take this in this year, as I found by my experience last year that one does not get much benefit from these mathematical discussions unless one has time to work a good many illustrative examples, and I can never spare so much time from my experiments.[76]

One had to devote long time to catch up with theoreticians' mathematical discussion. Experimentalists started to find it too demanding and not very rewarding to spare their time for theoretical work.

Franklin Institute, 200 (1925), pp. 147-160, pp. 157-158.

When asked by Kuhn about Bridgman's theoretical attempts, Kemble recounted that after all Bridgman failed "to get into the swim of quantum theory."

> I think that he never really put his mind to the theoretical problem as a participant in the seeking of solutions. He never taught it [quantum theory]: he was committed to his high pressure work. This had enough in it to absorb all of one side of his energy; his philosophical work seemed to be the other thing. He just never felt that the point had come for him to break away.[77]

By the end of World War I, it became clear that not only Bridgman, but the other staff members at the Harvard Physics Department who were all experimentalists, could no longer satisfactorily give courses on recent theoretical topics. Though Pierce had kept giving his course on "Radiation and Applications of Quantum Theory to Radiation" until 1926, this subject gradually became obsolete. The Department needed to appoint young theoreticians in order to retain fresh knowledge of quantum physics necessary for research and teaching.

2.1.3. Courses of Younger Theoreticians

Although his research had little to do with current topics in quantum physics, Bridgman took the initiative in establishing the theoretical program at Harvard. The faculty members working in atomic and molecular physics probably felt the need for theoretical courses more keenly than Bridgman. Some Harvard physicists were active in fields close to quantum physics: T. Lyman, who was doing spectroscopic experiments in the realm of short wave-length; William

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[76] Bridgman to Bartlett, Oct. 28, 1928, PWBP, HUG 4234.8.
[77] Interview with E. C. Kemble, AHQP.

Duane, whose interest was in radioactivity and X-rays; and F. A. Saunders, known for the Russell-Saunders coupling. Those experimentalists, however, were hostile to quantum theory.[78] Even Pierce, who was giving a course on Planck's quantum theory, preferred a purely classical explanation. Bridgman was less hostile to quantum physics than his colleagues. Having kept his theoretical interest since he was a college student, Bridgman took action to introduce the current theoretical development into both education and research at the Department.

The first theoretical researcher among the staff members at the Harvard Physics Department was E. C. Kemble, who did his theoretical graduate research in quantum physics under Bridgman's auspices. D. L. Webster could have been the first to establish theoretical research at the Department, but after serving as an instructor for half a year, he left for Stanford. In 1919, Kemble started to introduce current theoretical materials in his courses and to train research theoreticians, while developing his own research. In 1919-20, he started to deliver courses on electromagnetic theory of light, quantum theory with applications to the infra-red, photoelectric phenomena, specific heats, and X-rays and crystal structure,[79] as well as the conferences on theoretical topics. The next year he began to guide graduate research on quantum theory and infra-red spectra. In the early 1920s, when young students could not find many opportunities to study quantum physics in the United States, Kemble furnished rare chances to acquire knowledge of quantum theory and its applications. In 1922, under Kemble's supervision, J. H. Van Vleck wrote the first entirely theoretical doctoral

[78] Alexi Assmus, "Edwin C. Kemble, January 28, 1889-March 12, 1984," *National Academy of Sciences of the United States of America, Biographical Memoirs*, 76 (1999), pp. 179-197, p. 181.
[79] *Harvard University Catalogue, 1919-20* (Cambridge, Mass.: Harvard University, 1920), p. 385.

dissertation to be accepted by an American University. After teaching at Harvard for one year, Van Vleck accepted an assistant professorship at the University of Minnesota and then a professorship at the University of Wisconsin, training several theoretical physicists in the late 1920s and the early 1930s. A close friend of his, J. C. Slater, who did his experimental graduate research with Bridgman, attended Kemble's courses keenly, and later established himself as a theoretical physicist. After working with Bohr and Kramers at Copenhagen, he came back to Harvard and started to teach in 1924, cooperating with Kemble in raising theoretical research at Harvard. Besides giving the courses on relativity, quantum physics, and statistical physics, Slater and Kemble started to run seminars in theoretical physics in 1925 in order to guide theoretical works of graduate students.[80]

During the second half of the 1920s, Harvard's reputation remained quite high as a center of theoretical physics in the United States. In 1928, replying to the inquiries of Francis Birch, who was then applying for graduate work at Harvard, Bridgman explained the quality of theoretical research at Harvard:

> I am glad to hear that you are thinking of doing your work for a doctorate in physics at Harvard, and I am sure that we would all be very much pleased to have you come. If you did a piece of theoretical work on wave mechanics, or some of the other similar new developments, you would naturally work with Professor Kemble or Professor Slater. I think that in this field Harvard is as well equipped as any other institution in the country, with the possible exception of California Technical Institution [*sic*], where they have a strong theoretical staff. I should think it was about a toss-up betweent [*sic*] the two places.[81]

[80] *Harvard University Catalogue, 1925-26* (Cambridge, Mass.: Harvard University, 1925), p. 542.
[81] Bridgman to Birch, Aug 15, 1928, PWBP, HUG 4234.8.

At the California Institute of Technology, then two physicists, Paul Epstein and Linus Pauling, supervised research in quantum physics, and Caltech and Harvard were the only American institutions equipped with two full-time quantum scientists. Though in the 1930s the Harvard Physics Department could no longer keep this high status among the physics departments in the United States, it played an important role in disseminating quantum theory and training American theoretical physicists at stage of the reception of this new physical theory.

At Harvard, experimental physicists sensitive to the necessity of theoretical material first introduced relativity theory and quantum physics to the Department's courses. After World War I, however, the growth of quantum-physical research made them realize the need of theoretical physicists for research and education at the Department. Although Bridgman could teach the special and general theories of relativity and supervise E. C. Kemble's graduate study in quantum physics, he could neither complete his theoretical attempt nor continue his effort to master quantum mechanics as a tool for theoretical research. In 1919, Kemble was appointed instructor responsible for the courses in theoretical physics. He updated the courses on relativity and quantum physics and trained theoretical physicists at Harvard. By adding J. C. Slater to its staff in 1924, Harvard succeeded in establishing itself as one of the strongest centers of theoretical physics in the United States.

To understand the process of establishing theoretical physics at Harvard more fully, we have to examine other aspects of the effort of the Physics Department. In the next section, I will discuss the roles of the departmental colloquia and conferences and the lectures of visiting European physicists. Then in Section 2.3, I will describe the training, research activities, and appointments of four Harvard theoreticians.

2.2. The Departmental Lectures, Colloquia and Conferences

2.2.1. Colloquia and Conferences

Besides the formal courses, the Harvard Physics Department had been holding the "Physical Colloquium" every week, which consisted of announced meetings given by the instructors, advanced students, and sometimes invited physicists for the discussion of researches in progress made by the departmental members and of the contents of current journals in physics.[82] In addition to this colloquium, "Conference Course" started in 1915 in order to afford a longer time for discussion than the colloquium could.[83] In 1925, the Department abolished the conference course and instead started "Seminary [*sic*] in Theoretical Physics," given by Kemble and Slater. Though not the formal course, the public lectures on topics of general interest, sponsored by the Department, also started in 1920. These meetings and courses, occasionally given by Europeans, were invaluable chances to know what was going in and out of the United States. Their topics were both experimental and theoretical, but they turned out to be an important part of the departmental activity in theoretical physics, both for teaching and research, especially in the period when no formal research course for theoretical work existed at the Department.

Until the conference course started in 1915, the Physical Colloquium had been the only departmental meeting for general discussion. The colloquium was given every week, mainly attended by

[82] *Harvard University Catalogue, 1900-01* (Cambridge, Mass.: Harvard University, 1901), p. 405.
[83] *Harvard University Catalogue, 1915-16* (Cambridge, Mass.: Harvard University, 1916), p. 417.

instructors and graduate students.[84] It is not clear when and how this meeting began. When Bridgman entered Harvard College, it was already delivered every week.[85] The topics of discussion do not seem to have always been of too technical a nature. The editor of *Science*, J. McKeen Cattell, finding the titles desirable for publication in his journal, asked Lyman to send the reports of the Colloquium to him.[86] Lyman was not so willing to do so, not because of the nature of topics but because of the ephemeral and speculative nature of discussion. He wrote to Cattell: "The matter which is presented to our Colloquium is often in a form which would not be very suitable for publication in Science as it is intended more to start discussion than a finished exposition."[87] As *The Harvard University Catalogue* tells little about the Colloquium, its details remain unclear.

Some new materials, such as relativity, dimensional analysis (or the principle of similitude), and atomic and molecular physics, appeared as topics for the Colloquium. The Colloquium was announced in the *Harvard University Gazette* in advance. Between 1915 and 1917, E. C. Kemble, then a graduate student, delivered several talks on the theoretical aspects of atomic and molecular physics. Among the titles are, "The Stark Effect," "The Variation of Specific Heats of Solids with Temperature," and "The New Theory of Pyro-electricity."[88] On February 26, 1916, James B. Brinsmade, who was also a graduate student and was to cooperate with Kemble in their graduate works, gave a talk on "Bjerrum's Hypothesis Regarding Absorption Bands in Gases." During the presentation, Kemble hit upon the idea that was to lead him to his

[84] *Reports of the President and the Treasurer of Harvard College, 1905-06* (Cambridge, Mass.: Harvard University, 1907).
[85] *Harvard University Catalogue, 1900-01* (Cambridge, Mass.: Harvard University, 1901), p. 405.
[86] Cattell to Lyman, Dec. 24, 1912, PLDC, UA V 692.5.
[87] Lyman to Cattell, Dec. 26, 1912, PLDC, UA V 692.5.

doctoral thesis.[89] The Colloquium served as a research conference before the seminars on theoretical physics started at the Physics Department.

The Conference Course, also held once a week, proved to be profitable soon after its start in 1915. Many physicists and mathematicians outside the Physics Department attended it and gave addresses. In 1915-16, for instance, Byerly, a professor at the Mathematics Department, talked on "the Treatment of the Calculus of Variations."[90] In 1916-17, the number of the scientists outside the Physics Department who gave a lecture either at the Conference Courses or at the Colloquia increased drastically:[91] George Birkhoff, a professor at the Mathematics Department, gave a series of lectures on "the Integral Equations in Their Relation to Mathematical Physics"; C. A. Adams, a professor of the Department of Electrical Engineering, gave a series on "Dynamo Design Perspective"; E. B. Wilson, a professor of the Massachusetts Institute of Technology, gave a series on "Gravitation"; Irving Langmuir, a research engineer at General Electric, spoke twice on "the Constitution and Fundamental Properties of Liquids"; and R. C. Tolman, a Professor of the University of Illinois, discussed "the Principle of Similitude."

2.2.2. European Visitors

After World War I, European scientists started to visit the United States more frequently than before and delivered lectures at universities including Harvard. Every year during the period between 1921 and the

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88 Sopka, *Quantum Physics in America*, p. 1.64.
89 *Ibid.*
90 *Report of the President and the Treasurer of Harvard College, 1915-16* (Cambridge, Mass.: Harvard University, 1917), p. 224.
91 *Report of the President and the Treasurer of Harvard College, 1916-17* (Cambridge,

middle of the 1930s, including the years when American universities suffered from the financial damage caused by the Great Depression, the Harvard Physics Department had at least one lecture given by a European scientist. Furthermore, two German physicists joined the staff during this period: Friedrich Hund, a theoretical physicist from Rostock, was appointed visiting professor in 1929; and Otto Oldenberg, an experimentalist from Göttingen, accepted a professorship in 1930. The influential Europeans' lectures and the expenses for them were as follows:

On May 18, 1921, Einstein gave a "lucid exposition of the theory of general relativity" in German at a special meeting of the American Academy of Arts and Sciences held in Boston. He also visited Harvard and addressed the students.[92]

The next year, H. A. Lorentz served as a visiting professor at the California Institute of Technology from January through March. On his way back, he delivered lectures at Harvard on April 7, 10 and 11, on "Light and the Constitution of Matter."[93] He received $400 for three lectures.[94]

From October 1922 to April 1923, A. Sommerfeld served as Carl Schurtz Exchange Professor at the University of Wisconsin. He also gave a course of lectures at Harvard.[95]

Niels Bohr visited the United States in 1923 and delivered endowed lectures at Amherst College and at Yale University in October and November. On October 25 and 26, on his way from Amherst to Yale, he gave two lectures on "Theory of Spectra and the Atomic Constitution" at Harvard. He received $100 for each of the lectures.[96] Bohr was also invited to the Harvard Tercentenary in 1936, but could not attend it. Fourteen years after the first visit, he revisited Harvard in 1937 and gave a lecture on "Problems of Atomic Nuclei" on Feb. 8, for which he again received $100.[97]

Paul Ehrenfest, staying at the California Institute of Technology as a research associate in 1924, delivered three lectures at Harvard on "Problems of Quantum

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Mass.: Harvard University, 1918), p. 212.

[92] *Proceedings of the American Academy of Arts and Sciences*, 56 (1920-1921), p. 400. Einstein to Lyman, May 4, 1921, PLDC, UA V 692.5.

[93] Lyman to Wilson, March 30, 1922, PLDC, UA V 692.5.

[94] Millikan to Lyman, Jan. 7, 1922, PLDC, UA V 692.5.

[95] *Report of the President and the Treasurer of Harvard College 1922-23* (Cambridge, Mass.: Harvard University, 1924), p. 239.

[96] Lyman to Bacon, Oct. 6, 1923, and Lyman to Bohr, May 14, 1923, PLDC, UA V 692.5.

[97] Lyman to Bohr, Jan. 21, Feb. 3 and 5, 1937, PLDC, UA V 692.5.

Statistics" on April 21, 22, and 23. He received $100 a lecture.[98] In 1930, Ehrenfest lectured in the summer symposium at the University of Michigan. On his way back to Europe, he gave lectures at Harvard and MIT, on Jan. 23, 1931. This time he again received $100 a lecture.[99]

In 1925, the Physics Department had two guests. Manne Siegbahn of Upsala gave two lectures on "Precision measurements in X-rays," while P. Debye of Zurich, staying at MIT as a visiting lecturer in February and March, gave three lectures on "Theories of Magnetism" on February 19, March 3 and 5. For these lectures, Debye received the sum of $100.[100]

Max Born, while staying at MIT as a guest physicist, gave five lectures at Harvard on January 5 and 7, 1926: one lecture on "New Researches on Relations between Elastic and Thermal Properties of Anisotropic Bodies," one on "Investigation for the Purpose of Explaining the Occurrence of Certain Molecules and Crystal Lattices in Nature," and three on "Developments of the Quantum Theory." He received $250 for these lectures.[101]

Erwin Schrödinger conducted conferences at Harvard on March 15 and 17, 1927. Lyman requested him to deliver lectures "for advanced students on recent developments of your [Schrödinger's] theory," that is, wave mechanics.[102] The same year, Abram Joffe of Leningrad lectured at Harvard before he went to the University of California at Berkeley to serve as a guest lecturer during the spring semester. The expense of their lectures was covered by a special gift.[103]

William L. Bragg of Manchester, staying at MIT for several months in 1927-28, gave one lecture at Harvard.[104] James Frank of Göttingen gave three lectures at Harvard on January 9, 10 and 11, 1928. He received $50 a lecture.[105] The same year, Leon Brillouin, staying at the University of Wisconsin as a visiting professor, delivered a colloquium at Harvard.

Hermann Weyl of Zurich, staying at Princeton University as a visiting professor, was invited to Harvard by the Physics and Mathematics Departments and delivered lectures. Two lectures on "the Spherical Symmetry of Atoms" and "Gravitation and Electron" were given on April 29 and May 1, 1929, at the Physics Department. He received $100 per lecture at his own request.[106]

In 1929, the Physics Department appointed Friedrich Hund visiting professor. He gave a course on "Band Spectra and Molecular Structure" and was in charge of

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98 Lyman to Birkhoff, April 11, 1924, and Lyman to Ehrenfest, March 4, 1924, PLDC, UA V 692.5.

99 Lyman to Ehrenfest, Nov. 6, 1930, PLDC, UA V 692.5.

100 Lyman to Debye, Jan. 13, 1925, and Feb. 4, 1925, PLDC, UA V 692.5.

101 Born to Lyman, Dec. 16, 1925, and Lyman to Born, Dec. 5, 1925, PLDC, UA V 692.5.

102 Lyman to Schrödinger, Feb. 16, 1927, PLDC, UA V 692.5.

103 *Report of the President and the Treasurer of Harvard College, 1926-27* (Cambridge, Mass.: Harvard University, 1928), p. 240.

104 *Report of the President and the Treasurer of Harvard College, 1927-28* (Cambridge, Mass.: Harvard University, 1929), p. 265.

105 Lyman to Franck, Nov. 2, 1927, PLDC, UA V 692.5.

106 Coolidge to Lyman, Feb. 28, and Lyman to Coolidge, March 4, 1929, PLDC, UA V 692.5.

seminar in theoretical physics from February to May. Hund received $2,000 and the traveling expenses to and from Germany.[107]

A German experimental physicist, Otto Oldenberg was appointed professor at the Harvard Physics Department in 1930, and he took over the course on "Band Spectra and Molecular Structure." The next year, he started to give the "Laboratory Course in Atomic Physics" and the introductory course to quantum theory.[108]

In 1930, Max von Laue and Rudolf Ladenburg of the Kaiser-Wilhelm Institute at Dahlem-Berlin visited the United States at the expense of the Rockefeller Foundation and spoke in the Colloquium at Harvard.[109]

Jakov Frenkel, then Guest Professor of Theoretical Physics at the University of Minnesota, addressed the colloquium on March 2, 1931, the topic being "the quantum theory of light propagation in solid and liquid bodies." He was on the way back from the meeting of the American Physical Society held in New York in February. Lyman therefore offered only the traveling expenses from and to New York. In May, C. G. Darwin gave a course of Lowell Lectures in Boston.[110]

In the December of 1933, James Franck stayed in Cambridge, Massachusetts, and gave lectures at Harvard and MIT. Each of them paid $350 to him. K. T. Compton, the President of MIT, and F. A. Saunders, the chairman of the Harvard Physics Department, agreed "not to draw too heavily on Professor Franck's strength for lectures, but to find full opportunity for him to make personal contacts with our graduate students and staff in order to have plenty of personal discussions."[111] He gave special lectures on "the newer material in physics," not a continued series on a specified subject.

In 1934-35, P. A. M. Dirac was a member of the Institute for Advanced Study at Princeton. He delivered two lectures at Harvard in 1935, for which he received $100.[112]

During the January and February of 1936, R. H. Fowler stayed in Cambridge, Massachusetts on the condition similar to James Franck's. He received the sum of $1,000, which was to be equally borne by MIT and Harvard. Harvard actually paid three hundred and fifty dollars and offered him free board and lodging.[113]

American scientists praised Born as the most influential and most popular lecturer among these European visitors. During the second

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[107] Lyman to Hund, Nov. 25, 1927, March 31,and May 21, 1928, and Hund to Lyman, Dec. 14, 1927, and May 3, 1928, PLDC, UA V 692.5.
[108] *Harvard University Catalogue, November, 1931* (Cambridge, Mass.: Harvard University, 1931), p. 249.
[109] *Report of the President of Harvard College and Reports of Departments, 1930-31* (Cambridge, Mass.: Harvard University, 1932), p. 266.
[110] Darwin to Lyman, Feb. 18, 1931, PLDC, UA V 692.5.
[111] Compton to Saunders, Sept. 29, 1933, DPCC, UA V 691.10
[112] Lyman to Dirac, Dec. 19, 1934, PLDC, UA V 692.5.
[113] Saunders to Murdock, April 10, 1935, and Murdock to Saunders, April 13, 1935, PLDC, UA V 691.10.

half of the Fall Semester of 1925-26, he stayed at MIT to give lectures. The Harvard physicists, therefore, could have personal contacts with him and attend his lectures at MIT and Harvard. Born, who came to the United States with a new matrix mechanics, was to serve as its disseminator. When he left Europe, Heisenberg's first paper on this theory[114] had just appeared, while Born and Jordan's paper[115] was in press. The manuscript of another paper by Born, Jordan and Heisenberg was almost complete.[116] The second half of his lectures at Harvard were on these new developments.[117] As he visited several universities in the United States, many American physicists obtained fresher information of quantum mechanics than most of their European colleagues did.

J. C. Slater, who had been back from Copenhagen, had a chance to work with Born. Before Born left, Slater proved the equivalence of the Poisson bracket and the commutator and derived various quantum-mechanical theorems based on this discovery. At the same time, however, another talented young physicist showed up in Europe and went ahead of him. Slater later recollected this situation:

> I had a small opportunity to make contact with this work [matrix mechanics] during the latter part of 1925. Born spent several months in Cambridge, Massachusetts, lecturing at MIT, on the behavior of crystals, expanding on the work and also on the new quantum mechanics of Heisenberg, Jordan, and himself. Naturally I became acquainted with him, and had a chance to work on some of the aspects of the quantum-mechanical theory. I made a little progress beyond what he had already published,

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114 W. Heisenberg, "Über quantentheoretisch Umdeutung kinematischer und mechanischer Beziehungen," *Zeitschrift für Physik,* 33 (1925), pp. 879-893.

115 Born and Jordan, "Zur Quantenmechanik," *Zeitschrift für Physik,* 34 (1925), pp. 858-888.

116 Born, M., W. Heisenberg, und P. Jordan, "Zur Quantenmechanik II," *Zeitschrift für Physik* 35 (1926), pp. 557-615.

117 M. Born, *Problems of Atomic Dynamics* (Cambridge, Mass.: Massachusetts Institute of Technology, 1926), p. ix.

> but before I could get my work written up, a paper by a hitherto unknown genius appeared: P. A. M. Dirac's first paper on quantum mechanics. It included not only the small points I had worked out, but much more besides. But at least I got to know Born well, and kept scientific and personal relations with him during the rest of his life.[118]

As Europeans physicists knew little about Americans' studies in quantum mechanics, several "near misses" of this kind occurred between them in the 1920s.

The other European visitors were not very impressive to Americans Bohr, mainly because of his low voice and unclear pronunciation, was not a popular speaker.[119] Einstein visited the United States mainly for a political reason (to help raise money for the Hebrew University) and therefore gave lectures that were not very scientific. Though Kemble and Van Vleck remembered Ehrenfest's warm personality and encouraging words, no further contact between them seems to have followed.[120] In general, because of its tight budget, the Physics Department could not provide much financial support for the visiting physicists; therefore, they could not stay at Harvard long enough to have profound interaction with the physicists there. Although Hund stayed for four months in 1929, the theoretical physicists at Harvard, who had by then learned of the new results in physics during their stays at the European centers of research, regarded him rather as a colleague than as a rare source of valuable information.[121]

What impressed the European visitors who were suffering post-war economic disorder was American wealth.[122] Bohr received $3,000 from

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[118] J. C. Slater, *Solid-State and Molecular Theory: A Scientific Biography* (New York, London, Sydney, and Toronto: John Wiley & Sons, 1975), p. 21.
[119] Sopka, *Quantum Physics in America*, p. 2.38.
[120] *Ibid.*
[121] Slater, *Solid-State and Molecular Theory*, p. 62.
[122] The following numbers are cited from Sopka, *op. cit., Quantum Physics in America*,

Amherst College and $1,250 from Yale University for his lectures in October and November 1923. Sommerfeld's salary for one semester as Carl Schurz Professor at the University of Wisconsin was $4,000. An average middle-aged professor at Harvard then received around $4,000 as salary per academic year (9 months). Max Born's wife, Hedwig Born, described what then a trip to the United States meant to European physicists: "My husband feels an inclination to slay the golden calf in America and to earn enough through lecturing to build a small house in Göttingen."[123]

Most of the European physicists visited Harvard while invited by another American university. Harvard could invite only Hund, offering him a salary relatively small compared with the ones other universities paid to their visiting lecturers. Franck and Fowler were invited jointly by Harvard and MIT, and their stay did not cost much. Bohr was invited by Amherst College and Yale, Born and Debye by MIT, and Sommerfeld by the University of Wisconsin. Yet probably the most important center of gravity during the 1920s and 1930s was the California Institute of Technology, which had at least one exchange professorship in physics each year.[124] Lorentz, Ehrenfest, and later Einstein accepted this exchange professorship. To ask for information of the visiting Europeans, Lyman wrote to R. A. Millikan, the head of physicists at this institute.

> All physicists of distinction who come to this country gravitate to Pasadena sooner or later. We should be glad if during their goings and comings some of them would visit us here. In certain cases it may be possible to arrange short courses of lectures by these visiting gentlemen.

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p. 2.21.

[123] *The Born-Einstein Letters*, Irene Born, trans. (New York: Walker and Co., 1971), letter no. 22, p. 36.

[124] Millikan to Lyman, Nov. 21, 1930, PLDC, UA V 692.5.

> Could you be so good natured as to let me know from time to time when any of these distinguished people are expected.[125]

Millikan was polite enough to reply with a sociable compliment:

> You may be sure that we should like to cooperate with you to the fullest in giving physicists of distinction opportunity when they come to our shores to visit both the oldest and most distinguished and the youngest and freshest of our physical laboratories, namely, the Jefferson Laboratory and the Norman Bridge Laboratory. As a matter of fact, I don't think a trip to this country is complete without seeing something of these two widely separated institutions, with the intervening territory which must be traversed to see them both.[126]

The laboratories Millikan mentioned, the Norman Bridge Laboratory at Caltech and the Jefferson Laboratory at Harvard, must have made a sharp contrast to the visitors' eyes, when it came to the financial situation. While Millikan, the Director of the Norman Bridge Laboratory, was wondering on whom he should invest the large amount of budget he had at hand, Lyman, the Director of the Jefferson Laboratory, had to negotiate with the visiting physicists about the humble amount of their honoraria. In 1924, Lyman wrote to Ehrenfest about the Department's normal rate: "It has been our custom in the past to offer our visitors $100 a lecture and I trust this arrangement will prove satisfactory to you."[127] This rate applied to Lorentz in 1922, Bohr in 1923, and Weyl in 1929, although Harvard paid Lorentz $400 for his three lectures, following the suggestion of the generous Director of the wealthy Norman Bridge Laboratory. Lyman, however, tried to reduce the amount whenever he saw a chance: Born received $250 for his five lectures in 1926, and Fowler would have received $150 for his

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[125] Lyman to Millikan, Nov. 12, 1930, PLDC, UA V 692.5.
[126] Millikan to Lyman, Nov. 21, 1930, PLDC, UA V 692.5.

two lectures had he visited Harvard in 1931.[128] In 1927, Lyman wrote to James Franck, "We are accustomed to pay our distinguished visitors at the rate of $50. a lecture."[129] This rate applied to Dirac's lecture in 1935.[130] Some visitors received even less: Debye received $100 for his three lectures. In 1928, Lyman proposed a similar rate, $60 for two lectures, to H. A. Kramers from Utrecht.[131] Kramers was frank enough to write about the sum that seemed reasonable to him:

> I thank you very much for your kind letter of Jan. 19, 1928, in which you ask me to give two lectures at Harvard Univ., during my stay in America this year. I will certainly be very glad to follow this invitation, only I should like to remark that a payment of $60 for two lectures is even below what I was to obtain for lectures at the universities here in Europe. As a matter of fact, in connection with costs for travel and lodging, I had considered a sum of $50 as a reasonable minimum salary for lecture, in the USA. Hoping you will excuse my frankness in exposing these remarks to you, and hoping very much to see you and your laboratory this spring.[132]

Lyman and the Physics Department did not think that Kramers deserved the sum he suggested. Kramers decided not to come to Harvard that year.

Compared with wealthy institutions like Caltech or Berkeley, Harvard never felt relaxed about financial matters. The salary Hund received for two courses during one semester, $2,000 and traveling expenses, was lower than the average. As for Fowler's stay in January and February 1936, F. A. Saunders, the chairman of the Department, wrote thus: "We think this would be an exceedingly good thing to do,

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127 Lyman to Ehrenfest, March 4, 1924, PLDC, UA V 692.5.
128 Lyman to Fowler, Feb. 26, 1931, PLDC, UA V 692.5.
129 Lyman to Franck, Nov. 2, 1927, PLDC, UA V 692.5.
130 Lyman to Dirac, Dec. 19, 1934, PLDC, UA V 692.5.
131 Lyman to Kramers, Jan. 19, 1928, PLDC, UA V 692.5.
132 Kramers to Lyman, Feb. 6, 1928, PLDC, UA V 692.5.

and very cheap at the price."[133] Harvard was to pay $500, but turned out to pay only $350 and offered Fowler free board and lodging.

Harvard lacked a celebrity like Millikan who was awarded the Nobel Prize in 1923 and could attract visitors by his name, as well as by the money he could spend. But Bridgman, who, though not as famous as Millikan or K. T. Compton, had been communicating with several European physicists since the Fourth Solvay Conference in 1924, served as a mediator: Born arranged his schedule with Bridgman and Kemble,[134] and Debye had personal contact with Bridgman.[135]

In 1927, Bridgman made an effort to find an appropriate position for the French physicist Leon Brillouin. The correspondence between Bridgman and his friends at other universities concerning Brillouin's position illustrates the Americans' frank evaluation of European physicists. Bridgman and Brillouin met at the Solvay Conference and exchanged several letters on high pressure experiments in 1924. Three years later, Brillouin distributed the following letter among his American acquaintances including Bridgman:

> I have sent you, some days ago, some reprints of my recent articles on ondulatory mechanics; I hope they will interest you, I have joined a paper with my "curriculum vitae" and a list of my scientific publications. As I am not entirely satisfied with my actual situation in Paris, I should like to know wether [*sic*] I could find in America a place of Professor in an University, or at a Research Laboratory. I would be greatly indebted to you, if you could give me any valuable information on this question. You probably remember that we met at the last Solvay congress in Brussels; if you wish to get some references on my scientific work, you may write to the following professors:
> J. Perrin P. Langevin Paris
> P. Ehrenfest Leiden
> M. A. Lorentz Harlem

133 Saunders to Murdock, April 10, 1935, DPCC, UA V 691.10.
134 Born to Lyman, Dec. 16, 1925, PLDC, UA V 692.5.
135 Lyman to Debye, Jan. 13, 1925, PLDC, UA V 692.5.

A. Einstein	Berlin
P. Debye and V. Henri	Zurich.[136]

Bridgman remembered Brillouin as an able theoretical physicist. Knowing of one position at the University of Wisconsin left vacant by Max Mason, Bridgman wrote to C. E. Mendenhall, who was in charge of this appointment, and recommended Brillouin for that vacancy.[137] Mendenhall, who had just failed to invite Peter Debye as Mrs. Debye "objected so strongly to any plan to leave Europe," explained his wish honestly: "I should like very much to get two young or youngish men here instead of one more experienced & expensive--and I am afraid we might not be able to swing Brillouin as one of such a pair."[138] American physicists had just been impressed by a successful example of the policy of hiring "two young inexpensive men." In 1927, the University of Michigan strengthened its physics department by appointing S. A. Goudsmit and G. E. Uhlenbeck, who had just finished their doctoral studies with Paul Ehrenfest at Leiden. Brillouin, who had been born in 1889, was then in his late thirties, more experienced but more expensive. Mendenhall still asked Bridgman for more information of Brillouin, including what he would consider as an initial salary. Bridgman explained about Brillouin's command of English (Brillouin had been one of the candidates for the visiting professorship at MIT that Born had accepted), his ability as a physicist, his personality, and his family. Although Bridgman had no idea about a salary Brillouin wanted, he hinted that "it was the financial pressure that was driving him from Paris."[139]

Mendenhall wanted to appoint Brillouin for one or two semesters

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136 Brillouin to Bridgman, July 25, 1927, PWBP, HUG 4234.8.
137 Bridgman to Mendenhall, Aug. 7, 1927, PWBP, HUG 4234.8.
138 Mendenhall to Bridgman, Aug. 11, 1927, PWBP, HUG 4234.8.
139 Bridgman to Mendenhall, Aug. 14, 1927, PWBP, HUG 4234.8.

to see whether a permanent appoint would be mutually agreeable. Thus, the University of Wisconsin appointed Brillouin a visiting professor for the second semester of 1927-28. Bridgman took small advantage of his kindness. In October, he intimated to Brillouin of his arrangement concerning Brillouin's appointment at Wisconsin and asked him a favor:

> I hope that you will enjoy your coming trip, and that perhaps this will give you the opportunity to find a permanent position here, either at Wisconsin, or perhaps somewhere else. If you should find it convenient to stop in Cambridge on your way to Madison, I am sure that we should all be very glad to see you. I cannot, however, at this time give you anything in the nature of an official invitation, because Dr. Lyman, the head of our Department, is at present away fro [*sic*] a month, and Professor Saunders, the acting head, has been ill in bed ever since I returned from my summer in the country.[140]

Brillouin replied promptly:

> Many thanks for your kind letter of Oct. 2. I received during the summer the invitation from Prof. Mendenhall, and did not understand how he had the idea of calling me to Madison. I know now that you are the author of this suggestion, and I thank you therefor [*sic*].
>
> I shall be very glad to visit you in Cambridge on my way to Madison. I think I will come through Quebec and Montreal, where I have some good old friends.
>
> We shall have next week an interesting meeting at Brussels, for the new Solvay Congress; all the discussion will deal with quantum theory and ondulatory [wave] mechanics. I have received yesterday the report of Born and Heisenberg, and I feel that there are allready [*sic*] may obscure points; perhaps will the light come during the discussion!
>
> I shall get these good informations on the points of view of all the theoreticians, and should be very glad to bring you the latest news (I could say the latest fashion, because the evolution

[140] Bridgman to Brillouin, Oct. 2, 1927, PWBP, HUG 4234.8.

of the ideas is now so fast!).[141]

In this way, Bridgman could furnish chances to acquire information of the new fashion of physics without making a trip to Europe.

Bridgman continued his effort to find Brillouin a position the next year. In April, R. G. D. Richardson at Brown University told Bridgman that he had been looking for a man to take the initiative in building up Brown's Physics Department.[142] Considering as a candidate Arthur Haas from Vienna, a theoretician who had lectured at MIT in 1926-27, Richardson asked Bridgman for his evaluation of Haas. Though praising Haas as a "popular expositor" and author of a book that had been well reviewed, Bridgman wrote that he did not regard him as an important original researcher. In addition, he revealed an episode about Haas to Richardson:

> Furthermore, when he was making his lecture tour in this country he made a rather disagreeable impression on us here. He was quite anxious to lend prestige to his tour by starting out with a lecture here (at least he said so), and wrote several bargaining letters, gradually reducing his standard fee, until finally he asked us to allow him to come and lecture for nothing, which Lyman was strong minded enough to refuse. Here we associated this incident with some of the unpleasant characteristics of his race, although we may have been quite unwarranted in this, and it may have been [m]ostly the fault of his manager.[143]

To some European visitors, it seemed prestigious to start their lecture trips at Harvard. Besides its status as the best-known American university, its geographical location made it the first or last stopover for European visitors. Cambridge, Massachusetts was close to New York

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[141] Brillouin to Bridgman, Oct. 14, 1927, PWBP, HUG 4234.8.
[142] Richardson to Bridgman, April 20, 1928, PWBP, HUG 4234.8.
[143] Bridgman to Richardson, April 22, 1928, PWBP, HUG 4234.8.

where they got on or off the steamer between Europe and America.

As his remarks on Haas's race shows, Bridgman did not hide a racist tone in his private correspondence. Yet, since his prejudice was not against the French, he recommended Brillouin instead of Haas.

> Have you considered at all for your position L. Brillouin of the College de France. He is lecturing until June in the University of Wisconsin. He is a man of great ability in theoretical physics, and of much wider European reputation than Haas. What is more, he is anxious to find a position in this country, as the financial situation in France makes living almost intolerable. [...] I just learned in Washington that they are not going to try to get him permanently, because they are getting VanVleck from Wisconsin [*sic*]. [Van Vleck was then at Minnesota]. (Please to[*sic*] not mention this out loud yet). I was told that the negotiations for VanVleck were begun some time before Brillouin was approached, and at first went slowly, with little prospect of going through, which was Mendenhall's justification for approaching Brillouin, but that later V.V. accepted, leaving no place for B, considerably to his disappointment I judge (this latter being my own inference, B not having idscussed [*sic*] the situation with me himself).
>
> Unless you are already pretty well committed to Haas, I would certainly try to get into touch with Brillouin before he leaves the country.[144]

Van Vleck, appointed by the University of Wisconsin in place of Brillouin, was to stay there until he returned to Harvard in 1934. Though alone at Madison, Wisconsin, Van Vleck was productive enough in this period to complete his book, *Theory of Electric and Magnetic Susceptibilities.*[145]

In his reply, Richardson did not hide his racism, either; he wrote, "I did not realize that Haas was one of the chosen race." Prejudiced also against the French, Richardson was unwilling to consider Brillouin

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[144] *Ibid.*

[145] J. H. Van Vleck, *The Theory of Electric and Magnetic Susceptibilities* (London: Oxford University Press, 1932).

for the position, since "[i]n general the French have very great difficulty in adapting themselves to conditions abroad." He pointed out that he knew of "no case which a frenchman has made a good head of a dept in America."[146] American physicists were not just rich generous hosts eager to listen to whatever lectures Europeans delivered. When necessary, they mercilessly evaluated their guests according to their own criteria, preferences, and prejudices, circulating their judgments only among themselves.

American physicists staying in Europe could also serve as a source of information of European physicists' activities. For example, E. C. Kemble, one of the two theoretical physicists at the Harvard Physics Department, sent valuable information while he stayed in Europe in 1927 as a Guggenheim Fellow. He wrote to Lyman about some European physicists' reputations and activities:

> The German physicists have all been very cordial and we feel that we must do a better job by strangers in the future to square ourselves with the world. Sommerfeld is very much alive and I was much impressed by his lecture and seminar. Everyone seems to like Sommerfeld and the same is true of Franck. Born hasn't quite the same reputation for being "a good fellow" but I suspect the difference is due largely to the heavy burden of work which he carries in his teaching and as a result of his wife's illness. [...]
>
> This summer Born is conducting a seminar in conjunction with the mathematics department which is away over my head. It runs to operator theory and the like. The colloquium, on the other hand, is exceedingly interesting. It runs for an hour and a half, but partly on account of the varied program and partly on account of the coffee which everyone drinks at four o'clock, it never wears one out. Even more important to me are the lectures which Hund is giving on molecular structure. He is apparently the "smartest" man around and speaks with unusual clarity. If Harvard is ever able to import a German physicist for

[146] Richardson to Bridgman, April 23, 1928, PWBP, HUG 4234.8.

> a half year after the manner of Technology [MIT], I think Hund would be a very likely candidate. He is young, but that would be rather an advantage since it probably would not cost quite so much for one of the younger men.[...]
>
> [Robert S.] Mulliken has just arrived on the scene and I hope that with Hund's aid we can engineer an international agreement on the vexed question of band spectrum notation. Mulliken has just about established a world's record for output during the last year.[147]

Following Kemble's suggestion, the Harvard Physics Department appointed Hund as a visiting professor.

To sum up, at Harvard in the interwar years, foreigners' lectures served as an important source of information on new developments in Europe. In the 1920s, many European physicists visited America for their relatively short lecture trips and did not stay long enough to have profound interaction with American physicists, though there are some examples of their joint research. Despite its unfavorable financial situation, the Harvard Physics Department could take advantage of the reputation of the university they belonged to and asked many Europeans to give lectures while they stayed in America at other universities' invitations. The senior members at the Physics Department including Bridgman were anxious to invite them as visiting lecturers, trying to get information of their schedules, arranging their lectures, and negotiating about their honoraria.

2.3. The Harvard Quantum Theorists: Kemble, Slater, Van Vleck, and Oppenheimer

In this section, in order to see how the Harvard Physics Department's strategy to establish theoretical physics actually worked, I will closely survey the careers of four physicists, some of whom I have already mentioned briefly in the former part of this chapter: E. C. Kemble, J. C. Slater, J. H. Van Vleck, and J. R. Oppenheimer. The reform of curriculum and foreigners' lectures were important factors to help young physicists catch up with the development of theoretical physics. However, in the 1910s and 1920s, a young physics student had to do many things alone to become a theoretical physicist and to do research in quantum physics. I will discuss these physicists' scientific training, research interests, and scientific activities, mainly focusing on the questions of why they chose theoretical physics and how they established themselves as professional theorists. As my main purpose is to comprehend Bridgman's commitment to the program of nurturing theoretical physics at Harvard, I will examine their research as much as necessary for this purpose.

2.3.1. Edwin Crawford Kemble

Edwin Crawford Kemble is doubtless the first one to be discussed. Born in 1889, he was about ten years older than Van Vleck or Slater, who were almost as old as Werner Heisenberg or Wolfgang Pauli. Kemble did his graduate work between 1913 and 1917, when American universities including Harvard were not yet ready to accept or encourage purely theoretical research in physics, whether done by the staff members or students. Only after World War I did American physicists gradually start to realize the need to have theoretical researchers and train more of them. Kemble therefore had to make his way almost alone during his graduate research. Moreover, he had to wait for two years after graduation until he could secure a satisfactory academic position at Harvard. Immediately after he started teaching at

[147] Kemble to Lyman, June 9, 1927, PLDC, UA V 692.5.

Harvard, however, he became in charge of theoretical courses and started to supervise graduate students' theoretical research. Kemble established the Harvard Physics Department as one of the few American centers of theoretical research by pursuing his own research interest and by raising younger theoreticians.

Kemble was born in Delaware, Ohio, as a son of a Methodist minister. [148] Unlike Bridgman, who supervised his doctoral thesis, he wanted to be a missionary when young, and remained to be a church member throughout his life.[149] He graduated in physics from the Case School of Applied Science in Cleveland, Ohio, in 1911. While at Case, he found himself strong in mathematics and took some courses at its mathematics department. In physics, he took the courses on waves, vibrations, and acoustics by Dayton C. Miller (1866-1941). Like Bridgman, Kemble was interested in mathematical aspects of physics and wanted to study further toward a higher degree. Yet, since his financial situation then did not allow him to do so, he thereafter served for two years as an assistant at the newly founded Carnegie Institute of Technology.

Kemble entered Harvard in 1913 and started to receive a Whiting Fellowship, which paid \$300 including \$150 for tuition.[150] Gerald Holton has revealed that Wallace Sabine, impressed by Miller's recommendation for Kemble, was secretly financing this fellowship out of his own pocket.[151] Harvard at that time was not a very appropriate place to start the type of theoretical research that Kemble wished to pursue. He arrived there in the year when D. L. Webster received his

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[148] Holton, "On the Hesitant Rise of Quantum Physics Research in the United States," p. 178.
[149] Kemble to Carman, Nov. 10, 1956, ECKP, HUG 72.10.
[150] Lyman to Kemble, April 7, 1913, ECKP, HUG 72.10.
[151] Holton, "On the Hesitant Rise of Quantum Physics Research in the United States," p. 181.

Ph. D. only after adding some experimental part to his thesis, although he had wanted to submit an entirely theoretical one. Still, Bridgman had already started his course on relativity, and in 1915, G. W. Pierce inaugurated a course on "Radiation and the Quantum Theory." Kemble took both. While attending Pierce's course, he came across Morton Masius's 1914 translation of the second edition of Max Planck's *Wärmestrahlung,*[152] which pushed him to do his doctoral thesis in quantum physics. Later, he recalled how he felt at that time: "Anything with quantum in it, with *h* [Planck's constant] in it, was exciting."[153]

Kemble did not mind who his thesis advisor would be: he first chose Sabine, but after he left for war work in 1916, Kemble asked Bridgman to be his formal advisor.[154] In any case, no one at the Department knew quantum theory well enough to supervise Kemble in theoretical research. Bridgman, who happened to have more interest in quantum theory than his colleagues, accepted Kemble's wish. As Kemble had no one to ask for advice, the weekly Physical Colloquium was the only occasion on which he could discuss his ideas with his colleagues. Kemble's interest, shared by the spectroscopy experimentalists at the Department, was as follows:

> If the distribution of angular velocities of gas molecules followed the Maxwell-Boltzmann law, and if radiation were emitted in accordance with the classical electromagnetic theory, each emission and absorption frequency in a multi-atomic gas would be spread out into a continuous band whose width would depend on the [rotational] inertia, but would always be large

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152 Max Planck, trans. Morton Masius, *The Theory of Heat Radiation* (Philadelphia: P. Blakiston's Son and Company, 1914; reissued by Dover Publications Inc., 1959).
153 Interview with Kemble, conducted by T. S. Kuhn and J. H. Van Vleck on Oct. 1 and 2, 1963, AHQP.
154 Holton, "On the Hesitant Rise of Quantum Physics Research in the United States," p. 184.

> compared with the width of a normal spectrum line. The absence of such continuous bands in the spectra of gases is one of the most incontrovertible evidences known that there is something radically wrong with the classical mechanics, or the classical electrodynamics, or both.[155]

Kemble thus started to approach the problem of band spectra in gasses and attempted to find a way to apply Planck's theory to it.

On February 26, 1916, Kemble hit upon a clue to approach this problem, or the idea that was to guide his graduate work, while listening to his fellow student James B. Brinsmade talking on the theory of the band structure of infrared absorption spectra, developed by a Danish physicist Niels Bjerrum.[156] Bjerrum tried to explain the structure of the band spectra of gas molecules such as HCl, CO_2, and H_2O, by the assumption that one could calculate the width of these bands from a superposition of molecular rotation on molecular vibration. He applied Planck's ideas of quantization to the vibration and rotation of molecules.

In his doctoral thesis, titled "Studies in the Application of the Quantum Hypothesis to the Kinetic Theory of Gases and to the Theory of their Infra-red Absorption Bands," [157] Kemble suggested the possibility that the motion of the atoms in a diatomic molecule will no longer be simple harmonic motion if the amplitude of their vibration is large. He predicted that this anharmonic oscillator would produce the

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155 E. C. Kemble, R. T. Birge, W. F. Colby, F. Wheeler Loomis, and Leigh Page, *Molecular Spectra in Gases, National Research Council Bulletin*, 57(1926), p. 10.

156 N. Bjerrum, "Über die ultraroten Absorptionsspektren der Gase," in *Festschrift W. Nernst* (Halle: Knapp, 1912), pp. 90-98; "Über die ultraroten Spektren II. Eine direkte Messung der Grösse von Energiequanten," *Verhandlungen der Deutschen Physikalischen Gesellschaft*, 16 (1914),pp. 640-642; and "Über die ultraroten Spektren der Gase III," *Verhandlungen der Deutschen Physikalischen Gesellschaft*, 16 (1914), pp. 737-753.

157 Alexi Assmus had made a detailed account for Kemble's work in "The Americanization of molecular physics," *Historical Studies in the Physical and Biological Sciences*, 23 (1992), pp. 1-34.

higher frequency "overtone," estimating it in the cases of HCl and HBr. However, his thesis was not entirely theoretical: he also experimentally showed that the higher frequency "overtone" actually occurred with the help of Brindsmade. Despite Bridgman's effort to convince the senior members of the value of theoretical research, the Physics Department did not admit an entirely theoretical thesis.[158]

Kemble frequently asked Bridgman for advice while he was completing his thesis. While they worked on the same campus, they seldom needed to write to each other. However, when either of them was out of town, they exchanged letters to discuss Kemble's project.[159] One letter from Kemble to Bridgman shows that Kemble's "conversion" to Bohr's theory in January 1917, although eventually he would not even cite it in his thesis:

> You will be interested in hearing that I have decided that my theory of harmincs[*sic*] is all wrong. I'm converted to the Bohr theory - for the time being, at least - and am finding that it will work in the infrared region as well as in the other parts of the spectrum. It occurred to me not long ago to try the effect applying to a linear oscillator Bohr's assumption that the frequency of of [*sic*] the radiation absorbed or emitted is determined by the difference of the initial and final energies of the radiating system. The answer is obviously that an ideal linear oscillator should emit and absorb a whole series of harmonics, while an oscillator which does not have a linear law of force will give ride to a series of bands which have only an approximately harmonic relationship. The new assumption saves Kirchoff's[*sic*] law and explains our difficulty over the exact wavelengths of the bands without getting us into any new trouble.[160]

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[158] Alexi Assmus, "Edwin C. Kemble, January 28, 1889-March 12, 1984," *National Academy of Sciences of the United States of America, Biographical Memoirs*, 76 (1999), pp. 179-197, p. 184.
[159] Kemble to Bridgman, Aug. 16, 1916, PWBP, HUG 4234.8.
[160] Kemble to Bridgman, Jan. 4, 1917, PWBP, HUG 4234.8.

Although Bridgman was then busy with wartime work at the New London Experiment Station, he tried to catch up with the development of Kemble's theoretical research.

The above cited letter of Kemble tells that, not only having known Bohr's theory, Kemble had already been sure of its validity by January 1917. However, following Planck and Bjerrum, Kemble in his thesis assumed that the frequencies of absorption were the same as the actual internal molecular frequencies and apparently ignored Bohr's 1913 theory that equated radiation frequencies with differences in the energies of electron orbits.[161] Most likely, Kemble became convinced of the validity of Bohr's theory too late; there was no time for him to change his thesis thoroughly. In the winter of 1916-1917, while working on his thesis, Kemble was in Buffalo, New York, and started to engage himself in an engineering problem, the development of a new motor.[162] Not being well-off, probably he could not afford to stay at the university for a longer period and to spend too much time in revising his thesis. Kemble later minded his apparent disregard of Bohr's theory and even asked the librarian at the Physics Department to attach a note to his thesis: "It would appear that during the period when the theoretical work here described was in progress, 1916-17, I had not heard of the Bohr theory."[163] Furthermore, Kemble's papers at the Harvard University Archives lack material from 1916 to 1920. The historian of physics Alexi Assmus has suspected that, being too self-conscious and too historically conscious—after World War II

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161 Niels Bohr, "The Constitution of Atoms and Molecules I," *Philosophical Magazine*, 26 (1913), pp. 1-25; "The Constitution of Atoms and Molecules II: Systems Containing Only One Nucleus," *Philosophical Magazine*, 26 (1913), pp. 476-502; "The Constitution of Atoms and Molecules III: Systems Containing Several Nuclei," *Philosophical Magazine*, 26 (1913), pp. 857-875.
162 Kemble to Bridgman, Jan. 4, 1917, PWBP, HUG 4234.8.
163 Holton, "On the Hesitant Rise of Quantum Physics Research in the United States," p. 188.

Kemble would join the general education project to teach all Harvard undergraduates the history of science--he was so embarrassed of his shortcomings that he threw it away to leave no evidence behind.[164] However, he seems to have forgotten about his January 1917 letter to Bridgman.

After obtaining his Ph. D. in June 1917, Kemble started working as an engineering physicist at the Curtiss Aeroplane and Motor Corporation in Buffalo, New York. The United States was at war then. Although the Harvard Physics Department missed his talent, Kemble felt it his duty to work for a war work project and probably was financially in need. In August 1917, Lyman wrote to him about his decision to work as an engineer: "Of course we shall miss you very much next year but I believe that we shall be able to run after a fashion without your assistance. If you think it your duty to remain where you are I cannot quarrel with your decision."[165] Even while he was studying at Harvard, Kemble once considered to abandon his graduate research and to start to work at Williams College in Williamstown, Massachusetts, which was then looking for an instructor in physics.[166] Lyman wrote William McElfresh at Williams College that Kemble should remain at Harvard another year for he was "in just that condition when, if he leaves off his original work, he probably will never resume it."[167] Although in the end Kemble decided to continue his graduate work, obviously he was financially in need.

Kemble, however, did not totally give up his hope to acquire an academic position. While working at Curtiss, he stayed in contact with the Harvard Physics Department. He wrote to Lyman in 1917: "I must

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164 Alexi Assmus, "Molecular Structure and the Genesis of the American Quantum Physics Community, 1916-1926," p. 83.
165 Lyman to Kemble, Aug. 15, 1917, PLDC, UA V 692.5.
166 McElfresh to Lyman, March 9, 1915, PLDC, UA V 692.5.
167 Lyman to McElfresh, March 10, 1915, PLDC, UA V 692.5.

say that the thought of staying out of physics for two years or more as the prospect now is, does not seem at all attractive."[168] In November 1918, when Kemble had to leave the Curtiss Company because of the cease-fire but was unwilling to do so, Lyman informed him that Williams College was again looking for a temporary instructor in physics.[169] This time Kemble accepted the offer and worked in Williamstown for the rest of the academic year.

In February 1919, the Harvard Physics Department finally decided to have Kemble back and recommended Brinsmaid for the position at Williams College.[170] Although the Department offered Kemble a favorable position with the duty of teaching only one advanced course, he was careful enough not to give an affirmative reply promptly, having experienced a considerable decrease in the salary when he moved to Williams College. ' To E. H. Hall, who served as a head of the Jefferson Physical Laboratory while Lyman was abroad, he explained his duty to help his family and the plan of his own life. Then he told the salary he would receive should he stay at Williamstown and Williams College's generous offer of a rent-free suite room in the laboratory. Considering the difference in the cost of living between Cambridge and Williamstown, he suggested $2,000 as his initial salary. He rightly described this amount "a salary which has not been very unusual for the best of the Cambridge Ph. D.'s."[171]

Hall honestly replied to Kemble that his salary would be $1,400, a little higher than the usual one for a man in the first year of instructorship, that is, $1,200. Moreover, Hall explained that teaching work at Radcliffe would bring several hundred dollars in addition.

168 Kemble to Lyman, Aug. 5, 1917, PLDC, UA V 692.5.
169 Hall to Kemble, Dec. 25, 1918, PLDC, UA V 692.5.
170 Hall to Kemble, Feb. 21, 1919, PLDC, UA V 692.5.
171 Kemble to Hall, March 1, 1919, PLDC, UA V 692.5.

Though eager to get him back "not primarily or principally to do heavy work of elementary instruction but to help keep the Department at the front in research and the higher teaching," Hall could not yet promise anything about promotion.[172]

In reply, Kemble did not hide his disappointment; the suggested salary was unacceptably low. Furthermore, he was unwilling to teach elementary courses either at Harvard or at Radcliffe. He wrote: "I fear that if I tried to do a considerable amount of elementary teaching and research work in addition, I should break my health without accomplishing anything worth while. The candidate for that kind of a place should have a physique like Dave Webster."[173] He also asked for further information of ways to earn extra money, such as entrance examination work in the spring and fall, and of his prospect for the future.

The Physics Department then assigned Bridgman, Kemble's thesis advisor, the task of persuading Kemble.[174] In a letter to Kemble, Bridgman explained what he expected from the courses Kemble would give, expressing his enthusiasm about having a course in theoretical physics, Course 22, at Harvard:

> I am really enthusiastic about this scheme of courses. It comes pretty close to what I have been waiting for a long time. If we can get the courses well given, it ought to put Harvard pretty near the top in this country. What is more, it is good beginning to putting this country on the map in theoretical physics. Course 22 is designed especially for this, and would normally be taken only those students specializing in theoretical physics, of whom we shall hope for an increasing number. But you see that you are an essential part of this program. Don't you want to be a member of a Department that is trying to do this, and don't you

[172] Hall to Kemble, March 8, 1919, PLDC, UA V 692.5.
[173] Kemble to Hall, March 10, 1919, PLDC, UA V 692.5.
[174] Hall to Kemble, March 13, 1919, PLDC, UA V 692.5.

> feel the challenge in this?[175]

Bridgman was eager to raise theoretical research in America. For this purpose, he had been trying to build a center of theoretical physics at his department, expecting Kemble to lead it.

Right after this enthusiastic paragraph, Bridgman had to explain considerably about Kemble's expected salary and future promotion:

> Now for the question of ways and means and the financial prospects, which rightly are giving you considerable concern. It is in the first place necessary to recognize perfectly frankly that a job in pure physics like this can never pay as much as a great many commercial jobs. It is inevitable that this kind of a life means financial sacrifice. Of course during the war you have got into the commercial atmosphere, more or less, but I hope it hasn't dulled your old time idealistic ambition. If it has not, and if you are determined on pure physics as a career, I think that you cannot do better than to start at Harvard and grow up there. The reason is that the ultimate prospects are as good or better than they are nearly everywhere else. It is unfortunate that it is the tradition here to start the young instructor disproportionally far down in the financial scale.

Bridgman went on to explain the promotion. At the Physics Department, if young instructors satisfied the senior members of the department, the promotion would be made to a five year appointment as an assistant professor. After another five years as an assistant professor, depending on their grades and the finances of the department, he might be appointed associate professor or full professor for an unlimited term. Moreover, Bridgman added that work at Radcliffe and entrance examination would bring extra incomes.

Bridgman succeeded in persuading Kemble to accept an instructorship at the Harvard Physics Department. Though Course 22

[175] Bridgman to Kemble, March 16, 1919, PWBP, HUG 4234.8.

does not seem to have ever been given, in 1919, Kemble started to introduce new materials in theoretical physics to research and education at the Department. To start a seminar in theoretical physics, however, he had to wait for another theoretician to come to the Department. It was after John C. Slater joined the faculty and started to give a seminar with Kemble in 1925 that Harvard really stood in the front of theoretical physics.

2.3.2. John C. Slater

Slater was born in 1900 in Oak Park, Illinois.[176] His father had studied at the Divinity School of the University of Chicago before he became a university teacher of English literature. Slater's parents were Baptists; he also started to go to Sunday school when he was four years old. In the religious aspect, Slater followed his parents and remained a Baptist throughout his life. When he was seventeen years old, he entered the University of Rochester, where his father taught English literature. Slater took all the undergraduate courses in physics, chemistry, and mathematics, completing the degree requirements in three years. For graduate study, he chose Harvard. Van Vleck, a life-long friend of Slater, who had come to Harvard a few months before Slater's arrival, recalled Slater's reputation: "In September 1920 there was a discontinuity and upward change in the mean quality of the Harvard graduate students in physics because this group of fifteen or so had added to it a nineteen year-old—John C. Slater—who had just graduated from the University of Rochester."[177]

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[176] For Slater's life, see, Schweber, "The Young John Clarke Slater and the Development of Quantum Chemisty"; Slater, *Solid State and Molecular Theory*; and Philip M. Morse, "John Clarke Slater," *National Academy of Science of the United States of America, Biographical Memoirs,* 53 (1982), pp. 297-321.
[177] J. H. Van Vleck, "The First Ten Years of John Slater's Scientific Career," p. 11,

At Harvard, Slater attended Kemble's courses on statistical mechanics, the electromagnetic theory of light, and quantum theory. Kemble, who had just started to direct graduate students, required them to read the second edition of Sommerfeld's *Atombau und Spektrallinien*,[178] as well as the most recent papers on atomic and molecular structure. Slater also took the courses on electronics by Pierce and E. L. Chaffee, a few courses by Bridgman, and some given at the Mathematics Department.

The weekly colloquium was another important source of stimulus Slater enjoyed at Harvard. Almost fifty years later, he recalled it:

> [T]he way I got to see what was going on was the weekly colloquium, and everybody who talked in the weekly colloquium was drawing pictures of atoms on the board, and the weekly colloquium was an extremely lively place. While you may say that there was only Kemble who was interested in quantum theory then, this was really not at all true because most of the experimentalists were interested in this too. The more I got into the field, as I took Kemble's course and read Sommerfeld's book and so on, the more I realized that here was a real center of things.[179]

Slater came to Harvard at the right time. Kemble had just started to supervise theoretical graduate students, while the other older experimentalists were encouraging his attempt.

Although working closely with Kemble's group, Slater did not do his thesis with Kemble. He chose Bridgman as his thesis advisor and submitted a thesis titled "Compressibility of the Alkali Halides" in the spring of 1923. On entering Harvard, Slater started to work with

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John Clarke Slater Papers, AHQP.

178 Arnold Sommerfeld, *Atombau und Spektrahllinien*, 2nd ed. (Braunschweig: Friedrich Vieweg und Sohn, 1921).

179 Interview with John Clarke Slater, conducted by T. S. Kuhn and J. H. Van Vleck on October 3, 1963, AHQP.

Bridgman. When he applied for graduate work at Harvard, he was offered a choice of a fellowship at $350 or a half-time assistantship at $500. He took the assistantship and decided to work with Bridgman. "I became very fond of [Bridgman]," he later recalled, "and I think he did of me; our temperaments fitted together very well."[180] When asked by T. S. Kuhn the reason he decided to do his thesis with Bridgman, Slater replied thus:

> I'd been assisting Bridgman for the first two years. The third year I had to look around and decide—I guess I had started my thesis even before the third year—whom to work with, and I just convinced myself by seeing him in action that he was the best person in the department, and that I'd rather work with him. I was very much impressed with him as a person, general qualities and so on, and I'd learned the techniques, so I thought this was the very obvious thing to do.[181]

Despite his inclination toward theoretical research, Slater decided to work with "the best person in the department," Bridgman. Though he was good at and fond of his experimental work, after obtaining his Ph. D., Slater turned his serious attention toward theoretical physics. Bridgman, who had seen Slater do far more theoretical work than most of the graduate students, made a suggestion that he should go in for theoretical research.[182]

Slater decided to study in Europe for a while and applied for a National Research Fellowship and a Sheldon Fellowship. Bridgman wrote the recommendation letters for both fellowships. His letter for a National Research Fellowship describes how he regarded Slater: "It is impossible for me to speak in too high praise of Mr. Slater's ability; he is

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[180] Schweber, "The Young John Clarke Slater and the Development of Quantum Chemistry," p. 349.

[181] Interview with John Clarke Slater, conducted by T. S. Kuhn and J. H. Van Vleck on October 3, 1963, AHQP.

without exception the most brilliant and promising young physicist with whom I have come in contact. Not only are his abilities of a brilliant order, but he is also unusually sound, and I have the utmost confidence in the conclusions to which his thinking or his measurements lead him."[183] Slater accepted a Sheldon traveling fellowship and left for Europe.

Though planning to study with Bohr, Slater decided to visit Cambridge, England, first to work with R. H. Fowler, as Bohr was traveling during the fall semester. As for his experience at the other Cambridge, he later recalled: "I talked over my ideas on the breadth of energy spectra with Fowler, and he was perfectly polite and made absolutely no contributions. I just got absolutely nothing out of Fowler. [...] I got nothing scientific from the fellow."[184] Then he moved to Copenhagen.

Slater's experience in Copenhagen was not exciting, either.

> [...] I found them [Cambridge, England and Copenhagen] no more up-to-date and no more lively than Harvard was. I felt I was just as much at the center of things at Harvard as I was either at Cambridge or at Copenhagen, absolutely. Well, I felt just as much so as I did at Cambridge, and rather more up-to-date than at Copenhagen. I thought that Harvard was more lively than Copenhagen.[185]

Because of Kemble's efforts and several graduate students in quantum physics, Harvard had become an active center of physical research by the middle of the 1920s.

Slater's experience with Bohr and Kramers even turned out to leave

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182 *Ibid.*
183 Bridgman to Tisdale, April 1, 1923, PWBP, HUG 4234.8.
184 Interview with John Clarke Slater, conducted by T. S. Kuhn and J. H. Van Vleck on October 3, 1963, AHQP.
185 *Ibid.*

bitter feelings in his memory. While working in Copenhagen, Slater published a celebrated paper [186] with Bohr and Kramers, which contended that the continuously distributed electromagnetic field determined the probabilities of an atom's transitions from one stationary state to another. This paper included a striking assumption: conservation of energy is satisfied only statistically. Slater was unwilling to admit this and sent a letter to *Nature* to state that he originally assumed the existence of photons instead of claiming the statistical character of energy conservation.[187] Recounting his stay in Copenhagen, he expressed his feelings toward Bohr and Kramers: "I had a horrible time in Copenhagen. [...] I fought with [Bohr and Kramers] so seriously that I have never had any respect for those people since."[188] He could not stand the way the Europeans did physics:

> I have a great distrust of the hand-waving approach to anything. I had supposed, when I went to Copenhagen, that although Bohr's papers looked like hand-waving, they were just covering up all the mathematics and careful thought that had gone on underneath. The thing I convinced myself of after a month, was that there was nothing underneath. It was all just hand-waving.[189]

In Europe, Slater found out that what had seemed mystic to him in America was actually a fake. The way European did physics was incomprehensible to Slater. When he worked on the hypothesis that the photon was emitted during the stationary state, "Bohr was contemptuous

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[186] N. Bohr, H. A. Kramers, and Slater, "The Quantum Theory of Radiation," *Philosophical Magazine*, 47 (1924), pp. 785-802; "Über die Quantentheorie der Strahlung," *Zeitschrift für Physik*, 24 (1924), pp. 69-87.
[187] J. C. Slater, "Radiation and Atoms," *Nature*, 113 (1924), pp. 307-308.
[188] Interview with John Clarke Slater, conducted by T. S. Kuhn and J. H. Van Vleck on October 3, 1963, AHQP
[189] *Ibid.*

of it."[190] Kramers behaved in the same way. Although he was not always Bohr's "Yes-man," he was so in this respect. "He was trying to act like the wise papa who was telling the little boy how he has to know how to handle the great man, or how to behave towards the great man. Oh it was very much the case of the great man and the little boy (in the corner). I wasn't used to this. Nobody at Harvard had ever acted that way."[191] Perhaps Bohr was really a great man, but Slater did not understand why he could not discuss with him as frankly as he used to do with his colleagues at Harvard.

In the winter of 1923-24, suspecting that Bohr was appropriating his idea, Slater wrote to his father on this issue. Slater's father consulted with Bridgman about this "perplexing situation."

> Within a week of his arrival at Copenhagen during the holidays he had presented his theory and formulas to Dr Bohr for criticism. Since that time Dr Bohr and his associate Dr Kramers have spent a great deal of time in discussing the theory with John and also privately between themselves. In letters received to-day, but written several weeks ago, John states that Dr Bohr accepted the theory in the main, and that his plan was that he (Dr Bohr) and Kramers should write it up, embodying the suggestions and modifications which they had worked out. At first Bohr proposed that before this article was ready John should write a brief article himself and send it to Nature; but a few days later he withdrew this suggestion, not regarding it as desirable. At the time of writing John seemed to be somewhat reassured as to certain obviously questionable aspects of this method of announcing a theory worked out by an American at an English university. But it does not look good to me.[192]

Bridgman tried to be fair and calm him down. In reply, he explained Bohr's personality and Slater's quality.

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[190] *Ibid.*
[191] *Ibid.*
[192] John R. Slater to Bridgman, Feb. 1, 1924, PWBP, HUG 4234.8.

Perhaps you know that Bohr was in this country this last fall, and came in contact with a good many of our physicists, in particular spending several days here in Cambridge. The impression which he made on every one who met him was a singularly pleasing one personally; I have seldom met a man with such evident singleness of purpose and so apparently free from guile. I cannot believe without further evidence that he is wilfullysstealing[*sic*] any rightful credit from Slater. You have to trust people some times, and Bohr is certainly a man whom I would pick to trust. [...]

I have watched your son closely through three years, and I have the greatest confidence in his scientific future. The news that you write me of his new theory is not surprising to me, but is just the sort of thing that I am looking for him to do. This is not the only important piece of work that he will do. In view of all this, I am inclined to feel that you are attaching a disproportionate amount of importance to this mater of priority. I would much rather run the risk that he lose the credit for this particular piece of work than the [*sic*] he queer his career at the beginning. He is on the ground, and can size up the situation better than we. If he would rather lose some of the credit in order to keep serene his prospect for another year's work with Bohr, I for myself would be willing to abide by his judgement.[193]

Bridgman knew that this kind of problems could arise when several scientists with different cultural backgrounds collaborated. Trusting Slater's talent and soundness, he let Slater settle his own problem with Bohr and Kramers.

Evaluating Slater highly, the Harvard Physics Department tried to have him back as an instructor for the academic year 1924-25. In January 1923, even before his trip to Europe, he received an offer from G. W. Stewart at the State University of Iowa,[194] while Harvard had nothing to show him then. Though informed by Lyman that Slater was planning to study in Europe, Stewart did not give him up totally,

[193] Bridgman to John R. Slater, Feb. 4, 1924, PWBP, HUG 4234.8.
[194] Stewart to Lyman, Jan. 25, 1923, PLDC, UA V 692.5.

offering him an appointment at $2,200 starting in September 1924. By June 1923, as the situation had improved at Harvard, Lyman could tell Slater about a possible appointment in 1924-25: "I have heard from Dr. Bridgman that you may be willing to return to us after your study in Europe. While I think it unfair to bind you to any definite arrangement at present yet at the same time I should like to say that I am willing to promise you an instructorship for the year 1924-25."[195]

By February 1924, stimulated by Bridgman's letter asking about his plans for the next year, Slater started to think about coming home, after a bitter experience with Bohr and Kramers. On the other hand, as his mood had improved considerably, he was playing with an idea of staying in Copenhagen another year. He consulted with Bridgman:

> I am naturally wondering whether it would be a good thing, if it could be managed, for me to be here another year. I appreciate very much your wanting to have me at Harvard; and I am becoming surer all the time that that is what I want to do when I go back to America. In fact, if practical matters can be arranged, as I don't doubt they can, I think you can count on me. But I haven't yet been able to decide whether it would be better for me, and also for you, if I were to stay here next year or not.[...] It goes without saying that by staying I should get to know Bohr and the rest better than I can this year. And also they will get to know me better. On the other hand, I am not sure how desirable it all be to be so near a celebrity. Bohr is a very open minded and fair minded person; just the same, there is of course more or less feeling that one must think as he does, and I am unable to tell whether that would interfere with my happiness or not. However, I don't think this question amounts to much. I am sure that he has no conscious desire in that direction. Then there are other similar things that I can't be sure of. I am hoping that I may be able to do more or less useful work in quantum theory in the next year or two; and I don't know whether I could do more or less here than away, and whether, if I weren't here, they probably wouldn't get it all done

[195] Lyman to Slater, June 12, 1923, PLDC, UA V 692.5.

> up ahead of time. I have been debating all these things, and I don't know the answer.[196]

Seeing Slater swinging, the Physics Department offered him an instructorship at $1,600 and a Cutting Fellowship, making a total of about $2,200, the same salary Iowa had suggested.[197] Slater, however, cabled to ask for renewal of his Sheldon Fellowship to stay in Copenhagen the next year. By the time Lyman finished his reappointment to the fellowship, Slater had changed his mind again, cabling Lyman that he would accept Harvard's offer.[198] At the beginning of March, 1924, he made up his mind to come back to Harvard.

Before Slater left Europe, Stewart presented him a startling offer: an assistant professorship at $3,200.[199] Slater started to negotiate with Harvard, cabling Lyman that "Stewart offcers[*sic*] assistant professorship at thirty two hundred would be glad to remain Harvard for twenty five hundred with some reduction of elementary work."[200] Lyman promptly replied: "Offer instructorship probably with faculty rank. Total with fellowship twenty seven hundred. Elementary teaching equipment one full course through year advanced teaching same."[201] Slater again accepted Lyman's offer and declined Stewart's. It gradually became difficult even for their alma mater to appoint talented theoreticians.

A somewhat unpleasant experience with Bohr and Kramers may have given Slater self-confidence, as later it turned out that only he made a correct judgment among the three authors of the famous Bohr-Kramers-Slater paper. The historian of physics S. S. Schweber has inferred that "Slater's subsequent efforts to build up American physics

[196] Slater to Bridgman, Feb. 1, 1924, PWBP, HUG 4234.8.
[197] Lyman to Slater, Feb. 8, 1924, PLDC, UA V 692.5.
[198] Slater to Lyman, March 7, 1924, PLDC, UA V 692.5.
[199] Stewart to Lyman, May 24, 1924, PLDC, UA V 692.5.
[200] Slater to Lyman, May 26, PLDC, UA V 692.5.
[201] Lyman to Slater, May 27, PLDC, UA V 692.5.

may have been driven in part by what he considered his humiliating experience in Copenhagen."[202] After returning to Harvard, Slater started to give a course on "Kinetic Theory of Gases" in 1924.[203] The next year, he joined Bridgman in teaching the courses on relativity and electromagnetism and started a seminar in theoretical physics with Kemble.[204] In 1926, Slater was promoted to an assistant professor. Lyman explained to the Dean of the Faculty of Arts and Sciences Slater's reputation and the Department's need to promote him: "This young man is a person of the very high ability; he has already considerably distinguished himself, as the papers which I enclose will show. If he does not receive his promotion, I believe it will be impossible for us to retain him; he has already had more than one flattering offer."[205] By keeping Slater in addition to Kemble, Harvard could remain to be one of the American centers of theoretical physics.

2.3.3. John Hasbrouck Van Vleck

Neither Kemble nor Slater wrote an entirely theoretical doctoral thesis. It was John Hasbrouck Van Vleck who submitted the first theoretical dissertation to the Harvard Physics Department. Van Vleck was born in Middletown, Connecticut in 1899. His father was a famous mathematician and his grandfather an astronomer.[206] After graduating from the University of Wisconsin, he entered Harvard for graduate study

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[202] Schweber, "The Young John Clarke Slater and the Development of Quantum Chemistry," p. 356.
[203] *Harvard University Catalogue, 1924-25* (Cambridge, Mass.: Harvard University, 1924), p. 509.
[204] *Harvard University Catalogue, 1925-26* (Cambridge, Mass.: Harvard University, 1925), p. 540-542.
[205] Lyman to Moore, Feb. 9, 1926, PLDC, UA V 692.5.
[206] For Van Vleck's life, see, Fellows, *J. H. Van Vleck*; and P. W. Anderson. "John Hasbrouck Van Vleck," *National Academy of Science of the United States of America, Biographical Memoirs,* 56 (1987), pp. 501-540.

in physics in 1920. As Van Vleck knew of Slater's reputation, so Slater heard "tales about this wonderfully brilliant new graduate student,"[207] Van Vleck. They started to live in the same room in the graduate dormitory Conant Hall in 1921.

Van Vleck was the first thesis student of Kemble. He attended Kemble's course on quantum theory and read by himself Niels Bohr's "On the Quantum Theory of Line-Spectra"[208] and H. A. Kramers's "Intensities of Spectral Lines."[209] He also attended Bridgman's courses on relativity and electromagnetism and heat conduction, acoustics, elasticity and hydrodynamics. Recognizing Van Vleck's achievements highly, the Department offered him a stipend of $675 as a John Tyndall Scholarship from 1921 to 1922. In December 1921, at a meeting of the American Physical Society, he presented a summary of his quantum-theoretical calculation of the ground-state energy of the "crossed-orbit" model of the helium atom. He published it in the *Philosophical Magazine* in 1922.[210] That year, he submitted his doctoral thesis, "A Critical Study of Possible Models of the Normal Helium Atom," and finished his degree.

The Physics Department was eager to appoint Van Vleck. In 1922-23, he remained at the Department as an instructor. In January 1923, the Minnesota physicists, who had been impressed by his presentation at the 1921 meeting, offered him an assistant professorship at their department. He was then planning to study in Europe with Slater, applying for a National Research Fellowship. The Minnesota

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207 J. C. Slater, Introduction to Van Vleck's address "Reminiscences of the First Decade of Quantum Mechanics," *International Journal of Quantum Chemistry: Quantum Chemistry Symposia,* 5 (1971), pp. 1-2.

208 Niels Bohr, "On the Quantum Theory of Line-Spectra," *Kongelige Danske Videnskabernes Selskabs Skrifter, Naturvidenskabelig og mathematik afdeling,* series 8, IV.1 (1918), pp. 1-118.

209 Hendrik Anthony Kramers, "Intensities of Spectral Lines," *Kongelige Danske Videnskabernes Selskabs Skrifter, Naturvidenskabelig og mathematik afdeling,* series 8, III.3 (1919), pp. 285-386.

210 J. H. Van Vleck, "The Normal Helium Atom and its Relation to the Quantum

physicists' offer, however, was quite attractive: "a purely graduate position with due allowance of time for research and original work."[211] Lyman wrote to H. A. Erikson at Minnesota about this offer: "I regret to say that we have nothing whatever here that can compete with your very flattering offer to him and I should advise him to accept at once were it not for the fact that I believe a year spent in Europe would be very profitable to him at this time."[212] Lyman, who thought students were better off at Harvard, did not generally recommend them to go abroad to study.[213] However, he was instrumental in getting scholarships for Slater and Van Vleck. After all, Van Vleck accepted the offer from Minnesota and started to teach at the University of Minnesota in 1923. The following year, Slater took over the position at Harvard left vacant by Van Vleck.

2.3.4. J. Robert Oppenheimer

When Slater and Van Vleck left Harvard, a chemistry undergraduate sent a somewhat strange application for permission to take a course in physics.[214] He wanted to take Physics 6a, a course on "Heat and Elementary Thermodynamics" given by Kemble,[215] without completing its prerequisite. In support of the application, he detailed the work he had done so far:

> In preparatory school I took a full laboratory course in Physics, in which I received the grade of A. In addition to the regular work, I performed several experiments in mechanics, heat, and

Theory," *Philosophical Magazine*, 44 (1922), pp. 842-869.
211 Erikson to Lyman, January 17, 1923, PLDC, UA 692.5.
212 Lyman to Erikson, January 23, 1923, PLDC, UA 692.5.
213 Lyman to Briggs, Feb. 8, 1924, PLDC, UA V 692.5.
214 Oppenheimer to Kemble, May 24, 1923, PLDC, UA V 692.5.
215 *Harvard University Descriptive Catalogue, 1923-24* (Cambridge, Mass.: Harvard University, 1923), p. 65.

light; furthermore, I read rather widely in elementary books on optics, the theory of heat, and the physics of the molecule. I presented Physics as a subject for entrance, and received a 96 on the examination of the College Board.

This year I have taken Chemistry 2, in which I received a grade of A; Chemistry 3, in which my grade, up to the present, has been A; and mathematics with Professor Coolidge, in which I have been receiving an A. On the advice of Professor Coolidge and Professor Osgood I am going to take Mathematics 5 next year. During this time I have read several works on Thermodynamics and related subjects. A partiallist [*sic*] follows:

Ramsay; Lewis: Vol.1, Kinetic Theory.
Vol.2, Thermodynamics and Statistical Mechanics.
Vol.3, Quantum Theory.
Lewis and Randall: Thermodynamics.
Crothers (of Thomsen's laboratory): Molecular Physics.
Poincare: La Physique Moderne.
Walker: Physical Chemistry.
Ostwald: Solutions.
Gibbs: On the Equilibria of Heterogeneous Systems.
Jeans: The Dynamical Theory of Gases. (part)
Poincare: Thermodynamique (part)
Nernst: Thermodynamics and Chemistry.
(part of) Theoretische Chemie.
Sommerfeld: Atombau u. Spectral-linien(part)
Mac Dougall: Thermodynamics and Chemistry.

Whatever reading or work you may advise, I shall be glad to do; for I very sincerely hope that my petition may be granted.[216]

Forty years later, J. Robert Oppenheimer, who had sent this application, recalled a story about his request: "[Y]ears later I was told that when the faculty met to consider this request, George Washington Pierce met with them and said, 'Obviously if he says he's read these books he's a liar, but he should get a Ph. D. for knowing their titles.'"[217] His petition was accepted and Oppenheimer, though a chemistry undergraduate, started to take courses in the Physics Department.

[216] Oppenheimer to Kemble, May 24, 1923, PLDC, UA V 692.5.
[217] Interview with Oppenheimer conducted by T. S. Kuhn on November 18, 1963, AHQP.

Oppenheimer was born in New York City in 1904 as the first son of a wealthy German-Jewish family.[218] He entered Harvard in 1922. He ended up in receiving an A. B. degree in chemistry in 1925, but had taken or audited by then many courses in physics and mathematics: a course on Sturm-Liouville equation given by G. D. Birkhoff at the Mathematics Department and Bridgman's graduate courses on statistical mechanics and electromagnetic theory, as well as Kemble's course already mentioned. Furthermore, he attended Slater's seminar and learned of the Bohr-Kramers-Slater theory. Besides taking courses, Oppenheimer spent many hours at Bridgman's laboratory. He later described this experimental physicist "a man to whom one wanted to be an apprentice."[219]

After graduating from Harvard, Oppenheimer mastered quantum physics in Europe. He originally planned to work with Ernest Rutherford, to whom Bridgman sent the following recommendation for Oppenheimer:

> It is difficult for me to urge upon you any course of action in this matter, because I am not perfectly certain in my own mind as to what Oppenheimer's future career in physics is likely to be. He is very young, I think not yet twenty, and has just completed in three years our ordinary four year undergraduate course, summa cum laude, which is a very unusual achievment[*sic*], and sufficient indication of the very unusual quality of his mind. He has a perfectly prodigious power of assimilation, and has covered in his own reading vast fields outside the required amount in his academic courses. He has taken many of the advanced courses which we require here in physics for the PhD degree, but not all of them. This last year he has taken with me a theoretical course in the mathematical theory of electricity and

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[218] For Oppenheimer's life, see, Alice Kimball Smith and Charles Weiner, eds., *Robert Oppenheimer: Letters and Recollections* (Cambridge, Mass., and London: Harvard University Press, 1980).

[219] Interview with Oppenheimer conducted by T. S. Kuhn on November 18, 1963, AHQP.

> magnetism, and has done very well indeed. His problems have in many cases shown a high degree of originality in treatment and much mathematical power. His weakness is on the experimental side. His type of mind is analytical, rather than physical, and he is not at home in the manipulations of the laboratory. He has not taken all the elementary laboratory courses which we expect the average undergraduate to take. During this last year he started with me a small research on the effect of pressure on the resistance of alloys, and was evidently much handicapped by his lack of familiarity with ordinary physical manipulations. However he stuck at it, and by the end of the year had learned much, and obtained some results of value, all without being of much trouble to me personally, but he picked up many of the tricks of manipulation which he need [*sic*] from my mechanic.
>
> It appears to me that it is a bit of a gamble as to whether Oppenheimer will ever make any real contributions of an important character, but if he does make good at all, I believe that he will be a very unusual success, and if you are in a position to take a small gamble without too much trouble, I think you will seldom find a more interesting betting proposition.
>
> As appear from his name, Oppenheimer is a Jew, but entirely without the usual qualifications of his race. He is a tall, well set-up young man, with a rather engaging diffidence of manner, and I think you need have no hesitation whatever for any reason of this sort in considering his application.[220]

Oppenheimer was twenty-one years old then. Although obviously excellent, he was originally from chemistry and probably looked too young. Bridgman could not say much about his quality so confidently, but at least did not judge him by his race.

Oppenheimer proved to be a productive theoretician while staying in Europe. After being rejected by Rutherford, he studied with R. H. Fowler and published two papers based on the new quantum mechanics.[221] Then he moved to Göttingen to work with Born. By the

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[220] Bridgman to Rutherford, June 24, 1925, PWBP, HUG 4234.8.

[221] J. R. Oppenheimer, "On the quantum theory of vibration-rotation bands," *Proceedings of the Cambridge Philosophical Society*, 23 (1925-1927), pp. 327-335; "On the quantum theory of the problem of the two bodies," *ibid.*, 422-431.

time he submitted his doctoral dissertation, titled " Zur Quantentheorie kontinuierlicher Spektren," to the University of Göttingen in the summer of 1927, he had also published papers including one on the quantum theory of molecules co-authored with Born, which contained discussion of the "Born-Oppenheimer approximation."[222] Born wrote to S. W. Stratton, then the President of MIT, that among the five Americans studying with him at Göttingen, "[o]ne man is quite excellent, Mr. Oppenheimer, who studied at Harvard and in Cambridge, England."[223] Kemble, who happened to visit in Göttingen during his trip in Germany, was surprised at Oppenheimer's reputation, reporting to Lyman thus:

> Oppenheimer is turning out to be ever more brilliant than we thought when we had him at Harvard. He is turning out new work very rapidly and is able to hold his own with any of the galaxy of young mathematical physicist here. Unfortunately Born tells me that he has the same difficulty about expressing himself clearly in writing which we observed at Harvard. I think Oppenheimer plans to spend half of next year in Pasadena and half at Harvard.[224]

Oppenheimer returned to the United States in the summer of 1927. The following academic year, he stayed at Harvard, the University of California, Berkeley, and the California Institute of Technology. Then again he left for Europe to study with Paul Ehrenfest in Leiden and Wolfgang Pauli in Zurich. During his one year's stay in the United States, the Harvard Physics Department, urged by Kemble, Slater, and Bridgman, approached Oppenheimer. The other senior members, however, do not seem to have been enthusiastic in obtaining him. In

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222 Oppenheimer und M. Born, "Zur Quantentheorie der Molekeln," *Annalen der Physik*, 84 (1927), pp. 457-484.
223 Sopka, *Quantum Physics in America, 1920-1935*, p. 3.46.
224 Kemble to Lyman, June 9, 1927, PLDC, UA V 692.5.

April 1928, Lyman asked F. A. Saunders for his opinion of the Department's plan to invite Oppenheimer:

> We had a Department meeting yesterday when it was unanimously voted to adopt the tutorial system for next year.
>
> Bridgman, Kemble and Slater were enthusiastically in favor of inviting Oppenheimer. After listening to the arguments I, myself, am entirely won over. The chances of getting him are pretty small; we could only offer $2500 plus one half of the Cutting [Fellowship]. However the thing is worth trying.
>
> Before making the attempt I wish to have your opinion in the matter.[225]

In his reply, Saunders did not hide his skepticism over Oppenheimer's quality:

> Oppenheimer I am willing to try. As you said it is a gamble, and we may not get him. I can imagine him to be good as a tutor; also not so. But we can try. Certainly he will shed lustre on us if we can get him, and more still if we can keep him. I feel that he has a peat advantage over Mulliken [R. S. Mulliken, then an assistant professor at New York University] in being a better speaker, and probably a better teacher in the end; doubtless, too, incomparably better as a mathematical physicist; and much less skilful [*sic*] with apparatus. In fact we mustn't let him smash any!
>
> Kemble suggests doing something to attract men to us for experimental work. Naturally Oppenheimer doesn't seem to help in that direction. But we can't get any one man whose power of attraction is as great as KTCompton, and I'm not greatly worried by this, as there is something about a group such as ones that is healthy, stimulating, and in the long run move likely to be permanent than any department which verges toward being a one-man show.
>
> I don't think I'd favor our offering Oppenheimer an assistant-professorship, if we could. [...]
>
> I trust that something plenty can be found for Oppenheimer to do next year - I mean in a tutorial direction;

[225] Lyman to Saunders, April 4, PLDC, UA V 692.5.

> enough at least so that we can call him a tutor with a straight face.[226]

Lyman's and Saunders's letters reveal some interesting points. After having obtained Kemble and Slater, the Physics Department could no longer finance enough to appoint another theoretical physicist. Although Bridgman, Kemble, and Slater were enthusiastic in appointing Oppenheimer, Lyman and Saunders did not fully agree, partly because they underestimated him, and partly because they thought that they already had enough theoreticians. Furthermore, they not only knew that Harvard lacked such a center of attraction as Millikan at Caltech or the Compton brothers, but also did not want to have one. They preferred to have a group of experimentalists of a steady and trustworthy type, but did not take interest in initiating group research.

After all, the Harvard Physics Department decided to approach Oppenheimer.[227] They offered the rank of an instructor and tutor. The work would consist of lectures and individual instruction, which in total would not exceed the equivalent of two courses. The Department did not promise anything definite about promotion, but hinted that he would receive the rank of an assistant professor after the first year and then a permanent place. Lyman understood that other universities had offered Oppenheimer positions with better conditions, but this was the best Harvard could do then.

Oppenheimer considered Harvard's offer seriously, as he was fond of Harvard and the people there. Besides, he knew that Bridgman had been eager to strengthen the theoretical group at Harvard by appointing him. Bridgman had written to Oppenheimer in April 1927: "If this appeals to you at all I am sure that we would all be very glad indeed to

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[226] Saunders to Lyman, April 5, PLDC, UA V 692.5.
[227] Lyman to Oppenheimer, April 10, 1928, PLDC, UA V 692.5.

have you at Harvard again, and together with Kemble and Slater you ought to make a team that would get some significant theoretical work done."[228] However, Harvard's offer came too late. While in California, he had promised Caltech and Berkeley to divide his time between them on the condition that he should be granted a fellowship to spend another year in Europe. Oppenheimer succeeded in obtaining a National Research Fellowship, and the two institutions were satisfied with his plan.[229] In his letter to Bridgman written one year later, he expressed his feelings toward his former teacher and Harvard:

> I have waited an unconscionable time with answering; I hope that you will excuse me. For it was hard to make the decision, and harder still to write to you of it. I should have liked above all to come to Harvard; and it would have been to me some consolation for the fact that I should never be a physicist like you, that I could work a little in the same department. I could not come because I had made arrangements for the next years which proved to be irreversible.[230]

Bridgman also expressed his honest feelings: "We are truly sorry that we could not have you here for next year, and are inclined to reproach ourselves for letting the opportunity slip when you were here."[231] It was indeed a loss to Harvard. When Oppenheimer came back from Europe, he had already distinguished himself in the international community of theoretical physicists with his more than a dozen significant papers that he had published. In California, he was to be a center of a strong group of young theoretical physicists.

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[228] Bridgman to Oppenheimer, April 3, 1927, PWBP, HUG 4234.8
[229] Oppenheimer to Lyman, May 7, 1928, PLDC, UA V 692.5.
[230] Oppenheimer to Bridgman, May 16, 1929, PWBP, HUG 4234.8.
[231] Bridgman to Oppenheimer, June 3, 1928, PWPB, HUG 42234.8

Harvard remained active in research during the second half of the 1920s. Still, Slater, Kemble, and Bridgman were eager to promote further research in quantum physics, especially after matrix mechanics and wave mechanics became available for practical use. In a memorandum originally composed by Slater and then moderated by Bridgman, they outlined their plans of reform to strengthen research in physics at Harvard. They suggested that the Jefferson Physics Laboratory should undertake "as an effective research program a coordination of theory and experiment, and of one experimental field with another, all directed toward a more complete understanding of matter in its various aspects."[232] The objectives of their memorandum were as follows:

> Some method of deliberately fostering such cooperation must be devised if this Department is to maintain its position among the leaders in this country. The object of this memorandum is to suggest a method of securing this cooperation.[233]

For a similar purpose, Bridgman had been instrumental in establishing theoretical research at Harvard. Slater joined him in the effort to maintain the Department's position in America.

As to the mechanisms for enhancing such cooperation, they proposed:

> [P]eriodic meeting of the members of the Department for the sole purpose of discussion of scientific problems. Informal dinners [...] after which one or two members would give a general survey of the particular problems or experiments on which they are at present engaged, of the difficulties of the investigations and their significance, of the general lines of inquiry in which they are

[232] Schweber, "The Young John Clarke Slater and the Development of Quantum Chemistry," p. 362.
[233] *Ibid.*

> interested, programs for the future, and ways in which the technique at their significance, of the general lines of inquiry in which they are interested, programs for the future, and ways in which the technique at their command enables them to cooperate with others.[234]

Furthermore, they suggested the reform of graduate study. They recommended the policy that several students "work together at the same time on different aspects of the same topic." For instance, "one student might do an experimental job, and another the theoretical part of the same investigation, thus forming, together with the two instructors responsible for the supervision of the students, a group of four persons working together on the same problem." It would be desirable that not only students but also "the members of the department cooperate in suggesting and planning cooperative projects of research," and that the faculty members would discuss their research works with the students hoping to work with them. As Slater did with Bridgman, students should "spend some time as apprentice, or assistant to someone already working on a similar research, to acquire facility with the methods and at the same time contribute by doing routine measurements or computations.[235] Slater urged, and Bridgman endorsed, that the Department should encourage cooperation between its experimentalists and theoreticians, and staff members and graduate students.

Slater and Bridgman's plan was not implemented at Harvard. In the summer of 1930, Slater accepted a chairmanship of the Physics Department at MIT. His leave ominously foretold the decline of the Harvard Physics Department in the 1930s.

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[234] *Ibid.*
[235] *Ibid.*, pp. 362-363.

One can, however, safely maintain that Harvard successfully established and held itself as a center of theoretical research during the twenties. Though lacking ample financial resources or an attractive administrator, Harvard trained several productive theoretical physicists and appointed some of them as staff members, after the steady and long-standing effort to raise theoretical research. This effort, started under the initiative of the experimentalists sensitive to new needs in physics, was taken over by the younger theoreticians. Courses on theoretical subjects, foreigners' lectures, colloquia and conferences, and discussion at the Department played essential roles in the growth of theoretical research.

The Harvard Physics Department did not take any novel strategy for raising a young group of theoreticians in the 1920s. Its conservative and orthodox way would become clearer when compared with the contemporary trend of doing physics in the United States. The Physics Department at the University of Michigan, for instance, chose to add European theoreticians to the faculty and hold summer symposia in order to strengthen theoretical physics there. In 1926, it appointed the Munich-trained physicist Otto Laporte. In 1927, Samuel Goudsmit and George Uhlenbeck joined its faculty. Then another theoretician D. M. Dennison, who had been back from three years' stay in Europe (1924-27), came to Michigan. The Michigan Department started to sponsor summer symposia in theoretical physics in 1928, which made Ann Arbor a lively center of atomic physics. At Princeton, physicists took a similar strategy, with financial aid from the General Education Board of the Rockefeller Foundation. In 1929, Princeton appointed the mathematician John von Neumann and the physicist Eugene Wigner, on the condition that it allow both of them to spend half of each year in Berlin. Other American centers of physics developed their quantum-mechanical research by appointing American young physicists with training in Europe; in fact, some departments clearly preferred Americans to Europeans. In 1928, Caltech appointed Linus Pauling. The next year, J. Robert Oppenheimer joined him. The University of Chicago appointed Robert Mulliken (Chicago Ph. D., National Research Council Fellow, Guggenheim Fellow) and Carl Eckart (Princeton Ph. D., National Research Council Fellow at Caltech, Guggenheim Fellow) in 1928.

At Harvard, teachers and students started their program for quantum-mechanical research without much help from Europeans. The first American quantum-physicist, E. C. Kemble, completed his Ph. D. thesis on molecular physics without any experience in Europe. Slater and Van Vleck had chances to study in Europe, but stayed there only for short periods. Harvard appointed European physicists Friedrich Hund and Otto Oldenberg, but Hund stayed there only for half of an academic year. Oldenberg, an experimentalist, had little to do with the establishment of theoretical research.

Bridgman started his career as an experimentalist when physics in the United States underwent a change in its practice. The rise of theoretical research beginning in Europe urged American physicists to update their way of doing physics. Though Bridgman may have been aware of this shift since his college years, his discovery of the new way of preventing leaks led him to choose traditional experimental research as his profession. However, after he was appointed faculty member at Harvard, he found himself in a position to take lead in the reform of the education and research at the Physics Department. For establishing theoretical research at Harvard, Bridgman did all that an experimentalist in a field outside atomic physics could do: he taught courses in electromagnetism and relativity theory, encouraged graduate students' theoretical attempts, and invited his European acquaintances to deliver lectures on theoretical matters.

The Harvard Physics Department lacked sufficient financial means to start entirely new programs for research and education and expand its faculty. Under this financial condition, a few experimentalists including Bridgman made their own effort to nurture and encourage theoretical physics, and their students followed them. Bridgman also benefited from the establishment of theoretical research at their department. Although his experimental research did not directly have much to do with the contemporary results of theoretical study, Bridgman could learn of the latest activities of theoretical physicists. Working closely to active theoreticians, Bridgman was prepared to judge which aspects of contemporary physics he should scrutinize from an experimentalist's vantage point. In the following chapters, I will discuss how he actually developed his reflections on the nature of physical theory and how younger theoretical physicists reacted to them.