Chapter 4. *The Logic of Modern Physics* and the Birth of Operationism

Bridgman started to reflect on the foundations of physics after he took over the courses on electromagnetic theory from B. O. Peirce upon his death in 1913. However, although he kept reviewing articles and books on relativity theory, he could not concentrate on this subject, because of his military research during World War I and his involvement in the controversy over dimensional analysis in the late 1910s. After finishing the draft of *Dimensional Analysis* in September 1920, Bridgman resumed his scrutiny of relativity theory in the summer of 1921. Then, in the summers of 1921 and 1922, while preparing several articles on the logical structure of relativity theory for the Boston meeting of the American Academy of Arts and Sciences, to be held in December 1922, he gradually constructed the scheme of his operational perspective. In September 1923, Bridgman went on to complete a long typescript, in which he attempted to examine some fundamental concepts in physics from the operational point of view. One can regard this typescript as the first thorough manifestation of the general scheme and basic standpoint of his operational stance, which he was to refine during his sabbatical year in 1926 in Europe and to publish as *The Logic of Modern Physics* the next year.

Between 1923 and 1927, Bridgman underwent a stimulating encounter with another new-born physical theory, quantum theory and eventually quantum mechanics, through his participation in the Fourth Solvay Conference in 1924, Born's lectures at Harvard and MIT in 1925-26, his discussion with European physicists at the conferences in Zurich and Göttingen during the sabbatical year he spent in Europe, and his own reading of the literature on the theory. A glimpse at *The*

Logic of Modern Physics conveys Bridgman's enthusiasm and anticipation over the rise of quantum mechanics as an operational theory, which seemed to him to be constructed in the way he regarded as an ideal.

In this chapter, I will examine how Bridgman formed his operational perspective through his struggle with relativity and quantum theories and how he finally expressed the platform of operationalism in *The Logic of Modern Physics*. I will show that Bridgman's operational perspective was based on his peculiar interpretation of the status of measurement in the special theory of relativity, which had its origin in his unique understanding of the role of operation in building physical theory.

4.1. Bridgman and Relativity Theory

4.1.1. Ether in Electrostatics

As has been previously mentioned, Bridgman publicly and privately recalled that he consciously began an effort to explicate the meaning of fundamental concepts in physics in 1914, when he started to deliver courses on electromagnetism that included materials in the special theory of relativity. This, however, does not mean that before 1914 he had never had any chance to reflect on problems related to electromagnetism. As I have mentioned in Chapter 1, two manuscripts he wrote as a graduate student in physics show his early interest in the foundations of physics; in fact, one of these two papers dealt with the necessity and role of the luminiferous medium, ether, one of the problems that Einstein discussed in his paper on special relativity.[1]

[1] P. W. Bridgman, "The Role of the Medium in Electrostatics," MS, PWBP, HUG 4234.10. As for the date, there is a comment by Bridgman himself in the front page

Though the comment Bridgman later put down in the front page of this paper suggests that he wrote the manuscript in 1905 or 1906, in connection with the courses in electromagnetic theory he took at Harvard, its content shows that apparently he was not familiar with Einstein's relativity theory of 1905. At that time, Bridgman was not alone in his disregard of relativity theory. As Stanley Goldberg has pointed out, "during the years 1905-1907 there was no notice taken of Einstein's theory" in the United States.[2] The manuscript tells nothing about Bridgman's early response to the special theory of relativity; however, it still reveals some interesting points in his view before his encounter with relativity.

Bridgman's paper titled "The Role of the Medium in Electrostatics" discussed the meaning of an elastic medium in electromagnetic theory, especially electrostatics. Bridgman did not admit the existence of such a medium *a priori*. The importance of a medium through which electromagnetic energy is transmitted had been recognized more keenly in electrodynamics. Even in this case, Bridgman observed that "the medium is not logically necessary." However, he found that the introduction and use of a medium "have been most suggestive and helpful," which, he thought, "apart from its necessity, is ample justification" of its existence. In the rest of the manuscript, Bridgman attempted to explain some phenomena in electrostatics by using a notion of elastic medium. He understood that this attempt had been successful in electrodynamics and that it should be fruitful in electrostatics as well, "if there really is a medium." Though his discussion owed much to W. H. Bragg's paper published in the

of the manuscript: "Probably written in connection with Physics 9 or 10, 1905 or 1906. 25/4/61."

[2] Stanley Goldberg, *Understanding Relativity: Origin and Impact of a Scientific Revolution* (Boston, Basel, Stuttgart: Birkhäuser, 1984), p. 248.

Philosophical Magazine,[3] he did not follow Bragg's quantitative way of reasoning, since "for physical purposes a physical conception is greatly to be preferred to a mathematical one." Bridgman preferred physical model to mathematical discussion already in his student years. It is also notable that he did not care much about the existence of electromagnetic medium, emphasizing its convenience and usefulness.

4.1.2. The Encounter with Relativity

After immersing himself deeply in experimental research, Bridgman does not seem to have had enough time to survey the new development in theoretical physics. Probably G. N. Lewis and R. C. Tolman were the first colleagues to turn Bridgman's attention to the special theory of relativity. Lewis and Tolman were not only among the first Americans who noticed the importance of the theory, but also among the early scientists who broadened its application.[4] Around the end of 1910, Lewis, then at MIT, was formally invited to Harvard to "deliver say Ten or dozen lectures on the subject of 'Relativity.'"[5] There were probably many other occasions on which Bridgman discussed the relativity theory with Lewis and Tolman.

In October 1914, a month after starting to deliver courses on electromagnetism, Bridgman ordered five books in order to catch up with the recent development in theoretical physics. One of the volumes was *Das Relativitätsprinzip*,[6] a collection of articles by Einstein, H. A.

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[3] W. H. Bragg, "The 'Elastic Medium' Method of Treating Electrostatic Theorems," *Philosophical Magazine*, 34 (1892), pp. 18-35.
[4] G. N. Lewis, "A Revision of the Fundamental Laws of Matter and Energy," *Philosophical Magazine*, 16 (1908), pp. 707-717; G. N. Lewis and R. C. Tolman, "The Principle of Relativity and non Newtonian Mechanics," *Philosophical Magazine*, 18 (1909), pp. 510-523.
[5] Lyman to Lowell, Dec. 17, 1910, PLDC, UA V 692.5.
[6] H. A. Lorentz, Hermann Minkowski und Albert Einstein, *Das relativitätsprinzip, eine sammlung von abhandlungen, mit anmerkungen von A. Sommerfeld und vorwort von G.*

Lorentz, Hermann Minkowski.[7] Probably this was the first time for him to see Einstein's celebrated paper, which included the seemingly operational way of defining simultaneity, namely, synchronizing clocks located at different places by exploiting the light signal. Bridgman would later consider this way of synchronization as an ideal model of operational definition of physical concepts.

Between 1916 and 1919, while he was engaged in war work in New London and later involved in the controversy over dimensional analysis, Bridgman was occasionally informed of the new topics related to the special relativity theory. In September 1916, for example, Tolman, then at the University of Illinois, wrote Bridgman that he had been "so frightfully busy [that] summer trying to finish up [his] book on the Theory of Relativity."[8] Tolman published his monograph on special relativity *The Theory of Relativity of Motion*[9] in 1917, which Bridgman read in the summer of 1919 and thought about making use of in his class.[10] In November 1919, an American physicist Jerome Alexander sent him a paper on the atomic structure of the ether to ask him for an opinion. Bridgman wrote about three other articles on similar topics he had noticed in *Science Abstracts*, but he was personally "inclined to give the ether rather less substantiality than" the authors of these papers did, regarding "the ordinary laws of mechanics as something to be explained in properties of the ether," and not expecting to find the contrary, namely, that the atoms of ether obey the ordinary laws of mechanics.[11] Although it remains unclear how seriously he was committed to the idea of the "ether" around that time, the last sentence

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Blumenthal (Leipzig: B. G. Teubner, 1913).

7 Bridgman to Nijhoff, Oct. 4, 1914, PWBP, HUG 4234.8.

8 Tolman to Bridgman, Sept. 6, 1916, PWBP, HUG 4234.8.

9 R. C. Tolman, *The Theory of Relativity of Motion* (Berkeley: University of California Press, 1917).

10 Bridgman to Tolman, Nov. 9, 1919, PWBP, HUG 4234.8.

of his letter to Alexander tells Bridgman's basic attitude as an experimentalist: "But after all, of course, all this is a matter of taste until an ether theory has been made to yield new results or points of view that can be tested by experiment."

Bridgman started to pay attention to general relativity in May 1919, when Arthur Eddington was observing the solar eclipse in view to confirming the prediction Einstein had made in his paper on general relativity. That month Bridgman ordered a reprint of Einstein's 1916 paper on general relativity[12] and other books related to this topic.[13] Six months later, at the meeting of the Physics Section of the American Academy of Arts and Sciences, he reported on the "Temperature Effect of Gravitation" with E. B. Wilson, a physics professor at MIT.[14] In January 1920, Tolman asked Bridgman to send him Einstein's 1916 paper[15] as Tolman was preparing a talk on relativity which was to come out in the *General Electric Review*.[16] Bridgman, who was busy in doing high-pressure experiment and finishing the monograph on dimensional analysis, did not have time to examine the paper for a while and allowed Tolman to keep it until the end of the summer.[17] Bridgman collected papers and books on general relativity, but had not given them a serious scrutiny until he could secure enough time to concentrate on the topic.

As the historian of science Albert Moyer has inferred, what seems to have triggered Bridgman's intense study on general relativity is his

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11 Bridgman to Alexander, Nov. 9, 1919, PWBP, HUG 4234.8.

12 A. Einstein, "Grundlage der allgemeinen Relativitätstheorie," *Annalen der Physik*, 49 (1916), pp. 769-822.

13 Bridgman to Nijhoff, May 14, 1919, PWBP, HUG 4234.8. The authors of the books included H. Weyl, E. Freundlich, and P. Lenard.

14 Lyman and E. B. Wilson to Holden, Nov. 13, 1919, PLDC, UA V 692.5. The document that tells the content of their talk has not been found.

15 Tolman to Bridgman, Jan. 26, 1920, PWBP, HUG 4234.8.

16 R. C. Tolman, "Relativity Theories in Physics," *General Electric Review*, 23 (1920), pp. 486-492.

listening to Einstein's talk at the American Academy of Arts and Science in May 1921.[18] By the following summer he read the books on general relativity including the English translation of Einstein's popular primer *Relativity: The Special and General Theory,*[19] published in 1920. In the spring of 1922, Bridgman arranged a set of conferences on relativity at the American Academy of Arts and Sciences, in which H. B. Phillips, a professor at MIT, gave three lectures on "Relativity and Gravitation."[20] Bridgman himself gave a talk on the logical structure of relativity theory at the AAAS symposium in the following December, in which he first used the word "operation" as a key notion for understanding the meaning of concepts in physics. Later, in his own recollection, he mentioned this talk: "I think the word *operation* was first explicitly used in a discussion that I gave at the Boston meeting of the AAAS in 1923 at a symposium on relativity theory participated in by George Birkhoff [a professor of mathematics at Harvard], Harlow Shapley, and myself."[21] This symposium actually took place in December 1922. Bridgman prepared for the talk a few unpublished papers on relativity that describe the process of the generalization and sophistication of his operational perspective. The following summer, Bridgman further examined relativity theory, reading books and papers on the topic and asking Ludwig Silberstein to send proof sheets of his monograph on

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[17] Bridgman to Tolman, Aug. 8, 1920, PWBP, HUG 4234.8.
[18] 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. 254. Einstein's visit was reported in *Proceedings of the American Academy of Arts and Sciences*, 56 (1920-21), p. 400.
[19] Albert Einstein, translated by Robert W. Lawson, *Relativity: The Special and the General Theory* (London: Methuen, 1920). The original German is, Albert Einstein, *Über die spezielle und die allgemeine Relativitätstheorie, gemeinverständlich,* 3 Aufl. (Braunschweig: Vieweg, 1918).
[20] Lyman to Phillips, Jan. 6, Jan. 11, Feb. 23, and Feb. 29, 1922, PLDC, UA V 692.5.
[21] P. W. Bridgman, "The Present State of Operationalism," in P. Frank ed., *The Validation of Scientific Theories* (New York: Coliar Books, 1961), p. 76.

relativity,[22] partly for the preparation for his class lectures and partly for his own interest. His fifty-two page typescript completed by September 1923 contains his attempt to apply his operational scrutiny to the traditional concepts of mechanics, such as space, time, velocity, mass, force, momentum, and energy. To understand how Bridgman formulated his operational principle through the struggle with relativity, it is crucial to analyze his papers and correspondence on general relativity between 1921 and 1923.

Bridgman's first typescript on general relativity was titled "Remarks on Generalized Relativity" and dated "Sept. 1 1921."[23] In this typescript he listed up all the papers and books on relativity theory he had read by that summer. Among them were: Einstein's popular book, *Relativity: The Special and General Theory*[24]; Eddington's *Space, Time and Gravitation*[25] and *Report on the Relativity Theory of Gravitation*[26]; pamphlets by Freundlich[27] and Lenard[28]; the fourth edition of Weyl's *Raum, Zeit, Materie: Vorlesungen über allgemeine Relativitätstheorie*[29]; and Leigh Page's article read in the Connecticut Academy.[30] After all Bridgman found that "[t]he only real source of information in all this is Einstein's 1916 paper," which he "worked

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22 Bridgman to Silberstein, July 29, 1923, PWBP, HUG 4234.8.

23 P. W. Bridgman, "Remarks on Generalized Relativity, Sept. 1, 1921," typescript, PWBP, HUG 4234.74.

24 Albert Einstein, translated by Robert W. Lawson, *Relativity: The Special and the General Theory* (London: Methuen, 1920).

25 Arthur S. Eddington, *Space, Time and Gravitation: An Outline of the General Relativity Theory* (Cambridge: Cambridge University Press, 1920).

26 Arthur S. Eddington, *Report on the Relativity Theory of Gravitation*, 2nd ed. (London: Fleetway Press, 1920).

27 Erwin Freundlich, *Die Grundlagen der Einsteinschen Gravitationstheorie*, 4., erw. und verb. Aufl. (Berlin: J. Springer, 1920).

28 Philipp Lenard, *Über Relativitätsprinzip, Äther, Gravitation*, 3. Aufl. (Leipzig: S. Hirzel, 1921).

29 Hermann Weyl, *Raum, Zeit, Materie: Vorlesungen über allgemeine Relativitätstheorie*, 4. erweiterte Aufl. (Berlin: Julius Springer, 1921).

30 Leigh Page, "The Principle of General Relativity and Einstein's Theory of Gravitation," *Transactions of the Connecticut Academy of Arts and Sciences*, 23 (1920),

completely through, and verified nearly all the equations."[31] To him, "[t]he difficulty of tensor analysis seems to be much over estimated."[32] Bridgman, however, found "many great difficulties" in the paper, which were "not of a mathematical but of a physical character."[33] Among them, what troubled him most was the requirement of the general theory that the form of the equations should be the same in different coordinate systems. He expressed his feelings toward this principle thus: "I am more and more convinced that all the talk about generalized coordinates is bunk, and that Einstein has been deluding himself by a metaphysical preconception."[34] He went on to discuss the relation between the requirement of general relativity and the principle of special relativity.

> In the special theory we demand that the same actual equations, *numerical* coefficients and all, shall describe natural phenomena in all allowable systems. In the general theory, we merely deomand [*sic*] that the form of the equations shall be the same in different coordinate systems, the coefficients being expressed as certain literal functions of the g's. The explicit equations, when written out numerically are not at all the same for different coordinate systems.[35]

Unlike the principle of special relativity, the principle of general relativity only required that the equations should be written in the same form in different coordinate systems. To Bridgman this requirement of the general theory seemed to afford no restriction on the possible form of equation:

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pp. 383-416.

[31] Bridgman, "Remarks on Generalized Relavitity," p. 2.

[32] *Ibid.*

[33] *Ibid.*

[34] *Ibid.*

[35] *Ibid.*

> For, notice in the first place, that the idea that we have something absolute in our equation is a delusion. It is an essential presupposition that the element of interval ds can be determined physically, and that in terms of this the g's of any system of coordinates can be determined. Now to find ds we have in the first place to find a system of coordinates in which the special theory of relativity is applicable, and then having found this system we are to make our measurements according to the ordinary procedure. That is, the g's for any system of coordinates are referred back to [instead of "referred back to," Bridgman first put "defined in terms of"] a perfectly definite set of physical operations, which are the same for all coordinate systems. We have here nothing absolute; we have reduced all measurements to depend on a certain invariable procedure, and the definitions at the basis of this invariable procedure constitute an element of arbitrariness which is contained in any system of coordinates.[36]

In Bridgman's interpretation, "[w]e have, it is true, got rid of a certain amount of arbitrariness due to the particular mesh system used, but we can never get rid of the dependence of the results on the rules of operation by which we determine ds."[37] In any coordinate system, one should employ the invariable set of operations to determine the element of interval *ds*. Bridgman criticized that although freeing physical laws from dependence on any specific coordinate system, the principle of general relativity told nothing about their dependence on this invariable set of operations: "With the recognition that there do exist certain definite physical rules of procedure which mus[t] ultimately by [be] employed in connection with any system of coordinates whatever, I find it most difficult to see how the postulate of general invariance can give any restriction."[38]

Bridgman found "Eddington's ecstatic contemplation of the absolute vision which is afforded by the new theory" to be "pure tommy

[36] *Ibid.*, p. 3.
[37] *Ibid.*

rot."[39] In his *Space, Time and Gravitation,* Eddington detailed the way one could grasp the meaning of "absolute" through relativity theory:

> All physical knowledge is relative to space and time partitions; and to gain an understanding of the absolute it is necessary to approach it through the relative. The absolute may be defined as a relative which is always the same no matter what it is relative to. Although we think of it as self-existing, we cannot give it a place in our knowledge without setting up some dummy to relate it to. And similarly the absolute differences of space always appear as related to some mesh-system, although the mesh-system is only a dummy and has nothing to do with the problem.[40]

Bridgman also sought for the absolute in physics. However, unlike Eddington, he did not think that the principle of general relativity brought anything absolute.

Bridgman could accept the special theory of relativity with no difficulty as it was based on what was operationally realizable. On the other hand, he did not swallow general relativity as this notion seemed to him to be related only to forms of equations. In his understanding, the general theory specified nothing about actual physical procedures, though the theory tacitly depended on them in deriving equations and drawing concrete results. He was certain that he could not tolerate this structure of the general theory, though he still did not make clear what kind of physical theory he could regard as ideal.

Furthermore, Bridgman was probably dismayed by the apparent change in Einstein's way of constructing his theory that took place during the period between the special and general theories of relativity.

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[38] *Ibid.*

[39] *Ibid.*

[40] A. Eddington, *Space, Time and Gravitation: An Outline of the General Relativity Theory* (Cambridge, England: Cambridge University Press, 1920). Citation is from a later edition (New York: Harper & Brothers, 1959), p. 82.

In his 1905 paper on special relativity, while Einstein almost unnecessarily detailed the way in which actual measurements were carried out, in his 1916 paper this tendency totally disappeared. In the general theory, Bridgman could find nothing similar to what he understood as essential to the special theory.

In the September 1921 typescript, Bridgman reached no remarkable conclusion. He finished his reflection by reiterating his "crying need" "to work through the simple case of central symmetry, visualizing the relations, and trying to find how much can be deduced from the equivalence hypothesis alone and special relativity [that is, without general relativity], and if some other hypothesis is necessary, to express its physical import in simple terms, without the use of generalized coordinates, for this special case."[41] The summer vacation was about to be over; all the problems were left for the next summer.

The following September and October, Bridgman exchanged several letters with his old friend and one of American experts in the relativity theory, R. C. Tolman, as "[t]here is no one in Cambridge from whom I can get any satisfaction." [42] For example, Birkhoff at the Department of Mathematics at Harvard was "so remote in his clouds of mathematics that [Bridgman] can get nothing physical from talking with him."[43] Tolman was helpful. To Bridgman's discovery that general relativity required only that "no matter what the laws of physics are they can necessarily always be expressed in a *form* which will be invariant for any transformation of coordinates," Tolman replied that a German physicist Kretschmann had pointed out the same aspect of the principle of general relativity and that Einstein had agreed with him.

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[41] Bridgman, "Remarks on Generalized Relativity," p. 8.
[42] Bridgman to Tolman, Sept. 22, 1921, PWBP, HUG 4234.8.
[43] *Ibid.*

He referred Bridgman to their articles in the *Annalen der Physik.*[44] Tolman also explained to Bridgman that to reach specific results in the gravity theory, Einstein had postulated two elements other than the generalized principle of relativity: the principle of equivalence that "the laws of gravitation must be such that it is impossible to pick out a system of coordinates with reference to which the laws of motion will be the same as in free space in the absence of any gravitational field"; and the Mach principle that "the nature of the gravitational field will be determined by the distribution of matter in the neighborhood."[45]

As Tolman wrote to Bridgman, Erich Kretschmann had published a paper suggesting that the principle of relativity had no physical meaning.[46] Einstein had agreed with him that this principle was basically related to the mathematical formality of physical laws, though emphasizing its importance in the investigation of the axioms of physics.[47] Moreover, Einstein admitted that two other principles, the principle of equivalence and the Mach principle, played a main role in the physical discussion. Bridgman, who had not known these articles until Tolman suggested them, was glad to know Einstein's own comment on general relativity.[48]

By the next summer, Bridgman had read new books and articles on general relativity, whose authors included Ludwik Silberstein,[49] Max

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[44] Tolman to Bridgman, Sept. 27, 1921, PWBP, HUG 4234.8.
[45] *Ibid.*
[46] Erich Kretschmann, "Über den physikalischen Sinn der Relativitätstheorie," *Annalen der Physik*, 53 (1917), pp. 575-614.
[47] A. Einstein, "Prinzipielles zur allgemeinen Relativitätstheorie," *Annalen der Physik*, 55 (1918), pp. 241-244.
[48] Bridgman to Tolman, Oct. 8, 1921, PWBP, HUG 4234.8.
[49] Silberstein published the first edition of *The Theory of Relativity* in 1914 which, of course, did not include the discussion on the general theory. The second edition of the same book which included the general theory was published in 1924 (*The Theory of Relativity* (London: Macmillan, 1924)), and probably Bridgman received part of its draft before the publication.

von Laue,[50] and Ebenezer Cunningham.[51] Furthermore, Bridgman had worked through Einstein's comment on general relativity and was convinced that "the generalized principle of relativity by itself can yield no results, and that its use merely put Einstein in a position to make a better guess about what sort of equations would most simply degenerate into the Newtonian equations."[52]

However, the typescript completed on August 27, 1922 does not show any remarkable progress in Bridgman's criticism of the general theory of relativity. He only realized that the point at which he found it hardest to make physical contact with the theory was "in identifying the mathematical coordinates with the physical cones."[53] He therefore examined the actual measurements in astronomy and Einstein's discussion on measurement by a stationary meter stick or a stationary clock. This attempt seems to have produced no result, though one note Bridgman put in the typescript tells that while discussing measurements in astronomy with the astronomer Harlow Shapley, Bridgman learned that "no astronomical distances are known to better than 0.1 %."[54]

Four months later, toward the end 1922, Bridgman gave a talk at a symposium on space and time in the mathematical and astronomical section of the AAAS. By then, Bridgman had made an impressive step toward the construction of his operational view. The abstract and preliminary draft of his talk contains many fundamental concepts

[50] Max von Laue, *Die Relativitätstheorie*, 2v., 4. vermehrte Aufl. (Bd. 1, Die spezielle Relativitätstheorie, Bd. 2, Die allgemeine Relativitätstheorie) (Braunschweig: Vieweg, 1921).
[51] Ebenezer Cunningham, *Relativity, the Electron Theory and Gravitation* (London: Longmans, Green and Co., 1921). The first edition of this book did not discuss the general theory.
[52] P. W. Bridgman, "Remarks on Generalized Relativity, August 27, 1922," typescript, PWBP, HUG 4234.74, p. 1.
[53] *Ibid.*, p. 2.
[54] A handwritten comment by Bridgman, *ibid.*, p. 2.

similar to the ones he would later detail in *The Logic of Modern Physics.* Having given up an effort to reconcile himself with general relativity and started to regard it as physically unacceptable, Bridgman for the first time described the special theory as furnishing physicists with the method to find out and define the meaning of concepts in physics by using operations. In the first several lines of the abstract, Bridgman put an idea of defining concepts in the operational way:

> Restriction to physical concepts.
> Increased criticalness of fundamental concepts - largely due to Einstein.
> Possible failure of E's theories to be final
> Insistence that terms have a meaning.
> Quantities capable of measurement involve a procedure - rules of operation. These operations must be physically realizable.
> The concept *is* the complex of operations.
> Simple applications - no abs. [absolute] direction in space - no abs. velocity[55]

One can see that Bridgman developed the lessons of Einstein's special theory of relativity into a way to define scientific concepts by physically realizable operations. In the rest of the abstract, he went on to discuss how actual measurement of length and time worked as the operational definition of concepts.

The fifteen-page typescript of preliminary essay describes Bridgman's standpoint more thoroughly. Beginning his essay by his doubt whether "the theory of Einstein in its present form will ultimately survive,"[56] Bridgman believed that "entirely apart from the ultimate truth of the theory, there can be no question that matters can never

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[55] P. W. Bridgman, manuscript, "Abstract of Talk at Symposium on Space & Time, Math & Astron. Sect. AAAS, Boston, Dec. 27, 1922," PWBP, HUG 4234.74. There is a comment by Bridgman: "This was actually given."
[56] P. W. Bridgman, "Preliminary for Space & Time Symposium, Dec 27, 1922," PWBP,

return to their condition before the formulation of the theory" because "Einstein has made changes in our points of view which must have their permanent effect." [57] Bridgman observed that Einstein's "most important service" was "the insistence on the requirement that the quantities (or concepts in general) which we use in our equations have a meaning."[58] To Bridgman, Einstein seemed to insist that the meaning should be operational.

> It is obvious that in any exact formulation of a relation we are concerned with equations between numbers which are the numerical measures of various physical properties the the [*sic*] system under discussion. It is at once obvious, merely on saying it, that if the quantities which appear in our equations are to mean anything, we must be told the method by which we find the number measuring the property in any concrete case. In other words, we must be told the operations by which the numbers which enter our equations are determined. These operations must be physically realizable.[59]

Bridgman further contended that such concepts as length or time could have no meaning "unless the operations [were] specified by which concrete lengths and times [were] to be measured," and that a close examination showed that "what the physicist means by length is essentially merely the complex of operations by which he measures what he calls a length."[60] "If the operations of measurement change," he went on, "then the quantity obtained is no longer a length in the old sense, but something else, which must in rigor receive a new name, 'Length 2.'"[61]

HUG 42.34.74, p. 1.
[57] *Ibid.*
[58] *Ibid.*, pp. 1-2.
[59] *Ibid.*, p. 2.
[60] *Ibid.*
[61] *Ibid.*

Bridgman made similar remarks with regard to the concept of time. In this case he emphasized the importance of Einstein's discussion in the special theory. Bridgman suggested that for the physicist "who can only measure intervals, which he calls time, between events, by menas [*sic*] of definite rules of operations, involving the use of instruments which are called clocks," the situation was similar to the measurement of length.[62] However, there was one thing that had not attracted attention until Einstein pointed it out.

> It was a most important contribution of Einstein to attempt to more careful analysis than had hitherto been made of the operations by which we determine the interval of time between events occurring at different places, and in particular how we determine whether this interval is zero, or whether the events at different places are simultaneous. And every one knows that the result of this careful analysis of the physical operations was the discovery that it is not possible to establish a procedure which gives a unique answer to the question of the order in time of events at different places, but that the answer depends on the frame of reference used as the starting point.[63]

Bridgman thought that the most essential part of the special theory of relativity that led to its startling results was Einstein's apparently operational way of synchronizing clocks at different places, or his operational definition of simultaneity. Later in the typescript, he repeated almost the same statement concerning Einstein's definition of simultaneity: "It was of course a great service of Einstein to call attention to the fact that a comparison of times at two remote places involves an extension of the time concept, as it involves an extension of the physical operations by which time is measured."[64] For Bridgman,

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[62] *Ibid.*, p. 3.
[63] *Ibid.*
[64] *Ibid.*, p. 12.

"time is a relative concept, relative to the rule of operation by which it is measured."[65]

In his 1905 paper on special relativity,[66] Einstein explained how to synchronize two clocks at different places in the following way. Suppose that there are two clocks at different places, A and B. According to the invariance of the velocity of light which has already been *postulated* previously in the paper, the time required by light to travel from A to B equals the time it requires to travel from B to A. Now, let a ray of light start at the "A time" t_A from A for B, be reflected by a mirror at the "B time" t_B at B in the direction of A, and arrive again at A at the "A time" t'_A. Einstein defined that the two clocks were synchronized if

$$t_B - t_A = t'_A - t_B .$$

With clocks located at different places and synchronized in the way thus explained, simultaneity of events taking place at different places can easily be defined.

In thus defining simultaneity, Einstein adopted a seemingly operational way of defining concepts, presenting actual operations connected with the concept of simultaneity. Bridgman regarded this definition of simultaneity as the essential part of special relativity, focusing his attention solely on this point in analyzing relativity theory. He formulated the platform of his operationalism while examining Einstein's definition of simultaneity and praised it as the most remarkable example of the operational definition.

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[65] *Ibid.*, p. 3.
[66] A. Einstein, "Zur Elektrodynamik bewegter Körper," *Annalen der Physik*, 17 (1905), pp. 891-921.

It is, however, not difficult to grasp that Einstein's own stance in constructing physical theory was far from what Bridgman considered as ideal, when one closely analyzes Einstein's definition of simultaneity. Einstein *postulated* the invariance of the velocity of light before defining the concept of simultaneity, without any operational discussion concerning the speed of light. The apparently operational definition of simultaneity essentially relies upon this postulate of the invariance of the light velocity; without presupposing it, Einstein could not have shown the way of synchronizing two clocks. In fact, the reason for the success of the relativity theory, and probably the most elaborate part of the theory, was this postulate, the invariance of the velocity of light, and the principle of relativity, which involved no operational definition but were only postulated as principles. As we will see later, though Einstein presented the apparently operational discussion in his special theory, he would not approve Bridgman's interpretation of special relativity.

In the December 1922 essay, following his own interpretation of relativity theory, Bridgman went on to examine the operational details of measurements of length and time. He took up length first, listing several ways of measuring length: measurement by rigid meter sticks; measurement by light signals; measurement by triangulation; and a theoretical estimation of microscopic length, such as the diameter of electron. Bridgman then carefully scrutinized the validity of each method. In analyzing astronomers' way of measuring of length, that is, measurement by triangulation, he exposed what he had previously learned from Shapley: "The distance of the sun is obtained by a triagnulation [*sic*] from opposite sides of the earth's diameter, and no distance in astronomy is known with greater accuracy than this, which has an error of possibly 0.1%."[67] To Bridgman, this fact meant that "there is no a-priori reason for supposing that distance measured" in one way "is the same as distance measured" in another way.[68] As for measurement by light signals, he thought that the "ideal way of doing this is to send a signal to the distant object and reflect it back and obtain the distance from the elapsed time, assuming the velocity of light propagation constant,"[69] although it had never been done by astronomers. It is notable that Bridgman neither examined this type of measurement further nor questioned the invariance of the velocity of light. After comparing these ways of measurement, he found that one could not define even such a simple concept as length with satisfactory rigor and independence of other concepts.

> It seems to be a general fact that none of the so-called elementary physical concepts can be cleanly defined without bringing in a penumbra of other concepts. Even the ordinary procedure of measuring length with measuring rods involves the concept of time to a certain extent, for we demand that observations be made at the two ends of the measuring stick at the same instant of time, and we have hitherto been forced to regard the concept of simultaneity in a small neighborhood as one of irreducible simplicity. The extent to which other concepts are interwoven with those that we regard as simple without doubt must be recognized as depending on the scale of magnitude on which we are working, and it is most questionable if the concept of length for very large or very small distances can ever be as [*sic*] made as clean and self contained a thing as for moderate distances.[70]

He argued that the situation at the limits of experimental knowledge could be different from the one in the ordinary life, and that "there must

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67 Bridgman, "Preliminary for Space & Time Symposium, Dec 27, 1922," p. 6.
68 *Ibid.*, p. 5.
69 *Ibid.*
70 *Ibid.*, pp. 8-9.

be a fusion of various concepts, which can only be resolved by new empirical knowledge." [71] One can understand that against his anticipation, the actual application of Bridgman's way of determining the operational meaning of concepts did not always bring a satisfactorily clear result. Many times he found one physical concepts interwoven with others.

In the course of argument, Bridgman found a type of concepts to which he could not assign the corresponding operations. An example was the mathematical concept of continuity, which could "obviously never have a counterpart in physical operations." [72] Therefore, although considering it as possible that "the physicist may some day be able to prove that space is discontinuous," Bridgman expected that "the proof that it is continuous can never be given, and until the discovery of discontinuity, the assumption of continuity is only a fiction, adopted for convenience in the manipulation of mathematical approximation to the reality, and without physical meaning." [73] In writing thus, he was bearing in mind the recent development in physics, as his handwritten note tells: "Will not the first approach to discontinuities manifest itself in incorrect result when taking the derivative? Diff [Differential] equations break down first—Quanta & light. No retarded pot [potential] in small orbits." [74] In the light of quantum theory, he suspected that some day the mathematical continuity might become inapplicable to physical reality.

After discussing the concept of time briefly, Bridgman turned his analysis to Einstein's general theory of relativity. He repeated the same criticism as he had done the previous year, only more systematically.

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71 *Ibid.*, p. 9.
72 *Ibid.*
73 *Ibid.*
74 *Ibid.*

Bridgman noticed Einstein's intense satisfaction that he took in his generalized principle of relativity, "a principle which frees our results from the accident of any coordinate system and gives complete formal relativity to our results."[75] However, Bridgman doubted whether the general theory had really attained the complete relativity, since the *ds*, the element of space-time interval, "must be determined by actual physical operation."[76] These actual operations were "always the same," but "might conceivably be different":

> If for instance, we had put at the basis of our physical determination of lengths some other operation approximately equivalent to it, the ds so found might be essentially different in a strong gravitational field, and the results would be entirely different.[77]

In criticizing general relativity, Bridgman now applied his latest discovery that the operational meaning of length could vary from situation to situation. Bridgman found that the general theory of relativity depended on the presupposition that there was always an invariant scheme of operations independent of the coordinate system. Einstein, in Bridgman's observation, had overlooked this fact when he thought that he had attained general relativity of his theory: "although the coordinate system may be arbitrary there is always an invariant scheme of physical operations independent of the corrdinate [*sic*] system, the results of which cannot eliminate themselves from the final result."[78] Furthermore, Bridgman again doubted whether the principle of general relativity was really responsible for the successful results of the general theory of relativity.

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75 *Ibid.*, p. 13.
76 *Ibid.*
77 *Ibid.*
78 *Ibid.*

By the end of 1922, through detailed examination of general relativity, Bridgman had obtained a large part of the elements of his operational philosophy: the operational way of defining concepts in physics; the interpretation of Einstein's definition of simultaneity as a model of operational definition; dependence of the meaning of concepts on operations; and uniqueness of operations that define a concept. He had also applied his idea to the concepts of length and time and had reached some important results: the discovery that even such a simple concept as length or time could be defined by different operations at different scales; the recognition that "penumbra" of other concepts was sometimes involved in a detailed operational definition; the possible fusion of concepts at extreme experimental limits; and difference between mathematical and physical concepts.

As Bridgman became more self-conscious of his operational stance, he tended to judge the general theory of relativity as operationally unacceptable. Bridgman's effort to comprehend the logical structure and physical meaning of general relativity that had started in the summer of 1921 led him to discover and formulate his own operational standpoint. Through the struggle with general relativity, he started to recognize his operational perspective more self-consciously than before. Then, with the help of the special theory, he formulated it as a criterion for the operational validity of physical concepts. Invoking this criterion, he scrutinized the foundations of contemporary physical theory, including general relativity. By the end of 1922, he had almost reached what would later be called operationalism; it was only that his main concern at this stage was analysis of general relativity, not systematic presentation of his own philosophy of physics.

Meanwhile, Bridgman did not examine the special theory of relativity as intensely as the general theory. Bridgman did not feel it necessary to analyze the special theory, probably because it fitted to his basic stance. Special relativity, which furnished him with examples of the operational definition of physical concepts, was to be left unexamined for quite a long while after this formative period of his philosophy.

Bridgman often admitted that his standpoint in operational analysis was originated in his discussion of dimensional analysis. However, what actually first led him to analyze the foundations of physics was relativity theory. In 1914, more than a year before he participated in the debates over dimensional analysis, he started to pay a serious attention to relativity theory while preparing for the new courses in electromagnetism. In scrutinizing the validity of dimensional analysis, Bridgman already invoked the operational viewpoint, though not self-consciously. It is inferable that the encounter with special relativity had stimulated him to maintain the operational perspective before he entered into the dimensional debate.

During the controversy over dimensional analysis, Bridgman did not have time to examine special relativity. After this controversy was over, he started to scrutinize the general theory and rediscovered the significance of the special theory of relativity as a typical operational theory. In the typescript completed in the summer of 1921, he mentioned only a part of the special theory that had some connection with actual operations. It was in his talk given at the end of 1922 that he explicitly used Einstein's definition of simultaneity as an example of his operational definition.

To Bridgman, the special theory of relativity continued to seem to be compatible with his operational view of science. It ignited his interest in the foundations of physics and then furnished him with suitable examples of operational definition of concepts. Because of this compatibility between special relativity and the operational perspective as he understood, he felt no need for intense operational scrutiny of this theory until the last years of his life, when, as we will see later, he would have to find grave discrepancy between what he expected the theory to be and what it actually was.

4.1.3. Criticizing Relativity Theory

At the end of the summer of 1923, Bridgman completed a fifty-two page essay that shows a further progress toward the formulation and generalization of his operational platform.[79] In the essay, Bridgman clearly expressed his intention to reform contemporary physics by following the method that he understood Einstein had shown. Bridgman wrote that "[t]he general goal" of the essay was "to make impossible a repetition of the thing that Einstein had done," since "never again should a discovery of new experimental facts lead to a revision of physical concepts simply because the old concepts had been too naive."[80]

> Our concepts and general scheme of interpretation should be so broad and so well considered that any new experimental facts, not inconsistent with previous knowledge, may at once find a place waiting for them in our scheme. A program of consideration as broad as this demands a critical examination not only of the concepts of space and time, but of all the other physical concepts in our armory. I intend in the following to wander over this whole broad field of criticism.[81]

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[79] P. W. Bridgman, manuscript, "Critical Discussion of Relativity," September 1923, PWBP. HUG 4234.74.
[80] *Ibid.*, p. 2.
[81] *Ibid.*

He understood that Einstein had reformed a part of physics by introducing the operational way of defining or re-defining physical concepts, typically exemplified by his definition of simultaneity. Bridgman was ambitious enough to attempt to apply the same method to other physical concepts and broaden the effect of the revolution caused by relativity theory. Though the title of this essay was "Critical Discussion of Relativity," Bridgman intended in it to outline his program of reforming physics.

Bridgman took over most of the fundamental ideas of the program from his previous reflections on relativity theory. He reiterated that "no concept is to be admitted which does not bring with it its complex of operation," or that "in fact unless there is the complex of operations the concept has no meaning."[82] Furthermore, he again admitted that "concepts are in their nature only approximate things" as "all physical operations are only approximate and can be specified only approximately."[83] As for the question of how one could choose suitable operations to define a concept, he maintained that "we may make our measurements in any way that is convenient or useful in the coordination of experience."[84] However, Bridgman noted that "the operations are not entirely arbitrary": "external nature gets in its gear in some way as we have to recognize when we find that only a limited scheme of operations are convenient in our attempts at coordination."[85] This is how Bridgman understood external nature constrained scientific discourse.

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82 *Ibid.*, p. 3.
83 *Ibid.*
84 *Ibid.*, p. 4.
85 *Ibid.*

Furthermore, Bridgman discussed "the almost irresistible impulse for the physicist to 'explain' his phenomena."[86] He assumed that explanation was "the reduction of all phenomena to a recognition of relations which are entirely similar to the simplest phenomena," and that the reduction was made possible by invention of a "mechanism."[87] Those "mechanisms" included, for instance, atoms and ether. Then he asked: "[W]hat happens to our demand for explanatin [*sic*] when we are confronted with a new order of physical facts which we had not hitherto suspected, as we are now in the realm of quantum phenomena?"[88] His answer was as follows:

> An examination of what is actually being done in such fields will give the clue to what we would consider satisfactory. It will be seen that all that we can do is to correlate the facts, to endeavor to find certain rules, as simple as possible, so that when we are given one situation we can predict what will follow, or what other situations may be expected along with it. The rules need have no connection with the rules which we have previously used. Since the rules are different, the invention of a mechanism loses the insistence of its demand. In fact, I believe that ultimately, when we go far enough beyond our homely experience, we are in the very nature of our mental processes compelled to give up the invention of mechanisms in the ordinary sense. Ultimately all that we can do is to correlate facts by means of definite rules, and an explanation will involve the discovery in a new group of facts of the operation of the same rules which we have previously found in another group.[89]

One can see that Bridgman was keenly aware of what was happening in the frontiers of physical research while preparing his scheme of operational scrutiny. In fact, he intended not only to reform contemporary physical theory but also to imply how to solve the

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[86] *Ibid.*
[87] *Ibid.*, p. 5.
[88] *Ibid.*

problems scientists were facing in atomic physics. He was expecting that his operational program would show how to achieve satisfactory results in quantum physics and would lead to an explanation of microscopic phenomena without relying on any mechanism.

In the rest of the essay, Bridgman examined the operational background of traditional concepts in mechanics such as velocity and mass, as well as length and time on which he had previously tried his operational scrutiny. Although he was to develop a still larger part of the discussion in *The Logic of Modern Physics*, he treated some topics only in this essay and left them without giving further examination. Among the latter, two lines of discussion, one on the "correlation between phenomena on the earth and the fixed stars"[90] and the other on the invariance of the velocity of light, are worth our attention, since both of them have much to do with special relativity and therefore show Bridgman's first attempt to examine its foundations.

Bridgman's discussion on the correlation between phenomena on the earth and the far-away fixed stars of the universe was related to the first postulate of the special theory of relativity, namely, the principle of relativity that "[g]iven a system S in which the ordinary Gallilean mechanics holds, then the laws of nature in this system S are exactly the same as in any other system S' moving with uniform motion with respect to it."[91] Bridgman pointed out that "the laws of nature" could not mean all phenomenal behavior as "the fixed stars are moving with

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[89] *Ibid.*, pp. 5-6.
[90] *Ibid.*, p. 7.
[91] *Ibid.*, p. 7. Originally, the principle of relativity was put in this form: "The same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good." A. Einstein, "Zur Elektrodynamik bewegter Körper," *Annalen der Physik*, 17 (1905), p. 891 (The English translation is cited from, H. A. Lorentz, A. Einstein, H. Minkowski and H. Weyl, *The Principle of Relativity*, trans. W. Perrett and G. B. Jeffery (New York: Metheuen and Company, Ltd., 1923), p. 37.)

different velocities in the two systems."[92] "If," he argued, "the fixed stars are really essentially concerned with doings here, then the two systems are not alike."[93] However, he found it "an undenied fact" that the effect of the fixed stars "can be neglected in all the phenomena to which the classical relativity, as ordinarily understood, applies."[94] Bridgman noted that the key to understanding this situation was the fact that the effect of the translational motion of the earth was negligible when compared with that of the rotational motion. In his paper published the next year, Bridgman explained the same point thus: "the earth in rotating about its axis runs through the entire possible range of coordinates fixing its orientation in the universe in 24 hours, whereas in 24 hours its translational motion has changed its relative position in the universe by something of the order of 10^{-12}, taking the diameter of the stellar universe as 300,000 light years."[95] Therefore, he judged that the principle of relativity was physically justified, but was "logically difficult to justify."[96]

In the part of Bridgman's 1923 essay dealing with the operational definition of velocity,[97] one can find the other discussion on the foundations of the special theory of relativity. Bridgman pointed out that operationally there were two kinds of velocity distinguished by the ways of measuring them. One is the unidirectional velocity measured "by dividing the length passed over by the time required to pass over it."[98] In order to measure the unidirectional velocity, "the ability to compare the times of two events at two different points of space" is

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[92] Bridgman, "Critical Discussion of Relativity," p. 7.
[93] *Ibid.*
[94] *Ibid.*, p. 8.
[95] P. W. Bridgman, "A Suggestion as to the Approximate Character of the Principle of Relativity," *Science*, 59 (1924), pp. 16-17.
[96] *Ibid.*, p. 16.
[97] Bridgman, "Critical Discussion on Relativity," p. 37-41.
[98] *Ibid.*, p. 37.

required. Bridgman thought that this could be done "in several ways," but assumed that "when we make no further specification we always understand time measured by clocks set by light signals a [*sic*] la Einstein."[99] The other velocity is the "go and come" velocity measured by only one clock and therefore does not involve the process of synchronizing clocks. Bridgman noticed that there was an observation that, as for the velocity of light, "all we ever observe experimentally is the go and come time of light, and that the unidirectional velocity has never been measured."[100] The velocity of light is in principle measured by dividing the doubled distance between two points, say, A and B, by the time the light takes to travel from A to B, where the light is to be reflected, and then travel back from B to A. To measure the unidirectional velocity of light, it is necessary to find out a way to synchronize clocks at different places without making use of the light signal. As has previously been mentioned, Bridgman believed that it was possible to find such a way to synchronize clocks and to measure the unidirectional velocity of light.

> Of course if we try to measure the unidirectional velocity, and I believe that it is not a hopeless experiment, we must set our clocks at the two stations by some other means than the optical means adopted by Einstein, for we have seen that his method of clock synchronization is such that the velocity must be the same in both directions. Some mechanical method of setting clocks must be devised, which the naive intelligence will feel is satisfactory, and then we can try for the unidirectional velocity, using of course the mathematical definition of velocity. Of course in all strictness the query whether the velocity is the same in both directions has no meaning until we have attended to the specification of a method of synchronizing clocks.[101]

[99] *Ibid.*
[100] *Ibid.*, p. 40.
[101] *Ibid.*, pp. 40-41.

Though he presented no concrete way to synchronize clocks, Bridgman clearly understood in the summer of 1923 that synchronization of clocks was inextricably connected with measurement of the unidirectional velocity of light. However, he did not develop this discussion any further and missed a chance to notice possibility that the special theory might not be constructed in the way he regarded as operational. As will briefly be discussed later, decades later, when he resumed his reflection on the special theory of relativity, he would overlook this crucial connection between the unidirectional velocity of light and the synchronization of clocks and would repeat an almost hopeless attempt to find the operational meaning of the unidirectional velocity of light.

In the 1923 essay, Bridgman did not discuss the above discussed problems further. Concerning relativity theory, his main concern was with two other points: that Einstein's contribution to physics was in his recognition of need to define concepts in terms of actual operations; and that the structure of the special and general theories of relativity, especially that of the general one, was not rigorously consistent and therefore needed to be revised. Not attempting to carry on further analysis, Bridgman articulated the former point on different occasions and quoted it as the basis of his operational perspective; however, all the more for its significant position in his philosophical scheme, it was to go unexamined until the end of his life. As for the latter, though recognizing the importance of the revision of relativity theory, Bridgman did not do it by himself. He was excited with another challenging task, reforming physics by applying the operational analysis.

After the formative period of operational viewpoint, Bridgman could not devote much time and energy to the examination of relativity theory until the late 1950s, though he occasionally wished he could. In May

1924, upon returning from the Solvay Conference, Bridgman thanked Silberstein for affording the proof sheets of his book on relativity that he had read on the steamer to Europe, writing that "I wish very much that I had time to go into the subject seriously."[102]

> Everytime that I read a presentation of the subject, and particularly after reading your careful exposition of the various assumptions that have to be put in to the theory, I become more convinced that there is room for a great deal of serious work in the very foundations of the subject. It is unfortunate that this sort of work is rather unpopular, and by many physicists is looked down on as savoring of metaphysics.[103]

After the 1920s, experimentalists started to feel it difficult to concentrate on theoretical matters while taking care of their laboratory work. Bridgman deplored that many physicists would dismiss his philosophical concerns with relativity as metaphysics, but did not have enough time to take them up by himself.

Yet, from time to time Bridgman obtained a chance to reflect on this topic at least briefly. From his short comments on relativity, one can understand that he kept maintaining almost the same recognition as to Einstein's contribution to physics and the structure of relativity theory. After the publication of *The Logic of Modern Physics* in 1927, those who were interested in the foundations of physics started to write him to ask for advice on questions they had. Some of them provoked Bridgman's interest in relativity theory. On April 8, 1928, Bridgman wrote to E. B. McGilvary, a professor of philosophy at the University of Wisconsin, Madison: "[I]t would be a great mistake to think of Einstein's theory [in this case, the special theory] as a logically consistent structure; it is very far from this, and is on the contrary lousy with

[102] Bridgman to Silberstein, May 25, 1924, PWBP, HUG 4234.8.
[103] *Ibid.*

implicit physical assumptions."[104] Then he went on to describe how Einstein implicitly relied on the physical assumptions in discussing special relativity. Two months later, he wrote McGilvary about physicists' opinion of the general theory: "The essential point, as far as the physicist is concerned, (I do not attempt to speak for the philosophers who have written on relativity) is that the four-dimensional geometry of relativity is not taken as seriously as you intimate."[105]

Around the same time, Bridgman explained almost the same point to James McKaye at Dartmouth College, Hanover, New Hampshire, stating that "[y]our contention that the difference between relativity and non-relativity may be in large part verbal would be subscribed to, I believe, by most physicists."[106] He agreed with McKaye that Einstein had used the terms space, time, and velocity in senses different from ordinary ones, turning attention to the fact that "the essential contribution of Einstein lay in the clear recognition that the experimental situation is such as to make the use of these terms in new senses not only possible, but desirable." In the autumn of the same year, Bridgman discussed the vulnerability of general relativity with his faithful correspondent, Korzybski, repeating his favorite criticism of general relativity that "Einstein's own conception of the role which his use of generalized coordinates plays cannot stand the test of criticism, and that really nothing can be gained in this way."[107] Furthermore, Bridgman blamed Einstein for his attitude toward the criticism: "Einstein has actually admitted in print the justness of this criticism when he has been pushed to it, but it is not a congenial admission, and he slips away from it when the pressure is relaxed." As he explained in

[104] Bridgman to McGilvary, April 8, 1928, PWBP, HUG 4234.12.
[105] Bridgman to McGilvary, June 9, 1928, PWBP, HUG 4234.12.
[106] Bridgman to McKaye, June 6, 1928, PWBP, HUG 4234.8.
[107] Bridgman to Korzybski, Oct.17 [?], 1928, PWBP, HUG 4234.12.

another letter to Korzybski, Bridgman's criticism of general relativity had undergone no change since 1921:

> [M]y chief objection to Einstein&s [*sic*] treatment of generalized coordinates is that I do not believe that he correctly describes what he really has done. One gets the impression from Einstein that his requirement that the laws of nature be in a form independent of the choice of coordinates in some way limits the possible forms of those laws and plays an essential part in his results, whereas if one examines what Einstein actually did, he will find that this requirement actually played no part at all. And my private opinion is that it could play no part.[108]

Bridgman believed that he understood the significance and meaning of relativity theory better than Einstein.

Not just criticizing Einstein, Bridgman even attempted to revise the logical structure of relativity theory. In 1934, stimulated by Tolman's *Relativity, Thermodynamics and Cosmology*,[109] published that year, he analyzed the flow of gravitational energy between two gravitating masses held apart by cords attached to a frame, with an ambition "to some day work out a general theory of relativity starting from the consideration of just such simple situations as this."[110] Though he consulted with two experts then at Harvard, M. Vallarta and G. Birkhoff, neither of them could give him a satisfactory answer. Bridgman then wrote to Tolman to discuss this problem, explaining why he started such an attempt:

> It seems to me that something ought to be possible that would appeal much more to the blacksmith type of physicist like myself, and at the same time, make all the connections with

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108 Bridgman to Korzybski, Nov. 4, 1928, PWBP, HUG 4234.12.
109 Richard C. Tolman, *Relativity, Themodynamics and Cosmology* (Oxford: The Clarendon Press, 1934).
110 Bridgman to Tolman, Dec. 8, 1934, PWBP, HUG 4234.10.

experiment that gravitational theory now does, although perhaps not coinciding with present gravitational theory in higher order terms or being so elegant mathematically.[111]

Tolman, who was not encouraging, pointed out the difficulty of Bridgman's attempt:

> It may be very difficult to build up gravitational theory by the consideration of "simple" physical situation such as you suggest, since many of these situations may only remain simple so long as you apply the older stock of physical notions to them, and become very complicated as soon as you try to find the essentially new ideas which will be needed for the treatment of gravitation. If you consider the series
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> | | | | | |
> |---|---|---|---|---|
> | Classical Mechanics | → ← | Special Relativity | → ← | General Relativity |
>
> it takes a real process of generalization and invention to find out how to go from left to right, while the passage from right to left, after some great man like Einstein has given you what stands to the right, is one of strict implication. [112]

As Tolman suggested, theoretical research was not as simple as Bridgman imagined; it does not seem that Bridgman ever succeeded in his attempt. Nevertheless, Bridgman did not give up his program easily, reporting Tolman thus the following summer: "This summer I have been skimming again the parts of your new book that do not make too much mathematical demand on me, and I must confess that I still feel uncomfortable about the fundamental philosophy of general relativity. I expect I shall explode about it in print sometime soon, and make an awful ass of myself."[113] He was then preparing for the Venuxem Lectures to be given at Princeton University in December

111 *Ibid.*
112 Tolman to Bridgman, Dec. 15, 1934, PWBP, HUG 4234.10.
113 Bridgman to Tolman, July 29, 1935, PWBP, HUG 4234.10.

1935. The lectures' material was expanded and published as *The Nature of Physical Theory*[114] in 1936. In Chapter VIII of this book, Bridgman again discussed relativity but could make no remarkable progress.

In the Venuxem Lectures, Bridgman expected to address his criticism of theoretical physics directly to Einstein and other theoretical physicists then at Princeton,[115] but did not receive any substantial response from them. In the late 1940s, when he was asked to contribute an article to a collection of papers on relativity and Einstein, Bridgman found another chance to articulate his criticism of relativity to Einstein himself and listen to the theorist's own opinion of the operational interpretation of relativity. In the article titled "Einstein's Theories and the Operational Point of View,"[116] Bridgman repeated what he had talked more than ten years before in the Venuxem Lectures, maintaining that Einstein's largest contribution was in his setting up the criterion of meaning of physical concepts and criticizing the general theory from the operational viewpoint. Einstein's comment on Bridgman's criticism was direct and succinct:

> In order to be able to consider a logical system as physical theory it is not necessary to demand that all of its assertions can be independently interpreted and "tested" "operationally;" *de facto* this has never yet been achieved by any theory and can not at all be achieved. In order to be able to consider a theory as a *physical* theory it is only necessary that it implies empirically testable assertions in general.[117]

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[114] P. W. Bridgman, *The Nature of Physical Theory* (Princeton: Princeton University Press, 1936).
[115] Bridgman to Bentley, Aug. 12, 1936, PWBP, HUG 4234.10.
[116] P. W. Bridgman, "Einstein's Theories and the Operational Point of View," Paul Arthur Schilpp, ed., *Albert Einstein, Philosopher-Scientist*, 2 vols. (La Salle, Ill.: Open Court, 1949), pp. 335-354.
[117] Albert Einstein, "Reply to Criticisms," in Paul Arthur Schilpp, ed., *Albert Einstein,*

To Bridgman, this comment might have sounded a little too blunt. However, it would have become clear to him that the special theory was not structured in the operational way as he understood, and that theoreticians did not construct physical theory in the way he understood as ideal, if he had carefully examined this comment of Einstein.

As has briefly been mentioned, Einstein's definition of simultaneity, which Bridgman regarded as an ideal operational definition, is in fact one of the most telling examples that shows the difference between what theoreticians actually did and what Bridgman understood they did. In his 1905 paper, Einstein defined simultaneity by using light signals and showed how two clocks at different places could be synchronized. In using light signals in this way, Einstein presupposed that light travels at a definite speed independently of the motion of the source, but presented no experimental fact to support this presupposition. To examine the invariance of the light velocity, it is necessary to measure the unidirectional or one way velocity of light, though the speed of light actually measurable is the "go and come" velocity or two way velocity. If one accepts Einstein's discussion in the special theory, it is impossible to measure the one way velocity of light, since its measurement involves two synchronized clocks located at different sites. The only way to synchronize clocks that Einstein showed relied on the assumption of the invariance of the light velocity. Einstein broke this logical circle by *postulating,* not proving, the invariance of the speed of light. The one way velocity of light has no operational meaning within the logical structure of the special theory of relativity.

Philosopher-Scientist, pp. 663-688, p. 679..

As we have seen, in the 1923 essay Bridgman understood the inextricable connection between the synchronization of clocks and the one way velocity of light, but thought that he could experimentally examine the invariance of the light velocity, imagining that there were ways to synchronize clocks that did not depend on light signals. Within the scheme of Einstein's discussion, it is impossible to measure the one way velocity of light. However, this fact did not seriously bother Bridgman, who suspected that the special theory was not logically rigorous. Furthermore, as his discussion on the effect of the fixed stars shows, he believed that more experimental facts were necessary to prove the validity of this theory.

In fact, neither Bridgman nor many of his contemporaries estimated it very difficult to find a means to synchronize clocks without using light signals. They assumed that the invariance of the light velocity was experimentally examinable. For example, Tolman was one of the advocates of this viewpoint. He argued for this point in 1917 in *The Theory of the Relativity of Motion*,[118] and again in 1934 in *Relativity, Thermodynamics and Cosmology*.[119]

In his last years, Bridgman resumed reflection on the foundations of the special theory of relativity. By then, however, he had forgotten to take into account an important part of his criticism of the special theory in his 1923 essay that the measurement of the one way velocity of light cannot be discussed independently of the synchronization of clocks, or how to spread time through space. It took him considerable trouble to rediscover the interrelation between the one way velocity of light and the synchronization of clocks, as his note on the manuscript tells:

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[118] Richard C. Tolman, *The Theory of the Relativity of Motion* (Berkeley: University of California Press, 1917).

[119] Richard C. Tolman, *Relativity, Themodynamics and Cosmology* (Oxford: The Clarendon Press, 1934).

> [A] major change was my realization, early in 1961, that "one-way" velocity can have no physical significance by itself but is essentially a two-clock concept and has meaning only when the method has been specified by which time is spread over space. This realization negativated my efforst [*sic*] to find some physical method of measuring one-way velocity, and completely stultified the point of view contained in my MS "Two Footnotes to Relativity Theory" which I was on the point of offering to the American Journal of Physics. The second revision of the primer [...] begun in March 1961 is written in accordance with this new insight.[120]

The monograph he mentioned, *A Sophisticate's Primer of Relativity*, was published posthumously in 1962.[121] Bridgman suggested several ways to synchronize clocks that did not require light signals, but did not succeed.

Bridgman left confusions and contradictions in the monograph, probably because of hasty revision of a large part of the manuscript under an unfavorable condition of health and continuous pain caused by bone cancer. One remark put toward the end of the monograph reveals the recognition Bridgman finally attained, that the concept of one way velocity was not reliable:

> [T]he concept of "velocity" as generally understood involves assigning a time to distant events and therefore depends on the way we set distant clocks or the way we spread time over space. [...] Of the various sorts of velocity, it is only the one-clock velocities, including in particular round-trip velocity and self-measured velocity, which are physically significant in the sense that they do not depend on the arbitrary way in which time has been spread through space. This means that one-way two-clock velocities correspond to no physical "reality" and that

[120] S. Goldberg, "Being Operational vs. Operationism: Bridgman on Relativity," *Rivista di storia della scienza*, 1 (1984), p. 349.
[121] P. W. Bridgman, *A Sophisticate's Primer of Relativity* (Middletown, Conn.: Wesleyan University Press, 1962).

a determination of them is irrelevant in describing the physical nature of a system.[122]

Though Bridgman suggested several ways to synchronize distant clocks, he could trust none of them. Bridgman therefore had to admit that he could assign no operational meaning to the concept of one way velocity, since it depended on indefinite operations of synchronization. Furthermore, he might have understood that Einstein's definition of simultaneity, depending on the invariance of the light velocity, was also operationally unstable. He then might have to face the fact that it was not Einstein's original intention, but Bridgman's peculiar interpretation of special relativity, that led him to originate operationalism.

To understand relativity is one thing; to develop an idea derived from relativity is quite another. Although Bridgman's interpretation of special relativity might have been incorrect, he formulated an operational way of scrutinizing physical concepts by focusing on Einstein's definition of simultaneity. While participating in the controversy over dimensional analysis during the late 1910s, Bridgman implicitly invoked his operational interpretation of physics. During his struggle to understand general relativity in the early 1920s, he started to become self-conscious of his own operational view of science. He then "rediscovered" the meaning of special relativity and sophisticated his operational view. Though the origin of operationalism might not have been in relativity, Bridgman found the most successful application of his program of reform in Einstein's special theory of relativity. Sometime between August and December 1922, Bridgman realized that Einstein's definition of simultaneity represented what he considered as the operational definition. In 1923, he went on to apply the operational

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[122] The citation is from P. W. Bridgman, *A Sophisticate's Primer of Relativity* (New York and Evanston, Ill.: Harper Torchbooks, 1964), p. 146-147.

criterion to the meaning of fundamental physical concepts and completed an essay which was to be developed into *The Logic of Modern Physics*.

To acquire more momentum for the refinement and publication of his program, Bridgman still had to know more of what was going on in the frontiers of contemporary physics. He surveyed actual theorizing in atomic physics and, as we will see later, would even venture to suggest an operational guideline to a successful physical theory in that field. In the next section, I will examine how he encountered quantum theory before completing *The Logic of Modern Physics* and then will explore the various aspects of operationalism.

4.2. *The Logic of Modern Physics* and the Birth of Operationalism

As has been discussed in Chapter 2, Bridgman was one of the few physicists who encouraged research in quantum physics at Harvard. He supervised Kemble's graduate research in quantum physics and persuaded him to work for founding theoretical research at Harvard. Furthermore, Bridgman actively arranged European physicists' lectures that furnished the Harvard physicists with information of the recent development in Europe. Between 1923 and 1926, while preparing the ideas and draft of *The Logic of Modern Physics*, Bridgman could learn of the development of quantum theory by attending those lectures and conferences.

The Logic of Modern Physics reflected the excitement of a physicist who happened to witness a transition in physics and expected theorists to adopt the way of constructing physical theory that he suggested. Bridgman clearly expressed his observation and expectation in the "Introduction" of the *Logic*: "We have the impression of being in an important formative period; if we are, the complexion of physics for a long time in the future will be determined by our present attitude toward fundamental questions of interpretation."[123] Recalling this situation more than a decade after the publication of the *Logic*, Bridgman described how the epoch influenced the nature of his book: "The analysis which was the object of my observation was an analysis made by physicists at a certain epoch in history, with the background of assumption and presupposition prevalent at that epoch, and with the unexpressed purposes inherent in the scientific activities of the epoch."[124] Moreover, he explained that it was only after his effort to see "what is really involved in electromagnetic field theory, relativity theory, and those parts of quantum theory which had been developed by 1926," that he could "see what successful analysis consists in."[125] Having examined his scrutiny of dimensional analysis and relativity theory, I will analyze the part of quantum physics Bridgman had encountered by 1926.

4.2.1. The Contact with Quantum Physics

As has been mentioned in Chapter 2, in 1914, when he took over the courses on electromagnetic theory, Bridgman ordered several books on theoretical physics, including some in quantum theory. He continued his effort to update his knowledge in this field. For example, in May 1922, he ordered the third edition of *Atombau und Spektrallinien*[126] by A. Sommerfeld and *Physikalische Rundblicke*[127] by

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[123] P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927), p. ix.
[124] P. W. Bridgman, "Operational Analysis," *Philosophy of Science*, 5 (1938), p. 115.
[125] *Ibid.*
[126] Arnold Sommerfeld, *Atombau und Spektrallinien*, 3. Aufl. (Braunschweig: Friedrich Vieweg und Sohn, 1922).
[127] Max Planck, *Physikalische Rundblicke: gesammelte Reden und Aufsätze* (Leipzig: S. Hirzel, 1922).

M. Planck.[128] In 1924, invited to the Solvay Conference, Bridgman had chances to see and talk with the European physicists working in quantum physics, such as N. Bohr, A. Einstein, Paul Ehrenfest, and Erwin Schrödinger.

The European physicists' lectures at Harvard and MIT added fresh information to his knowledge: in 1922, H. A. Lorentz delivered lectures at Harvard on "light and the constitution of matter"; between October 1922 and April 1923, Sommerfeld stayed at MIT and also gave a course of lectures at Harvard; Bohr visited the United States in 1923, delivering lectures on "theory of spectra and the atomic constitution" at Harvard; and in 1924, Ehrenfest delivered three lectures on "problems of quantum statistics" at Harvard. Probably the most influential lectures were those given by M. Born during his stay at MIT in the second half of the Fall Semester of 1925-26. Having just completed papers on matrix mechanics written with W. Heisenberg and P. Jordan, Born afforded the American physicists the latest information on this theory.

Immediately after Born returned to Germany, Bridgman's sabbatical half year started in late January 1926. He spent most of this half year in Europe, attended conferences in Zurich and Göttingen with Sommerfeld, Born, and W. Pauli, and completed a large part of the *Logic* in such resort areas as northern Italy and coastal Yugoslavia.[129] Bridgman later described how he prepared the draft:

> The Logic of Modern Physics was written during a half sabbatical in 1926 under a stringent time limit. [...] In view of this time limit, I had to map out the questions that to me appeared most

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[128] Bridgman to Friedrich Vieweg und Sohn, May 17, 1922, PWBP, HUG 4234.8.
[129] A. Moyer, "P. W. Bridgman's Operational Perspective on Physics. Part II: Refinements, Publication, and Reception," *Studies in History and Philosophy of Science*, 22 (1991), p. 375.

> pressing and to be satisfied with discussions of which I could say "at least this much must be true and be part of the final picture," and not attempt the more ambitious program of a complete analysis. In short, I was compelled to be satisfied with a "necessary" as opposed to a "sufficient" analysis.[130]

He had to finish the draft "under a stringent time limit" as he had to come back to Harvard by the end of that summer. This period meant much to the development of quantum mechanics: matrix mechanics, introduced the previous fall, was discussed in seminars and conferences in Europe; Schrödinger published papers on his wave mechanics[131] that year; and Dirac completed his paper on "quantum algebra"[132] that summer. Though Bridgman could include neither a complete analysis of the foundations of physics nor detailed discussion of the current theoretical topics, he was excited with the completion of quantum mechanics and discussed in the *Logic* its implications to his viewpoint.

It seems, however, that Bridgman seriously started to study the literature of quantum mechanics after he completed and published the *Logic*. In the footnote in the section on "Quantum Concepts" in the *Logic*, Bridgman noted that the part "was written early in 1926 without access to recent literature."[133] Three decades after the publication of the *Logic*, he also recalled: "It would be expected on rewriting [the *Logic*] that the most radical changes would be with respect to quantum phenomena, since the book was written coincidentally with the appearance of Schrödinger's wave equation."[134] His bibliographical

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[130] Bridgman, "The Present State of Operationalism," p. 76.
[131] E. Schrödinger, Quantisierung als Eigenwertproblem," *Annalen der Physik*, 79 (1926), pp. 361-376 and 489-527; 80 (1926), pp. 437-490; 81 (1926), pp. 109-139.
[132] P. A. M. Dirac, "The Quantum Algebra," *Proceedings of the Cambridge Philosophical Society*, 23 (1926), pp. 412-418.
[133] Bridgman, *The Logic*, p. 186.
[134] Bridgman, "P. W. Bridgman's 'The Logic of Modern Physics' after Thirty Years," p. 519.

note on quantum mechanics titled "References. Q Mxcs [Quantum Mechanics]" [135] suggests that he started to collect articles in quantum mechanics only after 1927. Among the papers listed in the note were: Schrödinger's articles on wave equation [136] published in 1926; Heisenberg's paper on his attempt to derive matrix mechanics only by "observable" physical quantities,[137] published in 1925; the papers on the formalization of matrix mechanics[138] published in 1925-1926; and Born's papers on his probabilistic interpretation of the wave function.[139] Though he often mentioned quantum theory in the *Logic*, Bridgman learned the main part of quantum mechanics after its publication.

Written in 1926, the *Logic* did not reflect the philosophical discussion connected with the question of how physicists could comprehend the physical meaning of quantum mechanics. It was immediately after the completion of the *Logic* that an epistemological questions quantum mechanics raised started to attract a group of physicists' serious attention. Though Bridgman attended the Fourth Solvay Conference in 1924 and a few other conferences in 1926, he could not attend the Volta Centenary held in Como in 1927, which was the first public meeting where philosophical problems on interpretation were discussed. Bohr, for example, made the first public speech on his

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[135] P. W. Bridgman, undated handwritten note, titled "References Q Mxcs [Quantum Mechanics]," PWBP, HUG 4234.15.
[136] E. Schrödinger, Quantisierung als Eigenwertproblem," *Annalen der Physik*, 79 (1926), pp. 361-376 and 489-527; 80 (1926), pp. 437-490; 81 (1926), pp. 109-139.
[137] W. Heisenberg, "Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen," *Zeitschrift für Physik* 33 (1925), pp. 879-893.
[138] Born and Jordan, "Zur Quantenmechanik," *Zeitschrift für Physik*, 34 (1925), pp. 858-888; Born, Heisenberg, and Jordan, "Zur Quantenmechanik II," *Zeitschrift für Physik* 35 (1926), pp. 557-615.
[139] Born, "Quantenmechanik der Stoßvorgänge," *Zeitschrift für Physik*, 37 (1926), pp. 863-867; "Quantenmechanik der Stoßvorgänge," *Zeitschrift für Physik*, 38 (1926), pp. 803-827; and, "Das Adiabatenprinzip in der Quantenmechanik," 40 (1926), *Zeitschrift für Physik*, pp. 167-192. In citing the last paper, actually published in 1926, Bridgman wrote: "M. Born Das Adiabater Prinzip in der Quantenmechanik. ZS. f. P. 40.167.1927." This fact suggests that the note was made after 1927.

idea of complementarity in Como. The same type of discussion was further developed at the Fifth Solvay Conference, held the same year. Having missed chances to know a philosophical aspect of quantum mechanics directly from European physicists, Bridgman later make his own effort to catch up with the discussion on interpretation of quantum mechanics, as well as such components of the theory as matrix mechanics and wave mechanics.

4.2.2. The Aims and Foundations of the *Logic*

Unfamiliar with the details of quantum mechanics, Bridgman expected that this theory was constructed in the way he suggested in the *Logic*. In fact, what seems to have motivated him to publish the *Logic* was this expectation. In the "Preface," he clarified what made the scrutiny of physical concepts necessary: "All the quite recent activity with the new quantum mechanics seems to call for a new examination of fundamental matters which shall recognize, at least by implication, the existence of the special phenomena of the quantum domain."[140] Almost the same discussion on the necessity to examine the fundamentals of physics again appears in the "Introduction":

> The situation is rapidly becoming acute. Since I began writing this essay, there has been a striking increase in critical activity inspired by the new quantum mechanics of 1925-26, and it is common to hear expositions of the new ideas prefaced by analysis of what experiment really gives to us or what our fundamental concepts really mean.[141]

He then specified the purpose of the analysis he would make in the *Logic*: "Our problem is the double one of understanding what we are

[140] P. W. Bridgman, *op. cit, The Logic of Modern Physics*, p. v.
[141] *Ibid.*, p. ix.

trying to do and what our ideals should be in physics, and of understanding the nature of the structure of physics as it now exists."[142] To pursue this purpose, he would analyze "the fundamental concepts" in physics, as "an understanding of the concepts we now have discloses the present structure of physics and a realization of what the concepts should be involves the ideals of physics."[143] The *Logic* was therefore supposed to present not only the analysis of contemporary physics but also the way to construct a new theory in the quantum domain.

Bridgman's tool for implementing his ambition had essentially gone unchanged since the end of 1922. However, his discussion in the *Logic* shows the effort he had made to express it more impressively. First, in the "Preface," he located his position in a wider context than he had done before, mentioning that "the fundamental attitude of this essay is empiricism."[144] Then he began presenting his method by analyzing how Einstein succeeded in criticizing fundamental concepts related to space and time, though he wrote nothing definite "as to our permanent acceptance of the analytical details of Einstein's restricted and general theories of relativity."[145]

In Bridgman's interpretation, Einstein's most important attainment was his discovery "that classical concepts, accepted unquestioningly, were inadequate to meet the actual situation."[146] The impact of this discovery "has resulted in a critical attitude toward our whole conceptual structure which must at least in part be permanent."[147] However, at the same time, Bridgman believed that the

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142 *Ibid.*, p. x.
143 *Ibid.*
144 *Ibid.*, p. vi.
145 *Ibid.*, p. 1.
146 *Ibid.*
147 *Ibid.*

same point as Einstein had made could have been attained by "a sufficiently shrewd analysis," as it was related only to "our mental relations to nature," and not to the part which was "subject to change as we gain new experimental knowledge."[148] He therefore believed that "a revolution" in this part, namely, the part "which rests on the permanent basis of the character of our minds,"[149] should not happen again. His purpose of the discussion in the *Logic* was "to understand so thoroughly the character of our permanent mental relations to nature that another change in our attitude, such as that due to Einstein, shall be forever impossible."[150] The revolution might be allowed to happen once, as "after all physics is a young science, and physicists have been very busy," "but it would certainly be a reproach if such a revolution should ever prove necessary again."[151] Bridgman ventured to complete the revolution begun by Einstein.

In discussing the meaning of Einstein's revolution, Bridgman again came back to "empiricism." To Bridgman, "[t]he first lesson of our recent experience with relativity" was that the attitude of the physicist must be "one of pure empiricism."[152] Bridgman considered empiricism as an attitude that admitted "no *a priori* principles which determine or limit the possibilities of new experience."[153] Einstein's contribution to physics, in Bridgman's interpretation, was his presentation of "concepts of such a character that our present experience does not exact hostages of the future."[154] In the way that he regarded as Einstein's, Bridgman attempted to define the concept of length:

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148 *Ibid.*
149 *Ibid.*, pp. 1-2.
150 *Ibid.*, p. 2.
151 *Ibid.*
152 *Ibid.*, pp. 2-3.
153 *Ibid.*, p. 3.

> We evidently know what we mean by length if we can tell what the length of any and every object is, and for the physicist nothing more is required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*[155]

The last sentence is the final form of his operational principle. To put it simply, the operational definition of concepts may have been a little more sophisticated form of a part of common sense that "the true meaning of a term is to be found by observing what a man does it, not by what he says about it,"[156] as Bridgman himself put it. But it clearly meant more to the originator of this definition. Bridgman assumed that once the method of defining concepts in terms of operations had been established there would not be any "danger of having to revise our attitude toward nature." "For," he explained, "if experience is always described in terms of experience, there must always be correspondence between experience and our description of it."[157] Bridgman was convinced that by finding out the operational definition, he had introduced the most effective way to describe experience. To him, operations were the most essential part of experience. He therefore believed that if experience was described by concepts that were defined in terms of operations, there should be no discrepancy between what was described and what described it. In other words, "[f]rom the operational point of view it is meaningless to attempt to separate

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[154] *Ibid.*, p. 4.
[155] *Ibid.*, p. 5.
[156] *Ibid.*, p. 7.
[157] *Ibid.*, p. 6.

'nature' from 'knowledge of nature'." [158] He realized that the interpretive problems in electromagnetic theory, dimensional analysis, and relativity theory that had been troubling him were all created by the discrepancy between concepts and experience. To solve them, he introduced the principle that experience should be described by itself and legitimated only such concepts as could be defined in terms of operations. To him, this seemed to be the only way to guarantee the validity of physics as exact knowledge of nature.

The contemporaries often mistook Bridgman's claim for operational definition for a mere requirement of experimental verification. In 1929, Henry Schultz, a professor of economics and business at the University of Chicago, explained in his letter to Bridgman[159] that the economist sometimes felt the need to verify his or her concepts experimentally or statistically to show that they had some realistic basis. In the reply, Bridgman clearly denied the possibility of such a verification of concepts defined by operations:

> It seems to me that the idea of experimental verification does not apply to a concept defined in terms of operation. I should think that all one could ask is whether the operations exist (that is, can the operations be applied) which define the concept, and then whether the concept defined with such operations is useful or not, but it seems to me that there is nothing to verify in the ordinary sense in a concept understood in this way.[160]

Contrary to prevalent interpretation of his standpoint, what Bridgman stated in the *Logic* was not the necessity of experimental verification of scientific concepts. Such verification may become necessary only when one needs to examine whether concepts invented in some other way

158 *Ibid.*, p. 62.
159 Schultz to Bridgman, Feb. 13, 1929, PWBP, HUG 4234.12.
160 Bridgman to Schultz, Feb. 17, 1929, PWBP, HUG 4234.12.

than operational one also have an operational meaning. Basically, Bridgman intended to define, not to verify, scientific concepts by introducing "operations."

Furthermore, it is notable that, at least in Bridgman's scheme outlined in the *Logic*, operations themselves were not considered as the objective of physical research. Operations were, in a sense, only a tool to define concepts or to make the meaning of concepts clearer, but not what physicists sought for. By establishing the operational definition, Bridgman wanted to secure an exact means to describe nature. What he considered as the aim of physical research was an exact description of experience or nature. Bridgman made his effort to construct the operational criterion for the purpose of showing how to prepare the most suitable tools to describe nature.

However, Bridgman knew well that the physicist could be interested in what could be done with his or her knowledge, as he admitted in a letter to Korzybski, explaining his opinion of Bertrand Russell's *The Analysis of Matter*:[161]

> It seems to me that Russell's chief interest in understanding nature is in the *process* of reducing it to understandability, whereas the interest of the physicist is rather in discovering what new things he can do after he has made nature understandable. This doesn't mean that the physicist has an engineering interest, but he is interested in the further implications in the broadest sense of what he discovers.[162]

Bridgman recognized that the physicist was more interested in doing new things with new understanding of nature than in merely making nature understandable. As an experimentalist, he correctly observed

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[161] Bertrand Russell, *The Analysis of Matter* (London: Kegan Paul, Trench, Trubner, 1927).

[162] Bridgman to Korzybski, Jan. 29, 1928, PWBP, HUG 4234.12.

that experimental science had been broadening its territory by applying newly discovered operations to realms that had hitherto been out of its reach, though he did not attempt to analyze further this inclination of physicists. Furthermore, Bridgman knew that the physicist's interest in operations was different from an engineering interest; he understood that the physicist was interested in "discovering" what could be done with new knowledge. Though admitting the physicist's interest in operations, he clearly maintained the division between scientists' interest in the quest for knowledge and others' interest in the results of scientific research.

Still, Bridgman belonged to a group of scientists who regarded the goal of science as the acquirement of knowledge that had little to do with practice. The word "operationalism" sometimes causes misunderstanding that its originator Bridgman may have regarded operations as the goal of scientific research. As an experimental physicist, Bridgman sought for new operations in high pressure physics and knew well of the physicist's interest in operations. However, in his philosophical writings, his main concern was almost always with how to discover, understand, and describe nature, experience or reality; operational scrutiny was only a means to achieve this goal.

In connection with Bridgman's understanding of the role of operations in scientific activity, two other characteristics of his discussion on operations in the *Logic* are worth mentioning. In the *Logic*, contrary to the reader's expectation, Bridgman seldom analyzed the concept of operation and never gave it a clear definition. He was mainly concerned with examining various operations that he could use to define such concepts as length, mass, space, time, causality, identity, velocity, force, or energy. More significantly, he never analyzed the difference between operations that could actually be carried out and operations that would only appear in reflective writings. His operations were, as it were, "thought operations" that were only imagined and constructed in his writings. Einstein's method of synchronizing two clocks was also a set of "thought operations" in a sense that he never expected that anyone had ever attempted to synchronize clocks in that way. These two points, though never bothering Bridgman, formed serious limitations of his perspective presented in the *Logic* and made the reader wonder how Bridgman's experience as an experimentalist was reflected in his philosophical work.

4.2.3. Absolute and Unique Operations

Let us examine Bridgman's words and phrases in the *Logic* and private letters more closely, so that we can better understand what Bridgman expected from operations and why he picked up operations for defining concepts. In the *Logic*, what he first required of operations was their uniqueness: "We must demand that the set of operations equivalent to any concept be a unique set, for otherwise there are possibilities of ambiguity in practical applications which we cannot admit."[163] Considering the purpose of the operational scrutiny, one can easily accept this requirement. If a physical concept is synonymous with the corresponding operation, the lack of the unique correspondence between the concept and the operation will make the meaning of the concept ambiguous. When, on the contrary, the correspondence is unique, one can associate the meaning of the concepts uniquely with the operation. This is a merit, if any, of the operational definition.

By analyzing Bridgman's argument about "the relative character of

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163 Bridgman, *The Logic of Modern Physics*, p. 6.

knowledge," one will find another kind of uniqueness, or even absoluteness, that he expected of operations. According to Bridgman, if we admitted the operational definition of concepts, we should inevitably admit relativity of knowledge: "Relativity in the general sense is the merest truism if the operational definition of concept is accepted, for experience is described in terms of concepts, and since our concepts are constructed of operations, all our knowledge must unescapably be relative to the operations selected." [164] The meaning of concepts can vary if operations adopted to define them change. Bridgman, however, neither claimed that everything was relative nor abandoned the concept of "absolute." He admitted that the absolute would "disappear in the original meaning of the word."[165] "But," he continued, "the 'absolute' may usefully return with an altered meaning, and we may say that a thing has absolute properties if the numerical magnitude is the same when measured with the same formal procedure by all observers."[166] Then he tried to explain how the concept of length "might be made to reassume its desired absolute character."[167] After a long struggle with general relativity, Bridgman found what was absolute and what was not, although he did not discuss this point further in the *Logic*.

Bridgman detailed what he regarded as "absolute" in his letter to Korzybski written in October 1928. [168] After reiterating his usual criticism of the formal character of generalized coordinates, he explained what could be absolute in physics:

> [Y]ou must remember that the physical operations by which you measure length are definite, fixed, 'absolute' if you will, operations, which have not connection with a coordinate system

[164] *Ibid.*, p. 25.
[165] *Ibid.*, p. 26.
[166] *Ibid.*
[167] *Ibid.*, p. 27.
[168] Bridgman to Korzybski, Oct. 17, 1928, PWBP, HUG 4234.12.

> and do not change when you pass from one coordinate system to another. It is therefore inevitable that when you describe an absolute set of operations correctly you get an invariant result.[169]

Through his criticism of the general theory of relativity, Bridgman found out that fundamental operations would remain definite while the coordinate system could change. In the *Logic*, he implicitly invoked this absoluteness of operations as the ground of his operational definition: concepts or knowledge are relative as they are defined by operations, while operations that define concepts cannot be relative.

The operational analysis urges physicists to check the corresponding operations when they are not sure of the meaning of scientific concepts. This method is useful as far as operations can assign clear, definite, unique, or absolute meanings to concepts. Bridgman believed that this was possible, assuming two uniqueness, one explicit and the other implicit. An explicit assumption, the assumption of uniqueness of correspondence between concepts and operations, was that it is possible to assign a unique set of operations to a concept. Whether scientists can fulfill this requirement or not depends on their effort to define concepts operationally. Bridgman spent a major part of the *Logic* in finding out appropriate operations defining fundamental concepts in physics.

On the other hand, Bridgman only implicitly mentioned the other assumption. We can call it the assumption of absoluteness of operations or uniqueness of the results of operations, which means that operations that define concepts should always lead to the same result for all observers when performed with the same formal procedure, or that one can use only such operations for defining concepts. This

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169 *Ibid.*

assumption, though not mentioned clearly in the *Logic*, was essential to sustain the validity of the operational definition. Even when one can uniquely specify the corresponding set of operations, the meaning of concepts can become ambiguous, if the result of these operations varies from situation to situation. As Bridgman presupposed the uniqueness of the results of operations, he could be sure of the usefulness of the operational definition.

4.2.4. Mathematical Operations

I have so far discussed Bridgman's idea of *physical* operation. In the *Logic*, Bridgman introduced another type of operation, "mental operations": "[I]f the concept is mental, as of mathematical continuity, the operations are mental operations, namely those by which we determine whether a given aggregate of magnitude is continuous."[170] Bridgman did not discuss further this type of operation in the *Logic*. This kind of operation would later be renamed "paper and pencil operation" and would come to occupy an important position in his operational view. For the purpose of examining the meaning of "mental operations" and what operations Bridgman considered as realizable, I will turn to his criticism of Georg Cantor's diagonal proof of the non-denumerability of the real numbers.

Bridgman published an attempt to apply his operational method to *Mengenlehre*, or set theory, in the 1934 volume of the *Scripta Mathematica*.[171] What turned his attention to the foundations of mathematics was the news that the paradoxes of set theory were revealing the instability of mathematics. As he did in the *Logic*,

[170] Bridgman, *The Logic of Modern Physics*, p. 5.
[171] P. W. Bridgman, "A Physicist's Second Reaction to *Mengenlehre*," *Scripta Mathematica*, 2 (1934), pp. 101-117, 224-234.

Bridgman analyzed the operational background of mathematical concepts, believing that this method would resolve any paradox. As has been shown in connection with Bridgman's scrutiny of relativity, the operational criterion was too demanding to afford a sound basis to physical concepts. However, he ventured to apply a similar method to mathematical concepts. The most impressive example that described his understanding of mathematical concepts was his rejection of Cantor's diagonal proof of the non-denumerability of the real number. Bridgman summarized the proof as follows:

> Suppose that [...] a pairing of all the decimal fractions against the integers is possible. Then I undertake to produce a fraction which from its method of construction can have no place in the sequence, thus proving the assumed construction is not possible. Such a fraction is easily described, namely any decimal whose first digit is any number different from the first digit of the first decimal in the array, whose second digit is different from the second digit of the second decimal of the array, and so on indefinitely.[172]

Bridgman pointed out that "[i]t is impossible to actually carry out the operations involved in the diagonal Verfahren" since "the operation involved in producing the non-terminating decimal cannot be completed."[173] It will be possible to construct non-terminating decimal *theoretically*, or it will be possible to *imagine* that such a construction should be possible. However, of course, it is impossible to *actually* or *physically* carry out the sequential arrangement of infinite numbers, as no procedure that involves infinity can physically be carried out.

The operational definition requires that physical concepts should always have a corresponding set of operations that are physically realizable. In a similar manner, Bridgman required that mathematical

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172 *Ibid.*, p. 224.

concepts should always have a corresponding set of operations that were mathematically realizable. However, Bridgman's criterion for the mathematical operations was unacceptably strict even for contemporary physicists. Bridgman denied the possibility of constructing the non-terminating decimal that the diagonal proof specified, since he rejected the possibility of actually writing down an infinite sequence of digits. This rejection of the diagonal proof was originated in Bridgman's peculiar understanding of the ontological meaning of numbers and rules in general in mathematics. To him, a number was synonymous with a "program" to construct it by certain rules. These rules, as they were applied by a human being, could be applied only a finite number of times, and the number of rules should also be finite. Though some of those who read his article in the *Scripta Mathematica*, including Erwin Schrödinger and the mathematician Abraham Fraenkel, wrote Bridgman that his operational standard was not acceptable in mathematics, he did not relax his criterion.

To Schrödinger, Bridgman sent a copy of his article. On June 9, 1935, Schrödinger replied Bridgman, thanking for the copy but writing that "[m]y position is: that this procedure [the diagonal proof] actually proves, that the non-terminating decimals are not enumerable."[174] In this letter, however, Schrödinger only repeated the diagonal proof, which Bridgman did not accept.[175] Schrödinger then even sent a short typescript titled "Remarks on non-denumerability" to Bridgman.[176] After reading the typescript and the cover letter, Bridgman found out an unbridgeable gulf between their standpoints:

[173] *Ibid,*, p. 224.
[174] Schrödinger to Bridgman, June 9, 1935, PWBP, HUG 4234.10.
[175] Bridgman to Schrödinger, Aug. 18, 1935, PWBP, HUG 4234.10.
[176] Schrödinger to Bridgman, Sept. 3, 1935, PWBP, HUG 4234.10.

> Your letter makes clear where our difference of point of view lies. I think it goes back to what we understand by "existence", and what you try to do with things that "exist", that is, with "objects". I would not grant the status of "existence" to anything that could be defined *only* in terms of infinite processes; I would not refer to such a thing as an "object", and I would not attempt to handle it as an "object".[177]

Bridgman detailed his demand, writing that "we restrict ourselves to operations *actually carried out.*" He could not accept Schrödinger's proof because Schrödinger's usage of the term 'existence' was "meaningless" to him. He did not deny that mathematicians adopted different kinds of standards that proved to be useful in mathematical research, but could not approve them as totally rigorous: "it seems to me that such a use can be defended only in exploratory, orienting investigations, where any suggestive method is allowable, but the results obtained in such a way must later be justified by some more rigorous method."[178]

A decade later, stimulated by Fraenkel's lecture, Bridgman resumed his reflection over the diagonal proof. According to Bridgman, Fraenkel "referred to the proof of diagonal process as universally accepted and admitted to be sound beyond the peradventure of a doubt."[179] Attempting to reexamine diagonal process, Bridgman picked up W. V. O. Quine as a discussant. Quine wrote two letters and detailed the diagonal proof and its formal validity to Bridgman,[180] who nevertheless only found that the rule of his game was different from Quine's: "[Y]ou were not playing the same game that I was and that I

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[177] Bridgman to Schrödinger, Oct. 9, 1935, PWBP, HUG 4234.10.
[178] *Ibid.*
[179] P. W. Bridgman, "Some Implications of Recent Point of View in Physics," in *Reflection of a Physicist* (New York: Philosophical Library, 1955), pp. 84-118, pp. 103-104. The paper was originally published in *Revue Internationale de Philosophie*, 3 (1949), pp. 479-501.
[180] Quine to Bridgman, Apr. 10; Apr. 15, 1946, PWBP, HUG 4234.10.

had not made plain what my game was."[181] To Quine, Bridgman explained his criterion for mathematical concepts, asserting that the rule for constructing a number should be used "as the basis for an actual construction," thus no infinite process being allowed in the rule. It was beyond Quine's imagination that Bridgman's fundamental viewpoint was so different from a widely accepted one. Bridgman, therefore, had to clarify again how he applied his operational method to the concept of number:

> [M]y objections to the diagonal process rest on an unorthodox conception of the nature of numbers, unlike any with which I am acquainted, and to which I was led by application of the "operational" point of view. Numbers, as I understand them, are connected in some way with our activities. Briefly and incompletely, a number is something that we do, or perhaps better a result of something that we do. What the further characterizations may be I do not attempt at present to specify; except to note that a number is more immediately tied up with a purposeful activity on our part than are "things", like the table in front of me, which also is to be understood in terms of what I do or at any rate in terms of what happens to me. Consistent with this view of numbers, numbers are to be *constructed* and *exhibited*. It follows that numbers are always finite. [...] In my sense "construction" does not mean exhibition of the rule by which the number is generated.[182]

Bridgman stubbornly kept applying his intolerant criterion to mathematical concepts, although he did not have many chances to publish his discussion of mathematics. Yet, at least with Quine, he eventually reached agreement "to the extent of both seeing that the diagonal proof is a proof only in the context of the Platonic concept of the nature of number."[183]

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181 Bridgman to Quine, Apr. 13, 1946, PWBP, HUG 4234.10.
182 Bridgman to Quine, Apr. 21, 1946, PWBP, HUG 4234.10.
183 Bridgman, "Some Implications of Recent Point of View in Physics," p. 104.

Among physicists, Bridgman found a certain number of sympathizers with his operational view, even though some of them totally misunderstood Bridgman's original standpoint. Among mathematicians, however, he could not find many interested in discussing his reflection on numbers. People close to Bridgman who wrote to convert him were dismayed to find his assumptions unacceptably different from an ordinary view. If they had gone further and examined his criticism of physical concepts more carefully, they might have found almost the same stubbornness in his view of physics.

4.2.5. Explanation and Mechanism

As has been mentioned before, in *The Logic of Modern Physics*, Bridgman left the analysis or definition of operation untouched on. After introducing the operational method to define concepts, he swiftly switched to the discussion of physical theory. Though some part of discussion had little to do with his operational methodology, his analysis of physical theory in general revealed how he understood the construction and function of science.

In Chapter II of the *Logic*, Bridgman described what physicists did to construct physical theory and how the operational method should work in their effort. Bridgman observed that the purpose of physics was to find an explanation of phenomena, the essence of which consisted in "reducing a situation to elements with which we are so familiar that we accept them as a matter of course."[184] He understood that an effort currently made to build quantum theory was a justification of his definition of explanation, since "the endeavor of all these quantum explanations is to find in every new or more complicated

[184] Bridgman, *The Logic of Modern Physics*, p. 37.

situation the same elements which have already been met in simpler situations, and which are therefore relatively more familiar."[185] However, quantum phenomena were so new and unfamiliar that he felt "an instinctive need for explanation in other terms."[186] Bridgman called this situation "an explanatory crisis" and suggested what to do with this crisis:

> The first step in resuming our explanatory progress, after we have been confronted with such a crisis, is to seek for various sorts of correlation between the elements of our new experience, in the confident expectation that these elements will eventually become so familiar to us that they may be used as the ultimates of a new explanation. This is exactly what is now happening in quantum theory.[187]

Bridgman understood that physicists would not be able to find appropriate way to explain quantum phenomena in the terms they were accustomed to. To him, quantum theory seemed to suggest necessity to create a new way of explanation to which no one might feel familiar.

Furthermore, Bridgman pointed out that many explanations involved what he called a mechanism, though he again did not define it precisely. For the purpose of "understanding the attitude of mind that feels a mechanism is necessary,"[188] Bridgman discussed what kind of mechanism could satisfy physicists:

> The instinctive demand for a mechanism is fortified by observation of the many important cases in which mechanisms have been discovered or invented. However, the significance of such successful attempts must be subject to most careful scrutiny. The matter has been discussed by Poincaré, who

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185 *Ibid.*, p. 41.
186 *Ibid.*
187 *Ibid.*, p. 43.
188 *Ibid.*, p. 46.

> showed that not only is it always possible to find a mechanistic explanation of any phenomena [...] but there are always an infinite number of such explanation. This is very unsatisfactory. We want to be able to find the *real* mechanism.[189]

Bridgman referred to Henri Poincaré's discussion that any physical laws of any system could be formulated in the form of the principle of variation or the principle of least action, and that because of a range of arbitrariness of the Lagrangian involved in the formulation, an infinite number of mathematical forms could be assigned to physical laws.[190] Poincaré's original description can be interpreted as indicating that the same discovery proved that it was actually possible to give an infinite number of mechanistic explanations, not just mathematical formulations originated in classic mechanics, to any physical phenomena. This seems to be Bridgman's understanding, although he later recalled that he had not given "any very close thinking" to Poincaré's discussion while he was writing the *Logic.*[191]

Bridgman's criticism of Poincaré's discovery suggests that Bridgman had his own idea of the *real* mechanism describing physical reality. This real mechanism was built on the basis of "the operations by which we may set up a one to one correspondence between the properties of the mechanism and the natural phenomenon."[192] The fastest way Bridgman recommended to examine a mechanism was to

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189 *Ibid.*, pp. 48-49.

190 Bridgman quoted the German translation of the volume thus: "Henri Poincaré. Wissenschaft and [*sic*] Hypotese, Translated into German by F. and L. Lindemann, Teubner, Leipzig, 1906. See especially p. 217." The author's discussion therefore depends on the German translation Bridgman mentioned. Bridgman did not have the English translation of *La science et l'hypothèse* (see the next note.)

191 Bridgman to Korzybski, Nov. 13, 1927, PWBP, HUG 4234.12. In the same letter, Bridgman recalled that he had *reread* the French original of Poincaré's *La science et l'hypothèse* (Paris: Flammarion, 1902) prior to completing the *Logic* and that he did not have the English translation published in 1921.

192 Bridgman, *The Logic of Modern Physics*, p. 49.

count its degree of freedom: "If [...] a mechanism is to be taken seriously as actually corresponding to reality, we must demand that it have no more degree of freedom than the original phenomena, and we must also be sure that the phenomenon has no undiscovered features."[193]

In the concluding remark of the *Logic*, Bridgman more precisely explained how to construct a new theory that exactly described quantum phenomena:

> [The analysis of physicists' inventions to describe physical phenomena] will take the form of a search for new physical facts which shall give to our inventions the character of physical reality. In case prolonged search fails to disclose such phenomena (as is probably now the case with the field concept of electrodynamics), we must then find some way of embodying explicitly in our thinking the fact that we are dealing with pure inventions and not realities.[194]

Bridgman tried to show how to distinguish inventions from realities in physical theory. Though he could not contribute to theoretical development in quantum physics, he attempted to contribute to it by presenting a trustful guideline for theorizing in this new field.

Furthermore, Bridgman was convinced that his method was actually applied to the construction of quantum mechanics:

> Some of the general considerations of this essay may, with considerable plausibility, be expected to play a part in the future of both speculative and experimental physics. The most important effect may be expected from the clearer recognition of the operational character of our physical concepts. Indeed during the writing of this essay there has been a very marked increase in emphasis on the necessity of understanding in terms of physical operations such fundamental concepts as that of the

[193] Ibid., pp. 49-50.
[194] *Ibid.*, pp. 225-226.

> electron, by the new quantum mechanics [the mechanics of Heissenberg-Born [*sic*] and Schrödinger of 1925-26].[195]

In 1926, when physicists had not seen the epistemological implications of quantum mechanics, this new theory seemed to Bridgman to be developing in the way he described in the *Logic*. We will see in the next chapter how his evaluation of this theory would change in a few years.

4.2.6. The Author's Self-Evaluation

What Bridgman presented in *The Logic of Modern Physics* was neither a systematic philosophy nor philosophical evaluation of contemporary physics, although the volume is often mentioned as an attempt to describe the outline of operational *philosophy* and Bridgman himself is often regarded as a physicist-philosopher who originated operationalism. Bridgman understood what he had done in his best-known book and remained hesitant to give it a high evaluation.

Right after he published the *Logic*, Bridgman intimated to Korzybski that he was aware of "a great many" mistakes it might have: "With regard to the defects in the book, I am conscious enough that there are a great many, and I believe that this feeling nearly overpowered me when I was reading proof."[196] Having no idea whether he "was really saying much," he was not sure of the significance of the book: "If the book should ever go to a second edition, something of which I have no anticipation, I shall try to make various improvements." In another letter to Korzybski, Bridgman confessed what he had been doing as a physicist: "I do not spend much of my time doing things like my book, but I am one of those dirty physicists, all of whose time is occupied with the highly unabstract work of discovering whiskers on

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[195] *Ibid.*, p. 222.

the suspensions of galvanometers or rubbing dirt from electrical contacts."[197] He then warned that the reader should not take his book "more seriously than was intended" and clarified his objective: "My main object in writing the book was to arouse self consciousness as to the actual state of affairs in our physical knowledge. Once self consciousness is acquired, different people will give different accounts of the situation." Bridgman reiterated almost the same point to Bentley: "The book was intended to do two things primarily: to waken the self consciousness of the physicist to the sort of structure that he has, and to describe as truthfully as I could what seemed to me the nature of this structure."[198] Despite the apparently ambitious reform plans of physics presented in the *Logic*, the author's evaluation remained quite modest.

Ten years after completing the *Logic*, Bridgman again wrote Bentley how and why he had actually written it:

> I think that perhaps more than most I write to clarify my own ideas; I am fully as much interested in straightening out things for myself as I am in producing a systematic structure, which doubtless is not quite fair on the reader. Part of what I write must be taken in the spirit of the experimenter in the laboratory in recording everything he sees, a [*sic*] la Faraday, whether it appears at the moment to be pertinent or not. It seems to me of the utmost importance not to lose anything new that I see, and as I write I am continually seeing things that I had not seen before. In order not to lose them, I stick them down, even I am afraid at the expense of marring the logical coherence of the structure. As I see it the operational method involves a meticulous description of all one's observable activity. This I believe is the reason I put in various observations which, as you rightly point out, could be dispensed with.[199]

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196 Bridgman to Korzybski, July 24, 1927, PWBP, HUG 4234.12.
197 Bridgman to Korzybski, Nov. 13, 1927, PWBP, HUG 4234.12.
198 Bridgman to Bentley, Jan. 1, 1928, PWBP, HUG 4234.12.

To Bridgman, the *Logic* was a record of his observation and reflection. He wanted to write down whatever passed his mind, caring much about neither the book's readability nor its logical structure. On some later public occasions, he repeated the same statement that the book was of the nature similar to private notes on the reflection on the foundations of physics that had essentially been written for his own sake.[200]

Bridgman may have truly regarded the *Logic* as a private record of his reflection. Yet, he was aware that many other physicists probably shared the feeling that such a reflection was necessary. While the *Logic* was in the course of publication, Bridgman expressed this feeling in the book review of Lewis's *Anatomy of Science*: "Every physicist is becoming increasingly aware that one of the most important tasks both for him as an individual and for physics as a science is a reformulation of attitude toward fundamental concepts."[201] Though Bridgman probably wrote the *Logic* for his own satisfaction, his book and program represented the contemporary physicists' desire for the reform.

As to the incompleteness of the *Logic*, Bridgman often mentioned the time limit under which he wrote it. Explaining Bentley why he had not been able to discuss some apparently appropriate topics, he referred to this point:

> It is to be remembered that my Logic was written under a constantly felt time limit. I wrote it on a half sabbatical, and demandedn [*sic*] of myself that it be finished before returning to the laboratory. This meant that I had to decide pretty carefully how far back it was necessary to push the analysis to get what I wanted in my limit of time. It was the limit of time that prevented my going into such queations [*sic*] as the validity of

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199 Bridgman to Bentley, Dec. 14, 1936, PWBP, HUG 4234.10.
200 P. W. Bridgman, "P. W. Bridgman's 'The Logic of Modern Physics' after Thirty Years," pp. 518-519; *idem*, "The Present State of Operationalism," pp. 76-77.
201 P. W. Bridgman, review of *The Anatomy of Science*, *Physical Review*, 29 (1927), p. 349.

> the logica [*sic*] fundementeal [*sic*] to mathematics; I should judge from the way my later attempts in this direction have been received that this was fortunate.[202]

Bridgman assigned a surprisingly small space to the analysis of the concept of operation, and perhaps he should have given a more detailed explanation to his view of physical reality. However, the time limit prevented him from getting into these problems and other ones deserving careful discussion.

Because of the time limit and the author's writing habit, there were but a few references to other philosophers or scientists in the *Logic*. According to the following comment of "a distinguished psychologist and philosopher" asked by the publisher to review the manuscript of the *Logic* at Bridgman's request, Bridgman originally did not put any reference to the draft:

> I suggest that the author himself go over the manuscript with a view to supplying foot-note references and possibly textual explanations for the benefit of the reader who is not au courant with modern physical books and articles. He refers, quite casually, to this man and that man, to this phenomenon and to that effect, to this remark and to that generalization, without a single bibliographical note. He writes, in other words, as if he were addressing an audience of professors of physics rather than an educated public only vaguely and generally aware of what the professors of physics are doing. These additions would certainly make all the difference in the world to the saleableness of the book.[203]

As Bridgman wrote the draft basically for himself and completed it under a stringent time limit, he did not put any reference.

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[202] Bridgman to Bentley, Nov. 23, 1936, PWBP, HUG 4234.10.
[203] Hitchcock to Bridgman, Dec. 6, 1926, PWBP, HUG 4234.8. A. Moyer has suggested the possibility that this anonymous critique was John Dewey (A. Moyer, "P. W. Bridgman's Operational Perspective on Physics. Part I: Origins and Development," p. 239).

Furthermore, Bridgman was not aware of the influences of other scientists or philosophers.

In the "Preface" of the *Logic*, explaining the necessity of reflection on the foundations of physics, Bridgman mentioned philosophers and scientists whose works he had read before: "In spite of previous writings on the broad fundamentals by Clifford, Stallo, Mach, and Poincaré, to mention only a few, I believe a new essay of this critical character needs no apology."[204] Yet, he admitted no recognizable influence by any of these works: "None of the previous essays have consciously or immediately affected the details of this [book]; in fact I have not read any of them within several years. If passages here recall passages already written, it is because the ideas have been assimilated and the precise origin forgotten."[205] As has previously been mentioned (§1.1), in his high school years Bridgman read works of Karl Pearson, Mach, Poincaré, Clifford, and Stallo. Furthermore, when he conducted a seminar on the *Logic* at Columbia University in 1928, he included in the reading list[206] Stallo's *Concepts and Theories of Modern Physics,*[207] Clifford's *Common Sense,*[208] Mach's *Mechanics,*[209] and Poincaré's *La science et l'hypothèse.*[210] In the *Logic*, Bridgman mentioned works of William Clifford, Ernst Mach, Poincaré, Wilhelm Ostwald, N. R. Campbell, Eddington, Bertrand Russell, and Alfred North Whitehead.

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204 Bridgman, *The Logic of Modern Physics*, p. v.
205 *Ibid.*, p. vi.
206 Bridgman to Pegram, May 27, 1928, PWBP, HUG 4234.8.
207 J. B. Stallo, *The Concepts and Theories in Modern Physics* (New York: D. Appleton, 1881).
208 William Kingdon Clifford, *The Common Sense of the Exact Sciences* (London: Kegan Paul, 1885).
209 Ernst Mach, translated by James J. McCormack, *The Science of Mechanics: A Critical and Historical Exposition of Its Principles* (Chicago: Open Court, 1893). The original German version was, Ernst Mach, *Die Mechanik in ihrer Entwickelung*, 2. Aufl. (Leipzig: F. A. Brockhaus, 1889).
210 Henri Poincaré, *La science et l'hypothèse* (Paris: Flammarion, 1902).

It seems that he regarded their works as fundamental readings in philosophy of science.

However, in the *Logic,* Bridgman cited most of them only once, regarding rather minor points of the discussion. As we have seen in his comment on Poincaré's work, probably he did not give "any very serious thinking" to any of them. Some of them were cited only to be rebutted.

After the publication of the *Logic,* some readers pointed out a few possible misunderstandings regarding the citations. Bridgman admitted that he was uninformed of the recent works.[211] For example, to the comment that his reference to Whitehead might be misleading to readers who had not studied Whitehead for themselves, he replied: "With regard to Whitehead, I am afraid from what you say that I have done him an injustice. I have always found him most difficult to understand, and have therefore doubtless not always got his full meaning."[212] Like many of his contemporary scientists, Bridgman had a habit of citing other writers' works without sufficient care, which applied even to Whitehead, Bridgman's colleague at Harvard.

Since his student days, Bridgman enjoyed reading works of such philosophers as William James, Josiah Royce, George Santayana, and Hugo Münsterberg. Even after he started to work as an experimental physicist, he did not entirely cease to read philosophers' works. However, this does not necessarily mean that he understood or accepted them, as the above mentioned case of Whitehead shows.

After publishing the *Logic,* for example, he ventured to read Bertrand Russell's new book *The Analysis of Matter,*[213] but could not

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[211] Whyte to Bridgman, Aug. 4, 1927; Korzybski to Bridgman, Nov. 5, 1927, PWBP, HUG 4234.12.
[212] Bridgman to Whyte, Aug. 28, 1927, PWBP, HUG 4234.12.
[213] Bertrand Russell, *The Analysis of Matter* (London: Kegan Paul, Trench, Trubner, 1927).

help feeling that "it is rather sophisticated and far away from immediate contact."[214] After going through the book, he found the book "much more physical and therefore much more satisfactory and understandable than some of his earlier work."[215] However, Bridgman recognized a "great difference" between Russell's attitude and his own, which he regarded as "more typical of that of the average physicist." As we have seen, Bridgman realized that Russell discussed the physicist's way of understanding nature because of his interest in the process of making nature understandable, whereas Bridgman believed that physicists' interest was in discovering what new things could be done after nature had been made understandable. Bridgman did not pay attention to philosophical works that had no suggestion for scientists' practice. It is, therefore, understandable that to Herbert Feigl, a young Viennese philosopher who, after studying with Moritz Schlick, visited Bridgman in September 1930, Bridgman appeared "philosophically not too well informed."[216]

Surprised that the *Logic* found more readers among philosophers than among scientists, Bridgman confessed to Bentley his dismay and philosophical unpreparedness:

> My book, I find, has placed me in a position which is sometimes embarrassing. I am in no sense a philosopher, and have little appreciation or understanding of its fundamental problems. I went through a stage of interest in philosophy while in college, but found myself totally unable to make contact with it, and came away from that experience with the feeling that there were many things with which I could occupy myself with greater profit. Yet I find that my book has aroused far more interest among those who are philosophically inclined than among the

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214 Bridgman to Korzybski, Oct. 16, 1927, PWBP, HUG 4234.12.
215 Bridgman to Korzybski, Jan. 29, 1928, PWBP, HUG 4234.12.
216 Herbert Feigl, "The Wiener Kreis in America," in Donald Fleming and Bernard Bailyn, eds., *The Intellectual Migration: Europe and America, 1930-1960* (Cambridge, Mass.: Harvard University Press, 1969), pp. 630-673, p. 663.

> physicists; I am a little disturbed by this because I think that the book really had something of importance to say for the physicist. The result of this background of mine is that I do not feel myself capable of responding to many of the comments that have been made on the book without the expenditure of much time and thought in order to acquire an attitude on many questions which I could contemplate with intellectual satisfaction, but which for my immediate purpose are more or less unessential.[217]

As we will see later, before the publication of the *Logic*, Bridgman expected at least some responses from philosophers. However, their reaction turned out to be far greater than he imagined. Regardless of the author's preparation, the *Logic* would later even arouse some philosophical discussions in which Bridgman reluctantly participated. As he gradually became known as the originator of a philosophical movement called "operationalism," more and more people asked him to give a comment or speech on topics that had been unfamiliar to him. Though he gradually and awkwardly learned how to play the role of a Harvard professor of physics who invented a philosophy that reduced everything to actual operations, those who attempt to examine his "philosophy" may have to think of the embarrassment of an experimental physicist whose work happened to attract philosophers' unreserved attraction regardless of his own wish.

It may be appropriate to mention some of the possible predecessors who maintained views of science similar to Bridgman's. Regarding this point, Maila Walter has referred to two nineteenth-century scientists, the chemist and mathematician Benjamin Brodie and the mathematician Giuseppe Peano.[218] Brodie attempted to use chemical operations to define chemical elements and compounds, while Peano, intending to build the foundations of

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[217] Bridgman to Bentley, Jan. 1, 1928, PWBP, HUG 4234.12.
[218] Walter, *op. cit., Science and Cultural Crisis*, p. 114.

mathematics in terms of physical experience, tried to define numbers as operations. However, as Walter herself has commented, it is difficult to imagine that Bridgman was familiar with either of their discussions. Albert Moyer has pointed out that during the first two decades of the twentieth century, an MIT professor William S. Franklin published a very similar view to Bridgman's, and that they at least exchanged technical information by 1920. In several articles,[219] Franklin stated that the definition of fundamental concepts should be physical operations, even mentioning that "operational philosophy *is* physics." However, no documentation has been left that suggests that they exchanged their operational ideas. It is thus difficult to find the direct predecessor of Bridgman. One may feel reluctant to make an effort in this direction, when reflecting on the course of slow but steady development of Bridgman's view of physics, from the scrutiny of dimensional analysis in the 1910s to the publication of *The Logic of Modern Physics* in 1927.

4.2.7. Publication

The publication of the *Logic* went far smoother than that of *Dimensional Analysis*. On October 20, 1926, Bridgman asked the Macmillan to examine his 247-page manuscript titled "The Conceptual Foundation of Our Modern Physics." [220] One month later, the Macmillan agreed to publish it and sent the manuscript to "a distinguished psychologist and philosopher" for examination, as Bridgman was eager to know "what a philosopher would be likely to say

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[219] Franklin, "Popular Science," *Proceedings of the American Academy of Arts and Sciences*, 52 (1902-03), pp. 351-365; "Operative versus Abstract Philosophy in Physics," *Science*, 63 (1926), pp. 623-625.
[220] Bridgman to The Macmillan Company, Oct. 20, 1926, PWBP, HUG 4234.8.

about the discussion."[221] Though he would later be dismayed by philosophers' enthusiasm over the *Logic*, Bridgman had a little itch for philosophers' comment when he had just completed the draft.

The anonymous reader's comment was favorable, despite the author's apprehension: "This home-grown character of his logic can not, however, be qualified as a disadvantage. I find, on the contrary, that it is extraordinarily interesting to trace the way in which a working physicist of the present day seeks to rationalize his subject-matter."[222] Only the necessity to add footnotes and to modify some technical parts was suggested. Meanwhile, Bridgman himself asked a friend of his and philosopher R. F. Alfred Hoernlé to review the manuscript and received similar comments. He found Hoernlé's comments relieving and flattering: "It is a great gratification that the book did not strike a philosopher as absolutely foolish, or as been having already said many times before, for I really stand somewhat in awe of the philosophical tribe in spite of some impressions to the contrary which you may have got from certain passages in the book."[223] Bridgman did not intend to write the book for philosophers, but was cautious enough not to publish something philosophically pointless.

The "promotion people" of the Macmillan, who pointed out that the original title, "the Conceptual Foundations of Modern Physics," was "both too long and unnecessarily forbidding,"[224] suggested two other possibilities: "Physics and Modern Thought" and "Concepts of Modern Physics." Bridgman accepted neither of them. The former was "not an accurate enough description of what the book is," while the latter had

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[221] Hitchcock to Bridgman, Dec. 6, 1926, PWBP, HUG 4234.8.
[222] *Ibid.*
[223] Bridgman to Hoernlé, Dec. 26, 1926, PWBP, HUG 4234.8.
[224] Hitchcock to Bridgman, Jan. 3, 1927, PWBP, HUG 4234.8.

"already been used by Stallo."[225] In their stead, he proposed five suggestions, "arranged in their order of preference":

1. The Metaphysics of a Physicist.
2. Reality in the Concepts of Physics.
3. The Meaning of the Concepts of Physics.
4. The Nature of the Concepts of Physics.
5. Critique of the Concepts of Physics.[226]

The suggested titles, especially the first two, describe what Bridgman himself regarded as the main theme of the book. To him, the book was of a metaphysical nature whose main topic was "reality" of scientific concepts. However, after several exchanges of letters and cables, the Macmillan decided to adopt none of them and finally suggested "The Logic of Modern Physics." Bridgman and the company agreed upon this title on January 11, 1927.[227]

Immediately before the publication, when asked to list up special professions or organizations that were likely to be interested in the book, Bridgman replied thus: "Notices should be sent to physicists, philosophers, and probably mathematicians."[228] He added physical chemists to the list and suggested to send notices to all members of the American Physical Society and to Section B of the American Association for the Advancement of Science. He could give information of the equivalent organizations for mathematicians (the American Mathematical Society, the Mathematical Association of America, and Section A of the AAAS), but knew no organization of philosophers. Though expecting some readers of his book among philosophers, Bridgman was no union card philosopher himself.

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[225] Bridgman to Hitchcock, Jan. 5, 1927, PWBP, HUG 4234.8.
[226] *Ibid.*
[227] The Macmillan to Bridgman, Jan. 11, 1927, PWBP, HUG 4234.8.
[228] Bridgman to the Macmillan, Mar. 20, 1927, PWBP, HUG 4234.8.

Furthermore, when the publisher asked him for suggestions as to what special points they should emphasize in advertising the book, Bridgman replied that the book was primarily for physicists:

> With regard to points to be made in advertising, it should not be lost sight of that this book is written primarily for physicists by an experimental physicist, and that the philosophical questions which are treated have been forced on our attention by the experimental situation. Such a book is naturally of interest for philosophers as well as physicists, but it was not written primarily with the philosopher in view. In some of the advance advertising which I have seen, only the philosopher is mentioned, as when in your spring announcement the book is listed in the section of Philosophy. It would be most unfortunate if this possible impression as to the character of the book is not rectified in your later announcements.[229]

Bridgman intended the book for physicists and noticed its philosophical vulnerability. However, regardless of the author's original intention, the publisher shrewdly estimated its commercial value and advertised it mainly to philosophers.

4.2.8. Responses and Reviews

The commercial strategy of the Macmillan turned out to be correct. Six months after the publication, the Macmillan reported to Bridgman that the *Logic* was "moving very well": "[W]e hope before we are through to sell a good many of them, considering the limitations of the market."[230] In fact, by July 1928, it had sold more than two thousand copies.[231] Furthermore, its reputation was high: The American Library

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[229] Bridgman to the Macmillan, Mar. 20, 1927, PWBP, HUG 4234.8.
[230] Hitchcock to Bridgman, Oct. 21, 1927, PWBP, HUG 4234.12.
[231] Annual Statement of Royalty Account," Macmillan Co., July 29, 1927, and July 28, 1928, PWBP, HUG 4234.12.

Association included it among the "Forty Notable American Books of 1927," compiled for the League of Nations.[232]

However, already in June 1927, Bridgman found that the book was not very popular among physicists: "[I]t looks to me now as though the book is not going to attract the attention from physicists that I wished it might."[233] As we have seen, this impression became clearer by the beginning of the next year, when he admitted to Bentley that the book, though mainly written for physicists, attracted more interest among philosophers than among physicists.[234]

Among the reviews of the *Logic* and the letters from the readers one can find philosophers' quick, sensitive, though not always favorable, responses. A professor of philosophy at Brown University C. J. Ducasse wrote Bridgman that he had found himself "in very close agreement with" the *Logic*, using it in his seminar in the philosophy of science.[235] Furthermore, E. B. McGilvary of the University of Wisconsin used the book in a seminar in the philosophy of science in the spring of 1928 at Madison and in the course he delivered at Columbia University for the summer. McGilvary intimated Bridgman that he was "almost always siding with" Bridgman in the "many lively discussions" on the problems the book suggested.[236]

Philosophers' criticisms were mostly on the operational way of defining concepts. In a review published in *Mind*,[237] L. J. Russell, a professor of philosophy at the University of Birmingham, pointed out the extremeness of Bridgman's idea of defining concepts in terms of operations, writing that he had gone "whole hog with his view": "A

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[232] Advertisement for Macmillan Co., PWBP, HUG 4234.12.
[233] Bridgman to Klyce, June 12, 1927, PWBP, HUG 4234.12.
[234] Bridgman to Bentley, Jan. 1, 1928, PWBP, HUG 4234.12.
[235] Ducasse to Bridgman, April 25, 1928, PWBP, HUG 4234.12.
[236] McGilvary to Bridgman, March 29, 1928; Bridgman to McGilvary, April 8, 1928, PWBP, HUG 4234.12.
[237] L. J. Russell, Review of the *Logic*, *Mind*, 37 (1928), pp. 355-361.

purely operational view of the concept is impossible: and Prof. Bridgman shows it by not sticking to it."[238] Paul Weiss, a young philosopher entering the Graduate School at Harvard, described that Bridgman's way was "parsimonious with a vengeance."[239] A. Cornelius Benjamin at the University of Illinois warned that one could accept the operational definition only with "a word of caution": "It must not be supposed that the operational definition is to *replace* the formal definition, however valuable it may be as a supplement."[240]

However, all of the reviewers recommended the *Logic*: Benjamin stressed that it was "a book which no serious student in the philosophy of science can afford to overlook"[241]; Weiss, comparing it to Kant's work, concluded his review by stating that "an important part of the [modern] prolegomena is already completed"[242]; and even Russell, who was most critical to the *Logic* among the philosopher-reviewers, mentioned that his review "has failed of its purpose if it has not brought out the very great value and suggestiveness of Prof. Bridgman's treatment of the subject,"[243] recommending it "both to the student of scientific method and to the investigator in the laboratory."[244]

Reviews by scientists were favorable in general, too. J. Robert Oppenheimer wrote that the *Logic* was "urgently recommended to those interested in physics" and that though "[t]he book was written before the development of the quantum mechanics," "it may be noted that this theory conforms very closely to Professor Bridgman's predictions."[245]

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238 *Ibid.*, p. 356.
239 Paul Weiss, "A Critique of Science," *Nation*, 125 (1927), pp. 115-116.
240 A. Cornelius Benjamin, Review of the *Logic*, *Journal of Philosophy*, 24 (1927), pp. 663-665.
241 *Ibid.*, p. 665.
242 Weiss, "A Critique of Science," p. 116.
243 Russell, *Mind*, 37 (1928), p. 360.
244 *Ibid.*, p. 361.
245 J. R. Oppenheimer, Review of the *Logic*, *Physical Review*, 31 (1928), pp. 145-146, p. 145.

Quoting Heisenberg's phrase "Die Bahn ensteht erst dadurch, dass wir sie beobachten [The orbit comes into existence first through the fact that we observe it]," Oppenheimer concluded that "[t]hat is the operational definition of the electronic orbit."[246] R. C. Tolman, then working with Oppenheimer at the California Institute of Technology, told Bridgman about people's enthusiasm over the *Logic*, though he himself had not read it yet.[247] As usual, he did not praise Bridgman too much: "I think there are some parts of it that I should probably like very much and some that I should disagree with. I get the impression that you stress the desirability of our proceeding in theoretical physics so that we shall not make mistakes; it seems to me, however, far more important to proceed so that we shall make progress even with mistakes."[248] The reviewers of other journals, such as Frederick E. Beach, an associate editor of the *American Journal of Science*, supported Bridgman's view with almost no reservation and recommended the book to "all those interested in the philosophy of physics or, so to speak, the pictures which we try to form of nature and the models which we conceive to reproduce its operations."[249]

Other than the professional journals, favorable comments on the *Logic* appeared in such well-read magazines and newspapers as *Saturday Review of Literature*[250] and the *New York Times*.[251] To general readers, however, what authorized the value of the book might have been Bridgman's title, the Hollis Professor of Mathematics and Natural History at Harvard University, the oldest scientific chair in America, as

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[246] *Ibid.*, p. 146.
[247] Tolman to Bridgman, March 19, 1928, PWBP, HUG 4234.8.
[248] *Ibid.*
[249] F. E. B[each], Review of the *Logic, American Journal of Science*, 214 (1927), p. 326. See also: P. A. Constantinides, Review of the *Logic, School Science and Mathematics* (1929), pp. 668-670; and "De Omnibus Rebus et Quibusdem Aliis," Review of the *Logic, Quarterly Review of Biology* (1927), p. 578.
[250] Albert Parsons Sachs, *Saturday Review of Literature*, Aug. 6, 1927, p. 24.

the reviewer of the *San Francisco Chronicle* shrewdly observed: "Professor P. W. Bridgman of Harvard holds an enviable position in the world of science; therefore, when he writes a book on 'The Logic of Modern Physics' it behooves the follower of science, be amateur or professional, to listen."[252]

Reviews of the *Logic* were published in foreign papers and magazines, too. An English newspaper *London Observer* responded quickly to its publication, publishing an encouraging appraisal of the book.[253] Bridgman found it "the best thing" he had ever seen, explaining the reason to the editor of the Macmillan: "I am particularly pleased with it because it is evident that the writer is familiar with modern physical developments, which was not so apparent in the case of the writers of some of the other reviews."[254] Harold Jeffreys, a reviewer of the *Nature* and Cambridge astronomer and geophysicist, was exceptionally critical to the *Logic* for a scientist. To him, Bridgman's attempt "seems to be a case where pure phenomenalism is impracticable."[255] He believed that "a critical attitude towards fundamentals does not mean that we must deny the existence of anything we cannot perceive directly."[256] A review by a German philosopher of science Hugo Dingler, published in the *Physikalische Zeitschrift*, was much more supportive.[257] Although Bridgman would later find himself critical toward Dingler's apparently operational method (§5.1), Dingler was happy with the *Logic*, finding a physicist maintain a view similar to his own one.

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251 *New York Times Book Review*, Mar. 4, 1928, p. 38.
252 W. A. Brewer, Jr., "New Schemes of Measurement Set for Physics," *San Francisco Chronicle*, July 24, 1927; copy in PWBP, HUG 4234.12.
253 L. L. W., "The Physical Universe," *London Observer*, Oct. 2, 1927.
254 Bridgman to Hitchcock, Oct. 30, 1927, PWBP, HUG 4234.12.
255 Harold Jeffreys, "The Validity of Modern Physics," Review of the *Logic*, *Nature*, 121 (1928), p. 86-87, p. 87.
256 *Ibid.*

It seems that, after the publication of the *Logic*, Bridgman became more famous as a philosopher of science than as an experimental physicist, not only among general public but also among academics. Physicists and philosophers started to invite him to deliver courses in philosophy of science: In 1928, Bridgman gave a course on the *Logic* at Columbia University for the summer session; and in 1930, though he did not accept it, he was offered a lectureship during his sabbatical for a half course on "metaphysics" in his book at the Philosophy Department at the University of California at Berkeley.[258] In 1929, when William Malisoff started a project to launch a new journal, to be titled the *Philosophy of Science*, Bridgman, along with R. A. Millikan and G. N. Lewis, was invited to be one of the contributing editors.[259]

Furthermore, as we will see later (§5.1), the two European philosophers Dingler and Feigl independently came up with a proposal of translating the *Logic* into German.[260] After all, Dingler arranged publication of the German edition,[261] but Feigl played an even more important role in circulating Bridgman's name as a philosopher of science.

Feigl, then a young member of the Vienna Circle, had studied philosophy of science with Moritz Schlick and had obtained his doctorate from the University of Vienna in 1927. In 1929, he published a book on the relation between logic and empirical factors in science *Theorie und Erfahrung in der Physik*.[262] Feigl later recalled that probably through Albert E. Blumberg, an American graduate student

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257 Hugo Dingler, Review of the *Logic*, *Physikalische Zeitschrift*, 29 (1928), p. 710.
258 Pegram to Bridgman, Oct. 31, 1927; Wilson to Bridgman, May 6, 1930, PWBP, HUG 4234.8.
259 Malisoff to Bridgman, April 18, 1929; Bridgman to Malisoff, May 17, 1929, PWBP, HUG 4234.8.
260 Bridgman to Dingler, March 31, 1931, HUG 4234.10.
261 P. W. Bridgman, trans. W. Kamph, *Die Logik der heutigen Physik* (Munich: Max Huebner, 1932).

studying in Vienna, he and his colleagues had come to know Bridgman's *Logic.* He found Bridgman's operational analysis of physical concepts akin to "the positivistic view of [Rudolf] Carnap, [Philipp] Frank, and [Richard] von Mises, and even to certain strands of [Ludwig] Wittgenstein's thought."[263] Around 1930, Feigl decided to study in the United States and applied for a Rockefeller Scholarship. He wrote Bridgman about his wish to study at Harvard:

> I am at present engaged in research work concerning the logic and methodology of contemporary physics and should appreciate very much the opportunity of continuing my work under your guidance at Harvard next year. [...] [I]t has been a rare pleasure to me to find a physicist who at the same time has so interestingly contributed to the logic of physics. It would be of great advantage to me to be able to continue my work under your auspices. The desirable further training in pure physics would thus be combined with the unusual opportunity of having my more general result subject to the competent and understanding scrutiny of a physicist. I should, of course, not impose too much upon your time, but should be quite thankful for the occasional privilege of discussing my work with you.[264]

This letter was accompanied by a recommendation of Feigl's advisor in Vienna, Schlick:

> I do not have the pleasure of knowing you personally, but I have read your delightful book on the Logic of modern Physics [*sic*] and some of your other work on the general aspects of physics. My friends and students here are, like myself, greatly interested in the epistemological foundations of natural science, and I take the liberty of asking you on behalf of one of my former students, Dr. Herbert Feigl, if there is any possibility of his doing some work in this field under your auspices at Harvard.[265]

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[262] Herbert Feigl, *Theorie und Erfahrung in der Physik* (Karlsruhe: G. Braun, 1929).
[263] Feigl, "Wiener Kreis in America," p. 645.
[264] Feigl to Bridgman, March 27, 1930, PWBP, HUG 4234.8.

The Logical Positivists assumed that the author of the *Logic* was active in philosophy of science. They were eager to interact with an experimentalist-philosopher on the other side of the Atlantic who published a philosophical view of science similar to theirs.

Before Feigl's application, Bridgman had another chance to know Feigl as the author of *Theorie und Erfahrung in der Physik*. The Macmillan, suggested by Einstein to publish the English translation of the book, asked Bridgman in November 1929 for his opinion as to its value and probable appeal to the public.[266] Doubting its commercial value, Bridgman did not recommend its translation.[267] Though the Macmillan referred Bridgman to the book's author Feigl and his advisor Schlick, Bridgman does not seem to have remembered them well; in March 1930, having received Schlick's recommendation for Feigl, Bridgman asked a Harvard philosopher W. Ernest Hocking about Schlick's reputation as a philosopher of science. Hocking guaranteed that "a student sent by him would be well prepared in the philosophy of science."[268] At the same time, Bridgman cautioned Feigl not to raise his expectation improperly, although he was pleased with Feigl's desire to study under him:

> I think that I ought to say a few words of introduction of myself to you, in order that you may not come with false expectations, or be disappointed when you get here. My work on fundamental questions on physics has been entirely outside my formal acedemic [*sic*] activities. My book was written during my sabbatical leave; nearly all my time is occupied with the many details of my experimental work, and in particular at present I am engaged in writing a book collecting my experimental work on high pressures of the last 20 years. I do not offer any courses of instruction on such topics as would be suggested by

[265] Schlick to Bridgman, Feb. 28, 1930, PWBP, HUG 4234.8.
[266] Titterton to Bridgman, Nov. 9, 1929, PWBP, HUG 4234.8.
[267] Bridgman to Titterton, Nov. 11[?], 1929, PWBP, HUG 4234.8.
[268] Hocking to Bridgman, March 27, 1930, PWBP, HUG 4234.8.

my book but my set courses deal with the mathematical theory of electricity and magnetism (macroscopic theory), special relativity, and thermodynamics, and any discussion of fundamentals which I give is entirely incidental in these courses and consumes only a very short time. Any help with your own problems that you could get from me would have to be by private conversation. I may say, however, that I am intensely interested in these questions, and would welcome the opportunity of discussing them with some one who is devoting serious attention to them, and I would expect to derive much profit from this opportunity. I am particularly glad that your training has included so much physics; it seems to me that without this it is easy to waste a great deal of time.[269]

Having no one at Harvard to talk with about his ideas on the foundations of science, Bridgman could take advantage of Feigl's visit. Although Bridgman did not find logical positivism totally acceptable, Feigl opened an intellectual communication between Bridgman and such logical positivists as Carnap and Frank, which lasted until his very last years.[270]

When Feigl arrived in the United States, the Harvard psychologists and philosophers belonged to the same department. Feigl therefore played an important role in bringing Bridgman's idea to the psychologists' circle, too.[271] After the mid-1930s, the faculty members in psychology, E. G. Boring and S. S. Stevens, started to introduce a methodology similar to Bridgman's to psychology, forming one trend among American psychologists.[272] Another trend of behaviorism

[269] Bridgman to Feigl, March 23, 1930, PWBP, HUG 4234.8.
[270] For the responses of logical positivists and behaviorists to Bridgman's operational view and their interaction with Bridgman, see Walter, "Chapter 7. The Positivists and the Behaviorists: Defining Away Private Experience," *Science and Cultural Crisis*, pp. 163-198.
[271] For Feigl's interest in psychological problems, see: Feigl, "Logical Analysis of the Psycho-physical Problem," *Philosophy of Science*, 1 (1934), pp. 420-445.
[272] E. G. Boring, *The Physical Dimensions of Consciousness* (New York: Century Co., 1933); "Temporal Perception and Operationism," *American Journal of Psychology*, 48 (1936), pp. 519-22. S. S. Stevens, "The Operational Basis of Psychology," *American Journal of Psychology*, 47 (1935), pp. 323-330; "The Operational Definition of

stressing the importance of verbal behavior of human beings was later originated by B. F. Skinner, who had earned his doctorate from Harvard in the beginning of 1930. Though Skinner's behaviorism did not share much with Bridgman's operationalism as the others did, Skinner later recalled that, along with Bertrand Russell, Bridgman had an influence upon the young psychologist's reflection on the foundations of psychology.[273] Bridgman was not entirely happy with behaviorists' interpretations of his operational view, nor was his influence essential in the formation of behavioristic tenet among the American psychologists. However, Bridgman at least "gained support" for behaviorism and offered the "nucleus around which the operationist movement crystallized"[274] by presenting an operational methodology in physics, which had been "always scientific psychology's model"[275] (for details, see Chapter 6).

Not much has been known about the influence of Bridgman's operational view upon social scientists. In 1961, when preparing Bridgman's obituary, Kemble asked some social scientists including Henry Kissinger, who had attended Bridgman's seminar in 1953-54, to report on their evaluations of Bridgman's influence upon social science. Their replies revealed that the social scientists did not appreciate the operational view as much as the behaviorists in psychology and the logical positivists in philosophy did.[276] However, two years after the publication of the *Logic*, Henry Schultz, a professor of economy at the

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Psychological Concepts," *Psychological Review*, 42 (1935), pp. 517-527; "Psychology, the Propaedeutic Science," *Philosophy of Science*, 3 (1936), pp. 90-103. A historical overview of the trend in American psychologists during the 1930s was detailed by S. S. Stevens, in "Psychology and the Science of Science," *Psychological Bulletin*, 36 (1939), pp. 221-262.

[273] B. F. Skinner, *The Shaping of a Behaviorist* (New York: Knopf, 1979), pp. 66-68.

[274] Walter, *Science and Cultural Crisis*, p. 178.

[275] E. G. Boring, *A History of Experimental Psychology*, 2nd ed. (New York: Appleton-Century-Crofts, 1950), pp. 653-659.

[276] Bruner to Kemble, Dec. 14, 1961; Kissinger to Kemble, Dec. 6, 1961, ECKP, HUG

University of Chicago, wrote to Bridgman about his enthusiasm over Bridgman's operational view:

> For the last decade or so, I have been trying by word and deed to popularize what you have so well termed the "operational point of view" among my co-workers in the field of economics.
>
> I do not know how I came to adopt the operational attitude. Like Topsy, it just grew up. I suppose, however, that I was influenced by the writings of John Dewey, Charles S. Peirce, Ernst Mach, Henri Poincaré, and Karl Pearson. (I studied under Dewey and Pearson.)
>
> I was not making many converts to the operational viewpoint among economic theorists, and was beginning to feel somewhat discouraged, when your book on The Logic of Modern Physics appeared. It was just what I needed. It helped me to clarify my thoughts, it provided me with authoritative illustrations of the operational procedure in the physical science and—this is very important—it indicated the *social consequences* of a general and thoroughgoing adoption of this point of view. Now I rarely miss an opportunity to refer to your book and I try to the best of my ability to re-examine the accepted economic concepts from the operational viewpoint.[277]

One of Schultz's students at Chicago and future Nobel Prize Winner in economics, Paul A. Samuelson, came to Harvard for his graduate work during the 1930s and developed a quantitative method in economics. His econometric approach, though not totally under the influence of Bridgman's and Schultz's operational views, reflected their emphasis on the operational definition of concepts. In the introduction of his first book, *Foundations of Economic Analysis*, originally subtitled "The Operational Significance of Economic Theory,"[278] he asserted the importance of operational methodology in economics: "[O]nly the smallest fraction of economic writings, theoretical and applied, has been

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[277] Schultz to Bridgman, Feb. 13, 1929, PWBP, HUG 4234.12.

[278] Paul Samuelson, *Foundations of Economic Analysis*, Harvard Economic Studies, Vol.

concerned with the derivation of *operationally meaningful* theorems. In part at least this has been the result of the bad methodological preconceptions that economic laws deduced from *a priori* assumptions possessed rigor and validity independently of any empirical human behavior."[279] In this volume, one of his purposes was to "show that there do exist meaningful theorems in diverse fields of economics affairs" and reach "definite *operationally meaningful* theorems."[280]

As *The Logic of Modern Physics* attracted people's attention to its author, Bridgman suddenly started to be exposed to the controversies over methodology of science.[281] The philosophical vulnerability of his operational view was gradually revealed through the interaction with philosophers and psychologists that lasted for the rest of his life. The impact of Bridgman's operational scrutiny was not very strong for the philosophers or psychologists: they welcomed the *Logic* because, in a sense, they had long been prepared to accept any type of approval of their empirical methodology by scientists at work. The *Logic* furnished them with hints and support for what they had been forming among themselves, in their own contexts.

The interaction may not have been very fruitful to Bridgman, either. He was neither prepared for nor interested in the approval or disapproval from people outside physics. Though he was cautious of philosophers' opinions of the *Logic* in order not to publish something philosophically absurd, his main concern in the *Logic* was to show the most appropriate way to reform contemporary physics and thereby to help build a new theory in the microscopic realm. Bridgman addressed his reform program mainly to physicists. Thus, no criticism by

80 (Cambridge: Harvard University Press, 1947), p. vii.
[279] *Ibid.*, p. 3.
[280] *Ibid.*, p. 5.
[281] Maila L. Walter, *Science and Cultural Crisis*, pp. 163-193.

philosophers and psychologists seemed to succeed in altering Bridgman's view. Though Bridgman would later develop his own attempt to apply the operational method to the general problems connected with society, moral, and humanity, one may feel difficulty in finding out how the interaction with philosophers and psychologists affected his discussion in this line. It seems likely that Bridgman stubbornly maintained his own way of reasoning that he learned from physics even when he ventured to discuss matters outside physics.

How, then, were the physicists' responses to Bridgman's operational perspective? Before getting into this problem, it will be appropriate to analyze the impact of quantum mechanics to Bridgman's operational view, mainly for two reasons: As Oppenheimer's review of the *Logic* showed, the American physicists mostly appreciated the *Logic* in connection with the question of the physical interpretation of the physical meaning of quantum mechanics; and, soon after the publication of the *Logic*, Bridgman was to modify his operational perspective in light of Heisenberg's principle of uncertainty. In the following chapter, I will discuss how Bridgman assimilated the epistemological implications of quantum mechanics and how the physicists responded to Bridgman's operational view. Through these discussions, I will also reveal the part of Bridgman's view of modern physics that he did not detail in the *Logic*.