Chapter 5. Quantum Mechanics and the Transformation of Operationalism

While preparing the draft of *The Logic of Modern Physics* in 1926, Bridgman knew only a small part of quantum mechanics. With this limited knowledge, he expected that this new theory in atomic physics would satisfy his operational requirement. Before quantum mechanics appeared, he could regard only the special theory of relativity as physical theory that met his operational standard. Bridgman was not alone in recognizing quantum mechanics as an operational theory. As we have seen in Chapter 4, young American physicists like J. Robert Oppenheimer, who were actively working in quantum physics, considered quantum mechanics as constructed in the operational way, welcoming Bridgman's operational perspective as a guideline for explicating the meaning of unfamiliar concepts introduced by quantum mechanics.

In this chapter, I will first discuss Bridgman's effort to understand the new concepts that quantum mechanics presented, especially the meaning of Heisenberg's uncertainty principle, and then examine how other American physicists interpreted the operational reasoning in connection with the reception of quantum mechanics.

5.1. Comprehending Uncertainty

5.1.1. The Encounter with Uncertainty

Bridgman completed the manuscript of *The Logic of Modern Physics* while spending his sabbatical half year in Europe. Before leaving America for Europe, he audited Born's lectures at the Massachusetts Institute of Technology and Harvard, learning of new-born matrix mechanics directly from one of its founders. Furthermore, Bridgman attended a few conferences held in Germany in 1926. Even after submitting the typescript of the *Logic* to the Macmillan, Bridgman continued to survey the literature on quantum mechanics. His bibliographical note, probably written in 1927,[1] shows that he had known by 1927 matrix mechanics founded by Heisenberg, Born, and Jordan, Schrödinger's wave mechanics, and Born's application of wave mechanics to collisions between particles. He was also aware of Heisenberg's attempt to describe matrix mechanics only with observable physical quantities. His letter to Korzybski written on September 24, 1927 shows that this knowledge had not substantially changed what Bridgman expected quantum mechanics to be in the *Logic*:

> I am not sure that I can agree with your idea that our difficulties with quantum theory are mostly verbal, and that if a new language could be invented the difficulties would in a large measure disappear. Perhaps this is true from a superficial point of view, but if you ask why it is that our present verbal habits are not adapted to these new phenomena, I think you will have to answer that it is because ouy [*sic*] physical experience, which in the last analysis has determined our verbal habits, has not been broad enough.[2]

Bridgman had not yet known of the philosophical debate on the interpretation of quantum mechanics. Unable to imagine what kind of epistemological difficulty it might present, he only repeated almost the same statement in the *Logic* as to the construction of quantum mechanics, reducing its apparent difficulty to the general one that could arise between new experimental results and conventional verbal habits.

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[1] Bibliographical notes on quantum mechanics, undated, handwritten, PWBP, HUG 4234.15.
[2] Bridgman to Korzybski, Sept. 24, 1927, PWBP, HUG 4234.12.

Around the same time, controversy over the physical interpretation of quantum mechanics started in Europe. In September 1927, at the Volta Centenary held at Como, Italy, Niels Bohr delivered his first lecture on complementarity between classical and quantum-mechanical descriptions. In the following October, at the Fifth Solvay Conference, Einstein publicly criticized the quantum-mechanical way of describing physical phenomena. Bridgman missed a precious chance to attend active debates on quantum mechanics; despite the honorable invitation to the Como Conference, he could not attend it. However, through his friends who attended these conferences, he received sufficient information: R. C. Tolman was invited to the Como Conference and gave a talk; E. C. Kemble spent seven months in 1927 in Munich and Göttingen; and J. R. Oppenheimer continued his study in Europe. On October 16, Bridgman wrote to Korzybski about Werner Heisenberg's paper on the uncertainty principle,[3] enthusiasm of physicists around him over this principle, and his own interpretation of its importance:[4]

> [H]ave you seen Hiesenberg's [*sic*] paper in Zeitschrift fur Physik, vol. 43, on the phenomenological aspects of quantum theory? Several of my physicist friends who were in Europe during the summer say that it has made a tremendous impression, and is apparently taking everyone by storm. I have read it, and would give a somewhat different interpretation to the various facts which he considers so fundamental; I would prefer to say that the new quantum facts are giving us positive reason to state that the old concepts of space and time cannot be carried down to small scale phenomena, and that probably the number of operationally dependent concepts is to be reduced by one, and that the constant h is in some way connected with this reduction.

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[3] Heisenberg, "Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik," *Zeitschrift für Physik,* 43 (1927), pp. 172-198.
[4] Bridgman to Korzybski, Oct. 16, 1927, PWBP, HUG 4234.12.

Bridgman started to notice implications of quantum mechanics in operations in microscopic phenomena. In his interpretation, Heisenberg only contended that the concepts of position and its corresponding momentum were not independent of each other, and that their mutual dependence had something to do with the Planck constant. Bridgman was satisfied with the result of application of his favorite operational analysis to Heisenberg's uncertainty principle and did not understand why other physicists made a fuss about it.

In the paper Bridgman mentioned, Heisenberg derived the relation between the uncertainties of the position coordinate and its corresponding momentum, δq and δp,

$$\delta q\, \delta p = h/2\pi,$$

assuming a Gaussian error curve and the commutation relation $pq - qp = h/2\pi i$. Furthermore, he outlined his famous thought experiment, the measurement of the position and momentum of an electron by the γ-ray microscope: although one can in principle determine the position or coordinate q of an electron to any desired degree of accuracy, because of the Compton effect the impact of the light quantum upon the electron changes the momentum of the electron, thus bringing about the relation between the uncertainties of the position and momentum derived above. Heisenberg could save the meaning of classical concepts in quantum mechanics only within a certain limit: "All the concepts that are used in the classical theory for the description of a mechanical system can also be defined exactly for atomic processes. But the experiments that lead to such definitions carry with them an uncertainty if they involve the simultaneous determination of two canonically conjugate quantities."[5] In a similar way, he went on, such a concept as "orbit" could be saved, as "the orbit comes into existence through the fact that we observe it."[6] He then drew the conclusion:

> We have not assumed that quantum theory, unlike classical physics, is essentially a statistical theory in the sense that from exact data only statistical conclusions can be inferred. For such an assumption is refuted, for example, by the well-known experiments by Geiger and Bothe. However, in the strong formulation of the causal law "If we know exactly the present, we can predict the future," it is not the conclusion but the premise that is false. We cannot know, as a matter of principle, the present in all its details. [...] In view of the intimate connection between the statistical character of the quantum theory and the imprecision of all perception, it may be suggested that behind the statistical universe of perception there lies hidden a "real" world ruled by causality. Such speculations seem to us—and this we stress with emphasis—useless and meaningless. For physics has to confine itself to the formal description of the relations among perceptions.[7]

Heisenberg thus asserted that the causality did not hold in microscopic phenomena as one cannot know "the present in all its details." Reading Heisenberg's paper carefully, one cannot overlook that the uncertainty principle presents serious limitation to Bridgman's operational view.

Though having been known as a philosopher of science since the publication of the *Logic*, Bridgman spent most of his time in daily high-pressure experiment at his laboratory and not so much in thinking over abstract problems. However, he had to continue to reflect on some fundamental problems in physics including the implications of the uncertainty principle, as he let himself in for a set of five lectures on the

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[5] Heisenberg, "Über den anschaulichen Inhalt," p. 179.

[6] *Ibid.*, p. 185. The original sentence reads: "Die Bahn entsteht erst dadurch, daß wir sie beobachten."

foundations of physics at the Columbia Summer School in 1928.[8] He could not avoid facing the epistemological problems aroused by quantum mechanics and its interpretation. During the winter of 1927-28, he made his effort to analyze the implications of quantum mechanics. His letter to Scudder Klyce written on December 11, 1927 shows that he gave consideration to Schrödinger's attempt to interpret the wave functions in the classical sense, finding that "[m]any physicists do not accept by any means all of Schrödinger's views."[9] He could "heartily approve" of Heisenberg's and Born's ideas, "particularly when they emphasize that nothing can have physical reality unless it is subject to experiment, which means usually that it must be measurable." This statement fitted in Bridgman's operational view. However, he admitted, "there are other matters in which I cannot agree so easily, or would rather give a different interpretation to the facts." The most irritating problem was "Heisenberg's doctrine of the connecting between the quantum contact 'h' and the ultimate accuracy of physical measurement," that "if we measure the position of an electron with a high accuracy we have thereby sacrificed the accuracy with which the velocity can be measured, and conversely, the velocity can be measured with accuracy only by sacrificing the accuracy of our knowledge of position." Bridgman, though still very vaguely, started to recognize what the uncertainty principle implied for his understanding of operation.

By the beginning of 1928, stimulated by his faithful correspondent Bentley, Bridgman started to doubt the validity of the spatial and the temporal categories in the microscopic realm, in light of "the new

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[7] *Ibid.*, p. 197.
[8] Bridgman to Korzybski, Nov. 13, 1927, PWBP, HUG 4234.12.
[9] Bridgman to Klyce, Dec. 11, 1927, PWBP, HUG 4234.12.

physical facts being found in the quantum domain."[10] "This question," he wrote to Bentley, "is strongly hinted at by the new philosophy of physics that is growing up around the new wave-mechanics, which is associated principally with the name of Heisenberg."

> Heisenberg himself does not make the statement, but it seems to me entirely possible that we shall find that time and space as we know them as measurable things, emerge by the combination of minute elements of a different character. Of course Bohr has been saying for a long time that it is quite likely that we shall have to give up our concepts of space and time in the quantum domain, but the precise sense in which this is to be taken has always been obscure.[11]

Bridgman interpreted Heisenberg's principle of uncertainty as suggesting possibility that in the quantum domain time and space themselves were no longer measurable, but might emerge as combinations of other unknown elements. To him, this meant the invalidity of space-time opposition as a scientific concept.

Though admitting the invalidity of space-time opposition, Bridgman still believed that "our world is in some way divided in two parts." He then suggested entirely different categories, namely, the technique and result of operations: "Everything having to do with the results of operations falls into one category, and everything having to do with the technique of the operations into another." In this letter Bridgman neither detailed these categories nor made their division sharper, but regarded the division to "correspond to one which we actually make." The idea of dividing operation into its technique and result seems to have occurred to him while reading Bentley's paper. Bridgman was not yet sure of its actual working, admitted to Bentley

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[10] Bridgman to Bentley, Jan. 14, 1928, PWBP, HUG 4234.12.
[11] *Ibid.*

that "perhaps there is nothing in it."

Bridgman would never clearly explain the relation between the uncertainty principle and the division of the technique and result of operation. Yet, the latter seems to have been a product of his effort to explicate the meaning of the former. As I have discussed in Chapter 4, while establishing his operational perspective, Bridgman tacitly presupposed two kinds of uniqueness of operations. One is the uniqueness of procedures or rules of operations that specify a concept, the uniqueness that is necessary to warrant one-to-one correspondence between a set of operations and a concept defined by them. The other is the uniqueness of the result of operations that warrants that everyone can achieve the identical result by carrying out operations specified by the same rules. With these two tacit presuppositions he could confidently state that the operational definition would work without any ambiguity. Heisenberg's uncertainty principle, however, denied that one can always attain the identical result even when he or she follows the same procedures. Bridgman realized this implication of the uncertainty principle, and then felt the need to discuss details of the concept of operation, hitting upon the division of its technique and result.

However, before giving a deeper consideration to the division of the technique and result of operations, Bridgman was eager to solve a problem then bothering him most, the meaning of the uncertainty principle. On January 29, 1928, he complained to Korzybski about the "constant head ache" which he had "when trying to get the new quantum mechanics"[12]: "I don't like the philosophy that is growing up in connection, particularly the thesis that we must give up causality and have not[h]ing but pure chance when we get far enough down in the

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

range of phenomena. I have a feeling that part of the causality concept is conditioned by our own thinking mechanism so that we can never entirely get away from it." Learning more of the uncertainty principle, he started to dislike its implications, especially the failure of causality in the quantum phenomena. He belonged to the generation of scientists to whom causality seemed indispensable for the scientific explanation of natural phenomena.

In the spring of 1928, as Korzybski was planning to visit Pasadena, California, Bridgman told him about Tolman, who was then at Caltech. Bridgman had "the highest opinion of Tolman," though they "used to disagree violently on hin [*sic*] Theory of Similitude."[13] Bridgman also informed Tolman of Korzybski's visit and asked this old friend from high school to drop a visit to Harvard: "[A]mong other thing, I would like to glimpse a little the mental evolution that Pasadena and quantum mechanics is producing in you. This is one of my regrets at not going to Como last summer."[14] In reply, Tolman told Bridgman about the California people's enthusiasm over *The Logic of Modern Physics* and confessed that he had "a little spiel about" Bridgman in his speech given at Como, though he had modified it as Bridgman was not present. As for the problem that was bothering Bridgman, Tolman only wrote, "I find myself rather friendly to Heisenberg's uncertainty principle."[15]

Tolman's reply did not help Bridgman grasping the meaning of the uncertainty principle. However, before Tolman's letter reached Bridgman, he started to realize the intellectual impact of the principle while preparing for the Columbia lectures. On March 18, he asked Korzybski, "Are you picking up any of the new quantum mechanics in Pasadena, and have you come across Tolman?"

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[13] Bridgman to Korzybski, Feb. 12, 1928, PWBP, HUG 4234.12.
[14] Bridgman to Tolman, March 11, 1928, PWBP, HUG 4234.8.
[15] Tolman to Bridgman, March 19, 1928, PWBP, HUG 4234.8.

> In the intervals between soldering and wiping up the floor in the laboratory I have been thinking a little lately about Heisenberg's principle of indetermination, in connection with a talk which I had to give to a group of non-technical members of our faculty, and also by way of looking ahead to my Columbia lectures of next summer. I am getting tremendously excited about it. If the physical basis for the theory turns out to be unassailable, I believe that this is the initiation of the biggest revolution in mental outlook since at least the time of Newton, much bigger than Einstein, for example. It means that the universe is forever bounded in the direction of the very small by becoming meaningless beyond a certain point. This sort of a limit to our activities is one which I did not suspect, and is one which is hopeless to try to surmount. There are tremendous possibilities in the idea, once it gets into popular hands. The advocates of "free will" on the one hand and on the other the old fashion atheists with their "pure chance" will equally find justification, and probably the vitalists in biology will be equally comforted. In the mean time I find the greatest problem in the situation in learning how to deal intellectually with a situation that fades out on you by becoming meaningless. This being my present frame of mind I find it impossible to understand much of the present writing in physics, entirely apart from the mathematical difficulties.[16]

Bridgman was obviously not happy with the uncertainty principle whose implications he had finally realized after several months' continuous effort. He concluded the letter to Korzybski: "So you see I am as unhappy in my subject as you in yours. We ought to found a society of Unhappy Scientists." In this letter, he did not detail his interpretation of the uncertainty in the quantum domain, only implying that it had opened Pandora's box and released "pure chance," "free will," and vitalism, the ideas that seemed irrational to Bridgman. Later, toward the end of that year, Bridgman would publish an article on his interpretation of the implications of the uncertainty principle.

[16] Bridgman to Korzybski, March 18, 1928, PWBP, HUG 4234.12.

For the preparation for the Columbia lectures, to be given in June, Bridgman made an effort to materialize his discoveries. On April 22, he wrote Bentley that "there are number of things that need thinking over" before the lectures, "in particular the effect of Heisenberg's principle of uncertainty on our picture of the actual structure of nature."[17] At the same time, he resumed the analysis of the "possibility of separating technique of operations from result of operations": "I ignored this sort of thing in my book, but I do feel a difficulty in adopting the operational view about operations themselves." The progress, however, was not remarkable. On May 14, he had to admit to Bentley that he did not "see how the analysis [of the distinction between technique and result of operation] can be carried through."[18] As for the uncertainty principle, he only recognized that "the real significance of Heisenberg's principle of uncertainty is that the universe is bounded in the direction of the very small by becoming meaningless." Though knowing that "[h]ow to deal with a situation like this" would become "the greatest intellectual problem of the future," he could not put his thoughts together. All he could suggest was that "the ultimate attitude must be one of acceptance, without analysis." Operational analysis did not work for the uncertainty principle.

To Bridgman, the uncertainty principle seemed more revolutionary than relativity theory. Since it devastated what he regarded as grounds for meaning, he could not figure out how to analyze the situation. In *The Logic of Modern Physics*, published only one year before, he expected quantum mechanics to become an example of operational physical theory. However, after learning its epistemological implications, especially the uncertainty principle, he unwillingly admitted that this theory had revealed the invalidity of his

[17] Bridgman to Bentley, April 22, 1928, PWBP, HUG 4234.12.

operational analysis as developed in the *Logic*.

Bridgman gave the Columbia lectures in the middle of June, 1928, but was not sure "that the lectures did any good."[19] He found that "the class was small and not at all responsive." "Some of them had quite inadequate preparation, and were not prepared to profit by it." Besides, "[t]he weather in New York was torrid." This result of the Columbia lectures, however, did not discourage Bridgman. He continued to work on the problems of quantum mechanics. In his letter to Bentley, comparing several introductory volumes to quantum mechanics, Bridgman mentioned his impression of H. F. Biggs's *Wave Mechanics*[20] and G. Birtwistle's *New Quantum Mechanics.*[21] He could not accept Schrödinger's view described in the former: "[P]ersonally I have the feeling that Schroedinger goes a little too far in trying to keep our old pictorial way of looking at things."[22] He also read Bohr's first paper on the complementarity published in *Nature*,[23] finding it "very difficult reading." Though admitting that Bohr had "evidently got a lot of most important stuff," Bridgman could not deny that "parts of it are obscurely put." He even "thought of taking a couple of weeks off and rewriting the paper [of Bohr] in language which should be more intelligible."[24] Furthermore, Bridgman read Schrödinger's *Abhandlungen zur Wellenmechanik*,[25] sent directly by the author.[26] All this effort, however, did not change his pessimistic interpretation. He could only repeat a gloomy remark on the implications of quantum

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[18] Bridgman to Bentley, May 14, 1928, PWBP, HUG 4234.12.
[19] Bridgman to Korzybski, Aug. 5, 1928, PWBP, HUG 4234.12.
[20] H. F. Biggs, *Wave Mechanics* (Oxford: Oxford University Press, 1927).
[21] G. Birtwistle, *The New Quantum Mechanics* (Cambridge: Cambridge University Press, 1928).
[22] Bridgman to Korzybski, Aug. 5, 1928.
[23] N. Bohr, "The Quantum Postulate and the Recent Development of Atomic Theory," *Nature*, 121 (1928), pp. 580-590.
[24] Bridgman to Korzybski, Aug. 5, 1928.
[25] E. Schrödinger, *Abhandlungen zur Wellenmechanik* (Leipzig: Barth, 1928).

mechanics: "The situation is extraordinarily difficult; it seems to me that we are getting pushed into a position where we absolutely cannot say anything."[27] Even in his review of Norman Campbell's *An Account of the Principle of Measurement and Calculation,* he could not but put what he had just learned from Heisenberg's discussion: "I feel much more confidence now in urging the essentially unprecise quality of every physical quantity than I would before the formulation of the Heisenberg principle of uncertainty."[28] One can understand that the uncertainty principle had transformed Bridgman's view of physical world, though he still had not known how to draw a proper lesson from it.

5.1.2 Hugo Dingler and the Philosophy of Experiment

Around the same time, Bridgman had another chance to think over the definition of operation and the division between technique and result of operations. The *Physical Review* asked Bridgman to review *Das Experiment, sein Wesen und seine Geschichte*[29] by Hugo Dingler, then a professor of philosophy at the University of Munich. In this book, published in 1928, Dingler maintained a view apparently similar to Bridgman's operational perspective. Bridgman himself found it very close to what he had called the operational point of view, commenting in the review[30] that he was "heartily in sympathy" with Dingler's effort to express his definitions in terms of actual procedure.

Bridgman, however, was strongly against Dingler's way of defining operations. For example, Dingler defined a plane as "a surface exactly

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26 Bridgman to Schrödinger, Oct. 25, 1928, PWBP, HUG 4234.8.
27 Bridgman to Korzybski, Aug. 5, 1928.
28 Bridgman, Review of *An Account of the Principles of Measurement and Calculation* by Norman Robert Campbell, *Physical Review,* 32 (1928), pp. 999-1000, p. 1000.
29 Hugo Dingler, *Das Experiment, sein Wesen und seine Geschichte* (Munich: Ernst Reinhardt, 1928).
30 Book review by Bridgman, *Physical Review,* 32 (1928), pp. 316-317.

the same on both sides" and showed that this definition led to the same result as the practical procedure of instrument makers for making a test plane by working three surfaces together until any two of them placed together were in contact at all points. This argument Bridgman could not approve: "What assurance have we that a surface exists which is the same on both sides, or that the instrument maker's procedure for fitting three test planes will ever lead to a final or a unique result when the accuracy of measurement is indefinitely increased?"[31] Although Bridgman had never given a clear definition to the concept of operation, at least it was obvious to him that "the fundamental operations ought not to be defined in terms of their results." "[E]ven a patent office," he pointed out, "does not allow a description of an invention in terms of its function."[32] The patent office requires applicants to specify mechanism that realizes a certain function; the operational definition requires the fundamental operations to be "directly defined in such terms that they may be definitely and uniquely carried out."[33] In the *Logic,* Bridgman discussed that Newton defined his absolute time and absolute simultaneity in terms of, as it were, the results of operation, without specifying the concrete procedures. On the other hand, in Bridgman's observation, Einstein found a way to define simultaneity in terms of actually realizable operations and thereby showed that no concrete operation corresponded to Newton's simultaneity and absolute time. In the *Logic,* Bridgman attempted to define every important scientific concept in what he understood as Einstein's way.

If one only specifies the results of operation, it is possible that there is no corresponding realizable operation, as in the case of

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31 *Ibid.*, p. 316.
32 *Ibid.*, p. 317.
33 *Ibid.*

Newton's absolute time. Bridgman's operational definition required physicists to specify the technique, though he did not use this word in his review of *Das Experiment.* Furthermore, one should carefully choose suitable operations when defining a concept in the operational way. Dingler was pleased to find that his definition of a plane was close to the actual procedure of instrument makers; however, this could not satisfy Bridgman. He required the operations defining a concept to be realizable and to lead to the expected results definitely and uniquely.

Coincidentally, Dingler, not knowing that Bridgman's review was appearing, sent a copy of *Das Experiment* to Bridgman. In reply, Bridgman detailed the point most important to him: "the definitions should be such that when one attempts to actually carry out the operations in manual practice the next step in the process should always be uniquely defined."[34]

> It seems to me that when you define a plane as a thing which is exactly the same from its two sides, you are defining it in terms of the *results* of the operations, and would find it very difficult to give a precise and unique specification of what is to be done at any instant in actually producing such a plane. I have been interested to discuss this matter with a skilled mechanic, and to find what method he adopts. I could not get anything very precise out of him, and it was evident that his method was more of an art, depending on much experience and a method of successive approximations, rather than any procedure that could be definitely formulated.[35]

Bridgman derived an idea of the book review from an actual conversation with a mechanic. He found that the mechanic's procedure for making a plane was much more complicated than the one Dingler described. Bridgman feared that one might miss this

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[34] Bridgman to Dingler, Aug. 5, 1928, PWBP, HUG 4234.8.
[35] *Ibid.*

complexity of actual procedures of operations when specifying only their results.

Dingler did not miss a chance to address his philosophy of experiment to Bridgman. In reply,[36] Dingler made clear that he had never written that it was actually possible to produce a real plane as he defined. He mentioned the mechanics' procedure merely as "the necessary starting point of all our building of apparatuses." [37] Furthermore, Dingler shrewdly pointed out that Bridgman's requirement that one should define operations in the way that they could be definitely and uniquely carried out was also "a definition in terms of results"; this way of defining operations will be meaningless unless one has known the results of the operations in advance. Admitting that his own definition was "the definition by a leading idea which enables you to choose out certain forms out of the chaos of reality," Dingler warned that if Bridgman did not want to use Dingler's way of definition, there was only an "empiristic" way left, which, in Dingler's observation, "is never able to offer you what you require in your definition." "If you," Dingler went on, "put yourself on a really and consequently formulated empiristic standpoint, you know really nothing whatever of reality and all is possible." He asked Bridgman how he could be sure that the operations he defined would be definitely and uniquely carried out, or how he could know what would happen with the operations he chose if he did not specify their result: "If you define them by the use of bodies in America, how can you be sure, that things will lead to the same results in Europe?" Dingler pointed out one of the tacit presuppositions of Bridgman's operational perspective.

Then Dingler reiterated his discussion in *Das Experiment.* First, he explained that what physicists did in constructing physical theory

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[36] Dingler to Bridgman, Dec. 29, 1928, PWBP, HUG 4234.8.

was to choose "the fundamental forms of physics" from "the flooding reality" by following "certain simple rules":

> ...and this is allowed because these choosing acts are (in the case of an systematical proceeding in the construction of physics directly out of the untouched reality—which is the only way in systematic thinking, that shall lead us to definite results in the fundamental parts of all science) the very first acts that occurd [*sic*] in any systematical constructing of a systematic physics. In the case of these first acts my position is such that I stand vis-à-vis a perfectly untouched reality (the only possible starting point for the building up of systematic physics) and in this position my choosing is perfectly free. In this position I make myself an idea answering to the question [of] which forms could be the practically best for my purpose of errecting [*sic*] a systematic science of physics, and after this idea I seek real things that fulfill this idea, and with these formed realities I work practically on by building apparatuses etc.[38]

Perhaps Dingler correctly described the way physicists constructed their science and also what Bridgman every day did in his laboratory. Furthermore, in the *Logic*, Bridgman, in a similar manner to Dingler's choice of fundamental forms, explained how to choose the suitable operations that were useful for what he considered as the right way of constructing science. However, to Bridgman the most important thing was to specify the technique of operation, whilst in Dingler's argument, where the act of finding useful "forms" preceded everything, the details of operations played only an auxiliary rule.

Later, Bridgman accepted at least some of Dinger's points. In 1930, Bridgman admitted that Dingler had put his finger on the point where Bridgman's entire outlook was least satisfactory to the philosopher: "Your keen remark, that I also, in requiring that my operations be so specified that they can be carried out, have defined

[37] *Ibid.*

operations in terms of their results, I believe to be entirely justified."[39] However, Bridgman, not being a philosopher, did not go further into the matter of definition of his operations, only treating them as "ultimates." The effort to comprehend the implications of the uncertainty principle might have led Bridgman to notice probably unconsciously that he could never empirically prove that such operations as he regarded suitable for defining a scientific concept existed. Moreover, this recognition might have made him avoid any further reflection on the definition of operation. Even in 1932, concerning the operational definition, Bridgman could only stress the same position that "operations applicable to the actual world cannot be fixed by pure definition." [40] Bridgman probably missed one of important opportunities for deepening his understanding of the nature and presuppositions of operational reasoning.

Still, Bridgman should have examined Dingler's statement a little more carefully at least before he finally accepted Dingler's proposal for translation of *The Logic of Modern Physics* into German. Later, Bridgman was to find unacceptable the interpretation of Dingler and the translator put on the German version of the *Logic*, having seen neither the introduction nor the notes before its publication.[41] Bridgman worried that the translation might have given the wrong impression of his position, although a careful reader like Philipp Frank could imagine that the original author would not tolerate the metaphysical, anti-empirical interpretation of Dingler and the translator.[42] In fact, Bridgman had a chance to have a more faithful translator. In 1931, while the preparation for the translation proceeded by Dingler's

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[38] *Ibid.*
[39] Bridgman to Dingler, May 18, 1930, PWBP, HUG 4234.8.
[40] Bridgman to Dingler, July 24, 1932, PWBP, HUG 4234.8.
[41] Bridgman to Philipp Frank, March 30, 1938, PWBP, HUG 4234.10.
[42] Philipp Frank to Bridgman, Feb. 25, 1938, PWBP, HUG 4234.10.

arrangement, Herbert Feigl, who heard of this project from Bridgman, expressed his wish to translate the *Logic.* Bridgman felt it advantageous that he could directly discuss with the translator the changes he wanted to make and asked Dingler for advice.[43] Knowing Feigl as an active member of the Viennese Circle, Dingler opposed Bridgman's proposal and told him that the translation had already progressed.[44] Later, Bridgman was to find Logical Positivism of the Viennese Circle closer to his operational view than any other philosophical doctrine, though he did not like others to lump him with the Logical Positivists.[45]

Nevertheless; Dingler's analysis of experiment, probably more akin to Bridgman's view than Logical Positivism and than Bridgman himself was aware, gave a long-lasting impact to his philosophical reflection. After reviewing *Das Experiment,* Bridgman started to recognize the experimental method as a historical product obtained from "the experience of all the ages" (§3.1). Furthermore, *Das Experiment* furnished him with the only plausible expression for the definition of operation and experiment, "a consciously directed and repeatable activity."[46] Insisting that "perfect sharpness" for the definition of operation was neither attainable nor necessary, Bridgman never presented a formal definition of operation and only suggested that the best way to learn the operational approach was "to see it in action as applied to many concrete examples."[47] However, when necessary, Bridgman referred to variants of "directed, repeatable activity" as his almost favorite definition of operation until the late 1950s (§1.3).

Despite these, the correspondence between Bridgman and Dingler

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[43] Bridgman to Dingler, May 31, 1931, PWBP, HUG 4234.10.
[44] Dingler to Bridgman, April 28, 1931, PWBP, HUG 4234.10.
[45] Bridgman to Herbert Dingle, Jan. 28, 1952, PWBP, HUG 4234.10.
[46] Bridgman to Hart, May 27, 1953, PWBP, HUG 4234.10.
[47] *Ibid.*

ceased a few years after the publication of the *Logic*'s German translation. Thereafter, Bridgman neither mentioned Dingler's name publicly nor recognized his influence self-consciously. Probably the antipathy toward Dingler and his philosophy grew in Bridgman after he saw the German translation of the *Logic*. Or Bridgman simply found disagreeable the speculative appearance of Dingler's discussion, though he might have reluctantly admitted that, in order to use repeatable operations as the fundamental concepts, one should probably follow Dingler and start with his unchangeable "forms" or their likes.

But it is also possible that the uncertainty principle, though never appearing as a topic in their correspondence, subtly influenced Bridgman's memory of Dingler. Dingler came into Bridgman's sight when Bridgman was struggling to comprehend the implications of the uncertainty principle. The uncertainty principle, on one hand, urged him to alter his view of operation, showing that no operation is impeccably repeatable. On the other hand, his daily experiment was clearly recognizable and repeatable, representing suitable operations as the basis of scientific theorizing. As his letters in 1928-1929 shows, Bridgman was obviously troubled and confused during this period. After this period, Bridgman only hesitantly defined operations as "repeatable activities." The interaction with Dingler stimulated Bridgman to reflect on the nature of operation; however, while communicating with Dingler, Bridgman was conflicting with a more acute problem that obscured the impact of Dingler's philosophy of experiment.

5.1.3. "The New Vision" and John Dewey

By the end of November, 1928, Bridgman materialized the result of his effort to explicate the implications of the uncertainty principle. In

March 1929, he published it in a popular monthly magazine *Harper's* as an article titled "The New Vision of Science."[48] Bridgman tried to depict the grave implications of Heisenberg's principle of uncertainty not only to scientists but also to "the man in the street."[49] Before explaining the details of the principle, he emphasized that this principle was based on the discoveries in the realm of quantum phenomena, to which the American experimental physicists A. H. Compton and C. J. Davisson contributed; in fact, the latter's experiment conducted with L. H. Germer in 1927 that confirmed the wave properties of electrons[50] stimulated Bridgman to publish the essay. He repeatedly stressed that quantum mechanics "has been checked in many ways against experiment"[51] and accounted for many physical phenomena including ones observed in everyday life. For example, it explained that "a tea kettle of water boiling on the stove should not give out enough light in virtue of its temperature to be visible" although "the accepted theories of optics demanded that it should be visible."[52] Bridgman accepted the uncertainty principle and its implications because they were "the interpretation of direct experiment," not the ideas "reached by armchair meditation."[53]

In order to describe the meaning of the uncertainty principle, Bridgman used the billiard-ball game as an analogy. If collision of the balls could be described by the laws of Newtonian mechanics, it would be possible, even for a "graduate of high-school course in physics,"[54] to

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48 P. W. Bridgman, "The New Vision of Science," *Harper's Magazine*, 158 (1929), pp. 443-451.

49 *Ibid.*, p. 444.

50 For the Davisson-Germer experiment, see: Richard Gehrenbeck, "C. J. Davisson, L. H. Germer and the Discovery of Electron Diffraction" (Ph. D. dissertation, the University of Minnesota, 1973).

51 Bridgman, "The New Vision of Science," p. 447.

52 *Ibid.*, p. 444.

53 *Ibid.*, p. 450.

54 *Ibid.*, p. 445.

calculate and predict from their behavior before collision how the balls would move after collision. It seems natural to expect by analogy that the same calculation and prediction are possible in collision between "a bullet of radiation" and an electron. Compton's experiment, however, revealed that it is possible only to tell either "how the bullet of radiation bounces away," or how the electron bounces away, but "no one has ever been able to tell how both will bounce away."[55] This unpredictability was formulated in quantum mechanics as the principle of uncertainty of Heisenberg, whose spirit was, in Bridgman's words, "that there are certain inherent limitations to the accuracy with which a physical situation can be described"[56]: Bridgman understood that the unpredictability described by this principle was inextricably connected with the impossibility of measuring exactly both the position and velocity of the electron.

Bridgman went on to interpret this situation by "the logical analysis of the meaning of our physical concepts which has been stimulated by the relativity theory of Einstein," namely, Bridgman's operational analysis, and stated that "an electron cannot have both position and velocity," as "the physicist finds that in the sense in which he uses language no meaning at all can be attached to a physical concept which cannot ultimately be described in terms of some sort of measurement."[57] The fact that one can exactly measure only either the position or the velocity presented an even more striking paradox: "[B]y choosing whether I shall measure the position or velocity of the electron I thereby determine whether the electron has position or velocity. The physical properties of the electron are not absolutely inherent in it, but

55 *Ibid.*, p. 446.
56 *Ibid.*
57 *Ibid.*

involve also the choice of the observer."[58] Bridgman also mentioned the failure of the idea of cause and effect in connection with the unpredictability suggested by the uncertainty principle, but gave it another type of explanation, following Heisenberg: "The precise reason that the law of cause and effect fails can be paradoxically stated; it is not that the future is not determined in terms of a complete description of the present, but that in the nature of things the present cannot be completely described."[59]

These outcomes of the uncertainty principle, however, seem to have been less important to Bridgman than the one that disclosed an almost entirely unexpected nature of physical measurement. Notably, Bridgman regarded the limitation of measurement as a more fundamental implication of the uncertainty principle than the apparent failure of causality. What convinced Bridgman of the truth of the principle was Heisenberg's analysis of the nature of measurement, which was, as in the case of Einstein's definition of simultaneity, derived from his reflection on observation in general, not from the accumulation of experimental facts. Though many times emphasizing the importance of the new experimental facts, Bridgman admitted that "it is certain that something very much like this principle [of uncertainty], if not this principle exactly, covers an enormously wide range of phenomena,"[60] and that it would not essentially be affected by the discovery of new experimental facts or by further refinement of techniques of measurement. In other words, he found "an inevitableness [...] rooted in the structure of knowledge."[61] The principle taught him that no observation, no measurement, and

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[58] *Ibid.*
[59] *Ibid.*, p. 448.
[60] Ibid., p. 447.
[61] *Ibid.*, p. 448.

therefore "no knowledge of any physical property or of even mere existence is possible without interaction," or that "[t]he mere act of giving meaning through observation to any physical property of a thing involves a certain minimum amount of interaction,"[62] which prevents the physicist from analyzing physical phenomena completely: "If the analysis means anything, it must involve the possibility of observation; and observation involves interaction; and interaction cannot be reduced below a minimum." [63] Apparently startling suggestions of the uncertainty principle, such as the impossibility of measuring both the position and the velocity of an electron and assigning the meaning to both of them at the same time, or the unpredictability of its behavior, were all originated in the fact that no interaction involved in any measurement can be reduced below a certain limit. The same fact was especially meaningful to Bridgman, as it directly denied the basic assumption of his operational view, that, by following the same procedure, everyone would obtain the same result of operations.

The conclusions Bridgman drew from his observation were, therefore, quite gloomy. To scientists, he addressed thus: "We have reached the point where knowledge must stop because of the nature of knowledge itself: beyond this point meaning ceases"[64]; "The world is not intrinsically reasonable or understandable"[65]; "the possibility that the world may fade away, elude him, and become meaningless because of the nature of knowledge itself, has never been envisaged before, at least by the physicist, and this possibility must forever keep him humble"[66]; "the scientist will see that his program is finite."[67] Yet Bridgman

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[62] *Ibid.*
[63] *Ibid.*, p. 449.
[64] *Ibid.*, p. 450.
[65] *Ibid.*
[66] *Ibid.*
[67] *Ibid.*, p. 451.

pointed out that an even more important effect of "this revolution" would be on "the man in the street." "[B]eyond the ken of the scientist," he wrote, there would be "the playground of the imagination of every mystic and dreamer," which would admit "the substance of the soul," "the spirits of the dead," "the principle of vital processes," or "the medium of telepathic communication." [68] Bridgman lamented that beyond the reach of the scientist, nothing would stop all these irrational thoughts. Arnold Sommerfeld later described this essay as "pessimistic resignation," though Bridgman did not agree with him.[69]

Still, Bridgman did not entirely give up hope for assimilating a situation so foreign to ordinary experience that quantum mechanics created. He estimated that it would "doubtless be a long while before the average human mind finds a way of dealing satisfactorily with" this situation, or that "generations will be needed to adjust our thinking so that it will spontaneously and freely conform to our knowledge of the actual structure of the world." [70] However, at the same time, he suggested that, by renouncing "our present verbal habits" and "their implications," developing "new methods of education," and then applying it to "very young children in order to inculcate the instinctive and successful use of habits of thought so contrary to those which have been naturally acquired in meeting the limited situations of every day life," it would be possible that "understanding and conquest of the world about us will proceed at an accelerated pace," "since thought will conform to reality."[71] At this point, Bridgman did not seem to cling to any specific world view or any particular mode of understanding reality, but freely thought that one should modify the verbal habit and the way

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68 *Ibid.*
69 Bridgman to Dingler, April 25, 1930; Dingler to Bridgman, May 18, 1930, PWBP, HUG 4234.8.
70 Bridgman, "The New Vision of Science," p. 451.
71 *Ibid.*

of thinking accordingly to the new facts presented by science.

It is doubtful whether quantum mechanics really denied the validity of the concepts of the position and the momentum of an electron as Heisenberg and Bridgman maintained, and whether it totally abandoned the ability of predicting events in the future. Though the meaning of these concepts is not the same in quantum mechanics as, for example, in Newtonian mechanics, quantum mechanics is still described by them. Moreover, though quantum mechanics does not predict as deterministically as classic physics does, this does not mean quantum mechanics cannot predict anything. The gulf between quantum mechanics and classical mechanics is large; yet, the former has inherited many fundamental concepts from the latter, transformed their meaning, and used them.

Bridgman could not accept such transformation of fundamental concepts that was totally against the spirit of the operational definition. Nor could he just watch and criticize quantum mechanics from his own point of view, since it uncovered the nature of measurement that could directly invalidate his operational stand. Heisenberg's discussion on the measurement revealed the fact that it is impossible to measure all the necessary physical quantities exactly at the same time. Bridgman had to realize that his ambitious attempt to define all physical concepts in terms of operations whose technique and result were unique was but a forlorn dream. If read as a popular review of the meaning of quantum mechanics, Bridgman's "New Vision" seems a little too pessimistic; however, in fact, it was a product of his discovery of the failure of the operational program described in *The Logic of Modern Physics*.

Bridgman sent the manuscript of "The New Vision" to *Harper's Magazine* on November 27, 1928. Though fearing that the readers of

Harper's might find it "too abstract to be of interest," he emphasized the importance of the uncertainty principle to the editor: "[T]he consequences of the new discoveries are so important for everyone that all of us, sooner or later, will have to make considerable readjustments to meet the situation."[72] He was confident that "this article is entirely novel, and contains some new points of view," urging the editor to publish it "as prompt as possible," as "activity in this field is so great that any novelty is likely to be of short duration." Right before he sent the manuscript, *Harper's Magazine* had asked G. N. Lewis to contribute some articles. Though he suggested several subjects including the Heisenberg principle, the editor finally found that Lewis did not have time to keep his promise. Bridgman's manuscript, therefore, appeared in a right time. After making some corrections and additions, Bridgman submitted the final version of manuscript at the beginning of 1929.[73]

The article was published in the March issue of *Harper's Magazine.* But before that, Bridgman's astronomer friend Harlow Shapley, who read the article before publication, wrote that the editors "were doing a most courageous thing in publishing it."[74] Bridgman agreed and wrote to the editor: "The more I think of it the more I am inclined to agree with him; I hope I am not irretrievably wrecking the Magazine."[75] Contrary to his worry, his article had a wide range of readers and enjoyed various kinds of responses. More than one year after its publication, the editor of *Harper's Magazine* reported Bridgman that they still received "echoes" from his "notable paper," asking him to contribute another.[76]

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[72] Bridgman to Harper's Magazine, Nov. 27, 1928, PWBP, HUG 4234.8.
[73] Hartman to Bridgman, Dec. 17, 1928, PWBP, HUG 4234.8.
[74] Bridgman to Hartman, Jan. 1, 1929, PWBP, HUG 4234.8.
[75] *Ibid.*
[76] Hartman to Bridgman, April 10, 1930, PWBP, HUG 4234.8.

Immediately after "The New Vision" was published, Scudder Klyce asked whether Bridgman had written it for the purpose of refuting Arthur Eddington's view described in *The Nature of the Physical World.*[77] Eddington's book bore a similar view to Bridgman's, stating that Einstein's theory had taught physicists "that each physical quantity should be defined as the result of certain operations of measurement and calculation."[78] However, the last four chapters dealt with the religious implications of modern physics that reflected the author's religious background. Klyce probably noticed the similarity between Eddington's speculation and Bridgman's fear that all the mystical thoughts could come out in the microscopic realm. Klyce was not alone in finding their similarity. Replying to Bridgman's proposal to contribute an article, the editor of *Harper's Magazine* sent a copy of Eddington's *Nature* and wrote to him: "Incidentally, if you have not already seen Eddington's new book which is just out, 'The Nature of the Physical World,' you might be interested in noting his discussion of the Heisenberg principle."[79] Another editor of the Macmillan who also sent the book was "feeling sure that you would be interested in it."[80] Having not read Eddington's book before completing his own essay, Bridgman commented how he liked it:

> I have read it, and although I violently disagree with the conclusions which he draws from the physical evidence, I was fascinated, as I always am, by the charm and vividness of his exposition, and found many passages exceedingly suggestive because of the aptness of the analogies. The book will certainly

[77] Bridgman to Klyce, March 31, 1929, PWBP, HUG 4234.12.
[78] Arthur Eddington, *The Nature of the Physical World* (New World: Macmillan, 1929), p. 255.
[79] Hartman to Bridgman, Dec. 17, 1928, PWBP, HUG 4234.8.
[80] Hutchinson to Bridgman, Dec. 22, 1928, PWBP, HUG 4234.8.

> be read by a wide circle of readers interested in the philosophical implications of recent scientific discoveries.[81]

To Klyce, he reiterated the same comment: "With regard to my relation to Eddington, I had not read his book when I wrote the article, but did shortly afterward, and I may say that I do not agree at all with his fundamental position." [82] As I will discuss later, John Dewey also found the similarity between Eddington's and Bridgman's operational requirements and compared them in his *Quest for Certainty.*

Albert P. Weiss, then a professor of psychology at the Ohio State University, severely criticized "The New Vision" in a long article published in the December issue of the *Scientific Monthly.*[83] Quoting Bridgman's statement that "an electron can not have both position and velocity" because "it is impossible to measure exactly both the position and velocity," Weiss commented that this was "only an experimental limitation."[84] Weiss, who implicitly assumed that undisturbed reality existed independently of measurements of scientists, insisted that "[e]lectrons move even though the characteristics of their movements can not yet be formulated by some physicist."[85] Furthermore, he pointed out that the limitation of physical research Bridgman described in "The New Vision" was due to the fact that "[t]heoretical physics is the product of human interaction."[86] Before submitting the manuscript to the *Scientific Monthly,* Weiss had asked *Harper's* to publish it. As the editor of *Harper's* asked Bridgman for his opinion,[87] he could read Weiss's essay in the end of that summer, a few months before its

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81 Bridgman to Hutchinson, Jan. 1, 1929, PWBP, HUG 4234.8.
82 Bridgman to Klyce, March 31, 1929, PWBP, HUG 4234.12.
83 Albert P. Weiss, "Bridgman's New Vision of Science," *Scientific Monthly*, Dec. 1929, pp. 506-514.
84 *Ibid.*, p. 508.
85 *Ibid.*
86 *Ibid.*, p. 511.
87 Hartman to Bridgman, Aug. 23, 1929, PWBP, HUG 4234.8.

publication. "[P]ersonally," he wrote to the editor, "I do not feel the compulsion to change my position in any respect."[88] He found Weiss blaming him for the lack of knowledge in psychology, but mentioned that it only meant that he might have used terms loosely and "perhaps in technically misleading ways," thinking that the conclusions contained no error. Bridgman was sure that Weiss did not always understand the sense in which he had used his terms, probably because he had not read *The Logic of Modern Physics*: "The article [The New Vision] involved a good many of the points of views developed there [in the *Logic*], and I do not believe that the article can be fairly criticized without some knowledge of the book."[89]

Weiss regarded the uncertainty principle only as some kind of experimental limitation. This very experimental limitation, however, led Bridgman and perhaps Heisenberg to the recognition that an electron could not have both position and velocity. To Bridgman, who required all the physical quantities to be defined in terms of realizable measurements, the fact that the position and the velocity of an electron could not be measured at the same time meant that an electron did not have both position and velocity at the same time. Heisenberg, though not as demanding as Bridgman, contended that one could establish and perceive physical reality only through actual measurements. Although Bridgman would later criticize Heisenberg's way of theorizing, their views of the relation between science and physical reality had much in common. Weiss, on the other hand, did not identify nature with knowledge of it, criticizing the physicists' way of understanding physical reality.

Probably the event most pleasant to Bridgman that took place in connection with his *Harper's* essay was that the celebrated philosopher

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88 Bridgman to Hartman, Aug. 30, 1929, PWBP, HUG 4234.8.

John Dewey referred to "The New Vision" and *The Logic of Modern Physics* in his *Quest for Certainty* and sent a copy of it to Bridgman. In the *Quest*, Dewey quoted the best-known sentence in the *Logic*, "*the concept is synonymous with the corresponding set of operations*," praising his view as "an empirical theory of ideas free from the burdens imposed alike by sensationalism and *a priori* rationalism."[90] Furthermore, after comparing Bridgman's statement with other similar ones in Eddington's *Nature*, Charles Peirce's "How to Make Our Ideas Clear," the pragmatism of William James, the "instrumental" theory of conceptions, and the principle of "extensive abstraction," Dewey concluded thus: "On account of ambiguities in the notion of pragmatism—although its *logical* import is identical—I shall follow Bridgman in speaking of 'operational thinking.'"[91]

As Bridgman divided an operation into its technique and result, so Dewey separated an operation to be performed from its consequence. Dewey assumed that the operational definition consisted of two parts, "[a] definition of the nature of ideas in terms of operations to be performed and the test of the validity of the ideas by the *consequences* of these operations." Dewey, however, neither presupposed the uniqueness of the result of operations as Bridgman of the *Logic* did, nor shared the pessimistic conclusions Bridgman drew from the implications of Heisenberg's principle of uncertainty, though his explanation of this principle in the *Quest* owed much to "The New Vision." Dewey maintained that the uncertainty principle had revealed the inextricable connection between knowledge and act, regarding it as

89 *Ibid.*
90 John Dewey, *The Quest for Certainty: A Study of the Relation of Knowledge and Action* (New York: Minton, Balch and Co., 1929). Citations are from, Jo Ann Boydston ed. *John Dewey, The Later Works, 1925-1953*, vol. 4, (Carbondale: Southern Illinois University Press, 1984), p. 92.
91 *Ibid.*, p. 90.

the strongest support for the operational theory of knowledge: "What is known is seen to be a product in which the act of observation plays a necessary role. Knowing is seen to be a participant in what is finally known. Moreover, the metaphysics of existence as something fixed and therefore capable of literally exact mathematical description and prediction is undermined."[92] To him, the principle did not mean the denial of the understandability of nature. "[N]ature," he asserted, "intrinsically is neither rational nor irrational," but "it exists in a dimension irrelevant to either attribution." "There are operations by means of which it *becomes* an object of knowledge, and is turned to human purpose," and, therefore, "[n]ature is intellig*ible* and understand*able*."[93] To Dingler, reality itself was chaos, which could be made understandable by experimental science. In a similar manner, Dewey considered science as an endeavor to make nature intelligible and understandable for human purpose.

Bridgman wrote Dewey that he had read through *The Quest for Certainty* "with the greatest satisfaction": "[I]n fact it is the only one of the few works on philosophy which I have ever attempted of which I could approve or which I could even partially understand."[94] He was happy with the discovery that "the new concepts in physics are at least attracting the attention which they deserve in the fields of morals, social relations and philosophy." However, he did not miss the point where Dewey's understanding clearly differed from his own. "On page 210," he pointed out, "you say that nature remains intelligible and understandable in the face of these new facts although one's impulse may be very strong to rush to the opposite conclusion":

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[92] *Ibid.*, p. 163.
[93] *Ibid.*, p. 168.
[94] Bridgman to Dewey, Nov. 6, 1929, PWBP, HUG 4234.8.

> In my Harper's article, to which you refer, I did make the opposite conclusion, and I would like to expand a little what I meant by this. There is an instrument known as the Geiger counter, by which the entrance of a single electron into the apparatus may be made to give a recognizable effect. Perhaps some physicist at Columbia has the experiment set up; if he has, it is most instructive. By suitable arrangements with amplifying vacuum tubes, the effect may be so magnified that every electron which enters the apparatus produces a crack of sound of even painful intensity. As the source of electrons we may choose a speck of radioactive salt, and by placing this near the apparatus or far from it, we may regulate these cracks of sound so that they are frequent or infrequent. But whatever we do, the detailed distribution of the cracks is a purely haphazard matter. The physical fact is that we have absolutely no control over the individual cracks; we can neither force a crack to come when we want it to, nor can we predict when it wil [*sic*] come. If our understanding of Heisenberg's principle is correct we can in principle never control or predict the individual cracks. This situation I describe as non-intelligible and non-understandable; this is my operational definition of these words.[95]

By illustrating the function of the Geiger counter, Bridgman exaggerated the uncontrollability of nature. Quantum mechanics revealed that no completely deterministic physical theory was valid for such microscopic phenomena as nuclear fission. Bridgman correctly pointed out that one cannot predict or control the distribution of the cracks of the Geiger counter. This, however, does not necessarily mean that such phenomena are non-intelligible or non-understandable. It only suggests that the way of understanding and controlling nature has changed, that one has to find a way to understand nature that is different from the classical deterministic one.

It appears natural that Bridgman regarded nuclear fission as non-intelligible or non-understandable from his operational vantage point that no phenomenon could be intelligible or understandable

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[95] *Ibid.*

unless it was described with concepts defined in terms of the unique set of operations that would produce the unique result. At the same time, however, it is difficult to understand why he did not try to relax his standard a little and accept the transformation of the way of doing physics. Dingler and Dewey believed science to be an intellectual activity to find controllability and intelligibility in nature. Contrary to them, Bridgman, when facing the uncertainty, seemed to be interested in finding and exaggerating uncontrollable and non-intelligible aspects of nature.

Before knowing the uncertainty principle, Bridgman regarded nature as something intrinsically rational, intelligible, and controllable. In the *Logic*, he showed how to construct unambiguous physical theory by using operations that represented this rational nature. However, the uncertainty principle revealed the uncontrollability of nature and ruined Bridgman's belief in nature, science, and operation. After this shock, Bridgman became almost unnecessarily persistent in pointing out the uncontrollable aspects of nature.

Bridgman, on the other hand, noticed that there was another sense in which one could regard nature to be intelligible and controllable, suggesting this possibility to Dewey:

> The only sense in which Nature can be said to be intelligible in the light of Heisenberg's principle and with my meaning of the words is a statistical sense; if there are many cracks of sound we can predict or control their average behavior with some exactitude. It seems to me that the implications of this go beyond anything treated in your book; perhaps some day you will write another book dealing also with this.[96]

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[96] *Ibid.*

It turned out that not Dewey but Bridgman himself was to discuss the probabilistic and statistical way of understanding nature. Several years later, Bridgman detailed his criticism of probabilistic theory in his lectures at Princeton and in his book, *The Nature of Physical Theory.*[97]

Dewey's reply was modest. First of all, he admitted that he was "a layman in the field of science" and that he "suffered from a poor education in science as did most of [his] generation who did not specialize in some branch of it."[98] He even agreed with Bridgman about the intelligibility of nature "in the traditional and still current sense." However, he suggested the necessity to understand the intelligibility in a different light:

> I rather intended to indicate the need for giving a new meaning to the word and idea, and I do not think this meaning would be inconsistent with what you say about prediction and control being connected with statistical averages. "Nature" as we deal with it in ordinary experience is so to say a massed or macrospcoic [*sic*] fact in which regularities of frequency are important rather than bheavior [*sic*] of individual phenomena as such, and our conception of intelligibility must be built on the former. I was much struck by what you said in your Harper's article about the prospect that the new findings would probably result in letting loose another flood of occultism and obscurantism, and I certainly dont [*sic*] want to be guilty of contributing a drop to that flood.[99]

To put it simply, Dewey's standpoint was sounder than Bridgman's. Not sticking to an old idea of intelligibility, he preferred to modify the meaning of the concept according to the new discoveries rather than deny rationality of nature in general. In the last sentence, Dewey even

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97 Percy Williams Bridgman, *The Nature of Physical Theory* (Princeton: Princeton University Press, 1936).
98 Dewey to Bridgman, Nov. 22, 1929, PWBP, HUG 4234.8.
99 *Ibid.*

seemed to be reproaching Bridgman, who indulged himself too much in playing with uncertainty and irrationality.

Despite the difference in their views of nature's intelligibility, Bridgman would remain to be one of the sources of intellectual stimuli for Dewey. Even in 1936, Dewey's associate and Bridgman's faithful correspondent, Bentley, reported to Bridgman that Dewey notified him of Bridgman's paper, "A Physicist's Second Reaction to Mengenlehre,"[100] published in *Scripta Mathematica.*[101] On the other hand, Bridgman did not seem to take Dewey's comments seriously. He probably wanted to cope with the current difficulty without help from "a layman in the field of science."

Bridgman was almost upset by the uncertainty principle, which destroyed the grounds of his views of nature, science, and operation. While the philosophers Dingler and Dewey and the psychologist Weiss could easily suggest how physicists should behave in the age of quantum mechanics, Bridgman was to spend a considerable time and energy in adapting himself awkwardly to the new situation.

5.1.4. Theory and Reality in the Age of Quantum Mechanics

In explaining the uncontrollability of microscopic phenomena to Dewey, Bridgman referred to the Geiger counter, which he had not mentioned in "The New Vision." As this example shows, Bridgman continued to reflect on the implications of quantum mechanics during 1929 and made some progress in this line. He addressed the result to his colleagues in the speech he made as the retiring vice-president and

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[100] P. W. Bridgman, "A Physicist's Second Reaction to Mengenlehre," *Scripta Mathematica*, 2 (1934), pp. 101-117, 224-234.
[101] Bentley to Bridgman, March 5, 1936, PWBP, HUG 4234.10.

chairman of Section B of the American Association for the Advancement of Science in December 1929.[102]

It took a long time for Bridgman to prepare the retiring address. Already in the summer of that year, though not yet being able to formulate a precise title, he had decided what he was going to talk. In a letter to A. L. Hughes, a professor at Washington University, he wrote thus:

> It is going to be in the nature of a general stock-taking of our present situation in physics, and will deal with some questions which I suppose you would call the philosophy of science. I shall lay special emphasis on the importance of further experimental acquaintance with the elementary things and processes, such as experiments on single electrons or on the reactions between single photons and electrons.[103]

By October, Bridgman had finished the draft of his speech and sent its summary to Hughes. As the summary and its title "Permanent Element in the Flux of Present-Day Physics" show, Bridgman was ambitious enough to discuss the appropriate direction in which physical research should proceed:

> A search is made for the elements of permanent significance in our rapidly increasing experimental knowledge and our rapidly changing theoretical outlook, and the endeavor is made to find what the average physicist may best do under the circumstances. The details of most of our mathematical developments are probably transient in character and need not be studied too deeply by the non-specialist, but nevertheless certain broad fundamental principles have been uncovered which probably transcend the mathematics by which they were discovered and of which everyone should endeavor to acquire command. Specific suggestions are made as to how to acquire

[102] P. W. Bridgman, "Permanent Elements in the Flux of Present-Day Physics," *Science*, 71 (1930), pp. 19-23.
[103] Bridgman to Hughes, Aug. 26, 1929, PWBP, HUG 4234.8.

such a command by applying the principle to various elemental situations.[104]

Bridgman felt the need for "the average physicist" to prepare to cope with the new situation that quantum mechanics had brought about. As the *Logic*, written before the advent of quantum mechanics, could not work for this purpose, he would have to publish new guidelines one after another.

Bridgman began his retiring address by encouraging experimental research, especially the effort to increase accuracy of measurement. Yet his main interest was in the changes in the attitude of physicists caused by quantum mechanics. He first pointed out that "the long-sought goal"[105] of physics, that is, to give an explanation of nature, had been abandoned and replaced by another one, to describe nature, after the failure of several theoretical attempts. He explained the reason: "[N]o theoretical scheme of explaining nature can be regarded as secure until verified by every possible experiment, and when every such possible experimental check has been applied, the theory degenerates into a description."[106] When checked by every possible experiment and modified by their results, physical theory would become only an accumulation of experimental results.

Bridgman then pointed out another, more striking suggestion of the recent development in theoretical physics: "We used to demand that the ultimate goal of physical theories should be nothing less than the discovery of the underlying realities. To-day our demand for reality is much less insistent, in large part because we are much less confident that the ultimate reality, which we thought to be our goal, has any

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104 Bridgman to Hughes, Oct. 1, 1929, PWBP, HUG 4234.8.
105 Bridgman, "Permanent Elements in the Flux of Present-Day Physics," p. 20.
106 *Ibid.*

meaning."[107] Bridgman did not mean that physics became less powerful. He maintained that because the concept of reality lost its meaning, the discovery of the underlying realities could no longer be a goal of physics.

> I believe it is fair to say that the sense in which every one used reality a few years ago and the sense in which the majority use it to-day has "uniqueness" as a minimum connotation. It would not have been admitted that two entirely different explanations of the universe could each be equally real, but to-day we see that uniqueness in an explanation is an impossible ideal, and the quest for reality, in so far as reality connotes uniqueness, must be abandoned as a meaningless quest.[108]

For physicists, reality had lost uniqueness. As a "sufficient basis for this change of attitude," Bridgman referred to "the proof of Poincaré that any aggregation of phenomena, no matter how complicated, is always susceptible of an infinite number of purely mechanical explanations."[109]

In the *Logic*, Bridgman had found the same proof of Poincaré unsatisfactory, laid stress on the physicist's desire to find out only one "real" explanation that corresponded to reality, and presented his operational standard as criterion for such explanation (§4.2). Poincaré's proof, therefore, though mentioned as the basis for the change in physicists' attitude, had actually not caused the change in Bridgman's view of reality. In fact, Bridgman implied what forced him to abandon reality as a goal:

> However much one might have been inclined fifty years ago to see some warrant for ascribing physical reality to the internal processes of a theory because of its success in meeting the observed situation, certainly no one of the present generation

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107 *Ibid.*

108 *Ibid.*

109 *Ibid.*, pp. 20-21.

> will be capable of so naïve an attitude after our illuminating experience of the physical equivalence of the matrix calculus and the wave mechanics.[110]

What struck Bridgman and changed his understanding of reality was the fact that two mathematical theories formalized in entirely different ways could describe the same phenomena and could replicate the same results. Bridgman, who used to stick to the idea of one "real" mechanism and one "real" physical theory, could not but accept the *status quo* in physics and admitted, though reluctantly, that two different theories could be physically equivalent. Learning of the uncertainty principle, he had to abandon the ideal of operational definition; then, knowing of the mathematical formulation of quantum mechanics, he reconsidered the physicists' adherence to only one "real" theory as a goal.

By then, Bridgman had studied the details of quantum mechanics enough to understand that it was not constructed in the way he regarded as his ideal. He knew that "Heisenberg and his school" emphasized the requirement that "our theories should contain only observable quantities."[111] However, when judged in the "actual working out," this requirement, though having "at first a most satisfying aspect," turned out to be less satisfactory than in anticipation. One could have considered the requirement as a way to save the concept of reality by "modifying the concept of reality as to make it closely associated with the possibility of direct observation."[112] However, this anticipation was not correct. "In fact," he continued,

> I am inclined to think that Heisenberg's demand that only

[110] *Ibid.*, p. 21.
[111] *Ibid.*
[112] *Ibid.*

> observable quantities enter the theory played only a suggestive role in leading to one of the many possible solutions and was as sterile in actually compelling the adoption of his form of theory as was the corresponding demand of Einstein that the law of gravitation be written in an invariant form. For if one examines how the principle works in practice, it will be seen that all that is demanded is that the raw material which is fed into the calculating machine and the final results which are taken out shall connect with direct observations. All the intermediate processes and operations, the internal pistons and gears of the theory, have as much the character of pure inventions as anything which Poincaré might have proposed.[113]

He finally admitted that the way quantum mechanics was built was no different from the way the general theory of relativity was structured. In both of the theories, only a limited part of the concepts adopted in the theory corresponded to experimental facts.

Quantum mechanics was not what Bridgman had been expecting it to be. Yet, he still tried to swallow this situation in his own way. "As a consequence of all this," Bridgman observed, "the attitude of the physicist to-day is changing toward mathematical theory," whereas "he takes it far less seriously[,] recognizes that it contains less of reality and more of a purely suggestive character than he had realized, and lays more emphasis on the demands of simplicity and convenience."[114] Bridgman had never been happy with such mathematical theory as the general theory of relativity, since he regarded it as only a speculative conjecture that did not always correspond to physical reality. However, confronted with the advent and success of quantum mechanics, he could not but admit the rise of mathematical inclination in physics. Instead of the requirement for the correspondence to physical reality, now "simplicity and convenience" functioned as a criterion for choosing an appropriate physical theory.

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[113] *Ibid.*

The vice-presidential address was not the first time for Bridgman to expose his observation of the recent change in the physicists' attitude. After understanding the general structure of quantum mechanics, he occasionally made similar remarks. For example, on June 6, 1928, answering the question of James MacKaye concerning the Fizeau experiment on the theory of relativity, he wrote thus:

> I believe that the situation here is much like that in mechanics where, as Poincare [*sic*] showed, it is always possible to give an infinite number of mechanical descriptions of any phenomena. So here I believe it would be possible to give an infinite number of descriptions of the situation differing only verbally, some of which might be described as relativistic and some as non-relativistic. The description which we adopt is largely determined by considerations of convenience and simplicity. There is no question that Einstein's description is a very simple and convenient way of dealing with an experimental situation which is much more complicated than was realized thirty years ago.[115]

Bridgman started to take Poincaré's proof seriously, applied it to the special theory of relativity, and even found a new criterion for physical theory, "convenience and simplicity." He repeated the same statement in explaining the present situation of theoretical study of microscopic phenomena. On April 1, 1929, when asked for his opinion on an theoretical attempt in quantum-physical realm, Bridgman commented thus: "You have to remember in this connection the remark of Poincare [*sic*]—that it will always be possible to explain in a purely mechanical way any system, no matter how complicated, and, furthermore, that an infinite number of such explanations are possible. The question is whether the particular one of the infinite number of possible

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114 *Ibid.*

115 Bridgman to MacKaye, June 6, 1928, PWBP, HUG 4234.8.

explanations which you have is simple and convenient."[116]

Now Bridgman could not believe in "absoluteness," either. A little later, when asked to review Tobias Dantzig's *In Quest of the Absolute* by an editor of the Macmillan, Bridgman criticized the title first: "Certainly no one *now* [Emphasis added] imagines that he can reach the absolute, or in fact that there is any meaning in the concept."[117] In *The Logic of Modern Physics,* he could advocate the absoluteness of operations. However, after the encounter with the uncertainty principle and quantum mechanics, he could no longer maintain the same view. Bridgman's view of physics and reality started to transform drastically immediately after he published the *Logic.*

Though wondering what he could really trust, Bridgman still seems to have been sure that at least two points presented by quantum mechanics would survive even if another new theory might some day supersede quantum mechanics. These two points are:

> (1) that the measurable properties of electrons embrace some phenomena which we find convenient to describe in terms of the wave phenomena of ordinary experience, in addition to the older and more familiar phenomena which we have satisfactorily dealt with in terms of a particle picture; and (2) that there is some essential limitation to the sorts of measurement that can be made simultaneously on elementary things, which is formulated in Heisenberg's principle of uncertainty.[118]

He believed that despite some criticisms against quantum mechanics "these two points of view transcend the mathematics by which they were derived, and that, inspired and guided by the mathematics, we have come upon a point of view which is of more permanent value than

[116] Bridgman to Yost, April 1, 1929, PWBP, HUG 4234.8.
[117] Bridgman to Latham, June 23, 1929, PWBP, HUG 4234.8.
[118] Bridgman, "Permanent Elements in the Flux of Present-Day Physics," p. 22.

the mathematics itself."[119] Though he could not totally trust the mathematical part of quantum mechanics, he accepted the wave-like properties of electrons and the uncertainty principle. The conclusion he drew from these points was: "Here we reach the actual frontiers of physical exploration."[120] In the end of his vice-presidential address, Bridgman suggested twenty-one questions to test the intuitive grasp of this situation in physics.

The 1929 vice-presidential address shows how Bridgman's criticism of quantum mechanics had developed by then. Recovering from the shock of the uncertainty principle, he started to examine the theoretical scheme of quantum mechanics and tried to find out what part of it he could accept. To him, among other things, the uncontrollability and unintelligibility of microscopic phenomena that quantum mechanics revealed seemed to survive, for example, even after the theory itself became invalid. In a letter to Bentley written one month after the address,[121] he reiterated that the reason he made a "remark about nature transcending all our efforts to get into perfect mental contact with it" and implied that this "transcendence of nature" was "a permanent and essential attribute" was that he had found "that on the microscopic scale it is almost certain that we can neither control or predict individual phenomena." Then, as a device to amplify this inability to control or predict, he again referred to the Geiger counter. Furthermore, he told Bentley that it would be possible for people to adapt themselves to the new situation, although then the operational definition of concepts might no longer be valid:

This inability to control or predict may be brought up to the

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119 *Ibid.*
120 *Ibid.*
121 Bridgman to Bentley, Feb. 8, 1930, PWBP, HUG 4234.12.

> macroscopic scale by such devices as the Geiger counter. In the sense in which I was using words, I thought of inability to control or predict as synonomous [*sic*] with inability to get into perfect mental contact. It is of course morally certain that we are going to adapt ourselves to this situation in some way, and when we do, we can describe our accomplishment as perfect mental contact. I am willing to admit that it is probably more desirable to describe this situation as perfect mental contact, but it is not the sense of the words as I was using them.[122]

Even after the uncontrollable, unpredictable aspects of nature become clear, one can still find out a way to describe it. But that description of nature will not consist of operationally defined concepts.

Bridgman's operational perspective had many defects as a philosophical discourse that various philosophers would criticize severely, especially in the 1950s. Though he was aware of these philosophical criticisms, he would never accept them. It was the implications of the uncertainty principle and quantum mechanics, not philosophers' various comments, that made Bridgman realize the fundamental vulnerabilities of his ideas and led him to reconsider their validity.

Through the process of assimilation of quantum mechanics, Bridgman gradually noticed the need to reformulate his operational perspective, though he could not start to work on it promptly. On January 26, 1931, Bridgman wrote Hugo Dingler about his feeling that some part of what the physicist had been taking for granted should be abandoned: "You can hardly expect a physicist to be quite as pessimistic as a philosopher about the condition of physics or to agree that the remedy demands quite as extensive abandonment of present positions in physics as you propose, but there are nevertheless many

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122 *Ibid.*

features in which our points of view are similar."[123]

Bridgman had some chances to address publicly the development in his effort to reform his operational ideas. On April 21, 1931, when he delivered a talk at the University of Wisconsin, Madison, the topic he chose was again the significance of Heisenberg's principle of uncertainty, its title being "The Recent Change of Attitude toward the Law of Cause and Effect."[124] The address itself was in many points similar to his previous discussions on the uncertainty principle, but this was the first time for him to mention publicly the Geiger counter as a device to amplify the unpredictability of microscopic phenomena. Furthermore, in the concluding remarks, he suggested new ways to adapt to the situation created by quantum mechanics.

Bridgman declared that the purpose of scientific activity was "the understanding, prediction and control of events,"[125] suggesting to avoid unpredictable and uncontrollable aspects of nature in order to save the ability to predict and control in microscopic realm. This statement was based on his observation that "although single small-scale events are unpredictable, the statistical average of large numbers of them is highly regular and predictable."[126] As for the understandability, he estimated that it would take "a little more adjustment," "because it involves giving up an ideal which we had set ourselves."[127] Yet he was optimistic: "But even here the adjustment can hardly take more than one generation, and in science generations are short."[128] What one has to do is to get used to the situation where "small-scale events show only statistical

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[123] Bridgman to Dingler, Jan. 26, 1931, PWBP, HUG 4234.10.
[124] P. W. Bridgman, "The Recent Change of Attitude toward the Law of Cause and Effect," *Science*, 73 (1931), pp. 539-547.
[125] *Ibid.*, p. 547.
[126] *Ibid.*
[127] *Ibid.*
[128] *Ibid.*

regularities."[129] Bridgman had already seen that "a number of the younger generation have already achieved this degree of emancipation," which he hoped to attain "by deliberate effort."[130] Bridgman had started to look for a way to adapt to the change in method and purpose of physics.

Bridgman regarded his reflection on the implications of quantum mechanics as an important modification of his operational standpoint. In 1931, while the preparation for the German version of *The Logic of Modern Physics* was proceeding, Bridgman proposed to add a special preface for the German translation and an appendix containing extracts from his *Harper's* essay and Madison address,[131] instead of altering the original text that might become obsolete by the time of its publication. However, as the addition of the extra material seemed to make the expense too great, he was informed of the possibility of omitting it from the German version. Bridgman could not hide his disappointment: "I am a little disappointed that it is not possible to include this because certain points of view in the book will now be somewhat antiquated in view of recent developments and I thought that this was the best way to indicate what modifications would be necessary."[132] In a preface for the German translation, which turned out to include an appendix, Bridgman explained why he added an extra material, though assuming that an essential revision of his standpoint was unnecessary: "[T]he Heisenberg principle is of such fundamental importance that the book should not be allowed to appear in fresh form without some recognition of it."[133] Without adding his analysis of the uncertainty principle,

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129 *Ibid.*
130 *Ibid.*
131 Bridgman to Dingler, May 19, 1931, PWBP, HUG 4234.10.
132 Bridgman to Dingler, Dec. 21, 1931, PWBP, HUG 4234.10.
133 P. W. Bridgman, Preface for the German translation of *The Logic of Modern Physics*, May 1931, PWBP, HUG 4234.15.

Bridgman could not regard his operational platform as complete.

Some of Bridgman's colleagues appreciated his effort to assimilate the implications of quantum mechanics. R. C. Tolman read his Wisconsin address and wrote: "Let me tell you how much I like it and how entirely I feel in agreement with the way you have gone at it. I am glad you do not indulge in the same theological bosh that comes from some of our American physicists." [134] Max Born made a similar comment on the same address: "The standpoint you explain in this paper is exactly the same which I should uphold. But being a man of theory, people would not be convinced by my words as easily as by yours, for you are known as one of the most exact experimentalists of the world and not a representative of the phantastic [*sic*] type." [135] Their responses, especially that of Born, one of the founders of quantum mechanics, must have flattered Bridgman.

But, perhaps the response of Edgar Buckingham, an experimental physicist at the National Bureau of Standard, expressed the feeling closer to that of Bridgman, whose everyday life was also occupied by "highly unabstract" laboratory work. After praising Bridgman vice-presidential address on the uncertainty principle, Buckingham confessed his honest impression by citing a sentence from Ecclesiastes:

> One of the most interesting things about this paper of yours is to see that you are gradually approaching the old man's point of view that "all is vanity"! I, being a good deal older, got there sooner; but am nowhere near as competent to expound that idea as you are.
>
> However - the flow of natural gas has to be muzzled, so I will go to work.[136]

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[134] Tolman to Bridgman, Oct. 28, 1931, PWBP, HUG 4234.10.
[135] Born to Bridgman, April 23, 1932, PWBP, HUG 4234.10.
[136] Buckingham to Bridgman, May 2, 1930, PWBP, HUG 4234.8.

Bridgman might have agreed with him and thought, "A higher pressure has to be reached, so I will go to work!"

5.2. Transforming Operational Analysis

During the first half of the 1930s, while analyzing the foundations of quantum mechanics, Bridgman ventured to apply his operational method to the new problems, broadening his perspective on science in general. He began contact with Rudolf Carnap, one of the leaders of Logical Positivism, and started to pay attention to the role of logic in science. The new ideas he acquired through these opportunities were addressed at the Vexenum Lectures given at Princeton University in December, 1935, which was published the next year as *The Nature of Physical Theory*. In the *Nature* he systematically expounded the operational view that included some modifications mainly necessitated by the transformation in physics taking place right after the publication of *The Logic of Modern Physics*. This modified version of operational perspective was further developed in his book on the foundations of thermodynamics *The Nature of Thermodynamics*, published in 1941. In the following, I will examine how he formulated the new version of his operational perspective.

5.2.1. Broadening the Perspective

Among many striking aspects of quantum mechanics, the principle of uncertainty had the strongest impact on Bridgman. While studying the mathematical details of the theory, he started to find some other points of the theory difficult to accept. The most serious problem that annoyed Bridgman and eventually led him to criticize the formulation of quantum mechanics was the fact that quantum mechanics was heavily dependent on the concept of probability.

Bridgman first started to analyze the concept of probability in statistical mechanics. Since the publication of the *Logic*, he had been willing to apply his operational analysis to the concept of probability, though having not been able to spare his time on this problem. On July 6, 1930, Bridgman wrote Korzybski about his desire: "It seems to me that the whole subject of entropy and statistics and probability is in about as hopeless a muddle as could be imagined and I have for long hoped that I could find a little time off from my more technical duties to see whether the 'operational' point of view, consistently applied, would not let in a little light."[137] He was at least sure that "it is dead wrong to attempt to apply any ideas of statistics or probability or entropy to the behavior of the universe as a whole, because the notions of probability etc make sense only when applied to epxerience [*sic*] that can be repeated an indefinite number of times under identical conditions, so that these notions can apply only to part of a system and never to the system which included everything." Bridgman did not have much more to say than this about probability. However, encouraged by Bertrand Russell's words on the problem of probability, he decided to start to analyze the problem of probability by himself: "The most sensible thing that I ever heard Bertrand Russell say was that the whole subject of probability is in such chaos that any one who wants to make a real contribution had much better start at the beginning for himself, entirely disregarding everything done before."[138]

In the summer of 1930, as J. Slater left Harvard to accept a position at MIT, Bridgman temporarily took over the course on thermodynamics and statistic mechanics. While reading the textbooks on this subject by M. Planck and James Rice at the University of

[137] Bridgman to Korzybski, July 6, 1930, PWBP, HUG 4234.10.

Liverpool, he decided to try to find out "what quantum theory is doing in applying statistical ideas to elementary processes."[139] He examined Heisenberg's Chicago lectures, published in German,[140] but found it "hard sledding." Fortunately for Bridgman, then Herbert Feigl, who had published a philosophical treatment of probability, arrived at Harvard. At his suggestion, Bridgman went on to read several articles on the problem of probability, including those by R. H. Nisbet and H. Reichenbach.[141] After going through these primers, however, he found that he could make his own way, writing Bentley, "I still think I could do a better job myself, which is perhaps not surprising."[142]

Bridgman expounded his comprehensive criticism of the concept of probability at the Ninth Josiah Willard Gibbs Lecture, delivered at New Orleans, on December 29, 1931 and published in *Science* the next year.[143] In the lecture titled "Statistical Mechanics and the Second Law of Thermodynamics," he pointed out the traditional and logical difficulty in applying the notion of probability to each concrete physical event. When applied to one single event, probabilistic statements become almost meaningless; on the other hand, even when applied to a long sequence of events, there can be no guarantee that the sequence is not one of the excessively rare sequences. To prove any probabilistic statement experimentally, or operationally, it is necessary to repeat the identical experiment for an infinite number of times, which can, in reality, never be carried out. Though he was not aware of it, in the

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138 *Ibid.*
139 Bridgman to Slater, Aug. 10, 1930, PWBP, HUG 4234.10.
140 W. Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (Leipzig: Hirzel, 1930).
141 The articles he read are: R. H. Nisbet's article in *Mind*, Jan. 1926; H. Reichenbach's article in vol. IV of the *Handbuch der Physik* on the foundations of physics; *Erkenntnis*, 1:2-4, containing the account of the first Tagung für Erkenntnislehre der exakten Wissenschaften in Prag.
142 Bridgman to Bentley, Jan. 11, 1931, PWBP, HUG 4234.10.
143 P. W. Bridgman, "Statistical Mechanics and the Second Law of Thermodynamics,"

concept of probability, Bridgman recognized the kind of difficulty he found in Cantor's diagonal proof of non-denumerability of real numbers (§4.2). After discussing several problems caused by applying the notion of probability to actual cases, he concluded: "The most important result will be, I hope, a keen realization that in using statistics we are only using a paper and pencil model, which has logical difficulties within itself and difficulties of application to concrete physical situations." [144] Though some physicists stated that the notion of probability was of "the primitive [primary] character" for quantum mechanics, he suggested that "much more experimental work is necessary"[145] before accepting this, though not specifying the kind of experiment. In the end Bridgman expressed his honest feeling toward the notion of probability and quantum mechanics:

> But even granted that the primitive character of the notions of probability acquires an experimental verification, it seems to me that some of the logical difficulties will persist, justifying a doubt as to the possibility of ever setting up a logically completely satisfying correspondence between our models and our experience.[146]

From this short statement, one may possibly observe a subtle shift in his operational perspective: In the *Logic*, he insisted that physical theory should be constructed by operations, while in this Gibbs Lecture, his criticism was mainly on the difficulty in applying physical theory to concrete physical events. After quantum mechanics was formulated in 1925-1926, Bridgman could no longer discuss how physical theory

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Science, 75 (1932), pp. 419-428.
[144] *Ibid.*, p. 428.
[145] *Ibid.*
[146] *Ibid.*

should be constructed, mainly analyzing how to understand and use the present physical theories.

Bridgman successively applied his operational analysis to the astronomical concept of time and then to the validity of cosmic observations.[147] Around the same time he published his criticism of Cantor's diagonal proof.[148] Yet, the concern with quantum mechanics never left Bridgman's mind. Even when discussing with Korzybski recent criticism on the foundations of mathematics by Bertrand Russell and E. T. Bell, he could not but interpret it in connection with quantum mechanics: "The whole situation you present in mathematics was somewhat of a revelation to me. [...] The whole situation makes one wonder a bit at the mathematical confidence of the young men in quantum mechanics."[149]

Meanwhile, Bridgman seemed to communicate with Logical Positivists. On August 18, 1935, answering Warren Weaver's question on the possible connection between the uncertainty principle and the freedom of will, Bridgman pointed out that it was a "pseudo" problem "as the Viennese school calls" it:

> It seems to me that most of our difficulties of this sort arise from the imperfections of the tools with which we think; in particular, our tools have a limited range of applicability, but we are not poroperly [*sic*] conscious of this and attempt to apply them out of their legitimate range. In this way arise many problems which disappear when adequately analyzed. [...] It seems to me that the problem of determinacy as bearing on the freedom of our will and our course of action is a pseudo problem.[150]

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[147] P. W. Bridgman, "The Time Scale: the Concept of Time," *Scientific Monthly*, 35 (1932), pp. 97-100; "On the Nature and the Limitations of Cosmical Inquiries," *Scientific Monthly*, 37 (1933), pp. 385-397.

[148] P. W. Bridgman, "A Physicist's Second Reaction to Mengenlehre," *Scripta Mathematica*, 2 (1934), pp. 101-117, 224-234.

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

[150] Bridgman to Weaver, Aug. 18, 1935, PWBP, HUG 4234.10.

As Bridgman became familiar with some works of the Viennese Circle, he started to consider the role of logic and languages, though mainly regarding them only as tools for thinking. As will be shown later, the difference between Bridgman and logical positivists would become clearer in their correspondence.

In the same letter to Weaver, Bridgman also made a remark on the concept of "reality," which he had once abandoned in explicating the implications of quantum mechanics: "I think that all reality can mean is something about the way in which we think about the handle [of] our experience. Certain aspects of experience recur, and we have found out what to do in order to make them recur. Reality is a short hand restatement of what is involved in this complex." [151] In this reformulation, reality now means an aspect of experience that one can make recur. Defined in this way, reality becomes palpable, controllable, and intelligible in terms of repeatable operations. Recovering from the shock given by quantum mechanics, Bridgman steadily reconstructed his standpoint.

Since 1934, Bridgman had started to have a stimulating, though not very fruitful, interaction with Rudolf Carnap, who sent Bridgman his book, *The Unity of Science*. [152] Bridgman was grateful for the present, writing about his feeling toward Logical Positivism: "In general I have taken great satisfaction in thw [*sic*] writings of the Viennese circle, including many of your own, as being more nearly akin to my own views than nearly any other analytical writing with which I am acquainted,

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[151] *Ibid.*
[152] Rudolf Carnap, translated by M. Black, *Unity of Science* (London: Kegan Paul, Trench, Trubner & co., ltd., 1934). Originally appeared in *Erkenntnis*, 2 (1932), pp. 432-465 under the title "Die physikalische sprache als universalsprache der Wissenschaft."

and this last book of yours is no exception."[153] He, nevertheless, could not but mention the difference between their views:

> However, in my own thinking there is always one element which is more prominent than it appears to be in the thinking of the viennese [*sic*] circle; perhaps this is because I am primarily an experimental physicist. I find myself saying that language and thinking are human devices for dealing with certain situations and I therefore expect that although they may be highly perfected devices that nevertheless they will have imperfections and that they will not exactly meet all situations in all particulars. [...] [I]t is rather a general commentary on a difference which I find between my general point of view and that of the Viennese circle. I have the feeling of this difference particularly with regard to logic. Logic is a tool of human thought wielded by human beings. I do not think it is a perfect tool, ajd [*sic*] I believe that there will always be conceptual situations which can never be contemplated with complete logical serenity if one pushes his analysis far enough. If the Viennese circle said things like this out loud a little more enphatically [*sic*] I think we would be in almost complete accord.[154]

Despite his sympathy with the Viennese Circle, Bridgman felt that the status of logic in their scheme was much higher than in his scheme. Carnap did not think the difference was essential. Agreeing with Bridgman that language and logic were imperfect devices, Carnap explained that the Viennese Circle was a little more enthusiastic than Bridgman about improving these imperfect tools.[155]

Though the interaction was not very beneficial, Bridgman kept paying attention to the works of logical positivists. As he once wrote Bentley on March 19, 1936,[156] he felt more sympathetic to the Viennese Circle than to "any other bunch of philosophers," partly because he had

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153 Bridgman to Carnap, Sept. 19, 1934, PWBP, HUG 4234.10.
154 *Ibid.*
155 Carnap to Bridgman, April 14, 1935, PWBP, HUG 4234.10.

been informed of thus by Feigl, and partly because he himself found so through his reading. To some points in their discussion, such as their emphasis on the existence of "pseudo-problems" and their impatience with metaphysics, he could give a full agreement. However, he could not fully agree with them. For instance, though Bridgman understood the book Carnap sent to him, he "couldn't make it fit with the way" he liked to think.

Bridgman gained more interest in the problem of logic through conversation with Quine, who struck him "as a very sane and able person."[157] Apparently he felt more sympathy to Quine than to the Logical Positivists. Quine informed Bridgman of philosophers' discussion on logic, including Ludwig Wittgenstein's works. Bridgman's concern with logic was whetted by his recognition that logic had become of importance for physics because of its use in mathematics.[158] At the end of 1936, he even joined the Symbolic Logic Association.

5.2.2. Reformulated Operational Perspective

In December 1935, Bridgman outlined the reformulated operational view at the Vexenum Lectures given at Princeton University (published the next year under the title *The Nature of Physical Theory*).[159] In the beginning, Bridgman clearly stated that the purpose of his analysis was to understand the change in physical theory, and that for this purpose he would take the attitude as a "critic."[160]

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156 Bridgman to Bentley, March 19, 1936, PWBP, HUG 4234.10.
157 *Ibid.*
158 P. W. Bridgman, memorandum for the talk at the Symbolic Logic Association, Dec. 28, 1936, PWBP, HUG 4234.15.
159 P. W. Bridgman, *The Nature of Physical Theory* (Princeton: Princeton University Press, 1936).
160 *Ibid.*, Preface, p. 1-2.

Compared with his ambitious goal in the *Logic*, namely, to show a guideline for reforming physics towards a new theory, his aim in the *Nature* seems to be too humble. Yet, believing that "the ultimate task of the physicist is to understand," he considered the critical work of physics as "just as important [...] as the purely factual aspect," especially because he observed that "our present theories, even the successful ones, are not yet constructed so completely in accord with sound principles."[161] By the "present theories" he meant relativity theory and quantum mechanics, but obviously the latter, scrutinized in the chapter before the last, was the main target of his criticism. The chapters were systematically organized for this purpose: the discussion started with "Operations," then developing into "Thought, Language," "Logic," and "Mathematics"; after "Mathematics in Application" was examined, "Relativity" appeared as a topic; before scrutinizing "Wave Mechanics," he discussed "Mathematical Models and Probability." As he himself commented in the chapter titled "Operations," Bridgman's analysis in the *Nature* accentuated the importance of examining the properties of mental process than might seem to be necessary.[162] He explained that he did so partly because of the influence of "the emphasis of wave mechanics on the observer as a necessary part of any physical system," assuming that "the role of the observer cannot be adequately understood without an appreciation of the way he must think."[163] In the *Logic*, his object of scrutiny was physical theory; but then, quantum mechanics made him aware of the importance of the observer. In the *Nature*, therefore, he turned his attention to the human aspects of physics.

The position Bridgman reached after his examination of human

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161 *Ibid.*, p. 4.
162 *Ibid.*, p. 13.
163 *Ibid.*

mental processes was that "I can never get outside of myself," namely, "direct experience embraces only the things in my consciousness—sense impressions of various sorts and various sorts of cerebrations—and naught else."[164] Bridgman denied the existence of public or mass consciousness and declared: "In the last analysis science is only my private science, art is my private art, religion my private religion."[165] Though he admitted that in deciding what should be his "private science" he found it profitable to consider only those aspects of his direct experience in which his fellow beings acted in a particular way, he insisted that it did not obscure "the essential fact" that it was his and "naught else."[166] Though some may ask how there can be agreement as to experience unless there are external things that both you and I perceive, Bridgman's answer was simple: "[A]n external thing is merely a part of my direct experience to which I find that you react in certain ways."[167] He could not find any other "operational meaning" attached to the concept of an external thing.

The chapter "Operations," though revealing Bridgman's solipsist position that he had reached in the years after publishing the *Logic*, did not detail the properties of operations themselves. Bridgman again avoided analyzing the concept of operation. He only reiterated the usefulness of operations as a tool, or "a secure basis," for analyzing physical theories and concepts: "This method was suggested by the clear recognition that the ultimately important thing about any theory is what it actually does, not what it says it does or what its author thinks it does, for these are often very different things indeed."[168] As in the *Logic*, operations were not the purpose, only tools. The plural form of

[164] *Ibid.*, p. 13.
[165] *Ibid.*, pp. 13-14.
[166] *Ibid.*, p. 14.
[167] *Ibid.*, p. 14.
[168] *Ibid.*, p. 5.

the title shows that he laid more stress on the importance of "mental operations" than before, in view of the rise of what he called "mathematical theories," including quantum mechanics. He considered "Thought, Languages," and "Logic" as "devices," possibly imperfect, to analyze and describe experience; only "by experiment," was it possible to find whether they were successful or not. Bridgman even doubted the validity of thought, language, and logic in the age of relativity theory and quantum mechanics. For example, he scrutinized the validity of the law of excluded middle:

> Consider first tests applicable to physical objects. Can I make my test for example in such a way as to justify me in saying an apple is either green or it is not green? The operational situation is obvious enough. There must be some sort of test which I can apply to any particular apple with which I may be presented. This test may perhaps be a test performed with physical instruments, as for example, I may say that by definition the apple is green if the center of gravity of the reflected light has a wave length between 5200Å and 5600Å. The question now is, is this test such that we can always assert that the apple either satisfies it or it does not? It is obvious that it is not of this kind, since we know that because of instrumental uncertainty and errors of observation cases will arise in which we cannot say whether the wave length is greater or less than one of the critical values.[169]

Though one can assign a definite physical operation to the proposition "an apple is green," this proposition does not make sense operationally as this operation itself is uncertain. This means that the law of excluded middle cannot physically apply in this case. Even when the definite rule or technique corresponds to the proposition, the result of operations is not always unambiguous. From this observation, he drew the conclusion that "the law of the excluded middle is not a valid

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[169] *Ibid.*, pp. 37-38.

description of our actual physical experience," suggesting a third category, a category of "doubtful," in addition to positive and negative. In the like manner, he showed that even the concept of truth could have "no such static, absolute, meaning as we would like to give it," because of "the fundamental properties of activity itself."[170]

Bridgman regarded mathematics also as only a tool, though quite a useful one. For instance, "numbers have been an astonishingly successful device, and it is difficult to visualize situations in which they might lead to contradiction."[171] However, Bridgman again picked up an example from quantum physics that showed the limit of a tool for reasoning: quantum mechanics teaches that electrons do not have identity and therefore are not countable in the ordinary sense. He continued: "Indeed we are already pretty well convinced that the concepts of space and time have proved unsuccessful when carried inside the atom; and why may not the concept of number?"[172] Though admitting the usefulness of mathematics, Bridgman turned more attention to the negative aspect of mathematics, concluding thus: "We may [...] not anticipate complete success by mathematics in meeting all actual situations."[173]

Still, Bridgman was fully aware that mathematics, when applied to physics, showed considerable effectiveness. "What we have now," he observed, "is in effect mathematical models rather than physical models."[174] In Bridgman's understanding, though physical models are easier to visualize and have a simpler mathematical formulation, mathematical models promises "greater theoretical power."[175] He

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170 *Ibid.*, p. 42.
171 *Ibid.*, p. 51.
172 *Ibid.*, p. 52.
173 *Ibid.*, p. 58.
174 *Ibid.*, p. 62.
175 *Ibid.*

found "an enormously greater wealth of possibility among the structure of mathematics."[176] At the same time, however, Bridgman pointed out that "the ordinary physicist will want to keep his physical models as long as he can,"[177] as it presents "explanation" of physical phenomena by analyzing a complicated system into a familiar, simple one.

For example, the kinetic theory of gases constituted to be an explanation of the behavior of a gas "in a sense which the purely mathematical model, consisting of the first and second laws of thermodynamics and a characteristic function or two, could not, because there is evidence from other phenomena of the existence in the gas of molecules as postulated in the physical model."[178] In such a case, the elements in the physical model have such recognizable counterparts as the mass or momentum of molecules in the physical system. This correspondence between the model and the system is as simple as to call for no discussion and therefore makes it possible for the physical model to explain the physical system in a convincing way. On the other hand, it seemed to him that, in the mathematical model, the correspondence could be arbitrary, and that the possibility of giving an explanation has been given up.

Bridgman detailed why mathematical models should be adopted despite the physicist's preference of physical models:

> [T]he new physical experience of which we are trying to form a theory is so far removed from ordinary experience that we cannot find in it the counterpart of any of the objects of ordinary experience. Under such conditions it would seem that an explanation in the ordinary sense is impossible, and recourse to a mathematical model of some sort becomes just as satisfactory

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[176] *Ibid.*
[177] *Ibid.*, p. 63.
[178] *Ibid.*

as recourse to a physical model.[179]

"This," Bridgman went on, "of course is what has happened in wave mechanics." [180] Quantum mechanics had shown that the most fundamental concepts in physics, such as space, time, and identifiability, were no longer applicable to microscopic phenomena. Only by using a purely mathematical model could physicists deal with atoms. Understanding that the dominance of mathematical models was inevitable, Bridgman could no longer demand that every step in the mathematical manipulation of the equations should have its counterpart in the physical system. To him, Heisenberg's requirement that "only those quantities shall enter the equations which are intrinsically measurable" seemed to be "a sort of philosophical justification" formulated after the event for the success of quantum mechanics: "as a guiding principle it appears to have been sterile or even positively misleading, and it is certainly given up in Dirac's form of wave mechanics."[181]

If physics is full of mathematical models not corresponding to the actual physical system, then, what kind of criterion one can invoke to choose an only one trustful model? This was the question Bridgman tried to answer at the beginning of the chapter titled "Mathematical Models and Probability." His answer was simple: "we would have to choose [...] on the ground of convenience or of ease of calculation, or of simplicity in the argument by which the model was set up."[182] If all the assumptions of a theory are experimentally proved, that theory could be regarded as a "true" theory. But, as has been shown by quantum mechanics, "now that it is realized that there are essential limitations to

[179] *Ibid.*, pp. 63-64.
[180] *Ibid.*, p. 64.
[181] *Ibid.*, pp. 65-67.

the accuracy with which certain kinds of physical measurement can be made,"[183] it has also been recognized that such experimental confirmation is not actually realizable. Furthermore, since what can usually be verified by experiment is not the assumptions, but only their "indirect consequences," "we would be hesitant to apply the word truth to this situation, [...] because truth always carries, by traditional usage, the further implication of uniqueness": "As long as we were uncertain whether there might not be some other assumption [...] which would lead to the same measurable results, [...] we would not be willing to say that our particular assumption was 'true.'"[184] In such cases, Bridgman judged, "the concept of true is not applicable."[185] The same judgment can also apply to the status of the concept of "reality." However, Bridgman did not deny the validity of this concept, only concluding that "the concept of physical reality has to be modified in the realm of construction beyond the reach of accurate measurement, and in particular, that reality can no longer connote uniqueness."[186] He would later try to redefine the concept of "reality" in *The Nature of Thermodynamics*.

Now, most of physical theories can be neither "true" nor corresponding to "reality" but have been chosen for their "simplicity" and "convenience." Bridgman called them "possible theories."[187] To him, it seemed that "the discovery of inherent limitations to the accuracy of physical measurements disclosed by wave mechanics opens the door to a flood of 'possible' theories."[188] This same principle, namely the uncertainty principle, introduced the notion of probability

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[182] *Ibid.*, p. 93.
[183] *Ibid.*, p. 94.
[184] *Ibid.*
[185] *Ibid.*
[186] *Ibid.*, pp. 94-95.
[187] *Ibid.*, p. 95.

into quantum mechanics: "The inherent necessity of there always being error is secured by the device of reducing everything to a basis of probability."[189] He thus began his criticism of quantum mechanics by attacking the concept of probability.

Though admitting that quantum mechanics succeeded in reproducing the quantitative character of atomic phenomena far better than previous theories, Bridgman regarded this success not as "the result of deliberate design," but as "a more or less incidental by-product of the fundamental mathematical mechanism."[190] Among the operational defects of quantum mechanics, Bridgman first took up its dependence on the notion of probability. By exploiting the notion of probability at the expense of completely deterministic predictability, quantum mechanics succeeded in representing the experimental uncertainty as an intrinsic element in physics. However, Bridgman pointed out that this notion could apply rigorously only to a mathematical infinite ensemble, and not to a physical, therefore finite, ensemble. He repeated his favorite attack against mathematical infinity and expressed it in a form of paradox: "[W]hat theory is it that is not correct unless it leaves open the possibility that it may be incorrect? Answer: the theory of probability."[191]

Quantum mechanics, which depends on the notion of probability as an unanalyzable element, is unavoidably "a mathematical model," though pretending to be "formally a thoroughgoing operational theory [...] by labelling some of the mathematical symbols 'operations,' 'observables,' etc."[192] Bridgman recognized that it actually had nothing to do with operations in the laboratory: "[T]he exact corresponding

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188 *Ibid.*
189 *Ibid.*, p. 113.
190 *Ibid.*, p. 111.
191 *Ibid.*, p. 102.
192 *Ibid.*, p. 118.

physical manipulations are often obscure, at least in the sense that it is not obvious how one would construct an idealized laboratory apparatus for making any desired sort of measurement."[193] Furthermore, he could find no principle in interpreting what quantum mechanics predicted in terms of operations:

> The way in which conclusions about the properties of the mathematical model are to be translated into conclusions about the corresponding physical system is not capable of specification with logical precision. [...] We are really concerned more with an art than a science, an art which is to be learned only by observation of the way the inventers of the theory do it. [...] What we have here is a special sort of intellectual tool, of great utility in meeting in the situation of practice. The fact that the mode of action of this tool cannot be reduced to completely logical terms we have seen to be a commentary, not only on the character of the tool, but also on our preconception that all mental methods of satisfactorily meeting actual situations must be reducible to logically rigorous operations.[194]

In Bridgman's observation, quantum mechanics was not a tool constructed on a logical basis, and its elements did not correspond to actual operations. Quantum mechanics is not a science, but an art; to learn how to use it, one had to imitate what the inventors did, like an apprentice learning an art from a master.

Regarding quantum mechanics as a kind of instrument, Bridgman did not believe that discussion on physical reality based on quantum mechanics could be fruitful, though admitting that "[t]he mere fact that such a debate is possible as that carried out on the one hand by Einstein, Podolsky and Rosen, and on the other hand by Bohr, increases our disquietude."[195] In 1935, Einstein, Boris Podolsky, and

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[193] *Ibid.*, p. 118.
[194] *Ibid.*, p. 117.
[195] *Ibid.*, p. 119.

Nathan Rosen published an article, to be known as the EPR paper, and cast doubt to the completeness of quantum-mechanical description of physical reality.[196] Presenting a thought experiment consisting of two independent particles, the authors argued that there could exist elements in physical reality that were predictable with absolute certainty but had no counterpart in quantum mechanics. Responding to EPR, Niels Bohr analyzed the details of their experiment and pointed out an uncontrollable disturbance involved in the EPR experiment but overlooked by them.[197] Bohr discussed that this disturbance would make unpredictable the elements in physical reality that EPR claimed and would inevitably limit our knowledge of nature.

Despite apparent similarity between the EPR argument and Bridgman's operational requirement shown in the *Logic*, Bridgman basically approved Bohr's position, which seemed to him to be "essentially an argument from simplicity."[198] He judged EPR to be insisting that quantum mechanics was incomplete on the ground that it did not satisfy their standard for physical theory and physical reality. On the other hand, Bridgman observed that "Bohr [...] takes the position that our preconceived views as to the nature of physical reality must be modified in view of the recently discovered fact that a mathematical theory is possible which agrees with physical experience in all pertinent particulars but which takes a modified attitude as to the nature of 'reality.'"[199] Bridgman accepted Bohr's discussion that one should modify the concept of physical reality so that it would become compatible with the new theory, but not the contrary as EPR required.

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196 A. Einstein, B. Podolsky, and N. Rosen, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" *Physical Review*, 47 (1935), pp. 777-780.
197 For Bohr's position, see: N. Bohr, "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?" *Physical Review*, 48 (1935), pp. 696-702.
198 Bridgman, *The Nature of Physical Theory*, p. 119.
199 *Ibid.*

In the *Logic* and his following criticisms of quantum mechanics, Bridgman maintained the requirement as to physical reality and physical theory that were similar to, or even more demanding than, EPR's. However, in the *Nature*, he no longer clung to his old ideals: "what we mean by physical reality is to a large extent a matter of convention and convenience."[200] He even suggested a more radical attitude toward epistemological problems in general: "one is free to adopt the epistemology of wave mechanics if it is convenient."[201]

Even if so, however, quantum mechanics seemed to Bridgman to have too many defects to provide a sound basis for any kind of epistemology. Besides its dependence on probability, the dualism between the classical and the quantum-mechanical points of view annoyed him. For instance, the meaning of the independent variables in Schrödinger's wave equation, namely, space and time, appeared obscure to him.[202] In his understanding, points in space as such were neither identifiable nor physically meaningful, if not marked by the presence of material particles. What could mark points in space most precisely would be the smallest particles, or electrons. However, they were, as quantum mechanics had shown, proved to have no fixed abode, and their location was a probability matter determined by the equations themselves. To Bridgman, the variables of the equations appeared not to have ostensible physical significance, but to be purely constructional quantities adopted in the mathematical model.

Bridgman illustrated another example of dualism by analyzing the notion of a particle.[203] At least initially, quantum mechanics implicitly treated electrons as particles in the classical sense, but they turned out

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200 *Ibid.*, p. 120.
201 *Ibid.*
202 *Ibid.*, p. pp. 123-124.
203 *Ibid.*, p. 128.

to lack the most essential characteristics of the classical particle, namely, individuality and identifiability. Quantum mechanics handled this situation by first accepting the implications of the classical particles and then superposing on this picture a special statistics. By this procedure, what was problematic could be concealed. Clearly, however, no one could imagine a particle without individuality.

Bridgman's criticism was that the new theory arbitrarily adopted both the classical and the new quantum-mechanical concepts. Some argued that old concepts were still one-half applicable to what happened on the elemental level and that the other was pure chance. They accounted for the evolution of the classical concepts of space, time, and causality on the level of ordinary experience by postulating that the effect of the probabilistic element became gradually canceled out in the macroscopic phenomena by virtue of the regularities of large numbers. Bridgman did not like their self-deceptive argument. "In general," he summarized, "it seems to me that wave mechanics is less successful in bringing everything under a common point of view than one might expect from the way some of its expounders talk about it, and that we have in fact an apparently unresolvable mixture of classical and wave mechanical elements."[204] As in the cases of space coordinate and the notion of particle, one could grasp the meaning of the fundamental physical concepts in quantum mechanics only through a clumsy consideration of both the classical and the quantum-mechanical viewpoints. Bridgman stated that "the esthetic motive, if nothing else, will prove sufficiently strong to ensure the continuation of effort to get a somewhat more homogeneous appearing theory,"[205] suggesting a way to improve this situation, a way to understand the uncertainty principle more intuitively without the help of the old images:

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[204] *Ibid.*, p. 129

> To a number of persons the Heisenberg principle of indetermination has seemed evidence of something with deeper significance than a purely mathematical non-commutability of *p* and *q* numbers. One would like to find out how to handle this new principle intuitively, in its own right, divorced from the severe mathematical considerations that gave rise to it.[206]

Whenever discussing the implications of quantum mechanics, Bridgman finally came back to the uncertainty principle. After finding his older operational view invalidated by this principle, he regarded the principle as a basis of the new operational perspective. To him, the first step toward a new epistemology seemed to be an effort to find a way of understanding the uncertainty principle intuitively.

In the concluding remarks, Bridgman emphasized that physical theory was, after all, human invention whose limitations could be imposed "by lack of time, or by insufficient intellectual power, or by those cruder human traits such as a too self-seeking competitiveness."[207] Physical theory might have crucial defects and would not be valid forever. Bridgman insisted on the importance of criticism of the fundamental concepts in physical theory, though understanding that such work appeared outdated and that some might be even contemptuous of it. He described many other physicists' attitude toward this type of work thus: "[I]t is easy to say to a fundamental question in wave mechanics: 'I do not know the answer to that, but I suppose that Dirac or Bohr or Heisenberg has thought it through and knows the answer.'"[208] Bridgman would never take such an attitude, as he understood that no theory could be free from traces

[205] *Ibid.*
[206] *Ibid.*, p. 131.
[207] *Ibid.*, p. 135.
[208] *Ibid.*, p. 134.

of human invention or escape from the possibility of being someday superseded by another theory:

> Every new theory as it arises believes in the flush of youth that it has the long sought goal; it sees no limits to its applicability, and believes that at least it is the fortunate theory to achieve the "right" answer. This was true of electron theory. [...] It is true of general relativity theory with its belief that we can formulate a mathematical scheme that will extrapolate to all past and future time and the unfathomed depths of space. It has been true of wave mechanics, with its first enthusiastic claim a brief ten years ago that no problem had successfully resisted its attack provided the attack was properly made. [...] When will we learn that logic, mathematics, physical theory, are all only our inventions for formulating in compact and manageable form what we already know, and like all inventions do not achieve complete success in accomplishing what they were designed to do, much less complete success in fields beyond the scope of the original design, and that our only justification for hoping to penetrate at all into the unknown with these inventions is our past experience that sometimes we have been fortunate enough to be able to push on a short distance by acquired momentum?[209]

No physical theory is perfect. This relativistic view of science was what Bridgman achieved finally through his intellectual struggle with quantum mechanics. In a sense, it was a by-product of a revolution caused by quantum mechanics, which invalidated the old ideal of uniqueness of truth, reality, and knowledge.

One may suspect that it was a hard lesson for an experimental physicist in his fifties. To Bridgman, however, this conclusion was not pessimistic, "as most people seem to suppose," but optimistic, as he personally felt "more than before that [he was] master of the thing and can get what [he wanted] out of it."[210] Moreover, he explained his

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[209] *Ibid.*, p. *136*.
[210] Bridgman to Bentley, Aug. 2, 1936, PWBP, HUG 4234.10.

enthusiasm over the above discussion that he addressed to the Princeton scientists:

> I had constantly in mind the audince [*sic*] that I hoped to address, containing such mathematicians as Weyl and such physicists as Einstein and von Neumann, whose cocksureness I have always found profoundly irritating. The point which I was trying to ram home was that there is no justification for cocksureness, and I was trying to make this point by specifically pointing out the haze which I always run into when I push my analysis far enough.[211]

Bridgman was confident in his critical examination of the present physical theories. However, he was to find out that "no one [...] seems to like at all"[212] the lectures.

5.2.3. Responses to the Modified Operational Perspective

Not many responded to the *Nature*, and published reviews were not very encouraging. The reviewer for the *Review of Scientific Instruments*, Karl K. Darrow at the Bell Telephone Laboratories was an old acquaintance of Bridgman. Darrow's review[213] was actually a detailed, friendly, and careful summary of the book. He could not do otherwise, as he felt that "[review of the volume] ought to be confided to a professional philosopher, and not to a physicist"[214] like himself. Bridgman commented thus: "I do not remember that this review has anything especially important in it except that it was on the whole favorable, something so unusual as to make me remember."[215]

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211 Bridgman to Bentley, Aug. 12, 1936, PWBP, HUG 4234.10.
212 Bridgman to Bentley, Aug. 2, 1936, PWBP, HUG 4234.10.
213 Karl K. Darrow, Review of *The Nature of Physical Theory*, *Review of Scientific Instruments*, 7 (1936), pp. 374-375.
214 *Ibid.*, p. 374.
215 Bridgman to Bentley, Dec. 18, 1937, PWBP, HUG 4234.10.

A review by William Malisoff, published in the July 1936 issue of *Philosophy of Science* and titled "The Universe of Operations,"[216] was surprisingly critical to the book by a member of the editorial committee of the journal. He noticed the weakest point of Bridgman's discussion in general, pointing out that "[Bridgman's] demand for operational criteria has not operational basis."[217] In his judgment, the volume was "packed with characteristic Bridgman hardheaded opinion" and had "a dogmatic ring."[218] Malisoff's answer to Bridgman's question, "What theory is it that is not correct unless it leaves open the possibility that it may be incorrect?" was "the theory of operationalism."[219] Probably Bridgman read this review, as he once wrote to Bentley thus: "With regard to my own reading of what other people may say about my stuff, I have never been able to make much sense out of it, so that I have given up trying to read anything except what comes out in Philosophy of Science."[220] Though writing nothing about Malisoff's review, Bridgman would adopt its title, "the universe of operations," as one of the key concepts in his *Nature of Thermodynamics*, published in 1941.

In 1937, another criticism of the *Logic* and the *Nature* appeared in *Philosophy of Science*.[221] The author R. B. Lindsay at Brown University admitted the significance of operationalism but interpreted it narrowly as an extreme statement that "physical concepts should be defined in terms of actual physical operations."[222] He attempted to defend the "well-recognized" ways of theoretical physics, such as the use of mathematics, construction of models, and dependence on probability

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216 W. Malisoff, "The Universe of Operations," Review of *The Nature of Physical Theory*, *Philosophy of Science*, 3 (1936), p. 360-364.
217 *Ibid.*, p. 361.
218 *Ibid.*, p. 360.
219 *Ibid.*, p. 364.
220 Bridgman to Bentley, Dec. 18, 1937, PWBP, HUG 4234.10.
221 R. B. Lindsay, "A Critique of Operationalism in Physics," *Philosophy of Science*, 4 (1937), pp. 456-470.

and statistics.

Though Bridgman regarded Linday's article as "just a tissue of misunderstanding,"[223] he published his rebuttal the next year.[224] He tried to make clear that he never "talked of 'operationalism' or 'operationism'," the "grandiloquent words" for which he had "a distaste," and that many critics of operationalism had made statements as to the method which Bridgman would "by no means accept."[225] Admitting that his statement in the *Logic* that "meanings are synonymous with operations" was "going too far when taken out of context,"[226] he advised that the reader apply to this statement his own dictum that "what a man means by a term is to be found by observing what he does with it, not by what he says about it."[227] This was probably the first time that Bridgman applied his operational analysis to operationalism. As for Lindsay's accusation, Bridgman reiterated his recognition of the importance of "mental" or "paper and pencil" operations, which he had already detailed in the *Nature*. He understood the ultimate object of physical theory to be the description of a concrete physical situation and demanded that its ultimate outcome be expressible in terms of operations applicable in the concrete physical situation; yet he accepted that physicists could allow themselves "any latitude whatever" in their intermediate constructions.[228]

Whether Bridgman liked it or not, the words "operationism" and "operationalism" were spreading among scientists and philosophers, which made him better known as an originator of the philosophical

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[222] *Ibid.*, p. 456.
[223] Bridgman to Bentley, Dec. 18, 1937, PWBP, HUG 4234.10.
[224] P. W. Bridgman, "Operational Analysis," *Philosophy of Science*, 5 (1938), pp. 114-131.
[225] *Ibid.*, p. 114.
[226] *Ibid.*, p. 117.
[227] *Ibid.*, p. 117.
[228] *Ibid.*, p. 124.

position labeled by these terms than as an experimental physicist in high-pressure physics. The publication of his attempts to apply the operational analysis to social problems, such as the one discussed in *The Intelligent Individual and Society* (1938),[229] gave further impetus to this tendency. In the late 1930s, not only scientists but philosophers and psychologists began to examine Bridgman's operational standpoint. However, their interactions were not very fruitful. While most of criticisms and advocacies of Bridgman's discussion centered on the bold but vulnerable statements in the *Logic*, Bridgman responded to them by presenting his analysis mainly developed in the *Nature*. Many, perhaps including Bridgman himself, were not very conscious of the transformation of operationalism that had taken place between these two writings of Bridgman.

Without much help from others, Bridgman continued to make his effort to secure a sound perspective on the foundations of physics. His analysis of thermodynamics *The Nature of Thermodynamics*, [230] published in 1941, shows some of the results of such an effort he had made by then. Bridgman began the third chapter of the volume, "Miscellaneous Considerations," by a section on "The Universe of Operations of Thermodynamics" and concluded the same chapter by remarks on "Physical Reality." By then, he started to reflect on what was necessary to construct operationally satisfactory theory:

> One condition that we always try to impose when we can is that the system which we generate with any particular universe of operations must be a "causal" system. That is, when a system is completely described in terms of all the operations in the given universe, then the future course of the system, described in

[229] P. W. Bridgman, *The Intelligent Individual and Society* (New York: The Macmillan Company, 1938).
[230] P. W. Bridgman, *The Nature of Thermodynamics* (Cambridge, Mass.: Harvard University Press, 1941; New York: Harper & Brothers, 1961).

> terms of the *same* universe of operations, must be completely determined, or, in other words, it must repeat itself when the prior conditions are repeated.[231]

In thus discussing he did not intend to present controllable system or operations. He no longer clung to the idea that both nature and knowledge of it as such were controllable. Instead, he was attempting to show how to construct operational physical theory out of some limited but controllable area of nature, pointing out the necessity to choose a "causal" universe of operations for this purpose.

As for "physical reality," Bridgman could conclude nothing definite. He understood this concept in this way: "I have taken the position that the concept of 'reality' is one that we must learn to get along without, and that the strength of one's impulse to use it is a measure of one's operationally weak-mindedness."[232] However, Bridgman used this word several times in the volume, without giving any specific definition. The following paragraph illustrates the change his idea of "physical reality" had undergone:

> I think we are rather likely to ascribe "less" "physical reality" to something in which the connection of the final result to the instrumental operations is through a long and complicated series of paper and pencil operations than when the connection is more immediate. I think most of us would have the verbal impulse to ascribe a greater "physical reality" to the mass of a gram of water than to its absolute entropy as obtained by a statistical calculation by the Boltzmann formula.[233]

Now physical reality has become a matter of degree: some physical quantities can have more physical reality, while others less. The concept of "physical reality" had become unnecessary in Bridgman's

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[231] *Ibid.*, p. 183.
[232] *Ibid.*, p. 216.

scheme. Though finding the concept convenient in explaining some aspects of physical theory, he did not recognize that using this concept was "a matter of intellectual necessity." He finally realized that he "could make shift to get along somehow" even when he could not rely on this concept at all.

Carnap, who received *The Nature of Thermodynamics* from Bridgman, made inquiries on a few points in discussion in the volume. One of them was about "physical reality." Not being able to understand what Bridgman meant by this concept, Carnap asked: "Would it not be better to avoid it entirely? Is it not entirely superfluous for the theory even for the practice of science?"[234] Bridgman's answer showed no trace of his enthusiasm over physical reality he used to have:

> In aplces [*sic*] in this argument I have alluded to "physical reality" because the average physicist of my acquaintance does think in this way, but before I have got through I make my physicist see that what he means by the "physical reality" of some process occurring at the boundary of a region to which he is applying the first law [of thermodynamics] is merely that simple physical instruments placed there give the appropriate readings. Something equivalent to this is, I think, always involved in the physicis [*sic*] use of "physical reality." My primary concern was merely that he sees this—after he has seen it I rather think he will drop the use of the term without being urged by me.[235]

In *The Logic of Modern Physics*, "physical reality" was a criterion for the truth of physical theory. But now it had become the mere readings of physical instruments, and physicists could do without the term "physical reality."

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233 *Ibid.*, pp. 219-220.
234 Carnap to Bridgman, June 7, 1942, PWBP, HUG 4234.10.
235 Bridgman to Carnap, July 7, 1942, PWBP, HUG 4234.10.

In the modified operational scheme, Bridgman did not show how to construct operational theory, but could only suggest a way of understanding the operational meaning of the present physical theory. He criticized some physical theories, especially quantum mechanics, as he observed that they relied on concepts whose meaning was not operationally understandable. Bridgman no longer invoke "physical reality" as a standard to distinguish the only "true" theory from other "possible" theories, but adopted "simplicity and convenience" as a new standard to choose an appropriate theory. Though he had not discussed the details of "mental operations" in the *Logic*, in view of the rise of physical theory that heavily depended upon mathematics, Bridgman could not but recognize their importance. Finally, realizing the need to prepare an appropriate "universe of operations" in order to construct a satisfactorily operational theory, Bridgman attempted to examine thermodynamics in this light.

This entire transformation took place immediately after the publication of the *Logic*, the volume usually understood as the platform of Bridgman's operational perspective. Viewing the difference in Bridgman's standpoints before and after the advent of quantum mechanics, or the publication of the *Logic*, one may feel like exploring how other physicists and philosophers interpreted Bridgman's standpoint and applied it to apparently puzzling problems that quantum mechanics presented.

5.3. The Operational Perspective of the Harvard Physicists

5.3.1. Quantum Mechanics and Operationalism

To those who know the change in Bridgman's operational perspective caused by the implications of quantum mechanics, it may be surprising to see younger physicists often quote Bridgman's words in the *Logic* to interpret, or even justify, novel concepts of this new-born theory. To examine the earliest manifestation of this view, let us come back to J. Robert Oppenheimer's review of the *Logic* published in 1928:

> The book was written before the development of the quantum mechanics; and it may be noted that this theory conforms very closely to Professor Bridgman's predictions. Thus the failure of the equations of classical mechanics is to be traced to the change in the operational definition of the dynamical variables; it is in this sense that the new mechanics is essentially a new kinematics. Experimental considerations alone, according to Heisenberg, show that it is impossible to assign, in a single experiment, numerical values to two sets of quantities which do not "commute." The passage from one experiment to another is thus a passage from a situation where one set has numerical values, to one in which another has numerical values; and this is the physical basis of the transformation theory.[236]

Interpreting Bridgman's standpoint as a requirement for experimental considerations, Oppenheimer observed that quantum mechanics was constructed in the way Bridgman had suggested. We have seen that Oppenheimer recognized Heisenberg's definition of the electronic orbit as "the operational definition" (§4.2). As will be shown, among the American physicists, this was a widely accepted interpretation of Bridgman's view and its relation to quantum mechanics.

The authors of some early American textbooks of quantum mechanics favored Bridgman's view. For example, explicating the meaning of the uncertainty principle in *Quantum Mechanics*, published in 1929,[237] E. U. Condon and P. M. Morse quoted paragraphs from the *Logic* and stated that "[i]n thinking about all physics, especially in

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[236] J. R. Oppenheimer, Review of *The Logic of Modern Physics*, *Physical Review*, 31 (1928), pp. 145-146.
[237] E. U. Condon and P. M. Morse, *Quantum Mechanics* (New York: McGraw-Hill,

connection with the problems of quantum theory, the operational point of view stressed by Bridgman in his 'The Logic of Modern Physics' is a most important aid."[238] They quoted Bridgman's discussion of the difficulty with the extension of ordinary concepts to the scale of atomic dimensions, praising that "no one has put [it] so clearly as Bridgman."[239] When Condon wrote to Bridgman for permission for this quotation before the textbook's publication, the author of the *Logic* felt only "flattered," showing no objection.[240]

E. C. Kemble also referred the reader to Bridgman's operational definition in discussing the possibility of defining the concept of momentum experimentally in quantum mechanics.[241] The same kind of concern with the "operational definition" of physical concepts continued to appear even later. For example, William Shockley at the Bell Laboratories, who had studied solid state physics under Slater, wrote in 1950: "From the experimental view point, [...] the existence of holes and electrons as positive and negative carriers of current can be inferred directly by the experimental techniques of transistor electronics so that holes and electrons have acquired an *operational* reality in Bridgman's sense of the word."[242] Kemble and Birch's recollection of the influence of Bridgman's works upon the reception of quantum mechanics explains why so many physicists preferred the operational approach:

Bridgman helped to free us from the compulsive need of the

1929).

[238] *Ibid.*, p. 17.

[239] *Ibid.*, p. 19.

[240] Condon to Bridgman, May 17, 1929; Bridgman to Condon, May 21, 1929, PWBP, HUG 4234.8.

[241] E. C. Kemble, *The Fundamental Principles of Quantum Mechanics* (New York and London: McGraw-Hill, 1937), p. 58.

[242] William Shockley, *Electrons and Holes in Semiconductors, with Applications to Transistor Electronics* (Princeton: Nostrand, 1950), p. ix.

> preceding generation of physicists to find mechanical explanations for all the phenomena of nature. He made no direct contribution to the development of quantum theory, but his point of view did much to alleviate the initial confusion over the paradoxical combination of wave-like and corpuscle-like properties of radiation and matter with which quantum theory is concerned. His thinking made it easier for us to accept the observed behavior of microscopic systems at its face value without trying to force it into a logical mold dictated by the language and categories of macroscopic experience and common sense. He saved our time and energy by pointing out the futility of efforts to find the answers to verbal questions that are in principle beyond the reach of experimental test.[243]

Kemble and Birch understood that Bridgman taught physicists not to stick to the verbal problems but to concentrate only on the experimental situation, and that this paved the way for the acceptance of quantum mechanics. However, as we have seen, Bridgman, though laying emphasis on the importance of the operational aspects, was eager to discuss apparently philosophical problems in science. Indeed, Bridgman wrote the *Logic* because he recognized the necessity of a new type of philosophy for understanding the rapid transformation of contemporary physics. Kemble interpreted Bridgman, or the *Logic*, separately from the whole scheme of Bridgman's view of science, like other younger physicists who were unwilling to mention his criticism of quantum mechanics.

Though younger physicists quoted Bridgman's phrases when they needed to clarify the meaning of some concepts, this does not necessarily mean that they shared Bridgman's goal in the *Logic*, namely, to show a guideline for reforming contemporary physics and building a new type of theory in atomic physics. Nor did they view physical theory

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[243] E. C. Kemble and Francis Birch, "Percy Williams Bridgman, April 21, 1882-August 20, 1961," *National Academy of Sciences of the United States of America, Biographical Memoirs,* 41 (1970), p. 43.

in Bridgman's way. They invoked the operational definition when they needed a rationale to interpret some concepts in terms of their implication in experiment, or when they stressed the importance of experimental facts in theoretical research.

Furthermore, younger physicists did not favor Bridgman's essays on quantum as much as the *Logic*. It appears strange that not so many physicists mentioned Bridgman's own understanding of quantum mechanics, while quoting from the *Logic* in their textbooks on quantum mechanics. American quantum-physicists were not unaware of Bridgman's essays on quantum mechanics, as they frequently mentioned Bridgman's *Harper's* essay, his most celebrated reflection on quantum mechanics, in order to show how much impact the uncertainty principle could give to physicists.[244] However, they did not appreciate Bridgman's reflections on quantum mechanics as much as they did the *Logic*, avoided detailed analysis of them, and refrained from criticizing them openly.

Some historians have argued that American quantum-physicists invoked Bridgman's operational view to justify the reception of quantum mechanics.[245] Yet, as has been described, Bridgman himself remained skeptical about the soundness of quantum mechanics as physical theory. No physicist dared to refer to Bridgman's essays on quantum mechanics when justifying the reception of this theory. Bridgman's view as a whole was not appealing to American quantum-physicists; only some impressive phrases and sentences in the *Logic* that appeared at least superficially harmonious with the spirit of quantum mechanics

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244 For example, Kemble, *The Fundamental Principles*, p. 76; *idem.*, "The General Principles of Quantum Mechanics. Part I," *Reviews of Modern Physics*, 1 (1929), p. 176.

245 See, for example, S. S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States 1920-1950," *Historical Studies in the Physical and Biological Sciences*, 17 (1986), pp. 55-98.

attracted their attention. What the historians have regarded as Bridgman's influence upon the reception of quantum mechanics is no more than the fact that younger physicists often quoted some phrases of the *Logic* without detailing the author's premises and intentions, though one can still maintain that the *Logic* presented words, concepts, and arguments that appeared appropriate to mention in rationalizing the reception of quantum mechanics.

5.3.2. Broader Influence of Bridgman

There was another aspect of Bridgman's influence worth our attention. Van Vleck, in the eulogy of his old friend Slater, described what Bridgman's words meant to them:

> I had been at Harvard a few months before Slater's arrival since I commenced my graduate work there in February, 1920, after graduating from the University of Wisconsin. Slater and I had in many respects a common educational experience at Harvard, as both of us had most of our courses in physics under Bridgman and Kemble. Neither Slater nor I have ever written any papers concerning the relation of philosophy and physics, but I have a feeling that both of us were subconsciously influenced by Bridgman, who was not a union card philosopher, but had a philosophy sometimes called Bridgmanism. The essence of this philosophy, which is basically pragmatic, is that research physicists should not be distracted to the realm of metaphysics or politics, and should concentrate on explaining observable experimental facts. He felt that theory is meaningful only in the context thereof. In practically all of Slater's papers, apart from the few that are themselves experimental, the emphasis is on making calculations or developing theories that explain observed phenomena.[246]

[246] J. H. Van Vleck, "1920-1930. The First Ten Years of John Slater's Scientific Career," John Clarke Slater Papers, Niels Bohr Library, Center for History of Physics, American Institute of Physics, p. 1.

Van Vleck, along with Slater, learned from Bridgman that physicists should discuss nothing outside physics and concentrate on explaining experimental results. They learned this not through what Bridgman wrote, but through his experimental work and teaching in the classroom.

Answering the question of T. S. Kuhn about Bridgman's influence, Van Vleck told thus: "I never had a course on the philosophy of physics with [Bridgman] but I don't think you can help but be influenced by the fact that he laid emphasis on agreement with experiment and that physics had meaning only in so far as it could be interpreted in terms of experiment."[247] Oppenheimer also learned Bridgman's philosophy from how he worked and taught: "[Bridgman] didn't articulate a philosophic point of view, but he lived it, both in the way he worked in the laboratory, [...] and in the way he taught."[248]

Those young physicists were surprised to see Bridgman, who told them not to discuss problems outside physics, detail the metaphysical implications of Heisenberg's uncertainty principle in his 1929 *Harper's* article. Kemble mentioned only the physical part of the essay: "As Bridgman has shown, the uncertainty relation is bound up with our inability to trace out the details of collisions between photons, electrons, and apertures."[249] In his recollection of the formative years of American physics, Van Vleck attempted to show the "tremendous philosophical import of the uncertainty principle" by quoting a paragraph of the *Harper's* essay in which Bridgman discussed the revival of "the substance of the soul," "the spirits of the dead," and "the

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[247] Interview with J. H. Van Vleck, conducted by T. S. Kuhn on October 2, 1963, AHQP.
[248] Interview with J. R. Oppenheimer, conducted by T. S. Kuhn on November 18, 1963, AHQP.
[249] Kemble, *op. cit.*, "The General Principles of Quantum Mechanics," p. 176.

medium of telepathic communication."[250] But he carefully refrained from committing himself to those problems: "I will not myself try to answer the question as to whether nature is basically indeterministic, or whether behind the curtain it is really deterministic and the trouble is simply that the initial conditions are inevitably spoiled by the experiment so that the causality is lost to mankind. This is a question of metaphysics, not physics."[251] In the interview conducted by Kuhn, Van Vleck thus evaluated Bridgman's paper: "I think Bridgman's article in *Harper's Magazine* really stirred me up most emotionally about [the uncertainty principle]. I always appreciated its significance, but that was, so to speak, a very dramatic presentation of it, which I always quoted to my classes, as you [Kuhn] doubtless remember."[252]

Though rejecting metaphysical extrapolation of physical theory and laid emphasis on experimental facts, Bridgman was enthusiastic over philosophical criticism of the foundations of physics and application of his operational method to social problems, which most of his students did not recognize as the physicist's business. Bridgman was therefore not happy with the way his students' interpreted his standpoint. In 1938, Bridgman wrote to Philipp Frank, a physicist and philosopher of science then leaving Europe for Cambridge, about his intellectual solitude at Harvard, expressing his wish to discuss philosophical problems of science:

> It will be a great pleasure to see you in Cambridge next fall and to talk things over with you. I am afraid you will not find Cambridge the center of activity with regard to the questions of interest to you which you apparently suppose. My work here is

[250] J. H. Van Vleck, "American Physics Comes of Age," *Physics Today*, 17:6 (1964), p. 23.
[251] *Ibid.*
[252] Interview with J. H. Van Vleck, conducted by T. S. Kuhn on October 4, 1963, AHQP.

> done practically alone. I have no students and have practically no contacts with members of the Department of Philosophy, and, in fact, most of them are not at all sympathetic with our point of view. The only young philosopher here whom I have particularly interested is Dr. Quine. Unfortunately he will be away next fall. There are, however, several members of the Department of Psychology who are interested, and I have no doubt that when you come you will be able to find enough men for an interesting discussion.[253]

At Harvard, Bridgman could find only a few sympathizers: W. V. O. Quine at the Department of Philosophy and S. S. Stevens and E. G. Boring at the Department of Psychology. Bridgman's younger colleagues at the Physics Department praised and occasionally mentioned his philosophical work, but interpreted it only as requiring physicists to concentrate on explaining experimental facts. No matter what Bridgman's own standpoint might be, they did so because this was the most agreeable interpretation for them.

5.3.3. The Lack of Philosophical Concern

I have so far analyzed some examples in which physicists invoked *The Logic of Modern Physics* in explicating the meaning of unfamiliar aspects of quantum mechanics. In fact, however, such arguments were rare among most of American physicists. Recollections of the Harvard physicists describe American physicists' attitude toward philosophical issues. Slater remembered that the situation concerning quantum mechanics was so "obvious" and "simple" that he could not understand "why people were arguing" about philosophical problems of quantum mechanics.[254] To him, a fuss about the philosophical implications of quantum mechanics was "almost entirely a European

[253] Bridgman to Frank, March 30, 1938, PWBP, HUG 4234.10.
[254] Interview with J. C. Slater, conducted by T. S. Kuhn on October 8, 1963, AHQP.

phenomena." His personal experience with Bohr and Kramers may have caused his philosophical indifference (see §2.3). His main concern was to apply it to actual physical phenomena, or to "get on with the problem."[255] Oppenheimer spent a few years in Europe, but only remembered that "bad" philosophy was spreading there.[256] He was not concerned with it, as he had more to do with actually using quantum mechanics. Van Vleck also admitted that he was not interested in philosophy.[257] Kemble, older than these physicists, was an only exception among the Harvard theoreticians and published some philosophical work. Yet, he did not become involved in the philosophical discussion during his stay in Europe in 1927-1928.

At a symposium on quantum mechanics, held in New York City on December 31, 1928 under the auspices of the American Physical Society, most of young American physicists who read their papers only described the structure and physical meaning of this theory, without discussing its philosophical difficulty. As for the problem of physical interpretation of the wave function, one of the most serious problems that annoyed European physicists, American physicists attending the symposium mainly supported the statistical interpretation, not even mentioning other possibilities such as the Copenhagen school's interpretation.

Slater's argument, which clearly stressed the statistical nature of quantum mechanics,[258] represented the philosophical indifference of American quantum physicists in general. He stated that quantum mechanics was "an extension" of statistical mechanics, and that "this

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[255] *Ibid.*
[256] Interview with J. R. Oppenheimer, conducted by T. S. Kuhn on November 18, 1963, AHQP.
[257] Interview with J. H. Van Vleck, conducted by T. S. Kuhn on October 4, 1963, AHQP.
[258] J. C. Slater, "Physical Meaning of Wave Mechanics," *Journal of the Franklin Institute*,

simple observation is enough to explain many of its otherwise puzzling features."[259] In his understanding, as quantum mechanics could operate only with statistical ensembles of observations, one should not expect it to give any description to one single observation. The physical reality that classic statistical mechanics has constructed contradicts the outcomes of quantum mechanics, and this was one of the problems over which European physicists fiercely debated. However, Slater neither paid attention to the difference in the assumptions of two theories nor examined the implications of the concept of probability as Bridgman did.

5.3.4. The Purpose of Physics

It is difficult to detect the reasons for American quantum-physicists' indifference to philosophical problems, since, because of their attitude toward science in general, they seldom discussed matters of this kind. However, they occasionally felt the need to justify the lack of philosophical concern, especially when they declared to accept apparently strange concepts of quantum mechanics, without sufficiently scrutinizing their legitimacy. For example, in the first comprehensive exposition of quantum mechanics published in the United States, Kemble explained his attitude toward the dualistic nature of matter and radiation:

> Of course, the existence of two apparently conflicting sets of characteristics for radiation [the wave-like nature and the corpuscle-like nature] has been a commonplace for many years and to many physicists the adoption of a dualistic point of view as the starting point for a fresh attack on the fundamental problems of physics will seem an evasion of the fundamental

207 1929), pp. 449-455.

[259] *Ibid.*, pp. 449-450.

> question, "*Why* does light act in some respects like an assemblage of corpuscles and in other respects like a spreading wave phenomenon?" We assert, however, that in the last analysis the function of theoretical physics is to describe rather than to explain. Science seeks to interpret the infinitely complex world of direct experience as the outcome of fundamentally simple laws. The reduction of complexity to simplicity is the goal, and when it is attained, we prove that order underlies chaos and leave the question, "Why" still essentially untouched. Hence, discarding this question as ultimately unanswerable, we may address ourselves to the task of describing what we observe in the most compact manner possible. If the behavior of radiation can be at least approximately described by means of the dualistic point of view, its temporary adoption will be a step in advance. No claim of ultimate validity is made for the theory, however.[260]

Kemble thought that the purpose of science was to find out simple laws that describe complex experimental results. To him, the goal of scientific research was neither explaining them nor discovering underlying reality. He therefore did not claim ultimate validity for physical theory and would be satisfied only with making "a step in advance." He regarded the dualistic nature of matter and radiation as something "to be accepted as a brute fact to be described rather than explained or exorcized."[261] In the 1930s, Bridgman also started to take a similar attitude toward the goal and role of physical theory; Kemble published the same attitude in 1929. Kemble maintained this view independently of Bridgman's influence.

In *The Theory of Electric and Magnetic Susceptibilities,* Van Vleck exposed a similar view by a different phrase, "the formal numerical calculation":

> It is not the purpose of the present volume to inquire further

[260] Kemble, "The General Principles of Quantum Mechanics. Part I," pp. 159-160.
[261] *Ibid.*, p. 158.

into the broad questions of interpretation in quantum mechanics [...] but instead to find the procedure for calculating energy levels such as are involved in the study of electric and magnetic susceptibilities. [...] The aspects of quantum mechanics which we present are perhaps rather formal, but in the last analysis, a theory is most "physical" when it permits the calculation of a large number of experimentally observable quantities in terms of a few fundamental postulates. The triumph of the quantum mechanics is probably due more than in any one thing to its success and utility in making possible the formal numerical calculation of energy levels and spectral intensities.[262]

To him, the function of physical theory consisted of its ability of calculating observable physical quantities and reproducing the experimental results. Unlike Bridgman, he did not care much about the type of concepts and discussions used in the theory, but accepted it insofar as it could correctly lead to the experimental results.

In December 1937, stimulated by the paper of Einstein, Podolsky, and Rosen [263] and Bohr's rebuttal, [264] a symposium on "The Philosophical Concepts in Modern Physics" was held by the Franklin Institute. This was one of a few occasions on which American physicists, such as Condon, Kemble, and Slater, discussed philosophical matters, though unsystematically and briefly. Condon quoted John Dewey and advocated his pragmatic view of science, stating what he regarded as the purpose of physics: "I take it to be the object of physics so to organize past experience and so to direct the acquisition of new experience that ultimately it will be possible to predict the outcome of any proposed experiment which is capable of being carried out—and to make the prediction in less time than it would

[262] J. H. Van Vleck, *The Theory of Electric and Magnetic Susceptibilities* (Oxford: The Clarendon Press, 1932), p. 130.
[263] A. Einstein, B. Podolsky, and N. Rosen, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" *Physical Review*, 47 (1935), pp. 777-780.
[264] N. Bohr, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" *Physical Review*, 48 (1935), pp. 696-702.

take actually to carry out the proposed experiment."[265] This reminds us that Bridgman had expressed a similar statement about the purpose of physics in other words, "simplicity and convenience." Slater took up the quantized theory of electrodynamics in his paper titled "Electrodynamics of Ponderable Bodies,"[266] beginning the discussion by the statement that he would advocate "the operational idea":

> A theoretical physicist in these days asks just one thing of his theories: if he uses them to calculate the outcome of an experiment, the theoretical prediction must agree, within limits, with the result of the experiment. He does not ordinarily argue about philosophical implications of his theory. Almost his only recent contribution to philosophy has been the operational idea, which is essentially only a different way of phrasing the statement I have just made, that the one and only thing to be done with a theory is to predict the outcome of an experiment. As a physicist, I find myself very well satisfied with this attitude. Questions about a theory which do not affect its ability to predict experimental results correctly seem to me quibbles about words, rather than anything more substantial, and I am quite content to leave such questions to those who derive some satisfaction from them.[267]

Slater, following his own version of operational view, claimed that he expected from physical theory nothing but its ability to predict experimental results.

Kemble more thoroughly described the new version of operational reasoning. In the paper titled "Operational Reasoning, Reality, and Quantum Mechanics,"[268] he stated that the purpose of physical science was "the securing of maximum control over our physical

[265] E. U. Condon, "Mathematical Models in Modern Physics," *Journal of the Franklin Institute*, 225 (1938), pp. 255-261, p. 257.
[266] J. C. Slater, "Electrodynamics of Ponderable Bodies," *Journal of the Franklin Institute*, 225 (1938), pp. 277-287.
[267] *Ibid.*, p. 277.
[268] E. C. Kemble, "Operational Reasoning, Reality, and Quantum Mechanics," *Journal*

environment,"[269] not obtaining knowledge of nature. For him, "the ideal toward which the physicist strives is a universal rule or formula into which information defining any physical situation can be inserted and from which it is possible to derive all the information regarding the future which is inherent in the physical situation."[270] According to him, controlling the future was the goal of physics.

Following Ernst Mach and Henry Margenau, Kemble formulated the fundamental procedure in theoretical physics: "to translate the given data for a physical situation into the language of our constructs, apply the laws of physics formulated in terms of construct symbols and then reinterpret our conclusions in terms of experience."[271] In his understanding, "the province of the physicist is not the study of an external world, but the study of a portion of the inner world of experience."[272] Furthermore, he believed that "there is no reason why the constructs introduced need correspond to objective realities,"[273] since "[a]n analysis of the relationship between the world of experience and the world of quantum-theory constructs indicates that no such correspondence does in fact exist."[274] Though not denying the existence of outside world or objective reality, he excluded these concepts from physicists' concern: "[T]he concern of physics is the correlation of experiences. Its domain is the domain of the experimentally verifiable. Therefore the job of the physicist is to describe the experimental facts in his domain as accurately and simply as possible, using any effective procedure without regard to such *a*

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of the Franklin Institute, 225 (1938), pp. 263-275.
269 *Ibid.*, p. 264.
270 *Ibid.*
271 *Ibid.*
272 *Ibid.*, p. 274.
273 *Ibid.*
274 *Ibid.*, pp. 274-275.

priori restrictions on his tools as common sense may seek to impose."[275] As far as physics accurately described experimental results and functioned well to predict the future, Kemble saw no need to examine what tools it adopted.

As for the philosophical problems of quantum mechanics, Kemble observed that "the puzzles of quantum theory have originated for the most part in the notion that its constructs are, or should be, direct reflections of the realities of the external world,"[276] or, in other words, "that most of the paradoxes are not really problems for physicist although they may be of considerable interest for the professional or amateur philosopher."[277] Kemble, however, admitted that the physicist might have "semi-philosophical concern [...] to clarify fully the procedure whereby he passes from his sense-perceptions to the world of his constructs and back again," suggesting that "he will of necessity adopt the method of operational reasoning recently advocated by Bridgman and closely related to the positivism of Mach and the pragmatism of the American philosopher and scientist, Charles S. Peirce." [278] Here Kemble interpreted Bridgman's work broadly, including both physical and mental operations in his discussion. According to Kemble, the spirit of the ideal attitude of physicists was as follows:

> The essence of this practical method is the demand that merely verbal questions be ruled out of all scientific discussion. Unless this is done thought and communication become farcical. To clarify our ideas it proposes to define all concepts used in discussion by relating them directly to experience, rather than by assuming that they are entities with certain preassigned

[275] *Ibid.*, p. 266.
[276] *Ibid.*, p. 274.
[277] *Ibid.*, p. 266.
[278] *Ibid.*

properties.[279]

Kemble then applied this method to the wave-corpuscle paradox, the physical interpretation of the wave functions, and the indeterministic character of quantum mechanics, explicating their meaning in the experimental situation.

Kemble, over ten years older than Slater, Van Vleck, and Oppenheimer, was exceptionally concerned with philosophical matters among American quantum theorists. At the advent of quantum mechanics, he failed to catch up with the new development of theoretical research. While staying in Europe in 1927-1928, Kemble concentrated on finishing up an older quantum calculation for band spectra, not plunging into learning quantum theory.[280] Though he attended von Neumann's first lectures of quantum mechanics, they were "mostly over [his] head."[281] During the following three decades, Kemble, taking interest in questions of clarity in the organization of knowledge, published papers and a book on the foundations of quantum mechanics. However, regretting his disregard of theoretical research based on quantum mechanics, he wrote in 1969, "I did pay a high price for my interest in philosophy."[282]

Starting to write on the philosophical side of quantum mechanics, Kemble found Bridgman's operational approach "heaven-sent," partly because views of other physicists, such as Millikan and A. H. Compton, seemed "very naïve"[283] to him. Kemble's view of science described in his 1938 paper superficially shared much with Bridgman's views before

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279 *Ibid.*, pp. 266-267.
280 Alexi Assmus, "Edwin C. Kemble, January 28, 1889-March 12, 1984," *National Academy of Sciences of the United States of America, Biographical Memoirs*, 76 (1999), pp. 179-197, pp. 189-190.
281 E. Kemble, interview with T. Kuhn, October 1, 1963, AHQP.
282 Assmus, "Edwin C. Kemble," pp. 190-191.
283 E. C. Kemble, interview with T. S. Kuhn.

and after quantum mechanics. Both of them demanded that all the discussion should be based on experimental facts, and as a result they understood that the validity of physical theory was of temporary nature. They rejected the commitment to the metaphysical speculation based on physical theory, and ruled out even the concept of "physical reality."

However, the detailed comparison reveals many differences between Bridgman and Kemble. Among others, the following points stand out clearly. Because of his own way of understanding nature, science, and operation, Bridgman had tremendous trouble in assimilating the implications of the uncertainty principle and quantum mechanics. He modified or softened his original stance through this struggle. Though many of his students could easily adapt themselves to the situation brought by quantum mechanics by relying on "operationalism," the originator of this view underwent a totally different experience. Bridgman carefully examined the conceptual and logical structure of physical theory and would not accept them if he found any defect. As his criticism of the notions of probability showed, Bridgman's operational analysis was based on his strict criterion for physically acceptable concepts, which was not shared by young American physicists who accepted theoretical physics without much doubt. They regarded physical theory as a "given," whereas Bridgman could not consider anything as a "given" until he examined it thoroughly.

The attitudes Bridgman and his students took toward quantum mechanics conflicted with each other. Bridgman accepted part of its discussion directly connected with experimental situation, including the principle of uncertainty and Heisenberg's thought experiment, but did not approve of some of its fundamental concepts, such as the notions of probability and the dualistic nature of classical and quantum-mechanical description. On the other hand, Kemble, a theoretical physicist, accepted the traditional tools of theoretical physics and distinguished theory from experience, requiring no strict correspondence between them as Bridgman persistently did. But Kemble at least shared some philosophical inclinations with Bridgman, as, in discussing the philosophical problems of quantum mechanics, he made his own effort to make clear what physics could discuss and what it could not, venturing to protect quantum mechanics from philosophical objection. Physicists belonging to later generations, such as Slater and Van Vleck, were only concerned how quantum mechanics worked, justifying quantum mechanics as far as it reproduced the result of experiments correctly without examining tools and the province of theoretical physicists.

Working closely with Bridgman, Kemble, Slater, and Van Vleck had many personal contacts with him. This institutional situation taken into consideration, one may expect them to share at least partially a view called "operationalism." Superficial resemblance of their views seems to confirm this expectation. However, a close examination of their views and discussions reveals many points at which their opinions differed. Some of them might even suggest that they maintained irreconcilable views of science and nature. They worked at the same department but probably did not interact much about fundamental matters of physics, as Bridgman sometimes intimated to his philosophically inclined friends. Even if they had discussed these matters, they would have only found that Bridgman's operational view, though often regarded as a wide-spread viewpoint among American physicists at the era of quantum physics, actually contained many elements that were not easily shared even by his colleagues at Harvard.

As in the case of dimensional analysis, some American physicists actively discussed philosophical matters when they felt interested. Bridgman's operational reasoning was a product of his philosophical reflection taking place under the stimulus of dimensional analysis and relativity theory. When quantum mechanics appeared with its philosophically challenging problems, Bridgman and other American physicists, for instance R. A. Millikan and A. H. Compton, who belonged roughly to the same generation, started to philosophize about them. However, younger theoretical physicists, Slater, Van Vleck, and Oppenheimer disregarded those problems. The latter concentrated on applying the new theoretical tool to problems that had not been solved yet. They saw no need to pay attention to philosophical matters, or they were too busy with their theoretical research to do so. The new research field that quantum mechanics opened was far larger than that opened by relativity theory, attracting theoretical research interest that had matured in the United States after World War I.

Young theoretical physicists found it neither interesting nor necessary to discuss philosophical matters. They took it for granted that they should interpret unfamiliar concepts of quantum mechanics in terms of what they meant experimentally. *The Logic of Modern Physics*, published right in time for the arrival of quantum mechanics in the United States, seemed to them to be full of concepts and phrases that justified their philosophical indifference and their attitudes toward the new theory. Bridgman's later criticism of quantum mechanics dismayed young quantum physicists, but they only refrained from referring to it.

Chapter 6. Operationism in Psychology

When Bridgman published *The Logic of Modern Physics*, he mainly expected responses from physicists and philosophers. Several physicists around Harvard and a few philosophers close to the Viennese Circle reacted to operationism, but not to the extent Bridgman had imagined. However, in psychology, in which he had little or no interest, operationism caused a series of fervent controversies over methodological issues. Despite Bridgman's indifference towards their science, operationists among psychologists and their critics for over one decade made various efforts to evaluate the validity of operationism as research program.

As has been discussed, Bridgman at first expected his operational program to guide a reform in physics. Only after his encounter with the uncertainty principle did he start to invoke operationism as a tool to comprehend the meanings of unfamiliar scientific concepts. On the other hand, since the publication of *The Logic of Modern Physics*, contemporary physicists around Bridgman regarded his operational view as an aid for interpreting novel physical theories. While physicists never attempted to design an operational program for their research, some psychologists in the 1930s became enthusiastic over the idea of reforming their discipline by adopting the operational definition of concepts. Some psychologists tried to build systematic methodology out of operational material, and others pointed to the defects of such attempts, though eventually both groups lost their enthusiasm over the debate. Though Bridgman had only casual interactions with them, psychologists' discussions over the possibility of operational approach revealed its possibilities and limitations.

Psychologists had started to notice the significance of operational analysis of scientific concepts since the early 1930s. In 1930, B. F. Skinner, then a graduate student at Harvard, completed a paper titled

"The Concept of the Reflex in the Description of Behavior"[1] and referred to Bridgman's method in its second paragraph. The paper was later offered as the first half of his doctoral thesis and was published in the *Journal of General Psychology* in 1931. Though Skinner derived his historical approach mainly from Ernst Mach's criticism of mechanics, the paper was "the first explicitly operational analysis of a psychological concept"[2] and probably the first psychological publication to contain a reference to *The Logic of Modern Physics*. Skinner had kept a keen interest in Bridgman's operational analysis since he had known the *Logic* through his friend Cuthbert Daniel, who had come to Cambridge to work with Bridgman.[3] Shortly after Skinner finished the paper, while contemplating a doctoral examination before a committee from which he could not expect sympathies, he suggested a young faculty member John Gilbert Beebe-Center that he could serve psychology better by performing an operational analysis of half a dozen key terms from subjective psychology than by preparing himself for the perfunctory examination.[4] Skinner regarded an operational analysis of subjective terms as a "mere exercise in scientific method," or "just a bit of hack work, badly needed by traditional psychology."[5] However, having not yet shared this view, Beebe-Center was so astonished by the proposal that Skinner went no further.

Within a few years from Skinner's doctoral examination, the Harvard psychologists had drastically changed their attitudes toward operational analysis, partly because Herbert Feigl, who had come to Harvard in 1930, had energetically introduced them to Bridgman's operational examination of concepts and the work of the Viennese circle.

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[1] B. F. Skinner, "The Concept of the Reflex in the Description of Behavior," *Jornal of General Psychology*, 5 (1931), pp. 427-457.
[2] "Symposium on Operationism," *Psychological Review*, 52 (1945), pp. 241-294, p. 291.
[3] B. F. Skinner, *The Shaping of a Behaviorist* (New York: New York University Press, 1984), p. 41.
[4] Skinner, *The Shaping of a Behaviorist*, pp. 74-75.
[5] "Symposium on Operationism," p. 291.

The Harvard psychologists started to discuss the significance of the operational approach and began to use the term "operationism" "in seminars and at the laboratory lunches."[6] A graduate student S. S. Stevens took the lead by publishing in 1935 his attempts to apply an operational procedure to the definition of psychological concepts.[7] Edwin G. Boring, who had once severely criticized Skinner's historical approach,[8] now supported Stevens's attempts and recommended operationism as "an adequate modern substitute for positivism at hand."[9] Meanwhile, psychologists outside Harvard also became enthusiastic over operationalism: John A. McGeoch at Wesleyan read papers on operational procedures in 1935 and 1937[10]; Edward Chace Tolman at Berkeley published a paper on operational behaviorism and practiced an operational analysis of demands in 1936.[11] Other psychologists who dealt with operational procedures in their papers during the 1930s included Douglas McGregor at Harvard,[12] Robert H. Seashore and Barney Katz at the University of Southern California,[13] and J. R. Kantor at Indiana University.[14] Furthermore, Feigl was not the only philosopher who took a special interest in the methodological matters in psychology: logical positivists such as Gustav Bergmann, Carl

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[6] Edwin G. Boring, *A History of Experimental Psychology*, 2nd ed. (New York: Appleton-Century-Crofts, 1950), p. 656.

[7] S. S. Stevens, "The Operational Basis of Psychology," *American Journal of Psychology*, 47 (1935), pp. 323-330; "The Operational Definition of Psychological Concepts," *Psychological Review*, 42 (1935), pp. 517-527.

[8] Skinner, *The Shaping of a Behaviorist*, pp. 72-74.

[9] Maila L. Walter, *Science and Cultural Crisis: An Intellectual Biography of Percy Williams Bridgman (1882-1961)* (Stanford, California: Stanford University Press, 1990), p. 178.

[10] J. A. McGeoch, "Learning as an Operationally Defined Concept," *Psychological Bulletin*, 32 (1935), p. 688; "A Critique of Operational Definition," *Psychological Bulletin*, 34 (1937), pp. 703-704.

[11] E. C. Tolman, "An Operational Analysis of "Demands,"" *Erkenntnis* 6 (1936), pp. 383-392.

[12] Douglas McGregor, "Scientific Measurement and Psychology," *Psychological Review*, 42 (1935), pp. 246-266.

[13] Robert H. Seashore and Barney Katz, "An Operational Definition and Classification of Mental Mechanisms," *Psychological Record*, 1 (1938), pp. 3-24.

[14] J. R. Kantor, "The Operational Principle in the Physical and Psychological Sciences," *Psychological Record*, 2 (1938), pp. 3-32.

Hempel, and Paul Oppenheim felt it necessary to clarify their own interpretations of the operational approach on this occasion.[15] Articles on operationalism in psychology continued to appear in the 1940s: in 1944, H. Israel and B. Goldstein at Smith College observed that even ordinary psychologists could hardly remain indifferent to "the insistent demand" that every term which they used be operationally defined, though "[t]he ordinary physicist, chemist, or biologist shows calm disinterest in operationism and operational definitions, if he has heard of these things at all, and his scientific journals contain almost no mention of the terms."[16]

Psychologists of the 1930s welcomed the operational approach because they expected it to provide a rigorous procedure for defining, validating, and testing fundamental concepts in psychology by appealing to the concrete operations that determined the concepts.[17] Yet, this sort of approach was not totally novel to them. Two decades earlier, a group of psychologists called behaviorists had contended a similar view to operationism: they initiated a movement against introspectionism and ventured to found an objective psychology on the basis of behavioral data alone. The founder of behaviorism, John B. Watson, even dispensed with the concept of "consciousness." Many psychologists did not share his parsimonious standard but accepted a more softened version of behaviorism that absorbed the concept of "consciousness" by "reducing it to the behavioral operations by which it is observed."[18] Psychologists had thus been familiar to the behaviorist-operationist tenet since the early 1910s; in the 1930s, however, they still regarded the introduction of operationism as an advance. Psychologists continued to try a new

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[15] Sigmund Koch, "The 'Operational Principle' in Psychology: A Case Study in Cognitive Pathology" (Paper presented at the symposium "Reflections on P. W. Bridgman: A Centenary Symposium," Boston University, April 24, 1982).
[16] H. Israel and B. Goldstein, "Operationism in Psychology," *Psychological Review*, 51 (1944), pp. 177-188.
[17] Robert H. Seashore and Barney Katz, "An Operational Definition and Classification of Mental Mechanisms," *Psychological Record*, 1 (1937), pp. 3-24, p. 3.
[18] Edwin G. Boring, *A History of Experimental Psychology*, p. 656.

method for making their young science appear more objective and rigorous, especially when it was addressed by an active experimental physicist and was supported by philosophers of science.

Douglas McGregor, for example, agreed with Bridgman that the origin of operational definition was in Einstein's demand that the meaning of concepts of length and simultaneity should be sought in the definite physical operations involved in their application. Observing that the technique that Bridgman had called "operational" was "an important premise in most recent works devoted to measurement," McGregor took "this neo-positivistic view" as "something new, an 'operationism.'" [19] After analyzing the operations for determining the length of a physical object, McGregor reasoned that physicists were aware that the basis of all scientific measurement was "the psychological judgment, the operation of discrimination," though he observed that physicists tended to "limit the kinds of discrimination involved to those for which universal agreement can most closely be approximated."[20] He went on to compare physical and psychological measurements and concluded that no valid distinction existed between psychological and physical magnitudes. To him, psychology and physics were "indistinguishable," so far as the nature of the magnitudes was concerned.

> There is no operational distinction, for example, between the measurement of density and the measurement of the visibility function. Length is as psychological as it is physical. Hardness is as mental as pain. Observation and its operational tool, discrimination, are the fundamental factors in all scientific measurement.[21]

McGregor asserted that one could even measure the "mind."

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[19] Douglas McGregor, "Scientific Measurement and Psychology," *Psychological Review* 42 (1935), pp. 246-266, p. 247.
[20] *Ibid.*, p. 248.
[21] *Ibid.*, p. 265.

> From this point of view we find that psychological measurement and physical measurement are one and the same thing. We can measure the 'mind' with as much logical validity as the physicist can measure uniform acceleration, because 'mental' magnitudes and 'physical' magnitudes are equally 'psychological' or equally 'physical.'[22]

Though McGregor admitted that psychology and physics had different goals, he claimed "the ranks of the natural sciences" for psychology that used "physical magnitudes and physical operations to explain a set of phenomena peculiarly its own." In this way, he declared, psychologists could "secure in his new self-knowledge, proceed with his measurements, unimpeded by the hampering difficulties of the Cartesian dichotomy between mind and body." [23] E. G. Boring endorsed McGregor's statement as an important contribution to systematic psychology.[24]

S. S. Stevens, McGregor's colleague at Harvard, also encouraged by Boring, independently published his articles on the operational approach in psychology. Believing that operational procedures would ensure psychologists "against hazy, ambiguous and contradictory notions" and provide "the rigor of definition which silences useless controversy,"[25] he introduced "discrimination" as the *sine qua non* of operations for defining concepts and stated that "the sole business of psychology" was "to test and measure the discriminatory capacities of the organism."[26] In his interpretation, one can operationally define a concept by employing the operation of denoting or pointing to the thing or event that the term signifies. Analyzing that the operation of denoting became available only when the thing or event could be discriminated or differentially sensed, Stevens concluded that discrimination or differential response was the

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22 *Ibid.*
23 *Ibid.*, p. 266.
24 *Ibid.*, p. 246.
25 S. S. Stevens, "The Operational Basis of Psychology," *American Journal of Psychology,* 47 (1935), pp. 323-330, p. 323.
26 *Ibid.*, p. 325.

fundamental operation.[27] He went on to state that one could refine operational definitions through what he called "discriminatory analysis."

> In the early stages a concept is apt to include many things not yet differentiated. When closer scrutiny reveals that two quite different things can be discriminated within the original concept, we are obliged to say the concept means this one and not that one, or else that the concept is generic and includes them both.[28]

In other words, Stevens allowed discrimination to "usurp the position formerly enjoyed by 'experience' or the 'immediately given.'" [29] Furthermore, to him, discrimination meant "the concrete, 'physical' reactions of the organism to either internal or external environmental conditions." Stevens reached a conclusion similar to McGregor's: "The elementary discriminatory reaction on the part of human beings [...] is the fundamental operation of all science."[30]

Psychologists were serious enough about operationalism to activate and refine it for their practical purposes. In 1935, John A. McGeoch published his attempt to give an operational definition to the concept of learning,[31] and two years later an examination of the applicability of operational definition in general.[32] In the latter, McGeoch pointed out several advantages of operational definition: it offered a means of reaching scientific agreement; it made it easier for psychologists to cross the boundaries between the different fields of their science; it is a thoroughgoing relativism and empiricism. Moreover, he regarded operational definition as "the procedure actually used in building the

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27 S. S. Stevens, "The Operational Definition of Psychological Concepts," *Psychological Review*, 42 (1935), pp. 517-527, p. 518.
28 Stevens, "The Operational Basis of Psychology," p. 326.
29 S. S. Stevens, "Psychology: The Propaedeutic Science," *Philosophy of Science*, 3 (1936), pp. 90-103, p. 95.
30 Stevens, "Psychology: The Propaedeutic Science," p. 95.
31 John A. McGeoch, "Learning as an Operationally Defined Concept," *Psychological Bulletin*, 32 (1935), p. 688.
32 John A. McGeoch, "A Critique of Operational Definition," *Psychological Bulletin*, 34 (1937), pp. 703-704.

concepts employed in laboratory work" and "no more than an explicit formulation of the procedure which careful experimenters actually employ." On the other hand, McGeoch recognized certain limitations of operational definition: Psychologists made discriminations in terms of extra-operational criteria; operationism could probably not deal with subjective quality; one could not by operations define the operations themselves. Despite these limitations, however, McGeoch praised operational definition as "the best available procedure for the construction of the concepts of psychology."

E. C. Tolman presented a more practical application of operational psychology.[33] In his program, a demand was postulated to be a "constructed" variable that could be operationally defined. Tolman subdivided all demands into the three sub-classes: primary appetite demands, primary aversion demands, and secondary demands. He then showed how to define these types by standardized experiments. For example, in the case of primary appetite demands, Tolman suggested a simple standardized experiment using the apparatus called Obstruction Box, where one could find how B, the number of food crossings, would vary with variations in the time since feeding M and with the types of food S. Assuming that under certain simple conditions B would directly "mirror" the strengths of the appetite demand I, Tolman asserted that psychologists could establish the defining function for I under these conditions by finding out the shape of the function f in the relation $B = f(S, M)$. Meanwhile, R. H. Seashore and Barney Katz invoked operational definitions as "objective criteria" in their classification of mental mechanisms, which were also called behavior or adjustment mechanisms.[34] Though they did not detail the methodological aspect of their "operational definitions," Seashore and Katz elaborated a

[33] Edward Chace Tolman, "An Operational Analysis of "Demands,"" *Erkenntnis* 6 (1936), pp. 383-392.
[34] Robert H. Seashore and Barney Katz, "An Operational Definition and Classification of Mental Mechanisms," *Psychological Record*, 1 (1937), pp. 3-24.

systematic classification of the mental mechanisms according to the mode of response in a problem solving situation.

Psychology in the late 1930s saw as many operational programs as the number of operational psychologists, who soon started to criticize one another. The originator of operationism, Bridgman, could not escape their criticism, either. J. R. Kantor at Indiana University agreed with R. B. Lindsay that Bridgman's insistence upon laboratory operations put serious restrictions upon theoretical physics. Kantor interpreted science as a "form of interbehavior" in which scientists including physicists and psychologists determined the nature of objects and events not only by first-hand operations such as physical measurements but also by secondary operations derived from the former.[35] On the other hand, believing that "an enterprise concerned with pure artifacts is not scientific," Kantor blamed Bridgman for denying that "things have properties fixed and inherent, independent of anything that we may do about it."[36] In Kantor's opinion, Bridgman was "departing from the operational point of view"[37] in his assertion in *The Logic of Modern Physics* that "from the operational point of view it is meaningless to attempt to separate 'Nature' from 'knowledge of nature'."[38] A similar tendency in psychology represented by Stevens's use of the operational principle that admitted no crude data and reduced all experience to "the sum total of the discriminating reactions of human beings" appeared to Kantor to amount to "a thoroughgoing subjectivism."[39]

By adopting the operational principle modified to include investigative procedures and postulates, Kantor aimed to put psychology

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[35] J. R. Kantor, "The Operational Principle in the Physical and Psychological Sciences," *Psychological Record*, 2 (1938), pp. 3-32, p. 5.
[36] Kantor, "The Operational Principle in the Physical and Psychological Sciences," p. 6. Bridgman's statement is in *The Nature of Physical Theory* (Princeton: Princeton University Press, 1936), p. 43.
[37] Kantor, "The Operational Principle in the Physical and Psychological Sciences," p. 6.
[38] P. W. Bridgman, *The Logic of Modern Physics* (New York: The Macmillan Company, 1927), p. 62.
[39] Kantor, "The Operational Principle in the Physical and Psychological Sciences," p. 15.

and physics to operate "in a single natural continuum, with methods and techniques only so far different as the variant phenomena demand."[40] He rejected "the ingrained dualism" that had induced both physicists and psychologists to regard "phenomena as primary and real on the one side and secondary and unreal on the other."[41] Under the influence of this dualism, physicists turned the psychic over to the psychologists and proceeded to study the interrelationships of objective events, while psychologists were divided into two groups in their reactions to the operational method, one using objective procedures in order to maintain their faith in mentalistic processes as Stevens's "surface concession to objectivity"[42] did, and the other raising objections against the operational principle on the ground of, for example, subjective qualities. Criticizing both groups of psychologists, Kantor tried to remind his colleagues that the accumulation of psychological facts had been accomplished "by the employment of operational techniques involving organisms on the one side and actual objects, conditions, and situations on the other." Kantor observed that the basis of psychology had been the observation of the interbehavior of the organism to colored, sounding, or solid objects, which for the psychological purpose could never be reduced to "specialized partial mechanisms," namely, "the response of tissues to light rays or air waves," the "constructions developed by the physicist for other situations," or "the abstractions of the physicist."[43] He also reviewed the recent discoveries in physiology and neurology and concluded that psychology could not describe its phenomena in cerebral or other neural terms.[44] Kantor's reflection was so illuminating that even Stevens could not but admit that despite its possible defect it was

40 *Ibid.*, p. 3.
41 *Ibid.*, p. 13.
42 *Ibid.*, p. 15.
43 *Ibid.*, p. 17.
44 *Ibid.*, p. 24.

"rich in valuable applications of the operational attitude."[45]

The most problematic for operational psychologists were the issues of subjective quality, introspection, and verbal reports. Boring argued that such a psychological entity as a perceived duration or temporal pattern was "inferred from and defined by certain operations of introspective report"[46]: "This 'behavioral' inversion of the point of view toward experience makes the perception, not a private immediate experience, but a psychological *construct* which (in a rat or a person or myself) is just as public as is any other convincing inference from data." He expected that by following the operational principle psychologists could construct public concepts out of introspective reports. Tolman, on the other hand, asserted that experiential consciousness, as such, was no more necessary for psychology than for physics. Though he admitted that introspection might remain in psychology since verbal reports sometimes constituted "the best and simplest behavior for mirroring and defining these intervening variables," [47] he personally remained suspicious of verbal reports and preferred "to try to work out psychology with the aid of only the more gross forms of behavior." Tolman's motto was: "Rats, not men. Gross behavior, not verbal reports."[48]

Stevens demanded that all facts admitted to the body of scientific knowledge, including psychological one, be public and independent of the observer. For him, science, as well as language, was "a thing agreed upon by members of society."[49] Psychologists should therefore report their experiences in the way that others could verify them: "Operationism requires that we deal only with the reportable aspects of experience. Not only must the experience be reportable; it must be actually reported,

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[45] S. S. Stevens, "Psychology and the Science of Science," *Psychological Bulletin*, 36 (1939), pp. 221-263, p. 259.
[46] E. G. Boring, "Temporal Perception and Operationism," *American Journal of Psychology*, 48 (1936), pp. 519-522, p. 521.
[47] E. C. Tolman, "An Operational Analysis of 'Demands'," *Erkenntnis*, 6 (1936), pp. 383-392, p. 389.
[48] *Ibid.*, p. 390.
[49] Stevens, "The Operational Basis of Psychology," p. 327.

verbally or otherwise. Operational psychology knows precisely nothing of unreported consciousness."[50] Asserting that psychologists could deal with "the inner life"[51] when it appeared in the operation of report, Stevens accepted introspection as a useful tool so far as its results could actually be reported. He maintained that psychologists, when reporting the observations of their own consciousness, should play the role both of experimenter and subject-mater and treat the data as though they were obtained from "the other one," so that another psychologist could take over the task of experimenter at any moment without altering the results.[52] Still, Stevens felt urged to make some careful remarks on "language-responses":

> If the experimenter assumes that the words used by the subject mean what they would mean if the experimenter used them, he is apt to be in error, for the meaning of a word depends upon the past history of the person acquiring the meaning, and no two personal histories are identical. However, the experimenter can be more certain of the meaning, for the 'subject,' of some words than of others. An operational test in which words are applied to concrete objects or situations would show the meaning of such words as *yes* and *no* to be more certain and predictable than the meaning of words like *brown* and *beautiful.* Only such words as the former can be accepted as data if maximal rigor is desired.[53]

Though Stevens did not preclude the use of words as data, he judged the concepts resulting from language-responses to be "no more definite and precise than what is known of the meaning of the word in the mind of the subject." To obtain complete rigor, he continued, the experimenter should know the meaning of the word or the complete history of the subject concerning the particular words in question.[54]

As has been mentioned, McGeoch doubted whether operationism

[50] *Ibid.*
[51] Stevens, "The Operational Definition of Psychological Concepts," p. 522.
[52] Stevens, "The Operational Basis of Psychology," p. 328.
[53] *Ibid.*, p. 329.
[54] *Ibid.*, p. 330.

could deal with subjective quality. He personally wrote Kantor that behind Stevens's "discrimination" lied "the criterion of quality in terms of which the discriminations were made."[55] Agreeing that sensation qualities "in the final analysis" were "unreal constructions" that no observational technique could handle, Kantor advised psychologists to concentrate on the interbehavior of organism to objects instead of harboring sensation qualities. Furthermore, he analyzed the operational meaning of the term subjective in psychology:

> Actually the only operational basis for such a term as subjective is that the differentiation of color, say, is an activity of a particular organism which may vary from that of some other organism. Such relativity of discrimination based upon the diverse reactional biographies of individuals, varying illumination or other setting, hygienic conditions, and differences in interbehavioral position between individuals and objects can all be objectively described, just as the physicist describes the relativity of time, motion, mass, and energy.[56]

Kantor asserted that psychology would find a framework for describing objectively the diversity of individual reactions in the same way as physics had found out theory that could describe the relativity of physical quantities.

Unlike Bridgman, psychologists who adopted the operational principle in order to introduce the standard for objectivity to psychology did not hesitate to detail the nature of operations for defining scientific concepts. Stevens, who believed that "science deals only with those aspects of nature which all normal men can observe alike,"[57] required all operations involved in defining scientific concepts to be "public and repeatable"[58] and rejected any concept that did not correspond to such operations: "Nothing gets into scientific psychology which does not

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[55] Kantor, "The Operational Principle in the Physical and Psychological Sciences," p. 16.
[56] *Ibid.*, p. 17.
[57] Stevens, "The Operational Basis of Psychology," p. 327.
[58] *Ibid.*, p. 328.

appear in the form of overt changes in all or a part of the physical system which we call the subject, for only such changes can be dealt with by means of rigorous, univocal, repeatable operations."[59] Stevens admitted no private or inner experience to have anything to do with scientific psychology "for the simple reason that an operation for penetrating privacy is self-contradictory." [60] Assuming that operations were intrinsically of public character, Stevens adopted the operational definition for clarifying the meanings of psychological concepts.

Stevens's interest in the philosophy of science led him to discuss the grounds of acceptability of operations. He pointed out that "no formal yardstick"[61] that could function as such grounds of acceptability was available. Certain results get into the scientific textbooks and certain others are rejected, "not because they satisfy or fall short of some absolute standard of merit, but simply because they meet with the approval or disapproval of other scientists."[62] Experimenters depend upon the approval of their associates, and "knowledge itself and all that passes for scientific truth is conditioned upon its social acceptance."[63] Stevens was courageous enough to explore the logical consequence of the social character of truth:

> Insistence upon the social criterion of truth inevitably emphasizes the relativity of it. Truth is no more absolute than space or time. In fact science can speak of truth only as of a certain place and date, for what is true to-day was not true yesterday and may not be true to-morrow. Furthermore, truth to-day is not something definite and precise, of which one can take a convenient inventory, for agreement as to fact is never complete and opinion is in a perpetual state of flux. Knowledge, or truth, then, is not static, absolute or 'discoverable' in Newton's sense; it is dynamic, restless and relative.[64]

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[59] *Ibid.*, p. 329.
[60] Stevens, "Psychology: The Propaedeutic Science," p. 95.
[61] *Ibid.*, p. 96.
[62] *Ibid.*
[63] *Ibid.*, p. 97.
[64] *Ibid.*

Though eager to make psychology a branch of science, Stevens did not expect that science in his age could achieve static or absolute truth. Psychologists were sensitive enough to recognize the changing status of truth in science after the advent of relativity theory and quantum mechanics.

One may wonder how Stevens understood the relation between repeatable operations and relative truth. He did not explicitly discuss this matter, but probably assumed that operations, as well as scientific truth, were also public and therefore relative. In his understanding, an experimentalist could adopt an operation as a tool for scientific research only when other scientists approved its validity: "If he reports seeing things which no one else is able to see, or if he hears voice[s] which never speak to his colleagues, he is looked at askance."[65] To Stevens, operations were valid and repeatable only if his colleagues approved so. Though one may still ask Stevens how he can safely define scientific concepts by relative and unstable operations, this sort of question does not seem to have passed his mind.

Contrary to Stevens, Bridgman admitted that in some cases the conclusion that "everyone in the world except yourself had gone crazy" was inevitable.[66]

> Granted that every individual finds it desirable for his own purpose to concern himself only with what he observes other competent individuals agrees on, nevertheless in the last resort every individual finds it must be his own judge of what he shall accept to be satisfactory evidence of competence in another.[67]

Bridgman had reached this solipsist position in the years after publishing *The Logic of Modern Physics* and encountering the uncertainty principle.

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65 *Ibid.*, p. 96.
66 Bridgman, *The Nature of Physical Theory*, p. 14.
67 *Ibid.*

In the Venuxem lectures given at Princeton in 1935, Bridgman for the first time declared that "science is only my private science." While in *The Logic of Modern Physics* Bridgman expected operations to determine the meanings of scientific concepts in the way that everyone accepted, in the Venuxem lectures Bridgman maintained that operations, as well as science, were not public but private. He was aware that his point of view was directly opposed to a widely supported idea that science could refer only to "the body of knowledge universally held by competent persons."

Bridgman started to refer to his preference of private science in the middle of the 1930s, when his operational method gradually evolved into a tool for interpreting scientific concepts. Though the former had origins independent of the latter, they fit well to each other in Bridgman's view of science after his encounter with the uncertainty principle. While operations for defining concepts should be public, those for interpreting concepts can be private. With or without the aid of operations, scientists can apparently interpret any concept in their own individual contexts, though the question of how they can guarantee the validity of their interpretations still remains to be answered.

Seeing Bridgman disagree that science was a set of empirical propositions agreed upon by members of society, Stevens asked Bridgman to produce the negative case: "A physical law to which only Bridgman agreed would not be a part of physics—not, at least, until he won converts, and then there would be agreement."[68] Bridgman would have insisted that physics that everyone else accepted would not become his physics until he accepted it. But Stevens, who admitted only those scientific propositions based upon public and repeatable operations, did not expect that even psychology had anything to do with private experience. Furthermore, to him, an operation for penetrating privacy remained to be self-contradictory.[69]

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68 Stevens, "Psychology and the Science of Science," p. 227.

69 *Ibid.*, p. 228. See also, Stevens, "Psychology: The Propaedeutic Science," p. 95.

Having formulated their own version of operational program, psychologists started to rebut the criticisms of operationalism that Bridgman had not ventured to face. One of the harshest criticisms was that operationism reduced to a vicious particularism: critics stated that a strict servility to the operational principle nourished an ever-expanding multiplicity of concepts, instead of unification in science.

In *The Logic of Modern Physics*, Bridgman showed that a length, for instance, measured by a meter stick was a different concept from an astronomical length and even suggested calling them by different names since the measuring operations were different. Pointing out that the practical justification for retaining the same name was only that within the present experimental limits scientists had not detected a numerical difference between the results of these operations, Bridgman warned that an increase in experimental accuracy might show that the two different sets of operations that gave the same results in some domain of experience would lead to measurably different results in other domains.[70] This discussion of the concept of length led some physicists to label operationalism as particularism. In 1931, Henry Margenau pointed out that Bridgman's suggestion would, when carried out to its consequences, dissolve the world into "an unmanageable variety of discrete concepts without logical coherence."[71] He also feared that operationalism might impede the progress of science by emphasizing too strongly its most rapidly changing elements, namely, experimental methods. Furthermore, he asserted that physical concepts did not require "a mode of definition which is not applicable to others."[72] In 1937, R. B. Lindsay, who assumed that the aim of physical science was "to provide a simple and economical description in terms of a minimum number of concepts of the portion of human experience which we agree to call physical,"

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[70] P. W. Bridgman, *The Logic of Modern Physics* (New York: The Macmillan Company, 1927), pp. 9-25.
[71] Henry Margenau, "Causality and Modern Physics," *Monist*, 41 (1931), pp. 1-36, p. 16.
[72] *Ibid.*, p. 17.

expressed a similar doubt in his criticism of operationalism: "[L]ogically the operational method rigorously carried out implies that each concept is tied to a definite operation. [...] It has been and should be the physicist's steady purpose to keep concepts from a too close association with a particular operation."[73] To Bridgman the difference in operations mattered most; To Lindsay and Margenau, on the other hand, the unification of different operations by theoretical feats was essential.

In the late 1930s, psychologists outside the operationalists' circle began to address criticisms of the operational approach similar to those of Lindsay and Margenau. Admitting that "one of the ideals of scientific concept-makers is to reduce all concepts to a few fundamental ones," Arthur G. Bills at the University of Cincinnati complained that this ideal was not the likely outcome of operationally defined concepts.[74] Based on his observation that different sets of operations had arisen out of a different experimental need or situation, he concluded that "there is no universal set of operations" that would promise a unified system of psychological concepts. R. H. Waters and L. A. Pennington at the University of Arkansas followed Bills in their criticism that Bridgman's operational principle, when strictly applied, would lead only to the multiplication, never to the reduction, of the number of concepts: "The slightest change in any aspect of a set of operations would mean [...] a new concept and would demand, likewise, a new symbol for its designation."[75] They illustrated this situation by a simple example:

> Suppose we measure a table and get a certain result. We measure it again and get a slightly different value. Now do we

[73] R. B. Lindsay, "A Critique of Operationalism in Physics," *Philosophy of Science*, 4 (1937), pp. 456-470, p. 458. See also, Henry Margenau, "Methodology of Modern Physics," *Philosophy of Science*, 2 (1935), pp. 48-72, 164-187; and R. B. Lindsay and Henry Margenau, *Foundations of Physics* (New York: Wiley, 1936), p. 412.
[74] Arthur G. Bills, "Changing Views of Psychology as Science," *Psychological Review*, 45 (1938), pp. 377-394, p. 390.
[75] R. H. Waters and L. A. Pennington, "Operationism in Psychology," *Psychological Review*, 45 (1938), pp. 414-423, p. 418.

> have one length or two? Do we have one or two tables? Can a table have different lengths at the same time (as would happen if two people measured the table at the same time)? [...] If length is to be 'synonymous with the corresponding set of operations' then we must have two lengths for the same table, or two tables.[76]

Waters and Pennington also agreed with L. J. Russell's criticism of *The Logic of Modern Physics* that the concept of experimental error was meaningless to operationism.[77] Though operationism teaches that one operation is as valid and true as the other, scientists usually take it for granted that a certain amount of error always accompanies measurements. They therefore strike an average and determine the amount of experimental error. This means that scientists accept no single set of operations as valid, "that the truth of the matter is not accurately revealed by any such set," or "that each set approximates the truth."[78]

Waters and Pennington recognized that scientific endeavor consisted not alone in increasing detailed facts, but more importantly in systematizing and organizing these facts to make "a unified whole." To them, this scientific unification meant the development of theoretical tools such as principles and concepts on which basis the particular and concrete facts were envisaged as special case. However, operationism, which would never take scientists beyond "isolated and discrete facts," could not get from particular to general concepts. In short, Waters and Pennington judged that operationism failed to "represent the key to the solution of scientific problems."[79]

While Bridgman never bothered to mention the alleged particularism of his standpoint, operational psychologists attempted their own rebuttals. The interbehavioral operationist Kantor found

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[76] *Ibid.*, pp. 418-419.
[77] L. J. Russell, "The Logic of Modern Physics, Critical Notice," *Mind*, 37 (1928), pp. 355-361.
[78] Waters and Pennington, "Operationism in Psychology," p. 420.
[79] *Ibid.*, p. 419.

particularity place no limitation upon any operation "interbehaviorally conceived": "We all know that every event in the physical domain is also particular and that no physical law is ever exactly demonstrated in every physical event of which it is a construction." The structure of Kantor's interbehavioral operationism shared much with that of physical science. His interbehavioral operations were, like physical laws and physical concepts, some abstract derived from concrete diverse events. To those who found physical science successful the program of interbehavioral operationism might seem to be promising. However, for the same reason that no one could easily clarify how physical concepts emerged out of discrete events, Kantor, like Bridgman, could not but remain silent over the question of how they had reached their idealized operations.

Stevens made his own effort to make explicit the rules and procedure for generalizing from operations. To him, this did not seem to be difficult, "because science does generalize, and operationism seeks only to discover how scientists do what they do." Stevens's rule for scientific generalization was simple: "All objects or events satisfying certain criteria we call members of a class and to that class we assign a name or symbol."[80] While Kantor avoided detailing the process of generalization, Stevens at least tried to outline it. However, Stevens did not feel it necessary to discuss further: he neither wondered how these criteria emerged nor examined how they actually functioned in scientific theorizing.

Operational psychologists' programs, though not always consistent or convincing, achieved maturity and variety independent of Bridgman's original scope. Therefore, even Bridgman's occasional but explicit oppositions to some of their statements did not cause much commotion. At the Fifth International Congress for the Unity of Science, held in Cambridge, Mass. on September 4, 1939, Bridgman addressed to the Harvard psychologists and the refugee-philosophers belonging to the

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80 Stevens, "Psychology and the Science of Science," p. 233.

Viennese Circle that he believed that science was essentially private.[81] On Halloween 1940, Bridgman again defended the same standpoint at the inaugural lecture for the monthly meetings held by Stevens and Rudolf Carnap. These apparent objections of Bridgman did not startle many psychologists. Stevens, for example, dismissed Bridgman's as "his solipsistic brand of operationism."[82]

As operationists' tenet became more and more influential among psychologists, its criticisms also grew keener. The fact that two of the "most articulate apostles of operationism" Stevens and Kantor reached the diametrically opposed conclusions led A. G. Bills to suspect that "all is not well" with operationism.[83] Observing both Kantor's strict behavioral objectivism and Stevens's justification of "ultra-subjective categories" such as experience, Bills wondered what was actually a legitimate defining operation. Moreover, Bills pointed out that one could not tell by the name of a concept whether it is operational or not since psychologists did not name their concepts in terms of their operational validations.

Finding Bills disagree with Kantor's accusation that Stevens's position implied dualism, Stevens judged Bills' article encouraging and pointed to the possible defect of Kantor's brand of operationism that it tried to secure complete objectivity "by invoking what would appear to be essentially a realism."[84]

However, not only Kantor but also Waters and Pennington detected a dualism in Stevens's viewpoint. Though Stevens defined an operation

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[81] P. W. Bridgman, "Science: Public or Private?" *Philosophy of Science*, 7 (1940), pp. 36-48.
[82] S. S. Stevens, "S. S. Stevens," in Carl Murchisum, E. G. Boring, G. Lindzey, et al., eds., *A History of Psychology in Autobiography*, 6 vols. (Englewood Cliffs, New Jersey: Prentice-Hall, 1930-1974), vol. 6, pp. 395-420, p. 409. Vols. 1-3 were edited by Carl Murchisum, vol. 4 by E. G. Boring et al., vol. 5 by E. G. Boring and G. Lindzey, and vol. 6 by G. Lindzey. Vol. 1 was published in 1930, vol. 2 in 1932, vol. 3 in 1936, vol. 4 in 1952, vol. 5 in 1966, and vol. 6 in 1974.
[83] Bills, "Changing Views of Psychology as Science," p. 387.
[84] Stevens, "Psychology and the Science of Science," p. 259.

as "the performance which we execute to make known a concept,"[85] this statement did not satisfy Waters and Pennington: "It sounds very much as though we had a knowledge of the concept by some means other than operations and that now we were simply attempting such and such a performance as will reveal our meaning to another." [86] Without knowledge of a concept given in advance by some means other than operations, one could not tell whether a certain set of operations were sufficient for defining the concept. "[T]he meaning of any concept," they contended, "is not exhausted by the concrete physical operations utilized in its demonstration."

> Stevens asserts that the concept of animal spirits was rejected or could have been rejected earlier had the operational concept of definition been utilized. We must look upon his argument with some amusement since at almost every point it goes beyond the assumptions in operationism. The real cause of the rejection of the notion of animal spirits, we argue, was not the operations by which they were studied but because of the fact that some other criterion—some other notion of the truth—was more consonant with the scientific temper of the times.[87]

The operational definition would have caused no revolution in psychology. Waters and Pennington concluded their criticisms by a remark that operationism was no more than a reemphasis of the significance of the experimental method in psychology.

For several years since the outbreak of the war, critics of operationism in psychology had remained silent until H. Israel and B. Goldstein at Smith College published a detailed analysis of operationism in psychology[88] in 1944. Israel and Goldstein accepted Bridgman's proposal as a sound requirement for specifying the set of operations

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[85] Stevens, "The Operational Basis of Psychology," p. 323.
[86] Waters and Pennington, "Operationism in Psychology," p. 420.
[87] *Ibid.*, p. 421.
[88] H. Israel and B. Goldstein, "Operationism in Psychology," *Psychological Review*, 51 (1944), pp. 177-188.

corresponding to each concept. Furthermore, they admitted that ten years after the publication of the *Logic* Bridgman had tried to give a proper position to his original statement by regarding it as "a technique of analysis" or merely a "simple thing," not "a complete method" or "the new scientific method"[89] as operationists among psychologists called it. Israel and Goldstein accused the latter of applying the operational technique in what seemed to them to be an inappropriate manner. They judged that while Bridgman mostly referred to operations of "measuring the quantity or testing the identity of the phenomena"[90] for defining physical concepts, such psychologists as Stevens, B. F. Skinner, and E. C. Tolman relied upon those of "producing," "eliciting," or "varying" the phenomena.[91]

Israel and Goldstein favored Bridgman's operations since they were "subsidiary operations applied from outside to the principal terms of a primary function for the special purpose of defining them."[92] On the other hand, the psychologists' adoption of a different type of operations seemed to them to lead to the substitution of "purely functional meaning" for that "defined by Bridgman's testing and measuring operations."[93] In such strict operationism, one could only attempt to achieve the statement of functional relations between events that were identifiable by their functional connections alone.[94] Thus, the psychologists' operational program, if carried out strictly and consistently, should require "a revision of basic assumptions and drastic changes in actual methodological practice."[95] They would, for example, be urged to answer the following question: "[H]ow an operationist might ever be able to avoid discovering innumerable phenomena, one for every separate operation

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[89] *Ibid.*, pp. 178-179.
[90] *Ibid.*, p. 180.
[91] *Ibid.*, p. 181.
[92] *Ibid.*, p. 186.
[93] *Ibid.*, p. 185.
[94] *Ibid.*, pp. 185-186.
[95] *Ibid.*, p. 187.

which he might perform," or "[h]ow an operationist can make of his scientific knowledge anything beyond a huge collection of separate and completely unrelated items." However, Israel and Goldstein could find no such consideration in the operationists' scientific work. The reason was simple:

> In the critical matter of definition, the operationists do not actually rely upon their functionally connected operations alone to identify and distinguish phenomena. They start with a heritage of already identified variables, entities, and events, and for these they adopt the established definitions or supply working definitions of the ordinary kind. [...] It is only when the simple measuring operations cannot be performed or when they yield ambiguous results that the operationists resort to their use of functional connections as defining operations.[96]

In other words, the operationists had put the new method upon the old methodological base without serious regard for the resultant structure. Israel and Goldstein recognized that the operationists' device for establishing the existence and the identity of primary phenomena by stating their functional connections was "a major innovation in scientific logic."[97] They warned, however, that before justifying its use one would need to examine its implications and the consequences of the purely functional operationism.

Challenged by Israel and Goldstein, E. G. Boring felt urged to clear up the controversial matters and suggested that the *Psychological Review* organize a symposium on operationism. The editor accepted Boring's plan and invited the following to the symposium: Boring, Bridgman, Herbert Feigl, Harold Israel, Carroll C. Pratt, and B. F. Skinner. War work prevented Stevens and Tolman from joining.[98] The participants' papers and rejoinders were published in the September 1945 issue of the

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[96] *Ibid.*, p. 187.
[97] *Ibid.*
[98] E. G. Boring, *A History of Experimental Psychology*, 2nd ed. (New York: Appleton-Century-Crofts, 1950), p. 663; S. S. Stevens, "S. S. Stevens," pp. 411-412.

Psychological Review.[99]

The symposium was one of the rare occasions on which Bridgman had to face various comments on operationism raised in the field unfamiliar to him. To the question of whether operationism was "more than a renewed and refined emphasis upon the experimental method," Bridgman responded by pointing out that operational analysis was "restricted to questions of meaning" and could "have only partial congruence with the universe of experimental method."[100] The operational method was no longer a guideline for the reform in physics, but merely a theory of meaning. "A term is defined," he argued, "when the conditions are stated under which I may use the term and when I may infer from the use of the term by my neighbor that the same conditions prevailed."[101] Stressing that operational analysis was useful in checking whether such conditions were satisfied, Bridgman went on to describe the characteristics of operations adopted for this purpose: The operations that had practical value must be "repeatable and performable on demand." Admitting that one could neither specify nor repeat any operation with absolute precision, he indicated that certain operations could practically be repeated by the same person or different persons under the same or different conditions "without hesitation and with the accompaniment of no phenomena which demand the assertion that there has been failure to repeat."[102] Bridgman assumed that only such operations were useful in any enterprise. However, like other advocates of operationism, he did not clarify how one could distinguish from others a group of operations recognized as identical.

As Sigmund Koch and Maila Walter have observed, by the time the psychologists' symposium in 1945 took place, Bridgman's operationism

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[99] "Symposium on Operationism," *Psychological Review*, 52 (1945), pp. 241-294.
[100] P. W. Bridgman, "Some General Principles of Operational Analysis," *Psychological Review*, 52 (1945), pp. 246-249, p. 248.
[101] *Ibid.*, p. 246.
[102] *Ibid.*

had metamorphosed into "a general hermeneutics." His operations had no longer been ones for creating new concepts or reforming science. Boring recognized the change in Bridgman's ideas and pointed out that he had softened "his more extreme position about the pluralism of constructs" in *The Logic of Modern Physics*. Boring and his colleagues including Stevens neither shared nor criticized this transformed position of Bridgman: "After all that book is eighteen years old and Bridgman might be expected to develop his thinking in a couple of decades."[103]

Feigl, who presented a mature viewpoint of logical positivism, reached conclusion similar to Bridgman's. According to Feigl, operationism was neither a system of philosophy nor a technique for forming concepts or theories. Only the labor and ingenuity of the researchers would produce scientific results. Feigl regarded operationism as "a set of regulative or critical standards" for appraising the meaningfulness and fruitfulness of scientific concepts.[104]

Israel criticized the way Bridgman and the operationists in psychology termed different sets of operations equivalent. In *The Logic of Modern Physics*, Bridgman recognize two different sets of operations to be equivalent if these two sets yielded the same quantitative results in all cases considered. In so doing, however, he detached the concept of quantitative value from its operational context and assigned to it "the status of an absolute property, quantity of a kind which transcends the method by which it is determined." Strict operationists would not accept two quantitative results as the same unless the same set of operations produced them. Nor would they make the mistake of regarding two constructs as the same only because they happened to bear the same numerical designation. In order to gain "the freedom and convenience [...] necessary to sensible investigation and theory,"

[103] "Symposium on Operationism," p. 278.
[104] Herbert Feigl, "Operationism and Scientific Method," *Psychological Review*, 52 (1945), pp. 250-259, p. 259.

Bridgman and operational psychologists might need to introduce "the non-operational construct of absolute quantity." Yet, Israel reminded Bridgman of his original standpoint by asking whether he was "not doing [...] the same thing which he caught the old physicists doing at the time he proposed the operational corrective," and whether he was "not liable to make the old mistakes all over again."[105] Though Israel might have not known it, Bridgman had left unanswered a similar criticism by L. J. Russell for seventeen years.[106] Since neither Bridgman nor other participants in the symposium comprehended Israel's remarks properly, Israel practically received no response.

The advocates of operationism among the participants shared the belief that the operational definition was useful and necessary since science was public. In Boring's understanding, scientific concepts must be capable of operational definition since "science is empirical and excludes private data."[107] For Feigl, science was "by definition" a social enterprise that called for operations repeatable by any properly equipped observer. Accepting only "intersubjectively testable" statements as scientifically meaningful, he judged that "[p]rivate, immediate experience as such is only the raw material, not the real subject matter of science."[108]

For Skinner, not only science but consciousness or awareness was a social product "generated by the differential reinforcement supplied by a verbal environment"[109]: "The individual becomes aware of what he is doing only after society has reinforced verbal responses with respect to

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105 Harold Israel, "Two Difficulties in Operational Thinking," *Psychological Review*, 52 (1945), pp. 260-261, p. 261.
106 L. J. Russell, "The Logic of Modern Physics, Critical Notice," *Mind*, 37 (1928), pp. 355-361, p. 356.
107 Edwin G. Boring, "The Use of Operational Definitions in Science," Psychological Review, 52 (1945), pp. 243-245, p. 244.
108 Feigl, "Operationism and Scientific Method," p. 257.
109 B. F. Skinner, *The Shaping of a Behaviorist* (New York: New York University Press, 1984), p. 295.

his behavior as the source of discriminative stimuli."[110] Skinner explicated his formulation of the general interrelation between a stimulus, a response, and a reinforcement by the verbal community: "[T]he community reinforces the response only when it is emitted in the presence of the stimulus."[111] Following this scheme, he applied operational analysis to the subjective term by discussing how a verbal community could teach a person to describe stimuli to which the community had no access. He pointed out that though private stimuli only act upon the speaker, not upon the community, the latter can generate verbal behavior if other information about the stimuli is available, "just as a colorblind person can teach a person with normal vision to name colors correctly."[112] Skinner then listed four kinds of public information a community could use to generate verbal behavior in response to private stimuli. Skinner's radical behaviorism showed how psychologists could unequivocally define or denote their data and concepts, including the subjective terms.

Skinner followed Kantor in criticizing Boring and Stevens. To Skinner the operationism of Boring and Stevens seemed to be "a sort of *E pur si muove* in reverse,"[113] an attempt to seek for the operational definition of mental states but at the same time to preserve the old explanatory fictions such as consciousness, will, feeling, and so on. This doctrine would divide the world into public and private events and confine psychology to the former. Skinner found the present

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[110] B. F. Skinner, "The Operational Analysis of Psychological Terms," *Psychological Review*, 52 (1945), pp. 270-277, p. 277.

[111] *Ibid.*, p. 272.

[112] Skinner, *The Shaping of a Behaviorist*, p. 294.

[113] "Symposium on Operationism," p. 293. "E pur [Eppur] si muove" (literally, "it [the earth] still moves") is a phrase Galileo is supposed to have said in front of the cardinals of the Roman Inquisition in 1633. The phrase first appeared in Guiseppe Marco Antonio Baretti, *The Italian Library* (London: A. Millar, 1757), p. 52. In 1911, a Belgian family noticed that they owned a painting dated 1643 (1645?) that supposedly represented Galileo pointing to the phrase "Eppur si muove" written on a dungeon wall (Stillman Drake, *Galileo at Work: His Scientific Biography* (Chicago and London: The University of Chicago Press, 1978), p. 357).

operationism lacking “the bold and exciting behavioristic hypothesis that what one observes and talks about is always the ‘real’ or ‘physical’ world (or at least the ‘one’ world) and that ‘experience’ is a derived construct to be understood only through an analysis of verbal (not, of course, merely vocal) processes.” While radical behaviorism of Skinner allowed psychologists to consider private events, operationism of Boring and Stevens prevented them from dealing with private data. Skinner put this situation into an irony: “[W]hile Boring must confine himself to an account of my external behavior, I am still reasonably interested in what might be called Boring-from-within.”[114]

Feigl, Boring, and also Stevens maintained that science was and should be public, while Skinner admitted no essential distinction between private and public. Bridgman could accept neither of them. Having contended that science was private in some articles and books published before the symposium, he was prepared to rebut the commonly held view that science was public. To Feigl and Boring, Bridgman reiterated his favorite argument that “my science” was different from “your science” in the same way as “my pain” was different from “your pain.” He further called their attention to the recent political tendency to suppress individuality:

> All government, whether the crassest totalitarianism or the uncritical and naïve form of democracy toward which we are at present tending in this country, endeavors to suppress the private mode as illegitimate, as do also most institutionalized religious and nearly all systems of philosophy or ethics. Yet the private mode is an integral part of each one of us, ready to flare into action under the stimulus of any new exploitation of the individual.[115]

He maintained that no government could be successful unless it achieved the reconciliation between the public and private modes. In the like

[114] *Ibid.*, p. 294.
[115] *Ibid.*, p. 283.

manner, he warned, psychology might suffer "by its failure to recognize and insist on the social and the private modes of individual behavior." Psychologists ignoring this dichotomy seemed to him to be "engaging in an unnecessary gamble" and "riding for a fall."

To Skinner's argument, however, Bridgman reacted awkwardly. Bridgman feared that Skinner was trying to "establish the full operational equivalence of 'public for me' and 'private for me.'"[116] If Skinner succeeded in his ambitious attempt, Bridgman's advocacy of private science would lose its grounds. Though Bridgman asserted that the "most superficial observation" could show that the operations for dealing with "public for me" were "qualitatively different" from those for dealing with "private for me," he was neither completely sure whether Skinner's program would fail nor confident that he understood it correctly. Bridgman could only report his "inviolable isolation from his fellows": "My thoughts are my own, and I will be damned if I let you know what I am thinking about."[117]

The symposium marked the peak of psychologists' enthusiasm over operational method. After the symposium almost no article or report on operationism appeared in major psychological journals. In the midst of the postwar rise and reorganization of psychology as behavioral science, psychologists started to find the reduction of concepts to their corresponding operations to be "dull business."[118] According to Boring, by 1950 psychologists had found that the operational reduction took thought, study, and time, but might produce "little or no gain."

However, by reviewing the exchanges at the symposium one can comprehend the maturity and variety of psychological and philosophical arguments over operationism in the 1930s and 1940s. Though the boom of operationism brought no unification or agreement among

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116 *Ibid.*, p. 282.
117 *Ibid.*, p. 283.
118 Boring, *A History of Experimental Psychology*, p. 658.

various views, advocates explored possibilities of operational method as a tool for research, and critiques examined its defects as a philosophy of science. Asking and answering questions over operationism that could not have occurred to Bridgman, they sophisticated their language and standard for evaluating research program, though Bridgman himself might sometimes have felt isolated in the midst of heated discussion.