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## MODERN MARKETING STUDIES AND METHODOLOGICAL PROBLEMS

by

*Kazuyoshi Hotta*

### ***I. Introduction***

It is not so old in the history of marketing studies that the discussions have been developed in the methodological aspects. As is generally said, however, it is an actual fact that they have a history, at least, of over thirty years if we might regard the proposal by Paul D. Converse<sup>1)</sup> as the first.<sup>2)</sup> Since then, we can find them having assumed two types of controversies when limiting our considerations to the methodological dimension. The one has been the proposals concerning the scientific status of the studies on marketing which have appeared in the form of "Is Marketing a Science?" or "Can Marketing Be a Science?". The other is, while relatively recent, the proposals insisting actively on the extensive expansion of marketing concept, though basically within the framework mentioned above.

A group of the proposals which have been intermittently offered could be tentatively classified into the following three stages;

- (1) the period in which initial efforts to express personal subjective and often normative views on "marketing as a science" have been continuously developed (1945—early 1950s),
- (2) the period in which the conditions or rules constituting a science have been pursued (mid 1950s—about 1965), and
- (3) the period in which, under the circumstances that typical technical considerations were dominant, critics and refutations have appeared in the field of semantics on the one hand and approaches to the theory of science, namely the methodology, have been seen on the other hand (about 1968—present).

In this article, we can not afford to handle all of the proposals which belong to these three stages, but we do express our approval for the tendencies that a value of the argument of this kind—hitherto having been often neglected at all

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1) Paul D. Converse, "The Development of a Science of Marketing," *Journal of Marketing*, Vol. 10, No. 1, (July 1945), pp. 14-23.

2) Kenneth D. Hutchinson, "Marketing As a Science: An Appraisal," *Journal of Marketing*, Vol. 16, No. 3, (January 1952), pp. 286-293.

—has come to attract the interest of many marketing scholars and the tendencies that marketing studies have made contact with modern theory of science in the third stage, especially in the 1970s. Now from the same standpoint, we would like to attempt to offer some considerations on the methodological rules upon which any theory should depend in order to be regarded as or to assert itself scientific. Our analysis will be directed to the proposal of Robert D. Buzzell who offered concisely the methodological rules.

## II. *Confusions in the Methods of Studies on Marketing and the Conditions of Science Construction*

The debates, which have developed around the two problems mentioned above, caused various confusions in the ways of defining marketing. On all such occasions, many of marketing scholars expressed their misgivings that the controversies might not take a course toward the deepening of marketing theory.<sup>3)</sup> In fact, the classificatory study by the marketing staff of the Ohio State University showed that the term "marketing" was given relatively different meanings by each scholar, and was classified into twelve types.<sup>4)</sup> In 1974, Robert Bartels of the same university cried "The Crisis of Identification in Marketing"<sup>5)</sup>, in the next year, R. Angelmar and C. Pinson reasked "The Meaning of 'Marketing'"<sup>6)</sup>, and such proposals have continued to be offered until today.

Behind such a confusion we could point out that the argument was rarely deepened to the methodological dimension regarding the acquisition of scientific recognition, that there might be a kind of sheer blind belief that "marketing (discipline) should be practical or utilitarian", and that the belief itself never became an object of criticism.

There is no doubt that in many cases our scientific knowledge has been originally developed to respond to some practical requirements in daily life. Therefore, the practical aspect of the scientific knowledge, in other words, that of a technical application of the knowledge, should not be neglected or underestimated. But if it means that the scientific studies on marketing would be synonymous with giving some guides to a marketer who makes a rational decision

3) Kazuyoshi Hotta, "Marketing Kinko Gainen o meguru Ronso," (Controversies on the Concept of Marketing Function), *Mita Shogaku Kenkyu*, (Keio University, Tokyo, Japan), Vol. 17, No. 5, (1974), pp. 55-60. See especially a series of discussions by R. S. Alexander, F. M. Surface, R. F. Elder, W. Alderson, H. E. Agnew, R. B. Jenkins, J. C. Drury, F. M. Jones, E. D. McGarry and the others since the definition of 1935.

4) Marketing Staff of the Ohio State University, "A Statement of Marketing Philosophy," *Journal of Marketing*, Vol. 29, No. 1, (January 1965), p. 43.

5) Robert Bartels, "The Identity Crisis in Marketing", *Journal of Marketing*, Vol. 38, No. 4, (October 1974), pp. 73-76.

6) Reinhard Angelmar and Christian R. A. Pinson, "The Meaning of 'Marketing'", *Philosophy of Science: Official Journal of the Philosophy of Science Association*, Vol. 42, No. 2, (June 1975), pp. 208-214.

under some peculiar situations—the guides applied only to him—, then it should be rejected from the methodological point of view, because some theories of general nature must necessarily exist as a premise in order to offer him any practically useful guides and because it is a predictive proposition logically derived from the theories that has an aspect of practical techniques. Therefore, it is the task of science in marketing to explain and predict marketing phenomena in a satisfactory manner. The task of marketing theorists is to discover any possible universal theories for them.

Nevertheless, the basic attitudes in this field, which hitherto supported the blind belief in the practical aspects of the theories, have been, explicitly or implicitly, in pursuit how to use the results from other fields of knowledge as a way from some particular viewpoints of value and how to get some optimal or reasonable solutions of specific problems. We have to point out, however, that such attitudes are different from those of theorists who are interested in testing their theories. In other words, theorists do not neglect the technical application of scientific knowledge because it plays a critical part in the test of the theories, namely because it does a critical part in confirming whether or not the theories are false and whether or not they can endure the falsification tests, enrich their empirical contents, and increase the degree of their universality and falsifiability. In comparison with this, the traditional attitudes in the field of marketing studies have been, we could say, carried out by outstandingly normative problem-solving orientation, by instrumentalistic viewpoint which regards knowledge only as something like the arithmetical rules or as an instrument for describing real phenomena, or by conventionalistic artifice which regards theories as convention and prepares some evasive answers against falsification proposition.

Now, it is not necessary here to repeat that marketing plays an important role in our modern competitive society. However, marketing phenomena bring about much complicated situations because they have increasing significance of socio-cultural value relations to various directions. Reflecting such situations, studies on marketing spread widely over to various degree of theoretical levels from each point of view as well as by assuming distinctive attitudes mentioned above.

Generally speaking, it is not the mere presence of any events or the appearance of the objects that enable a science to come into existence, but the establishment of the rule of science which directs studies under impetus from the socio-cultural significance of the events. In social sciences it is especially indispensable to set up the rule of science because a field of knowledge has strong relations with other existing disciplines and because the relations should be arranged if the discipline should require its relative originality as an independent field of scientific knowledge.<sup>7)</sup>

7) Saburo Kojima, *Doitsu Keiken-shugi Keiei-Keizaigaku no Kenkyu*, (A Study on German Empiricists' Business Economics), Tokyo, Yuhikaku, 1965, pp. 5-7, 9, 11, 12, 13-14, 27.

An aspect peculiar to studies on marketing, however, has not been the arrangement of the relations with other disciplines to assert its relative originality, but rather the assertion as if a new system of scientific knowledge would be established through active and unprincipled taking-in of knowledge from the neighboring fields. It has been an effort to explain empirical objects as a whole and has been conducted by strong contingent problem-solving orientation. But there are by far various conditions around naked empirical objects or specific problem situations. They are really chaotic and they change their shape from the points of view. Therefore many of marketing studies have been hitherto obliged to assert something fragmental from each point of view reflecting the complexity of empirical objects. In other words, such studies on marketing, it may be said, have repeated aloud that marketing phenomena have increased its socio-cultural significances. They have only insisted on something like theory, rearranging and classifying individual empirical objects consisted of occasional events. There should be a rule of science in order that they might be realized as an independent field of science, and it is this rule that distinguishes a theory from statements of theoretical pretension in nature.

The assertion of theoretical pretension is, in many cases, an observation or a description of individual and particular events, or a statement related to some special normative value. In the former case the statement is a singular or particular one which is supported by conditions restricting the situations. How many statements about these situations we might simply enumerate, we could not acquire theories or hypotheses as universal statements in the sense of methodology; that is the inductive inference, which is composed of observation or description of individual particular events, and emphasizes that a universal statement could be based upon singular statements; but it is impossible without setting up a principle of induction, as Karl R. Popper points out, in order to make this way of inference logically admissible; this principle of induction is, however, not a universal statement but a singular one derived from the naked empirical facts; thus, although it is obliged to assume another induction principle of higher level for justifying the inference, nevertheless we could not evade and overcome the contradiction because the nature of the principles is descriptions of experiences; these processes mean indefinite regression or a priori presumption of its justification. Therefore, the principle of induction could not be a statement with falsifiability at all.<sup>8)</sup>

Also, a statement which is related to and evaluated through some special normative value could not be reasonably refuted and criticized at all; it is of metaphysical nature because it is nothing but an expression of personal subject. Whatever intense and overwhelming the personal feeling of the conviction might be experienced, it is never consistent with the idea of scientific objectivity. It has

8) Karl R. Popper, *The Logic of Scientific Discovery*, New York, Harper and Row, Publishers, 1965, pp. 27-30, 252-254; "It [the principle of induction] could therefore only be introduced as a primitive proposition (or a postulate, or an axiom)".

only a personal significance.<sup>9)</sup>

### III. *The Situation of Methodological Problems in Marketing Studies*

There have been proposed, as already stated, many ideas on this issue of the scientific status of marketing studies since Paul D. Converse did. Among them it was the proposal by Robert D. Buzzell, as W. J. Taylor pointed out,<sup>10)</sup> that made a clearer statement on the criteria of science.<sup>11)</sup> Of course, we know that before his proposal this issue had been seriously caught in its own way, at least by W. Alderson and R. Cox,<sup>12)</sup> R. S. Vaile,<sup>13)</sup> R. Bartels,<sup>14)</sup> K. D. Hutchinson,<sup>15)</sup> W. J. Baumol,<sup>16)</sup> A. R. Oxenfeldt,<sup>17)</sup> and so on.<sup>18)</sup>

Some of them answered to the scientific status of the studies affirmatively, others negatively, from their own point of view. Generally speaking, we could say, however, that they were no better than subjective views on marketing studies

9) Karl R. Popper, *op. cit.*, pp. 43-44, 46.

10) Weldon J. Taylor, "Is Marketing a Science? Revisited", reprinted from the *Journal of Marketing*, Vol. 29, (July 1965), pp. 49ff., in Jerome B. Kernan and Montrose S. Sommers (eds.), *Perspectives in Marketing Theory*, Appleton-Century-Crofts, 1968, p. 50.

11) Robert D. Buzzell, "Is Marketing a Science?", reprinted from the *Harvard Business Review*, Vol. 41, (January-February 1963), pp. 32ff., in J. B. Kernan and M. S. Sommers (eds.), *op. cit.*, pp. 36-48.

12) Wroe Alderson and Reavis Cox, "Towards a Theory of Marketing", *Journal of Marketing*, Vol. 13, No. 2, (October 1948), pp. 137-151.

13) Roland S. Vaile, "Towards a Theory of Marketing—a Comment", *Journal of Marketing*, Vol. 13, No. 4, (April 1949), pp. 520-522.

14) Robert Bartels, "Can Marketing Be a Science?", *Journal of Marketing*, Vol. 15, No. 3, (January 1951), pp. 319-328.

15) Kenneth D. Hutchinson, "Marketing As a Science; An Appraisal", *Journal of Marketing*, Vol. 16, No. 3, (January 1952), pp. 286-293.

16) William J. Baumol, "On the Role of Marketing Theory", *Journal of Marketing*, Vol. 21, No. 4, (April 1957), pp. 413-418.

17) Alfred R. Oxenfeldt, "Scientific Marketing: Ideal and Ordeal", *Harvard Business Review*, Vol. 39, (March-April 1961), pp. 51-64.

18) See, for example, Reavis Cox and Wroe Alderson (eds.), *Theory in Marketing*, R. D. Irwin, Inc., 1950; N. E. Miller, "Social Sciences and the Art of Advertising", *Journal of Marketing*, Vol. 14, No. 3, (January 1950); Wroe Alderson, "A Systematics for Problem of Action", *Philosophy of Science: Official Journal of the Philosophy of Science Association*, Vol. 18, No. 1, (January 1951), pp. 16-25; S. F. Otteson, "Research and the Science of Marketing", in *Marketing: Current Problems and Theories*, edited by S. F. Otteson, Indiana Marketing Symposium, Indiana Business Report, No. 16, School of Business, Indiana University, Bloomington, Indiana, (December 1952), pp. 11-18; R. S. Stainton, "Science in Marketing", *Journal of Marketing*, Vol. 17, No. 1, (July 1952), pp. 64-65; J. W. Newman, "New Insight, New Progress, for Marketing", *Harvard Business Review*, Vol. 35, No. 6, (November-December 1957), pp. 95-102; H. D. Mills, "Marketing As a Science", *Harvard Business Review*, (September-October 1961), pp. 137-142; E. B. Weiss, "Will Marketing Ever Become a Science?", *Advertising Age*, August 20, 1962, etc.

as a science.<sup>19)</sup> In contrast with them, the Buzzell's view is, as it will be shown later, superior to those earlier views in the point that it shows explicitly the conditions of scientific construction.

After taking notice of the degree of scientific nature of marketing studies and reviewing some of earlier proposals, Buzzell examines whether there are any science of marketing in the same sense as in natural sciences and to what extent scientific methods will be applied to marketing.

At first, on the scientific status of marketing studies, Buzzell claims that an independent science must be; 1) a classified and systematized body of knowledge, 2) organized around one or more central theories and a number of general principles, 3) usually expressed in quantitative terms, and 4) knowledge which permits the prediction and, under some circumstances, the control of future events.<sup>20)</sup> Thus he concludes that when we consider the present state of marketing studies in the light of these criteria we can find few accepted principles and no central theory, although there is a substantial body of classified knowledge about marketing, and that we should recognize, therefore, the predicting ability was much limited.

Originally, as Robert Bartels points out in his chronological study,<sup>21)</sup> major interests in marketing studies have been stimulated by the fact that there were, in traditional economic theories, inconsistencies of hypotheses with facts or deficiencies of analysis of vertical trading and/or competitive relations in markets. In the face of this situation, however, marketing students have assumed an attitude of "thorough-going empiricism"<sup>22)</sup> according to Buzzell or "naive empiricism" in our terms. That is, it was a simple accumulation of facts, or more precisely speaking, the accumulation of observational statements that described the observed facts about marketing. According to Buzzell, there appeared little principles, as a result of such concentrated efforts, although those accumulated facts about marketing phenomena were defined, classified, analyzed, and systematized to some degree.<sup>23)</sup>

It means that any inductive principles necessary to support inductive inference could not be discovered. We have already pointed out on this issue that it is logically impossible to justify the principle of induction. We could regard that Buzzell's view would take part with our thesis that "a science does not begin with gathering of naked facts", when he opposes such a naive empiricism reflecting exclusively technocracical attitudes toward marketing studies as that of

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19) Christian R. A. Pinson, Reinhard Angelmar, and Eduardo L. Roberto, "An Evaluation of the General Theory of Marketing", *Journal of Marketing*, Vol. 36, No. 3, (July 1972), pp. 66.

20) Robert D. Buzzell, *op. cit.*, p. 37.

21) Robert Bartels, *The Development of Marketing Thought*, Richard D. Irwin, Inc., 1962.

22) Robert Buzzell, *op. cit.*, p. 38.

23) *Ibid.*, p. 39.

Bursk<sup>24)</sup> and Newman<sup>25)</sup> and when he claims that it is probably hopeless to expect much progress in the development of science in marketing as a result of simply stockpiling more and more current facts.<sup>26)</sup>

This statement of Buzzell is very important in that he expresses his basic standpoint as to whether or not a science or theory were composed of gathering observational statements and composed of simple enumeration of empirical facts, in other words, whether an empirical theory would be acquired by inductive inference or deductively by hypothetical one. Buzzell strongly supports the assertion of William Lazer that "marketing thought should not proceed merely by the accumulation of observations which are unregulated by theory. It is generally accepted that fruitful observations cannot be made, nor their results arranged and correlated, without the use of hypotheses which go beyond the existing state of knowledge."<sup>27)</sup>

The statement of W. Lazer would, in fact, implicate some very important issues in the methodological point of view; that is, they are the issues that the statement confirms a theory as deductive structure of hypotheses, that our observation is one guided by a theory and not the reverse, and that the statement makes it clear, when it is a discovery of knowledge, the theory for explanation is required higher degree of universality, even if the new theory in question was spurred by some background knowledge.

Buzzell examined critically some of the works which affected seriously marketing studies in the 1960s. He opposed such persistences as we can see in the article "Interdisciplinary Contributions to Marketing Management"<sup>28)</sup> and opposed the considerations that scientific studies in general had, as it were, two types of discovery disciplines and application disciplines, or that marketing studies would rather belong to the latter type in the sense of an application of results of other sciences, especially of behavioral sciences, or that marketing studies would purposively unite, as Newman insists on,<sup>29)</sup> knowledges which would serve to understand the situation where a practical problem-solver stood and to solve the problems depending exclusively upon behavioral sciences.

Buzzell insisted that the task of this discipline was the pursuit of an epistemologically reliable knowledge on marketing phenomena and that it was

24) Edward C. Bursk, *Text and Cases in Marketing: A Scientific Approach*, Prentice-Hall, Inc., 1962.

25) Joseph W. Newman, "Put Research Into Marketing Decisions", *Harvard Business Review*, (March-April 1962), pp. 105-112.

26) Robert D. Buzzell, *op. cit.*, p. 43.

27) William Lazer, "Philosophic Aspects of the Marketing Discipline", W. Lazer and E. J. Kelley (eds.), *Managerial Marketing: Perspectives and Viewpoints*, rev. ed., R. D. Irwin, Inc., 1962, p. 611.

28) Eugene J. Kelley and William Lazer, "Interdisciplinary Contributions to Marketing Management", W. Lazer and E. J. Kelley (eds.), *op. cit.*, pp. 586-606.

29) Joseph W. Newman, "New Insight, New Progress for Marketing", *Harvard Business Review*, (November-December 1957), p. 95.



nothing less than the task of science: explanation and prediction. Supporting the Baumol's insistence<sup>30)</sup> and pointing out that "while the problems of marketing do, in fact, fall within the spheres of such fields as economics, sociology, and psychology, . . . marketing has its special problems and may, therefore, well find it usefull to develop further its own body of theory",<sup>31)</sup> he seems to attempt to show a methodological rule in scientific studies on any events that there should be the determination of viewpoint and principle of choice, because theoretical problems precede in the studies above all things.

Baumol defines that a theory as a systematized explanation is a structure describing the workings and interrelations of the various aspects of some phenomenon. He claims it, as a procedure which theorists should adopt in their intentions to propose such a structure, to examine some aspect of reality and to attempt to describe and explain much complicated chaotic reality with "the construction of a simplified small-scale model which behaves in at least some ways like the phenomena under observation."<sup>32)</sup>

A theory, as pointed out by Baumol, has abstract simple form. Buzzell examines Aspinwall's "The Characteristics of Goods Theory"<sup>33)</sup> as an example in order to know whether any theory with such a form has been developed in the field of marketing, and concludes from the standpoint of reductionism that its explanation is insufficient for the following reasons, that when the thesis that a theory should be as simple as possible is considered, Aspinwall's theory does not accord with the requirement of simplicity because it is not sufficiently reduced to the basic propositions, and that the form of inference is a vicious circle in the sense of basing its argument on only the explanandum.<sup>34)</sup>

Thus the proposal by Buzzell has, in some respects, important implications in methodology of science. But it is not unquestionable if only the four items which he has shown as requirements of science are sufficient for characterizing marketing study as an empirical science.

Therefore, in the next, we examine and propose from our point of view, the one of critical rationalism, some necessary and sufficient conditions which should be satisfied in order that a theory is regarded as scientific.

#### *IV. Theory as a Scientific Statement*

"The scientific statement which we call a theory is a net cast to catch the world—to rationalize, to explain, to master it, and we always attempt to weave

30) William J. Baumol, *op. cit.*, pp. 27-35.

31) Robert D. Buzzell, *op. cit.*, p. 40.

32) William J. Baumol, *op. cit.*, pp. 28-29.

33) Leo V. Aspinwall, "The Characteristics of Goods Theory", in W. Lazer and E. J. Kelley (eds.), *op. cit.*, pp. 633-643.

34) Robert D. Buzzell, *op. cit.*, pp. 40-41.

the net ever finer."<sup>35)</sup> And the objectivity of this scientific statement is assured by the fact that the statement is intersubjectively testable. A theory is the statement with intersubjective testability.

In the sense mentioned above, it is only when any events will recur according to regularity that our observation is testable to anyone in principle; namely it could be claimed that our proposal treats not a temporal accidental events but in principle testable one, only when it is in compliance with the regularity and recurrability. Thus a scientific statement as a theory must be always a universal hypothesis in its nature and be obedient to mutual reasonable regulation through critical arguments.<sup>36)</sup>

This fact has some important implications as follows: recurrence of certain events with regularity means that any scientific statement should possess a given viewpoint and a principle of choice and it does not mean that something like an ultimate essence or ultimate existence is solemnly latent in the depth of phenomena; the empirical events in the real world take the complicated chaotic phases and we can not grasp them as a whole; therefore the fact does not allow us to explain the empirical events before we get epistemological objects through the viewpoint and principle of choice—extreme concept.

Needless to say, this epistemological objects are not the self-identical world but the world which we are going to realize through our freedom of choice in experience. It is the world which exists only in our speculation; it is not the world upon which we depend but the world which absolutely depends upon us.<sup>37)</sup> Therefore we can never get rid of a partial recognition. In addition, our observation from a certain viewpoint and by a principle of choice is always the one in the light of theory. It means that any observation can not be realized independently of theoretical words.<sup>38)</sup> Observation does play the critical part in science, but the observation is always selective and any theoretical problem or hypothesis always precedes all observations.<sup>39)</sup> We can support, therefore, Buzzell's view asserting that a theory is never formed by the gathering of the observational statements. In other words, it is, and can not be other than, theory that suggests the data which should be gathered.<sup>40)</sup> The observational statements on the gathered data are not directly concerned with theory construction but they occupy the position as the statements that make a test of the theory (hypothesis)

35) Karl R. Popper, *The Logic of Scientific Discovery*, New York: Harper and Row, Publishers, 1965, p. 59.

36) *Ibid.*, pp. 44, 45. See also footnotes \*1) and 5) to the section 8.

37) Saburo Kojima, *op. cit.*, pp. 22, 26.

38) Karl R. Popper, *op. cit.*, p. 59, footnote \*1).

39) Karl R. Popper, *Objective Knowledge: An Evolutionary Approach*, Oxford/Clarendon Press, 1972, translated by Hiroshi Mori into Japanese, "Kyakkan teki Chishiki: Shinkaron teki Apurouchi", Tokyo, Bokutaku-sha, 1974, pp. 383-385.

40) Carl G. Hempel, *Philosophy of Natural Science*, Prentice-Hall, Inc., 1966, translated by Kurosaki into Japanese, "Shizen-Kagaku no Tetsugaku", Tokyo, Baifukan, 1967, p. 19.

tentatively proposed—as the test propositions. It is, and could not be other than, the case that our proposal should be called empirical.

Now, to explain some event scientifically, in other words, to attempt to present a universal hypothesis, means to set forth a theoretical hypothesis which deduces logically the event to be explained. It means the causal explanation of the event. Thus the causal explanation means the discovery of the sufficient explanation of all that need to be explained, by the help of a set of statements composing of both statements describing the event to be explained (explanandum) and explanatory statements which explain the event (explanans). In other words, it means that it deduces the statement describing the event or the specific singular prediction by the help of particular singular statements (initial conditions) and one or more universal law statements.<sup>41)</sup>

This is an essential qualification for an explanation to be scientific. Because a universal statement or a theory is itself a product of reasonable speculation of human beings and has an abstract universal form to a higher degree, it has, directly, nothing to do with a real world. It is nothing but initial conditions that relate theory with a real world. Therefore, how many singular statements may be simply enumerated, they are insufficient as explanans by themselves. They are only contingent gatherings of facts and nothing else could be expected.

Thus a scientific explanation will, if it is sufficient, be shown as the one which has the following logical structure;

universal law statements	
	explanans
initial conditions	
<hr/>	
statement describing	
the event or prediction	explanandum

Here, singular statement or prediction as explanandum corresponds to the result and initial conditions to its causes. The task of scientific study is, as we can see above, composed of something theoretical—explanation—and something practical—prediction or technical application which will be analyzed in the next section.

The task of theorists is, needless to say, a discovery of law statements as simple and universal as possible, and it is necessary that the universal law statement or hypothesis should endure falsification test or provide falsifiability in order to be supported. Thus it should satisfy the requirement of objectivity through intersubjective tests. The simplicity of a theory, therefore, is appraised from

41) Karl R. Popper, *The Poverty of Historicism*, New York: Harper and Row, Publishers, 1964, pp. 35–36; ditto., *The Logic of Scientific Discovery*, New York: Harper and Row, Publishers, 1964, pp. 60–61; ditto., *Objective Knowledge: An Evolutionary Approach*, Oxford/Clarendon Press, 1972, translated by Mori into Japanese, *op. cit.*, pp. 217, 388–390.

the viewpoint of increasing the falsifiability.

But in the history of science, a standpoint, like that of Poincaré<sup>42)</sup> for example, has been emphasized, which sacrificed falsifiability by regarding any theory as a convention to be callous to any falsification tests, and supported the simplicity of theory from an aesthetic point of view.

The requirement of a simple theory suggested by Buzzell is not unambiguous in this point. We do emphasize that we can evaluate the simplicity of theory only to the extent that it increases the falsifiability because we accept the falsifiability as the criterion of demarcation which makes capable of distinguishing empirical sciences from metaphysics,<sup>43)</sup> and because we do stand against the conventionalistic artifices which promise a simple theory as truth because of its aesthetic reason without regard to testing the theory.

Moreover, if universal law statements or hypotheses could cite nothing but the explanandum as an evidence, the explanation would be vicious and could not evade its ad hoc nature. Therefore the explanation would be complete when the explanans, especially the universal law statements, would not only deduce the explanandum logically but also be testable independently of the explanandum.<sup>44)</sup> Thus a theory, which we propose in attempt to explain the explanandum, should assert more than empirical events which are included in the explanandum in order that the theory is scientifically sufficient and explanatory.

As described above, we know now that the task of scientific study is explanation and prediction. The explanation, when it is a discovery of knowledge, is always the explanation of known events by means of unknown theories. We logically deduce the explanandum, with initial conditions, from the unknown theories which are tentative hypotheses, hypotheses never justified in any ways at all. Therefore the truthfulness of such theories may be questioned. We have to avoid such attitudes as presupposing theories a priori true, or such attitudes as overcoming critics or refutation through introducing ad hoc auxiliary hypotheses under conventionalistic artifices. Rather, the theories as universal statements, which are often shown in the form of universal non-existential statements, have to be testable and, as we have already stated, especially to be testable independently of the explanandum in order to avoid a vicious circle.

Thus in a scientific explanation, the thesis must be accepted that the auxiliary hypotheses should not be introduced in the direction of conventionalistic artifices of lowering falsifiability aloof from any reasonable criticism and falsification, but in the direction, and only in that direction, of contributing the increase of the

42) Henri Poincaré, *La Valeur de la science*, 1905, translated by Yoichi Yoshida into Japanese, "Kagaku no Kachi", Tokyo: Iwanami Shoten, 1977.

43) Carl G. Hempel, *Philosophy of Natural Science*, translated by Kurosaki into Japanese, *op. cit.*, pp. 64-72; Karl R. Popper, *The Logic of Scientific Discovery*, pp. 136-145, especially see the section 43, footnote \*2).

44) Karl R. Popper, *Objective Knowledge: An Evolutionary Approach*, translated into Japanese, *op. cit.*, pp. 218-219, 388, 391.

falsifiability of the statements of universal nature.

### ***V. Summary and Conclusion: Theory, Its Technical Application, and Falsifiability***

We have already shown the tasks of science; explanation and prediction. Here among them, we would like to examine some characteristics of the prediction which is deduced from the logical structure already mentioned.

In the case of prediction, it is generally assumed that the explanans (theories and initial conditions) are already known. In such a case, we seek to find a kind of logical conclusion that is not yet known to us through any observations. This is prediction<sup>45)</sup> and is in contrast to an exploration of explanation explaining the known by means of the unknown.

In the scheme of explanation, as already stated, the relation of the affairs stated by singular initial conditions to the affair stated by the explanandum is corresponding to the relation of the cause to the result which is logically connected with by the theories or laws.

On the other hand, in the scheme of prediction, when the explanandum is related to any given normative value or when some practical requirement occupies the place of the explanandum, it is seen as an end or a purpose. This is technology or technical application of knowledge, which regards initial conditions to be discovered as a means of technically realizable nature.<sup>46)</sup> As a result, it shows a certain normative instruction suggesting the ways achieving some desirable end or avoiding some undesirable one by the help of the initial conditions (means) and the given related theories. It aims at finding out a solution to the problem which technical experts are asked for. This kind of problem has at least a theoretical nature only when they will contribute to the attempt to eliminate an error in their solving the problem and when they will contribute to the test of the explanans through comparing the prediction deduced from them with real observable affairs.

In general, that does not mean in any sense that the theory is the final, even if it is assumed to be given or known in the deduction of the prediction. Rather any theory is a hypothesis that is tentatively proposed and not justified in any way at all. Since prediction is a logical consequence of this kind of hypothesis, its empirical application is a form of the deductive test of the theory; that is, it is the object to find out to what extent it could endure the proposed practical requirement. In this case, the test is composed of the comparison of a

45) Karl R. Popper, *op. cit.*, translated into Japanese, *op. cit.*, p. 392.

46) Karl R. Popper, *op. cit.*, translated into Japanese, *op. cit.*, pp. 392, 393; ditto., *The Logic of Scientific Discovery*, 1965, p. 60; ditto., *The Open Society and Its Enemies: Vol. II, The High Tide of Prophecy: Hegel, Marx, and The Aftermath*, Princeton, New Jersey: Princeton University Press, 1966, pp. 261-263, and footnote 7) of the chapter 25, pp. 362-364.

given singular statement (prediction) deduced from the theory by the help of other statements already accepted with the results of practical applications and experimentations.<sup>47)</sup> Thus, if individual conclusions from them are accepted, we do not find any reason to abandon the theory, and it survives the tests.

However, that does not mean that the theory has been confirmed as truth, but means, and only does mean, that the theory has not been false, at least in the light of extensive strict tests hitherto. On the other hand, if the decision which is made in a comparison of individual conclusions with the test is negative and if the conclusions are falsified, it means the falsification of the theory deducing the conclusions. Therefore, it is the characteristic role of the prediction to prepare the falsificational proposition against the theory.

But if we overemphasize this aspect of prediction and fix our eyes only upon its technical usefulness, then we come to take our course without relations to the progress of theory. For we can get some practically useful guidelines from a false theory. So it will be very important to ask if the previous studies on marketing would strikingly show this tendencies. And this seems to have been the question of Buzzell.<sup>48)</sup> We do not find, however, any active correspondences to the question of Buzzell hitherto. In that sense, it is the aim of this essay to attempt to propose some methodological rules necessary for the studies on marketing in order to require scientific status on the basis of the theory of science or philosophy of science.

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47) The other test forms of a theory are the following: 1) the logical comparison of the conclusions among themselves, by which the internal consistency of the system is tested, 2) the investigation of the logical form of the theory, with the object of determining whether it has the character of an empirical or scientific theory, or whether it is, for example, tautological, and 3) the comparison with other theories, chiefly with the aim of determining whether the theory would constitute a scientific advance should it survive our various tests. Cf. Karl R. Popper, *The Logic of Scientific Discovery*, 1965, pp. 32-33.

48) Robert D. Buzzell, *op. cit.*, pp. 47-48.