

Studies in
Topological and
Vector
Psychology

Kurt Lewin

新闻学与传播学经典丛书·英文原版系列

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Vector Psychology
拓扑研究与媒介心理学

Kurt Lewin 著
〔美〕库尔特·勒温

中国传媒大学出版社

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随着中国高等教育教学改革的推进，广大师生已不满足于仅仅阅读国外图书的翻译版，他们迫切希望能读到原汁原味的原版图书，希望能采用国外英文原版图书进行教学，从而保证所讲授的知识体系的完整性、系统性、科学性和文字描绘的准确性。此套丛书的出版便是满足了这种需求。亦可使学生在专业技术方面尽快掌握本学科相应的外语词汇和了解先进国家的学术发展的方向。

本系列丛书在原汁原味地引进英文原版图书的同时，将目录译为中文，作为对原版的一种导读，供读者阅读时参考。本系列丛书有些因为出版年代比较久远，也囿于当时印刷水平的限制，有些地方可能与现在的标准不太一致，在不影响读者阅读的前提下，我们未对其进行处理，以保证英文原版图书的原汁原味，

从事经典著作的出版，需要出版人付出不懈的努力，好在有全国新闻院系的专家教授们的大力扶持，为我们提供了备选书目并对英文目录进行了翻译，因此使我们得以在学术出版的道路上走得更远。我们自知本系列丛书也许会有很多缺陷，我们也将虚心接受读者提出的批评和建议。

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PART ONE
FORMALIZATION AND PROGRESS
IN PSYCHOLOGY

by

Kurt Lewin

FORMALIZATION AND PROGRESS IN PSYCHOLOGY

I

The series of larger research articles which it is hoped will begin with this publication, is planned both as a new start and as a continuation. It is a continuation of a series containing twenty articles published during the years 1926 to 1937 under the general title: "Investigations in the Psychology of Action and Emotion" (37). It is a new start, because the new studies grew not only geographically in a new environment: they try to apply, in addition to the methods previously used, more elaborate statistical techniques whenever this appears suitable, and to adapt those techniques to the spirit of the former studies.

At such an occasion it might be appropriate to review briefly the state of affairs: what have been the guiding principles of the work thus far? what has been the result? and what are the steps to follow?

Nearly all the studies mentioned have grown out of certain theoretical expectations. They have been designed to prove or to disprove certain assumptions. In other words, they were definite questions put to nature. The attempt was made to formulate these questions as sharply as possible, because nature likewise cannot give a clear and definite answer to a vague question.

On the whole, psychologists were at that time rather adverse to theory. Governed by a naïve metaphysical belief, they were apt to consider "fact finding" the only task of "scientific" psychology, and were particularly sceptical of the idea of psychological laws in the field of needs, will and emotion, that is, in fields other than perception and memory.

In the last five years a very marked change in the attitude of American psychology, which today is by far the most important center of psychological work, has been noticeable. A definite interest in psychological theory has emerged, due partly to the effort of a few psychologists (particularly Tolman and Hull in animal psychology). The need for a closer fusion of the various branches of

practical tasks of mental hygiene and education demand conceptual tools which permit prediction. Neither demand can be met without theory. A diagnosis made more than fifteen years ago (38) has proved more correct than could have been hoped for: the last decade has shown that the time is ripe in psychology for scientific research on a much more theoretical level.

Now, however, it seems necessary to point to certain dangers of theorizing. Enthusiasm for Theory? Yes! Psychology can use much of it. However, we will produce but an empty formalism, if we forget that mathematization and formalization should be done only to the degree that the maturity of the material under investigation permits at a given time.

Philosophically, there seems to exist only an "either-or": if scientific "facts" and particularly all so-called dynamic facts are not merely "given data," but inseparably interwoven with theoretical assumptions, there seems to be no choice other than to base every statement in psychology on theoretical assumptions.

For the psychologist, as an empirical scientist, the situation looks rather different. He finds himself in the midst of a rich and vast land full of strange happenings: there are men killing themselves; a child playing; a child forming his lips trying to say his first word; a person who having fallen in love and being caught in an unhappy situation is not willing or not able to find a way out; there is the mystical state called hypnosis, where the will of one person seems to govern another person; there is the reaching out for higher, and more difficult goals; loyalty to a group; dreaming; planning; exploring the world; and so on without end. It is an immense continent full of fascination and power and full of stretches of land where no one ever has set foot.

Psychology is out to conquer this continent, to find out where its treasures are hidden, to investigate its danger spots, to master its vast forces, and to utilize its energies.

How can one reach this goal? At first, in what might be called the "speculative epoch," the attempt was made to dig down deep into the ground. A peculiar something was reported to lie under ground as the hidden source of energy. One gave it the name "association." New investigators drove their shafts down at somewhat different places. They found something different which they called "instinct." A third group of explorers reported a different entity, "libido." And all claimed to have found *the* foundation on which the land rested. By this time, psychologists had become rather

tired of the various claims. It had become clear that the continent was much larger than was suspected at first. Perhaps there was more than one source of energy. The whole depth-sounding process had become rather open to suspicion, particularly since no explorer seemed able to bring his material up to the surface for inspection in broad daylight. How was one ever to prove a real connection between the entities supposedly existing underground and what was going on at the surface? There, open to all eyes, and unquestionable, interesting phenomena presented themselves. The psychologist now turned to extensive travelling over the surface of the continent, eager to find new phenomena, to describe them exactly, to count and to measure them, to register their growth.

This procedure, however, did not prove altogether satisfactory either. After all, what the psychologist observed were human beings. Children needed help and education; delinquent people needed guidance; people in distress wanted cure. Counting, measuring and classifying their sorrows did not help matters much. Obviously one had to go to the facts "behind," "below the surface." How to accomplish this without the fallacies of the speculative epoch? That is the dominant methodological question of psychology today, at the beginning of its "Galilean period."

The answer is something like this: to make oneself master of the forces of this vast scientific continent one has to fulfill a rather peculiar task. The ultimate goal is to establish a network of highways and superhighways, so that any important point may be linked easily with any other. This network of highways will have to be adapted to the natural topography of the country and will thus itself be a mirror of its structure and of the position of its resources.

The construction of the highway system will have to be based partly upon assumptions which could not be expected to be fully correct. The test drilling in exploring the deposits would not always lead to reliable results.—Besides, there is a peculiar paradox in the conquering of a new continent, and even more so in that of a new scientific field. To make the proper tests, some machinery has to be transported, and such transportation presupposes more or less the same road, the construction of which is contingent upon the outcome of the test. In other words, to find out what one would like to know one should, in some way or other, already know it.

What should science do to resolve this paradox? If it is wise,

it follows the same procedure used in a systematic exploration of the resources of a new land: small paths are pushed out through the unknown; with simple and primitive instruments measurements are made; much is left to assumption and to lucky intuition. Slowly certain paths are widened; guess and luck are gradually replaced by experience and systematic exploration with more elaborate instruments. Finally highways are built over which the streamlined vehicles of a highly mechanized logic, fast and efficient, can reach every important point on fixed tracks.

By and large, the actual development of a science seems to follow this general pattern. Yet frequently somebody, thinking he knows where an important treasure lies, tries to build a superhighway straight to this point without regard for the natural structure of the country. Much enthusiasm and work is put into such roadbuilding, but after some time it becomes apparent that this superhighway is a dead end leading nowhere.

Formalization and mathematization in psychology, if prematurely done, may lead us to the building of such logical superhighways. Formalization will have to be achieved if psychology is to become an acceptable science, and psychology can and must take definite steps in that direction now. However, the promising beginning and the growing interest for such undertaking will soon turn into disappointment, if certain dangers, arising partly from recent trends in philosophy and logic, are not frankly discussed and avoided.

I feel somewhat obliged to take this matter up, because *Principles of Topological Psychology* (45) and *The Conceptual Representation and the Measurement of Psychological Forces* (47) deal mainly with the conceptual tools of psychology. Some of the critics, who did not realize that these conceptual tools have been used for more than a decade in a great number of investigations in a variety of fields, seem to have concluded that my main interest in psychology is formalization or mathematization. Nothing can be more erroneous. As psychologists we are interested in finding new knowledge about, and deeper insight into, psychological processes. That is, and always has been, the guiding principle. Theory, mathematization and formalization are tools for this purpose. Their value for psychology exists only in so far as they serve as a means to fruitful progress in its subject matter, and they should be applied, as complex tools always should, only when and where they help and do not hinder progress.

II

Some psychologists interested in "strict logical derivations" have criticised our experimental work for not being written in the form: (a) definition, (b) assumption, (c) conclusion. On the other hand French (23) writes: "In the course of fifty years (Psychoanalysis) has developed an extensive system of scientific concepts but the concepts have grown step by step as a necessary and inevitable product of Freud's attempt to orient himself in a bewildering chaos of psychological facts that no one previously has been able to understand. Due to close contact of these new concepts with the facts, one set of concepts was devised to explain one set of facts and a new problem would give rise to an entirely new set of concepts. Topological psychology on the other hand starts with a selfconsistent mathematical discipline and then goes to look for facts to fit it." (p. 127)

As an answer I may be permitted to survey the actual historical development. My work in psychology began with experiments on association and the *determinierende Tendenz* (35, 36). The intention was not to criticize associationism but rather to refine the measurement of the "strength of the will" as developed by Ach (66). His work at that time, I believe, was the most precise theoretically in the field of will and association. After three years of experimentation with hundreds of series of nonsense syllables, and after thousands of measurements of reaction times (at that time one had to measure in 1/1000 seconds) I became convinced that there was no point in trying to improve the exactness of this measurement. The attempts were all based on the assumption of the classical law of association as stated, e. g., by G. E. Müller. The experiments however seemed to prove conclusively, contrary to my expectation, that this assumption had to be abandoned or decidedly modified. It was necessary to distinguish two rather different types of habits (associations): "need habits" (like alcoholism) and "execution habits" (like pulling a lever up rather than down). The first type represents a "tension" (source of energy), a need such as hunger, which demands satisfaction either directly or through substitution. The execution habit, on the other hand, is in itself no source of action. It is equivalent to a pattern of restraining forces determining a certain path. Without a need or quasi-need the execution habit does not lead to action.

After an interruption due to the first World War, a systematic attempt was made to test the positive assumption growing out of

this criticism of the law of association. The first step was an attempt to achieve a more precise conceptual analysis. Dynamically, an "association" is something like a link in a chain, i. e., a pattern of restraining forces without intrinsic tendency to create a change. On the other hand, the tendency to bring about action is basic to a need. This property of a need or quasi-need can be represented by co-ordinating it to a "system in tension." By taking this construct seriously and using certain operational definitions, particularly by correlating the "release of tension" to a "satisfaction of the need" (or the "reaching of the goal") and the "setting up of tension" to an "intention" or to a "need in a state of hunger," a great number of testable conclusions were made possible.

After these basic conclusions had been proved valid, mainly through the experiments of Zeigarnik (37c) and Ovsiankina (37f), the theory was expanded to include problems like psychological satiation; substitution on the reality and irreality level and in play situations, the measurement of substitute value, the level of aspiration, its shift after success and failure, the effect of distance from the goal upon the strength of psychological forces; in short, the pattern of goals and needs, their interrelation, and the ways of satisfying them, were studied. Today, a multitude of problems including personality and personality development, cognitive structure, social and cultural relations are being attacked with a set of related concepts.

If one looks through our publications in the order that they have been published during the last decade one will, I think, agree that the various theoretical assumptions and constructs have been developed rather slowly step by step. The assumptions were made rather tentatively at first and with a fair amount of hesitation. Only to the degree that more and more empirical facts could be brought together experimentally, the theory gained in firmness and more specific statements emerged.

This gradual elaboration based on empirical facts and a great variety of experiments holds true particularly for the mathematical aspect of the theory. The application of topological and vector concepts was first made in a way which left it open whether we had to deal merely with a pedagogical device or rather with a real scientific representation. Only to the extent that these conceptual tools proved to be valuable in formulating problems, and permitting derivations which could be tested experimentally did they become essential parts of the theory and of its dynamic constructs.

French's (23) criticisms of the *Principles of Topological Psychology* overlooks the fact that this first attempt at a systematic survey of the conceptual tools used in our research was not made till after many years of empirical work with them. What French says about the gradual growth of psychoanalytic concepts out of psychological facts can as well be said in regard to the use of topological and vector concepts in field theory. As a matter of fact, the feeling for the necessity of rather slow and careful theorization was the main reason which restrained us from using strict, so-called formalistic, derivations in those early experimental studies. That does not mean that I considered those derivations to be not fully stringent or that I did not esteem the value of a mathematical logical language which I had found very helpful when treating problems of comparative theory of science (74). However, it would have been premature to present certain ideas "more geometrico," i. e., by setting forth so-called formal definitions, assumptions and deductions without being able to do so in well-defined mathematical symbols, in the form of equations or similar representations of functional dependence. If one uses terms of everyday language such as "frustration," "need," "learning" without being able to co-ordinate mathematical entities to them, one might as well use the normal form of reasoning. To present statements employing amathematical constructs "more geometrico" suggests a degree of exactness of derivation which, I am afraid, can not generally be reached with those types of constructs. This holds true even when these conceptually rather vague constructs are operationally well defined. We will come back to this point later.

One can go even one step further. The dynamic constructs used for example in the study of Zeigarnik may be said to be already of that type which readily lends itself to a strict mathematical representation. However, we felt that it would be wiser to wait with the formalistic representation until these constructs would have proved more thoroughly to be empirically fruitful. Like French, I feel very strongly the necessity of keeping theories sufficiently plastic in earlier stages of a study. A too high degree of formalization is likely to endanger this plasticity.

In regard to the emphasis on empirical foundations and the closeness to facts, topological psychology can certainly not be considered the more speculative approach. The difference between it and psychoanalysis lies rather in (10, 46):

1. A new, higher level of aspiration of the first in regard to the conceptual side of the psychological constructs.

2. A greater readiness to face the logical consequences of a theory without explaining nonfitting cases as exceptions, or by resorting to a suitable combination of counteracting assumptions so that practically no event which might disprove the theory can be envisioned. Field theory recognizes only such theories to be of any positive value for which the possibility of disproving exists in principle. One should not be permitted to use in one part of psychology one type of concept and an "entirely new set of concepts" (French) in regard to a second group of psychological facts. Freud doubtless was very successful in changing the attitude of psychopathology in the direction of determinism. However, his own method does not go the whole way. That is particularly apparent in Freud's recently repeated (68) advice to the psychoanalysts: one should be satisfied to find one explanation for each case and should not be disturbed if in a second case, in spite of the same conditions, different behavior results. Determinism (lawfulness) as presupposed by field theory means that the same conditions always lead to the same effects; therefore, a theory has to be changed whenever different effects result from conditions which have to be called the "same" from the point of view of this theory. Freud's famous principle of "over-determinism" of psychological processes is actually a principle of incomplete determinism. His unsympathetic attitude toward experiments is but another symptom of this point of view.

3. Stricter requirements in regard to the empirical proof of a theory. On the whole, only experimental proof is accepted.

4. More attention given to the fundamental differences between historical and ahistorical questions.

Thus, topological and vector psychology, although firmly based on empirical data, nevertheless holds strongly that psychology has to take a decisive step in the direction of stricter conceptualization.

Psychology cannot try to explain everything with a single construct, such as association, instinct, or gestalt. A variety of constructs has to be used. These should be interrelated, however, in a logically precise manner. Moreover, every theoretical statement brought forth to explain certain empirical data should be carefully examined not only in the light of these data but in the light of the totality of empirical data and theoretical statements of psychology. In other words *ad hoc* theories should be avoided. Bringing to

gether the total field of psychology and doing that in a logically consistent manner might well be viewed as one of the basic purposes of our approach. The demand for a new level of precision in regard to the conceptual properties of the constructs, with a view to an ultimate strictly mathematical representation, is but a means to this end. On the other hand, it has been realized that without such mathematization the development of a consistent scientific psychology is impossible in the long run.

III

Occasionally criticisms have been made that the number of subjects in some of our experiments was not sufficiently large. It is probable that, in one or the other experiment, a greater number of cases would have added to the reliability; and, of course, additional confirmation is always desirable. But, where other investigators have repeated our experiments in a competent manner, our results have stood up very well on the whole. Besides, different types of confirmation are most desirable for different types of questions. For instance, if one wishes to find out how the frequency of resumption depends upon the point at which an activity has been interrupted one will have to use a relatively great number of cases to get reliable results, for the problem involved is how within one situation a gradual quantitative change of one factor changes another factor quantitatively. In such cases the problem of the exactness of measurement is paramount and therefore a great number of cases is important.

Take, on the other hand, such questions as whether the effect of an intention is that of a link (association) or the creation of a quasi-need (equivalent to a tension system). If the latter theory is correct, one should expect, e. g., a fair number of resumptions after interruption. The study of about one hundred interruptions by Ovsiankina (371) shows indeed 80 per cent of resumptions. There is some merit in trying another group of one hundred interruptions. If however, this group again shows about 80 per cent of resumption, one can follow two lines. Either one tries to determine the actual percentage of resumption as accurately as possible, or one is mainly interested in the question whether the effect of an intention can be adequately understood as the creation of a tension system. For the latter question it is at present of minor importance whether the percentage of resumption is 75, 80, or 85 per cent, because any of these figures would be in line with the general as-