Introduction to experimental psychology

Many years of teaching experience at the Bachelor's and Master's level gives me reason to draw the following conclusion. Every year psychological education gradually loses its empirical and research orientation. Pseudoscientific trainings, therapeutic techniques, rankings, etc. are being introduced into educational practice. They displace the research training of students, turning scientific and psychological education into a kind of training and therapeutic centers. The social space is gradually filled not with professional psychologists, but with illiterate "coaches," "cockroaches," "consultants," "coaches. As a result, the quality of training of student psychologists is such that few graduates are able to independently plan and practically carry out research as part of the graduation (dissertation) work. In this joyless situation, it is necessary to put the teaching of such educational disciplines as "Experimental Psychology" on a proper level. I hope that the proposed textbook will help at least a little to those students who want to receive quality research training.

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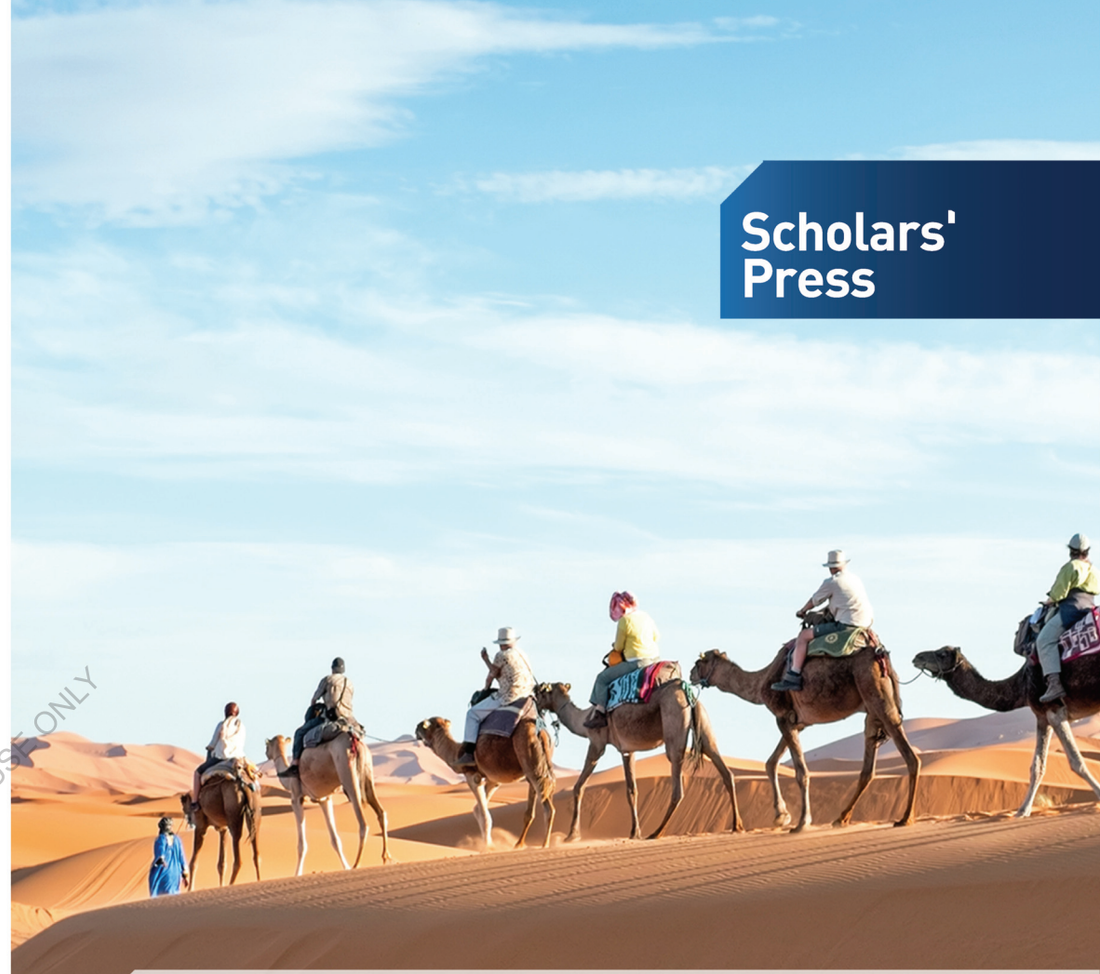
Experiment in psychology

Ageyev



Scholars' Press

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Training manual for Master's students

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Valentin Ageev

INTRODUCTION TO EXPERIMENTAL PSYCHOLOGY

Ageev V.V.

Introduction to experimental psychology. A training manual for graduate students.

The manual is intended for studying features of the organization of psychological experiment as one of the main methods of learning psychological reality.

The following main topics are considered in the textbook: logic of psychological research; theoretical foundations of psychological experiment; planning of psychological experiment. Problems of validity of psychological experiment, substantial and formal planning; possible sources of artifacts are considered.

PLEASE NOTE

The purpose of the manual "Introduction to Experimental Psychology" is to form a research culture of psychologists-magistrates as specialists capable of independently studying the human and animal psyche under conditions of psychological experiment.

The objectives of the training manual:

1. Introduce students to the principles and internal logic of psychological research.

2. To form in students knowledge of theoretical bases of psychological experiment.

3. Ensure that students learn how to plan psychological experiments.

4. To form a system of skills to control the validity of the results of psychological experiment.

5. Develop research skills to control false leads.

Introduction

In modern scientific psychology, the problem of psychological empirical research takes almost the first place. The presence of two alternative approaches (paradigms) to the theory and practice of psychological research [natural science and cultural] indicates that so far there has been no general solution to this problem.

The General Psychology course has a section that outlines the main methods of psychological research. However, the time allocated for this purpose does not allow to acquaint students with the theory and practice of psychological experiment in a necessary measure. It, in turn, does not promote formation in students of the own world outlook and research position on the nature of the psyche and methods of its study.

In our opinion, this textbook to some extent eliminates this gap and allows students to master not only the basic psychological methods, but also to get acquainted in sufficient detail with psychological experiment as a fundamental method of research of the mental sphere.

The textbook can be useful in the study of experimental psychology by students of both psychological and non-psychological specialties.

Part 1.

BASIC PRINCIPLES AND INTERNAL LOGIC OF PSYCHOLOGICAL RESEARCH

1.1. SUBJECT SPECIFICITY OF EMPIRICAL PSYCHOLOGICAL RESEARCH

1.1.1 Logical prerequisites for describing the psychological study

The most general basis of logic of the natural science description of psychological research could be considered a system of natural science concepts. As the subject of natural-science psychological research is behavior, the logic of interaction, or the logic description of behavior, can become the language of the description of the psychological research itself.

In the domestic psychological tradition (Rubinstein S.L., Ponomarev Ya.A., Brushlinsky A.V., etc.) the ***environment, system*** and ***interaction of*** environment and system are used as basic concepts. From this point of view, the relations ***"man and the world"***, ***"individual and environment"***, "***active subject - environment"***, ***"person - situation"*** are the concretization of the general ***relationship of interaction between the*** system and environment.

In personality psychology, this opposition is considered within the framework of ***personality and environment relations***. This tradition is primarily related to the name of K. Levin.

However, with the exception of the works of K. Levin himself, there were practically no attempts in psychology to develop a logical method of external description of behavior. In social psychology, the problem of behavioral logic (logic of action) was addressed by T. Parsons. However, his theory of social action is not sufficiently formalized. And this should be one of the necessary conditions for its possible comparison with other models.

Thus, the basic concepts used by the creators of various versions of logical description of behavior are: ***system*** (*agent*, *subject, etc.*), ***environment*** (*world*, *environment, many objects*, etc.), ***action*** (*operation*, *behavior*, *act, etc.*) and ***interaction***.

The system is recognized as initially active. Its basic concepts are ***"state"*** and ***"time"***.

When developing a model of logical description of observed human behavior in the environment, it is necessary to observe the following conditions.

1. Be guided by the ***principle of reality***, i.e. distinguish observable and non-observable variables from their relationships. Build logic based on the primary ***principle of observability***.

2. To take into account both the change of ***world states and*** the variability of ***states of the subject*** (human, system, etc.). Provide descriptions of ***interaction between the*** system and environment, not only the ***impact of the*** system on the environment.

3. Identify two forms of behavior: directed ***at the environment*** (*performing action*, *transformation, etc.*) and characterized only by the ***change of space-time states of the*** system (*locomotion*, *search activity*).

4. To provide for the possibility of two variants of behaviour description: ***active appropriate*** behaviour and ***reactive reflexive*** behaviour. According to this, there are two types of behavioral explanation: ***teleological*** and ***causal behavior***.

1.1.2 Psychological prerequisites for describing the psychological study

In natural science psychology, the logical analysis of action is linked to the traditions of ***behaviorism*** and ***neo-behaviorism***. The scheme ***"stimulus-response" in no*** way can claim to be similar to the models of interaction between ***system*** and ***environment***. In the non-gevioristic scheme, the medium is put out of brackets (the stimulus can be interpreted as both an impact and an element of the medium), while the system possessing the psyche is represented by its internal (mental) state and external manifestation - the reaction, which itself needs to be deciphered.

The neobehavioristic model is a ***"causal"*** model that provides for one type of event determinacy: ***past - present***.

N.A.Bernstein also noted that two models of the world are formed in the psyche: "the past is the present" and "the future". It should be kept in mind that causal

and teleological models of mental reality, mental images of the world and models of behaviour explanation are alternative, not just complementary.

Central to natural-science psychology is the postulate of direct non-observability of the psyche, because the psyche is always understood as the psyche belonging to another, as the psyche of the object of study.

From this it follows that if the psyche as a phenomenon is not observed directly, if we refuse the introspective method of cognition of mental phenomena and do not recognize its objectivity, then we must find him some replacement. And we find this replacement in the fact that we begin to study the objective observable reality. Such study is based on well-known methodological principles: unity of consciousness and activity, unity of mentality and behavior, etc.

In natural science psychology the psyche has no ontological status, but is a gnoseological explanatory principle. It means that psychology studies the interaction with the environment of such systems, for the explanation of whose behavior the psyche is a necessary means of explanation. Behind all the phenomenology of natural science psychology as an empirical science lies a fundamental postulate:

if the body ***acts***, i.e. behaves ***differently*** than a normal physical body, then there is "something" that makes it act differently. This "something" that makes it act differently is the psychic reality.

The analysis of the difference between the motion of a living body and that of a non-living body is contained in the work of V.V.Davydov and V.P.Zinchenko. "... Movement is a property of a thinking body. So, our task is to thoroughly investigate the way such a body operates, as opposed to a nonthinking body. The cardinal difference lies in the ability of the thinking body to actively build a trajectory of its movement in space, according to the form of the trajectory of any other body ... ". (V.V. Davydov, V.P. Zinchenko).

The authors see the specificity of the motion of a living body in the fact that "... for a creature with a psyche is characterized by a search that has an inner inconsistency ...". (V.V. Davydov, V.P. Zinchenko). The man anticipates the future, builds an image of the future that determines the present, determines the actual real behavior. The specificity of human action is that the active movement has not only a performing but also a research function.

The definition of "psyche" as an explanatory principle used by natural psychologists makes it possible to reveal the reasons for the multiplicity of types

of explanations, which was pointed out by J. Piaget. He sees the main reason for multiplicity of forms of explanation in the variety of "models". But the very variety of models is a consequence of the fact that the researcher has an opportunity to choose any variant of description of mental reality, and his subjective arbitrariness is objectively not limited by anything. Because it is believed that the psyche of the other is an unknown quantity. And the state of psychic reality cannot be registered either directly or indirectly.

Mental reality models used in psychological research can be as complex as you like, but ***there is*** always an ***explanation for extrapolation to the process of investigating the internal logical structure of the subject***.

Psyche as an element of the system whose behavior is recorded is always taken in some respects to the system and environment. Psyche reflects the conditions surrounding the body, and therefore is a regulator of movements and actions. ***Reflective*** and ***regulating*** functions of the psyche are basic.

If the psyche did not perform the functions of environmental reflection and regulation of behavior, it would be simply unnecessary. If behavior did not include these functions in a necessary way, it would not be adequate to the environment. Hence, it is necessary to consider the behavioral act and mental processes included in it as a unified system.

The allocation of a special communication function is based on the role of communication as a form of behavioral activity of people. However, this function is implemented in the course of knowledge exchange with mutual regulation of human behaviour. Thus, it can be "divided" into cognitive and regulatory functions of the human psyche in interaction with another person.

Thus, the psyche in a natural science psychological study is an ***explanatory principle*** used to explain the peculiarities of movement (*behavior*, *action*, *activity*, etc.) of some living systems that differ from the movement of inanimate, physical systems.

Psyche is unknown, and models taken from any other area of human knowledge can be used to describe it. A natural psychologist studies the psyche in its relations with the natural environment (reflection), in its relations with the system (regulation of behavior) and in its relations with the social environment - with another person (communication).

The heuristicity of mental models is determined by the individual erudition of each researcher-psychologist in other areas of human knowledge and by the

inclusion in the research psychological activity of specialists who are not psychologists. The adequacy of mental models is determined by the psychological intuition of the authors of the model itself and the depth of their penetration into the specifics of the psychological method.

Questions for discussion

1. Kurt Levin as a natural science experiment theorist.

2. Concepts as means of logical description of behavior.

3. Conditions for creating a logical description of behavior.

4. The causal nature of the psychological experiment.

5. The fundamental postulate of natural science psychology.

6. The reflective-regulating nature of the psyche.

1.1.3. Structure of a natural science psychological study

A psychologist who investigates psychic reality by the natural science method, i.e. considering the psyche as an ***integral part of the objective reality***, should also identify the psychic carrier. Such carrier can be an ***individual***, ***an individual***, a ***contact group***, a ***social community***. Further structuring of objective reality can be carried out on the basis of the criterion of interaction. The part of objective reality that directly interacts with the selected system (the *object possessing the psyche*) is defined as the *environment,* while the rest of the system interacting indirectly through the environment is usually not considered.

Thus, the researcher initially works with two components: the system and its environment, and the relationship between them. This relationship is defined as ***interaction*** and includes the relationship of the system to the environment and the environment to the system. The relationship of interaction is the basic material for any natural science psychological analysis.

The attitude of the system to the environment is defined as an ***impact***, an ***action***, ***an act, the*** features of which are determined by mental reality. The effects of the environment on the system have an extra-psychic determinant, excluding the

case when the nature of the environment is identical to the nature of the system ("subject-to-subject" interaction - communication).

With its effects, the system (human) makes changes in the environment and the - environment in the system. Therefore, firstly, it is convenient to consider interaction of system and environment in time. Thus, interaction is reduced to an exchange of the influences, not divided in time. Secondly, it is convenient to introduce the concepts of "system state" and "environment state" and to consider the interaction process as a sequential change of system and environment states.

Since we record changes in both the environment and the system, the transition of the system from one state to another and the transition of the environment from one state to another is due to the impact of the system on the environment or environment on the system rather than any third cause. If this third cause does occur, then we must include the source of the change either in the system or in the environment.

By marking the transitions between states by the symbol ***R***, we obtain the classic behavioral scheme "stimulus-response": ***S - R.*** By including ***S - R in*** the scheme as a component of ***O,*** we obtain the known scheme of neogeviorism: ***S - O - R*** (stimulus - organism - reaction).

In any case, in natural-science psychological research we remain within the framework of positivistic description of reality, as the basic dogma of natural-science psychology is that the ***influence of environment on the system is mediated by psyche ψ.*** At least in psychology only such influences which lead to mental changes are considered. On the other hand, in changes of the state of the system and, consequently, in influences on the environment, the psychologist is interested only in the extent to which these influences are determined by the psyche. Separating the psyche as an ***independent reality*** allows us to speak about psychology as an independent science.

1.1.4. Types of natural science psychological research

When organizing a psychological study, the researcher intervenes in the ***natural process***. Any empirical study is analytical, as we always have to distinguish some aspects of the ***natural process*** by abstracting from others.

In a natural-scientific psychological study by its nature, the holistic psyche is not considered, but its subsystem, property, etc. is considered. In this case, not the

whole environment is observed or analyzed, but only a part of it. Accordingly, in the system we take some of its observed manifestations and register the peculiarities of its internal state with the help of devices.

All of the above applies to both states and relationships that are realized over time. Since the psychic reality of the person under study in a classical natural-scientific psychological experiment can only be "constructed" in terms of the theory to which the experimenter adheres, as far as psychological research is built as ***verification of the*** researcher's ***assumptions*** about the psyche of the subjects.

The psychological study can be reduced to a simulation study. As a model, an object is used whose behavior in some way is similar to that of a system possessing a psyche. Thus, the researcher applies the simplest analogy: ***similarity of behavior of*** systems testifies to ***similarity of*** their ***internal features***.

Two main variants of models are possible: "causal" and "teleological" and, accordingly, two plans of psychological research construction.

Obviously, the theory (model) of the studied psychic reality is not enough for research planning. A researcher, at least, should have a model of the system (in our case - a person), a model of the environment and the set of relations on the set of the ***psyche - system - environment***.

The psychologist may choose to vary objects and environments or observe them selectively, as well as vary or record the time of the experience.

1.1.5. Fact and artifact in psychological study

For natural-science psychology, the basic concept is ***"behavior"***. It is behavior that is the "material" with which the researcher-psychologist works. The distinction between ***material***, ***subject,*** and ***object*** is an important distinction of psychology from other sciences, where the concept of 'material' is not used at all. The psychologist, on the other hand, can only judge the psychic reality of another person through the analysis of the material. Another thing is that the concept of "material" should include not only behavior, but also its results. The result is a change in the state of the environment under the influence of the system. And influence (behavior) is such an observed change of the state of the system, which leads to a change in the observed state of the environment.

From here it is possible to pass to the characteristic for natural-science psychology ***causal*** neohevioristic scheme of ***stimulus - intermediate variable - reaction*** or to the characteristic for cultural psychology ***teleological*** scheme of ***purpose - action - result***.

This manual will only use the ***causal model*** characteristic of natural science psychology. Here, the subject is affected by both stimulus and background conditions at the same time, which gives rise to problems of controllability of experimental variables and ecological validity typical for psychology. In other words, the problem of transferring laboratory research data into the natural environment. If the background affects the psyche more intensively than the stimulus, the result of the study is ***not a fact,*** but ***an artifact***.

From here, all sources of artifacts for psychological experiment can be divided into two main types: the ***inner states of the*** subject and ***the state of the environment***. Donald Campbell also identifies background artifacts: background effects, instrumental error, reaction of the subject to the experiment, etc.

If the registration and analysis of behavior is the subject of the causal approach, then the study of the products of activity is the subject of the bodyologic approach. Indeed, using the teleological approach in psychological research, we take the result of human activity (of the subject) as the final point of the process and then try to interpret the result from the point of view of those internal mental reasons (intentions, plans, motives, goals, abilities of the subject) that could potentially influence the behavior and, consequently, the peculiarities of the result.

In this case, in addition to not taking into account internal tasks, there is a danger of "looking through" the subject. That is, there are always changes in the environment not considered by the experimenter and made by the subject. In addition, there are changes in the environment that the subject does not realize, but which are also a consequence of his or her activity.

Obviously, even in the case of cooperation between the subject and the experimenter, it is theoretically possible that unconscious results of the subject's activity may appear. The same applies to the activities of a researcher to control experimental variables.

Thus, the source of artifacts in a psychological experiment can be both the environment and the subject (and the experimenter as the background of the environment).

In addition, the source of artifacts may be the inadequacy of the psychological theory used - an experimental procedure. The researcher can look through, not take into account the mental variables that affect the behavior of the system (-person).

A number of side effects may not be noticed by the psychologist (look through the facts) due to incomplete consideration of changes in the environment that the subject has made. In addition, the effect of "natural" development should be taken into account. The product of the subject's activity can be taken as "natural" changes in the environment.

D. Campbell provides a list of artifacts typical of experimental psychological research.

1. ***background*** - specific events that occur between the first and second dimensions along with experimental effects;

2. ***natural development*** - changes in subjects that are the result of time flow (not related to specific events), for example, growing up, increased hunger, fatigue, etc.;

3. ***Test effect*** - *the* effect of performing measurement tasks on the results of the retest;

4. ***Instrument error, instability of the measuring instrument*** in which changes in the calibration of the instrument or changes in the characterization of the observer or evaluation indicators may cause changes in the measurement results;

5. ***statistical regression*** that occurs when groups are selected on the basis of marginal indicators and scores;

6. ***selection of subjects*** - non-equivalence of groups by composition, causing a systematic error in the results;

7. ***sifting during the experiment*** - non-uniformity of test subjects dropping out of compared groups;

8. ***interactions of the selection factor with natural development***, etc., which in some quasi-experimental plans with several groups are mistaken for the effect of an experimental variable.

Factors that jeopardize the external validity or representativity of the experiment are:

9. ***reactive effect, or test interaction effect***, is a possible reduction or increase in the sensibility, or susceptibility, of subjects to experimental exposure under the influence of preliminary testing. The results of pre-test subjects will not be representative of those who have not been pre-tested, i.e. the general population from which the subjects were selected;

10. effects of ***interaction between the selection factor and experimental impact;***

11. ***conditions of the organization of the experiment, causing the reaction of the subjects to the experiment,*** which does not allow to spread the obtained data on the influence of the experimental variable on the persons exposed to the same influence under non-experimental conditions;

12. ***Mutual interference of experimental influences***, often occurring when the same subjects are exposed to several influences, as the influence of earlier influences tends not to disappear. This is particularly true for plans for single-group experiments.

Questions for discussion:

1. Psyche as part of objective reality.
2. Exposure as a basic concept of natural psychology.
3. ***S-R*** as the basic scheme of experimental psychology. ψ
4. Psychological research as testing of hypotheses.
5. A fact and an artifact of psychological research.
6. The most typical artifacts.

1.1.6. Main characteristics of empirical psychological research

Psychological research can be considered a system. As elements of research system it is possible to allocate: object, subject, method, conditions (otherwise - environment) and result. Under result in our case is understood either behavior or product of activity, i.e. change of environment condition.

Basic ontological principles of psychological research:

1. ***The principle of representativity*** determines the relationship of the object with the object, conditions, method and result. The object should be selected in accordance with the research objective.

2. ***The principle of validity*** characterizes the relations of the subject with elements of the research system. The subject matter of the research should not be substituted during the research.

3. ***The principle of reliability*** characterizes the relations of the method to other elements of the system and provides invariance of the result obtained by this method.

4. ***The principle of standardization of*** conditions.

5. ***The principle of*** result ***invariance*** is ensured by applying the above principlej and implies reproducibility of this result in other studies and comparability with results obtained by other researchers.

A simpler interpretation is also possible. The fact is that the principles reflect the correspondence of the researcher's idea to the real system. Therefore, principles can be regarded as reflexive relations between an object, subject, method, conditions (environment) and the result.

Consequently, the ***right choice of*** object is reflected in the ***principle of representativeness***. The ***correspondence of a subject***, theoretically singled out by a researcher, actually studied is expressed in the ***principle of validity***. ***Correctness*** (invariance) of the ***method*** choice is expressed in the principle of ***reliability***. The ***principle of reproducibility of the*** result is an expression of correctness of realization of all listed principles. The only reservation can be attributed to the principle of standardization of conditions.

Most likely, the ***correspondence of*** real research conditions to the ideally assumed should be characterized as the ***external validity of the*** research. Linked to this principle is the historical discussion about the possibility of applying a laboratory experiment in psychological research. Ideally, the researcher believes that the conditions of the experiment should correspond to real life conditions or sufficiently model environmental factors essential for the subject of study. In fact, the environment is simplified, made "artificial", and specific experimental "interference" is introduced (tests, devices, communication with the experimenter). This generates the problem of transferability of results obtained in the experimental situation to the life situation.

This problem requires special discussion, but standardization of conditions is the way to solve the problem of external validity of experimental research in psychology.

Basic gnoseological principles ofresearch:

1. the principle of recording facts;
2. the principle of factor planning;
3. the defect control principle;
4. the principle of elimination of artifacts;
5. the principle of outcome evaluation.

Questions for discussion:

1. Elements of a psychological research system.
2. Ontological principles of psychological research.
3. Gnoseological principles of psychological research.
4. The principle of reliability of psychological research.
5. The external validity of the psychological study.
6. The principle of representativity.
7. The principle of replicability.

1.2. SUBJECT-OBJECT APPROACH TO EMPIRICAL PSYCHOLOGICAL RESEARCH

1.2.1 The object specificity of empirical psychological research

Mentality is a special object of cognition *(object in the broad sense)*, so the logic of empirical natural-science psychological research is different from the logic of empirical research in the natural sciences. But psychology is specific not only in

its cognition object, but also in the carrier of the psyche objectifying it in its behavior *(object in the narrow sense of the word)*. As carriers of psyche (objects in the narrow sense of the word) can act a living individual, a person as an individual, a group of people, a community. If we draw adividing line between a human being and the rest of the animal world and continue to speak only about the human psyche, we can consider that the concept ***"object of psychological research"*** by its essential qualities will in many respects be identical to the concept ***"subject of psychological research"***.

In both cases, a person with a psyche knows and acts. And when a person cognizes the psyche of another person, it is possible to change positions and, accordingly, the person whose psyche is being investigated can change roles with the person who studies the psyche, and vice versa.

The problem of subjectivity of psychological research is not new. It is often seen as specific to observation, measurement and experiment.

Psychology has long been faced with the task of theoretical consideration of a psychological experiment and solving the problem of its description. This problem is a consequence of two main contradictions of psychological experiment: the ***contradiction*** between the experimentalist's task to investigate the subject's psyche as an object *(in the narrow sense)* and the impossibility to solve this task without including the subject as an experimental subject; and also the ***contradiction*** between the task to investigate the subject's subjective reality as objective and the impossibility to measure it directly due to its subjectivity.

Although the experiment in psychology was applied before W.Wundert, it is known that it was W.Wundert who gave the theoretical substantiation of its application. He assumed that the experimental method was introduced only into physiological psychology, since physiology is an auxiliary science to psychology, as physics is to physiology itself. Thus, physiology provides psychology with ***an experimental method*** that is applied and developed according to purely psychological purposes. Thus, W. Wundert has already oriented psychology to the methodological paradigm of natural sciences.

However, V.Wundtom considered the experimental procedure in theory as a ***system of influences on the experimentalist,*** as which the subject acted, to control the process of his introspection. The assistant, who was engaged in organization of these influences, was not considered in the normative model of the experiment. Such a fusion of a subject and an object of psychological research contradicted the principles of classical natural science and was rejected

by behaviorists in its time. They consistently carried out the natural science program in psychology, considering the experiment as a system of hardware influences on the object, carried out by the experimenter in order to cognize the properties of the object.

The experimental procedure was considered from the standpoint of the experimenter, and the natural-science experimental model was transferred to psychology without changes. W. Wundert believed that scientific psychology should seek to include in the sphere of science the cognition of man and animal, considered as part of nature.

But the human experiment, unlike the animal experiment, includes in its structure the ***instruction to*** the test subject. Its meaning was revealed in the first psychological experiments. The study of peculiarities of perception of the instruction by the subjects was started by psychologists of Würzburg school. But if the subject is able to transform a normative problem, then he or she is no longer reactive, but active in an experimental situation. The problem of considering this activity required a constructive solution.

For the first time, L.S. Vygotsky made an attempt to analyze the psychological experiment in terms of the subject's activity as an experimental subject. He noted that before that all psychological methods had been constructed according to one scheme: ***stimulus - reaction***. This scheme dates back to V.Vundt.

The normative structure of the modern L.S. Vygotsky experiment differed from the schemes of the first experiments only in understanding and using its components, not in structure. L.S.Vygotsky pointed out that the scheme "stimulus-response" considers a subject's psyche as ***reactive***, and reactivity is characteristic only for lower mental functions. He considered activity as property of the higher mental functions, therefore emphasized that the experiment under the scheme "stimulus-response" is identical to experiment in natural sciences and is adequate only for research of the lower mental functions. In other cases, the so-called ***instrumental method*** should be applied, providing for the subject's active intervention in the situation, his or her active role, active - behavior consisting in introduction of new stimuli (signs).

As L.S.Vygotsky himself noted, the composition of the instrumental act is similar to the structure of the labor act. The model of L.S.Vygotsky described the normative structure of the psychological experiment, but did not consider the process of its transformation into reality and did not reveal the causes of the subject's activity in the experiment to transform the experimental situation.

To solve the problem of the subject's active role in the experiment, an ***individual-active approach*** was called for, at which the subject is used as the beginning of coordinates of the description of the psychological experiment.

The methodological basis of the psychological experiment is the concept of ***- activity*** developed in Soviet psychology and identification of its main characteristics. Based on the approach, considering activity mainly as an ***individual subject activity, it is*** possible to give the following definition of psychological experiment: ***experiment is activity of the person being tested, directed at performance of the task***.

There are two forms of specific human activity in the experiment: ***activity*** and ***communication***. Therefore, the previous definition can be supplemented by the following provision: ***any psychological experiment can be considered as communication between an experimenter and a subject with a known background in an experimental task realized in the form of an experiment***. The purpose of experimental communication is to construct a certain activity of the subject, to carry out activity in accordance with the norm of the experiment, to obtain the product of activity in the form of an experimental result and to interpret this result.

Thus, the scheme is proposed: ***communication between the experimenter and the subject - individual activity of the subject***. In this description, there is no activity of the experimenter to organize the experiment. Individual activities of the subject and the experimenter and communication between them are not considered as components of joint activity on research of the subject's psyche.

With this approach, an opportunity arises to define a psychological experiment normatively as a task of joint activity of the experimenter and the subject, and procedurally as a process of joint activity on research of the subject's psyche. From these positions it is possible to consider the problem of influence of the experimenter on the subject and on acceptance of the task by the subject.

Consistent carrying out of the individual-activity approach in solving the problem of acceptance of an experimental task by a subject and its adequate realization supposes that the main determinant of these processes is the subject's ***motivation,*** as it is motivation that is a variable that determines individual activity. However, this approach allows only taking into account the influence of motivation ***after the fact,*** but not its management. This happens because setting the control task requires taking into account the peculiarities of interaction

between the subject and the experimenter, i.e., going beyond the subject's activity in the description of the experiment.

Besides, the task of management implies consideration of this interaction not as a source of artifacts, but as a necessary condition for obtaining adequate knowledge of the subject's mental features. Similarly, the problem of inclusion of the subject into the experiment can be solved only from the point of view of the ***social-psychological approach***, considering a potential subject and the experimenter as a small group.

Thus, it is possible to note one-sidedness of the activity approach to the decision of a problem of psychological experiment, and also impossibility to solve with its help the problems facing the methodologist.

The social-psychological approach to the psychological experiment shifts the emphasis when describing its procedure to the interaction of the subject and the experimenter. In researches of social psychologists the influence of various factors defined by interaction of the subject and the experimenter on the results of psychological experiment has been revealed: the personality of the experimenter, rumors about experiments, anticipating the evaluation of the subject, affiliative motivation, etc.

These effects can be divided into two groups: effects caused by the ***situation*** (selection, voluntary research, expertise, etc.) and those related to the ***personality properties of the*** subject and the experimenter. The latter includes, in particular, the so-called "Pygmalion effect".

The extreme expression of the absolutization of "interaction effects" is the assertion that the data from the laboratory experiment are more related to the motives and feelings of the subjects regarding their role in the laboratory than to their life outside it.

An understanding of the subject as a material being and recognition of an important role in cognition of the subject's material activity is necessary, but not enough in itself. A scientific understanding of the cognitive relationship presupposes the consistent realization of the point of view of the unity of reflection and activity. But this, in turn, is only possible if the subject and his activities are understood in their socio-cultural and historical condition. If it is recognized that the subject's subject-practical and cognitive activity is mediated by the subject's attitude to other subjects.

From here it is possible to draw a conclusion that the interaction of the subject and the experimenter in solving the experimental problem is a basic model of the subject's life outside the laboratory. And experiments with the isolation of the subject, "deception techniques" are arbitrary models of private life situations. The foundations of such an approach to the experiment in psychology are laid by S.L. Rubinstein: "Since an experiment in its very essence always involves direct or indirect influence of the experimenter, the question is not so much to eliminate its influence, but to take it into account and organize it correctly" (S.L. Rubinstein).

S.L.Rubinstein emphasized that in order for the subjects to accept the experimental problem, the experimenter must move with the subject to the position of the participant of joint activity aimed at solving a common life problem that goes beyond the experimental situation. Otherwise, the subject will transform the normative problem based on personal motivation unknown to the researcher (S.L. Rubinstein). It should be noted that there is one exception to this general rule when the subject is motivated by self-knowledge and is directly interested in the truth of the research data.

As a whole, it is possible to draw a conclusion that the model of psychological experiment is the most reasonable, considering it as a ***system of joint activity of the*** experimenter and the subject, included in social activity, having external purposes in relation to research, having the direct purpose of cognition of features of mentality of the subject.

The experimenter undertakes the tasks of organization and management of the joint activity, while part of the executive task defined in the instruction is taken over by the test person. In this definition of the procedure of the experiment, the beginning of the description coordinates is taken out of the limits of the experimental situation, which makes it possible to present the whole experiment.

It also follows from this that the central place in the interpretation of the data of psychological research is occupied by the consideration of the influence on them of the integral system of joint activity of the subject and the experimenter.

Questions for discussion:

1. The problem of subjectivity in psychological research.
2. Experiment as an impact on the experimenter.

3. An instrumental method in psychology.

4. Individual-activity model of the experiment.

5. Social-psychological model of the experiment.

6. Experiment as a joint activity.

1.2.2 Communication between researcher and subject; role of instruction

Despite the fact that, according to some psychologists, the experiment as a way to obtain scientific data, psychology borrowed from natural sciences, in the ***very beginning the*** psychological experiment was significantly different from the natural science experiment in physics, chemistry, biology and physiology. The experiment in psychology proved to be essentially psychological from the very beginning. From natural sciences only the idea of experimenting as the directed control and measurement of variables in investigated object and in its interaction with environment was brought. These variables themselves had very different nature: external, object, and internal, subjective.

Contrary to popular belief, the ***experiment*** as a method ***was not borrowed by*** psychology from natural sciences. The ***idea of experimentation*** was borrowed. From the very first steps, the psychological experiment was formed entirely ***independently***. A unique feature and fundamental characteristic of the psychological experiment was that for the first time in the structure of an experimental, experimental method, ***instruction to*** the subject appeared.

The instruction sets a task for the test subject, who must understand and accept it. This means that there is no organized ***communication*** between the researcher and the subject in any natural science. But the instruction is not always an explicit part of the experiment. In an experiment with children, the instruction is reduced and is either included in the general context of the experimenter's communication with the child, or is an integral part of the task, or absent at all (the experiment on children from 0 to 2 years of age). The same applies to the medical-psychological experiment.

Finally, there may be a situation where the subject does not know that a psychological experiment is being conducted. Of course, there is an ethical problem here, but sometimes (in forensic psychological practice, in child,

medical, social psychology, etc.) this is the only possible way to conduct research and get rid of the "reaction to the experiment" of the subjects.

In this case, communication between the subject and the experimenter, in whatever form it takes, is an integral part of any experiment in psychology.

Instructions are not identical to the subject's task, the last one is always there, even in the absence of instructions. Moreover, communication between the subject and the researcher, even in a laboratory experiment, does not come down to instructions.

In psychological experiment the ***purpose of the experimenter*** is ***revealing of the*** investigated psychological phenomenon and its regularities, and for the test subject it is a new ***problem situation***. Thus, the subject performs not only that task which is set by the instruction, but also solves a concrete personal task in a concrete situation. Therefore, even the simplest model of psychological laboratory experiment, where two people participate simultaneously and the form of their communication is instruction, is a complex system. In it, it is necessary to distinguish between actions performed by the subjects according to the instruction and actions that are conditioned by their personal characteristics.

In addition to instructions, the behavior of the subject in the experiment is influenced by different types of ***installations*** that arise in the experiment based on objective conditions and interaction between two people. These attitudes are not understood, but they change the nature of the study.

The problem of instruction to the test subject and the need for it in the study is not trivial. An experimental task, fixed in the instruction and accepted by the subject, changes the course of mental processes. Since distinctions between the content of the concepts ***"task of the subject"***, "***research situation", "instruction to the subject"***, "***communication of the researcher and the subject" have*** been introduced, it is reasonable to introduce some relations between such components.

The structure of the experimental task should correspond to the structural description of the subject's activity in the situation of the experiment, and the structure of instructions, in its turn, should correspond to the structure of the task. Possible deviations generate artifacts that can be called task artifacts and instruction artifacts.

The task for the test person is to translate into natural language the model of psychic regulation of behavior adopted in psychic science. In different types of

experimental activity, the instruction has different forms. It may be a game rule, an educational task, a work task, etc.

Different authors define the specifics of the psychological experiment in different ways. For example, it is necessary to keep in mind that in different scientific disciplines the experiment has its own specific features, which is primarily due to the nature of the object and the subject of study.

First, individual uniqueness, a unit of the human psyche acting as an object of psychological research. In psychological research, the identity of psychic carriers is, strictly speaking, conditional. The studied psychic carriers (individuals, groups), even being similar by sex or age, professional belonging or psychological and socio-psychological characteristics, differ in a number of other, not less essential features. Completely identical people with absolutely identical psyche do not exist and cannot be. And this, in turn, complicates the comparative analysis, evaluation and interpretation of experimental material.

Secondly, one should bear in mind the high dynamics, instability of mental phenomena. Even one and the same person manifests himself or herself in a completely different way and is characterized by specific mental states in different specific circumstances, at different stages of his or her life path, in different periods of activity. In extreme situations, a person behaves in a different way than in normal conditions usual for him or her. In laboratory studies, the psychic manifestations of an individual differ from corresponding manifestations in natural conditions.

The human psyche acting as the subject of psychological research is at the same time a regulating factor of behavior of the person acting as the subject of psychological research. He or she reacts actively to all influences and interacts with the experimenter in a certain way, evaluating him or her and the experimental situation as a whole. It means that the phenomena under study in a psychological experiment can be determined not only by personal features, but also by the influence of influences arising directly in the experimental conditions. This, in turn, may lead to distortion of the results obtained in the experiment.

Questions for discussion:

1. Feature of a natural experiment in psychology.
2. Instruction as a means of organizing communication.

3. Experiment as a problem situation for the subject.

4. Psychological structure of the experimental problem.

5. Individual uniqueness and experiment.

6. Instability of the psyche under experimental conditions.

1.2.3 Specifics of the empirical psychological method

In considering the ***subject matter of the subject***, researchers often put two components into the subject content of this concept:

1. the notion of ***an "agent of influence."*** Absolutely active and independent of the environment in its activity, the system itself capable of influencing the environment with the expected effect.

2. the concept of ***"subject of knowledge."*** A cognizant being, opposite to the cognizable object.

The introduction of the "subject of research" component, in turn, requires consideration of the subject of research as a dual subject-object entity. Thus, the research environment should be divided into two components: the ***object environment itself*** and ***the research subject***. It should be noted that the system under study has a dual subject-object nature, just like the researcher himself.

We will consider the environment as consisting of three components: the ***environment*** itself, the research ***tool***, ***the subject of the*** research. Accordingly, from the point of view of the subject, the subject of research, on the contrary, will be isolated from the environment, and the subject himself will be part of the experimental environment.

To define specificity of logic bases of psychological research which follow from the nature of its object, it is possible to compare those rules to which psychological research is subordinated, with those rules which characterize research in natural sciences, for example, in physics. As physical and quantum-mechanical metaphors are often used in psychology, let us take the model of ***classical physical*** and ***quantum-mechanical*** research for comparison.

Let us consider the ***ratio of observability***, namely the ratio ***"the subject observes the object"***.

Physics claims that ***the subject can observe the*** object, but ***the subject cannot observe the*** subject. Therefore, the consequence of the duality of the subject-object entity and the object of psychological research is that the statement ***"the object can observe the subject"*** is valid, since the ***subject*** in a natural science psychological research is ***equivalent to the object***.

Moreover, a number of other statements are fair in psychology. For example, ***"the subject observes the subject".*** This statement can be interpreted in two ways. First, as an equivalent form of statement, given the principle of subject-object duality. Second, in the case we are talking about the same thing rather than different subjects, we have an assertion describing the procedure of self-monitoring.

Let's now consider the ***ratio of determinacy*** in the pair subject - object.

In this case, the second component of the concept "subject" can be used - "agent of action". For a classical natural science experiment, the influence of the experimenter determines the change of the object state. But there is no ***direct impact of the subject*** on the object in the same way as an object on the subject of research. In any case, these influences are in no way taken into account when planning cognitive activity. In physical experiment ***any impact of the subject on the*** object is ***mediated by*** devices. Thus, it is fair to say that "the subject does ***not determine the object*** (subject)" and "the object does ***not determine the subject***".

This is not the case in psychology, as there may be ***direct interaction between*** two or more people during an experimental study.

The agent of influence in a psychological experiment can be ***not only the subject***, ***but also the object***. In general, determinism in psychology is different from determinism in physics. Let's look at the statements:

(a) ***Reasoning***: If object A is a determinant of an effect resulting in a new state of object B, the effect of object A precedes the resulting state of object B;

b) ***teleological explanation***: if the image of the resulting state of object B is a determinant of the action of subject A, then the image of the resulting state of object B precedes the action of subject A.

It can be seen from this that the classical physicist interpretation of the psychological study data is impossible. We use the already mentioned division of the research environment into the ***environment itself*** (conditions, situation, etc.), an ***instrument*** (device, test, etc.) and ***an experimenter*** (experimenters). Let

us consider possible relations on a set of elements: ***object***, ***subject***, ***instrument***, and ***psyche***. Conditions of research (environment in the narrow sense of the word) will not be considered yet.

We will distinguish between two types of instruments, or "instruments": ***measuring instruments*** and ***impact instruments***. Among the first are ammeter, ruler, psychological test. The second type can include an educational game, a simulator, a test bench, etc.

The measurement in psychology is different from the classical measurement. The classical version assumes that the state of the device is in no way influenced by the state of the subject. But the researcher, carrying out real testing, in each case falls back, willingly or unwillingly, from the standard. The researcher has to involve the subject in the examination, carry out a preliminary conversation, give instructions explaining the purpose of measurement, etc., inevitably introducing variations into the process of communication.

Moreover, when using projective tests, the researcher acts as a "part" of the measuring instrument during data interpretation. He or she identifies in the test person's answers signs that indicate certain properties of the test person's psyche, and thus "affects" the measuring instrument and its readings. Ideally, the psychological measurement should meet the requirements of "objectivity". That is, the subject's characteristics should not affect the measuring instrument and, consequently, the measurement result. In this case, psychological measurement does not differ from measurement in any other natural science. However, there is an essential difference in the relations between a subject of measurement and a device in physics and a device in psychology.

Properties of the psychological measurement object can be revealed only if the subject changes the state of the device with his or her influence.

At the same time, there is the following pattern: the higher the complexity of the mental system, whose properties we measure, the greater the change in the measuring instrument introduces the subject. In other words, the greater the tool reveals the properties of the subject, the greater changes it makes to the measuring instrument. Moreover, these changes are often irreversible.

Let's illustrate that pattern. The forms of the personality questionnaire are filled in by the subject and not passed on to another subject. The products of creative activity (when testing creativity) are unique, and the transformed material cannot be restored. But the device for measuring reaction time does not need to be repaired after the test person has worked on it.

In general, psychologists seek to ensure that the measuring instrument is "reusable" so that the test subject does not make irreversible changes in the state of the "instrument" (test). But in some cases, this cannot be avoided.

In general, in psychology ***both the object influences the*** tool and ***the tool influences the*** object. And this is ***the main difference between*** psychological dimension and classical physics. In principle, there is an impact of the instrument on the object of measurement, but it is minimized and its effect tends to zero. In psychology, this effect is impossible to achieve. Therefore in psychology, psychological impact and testing are practically indistinguishable. It means that the same techniques can be used for different purposes. For example, both as an intelligence test and as an educational game.

It shows the similarity of psychology to quantum physics. Namely, both the object and the measuring tool interact there, irreversibly changing their properties. It occurs because both the tool and the object of measurement are objects of one level of complexity and their behavior is subject to quantum mechanical laws.

But this is where the analogy ends, because in psychology the object of measurement is obviously more complicated than the measuring instrument.

If we use the intuitive word "complexity", we will get the following relations between the components of the system under consideration. In psychology: the ***complexity of the subject*** is equal to the complexity of the ***object***, the complexity of the ***subject is*** higher than the complexity of the ***instrument*** and, consequently, the ***complexity of the*** measurement subject is higher than the complexity of the ***instrument***. Moreover, the same pattern is repeated here: ***the higher the complexity of the*** psychological functional system, ***the lower the complexity of the*** measuring instrument. For example, specialists in personality psychology conduct interviews, use questionnaires, drawings, pictures, and simple materials (pencil, paper, plasticine, etc.). Professionals in the field of cognitive processes use more complex tools (computer tests, adapters, etc.). The most complicated equipment is that of psychophysiologists.

Accordingly, in classical natural science the ***subject is*** more complex than an ***object*** and a ***measuring device,*** and ***the object is*** either equal in complexity to an ***instrument*** (quantum mechanics), or more complex than an instrument (biology), or less complex than an ***instrument*** (mechanics).

But the most important difference is with respect to control, which is derived from a relationship of mutual influence.

In psychological research, ***the object controls the*** measuring instrument, not the other way around. For example, the subject performs manipulations with dice, solves a chess problem, etc. While in natural science, the ideal scenario is when a ***measuring instrument*** fully ***controls*** or records (when measuring) the "behavior" of an object.

An experiment is always a joint activity of the subject and the experimenter. And to the extent that the ***subject*** is an activity, the experiment can be considered a ***psychological experiment***. The more opportunities for activity are created by the environment, the less the environment controls and regulates the subject's activity, the more the subject shows his or her subjective qualities.

It is clear from this that the control of the subject's behavior reduces it to a biological individual. At the same time, it can only show its object properties. Accordingly, psychological research in this case gives way to physiological, biomechanical, ergonomic, etc.

On the other hand, the more the subject exhibits his or her subjective properties, the more the subject properties of the researcher and interaction between the subject and the researcher affect his or her behavior and the results of the psychological measurement. In this case, two main artifacts emerge. The ***first artifact*** is caused by the experimentalist's influence on the subject's psyche. One of its manifestations is the "Pygmalion effect". This is the case when the experimenter unconsciously changes the subject's mental state, adjusting it "to the hypothesis". The ***second artifact*** ("facade effect") is defined by the subject's behavior and his or her aspiration to create an image of his or her "Ego" and to change behavior in accordance with his or her motives and goals.

Direct interaction between the subject and the experimenter, when they act as subjects of communication, is considered an integral factor in any psychological experiment.

A distinction should be made between a variation in data that is a consequence of the subject's subjective nature and a variation that is due to the relations between the subject and the experimenter. Part of the second variation is subject to some control due to the possibility of typification (and, consequently, objectification) of the relations between the subject and the experimenter. Unfortunately, neither the subject nor, moreover, the subjecc tive components can be predicted.

Questions for discussion:

1. Subjectivity of participants of psychological experiment.

2. The reversibility of the subject-object relationship.

3. The ratio of determinacy in a psychological experiment.

4. The complexity of the subject, the complexity of the object and the complexity of the instrument in a psychological experiment.

5. Experiment as a joint activity.

6. Artefacts of joint activities.

PART 2.

THEORIES OF THE PSYCHOLOGICAL EXPERIMENT

2.1. FEATURES OF THE SUBJECT AREA AND CONTENT OF HYPOTHESES

Classifications of types of psychological experiments are based on different bases. One of these bases, or criteria of classification, is accessory of an initial causal hypothesis, conditions of carrying out experiments or selection of subjects to special areas of psychological reality. I.e., their attribution to a certain **subject area**.

Ways of transition from the "world of empiricism" to the "world of theories" that have developed in this or that area of psychological knowledge include orientation of researchers to the norms of establishment and interpretation of psychological regularities justified within the framework of specific theories and more general research paradigms.

With this approach, however, there is a possibility of misinterpretation of mental mechanisms discovered in the experiment. A classic example is the so-called ***Hottorn experiment***, which, according to its aims, should have been attributed to such a subject field of research as the psychology of labor.

In this experiment, conducted in the early 20th century in Hottorn, a suburb of Chicago, numerous environmental factors (factory conditions) changed and the impact of these changes on labor productivity was assessed. As it turned out, any change, regardless of its type (and in case of its absence in the control group), contributed to the improvement of workers' indicators. Such interpretation was accepted as adequate: people, knowing the objectives of the experiment, responded positively to the very fact of the attempt to improve their working conditions.

Therefore, this experiment is now sometimes described in the sections on "experimental effects". The regularities revealed in it turned out to be more general than the framework for managing working conditions. But the main thing is that interpretation of basic processes from the point of view of their

subject attribution, as well as mechanisms of influence of controllable factors on them remained problematic.

The special way of development of psychological knowledge is connected with theoretical rethinking of processes, which seem to belong to the undoubtedly accepted sphere of mental reality. At a substantiation of new representations about regulation of mental laws, the experiment starts to play a role of a source of arguments, unobvious (in the old system of theoretical interpretations), but changing representations about psychological causality.

In each subject area of psychological research it is possible to specify essentially different interpretation schemes of the studied processes. There is a fairly large gap between what the experimental material and controlled factors look like and the basic process under study. Orienting oneself on the type of tasks performed by the subjects may be misleading as to what the subject area to which the study should be "attributed".

At the same time, one should not forget about the subject attribution of psychological laws to this or that area of psychological knowledge. Each of them has developed the special norms for check of hypotheses connected with type of construction of the psychological theory itself and possibilities of gathering empirical material. The matter is that the experiment in each of these areas carries out specific approaches to methods of selection of variables and management of experimental factors, different understanding of causality and consideration of different ways of substantiation in the organization of meaningful conclusions.

2.2. PECULIARITIES OF THE WAY THE EXPERIMENT IS CARRIED OUT

2.2.1. Experiment real and mental

Verification and falsification of hypotheses based on empirical data makes it possible to introduce such a criterion of differentiation as mental and practically realized experiments. ***Falsification***, i.e. rejection of a hypothesis as incorrect, not corresponding to empirical regularities, can be grounded both by real

research, and by substantial-logical arguments in the interpretation of expected dependencies. The use of substantial and logical arguments, however, does not make the hypothesis empirically tested. ***Verification***, i.e. acceptance of the hypothesis as corresponding to reality, moreover, is possible only on the basis of obtaining empirical data in favor of the assumed dependence. However, not all hypotheses are transferred to the empirically tested level. Some of them are not empirically tested exactly on the basis of substantial justifications (not every hypothesis is experimentally tested). Others cannot be tested due to the lack of operational (methodological) means at this stage of scientific knowledge. Others require special substantiation first at the level of mental experimentation in order to proceed to the construction of real experiments.

An experiment that is practically carried out for the purpose of ***obtaining*** empirical arguments for or against the understanding of the psychological pattern assumed in a substantial hypothesis is called ***real*** or practically realized. It is contrasted with a ***mental experiment***. It allows to ***assume reception of this*** or that data at controllable experimental influences. But these suppositions are not realized in practical activity of the researcher. In contrast to the planned experiment, when only the decision on the established empirical regularity is connected with the stage of its practical realization, a mental experiment is aimed at substantiation of the possible decision on the type and mechanisms of the supposed logical connection between an independent variable (IS) and a dependent variable (DV). In a mental experiment a ***certain way of*** concluding about the established dependence is assumed. In a practically carried out experiment there are at least three of these methods and the choice of a particular method is determined by making a decision on the experimental fact. On the basis of this decision, the experimental ***hypothesis***, ***counterhypothesis,*** or ***they*** can be ***rejected*** (falsified) or ***together*** (when the data do not allow a choice between them and suggest a search for a third hypothesis).

The relationship between substantial and formal planning for mental and real experiments is common in all situations where the control problems of variable mixtures are not taken into account. The advantage of mental experiments is the ability to assume the detection of dependence in the absence of mixing. In practical experiments, one cannot do nothing but assume the absence of mixing. Here it is necessary to provide their real control to be able to take into account their influence on the obtained experimental effects.

In mental experiments we can assume any ideal conditions and mentally model the studied basic processes in any indicators of their manifestations, even if

there are no real methods for the operationalization of variables. When evaluating the psychological hypothesis from the point of view of the results implied in a mental experiment, there is no need to correlate the result of the NP action with the evaluation of the internal and operational validity of the experiment.

2.2.2 Experiments conducted in laboratory and "field" conditions

The division of types of experiments into ***natural***, or ***artificial*** and ***laboratory***, so-called ***field conditions*** is based on the evaluation of conformity of the independent variable (IS), dependent variable (DV) and additional variables (AP) given in the experimental model with the real situations to which the generalizations are supposed to be transferred. Experimentally set variables can also correspond to theoretical constructs, according to which the theoretically assumed regularity is operationalized in concrete ***methodological procedures of*** practical implementation of the experiment.

To achieve this compliance, the researcher carefully operationalizes the concepts and cleans the conditions. The result is usually a laboratory experiment to be performed. If a good correspondence between the techniques representing NP and WP and psychological concepts is achieved, the ***operational validity of the*** experiment is highly appreciated. In its turn, the achievement of high operational validity allows generalization as a transition from empirically established dependence to the evaluation of a theoretical model or theoretical causal interpretation. Operational validity as a means of evaluating the conformity of methods - supposed psychological variables - acts as the main means of constructing a laboratory experiment.

It should be borne in mind that the form of conducting experiments (in the laboratory or "field" conditions) does not determine the logic of subsequent generalizations. In laboratory conditions, too, those relations (psychological laws) that are adequate to both natural and real situations can be modeled. Thanks to this adequacy, a researcher has an opportunity to discover those spheres of real life activity, to which natural relations established in the laboratory are related.

Achievement of conformity of the model given in an experimental situation to psychological realities (but not to psychological theories) is estimated from the point of view of external validity. Another question is to what extent it is

possible to establish "pure", not conceptualized "empirical" laws. In fact, any psychological understanding includes the context of theoretical interpretations. This happens even when the researcher himself believes that he modeled a life situation in the experiment without explaining it in any way.

As soon as the variables implied in the hypothesis in a real experimental situation begin to enter into complexes of relationships with other variables (variables of life conditions), another type of experiment appears - ***artificial***. We can say that both ***laboratory*** and ***artificial experiments*** are variants of such model situations, which are opposed to real situations as "field" conditions of conducting experiments. It is important to emphasise that it is not in itself the conduct of experiments under laboratory conditions that makes it possible to attribute an experiment to a type of laboratory experiment, namely, a change in the researcher's position to the type of variables represented in the experimental model.

If a similar type of interrelationship of variables is recreated in a laboratory environment, but the situation is real, the degree to which NPs, WPs and DPs correspond to these real conditions will be assessed. The experiment in this case will be considered as "improving" reality in the sense that its artificial "truncation" is aimed at clarifying the links between the main, according to the experimental hypothesis under test, variables.

Both in the laboratory and in the "field" conditions, an experimental model can be recreated to represent a certain theoretical understanding of the interrelationship of variables. It is rather problematic to distinguish between types of experiments on the relationship between their construction and certain theoretical schemes. However, for each theory under consideration, it is possible to discuss operationalization of some or other hypothetical notions about the subject regulation of the processes under study.

Questions for discussion:

1. Subjective attribution of psychological patterns.
2. Principles of verification and falsification in a psychological experiment.
3. Perfect and real experiments.
4. A mental experiment.
5. Artificial, laboratory and field experiments.

6. Operational and constructive validity.

2.3. FEATURES OF PSYCHOLOGICAL HYPOTHESES

2.3.1. Critical experiment and possibility of competing explanationsi

In relation to an experiment with checking one theoretical assumption, where empirical data "pros" and "cons" are considered within the same explanatory scheme, another type of research can be distinguished - a ***critical*** experiment. In such an experiment, it is assumed that it is possible to choose between different explanatory schemes on the basis of the experimental data obtained. In other words, it is possible to choose one of the competing ***theoretical interpretations***. In this case, different psychological explanations are behind the experimental and counter hypothesis as empirically loaded statements. This means that at least two theoretical hypotheses are supposed to be correlated.

In real research it is very rare to find the so-called ***critical experiment***, for which positive and negative outcomes would be associated with different interpretation schemes. Usually two hypotheses are compared in an experiment that suggest positive and negative outcomes within the same psychological interpretation. The counter-hypothesis sounds like a negation of the connection postulated in the experimental hypothesis, but not as an explanation of the connection within the framework of another interpretation scheme.

Such an experiment is usually called a ***control*** experiment because it creates conditions under which it would be equally likely to obtain data both for and against the experimental hypothesis. In other words, conditions are controlled under which it is the efficiency of the assumed causal dependence that determines the change of WP indices in the expected direction.

2.3.2. Features of demonstration experiment

The traditional control experiment aimed at testing the causal hypothesis can be contrasted with so-called ***demonstration experiments***. They should be called

demonstration experiments rather than experiments, since they cannot move to the counter-hypothesis assessment when negative results are obtained. The conditions for obtaining data in the direction of possible empirical support for the assertions implicit in counter-hypotheses are absent here. That is, there is no stage in the ***decision making*** about an experimental fact, since there is no choice in interpreting the pros and cons alternatives. Two types of demonstration experiments are best known and most typical.

The first kind is represented by initiation of effects that are almost unambiguously reproduced in a similar situation (or similar organization of impacts) by any person who follows instructions. The expected effect in this case is observed in almost 100% of cases. In such experiments it is impossible to obtain data against this or that hypothesis. These data unambiguously testify in favor of the demonstrated laws and solve rather the diagnostic task of detection of an implied (latent) psychological reality. Reproducibility of such experiments means the possibility of postponing the solution of this diagnostic problem in any future period of time, in relation to which it is possible to assert the absence of changes in the very basic process under detection.

The second type of demonstration experiments is characterized by that it can unequivocally show correctness or adequacy of the psychological hypothesis only in relation to the already existing event. They cannot be reproduced again in the sense that their actual genesis depends on the activity of the cognizing or acting subject, on personal, not only stimulating factors.

The inability to predict certain regularities as reproducible with a hundred percent probability should not be mixed with the inability to demonstrate them. No consequences can be deduced from demonstration experiments against the corresponding author's understanding of psychological causality. It is possible to argue with such constructed theoretical hypotheses only in the "world of theories". In the "world of empiricism" there can be no arguments "against". Such argument of an internal order in relation to the considered theory does not suppose derivation of consequences into outside, into other interpretation schemes or into the plan of possible refutation of initial psychological understanding.

Questions for discussion:

1. Features of a critical experiment.
2. Feature of the demonstration experiment.

3. Two kinds of demonstration experiences.

2.4. DETECTABLE REGULARITIES AND PECULIARITIES OF THE EXPERIMENT

2.4.1 Features of different explanatory approaches

In one and the same area of psychology, different types of construction of psychological explanations replace each other and coexist. The book by X.Heckhausen [X.Heckhausen. Motivation and Activity, 2003. - 860 p.] shows how psychological interpretations of the concepts of "motive" and "motivation" are connected with forms of organization of experimental plans and more general principles of postulated explanations. These general principles, or research "paradigms" developed in this psychological problem, connect causality with the initial conditions of the situation or other factors of motives' effectiveness. In particular, with established individual dispositions.

In these paradigms, the emphasis is on different problems of experimental study of motivation. For example, representativeness of the observed results as a result of changes in motivation levels; ways of actualization of motives; interrelation of hypothetical constructs of motivation and motivation in specification of their relations with independent and dependent variables. Theories of motivation themselves are classified according to the criterion "type of explanations". Among them, mechanistic and cognitive strategies of explanations, "controlling" components of purposeful motivated activity and its instrumental (executive) components are specifically considered. Change of explanatory models is considered by the author in interrelation of the methods of research organization and the explanatory schemes guiding it.

The type of experimental dependence to be established can be analyzed in another aspect of the relationship to the theory. Namely, from the point of view of reconstruction of specific forms of regulation of basic processes, in relation to which it is impossible to postulate the "influencing" character of the NP used and to consider changes in WP only as "responses". Among the most known types of experimentally established dependencies, first of all, we should name those that were formulated within the framework of using "double stimulation

techniques". The change of the general principle of understanding psychological regulation was carried out in L.S.Vygotsky's cultural-historical concept in the context of change and research paradigm.

2.4.2 Cultural-historical approach to psychological research

L.S.Vygotsky's cultural-historical concept has become the theoretical basis for a new type of experimentation in psychology. In a number of methodical methods presented within the framework of this concept, developed for testing psychological hypotheses, the activity of the subject was the necessary condition without which it is not necessary to speak about actualization of the studied basic processes. Experimental conditions determined the possibility of the subject's activity actualization, which could be carried out by means of different level basic processes.

Specific methodological conditions allowed the subject to demonstrate the possibility of transition to a new level of psychological regulation (attention, memory, and thinking) rather than to determine this transition. Thus, independent variables in these schemes of research construction are by no means ***influencing factors***. They act as conditions that set certain possibilities for the subject, who himself is implementing (or not implementing) the new methods of psychic regulation potentially presented in them.

The experimental procedure, embodied in the so-called ***double stimulation technique,*** played here the role of an experimental model representing the theory of relationships between variables. However, this model also assumed such a degree of inclusion of the activity of the subject himself that the context of psychotechnique in the analysis of the processes occurring in it could overshadow the context of the fact that this is primarily an experimental procedure. The subject's activity in self-regulation clearly distinguished the action occurring in this procedure from other experimental models known in psychology.

It is usually stressed that the ***historical and genetic method*** proposed by L.S. Vygotsky allowed to demonstrate results that are unattainable with the usual ***slice method***. Behind development of this method stood the substantiation of a new psychological hypothesis about the mediated nature of higher, "cultural" mental functions differing in origin, structure and property of arbitrariness from "natural" functions. No less important event was the explication of empirical

consequences from this general hypothesis, which could be tested in a method representative of the processes believed, according to the theory.

The introduced term ***"mediating" implied the*** formation of "psychological instruments", or "stimuli", initially related to interaction with a partner in a situation of communication, and then turned by the subject to himself as a means of controlling his own psyche. As far as thinking is concerned, such "tools" have become words as a sign. The "ingrowth" of stimuluss-inside means is a transition from external signs to interiorized signs. Various means, such as "tying a knot for memory" and the meaning of the word, have a common property. This common property is their artificial nature, they are created by man on purpose, and are elements of culture. They are dialogic in the sense that they are born only in human cooperation.

L.S.Vygotsky together with his employee L.S.Sakharov on the basis of "artificial words" N.Aha developed the principle of double stimulation technique. In this technique the artificial name of the group of objects acted as the ***stimuli*** (***second row stimuli***) used by the subject to solve the problem of classification of these ***objects*** (***first row stimuli***). The development of the named principle allowed to find different operational means to check hypotheses on formation of higher mental functions.

In general psychology, a significant role was played by subsequent changes in the methodological procedure for the formation of artificial notions in the studies of J. Bruner and O. K. Tikhomirov. The role of the experimenter in organizing the sequence of objects disclosed to the subjects and the activity of the subject in collecting information have changed. In modern schemes of experimentation computerization of the methods of artificial concepts formation allowed to proceed to the analysis of ***microgenesis of*** intellectual strategies regulation by external (controlled) factors and factors of internal conditions, as which were motivational, personal and style factors.

Another well-known experimental procedure based on the principle of ***double stimulation techniques***.

In the study of mediated memorization A.N.Leontiev the corresponding experimental model looked like the organization of conditions of random memorization. In it, the first stimulus series is presented by a list of words that the subject should have remembered according to instructions. The second row of stimuli was represented by picture cards. They could be used by the subject to memorize the words, which would thus become mediated.

Even before the development of this method, psychologists identified two forms of memorization of unrelated stimuli: mechanical and intellectual (logical) memorization. In a situation of ***direct memorization,*** some subjects were unable to perform direct instruction because they could not avoid using any memorization techniques. The use of auxiliary means, or the formation of the instrumental function of mnemonic signs, was considered in the cultural-historical concept as the main line of formation of higher forms of memory - ***mediated memory***.

Concretization of the historical and genetic method in the study of A.N.Leontiev took into account two lines of memory improvement during the historical development of mankind. The ***first line*** is the improvement of external means. Namely, transformation of mnemotechnical sign into written sign. In this case, the formation of the sign's signaling function denies the function (memory) with which its birth was connected. With regard to the demonstration of the idea of the system structure of consciousness, L.S. Vygotsky formulated this idea as follows: if for a preschooler to ***think means to remember***, then for a schoolboy to ***remember means*** to ***think***.

The second line of development of any forms of memory is the transition from the use of external to internal storage facilities. Experimental development of this line of formation of higher mental functions was realized in the following variant of the ***double stimulation technique***.

A.N.Leontiev's classic experiment, in which this method was applied, became a starting point for the formation of an independent research paradigm in the study of memory, perception, thinking as activities similar in its internal structure to the structure of the subject activity.

Questions for discussion:

1. The relationship of the psyche paradigm to the psyche research paradigm.
2. Cultural-historical paradigm of the psyche as the basis of the instrumental research paradigm.
3. NP as the conditions that set out the possibilities of activity.
4. A technique of double stimulation.
5. Historical and genetic method of experimental research.
6. Two lines of development of an arbitrary memory form.

2.5. SPECIFICITY OF PSYCHOLOGICAL EXPERIMENT

2.5.1. Specifics of experimental communication

Psychological experiment is a joint activity of the subject and the experimenter. This joint activity is organized by the experimenter and is aimed at studying the peculiarities of the subject's mentality. The process that ***organizes and regulates*** collaborative activities is ***communication***. A subject comes to the experimentalist with his or her life plans, motives and goals of participation in the experiment. And, naturally, the result of the study is influenced by the peculiarities of his personality, which are manifested in communication with the experimenter. The social psychology of the psychological experiment deals with these problems.

A psychological experiment is seen as a holistic situation. The impact of the testing situation on the intelligence of children was discovered as early as in 10-20 years. of the 20th century. In particular, it was found that the assessment of children's intellectual development on the test of A. Bine - T. Simon depends on the social status of his family. It appears in any study, in any sample, at any time, in any country (with rare exceptions). Psychology initially interpreted this fact as a dependence on the "social order" or assumed, using F. Galton's hypothesis, about the inheritance of abilities. It was supposed that the elite of society should consist of highly gifted people and attract them to its composition.

However, if in a testing situation different approaches are used in communicating with children from different social strata, as well as the speech turnings usual for a child, there is no difference in the intellect of children from different social strata.

It should be noted that all psychologists recognize the importance of the impact of the experiment's situation on its results. Thus, it has been revealed that the experiment procedure has a greater impact on children than on adults. Explanations of this are in the child's psyche.

1. Children are more emotional with adults. An adult is always a psychologically significant figure for a child. He is either useful, or dangerous, or cute and trustworthy, or unpleasant and should stay away from him. Consequently, children tend to like a strange adult or "hide" from contact with

him. Relationships with the experimenter determine the attitude to the experiment (but not vice versa).

2. The manifestation of personality traits in a child depends more on the situation than in an adult. The situation is constructed in the course of communication. The child must successfully communicate with the experimenter, understand his questions and requirements. He or she speaks his or her native language while communicating with his or her immediate environment, learning not literary language, but speech, adverb, "slang". An experimenter who speaks a literary and scientific language will never be "emotionally" his or her own, unless the child belongs to the same social stratum. A system of notions and ways of communication (manner of speaking, mimicry, pantomimetics, etc.) that is unusual for a child will be a powerful barrier to its inclusion in the experiment.

3. The child has a more vivid imagination than the experimenter, and therefore can interpret the situation of the experiment differently, "fantastically" than the adult. In particular, when criticizing the experiments of J. Piaget, some authors make the following arguments. A child may see the experiment as a game by "his" laws. The experimenter pours water from one vessel into another and asks the child if the amount of fluid has been preserved. The child may find the correct answer trivial, uninteresting, and he will play with the experimenter. He or she may imagine he or she has been offered a magic trick with a glass or an offer to play a game where the laws of conservation do not apply. It is unlikely that a child will reveal the content of his fantasies. These arguments can only be fantasies of critics J. Piaget. After all, the rational perception of the situation of the experiment is a symptom of a certain level of intellectual development. However, the problem remains unsolved, and experimenters are advised to pay attention to whether the child understands the questions and requests addressed to him or her correctly, what he or she means by giving this or that answer.

The founder of the study of socio-psychological aspects of psychological experiment was S. Rosenzweig. In 1933 he published an analytical review on this problem, where he identified the main factors of communication that could distort the results of the experiment:

1. Errors in the "relationship to the observed." They are related to the subject's understanding of the decision-making criterion in choosing a reaction.

2. Mistakes related to the subject's motivation. A subject may be motivated by curiosity, pride, vanity and act not in accordance with the goals of the

experiment, but in accordance with his or her understanding of the goals and meaning of the experiment.

3. Errors of personal influence connected with perception of the experimenter's personality by the subject.

A subject may participate in the experiment either voluntarily or by coercion. Participation in the experiment itself generates a number of behavioural manifestations in the subjects that are the causes of the artifacts. The best known are the ***placebo effect***, ***the Hottorn effect***, and ***the audience effect (the social facilitation effect).***

The placebo effect was discovered by medics. It is detected when the subjects are confident that the drug or the doctor's actions are contributing to their recovery. Regardless of the real situation, they have an improvement. The effect is based on mechanisms of suggestion and self-induction.

Hottorn's effect was demonstrated in the course of social and psychological research in factories. Attraction to participation in the experiment, which was conducted by psychologists, was considered by the subjects as a manifestation of attention to him personally. The participants of the research behaved in the way the experimenters expected from them. The Hottorn effect can be avoided by not informing the subjects of the study's hypothesis or suggesting a false hypothesis, or by introducing the instructions in as indifferent a tone as possible.

The effect of social facilitation (*amplification*), or audience effect, was discovered by G. Zayonts. The presence of any external observer, in particular, an experimenter and an assistant, changes the behaviour of the person performing this or that work. The effect is clearly manifested in athletes at competitions. It reveals a difference in the results shown in public and in training.

Г. Zionz found that the presence of spectators during training embarrasses the subjects and reduces their performance. When activity is mastered or reduced to mere physical effort, the result is improved. After further research, the following dependencies were found.

1. It is not any observer who has influence, but only competent, relevant to the performer and capable of giving an assessment. The more competent and significant the observer is, the more significant this effect is.

2. Influence is the more difficult the task. New skills and abilities, intellectual abilities are more susceptible to impact (towards lower efficiency). On the

contrary, old, simple, perceptual and sensorimotor skills are easier to manifest, and the productivity of their implementation increases in the presence of another significant observer.

3. Competition and joint activity, the increase in the number of observers increases the effect (both its positive and negative tendency).

4. The "anxious" test subjects experience more difficulties than emotionally stable individuals when performing complex and new tasks that require intellectual effort.

5. The action of the "Zionz effect" is well described by the law of optimum motivation of Yerks-Dodson. The presence of an external observer (experimenter) increases the motivation of the subject. Accordingly, it can either improve productivity or lead to "remotivation" and cause disruption of activity.

The motivation to participate in the study should be distinguished from the motivation arising in the course of the experiment when communicating with the experimenter. It is believed that in the course of the experiment the subject may get any motivation. M.T.Orne believed that the main motive of the subject is the desire for social approval, the desire to be good. He wants to help the experimenter and behaves in a way that confirms the experimenter's hypothesis. There are other points of view as well. It is believed that the subject tries to prove himself from the best side and gives the answers which, in his opinion, are more appreciated by the experimenter. In addition to the manifestation of the ***"facade effect"***, there is a tendency to behave emotionally stable, "not to give in" to the pressure of the experimental situation.

A number of researchers suggest a model of a ***"malicious test subject"***. They believe that the subjects are hostile to the experimenter and the research procedure and do everything to destroy the experimenter's hypothesis. But the more common view is that adult subjects tend only to follow the instruction precisely, not to give in to their suspicions and guesswork. Obviously, this depends on the psychological maturity of the subject's personality.

Studies conducted to determine the role of social acceptance motivation produce very mixed results. Many early works have confirmed this role. Subsequent studies have denied that the subjects' motivation is highly valued for their results. L.B.Christiansen summed up the discussion. From his point of view, all variants of the subject's behavior in the experiment can be explained by the actualization of one motive. Namely, the desire for positive representation. In other words, the desire to look as good as possible in one's own eyes.

An adult subject, entering the situation of the experiment, orients and behaves according to the situation, but is encouraged by the desire "not to lose face" in front of himself. He pays attention to rumors about the experiment and its objectives, the instruction and messages of the experimenter in the process of conversation, the specific features of the personality of the experimenter, the conditions of the study (laboratory equipment, the condition of the room, the comfort of the environment, etc.), takes into account the peculiarities of communication with the experimenter in the course of the experiment. Based on these features, the subject builds an "internal" model of the experimental situation.

The "deception" method, if a substitution of the experiment's goals is detected by the subject, will not be effective. Test subjects who suspect that they are trying to manipulate their behavior with the help of instructions, deceive them, etc., abstain from actions expected by the experimenter and resist his influence. They explain this resistance by the fact that it is unworthy to manipulate a person beyond his or her will. At the same time, the experiment activates the self-representation motive because its conditions are unnatural and different from the previous experience of the individual.

Demonstrating individuals tend to turn the experiment into a theatre. They feel like on stage, behave unnaturally and deliberately. "Anxious" personalities can behave shyly, tense and so on. The motivation for self-representation is strongest if the subject believes that his or her behaviour in the experiment is personally deterministic. That is, his actions are not the result of experimental influences, but the manifestation of real intentions, feelings, beliefs, abilities, etc. If the subject believes that his or her behaviour in the experiment depends on the conditions, content of tasks, and interaction with the experimenter, then the motivation of representation is not manifested in his or her behaviour.

L.B.Christiansen, the most famous expert on the problem of self-representation on the course of the experiment, made a disappointing conclusion based on his own and others' studies. This conclusion is that the self-representation motive is extremely difficult to control because the conditions under which it appears and the directions of its influence on the experimental results are not determined. For example, the self-representation motive interacts with the motive of social acceptance. Experienced individuals are particularly eager to manifest themselves "in the best way" when the experimenter cannot directly incriminate them in lying. If subjects are asked to evaluate their intelligence, it is particularly inflated when the experimenter is not going to "test" their

intelligence. If, however, subjects know that after subjective evaluation of their intellect they should perform the test, they evaluate it much lower.

In addition, if the subject believes that the experimenter is manipulating him/her, the self-representation motivation is also stronger. Thus, both the motivation of self-representation and the motivation of social approval (contrary to L.B. Christiansen's original hypothesis) are equally actualized in the psychological experiment. To control the influence of the subject's personality and communication effects on the results of the experiment, a number of special techniques are proposed. Let us enumerate them and give a characteristic to everyone.

1. ***The placebo blind method***, or ***"double blind experience***." The Rosenthal effect (Pygmalion effect) is controlled. Identical control and experimental groups are selected. The experimental procedure is performed in both cases. The experimenter himself does not know which group gets the "zero" effect and which is actually manipulated. This plan makes it possible to exclude both the effect of the test person's expectations and the effect of the experimenter's expectations. Psychopharmacologist H.K.Beecher investigated with the help of this experimental plan the effect of morphine on pain sensitivity. Working on the "placebo blind" scheme, he could not distinguish the control group data from the experimental one. When he did the experiment in the traditional way, he got classic divergent curves. "The Double Blind Experience controls the Rosenthal and Hottorn effects.

2. ***The "deception method"*** is based on the deliberate misrepresentation of the subjects. In its application, ethical problems naturally arise. Therefore, many social psychologists of humanistic orientation consider it unacceptable. The experimenter comes up with false research goals and hypotheses, independent of the main ones. The invented goal and hypothesis are communicated to the subject. The content of a false hypothesis varies depending on the nature of the experiment. Both simple "common sense" hypotheses and complex theoretical structures called "cognitive placebo" can be applied. A possible variant of the "deception method" is simple concealment of the true aims and hypothesis of the experiment. But in this case, the test subjects will come up with the variants themselves. And instead of taking into account the influence of a false hypothesis, we will have to understand the subject's fantasies in order to eliminate the influence of this uncontrolled variable. Thus, it is better to offer the subject at least some variant of the hypothesis than not offer any. The method of "cognitive placebo" is preferable.

3. ***The method of "hidden" experiment is*** often used in field research, when implementing the so-called "natural" experiment. The experiment is so included in the natural life of the subject that he or she is not aware of his or her participation in the study as a subject. In essence, the "hidden" experiment method is a modification of the "deception method" with the only difference that the subject should not be given false information about the study goals and hypothesis because he or she is already involved in the study by deception and does not know about it. There are even more ethical problems here, because by using the "deception method," we inform the subject about his or her involvement in the study (even forced research). Here, the subject is fully controlled by another person and is the object of manipulation. At the same time, there is a very high risk of all kinds of abuse by unscrupulous researchers. At the same time, this model is often used in social psychology. The main difficulty in conducting such an experiment is taking into account uncontrollable variables, as this experiment can only be natural. The method of "natural experiment" proposed by A.F.Lazursky is one of modifications of this research technique.

4. ***The method of independent measurement of*** dependent parameters is very rarely used, as it is very difficult to implement it in practice. The experiment is conducted with the subject according to the usual plan. But the effect is not measured during the experiment, but outside it. For example, in controlling the results of the former subject's educational or work activity.

5. ***Control of the subjects' perception of the situation***. Usually the scheme of post-experimental interview proposed by Orna is used for this purpose. In addition, measures are used to take into account or control the attitude of the subject to the experimenter and the experiment, his or her understanding of the instruction, acceptance of the research objectives. Unfortunately, the data obtained in the post-experimental interview only allow us to reject unsuccessful samples or take this information into account when interpreting the results of the experiment, when nothing can be corrected.

It should always be remembered that there is no absolute method and technique, they are all good or bad depending on the specific situation. But none of them provides absolutely reliable knowledge.

Questions for discussion

1. Features of experimental communication of children.

2. Communication factors that distort the experimental result.
3. placebo effect, Hottorn effect, audience effect.
4. The effect of the subject's personality on the experimental result.
5. Methods to control the effects of communication.
6. The role of motivation in a psychological experiment.

2.5.2 The experimenter: his personality and activities

The classical natural science experiment is theoretically normative. It means that if a researcher could be removed from the experimental situation and replaced with an automaton, the experiment would correspond to the ideal one. But human psychology refers to disciplines where it is impossible to do so. Consequently, the psychologist has to take into account the fact that any experimenter, including himself, a human being, and nothing human is alien to him.

First of all, it concerns errors. That is, involuntary deviations from the norm of the experiment (an ideal experiment). But it is not limited to errors because they can be corrected sometimes. More serious are the steady tendencies of the experimenter's behavior, which influence the course of the experimental situation and are the consequence of unconscious mental regulation of behavior.

The experiment, including the psychological one, should be reproduced by any other researcher. Therefore, the ***scheme of*** its conduct (the ***norm of the*** experiment) should be as objective as possible. This means that the reproduction of the results should not depend on the professional actions of the experimenter, external circumstances or case.

From the point of view of the ***activity approach***, the experiment represents the activity of the experimenter. It influences the subject by changing the conditions of his activity to reveal the peculiarities of the subject's mentality. The procedure of the experiment is a proof of the degree of the experimenter's activity. It organizes the work of the subject, gives him/her a task, assesses the results, varies the conditions of the experiment, registers the subject's behavior and results of his/her activity, etc.

From the point of view of the ***social-psychological approach***, the experimenter plays the role of a leader, teacher, initiator of the game, and the subject appears as a subordinate, performer, student, and slave participant of the game. The scheme of the experiment, if we consider it as an experimenter's activity, corresponds to the model of neogeviorism: ***stimulus - intermediate variables - reaction***. The experimenter gives the subject a task (***stimulus***) and the subject (***intermediate variable***) performs it. If the researcher is interested in confirming (or refuting) his hypothesis, he may unknowingly distort the course of the experiment and interpret the data. In doing so, he will seek to ensure that the subject "works under the hypothesis", creating privileged conditions only for the experimental group. Such actions of the experimenter are the source of artifacts.

American psychologist Rosenthal called this phenomenon "Pygmalion effect" after a character in the Greek myth. *(The sculptor Pygmalion of Crete made a statue of a beautiful Galatea girl. She was so good that Pygmalion fell in love with Galatea and begged the gods to bring the statue to life. The gods responded to his requests)*. A researcher interested in confirming a theory acts involuntarily so that it can be confirmed. But this effect can be controlled. For this purpose it is necessary to involve in carrying out of research the experimental assistants who do not know its purposes and hypotheses. The most complete control is to double-check the results of other researchers who are critical to the hypothesis of the author of the experiment. However, even in this case, we are not guaranteed against artifacts, because the controllers are the same people as the author of the experiment.

N. Fridman called a scientific myth that prevailed until the 60s of the 20th century in American psychology, the view that the procedure for conducting experiments is the same, and the experimenters are equally impartial and qualified. In fact, experimenters are not anonymous or faceless, they observe, record and evaluate the results of their experiments in different ways.

At the same time, the main problem is differences in motivation of experimenters. Even if all of them strive to learn new things, their ideas about the ways, means, goals of learning are different. All the more so because researchers often belong to different ethnocultural communities. At the same time, all the experimenters dream about the "ideal subject". The "ideal subject" must have a set of appropriate psychological qualities. In other words, to be obedient, intelligent, willing to cooperate with the experimenter, able to work, friendly, non-aggressive, and devoid of negativism.

A reasonable experimenter understands that this dream is not feasible. However, if the behavior of the subject in the experiment deviates from the expectations of the researcher, he may show hostility or irritation to the subject. Specific manifestations of the Pygmalion effect are as follows. Expectations of the experimenter may lead him to unconscious actions that change the behavior of the subject.

Rosenthal, the most famous expert on the problem of the influence of the researcher's personality on the course of research, found that the significant influence of the experimenter on the result of the experiment was revealed in experiments with learning, in the diagnosis of abilities, in psychophysical experiments, in determining the reaction time, conducting projective tests (Rorschach test), in laboratory studies of labor activity, in the study of social perception.

The expectations of the experimenter are conveyed to the subject in the following ways.

Firstly, since the source of influence are unconscious attitudes, they are manifested in the parameters of the experimenter's behavior, which are regulated unconsciously. These are, first of all, mimics and pantomimics (head nods, smiles, etc.).

Secondly, "paralinguistic" speech techniques play an important role in influencing the subject. Namely, intonation while reading instructions, emotional tone, expression, etc. (In particular, in experiments on animals, the experimentalist may unknowingly change the ways of handling them). The influence of the experimenter before the experiment is especially strong when choosing subjects, the first conversation, reading instructions. In the course of the experiment, the attention shown by the experimenter to the actions of the subject is of great importance. According to experimental studies, this attention increases productivity of the subject's activity. In this way, the researcher creates the subject's primary attitude to the experiment and forms an attitude to himself.

It is known that it is the ***"first impression effect" that*** leads to the fact that all further information that does not correspond to the created image can be discarded as random. The experimenter's expectations are also affected when he records the results of the experiment. In particular, the effect of the researcher's relationship on the errors made by him when recording the results of the experiment has been established. The experiment was devoted to studying the "phenomenon of telepathy". Two equal groups of people who believe and do not

believe in telepathy were selected. They were asked to record the results of the subject's attempts to guess the content of the "telepathic message" made by another subject. Those who believed in telepathy increased the number of guesses by 63% on average, while those who did not believe in telepathy decreased it by 67%. The effect of waiting is manifested not only in fixing the results of human actions, but also in experiments on animals.

L.Berger identified the following types of experimental errors in the evaluation of the subject's performance.

1. ***Underreporting of*** very good results. The reason is the researcher's subconsciously "linking" the subject's data to his or her own achievements. Overestimation of low scores is also possible. In any case, the scale is deformed and compressed because the marginal results are close to the averages.

2) ***Avoiding*** extreme scores (both low and high). The effect is the same - grouping the data above the average.

3. ***Overestimating the*** significance of one test subject property or one job in a series. Through the prism of this setting, the personality and the tasks are evaluated.

4. Overestimating the value of the ***task following after the*** selection of the personality trait of the test subject that is essential for the experimenter.

5. 5. A similar case, but ***evaluation is mediated by the concept of the*** connection or opposition of certain personality traits.

6. Errors due to the influence of events ***emotionally related*** to a specific subject.

Researchers are trying to identify more specific dependencies of the result of the experiment on the personality of the experimenter and suggest three options to answer the question about the "distorting" influence of the experimenter on the results.

- The unfeasible ideal of experimental psychology - the influence of the experimenter is never present or it is insignificant and can be neglected. The hypothesis is not plausible.

- The personality of the experimenter always and constantly influences the course and results of the experiment. In this case, the effect can be considered a systematic error of measurement - a constant, it is easy to take it into account, ie "put out for brackets".

- This effect is manifested in different ways, depending on the type of experiment, the personality of the experimenter and the personality of the subject. Accounting becomes a complex task of identifying and controlling a large number of relevant psychological variables in each specific experiment. It is experimentally established that the influence of the personality of the experimenter is maximal in experiments on psychology of personality and social psychology and minimal in psychophysiological and psychophysical experiments, researches of sensory and perception. "Average" influence is observed at research of "global" individual processes - intellect, motivation, decision making, etc.

Methods of accounting and control of the influence of the experimenter on the result of the experiment.

Approximately 98% of psychologists consider the influence of the experimenter to be a serious methodological problem. In fact, however, much less care is taken about his control and accounting than about the presence of good furniture, lighting and painting of the laboratory walls.

A. Anastasi believes that in the majority of correctly conducted studies the influence of these factors is practically insignificant and recommends to minimize it, not to resort to methodological research, but to use common sense. If this fails, it is necessary to consider the influence of the experimenter when describing the conditions of the experiment. The following methods of controlling the influence of the experimenter are most often recommended and used.

1. Automation of research. The influence of the experimenter is preserved during recruitment and initial conversation with the subject, between separate series and on the "output".

2. Participation of experimenters who do not know the purpose of the study (already described earlier as "double blind experience"). Experimenters will make assumptions about the intentions of the first researcher. The impact of these assumptions must be controlled.

3. Participation of several experimenters and use of a plan that allows to eliminate the factor of influence of the experimenter.

4. The problem of selection criteria for experimenters and the limit number of control groups remains.

The influence of the experimenter is completely unrecoverable, as it contradicts the essence of psychological experiment, but can be to some extent considered and controlled.

Questions for discussion

1. The normative nature of the psychological experiment.
2. Experiment in terms of different approaches.
3. The role of the researcher's personality in the experiment.
4. Types of experimenter error.
5. Main methods of controlling the influence of the experimenter
6. The effect of the first impression.

2.5.3 The subject: his activities in the experiment

The experiment, where the object is the human psyche and the subject is a concrete sphere of human psyche, differs in that it cannot be carried out without including the subject in joint activity with the experimenter. A subject should not only know the goals and objectives of the research (not necessarily the true ones), but also understand what and for what he or she should do in the course of the experiment, moreover - accept this activity personally.

From the point of view of the subject, the experiment is a part of his personal life (time, actions, efforts, etc.), which he communicates with the experimenter in order to solve some of his personal problems. The subject may be active in learning, playing, working, and communicating. His activity is emotional or creative. In any case, he or she must manifest it either spontaneously or deliberately so that the experimenter can solve his or her research problems.

Therefore, some researchers tend to define an experiment in psychology "from the point of view of the subject" as the activity of the subject(s) organized by the experimenter to perform a behavioral task. Depending on the goals of the experiment and the specific features of the group of subjects (age, sex, health, etc.), the tasks may be creative, labor, game, educational, etc. But always, if we look at the experiment from the subject's perspective, it is a model of real

activity. Therefore, in any experiment there is an element of game, imitation of life situation. But any experiment is also a "game seriously", because we are not given a parallel life. It means that the process and the result of the research have an impact on the life of the subject. Moreover, by participating in it he or she intends to solve some personal problems.

Communication between the subject and the experimenter is a necessary condition for organization of their joint activity and regulation of the subject's activity. A human being is included in the experiment as an integral being. Therefore, organization of an experiment requires taking into account the main, i.e. currently known, psychological regularities that determine human behavior under conditions corresponding to the experimental ones.

Considering the experiment as an activity of the subject, G.E.Zhuravlev singles out several plans of its description:

1. ***Physical.*** Persons participating in the experiment; objects manipulated or transformed by the subject; means at the disposal of the subject; conditions under which the experiment takes place. Similar components are distinguished in the activities of the experimenter.

2. ***Functional.*** Methods of action which are prescribed for the test person; necessary level of competence of the test person; criteria of evaluation of quality of the test person's activity; time characteristics of the test person's activity and conducting the experiment.

3. ***Symbolic symbolic (instruction to the test subject).*** Description: purposes of the research and goals of the subject's activity; ways and rules of actions; communication with the experimenter; acquaintance with motivation, payment, etc.

As already noted, the most important point that distinguishes a psychological experiment involving people from other types of natural science research is the ***presence of instructions***. A person being tested, when receiving it, undertakes to fulfill all requirements in good faith. Sometimes the instruction is reduced (in experiments with infants, patients of the mental illness clinic, etc.), but communication of the subject with the experimenter always takes place. The person receiving the instruction must understand and accept the task. If he or she does not understand the task, he or she will perform the operations specified in the instruction incorrectly. To control understanding of the instruction, not only the subjects are interviewed, but a short preliminary training series is also

included in the experiment. Successful execution of operations in a control series serves as a criterion for understanding the instruction.

At the end of the experimental series, interviews are conducted to find out the difficulties in performing the task and the reasons for deviations of the subjects from the requirements of the instruction. A subject may not accept the experimental task and refuse to perform it. Worse, if due to misunderstanding or rejection of the task the subject replaces the external objective task with his, subjective task. The experimenter should make sure, conducting a post-experimental interview, that such a substitution did not take place. The description of the structure of the subject's activity is an integral part of the norm of the experiment.

The subject must perceive, understand and accept this norm, the identity of the experimenter and carry out the relevant activities. This activity is reduced to the performance of certain tasks by means of a set of tools that the experimenter varies in the course of overcoming obstacles (hindrances, noise, difficulties) that he also changes. Independent variables are the means, obstacles and goals that the experimenter presents to the experimenter.

The human psyche is a holistic system. Therefore, the course and result of a psychological experiment are influenced not only by the studied side of the subject's psyche, but also by the whole psyche. Hence, there is a necessity to take into account and register a much larger number of psychic manifestations than is necessary based on the hypothesis of the research. At the same time, the problem of understanding and accepting the task is by no means trivial.

For example, almost all of the criticisms about J.'s interpretation. Piaget's results of his classical experiments come down to the fact that he offered children a task in "adult" form that was not adequate for them. The children simply did not understand the task and gave answers, substituting the experimentalist's objective task for his own subjective task. However, as soon as the experimenters formulated the same problem adequate to the child's life experience, Piaget's phenomena "disappeared". 5-6-year-old children began to demonstrate the level of cognitive development corresponding to the stage of specific operations.

The classic "instruction effect" variant appears when measuring reaction time. Experimenters know that the instruction that adjusts the subject to detect a signal increases the reaction time, and instruction that requires the fastest response

speeds up the reaction. In addition, the test subjects themselves may differ in terms of which installation (motor or sensor) dominates them.

Questions for discussion:

1. An experiment from the perspective of the subject.
2. Communication as a way of organizing an experimental situation.
3. Plans to describe a psychological experiment.
4. Instruction as a key element of the experimental situation.
5. Integral psyche as a decisive factor in the experimental situation.
6. "The J. Piaget Phenomena" as an example of incorrect instruction.

2.5.4. The personality of the subject and the situation of the experiment

A psychological experiment is a meeting between the subject(s) and the experimenter. However, it is followed by parting. The situation of the experiment can be considered both from the external side ("input" and "exit" from the situation) and from the internal side (what happened during the experiment). A person reacts not only to the experiment as a certain incomprehensible whole, but also identifies it with some class of real life situations, which he or she faces, and accordingly builds his or her behaviour.

At the same time, the experimenter not only recruits a representative group and divides it into randomized subgroups, as a breeder-biologist does, but actively ***involves*** people in the experiment. It means that the researcher is not indifferent to what uncontrolled psychological features distinguish people involved in the study from all others. What motives were they encouraged to participate in the psychological study as subjects. A subject may participate in a study voluntarily or forcibly, beyond his or her will. By participating in a "natural experiment," a subject may not know that he or she has become a subject.

Why do people voluntarily participate in the study? The problem is to find out the motivation of the test subjects volunteers. In classic experiments with sensory deprivation, it was found that half of the subjects agreed to participate in experiments (long and tiresome), driven only by curiosity. The subject often

wants to learn something new about himself. In particular, in order to understand the relationship with others.

Voluntary participation in the experiment is taken by subjects who want to earn money, get credit (in the case of psychology students). They are often driven by simple curiosity or persuasion from friends. And it is very rare that the subject seeks simply "to serve science". There is extensive literature on the personality traits of the test subject volunteers.

It's different if the subject is forced to participate in an experiment. Studies on this problem show that most subjects forced to participate in the experiment were opposed to it, were critical of the experiment, and the experimenter was hostile and distrustful. Often, they seek to destroy the experimenter's plan, to "outplay" it. Such experimenters see the situation of the experiment as conflict. Unfortunately, most often experiments are conducted with subjects who are forced to participate. And only about 7% of those involved in the research are volunteers.

Psychologists have long been interested in what the volunteer test subject is and have come to the conclusion (Rosenthal) that the volunteer test subject differs from the involuntarily attracted test subject in a number of personal characteristics, above all:

1. a higher level of education;
2. the highest social class status;
3. a higher level of intelligence;
4. a more expressed need for social acceptance;
5. with more sociality.

Apart from the fact that the subject is included in the research situation, he or she eventually gets out of it. At first glance, this should not seem to worry the researcher, because he has solved his problems. But this is not always the case for the subject. A subject who is interested in obtaining social approval may not get it. Those who are eager to show competence may perform the task poorly, etc. In other words, the subject is often left alone with the same problems that prompted him or her to take part in the experiment.

Besides, he gains experience of participation in experimental psychological activity and is defined in emotional attitude to psychological experiments, psychologists and psychology in general. As long as psychology was not so

popular, it was possible to neglect it. Today, however, information about psychology from the side of the subjects is capable of forming an opinion about it in society and of serving as a help or an obstacle in the initiation of research work.

The competence of the subject may affect his or her behaviour and results when participating in other psychological studies. As a rule, psychologists evaluate a competent subject negatively, there is even the term "spoiled subject", i.e. one who knows the scheme of the experiment and is able to reproduce the results "under hypothesis" (or against). That is why most experimenters prefer "naive subjects".

M.Metlin introduced the classification, dividing all subjects into positive, negative and trustful ones. Usually experimenters prefer the first and the last. The study may be conducted with the participation not only of volunteers or forced laborers, but also anonymous and communicating their passport data to the subjects. It is assumed that in case of anonymous research the subjects are more open, and this is especially significant in the course of personal and social-psychological experiments. However, it turns out that in the course of the experiment non-anonymous subjects are more responsible for the activity and its results.

The solution of a scientific and practical (diagnostic) problem usually comes down to a certain change in the fate of the subject: he or she may or may not be hired, in higher education, prescribe or not prescribe treatment, etc. "Entrance" into a psychodiagnostic situation is characterized by "external" and "internal" motivation that motivates the subject to participate in the examination. In the first case, he or she is forced to be examined, in the second case, he or she becomes a volunteer. Thus, the ***first parameter*** describing the psychodiagnostic situation is "voluntariness/forcedness" of the subject's participation in the experiment. It is clear that the subject of the choice, with voluntary participation, is the subject of another person (the psychologist himself/herself, psychodiagnostics, representatives of the administration, doctors, etc.).

At the end of the survey, the subject can obtain the results and determine his or her behaviour and life path based on them. Otherwise, his or her life path is changed by another person (psychologist, psychodiagnostic, administrator, etc.). In this case, the decision of the administrator or persons to whom the psychologist has entrusted the data does not depend on further actions of the subject, but is determined only by the will of others. Consequently, in the first case, the subject of choice (decision-making) is the subject of the test subject,

while in the second case, the subject of choice (decision-making) is another person. As a decisive factor determining the testing situation, the subject of making a decision is the subject of the decision: the subject or another person.

This feature characterizes both the "entrance" and "exit" of a psychodiagnostic situation. Thus, four extreme variants of scientifically-practical psychodiagnostic problems (situations) are theoretically possible.

1. Voluntary participation in the experiment, independent choice of further life behavior.

2. Forced participation, independent choice of behavior.

3. Forced participation, choice of behavior after the examination is imposed.

4. Voluntary participation in the survey, choice of further behavior is imposed.

According to this classification, four types of psychodiagnostic situations occurring in psychological practice are possible.

Type I. The situation of voluntary psychological consultation. A person who contacts a counsellor of his or her own free will, trusting in the counsellor's competence, commits to be frank and to participate actively in decision making. A typical type of psychological counseling is family and marriage counseling. As a rule, the final choice about future behavior remains with the client.

Type II. Professional selection, psychological selection in educational institutions, etc. It is up to the examinee to decide whether to choose a training or training profile, but once the test assignments have been completed, it can no longer affect the outcome of the situation.

Type III. Mass surveys in which participation is mandatory (sociological, demographic, etc.). Many psychological information surveys conducted by decision of the administration or public organizations fall under this type when diagnostic information is reported to the subjects. This can be considered a survey of psychology students involved in psychological experiments, particularly in the development of test methods.

Type IV. Plenty of diagnostic situations that arise in the daily work of the psychologist and occur in everyday life. Here, the decision on the subject's fate is made in addition to his or her will and desire. Such situations include the certification of managerial and engineering staff. Compulsory assessment, in particular forensic assessment, is also considered a situation of this type.

In addition to the "external" organization of the experimental research situation, there is also an "internal" one. It can be defined by the style of communication between the subject and the experimenter. The influence of the peculiarities of the "internal" structure of the situation on the behavior of the subject is more significant than that of the "external" situation.

Taking into account the influence of socio-psychological factors on the result of experimental research is extremely difficult.

The first direction of artifact control is aimed at freeing oneself from the influence of additional factors of the experimental situation, personalities of the subject and the experimenter during the study of the psyche. However, this way is not connected with psychological analysis of the experimental situation and does not lead to conclusions formulated in psychological language. Therefore, this direction is practically useless for solving the discussed problems.

The second direction is connected with construction of substantial models of interaction of the subject with a situation and consideration of influence of psychological factors in psychological experiment. But the main problem has not yet been solved: how the significance of the influence of the factors of the experimental situation and the relative "weight" of this influence are determined. Here it is a question of creation of the ecologically valid formalized models of psychological experiment. Problems of this kind are solved by specialists of many natural sciences.

At the same time, it was established that the influence of the experimental situation factor cannot be ignored. It is an indispensable condition for conducting a psychological experiment. In practice, studies are organized and conducted in such a way that the influence of known social-psychological factors is minimal and could be neglected. Here the individual skill and intuition of the experimenter helps.

But it doesn't always work out. It is good if the experimenter has as an experienced adult in a normal emotional state, which is similar to him on social status, cultural, national and racial belonging. It is good if the conditions of the experiment do not affect the honor and dignity of the subject and he does not feel any feelings for the experimenter.

In all other cases, it is possible to recommend the use of artificial methods of conducting the experiment, and if this is not possible, then to analyze in detail the situation of the experiment and all presumed socio-psychological factors. That is, possible causes of artifacts.

Questions for discussion:

1. Peculiarities of the subjects' motivation.
2. Features of the test subject volunteer.
3. Test subject classification.
4. Four types of psychodiagnostic situations.
5. The first area of artifact control.
6. Second line of artifact control.

2.6. PROCEDURE AND MAIN CHARACTERISTICS OF PSYCHOLOGICAL EXPERIMENT

2.6.1. Organization and conduct of a pilot study

All methods used to obtain empirical data can be roughly divided into ***active*** and ***passive***.

The first includes ***all kinds of experiments*** and ***quasi-experiment***. The latter include ***observation***, ***clinical method***, ***product of activity analysis method***, ***measurement method***, ***correlation method, information collection method***, ***"archive method"***, etc. Using the methods of the first group, the researcher actively evokes and changes a phenomenon or process, affecting the object. Using the methods of the second group, he only registers natural processes and phenomena.

Behaviour can be ***recorded*** directly or indirectly using appropriate recording tools (questionnaire, test, etc.). It is possible to ***influence*** the subject directly (in the course of an interview) or indirectly by organizing the subject's activities in the laboratory experiment with the help of research instruments (devices, tasks, etc.).

A researcher may use either a natural science approach to research or a "method of understanding". The "method of understanding" is a direct interpretation of a

subject's behavior, his or her inner world by empathizing or recreating the psyche under study by the researcher's own subjective reality.

Surveillance can be instrumental when tools for video recording, tape recording, etc. are used. In the conditions of the experiment and during the measurement, the subject actively performs the tasks of the researcher, but during observation such tasks are not assigned to the subject, and he always behaves naturally.

There is another criterion for distinguishing research methods into two large groups. The criterion of conformity of the ***method*** is ***ideal research***. This criterion differentiates between the following methods of research organization:

Experimental study, ***systematic observation*** and ***correlation study***. Their peculiarity is that the researcher tries to establish causal or correlative relationships between main variables by controlling external variables. For this purpose, he purposefully selects groups of tested or observed individuals and plans the sequence of his actions in a certain way.

2. ***Natural experiment***, ***observation***, ***conversation***, ***clinical method, method of description of individual cases***, etc. are used to reveal the peculiarities of human behavior. They serve as a source for empirical generalizations and inductive hypotheses, which can later become a material for theoretical reasoning and be tested in critical experiments. The methods of control of variables (independent, dependent, external) are not systematically applied, though it is possible to use complex techniques of data fixation (observation maps, audio and video equipment, tests, etc.).

3. ***Quasi-experiment***. "Intermediate" between natural methods and methods where strict control of variables is applied. Under quasi-experiment it is accepted to understand such method at which it is impossible to realise completely the normative scheme characteristic for ideal experiment. These disadvantages can be partially compensated by using special quasi-experimental plans.

Traditionally, experimental research is contrasted with all non-experimental methods that are considered in the methodology of science from the point of view that they are not enough to become a full experimental research.

Okay, F.-J. McGeagan opposes the experimental method as follows:

- to the classic clinical method;
- to a natural observation;

- a survey;
- to archive research;
- to establish a correlation link;
- to a quasi-experiment.

M.V. Metlin gives another list of methods opposed to the experiment:

- A natural observation to which a field study and a field (natural) experiment adjoin.
- The survey.
- Correlation research.
- Archival research.
- Study of individual (single) cases (case-study or case-history).

P.K.Cozby contrasts the experiment with the correlation method as well as the pre-experimental methods: survey, field observation and field experiment. He deals separately with measurement methods, including the archival method, self-assessment (self-assessment) and testing.

Experimental research in psychology differs from other methods in that the experimenter ***actively changes the*** values of the independent variable, while other methods allow ***only registering*** selected levels of the independent variable. The presence of the main and control groups of subjects is an ideal variant of experimental research. In non-experimental studies, as a rule, all groups are equal, so they are compared.

On formal grounds, several types of experimental research are distinguished. A distinction is made between a ***research*** (search) experiment and ***a confirmation*** (confirmation) experiment. Their difference is determined by the level of problem development and the availability of knowledge about the relationship between dependent and independent variables.

A search (exploratory) experiment is performed when it is not known whether there is a causal relationship between an independent variable and a dependent at all. Therefore, the search experiment is aimed at testing the hypothesis that there is or is no causal relationship between variables A and B.

If there is information about the existence of a qualitative relationship between two variables, then a hypothesis is made about the type of this relationship. The researcher then conducts a confirmatory experiment, which reveals the type of functional quantitative relationship between independent and dependent variables.

The experimental research algorithm looks like this:

1. The hypothesis of a qualitative causal connection A and B is formulated.

2. A search experiment is in progress.

3. If the qualitative hypothesis cannot be confirmed, another qualitative hypothesis is proposed and a new search experiment is conducted; if the qualitative hypothesis is confirmed, a quantitative functional hypothesis is proposed.

4. A confirmatory experiment is being conducted.

5. A hypothesis about the type of relationship between variables is accepted (or rejected) and clarified.

In psychological research practice, the concepts of "critical experiment", "pilot experiment", "field experiment" (or "natural experiment") are also used to characterize different types of experimental research.

A critical experiment is conducted in order to test all possible hypotheses simultaneously. Confirmation of one of them leads to a refutation of all other alternatives. The implementation of a critical experiment in psychology requires not only careful planning, but also a high level of scientific theory development. Since it is not deductive models but empirical (inductive) generalizations that dominate in natural science psychology, researchers very rarely conduct a critical experiment.

A "pilot experiment" (trial, first experiment or series of experiments) is conducted to test a basic hypothesis, research approaches, plans, etc. Usually a pilot experiment is conducted before a "big", laborious experimental study, so as not to waste time and money later. The pilot study is conducted on a smaller sample of subjects, according to a reduced plan, and without strict control of external variables. The reliability of the data obtained from the pilot study is low. But its implementation allows to eliminate gross errors related to hypothesis, study planning, control of variables, etc. Besides, in the course of

the pilot study it is possible to narrow down the "search area", to concretize the hypothesis and to specify the methodology of the "main" study.

A field (natural) experiment is conducted to investigate the relationship between real variables in everyday life (in real conditions). A field experiment can be referred to as a quasi-experiment, since it is impossible to strictly control external variables, select groups and distribute test subjects within them, manage an independent variable and accurately register a dependent variable.

But in some cases a "field" (natural) experiment is the only possible way to obtain scientific information (e.g. in developmental psychology, ethology, social psychology, clinical or labor psychology, etc.). Proponents of natural experimentation argue that a laboratory experiment is an artificial procedure, producing "ecologically" invalid results, as if "taking" the subject out of the context of everyday life. But in field studies, there are immeasurably more errors and interference affecting the accuracy and reliability of data than in laboratory studies. Therefore, a natural experiment should be planned as close as possible to the scheme of the laboratory experiment and the results obtained in the "field" should be double-checked by more stringent procedures.

Experimental research in psychology, as in any other science, is conducted in several stages. Some of them are mandatory, some may be absent in some cases, but the sequence of steps is always the same.

Questions for discussion:

1. Classification of research methods.
2. Experiment and other methods on F.J. McGuigan.
3. Experiment and other methods on MV Metlin.
4. Types of experimental research.
5. The algorithm of experimental research.
6. Critical, aerobatic and field types of experiment.

The main stages of experimental research in psychology

1) Any experimental study begins with the **definition of** its **topic**. The topic limits the research area, the range of problems, the choice of subject, object and method. However, the first stage of the research itself is the **initial formulation of a subjective problem**. A researcher should clearly imagine what he or she is not satisfied with in contemporary psychological knowledge, where he or she feels the problems, what facts and regularities cannot be explained, what theories give contradictory explanations of human behavior, etc.

The empirical study is conducted in three main cases:

- to test the hypothesis of the existence of ***phenomena***;
- to test the hypothesis that the ***phenomena are connected***;
- to test the hypothesis ***of a causal connection***.

2. After the initial formulation of the problem comes the **stage of work with scientific literature**. The researcher should familiarize with empirical data obtained by other psychologists and attempts to explain the reasons for the phenomenon of interest. The **first step** represents ***search of*** definitions of basic ***concepts*** which are contained in psychological dictionaries, and also in dictionaries and encyclopaedias on adjacent disciplines. There are also references to major publications on the problem. The **next step** is to ***compile a bibliography*** on the topic of research using library systematic catalogues. The result of the work on the literary review is to clarify the problem, formulate a hypothesis and idea, plan the experimental research. It is possible to abandon the research because the problem may be unsolvable or, on the contrary, so researched that nothing new can be added to the existing results.

(3) The next stage is the **formulation of the scientific problem, scientific and experimental hypotheses and definition of variables**. The initial formulation of the problem is already implicitly suggesting variants of answers to it (i.e. hypothesis). Unlike the theoretical hypothesis, it is desirable to formulate an experimental hypothesis in the form of the following statement: "if ... ... then ... ».

4. The **hypothesis must be concretized and operationalized**. Operationalization means that the variables A and B included in the statement "if A, then B" should be controlled in the experiment. Namely, A must be controlled by the experimenter, B must be registered by the experimenter.

Definition of variables in terms of the experimental procedure and their operationalisation complete the stage of formulating the hypothesis. At the same time, the subject of the experimental study is specified.

It should be remembered that the researcher does not control the psychic reality itself, but the external parameters of the situation affecting the subject's psyche. When registering an independent variable, he assumes that ***there is a functional relationship between the independent variable and behavioral parameters***. This statement represents the ***main premise of*** any experimental psychological study. In addition to independent and dependent variables, external variables that may affect the dependent variable must be defined and operationalised.

5. According to the formulated hypothesis, a researcher should **choose an experimental toolkit** that would allow him to manage an independent variable and register a dependent variable. In this case, we are talking about a specific technique and measuring equipment of the psychological experiment. Besides, the conditions of the experiment should either exclude the influence of external variables or keep the constant of the value of their influence on the dependent variable. The nature of the equipment used is determined by the method chosen or designed by the experimenter.

6. **Planning for an experimental study** is the central stage of the entire research procedure. First of all, it is a matter of identifying external variables that may affect the dependent variable. Planning is necessary to ensure ***external and internal validity of the*** experiment. The next step is to choose an experimental plan. The specific plan depends on what the experimental hypothesis is, how many external variables you have to control in the experiment, what opportunities the situation offers for research, etc. With limited time and resources (including financial resources), choose as simple experimental plans as possible.

Complex plans are used to test complex hypotheses that require managing several independent variables and/or taking into account many additional variables. A researcher may conduct an experiment with one subject. In this case, he uses one of the study plans for one subject. If a researcher works with a group, they can select a series of plans using both experimental and control groups.

The simplest are the plans for two groups (basic and control). If more complex controls are required, plans for several groups are applied. The other option, which is often used in psychology, is a factor plan. It is used when it is

necessary to reveal the influence of two (or more) independent variables on one dependent. In this case, independent variables can have several levels of values. The simplest factor plans of type 2 x 2 or 2 x 2 x 2 imply the use of two and three independent variables with two levels of values, respectively.

A process classification of plans to study the relationship of the two variables was created by D. Campbell. In this case, the main ones are:

- a simple plan for two groups with pre-testing (test - impact - retest);
- a plan for two randomized groups without prior testing (randomization - impact - test);
- Solomon's plan for four groups combining both of these plans.

These plans are called plans for true experiments. In case the plan of true experiment is impossible (or not expedient) to realize, the researcher applies one of quasi-experimental plans.

7. The **selection and categorization of the test subjects** shall be conducted in accordance with the adopted experimental plan.

The choice of the population depends on the research objectives. The psychogenetics specialist chooses mono and disigot twins, as well as their parents and grandparents, as test subjects. When studying aggressive behavior in persons with post-traumatic stress syndrome, a medical psychologist investigates a sample of local war veterans.

All potential subjects are characterized by different gender, age, social status, educational level, health status, etc. In addition, they have different individual psychological characteristics, such as different levels of intelligence, neuroticism, aggressiveness. In order for the sample to represent (represent) the general population, potential subjects should be given equal chances to become real participants in the research. The technique of randomization is that all representatives of the general population are assigned an index and then randomly selected into a group of necessary numbers to participate in the experiment.

In this case, we have three groups:

1. the entire general population;
2. the randomization group from which the selection is made;

3. experimental randomized sampling.

One of the main requirements for the sample is representativeness. The sample should qualitatively and quantitatively represent the general population, the main types of potential subjects existing in the population. Test subjects must be correctly assigned to experimental and control groups so that all groups are equivalent. In addition, the researcher distributes the groups in relation to different experimental conditions so as to control or take into account possible effects of sequence, differential transfer, etc.

8. The **practical implementation of an experiment is** obviously the most responsible part of the study, requiring the researcher not only knowledge and skills but also the ability to experiment. Any idea, even the best idea, can be spoiled by sloppy experimentation. During the experiment, the researcher organizes a process of interaction with the subject, reads the instruction, and conducts a training series if necessary. He or she varies the independent variable and registers the subject's behavior, either on his or her own or with the help of an assistant. The experimenter interviews the subject at the end of the experiment (post-experimental interview).

Key milestones in the practical implementation of the experiment.

A) ***Preparation of the experiment***. The researcher prepares the experimental room and equipment. If necessary, several trial experiments are conducted to debug the experiment procedure. The most important point is to develop a revised instruction manual. It should consist of short sentences, each of which includes no more than 11 words. In the instruction, semantic blocks are highlighted with the help of paragraphs. It is checked on comprehensibility and simplicity, conducting preliminary experience on 5-10 subjects.

B) ***Instructing and motivating the subjects***. Instructions should include motivational components. The subject should know what opportunities are offered by participation in the experiment. This may include cash payments, the opportunity to get information about their abilities and personality traits, assistance in solving personal problems, etc. Since most subjects are not used to the situation of the experiment, they experience anxiety, and their attention may fluctuate. In addition, the speed of understanding the instruction depends on individual cognitive abilities, temperament, language skills, etc. Therefore, it is necessary to check whether the subjects have understood the instruction correctly and check it if necessary, however, avoiding additional detailed comments.

C) ***Experimenting***. The first step is to ensure that the subject is capable, healthy and willing to participate in the experiment. The experimenter should be preceded by instructions recording the order of his or her own actions during the experiment. Usually, an assistant will also participate in the experiment. He takes on auxiliary tasks. Most often, it is the assistant who keeps a protocol that records the test subject's answers. In addition, the assistant makes a general observation of the subject's behavior and condition, as well as any deviations from the standard procedure of the experiment. The assistant also monitors the operation of the equipment.

It is especially recommended to register additional signs of the subject's behavior, his emotional reactions in the course of the experiment. The necessary final stage is the post-experimental interview. At the end of the experiment, the subject should be interviewed and thanked for their participation in the study.

9. The **choice of methods of statistical processing**, its implementation and interpretation of results is the next stage of the study. The methods of data processing should be chosen ***at the stage of planning an experiment*** or ***when formulating an experimental hypothesis***. An experimental hypothesis in a natural science experimental study is transformed into a statistical hypothesis. Possible types of statistical hypotheses:

(a) The **similarities or differences between** two or more groups of subjects;

b) **on interaction of** independent variables;

(c) **On the statistical relationship between** NPs and STs;

d) **on the structure of latent variables** (correlation study).

It should be borne in mind that statistical assessments provide information ***not on the availability***, ***but on the validity of*** similarities and differences in experimental research results.

10. 10. **Conclusions and interpretation of** results complete the research cycle. The result of the experimental study is to confirm or disprove the hypothesis about the causal relationship between the variables: "If A, then B". Confirmation of statistical hypotheses is the decisive (but not the only) argument in favor of accepting the experimental hypothesis. Further, the researcher compares his conclusions with those of other authors and formulates hypotheses about causal similarities or differences between his own data and those of his predecessors. Finally, he interprets his conclusions in terms of a theoretical hypothesis. The experimenter must answer the question whether the confirmation or refuting of

an empirical hypothesis can be considered as a confirmation or refuting of a theory. It is quite possible that no theory can explain the results obtained.

In addition, the researcher makes assumptions about the possibility of transferring the data obtained to other situations, populations, etc.

Questions for discussion:

1. The stage of clarifying the hypothesis and defining variables.
2. The planning phase of the pilot study.
3. The stage of selecting statistical processing methods.
4. Stages of the practical implementation of the experiment.
5. The stage of selection and distribution of subjects into groups.
6. The stage of interpretation and conclusions.

2.6.2 An ideal experiment and a real experiment

The concept of "perfect experiment" was put into use by D. Campbell. An ideal ***experiment*** involves the experimenter changing only an independent variable, the dependent variable is controlled. The other conditions of the experiment remain unchanged. Ideal experiment assumes equivalence of subjects, invariability of their characteristics in time, "absence" of physical time itself, possibility to conduct and repeat the experiment infinitely.

The ideal experiment opposes the ***real experiment*** in which not only the variables of interest to the researcher, but also a number of other conditions change. The correspondence between the ideal experiment and the real experiment is expressed in its characteristic, such as ***internal validity***. Internal validity characterizes the degree of influence of change of independent variable on change of dependent variable. The more influence on change in a dependent variable of uncontrolled conditions, the lower is the internal validity of the experiment. Therefore, it is more likely that the facts discovered in the experiment are artifacts.

Planning of the experiment is necessary to control the validity. I.e. to increase the role of an independent variable in changing a dependent variable by

controlling other variables. Variables that are the source of artifacts are either eliminated or their effect is averaged by the application of appropriate experimental plans. High internal validity is the main sign of a good experiment (close to "perfect experiment").

But not all variables affecting the research result can be considered or excluded. Those of them which violate ***internal validity are*** called ***"side variables"***. Side variables that are completely unrecoverable include the influence of time factor, task factor and individual difference.

Validity. This notion is well analyzed in the system of relations ***"experiment - theory - reality"***. On the basis of theory, a hypothesis is put forward, which is ultimately tested in the experiment. The methods and plan of the experiment must correspond to the hypothesis being tested. The degree of this correspondence characterizes the ***operational validity***. In the experiment itself, we should take into account, eliminate, etc., the influence of side variables on the dependent variable as much as possible.

Internal validity characterizes the measure of influence of an independent variable on the dependent in relation to other factors. The higher the probability that the experimental effect (change in the dependent variable) is caused by the change of the independent variable.

The experiment must reproduce an external reality. An experiment that fully reproduces external reality is called a ***matching experiment***. Of course, full reality matching is unattainable. The measure of correspondence of the experimental situation of external reality characterizes the ***external validity of the*** experiment.

Additional variables that must be taken into account in the experiment affect the external validity. If the ***validity of*** experimental results depends on ***internal validity***, then the ***transferability of*** results from laboratory conditions to real processes and their generalization to other spheres of reality depends on ***external validity.***

The relationship between theory and reality is expressed in the degree of adequacy of theory and its prognosticity.

Д. Campbell introduced another important concept characterizing the validity of the experiment, namely, ***structural validity***. Structural validity expresses the adequacy of the ***method of interpretation of the*** connection between NP and

WP obtained in the experiment - the ***method of interpretation of the*** cause-and-effect connection formulated in a hypothesis (theory).

Thus, the ***internal validity*** determines the degree of reliability of the ***cause-and-effect ratio*** formulated in the scientific hypothesis (theory), and ***the structural validity*** determines the degree of adequacy of the ***cause-and-effect ratio*** transformation into the ratio of ***independent and dependent variables*** formulated in the experimental hypothesis.

Д. Campbell notes that internal validity control requires the elimination of alternative explanations of the relationship between dependent and independent variables, and structural validity control requires the elimination of alternative theoretical interpretations of the cause-effect relationship.

From D. Campbell's point of view, a good experiment must:

1. to identify the time sequence of the alleged cause and effect;

2. to show that probable causes and effects are interrelated (covariant);

3. to exclude the influence of side variables that could explain the experimental result;

4. to rule out alternative hypotheses about the theoretical explanations of this connection.

Internal validity is considered to be a prerequisite for any experiment. D. Campbell identified eight main factors that violate the internal validity of the experiment.

The first group. Sampling factors.

1. ***Breeding***. Non-equivalence of groups by composition, which causes a systematic error in the results.

2. ***Statistical regression***. A special case of selection error, when groups were selected on the basis of "extreme" indicators (correlation due to group heterogeneity).

3. ***Experimental sifting***. Uneven drop-out of subjects from compared groups, resulting in the groups' non-equivalence in composition.

4. ***Natural development***. Change of subjects, which is a consequence of the flow of time, without relation to specific events: change of state (hunger,

fatigue, sickness, etc.), human properties (age changes, accumulation of experience, etc.).

Group two. Side variables, whose effects lead to the following effects:

1. ***The effect of "history."*** Specific events that occur between the initial and final testing, in addition to experimental effects.

2. ***The effect of testing***. Effect of preliminary testing on the final result.

3. ***Instrumental error***. It is determined by the reliability of the method of fixation of the test subject's behavior, i.e. the reliability of the test. It is reliability that affects the validity, according to D. Campbell, and not vice versa.

4. ***Interaction of factors:*** selection; natural development; history (different stories of experimental groups), etc.

Questions for discussion:

1. It's a perfect and real experiment.
2. Validity of the experiment.
3. Types of variables.
4. The tasks of the "good" experiment.
5. The first group of factors disturbing the internal validity.
6. The second group of factors disturbing the internal validity.

2.6.3. Real experiment and "full compliance experiment".

A real experiment is different from both the ideal experiment and the reality it simulates. This difference is expressed by the notion of "external validity" of psychological experiment. The external validity testifies to what extent the results obtained in the experiment will correspond to the life situation that served as the "prototype" for the experiment. Besides, external validity characterizes the possibility of generalization and transfer of results obtained in the experiment to the whole class of life situations.

External validity, as defined by R. Gottsdanker, first of all affects the reliability of the conclusions, which are given by the results of the real experiment, in comparison with the experiment of full conformity. To achieve high external validity, it is necessary that the levels of additional experimental variables correspond to their levels in reality. An experiment that has low external validity is considered wrong.

It should be kept in mind that an experiment ***is*** always ***wrong if the source of the hypothesis is reality, ordinary knowledge, not theory***.

An experiment that does not correspond to reality may have perfect internal and operational validity. It is another thing that direct transfer of its results to reality is impossible without taking into account the influence on the dependent variable in addition to the independent variable and additional variables. Obviously, achieving full external validity is impossible in principle, so any "pure" analytical study is outwardly invalid. At the same time, it is recommended to take into account the impact of additional variables on the experimental effect as much as possible.

The care about the external validity of the experiment is especially taken by researchers working in applied fields: clinical psychology, pedagogical and organizational psychology. In order to solve their problems, they most often have to resort to experiments that simulate reality. As a matter of fact, it may be considered that the historical discussion of the supporters of laboratory experiment and natural experiment was a reflection of the different methodological approach of specialists engaged in fundamental and applied psychology.

At present, the factors influencing the external validity are considered to be unrecoverable features of the experiment that distinguish it from the real situation. D. Campbell puts an equality sign between the external validity, representativeness of the experiment and the possibility of generalization of its results. First of all, he refers to the factors threatening external validity, the effects associated with the peculiarities of the psyche as an object of research: learningability, the presence of memory, the ability to respond emotionally to situations. D. Campbell names the main causes of external validity disorder.

1. ***The effect of testing***. Reduced or increased susceptibility of test subjects to experimental effects. For example, prior control of students' knowledge may increase their interest in new learning material. Since the general population is not subject to pre-testing, the results may not be representative.

2. ***Terms of research***. They cause the subject to react to the experiment. Therefore, his data cannot be transferred to people who did not participate in the experiment. Such people are the entire general population, except the experimental sample.

3. ***Interaction of selection factors and the content of*** experimental impact. Their effects are artifacts (in experiments with volunteers or subjects participating under duress).

4. ***Interference of experimental influences***. Test subjects have memory and learning ability. If an experiment consists of several series, the first effects do not pass for them without a trace and affect the appearance of effects from subsequent effects.

The majority of the reasons of external validity disorder are connected with features of psychological experiment carried out with participation of the person which distinguish psychological research from experiment carried out by experts of other sciences.

R.L.Solomon was the first to pay attention to the interaction between the testing procedure and the content of experimental exposure in 1949 when conducting a study of schoolchildren. Pre-testing reduced the effectiveness of learning. The study of social attitudes showed that preliminary testing influenced a person's attitudes and his or her susceptibility to persuasion. In Hovland's experiments, on the contrary, it weakened the persuasive effect of films.

The resulting effect is greater the more unusual the testing procedure is and the more similar in content the experimental effect is to the test. In order to avoid the effect of pre-test, Campbell recommends using experimental plans with groups not pre-tested.

As already noted, the interaction of group composition and exposure is associated with non-random participation of subjects in the experiment. The reaction can be of two types: readiness of volunteers to be exposed and refusal, negative reaction of those who are forced to participate in the experiment. Withdrawal of subjects during an experiment may be caused by experimental exposure. For example, subjects who fail on achievement motivation assignments may choose not to participate in subsequent series.

The problem of internal validity is solvable in principle, because it is possible to select appropriate procedures for planning the experiment and mathematical processing of the results to ensure a given level of reliability. According to

D.Campbell, the ***problem of external validity*** as representativeness of the experiment in relation to reality is fundamentally ***insoluble,*** because inductive generalization can never be completely objective.

The problem of external validity as an adequacy of the situation of the experiment corresponding to it life situation is also unsolvable by logical and mathematical means. It requires involvement of the whole set of scientific psychological knowledge to describe the situation as a whole.

Questions for discussion:

1. The experiment of full conformity.
2. Factors that compromise external validity.
3. Reasons for external validity disorder.
4. Interaction of testing procedure and content of experimental exposure
5. Solvability of internal validity problem.
6. Insolvability of external validity problem.

2.6.4 Experimental sample

The choice of the object of research is the next task that the psychologist will have to solve after defining dependent and independent variables. In applied research, the psychologist's freedom is already limited by the fact that the object is known from the very beginning. In principle, the psychologist is free to choose the object that more corresponds to the tasks of research.

The ideal object of psychological research can be either the psyche of an individual or the psyche of a group. In the first case, we are talking about general psychological experiment, in the second case - about socio-psychological experiment. But in a concrete experiment, not only the real situation should correspond to the characteristics of the ideal situation, but the results obtained in the real situation should be applicable to all other situations. If all people were similar to each other, or were absolutely the same, there would be no problems.

The experiment can be conducted with one subject and the results can be used to explain the behaviour of all other subjects. But people are different by age, sex, race, nationality, culture or religion, social or economic status, etc. Consequently, it is not possible to simply generalize the data obtained by examining one subject.

The single test subject experiment is conducted when:

1. individual differences can be neglected, the study is extremely large in volume and includes many experimental samples;

2. The subject's psyche is a unique object, such as the psyche of a brilliant musician or a creatively gifted chess player;

3. the subject is required to be particularly competent in conducting the study (experiment with trained subjects);

4. It is impossible to repeat this experiment with other subjects.

Special experimental plans have been developed for experiments with one subject. But most often the research is conducted with an experimental group in which all subjects are objectively different, but selected and divided into subgroups using a strategy.

There are four basic design options for experimental groups.

First option. The study is conducted with two different groups, the experimental and the control groups, which are put in different conditions. This is the most common method.

Option two. It involves the study of one group. Its behavior is studied under both experimental and control conditions. It is applied when there is only an experimental group and it is impossible to form a control group. But this plan does not control the "sequence effect" in any way and is used only in those rare cases when the sequence effect can be neglected.

The third option. Use of design of groups by a method of "pair design". For each participant of group the equivalent (or similar) to it participant is selected. They are distributed to different groups. Accordingly, the control and experimental groups become similar in the composition of subjects. In this case, it is impossible to observe full equivalence of the groups in both experimental conditions. However, this method is much better than an experiment involving one group in different conditions.

Option four. It's mixed. All groups are put in different conditions. Several groups are formed in this case. This method is used in factorial planning of the experiment.

The formation of a sample of subjects (experimental group) should be based on the following three criteria.

1. ***Content criterion*** (criterion of ***operational validity***). Operational validity is determined by the correspondence of the ***experimental method*** - the ***hypothesis being tested***. The selection of the experimental group should be determined by the subject and hypothesis of the research. Thus, the experimenter should create a model of an ideal psychic carrier for experimental research in his own case and, if possible, describe it following this description when forming the experimental group. The characteristics of the real experimental group should deviate minimally from those of the ideal experimental group.

2. ***Criterion of test subjects equivalence*** (criterion of ***internal validity***). The results obtained from the study of an experimental sample should be extended to each participant. That is, it is necessary to take into account all significant characteristics of the studied psyche whose differences in expression can significantly affect the dependent variable. The procedure of random selection of equivalent groups and equivalent subjects is called randomization.

3. ***Criterion of representativity*** (criterion of ***external validity***). The group of people involved in the experiment should represent the entire population to which we can apply the data obtained in the experiment. The size of the experimental sample is determined by the type of statistical measures and the chosen validity of the acceptance or rejection of the experimental hypothesis. It can be equal to the whole set of people whose behaviour we are interested in. But an experimental sample can represent only a part of the set of people we are interested in.

The selection of the pilot group is done through various strategies. The task of selection is, ***first of all,*** to control the "mixing effect". It means the influence of individual differences between subjects on the relationship of independent and dependent variables. For example, people of different temperaments react differently to public rewards for good work. In this case, the influence of a side variable on internal validity is controlled. ***Secondly***, the experimental group should represent the population under study, i.e., provide external validity of the experiment.

The use of actually existing groups generates a systematic mixing of the independent variable with the individual properties of subjects.

Compiling a representative group of subjects whose characteristics correspond to those of the population of interest. Sometimes it is impossible to find a way to create a representative group. In this case, the approximate modeling method is used.

Simulation of the population by random selection (randomization). An experimental sample is made so that each person is given an equal chance to participate in the experiment. Each person is assigned a number. With the help of a random number table, an experimental sample is formed. But this procedure is difficult to implement, because each representative of the population we are interested in must be taken into account. In practice, they resort to simpler methods of random selection.

Stratometric selection method. The general population is considered to be the population of groups with certain characteristics. Test subjects with relevant characteristics are selected for the experimental sample so that participants from each stratum are equally represented. The most commonly used characteristics are gender, age, political preferences, education and income levels. This strategy is most often used by psychodiagnosticians in test design, as well as pedagogical psychologists. It is mainly used by sociologists and social psychologists in public opinion polls, social attitudes research, etc.

The strategy of paired selection. The experimental and control groups are composed of subjects equivalent in terms of significant experimental side-parameters. Ideal is to use twin pairs. A variation of this strategy is the selection of homogeneous groups, in which the subjects are equated in all characteristics, except for the additional variables of interest to the researcher. Another option is to select a significant additional variable. All subjects are tested and ranked by the level of severity of the variable. Groups are formed so that subjects with the same or similar values of a variable are placed in different groups.

There are ***six strategies for*** constructing groups:

1. randomization;
2. a pair selection;
3. randomization with stratum extraction (stratometric selection);

4. approximate modeling;

5. representative modelling;

6. to involve real groups.

There are two main ways of attracting subjects to the group: a) selection; b) distribution. ***Selection*** is performed at randomization; at randomization with stratum isolation, at representative and approximate modeling. ***Distribution*** is carried out at drawing up of groups from equivalent pairs and research with participation of real groups.

It is believed that the best external and internal validity is achieved with a strategy for matching equivalent pairs and stratometric randomization. The individual features of the subjects are controlled to the maximum extent possible using these strategies. In other cases, there are no guarantees of the subjects' equivalence, controllability of individual differences and group representation.

Involvement of volunteers or forced participation in the experiment violates the representativeness of the sample. The strategies of paired modeling, approximate modeling and stratometric randomization, in contrast to the strategy of randomization (random selection of groups), assume that an additional parameter (individual features) is known, which can have a significant impact on the result of the experiment. An error in the selection of this parameter and / or missing other parameters lead the experimenter to failure.

The independent problem is the number of experimental samples. Depending on the goals and possibilities, it can range from one subject to several thousand people. The number of subjects in a single group (experimental or control) in most experimental studies ranges from 1 to 100. It is recommended that the number of compared groups should be ***at least 30-35*** for statistical reasons.

If factor analysis is used for data processing, there is a simple rule. Reliable factor solutions can only be obtained if the number of test subjects is at least ***three times*** greater than the number of registered parameters. It is reasonable to increase the number of test subjects by at least 5-10% more than required, since some of them will be "rejected" during the experiment or during the analysis of experimental protocols (they did not understand the instruction, did not accept the task, gave "deviant" results, etc.).

Questions for discussion:

1. Conditions of the experiment with one test subject.
2. Four designs of experimental groups.
3. Criteria for forming a sample of test subjects.
4. Strategies for selecting the experimental group.
5. Six test subject group design strategies.
6. There are two main ways to bring the subjects into the group.

2.7. EXPERIMENTAL VARIABLES AND METHODS OF THEIR CONTROL

In experimental psychology, the experimenter tests the hypothesis of a causal relationship between two phenomena. The concept of "causality" is one of the most complicated in science. There are a number of empirical signs of a causal relationship between the two phenomena.

First sign. The ***separation of*** cause and effect in time and ***the precedence of*** cause and effect. If a researcher discovers changes in the subject's psyche after the experimental exposure, compared to a similar person who has not been exposed, he has reason to believe that the experimental exposure caused the change in the subject's psyche. The presence of exposure and comparison of the subjects' psyche are necessary conditions for such a conclusion, since not always the previous event is the cause of the subsequent one *(the geese flying southwards is not the cause of snowing after a month).*

Second sign. Presence of ***statistical connection*** between two variables (between independent and dependent variables). A change in the value of one of the variables must be accompanied by a change in the other. In other words, there should be a linear (or nonlinear) correlation between the variables. But existence of correlation is not enough condition for conclusion about cause-effect relation, because correlation can be either random or conditioned by the third variable.

The third sign. A cause-effect relationship is recorded if the experimental procedure excludes other possibilities to explain the relationship of the two variables than the causal one, and all other alternative causes of the consequence are excluded. The experimental hypothesis of a causal relationship between two phenomena is tested as follows. The experimenter simulates the assumed cause.

It acts as an experimental influence (independent variable). And the consequence as a dependent variable (change of state of mind) is registered with the help of some measuring instrument.

Experimental impact serves to control an independent variable that is a direct factor in changing the dependent variable. In an experimental situation, the experimenter must control the external variables. Among the external variables, the experimenter should control:

1. ***side variables***. They generate a systematic shift of the experiment result leading to unexpected data *(time factor, problem factor, individual features of the subjects);*

2. ***additional variables***. They are essential for the link between cause and effect under study. Therefore, when testing a private hypothesis, the level of the additional variable should correspond to its level in the studied reality. The additional variable that is especially significant for the experiment is called the ***"key" variable***. A ***"control" variable is an*** additional variable that becomes the second main variable in a factor experiment.

The essence of the experiment is that the experimenter, by varying the independent variable, registers the change in the dependent variable and controls the external variables. Researchers distinguish different types of independent variables: qualitative and quantitative.

Among the dependent variables, the base variables are distinguished. A base variable is the only dependent variable that is affected by a change in an independent variable.

Questions for discussion:

1. The first sign of a causal connection.
2. The second sign of a causal connection.
3. The third sign of a causal connection.
4. Psychological feature of side variables.
5. Psychological feature of additional variables.
6. Psychological feature of control and key variables.

2.7.1 Independent variable

The researcher should seek to change only an independent variable in the experiment. An experiment where this condition is met is called a ***"pure" experiment***. But most often in the course of an experiment, while varying one independent variable, the experimenter changes a number of others at the same time. This change can be caused by the experimenter's action and is caused by the connection of variables.

The central problem with the experimental study is the isolation of an independent variable and its isolation from other variables. Independent variables may act as independent variables in a psychological experiment: 1) characteristics of tasks; 2) peculiarities of the situation (external conditions); 3) manageable peculiarities (states) of the subject.

Peculiarities (conditions) of the subject are often called ***"body variables"***. Sometimes a fourth type of variables is distinguished - constant characteristics of a subject (intellect, sex, age, etc.). But they belong to additional variables, because they cannot be influenced, but their level can only be taken into account when forming experimental and control groups.

The characteristics of the task are what the experimenter can manipulate more or less freely. Traditionally, from behaviorism, it is believed that the experimenter only varies the characteristics of the stimuli, but in fact, he has much more at his disposal.

The experimenter can vary the stimuli or material of the task, change the type of the subject's response (verbal or non-verbal), change the grade scale, etc. He or she may vary the instruction, changing the goals that the subject must achieve during the assignment. The experimenter may vary the means the probationer uses to solve the task and put obstacles in front of the probationer. He or she may change the system of rewards and penalties while performing the task, etc.

The peculiarities of the experimental situation should also include those variables that are not directly included in the structure of the experimental task performed by the subjects. These variables may be room temperature, setting, presence of an outside observer, etc.

Experiments to identify the effect of social facilitation (social reinforcement) were conducted according to the following scheme: the subject was given a

sensomotor or intellectual task. He first performed it alone and then in the presence of another person or several people. The change in the subjects' productivity was assessed. In this case, the subject's task remained unchanged, only the external conditions of the experiment changed. What in this case can the experimenter vary?

First, the physical parameters of the situation: the location of the equipment, the appearance of the room, lighting, sounds and noises, temperature, furniture placement, wall painting, time of the experiment (time of day, duration, etc.). That is, all physical parameters of the situation, which is not a stimulus.

Secondly, peculiarities of communication and interaction between the subject(s) and the experimenter. Judging by publications in scientific journals, in recent years, the number of experimental studies, which apply a variety of external conditions has increased dramatically.

Physical, biological, psychological, socio-psychological, and social characteristics of subjects are referred to as ***"organism variables"*** or unmanaged characteristics of subjects. Traditionally, they are referred to as "variables", although most of them are invariable or relatively invariant throughout life. The influence of differential-psychological, demographic and other constant parameters on individual behaviour is studied in correlation studies. However, the authors of most textbooks on the theory of psychological method refer these parameters to the independent variables of the experiment.

As a rule, in modern experimental research differential psychological features of individuals, such as intellect, sex, age, social status, etc., are taken into account as additional variables controlled by the experimenter in a general psychological experiment. But these variables can turn into a "second main variable" in a differential-psychological study, and then a factor plan is used.

Questions for discussion:

1. Psychological features of the "pure" experiment.
2. Types of independent variables in the experiment.
3. Psychological characteristics of organism variables.
4. Physical parameters of the situation as an independent variable.
5. Features of communication as an independent variable.

6. Psychological features of additional variables.

2.7.2. Dependent variable

Natural science psychologists deal with the behaviour of the subject. Therefore, parameters of verbal and non-verbal behavior are chosen as a dependent variable. These include: time spent by the subject in solving a task, changes in facial expressions when watching an erotic movie, time of motor reaction to a sound signal, etc.

The choice of the behavioral parameter is determined by the initial experimental hypothesis. The researcher should make the hypothesis as specific as possible. That is, to achieve that the dependent variable is operationalized - can be registered during the experiment. The behavior parameters can be conditionally divided into formal-dynamic and substantial. Formal-dynamic (or spatio-temporal) parameters can be easily registered by hardware. Such parameters can include the following.

1. ***Exactly***. The most frequently recorded parameter. As the majority of tasks shown to the subject in psychological experiments are tasks for achievements, accuracy or the opposite parameter - error of actions - will be the main registered parameter of behavior.

2. ***Latency***. Mental processes take place secretly from an outside observer. The time from the moment the signal is presented until you start selecting a response is called latency time. In some cases latent time is the most important characteristic of a process, for example, when solving thought problems.

3. ***Duration***, or execution ***speed***. It is a characteristic of an executive action. The time between selecting an action and the end of its execution is called the speed of the action (as opposed to latent time).

4. ***Tempo,*** or ***frequency of*** action. The most important characteristic, especially when investigating the simplest forms of behaviour.

5. ***Productivity***. The ratio of the number of errors or the quality of the action to the time of execution. Serves as the most important characteristic in the study of learning, cognitive processes, decision making, etc.

Content parameters of behavior assume initial categorization (typology) of behavior forms either in terms of ordinary language or in terms of the theory

whose assumptions are tested in this experiment. The recognition of different forms of behaviour is the work of specially trained experts and observers. It takes a lot of experience to unmistakably distinguish different levels of aggression or surprise, to characterize one act as a manifestation of submission and another as a manifestation of subservience.

The problem of fixation of qualitative peculiarities of behavior is solved by: a) ***training of*** observers and development of observation maps; b) ***measurement of*** formal and dynamic peculiarities of behavior with the help of tests. The dependent variable must be valid and reliable. Reliability of a variable is manifested in the stability of its registration under the same experimental conditions at repeated registration. Validity of a dependent variable is determined only under specific conditions of the experiment and in relation to a specific hypothesis.

There are three types of dependent variables: 1) one-dimensional; 2) multidimensional; 3) fundamental.

In the first case, only one parameter is registered, and it is considered to be a manifestation of a dependent variable.

In the second case, the dependent variable can only be described by several parameters simultaneously. For example, the level of intellectual productivity is manifested in the time of solution of the problem, its quality, the difficulty of the solved problem. These parameters can be fixed independently.

In the third case, when a relationship between individual parameters of a multidimensional dependent variable is known, the parameters are considered as arguments and the relationship itself is considered as a dependent variable.

There is another important property of a dependent variable - its ***sensibility*** (sensitivity) to changes independent. The fact is that a change in the independent variable affects the change in the dependent variable. If we change the independent variable, but the dependent variable does not change, it means that the dependent variable is not sensible to change the independent variable.

Two variants of manifestation of non-sensitivity of a dependent variable were named ***"ceiling effect"*** and ***"floor effect"***. The first case occurs when the presented task is so simple that its execution level is much higher than all levels of the independent variable. The second case, on the contrary, occurs when the task is so complicated that its execution level is lower than all possible levels of the independent variable.

As with other components of psychological study, the dependent variable must be valid, reliable, and sensitive to changes in the level of the independent variable. There are two main methods for registering changes in the dependent variable.

The first method is most often used in experiments involving a single subject. A change in the dependent variable is recorded ***directly during the experiment*** following a change in the level of the independent variable.

The second method of registering a change in the level of an independent variable is called ***delayed measurement***. A certain period of time passes between exposure and effect. Its duration is determined by the time of distance of the effect from the cause.

Questions for discussion:

1. Behavior as a dependent variable.
2. Formal dynamic behavior parameters.
3. Substantive behavioral parameters.
4. Three types of dependent variables.
5. Sensitivity of the dependent variable.
6. "Floor effect" and "ceiling effect."

2.7.3 Relationships between variables

The construction of modern natural science experimental psychology is based on the formula of K. Levin: behavior is a function of personality and situation B = f (P;S). Neobehaviorists put in the formula instead of P (personality) - O (organism), which is more accurate if we consider not only people but also animals to be tested, and the personality to be reduced (reduced) to the body.

Most experts in the theory of psychological experiment, in particular McGeagan, believe that there are only two types of laws in psychology: 1) "stimulus - response"; 2) "organism - behavior". The ***first type of*** psychological laws is revealed in the course of experimental research when the stimulus (task, situation) is an independent variable and the dependent variable is the subject's

answer. The ***second type of*** psychological laws is a product of a method of observation and measurement, as properties of an organism cannot be controlled by psychological means.

In a classical natural-scientific psychological experiment, the functional dependence of the species is established: R = f(S), where R is the answer and S is the situation (stimulus, task). Variable S varies systematically, and the changes in the subject's response that it determines are recorded. In the course of the study, the conditions under which the subject behaves in one way or another in a certain way are revealed. The result is recorded as a linear or nonlinear dependence.

Another type of dependencies is the dependence of behavior on personal properties or states of the subject's body: R = f(O), or R = f(P). The dependence of the subject's behaviour on this or that state of the organism (disease, fatigue, activation level, frustration of needs, etc.) or on personal properties (anxiety, motivation, etc.) is investigated. Research is carried out with the participation of groups of people who differ by this characteristic.

The above dependencies are the simplest forms of relationships between variables. More complex dependencies are also possible, which are established in a particular experiment. For example, factor plans allow us to reveal dependencies of type R = f(S1,S2), when the subject's response depends on two varying parameters of the situation, and behavior is a function of the body and environment.

In general, K.Levin's formula expresses the ideal of experimental natural-science psychology. It makes it possible to predict the behavior of a particular person in a certain situation. The variable "personality", which is part of this formula, can hardly be considered only as "additional". The tradition of neoheviorism suggests using the term "intermediate" variable. Recently, the term "moderator variable" (intermediary variable) has been assigned to such "variables" (personality properties and states).

There are at least six main possible relationships between independent and dependent variables.

1. ***No addiction***. The dependent variable is not sensitive to changes in the independent variable.

2. ***Monotonically increasing dependence is*** observed when the increase in values of the independent variable corresponds to the increase in values of the dependent variable.

3. ***Monotonically decreasing dependence is*** observed if the increase of values of the independent variable corresponds to the decrease of values of the dependent variable.

4. ***Non-linear addiction.*** It is found in most experiments, which reveal peculiarities of psychic regulation of behavior. It has a non-linear form.

5. ***The inverted U-dependence*** is obtained in numerous experimental and correlation studies both in personality psychology, motivation and social psychology;

6. ***Complex quasi-periodic dependence of the*** level of dependent variable on the level of independent.

Questions for discussion:

1. K.Levin's formula as the basis of natural-science experimental psychology.
2. Two types of laws of natural science psychology.
3. Basic types of relationships between dependent variables.
4. Inverted relationship between dependent variables.
5. Monotonic relationship between dependent variables.
6. A non-linear relationship between dependent variables.

2.7.4. Control of variables

A distinction must be made between the control of an independent variable and the control of external (side and additional) variables. Control of an independent variable in the ***first sense*** consists in its active variation or knowing the regularities of its change. ***In the second sense, the*** notion of "control" is to control external variables of the experiment.

There are two main ways to control an independent variable. These methods are the basis for two types of empirical research: active and passive. In psychology, the active methods include the activity method (experiment) and the communicative method (conversation), while the passive methods include observation and measurement.

In the experiment, the control of an independent variable is carried out with active variation. During observation and measurement, the control is carried out by selecting (breeding) the required values of the independent variable from among the already existing variables independently of the researcher.

There are several basic methods to control the influence of external variables on the result of the experiment:

- elimination of external variables;
- the constancy of the conditions;
- balancing;
- counterbalancing;
- randomization.

2.7.5 Ways of controlling the external variable

1. Elimination. An experimental situation is controlled in such a way as to exclude any presence of an external variable in it. For example, psychophysical laboratories often create experimental chambers that isolate the subject from external sounds, noise, vibration and electromagnetic fields. But it is often impossible to eliminate the influence of external variables.

2. Creation of constant conditions. If external variables cannot be excluded from the experimental situation, the researcher has to make them invariable. In this case, the effect of the external variable remains constant on all subjects, for all values of the independent variable and throughout the entire experiment. However, this strategy does not allow to avoid the mixing effect completely. Data obtained with constant values of external variables can be transferred only to those real situations where values of external variables are the same as they were in the research. However, this does not guarantee a mixing effect either.

The research technique and equipment of the experimental premises (sounds, fragrances, wall painting, type of fittings, location of furniture, etc.) should be standardized. The researcher aspires to equilibrium additional variables by constant variables, i.e. to equilibrium groups of test subjects on the basic individual characteristics significant for research (level of education, sex, age).

The experimenter should present the instruction equally to all subjects. He should strive to keep the intonation and power of the voice unchanged. It is recommended that instructions be recorded on a tape recorder and that the recording be presented (except in special cases).

3. Balancing. In cases where it is not possible to create the constant conditions of the experiment or the constant conditions are insufficient, apply the technique of balancing the effect of external variables. Balancing is used in two situations: 1) when it is impossible to identify an external variable; 2) when it is possible to identify it and use a special algorithm to control this variable.

A way to balance the influence of non-specific external variables. This means that in addition to the experimental group, a control group is included in the plan of the experiment. The study of the control group is conducted under the same conditions as the experimental group. The difference is that only the subjects included in the experimental group are subject to experimental effects. Thus, change in the dependent variable in the control group is due to external variables only, while in the experimental group it is due to the joint action of external and independent variables. In this case, it is impossible to distinguish the specific impact of each external variable and the specifics of the impact of an independent variable due to the effect of interaction of variables. In order to determine how this or that external variable affects the dependent variable, a plan including more than one control group is used. In general, the number of control groups in the experimental plan should be $N = n + 1$, where n - the number of external variables. The second control group is placed in experimental conditions, where the action of one of the external variables affecting the dependent variable of the experimental and control groups is excluded. The difference in the results of the 1st and 2nd control groups allows to highlight the specific impact of one of the external variables.

The balancing procedure is slightly different when controlling known external variables. Typical consideration of such a variable is to determine the level of influence of the test subjects' gender identity on the results of the experiment, since it is known that many data obtained in the male sample cannot be transferred to the female sample. Sex is an additional variable, so planning is

limited to identifying the effect of an independent variable on the dependent in each of the two experimental groups. Similarly, an experiment is constructed to compare the effect of different hardware techniques depending on the age of the subjects, etc.

4. Counterbalancing. This technique of controlling an additional variable is most often used when the experiment involves several series. A subject is sequentially placed in different conditions and previous conditions may alter the effect of subsequent conditions. For example, in a differential hearing sensitivity study, it is important to consider whether the subject was exposed first and second to a loud or softer sound. Also when performing intelligence tests, the order in which the test person is asked to perform the task is important: from simple to complex, or from complex to simple. In the first case, the more intelligent test subjects are more tired and lose motivation, as they have to solve more problems than the less intelligent. In the second variant of presentation of tasks less intellectual test subjects experience stress of failure and are compelled to solve more problems, than their more intellectual colleagues. In these cases counterbalance is used to eliminate the effects of sequence and effect. Its meaning is that one order of presenting different tasks, stimuli, and effects in one group is compensated by another order of presenting tasks in another group.

Counterbalancing is used when several series of studies can be conducted. It should be noted that a large number of experiments may cause the subject to become tired. But this plan allows you to control the effect of the sequence. However, counterbalancing does not completely eliminate the effect of changing the order of assigning tasks on the value of the dependent variable. It is called differential transfer. Transition from situation 1 (when it is created first) to situation 2 is different from transition from situation 2 (when it is created first) to situation 1. This effect causes real differences between two different experimental situations to be exaggerated during registration.

The counterbalance technique is that each subject receives more than one exposure option, and the effect of the sequence is purposefully distributed to all experimental conditions.

When balancing, each subject is given only one experimental effect. The external variable is balanced by identifying the effect of its action on the members of the experimental group as compared to the effect obtained in the control group. A subject may end up only in the experimental group or only in the control group and be influenced by an external variable in both groups. If

balancing is used in studies of independent (non-contiguous) groups, then counterbalancing is used in studies with repetitive effects (linked groups).

5. Randomization. Randomization is a random selection procedure that guarantees equal opportunity for each member of the population to participate in the experiment. Each test subject is assigned an ordinal number and the test subjects are selected in the experimental and control groups using a random number table. Randomization is a way to exclude the influence of individual features of subjects on the result of the experiment. Randomization is applied in two cases: 1) when it is known how to control external variables in an experimental situation. However, we do not have an opportunity to use one of the previous control techniques; 2) when we assume to operate on some external variable in an experimental situation, but we cannot specify it and apply other techniques.

Assuming that the value of the additional variable(s) is subject to probabilistic laws, the experimental and control groups will comprise a sample that has the same levels of additional variables as the general population.

According to many specialists, including D. Campbell, group equalization by means of randomization procedure is the ***only reliable*** way to exclude the influence of external (additional) variables on the dependent. D. Campbell defines randomization as a universal method of group equalization before experimental influence. Other methods, for example, paired comparison method, are characterized by it as unreliable and leading to invalid conclusions.

Questions for discussion:

1. The two meanings of controlling an independent variable.
2. Eliminating.
3. Creating constant conditions.
4. Balancing.
5. Counterbalanced.
6. Randomization.

2.8. VALIDITY OF PSYCHOLOGICAL EXPERIMENT

2.8.1 Validity and use of mental samples of the experiment

Evaluation of the validity of real psychological experiments is made by using mental samples of the experiment. The concepts of ***"mental experiment"*** (ME) and ***"mental sample" of the*** experiment should not be confused. ME can be understood, first, as the accepted norm of psychologist's reflections at all stages of following the logic of the experimental check of the psychological hypothesis. Secondly, ME can be considered in the context of the psychologist's use of mental samples for the purpose of evaluating threat control and the conclusion about empirical dependence. Thirdly, DOE may represent such an experiment, which is unrealizable due to lack of means for operationalization of variables, adoption of certain ethical norms or economic considerations, etc.

In the first two cases, the method of mental experimentation is to discuss an experimental model that sets the relationship between the variables of interest to the researcher when analyzing the actual or planned for real data collection experiment. In the third case, DOE may present a scheme of a deliberately unrealizable study. However, it presents the way the conclusions could be organized if the researcher had access to the supposed methods of experimental control.

When planning a psychological experiment, ME can also be understood as the course of the experimental activity, outwardly really deployed in the stages of the experiment. As a matter of fact, all stages of planning an experiment are variants of mental experimentation in order to determine the best forms of experimental control, to choose the best of possible experimental plans.

Along with planning, the function of a mental experiment is to justify or evaluate the validity of the actual experiments. Mental samples, in relation to which properties of really carried out experiment are estimated, allow to discuss the basic aspects of "correctness" of construction of experimental model. Correctness means only the degree of approximation to the best mental embodiment of experimental conditions corresponding to a specific experimental hypothesis.

The experimenter may correctly or incorrectly select and justify variables, methods as means of operationalization of these variables. The experimenter

may introduce NP mixtures with other variables or successfully avoid mixtures. He or she may obtain more or less reliable data by setting a number of samples for each NP condition; he or she may ensure that the conditions of side variables are randomly scattered across NP levels or fail to control unsystematic variability (NP, WP, side factors).

It is this kind of reading of functions of mental samples is presented in the concepts of ideal and infinite experiments, experiments of full conformity and perfect experiment. All these four terms serve to clarify the criteria according to which it is necessary to evaluate the success of planning, organizing and conducting an experiment that is actually carried out.

"Mental experiment" is one of such standards, using which a psychologist can answer many questions about the level of empirical support of the causal hypothesis being tested. However, there is no recipe for how to use these norms in each case. Sometimes they must be abandoned if the type of research is not such that the established norms of the experimental assessment of the hypothesis can be applied to it. However, this refusal should not be confused with an inability to properly organize and conduct a psychological experiment.

2.8.2 Types of validity in assessing the effectiveness of a psychological experiment

Types of validity are means of comparing real experiments with their mental samples. Evaluation of validity is related both to the evaluation of the implementation of the selected forms of experimental control, and evaluation of the system of inferences in the organization of research in terms of compliance with the standards of the experimental conclusion. Norms related to possible generalizations from psychological experiments imply the difference of types of validity.

Internal and external validity - aspects of the right experiment, whether it is an experiment for scientific or practical purposes, are necessarily discussed. The differences in the conclusions from these experiments will concern how generalizations are constructed: whether they are transposed to other people, other situations, activities or the theoretical model.

The internal validity of the experiment provides the conclusion only about the investigated dependence. That is, the relationship between the independent and

dependent variables, but nothing informs about the possibility of its propagation beyond the limits of this experimental situation. If the obtained data are characterized by unreliability or the presence of mixtures (systematic, nonsystematic, concomitant), then the statement formulated in the experimental hypothesis cannot be considered reasonable, even if the experimental effect corresponding to it is obtained.

If the internal validity of the experiment is evaluated highly, it also does not follow that the experimentally established dependence corresponds to anything in reality.

External solidity, which involves solving compliance problems, provides the ability to generalize into those types of situations or activities to which experimentally controlled variables correspond. When it comes to theoretical generalizations, the assessment of external validity gives way to the assessment of ***structural validity***. Solving the questions about structural and external validity leads a researcher to choose a type of experiment: natural ("duplicating the real world"), artificial (improving the real world) or laboratory.

The notion of structural validity covers the corresponding aspect of estimating the correctness of the experiment's construction, as far as the transition from notions (psychological constructs) presented in the theoretical hypothesis to their empirical representations in the form of NP, WP, DP was justified. And also to what extent the explanation of the established dependence really follows from the theory presented by the author. Even before the choice of concrete methods or already at their substantiation the experimenter carries out transition from theory to empiricism which is connected with plurality of experimental embodiments of theoretical statements.

The evaluation of the validity of the transition from theoretical positions to the experimental hypothesis and counterhypothesis is a question of structural validity of the experiment. Solving the problems of operationalization of variables in methodological procedures of their measurement (and control) is a question of ***operational validity***. If generalization of dependence involves its transfer to real types of human situations and activities, it is a solution of problems of conformity of variables from the point of view of ***external,*** or, as it is sometimes called now, ***ecological validity.***

Assessment of operational validity covers the stage of transition from already formulated experimental and counterhypotheses to procedures of their methodological implementation. One and the same variable may be represented

by indicators of different methods. One and the same experimental mental model can be implemented in case of quite different "technical" or operational implementation of controlled and measured variables.

Population hypotheses that aim to transfer generalizations to specific groups and entire populations can be evaluated using mental models, but cannot be tested mentally. Those additional variables that must be taken into account by the researcher for generalization purposes do not provide a justification for the conditions of necessity or sufficiency in the context of representation in real human behavior or in real situations of the actions of exactly the mechanisms that were considered in DOE.

Factors threatening the internal validity of the experiment are considered in connection with the discussion of formal planning as a condition for making decisions on an experimental fact. The development of inter-group or intra-individual plans is primarily aimed at ensuring control over internal validity. With respect to strategies for selecting test subjects from populations, the relationship between external (population) and internal validity should be discussed, since these strategies address two issues simultaneously: ensuring the representativeness of a sample of test subjects, and the equivalence of compared groups.

Questions for discussion:

1. A mental experiment and a mental sample of the experiment.
2. Validity as a means of correlating real experiments and their mental samples.
3. Inner validity.
4. External (environmental) validity.
5. Structural integrity.
6. Operational validity.

2.8.3 Specificity of evaluation of validity of laboratory experiment

A special case is the generalization related to the organization in the experiment of "cleared" conditions to test the so-called "exact" hypotheses. Usually it is a

function of a laboratory experiment. It is necessary to obtain data under conditions corresponding to causal dependencies in a specially formulated model in order to empirically estimate the explanatory power of this theoretical model. However, generalizations of theoretical character are connected not only with such a course of reasoning as "experiment - model - theory". Estimation of the power of one or another generalizations is also revealed in the analysis of the properties of the theory itself - in relation to the "world of theories". (not to the world of "psychological reality"). Regardless of whether the situation reflecting the theoretical model or the model of the "outside world" is modeled in the experiment, both the explanatory power of the statement formulated as an experimental hypothesis and the possibility of transition from one or another theory as a system of explanations to this empirical statement are assessed. The empirical hypothesis (EH) will include variables that can be not only observed, but also measured on a scale.

Structural validity is discussed for all types of experiments with scientific purposes. It estimates the adequacy of transition ***from a scientific (theoretical) hypothesis to an experimental hypothesis*** or ***from a "working" to an interpretative*** hypothesis. The solution of operational validity issues covers the stage of transition from already formulated experimental and counterhypotheses to methodological procedures of their implementation in an experimental model or experimental situation. R.Gottsdanker considers the concept of operational validity only in relation to such type of experiment as laboratory. In this case, the two mentioned stages of the development of methodological procedures are not distinguished, as it is assumed that the psychological construct is represented in the very method of its measurement.

2.8.4. Validity of experiment and validity of conclusions

In general, the validity of the experiment is said to imply all forms of experimental control aimed at providing all kinds of validity. The results of a valid experiment can serve as a basis for a reliable conclusion, if in general a logical system of inferences is implemented, which includes mutual transitions between different levels of hypotheses tested in the experiment. If all possible or the most obvious threats of internal and external validity are controlled, if the problems of operationalization of variables and conformity problems are well solved, then such an experiment is evaluated as valid or "suitable" (correct). It is possible to draw reliable (valid), or "correct" conclusions from a properly

constructed experiment. Bearing in mind that there are no generalization errors or inferences errors. But you should keep in mind that you may also make mistakes in conclusions.

Errors in the conclusions, or unreliable conclusions, may result from both incorrect generalizations and invalid experiments. In understanding experimentation as a sensual subject activity, a scientist should not, therefore, be limited to describing and evaluating how variables are operationalized or how they are registered. The formulation of a system of hypotheses, the evaluation of the validity of the experiment and the implementation of conclusions are included as standards, or normative regulators, of this activity.

The choice of a particular form of experiment is related to the development of one or another experimental plan (as data acquisition schemes under different NP conditions) and to other types of control (choice of the type of experiment, implementation of "primary" control, "additional variation" of variables, introduction of an extended variable, etc.). Solution of planning problems means, in this case, attributing the experiment to the system of classifications of experiment types and types of experimental schemes. This, in its turn, allows us to indicate possibilities and limitations of the following conclusions.

It is the assumptions about the possibilities of subsequent generalizations that guide the researcher in deciding the issues of substantive and formal planning of experiments. Thus, the problem of conclusions and generalizations turns out to be divorced into two rather detached from each other stages - mental planning of experimental schemes and procedures and justification of conclusions on the basis of the results of already conducted research. In real experimental studies, this corresponds to two stages of generalizations: as initial assumptions about the type of dependence between NP and WP and as final conclusions about the possibility of generalizations as a transfer of the established dependence outside the experimental situation (generalizations on the theoretical model, on other situations, activities, subjects, etc.).

Questions for discussion:

1. Psychological feature of the laboratory experiment.
2. Structural Validity as a Characteristic of Laboratory Experiment.
3. Validity of the experiment.
4. Validity of conclusions.
5. Choosing a specific form of experiment.
6. Conclusion mistakes.

PART 3.

PSYCHOLOGICAL PLANNING

EXPERIMENT

3.1. QUASI-EXPERIMENT PLANNING

3.1.1 General characteristics of quasi-experiment as studies with restricted forms of control

3.1.1.1 Restrictions on the implementation of experimental impacts

The experiment in psychology is a kind of ideal starting point for evaluating methods close to it in terms of deviations from its norms. In real practice, there are more such psychological studies that are only similar to it. Namely, they do not fully implement the experimental approach, so they cannot claim full experimental control of variables.

Quasi-experiments in psychology are those studies that are also aimed at testing causal hypotheses and include some kind of variable management schemes. However, due to a number of reasons (complexity of studied processes, necessity to preserve real conditions for research of experimentally controlled dependencies and a number of others), a psychologist can consciously organize collection of empirical data so that full control of an independent variable or its mixing with other variables is not achieved.

3.1.1.2 Different approaches to understanding the quasi-experiment

In an expansive interpretation, the term "quasi-experiment" covers methods of planning psychological research and organizing the collection of empirical data, which include some or other elements of the experiment, but ***not all stages*** implied by the general logic of the experimental method.

In a narrow interpretation, the term "quasi-experiment" is used when one tries to emphasize the specific forms of research organization if it is aimed at testing the causal hypothesis, but cannot be called an experiment due to insufficient control over the experimental impact and side factors. The lack of experimental control makes such studies quasi-experimental. These studies retain the focus on meeting the basic conditions of causal inference, but in order to establish the causal relationship between the variables require the identification of all those threats to a reliable inference that arise as a result of reduced experimental control.

The most important condition for the reliability of the conclusion about the cause-and-effect relationship, asserted on the basis of experimental data, is the elimination of competing explanations. The conclusion that a causal relationship between the variables has been established is possible only when the experimenter controls the factor acting as a cause and uses sufficient experimental control to ensure all kinds of validity specific to the psychological experiment: structural, operational, external, internal, etc., and to ensure the validity of the causal relationship.

In a quasi-experimental study, the level of control of factors threatening different aspects of validity that characterizes the true experiment is obviously unattainable. This leads to changes in the experimental schemes and imposes certain limitations on the logic of the conclusion, because when it is necessary to apply quasi-experimental plans, there are many sources of competing explanations. They need to be controlled either in special schemes of research implementation or in terms of output control.

3.1.1.3 Targets quasi-experimenta

The objectives of quasi-experimental research and the limitations of quasi-experimental conclusions are closely related to each other and result from the following factors.

1. The desire to explore complex causal dependencies that lose their specificity in the laboratory leads to the fact that psychological experiments are conducted in "field" conditions. Thus, the most adequate solution to the problems of compliance of NP, WP and DP is achieved, but accidents are worse controlled (systematic mixing and unsystematic variability). This means that the

probability of alternative causal explanations for changes in psychological indicators increases.

2. To remove the threat to the validity of the conclusion, such as the subject's knowledge of the fact of the experiment itself, a "disguised" experiment is conducted. In "disguised" or "blind" experiments, the subjects do not know what is being experimented upon, and all realities of living conditions are uncontrollably distributed as side variables at the levels of the assumed experimental factor (e.g., control class students are not aware of the difference between the school curriculum of the experimental class and their own). The external validity of such a study should be evaluated sufficiently high taking into account the achieved correspondence of the variables to life realities. However, naturally occurring mixtures (background factors, natural development, etc.) reduce the internal validity of such an experiment. The disguise of the study removes the threat to validity such as the desire of the subjects to get into the experimental group or to be exposed to experimental influences. Thus, the researcher avoids the effects of "adjusting" the strategies of subjects to the expectations of the experimenter, but cannot avoid those changes in the situation and motivation of subjects that are not planned by the researcher, but can occur along with the expected change in NP level.

3. In addition to cognitive "waiting effects", subjects may also demonstrate other trends in behavioral changes, responses, etc., due to the actualization of specific types of motivation when experimenting in the laboratory. The use of "natural" groups and the "disguise" of differences in NP levels are necessary when threats to a valid conclusion are expected because of the different desirability of different levels of variables. For example, the "naturally" actualized "motivation for experimentation" in a laboratory experiment may cause subjects to be willing to perform a difficult experimental task to show a high level of performance, and reluctance to perform an easy task where it is impossible to "show their abilities".

4. Finally, there are dependencies that cannot be managed. To check those psychological hypotheses in which cause-effect factors are not external influences, but are believed in the psychological reality itself (internal, subjective), the selection of psychological variables becomes a special problem. To solve this problem, strategies are used to select and select groups that differ by the variable measured in one way or another (representing intellectual, personal or other properties inherent in the subject). The choice when and on whom to conduct WP measurements is an implementation of a quasi-

experimental plan "without any influences" by the experimenter. The analogue of WP here is that variable, functional control of which is performed by selection of groups.

Artificial and laboratory experiments organized for scientific understanding of the laws of mental regulation of human activity have the advantage that they seek to build an accurate model of relationships between variables and to isolate a single NP. However, the special laboratory situation of the experiment, which creates conditions for "purity" of NPs and control of blending, is itself a factor reducing internal validity. This factor acts as the "experimenter's effect" and the "test subjects' bias" effect. Peculiarities of human perception of the experimental situation (and of the experimenter) and the attitude of the subject to the experiment formed on this basis distort the usual course of thoughts, feelings and behavior of subjects. In the conditions of psychological experiment, a person unwittingly accepts the rules of a special intellectual game. Namely, he or she starts to act in the way the experimenter thinks he or she wants, or in the way a normal person acts in his or her view, or in the way such a person acts when being observed.

A human being as a person manifests himself not so much in reactive but in proactive actions. His thinking is internally self-regulating rather than externally manageable. The closer a researcher gets to such a psychological reality, which takes place in ordinary forms of life, the more chances he has to reveal real causal dependencies. In other words, it is not possible to perform substitution of variables or distortion of their connection (due to inevitable transformations in movement towards more and more "pure" conditions).

It should be kept in mind that it is not that complex variables occur in quasi-experiments, or that "field" conditions are preferred. In a "field" study both a true experiment and a quasi-experimental study can be presented. In case of complex variables, a psychologist may indicate the key variable that, according to the hypothesis, is influenced by NP.

The more important difference between a quasi-experiment and an experiment is that the limitations of forms of control for many processes (personal and intellectual regulation of human decisions and actions) are fundamental. No situational or otherwise given influences characterize the activity of the subject himself. Besides, while studying dependencies connected with naturally formed forms of human activity manifestation, it is practically impossible to reduce the psychological reality to separate processes fixed by WP indices.

It is impossible to divide into a single system of psychic self-regulation single basic processes, which would not be in interactions with other (side), from the point of view of the basic hypothesis, processes or variables of "internal conditions".

The recognition of the principle of human activity as a person, as a subject of cognition, as an "actor" only makes it relatively possible to apply to a psychological experiment the principle of isolated conditions according to which each variable is thought of as a separately presented characteristic of subjective (psychological) reality. The psychological experimental hypothesis often presents a compromise between acceptance of the principle of isolated conditions, which implies control of individual or isolated variables, and the subject's activity principle. Quasi-experiment is a means of implementing such compromises.

Questions for discussion:

1. A "wide" and "narrow" interpretation of the quasi-experiment.
2. Factors that determine the purpose of the quasi-experiment.
3. Laboratory situation as a factor reducing internal validity.
4. The difference between a quasi-experiment and an experiment.
5. The principle of isolated conditions.
6. Elimination of competing explanations as a condition for reliable conclusions.

3.1.1.4 Measurement of indicators before and after impacts

In the first three (out of four) cases, different quasi-experimental plans with a lack of control before the pilot activities are usually applied. In these, the experimental factor is present as an unmanageable difference in effects attributed to equivalent experimental and control groups (or conditions). In addition to these plans, control methods such as additional variation control and statistical monitoring can be used. Additional variation involves either using levels of additional variables to expand generalizations about the relationship

(then called extrapolating variation) or using intermediate levels of NP to specify the type of relationship (then called intropolating variation).

For the fourth case, a qualitatively different method of control is used. Namely, control by choosing when and on whom to make measurements. Another name for such schemes is control after, or ***post factum***. In this case, the hypothesis assumes not organized experimental influences, but analogues of experimental factors that set its functional control by selecting groups on the measured or given interindividual differences between people.

"After" in this form of control means that the experimenter selects groups of people to measure WP, believing that the differences are already established or existing without regard to the research plan. This may be a sign of gender, motivation, professional experience or experience associated with some exposure to which the subjects have been previously exposed, etc. An important difference between such a study and a true experiment is that non-equivalent groups are compared, and it is a sign of non-equivalence that is an analogue of NP.

In this research plan, changing the way the variables are controlled changes the logic of intergroup comparisons. The groups differing by a certain parameter are given the same experimental influence, and the difference in the results of the subjects (in intergroup comparison of sample values of a dependent variable) is attributed to the main base variable, in which the groups differ from each other. Further, it is possible to identify a baseline side variable (BFS) with which the controlled difference and a new grouping of subjects were supposed to be mixed. Such quasi-experimenting can be as long as desired. It continues as long as competing hypotheses are formulated based on factors of internal conditions as a mixing field of the base process under study and the BPP.

3.1.2 Quasi-experimental plans

T.D.Cook and D.T.Campbell have developed theoretical bases of application of quasi-experimental plans in psychological research. In their opinion, there are two types of quasi-experimental plans: a) experiment plans for non-equivalent groups; b) discrete time series plans.

A quasi-experiment is any study aimed at establishing the causal relationship between two variables that lacks a prior procedure for group equalization. Or

"parallel control" with participation of the control group is replaced by comparison of results of repeated testing of the group (or groups) before and after exposure.

If strict definitions of experimental and quasi-experimental studies are used, an experiment with one subject should be referred to as quasi-experimental. At the same time, quasi-experimental time series plans are in fact a modification of the pre-experimental plan:

O1 X O2

O3 O4

Two natural groups are chosen, for example, two parallel school classes. Both groups are tested. Then one group is exposed (put into special conditions of activity) and the other group is not. After a certain time, both groups are tested again. The results of the first and second tests of both groups are compared. The difference between O2 and O4 indicates natural development and background impact. The difference in the results of primary testing of the two groups allows to establish the measure of their equivalence with respect to the measured variable. To reveal the effect of an independent variable, one should compare δ_{O12} and δ_{O34} rather than O2 and O4. I.e. the value of indicator shifts in time. The significance of the difference in the indicator increments will indicate the effect of the independent variable on the dependent variable.

This plan is similar to a true experiment plan for two groups with pre- and post-exposure testing. The main sources of artifacts here are differences in group composition. First of all, the results of the experiment may be affected by the "mixing effect". I.e. interaction of the group composition with testing factors, background events, natural development, etc., can affect the results of the experiment. For example, if parallel classes A and B are selected for participation in the experiment, B may contain children with less IQ than A. Therefore, differences in results may be due to the higher learning ability of the first group as compared to the second. The more similar the experimental and control groups are, the more valid the results obtained with this plan.

Д. Campbell distinguishes between two group choices. In the first case, the study involves natural groups that are not selected in relation to the procedure itself. Therefore, the group composition effect may be present, but it is not so

significant. In the second case, the experimental group is formed of volunteers, while the similar control group has to be completed by another method (coercion, promise of payment, etc.). In this case, the group composition factor may have a decisive influence on the difference between the results of the experimental and control groups.

There are many other options for quasi-experimental plans for non-equivalent groups. Namely, so-called "patchwork plans", plans of "multiple series of measurements", a plan with control samples for preliminary and final testing, etc.

A plan with preliminary and final testing of different randomized samples differs from the true experiment in that one group is pre-tested and the final (after exposure) group is equivalent (after randomization) to the group that has been exposed:

$$\begin{array}{llll} R & O_1 & (X) & \\ R & & X & O_2 \end{array}$$

This plan is also called a "simulation plan with initial and final testing". Its main disadvantage is the impossibility to control the influence of the "history" factor, i.e. background events occurring along with the influence between the first and second tests.

A more sophisticated version of this plan is a scheme with control samples for preliminary and final testing. Four randomized groups are used in this plan, but only two of them are affected, with one being tested after exposure. The plan is as follows:

$$\begin{array}{llll} R & O_1 & (X) & \\ R & & X & O_2 \\ \hline R & O_3 & & \\ R & & & O_4 \end{array}$$

If the randomisation is successful, i.e. the groups are really equivalent, this quality plan does not differ from that of the "true experiment". It has the best external validity because it allows to exclude the influence of the main external variables that break it. Namely, interaction of preliminary testing and experimental influence; interaction of group composition and experimental influence; reaction of subjects to the experiment. It is impossible only to exclude

the factor of interaction of the groups composition with the factors of natural development and background, as there is no possibility to compare the influence of preliminary and subsequent testing on the experimental and control groups. The peculiarity of the plan is that each of the four groups is tested only once: either at the beginning or at the end of the research.

This plan is very rarely used. D. Campbell even claims that this plan has never been implemented.

Quasi-experiment plans are used much more often than the above ones, which are commonly referred to as ***"discrete time series"***. There are two grounds for classifying these plans. The study is conducted 1) with one or more groups; 2) with one impact or a series of impacts. Plans in which a series of homogeneous or dissimilar influences with testing after each influence is realized have received in a domestic psychological science by tradition the name ***"forming experiments"***. In their essence they, of course, are quasi-experiments with all violations of external and internal validity inherent in such researches.

By using such plans, we must be aware from the outset that they lack the means to control external validity. It is impossible to control the interaction of preliminary testing and experimental influence, to eliminate the effect of systematic mixing (interaction of group composition and experimental influence), to control the reaction of subjects to the experiment and to determine the effect of interaction between different experimental influences.

Quasi-experimental plans constructed on the scheme of time series on one group are similar in structure to experimental plans for one subject.

The discrete time series plan is most commonly used in developmental psychology, pedagogical, social and clinical psychology. The essence of it is that the initial level of a dependent variable in a group of subjects is determined by a series of consecutive measurements. The researcher then influences the subjects of the experimental group by varying the independent variable and makes a series of similar measurements. The levels of the dependent variable are compared before and after exposure. The scheme of such a plan looks like this:

$$O_1 \quad O_2 \quad O_3 \quad X \quad O_4 \quad O_5 \quad O_6$$

The main drawback of the discrete time series plan is that it does not provide an opportunity to separate the result of the influence of an independent variable from the influence of background events that occur during the study. To eliminate the effect of "history", it is recommended that experimental isolation of subjects be used.

The modification of this plan is another quasi-series experiment on the time series scheme, in which exposure prior to measurement alternates with no exposure prior to measurement:

$$X \quad O_1 \; - \; O_2 \quad X \quad O_3 \; - \; O_4 \quad X \quad O_5$$

The alternation can be regular or random. This option is only suitable if the effect is reversible. When processing the data obtained in the experiment, the series is divided into two sequences and the results of those measurements, where there was an effect, are compared with those of those measurements, where there was no effect.

The plans of the temporary series are often implemented in practice (in Soviet pedagogical psychology, the experiment forming the experiment was considered almost the only option for evidentiary research). In their implementation, the well-known "Hottorn effect" is often observed. It was first discovered by Mayo, Dixon, Rothlisberger et al. in 1927-1932, when the research was conducted in Hottorn, a suburb of Chicago. It was believed that a change in the organization of work would increase his productivity. As a result, surveys of workers revealed that participation in the experiment itself increased their motivation to work. The convicts understood that they were personally interested in them and began to work more productively. To control this effect, a control group is used.

The scheme of the time series plan for two non-equivalent groups, of which one is not affected, looks like this:

$$\begin{array}{ccccccccccc} O_1 & O_2 & O_3 & O_4 & O_5 & X & O_6 & O_7 & O_8 & O_9 & O_{10} \\ O'_1 & O'_2 & O'_3 & O'_4 & O'_5 & & O'_6 & O'_7 & O'_8 & O'_9 & O'_{10} \end{array}$$

Quasi-experiment allows you to control the effect of the background impact factor (the "history" effect). This plan is usually recommended for researchers conducting experiments with natural groups in kindergartens, schools, clinics or at work. It can be called a plan for a formative experiment with a control sample. It is very difficult to implement this plan, but if the groups are randomized, it becomes a "true formative experiment".

It is possible to combine this plan with the previous one, which alternates series with the impact and its absence in one sample.

Questions for discussion:

1. Plans for *post factum.*
2. Two types of quasi-experimental plans.
3. A plan with preliminary and final testing.
4. Discrete time series" type plans.
5. Plans for forming experiments.
6. The problem of the validity of planned experiments.

3.1.2.1 Methods of reducing control in the implementation of quasi-experiments

A quasi-experiment requires, first, that all likely threats to a valid conclusion from uncontrolled sources of influence or interindividual differences are objective. Secondly, to choose, taking into account the control of these threats, the corresponding quasi-experimental plan defining the scheme of data acquisition and logic of the subsequent comparison of WP at the control of a conclusion. Thirdly, it is usually assumed that threats poorly controlled by this plan are excluded by means of statistical control.

In formal planning, the method of control may look so that the study is closer in its scheme (and in the methods of setting the variables) to a greater extent to the experimental. If we consider that quasi-experimental research is carried out mainly by selecting groups and establishing effects of interest to the researcher

on the basis of intergroup comparisons, then strategies for selecting or selecting subjects in groups and reveal the features of this approach.

There are two main directions to reduce the validity control in the formation of experimental and control groups: 1) non-fulfillment of the condition of randomization of the selection of subjects into groups, 2) consideration of the difference between the groups, which was introduced as the basis for the groups' non-equivalence, as an analogue of the NR. According to D. Campbell, in whose works the term "quasi-experimenting" was introduced, it is the first direction that gives the criterion of transition to quasi-experimental plans. It is assumed that many studies of this type are conducted in real, i.e. "field" conditions and groups are selected mainly as real ones. At the same time, a researcher does not control not only the composition of subjects, but also the intragroup dynamics of relations (the so-called background, or "intragroup history"). He may, however, be guided by externally defined criteria of similarity or difference between the groups themselves.

In addition to selecting groups by external criteria, a researcher may also use specific strategies for selecting subjects to be tested in a sample of potential subjects or to consider their characteristics collected as statistical or biographical material.

3.1.2.2 Strategies for selection of groups by a given attribute

The selection of test subject pairs is one such strategy for determining the composition of experimental and control groups. If the feature that distinguishes subjects in these groups was chosen as an already existing variant, then in formulating the hypothesis and building the data collection plan it can be formulated as an analogue of WP, or as a reconstructed causal variable (analogue of NP).

3.1.2.3 Quasi-experimental plans with special impact organization

For many psychological experiments, admissible zones of generalization take place and possibility to transfer the received results to other situations, kinds of activity, groups of people is justified. It gives the chance to carry out the experiments possessing good external validity (under condition of high internal

and operational validity, without which it is impossible to make decisions on experimental facts). Sometimes approximation to natural, or "field" conditions limits possible generalizations.

These are, in particular, "field" experiments that are conducted under conditions of actually functioning training groups. In them, the NP (training method) is set in the complex realities of training activities (in a particular institution). However, there may be no theoretical justification for the advantages of the new method. It is the underlying link of theoretical understanding of the bases of the established regularity, rather than a high estimation of external validity, that allows to transfer knowledge about the established effects of NP influence on other types of training (or construction of training subjects) and training activities in other institutions of similar type.

The objectives of psychological and pedagogical research in higher education can serve both to test general psychological hypotheses, for which the corresponding "field" conditions are no more than "background" (or other variants of additional variables), and to test special hypotheses that imply the consideration of the specifics of educational activity and communication in the university. In this case the variables "structure of the learning situation", "personal qualities", "communication style" assume control over them as analogues of NP, and generalizations outside the studied situation will depend on the validity of the non-specificity of the established pattern (for specific learning conditions and the chosen subject area).

In pedagogical research, a plan with a non-equivalent control group (one of the quasi-experimental plans with reduced control before the organization of impacts) is distributed. If the experiment uses real groups, such as training groups, then the experimental and control conditions cannot be considered equated, because there may be differences between the groups, which may "superimpose" on the studied pattern and cause incorrect interpretations.

Questions for discussion:

1. Formal planning of quasi-experiments.
2. Two main directions for reducing validity control.
3. Selection of groups according to a given attribute.
4. Feature of "field" experiments.

5. The objectives of psychological and pedagogical experiments.

6. A plan with a non-equivalent control group.

3.1.2.4. Statistical control in quasi-experimental research, and

In D. Campbell's classification of experimental research plans, the term "quasi-experimenting" covers quite different types of research schemes. One part of the schemes is based on true experimental plans. In other words, they separate an independent variable controlled by a researcher. But unlike true experimentation, there are some limitations in the control of factors that pose threats to internal or external validity. These limitations, first of all, look like failure to achieve equivalence between experimental and control groups (as a result of failure to fulfill the condition of randomization or for other reasons). Converges relevant quasi-experimental plans with experimental controlled exposure. The reduction in control concerns the pre-experimental stage.

The other part of quasi-experimental schemes, including only the choice of the researcher, when and on whom to conduct measurements of psychological indicators, is characterized by the absence of experimental impact. Therefore, it assumes control of mixtures of influence of the studied effect of the main variable with influence of the base side variable (BSP). Since the experimenter has no possibility to arbitrarily set different levels of NP to different subjects, such a study, in general, refers to the type of "passive observers". And statistical control here performs the same function of variation of possible BPP levels.

In a correlation study, statistical control means not only covering in the assumed sample all levels of random variations of side variables, but also considering the empirically obtained correlation coefficient between measured variables as a measure to evaluate the statistical null hypothesis (about the absence of a relationship between two or more rows of sample indicators). All possible forms of output control both from the point of view of objectification of possible side variables and from the point of view of conformity problems (i.e. external validity of the study) are feasible in this case before the study or during discussion (reflection) of the conditions of variables measurement.

In a quasi-experimental scheme involving the choice of when and on whom to measure variables, statistical control includes a special stage, which is not

present in the usual correlation study. It is called statistical ***control after*** (after measurements).

Questions for discussion:

1. Statistical control in correlation study.
2. Mixing an independent variable with a side variable.
3. Statistical control of the base side variable.
4. Statistical control as a means of evaluating the null hypothesis.

3.2. PLANNING A ONE-FACTOR EXPERIMENT

3.2.1 Substantive and formal planning3.2.1.

3.2.1.1. Approaches to defining the term "experiment planning"

The following traditions in understanding the planning of a psychological experiment can be distinguished.

1. Selecting a scheme, or data acquisition plan, to test the causal hypothesis. R.Gottsdanker follows this notion of planning, when a researcher chooses one or another of the available schemes, comparing their possibilities from the point of view of solving the problems of approaching the experiment to an impeccable one.

2. Determining the type of variables from the point of view of given hypothetical constructs and solving the questions about operationalization of variables, and on this basis - the choice of methodological means reflecting the interrelation of aspects of substantial and formal planning.

3. Planning with the purpose of subsequent use of statistical decisions on the experimental fact, and, therefore, indication of that minimum effect (in WP differences) which will be accepted as criterion for making a decision on the experimental fact.

3.2.1.2 Selection from possible experimental plans

R.Gottsdanker proposes to analyze possible experimental schemes by the following main features:

- ***type of circuits***. Are comparable NP conditions offered to the same subject or different groups of people? In the first case, they talk about intraindividual schemes, and in the second case - about intergroup schemes. Intraindividual schemes are defined as cross-individual schemes in which all NP conditions are supposed to be presented to each subject, but their sequences vary with respect to the selected subgroups;

- ***type of experiment***. Whether the condition of a single NP is achieved, which is typical for laboratory experiments, or whether the NP is presented in a complex of other conditions that, to varying degrees, claim to fulfill the condition of compliance of the experimental model with the external reality to which the generalization will be made;

- ***the forms of validity threat control***. Whether all possible sources of internal validity threats are controlled by Side Variables (SVs) that may provide systematic, unsystematic, concomitant mixtures (SVs with SVs) or unreliable data;

- ***the number of controllable factors***. Whether the experimental scheme is a factor one or a scheme with one controlled NP; is the quantitative measurement of the main results of the action of the variables (and their interactions with the factor schemes) expected?

- ***the degree of agreement with the criteria of the*** mental standards of the experiment. Whether the best representativeness of the really conducted experiment in comparison with mental samples, following which would provide construction of faultless experiment, is reached.

The idea of the experiment as a hypothetical-deductive method is omitted, while the evaluation of psychological hypotheses is considered in one main aspect - control of threats to internal and external validity. The main attention is given to substantiation of advantages of the used strategy of selection of subjects into groups, their selection from the population in case of intergroup schemes, or distribution of experimental conditions in their general sequence (at control of

factors of tasks, time, nonsystematic variability) in case of intraindividual schemes.

Differently for these schemes, planning problems are solved to achieve acceptable data reliability. Reliability estimation depends on control of nonsystematic variability, variability of used variables and selected number of experiments for different experimental conditions. From the point of view of controlling the data unreliability factors, the possibilities of averaging WP indices under different conditions or for different groups of subjects are also discussed, etc. Behind the selected number of samples corresponding to the same NP condition there is an accepted criterion of quantitative evaluation of the required value of the experimental effect, without achieving which the experimental hypothesis is considered to have failed the experimental test and should be rejected.

Essentially, elements of meaningful planning are present in all experiments. Selecting a scheme is the final stage of planning. Formal planning begins with the stage of choosing between intraindividual and intergroup ways of comparing WP. Taking into account additional variables or discussing the commonality of the experimental revealed trend for other subjects is a continuation of substantive planning issues, as they are directly related to controlling the level of generalizations of the established dependence.

Thus, even a valid experiment can give rather weak empirical arguments if the planning was mostly formal.

Questions for discussion:

1. Traditions in understanding the planning of a psychological experiment.
2. Signs as a means of analyzing experimental schemes (plans).
3. An experiment as a hypothetical- deductive method.
4. Planning as a means to achieve data reliability.
5. The ratio of substantive to formal planning.
6. Formal planning as a means of obtaining inaccurate data.

3.2.1.3 Substantive planning and choice of type of experiment

Substantive planning includes stages of ***formulating experimental hypotheses*** and ***justifying their interpretation components*** related to the introduction of hypothetical constructs.

For carrying out of experiment both in laboratory conditions, and in "field", at a stage of the substantial planning its ***constructive validity*** connected with the control of ways of concretization of theoretical concepts in hypothetical designs, and acceptability of the experimental approach for check of a psychological hypothesis are proved also. Discussion of the used methodical arsenal of ***fixation of variables*** can also be referred to substantial planning. I.e. solution of problems of operationalization of variables and assertion of assumptions about the essence of psychological causality or type of psychological laws that are postulated or implicitly present in the formulation of the experimental hypothesis.

Consideration of the ratio in the expected empirical data of natural and random is also a problem of substantial planning. However, it is not always sufficiently discussed (or understood) by the researcher. Data obtained for a single subject or a single sample of subjects can be considered random in the sense that, with a large number of subjects (or a few samples), they will be uncharacteristic of the main body of results. When using the concept of distribution of sample values of WP, which includes transition to statistical decisions about its type, individual data are already a part of a number of values of the measured variable. In this case a randomness means only variability of the variable itself and not the degree of difference of an individual case from the characteristic, i.e. most frequently encountered, indicators.

The notion of randomness is also used to point to factors unplanned by the experimenter (e.g. PPs) and to highlight the fact that the causal link under study has to "break through" the sum of other components. In the latter case, it is assumed that the natural causal link can only manifest itself in a certain set of circumstances, not always. Only under certain combinations of test subjects' properties, not for all subjects, etc.

When planning an experiment that assumes that the investigated pattern is regulated by a set of cause-effect conditions or should as if "break through" through a set of accidents, it is the principle of equal probability of obtaining data (with equal chances of obtaining data both in favor and against the

experimental hypothesis) guides the construction of the experimental model. The paradox is that the deterministically formulated hypothesis is estimated as probabilistic. A regularity is understood as a violation of a randomness represented by changes in WP in this or that direction. From the point of view of constructing a situation of variable control, this shift can only be attributed to an action of WP (that is why it is called the main result of the action).

Thus, the identification of trends (directed violations of equidistant outcomes) can be seen as a manifestation of a general empirical pattern. Although in relation to an individual case, the pattern-trend may not be a deterministic cause. In order to establish the trends manifested in the group of subjects ("medium group"), it is sufficient that the dependence is manifested for a part of the subjects, which provides a shift in the general indicators.

The path from the protected theoretical understanding of causal dependence to empirically validated statements (as experimental and counterhypotheses) and means implementation of the stages of substantive planning. This is true if two other problems are not considered. The problems of competing dependence explanations in the same components of the methodical embodiment of the experiment, and increasing the level of dependence generalization on the basis of different experiments, which differ exactly in the components of the methodical embodiment of the variables.

Both of these problems can, in turn, include solving questions about representation in this or that methodical procedure of possibility to measure not only a psychological variable, but also a psychological construct corresponding to it. These decisions will also take into account elements of formal planning, since psychological dimensions will always have an approximate character. Error of measurements should be considered as the basis for establishing different dependencies at some dispersion of data. The probabilistic character of estimating the type of empirically established dependences determines the problem that there will always be an open question about the possibility of reinterpreting that hypothetical construct which was connected with the basic process reconstructed according to WP values.

Formal planning also refers to the choice of a research plan, or pilot scheme. However, the choice of plans as forms of experimental control cannot be presented only at the level of comparison of their advantages among themselves and in terms of possibilities for subsequent generalizations.

Similarly, in any area of psychological knowledge, it is possible to trace interrelationships of changes in research approaches and the ways of reconstruction of a psychological reality behind them, on the one hand, and relative autonomy of use of experimental schemes, on the other hand. Content planning does not replace, but assumes transition to the stage of formal planning. Formal planning is usually put out of brackets for substantive planning only in that part of it, where methodological subtleties of the scheme's substantiation do not essentially influence the understanding of the phenomena or processes under study. Even the decision on whether to conduct an intergroup or intraindividual experiment includes an assessment of the possibility of subsequent generalizations of the dependence presented in the hypothesis.

Questions for discussion

1. The stages of meaningful planning.
2. Substantive planning as justification for structural validity.
3. Substantive planning as justification for the operationalization of variables.
4. The problem of competing explanations of addiction.
5. The problem of increasing the level of generalization of dependence.
6. Formal planning as the choice of "scheme" of research.

3.2.1.4 Dependence of generalization on the type of experiment to be performed

The choice of experimental plans as schemes for setting NP levels is related to the content of the hypothesis and the expected possibilities for further generalization of the dependence under study. In this approach serious attention is paid to the evaluation of generalization possibilities in terms of achieving good external or operational validity in the experiment. Therefore, this approach to planning cannot be called purely formal. Substantive aspects of discussion of causal dependence, postulated in the psychological hypothesis, become as if unimportant in the analysis of advantages and disadvantages of experimental schemes as plans to control threats to validity.

The dependence obtained using intraindividual schemes characterizes, first of all, a specific subject. It may be non-representative for a group of subjects.

Psychology knows other ways of generalization based on intraindividual experiments. G. Ebbingauz's studies of patterns of memorizing meaningless syllables, L. Fechner or S. Stevens' psychophysical experiments and many other schemes of individual experiments formed the basis of generalizations. These generalizations then were repeated as general laws for the majority of people if conditions of identity of procedural components of carrying out of experiments were fulfilled and subjects themselves were not deviated on their individual features so that their data served as a basis for the analysis of an individual case.

The transfer of the dependence obtained in the intraindividual experiment to the widest possible population (for example, to all people with a healthy mentality) is possible only with the creation of such laboratory conditions, which assume the representation in the components of the methods of the principle of psychological explanation set by the theory. The construction of the most experimental model of interrelation of NP and WP (at "purification" of conditions and control of SP) is carried out in such a way that generalization of the relatively empirically established type of dependence allows spreading the explanatory theoretical model to all other cases of actualization of similar basic processes.

At performance of requirements to an estimation of internal and operational validity of corresponding laboratory experiment, the latitude of transfer of postulated causal dependence is defined by an assumed degree of adequacy (conformity) of theoretical model of that psychological reality which explanation it serves. Thus, on the basis of the results of an intraindividual laboratory experiment, the following way of generalization is possible: first for the "world of theory" and then for all those cases of "psychological reality" that are meant in the presented theoretical model.

3.2.1.5. Statistical solutions and formal planning

Formal planning as a choice of schemes is combined with the justification of the validity or significance of the empirical results obtained. The following tasks of formal planning of the study are singled out: 1. ensuring validity of the experiment, 2. ensuring conditions for making decisions about the experimental

effect or effect of the NP effect, and 3. application of data processing schemes adequate to the used measuring scales and the method of data collection. In the narrow sense, "planning the experiment" includes two points related to taking into account subsequent statistical decisions.

First, a discussion on how the experimental effect will be assessed. The decision may concern the choice between ***communication measures*** and ***differentiation measures***. Statistical communication measures can be used to establish covariance between NPs and STs, while differentiation measures can indicate that there is no difference in values of STs between different experimental conditions. Relevant statistical hypotheses no longer include assumptions about the legal effect of NPs, but are formed only as hypotheses about comparison of sample indices of STs (averages, dispersions, etc.). In case of one and the same experimental plan it is possible to use different data processing plans. The type of experimental hypothesis sometimes suggests which way of establishing an experimental fact by sample WP values should be preferred. Often the same data can be processed in different ways to make sure of the advantages of this or that method of presenting the obtained dependencies.

Second, the establishment of a minimum effect sufficient to make a judgment on the differences obtained in experimental and control conditions or an observable link between changes in NPs and STs. Establishment of the minimum effect also includes determination of the probability of the first and second kind of errors (α- and β-level). For an α- error, the general rule is to specify the percentage or possible probability of *rejecting the null hypothesis* as a hypothesis of no difference or no relation when it is true. There is no such general rule for β-error because of its relation to the value of the established effect.

Statistical solutions are not always required to establish an experimental effect. There are effects that are said to be "hitting the eye". In other words, the changes in WP values when comparing different experimental conditions are so great that due to their "obviousness" there is no need to use statistical criteria to assess their significance. There are other effects that are detected as unobvious but statistically significant shifts in the values of WP. Common sense or theoretically grounded expectations make it possible to make a decision what changes in WP values can be considered sufficient to conclude about the result of the NP action.

The value of the minimum effect is connected with the number of experimental data, i.e. with the number of sample values of WP indices. An increase in the sample (the number of test subjects or the number of experiments) can

significantly reduce the value of the effect sufficient to make a decision about the NP effect. However, the sample size is also associated with the solution of problems of substantial planning (control of the time factor due to the fatigue of subjects, control of sample representativeness in relation to the population, etc.). Therefore, a reference to statistical tables that present links between the minimum difference (WP values) and significance levels in relation to the use of specific statistical criteria cannot serve as a sufficient basis for determining the required number of samples or test subjects.

The latter concerns practically all kinds of use of quantitative evaluation of psychological effects. The psychologist, passing to the level of checking statistical hypotheses, starts to work with sample values of variables and probability models for assessment of statistical hypotheses, discussing or as if putting in brackets the question of applicability of corresponding models from the point of view of content of variables.

Finally, planning an experiment can be understood as mathematical planning. It begins with the choice of a mathematical model describing the events and their interrelationships, and also includes the previously mentioned moments of determining the minimum effects and α-, β- errors in the rejection of null hypotheses.

Questions for discussion:

1. Intraindividual planning schemes.
2. The tasks of formal research planning.
3. Planning as a way of evaluating the experimental effect.
4. Planning as setting a minimum effect.
5. Planning as mathematical processing.
6. *α- Errors* and *β- Errors* as a result of planning.

3.2.1.6. Validity as an experimental control objective

In the most general case, the validity of the experiment means all forms of experimental control that provide a valid or reliable conclusion. However, the control of conclusions is also outside the experimental control. The results of a valid experiment can serve as a basis for a reliable conclusion, if in general a logical system of conclusions is implemented. I.e. mutual transitions between different levels of hypotheses tested in the experiment are justified, the problem of asymmetry of the conclusion is taken into account, the latitude and level of data generalizations are justified (as transferring the conclusions beyond the limits of the experiment).

In order to discuss the adequacy and validity of generalizations, a researcher must first be sure that the dependence obtained in the experiment really represents (represents) the relationship between independent and dependent variables implied in the hypothesis. That there was no voluntary or involuntary substitution of the studied pattern. Any inconsistency means a threat to the suitability, or validity, of subsequent conclusions.

Second, the establishment of experimental dependence implies that all competing threats to valid judgement from mixing NPs with side or concomitant variables have been eliminated. If any condition of NP was not accidentally related to the active level of the mixing variable, the question remains which one of them (independent or mixing variable) should be attributed to the obtained shift in SP values. I.e. the case when it is possible to reject the null hypothesis or when the difference between the sample values of WPs at the selected significance level is reliable, the validity is bad and the experimental effect can be considered as an artifact.

If the generalizations are incorrect, they are called artifact (false). Bad experimental control can be one of the sources of wrong conclusions. In other words, an invalid experiment leads to incorrect generalizations.

The extent to which the choice of a specific form of experiment (experimental plan) is linked to the development of a system to control all possible threats to the established dependence is not yet considered. Let us only note that the experimental plan as a scheme of data collection (fixation of WP under different NP conditions) includes also an indication of other directions of experimental control (choice of the type of variables, implementation of "primary" control, elimination of side or stabilization of additional variables, introduction of an extended variable, etc.).

Formulation of the system of hypotheses, evaluation of the validity of the experiment and implementation of the conclusions are included as components of the general goal - to avoid threats to the validity conclusion.

3.2.2 Experimental plans

Content. Planning of the experiment. The main experimental plans: plans for one and two independent variables, factor plans, Latin and Greek-Latin square planning. Interaction of independent variables, types of interaction. Plans of experiments on one subject. Analysis of learning curves. Planning on the method of time series. Control of asymmetric transfers and placebo effect. Pre-experimental and quasi-experimental plans, including plans of time series. Experiment *ex-post-facto*. Correlation study and its planning. Types of correlative study plans. Perspectives of the experiment development: multidimensional experiment, differential-psychological experiment, cross-cultural research.

Basic concepts. Research plan, true experiment plan, quasi-experimental plan, impact, artifact sources, factor plan, impact, artifact sources, factor plan, Latin square, rotation plan, asymmetric transfer, symmetrical transfer, alternative impact plan, equalization plan, *ex-post-facto plan,* correlation, correlation coefficient, longitude, natural development.

3.2.2.1 Plans for one independent variable

The plan for a "true" experimental study differs from the others in the following essential features:

1. using one of the strategies to create equivalent groups, most often randomization;

2. by having an experimental and at least one control group;

3. completion of the experiment by testing and comparing the behaviour of the group exposed to the experiment (*X1*) with the group not exposed (*X0).*

In psychology, the planning of the experiment began to be applied from the first decades of the 20th century.

The classic version of the plan is a ***plan for two independent groups***.

There are three main versions of this plan. In describing them, we'll use the schemes suggested by D. Campbell.

1. experimental group	***R X O1***
2. Reference Group	***R O2***

Here R-randomization, X-impact, O1 for the first group, O2 for the second group.

3.2.2.2 Plan for two randomized groups with post-exposure testing

The author of this plan is well-known biologist and statistician R.A.Fisher. The equality of experimental and control groups is an absolutely necessary condition for the application of this plan. Most often a *randomization* procedure is used to achieve equivalence between groups. This plan is recommended when it is not possible or necessary to perform preliminary testing of the subjects. If the randomization is qualitative, this plan is the best, allowing control of most sources of artifacts. In addition, various variants of dispersion analysis can be used for it.

After randomization or other group equalization procedure, experimental exposure is carried out. In the simplest variant only two gradations of independent variable are used: there is an impact, there is no impact.

If more than one impact level is needed, plans with several experimental groups (by number of impact levels) and one control group are used.

If it is necessary to control the influence of one of the additional variables, the plan with two control groups and one experimental one is applied. Behaviour measurement provides material for comparing the two groups. Data processing comes down to applying traditional mathematical statistics estimates. The corresponding procedures are described in detail in the textbooks of mathematical statistics for psychologists.

Application of the plan for two randomized groups with post-exposure testing allows control of the main sources of internal disability. Since there is no -

preliminary testing, the effect of interaction between the testing procedure and the content of the experimental exposure and the test effect itself is excluded. The plan allows to control the effect of group composition, spontaneous disposal, the effect of background and natural development, the interaction of group composition with other factors. Also the plan allows to exclude the regression effect due to randomization and comparison of experimental and control groups data.

However, in most pedagogical and socio-psychological experiments it is necessary to strictly control the initial level of the dependent variable (intelligence, anxiety, knowledge or personality status in the group). Randomization is the best procedure possible, but it does not provide an absolute guarantee of the correctness of choices. When there are doubts about the results of randomization, a plan with preliminary testing is used.

1. experimental group	***R O1 X O2***
2. Reference Group	***R O3 O4***

Questions for discussion:

1. Validity of experiment and validity of conclusions.
2. Signs of true experimental research.
3. A plan for two independent groups.
4. Plan for two randomized groups with post-exposure testing.
5. A plan with two control groups and one experimental.

3.2.2.3. A plan for two randomized groups with preliminary and final testing

The pre-test plan is popular with psychologists. The psychologist knows very well that each person is unique and different from the others, and subconsciously seeks to catch these differences through tests, not trusting the mechanical procedure of randomization. However, the hypothesis of the majority of psychological researches, especially in the field of development

psychology ("forming experiment"), contains the forecast of certain change of an individual's property under the influence of external factor. Therefore, the plan of "test-effect-test" using randomization and control group is very common.

In the absence of an equalization procedure for groups, this plan is converted into a quasi-experimental one.

The main source of artifacts that violate the external validity of the procedure is the interaction of testing with experimental exposure. For example, testing the level of knowledge in a certain subject before conducting a memorization experiment may lead to actualization of the initial knowledge and an overall increase in memorization productivity. This is achieved by actualizing mnemonic abilities and creating a memorization setting.

However, other external variables can be controlled using this plan. The "history" factor ("background") is controlled, because both groups are exposed to the same ("background") effects between the first and second tests. At the same time, D. Campbell points out the necessity to control "intragroup events" as well as the effect of simultaneous testing in both groups. In reality, it is impossible to ensure that the test and retest are conducted in them simultaneously. Therefore, the plan turns into a quasi-experimental one, for example:

R	***O1***		***X***	***O2***
R		***O3***		***O4***

Usually, the control of the testing non-uniformity is carried out by two experimenters who test two groups simultaneously. The procedure of randomization of the testing order is considered optimal. I.e. testing of members of experimental and control groups is carried out in random order. The same is done with/without presentation of experimental influence. Of course, this procedure requires a significant number of subjects in the experimental and control samples (at least 30-35 subjects each).

The natural development and the effect of testing are controlled by the fact that they appear equally in the experimental and control groups. And the effects of group composition and regression are controlled by a randomization procedure.

When processing data, parametric criteria *t* and *F* (for interval scale data) are usually used. Three values of *t are* calculated*:* comparison 1) *O1 and O2*; 2) *O3 and O4;* 3) *O2 and O4*. The hypothesis of a significant influence of an independent variable on a dependent variable can be accepted if two conditions are met: a) the differences between *O1 and O2* are significant, and between *O3 and O4 are* insignificant; and b) the differences between *O2 and O4* are significant. It is much more convenient to compare not absolute values, but the values of indexes growth from the first to the second test $(\delta_{(i)})$. $\delta_{(i12)}$ and $\delta_{(i34)\ are}$ calculated and compared using the Student's *t-criterion.* In the case of significance of differences, an experimental hypothesis of the influence of an independent variable on a dependent is accepted.

It is also recommended to apply the Fisher covariance analysis. The pre-test indicators are taken as an additional variable and the test subjects are divided into subgroups depending on the pre-test indicators.

Application of the "test - impact - retest" plan allows to control the influence of "side" variables that disturb the internal validity of the experiment.

External validity is related to the possibility of transferring data to the real situation. The main point that distinguishes the experimental situation from the real one is the introduction of preliminary testing. As it has already been mentioned, the plan "test - impact - retest" does not allow to control the effect of interaction of testing and experimental impact. This happens because the pre-test subject "sensitizes", i.e. becomes more sensitive to the effect, as we measure in the experiment exactly the dependent variable that we are going to influence by varying the independent variable.

Previous	Impact of	
	Yes, .	No .
Got .	***O2***	***O4***
No .	***O5***	***O6***

The plan of R.L.Solomon, which was proposed by him in 1949, is used to control external validity.

3.2.2.4. Solomon's R.L. Plan

Used in a four-group experiment:

1. Experiment 1:	***R***	***O1***	***CHO2***	
2. Control 1:	***R***	***O3***		***O4***
3. Experiment 2:	***R***		***XO5***	
4. Control 2:	***R***			***O6***

The plan includes a study of two experimental and two control groups and is essentially multi-group (type 2 x 2).

R. Solomon's plan is a merger of the two previously reviewed plans. The first when no preliminary testing is done and the second when preliminary testing is done. With the help of the "first part" of the plan, it is possible to control the effect of interaction between the first testing and the experimental impact. P. Solomon uses his plan to identify the effect of experimental exposure in four different ways: by comparing 1) *O2* to *O1*; 2) *O2 to O4*; 3) *O5* to *O6* and 4) *O5 to O3*.

If we compare *O6* with *O1* and *O3*, we can identify the combined effect of natural development effects and "history" (background effects) on the dependent variable.

Д. Campbell, criticising the proposed R. Solomon data processing scheme, proposes not to pay attention to preliminary testing and to reduce the data to a scheme 2 x 2, suitable for dispersion analysis.

Comparison of average columns allows to reveal the effect of experimental influence (influence of independent variable on dependent). The average rows show the effect of preliminary testing. Comparison of averages by cells characterizes the interaction of the effect of testing and experimental impact, which indicates the degree of violation of external validity.

In the case when the effects of preliminary testing and interaction can be neglected, we proceed to *O4* and *O2* comparison using the covariance analysis method. The data of preliminary testing are taken as an additional variable according to the scheme given for the "test - effect - retest" plan.

Finally, in some cases it is necessary to check if the effect of the independent variable on the dependent is preserved in time. For example, find out if the new learning method leads to long-term memorization of the material. For this purpose, the following plan is used:

1. Experiment 1	***RO1***		***XO2***
2. Control 1.	***RO3***		***O4***
3. Experiment 2	***RO5***	***X***	***O6***
4. Control 2.	***RO7***		***O8***

Questions for discussion:

1. A plan for two randomized groups with preliminary and final testing
2. The main source of artifacts in this regard.
3. Plan "test, impact, retest."
4. R.L.Solomon's plan.
5. Four ways to detect the effect of experimental exposure.
6. D.Campbell's criticism of R.Solomon's plan.

3.2.2.5 Plans for one independent variable and several groups

Sometimes comparing two groups is not enough to confirm or disprove an experimental hypothesis. Such a problem arises in two cases: a) when it is necessary to control external variables; b) when it is necessary to reveal quantitative dependencies between two variables.

Various variants of factor experimental plan are used to control external variables. As for revealing the quantitative dependence between two variables, the necessity to establish it arises when checking the "exact" experimental hypothesis. In an experiment involving two groups, it is at best possible to establish the fact of causal relationship between independent and dependent variables. But an infinite number of curves can be drawn between two points. In order to make sure that there is a linear relationship between the two variables, you should have at least three points corresponding to the three levels of the independent variable. Consequently, the experimenter should identify several randomized groups and put them under different experimental conditions. The

simplest option is a *plan for three groups and three levels of the independent variable:*

Experiment 1: ***RX1O1***

Experiment 2: ***RX2O2***

Control: ***R O3***

The control group in this case is the third experimental group, for which the level of variable $X = 0$.

In implementing this plan, each group is given only one level of independent variable. It is possible to increase the number of experimental groups corresponding to the number of levels of the independent variable. To process data obtained with the help of this plan, the same statistical methods as those listed above are used.

3.2.2.6. Plans of experiments for one subject

Experiments on samples with control of variables began to be used in psychology from 1910-1920s. Experimental studies on equation groups were especially widespread after the theory of experiment planning and processing of its results (dispersion and covariance analyses) was created by the outstanding biologist and mathematician R.A.Fisher. But psychologists used the experiment long before the theory of planning of experiment appeared. The first experimental studies were conducted with the participation of one subject. Often, it was the experimenter himself or his assistant. Beginning with G. Fechner, the technique of experimentation came to psychology to test theoretical quantitative hypotheses.

The classic experimental study of one subject was the work of G. Ebbingauz, which was conducted in 1913. G.Ebbingauz investigated the phenomenon of forgetting by memorizing meaningless syllables (invented by him). He memorized a series of syllables, and then tried to reproduce them after a certain time. The result was a classic curve of forgetfulness characterizing the

dependence of the volume of stored material on the time elapsed since memorization.

In modern empirical scientific psychology, three research paradigms interact and struggle.

The first paradigm. Representatives of the first paradigm, which originates from the natural-scientific experiment, consider the only reliable knowledge to be that which is obtained in experiments on equivalent and representative samples. The main argument of the advocates of this position is the need to control external variables and exclude individual differences to find common patterns.

Second paradigm. Representatives of the second paradigm, based on the methodology of experimental behaviour analysis, criticize supporters of statistical analysis and planning of experiments on samples. In their view, research should be conducted with a single subject and with certain strategies that will eliminate the sources of artifacts during the experiment. Supporters of this methodology are such well-known researchers as B.F.Skinner, G.A.Murray and others.

The third paradigm. The third paradigm, based on classical idiographic research, is contrasted with both experiments involving a single subject and plans to study behavior in representative samples. An idiographic study involves the study of individual cases: the biographies or behavioral patterns of individuals. Examples are the remarkable works of A. Luria "The Lost and Returned World" and "A Small Book of Great Memory".

In many cases, studies involving a single subject are the only option. The methodology of investigation of one subject was developed in 1970-1980s by many authors: A.Kezdan, T.Kratohvill, B.F.Skinner, F.-J.McGuigan and others.

This experiment identifies two sources of artifacts: (a) errors in planning strategy and research; and (b) individual differences.

If the "right" strategy is put in place for a single test subject experiment, the whole problem will be reduced to taking into account individual differences. A single-test subject experiment is possible when: a) individual differences can be ***disregarded for the*** variables studied in the experiment, all subjects are considered ***equivalent,*** so it is possible to transfer data to each member of the population; b) the subject is ***unique*** and the problem of direct transfer of data is irrelevant.

The strategy of experimenting with one subject was developed by B. Skinner to investigate the learning process. Data during the study are presented in the form of "learning curves" in the coordinate system "time" - "total number of responses" (cumulative curve). The learning curve is initially analysed visually. Its changes in time are considered. If the function describing the curve changes when the impact *A* on *B changes,* it may indicate the presence of causal dependence of behavior on external influences *(A* or *B).*

A single test subject study is also called ***time series planning***. The main indicator of the influence of an independent variable on a dependent in the implementation of such a plan is a change in the nature of a subject's responses to the influence of changes in experimental conditions in time. There are several basic schemes for applying this paradigm. The simplest strategy is the scheme *A - B.* The subject initially performs activity in conditions *A*, and then - in conditions *B*.

When using this plan, a logical question arises, would the answer curve have remained the same if there had been no impact? In other words, this scheme does not control the placebo effect. Moreover, it is not clear what caused the effect. Maybe it wasn't Variable *B that had the effect,* but some other variable not considered in the experiment.

Therefore, a different scheme is used more often: *A - B - A.* Initially, the behavior of the subject under conditions *A is* registered, then the conditions change *(B),* and in the third stage the previous conditions return *(A). The* change in the functional relationship between independent and dependent variables is studied. If the change of conditions at the third stage restores the previous type of functional relationship between the dependent and dependent variables, then the independent variable is considered to be the reason that can modify the subject's behavior.

However, neither the first nor the second time series planning options allow for the consideration of the cumulative impact factor. It is possible that a - combination of a sequence of conditions *(A* and *B)* leads to the effect. It is also unobvious that after returning to the situation *B, the* curve will take the same form as it was at the first presentation of conditions *B.*

An example of a plan that reproduces the same experimental effect twice is the scheme *A - B - A - B.* If the second transition from conditions *A* to conditions *B* reproduces a change in the functional dependence of the subject's answers on

time, it will prove the experimental hypothesis that the independent variable (*A*, *B)* affects the subject's behavior.

There are different time series planning options. A distinction is made between ***regular*** series ***alternating*** schemes *(AB - AB),* ***stochastic*** series and ***position equalisation*** schemes (e.g. *ABBA*). Modifications of the circuit *A - B - A - B* are the circuit *A - B - A - B - A* or longer: *A - B - A - B - A - B - A*.

The use of longer time plans increases the guarantee of effect detection, but leads to fatigue and other cumulative effects.

In addition, plan *A - B - A - B* and its various modifications cannot eliminate three significant problems.

1. What would happen to the subject if there was no effect (placebo effect)?

2. *Isn't the* sequence of effects *A-B in itself* another impact (a side variable)?

3. What reason led to the effect: if there was no effect in place *B,* would the effect be repeated?

To control the placebo effect, the *A - B - A - B* series includes conditions that "simulate" either *A* or *B*.

There are many techniques for conducting studies involving one subject. An example of plan *A to B* development is the ***"alternative impact plan"***. The effects of *A* and *B are* randomly distributed over time, e.g. to the days of the week. Then all moments when there was an impact *A* are identified. *A* curve is drawn connecting the corresponding consecutive points. All moments in time when there was an "alternative" effect *B are allocated*, in the order in time are also joined. A second curve is drawn. Then both curves are compared and it is revealed which influence is more effective. The efficiency is determined by the value of the curve growth or drop.

Synonyms for the term "alternative impact plan" are: ***"series comparison plan"***, ***"synchronized impacts plan"***, ***"multiple schedules plan"***, etc.

The other option is a ***reversible plan***. It is used to investigate two alternative forms of behaviour. Initially, the basic level of manifestation of both modes of behaviour is recorded. The first behaviour can be actualised through a specific impact. The second, incompatible with it, is provoked simultaneously by the other type of exposure. The effect of two impacts is evaluated. After a certain period of time the combination of effects is reversed so that the first form of behaviour receives the impact that initiated the second form of behaviour and

the second one receives the impact corresponding to the first form of behaviour. This plan is used, for example, when investigating the behaviour of young children.

In learning psychology, a method of criteria change, or ***"criteria escalation plan", is*** used. The essence of this method is that a change in a subject's behaviour in response to an increase in the level of exposure is recorded. An increase in the registered behavioral parameter is recorded, and the next exposure is made only after the respondent reaches a given level of criteria. After stabilization of the execution level, the subject is given the next level of exposure.

The method to exclude the "sequence effect" is the inversion of the sequence of effects: plan *A - B - B - A*. The sequence effects (order effects; transfer effects) are related to the effect of the previous effect on the subsequent one. The transfer may be positive or negative, symmetric or asymmetric. The sequence *A - B - B - A is* called a ***position equation scheme***.

As R. Gottsdanker notes, the effect of variables *A* and *B is* due to the effects of early or late transport. Effects of *A* are due to late transference and *B* to early transference. In addition, if a cumulative effect is present, then two consecutive effects of *B* may affect the subject as a single cumulative effect. An experiment can only be successful if these effects are negligible. The variants of plans considered above with regular alternating or random sequences are usually very long, so they are difficult to implement in practice.

It can be said that exposure presentation schemes are applied depending on the specific possibilities that the experimenter has.

A random sequence of actions is obtained by randomizing the tasks. It is used in experiments requiring a large number of samples. Random alternation of actions guarantees from manifestation of effects of the sequence.

If the number of samples is small, a scheme of regular alternation of type *A - B - A - B is* recommended. Attention should be paid to the frequency of background effects, which may coincide with the action of an independent variable. For example, if you give one intelligence test in the morning and the second one always in the evening, the efficiency of the second test will decrease under the influence of fatigue.

The position-uniform sequence can be useful only when the number of impacts (tasks) is small and the impact of early and late carryover is insignificant.

It should be kept in mind that none of the schemes completely excludes the manifestation of differentiated asymmetric transfer. I.e., when the effect of previous exposure *A* on the effect of exposure *B is* greater than that of previous exposure *B* on the effect of exposure *A* (or vice versa).

The main artifacts in the study on one subject are virtually unrecoverable. It is difficult to imagine how effects associated with irreversible events can be eliminated. If the effects of order or interaction of variables can be controlled to some extent, the already mentioned asymmetry (differential transfer) effect is unrecoverable.

No less problems arise when determining the initial level of intensity of the registered behavior (the level of a dependent variable). The initial level of aggressiveness, which we registered in a child in a laboratory experiment, can be atypical for the child, as it is caused by recent previous events, for example, a quarrel in the family, suppression of his activity by peers or kindergarten teachers.

The main problem is the possibility of transferring the results of the study of one subject to each member of the population. It is a question of taking into account individual differences that are relevant to the study. Theoretically, it is possible to present individual data in "dimensionless" form. At the same time, the individual values of the parameter are normalized by a value equal to the spread of values in the population.

Identification of a general pattern by excluding individual differences is solved each time on the basis of a substantial hypothesis about the influence of an additional variable on the interindividual variation of the experiment results.

The results of experiments involving a single subject depend on the experimentalist's bias and the relationship that develops between him and the subject. In a long series of successive exposures, the experimentalist may unconsciously (or consciously) act so that the subject is provoked to behave in a way that confirms the experimental hypothesis. That is why ***"blind experiments"*** and ***"double blind experiments" are*** recommended in such studies. The first option is that the experimentalist knows and the subject does not know when the latter gets the placebo and when to act. The second option is that the experiment is conducted by a researcher who is unfamiliar with the hypothesis and does not know when the placebo or exposure is given to the subject.

Experiments involving a single subject play an important role in psychophysiology, psychophysics, psychology of learning, cognitive psychology. The methodology of such experiments is characteristic for the psychology of programmed learning and social management, clinical psychology, and especially for behavioral therapy, the main propagandist of which is G.Yu.Eisenk.

Questions for discussion:

1. Plans for one independent variable and several groups.
2. Plans of experiments for one subject.
3. The first research paradigm.
4. The second research paradigm (B.F.Skinner, G.A.Murray, etc.).
5. The third research paradigm (A.R.Luria).
6. Position equalisation diagrams.

3.3. PLANNING A MULTIFACTORIAL EXPERIMENTA

3.3.1 Variables and circuits in factor experiment

3.3.1.1 Independent and associated variables

If changes of more than one controlled independent variable are considered in the experimental plan, such experiment is called factor (multifactor). For a factor experiment, the plan will include an indication of the conditions in which the levels of two or more variables are combined. According to the principle of isolated conditions, the functional control of each NP occurs independently of the other.

The second independent variable may be introduced to control changes associated with the same baseline process as the first NP. Or to clarify the

mental mechanisms behind the changes in WP. This specification of hypotheses can also be presented as a choice on the basis of the obtained results of one of the implied basic variables reconstructed as central components of WP.

Other types of refinement of psychological hypotheses based on the use of factor schemes are the control of concomitant mixing and multilevel experiments. There can be one WP in a factor experiment. If there are several of them, the general scheme of data processing is usually the same for different indicators. The changes concern only the aspect of applicability of some or other statistical criteria, depending on the type of scales. A factor experiment should not be confused with a multidimensional one, which is characterized by multiple changes in both NP and WP.

In psychology, a form of variable control is widely known that provides for consideration of the influence of other variables present in the organization of the experimental impact itself. It is called the ***"accompanying mixing control scheme"***.

It should be kept in mind that introducing two or more variables into the experimental situation sets a new criterion for comparing experimental plans. Variation of NP can be presented in this case both in intraindividual and intergroup schemes. How many experimental conditions or groups in which different levels of experimental factors are combined depends on the number of these levels. In the case of a ***complete plan*** that covers all formally assumed combinations of the first and second, and so on, variables, this number is equal to the product **n** x **m,** where **n is the number of levels of the** first factor and **m** is the number of levels of the second factor. For a simple case of a two-level NP, the complete plan looks like a 2 x 2 matrix. The 2 x 3 plan means that the second independent variable was represented by three levels. Accordingly, six experimental conditions differed by the two variables were used.

Besides complete plans, so-called ***patchwork plans*** are actively used in psychology. Due to some circumstances, not all groups of WP measurements supposed by a complete plan take place in such plans. Sometimes the source of these plans is a new competitive hypothesis, for empirical estimation of which a researcher adds new data for comparison with the results of the groups in the previous experiment. Another reason for developing patchwork plans is impossibility to implement a complete experimental comparison scheme for economic reasons, or impossibility to combine certain conditions of two WPs.

The appearance of the very ***fact of measuring*** WP (testing effect) as a variable mixed with the measured experimental effect is an important reason for applying factor plans.

Introduction of a new NP into the experimental scheme may serve the purpose of including a new relation into the hypothesis under test (the effect of the second variable on the same basic process or the effect of combining their conditions). Introduction of the second NP is also possible for the purpose of dilution of different basic processes actualized at different levels of the basic variable (manifested by introducing control of the second variable). Finally, factorial schemes arise to achieve the goal of controlling the concomitant mixing of the primary NP with the other NP internally related to the way it is controlled.

If the psychological hypothesis assumes the influence of two or more experimental factors on WP, then such hypothesis is called combined. The verification of combined hypotheses can be considered as the main advantage of factor experiments. However, it is necessary to consider one more aspect of factor planning connected with the fact that the experimental conditions themselves (to be more precise, the order of their presentation) lead to procedural appearance of the second independent variables. This takes place in schemes of multilevel experiments.

Questions for discussion:

1. Factor (multifactor) experiment.
2. A multi-level experiment.
3. Control circuit of the concomitant mixing.
4. Intraindividual and inter-group schemes.
5. "Full" and "patchwork" plans.
6. Combined hypotheses.

3.3.1.2. Multi-level experiment as a factor

Conducting experiments using more than two levels of the same NP also leads to the appearance of factor schemes. A distinction should be made between ***qualitative*** and ***quantitative*** NPs. Two features are usually included in the concept of a multilevel experiment: 1) the NP is represented by more than two levels; 2) the procedure for presenting these levels of the same NP is controlled

by a special scheme. This scheme implies equalisation of the orderly position of each level in the general sequence of conditions. Thus, a multilevel experiment is opposed to a two-level (bivalent) experiment.

In a bivalent experiment, the experimental and control conditions may differ qualitatively or quantitatively. ***Quantitative measurements of*** variables are usually referred to when indicators meet ***interval*** or ***ratio scales***. Qualitative measurements of variables are discussed in all cases where quantitative measurements are not possible.

A multilevel experiment with one (main) NP is often based on factor patterns. This is possible because in this case ***the order of presentation of*** NP conditions can be considered as the second experimental factor.

Two experimental schemes are best known: (a) equalization according to the scheme of the ***full Latin square***, and (b) equalization according to the scheme of the ***balanced Latin square***. Both schemes represent variants of experimental plans in which all levels of the first NP are shown to each subject. In this case, the second NP is formed due to the distribution of subjects into groups, each of which is presented with one of possible sequences of the first NP levels.

Group test subjects	Unbalanced Latin square	Balanced Latin square
1	ABVDE	ABVDE
2	VDGAEB	BGAEVD
3	DWAYBG	WADBEG
4	BGEVAD	GEBDAV
5	GABDWA	DWAYGB
6	EADBGV	EDGVBA

Scheme. Latin square in planning a multi-level experiment. Capital letters indicate six levels of the experimental factor.

The balanced square scheme is distinguished by the fact that each NP level is immediately preceded by each other once. The sequence effects associated with the impact of one UP level on another are not removed by these plans. But they can be controlled by averaging the obtained PP indices for each level occupying a different place in each sequence. Position equalization schemes can act as intraindividual plans. But different sequences of levels, in each of which each WP condition is presented only once, can be presented to different groups of

subjects. In such a case, the experiment is called crossindividual. Equivalent groups will perform experimental tasks at all levels of NP, but will differ from each other in the order in which the levels are presented. Therefore, the order of presentation becomes the second NP in this case.

Experimental control in such a cross-individual scheme covers all variables associated with interindividual differences. All subjects pass through each level of the first (basic in terms of the hypothesis being tested) NP. In this case, the control of the sequence effects is carried out by averaging the WP indices by the set of positions of the same level in all sequences.

The effects of the sequence are one of the main threats to internal validity in any multi-level experiment, be it intraindividual or crossindividual.

The schemes of positional equalization and random sequence (randomization) do not change fundamentally in the transition from bivalent experiment to multilevel experiment. But they usually assume additional efforts of the experimenter at drawing up a sequence of samples on leveling the number of levels in different parts of the sequence. In other words, in the time perspective of their implementation.

Thus, instead of a random strategy, a quasi-random experiment is usually used in an intraindividual multi-level experiment. That is, a random order of different levels of the experimental factor is assumed in a selected separate section of the general sequence. The quasi-random sequence control includes the violation of randomization. This happens because when compiling the general sequence of samples, their representation in different parts of the sequence is additionally equalized (balanced). In the opposite case, an irregularity in the distribution of higher and lower levels of the factor (by level submission numbers) may occur accidentally.

Control of the time factor in intraindividual multi-level schemes becomes a separate problem. It can be partially solved by switching to a crossindividual experiment. Then the simplest option would be, for example, a reverse equalization scheme. I.e. the first group of subjects gets ABSD conditions, and the second group gets DSBA conditions. That is, the same sequence, but in reverse order. If the psychological hypothesis allows analysis of the group averaged indicators, then in such a scheme the question about the degree of control of side variables remains unsolved. In particular, it is possible to consider background changes of WP indices in time as linear ones. Position

equalization is an adequate scheme only if the latter condition is fulfilled and transfer effects are symmetrical.

Restrictions in the transition to cross-individual schemes are primarily related to the solution of problems of meaningful planning.

The Latin square can be applied in both types of schemes: ***intraindividual*** and ***crossindividual***. Like other schemes, this form of control does not remove transfer effects, and therefore in cases of heterogeneous or asymmetric effects (effects of one level of NP on the other) data averaging occurs at poor internal validity.

If all effects of one level of NP on the other were symmetric and related to only one previous level of NP, the balanced square would be considered the best factor scheme for a multilevel experiment. However, in a multilevel experiment, the experimental samples form a series in which not only the NP levels differ in themselves, but also the preceding NP levels. As a result, there are effects of the series, such as the centring effect, where the middle members of the series find themselves in the most favorable conditions.

Questions for discussion

1. Qualitative and quantitative independent variables.
2. The order of presentation as an experimental factor.
3. Full and balanced Latin square.
4. The effect of consistency as a threat to internal validity.
5. Quasi-random control.
6. Time factor control.

3.3.2 Features of hypotheses tested in a multifactorial experiment

3.3.2.1. Hypotheses with one relation and combined

It is possible to draw a conclusion from the discussed properties of variables in the factor experiment about the difference between psychological hypotheses tested in it. First, they are hypotheses with one attitude. In these cases, the introduction of the second NP serves the purpose of increasing internal validity or expanding the scope of generalization of the main experimental effect

considered as the result of the first NP. Secondly, these are combined hypotheses, in the formulations of which the directed effects of each NP on WP and possible interactions between experimental factors are presented.

The use of groups of subjects with different levels of motivation (volunteers or forced participants), different experimental material (different types or levels of difficulty of the task) or variations in other aspects of experimental conditions are often aimed at expanding the scope of generalization of the dependence under study. An additional variable present in the experimental hypothesis: population of potential subjects, type of experimental influences, ways of fixation of WP are all potential sources of using factor plans.

Besides the considered aspect of blending control by introducing a secondary (control) NP, testing of one-relationship hypotheses in factor planning can be oriented towards establishing quantitative dependencies. Then by introducing the second variable the type of functional dependence, general and distinctive characteristics of the investigated causal relation from the point of view of other levels of considered conditions are specified.

From a planning point of view, the introduction of the second variable makes it possible to clarify not so much the type of functional relationship presented, for example, as a change in learning indicators depending on the level of motivation, as the preservation of the type of established relationship at other levels of difficulty of tasks.

3.3.2.2 Types of factor interaction

The most interesting are the factor experiments planned to test combined hypotheses. Such hypotheses suggest not only the results of the action of individual variables, but also the determination of the type of interaction between experimental factors. Hypotheses that include assumptions about interactions of NPs cannot be tested in the sum of usual one-factor experiments that reveal the effect of each NP separately. Thus, factor experiments may reveal patterns of this kind that are not detected in the sequential planning of all new one-factor control experiments.

The number of experimental factors determines how many types of interactions can be established according to the obtained data. If there are two independent variables, the interaction between them is called ***first order interaction***. Three

types of such interactions are conventionally distinguished, called zero, intersecting, and diverging according to their visual representation. If there are three independent variables, the ***second order interaction*** appears.

The diverging interaction can be observed exactly in those cases when the second NP allows to dilute in the WP values the contribution from the main (base) variable and the variables accompanying the base. Zero interaction implies that the action of the second NP has the same effect on WPs under all conditions of the first NP.

3.3.2.3 Plans with three or more NPs

When testing hypotheses that include a complex influence on the basis process of more than two factors, it becomes difficult to implement multi-level experiments because the full set of combinations of all conditions requires more than a dozen conditions. For example, a complete plan for three NPs with three different levels gives 27 comparable conditions (**3** x 3 x **3**).

One of the ways to reduce the dimension of the plan is the Latin square. With a full set of two variables being varied, the levels of the third variable are distributed on the obtained situations so that their presence is ensured for each pair of combinations. Let us denote conditions of the first and second variables as X and Y, and conditions of the third variable Z for clarity as A, B and C. The Latin plan is named after the accepted symbols of experimental conditions, which include combinations of levels of two variables marked with Latin letters. Then the plan of the three-factor experiment will demonstrate the possibility of preserving 9 conditions (complete plan **3** x **3**) with the introduction of the third factor.

Evaluation of the results obtained in such a factor experiment usually involves the use of dispersion analysis schemes. It makes it possible to quantitatively estimate different sources of WP variability, including first and second order interactions.

	X1	X2	X3
Y1	A	B	C
Y2	B	C	A
Y3	C	A	B

Scheme. Planning of a three-factor experiment on the scheme of "the Latin square".

Even more complexity and at the same time savings in planning allow for the implementation of the so-called ***Greco-Latin plans,*** which introduce a fourth NP. Its conditions, denoted by Greek letters, are paired with Latin symbols of the third variable.

3.3.2.4. "Nesting."

The peculiarities of formal planning of experiments are often related to the specific problems in a particular subject area dictating the primary role of certain variables and forms of their control. Thus, in sociopsychological surveys and the use of psychodiagnostic tools for measuring personal dispositions, there is an acute problem of taking into account factors of social desirability of certain answers of respondents or subjects. Specially analyzed by D.Campbell, the problem of influence of preliminary measurement of the indicator on the studied effects leads to the necessity of taking into account these influences as independent factors in the development of schemes of applied social and psychological research.

The development of experimental schemes in psychological and pedagogical research solves not only the problems of NP management and control of various kinds of mixtures. The problems of external and internal validity in such studies are often more interconnected than, for example, in laboratory experiments. Therefore, special attention is paid to the control of different kinds of interactions: NP with a group composition, NP with a time factor, NP and WP with a chosen method or "technique" for measuring a variable. In the latter case,

when analyzing the effects of "learning methods", it is recommended to follow the rule of multiplicity of WP measurements, which are differently sensitive to differences in experimental effects and shifts in the basic processes behind them.

If teachers used both methods in different classes, it could be revealed, for example, that some teachers work better than others, regardless of the teaching method used. It could be that some teachers are more effective with the former method and others with the latter. So there should be more than one teacher to monitor the combination of their individual preferences with the method used. Finally, the variable 'teacher' could be differentiated into two subgroups, male and female. Obviously, it is not possible to combine this preference variable with the variables "teacher" and "sex" because these variables are "nested" in each other.

A teacher is he and a person of a certain sex, and it is for him that this or that method is preferred. The variables 'teacher' and 'method of teaching' can then be combined in a ***non-stating scheme***. Suppose five men and women were teachers in each method of teaching. Two types of interactions should have been considered then: the teacher and gender variables would have crossed with the method of teaching. Controlling these interactions is necessary to make a generalization that takes into account the benefits of a particular learning method and that applies to teachers' use of the method regardless of their gender and individual differences.

Variable "teacher"			
men		women	
first	second	first	second
Variable "teaching method"			

Scheme. "Nesting."

The corresponding research plan ("nesting") with such a task of variables will require different methods of statistical processing than the usual combinatorial three-factor plan **2** x **2** x **2**. Here we meet with the necessary interrelation of solving the problems of substantive planning of the experiment, the choice of the plan of its implementation and the method of further data processing. The consideration of these subtleties in the planning of factor psychological experiments is necessary in connection with both the orientation to the

subsequent plans of data processing and the discussion of control over the conclusion.

In order to achieve the goals of adequate generalization, such complication of experimental schemes as ***"additional variation"***, ***sequential detailing of*** experimental effects, etc. are carried out.

Questions for discussion:

1. Combined hypotheses.
2. Secondary (control) independent variable.
3. Interaction of the first and second orders.
4. Plan "Latin square."
5. The "Greek-Latin square" plan.
6. The "nesting" plan.

3.3.2.5 Dependence of experimental effect on WP indicators

So far, factor plans have been discussed in terms of their representation of combinations of NP conditions. However, results from the same plan may look different if different indicators of the baseline process (different STs) are selected.

Representation of WP means evaluation of the selected indicators (respondents, their strategies, time of decision making, etc.) from the point of view of representation in them of the most important aspects of activity or components of psychological regulation of the studied processes. Two different indicators of intellectual strategies (average time of attempts and average number of decision attempts) can demonstrate different experimental dependencies. Choosing one of them as the only indicator of the subject's intellectual activity will distort the whole picture. Multiple representation of basic processes, i.e. their description from the point of view of different trends of changes in different WP indices, serves the purpose to clarify not only theoretical views, but also to estimate

representativeness of separate indices when fixing quantitative changes in psychological reality.

It is evident from the given comparison that when using only one WP, the experimenter could formulate different generalizations about the type of cause-effect relation in the factor experiment under consideration. The type of interaction obtained for one index does not necessarily repeat the type of interaction obtained for another WP. Thus, a substantial generalization about changes in psychological regulation of processes behind the fixed WP values cannot be reduced to a simple transfer of the obtained type of regularities to their other aspects. Discussion of the type of dependence, as applied to the reality under study, presupposes a "breakthrough" in generalization connected with reconstruction of types of changes in basic psychological processes.

The representativeness of the revealed relationship between NP and WP depends on the totality of all the mentioned components, as well as on the selected plan and method of presenting the results. For a multilevel experiment, the better representativeness of the curves reflecting the relations between NP and WP is achieved by choosing a cross-individual scheme (in comparison with intergroup and intraindividual experiments). This is achieved by controlling the effects of the sequence by averaging the data of all subjects at a given level of NP, when each subject is represented at each level. In this case, averages can be considered in terms of building a group-wide typical relationship.

3.3.3 Multi-factor experiments and multi-factor data presentation schemes

3.3.3.1 Special effects clarified in multifactor circuits

Many psychological regularities have at their basis such base variables, which are actualized only when a number of controlled conditions are combined. That is, they are inherently connected with the use of factor schemes. For example, the description of the "Strupup effect" published in the 1930s and included together with the author's name in modern textbooks on cognitive psychology is a vivid example of factor conditioning of psychological reality. In short, its content can be presented as a difficulty in the arbitrary performance of an action, if the conditions of non-congruence (inconsistency, opposite orientation) of different intentions to the action, caused by the difference between the source

and content of commands, are organized. This effect has been traced in different modality areas of stimulation: auditory, visual, etc.

The example of another factor experiment, also allowing to speak about specificity of actualized basic processes, but already subject to primary control in any psychological experiment. It represents that area of phenomena with which the researcher deals in all psychological experiments, including interaction with the subject. These are factors associated with the actualization of various "experimentalist effects". Personal properties of the experimenter (gender, age, race, "hostility", "intelligence", "attentiveness", etc.) may be in complex interactions with corresponding properties of subjects.

The problem of "good" or "bad" experimenters is known in research practice, manifesting itself, for example, as a willingness of volunteers to work with one experimenter and unwillingness to continue communicating with another. As an opportunity to differentiate situations where the experiment, in the opinion of an external observer or the experimenter himself, took place and did not take place in terms of actualization of the "motivation of the expertise", etc. The personal nature of these features should more often be talked about conditionally, since the orientation of subjects to the form of support or other feedback takes place even when the experimenter is present only in absentia. That is, the "effects of the experimenter" are multiplied by the "effects of the subject".

Modern researches of influence of interactions of personal features of the experimenter and the subject on experimental effects testify to necessity of their consideration in a context of "the nature of experimental tasks". That is, the experimental material, or the problem factor. The most conducive to the manifestation of the effects of the experimenter are the following four characteristics of the tasks: 1) participation of the experimenter in the activities of the experimenter, 2) ambiguity of tasks, 3) difficulty of tasks, 4) correspondence between the nature of the task and the property of the experimenter under consideration. The study of these problems has given rise to many "centaur schemes" in which one variable (the problem factor) changes in an intraindividual sequence and the second variable (the personality property of the experimenters or subjects) is controlled by selecting groups that differ in the property under consideration. The intergroup comparison sets two or more conditions of the second variable in the general factor scheme.

Summarizing the results of the experiment indicates the need to control all the conclusions from the studies, which are obtained for the subjects who want to be exposed to psychological effects. For example, for persons who have voluntarily

come to the group of psychotherapy without pragmatic goals (such goal may be, for example, the increase of communicative competence), the psychologist's analysis of their inherent personal properties may contribute to the decrease in the quality of generalization of results.

It is unlikely that the regularities of self-regulation observed for these volunteers can be transferred to other test subjects by the criterion that they do not need this type of psychologist. Here, one of the criteria of success of many kinds of activity of practical psychologists is viewed: ***if a person is ready to pay*** for pleasure from work with the psychologist (as a "client", participant of the group, etc.), then most likely, ***he or she ensures the effectiveness of*** this work.

Questions for discussion:

1. Representation of a dependent variable.
2. The Strupe effect.
3. "Good" and "bad" experimenter.
4. Four characteristics of the tasks most conducive to the manifestation of the "experimenter effect".
5. Research "centaur schemes."
6. Willingness to participate in the experiment as a factor ensuring its effectiveness.

3.3.4. Multi-factor plans

Multi-factor experiments are used when it is necessary to test complex hypotheses about relationships between variables. The general appearance of such a hypothesis: "If *A1, A2,...*, *Ap,* then *B.*" Such hypotheses are called complex hypotheses (combined, etc.). In this case, there may be different relations between the independent variables themselves. Factor experiments are a particular case of multidimensional research, in the course of which they try to establish relations between several independent variables and several dependent variables. As a rule, in a factor experiment, two types of hypotheses are tested simultaneously:

1. hypotheses about the separate effects of each of the independent variables;

2. hypotheses about the interaction of variables, namely, how the presence of one of the independent variables affects the effect on the other.

The factor experiment is based on a factor plan. The factor plan of the experiment consists in combining all levels of independent variables with each other. The number of experimental groups is equal to the number of combinations of levels of all independent variables.

At present, factor plans are most common in psychology, as simple dependencies between two variables are almost never found in it.

There are many variants of factor plans, but not all are applied in practice. Factor plans are most often used ***for two independent variables and two levels of*** *type 2 x 2. The* principle of balancing is used to make the plan. A 2 x 2 plan is used to identify the effect of two independent variables on one dependent variable. The experimenter manipulates possible combinations of variables and levels.

Four independent randomized groups are less frequently used. Dispersion analysis by Fisher is used to process the results. Other versions of the factor plan are also rarely used, namely: 3 x 2 or 3 x 3.

Plan 3 x 2 is used when you want to set the type of dependence of one dependent variable on one independent variable. In this case, one of the independent variables is represented by a dichotomous parameter. An example of such a plan is an experiment to identify the impact of external observation on the success of intellectual tasks. The first independent variable varies in the following way: there is an observer, there is no observer. The second independent variable is the levels of difficulty of the problem. We get a 3 x 2 plan.

Option 3 x 3 applies if both independent variables have multiple levels and it is possible to identify the types of relationships of the dependent variable with the independent variables. This plan allows you to identify the impact of support on the success of tasks of different difficulty.

2nd variable	1st variable	
	Got .	No .
Got .	1	2
No .	3	4

1st variable	2nd variable		
	Easy	Average	Difficult .
There's an observer	1	2	3
No observer.	4	5	6

Task level of complexity	Stimulation intensity		
	Low	Average	High
Low	1	2	3
Medium	4	5	6
Tall	7	8	9

In general, the plan for two independent variables looks like ***N x M***. The applicability of such plans is limited only by the need to set a large number of randomized groups. The volume of experimental work increases excessively with the addition of each level of any independent variable.

The plans used to study the impact of more than two independent variables are rarely applied. For three variables, they share a common view of ***L x M x N***.

The most frequently used ***plans are 2 x 2 x 2***: "three independent variables - two levels". Obviously, adding each new variable increases the number of groups. Their total number *is* 2n, where *n is the* number of variables in case of two intensity levels and *K is the number of variables in* case of *K-level* intensity (we believe that the number of levels is the same for all independent variables). An example of this plan is the development of the previous one. In the case when we are interested in the success of an experimental series of tasks, depending not only on the total stimulation, but also on the ratio of encouragement to punishment, we apply the plan **3 x 3 x 3**.

	L1	*L2*	*L3*
M1	*A1*	*B2*	*C3*
M2	*B2*	*C3*	*A1*
м3	*C3*	*A1*	*B2*

The simplification of a complete plan with three independent variables of the type ***L x M x N*** is the "Latin square" planning method. The ***"Latin square" method is*** used when it is necessary to investigate the simultaneous influence of three variables with two or more levels. The principle of the "Latin square" is

that two levels of different variables meet experimentally only once. Thus, the procedure is greatly simplified, not to mention the fact that the experimenter gets rid of the need to work with huge samples.

Suppose we have three independent variables, with three levels each:

1. *L1,L2,L3.*

2. *M1,M2,M3.*

3. *A, B, C.*

The same method is used to control external variables (***counterbalancing***). It is not difficult to notice that the levels of the third variable *N (A, B, C)* occur once in each row and in each column. Combining the results by rows, columns and levels, we can identify the influence of each of the independent variables on the dependent as well as the degree of pairwise interaction of the variables.

The "Latin Square" allows for a significant reduction in the number of groups. In particular, the **2 x 2 x 2** plan becomes a simple table. Application of Latin letters in cells for designation of levels of the third variable *(A* - is, *B* - *is* not) traditionally, therefore the method is named "Latin square".

The more complex ***"Greco-Latin square"*** plan is very rarely used. With its help it is possible to investigate the influence on the dependent variable of four independent ones. The essence of it is as follows. Each Latin group of the plan with three variables is joined by a Greek letter, indicating the levels of the fourth variable.

The Fisher Dispersion Analysis method is often used for data processing. The methods of "Latin" and "Greco-Latin" square came to psychology from agrobiology, but were not very widespread. The exceptions are some experiments in psychophysics and psychology of perception.

The main problem that can be solved in a factor experiment and cannot be solved by applying several usual experiments with one independent variable is the evaluation of the interaction of two variables.

2nd variable	1st variable	
	Got .	No .
Got .	*A*	*B*
No .	*B*	*A*

	L1	L2	L3
M1	A_α	B_β	C_γ
M2	B_β	C_γ	A_α
M3	C_γ	A_α	B_β

It is possible to consider possible results of the simplest factor experiment **2 x 2** from the positions of interaction of variables. For this purpose, we need to present the results of the experiments on the graph where the values of the first independent variable are deferred along the abscissa axis and the values of the dependent variable are deferred along the ordinate axis. Each of the two lines connecting the dependent variable values at different values of the first independent variable *(A)* characterizes one of the levels of the second independent variable *(B). The* following variable interactions are possible: zero; divergent (with different signs of dependence); overlapping. The evaluation of the interaction value is carried out with the help of dispersion analysis, and the Student's *t-criterion* is used to evaluate the significance of differences in group $\overline{X}$.

The method of ***balancing is*** applied in all the considered variants of experiment planning. In other words, different groups of subjects are placed in different experimental conditions. The procedure of balancing the group composition allows comparison of results. However, in many cases it is necessary to plan the experiment in such a way that all the participants get all the variants of influence of independent variables. Then the ***counterbalancing*** technique comes to the aid.

Plans that embody the strategy of "all subjects - all impacts", McCall calls ***rotation experiments***, and D. Campbell calls ***"balanced plans"***. To avoid confusion between "balancing" and "counterbalanced", the term ***"rotary plan"*** can be used.

Rotary plans are made by the method of "Latin square". But, in contrast to the above example, rows are marked by groups of subjects, not levels of the variable. The impact levels of the first independent variable (or variables) are marked by columns. And the table cells indicate the levels of impact of the second independent variable. An example of an experimental plan for three groups *(A, B*, C) and two independent variables *(X, Y)* with three intensity levels (1st, 2nd, 3rd) is given below. It is not difficult to notice that this plan can also be rewritten so that the cells have the levels of variable *Y*.

Д. Campbell includes this plan among the quasi-experimental ones on the basis that it is not known whether the external validity is controlled by it or not. Indeed, it is unlikely that in real life the subject can receive a series of such effects as in an experiment. As for the interaction of group composition with other external variables (sources of artifacts), according to D. Campbell, group randomization should minimize the influence of this factor.

The sums by columns in the rotation plan show the difference in the level of effect at different values of one independent variable *(X* or *Y*), and the sums by rows should characterize the differences between groups. If the groups are randomized successfully, there should be no inter-group differences. If the group composition is an additional variable, then it is possible to control it. The counterbalancing scheme does not allow to avoid the effect of training, although data from numerous experiments with the "Latin square" do not allow to draw such a conclusion.

Group	Levels 1 of variable		
	X1	*X2*	*X3*
A	*Y1*	*Y2*	*Y3*
B	*Y2*	*Y3*	*Y1*
C	*Y3*	*Y1*	*Y2*

To summarize the various options for the pilot plans, a classification can be proposed. Experimental plans can be distinguished on the following grounds.

1. ***Number of independent variables***. One or more. Depending on their number, either a simple or factor plan is applied.

2. ***Number of levels of independent variables***. At two levels it is a question of establishing a qualitative link, at three or more levels it is a question of establishing a quantitative link.

3. ***The subject of the exposure***. If the scheme "each group has its own combination" is applied, it is an inter-group plan. If the scheme "all groups - all effects" is applied, it is a rotation plan. R. Gottsdanker calls it a cross-individual comparison.

The planning scheme of the experiment can be homogeneous or heterogeneous (depending on whether the number of independent variables is equal to or not equal to the number of levels of their change).

Questions for discussion:

1. Complex hypotheses and multifactorial experiments.
2. Two types of hypothesis in a factor experiment.
3. Factor plans for two independent variables and two levels of type 2 x 2.
4. Factor plans *3 x 2 and 3 x 3.*
5. Rotary plans.
6. Grounds for distinguishing experimental plans.

3.4. CREDIBLE AND FALSE CONCLUSIONS

3.4.1 Conclusion control as the final stage of the pilot study

After the stages of planning and practical implementation of the psychological study, obtaining and discussing the results, conclusions or generalizations should be made. The possibilities of generalizations depend significantly on the research method implemented (observation, measurement, correlation research, quasi-experiment or experiment). It should be borne in mind that causal explanations of the empirical pattern are possible only when conducting an experimental study. The type of experiment, i.e. the experimental method of solving the problems of correspondence of the components of the experimental model to the variables represented in real conditions or in the theoretical model, in its turn, sets and limits the possibilities of generalizations.

The orientation of the experiment, i.e. it was conducted for practical or scientific purposes, also determines the ways of subsequent generalizations. The conclusions about the necessity to reject the hypothesis under test or to consider it empirically confirmed, as well as about the possibility of transferring generalizations beyond the experimental situation may turn out to be reliable or unreliable. When distinguishing between the organization of theoretical conclusions, which include the assessment of structural validity and the principle of falsification (asymmetry of conclusions), and the organization of empirical conclusions aimed at justifying practical recommendations or adopting

a "working hypothesis", one can identify a number of common problems that a researcher should solve if he does not want to avoid the error in generalizations.

Assessment of the validity (reliability) of the conclusions made on the basis of empirical data analysis includes a number of directions of the researcher's reasoning. The possibility to make a mistake in the level of generalization concerning the psychological hypothesis being tested, in making a decision about the empirical fact or about the type of dependence obtained remains always, including in the case of obtaining reliable experimental results.

Based on a properly planned and properly conducted (valid) experiment, the researcher gets reliable results. However, their generalization presupposes a number of inferences in the course of which a researcher may show logical incompetence or errors of reasoning. With regard to experimental research, this means not only a violation of the rules of logic (when comparing general and private assumptions in the conclusions), but also the substitution of the standards of hypothetical-deductive reasoning with the adoption of unfounded arguments. As a result, the conclusions drawn turn out to be invalid, unreliable, and biased, i.e. false or artifact.

3.4.1.1 Main sources of false conclusions

There are two main ways to reach erroneous or artefact conclusions.

- Conducting ***an invalid experiment*** and, as a result, making erroneous decisions about the results of the variables (i.e., the experimental effects obtained);

- ***Insufficient*** output ***control***. I.e. violation of the rules of hypothetical-deductive reasoning and implementation of incorrect deductions or incorrect generalizations.

In the second case, it is implied that artifact findings can be made even after reliable data have been obtained in a valid experiment. Here you can also specify two main paths to possible errors.

- On the one hand, these are unreliable conclusions in statistical decisions (errors in making decisions about rejecting or not rejecting null hypotheses). They cover the ***transition from the level of statistical hypotheses*** to the level of

psychological hypotheses (experimental hypothesis, counter hypothesis or competing hypothesis).

- On the other hand, errors in the conclusions can characterize a researcher's ***transition to*** such ***generalizations*** when groundless arguments are used or when the logic of correlating empirical results with theoretical hypotheses is replaced by subjective preferences in the direction of substantiation.

Discursive and intuitive components are inextricably linked in the regulation of the researcher's thought. Although science does ***not have rules for producing "right" hypotheses***, there are rules for organizing reliable conclusions that have developed in the practice of research paradigms. In psychology, this is, first of all, an understanding that the application of a certain method implies not only a substantial assessment of hypotheses, but also the implementation of norms for comparing empirical results and conclusions based on them. Therefore, planning the study directly determines the possibilities of subsequent generalizations.

Theoretical orientation of conclusions connected with acceptance of positions of this or that psychological school, corresponds thus with performance of such rules of experiment, as performance of conditions of a causal conclusion, experimental control of variables, etc. It is at the stage of conclusions that the psychologist relies the most on non-normative components in individually carried out generalizations. Researchers may be critical to the course of their reflections to different degrees, but as responsible professionals, they must present those possible pitfalls that they need to overcome on the way to correct conclusions.

Typical directions for errors in conclusions made by psychologists in generalizations.

1. The inclusion in the findings of allegations that are unreasonably believed to be grounded but not empirically corroborated (i.e., not related to the research findings or of an evaluative nature).

2. Undue generalizations when dependency is transferred to other psychological realities (other situations, activities, populations).

3. Substitution of the norms of the experimental method with subjectively obvious but logically unreasonable inferences.

4. Violations of the rules of correlation of theoretical hypotheses and empirical results and a number of others.

Any experiment is made to generalize the results outside of the experiment, but this does not mean that any generalizations can be made from the experimental data. Estimation of the validity of the conducted research is an essential condition of control over the conclusions related both to the decisions on the established dependence and to the necessity to formulate competing hypotheses, the source of which may be the "technical" conditions of the experiment.

Questions for discussion:

1. An experiment as a possible causal explanation.
2. Invalid experiment as a basis for artifact findings.
3. Insufficient control over the conclusion as the basis for artifact generalizations.
4. Inadequate conclusions in statistical decisions as a basis for artifact generalizations.
5. Linking discursive and intuitive components in generalizations.
6. Typical errors that psychologists make in generalizations.

3.4.1.2 Evaluation of the validity of the experiment as a condition for reliable conclusions

Sometimes the evaluation of the reliability of the conclusions is associated with the possibility to draw such conclusions on the basis of the experiment, which would be reasonable in case of approaching this experiment to perfect. A broader interpretation of the reliability of the conclusions includes an assessment of their correctness from the point of view of the logical competence shown by the researcher and, thus, the legitimacy of the generalized statements made in the conclusions. Evaluation of the subjects' representativeness, experimental conditions, and validity of the obtained results is an important stage in controlling such an aspect of generalizations as substantiation of transferring the established dependencies to other situations, activities, other subjects, etc.

The first step in assessing the validity of the conclusions is to evaluate the correctness of decision-making on the experimental effect, or empirically established relationships between the variables.

General lines of reasoning following statistical decisions about the possibility of making an experimental hypothesis.

First, it is the reasoning implemented by the researcher in transitions between different levels of tested hypotheses: statistical and substantial (i.e. psychological), experimental (including counterhypothesis) and theoretical.

Secondly, it is reasoning in substantiation of the interpretation link of experimental and theoretical hypotheses. That is, psychological understanding of the accepted causal explanation, and other (competing) psychological explanations.

3.4.1.3 Evaluation of the adequacy of generalizations

Errors in generalizations can occur because they include "substandard", i.e., not clothed in the form of logical requirements, reasoning about the permissible levels of the spread of experimental dependence on other levels of the psychological reality under study. These generalizations concern statements about transfer of experimentally revealed regularities to other types of situations, other samples of subjects, etc. At the same time, the grounds for such spread of generalizations beyond the limits of the experiment are discussed in detail, taking into account the methods of selection of subjects, factors of the tasks performed by them, representativeness of the established dependence from the point of view of levels represented in the experimental situation by additional variables, but first of all, aspects of external and constructive validity.

In theoretically directed generalizations, the most important aspect is to take into account the type of experiment carried out. Recall that in laboratory experiment generalization assumes the path "model - experiment - theory".

Recognition of the limitation in breadth of generalization of this or that pattern is related not only to the assessment of the type of research conducted and the levels of additional variables presented in it. Restrictions arise when solving questions about the substantive correspondence between the type of psychological regulation presented by the basic processes studied in the

experiment and the implied (hypothetical) scientific model. In a number of life or professional situations differing in the subject orientation of activity, significance of psychological variables for the subject, possibility of manifestation of activity, etc., the experimentally established dependence can be not revealed, being subject to other psychological determinants.

Taking into account the scheme of conducting experiments (in controlling the conclusion) involves evaluating the representativeness of individual data or obtained "averaged" dependencies for other people or samples. That is the decision of a question on a parity of casual and natural, concerning an analyzed individual case, groups of people or populations.

Finally, the correctness of the conclusions is related to the justified transition from the logic of the experimental proof "from within", i.e., in relation to the conducted research, to the logic of proof "from outside". In the latter case, we mean a critical comparison of the validity of generalizations made by the author of the study with other possible theoretical interpretations of the established dependencies or "common sense" arguments. The consideration of arguments only in favor of the psychological explanation presented in the experimental hypothesis and the exclusion from the discussion of the results of comparing it with other possible theoretical interpretations also contradicts the standards of experimental reasoning, as well as the rejection of the conclusion asymmetry described earlier in the empirical testing of the generalized statements truth.

Questions for discussion:

1. Psychological content of reliability of conclusions.

2. The first stage of assessing the credibility of the findings.

3. Negative reasoning as a factor of generalization errors.

4. Scheme of generalization "model - experiment - theory", typical for laboratory experiment.

5. The question of the ratio of random to natural as a way to avoid generalization errors.

6. The transition from the logic of proof "from within" to the logic of proof "from outside" as the basis for generalization errors.

3.4.2 Scheme of conclusion about psychological hypothesis on the basis of results and evaluation of validity of experiment

The purpose of any experiment is to check the validity of the formulated statement about causal dependence only from the point of view of correspondence or non-conformity of the empirically established link between NP and WP. The interpretation part of the hypothesis (its psychological explanation) is not evaluated from the point of view of its truth in the experiment itself. The hypothetical constructs used to describe the relations between NP and WP belong to the world of theories, and the evaluation of the adequacy of these or those theories includes not only the theory's relation to the world of empiricism, but also many other components. For the psychological theory, for example, assumptions about the type of a psychological explanation and a method of reconstruction of the psychological law are essential.

Thus, with the help of the instrumental-genetic method (in particular, during the implementation of the "double stimulation" methods) within the framework of L.S.Vygotsky's cultural-historical theory, other types of psychological regularities were reconstructed than those that were called dynamic by K.Levin and demonstrated by him on a different path of explanation (with the appeal to the structures of stresses in the psychological field).

The possibility to take place or not to take place this or that psychological event was implied in the construction of both types of research, but both types of psychological explanations implied deterministic statements about why this event could take place. The received results were connected here, first of all, with estimation of structure of the situation in which the subject was. From this point of view, they included justifications that, in later experimental terminology, should have been classified as problems of structural and operational validity.

The same two theories demonstrate different types of explanations from the point of view of the accepted in them understanding of psychological causality and character of the involved analogies ("stimuluss-means" or "pluses-minuses" in the psychological field are metaphorical, but these metaphors reveal different types of possible reflections on psychological reality).

The conclusion about the psychological hypothesis will not be considered here in the specified substantial differences of types of psychological explanations. The control over a conclusion assumes more formal aspect of an estimation of acceptability of the checked statement from the point of view of the analysis of

logically possible correlations of the empirical result and an estimation of validity as the realized forms of experimental control. If in research the experimental effect expected according to the formulated psychological regularity has been received, it does not allow to accept or reject EG or CG automatically yet. The next condition influencing acceptance of the conclusion about empirical dependence is the evaluation of validity of the psychological experiment, including real methodological ("technical") conditions of its realization. The evaluation of validity provides an opportunity to make the following conclusions.

In the case of high ***internal*** and ***operational*** validity of the experiment, the data expected under the experimental hypothesis are considered to have passed the test and confirmed. However, due to the principle of asymmetry of conclusions considered earlier, the researcher can not, on the basis of obtaining the expected experimental effect, consider proved the theory from which the EG is expressed. The conclusion can sound approximately as follows: the obtained experimental data do not contradict the formulated EG and, accordingly, the proposed theory. The fact that the psychological hypothesis has withstood the test by an experimental way does not yet testify to the "correctness" of the psychological explanation type supposed by it. It is clear that such a conclusion also does not affect estimations of the experimental effect "from the outside", i.e. from the point of view of so-called competing explanations.

The low ***validity*** and the identified data in favor of the EG mean that the experiment should be called a failure. Due to uncontrolled conditions in the studies placed here, it is impossible to exclude the mixing of side, in this case artifact, variables with the experimental effect. Therefore, a positive conclusion about the acceptance of EG in them can not be made, although the corresponding experimental hypothesis data were obtained. The problems of adequate organization of data collection are solved on the levels of both substantial and formal planning. In other words, by taking into account all the subtleties of experimental control. And in case of successful solution of conformity problems (conformity of independent, dependent, additional variables), i.e. in case of good ***external validity, the*** conclusion may be unreliable if the experimental conditions themselves are not sufficiently "pure" and operationally justified.

Research to obtain negative effects (i.e. data "against" the EG) in a high-altitude experiment means that a strict conclusion in favor of the counter hypothesis is possible. This is exactly the main way of rejecting theoretical positions on the

basis of their experimental verification, which is assumed by the logic of "output asymmetry". That is, it is a case of strict rejection of theories based on obtaining "negative" results. The experimental method is considered to be the strictest way of empirical testing of theories, as it allows rejecting "wrong" hypotheses as not corresponding to reality. However, in the logic of comparing different psychological hypotheses that have different empirical reinforcements (in the set of carried out experimental works and in the history of changing some hypotheses by others), different criteria are applied to justify the validity of such negative conclusions.

Thus, a single negative result in itself does not entail rejection of a substantial hypothesis. Often there must be some accumulation of such "negations" so that the data in favor of the counter hypothesis are indeed accepted as sufficient arguments to reject the theoretical hypothesis being tested. In any empirical study, it is possible to try to look for deviations from a perfect sample, on the basis of which the negative effect can be associated with the artifacts of conduction.

Data "against" the EG at low experimental validity. If the previous situation includes studies with obtained negative effects, which give a movement on the path of development of theoretical knowledge, then this last field has a negative assessment in another sense. No conclusions can be drawn from such experiments, except for the low qualification of the researcher.

Questions for discussion:

1. The purpose of the experimental study.

2. The connection between theory and type of explanation.

3. Validity assessment as a condition for making a decision about the investigated connection.

4. High internal and operational validity of the experiment as a condition for confirming the hypothesis.

3.4.3 Substantive conclusions and logical competence in summarizing the data from the psychological study

3.4.3.1 Conclusion on the support of the theory by experimental facts

Taking into account how the conformity problems were solved, i.e. the evaluation of the approximation of an experiment to a mental sample of a full conformity experiment, makes it possible to distinguish between such types of experiments as ***laboratory***, ***artificial*** and ***natural***. The orientation and latitude of the subsequent generalizations essentially depend on the orientation to the type of experiment carried out.

For an experiment ***with scientific purposes,*** generalization of the type of causal dependence obtained, if there was a ***pure laboratory experiment***, is carried out in three stages. The *first stage* makes a conclusion about the type of dependence, the *second one* - about the validity of the theoretical model used, the *third one* realizes the path from theory to reality. In other words, the correspondence of "psychological reality" - its explanation with the help of the scientific hypothesis tested in the laboratory experiment - is considered. Reinforcement of the theory with the data obtained in the "pure" experiment makes it possible to spread the highest level generalizations supposed in it to all those types of psychological reality, which can be considered in the appropriate meaningful context.

For experiments ***with practical purposes, the*** output logic is sometimes simplified. Generalizations from the experiments "duplicating" or "improving" the real world, directly correlate the established psychological laws to possibility of their display in these or those situations or kinds of activity, concerning only the person participating in the experiment or other people.

For example, if the hypothesis that the new method of learning is more effective than a traditional one is tested, then obtaining data for the EG is directly interpreted as proof that the new method is "better" than the traditional one. Often the next unreasonable step is taken. Automatically, it is also considered to be "proven" the theoretical link of generalizations, which formed the basis for the development of this new method of learning. In such generalizations, the researcher goes beyond the principle of falsifying hypotheses. This principle is concretized in such a way that if it is possible to reject the hypothesis that there are no differences in WP between experimental and control conditions, it is possible to accept the EG statement as not contradicting the empirical one.

However, on the basis of this, it is not yet possible to consider as proved the truth of the theory from which this hypothesis follows. One can always expect both new empirical facts contradicting it and newly formulated explanations coming from other theories. In this sense, any directed hypothesis is always open for further verification.

In the case of ***"field" experimental studies*** characterized by *high structural validity*, the question of the breadth of permissible generalizations in the case of "positive" results can be solved positively, even if the corresponding theoretical hypotheses were developed for other conditions or populations. However, the variation of conditions and populations should not exceed the limits beyond which the use of the same constructs is already senseless, since other factors begin to play a dominant role. "Proof", for example, of many socio-psychological hypotheses can be understood precisely as their compliance with certain conditions of society. The impossibility of transferring them to other conditions for interpretation does not mean that their empirical support has decreased.

The problem of "proof" of theoretical hypotheses, i.e. generalizations at a higher level than the postulated experimental hypothesis, is related to the following points. First, these are methodological disputes at the level of rational knowledge itself, i.e. comparison of systems of theoretical constructions. Secondly, it is an evaluation of theory from the point of view of development of a complete research program, which includes analysis of the system of experiments within the framework of this or that school. Thirdly, it is the analysis of the number of significant results when testing this hypothesis in the research program.

3.4.3.2 The problem of the emergence of new hypotheses

When describing the experimental method, the question ***"where do experimental hypotheses come from?" was*** not discussed. They are formulated by a researcher solving scientific problems or wishing to achieve practical goals by means of scientific cognition. ***There are no rules according to which a researcher must derive new regularities on the basis of new experimental data,*** so he must formulate his hypotheses himself. New hypotheses are formulated either to explain newly observed, sometimes unexpected phenomena or to eliminate unnoticed contradictions in previously existing concepts. The "new"

hypotheses in this statement are as if contrasted with the "old" hypotheses, which have passed the test by experience. In psychological science, new hypotheses often arise in systems of new interpretations, new understanding of the subject of study, and co-exist as "contemporaries" within the framework of different psychological schools.

In addition, there is the problem of multi-level generalizations. In other words, there is always a gap between the explanation of a relationship at the level of discussing the results of the variables' action and at the level of a causal description of the resulting empirical relationship as a regularity, which requires a "qualitative leap" in generalization. This gap is filled by the researcher, ***first, by*** introducing into the psychological explanation certain ideas about how psychological laws or psychological mechanisms of regulation of activity, behavior, and communication work. ***Secondly***, this gap is filled with "missing links" between the description of dependence and generalization. It is difficult to define completeness of generalization sufficient for understanding of a regularity, and in psychology there is a mass of concepts concretized in various degrees of completeness.

New explanations and new hypotheses in psychology can arise exactly on the way of deeper and fuller coverage of those empirical dependencies, which are not new in themselves, but allow reinterpretation as a result of development of psychological concepts themselves.

Finally, new hypotheses arise when the "old" problem is introduced into a new context of discussion, begins to be analyzed in a complex of other, sometimes non-psychological, approaches.

Within the field of research such as "knowledge engineering" or "cogytology", different approaches coexist with respect to a number of issues related to the specification of the components of intellectual strategies. i.e. affecting an "old" field such as the psychology of thought. Psychological analysis of the use of information technologies makes it possible to formulate new hypotheses concerning psychological mechanisms of mediating human intellectual activity and to take a new look at the possibilities of human thinking development. In the same field of knowledge, models are also developed within the framework of so-called computer metaphor realizing one of reductionism variants in psychological explanations. Thus, the new hypothesis does not always mean movement on the way of psychological knowledge development.

The criterion ***"newer"*** cannot be synonymous with a ***"more substantial"*** hypothesis.

The experiments are conducted ***to test*** the ***hypothesis***. The approach to psychological interpretation for the purpose of generalization of the received knowledge includes an estimation of how well the hypothetical constructs presented in a hypothesis allow the researcher to make reasonable transitions between different levels of generalization of the revealed dependence and used psychological representations. This reasonableness is set at the stages of meaningful planning and analyzed again in the discussion of the obtained results. Substantive problems can, however, be solved at a different degree of reflexion of the path accomplished with proving the psychological hypothesis. In this case, the logic of the conclusion's implementation in the implementation of the experimental method should include the following components:

1. ***the hypothetical-deductive way of reasoning*** about empirical reality with the specified asymmetry of the conclusion about the scientific hypothesis ("to reject the assumption if the facts contradict it");

2. ***the construction of plans, or experimental schemes***, within the framework of an inductive conclusion about the result of the experimental factor and the possibility of a causal explanation of changes in the WP;

3. ***conclusion about the EG based on the*** analysis of the obtained effect by correlating the result with the evaluation of validity of the experiment;

4. ***substantiation of substantial grounds for generalizations of*** dependence outside the experiment.

Questions for discussion:

1. Three stages of a "clean" laboratory experiment.
2. Features of generalizations in experiments that duplicate or "improve" reality.
3. The problem of proving theoretical hypotheses.
4. The problem of new hypotheses emerging.
5. The problem of multi-level generalizations.
6. The main components of the output implementation logic.

3.4.3.3 The problem of incorrect generalizations as sources of false conclusions

Errors on the way to final generalizations lead to incorrect (artefact) conclusions. Artifact findings may be of the following nature:

1. the wrong conclusion because of the ***wrong statistical decisions***. For example, a researcher may "see" that it is necessary to reject both experimental and counter-hypothesis and to search for so-called third competing hypothesis;

2. wrong conclusion about the action of NP ***because of inattention to the strict evaluation of the validity of the experiment,*** which lies behind the error of accepting as a positive "confirmation" of the EG, an artifact result or the possibility to "look" in the experiment the true dependence;

3. incorrect generalizations ***due to insufficient consideration of significant additional variables*** or errors in understanding the relationship between a theoretical statement and an empirically loaded statement (EG);

4. ***replacement or distortion of the*** considered ***norms of the experimental conclusion*** by evaluation judgments, appeal to authority and other implicit "concessions" of the logic of reasoning substantially unfounded criteria. These errors are related to value relations to the content or conclusions of the problem and insufficient criticality to what has been missed in the study.

It makes sense to dwell on the last group of "errors in conclusions" on purpose, as they threaten substantial generalizations in those researches which were well planned. Special literature discusses a number of the following errors most often encountered in psychological studies.

"Missing links" in causation. Causal factors that are hypothetically responsible for the occurrence of a phenomenon or that cause connections of variables can be described without sufficient detailed consideration. This leads to the appearance of a "missing link" between explanation and generalization.

Incorrect identification of the underlying cause. In formulating generalizations that include explanations of empirical dependencies, a private cause can be assumed to be the main and complete cause.

Substitution of one statement by another. The author of the research can non-critically assume that one situation recorded by him guarantees the content of the other, and report only on the latter.

Error in value judgments. Substantive conclusions about the support of the psychological hypothesis by experienced data are introduced into the context of value relations so that the desired evaluation of the significance of the effect from the point of view of social or ethical criteria is associated with the problem of "proving" the author's psychological assumptions.

Wrongful appeals to authority. The assertion that some expert (group of experts) has some opinion may be offered as a sufficient or significant argument to support this hypothetical judgment. An error of judgment here occurs if a reference to an opinion of authority is used instead of the arguments that have been verified to be true. In such a context it would be more correct to evaluate not the expert's authority but the validity of substantive arguments in favor of this opinion.

It is not the scientist's regalia that is important for a meaningful evaluation of the findings of the study, but the reasoning in this particular case. After all, an authority, i.e. a recognized expert in this field of knowledge, can make the following conclusion: "If you agree with me on 9 out of 10 statements that I defend, you must agree with the tenth".

This example is given, in particular, by Australian psychologists when considering the arguments put forward by G. Eisenk to protect the hypothesis of hereditary factors of the intellect. It has been noticed more than once that an appeal to the opinion of experts, who supposedly knowingly have a better understanding of the problem than other researchers who did not get into the group of experts, arises whenever the value or socio-political attitudes of society in their approach to this or that problem are obvious. This also happens when the scientific weight, scientific or personal authority of a researcher is so high that the discussion of the hypotheses he advocates is beyond the requirements for the proof of judgments made by other authors.

Appeals to the fact. A mistake of judgment is to argue that, as a sufficient or significant argument, evidence to support a point of view is "established" or "generally accepted fact".

The argument must be made, as such evidence is likely to be controversial. Its argumentativeness will be particularly high if a description of a phenomenon in terms of a particular theory is proposed as a fact. Such a description may not be

considered to be "established" or "generally accepted" by an advocate of another theory. Finally, an appeal to the phenomenal givenness of a psychological event is not yet an argument, since the question to which the answer is linked to such an appeal is equally important. The system of reasoning, which includes a description of psychological reality, generates a psychological "fact.

The decision that a psychological fact has taken place can be based on very different systems of evidence. For example, the use of an observation method is characterized by the problem of limited interpretation, which is always included in the description of observed phenomena. For an experimental method, making decisions about the type of fact to be established involves much stricter control of the path from data collection to statements about the dependencies obtained (as psychological facts). Whatever method is meant, the conclusions always present the level of generalizations within which only established facts make sense.

Questions for discussion:

1. The nature of the artifact findings.
2. "Missing links" as the basis for mistakes in conclusions.
3. Substitution of one statement with another as a basis for errors in the conclusions.
4. Appeal to authority as the basis for mistakes in conclusions.
5. Error of value judgments as a basis for errors in conclusions.
6. Appeal to the fact as the basis for error in the findings.

3.4.3.4 Substitution (reduction) of conclusions

In psychology reductionism is considered as a substitution of psychological explanations with non-psychological ones, or as a search for an explanation of a certain number of different phenomena by means of bringing them to one principle of explanation. It can be manifested as the drawing of empirical conclusions under the interpretation schemes, which often lie in the sphere of other sciences or fields of knowledge (culturology, sociology, physiology, etc.),

or as the organization of conclusions within the accepted explanatory paradigm of some or other psychological schools. Then, speaking of reductionism, other researchers emphasize the inadequacy of the notions of psychological reality or schemes for obtaining empirical data for the subject of study.

On the basis of such variants of reductionism, when the authors discuss the declared psychological hypotheses, conclusions can be drawn that simplify the basic processes under study or the relationships between variables. Reductionism of sociological, psychophysiological or other type is a methodological conclusion of non-psychological sciences about structure of psychological explanations.

It is essential that the criteria for "right" conclusions adopted by the scientific community change over time, with changes in research paradigms and thinking styles, as well as in the authors' social attitudes. The relativity of the notion of valid conclusions is all the more understandable when one considers that the same empirical results can be used to answer different questions and introduce them into different contexts of other explanatory schemes.

Closely related to the problem of reductionism is also the problem of multiple and different levels of theoretical explanations. The same phenomenal (empirically registered) regularities can be discussed from different methodological positions - teleological, causal, psychophysiological, etc. And this is not only about the author's preferences. The point is that the psychological science itself in its development has demonstrated the productivity of different forms of building psychological theories. That in psychology different theories coexist, some authors can be characterized as a crisis (or "schism") of psychology. But another methodological assessment of this state of affairs is also possible - positive. In this case, different theoretical hypotheses may be considered equal. And the more field of these guesses, the closer psychology is to understanding its facts and laws. It would be a crisis to establish "unanimity".

3.5. INTERPRETATION AND PRESENTATION OF RESULTS

3.5.1 Research results, their interpretation and generalization

Suppose that the statistical hypothesis about the difference between the results of experimental and control groups is confirmed. What conclusions can we draw

after processing the experimental results? The result of any study is the transformation of "raw" data into a decision to detect a phenomenon (difference in behavior of two or more groups), a statistical relationship or causal dependence. Confirmation or disproof of a statistical hypothesis about the significance of the found similarities (differences, relations, etc.) must be interpreted as confirmation (non-confidence) or disproof of the experimental hypothesis. As a rule, the researcher tries to confirm the hypothesis about the differences in the behavior of control and experimental groups. In this case, a null hypothesis is a hypothesis about the identity of groups.

In the statistical output, different solutions are possible. The researcher may accept or reject a statistical null hypothesis, but it may be objectively true or false.

Accordingly, there are four possible outcomes:

1. to accept the correct zero hypothesis;

2. rejecting a false null hypothesis;

3. to accept a false null hypothesis;

4. to reject the correct zero hypothesis. Two solutions are right, two are wrong. Erroneous variants are called errors of the 1st and 2nd kind.

Error 1 is made by a researcher if he rejects the true null hypothesis. Error of the ***2nd kind*** consists in accepting a false null hypothesis (and rejecting a true statistical hypothesis about differences).

The greater is the number of subjects and experiments, the higher is the statistical reliability of the conclusion (the accepted level of significance), the less is the probability of first type errors.

Error of the 1st kind is especially significant in the clarifying *(confirmation)* experiment, as well as in cases when the acceptance of the wrong hypothesis about differences is of practical importance.

Error of the 2nd kind (rejection of the correct research hypothesis and acceptance of the null hypothesis) is especially significant during the trial *(exploratory)* experiment. Rejection of the research hypothesis at the initial stage may permanently close the way for researchers in this subject area. Therefore, the level of statistical reliability when conducting an exploratory experiment on small samples tend to decrease. Since the researcher tries to obtain confirmation of his hypotheses, the subjective significance of errors of

the 2nd kind is much lower than the subjective significance of errors of the 1st kind.

But for science as a sphere of human activity it is more important to get the most reliable knowledge, rather than invalid and unreliable results. Therefore, the strategy of research in any field of psychological science should be like this:

transition ***from exploratory*** (search) experiment ***to confirmation*** (specifying), ***from low levels of*** reliability - ***to high***, from research ***on small samples*** - to research on ***large***.

In specific studies, the significance of errors of the 1st and 2nd kind may strongly depend on the goals pursued in the experiment, the subject of the study and the nature of the research task being solved, etc. In everyday and professional life, we often encounter such situations when we need to assess the comparative significance of errors of the 1st and 2nd kind. If the statistical hypothesis is rejected, the researcher can implement it in different ways. He can complete the experiment and make an attempt to propose new hypotheses. The experimenter can conduct a new study on an extended sample using a modified experimental plan, etc.

From the point of view of critical rationalism (K. Popper), the "negative" conclusions rejecting the experimental hypothesis are the main result of any experiment, since the experiment itself is a way of "culling" unviable hypotheses. Rejection of an experimental hypothesis does not mean that the theory which resulted in it should be immediately discarded. Perhaps the theoretical hypothesis is incorrectly formulated. It is not excluded that the theoretical hypothesis is correct, but its experimental version has been incorrectly formulated. At the same time, often even the confirmation of the experimental hypothesis does not testify to the confirmation of the theory.

In contrast to classical natural science, the experimental result in psychology should be invariant (invariant) in relation not only to all objects of the given type, to space-time (and some other) conditions of carrying out of experiment, but also to features of interaction of the experimenter and the subject, and also to the content of activity of the subject.

1. Generalization in relation to objects. If we carried out an experiment on 30 subjects - men aged 20 to 25 years, belonging to middle-class families studying

in 2-3 years of university, then obviously we need to solve the following problem: which population should we cover? The ultimate generalization will be to refer the conclusions to all members of the human race. Usually, researchers finish the first experimental part of their work with an extremely broad generalization. Further research practice is reduced not only to clarification, but also to narrowing the range of applicability of the found regularities.

B.Skinner's researches on operative training on rats, pigeons, etc. have given the results which the author has extended to representatives of other species occupying the top steps of an evolutionary ladder, including even to the person. I.P.Pavlov's experiments on development of classical conditional reflexes in dogs allowed to reveal regularities of higher nervous activity common for all higher animals. The phenomena of J.Piaget are reproduced in the study of groups of children in France, USA, Russia, Israel, etc.

The limitations of generalization are the extra-psychological characteristics of the population: 1) biological; 2) socio-cultural. The main biological characteristics are gender, age, race, constitutional features, physical health. Differential psychological research reveals changes in the relationship between two variables, which relate to additional features of the object of study.

Sociocultural specificities are the second most important limitation to summarizing results. The problem of the possibility of extending data to representatives of other peoples and cultures in cross-cultural research is solved. Similar work is carried out to clarify the influence of such additional variables as the level of education and income of the subjects, class affiliation, etc. on the results of the experiment. It happens that the results of the experiment can be applied only to the population whose representatives were included in the experimental groups. But in this case there is a problem: can the data obtained from the experimental sample be extended to the entire population? The solution to this problem depends on whether the requirement of representativeness was observed in the course of planning the study and forming the experimental sample.

To verify the conclusions, first, additional experiments are conducted on groups of representatives of the same population not included in the initial sample. Secondly, they try to maximize the number of experimental and control groups in the clarifying experiments.

2. Conditions of research. In psychological research, it is not space-time factors (as opposed to physical factors) that are important, but rather the conditions of

the subject's activity, let alone the specifics of the tasks. To what extent do the result of a variation in the subject's instruction, task material, actions, type of motivation, presence or absence of "feedback" influence the subject? All of these questions cannot be answered by simply conducting one experiment. The researcher should vary in subsequent experimental series additional variables related to the characteristics of the experimental task to determine whether the results are invariant with respect to the subject's task.

Psychophysical studies of absolute thresholds of sensitivity became a classic example of the influence of features of the problem solved by the subject on the result of the experiment. "Blind Experience" allows to exclude the influence on the result of the subject's knowledge about when and what influence he or she gets.

3. The experimenter. The problem of the experimenter's influence on the research results was given sufficient attention in the manual. It should only be recalled that psychology, in contrast to other scientific disciplines, cannot completely exclude the influence of personal traits, motivation, competence of the researcher in the course of the experiment. "Double blind experience" makes it possible to control the influence of the experimenter's expectations on research results. However, full control of the impact of individual features of the experimenter involves the application of the factor plan of the type K x L x M, where the additional variable are experimenters who differ by gender, nationality, age, individual psychological characteristics, etc.

The invariance of the results in relation to the personality of the experimenter is especially often violated in socio-psychological and differential-psychological studies. Variation of research results determined by the influence of the experimenter is described in most practical manuals on conducting psychological experiment.

Questions for discussion

1. Psychological content of reduction of the leads.
2. Different solutions for statistical output.
3. Errors in conclusions 1 and 2.
4. A general strategy for psychological research.
5. Experiment as a way of "culling" unviable hypotheses.
6. Invariance of the result as a criterion of psychological experiment.

KEY FINDINGS

A researcher can make two mistakes regarding the hypothesis: 1) accept an incorrect experimental hypothesis and 2) reject the correct experimental hypothesis. In an exploratory (search) experiment, the second kind of error is more dangerous. In the confirmation (specifying) experiment the error of the first kind is more important. The increase in sample size and statistical reliability of the conclusion helps to minimize the error of the first kind. Researchers are in danger of illegal generalization of research results. Limiters to generalization (generalization) of results are:

1. sample characteristics;

2. the content of the experiment (tasks for the test person, effects, environment);

3. the identity of the experimenter.

Two strategies for additional research are possible:

1. limitation of generalization (generalization) by introducing additional variables into the experiment plan;

2. an inductive pathway based on rechecking results in other randomized experimental samples.

The researcher should keep in mind that ***no experiment can ever*** provide absolutely reliable knowledge.

Also, ***experiment*** is the ***best way to criticize*** and select ideas, but ***experiment is not a way to generate new knowledge***.

CONCLUSION

The essence of the psychological experiment is still ambiguous and has not been fully explored. The understanding of the experiment by modern natural-science psychologists is a concentrated result of its transformation during the centuries-long history of the methodology of natural science. And even the fact that the experiment in natural science psychology differs significantly from the experiment in natural science by the presence of instructions does not change the internal logic of natural science experimentation.

A separate problem is the nature of the psyche, which is available to the natural science experiment. At different times thinkers offered different solutions to it. Unfortunately, modern experimental psychology proceeds from the most primitive representation of its nature, which is expressed in the behavioral (non-gevioristic) basis of the natural-scientific experiment in psychology.

In fact, the whole problem of natural-science experimental psychology is reduced to an adequate choice of variables and an adequate method of mathematical processing. It is very doubtful that such a scheme of research can suit a researcher who is interested in truly human properties and qualities.

At the same time, the long history of natural science experiment in psychology shows that it is capable of solving a certain class of research problems and obtaining results that are successfully applied in practice. The whole question is to what extent these results reveal the deep essence of the human being and his psyche, and whether it is possible to investigate the human being and his human nature in ***a situation where the presence/absence of*** something is ***discovered.***

GLOSSARY

Arte factum (lat. *arte factum* - made artificially) - the result of research, which is the result of changing the dependent variable under the influence of side variables. An artifact is a consequence of errors or insufficient control of research conditions. One and the same phenomenon may be an artifact within one experimental scheme and a fact within another, so the phenomena not explained by the accepted theory are often treated as artifacts.

Balancing is a method of control of action of external (additional) variables at which each group of subjects is presented with different combinations of independent and additional variables.

Perfect experiment - includes the following features: 1) an experiment in which all sources of systematic mixtures are eliminated - an ideal experiment; 2) an experiment in which an infinite number of samples are applied to an infinite number of subjects, allowing an infinite number of side variables to be taken into account; 3) an experiment of complete conformity, fully copying reality [R. Gottsdanker, 1982].

Validity - compliance of a particular study with accepted standards (perfect experiment).

External validity - the correspondence of a particular study to natural reality and/or other similar studies. It determines the possibility of transferring and/or generalizing the results to other objects and conditions of the research. It depends on sample representativeness and correspondence of additional variables controlled in the study, their variability in other conditions. A private form of external validity is environmental validity, which determines the ability to extend the conclusions of a particular study to real conditions, rather than to other laboratory conditions.

Internal validity - the correspondence of a particular study to the ideal; evaluates the change in the dependent variable, determined by the influence of an independent variable, not other reasons. Internal validity depends on systematic changes in the influence of the independent variable and other variables on the non-equivalence and changes in the groups being compared during the experiment.

Structural Validity - characterizes the accuracy of the theoretical hypothesis implementation in the experimental hypothesis and, accordingly, in the procedure of the experiment. It is one of the manifestations of internal validity. It defines the area of phenomena investigated in the experiment. In psychological diagnostics constructional validity characterizes the degree of presence of the measured property in test results.

Criteria Validity - reflects the conformity of the diagnosis and prognosis obtained from the data of testing to the activity and life indicators; includes current and prognostic validity.

Operational validity is the correspondence of the experimenter's operations to the theoretical description of the variables controlled in the study. The conditions varied by the experimenter should correspond to the independent variable. Operational validity is one of the manifestations of internal validity.

Validity is **substantial (obvious)** - compliance of the research objectives and procedure with ordinary ideas of the subject about the nature of the phenomenon under study. It has motivational value for the subjects and is one of the components of external validity in some studies.

Ecological validity is a type of external validity, characterizes the compliance of the procedure and conditions of laboratory research of "natural" reality.

Verification is a practical confirmation of the experimental hypothesis, the term is suggested by O. Cont.

Variable interaction - change of a dependent variable under the influence of several independent variables in a factor experiment. There are 3 types of interaction: zero, divergent and overlapping. They are characterized by the difference in values of a dependent variable at different combinations of levels of independent variables.

Reproducibility of the experiment is an opportunity to repeat the experiment by another experimenter based on the author's description of the method.

Sampling - a set of subjects selected to participate in a direct study using a specific procedure (more often randomization) from the general population. Sample size - the number of subjects included in the sample population. The sample is divided into experimental and control groups.

Halo *effect* (Greek *halos* - circle, disk; *halo effect,* from English *halo* - *halo,* shine and lat. *effectus* - action, result) - the tendency of the researcher to

exaggerate the value of one of the parameters of the situation and extend its assessment to other parameters.

General set - a set of all conceivable objects equivalent to the finite set of properties.

Hypothesis - an assumption of the existence of a phenomenon whose truth or falsehood is not deductively possible, but can only be tested experimentally. An experimental hypothesis is an interpretation of a theoretical hypothesis in terms of dependent, independent and additional variables. Counter-hypothesis - a hypothesis alternative to the main hypothesis.

Histogram is a bar graph showing distribution of random values (dependent variable) relative to the levels of independent variable.

Gutmana Scale - a scaling technique in which tasks in the scale are arranged in ascending order of difficulty. It is assumed that a subject who does not perform the *i-th* task will never perform *i+1* task, i.e. the Rush model is applicable for the scale. Each task on the Guttmann scale has an important correlation with the general indicator and is discriminatory. The disadvantage of the Guttman scale is a narrow range of grades.

Double blind experience is an experiment conducted by an assistant experimenter who does not know the true purpose of the study. The subject also does not know the true purpose of the experiment. It is conducted to control the effects of Hottorn and Pygmalion.

Job discrimination is a property of the test job to distinguish the test subjects from the "maximum" and "minimum" values of the scale. To evaluate it, a discrimination coefficient is used, which characterizes the criterion validity of the task in relation to the total test result.

Dispersion analysis is a statistical method for assessing the influence of independent variables and their combination on a dependent variable. It is used for processing data of factor experiments. It is based on decomposition of the total dispersion and comparison of its individual components using the Fisher criterion.

A natural experiment is an experiment carried out under the conditions of ordinary life of subjects. The concept is proposed by A.F.Lazursky. It is characterized by high level of ecological validity and low level of internal validity.

An experimental problem is a task which is given to the subject in the instruction.

Idiographic approach (Greek: *ιδιωσιζ* - special distinction, feature, γραφη - record) - an approach that focuses the researcher on the study of unique, single objects, unique events and processes. It is opposed to the nototheticаl approach.

Measurement - a procedure for establishing a mutually consistent relationship between a set of objects (phenomena) and a set of signs (numbers). In a narrower sense - a type of empirical research in which a researcher identifies qualitative and quantitative characteristics of an object (set of objects) with the help of external means (devices, tests, etc.).

Instruction - description of the task which the experimenter presents to the subject before the experiment. It includes (if necessary) an explanation of the essence of the research, the goal and actions of the subject during the performance of tasks, conditions of the task, principles of the result evaluation, examples of solving tasks, etc.

Introspection (lat. *introspecto* - look inside) - a method of psychological research in academic psychology (end of the X1X - beginning of the XX century), self-monitoring of the subject for his mental state, allowing him to directly comprehend mental reality (Titchener D.). Wundt considered the introspection as an arbitrary observation of the subject's own psychic reality in the course of a psychological experiment and contrasted it with "inner perception" in natural conditions.

Ipsative assessment (from lat. *ipse* - itself) - The term "***ipsative***" means that a particular person serves as a measure or benchmark for himself. I.e. assessment with respect to oneself, not with respect to any established average statistical norms. For example, an ipsative personality test might find that a particular person's need for achievement is higher than his or her need for affiliation. But a hippsative test cannot show whether the assessed needs are higher or lower than the available cultural norms.

Quasi-fact is the result of interpretation of single, unique observations obtained by the method of "investigation of a single case".

Quasi-experiment is a research plan in which the experimenter refuses full control over the variables due to its impossibility for objective reasons. Any real experiment can be considered quasi-experimental as it deviates from the "ideal" experiment.

Context analysis is a method of systematized quantitative evaluation of text content. It was first applied by K. Jung in the analysis of results of associative experiment. It is widely used in psychological diagnostics (projective techniques), special psychology, psycholinguistics, etc.

Counterbalance (or position equalization) - a way to control the effect of order by alternating effects. Each group of subjects receives the same set of effects, only their sequence in the series changes. Each set of actions is given the same number of times. A distinction is made between reverse (reverse) equations, full equation, Latin square, full balanced square.

Variable control - the entire set of strategies for organizing, planning and conducting an experiment, used to maximize its internal and external validity. In intergroup plans, each combination is presented to different groups of subjects. Cross-cluster plans stipulate that each subject or group of subjects is presented with all levels of variables in their combinations, but in a certain sequence (with an equal number of each combination).

Correlation research (or passive observation) - is not aimed at establishing causal relations between variables, but at identifying statistical significance between two or more variables. It is used when manipulation of variables is impossible. It cannot prove cause-effect relations, but it can prove their absence.

Cross cultural research - research aimed at identifying cultural determinants of group-wide features and individual behavioural differences. Cross cultural research uses inter-group comparison schemes for natural or selected groups.

The Likerta scale is one of the design options for installation questionnaires. Proposed by R. Likert in 1932. The statements are selected on the basis of correlation with the overall result. Control group methods are validated.

The Latin square is a square matrix *n* x n, each of its first rows and columns are rearrangements of elements from a set of actions (their number is *n*). It is used at planning of interlevel experiments where it is necessary to estimate influence of an order of presentation of levels of an independent variable. The effects of homogeneous and heterogeneous transposition are controlled. Effects of series and centering are preserved.

Method (Greek μετοδοζ - path) - a general method (principle) of scientific cognition of an object or practical activity, realizing the cognitive position of the research subject to the object of study

Methodology - the system and sequence of actions of the research, based on the method; means (tools, instruments, environment), allowing to solve the research task. With the help of the method the behavior characteristics are fixed and affect the object. As a rule, there are many methods (methodical redundancy) to register similar sides of an object, which provides mutual verification of data obtained by different methods.

Variability measures - statistical indicators of the spread of the variable values relative to the central trend measure. Main measures of variability: linear mean deviation, dispersion, standard deviation, coefficients of variation and excitation.

Measures of the central trend - statistical indicators characterizing the most pronounced, representative value of a variable in the sample. Basic: arithmetic mean, geometric mean, harmonic mean, median, fashion.

Metaphor - transfer of word meaning from one class of the object to another class based on the analogy. Metaphor is a method of semantic modeling of objects in research by transferring properties of a known object to an unknown one.

Surveillance is a method of passive and direct reality investigation. The purpose of observation is to determine the existence and external features of phenomena for their subsequent typologization, classification, etc.

Reliability is a property of a method (technique) to reproduce research results under the same conditions.

The Nomothetical Approach (Greek: νομοζ - law, τεθμοζ - establishment) is an approach that orients a researcher towards finding common laws that describe the existence and development of objects The advocates of the Nomothetical Approach consider it to be the only scientific approach. They see the behaviour of specific objects as a manifestation of general laws. The Nomothetical approach is contrasted with the ideographic approach.

Normal distribution - distribution of probability density $p(x)$ values of a dependent variable in relation to an independent variable under the influence of many noninteracting factors. The curve of the normal distribution equation is a symmetric, one-modal curve, symmetrical with respect to the ordinate carried out through a point of average statistical value. It is widely used in psychometry.

The null hypothesis is a part of a double statistical hypothesis consisting of an alternative hypothesis (H1 is the hypothesis of difference) and a null hypothesis (H0 is the hypothesis of no difference). It states that: 1) the independent variable

does not affect the dependent variable; 2) there are no differences between the results of compared groups; 3) the connection between the parameters is statistically insignificant.

Generalization (generalization) - a way of formulating new knowledge in the form of laws, regularities and features. Generalization is achieved by highlighting the most important properties of objects, phenomena and abstraction (distraction) from insignificant properties. By means of generalization a potentially infinite set of data of single observations is replaced by a finite set of scientific facts.

Objectivity (independence from individual consciousness) - a characteristic of knowledge, providing its accessibility for checking by scientific method, is achieved by developing a coordinated approach of different experts on the object and method of research.

Homonymes are words that sound the same, but differ in meaning. Terms that have different meanings in different theories, as well as scientific terms and words in everyday language are often homonyms.

Selection - a method of creating experimental and control groups, providing external validity of the experiment. Selection is combined with the distribution of subjects in groups that provide internal validity.

Screening - the prescreening of subjects on the basis of specified features, for example: the level of intellectual development, mental or somatic health, etc.

Measurement error is a statistical indicator that characterizes the falseness of the dependent variable. Scattering measures are used as estimates of the measurement error, in particular - the average error.

Paradigm (Greek, παραδειγμα - sample) - scientific standard, generally accepted at a certain stage of science development approach to the study of reality, includes the objectives of science, methods and techniques, the system of criteria for assessing the results of research, basic knowledge (methods, theories and facts). The evolution of scientific knowledge is reduced to the formation, development and revolutionary change of paradigms (Kun T.).

Variables are a parameter of reality that may vary and/or change in an experimental study. A distinction is made between: independent variables - variable by the experimenter; dependent variables - variable under the influence of changes of independent variables; external (side) variables - inaccessible to the control, but affecting the dependent, the source of error; latent variables -

inaccessible directly to the measurement, are fixed by analysis of joint variation of dependent variables; additional variables - external variables considered in the experiment, etc.

Pilot study - a study preceding a new series in which the quality of the methodology and plan is checked. Side variables are identified and the experimental hypothesis is refined. Usually it is carried out according to a simplified scheme, on a minimum sample and a low level of reliability of acceptance of *H0.*

Research plan - a project *of research* operations with specially selected groups. It includes determination of group composition, selection of variables, alternation of impacts, measurement scales of independent and dependent variables, etc.

placebo effect - reaction of a subject to "empty" (zero) influences, corresponding to the reaction in the presence of real influence. It was discovered by Feldman in 1956: patients were relieved by believing in a therapeutic agent rather than by using it; it occurs when beta-endorphins - natural analgesics and antidepressants - are released. In experimental psychology, placebo is a "blank" exposure, no exposure of which the test subject has not been warned.

Position equalisation - see counterbalance.

Behavior indicators (parameters) - quantitative characteristics of the subject's behavior, manifestation of a dependent variable.

Field research - research in natural conditions, maximizing external and environmental validity. The term is used in social psychology and sociology.

Population - a natural set of individuals with a certain set of properties, potential participants in the study; general population.

Sequence - order of experimental influences; alternation of levels of independent variable in experiments on individual schemes. There are three schemes: 1) random sequence of actions; 2) regular alternation; 3) position equalization (counterbalancing).

Sequences of effect - systematic influence on the result of the experiment of side variables associated with the order of presentation of experimental effects to the test subject. Not available in intergroup comparison experiments. It is of crucial importance in individual experiments.

Experimenter's bias is the researcher's attitude towards the subject, which affects the interpretation of the latter's behavior during the experiment.

Projective **techniques** (latte - throwing forward) - psychological techniques of personality diagnostics with the help of analysis of its actions and statements about weakly structured material.

Psychodiagnostics (buckwheat ψσχη' - *the* soul and διαγνοστικοζ - able to recognize) - 1) the field of psychology, studying and developing methods for determining individual psychological differences; 2) the field of practical psychology, engaged in the assessment of psychological differences between people

Psychometry (from Buckwheat ψσχη' - soul and μετρον - measure) is a field of mathematical psychology that develops mathematical bases of psychological measurements.

Individual differences are the main source of internal validity disorder in general psychological group experiments. In the plans of intergroup comparison it is necessary to equalize the groups according to the controlled features or to consider these features as additional variables in factor planning.

Randomization - a strategy of random selection or distribution of subjects, in which all subjects have equal chances to enter the group. It is used for selection of members of the population into an experimental sample, as well as for distribution of subjects into experimental and control groups. It provides internal validity, controls the mixing effect.

Distribution - a strategy of creating experimental groups from selected test subjects (either volunteers or a real group). It is used to increase internal validity of the study. There are several reflections: paired distribution, randomization, pre-stratified randomization, and so on.

Rush's model is a stochastic model of the test, proposed by G.Rush in 1960, based on the idea that the probability of answering the test problem is a function (additive or multiplicative) of the "force of the task" (difficulty) and the intensity of the property (ability). G.Rush's measuring scales are the scales of relations.

Reductionism (lat. *reductio* - reduction, reduction) - explanation of complex processes through features of simpler processes, the lower level of organization. However, recently there has also been a "reduction upwards" - the reduction of simpler processes to processes of a higher level (for example, mental to cultural).

Sample representativeness - the correspondence of properties of the investigated **sample to** properties of the general population. It is achieved by random selection of an object from the population (randomization procedure), selection of pairs, whose members are equivalent and belong to different groups, or a combination of these methods.

Semanticos **differential** (Greek *semanticos* - signifying and Latin *differentia* - difference) is a method of quantitative and qualitative analysis of meanings and group consciousness. It is used in psycholinguistics, psychosemantics, psychodiagnostics. The method of semantic differentiation was proposed by Ch. Osgood in 1957 to measure individual differences in interpretation of notions.

Synonyms are words that differ in sound but are similar or equivalent in meaning. In more advanced sciences, synonyms are rare. In a number of branches and directions of modern psychology synonyms are used very often.

Mixing semantic (procedural) - a source of violation of internal validity. It is caused by the fact that the action of an independent variable is accompanied by the influence of related facts, which change depending on the level of the variable and which cannot be simultaneously controlled. These include time factors, presentation sequences, individual differences, etc.

Accompanying mixing is a source of internal validity disorder caused by the inevitable combination of the main and accompanying effects in the experiment. One of the manifestations of concomitant mixing is a placebo effect.

Group building strategies - methods of sampling and distribution of subjects to groups in intergroup comparison experiments for averaging individual differences (external validity) and population representations (internal validity) Distinguishes between randomization, stratimetric randomization (for sampling and distribution) and selection of equivalent pairs (only for group distribution).

The scheme is experimental (otherwise - experimental plan) - the order of presenting different levels of independent variable(s) to groups of subjects or individual subjects (subjects). There are individual plans when all levels and combinations of independent variables are presented to one test subject.

Sentism is a worldview based on an overestimation and absolutization of the current level of development of the scientific method and scientific knowledge, as well as the opportunities provided by modern science to solve practical problems.

The Thurstone scale is a type of installation questionnaire. Proposed by L. Thurstone and E. Cheyvoy in 1929. The statements included in the scale are selected on the basis of expert evaluation on an 11-point scale. The statements included in the scale are selected on the basis of expert evaluation on an 11-point scale. The test subject's attitude indicators are the median scale grade given on an 11-point scale.

Test - a scientific and practical method of psychological measurement, - consisting of a final series of brief tasks, aimed at diagnosing the individual severity of properties and states. The term was proposed by J.Kettell in 1890.

Factum (lat *factum* - made, accomplished) is a well-proven empirical knowledge fixed in the form of a scientific statement. In the narrow sense - knowledge about the existence of an object, phenomenon, process, identified by scientific method; the result of theoretical interpretation of empirical data.

Factor - parameter of external conditions or features of the object that affects the change of the dependent variable. It is used to describe the factor experiments. It distinguishes between time factors, task factors and factors of individual differences [P. Gottsdanker].

Factor analysis - a set of mathematical methods to reveal hidden features, as well as their relationships based on the analysis of statistical relationship matrices (correlations, "distances") between measured features. The main task of factor analysis is to reduce the set of test measurements to a small number of basic ones (reduction of the number of variables) with determination of the measure of determination of primary variables by basic ones.

Phallbilism is a methodological principle according to which theories are not only erroneous, but always erroneous. Error is a property of any theory. The task of the researcher-theorist or experimenter is to detect the theory of error.

Falsifiability is the property of any scientific theory to be rebuttable. According to K.Popper, any scientific statement, as opposed to a non-scientific one, must be refuted (falsified). A scientific statement must not only be proved, but a set of statements must be defined that are incompatible with it. A theory is refuted when a reproducible effect contrary to the conclusions of theory is found.

Experiment - a study planned and controlled by the subject, in which the experimenter (subject) influences an isolated object (objects) and registers changes in its state. It is performed to test a hypothesis about the causal relationship between the impact (independent variable) and changes in the state

of the object (dependent variable). In psychology, an experiment is a joint activity of a subject and an experimenter to study the subject's mental features by observing his or her behavior during experimental tasks.

A critical experiment is an experiment aimed at testing hypotheses resulting from two alternative theories. The result of a critical experiment is to refute one theory and accept the other.

Laboratory experiment - an experiment that is carried out under special conditions designed by the researcher, with the release of an independent variable, and taking into account or elimination of the influence of side variables. Most often a psychological laboratory experiment is conducted in specially equipped rooms, with the help of equipment and computer equipment (controlled laboratory experiment).

The primary effect, or "**first impression effect**", is the effect of the first impression of the subject's personality on the experimenter's interpretation and evaluation of his or her further behavior and personality traits. It was studied in detail by S. Ash (1940).

The transfer effect is an advantageous influence of one of the levels of an independent variable in their consecutive alternation. A distinction is made between homogeneous and heterogeneous, symmetrical and asymmetrical carryovers. Homogeneous and symmetrical transfers are eliminated at regular alternation and positional equalisation (individual experiment), and also at reversive equalisation (crossindividual experiment). Non-symmetric transfer is averaged by application of a random sequence.

Sequence effect - (see Sequence effect) - the effect of the sequence of presentation of effects in intra- and crossindividual experiments.

The Pygmalion effect is a modification of the subject's behaviour in an experiment under the influence of unconscious influences of an experimenter seeking to confirm his hypothesis or opinion about the subject's personality. It is controlled by a double-blind experience. It is a consequence of a violation of internal validity.

The effect of series is the effect of asymmetric transfer in a multi-level experiment, when the genus of effects has several levels. It depends on the distance of the level of exposure to the subject from the ends of the series. Explains from the subject's adaptation to a previous exposure at a lower or higher level than that required.

Hottorn Effect - the effect of the test subjects' attitude to the study on their behavior and productivity was discovered in 1924 at the Hottorn Plant of Western Electric in a Chicago suburb. It's also the Mayo effect.

The centering effect is a partial manifestation of the effect of a series that enhances the action of an independent variable. It is explained by the fact that the levels shown in the middle of the sequence are preceded by lower and higher levels (in case of their random or position-levels alternation).

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Training Manual

(for graduate students)

Valentin Vasilyevich Ageyev

INTRO TO EXPERIMENTAL PSYCHOLOGY