DOCUMENT RESUME

ABSTRACT
        Neglected activities in the research-to-application
process and various characteristics of basic studies in psychology
are the causes of failure in translating research into operational
applications. A model of activities involved in the research
application process includes two areas of strength: (1) basic
research on a constructed theory and (2) the development of
technology and its practical application. Collation and
interpretation of research--a demanding task--and the function of
translating, or facilitating communication between research
communities and the world of practice, are intermediating activities
that need to be emphasized and recognized. The second cause of
failure was conceptualized during an investigation of learning
research: characteristics of experimental procedure found to
determine a study's application potential became more applicable as
experimenter control declined. For instance, an artifically
controlled environment makes a study's application less practicable
than a natural one. A similar continuum operates on the objective,
task, stimulus, response, motivation, and time elements of basic
research. One way to make basic research more applicable would be to
define an investigatable problem through an analysis of an
operational problem rather than through a review of prior basic
research or through theory alone. (JH)

CHUCKHOLES IN THE BUMPY ROAD
FROM RESEARCH TO APPLICATION*

by

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Several years ago, we were approached by representatives
of the Psychological Sciences Division of the Office of Naval
Research with a somewhat flattering invitation to try to
develop practical innovations from a sample of research studies
the Navy had previously sponsored.  We were issued the invita-
tion, I believe, because several of our own research studies
had led to new procedures and new equipment designs adopted
by the Navy.  We had not thought very much about *why* some of
our research had changed the way the Navy does things and were
inclined to feel that we didn't really do research any differ-
ently from anyone else.  Our immediate assumption was that
most research could find application if a modest amount of
effort were spent on that objective.  Operating under this
assumption, we undertook a study for ONR whose objectives were
to describe the research-to-application process and to develop
potential applications from a representative group of psycho-
logical studies.

The first step was to decide which area of psychological
research should be the guinea pig.  Because a very large amount

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 New Orleans, August 1974.

of basic research was being performed on human learning at
that time, and because of the tremendous social significance
of any research-related improvements in training technology,
we elected to focus our investigation on the development of
applications from studies of learning.

The results of this effort were both disturbing and frus-
trating (Mackie and Christensen, 1967). After nearly two
years of analysis and inquiry, our general findings were that:
(1) the great majority of learning research studies (not just
those sponsored by ONR) were having virtually no impact on
instructional practice in the Navy or elsewhere; and (2) our
own efforts to develop applications from the findings of
ONR-sponsored research on learning were almost totally unsuc-
cessful. The reasons seem to center, in the first instance,
on some seriously neglected activities in the research-to-
application process and, in the second, on a number of char-
acteristics of the research studies themselves. I should like
to go into these problems in some detail.

Breakdowns in the Research-to-Application Process

Let us first examine a model of the research-to-application
process as we assumed it to exist and as we found it in prac-
tice. This is shown schematically in Figure 1 which has been
coded so as to reflect those elements of the process we found
to be active and effectively linked and those which were not.
The elements of the process enclosed in heavy dark lines, which
indicate very active subsystems, show two highly developed
but essentially isolated sets of activities. On the one hand,

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Legend: ———— existent, highly active
        ——— existent, somewhat active
        - - - - virtually non-existent

APPLICATION
• OPERATIONS
• SERVICES
• SYSTEMS

APPLIED RESEARCH

DEVELOPMENT
• TECHNOLOGY
• HARDWARE
• SOFTWARE

TRANSLATION
(GENERAL→SPECIFIC)

COLLATION AND INTERPRETATION
(SPECIFIC→GENERAL)

THEORY CONSTRUCTION (GENERAL)
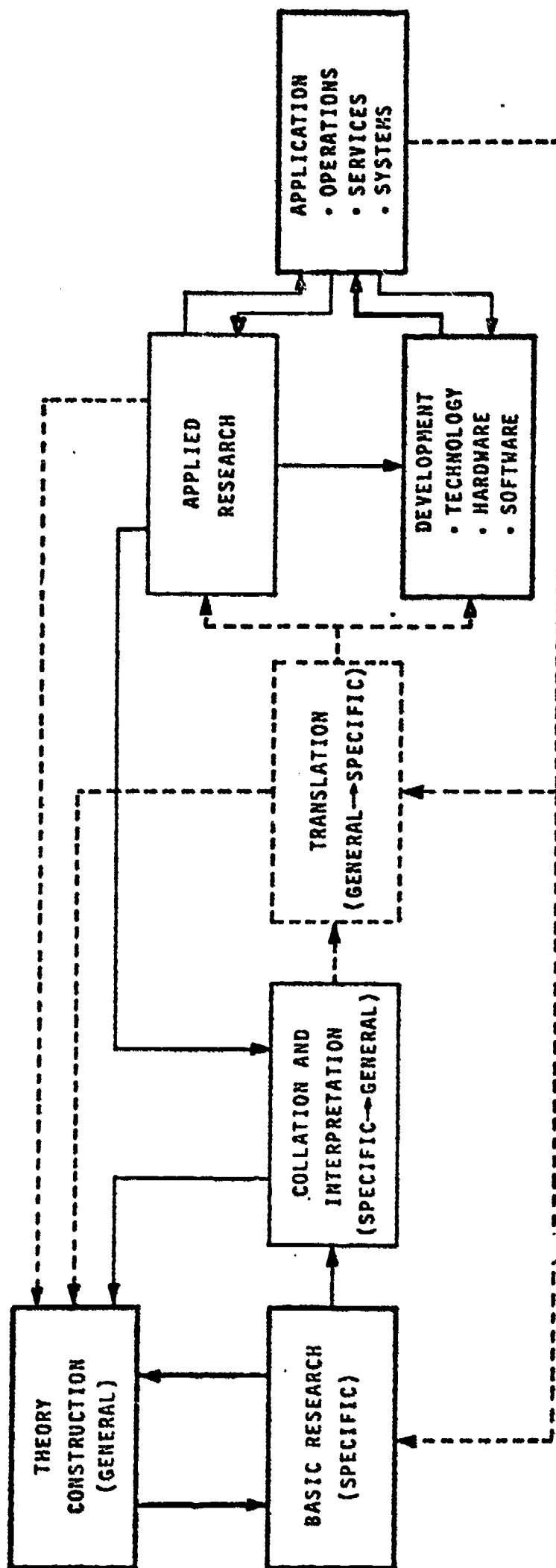
BASIC RESEARCH (SPECIFIC)

Figure 1.  A model of the process of research to application (in theory and in practice).

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there is the very active enterprise of basic research and theory construction which takes place mainly in the university laboratories throughout the country. This is typically a closed-loop system with built-in reward systems that rarely demand interactions with the world of practical operations.

At the other extreme is a highly isolated subsystem consisting of applied research, development, and the world of everyday problem solving. Within this subsystem we found, insofar as instructional technology was co. erned, that learning research was taking a backseat to \'\' promotion of commercial enterprises such as audiovisual teaching aids, programmed textbooks, and computer-aided instruction. In fact, more of what was going on in the classrooms seemed to reflect the thinking of salesmen for a new educational technology than that of learning researchers. In the past few years, a number of mission-oriented educational research laboratories have emerged that have changed this situation somewhat. Nevertheless, the isolation reflected in the figure between basic research and actual educational operations largely still exists for reasons I will now suggest.

The figure identifies two seriously neglected and under-developed activities that are responsible for the isolation of basic research from application. I have called these functions (1) Collation and Interpretation, and (2) Translation. Collation and Interpretation have their usual meanings here in the sense of critical, comprehensive reviews, together with insightful attempts at generalization, of research results

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on some area of human behavior. These functions appear to be increasingly difficult for anyone to perform because of the vast quantity of basic research being produced and its ever-narrowing, specialized nature.

The problem is severe and appears to be growing. In his address at the dedication of the APA building in 1965, Donald Hornig, Director of the Office of Science and Technology and Chairman of the President's Science Advisory Committee, said that the notion that basic research today produces still undefined benefits tomorrow is generally accepted as far as the natural sciences and engineering are concerned. But he emphasized that this trend is not nearly so evident in psychology (Hornig, 1965):

> ...Despite the fact that the behavioral sciences affect every aspect of our lives and interact with every function of the government, I believe we would all agree that they have not been applied to government problems in the same systematic way as the natural sciences nor has a clear picture emerged as to how their development should be fostered. It is my view that we must move to remedy both these problems. The question is how....

> ...As the scientific and technical enterprise has grown in scale, increasing concern has developed as to whether the fruits of all this activity, the information derived, is being made promptly available to those who might make use of it....To many the output of science is a collection of facts which can be stored and made available when needed. It is assumed that it will be put to use. But anyone who has ever worked in the sciences is aware that in fact recorded information is often singularly useless,

> that much depends on how it is presented,
> whether written or oral, with what atti-
> tude the potential user approaches the
> information, the degree to which he is
> stimulated to *use* that information. And
> information which is not used in some
> way helps no one....
>
> I confess that I was surprised to learn
> that you are one of the largest single
> publishers of technical journals in the
> world. Naturally, when one is told that
> your society publishes more than a mil-
> lion pieces of scientific literature a
> year one wonders what happens to all of
> the ideas and facts they contain.

Interpretations of research findings also appear to be
increasingly difficult because interpretative statements by
the investigator are actively discouraged by many journal
editors on the basis of scientific conservatism and shortage
of journal space. And, it is a rare individual who is willing
to risk interpretation of someone else's research when the
original investigator's interpretation is unavailable.

It is also unfortunately true that a great deal more
effort seems to be required to produce a creditable review
article than to publish a very narrow research study in a
specialized area where few critics will be as knowledgeable
of the subject matter as the author himself. At least one
well-known investigator remarked to us that an invitation to
perform a comprehensive review used to be considered a pro-
fessional honor; nowadays the reviewing function is largely
considered a task for one's graduate assistants.

Whatever the reasons, the neglect of interpretative
reporting results in a breakdown in the process of developing

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the generalizations that appear to be essential if this model of the research-to-application process is valid. It is a necessary step in proceeding from the myriad stimulus, response, and process variables in great numbers of otherwise obscurely related studies to some kind f generalized formulation that in turn can be translated iఓ.. meaningful operations in a working environment.

This brings us to the next stage in the process, one that we found to be neglected almost to the point of nonexistence. We have elected to call this process Translation. The functions that are associated with this stage in the system have been variously referred to as "bridging the gap," "operationalizing," "social engineering," etc. Whatever the label, the essential activity is one of relating the operations, variables, and functional relationships found to be important in the laboratory to corresponding processes and variables in some operational environment.

The Translation function is also essential to communication, both from the research community to the world of practice and the reverse. We found effective communications between these two subsystems to be almost totally lacking in the field of learning because of the absence of a common technical language, the failure to develop adequately trained personnel to act as intermediaries, and the previously-described neglect of the Collation/Interpretation function. Though we observed this problem in the context of research on human learning, similar problems almost certainly exist in other

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areas of psychological research. For example, in his formulation of a proposed research and development model for clinical psychology, Broskowski (1971) has described a role for an individual who "neither does basic research nor dispenses the products of application to the individual consumer." Rather, he is conceptualized as a "man in the middle" who serves as the necessary interface between the basic sciences and applied endeavors by stimulating and mediating interactions among the pure scientists, the product-oriented developer, and the consumer.

It is Broskowski's contention that some Ph.D.-level psychologists must be trained to "systematically conduct and utilize relevant research for the development of procedures and techniques to help solve...various clinical problems in individuals, groups, and institutions." It is his view that the science-professional model on which the training of clinical psychologists traditionally has been based has produced professionals who are either poorly trained scientists or poorly trained clinicians. Instead of emphasizing one or the other end of the process, Broskowski contends that some universities should train specialists to bridge the gap between the poles.

It should be noted, in Figure 1, that the process not only breaks down in going from research to application but also in the reverse process, the feeding back of operational problems from the operational world to the research laboratory. The fact that those who conduct basic research are so thoroughly

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isolated from problems in the operational environment is a
very significant influence, I feel, on their research designs,
which brings us to the second fundamental obstacle in the
road from research to application.

*Problems Associated with Experimental Procedure*

Assuming that the Translation function is a meaningful
one, the development of the research translator, social engi-
neer, clinical middleman, or whatever, seems to be an appealing
concept. We encountered a difficulty in our study, however,
that makes the issue somewhat more complex. The problem was
that many research studies seemed not only to defy application
but translation as well. The results of psychological research
on learning, as well as other behavioral areas, appear to
be distressingly specific to the task conditions, independent
and procedural variables, and dependent measures selected for
use by the investigator. Sensing this specificity, many
potential users, rightly or wrongly, are inclined to disregard
research whose title does not suggest a direct connection with
the particular behavior or operation with which they are con-
cerned. Since few research studies meet this stringent crite-
rion, published research articles have an extremely small
readership even among those practitioners who presumably
should be the most interested (Garvey and Griffith, 1965a, b).
Incidentally, I make no distinction in this regard between
the utilization of the *results* of a research investigation
and its *methodology*, either of which may prove translatable
to an operational setting. In fact, psychologists have

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contributed more to change in operational systems on the basis of the methods by which they study human behavior than they have in the form of directly useful facts about human behavior.

It seemed to us that the research studies we reviewed not only suffered from specificity of results but also differed greatly with respect to the apparent difficulty a translator would have in deriving applications from them. For convenience, let us assume that research studies vary along some dimension that we will call Application Potential (A/P). If various research studies differ in A/P, in what particulars do they differ? In my view, they differ in a number of highly significant ways that I have attempted to summarize in Figure 2.

First of all, they differ with respect to the basic motivation of the experimenter. Studies with low A/P have as their primary objective the *understanding* of some usually very limited aspect of behavior. In contrast, studies with high A/P have as their objective the desire to *change* something, usually to improve something. The two types of goals are not mutually exclusive, of course. However, there is a matter of primary emphasis that has important consequences for experimental design. Perhaps the most significant of these has to do with the selection of the experimental task.

In low A/P studies, the criteria for selection of the experimental task are typically (1) how well it relates to the experimenter's theoretical position and/or (2) how conveniently it can be generated by the type of equipment that happens to be on the laboratory shelf. In sharp contrast, in studies with
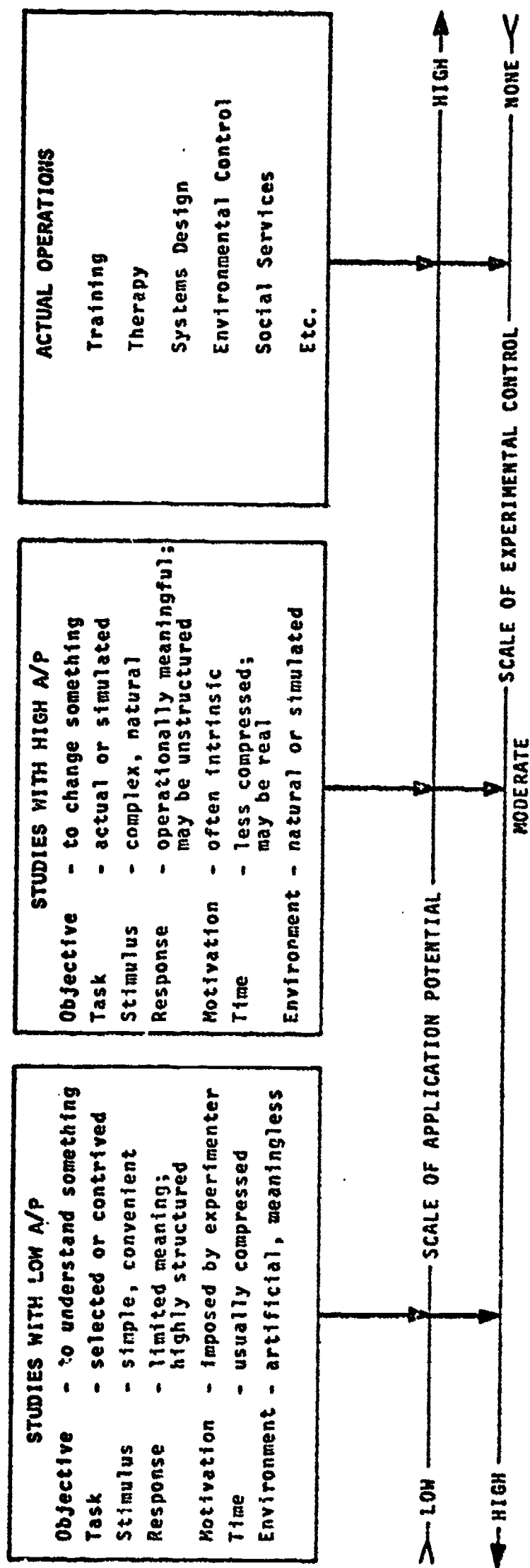
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**ACTUAL OPERATIONS**

Training

Therapy

Systems Design

Environmental Control

Social Services

Etc.

**STUDIES WITH HIGH A/P**

| | |
|---|---|
| Objective | - to change something |
| Task | - actual or simulated |
| Stimulus | - complex, natural |
| Response | - operationally meaningful; may be unstructured |
| Motivation | - often intrinsic |
| Time | - less compressed; may be real |
| Environment | - natural or simulated |

**STUDIES WITH LOW A/P**

| | |
|---|---|
| Objective | - to understand something |
| Task | - selected or contrived |
| Stimulus | - simple, convenient |
| Response | - limited meaning; highly structured |
| Motivation | - imposed by experimenter |
| Time | - usually compressed |
| Environment | - artificial, meaningless |

SCALE OF EXPERIMENTAL CONTROL — HIGH / MODERATE / NONE

SCALE OF APPLICATION POTENTIAL — LOW / MODERATE / HIGH

Figure 2.  Contrasts between research studies with low and high application potential (A/P).

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high A/P, the experimental task is often *imposed* on the experi-
menter. That is, the starting point for research is often
an actual operational task, or part of an operational task,
or in some cases a carefully designed simulation of it.

A related way in which studies with low and high A/P
differ is in terms of their stimulus and response character-
istics. In studies having low A/P, the stimulus tends to be
simple, experimentally convenient, and to reflect the interest
of good experimental control. The subject's response tends
to have meaning only in relation to the experimental task
(i.e., "if stimulus A is presented, press the lever marked X
as quickly as possible," etc.). In contrast, in high A/P
experiments, the stimulus is often *inherent* to the experi-
mental task. By this I mean that the stimulus condition is
not contrived by the experimenter but rather is presented in
some natural context. The experimenter may have little con-
trol of the exact character of the stimulus although he may
program it according to some orderly plan. In fact, in many
high A/P studies, determining the precise nature of the
stimulus may in itself be a significant part of the investi-
gation. For example, in a series of studies that we have con-
ducted into the question of how people learn to perceive and
classify complex underwater sounds, it was necessary to per-
form detailed analyses of the physical properties of the
stimuli themselves before we could even begin to understand
the nature of the subjects' responses.

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The experimental stimuli in a high A/P study may be
synthetic and yet be based upon operationally meaningful
stimulus conditions. For example, in her excellent studies
of how young children learn to read, Gibson (1965) has demon-
strated the value of carefully constructed synthetic stimuli
that reflect specific hypotheses about perceptual processes.
This is in sharp contrast to the stimuli employed in many
learning research studies which take the form of nonsense
syllables or abstract figures that are devoid of meaning out-
side of the experimental task itself.

It is not surprising that the use of such contrived
stimuli lead to a blind alley insofar as research translation
is concerned. There is abundant evidence that the human
response to stimulus conditions is very specific; in fact,
the entire field of human engineering is built upon the premise
that relatively minor modifications in stimulus conditions can
result in rather major differences in human response. Examples
are also available from other fields, such as the experimental
analysis of behavior, which illustrates the requirement for
extensive attention to the nature of the stimulus and the con-
tingencies of reinforcement. And, it surely seems that a key
element of successful psychotherapy must be the identification
of critical stimuli to which new patterns of response must be
developed.

This suggests another way in which studies with high and
low A/P differ. In low A/P studies, the response repertoire
often is very limited and may have no significance outside the

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experimental context. In high A/P studies, the response
alternatives are typically quite meaningful in relation to
some operational criterion. In addition, there may be many
response options and not all of these may be fully specified
in advance by the experimenter.

Studies with low and high A/P can also differ with respect
to the motivation of the participating subjects. In low A/P
studies, the motivation is generally *imposed* by the experi-
menter through some direct appeal to do well, monetary reward,
or the hint of extra credit in Psychology 1-A. In high A/P
studies, the motivation is usually much more operationally
related. Often the experimental objectives are clearly in the
interest of the subject himself, and his reward may be a
natural by-product of successful performance on the task.
This latter condition is most likely to be met if the experi-
mental task is a part of, or clearly relatable to, an opera-
tional one which the subject will eventually be called upon to
perform in the world outside the laboratory. Neither condition
insures high subject motivation, of course, but in the latter
circumstances, the experimenter at least has some assurance
that he has not introduced a kind of artificial motivation that
will fail to transfer to the operational setting.

Still another characteristic on which low and high A/P
studies differ is that of time. In Figure 2, I have described
the low A/P study as "time compressed" because so many research
studies are unfortunately limited to the duration of a class-
room period. For some inquiries this may be adequate, but for

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studies directed at somehow changing behavior or improving operations, it obviously imposes a severe limit on the probable effects of the experimental variables. In contrast, studies with high A/P tend to be much less time compressed and may in fact occur in real time (although there is rarely *enough* time, especially when interest centers in long-term effects that hopefully bring about change).

A final difference between low and high A/P studies is the difference in the experimental environment itself. In the low A/P case, the experimental environment is typically sterile and deliberately shielded from the influence of variables that are not the focus of attention of the investigator. In the typical high A/P case, the experimental environment is often much more like the operational one or a careful simulation of it. In high A/P studies, therefore, many more uncontrolled variables are likely to influence the results and the effects of the independent variable are likely to be evident only if they are highly influential.

To review, research studies which have high probability of application, which appear to be amenable to translation and application by well-trained behavioral engineers, are different in a variety of ways from studies whose probability of application and ease of translation are very low. These differences are often under the experimenter's control, that is, they represent design options. Since we all probably agree that research utilization is a good thing, the question is, "Why don't we design the majority of research studies in such a way

as to increase the probability of application?" It seems
that there are at least two answers. The first is the pre-
viously described distance between basic research personnel
and the world of operational problems. This is a problem that
I feel can and should be corrected through the development of
the Translator. We need a new kind of professional to fulfill
this role, one who is qualified and comfortable in relating
to two different worlds at the same time.

The second problem is more difficult to cope with and it
strikes at the very heart of behavioral research. Studies
having high application potential are generally characterized
by much less experimenter control than studies with low appli-
cation potential. The risk is that studies designed for high
A/P may, because of lack of control, leave many unanswered
questions concerning the roles of various variables on the
behavioral outcome. On the other hand, restrictions on the
nature of the experimental task, the stimulus-response con-
ditions, time, the environment, and the subject's motivation
in the interests of experimental control may lead the investi-
gator to a clear understanding of a laboratory phenomenon that,
because those very controls were exercised, is difficult if
not impossible to relate to operations in the external world.

I certainly have no simple answer to this dilemma. Per-
haps we simply must live with a basic conflict brought about
by fundamentally different research objectives: to *understand*
something versus to *change* something. I personally am more
optimistic than this however. In the interest of increased

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research utilization, I believe it is possible to train investigators to design studies with considerably higher A/P than is frequently the case without sacrificing the kind of experimental control associated with rigorous scientific inquiry. In my view, the relevance of much basic research is difficult to establish simply because the investigator did not start with an operational problem as the point of departure for his investigation. This is not a trivial matter. A very different experiment is designed if the investigator starts with an analysis of an operational problem rather than with a review of prior basic research or theory alone.

There is clear evidence in the literature (e.g., Gibson, op. cit.) that studies directed at understanding basic behavioral phenomena, that constitute a basis for the elaboration of psychological theory, and that inherently have high application potential, *can* be designed if the researcher adopts *as his point of departure* a thorough understanding of an operationally meaningful behavior problem. This does not mean that the results of such studies will invariably be in a form that can be immediately used by operational personnel. Almost always, the additional step of translation by a behavioral or social engineer will also be required. Thus it is essential that we take seriously the need to develop highly competent professionals who can fulfill these middleman functions. Remember that the requirement exists for greater communications in *both* directions and it may well be that the behavioral engineer must be responsible for a kind of backwards translation,

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that is, for describing real-world problems in terms of variables the research community can deal with. But if the research to utilization model has any meaning, if the research translator is to be able to do his job, we must begin to allocate a greater proportion of the research and development effort to studies *designed* to have high application potential. This may imply a reevaluation of the criteria for judging scientific merit in the field of psychology.

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