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While reading Pirsig's (1975) bestseller Zen and the Art of Motorcycle Maintenance my interest was piqued by his law,"the number of rational hypotheses that can explain any given phenomenon is infinite," (p.107). I recognized it as a slightly more general statement of something I know to be true and was sure I could prove. Pirsig then asserts,

> If true, that law is not a minor flaw in scientific reasoning. The law is completely nihilistic. It is a catastrophic logical disproof of the general validity of all scientific method. (p.108).

Though I had good reason to believe this to be false, I decided to follow his reasoning carefully. For if he could prove his case, then I would have to seriously rethink my position on a whole series of related issues.

I have been teaching humanistically oriented graduate students that it isn't Science that is the roadblock keeping them from doing the exciting and progressive activities in their areas. Rather it is the politics and sociology of science that are the real culprit. What was being stolen was a lever which could help these students understand the phenomena they wonder about.

Pirsig's statements raised the possibility that when you pare away the politics and the sociology you are still left with an unusable system. His argument proceeds,

> If it is the purpose of scientific method to select from among a multitude, and if the

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> number of hypotheses grows faster then experimental method can handle, then it is clear that all hypotheses can never be tested. If all hypotheses cannot be tested, then the results of an experiment are inconclusive and the entire scientific method falls short of its goal of establishing proven truth. (p.108).

As a convenience I will chop the statement into the various parts.

Argument 1

Premise A. "If the purpose of scientific method is to select from among a multitude,"

Premise B. "and if the number of hypotheses grows faster than experimental method may handle,"

Conclusion 1 "then it is clear that all hypotheses can never be tested.

Argument 2

Premise C. "If all hypotheses cannot be tested",

Conclusion 2 "....then the results of an experiment are inclusive."

Conclusion 3 "....and the entire scientific method falls short of its goal of establishing proven truth."

It seems to me that selecting from among the multitudes of alternative explanations is one of the legitimate purposes of science. We need ways of making meaningful statements. We need ways to systematically believe more in some explanations than in others. And scientific method has a role in this process. While there surely are other purposes, I still agree with Premise A. Anyone who has done experimental research will most likely agree with the essence of Premise B. Within a single research context, the scientific method doesn't appear to be able to handle the multitude of explanations.

There always seem to be more questions at the end of a research effort than at the beginning. Consequently I agree with the first conclusion. All hypotheses can never be tested. The second conclusion does follow from the first. The results of a single experiment are inconclusive.

To illustrate the inconclusiveness just consider any experiment which has conclusions based on standard univariate statistical evidence (e.g. as Student's t-test or an F ratio). The logic which underlies the tests come from the work of Neyman and Pearson (1933) on uniformily most powerful tests. Without reviving too many bad memories, perhaps you can recall the type I and type II errors of your first statistics course. Type I error (α) is the probability of falsely rejecting a correct null hypothesis. This is the error which is set by the sociology of science at .05 or .01 or whatever you happen to get. This value is ritualistically reported in most all "scientific" papers. But what ever happened to type II error (β)? It is the probability of falsely failing to accept a correct alternative hypothesis. You most likely calculated it in those statistics classes, but haven't seen it since. It is the Neyman Pearson Lemma which allows you to set the level of type I error you will tolerate and establishes the testing procedure which minimized type II error for <u>all possible positions of the alternative hypothesis</u>. This means if your null hypothesis is that difference between two groups on some variable is 0.0 then the standard procedures fix α and minimizes β for each value the alternative may take. Well, the alternatives may take on any one of an infinitive number of values from negative infinity to positive infinity.

In fact there are infinite number of alternative explanations for this experimental result. One infinite subclass of all the explanations for this result has an alternative hypothesis at each of the infinite number of positions the mean difference could assume. We don't calculate type II error in real experimental contexts because there are an infinite number of them and we have no way of selecting from the multitude within a single experiment.

There are other demonstrations of conclusion 2, but suffice to say it is well founded. Conclusion 3 however, is not true and does not follow from Conclusion 2.

It is not the purpose of the scientific method to turn the results of a single experiment into proven truth. Scientific method is designed to aid in transforming phenonmena into evidence which, in turn we use to increase our belief in the statements we make about behavioral phenomena. Scientific method is part of a process of moving toward certainty.[1] The rest of this paper looks briefly at that process.

[1] "Proven truth" is too stringent a criterion for science. It went out with the principle of verification which was the cry of the logical positivists of the Vienna Circle. Instead of verification science uses a less restrictive principle of confirmation. Confirmation is temporal and spatially local, and conforms to the kind of certainty that is reasonable for probabilistic statements about the generalizability of outcomes. The consequences of seeking scientifically unreasonable levels of certainty is most elegantly and powerful spoken to in the Bronowski "Ascent of Man" series episode entitled "Knowledge and Certainty." I highly recommend viewing or reading it.

Flows of the Research Process

There are many different ways to describe the flows of the research process. I learned it under the name of the logic of networks and hierarchies.[2] But there are perfectly analogous explanations in the terms of field theory or in systems terms. I choose to use systems terms to describe the flow merely to show that it doesn't matter. The meaning is unchanged.

The standard research methodology course attempts to represent the research process in terms of an arbitrarily linearized sequence of activities typically starting with "decide on research question," "select your measurements," "decide on significance levels," "select sample." The error is that what is described is someone's model for the process of a researcher rather than the process of research itself. I believe that if people understand the substance of the process of research, they then can choose for themselves efficacious forms which respond to the unique aspects of their respective domains.

Figure 1 symbolizes one flow of the research process. The flow shown in the figure is for a general illustration in pencil and paper psychology. But the translation of this into the analogous components for a clinical research setting or action research is straightforward.

Insert Figure 1 about here

[2]This material as well as the material on the logic of networks and hierarchies as well as much of the basic philosophy which underlies paper were taught to me by Don E. Dulany, in a course 8 years ago and by his writings (Dulany, 1968).

The real crunch of the research process is where the evidence (in this case analytical evidence) from the phenomenological side, meets the deductions from the theory side. The result is an increase or decrease in your belief in the assertions you make about "the way things are." The knowledge gained from any single set of evidence is very partial and only has an incremental or decremental impact on your beliefs. The core of the phenomenological side is to choose the phenomenological domain in a way which allows you to develop the evidence which can be matched with deductions from the theory side. Science, as well as most scientists, protects this choice process. The scientists protect it out of self interest mostly. Since most theories evolve rather than spring fully grown from the mind of a researcher, it is reasonable to have very few constraints on the process of choosing the phenomenological domain. The government and foundations try to influence the choice by the projects they fund. Editorial boards influence it by the topics they will publish. But there is simply too much power inherent in this choice for it to be vested centrally. This does not mean, however, that the choice should be capricious. It is only reasonable that the researcher understand his/her purposes in engaging in research efforts.

So a person sits in a room and phenomenologically reacts to a questionnaire. The manifestations of the reactions are pencil marks on paper. These are then coded into data ready for analysis. A whole theory of data is used as evidence that the transformations which the phenomena have undergone still maintain the meaning they originally possessed. Notice that prior theory is

serving as evidence to increase your belief that the data are proper. Then theories of analyses are used in transforming coded data (e.g. statistical theories or theories behind exploratory methods) into results, and results into analytical evidence.

The flow on the theory side can not be described accurately as a linear process in time. The feedback to both the network of constructs and to high level theory makes all the cycles flow together. An idealization of the flow might begin with high level theory. This lofty stuff is characterized by its simplicity. "People strive toward self acutalization." "Organizations must regenerate their raison d'etre or die." These are statements of high level theory. Each of them has been elaborated into a network of interrelated constructs. Cronbach and Meehl (1955) called it a nomological network to underline the lawfulness of the interrelations. The network is characterized by its complexity. The more connections the better. The laws can connect contructs with other constructs in the network, called internal principles by Hempel (1966 and 1969) or constructs with deductions from the network, called bridge principles. The more elaborate the network, the more deductions that can be made; the more opportunity for gathering analytical evidence for confirmation.

I would like to illustrate this process with some material based on Jungian psychology. An advanced doctoral student is working on the extension of major parts of Jungian theory to group learning/communication situations (Taylor, 1976). The example has evolved from our discussion of his research, and represents only a small part of his dissertation effort.

The high level theory which is a central theme overlying much of Jung's work, Jung (1966) attributes to Heraclitus.

> Old Heraclitus, who was indeed a very great sage, discovered the most marvelous of all psychological laws: the regulative function of opposites. He called it enantiodromia, a running contrariwise, by which he means that sooner or later everything runs into its opposite... The only person who escapes the grim law of enantiodromia is the man who knows how to separate himself from the unconscious, not by repressing it--for then it simply attacks him from the rear--but by putting it clearly before him as that which he is not. (pp. 72-73).

Much of Jung's work can be viewed as an elaborate network of constructs congruent with his simple statements about enantiodromia. The constructs were such things as conscious-unconscious, ego-shadow, thinking-feeling, sensing-intuiting. Through most of his career Jung didn't believe that the group context fostered individuation and left this domain unworked. Taylor believes enantiodromic principles can be used in a group context to foster personal growth and individuation. A small part of the network of constructs in his study involves sensing, intuiting and the notions of support and confrontation (a demand for more). Sensing and intuiting are used here as orienting styles rather than personality types. As styles they are conceived of as much closer to consciousness and much more open to social influence and consequently more capable of being impacted in group learning situations.

The internal principles hypothesized by Taylor are if a person feels supported in his/her sensing function and demanded more from (confronted) on his/her intuitive function, this joint condition will lead to more expressions of his/her intuitive function. Contrariwise, if you support his/her intuitive function and demanded more from (confronted) his/her sensing function, this joint condition will lead to less expressions of his/her intuitive function.

"I hear you when you speak of your ideas and insights (intuiting function supported) and I would like to hear more about the practical, concrete facts (confrontating the sensing function)." This expression of the joint condition of support for intuiting and confrontation for sensing should result in less intuitive expressions by the "you". The emphasis is on a person in a group experiencing the support and confrontation jointly. Support alone or confrontation alone isn't very effective. Since the person himself/herself must experience these, it seems natural to express the deductions from the network using self-reports.

Support was measured by responses to statements such as:

In this group, I felt accepted when I was:

being insightful (seeing new relationships among the facts).

definitely no 1 2 3 4 5 6 7 definitely yes

being accurate with the facts.

definitely no 1 2 3 4 5 6 7 definitely yes

Confrontation was measured by responses to statements such as:

In this group, I felt people were trying to get me to:

be more insightful (see new relationships between facts).

definitely no 1 2 3 4 5 6 7 definitely yes

be more accurate with the facts.

definitely no 1 2 3 4 5 6 7 definitely yes

The intuitive and sensing orienting style were measured by responses to self descriptive statements such as:

Myself, the way I am

theoretical __:__:__:__:__:__:__ practical

concrete __:__:__:__:__:__:__ abstract

matter of fact __:__:__:__:__:__:__ unimaginative

The joint condition of support and confrontation is represented by the algebraic operation of multiplication. So that the deductions from this small part of a network come down to statements like:

1. The product of ratings of support for sensing and confrontation on intuiting will predict (in a positive direction) the ratings for intuiting.
2. The product of ratings of support for intuiting and confrontation on sensing will predict (in a negative direction) the ratings for intuiting).
3. The products in 1 and 2 will be more predictive than the effects of support by itself or confrontation by itself.

These kind of statements are in a form that allows the researcher to match

them readily with empirical evidence.

As you individually confirm the deductions from the network you incrementally increase your belief that the interrelations are as you asserted. It is a nice way of dealing with the problem of knowing and believing. Say you have an elegant theory, simple and straightforward, from which you articulate a network of five constructs, twenty internal principles, and two hundred deductions. If you could confirm all two hundred deductions you would end up strongly believing that you could explain what was going on in the domain. If there is some disconfirming evidence it is typically localized in part of the network. You can modify the network. Otherwise you can decrease your belief, eventually getting to the point where you try a new theory.

Elaboration of the network is also the way in which meaning accrues to the constructs. As we lawfully connect a construct to another construct or to a deduction we learn more about the meaning of the construct. Meaning is always open and growing. Our constructs may have more meaning than we could ever write down and that is okay in Science. We can deal with desires, longings, fears, hopes, wishes, and whatever. As long as we have the constructs involved in lawful interrelations we can increase their meaning and lay them open to the same evidential process as any other scientific term.

The skeptical may ask, "What ever happened to the 'operational definition?' "Intelligence is what the intelligence test tests," etc. To say a concept is the same as the measure we use to assess it, is to deny the reality that we can measure things in many ways--each measurement

partially reflecting the underlying concept. To say that a concept has no meaning beyond the definition, denies the reality that meanings grow as we lawfully connect a concept to other concepts. No matter how we measure "anxiety" we know more about it after we relate it to "intelligence" than before. Its meaning has grown. Operational definitions, once stated, are fixed and immutable. This does not give us a way to increase our understanding. These are arguments, but Hempel (1969 p. 88-97) presents proof that operational definitions are meaningless. In capsule form, he shows that if all basic concepts are operationally defined in terms of a set of measurements, and all other concepts are defined in terms of these basic concepts we have a system in which all meaning is stipulative and no meaning is substantive--all form and no substance. Besides, to restrict constructs to "observational terms" is ridiculous. We really "observe" very little. In what sense do we observe an intelligence test score? All we get from a person is a series of marks on a page. We use a lot more than observation to transform that datum into a meaningful term even in the most supposededly "operational" context. When we merely add the number of items correctly answered to get a total correct score we are using a whole series of elements from data theory. We are assuming the answer we have coded as correct really is correct; that the probability of marking the correct answer increases as intelligence increases, and that all items have equal weight in reflecting intelligence. Many domains in humanistic research are concerned with laws that pertain to hypothetically unobservable entities, processes or states (e.g. feeling, thinking, sensing and intuiting). We need, and have, an open way of increasing our understanding of these constructs.

The opportunity to elaborate the network of constructs provides the needed openness. We could add the construct of differentiatedness to all the Jungian functions. A whole parallel level of relations can be established for differentiated versus undifferentiated feelers or thinkers. With each new construct comes the opportunity for richer meaning--- more understanding. The system of knowing is dynamic.

Notice that I've avoided using the typical distinction between "observational terms" and "theoretical terms." It is much clearer to talk about evidential terms and hypothetical terms. Prior theory, as pointed out earlier, can serve as evidence that the meaning of "observed" behavior is being preserved. Evidence flows in an osmotic like manner. If you have a great deal of evidence that your methodology is correct then you believe even highly unusual results produced by that method. If you are sure, from prior research or other evidence, that certain relations exist among two constructs, and a new method shows these relations, then you increase your belief that the new method is working as it should.

There is another aspect to the theory side of the flow. What if another researcher says your theory is all wet and pumps his/her own. In recent years the clash of scientific egos rarely happens so directly. But if it should occur you are in a good competitive position. You have one theory in a domain, from which flows twenty laws and 200 confirmed deductions. To find all alternative explanation for all 200 deductions from another simple high level theory is difficult if not impossible. Typically it should take an ad hoc hodgepodge of disaggregating assumptions to reduce these results to the point where each

could be explained. As Pirsig pointed out, any one of the results could be explained by an infinite number of rational hypotheses.

But the point of the theory side of the flow is not to provide a battleground for scientific egos. Rather it is the way Science accumulates knowledge and reasonable certainty over research contexts. It is the way that Science allows us to select from among the multitude of alternative explanations.

Some Final Comments

Science is an uniquely human endeavor. It is not indulged in by white rats, stars or atoms. This has special implications for a science which studies people. We can see the special meaning by looking at the criteria for evaluating whether or not a macromethodology (i.e., a philosophy of science) is acceptable. There are two standard criteria for the evaluation of macromethods in Science. First is that evidence must be used in logically consistent ways. This really isn't a very restrictive criterion, since the logical rules are elementary. Methods which entail logical inconsistencies do not last long. This is not to imply that behavior of people must be logical. Behaviors are phenomena and, logical or alogical, they are to be explained and understood. The second standard criterion stems from macromethods being a record of the activities of a scientist. The criterion is that the record be accurate. This is where the specialness of philosophy of science in psychology starts to come through. In human sciences the philosophy of science forces a kind of parity between the "scientists" and the "subject". Subjects are people and scientists are people. If your philosophy

of science says that people can not think; then the macromethod is an inadequate record if scientists think. If the macromethod assumes that all individuals are the same save for random, chaotic fluctuations, but scientists have systematically different points of view; then the macromethod is inadequate. There is a forced parity which can save us from making silly assumptions about the nature of the phenomena we study. It also is an enabling mechanism. Anything that has phenomenological reality in us, and consequently could be part of the record of our activities as scientists must also be legitmate as a domain of study for our science. In no way does this force us to assume all people are like us. It merely asserts that if we have particular thoughts and feelings that others may also.

There is a third criterion for the evaluation of macromethodology. It merely asks if the macromethod allows you as a scientist to respond to the "important" problems in the field. If no, the macromethod is inadequate. This criterion has two thrusts. First, it underscores that value and valuing are deeply human processes which are a part of human activities, including scientific ones.

If we wished to study values there would be no special difficulty. Statements about values can be handled in the same manner as other assertions about hypothetical, unobservable entities. However, the values we typically wish to track are those of the scientist. Studying these values scientifically detours us from our basic interests in other phenomenological domains. It is not incumbent on the student of clinical populations to scientifically

study his/her values. As mentioned earlier the choice of domains is pretty well reserved for the individual scientist. The minimum condition I suggest is for the scientist to be aware of and candid about his/her values and to try to track the impact these values have on the decisions made in the research process. Those who argue that values don't belong in science are in a very tenuous position. Such statements have little credibility in the face of the daily reality of scientific activity. This reality includes standards relating to the human use of human research subjects, ethical standards for the delivery of psychological services, and many other similarly oriented writings.

That standards have not been articulated for much of the daily conduct of scientific activity does not indicate these domains should be valueless. It speaks more to the individual scientist's perogatives over value positions in these areas. It is unreasonable to construe these perogatives to include the option of appearing to be without values. The incidental effect of such a stance seems to be to keep the values that are active from open scrutiny. It is difficult to argue in favor of such a position.

The second thrust of this last criterion for the evaluation of macromethododology says find the problems that grab you and pursue them. Use or develop the methods which allow you to respond to those problems. Science is an enabling force in the pursuit of knowledge and understanding. Any attempt to use the canons of Science to disallow or disenable is antihueristic and antithetical to its basic purpose.

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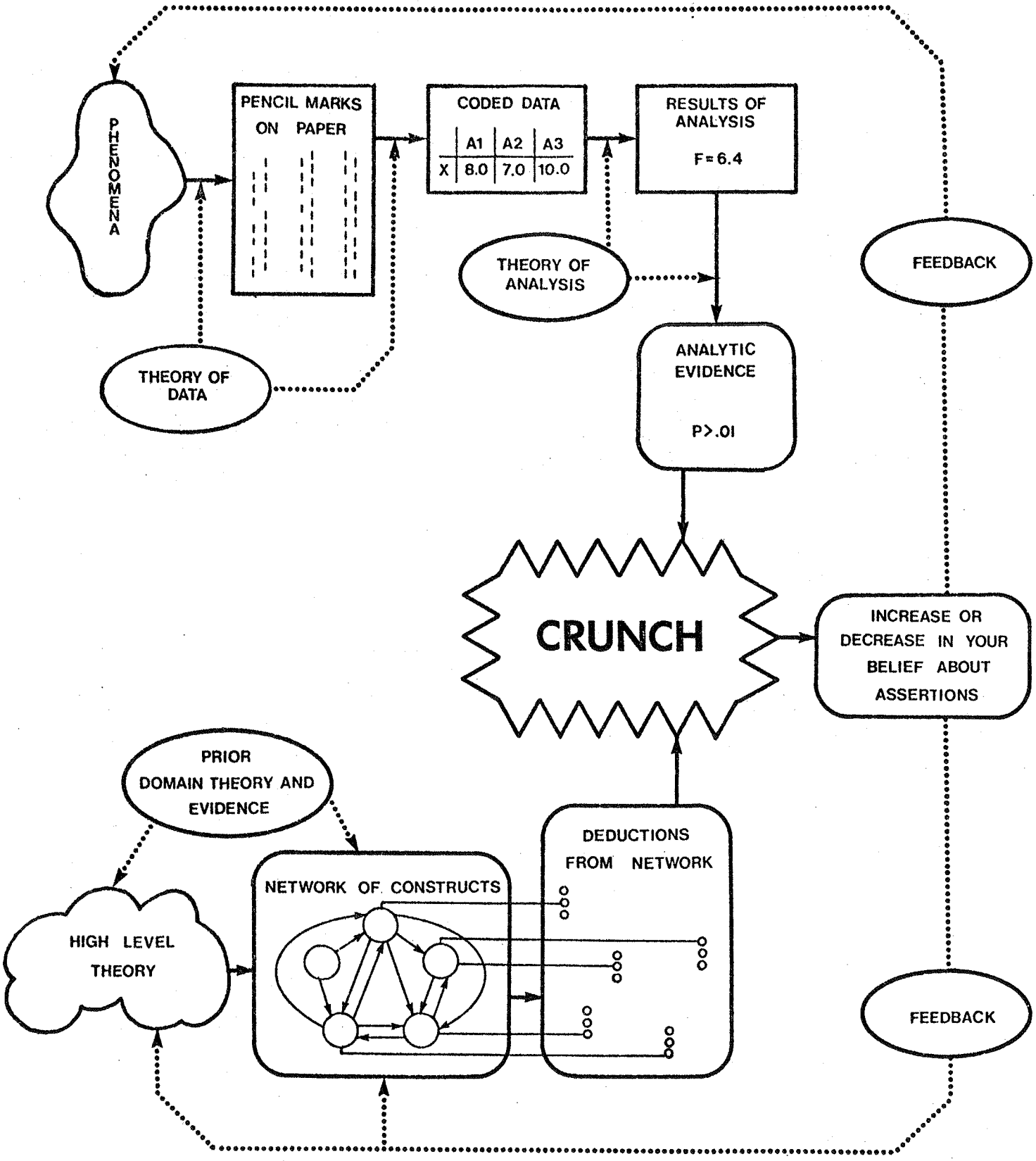

FIGURE 1. FLOW OF THE RESEARCH PROCESS