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REGULAR ARTICLE

Why Psychology Cannot be an Empirical Science

Jan Smedslund [1]

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Abstract The current empirical paradigm for psychological research is criticized because it ignores the irreversibility of psychological processes, the infinite number of influential factors, the pseudo-empirical nature of many hypotheses, and the methodological implications of social interactivity. An additional point is that the differences and correlations usually found are much too small to be useful in psychological practice and in daily life. Together, these criticisms imply that an objective, accumulative, empirical and theoretical science of psychology is an impossible project.

Keywords Empirical science · Generalized human mind · Irreversibility · Infinite number of factors · The pseudo-empirical · Social interactivity · Practical relevance

In a recent paper, Mammen & Mironenko (2015) begin with recognition that psychology is a science in crisis, both with respect to theoretical coherence and practical efficiency and suggest that a further development of the Russian and Danish activity theory traditions may open a way ahead. In spite of sympathizing with many of the arguments and analyses in that paper, I think they may not succeed, because they fail to consider some of the serious *limitations* of the project of psychology. These limits are indicated in Joachim Israel's metaphor about "inflating a balloon from the inside" and Piaget's observation (paraphrased) that "one cannot accommodate to what one has not assimilated". This was directly illustrated in a study of mine where a person was unable to learn a simple observable regularity in 4800 trials (no accommodation), but where this became possible after the regularity was explained and understood (assimilated) (Smedslund, 1961). The preceding assertions are amplified in a well-known adage quoted to me by my teacher David Krech: "What is new in psychology is not good, and what is good is not new" and echoed by Kukla (2001, p 235) who wonders about how much of psychology can be deduced (a priori) from the fact that humans have developed *science*. Since we *know* that we have developed science, we also *know*

✉ Jan Smedslund
jan.smedslund@psykologi.uio.no

[1] University of Oslo, Oslo, Norway

much about what follows from this psychologically. In line with the preceding, I am inclined to think that the task of psychology must necessarily be limited to explication and analysis of what is already implicitly familiar. Hence, I think Mammen & Mironenko's attempt to advance psychology as an empirical and theoretical science by further clarifying some foundational issues may not be successful because they fail to take into account the mentioned *limitations* of the project. I try to show this by means of a different line of critical arguments, with the qualification that I find the relation between my own and M&M's approach somewhat unclear. The two should perhaps be treated as independent because they have so different concerns. However, in so far as Mammen & Mironenko aspire only to improve the ontology of the dominant paradigm and overcome dualism, while otherwise maintaining theorizing about empirical studies of averages of many unknown persons, etc., they appear to face the same difficulties that I criticize here. I do not share their concerns about overcoming dualism, simply because I accept that there are two incommensurable ways of talking about the (same) world, and that it is totally impractical to talk about personal life in neural terms and vice versa. As I see it, psychology takes its departure in, and consists of reflections about and analyses of, persons in terms of ordinary language. Whether or not this can result in a "science" is the topic of the present article.

Four reasons are discussed for why the current psychological paradigm may have to be abandoned, namely *irreversibility*, *infinite numbers of determinants*, the *pseudo-empirical*, and the *social interactivity*. They all exemplify limitations that make it difficult to develop an empirical science in the classical sense, e.g., formulating and testing general hypotheses.

Irreversibility

A basic feature of psychological processes is their *irreversibility*. Every experience changes a person in a way that cannot be completely undone. Reversibility means a possibility to return to the state of the person before an experience or action, in other words a return to a state where the experience or action had not yet taken place. This would imply that all memory or effect of the experience at any level would be completely absent. I take it that this possibility can be rejected and that one must assume that persons are continuously and irreversibly changing. Even when a person appears to be stable and repetitive over a period, this is partly misleading since there is always change in the form of explicit or implicit memories of the repetitions of events and outcomes.

If we assume that psychological processes are irreversible, it follows that all apparent constancy in psychological phenomena is conditional on stability of outcome and will disappear if and when the outcome is changed. This conditional nature of stability means that psychological research cannot be a search for ever-lasting invariance (laws), but only for more or less local and temporary regularity. Hence, psychological findings are in principle *historical*, and different from findings in the natural sciences that often involve genuine invariants, and are in principle reversible.

The irreversibly changing character of psychological processes has constituted a problem from the beginning of modern scientific psychology. A person cannot easily be used to test a hypothesis experimentally, because he or she is changed by, and

remembers being exposed to, one condition when encountering a second. This renders the outcome ambiguous, stemming either from change of the person or from the difference in experimental condition. The effect of learning can be eliminated by using another person in the second condition. However, the comparison will still be ambiguous because of the ubiquitous presence of individual differences. One has tried to control both for learning and for individual differences by using average results of *matched* groups, but due to the indefinitely high number of possibly relevant factors to be matched this turned out to be an unsatisfactory solution. One then turned to a purely statistical strategy of comparing average performance of groups randomly selected from a sample of the population and thus controlled both for learning and for effects of individual differences. The final result was the current RCT-procedure. Despite it's in many ways impeccable logic, this methodological position has a principal weakness, namely the direct contradiction involved in doing empirical studies of irreversible processes. The logic of inductive inference entails that what is observed under given conditions at one time will occur again under the same conditions at a later time. But this logic can only be applied when it is possible to replicate the same initial conditions, and this is strictly impossible in the case of irreversible processes. Experimental studies of the RCT-type attempt to circumvent this problem by using average response-values for groups of randomly selected persons in objective, and hence repeatable conditions. However, predicting future results from average responses to objective situations, ignores psychological irreversibility and, hence, can only support theoretical views at the aggregate level, at the cost of knowledge about the processes that go on in individual participants. See also (Lamiell, 2015).

A person will repeat an act in a given situation as long as it is expected to lead to a goal. However, this stability is only partial, because the person always changes by remembering each successive occasion. Partial stability only occurs when outcomes are stable, and this is frequently made possible because of already established societal or local inter-individual rules. However, situations and outcomes often change from the intervention of *fortuitous* events. In other words, many of the circumstances determining the outcome of acting and individual change lie outside the person's control. Hence, observations of individual stability cannot be interpreted as evidence of eternally valid principles, but can only demonstrate time-limited situational stability. This "historical" quality of psychological phenomena makes them uncertain candidates for prediction. Unlike historians, psychologists are not primarily interested in what was true earlier but in what will be true in the immediate future.

The main conclusion to be drawn from irreversibility is that empirical research in psychology cannot be seen as building an accumulative scientific discipline about individuals, but only as mapping temporarily stable statistical tendencies at the aggregate level. These mappings must, be evaluated on pragmatic grounds, i.e. as yielding possibly useful knowledge in limited domains for a limited time. This is because psychological processes are stable only when the outcomes are stable. Increasing general recognition that stability in psychology is conditional, probably explains the decline of the use of the term "law" over the last century (Teigen, 2002). The regularity that is nevertheless observed cannot be taken to reflect permanent laws, but only temporarily stable feedback-loops. These are like whirls in a stream which are stable only as long as the total flow of water does not vary and the stones on the bottom maintain their positions. One cannot build an accumulative science on conditional

invariance. The findings in psychological journals may superficially appear to be of the same order as those reported in physics or chemistry and, hence, psychology may superficially look like an empirical and accumulative science. The conditional and transient nature of psychological findings is rarely acknowledged, perhaps because the publication and preservation of empirical findings will then appear unjustified since they cannot be taken to be useful in the future. The scarcity of attempted replications also allows psychologists to avoid thinking about this problem.

As time goes by, the likelihood of successful replication of psychological findings can be expected to diminish because, due to irreversible historical change, it gets more difficult to approximate the original external conditions. Hence, the assumption that one is "accumulating" knowledge becomes doubtful, and the rationale for assembling data is weakened. The most valuable remaining function of data may be to stimulate thinking and to facilitate insights that could also, but perhaps not so easily, have been reached *without* empirical illustration (see the section on the pseudo-empirical, below).

Are there *reversible* psychological processes? I think the answer is yes, but these are limited to logical *necessity*. They have to do with *language* and with *logic*, and more specifically with the relations between the relatively stable meanings of words and propositions. *E*xperiences of events are always irreversible, but recognized logical connections are timeless and hence, reversible. For example, in my studies of cognitive development I observed preschool children give the following argument for the conservation of quantity: "It *looks* more in the ball, but we did not add anything or take anything away so it *must* be the same." The children took this to be necessarily true, given what is meant by the words. Given these meanings, the logical network they form is *invariant*, and the terms "add" and "take away" refer to strictly reversible operations.

Seen from a meta-level it appears that humans seek knowledge of each other at two levels. The most elementary one is by induction (empiricism). "Since you did this yesterday, I expect you to do it today". This works only in the limited domains temporarily characterized by reversibility and invariance, more specifically, with respect to rules and purely logical matters. The attempt to imitate the passive empiricism of natural science (observing what happens under given conditions) fails precisely because of the general unpredictability of psychological processes if they are not agreed upon in advance by the participants. This passive (observational) stance in psychology research merely succeeds in demonstrating the absence of universal and eternal laws, and places academic psychology solidly in the category "historical science". In contrast to this passive inductive approach, it appears that humans have realized a long time ago that the best way to build predictable human environments is to make *agreements* because these stabilize behavioral outcomes and allow for valid inference. In psychology it is much more profitable, to actively and collectively *arrange* what is going to happen in the future, than by merely observing ongoing activity in given conditions without interfering.

Infinitely Many Possible Contexts

A direct consequence of irreversibility and that people are continuously changed by their experience, is that since experience is infinitely varied and often fortuitous, people

come to perceive the world and act in infinitely variable ways. This makes psychology much more complex than natural science and the interpretation of observations much more difficult. Consider the following example:

A person raises an arm and extends a finger upwards. Physically this movement can be described at different levels, e.g. geometrically, anatomically, in terms of neural processes, and at a biochemical level. The number of possible alternative descriptions of the observation is limited, especially since physical processes are only influenced by the here-and-now-context.

Consider now the possible psychological descriptions of the exact same hand-rising event. The observed event may mean literally *anything*. Nothing is excluded. It may be pointing to, or symbolizing momentary, earlier, expected, or hypothetical observations of birds, airplanes, clouds, planets, stars, previous, present or future solar or lunar eclipses, paradise in various versions, evaporation, disappearance, prohibition, insight, attention, and innumerable other specific meanings, all according to whatever is the exact context. The movement may also be intended to distract or deceive in innumerable ways. In summary, the same event can have *infinitely* many possible psychological descriptions, in contrast to a *finite* number of possible physical descriptions. The difference reflects that whereas possible contexts of physical processes are limited to the here-and-now, psychological contexts also extend to the there-and-then, as well as to the hypothetical, the imaginary, the symbolical, etc., and to all these in innumerable ways.

The theoretical, methodical, and practical consequences of this infinite-finite distinction are extensive. Taken together they mean that the uncertainty about the meaning of an observation in psychology is of a higher order than in physical science.

One implication is that the language of psychology must be rich enough to describe all the infinitely varied possible contexts. This means that the professional language of psychology must be as rich as the ordinary language we are living by. Academic (technical) languages are all much simpler than this, and suffice in the different domains of natural science, but cannot cope with the challenge of describing infinitely rich psychological contexts. Hence, psychology must rely on everyday language in order to adequately describe what is going on. In a sense, this means entering what traditionally has been the domain of storytellers and writers, with the restriction that psychological descriptions should refer to something real.

The preceding also has consequences for the construction of theories. A scientific theory can only include a small number of variables and, hence, theorizing forces one to exclude much of what influences each unique person. Therefore, theoretical descriptions in psychology can at best only lead to probabilistic predictions, and have very limited usefulness in practical work.

It may be objected that psychological research conducted in a technical language may sometimes be helpful in practice, *even though* it involves only a few selected variables. This argument would have had some merit if the domain of psychology had contained single variables accounting for large proportions of the total variance. However, this is not the case. On the contrary, theory-testing data in psychology almost uniformly involve *small* differences and *low* correlations. Such weak tendencies cannot be of much help to the practitioner who must deal with the manifold of circumstances that influence each person in a given life-situation. Ordinary language is the only one rich enough to approximately describe what is encountered in real life, and why, and therefore able to support practice.

In conclusion, the infinitely varied contexts of psychological phenomena and the absence of major factors make general theories useless for the practitioner, and only local descriptions expressed in ordinary language are rich enough to serve the psychological profession. In addition to the obstacles of *irreversibility* and *infinite variety of contexts*, analysis of ordinary language also reveals a third limit to what psychological research can accomplish, namely its tendency to produce *pseudo-empirical* hypotheses.

The Pseudo-Empirical

A consequence of the unavoidable reliance on ordinary language is that empirical psychology is confronted with some additional bothersome issues. Take, for example, the following observation: a person has a certain facial expression and bodily stance leading to the interpretation that the person is surprised. One may assume that the person is surprised because something unexpected has happened, and one may test this hypothesis empirically by determining whether or not surprised persons have in fact experienced something unexpected. If the hypothesis is confirmed, this could be regarded as an empirical finding. However, the inference from being surprised to having experienced something unexpected is also logically necessary (you cannot be surprised without having experienced something unexpected) and this is knowable *without* collecting data. The described example is what I have labeled *pseudo-empirical*, that is, a finding falsely treated as empirical (Smedslund, 1991). If data appear to show that a person is surprised *without* having experienced anything unexpected, one would have to conclude that the person had not really been surprised, or that he or she nevertheless *had* experienced something unexpected, or that there was some methodical error. It does not make sense to be surprised about nothing. Given that what makes sense is what follows from what is taken for granted, one may formulate the following somewhat intriguing sentence:

> *Given what is taken for granted, hypotheses that make sense are true, and hypotheses that do not make sense are false.*

The example of surprise shows that the role of empirical study cannot be to prove or disprove that a hypothesis that makes sense is true, but only to show that the assumed pre-conditions are true. One of these is, for example, that the person is not trying to deceive you.

Hypotheses that do *not* make sense are rarely formulated, because we know in advance that they are false. Example: No one can believe that "all persons who own sailboats prefer apples to bananas" is true, and one also takes for granted that all empirical tests must falsify the hypothesis. Why? Because one cannot accept data indicating that the hypothesis is true! It is simply impossible to envision a world where ownership of sailboats and fruit-preferences are linked without exception, and where it is impossible to sell one's sailboat and not cease to prefer apples. If data consistently upheld the hypothesis, one would have to change one's conception of the world.

Instead of making a sharp distinction between empirical and pseudo-empirical one may also envisage a gradual distinction based, not on formal logic, but on the amount of change in world-view required by a given finding. Anyhow, the two examples of

making sense and not making sense were selected for their simplicity and were not taken from actual research reports. However, there is much evidence showing that also real psychological research can be pseudo-empirical. (Arnulf et al. 2015; Semin et al. 1987, 1988; Smedslund 1994a, b, 1997a, b, 2000, 2008; Wallach & Wallach 1998).

In cases of research where the independent and the dependent variable are logically connected, the charge of pseudo-empiricality holds up. A simple way to test for pseudo-empiricality is to consider if a *negation* of the hypothesis is possible and acceptable. If a negation is unacceptable (absurd, senseless), the hypothesis is not empirical since it expresses a necessity and could have been stated in advance. It is of course debatable if there is a sharp distinction between the analytic and synthetic and it may be that negations of a proposition may be more or less absurd. Therefore, what makes sense may not always involve demonstrable logical implication. There may also be cases where "plausible" hypotheses are simply based on previous observations of some degree of co-variance of logically unrelated events. In such cases making sense only means expectancy of probable repetition.

Empirical hypotheses in psychology are frequently tested in special laboratory contexts and often involve *magnitudes.* Since ordinary language only permits statements of "more" and "less", it does not allow one to logically derive the outcome of more refined measurements. Example: One can prove logically the formulation, "the more unexpected, the more surprised", but not the exact mathematical form of the relation between measurements of the two variables in specific settings. However, exact quantitative theories in psychology do not tend to generalize beyond specific measuring instruments and special conditions. This was, for example, demonstrated in the failure of Clark Hull's attempts to build a general mathematical theory of psychology (1940) (1955), and the many disappointing findings explain why contemporary quantitative models tend to be highly instrument- and situation-specific.

One cannot categorically exclude that one could formulate general hypotheses in psychology that are neither necessary nor self-contradictory, but simply express probabilities. However, such genuinely empirical general hypotheses would propose discovery of hitherto unknown lawfulness, which is unlikely to be found because of what we all know about psychological processes. One may assume that since goal-directed, reflecting human beings have lived in societies over innumerable generations, any practically useful psychological regularity in daily life would have been discovered long ago and also incorporated in language. Hence, the prospects of genuinely empirical research in psychology are radically curtailed by language, and by what is already generally known about the nature of psychological phenomena. Hence, empirical research depends on two rather shaky premises, namely that it is possible to discover hitherto unknown major psychological lawfulness, and that such a discovery would make a noticeable difference in the way people live and interact, and psychology is practiced. It appears to me, that the outcome of more than a hundred years of research is consistent with a skeptical view of this possibility.

In conclusion, the preponderance of pseudo-empirical hypotheses is a serious weakness of the current tradition and the goal of finding some hitherto unknown genuinely empirical regularity is unlikely to be reached. It remains to discuss one more weakness of the current research-strategy, namely a failure to face squarely the methodological consequences of the *social interactivity o*f psychological processes.

Social Interactivity

The findings of modern psychology consist almost exclusively of average data from many individuals *unknown* to the researcher. However, one can make no valid inferences from averages to individuals, and, after all, individuals are what psychology is about (Lamiell, 2015) (Speelman & McGann, 2013). Hence, there are many types of psychological phenomena that are inaccessible to the current day researcher. What is studied is frequently of the type "stimulus-response", i.e. a linear sequence, from input to output. One observes how people on the average respond to research instruments and experimental conditions. This methodology leaves out information about central areas of what is essentially human, namely the *interaction* between persons. Personal processes are intimately intertwined with, directed at, and dependent on, other persons and, hence, cannot be accessed and understood by studying average responses of unknown isolated people. The most important parts of human life are interactions with other persons, and what goes on in these interactions. It is, for example, relatively uninformative to describe the average frequency of a hostile first response to a given person, not knowing whether it will be maintained after a friendly vs. an unfriendly counter-response by that person, and so on. In general, information about complex interaction patterns and response-repertories is lost if one merely studies first responses to standardized questions or static situations. Also, much personal information is only forthcoming in interaction with trusted others. Therefore, the psychologist may need to be a trusted other to have access to such information. To be sure much information can be extracted from long interviews in so-called qualitative research, but this departs from the dominant paradigm in that the criteria of objectivity and generality are abandoned. While one gets to know more closely a number of individuals, the researcher is now engaged personally and also the findings cannot be generalized. Without objectivity and generality, the meaning of the term "research" is reduced to simply "getting to know some persons".

The absence of direct acquaintance with persons in their actual life-situations means that the ability to understand and predict is much impaired. Reliance on the *average* response of groups of unknown persons in standardized situations can by necessity only lead to weak probabilistic predictions. This follows from the ubiquitous presence of large individual differences and the infinite number of influential situational determinants.

Turning to the study of individuals which, after all, is what psychology is about, the most demanding task is to understand and predict the behavior of *opponents* or *enemies*, because they try to *avoid* being understood and predicted. This is richly illustrated in the complexities of game theory. An alternative is to study *friends* or *allies* who are less averse to being predicted and understood. This theme is not much treated in general text books of psychology because it necessarily involves abandoning the goals of objectivity and generality. Entering into friendly relations with persons, the psychologist definitely influences what is observed and is immersed in the unique. It is only possible to know many important psychological processes from a position of being personally engaged and focused on the concrete and unique.

Leaving the study of average responses of many unknown individuals, and focusing on interaction with persons, raises questions such as how to build friendly relations, what are the important concepts involved and what kinds of inferences can be made

from them. These questions and the possible answers are already familiar in everyday life and in psychological practice, and empirical hypotheses in this area are, therefore, likely to be pseudo-empirical.

Take, for example, hypotheses about the central concept of *trust.* Five necessary, and jointly sufficient, conditions for trusting a person, namely attribution of *care*, *understanding*, *own-control*, *self-control*, and *relevant know-how* are commonsensical and have been described and logically proved (Smedslund, 1997). The existence of these logical relations means that empirical studies of the five determinants of trust are bound to be pseudo-empirical, whereas specific hypotheses about the magnitude of the various relations in various conditions while being empirical are likely to be valid only temporarily and locally. They illustrate a domain where we all know much, and upon reflection tend to agree about what is necessarily so. Persistent disagreement may indicate differences in terminology.

The central role of the concept of trust derives from the fact that other persons are the most important part of the surroundings for a person, and because these others are capable of benefitting and harming the person. To trust someone is to think that he or she will benefit you and not harm you. However, persons do not merely take the passive stance of inferring whether or not they can trust another person in the future from what the person has done in the past. Instead, common sense suggests that in order to predict what another person will do in the future it is best to try to *make him or her* trustworthy and predictable, by becoming acquainted and by making agreements, i.e. to take an *active* stance. Ordinarily, people live together in societies with already established common rules, thus insuring some predictability and mutual trust. Individual relationships are also managed with the support of unwritten and written contracts or agreements, and short-time appointments. In such circumstances people usually are able to live highly predictable lives. Social rules and individual agreements are expected to be kept with narrow margins. In sharp contrast to this is the stance of current scientific psychology that builds on average responses of unknown persons, and which only generates predictions that barely exceed chance and are useless in ordinary life where no one would accept that, e.g., agreements where kept only at a level slightly better than chance!

One may conclude that studying people by averaging data from first responses of unknown persons in special situations, places psychologists at a serious disadvantage. This is so, because average first responses to static situations do not yield useful information about individuals, and because such studies disregard our common sense knowledge that a person's behavior can be best predicted from trust based on personal interaction, and by making agreements based on this. In addition, there are of course many other aspects of the social nature of persons, known to everyone, but hardly accessible by current research-methodology.

Conclusion

Can the current empirical research paradigm be saved? Based on the preceding arguments, I think the answer must be probably not. Only by ignoring the implications of irreversibility, infinity of contexts, pseudo-empiricality, and social interactivity, can the project of studying the generalized human mind continue, but then as a closed,

esoteric academic discipline, and at the expense of little contact with everyday life and psychological practice. One can, of course, continue to produce small statistical differences and correlations in the same way as has been done for more than a century. But the few genuine advances in knowledge that *have* been made in psychology, such as recognition of the importance of the (social) context, may have grown out of prolonged reflection over both practice, research findings and common sense (Smedslund & Ross, 2014). It is becoming increasingly apparent that one cannot noticeably improve psychological practice by assembling data and theorizing about average responses from groups of unknown persons in special conditions.

I expect many counter-examples from defenders of the current paradigm to be centered on the notion of probability, and on the argument that research findings can demonstrably provide the practitioner with slightly better odds than without them. Formally this is true. One frequently refers to the parallel to medical practice which can be improved by research. But although it is true that both kinds of practice involve indefinitely numerous factors, the difference is that medical practice involves single factors accounting for a considerable part of the total variance, whereas psychological practice rarely involves single factors explaining more than a few per cent. In psychology, there is an unbridgeable gap between odds at the group level and odds for specific individuals. The crucial issue here is the huge discrepancy between explaining a few percent of the variance at the level of group averages and predicting approximately without error at the individual level. In everyday life many events can be anticipated almost with certainty (because of implicit or explicit agreements), and tolerance for uncertainty is low. This is the case with traffic rules as well as with the keeping of contracts and appointments.

In psychological practice, which deals with people who have *failed* to manage their lives, the initial low predictability associated with interventions is handled by trying out small tentative steps at a time, and the ensuing concrete changes in a client are accepted as useful only when they are becoming sufficiently dependable over time and situation to be noticeable in ordinary everyday life. In current academic research, findings are taken to be interesting if they deviate sufficiently from *chance*, whereas in daily life and in psychological practice, results are accepted only if they do not deviate too much from *perfection*. I cannot see how this discrepancy can be successfully bridged.

I have tried to show here that, as I see it, the currently dominant empirical research-paradigm cannot be successful because of limitations imposed by the nature of psychological processes. See also (Smedslund, 2009). Therefore, it cannot provide the required "evidence-base" for psychological practice. However, one may assume that an entrenched paradigm, such as the current mainstream psychology, will not be abandoned unless a sufficiently attractive alternative is made available. This possibility has to be further discussed elsewhere.

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Jan Smedslund (PhD) University of Oslo, 1955. Professor of Psychology, University of Oslo, 1966-1999. Many appointments abroad, among them University of Geneva 1957/58, Harvard 1963/64, Center for Advanced Study in the Behavioral Sciences 1967/68, Oxford, 1983, Stanford 1985/86. Licensed clinical psychologist 1978-2008. Six books and numerous articles.