AGGRESSION RESEARCH AND INTERVENTION STRATEGIES FOR THE 1990’S*

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Abstract

In the past decade, research has made some important contributions to our knowledge concerning criminal violence. Enough research has been accomplished that we can examine lines of inquiry that appear to have been productive and areas of work (and sometimes styles of work) that have been less successful. Some research has had relatively immediate relevance to intervention but most has not. The most pressing research issue for the nineties is to develop methods that link research with intervention so that prevention and treatment practices in common use may become more effective.

The link between research and intervention requires a theory with enough empirical support and specificity to provide a rationale for a particular intervention, its implementation, and evaluation. Some examples of research and development efforts that exemplify this linkage are described to suggest how the relationship between research and intervention may become closer and more productive in the future.

I take it as agreed that our primary research goal is to accumulate knowledge about criminal violence that can be used to improve policy and practice. Continuing research is thus central both because there is a great deal we have yet to learn and because the magnitudes of the effects of our present social policies and treatment programs are often not very large.

However, neither the cumulation of knowledge nor improved practice are necessary consequences of continuing research.

The most striking historical example of the nonuse of knowledge (i.e., the lack of inevitability of scientific progress) is Mendel’s discovery of particulate inheritance, which, although it addressed the central problem of mechanism in the theory of evolution through natural selection, went unrecognized for 35 years (Whitney, 1990).

More recently, the clearest example of nonuse involves Paul & Lentz’s (1977) conclusive demonstration of the superiority of social learning token economy procedures in the

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treatment of chronic schizophrenics (as compared to phenothiazines, milieu therapy, or traditional mental hospital treatment). It is a continuing marvel that this treatment of choice has, to an overwhelming extent, been ignored by both the scientific and the service communities (Backer, Liverman, & Kuchnel, 1986; Bellack, 1986). Such a marvel, indeed, that I will return to Paul & Lentz's demonstration later in my presentation.

Scientific progress is thus not assured, even with continued research activity. Because, as Stephen Lewis likes to remind us, the future is not yet written, it is our responsibility to adopt strategies for the 1990s and beyond that are the most likely to lead to more powerful theories which can, in turn, inform policy and practice and, furthermore, strategies that will lead to actual implementation and use of research findings.

It may help to facilitate this discussion to differentiate between science and technology. Science attempts to construct causal theories that will account for observed phenomena whereas technology involves the achievement of some desired effect with increasing efficiency. Science would thus attempt to explain criminal violence and technology would attempt to prevent or reduce it. Historically, the relationship between science and technology has been symbiotic (for a discussion of this distinction, see Grove, 1989).

The scientific questions we choose to ask, must, of course, be based upon what is already known. The key to any successful strategy is that it be part of a cumulative enterprise, as opposed to following fads, perseverating in a method of study that has not led anywhere, or prostituting science in the service of ideology.

I can't tell, any more than you, what a science of aggression or a technology of prevention or correctional/forensic treatment will look like in the year 2000. I can, however, delineate trends that have occurred in the last 20 years (since I've been in the "life" as it were) and extrapolate these into the likely context of the next decade.

First, what scientific trends can be seen presently that are likely to continue into the near future? The easiest to see, perhaps, are technological: the increasing computational power and convenience of computers and in a related development, the greater use of multivariate statistical techniques that require large samples.

Some of these developments can clearly be seen in the work of my colleagues and myself on the prediction of violence among released maximum security psychiatric patients. Early
followup studies were conducted on small samples of convenience (typically defined by bureaucratic documentation) and employed univariate analyses (Quinsey, Pruesse, & Fernley, 1975a; 1975b; Quinsey, Warneford, Pruesse, & Link, 1975). These were followed by studies of larger samples (Pruesse & Quinsey, 1977) and later by studies using multivariate approaches (Quinsey & Maguire, 1986). Most recently, this line of investigation has employed theoretically relevant predictors that were gathered specifically for the multivariate prediction of violent recidivism among samples of patients defined by their treatment status (Harris, Rice, & Cormier, 1989; Rice, Harris, & Cormier, 1989; Rice, Quinsey, & Houghton, in press), offence history (Quinsey, Rice, & Harris, submitted; Rice, Harris, & Quinsey, in press), or treatment status and offence history (Rice, Quinsey, & Harris, 1989).

More generally, multivariate causal modelling will be necessary to deal with increasingly high quality data on large samples from longitudinal studies of delinquent behavior (e.g., Patterson, DeBarsyshe, & Ramsey, 1989); genetic investigations, including twin and cross fostering studies (e.g., Hahn et al, 1990), and comparative treatment outcome investigations (e.g., Kazdin, 1987).

Increasingly, investigators in the area of human aggression will be able to ask questions of proximal causation using brain imaging techniques and questions of ultimate causation employing genetic and evolutionary methods (Buss, 1984; Cairns, & Gariepy, 1990; Cairns, Gariepy, & Hood, 1990; Loehlin, 1989; Wilson & Daly, 1985). Theory construction in the area of personality will, I believe, increasingly rely on genetic investigation (e.g., Buss, 1984).

On the more applied side, the greater sophistication of intervention evaluators and the increased pressure for cost containment will lead to pressure for definitive or conclusive evaluations instead of suggestive ones. I believe that this will lead to the increasing use of meta-analyses of treatment outcome studies as well as to large outcome intervention comparisons using random assignment.

The meta-analytic revolution is already well underway (e.g., Andrews et al, 1989; Bowers & Clum, 1988; Christensen et al, 1987; Clum & Bowers, 1990; Dobson, 1989; Gendreau & Andrews, 1989; Giles, 1990; Gottshalk et al, 1987) but large scale comparative treatment outcome studies, particularly those involving random assignment are a rarity. In a recent, state of the discipline, review for the Canadian Psychological Association, Rice
and Quinsey (1986) outlined some of the phenomena that limited the impact of research on correctional and mental health institutions. These phenomena included the relative imbalance between the many studies of assessment compared to the few treatment evaluations and the almost complete absence of longterm, truly experimental treatment outcome studies.

The more definitive conclusions regarding treatment efficacy that will come from meta-analytic and larger scale treatment outcome studies are required because, as Parloff (1982, also cited in Rice & Quinsey, 1986) asserted concerning the then existing literature on the effectiveness of psychotherapy, anything less "can be expected to exert all the impact of a quixotic Bambi planted firmly in the path of the onrushing Godzilla of cost-containment policies" (p. 725).

These aforementioned trends are likely to continue affecting the cutting edge of research in the 1990s. It will be true in the future, as it is now, that a great deal of scientific activity will basically recapitulate work that has already been accomplished. In fact, it is most striking in reviewing the literature to find studies that could have been published 20 or 30 years ago in the same journal issues with articles that clearly could not have been.

Progress is thus not only not inevitable but also extremely patchy (cf. Andrews & Wormith, 1989). For example, historians Neustadt and May (1986) wrote a series of recommendations for policy makers designed to help them use history as a decision aid. This book is remarkable in that it is completely innocent of any theoretical or applied work conducted on decision theory. One might argue that this ignorance simply results from disciplinary boundaries, except that the same phenomenon is demonstrated within the area of clinical prediction, where typical clinical practice appears unaffected by empirical results. A recent Psychological Review paper (Kleinmuntz, 1990) was appropriately titled "Why we still use our heads instead of formulas".

In my darker moments, I think that the sum total of what a single person can know is a small constant. In the distant past, we were ignorant because little was known, now we're just as ignorant because there is too much for an individual to find out and what is known is too abstract to be comprehensible to most of us. Increasing sophistication has costs in the form of a potentially widening gap between researchers and practitioners, not to mention the general public. Concrete evidence of this phenomenon can be found by looking at the psychology section of any general bookstore.
Case Study: Token Economy Programs For Assaultive Psychiatric Patients.

I will attempt to illustrate some of what I think has been learned about institutional treatment programs for violent offenders over the past 20 years by presenting some data on a behavior modification program run at the Oak Ridge Division of the Mental Health Centre in Penetanguishene these many years. Hopefully, this example will also illustrate in concrete terms one of the central problems that must be dealt with in the future.

I am, therefore, presenting a history of one project in an attempt to describe present problems that require solution if the field is to move ahead on the applied front.

We must be aware, however, when thinking about what we've learned from history, of the ubiquity of hindsight bias. The hindsight bias is the tendency to believe mistakenly, once outcome knowledge is in hand, that one would have predicted the outcome (Hawkins & Hastie, 1990). It is akin to a perceptual illusion. This phenomenon leads to a serious underestimation of the amount of scientific and conceptual change that has actually occurred.

The hindsight bias is nicely illustrated by a quote from Allyon & Azrin in their discussion of implementing behavioural procedures in a psychiatric hospital. "First they tell you you're wrong, and they can prove it. Then they tell you you're right, but it's not important. Then they tell you it's important, but they've known it for years" (Kettering, quoted in Allyon & Azrin, 1968).

My general conclusion from the hindsight bias literature is that we're not now as smart as we think we are but that people used to be smarter than we now think they were.

Returning to the behavioral point system within the Activity Treatment Unit of Oak Ridge, the program awarded points that were accumulated weekly for off-ward and on-ward work, room and self-care, and ratings of mood and cooperation (Ellsworth, 1971). These points were accumulated weekly and the weekly net totals determined the patients' privilege levels for the next week. Points were subtracted for misbehaviors according to a fixed fine schedule from the accumulating total as well as the current total, so they could, if large enough, result in an immediate drop in privilege level and an increase in staff surveillance.
This program has been described in several sources (Harris, 1989; Harris & Rice, in press; Quinsey, 1981; Quinsey & Sarbit, 1975; Rice, Quinsey, & Houghton, in press).

This program was exceedingly difficult to implement and to keep going (Quinsey, 1981) and I will not go into all the difficulties that arose (cf. Laws, 1974). Suffice it to say that administrative support of the program was erratic and frontline staff’s commitment to the program tended to be, on average, lukewarm. I won’t try to let you know how bad it was, however, lest I sound like one of Monty Python’s "Four Yorkshiremen"; "you think you had it tough..."

Despite these difficulties, one can offer a persuasive rationale for an incentive system to manage patient behavior. For example, consider the following quotation from an early Penetanguishene annual report.

"The boys [read as "patients" throughout] should have no little share in their own self-improvement, and in order that they may be stimulated to cooperate heartily in the work, an incentive in the shape of a system of fixed rewards becomes a necessity. To that end such means as are at present available should be at once taken to ensure the establishment and perpetuation of a system of grading, whereby the boys would, from the beginning, feel that their early restoration to liberty altogether depended on the manner in which they conducted themselves and the progress made by them. In order that such a system should be thoroughly effective, certain conditions would require to be carefully observed, and the regulations governing the application of the system strictly enforced. With reference to these regulations and conditions, I would here take the liberty of offering suggestions:

1st. The opening of a well arranged and comprehensive set of grade books, wherein each boy, on his admission, should be started on a definite grade line.

2nd. That the boy should, at the outset, know the grade line on which he is started.

3rd. That he should know, month by month, the extent of his progress.

4th. That, in order that that progress be continuous, and as incentive to increased exertion, it is necessary that the boy should feel that, on his attaining a certain point on the grade book, his remission will be prompt and certain."

These arguments supporting incentive systems to manage inmate and patient behavior are as sensible and persuasive now as they were when they were written in the 1882 Annual Report of the Ontario Reformatory at Penetanguishene (Quinsey, 1982).
How about cumulative knowledge in behavioral treatment? Was the behavioral treatment program that we set up better than what was originally envisaged at the reformatory at the same site some 80 to 100 years earlier? Was it effective at all?

With respect to management issues, the program did appear to increase the fairness and consistency with which privileges were awarded and taken away by staff. There was also evidence that assaults were unlikely in locations in which the patients had to have exhibited success in the program in order to obtain access, and most likely in situations that patients were eligible to be in regardless of their privilege level (Quinsey & Varney, 1977; Harris & Varney, 1986; Rice, Harris, Varney, & Quinsey, 1989).

Quinsey and Sarbit (1975) showed that patients were responsive to small alterations in the point system that allowed patients (N = 18) to buy commissary items or rent certain privileges with their points (in the form of tokens). Increases were found in points earned for on-ward work, mood and cooperation ratings, and total points earned, but no significant differences were observed in room and self-care ratings or in fines incurred. Although the group data were quite consistent in showing orderly improvements in point earnings over a four month period, individual data indicated that only some patients were responsive to this change in contingency.

It is, of course, unclear that patients in this token economy were actually learning new behaviors and there was very good reason to believe that they were not. First, a large number of unpublished observations indicated that when additional contingencies were arranged for individual patients using the same general method as ordinarily used for awarding points in this program (e.g., tokens for particular behaviors), these behaviors increased in a step function. These all-or-none increases in what were often complex behaviors suggested strongly that the program functioned primarily as a motivational system. The fact that modeling, shaping, and chaining were seldom used in the program increased the plausibility of this interpretation.

An evaluation of this program was conducted by examining predictors of high point earnings in the 7th through 12th weeks after admission (N = 113) and the prediction of violent and general recidivism over a 6.5 year average followup period, N = 92, (Rice, Quinsey, & Houghton, in press).
Intercorrelations among mood, cooperation, room care, self care, work scores and point earnings for the second week of treatment were all .77 or higher. Number of days confined was also highly negatively correlated with other program variable predictors. Because of these high intercorrelations, only gross point earnings and number of days confined in the first two weeks were retained as predictors.

Point earnings significantly increased over the 12 week period. Points earned in weeks 7 through 12 were predicted by point earnings in week 2, less confinement in the first 2 weeks, having criminal charges leading to admission, and having been found unfit for trial or not guilty by reason of insanity. Considering only those patients who started off poorly in the program, those who improved were less likely to have been referred from another psychiatric hospital and less likely to have been married.

Length of stay in Oak Ridge (mean of 11 months and median of 4 months) was predicted by having charges leading to admission, being referred from another psychiatric hospital, having been confined more frequently in weeks 7 through 12, and having been more assaultive in the first twelve weeks in the program.

Fifty-one percent of the patients were arrested and/or readmitted to Oak Ridge. This form of failure was predicted by number of previous months in institutions, youthfulness, not having been found fit for trial or not guilty by reason of insanity, and having been referred from another psychiatric hospital.

Twenty-four percent of the patients were arrested or returned to Oak Ridge because of the commission of a violent offence against persons. Violent reoffenders had spent more total previous months in institutions, were less likely to be psychotic, less likely to have been employed prior to admission, and were more likely to have been confined during weeks 7 through 12 of the program.

The best predictor of future point earnings was past point earnings. At least half of the patients started off very well in the program and continued to do well. Patients that did not improve were most often referrals from other hospitals who were assaultive and management problems. Months until discharge was related to program variables but added little to preadmission predictors. Men found not guilty of insanity or unfit for trial did better in the program but were held for longer periods of time. Program variables were very weakly related to general or violent recidivism.
There are a variety of cogent explanations for these very disappointing results. First, there are issues of treatment design: The program seldom involved immediate tangible reinforcements; the program was neither individualized nor focussed on the risk factors or "criminogenic needs" of the patients, and, in fact, the contingencies did not even make contact with the behavior of almost half of the patients (who were performing well from the outset).

Second, there were problems concerning program intensity, integrity, and fidelity of implementation. Although inter-rater reliabilities were quite acceptable for most of the rated behaviors, their high intercorrelations indicated that they all reflected either a halo phenomenon or a measure of general psychiatric disturbance. Inter-rater reliabilities on the mood and cooperation ratings were variable. These data speak to the lack of specificity of measurement and thus, reward. Although the program was in effect continuously, it was only targeted at a small proportion of the patients' behaviors, particularly those involving compliance and security issues.

Harris and Rice (1990) documented ever increasing fines and increasing numbers of punishable acts specified by this program over a 15 year period, despite strenuous efforts of psychology staff to resist these trends. The increasing punitiveness of the program was correlated with increasing assaultiveness of the patients. In general, it appeared that the clinical utility of the program was eroded over time by drift from behavioral principles of treatment.

At least one of the reasons for the difficulty in developing and maintaining a more therapeutic behavioral ward program is the faith that treatment staff had in medication as the preferred and perhaps only form of treatment. Harris (1989) has documented in detail that continuous adjustments of phenothiazines did not affect these patients' progress (that is, patients either responded quickly to the medication or not at all) but occupied a great deal of staff attention.

I have outlined this series of studies at some length for several reasons. First, these data are of interest in themselves because they are obtained from a 15 year programmatic effort involving a more systematic approach to the treatment of violent patients than is found in many psychiatric hospitals. Second, these studies illustrate the single most important problem in the behavioral treatment literature: Program integrity. We simply must come to grips with the organizational, ideological, and staffing problems that interfere with the delivery of effective treatment.
Third, this history shows the potential scientific costs of evaluating treatment programs that lack fidelity of implementation. From the data I have described, one might be tempted to conclude that we, in fact, are no further ahead than we were in 1882, and that behavioral approaches to violent psychiatric patients simply do not work. Nothing could be further from the truth.

Paul and Lentz (1977) have shown the surprisingly large effects that a token economy program can have when issues of implementation have been thoroughly and rigorously dealt with. The treatment effects found in this investigation were so large and reliable that they swamped individual differences of the kind found to predict program success and recidivism in the Oak Ridge studies.

Our applied task in the future, therefore, is not only to develop more effective treatments for violent persons but to develop the political support and the organizational capacity to deliver them effectively. This task will require scientist-practitioners who provide treatment to work in the settings and programs they evaluate (Rice & Quinsey, 1986).

With respect to program development, it is apparent from this micro-history that treatment programs must be targeted at the specific relevant problems of individual violent offenders. Programs can thus be designed for types of offenders based upon their problem distribution. The identification of offender types through cluster analyses of problem data and the development of treatment programs based on these patient clusters has proceeded at Oak Ridge (Rice & Harris, 1988; Rice, Quinsey, Harris, & Cyr, in press) and is being extended throughout the Ontario system for mentally disordered offenders.

In conclusion, a good deal of empirical and conceptual progress has been made in the past two decades, although the application of scientific progress to institutional treatment has been much slower. We know enough now to sharpen the scientific questions regarding violent offenders and have identified the principal barriers to implementation of treatment programs.

Doubtless, the research and evaluation activities of the nineties will continue to be as interesting and challenging as they have in the past. It is because scientific progress is not inevitable and its direction unclear that we remain motivated to ask questions concerning violence and interested in not only the answers but the implications these answers have for making the world a safer place. I will close with a poem about science and its applications.
Paradox
Not truth, nor certainty. These I foresaw
In my novitiate, as young men called
To holy orders must abjure the world.
"If... then..." this only I assert;
And my successes are but pretty chains
Linking twin doubts, for it is vain to ask
If what I postulate be justified.
Or what I prove possess the stamp of fact.
Yet bridges stand, and men no longer crawl
In two dimensions. And such triumphs stem
In no small measure from the power this game,
Played with the thrice-attenuated shades
Of things, has over their originals.
How frail the wand, but how profound the spell!


References:


