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SEEKING ENLIGHTENMENT ON THE DARK SIDE OF PSYCHOLOGY

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A life devoted to the study of antisocial behavior and anomalous sexual preferences is examined. Although successive ontogenetic intellectual metamorphoses have yielded theoretical and applied progress, future reincarnations will be necessary for the achievement of full enlightenment.

Key words: *forensic psychology; violence; sex offenders; risk appraisal; forensic psychiatric institutions; autobiography*

IN 1971, I LEFT academe, biopsychology, and the study of aversive conditioning (Quinsey, 1970, 1971, 1972) to accept a psychology staff position at the Oak Ridge maximum security psychiatric facility in Penetanguishene, Ontario. I made this move from a postdoctoral fellowship at Dalhousie University because I wanted to study aggression and to “do good” by applying scientific knowledge to important but understudied applied problems (my former PhD advisor, on the other hand, more simply considered a move to a remote “prison” to be a form of academic suicide). However, Oak Ridge seemed ideal for my purposes because institutional violence was a serious issue there and involved observable, relatively frequent, and serious aggressive behaviors. In addition, decisions to release serious offenders held on an indeterminate basis offered the opportunity to apply techniques developed by psychologists and statisticians to improve prediction. The

superintendent of the hospital, Dr. Barry Boyd, was fond of remarking that he knew that half his charges were not dangerous but did not know which half. When I arrived at Oak Ridge, I was informed that the single most difficult problem for the clinical staff was in deciding how to treat and when to release sex offenders, particularly pedophiles.

I thus had a research agenda—institutional violence, prediction of violent recidivism, and the assessment and treatment of sex offenders. Agenda notwithstanding, my primary initial responsibilities involved developing on-ward behavioral programs for a heterogeneous group of forensic patients, many of whom exhibited a good deal of institutional aggression and misconduct. My previous training, initial reading of the literature, and experience of the implosive or flooding variety¹ in the grim, barren, and claustrophobic Oak Ridge forensic psychiatric setting (not to mention my

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juvenile history of minor delinquency) provided me with a set of beliefs and hypotheses about each of the items on my agenda. As it turned out, virtually all of my ideas and beliefs were wrong, sometimes not totally wrong, but wrong nonetheless. In high school, I had dismissed Nietzsche's aphorism that the "sign of a great man is change" on the grounds that it was possible that a man might have had the correct idea in the first place. Given that I have changed my mind about so many things in my scientific career, I am now inclined to believe that Nietzsche was right (the astute reader will have noticed that my current opinion about this aphorism provides further evidence of its veracity).

Despite being proven wrong on so many occasions, I persisted in following my research agenda. My ability to conduct a program of research was in part made possible by a long series of research grants from the Ontario Mental Health Foundation; this support was critical because I was not eligible to apply for funds from the major federal granting agencies because I did not hold an academic appointment. Of course, I was also supported by my home institution, although management was usually much more interested in my administrative and clinical service than in my research. Over considerable bureaucratic resistance, I established and became the first director of the Department of Research at the Penetanguishene Mental Health Centre in 1975 and began a collaboration with department staff and the two subsequent directors of the research department, Marnie Rice and Grant Harris, that has lasted until the present. In 1988, I returned to academe by accepting a professorship at Queen's University, Kingston; my collaborators then expanded to include my highly capable graduate students, many of whom have gone on to establish successful research careers.

The following review focuses almost exclusively on research with which I have been directly involved—it largely ignores the distributed and gargantuan scientific and clinical effort of which my work is a tiny part. Still, I have been laboring in the dark side of psychology for more than 35 years, so it is not unreasonable to consider whether I have learned any

lessons that might help younger colleagues and to assess the degree to which I remain unenlightened.

INSTITUTIONAL VIOLENCE

I at first focused on individual highly assaultive patients, conducting operant studies² of inhibitory deficits and designing individualized treatment programs, such as a social skills game that could be used with low-functioning patients (Quinsey & Varney, 1977b). Early on, I discovered that hospital records provided serious underestimates of institutional assaults, and I designed a method to record them through daily interviews of patient assailants and staff and patient victims and witnesses (Quinsey & Varney, 1977a). The results of this more accurate tally of assaults were unexpected and changed the direction of our research. Institutional violence was related to psychopathology, as expected—a small number of the lowest functioning patients committed the majority of assaults. However, it turned out that psychopathology could not provide a complete explanation. First, assaults were highly lawful, occurring differentially in certain locations and at certain times. Assaults were rare in off-ward work areas because patients had to exhibit a period of stable behavior to be eligible to attend, but assaults were common on wards in situations where patients mingled together and were not engaged in structured activities. Second, the targets of assaults were not randomly chosen—attendant staff were much more likely to be victims than were patients, and some staff were much more likely to be assaulted than other staff working on the same shifts and wards. It soon became clear that certain staff members were likely to be assaulted because they lacked interpersonal skills or were authoritarian and domineering. Patient respondents often cited staff teasing, bullying, or abuse as the reason for an incident, whereas staff usually attributed the assault to no apparent reason or the patient's psychotic condition. These results led to a series of studies, culminating in the development of a week-long staff training program involving the didactic teaching of security procedures, coaching and

role-playing to teach de-escalation skills, and gymnasium practice of restraint techniques and self-defense. An extensive quasi-experimental evaluation involving interventions implemented at different times on different wards showed the program's effectiveness in improving patient morale on the wards, changing staff behaviors, and reducing work days lost because of staff injury (Rice, Harris, Varney, & Quinsey, 1989).

A related series of studies consisted of somewhat sobering evaluations of ward-based token economies. These evaluations involved follow-up studies demonstrating that success in the program was unrelated to subsequent recidivism and studies of in-program performance showing that the most successful patients were those who had performed well initially (i.e., the program functioned to identify patients who were doing well rather than to improve their behavior). In studies of individual patients in which we made tangible rewards contingent on particular targeted behaviors, improvement occurred in an all-or-none fashion, suggesting that the contingencies acted in a motivational rather than an instructional manner. Our behavioral programs were successful in contributing to rational and more humane patient management but not in effecting therapeutic change.

We learned later that highly sophisticated and meticulously implemented behavioral programs of the kind developed by Paul and Lentz (1977) were required to achieve the treatment effects we sought; in the end, we failed to develop behavioral programs of high enough quality to achieve them. However, our evaluations of behavioral programs eventually led to the development of methods to classify patients according to their individual profiles of clinical problems and to methods of relating these profiles to institutional programs so as to facilitate overall treatment system planning (Quinsey, Cyr, & Lavalley, 1988; Rice, Harris, Quinsey, & Cyr, 1990). These new methods were developed because our studies (e.g., Quinsey & Maguire, 1983) had revealed that clinicians showed poor agreement about how successfully individual patients could be treated (although they tended to view psychotic

patients as potentially treatable with medication and personality-disordered patients as less treatable using any method) and because our bureaucratic experience showed that administrators and clinicians lacked a principled method to organize clinical services.

Our method began with clinicians identifying and rating the severity of problems, from a lengthy list, that each individual patient exhibited, a factor analysis of these problems, and a cluster analysis to assign patients to groups based on their pattern of problems. This method was used to capture the clinically relevant characteristics of all the patients in the institution at a point in time. Because programs are delivered to individuals with problems rather than to problems per se, because some problems have security implications (such as assaultiveness) and some do not (such as paraphilic interests), and because programs for some kinds of problems are ward based (e.g., token economies) whereas others can be delivered in programs situated in nonliving areas, these analyses led naturally to an institutional organizational scheme.

RISK APPRAISAL

By the mid-1970s, there was an ambivalent, if not schizophrenic, attitude toward the prediction of violent behavior. On one hand, many forensic clinicians endorsed the proposition that "of course, one can't predict dangerousness" and that the base rate of violent recidivism was probably too low to permit meaningful risk appraisal. At the time, both of these beliefs seemed to be supported by the widely publicized results of the Baxstrom study of offenders released from a forensic psychiatric facility in New York State because of a judicial decision (Steadman & Cocozza, 1974). These beliefs notwithstanding, however, forensic practice remained much as it had for many years—forensic clinicians would interview an offender, review the offense description and history of institutional conduct, read the psychological testing reports, and come up with a qualitative opinion about risk. Risk appraisal based on clinical judgment was a fundamental part of the work undertaken by psychiatrists and

other forensic clinicians, and there were no feasible alternatives in sight.

I started a long series of follow-up studies of violent recidivism in the early 1970s.³ These studies appeared to confirm low rates of violent recidivism, particularly among psychotic offenders. In addition, we conducted a study of clinical judgment that profoundly affected the course of our work on prediction. Quinsey and Ambtman (1979) showed that experienced forensic psychiatrists appraising risk from précis of clinical files showed poor agreement among themselves but on average made the same risk appraisals as high school English teachers and based their judgments on the same features of the file (the nature of the index offense and the offender's prior history). Postadmission assessment data (psychiatric and psychological reports, nursing notes on institutional conduct, etc.) had no effect on the appraisals of either psychiatrists or teachers. These findings were later replicated with other mental health professionals (Quinsey & Cyr, 1986) and in a prospective release study showing that unaided clinical prediction by institutional forensic clinicians continued even when more valid actuarial methods were available (Hilton & Simmons, 2001).

I had begun studying clinical judgment mostly ignorant of the already old and large literature demonstrating the superiority of simple actuarial instruments over clinical judgment in the prediction of pretty much anything. Kelly and Fiske (1951), for example, found in an elaborate study that simple and inexpensive measures outperformed complex ones in predicting the success of clinical psychology trainees. This kind of finding had been perennially contested and refuted over decades. Although I had expected that clinical judgment would be far from perfect, it had not occurred to me that there would be absolutely no evidence of any clinical expertise in risk appraisal. This is not to assert that clinical judgment is random or completely useless in the prediction of violence, just that it was likely to be less accurate than actuarial prediction and that clinical training or professional education was irrelevant.

Still, it looked as if even meaningful actuarial prediction of violent and sexual recidivism was impossible because of the low to moderate

correlations between actuarial predictors and outcome and the low base rate of violent recidivism. However, accumulating information soon began to indicate that the base rate problem was not as intractable as it had appeared. It was shown mathematically, for example, that base rates quickly rose among offenders who had been repeatedly passed over for release when held under indeterminate conditions, even when the accuracy of risk appraisal was quite modest (Quinsey, 1980). These calculations subsequently received empirical support in follow-up research (Quinsey & Maguire, 1986). Still further follow-up studies demonstrated that base rates of violent and sexual reoffending were high enough in a wide variety of offender populations to permit useful prediction when the follow-up period was long enough. The last impediment to a successful prediction enterprise was removed with the identification of an appropriate measure of predictive accuracy (the Receiver Operator Characteristic, or ROC) that could replace the sometimes misleading measures of association, such as correlations, that investigators had been using (Rice & Harris, 1995). Use of the ROC statistic permitted the empirical demonstration that actuarial instruments, such as the Violence Risk Appraisal Guide (Harris, Rice, & Quinsey, 1993), produce large effect sizes and were accurate enough to be used in making dispositional decisions based on risk.

Further follow-up studies replicated our findings on the accuracy of actuarial prediction of violent recidivism and generalized them to new populations, such as federally sentenced sex offenders, developmentally handicapped individuals, and civilly committed psychiatric patients (Harris et al., 2003; Quinsey, Harris, Rice, & Cormier, 2006). Lastly, we investigated the measurement of variations in short-term risk occasioned by phenomena that change during periods in which there are opportunities to reoffend (Quinsey, Jones, Book, & Barr, 2006; Zamble & Quinsey, 1997); the dynamic predictors found in this research can be used in the ongoing management of the risk posed by supervised offenders. Our program of research on risk appraisal is more fully described in Quinsey, Harris, et al. (2006).

It is clear that actuarial assessment methods for appraising the long-term risk of recidivism are currently more accurate than any other method that has been tried. Although there are variations in the accuracy of the many actuarial instruments that have been devised, such as the Statistical Information on Recidivism Scale (Nuffield, 1982) and the Static-2000 (Hanson & Thornton, 2000), it is generally true that they all correlate positively with one another (variations in accuracy are partly related to how accurately the items in them are scored and partly to the specific outcomes the instruments are designed to predict). And, although it seems that any reasonably diverse set of individual (as opposed to aggregate) correlates of crime can be used, those relating to early criminal or aggressive behavior, such as age at first arrest, and those relating to the pervasiveness of antisocial behavior, such as the Psychopathy Checklist-Revised (Hare, 2003), are among the most useful.⁴

Research on risk appraisal and treatment program efficacy led naturally to a consideration of the nature of individual differences in antisocial propensities. Among the most important individual differences relating to both risk of reoffending and resistance to extant methods of treatment is psychopathy. We have performed a number of investigations of psychopathy (e.g., Belmore & Quinsey, 1994; Book, Quinsey, & Langford, 2007) and have considered psychopathy in the context of Darwinian life history theory (Barr & Quinsey, 2004; Seto, Khattar, Lalumière, & Quinsey, 1997). Our group found evidence that items from the Psychopathy Checklist-Revised (Hare, 2003), items formed from the criteria for antisocial personality disorder from the *Diagnostic and Statistical Manual of Mental Disorders* (American Psychiatric Association, 1994), were taxonic (Harris, Rice, & Quinsey, 1994; Skilling, Harris, Rice, & Quinsey, 2002)—thus, highly antisocial individuals are different in kind rather than degree from others. The identification of a natural class, or taxon, has potentially important implications for the identification of the etiology of this condition.

Advances in theory raise questions pertaining to the relationship between actuarial

prediction and theories of violent or sexual offending. The relationship is not as straightforward as it first seems. An actuarial method simply involves making probabilistic predictions based on outcomes from previous cases. But the items of an actuarial instrument need not be atheoretical. One could include only items that have a demonstrably causal relationship to criminal behavior.

At present, theories in psychology, even if well developed and consilient with more advanced theories in the harder sciences, seldom yield numerical predictions regarding the behavior of individual subjects. With very few exceptions, psychological theories are verbal and relative (they relate to qualitative, ordinal, or relative probabilistic outcomes); they do not predict absolute numbers of anything. Psychological theories require optimizing or “actuarializing” in order to generate numerical probability estimates. Actuarial instruments for the prediction of violent or sexual recidivism over a period of years achieve ROCs above .80 under optimal conditions (Harris et al., 2003). Because outcomes are imperfectly measured in recidivism research, the ceiling for accuracy is far less than perfection, with the consequence that it is unlikely that new methods of prediction of long-term outcome, regardless of how they are developed, will result in greatly improved accuracy.

On the other hand, causal theories are attractive because they may suggest interventions to reduce the likelihood of an individual engaging in criminal behavior. Indeed, program evaluation research can serve to test causal theories of criminal recidivism. These uses of causal theories, however, are quite different from those of actuarial prediction models and do not depend on a precise estimate of an individual’s risk.

SEX OFFENDERS

First, some background about the past zeitgeist concerning the nature of sexual behavior. Kinsey, Pomeroy, Martin, and Gebhard (1953) asserted that

learning and conditioning in connection with human sexual behavior involve the same sorts of processes as learning and conditioning in other

types of behavior. But man, because of his highly developed forebrain, may be more conditionable than any of the other mammals. . . . The sexual capacities which an individual inherits at birth appear to be nothing more than the necessary anatomy and the physiologic capacity to respond to a sufficient physical or psychologic stimulus. All human females and males who are not too greatly incapacitated physically appear to be born with such capacities. . . . But apart from these inherent capacities, most other aspects of human sexual behavior appear to be the product of learning and conditioning. . . . The type of person who first introduces an individual to particular types of sociosexual activities may have a great deal to do with his or her subsequent attitudes, his or her interest in continuing such activity, and his or her dissatisfactions with other types of activity. (pp. 644-645)

In sharp contrast to Kinsey et al.'s *tabula rasa* view of the development of sexual interests, Kurt Freund (1967) was inspired by European ethological and nativist theories, leading him to a deep pessimism about the prospects of treatment for altering anomalous sexual preferences. Freund developed the phallometric method of measuring sexual preferences for partners of different ages or sex. Variants of this method were extremely successful in differentiating homosexual from heterosexual men, sex offenders against children from men who preferred adults, and sex offenders against children among themselves according to their histories of victim choice. The ease of replication and magnitude of effects were remarkable and often provided information at variance with offenders' self-reports (Quinsey, Steinman, Bergersen, & Holmes, 1975). Later, phallometric assessment methods were developed that successfully used audiotaped stimuli both for rapists (Quinsey, Chaplin, & Varney, 1981) and for sex offenders against children (Quinsey & Chaplin, 1988a), new methods were developed to eliminate faking (Quinsey & Chaplin, 1988b), and covertly measured viewing time was developed as an alternative method of measuring sexual preferences (Harris, Rice, Quinsey, & Chaplin, 1996; Quinsey, Rice, Harris, & Reid, 1993). Although assessment of sexual age and gender preferences was never controversial, there was a continuing controversy about the ability of phallometric methods to discriminate

rapists from nonrapists. This controversy was eventually laid to rest with a meta-analysis demonstrating the discriminant validity of phallometric assessments (Lalumière & Quinsey, 1994; Lalumière, Quinsey, Harris, Rice, & Trautrimas, 2003).

Phallometric technology appeared to offer an objective way to assess treatment needs and therapeutic progress. British psychiatrists Marks and Gelder (1970) published a series of case studies of fetishists and transvestites, showing spectacular changes in phallometrically measured sexual interest as a function of aversion therapy. These methods were then applied to homosexual men, pedophiles, and (subsequently) rapists. By the early 1970s, many treatments for sex offenders were inspired by the view that sexual interests were learned and malleable and could be measured with phallometric technology and altered with standard classical conditioning techniques using electrical aversion therapy or operant techniques involving a signaled punishment procedure. I shared these views and was optimistic about the prospects for treatment.

In my laboratory, pre-post measures of changes in sexual preference looked quite promising in that procedures such as signaled punishment used in aversion therapy reliably decreased sexual responding to deviant cues (Quinsey, Chaplin, & Carrigan, 1980). Nevertheless, difficulties soon became apparent. First, the actual parameters of aversion therapy, such as shock intensity, were insufficient to produce conditioning in human subjects, bringing a straightforward theoretical account of the changes occasioned by such therapy into doubt (Quinsey & Varney, 1976). More importantly, however, our follow-up studies of treated sex offenders against children revealed no effect of treatment on recidivism, despite promising pre-post comparisons on various treatment targets such as phallometrically measured sexual deviance (Rice, Quinsey, & Harris, 1991). Indeed, it appeared as if pretreatment phallometric assessment was more closely related to outcome than was posttreatment assessment. It is important to understand that the evaluation of the Oak Ridge treatment program for sexual offenders against children

found an effect on recidivism in the wrong direction, not a small effect of treatment that failed to reach statistical significance because of insufficient power. This finding was replicated in an evaluation of the Regional Treatment Centre Sex Offender Treatment Program at Kingston Penitentiary with a larger and more heterogeneous sample of sex offenders (Quinsey, Khanna, & Malcolm, 1998).

Meanwhile, treatment programs for sex offenders continued to become more complex and eclectic. Our program added heterosocial skills training as a treatment modality on the grounds that men with anomalous sexual age preferences who could establish relationships with appropriately aged partners might learn to prefer or at least tolerate them as sexual partners and that sexually aggressive men might learn to behave more appropriately (e.g., Whitman & Quinsey, 1981); we also added sex education with an emphasis on values and a pretreatment problem identification group program (Quinsey, Chaplin, Maguire, & Upfold, 1987). Other sex offender treatment programs also added a variety of psychotherapeutic or evocative treatments, such as empathy training, dealing with victimization, and so forth. It now seems incongruous that prior to and during the time these changes in treatment programs for sex offenders were being effected, treatment to modify homosexual preferences among men were first restricted to "ego-dystonic" homosexuals and then largely abandoned when homosexuality was removed as a diagnosis from the American Psychiatric Association's *Diagnostic and Statistical Manual*. The new consensus was not only that it was unethical to attempt to modify homosexual preferences but that it was impossible as well (the latter a belated confirmation of Kurt Freund's long-held opinion).

The augmented sex offender programs did not produce large treatment effect sizes in follow-up evaluations, and the efficacy of treatment for sex offenders in reducing recidivism remains moot (e.g., Rice & Harris, 2003). Notably, the best controlled evaluation of sex offender treatment to date (Marques, Wiederanders, Day, Nelson, & van Ommeren, 2005) found no reduction in recidivism to

result from a well-implemented state-of-the-art treatment program for sex offenders.

In sum, most of the initial assumptions of investigators and clinicians (including myself) regarding sexual anomalies were not confirmed by subsequent work. Sexual age and gender preferences do not appear to be learned and malleable (e.g., our attempts to increase sexual arousal of normal subjects to slides of women through Pavlovian conditioning by pairing the slides with highly arousing videotapes were vitiated by habituation; Lalumière & Quinsey, 1998). Although sexual age and gender preferences can be measured with phalometric technology (for reviews of the assessment and treatment literature on sexual offenders against children, see Camilleri & Quinsey, in press; Quinsey & Lalumière, 2001) and responses to deviant categories can be reduced with standard conditioning techniques, these alterations now appear not to involve the preferences themselves but only their measurement. Fifty years after Kinsey et al. (1953) wrote the passage quoted at the beginning of this section, it appears that the role of learning in the development of sexual age and gender preferences is limited or nonexistent (for a review, see Quinsey, 2003). Particularly important in this regard was the demonstration of the fraternal birth order effect—the finding that men were more likely to have homosexual interests the more older brothers they had (Blanchard & Bogaert, 1996) and the subsequent demonstration that only brothers born to the same mother (whether they were known to the subject or not) were responsible for the effect (Bogaert, 2006). The fraternal birth order effect provided compelling evidence for a neurohormonal explanation of homosexual sexual preferences (and, by extension, for a similar mechanism in other anomalies of sexual preference) over and above that provided by genetic studies. These empirical results led my colleagues and me to fundamentally reconsider theories of sexual anomalies and sexual coercion. I had accidentally acquired Symons's (1979) book on human sexuality, and it first inspired us to frame questions concerning the etiology of anomalous sexual preferences in terms of the theory of

natural selection (Quinsey & Lalumière, 1995). We have since developed these theories further and applied them to sexual coercion (Harris, Rice, Hilton, Lalumière, & Quinsey, 2007; Lalumière, Harris, Quinsey, & Rice, 2005).

Although a Darwinian approach now seems to me to provide the best set of overarching theoretical concepts within which to understand sexual behavior, I was slow in adopting it because of the major conceptual shift that was required. Because it is so frequently misunderstood, I will summarize some of the main points in the next few paragraphs.

One of the key ideas in an evolutionary theory of sexual behavior is that human male and female reproductive interests are correlated but not identical. There is, therefore, potential and real sexual conflict in humans at the genomic and behavioral levels, as in most other sexually reproducing species. Sexual coercion can therefore be situated in the broader context of sexual conflict caused by sexually dimorphic reproductive strategies: in particular, men's greater interest in partner novelty and women's interest in male investment in relationships and parental assistance (e.g., Landolt, Lalumière, & Quinsey, 1995). As noted earlier, greater male mating effort, risk acceptance, and dominance striving is related to greater male than female variance in reproductive success.

Because sexual behavior and interests have been shaped by reproductive success in ancestral environments, rape is expected to be directed at reproductively relevant targets and involve reproductively relevant behaviors (Quinsey, 2003). Anything that causes men to disregard the preferred mating strategies of women is expected to increase the likelihood of rape. Anthropological, historical, and psychological evidence suggests that warfare, alcohol intoxication, psychopathic personality characteristics (Harris et al., 2007), misogynist attitudes, and hyperdominant or sadistic sexual interests (Lalumière et al., 2003) contribute to rape. Perhaps unexpectedly, men who perceive themselves as highly successful with women are more likely than other men to engage in date rape, presumably because if a current dating partner breaks off their relationship

because of sexual coercion other partners are readily available (Lalumière et al., 1996).

Although women show greater preference than men for traits in sexual partners associated with long-term mating strategies and parental investment, such as resources and status, and less in partner novelty, they nevertheless vary in their degree of interest in casual relationships (e.g., Landolt et al., 1995). Provost, Kosakoski, Kormos, and Quinsey (2006; Provost, Troje, & Quinsey, 2008) found that women using short-term mating strategies appear to prefer genetic over parental contributions of mating partners more than women using long-term mating strategies (in ultimate terms because short-term partners were unlikely to have made parental contributions in ancestral environments).

Although men can interfere with women's reproductive strategies through sexual coercion, women can interfere with men's reproductive strategy of paternal investment through cuckoldry. On average, ancestral men who invested in children who were unrelated to them were less reproductively successful than men who invested only in their own children. Because there is evidence for genetic contributions to female infidelity and cuckoldry is not extremely rare, men may well have developed psychological adaptations to the threat of it. Volk and Quinsey (2002) showed for example that men but not women are more willing to adopt babies that they believe resemble them. In an offender sample, Camilleri and Quinsey (2007; Camilleri, Quinsey, & Tapscott, in press) found that partner rapists experienced more cuckoldry risk events prior to committing their offenses than did non-sexual partner assaulters, supporting the idea that men in committed relationships may use sexual coercion in response to cues indicating sperm competition. In a community sample, direct cues of infidelity predicted self-reported propensity for partner-directed sexual coercion.

ENLIGHTENMENT?

Enlightenment is evidently an ongoing process. Who would have guessed 35 years ago that individual differences in antisociality

would have substantial genetic loadings (much less that interactions between particular genes and childhood environmental influences would be identified), that the influence of shared family environment (such as social class) would have negligible effects on most traits of interest to psychologists, that cumulating evidence would suggest so strongly that adult sexual orientation was a result of neurohormonal influences in utero, that the effects of behavior modification programs would depend so completely on precision of implementation, that sex offender treatment programs would have such small effects on recidivism, and that empirically developed actuarial models would still, in 2008, be outperforming not only clinical judgment as in the past but also theoretically informed methods of predicting violent recidivism?

By induction, we may infer that many of the things we now believe to be true will be shown to be either incomplete or plain wrong. But scientific change creates problems for social policies and clinical practices based, even loosely, on past scientific conceptions. These problems are exacerbated because such change is almost always contested. It is not usually clear to bureaucrats, practitioners, and sometimes researchers that the field has actually changed until long after the fact. The most difficult situation in the applied area is one in which scientific evidence shows that a practice is ineffective but does not show how it might be improved.

Moreover, "doing good" is harder than it looks. The application of scientific findings to social policy and clinical practice is seldom straightforward. Not only is the science of varying quality and seldom directly on target (unless one does it oneself), but its interpretation is affected in important ways by bureaucratic practice, professional guilds, and the changing priorities of advocacy and ideological groups (cf. Weiss & Bucuvalas, 1980). For applied researchers, there is always the possibility of alienating one's practitioner or social policy constituency by producing data at odds with prevailing beliefs and practice. Naturally, those who deliver services cannot be expected to embrace findings implying that what they have spent their careers doing is ineffective, much less to embrace the bearer of such findings (for an extensive historical illustration, see Debré,

1998). As a reviewer remarked when recommending rejection of a literature review I had submitted—"There's something here to offend just about everyone."

Turning to basic as opposed to applied science, I was originally trained as a behaviorist, albeit in a biopsychology program. Very slowly I became convinced of the limitations of purely behavioral and, more broadly, psychological explanations of behavior, as admirable as some of these are. I changed my mind mostly because theories of behavior appear to advance more quickly the more they are consistent with more advanced areas of science—that is, rather than remaining at the behavioral or cognitive level, involving concepts and methods from genetics, neuroscience, and the new science of development for proximal explanations and from Darwinian theory for ultimate explanations. Psychology, I now believe, is most usefully considered to be one of the life sciences.

It really is dark on the dark side of psychology (dealing with violence and sex offending is not for everyone). Although mountains of data must be examined to find out anything (even then, it is easy to be mistaken) and conclusions are always provisional, it is still usually easier to make policy-relevant scientific findings than it is to use these findings to effect changes in policy and practice. For all the difficulties, however, it should be clear from the foregoing that we have made some theoretical progress not only in understanding antisocial behavior and sexual anomalies but also in applied areas, such as risk appraisal. Research on the dark side is a worthwhile and often rewarding endeavor, as well as a subject of endless intellectual fascination. Nevertheless, many of the most important rewards in my career have come more from my companions on this intellectual journey than from the journey itself.

I look forward to future reincarnations in order to learn more.

NOTES

1. Oak Ridge was designed with no thought that there would be professional staff in any numbers. When hired, I was the only psychology staff member in the 300-bed institution. My tiny shared office was on an admission ward. One worked trying to ignore upset patients endlessly banging the metal doors of their cells. This was a very energetically cheap behavior—patients

needed only to face backward and strike the door with their heels, making the door slam into the track in which it slid; the ward acted as an echo chamber. After windy nights in winter, I would sometimes arrive at work to find snow on my desk. The 150-bed unit to which I had been assigned was riven with controversy about whether "programs" should be instituted. The attendant union was largely against these practices. There were, however, advantages to working in a rural setting. For example, in summers during the early 1970s, I would canoe across the narrow mouth of Penetanguishene Bay to get to work (for reasons that I cannot now fathom, I would tie my life jacket to my briefcase). In winter I would snowshoe, but in late fall and early spring it was more than a half hour (long!) drive to work. Oak Ridge was extensively renovated over the years; the research department's current quarters owe their existence to Dr. Malcolm MacCulloch, a British psychiatrist who was medical director in 1987.

2. Shortly after I started work at Oak Ridge, Ray Berry, the advisor for psychology in the provincial government, alerted us to the existence of a fund created from money that the provincial psychiatric hospitals had failed to spend (these were apparently the good old days!). In true bureaucratic fashion, I was given a couple of days to create a list of equipment to buy. I was surprised to have my complete wish list approved, and I shortly set up an operant lab and a psychophysiological lab, both controlled by electromechanical equipment. My relay racks remained in service long after the rest of the world had adopted computers. However, we eventually switched to computers, rendering one of my few skills totally obsolete.

3. Over time, these studies involved increasingly large data sets. At first we could perform the analyses by hand, especially when I became the proud owner of an electronic calculator that could cumulate sums of squares. Later we had our data punched into computer cards in Toronto. I contracted a computer scientist at York University who would run our analyses on the mainframe there.

4. These observations explain the commonly made finding that murderers are not as a group the individuals most likely to commit postrelease violent offenses. For example, men who have committed spousal homicides often score low on actuarial predictors of violent reoffending and show profound deficits in psychometric and behavioral measures of assertion (Quinsey, Maguire, & Varney, 1983).

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