



Essay Review

Behold the Paradigm Shift!

By

Leif Edward Ottesen Kennair

A review of *Myths of Childhood* by Joel Paris.
New York: Brunner-Routledge, 2000.

*Nullius addictus iurare in verba magistri
(I am not bound to believe in the word of
any master)*

-- Horace (First Century BC)

These words set the stage for Joel Paris' book *Myths of Childhood*, dedicated in part to his teachers, who he claims to have spent a lifetime trying to prove wrong. In science that would not be so bad – in science that is the name of the game. Within the authoritarian, almost totalitarian, field of psychiatry this is most irregular. There are even theories that may describe this as pathological. These theories are, of course, based on misunderstanding or misreading, not on science (Scalise Sugiyama, 2001).

In my recent review of La Cerra and Bingham's (2002) *Origin of Minds* (see Kennair, 2003), the major obstacle to finding their theory both new and convincing was not that they were critical of mainstream Evolutionary Psy-

chology. Rather it was the fundamental lack of novelty and too obvious foundation on old, mythical psychology – the old paradigm of psychology. This old paradigm has only recently become visible. The new empirical research, with at least a part of its base in biological disciplines – such as behavioural genetics and evolutionary psychology, but also other disciplines such as cognitive neuroscience and psychopharmacology – is revolutionising our understanding of human nature, brain-mind processes and mental disorder.

Steven Pinker (2002; see Kennair, 2002b for a review) provides the synthesis of the old paradigm by showing how powerful the traditional ideas of romanticism, empiricism and dualism are within psychology and all areas of research that attempt to describe or understand human nature. This view is paralleled in Paris' *Myths of Childhood* and in Kennair et al. (2002) within psychiatry and clinical psychology. The old truths of human nature – like the claim that

the normal development of normal personality usually is dependent on what normal parents do (Psst! It isn't, you know...) – are coming under serious attacks from sciences like behavioural genetics, evolutionary psychology, cognitive neuroscience and psychopharmacology.

I teach the behavioural genetics of normal personality at the Norwegian University of Science and Technology. When I get to address the students they have already waded through the typical approaches – theories based on psychodynamics, humanistic psychology and learning psychology. All of these have a major focus on parents, not peers nor adults outside the family. Consequently, what I come along and claim is that all they have been taught to believe in the previous lectures is wrong, and contrary to the other theories “claiming” to be true and being easy to believe (as they are congruent with the current cultural myths of mind), behavioural genetics is based on empirical research. And – most importantly empirical research that tests both the genetic and environmental influence! But, it is *not* easy to believe...

For years we have focused on different “schools” of psychology – and been taught how these differ. Well, actually they did not differ on some very crucial ideas... ideas that have proven to be false. I believe it is quite fair to talk about nothing less surprising than a classical Kuhnian shift of paradigm within current psychology (Kuhn, 1996). Some very important and never-before challenged ideas are being toppled by new research and improved methods, which are making human nature ever more accessible to natural scientific study. A cluster of such ideas are the ‘myths of childhood’.

What is the Myth of Childhood?

It is not news – everyone knows it: give me the child before age 7 or age 5 and I will give you the man, the pathology or the believer. But is it true? Of course it is true – it is both common sense and scientific fact! Wrong. It may be common belief, but it is not scientific fact. All the same this idea forms the basis of most professional advice on parenting and creates the

basis of most clinical psychological or psychotherapeutic work. And it is merely a myth.

Now, as Paris states, most academics that are placed in a situation where one of their pillars of their understanding of human nature – and their own life narrative, no less – is challenged, are quick to point out that: “Absence of evidence is not evidence of absence”! Probably many of you have already come to that conclusion. Well, that is true, of course. But, that would only be relevant if the research had not been done. Scientific inquiry has been made – the results are negative, so far. Indeed, arguing from the void is not an unusual tactic, but it is an increasingly ingenious and tedious approach to lack of evidence for what most professionals found their careers and practice on.

The myth of childhood includes the idea that childhood adversity is the cause of adult mental disorder or distress. It includes the idea of psychological determinism and the idea of primacy – that the earlier the trauma or adversity the graver the adult disorder. It is also closely tied to beliefs that the quality and style of parenting determines both psychological disorder as well as normal personality development. Most psychologists, psychiatrists, social workers, nurses, doctors, professors of literature or academics in general believe that these ideas are both correct, and based on so much science and common sense that suggesting they are incorrect is close to ridiculous. Everybody in our culture believes in this, surely it is true! No. The world is round, not flat. Empirical research, not cultural consensus teaches us what is true. In this case Dickens was closer to the truth than Freud or Watson.

The myth of childhood is a part of western culture, pop-psychology and academic psychology. It is the idea that the experiences a child has, especially the earliest experiences, are more important than later experiences – and through the idea of psychological determinism, these experiences create the personality of the adult. Further, if these experiences are traumatic or in some specific way not good, they

are the cause of any psychological disorder or distress. Finally, the experiences that may be called “parenting” are the most important of these experiences.

Paris (2000, pp. xi-xii) lists the following three main factors of the myth:

Myth 1: *Personality is formed by early childhood experience.*

Myth 2: *Mental disorders are caused by early childhood experiences.*

Myth 3: *Effective psychotherapy depends on the reconstruction of childhood experiences.*

Don't believe the hype! These *are* myths. Paris calls them cultural *shibboleths* – they are taken for granted, not questioned. But they ought to be! They are not supported by science. Scientific inquiry has been performed. The results are negative. Yet they are believed. And by those who believe them, those who practice them, are revered and provided with authority and power. This is psychiatry and clinical psychology's “dark secret”.

The Myths Exposed

Joel Paris reviews the literature on childhood experience and adult personality and mental disorder. His findings are not surprising, to those who are familiar with the literature on developmental psychopathology, behavioural genetics and the general lack of science in clinical practice.

Let's cut to the chase:

1. Primacy

According to Paris (2000, p. 17) the belief in *primacy* is the belief that:

1. The earlier in childhood adverse experiences occur, the stronger will be their long-term sequelae.

2. Early childhood experiences, particularly socialization and attachment in the family, shape normal and abnormal personality, so that a secure and happy childhood is the strongest predictor of good adult functioning.

3. Conversely, adverse and negative ex-

periences during childhood can seriously distort personality structure and are strongly implicated as risks for psychopathology in adulthood.

There is no evidence that any of the above is true. There is no evidence that early adversity is more important than later adversity. Temperamental and other genetic predispositions cause interactions with environments, such as parents and parenting style. These factors also provide the basis of continuity in personality. Further they may predict better than experiences, what type of disorder one may suffer from as an adult. Remember – experiences, or learning, may only influence the expression of genes – one cannot learn something one has not got the genetic potential to express.

Primacy is not the cause of normal personality. Neither is parenting (see also Harris, 1995, 1999). Experiences may be important – most theories of which experiences influence normal personality have been falsified. Currently there are no specific experiences that there is consensus for actually form personality is a specific manner, and which there is scientific evidence for. Harris' group socialisation theory is an interesting hypothesis, as is Pinker's (2002) more tongue in cheek suggestion that random expression of genes may be important.

Adversity in early years does not predict mental disorder. As clinicians we see those who have mental disorders. These are, as all behavioural and mental traits (Plomin et al., 2000; Pinker 2002; Turkheimer, 2000) influenced by genetics – thus problems in the family may reflect common disorders and heritability rather than cause via primacy effects. Also, those not treated for mental disorder report much the same type of childhoods that those in treatment do. Thus if someone reports early childhood adversity, and now present with symptoms of mental disorder, we often become victims of a faulty theory that prejudices our formation of narrative, sloppy logic and a distorted sample. We just cannot conclude that the adverse ex-

perience caused the pathology! Most people with that experience do not develop pathology. If the experience, which is picked at random as specific pathogen experiences are not known, was important it must have been in interaction with genetic predisposition. But we neither know this. Mere speculation is not helpful... mostly because the idea of primacy is false, and documented effective therapy is not dependent on reconstructing the past.

2. Adversity and outcome

Parenting does not influence normal personality. I repeat this. There is little evidence that negative parenting, apart from upsetting children and affecting them at the time, has any long term consequences. This makes for difficult assessments for e.g. child care services – what do we do when we do not know? For the clinician with an adult patient, the problem is as described above – it is impossible to claim that the described parent behaviours caused the patient's mental disorder. Also, the patient's disorder will predict the report of experiences better than the opposite.

Much of the results showing increased frequency of mental disorder in groups who have experienced abuse fail to show more than a certain increase – also there are methodological problems when attempting to establish cause. Even the most extreme abuse will in most cases not cause disorder. Also genetic factors must be controlled for.

Paris (2002, p. 36) concludes as follows:

The outcome of adversity in childhood is highly variable. If possible, it is better for children to avoid trauma, family discord, family breakdown, or serious poverty. Each of these difficulties may increase the risk for psychiatric disorder, at least in *some* children and in *some* adults. The more adversities that are present, the more likely pathology is to develop.

Yet even when exposed to multiple adversities, most children manage to escape

any form of mental disorder.

Thus it is true that extreme abuse, neglect or negative life events may statistically jeopardise the development of children. But it may be a passing effect – at least in groups studied longitudinally the amount of disorder declines with time – and in any case resilience is more typical. It is not typical that studies of children or grownups in extreme situations show that most will develop mental disorder (such as posttraumatic stress disorder) – rather the great majority does not.

3. Concluding the Exposure

These findings are quite surprising to most. Many are even infuriated and feel affronted by these results. The more rational reaction would be curiosity, a change of what beliefs one takes for granted and revision of personal professional practice – and what kind of practice one accepts from professionals for one's own mental disorders or that of one's loved ones. Alas, this rational response is very rare. All the more important to repeat what we know, and repeat what we *do not know!*

Also, *resilience* is more typical than not. Most people who experience extreme trauma survive without sequelae – i.e., mental disorder, unhappy memories may of course be troublesome and painful, but most survive these too. Parenting only has to be good enough, as both Scarr and Winnicott have pronounced. Just imagine the childhoods of Socrates, Snorre Sturlason and Shakespeare – they seem to have survived childrearing practices that we probably would have considered atrocious today. Evolutionary analysis simply informs us that our species had to be able to develop normally and survive emotionally even if there were no culturally trained child rearing experts around.

Nota bene this does not mean that parents may abuse or neglect their children – that is a ridiculous interpretation. Neither does it mean that parents do not matter – parents are there to protect, feed, love and take care of their children. Those who come to such horrendous con-

clusions after reading Paris' book, or more typically Harris' (1995, 1999) work, ought to be ashamed of themselves – if the only reason to love children is to be allowed to influence every aspect of their mental health and personality then one ought to buy a dog, not have a child.

Why do Mental Health Professionals Believe in Unsubstantiated Claims?

Robyn Dawes (1994, 2001) has written several books attempting to teach his clinical colleagues how their own psychology works against them and makes them believe unsubstantiated claims or misunderstand causal or statistical patterns. Alas, even if they infuriated many clinical readers, his books never became as influential as some of us hoped. Dawes (2001) analysis of stories and belief is most important though.

I have several times been impressed at how much my colleagues know about research methods and critical thinking about statistical analysis when presented with findings based on large numbers. On the other hand, being overly critical to such work is hardly balanced by the equally impressive ability to be swayed by a well told yarn, an illustrative anecdote or personal experience. $N=1$ is for most mental health professionals more persuasive than $N=20$, or $N=1000$ for that matter.

Paris is also interested in the problem of why his colleagues believe what has not been proven to be true as if it were scientific fact. More particularly he wonders why clinicians still believe in the general psychoanalytic model, rooted in the theory of primacy. He suggests three explanations: belief in narrative, toleration of cognitive dissonance, and the need to account for failure. A fundamental part of Paris' analysis is the belief that therapy works – no matter what method one uses. We will address this belief later. At the same time he is aware that the reason is probably not to be discovered through following the heuristic of primacy, and that this therefore is not a necessary ingredient. With these starting points in place Paris provides a

reasonable explanation.

Everyone in our culture believes in primacy, in their personal mythologies or life narratives, no less those who have themselves been through an analysis. Paris compares giving up such “deeply rooted beliefs” as “almost equivalent to losing one's personal identity” (p. 157). Indeed in a narrative tradition that is absolutely so. Further, the ability to accept cognitive dissonance is typical for humans. Paris suspects that the best clinicians are far from purists, and that the most successful therapists perform therapies that maximise the effect of general effects, although they might later explain it in theoretically loyal terms. I will later show that general effects are overvalued by Paris, but as far as achieving what general effects may, I believe his analysis is correct here too. The last explanation, accounting for failure, is something I see time and again. Psychodynamically oriented therapists attribute success or failure more to the individual therapist than cognitive therapists – instead of attempting to follow the manual closer (external locus) they believe the fault is in their own ability (internal locus). That also means that if a cognitive therapist presents a successful case, it will be considered gloating – even if the therapist presenting the case believes the method was what was effective. Paris' conclusion is that some patients have a limited ability to change, due to their temperament or genetic makeup or type of disorder (more precisely, all three). Yet again, this is probably true. The problem with the size and complexity of the basic psychoanalytical or psychodynamic theory – which is the basic idea of how the mind works adopted more or less by most therapists (Kennair, 2000) – is that almost everything may be explained within it, and thus revision is rarely the first alternative. Thus it survives in the minds of psychotherapists, and in our culture – although there is almost no scientific reason for believing in it.

This is a most interesting problem. Psychoanalytic theory itself is one of our centuries most used framework for attempting to discover

why some people decide to grip hold of their most cherished ideals, beliefs and fantasies despite the lack of benefit these provide and reality suggesting otherwise.

Kennair et al. (2002) present a preliminary taxonomy of why people choose not to adopt scientifically based or evidence-based methods of therapy. In general the training of therapists and the self-generating academic culture prevents many professionals and policy makers from believing scientific inquiry is possible. Further, there are not yet clear incentives to make any changes – due to the authority, influence, power and apparent knowledge and wisdom one has through one's training, and the positive sanctions society and government provides no matter what documentation one has of actual effect.

The Professional Arrogance of Ideological Masters

Paris has not just set out to prove his masters wrong. He also attempts to limit the overconfidence of mental health professionals in proclaiming insight and wisdom on almost every aspect of Human Nature on the basis of a theory notoriously thin on empirical science. This is not common only to mental health professionals – there are too many physicists who believe they may explain consciousness without considering cognitive neuroscience or religious fundamentalists who believe they can evaluate biological theories without studying biology.

As high priests of the major theories of human nature, clinical psychologists (who at least have read some basic academic scientific psychology – although that probably cannot correct later training in myths) and psychiatrists (who usually only have training in the myths of psychoanalytic theory) make pronouncements on a plethora of fields. The least problematic is their interest in film, literature and the arts, although this has caused a stagnation of these fields' ability to investigate human nature in any original manner. The theory is used by authors and critics alike, so there is an inbreeding of ideas. Further, the ideas are false. This makes almost

no psychiatric analysis of the arts the least interesting. This is changing – and the change is coming from within academic literary criticism and scholars that have read modern empirical scientific psychology (e.g. Carroll, 1995; Scalise Sugiyama, 2001).

More troubling is the idea that advice on parenting and the treatment of mental disorder is based on an equal lack of knowledge! Here parents, children and mental health patients are at the mercy of ideologists rather than scientifically informed professionals.

Knowledge of a specific ideology does not make someone wise about the world, unless the specific ideology is a theory founded on empirical science. What do we know about parenting? Actually, as Paris points out, no advice to normal parents with normal parenting skills can improve their practice in any known manner. Paris claims we know how to harm a child's development (I would add, statistically – even the most extreme maltreatment may not influence the child's development in a systematic and predictable fashion), but we do not know how to form a child in any systematic positive way. Thus, parenting advice from the most impressive mental health practitioner is mere speculation. An obvious claim like: “do not abuse or neglect your child,” is not as impressive as more detailed suggestions that will improve the personality, intelligence or mental health of children. But the former is statistically true, the latter is mere speculation – and in most cases speculation informed by a bogus ideology not the scientifically informed professional competence one believes one is paying for.

Paris calls for public education, research and an end to advice about how to raise normal children. These are commendable requests. Let us hope we will see our professions move in that direction... alas, for this to happen we have to influence a whole culture. We have to teach the public that we do not know some things, and do know other things. The odd thing is (even if we should wish to actually tell the public that clinical psychologists and psychiatrists

are not omniscient and omnipotent) that in our culture the things we actually do know are considered unknowable (like effects of genes or of psychotherapy), while the things we do not know (why Jack became obsessive compulsive or why Jill became open to new experience, extravert, neurotic, agreeable and conscientious) are what the public, policy makers and mental health professionals themselves believe we do and ought to know!

Consequences for Clinical Practice

Psychotherapy, no matter what type, is often about discovering how the patient became mentally ill. The best treatments do not focus on aetiology, but get about changing factors that maintain the mental disorder (see Wells, 1997, for an example). But most patients and most therapists want to know why and how. This is knowledge that is not readily available. Our best answer is that we do not know. This is, as noted above, unsatisfactory. And it goes against every conception held in our culture about the role of the psychotherapist – and the function of psychotherapy.

Within psychoanalysis there was a shift from historical truth to narrative truth (Schafer, 1980; Spence, 1982). This did not alter practice much. The archaeological detective work of the therapist was still in focus, digging to find the narrative experience that caused disorder or distress. Because agnosticism was not the same as atheism – the myth of childhood was not challenged, only the ability to know if the episodes that were constructed in therapy actually happened or not. Changing narrative is probably effective therapy, although narratives are disorder state specific, so change often comes about simultaneously anyway.

Going back to childhood to make changes, through providing to the adult what the child lacked, is not theoretically likely possible (Kennair, 2000). All in all the focus on childhood in the treatment of adults is often quite problematic. Not only does the mind change as it matures, different genes are expressed and life history progresses, it is of little or no

known consequence to any effective therapy. One thing is the lack of change of focus in psychoanalysis despite the narrative challenge, the same focus on childhood experience is central to mainstream CBT despite the fact that the hypothesised aetiological structures change with mental state rather than with therapy (Swallow, 2000) – that is the hypothesised depressogenic basic assumptions supposedly learnt early in life are only present while the patient is depressed, not before nor after – this holds true whether the patient has received CBT, any other treatment or no treatment at all. This does not mean that CBT does not work; it means that the theoretical assumptions within CBT are false. CBT for depression is evidence-based, not knowledge-based, alas. A clear exception is the work on anxiety disorders by Adrian Wells (1997) – which actually has mapped disorder specific information processing, developed methods to intervene with maintaining factors, and does not spend time on aetiology. It will be interesting to see how long it takes before mainstream psychology and behavioural therapists quit teaching Mowrer's two-factor theory for the acquisition of simple phobia – given Kendler, Myers and Prescott's (2002) findings. We do not know the experiential cause of any mental disorder. Even if there is a greater chance of developing psychopathology after extreme abuse or neglect, most will not develop disorder due to such experiences. How shall one discover which minor distressing experiences or more mild forms of neglect may be the cause of our patients' pathology? The fact is we cannot. Every attempt at managing the impossible is ridiculous – a waste of patients' time, money and energy. Help them get better – don't attempt to discover what for all practical reasons is hidden in a web of genetic and environmental interactions too complex for the individual therapist to unravel.

Too much psychotherapy, within all methods of treatment, consists mainly of trips down memory lane – in the dark. Memory is selective, state dependent, reconstructive and crea-

tive. Thus basing anything on memory is at best shaky. The fact that establishing false memories and also an unwarranted expanded focus on upsetting memories (which are state specified, not a true representation of the patients history – wait until the patient is better, and note that even a waiting list may cause the patient to develop a new and adaptive narrative!) – may consolidate disorder ought to caution psychotherapists eager to play the intra-psychic Sherlock Holmes, or even worse Indiana Jones. If the patient has troubling emotions or memories, focusing on these will prolong distress – at least in the situation. Teaching the patient that the situation that caused painful memories are the reason the person is troubled is counterproductive to the patient getting better – and probably not true.

If possible one should spend less time discussing reasons why patients may be suffering, in meetings where therapists impress each other with the magic words of false theories and the ability to construct fanciful, albeit culturally congruent, just-so stories about how the patient was corrupted and mangled by a random selection of state specific memories... many of which were elicited by the therapist. More time should be spent discovering disorder maintaining factors – and attempting to adjust these, so that the patient may become as well as possible as soon as possible. Too mechanical? No. Most humane!

A Few Critical Remarks

Paris describes his own professional conversion, from a believer in the myths, anecdotes and classical authoritarian theoretical psychiatry/ psychoanalysis. This is a difficult journey. Cutting loose what one does not have sufficient evidence to believe, and accepting a whole new framework is not something many academics get a chance to do in life. I suspect losing one's faith always is particularly confusing, painful and the kind of mental adjustment most rather would avoid – and most do avoid it.

Paris makes a brilliant attempt at presenting the new psychology. But, at some spots along

the way I find that he trips up a little. I will be spending some space on a few critical points. This ought not to detract from the value of Paris' work as a whole; only suggest that improvements are possible, as well as greater consistency. The inconsistency is to be found in three clusters: behavioural genetics, evidence-based mental health care, and the problem of avoiding suggesting what *may* be true in a book on what *is* and what *is not* known to be true or likely. Hopefully this may be constructive criticism.

1. The Problem of Behavioural Genetics

If a child resembles its parents, the most likely reason is genetics. This is such a novel and unfamiliar thought within mainstream psychological science and academics that most editors and researchers do not yet believe they need to pay attention to it. If a child is anxious and the parents overprotective... well, why did the child grow up anxious? Yes, more stimulation might have helped – but most likely there is a cause to be found in the genetics of both parent and child. Very likely changing parent behaviour will be difficult, and there is a possibility that many of these children will find great discomfort in more social stimulation or anxiety provoking situations – and will this generally lead to less anxious adults? We do not know.

Paris provides a splendid and clear description of behavioural genetics in this book. And it is especially written to communicate to clinically oriented psychiatrists and clinical psychologists. This makes it doubly interesting. This is a subject that mental health professionals need to learn and understand better!

Paris presents the framework of behavioural genetics as the most fruitful and informative approach to discovering how childhood may predict disorder in adulthood. Thus, as most of the research literature – and its conclusions on child development and parenting suggestions – is uninformed of possible (and highly significant, as we have learnt from behavioural genetics) genetic influences, using this to back up one's arguments can become problematic.

Quite simply put: Without studying child development and developmental psychopathology in a framework that assesses both genetics and environment something is going to be missing, and the conclusions are based on incomplete data. This means that we know less than we think we do, even when empirical research on development is available, because the research is not good enough to inform us. This research used to be good enough, but then the paradigm changed, and as the paradigm changes the possibility of using old conclusions changes too.

I have been told that it is highly unlikely that behavioural genetics will inform us of the development of psychopathology (see Kennair et al., 2002). Such a comment may only be made if one is not aware how much behavioural genetics already has informed us – that the future of developmental psychopathology is within behavioural genetics seems to me to be quite certain.

2. The Problem of Evidence-Based Mental Health Care

Paris is overgenerous in his description of the field of mental health care being evidence-based (p. xiii). This is not the case (Kennair et al., 2002) – but Paris is very clear that he is in favour of scientific underpinnings for psychiatric and clinical psychological practice. At the same time he is not equally positive, in the text, to diagnostic systems, and does not focus much of standardised diagnoses. Evidence-based mental health care has its foundation in treatment of diagnoses. Thus it is difficult to argue consistently for evidence-based mental health-care, and at the same time avoid setting diagnoses, or including this in ones framework for understanding the patient, the disorder and assessing treatment of choice (see Kennair et al., 2002, and the following debate between Kennair and Panagiotis Pefanis in same edition).

Further, there is today ample evidence to claim that certain methods are better than other methods. Focusing merely on maximising the effect of “unspecified” factors seems to be a

premature conclusion to applying psychological and psychiatric science to the development of psychotherapy. Here I find that Paris’ argumentation is a little inconsistent. He correctly identifies that the only reason that general effects or unspecific factors may be given such names are that they have not been specified, yet. The mind is not a general problem solver (Kennair, 2002a; Symons, 1992; but see La Cerra & Bingham, 2002, for an alternative view and Kennair, 2003, for a critique) – specific biopsychosocial states arise due to rather specific contexts (Sloman & Gilbert, 2000) and there is quite specific information processing typical for different specific anxiety diagnoses (Wells, 1997). As an example Paris both points out the likely specificity of interventions of DBT as explanations of Marsha Linehan’s (1993) better-than-treatment-as-usual results – and then later suggests that these are a result of an optimal utilisation of general factors.

The numbers suggested by the work of Hubble, Duncan and Miller (1999) hide the truth about the state of evidence-based treatment and psychotherapy research. I would rather recommend reading Nathan and Gorman (1998). For example, for generalised anxiety disorder (GAD) it is found that this disorder is cured at a rate of 60% with behavioural techniques, compared with a mere 4% of psychoanalytic success (Fisher & Durham, 1999) – which actually is worse than a waiting list control. The major point of this example is that there are differences between different specified therapies. In Hubble, Duncan and Miller (1999) one has focused on many different therapies (some for which there is little evidence of effect – thus this is not much worth), for all conditions (not always highly defined – one needs standardised measures and diagnoses, for reliability reasons)... and conclude that general effects are more important than specific techniques. Well, given what was studied, that is what one would find – if you study a bunch of ineffective techniques you are not going to conclude that technique provides efficiency.

This is why their conclusion is wrong: If someone has a panic disorder, OCD (focus on thoughts) or social phobia the treatment of choice is cognitive behavioural therapy (CBT). If the disorder is agoraphobia, OCD (focus on behaviour) or a single phobia then behavioural therapy is efficient – and more efficient than general conversation. For manic depression one ought to stick to psychosocial interventions aimed at increasing compliance in taking mood stabilising medication, where the latter is the treatment proper. Within the field of psychosocial interventions to schizophrenia and psychosis the international community has shifted clearly and dramatically in recent years toward a cognitive approach – at the same time medical interventions still are best documented. No treatment of specific symptoms of borderline personality disorder is better documented than Dialectic Behavioural Therapy (Linehan, 1993) – which actually was tested against treatment as usual (sic!) – thus the Dodo bird verdict itself was tested. There are several hundred psychotherapies, but not all of these have been studied – thus it is not correct to suggest that anything goes. Even in the first assessments of psychotherapy (Eysenck, 1957) behavioural therapy – i.e. a scientifically founded approach to treating psychopathology – fared better than other treatments. A hidden or forgotten conclusion? I recommend Nathan and Gorman (1998) for their introduction with a history of psychotherapy research – the rest of the chapters tell the tale of some therapies being more efficient and better documented than other methods.

Thus, even if the nature of e.g. depression (episodic, remitting) makes it seem as if all treatments might work, including placebo (see Hrobjartsson & Gotzsche, 2001, 2003 for studies of placebo effects) – some treatments do work better than placebo to a certain degree (Interpersonal psychotherapy (IPT), SSRI, CBT) or to a lesser degree (manualised short-term psychodynamic therapy). Differences between the less than optimal treatments show that there are some specific factors involved – the latter

does not fare as well as the three first; IPT has a somewhat swifter initial effect; and CBT may be a tiny bit more protective against relapse. These are also the best treatments available, and it is arguing from the void to claim that all treatment would be as good.

It should be further noted that although Paris is very positive in his general treatment of CBT, he might be too generous toward this mode of therapy. I am a qualified cognitive therapist and practice, teach, supervise and write on CBT, all the same I find it most important to seek out and remove the myths of childhood within CBT. The two most popular CBT approaches to personality treatment are *not* as evidence-based as many believe (Linehan's treatment – a CBT-inspired treatment – is not as popular among therapists, yet) – this includes Beck's orthodox model of depression and personality and Young's (1999) schema-focused approach. The former believes depression and personality are formed due to early childhood experiences; the latter is even more radical – verging on what I call blaming the parents. It would have been interesting to hear Paris' view of Young's form of treatment. Note: CBT is historically a revision of psychoanalysis, and most personality disorder treatments today include all the myths of childhood inherited from psychoanalytic models of development. The inclusion of behaviourism does not negate, but strengthens the empiricist prejudice.

3. The Problem of Suggesting That Something May Be True in a Book on How We Do Not Know What Is True

The last inconsistency cluster is commented by Paris himself – he has little evidence that his suggestions for better treatment are true. So, why do I mention this, then? Because the last thing we need is exchanging one master's beliefs for another's... as simple as that. Let's be purists. Let us do the research first, and not make recommendations based on pet theories, intuition or what we believe to be common sense and even relatively supported by science. Either it is supported by science or it is not.

Stories, anecdotes, the sayings of sages or masters are ever so powerful. Even more so when presented with due care – reflection is convincing. The book’s power is awesome, but lies in its negative thesis. The book’s positive thesis is there merely due to tradition. It feels too empty just debunking common wisdom, the popular meta-theory of psychology and academia’s general model of human nature. But that is important. And the addition of what we have learnt so far from behavioural genetics ought to be enough. My advice is: do not speculate! As Nathan and Gorman (1998) point out: “In the absence of science, opinion prevails”. After writing such a smashing book, Paris ought to have avoided putting himself in a position of a master whose claims we may choose not to believe.

4. Concluding My Critical Remarks

These comments should not detract from the worth of this book. The author is a little inconsistent, but is in general on the right track. Mere small corrections are needed to achieve a truer and more coherent picture.

All things considered, the behavioural genetic approach and an evidence-based perspective are very recent and alien concepts for most clinicians. I applaud Paris’ conversion, but find that some signs of the old paradigm creep into his thinking... in the form of the inconsistencies described above. His message is clear, though: Behavioural genetics and an evidence-based approach will need to inform our thinking and guide our practice.

Influencing a Culture

Our academic environment and culture believes in the myths of childhood. Everyone – from lawyers and mental health professionals to screenplay writers and policy makers – believes in these myths. It will not be a simple task telling a whole culture that they are wrong... but it has been done before. People believed the world was flat. They experienced that it was flat. Scientists told them that it was not, and in time we have been shown that it was not. People experience that childhood events make

sense of their current situations. That is how memory works – here and now state influences the creative generation and reconstructive retrieval of memories. Simply put: If you are depressed then you will remember sad events. And then everything makes sense. But it is not true that depressed people have had sadder childhoods, because the story changes, as memory changes, when they no longer are depressed.

Many mental health professionals believe that their old paradigm models of human nature may help them understand every aspect of the human condition. They offer insights on modern culture, classical culture, parenting and social policy – much on the basis of their special intuition, which mostly is based on the tenets of theories that lack documentation or scientific foundation. Paris questions the authority of these experts of the new western folklore, the shamans and wizards of the non-science of psychology. His conclusion is simple: They cannot base their authority and answers on knowledge, but scientifically we do not know or actually know that it is not as they suggest!

In this way Paris is part of a larger movement – that of behavioural genetics, but also evolutionary psychology, cognitive neuroscience, psychopharmacology and other biopsychosocial empirical approaches. And he places the new approaches to the scientific study of human nature into a larger movement yet: Science. In this one has to support Paris, and indeed I do (Kennair et al., 2002).

But how may one succeed in influencing a culture... How can one debunk the ideas of Freud, Klein, Winnicott, Kohut and Kernberg, Watson and Skinner, Rogers and Maslow, and almost every writer and philosopher of the mind of the last hundred years? The wise man was asked: How may one man eat a whole elephant? He answered: Piece by piece. (He ought to have included the need of a large freezer...). This may be the only way... Sandra Scarr (1992, 1993; Scarr & McCartney, 1983) did not do the trick, but some of us read her work and

did not merely dismiss it. Robert Plomin, Michael Rutter and others have published books explaining what science tells us (see especially Plomin et al., 2000). Judith Rich Harris (1995, 1999) has made a few laudable attempts, and Steven Pinker (1997, 2002) at least got a lot of attention. Joel Paris communicates directly to mental health professionals, but his message is so foreign he might have to repeat it.

Conclusions

This is a most important book. I believe it would communicate well with therapists and clinicians. The topic is of utmost significance – too many psychotherapists use too much time digging about in the past of their patients looking for the reasons or causes of why their patients became anxious, depressed, psychotic, obsessive, emotionally labile etc. Most evidence based treatments do not – their effect is usually to be found in a focused attempt at changing factors that maintain the disorder (e.g. Linehan, 1993; Weissman, Markowitz & Klerman, 2000). Even within cognitive therapy – which by most is wrongfully considered *per se* and *en masse* to be evidence-based – therapies based on changing maintaining factors (treatments of anxiety, e.g. Wells, 1997) are more efficient than those that attempt to explain the aetiology of the disease (such as Beck's original treatment of depression or Young's (1999) approach to personality disorders).

I therefore fully recommend *Myths of Childhood* to all experienced psychotherapists. Also I believe it should be required reading for any course on developmental psychopathology – and thus included in the curriculum of courses on psychopathology, clinical psychology, psychiatry. Also nurses, social workers and other related professions ought to be informed of the contents of this book.

The author makes a good case for the need of Hollywood screenplay writers, and I would add literary critics (Kennair, 2003), to revise their ideas of developmental psychology and human nature. Carroll (1995) and Scalise Sugiyama (2001) provide alternative approaches –

that mirror Kennair et al.'s (2002) attempt at educating clinicians and policy-makers: there exists a science of human nature and there is thus a scientific foundation for practice.

As the mental health care professions evolve toward a scientifically based practice, which is both evidence-based (one knows *that* it works) and knowledge-based (one knows *how* it works), I believe we will see that this will involve the demise of many a popular classical “truth” or myth of psychology. We are, to our great surprise, in a position where we may actually perceive the established paradigm of Human Nature of psychology, as we see how several new findings debunk old “truths”. Further, one may note how this research is received by those who cling to the old order. Until now, there seemed to be a plethora of different schools, holding different and to a large degree irreconcilable tenets on the major problems of psychology. Pinker (2002) shows how these traditional approaches actually share many ideas that have become a part of our culture. Paris' book and our own attempt (Kennair et al., 2002) both support Pinker's analysis. The empiricist, romantic and dualistic ideas of old psychology are being debunked by new approaches. Genetic influences are more important than one used to believe, and the specific way the environment influences us is not specified by the old theories of psychology – they are wrong. This is true, even if one may not *yet* answer the question of how the environment actually does influence us.

If clinical psychology and psychiatry are to become scientifically informed practices – both knowledge-based as well as evidence-based – they have to become more interested in the basic sciences of human nature, and their empirical findings. It is actually quite shocking that while literary critiques have read evolutionary psychology and are in a position to inform their colleagues of the new findings within scientific psychology (e.g. Scalise Sugiyama, 2001), a parallel campaign has to be initiated to inform psychiatrists and clinical psychologists of the

same! Both camps have been confused by the same ideas – environmentalism, post-modernism, dualism, romanticism, and lack of belief in a real and material world that may be studied and discovered through science... Paris' book is an important and necessary step away from the culture of belief and tradition, toward a culture of knowledge and science.

Leif Edward Ottesen Kennair, Chief Psychologist, Nordfjord Psychiatric Centre, N-6770 Nordfjordeid, Norway. E-mail: leiedoke@online.no.

References

- Carroll, J. (1995). Evolution and literary theory. *Human Nature*, 6, 119-34.
- Dawes, R. (1994). *House of cards: Psychology and psychotherapy built on myth*. NY: Free Press.
- Dawes, R. (2001). *Everyday irrationality: How pseudo-scientists, lunatics and the rest of us systematically fail to think rationally*. Boulder, CO: Westview Press.
- Eysenck, H. J. (1952). The effects of psychotherapy: an evaluation. *Journal of Consulting Psychology*, 16, 319-324.
- Fisher, P. L., & Durham, R. C. (1999). Recovery rates in general anxiety disorder following psychological therapy: an analysis of clinical change in the STAI-T across outcome since 1990. *Psychological Medicine*, 29, 1425-1434
- Harris, J. R. (1995). Where is the child's environment? A group socialization theory of development. *Psychological Review*, 102, 458-489.
- Harris, J. R. (1999). *The nurture assumption: Why children turn out the way they do*. NY: Touchstone Books.
- Hrobjartsson, A., & Gotzsche, P. C. (2001). Is the placebo powerless? An analysis of clinical trials comparing placebo with no treatment. *New England Journal of Medicine*, 344, 1594-602.
- Hrobjartsson, A., & Gotzsche, P. C. (2003). Placebo treatment versus no treatment (Cochrane Review). *Cochrane Database of Systematic Reviews*. CD003974.
- Hubble, M. A., Duncan, B. L., & Miller, S. D. (Eds.). (1999). *The heart and soul of change: What works in therapy*. Washington, D.C.: American Psychological Association.
- Kendler, K. S., Myers, J., & Prescott, C. A. (2002). The etiology of phobias. An evaluation of the stress-diathesis model. *Archives of General Psychiatry*, 59, 242-8.
- Kennair, L. E. O. (2000). Developing Minds for Pathology and Musicality: The role of theory of development of personality and pathology in clinical thinking illustrated by the effect of taking an evolutionary perspective. *Nordic Journal of Music Therapy*, 9, 26-37. <http://www.hisf.no/njmt/artikkelkennair91.html>
- Kennair, L. E. O. (2002a). Evolutionary psychology: An emerging integrative perspective within the science and practice of psychology. *Human Nature Review*, 2, 17-61. <http://www.human-nature.com/nibbs/02/ep.html>
- Kennair, L. E. O. (2002b). Human nature and the limits of blank slateism. *Human Nature Review*, 2, 483-491. <http://human-nature.com/nibbs/02/slate.html>
- Kennair, L. E. O. (2003). An alternative paradigm after all? *Human Nature Review*, 3, 24-35. <http://www.human-nature.com/nibbs/03/paradigm.html>
- Kennair, L. E. O., Aarre, T. F., Kennair, T. W., & Bugge, P. (2002). Evidence-based mental health - The scientific foundation of clinical psychology and psychiatry. *Scipolicy - Journal of Science and Health Policy*, 2. <http://www.scipolicy.net>
- Kuhn, T. (1996). *The structure of scientific revolutions*. 3rd ed. Chicago: University of Chicago Press.
- La Cerra, P., & Bingham, R. (2002). *The origin of minds: Evolution, uniqueness and the new*

- science of the self*. New York: Harmony Books.
- Linehan, M. M. (1993). *Cognitive-behavioral treatment of borderline personality disorder*. New York: The Guilford Press.
- Nathan, P. E., & Gorman, J. M. (Eds.). (1998). *A guide to treatments that work*. New York: Oxford University Press.
- Paris, J. (2000). *The myths of childhood*. Philadelphia, PA: Mazer/Brunner.
- Pinker, S. (1997). *How the mind works*. Harmondsworth, UK: The Penguin Press.
- Pinker, S. (2002). *The blank slate: The modern denial of human nature*. London: Allen Lane/ Penguin.
- Plomin, R., DeFries, J.C., McClearn, G. E., McGuffin, P. (2000). *Behavioral genetics*. 4th ed. New York: Worth Publishers.
- Scalise Sugiyama, M. (2001). New science, old myth: An evolutionary critique of the oedipal paradigm. *Mosaic* 34.1 (March), 121-36.
- Scarr, S., & McCartney, K. (1983). How people make their own environments: A theory of genotype → environment effects. *Child Development*, 54, 424-35.
- Scarr, S. (1992). Developmental theories for the 1990s: Development and individual differences. *Child Development*, 63, 1-19.
- Scarr, S. (1993). Biological and cultural diversity: The legacy of Darwin for development. *Child Development*, 64, 1333-1353.
- Schafer, R. (1980). Narration in the psychoanalytic dialogue. *Critical Inquiry*, 29-53.
- Spence, D. P. (1982). Narrative truth and theoretical truth. *Psychoanalytic Quarterly*, 43-69.
- Swallow, S. R. (2000). A cognitive behavioral perspective on the involuntary defeat strategy. In L. Sloman & P. Gilbert (Eds.). *Subordination and defeat: An evolutionary approach to mood disorders and their therapy*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Symons, D. (1992). On the use and misuse of Darwinism in the study of human behavior. In Barkow, Cosmides & Tooby (Eds.), *The adapted mind: Evolutionary psychology and the generation of culture* (pp. 137-62). New York: Oxford University Press.
- Turkheimer, E. (2000). Three laws of behaviour genetics and what they mean. *Current Directions in Psychological Science*, 9, 160-164.
- Weissman, M. M., Markowitz, J. C., & Klerman, G. L. (2000). *Comprehensive guide to Interpersonal Psychotherapy*. New York: Basic Books.
- Wells, A. (1997). *Cognitive therapy of anxiety disorders: a practice manual and conceptual guide*. Chichester, UK: Wiley & Sons.
- Young, J. (1999). *Cognitive therapy for personality disorders: a schema-focused approach*. 3rd ed. Sarasota, Fla.: Professional Resource Press.