

Correspondence

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Implications of evolutionary theory for psychiatry

It may well have been a coincidence that the announcement of the ‘breaking’ of the human genetic code and the publication of an editorial on psychiatry and Darwinism in the *Journal* occurred within the same week, but it is to be hoped that both of these events signal a new beginning. Abed (2000) asks whether the time has come for psychiatry to reconsider Darwinism: in fact, one could argue that if psychiatry as a science is to survive, there is no other option. Since its first publication in 1859, Darwin’s evolutionary theory has transformed our understanding of the living world. The model has stood the test of time despite heavy resistance by religious groups, exploitation by Fascism and enthusiastic misinterpretation. The proliferation of papers on the subject in scientific journals over the past 30 years strongly suggests that it is here to stay. Evolutionary psychology has already established itself (Barkow *et al*, 1995). In contrast, only a few articles have been published by psychiatric journals, and evolutionary theory is largely ignored in psychiatric training worldwide.

If psychiatry has survived until now without using evolutionary theory, what would be the advantage of a theoretical shift? Psychiatry badly needs a theoretical framework (Kandel, 1998) that allows for the synthesis of knowledge accumulated by different schools that do not speak the same language and therefore do not interact with each other. Evolutionary theory is capable of integrating genetic, environmental, developmental and social explanations of behaviour and is therefore an excellent candidate (Leckman & Mayes, 1998). Furthermore, as Abed points out, the usefulness of the model can be tested by theory-driven research. Psychiatry has to take up the challenge. The application of modern evolutionary theory should lead to a more accurate understanding of human

behaviour, including the origins and treatment of mental illness. Psychodarwinism became a term of abuse following atrocities perpetrated during the first half of the 20th century. It is time to learn the lessons of the past and move on. Attachment theory is one successful example of using evolutionary principles in psychiatry, and there will be more to come.

Abed, R. T. (2000) Psychiatry and Darwinism. Time to reconsider? *British Journal of Psychiatry*, **177**, 1–3.

Barkow, H. J., Cosmides, L. & Tooby, J. (1995) *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*. New York: Oxford University Press.

Kandel, E. R. (1998) A new intellectual framework for psychiatry. *American Journal of Psychiatry*, **155**, 457–469.

Leckman, J. E. & Mayes, L. C. (1998) Understanding developmental psychopathology: how useful are evolutionary accounts? *Journal of the American Academy of Child and Adolescent Psychiatry*, **37**, 1011–1020.

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Delighted though I was to see Abed’s editorial on evolutionary theory (Abed, 2000), I have reservations about its ability to provide an integrated scientific psychiatry as the author implied. Rather, it provides a welcome additional frame of reference. Like all ultimate theories, it applies to everything but lacks power with specifics (for example, to clarify whether an antidepressant or psychotherapy is best for an individual patient). Evolutionary theory seldom generates new treatments. It offers ultimate causes over which we have no control.

Although I am an enthusiast of both, I am concerned that evolutionary theory has the same drawback as psychodynamic theory; it can accommodate any combination of facts. If I devise and test a theory that adolescent males will be less or more inclined to form lasting sexual relationships than older men, I can explain either. If they desire to form casual relationships, then I can argue that in the ancestral environment

this benefited their genes at a stage in life when it was difficult to get a permanent mate. And if they do not, I can argue that their male ancestors propagated best by acquiring a mate in youth, reserving infidelity until later. Hence, it is difficult to establish whether a proximate or ultimate cause has determined the outcome. A true sociological explanation for the sexual strategies of adolescent males might be hidden by our adherence to evolutionary theory. Furthermore, the specific evolutionary mechanism alongside the sociological mechanism might be different from the one proposed.

An unmentioned benefit of evolutionary theory is reassurance. If cyclothymia was adaptive in the ancestral environment (by optimising peak function), then the risk of depression may have been increased in subsequent generations. Instead of ‘defective’ we can think of ourselves as highly adapted. When vandals wreck the playground where my children play I can reflect that this is normal behaviour for male primates. By exerting themselves against the environment they intimidate rivals – a pleasant zoological perspective preferable to saying that society is falling apart.

Abed, R. T. (2000) Psychiatry and Darwinism. Time to reconsider? *British Journal of Psychiatry*, **177**, 1–3.

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Comments on the UK700 case management trial

The UK700 Group (2000) presents a comparative cost analysis of intensive case management (ICM) *v.* standard case management for patients with severe mental illness. It failed to find any significant difference in duration of in-patient treatment between the two groups at 2 years, and the cost of care was thus roughly equal. The authors conclude that “the policy of advocating intensive case management for all patients with severe psychosis is not supported...”.

While the execution of the UK700 study is admirable in terms of its sheer number of subjects, the design is critically restricted by the very nature of the ‘intensive case management’ offered. Indeed, the mean number of contacts per client was 100 (s.d.=64) *v.* 64 (s.d.=30) in the control group; this equates to around one visit per week and one per fortnight, respectively. Comparison with our local (ICM) service shows that our case managers visit clients

far more regularly than this (daily, if necessary). While this might seem excessive, it appears that it is crucial to maintaining such patients in the community, and is ultimately cost-effective. Indeed, Preston & Fazio (2000) showed that for our ICM service, with a capped case-load per case worker of around 10 patients, and a mean number of annual community contacts of 164 (s.d.=20) *v.* 56 (s.d.=100) for non-intensive patients, in-patient bed-days fell dramatically (from a mean of 118 days (s.d.=113) per year before ICM, to 57 days (s.d.=91) in the second year of the ICM intervention). The control group showed no such reduction in bed-days, and the overall cost saving (factoring in the increased out-patient costs for the ICM group) at the end of the 2 years was AU\$801 475 for 65 patients ($P < 0.001$).

Thus, it is important that the precise nature of the intervention is examined before dismissing ICM as a cost-effective model of service delivery.

Preston, N. J. & Fazio, S. (2000) Establishing the efficacy and cost effectiveness of community intensive case management of long-term mentally ill: a matched control group study. *Australian and New Zealand Journal of Psychiatry*, **34**, 114–121.

UK700 Group (2000) Cost-effectiveness of intensive *v.* standard case management for severe psychotic illness. UK700 case management trial. *British Journal of Psychiatry*, **176**, 537–543.

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We read with interest the paper regarding the cost-effectiveness of intensive *v.* standard case management for severe psychotic illness (UK700 Group, 2000).

We feel very strongly that more comment should have been made on the topic of training (or lack of training) of the case managers involved. The findings of this large, well-designed trial are very similar to those of Muijen *et al* (1994), who found no difference in outcome between the use of community psychiatric nurses (CPNs) configured into case management teams *v.* CPNs working in a generic way. The main implication of this study was that merely reconfiguring services into different working arrangements provides no additional benefits to patients. However, it could be argued that benefits may accrue if training in research-based interventions is provided. Indeed, such training for CPNs and other health care professionals has

been developed in the Thorn and similar programmes, which focus specifically on providing skills in assertive community treatment, family interventions, psychological interventions etc. One could argue that these groups of skills, which comprise what is loosely known as psychosocial interventions, are essential to effective case management.

In the UK700 study, we note that the case managers received a 2-day induction course in case management and an unspecified amount of instruction in outreach practice given by a team leader in the assertive community treatment service from Boulder, Colorado. It seems to us that such training input is insufficient to provide the skills necessary to deliver truly effective psychosocial care. (The Thorn programme comprises 36 days of direct training plus the equivalent of 50 days of further study and project work.) We are therefore not surprised that the case managers with smaller case-load sizes could not improve on the outcomes attained by those working with the more average size case-loads.

Surely studies of training *per se* are now needed, with random allocation of case managers to training in research-based interventions or to standard practice, and measurement of outcomes for both the trainees (in terms of skills acquisition and knowledge gain) and their patients (in terms of clinical outcomes).

We are at present spending enormous amounts of money on training throughout the National Health Service and yet the vast majority of this training remains completely unevaluated. Although randomised controlled trials of training interventions will be costly, the price of not knowing whether training makes a difference is much greater.

Muijen, M., Cooney, M., Strathdee, G., et al (1994) Community psychiatric nurse teams: intensive support versus generic care. *British Journal of Psychiatry*, **165**, 211–217.

UK700 Group (2000) Cost-effectiveness of intensive *v.* standard case management for severe psychotic illness. UK700 case management trial. *British Journal of Psychiatry*, **176**, 537–543.

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Lithium and mortality

In their study of mortality in patients with affective disorder commenced on lithium, Brodersen *et al* (2000) paint an unfairly negative picture of the efficacy of lithium.

They compared mortality in patients with affective disorders who were started on lithium, irrespective of their compliance with treatment, with that of the general population. This gives a false impression that lithium could increase mortality. To assess the efficacy of lithium, they should ideally have compared those who were compliant with the treatment with those who were not and also with the general population, as Kallner *et al* (2000) did. The latter study clearly demonstrates that even though affective disorder patients have an increased mortality compared with the general population, lithium has a definite antisuicidal effect. Moreover, in unipolar depression, suicide rates increased only after patients discontinued lithium. These two studies also show how the methodology can affect the findings.

Brodersen, A., Licht, R. W., Vestergaard, P., et al (2000) Sixteen-year mortality in patients with affective disorder commenced on lithium. *British Journal of Psychiatry*, **176**, 429–433.

Kallner, G., Lindelius, R., Petterson, U., et al (2000) Mortality in 497 patients with affective disorders attending a lithium clinic or after having left it. *Pharmacopsychiatry*, **33**, 8–13.

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Authors' reply: Gracious & Falodun find that our study of mortality in affective disorder patients commenced on lithium (Brodersen *et al*, 2000) paints an unfairly negative picture of lithium's efficacy. They object to our intention-to-treat analysis of all patients commenced on lithium irrespective of compliance, which showed a significantly elevated standardised mortality ratio (SMR) of 2.5. They suggest that we should have compared compliant with non-compliant patients and with the general population, as did Kallner *et al* (2000).

Kallner *et al* actually reported – even in patients compliant with lithium – that mortality in general (SMR=1.6) and suicide in particular (SMR=14.0) were significantly elevated. They also found that mortality was even higher in non-compliant patients, a result which may very well be valid. However, comparison of compliant with non-compliant patients introduces a considerable selection bias, since patients are not randomly allocated to the two groups. Rather, patients with comorbidity, such as