Correspondence

EDITED BY STANLEY ZAMMIT

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A new dawn for the yellow journal?
I welcome the new Editor’s plans to bring the Journal firmly into the 21st century by making it intellectually stimulating but also inviting and readable for all (Tyrer, 2003). The previous Editor may have done much to improve the Journal’s impact factor to the scientific community by increasing its citation rate but what has not been studied are the views of the core readership. Should not a survey of readers be carried out to see what people think of the Journal and who reads how much and of what? I suspect the answer may be not much of very little, and that for most of us the Journal has a fairly short ‘wrapper-off to bookshelf time’.

The Journal’s core readers are many thousands of jobbing psychiatrists. We are looking for important new information that has bearing on our day-to-day clinical practice. Yes, we have the Psychiatric Bulletin, with its zippy and original offerings, but sometimes a subject needs a more academic and lengthy airing. Perhaps the readership could suggest subjects for editorials, and why not have each book review written by both an expert in the field and an ordinary reader, so as to capture different perspectives? I hope that the new Editor can increase the interaction between the Journal and all psychiatrists. Good luck.


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Editor’s response: Dr Haw is probably right in her assertions that the jobbing psychiatrist is likely to become the jobbing psychiatrist when reading the Journal – jumping from one item to the next with little close examination of the content – and it is clear from a recent paper that the ability of good ghostwriting to make an arresting impact on the reader pays dividends (Healy & Cattell, 2003). We are taking notice of this by trying to improve and shorten the titles of papers submitted to the Journal; prospective authors please note. However, Dr Haw has stimulated me to go further; I have a hypothesis that readers of the Journal might help me in testing. It is a hypothesis that is best kept blind at this stage, and I am disclosing it only to the Associate Editors. For each of the main sections of the Journal (editorials, debates, original papers, review articles, book reviews and correspondence) I invite readers to score on a four-point scale (0 = rarely or never read, 1 = seldom read, 2 = frequently read and 3 = regularly or always read) in which ‘read’ is taken to be a reasonably full examination of the article (a good test of this is that you could summarise the main impact of the article to others). Could you send your responses to me at the address below by the end of January 2004, and I will report the results – and the hypothesis – shortly afterwards.

Meanwhile, I hope our readers are aware of the open-ended responses to the open-ended questions paint a picture that is a world away from that reported by Turner and colleagues; how is this contradiction to be explained? First, the question of validity. Translation/back-translation of psychiatric inventories originating in the USA and Western Europe does not by itself overcome the category fallacy to which Kleinman (1987) pointed: particular phenomena may be identified in different settings but it does not follow that they mean the same thing in each setting. Moreover, refugees in distressed and insecure circumstances may be particularly susceptible to the demand characteristics of questionnaires. Second, and fundamentally, how human beings experience an adverse event, and what they say and do about it, is primarily a function of the social meanings and understandings attached to it. No psychiatric category captures this active appraisal and meaning-making.

Quantitative methodologies serving psychiatric categorisations risk a distorting pathologicalisation of refugee distress, with what is social and collective being reassigned as individual and biological (Summerfield, 1999). Turner et al caution against ‘the tendency of some to reject the diagnostic paradigm in refugee populations’, but they do not make a persuasive case here that they know better than the Kosovan refugees themselves, and that many of the refugees really do need psychiatric treatment. There is simply no good evidence to back their conclusion that refugee populations anywhere are carrying a major burden of clinically significant mental ill health. As the answers to my questions demonstrated, refugees see recovery as primarily something that must


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Mental health of refugees
Using quantitative measures Turner et al (2003) found that about half of a sample of 842 Kosovan refugees in the UK had post-traumatic stress disorder, with substantial comorbid depressive disorder and anxiety disorder. But there is more to be reported. I was involved in having a few open-ended questions tackled on to the study, tapping subjects’ own views of their health/mental health and what they saw as their most urgent priorities for recovery. Only a tiny number saw themselves as having a mental health problem of any kind, bearing out observations by refugee workers in the reception centres housing them that there was no interest in counselling. Almost everyone nominated work, schooling and family reunion as their major concerns. This chimes with what I and others have found in clinical settings with refugees over many years. Significant psycho-pathology is uncommon (Summerfield, 2002).

The responses to the open-ended questions paint a picture that is a world away from that reported by Turner and colleagues; how is this contradiction to be explained? First, the question of validity. Translation/back-translation of psychiatric inventories originating in the USA and Western Europe does not by itself overcome the category fallacy to which Kleinman (1987) pointed: particular phenomena may be identified in different settings but it does not follow that they mean the same thing in each setting. Moreover, refugees in distressed and insecure circumstances may be particularly susceptible to the demand characteristics of questionnaires. Second, and fundamentally, how human beings experience an adverse event, and what they say and do about it, is primarily a function of the social meanings and understandings attached to it. No psychiatric category captures this active appraisal and meaning-making.

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question about respondents’ main concerns. The responses to this question are in the respondents’ own words but if anxiety, tension, nervousness, stress or trembling are grouped together as likely anxiety symptoms, these were in fact the most frequent of the first priority problems and overall were reported by 21% (of 509 respondents). Sleep disturbance was reported by 16%, depression, hopelessness, sadness, mental problems and (poor) concentration by 8%. Many reported additional somatic complaints or general health problems, probably including a significant additional burden of psychological difficulty. Surprisingly, worries about family and friends were reported by only 17%. Concerns about work/economy (6%) and school/language (3%) were infrequent.

Rather than contradict the responses to the more structured questions, answers to these open questions reinforce our more quantitative findings.

**Declaration of interest**

This work was undertaken with funding from the National Health Service (NHS) Executive London, Research and Development Programme. The views expressed in this publication are those of the authors and not necessarily those of the NHS Executive or the Department of Health.

**S.W. Turner**

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**Effect of clozapine on mortality**

Duggan et al (2003) elegantly model the effect of clozapine on suicide, and highlight that 53 lives could be saved each year if all patients with treatment-resistant schizophrenia were offered clozapine treatment. The model does not, however, take into account the effect of clozapine on mortality from causes other than suicide. Clozapine is associated with weight gain, diabetes mellitus, and increased mortality from pulmonary embolism and other adverse events in addition to the risk of agranulocytosis (Walker et al, 1997). Fontaine et al (2001) estimated mortality due to clozapine-associated weight gain using data from the Framingham Heart Study. They conclude that the reduction in the suicide rate would be almost entirely offset over 10 years by the increased mortality associated with weight gain of 10kg. Walker et al (1997) report that mortality from causes other than suicide is increased with clozapine treatment, although overall mortality is lower. To completely model the effect of clozapine on mortality, the effects of the alternatives – active treatment and no treatment – on mortality, including suicide and adverse events related to treatment with other antipsychotics, should be included. These remarks do not detract from the main point that clozapine is still the most effective intervention for treatment-resistant schizophrenia, and mortality is only one outcome to be weighed in the overall risk–benefit analysis.

**Health care contact and suicide**

We read with interest the study by Gairin et al (2003), which highlighted the suboptimal working relationship between the accident and emergency department as a first point of contact and psychiatric services. Thirty-nine per cent of suicide victims got in contact with the accident and emergency department at some point in the last year of their lives and, according to the National Confidential Inquiry into Suicides in England and Wales, only a quarter of suicides are preceded by mental health service contact in that same period.

Although I appreciate the above point, I still think that contact with primary services has an equal if not greater role to play...
in reducing suicide, especially in those age groups whose members are less likely to attend the accident and emergency department at times of crisis, such as children and the elderly. Duckworth & McBride (1996) have reported that 80% of elderly suicide victims received no psychiatric referrals, and according to Harwood et al (2001), only 15% of elderly people who died by suicide were under psychiatric care at the time of death.

In our study, analysing coroners’ inquests of 200 cases of suicide in old age in Cheshire, 1989–2001 (Salih & El-Nimr, 2003), the role of primary care was emphasised. Interestingly, even those victims who were known to psychiatric services still preferred to contact their general practitioners (GPs) in the last few weeks before the fatal act.

One conclusion might be that people whose GPs acknowledged their mental health problems and cared to refer them to a specialist service were able to build a more meaningful therapeutic relationship with their doctors and readily contacted them as a final desperate act in the last period of their lives. A well-trained GP can act not only as an effective first point of contact but also a final one!


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**Assertive outreach in Tyneside**

Priebe et al (2003) have questioned whether the findings of the Pan-London Assertive Outreach Study can be generalised to assertive outreach services in the rest of the UK. Cornwell & Haveman (2003) evaluated the Newcastle and North Tyneside assertive outreach service using the same research instruments as those in the Pan-London study, so direct comparisons can be made. After 17 months of operation, the Newcastle and North Tyneside team was similar in size to the London mean (n=36) with a score on the Dartmouth Assertive Community Treatment Scale (Teague et al, 1998) of 3.5 (medium fidelity to the model). The team has care programme approach (CPA) responsibility and small case-loads, operates out of office hours but without dedicated in-patient beds and, at the time of evaluation, no consultant psychiatrist. It thus corresponds to a Cluster B team in the Pan-London study (Wright et al, 2003).

Patient contact frequency and duration was higher than the London mean with an average face-to-face contact of 94 minutes per week. There was also a greater focus on patient engagement, with this being the primary focus in 33.1% of contacts. Engagement with the service in assertive outreach patients was compared with a random sample of community mental health team (CMHT) patients on enhanced CPA. There was no difference in the level of engagement, raising the possibility that the focus on engagement was having an impact in a previously hard-to-engage patient group.

Similar to the London study (Billings et al, 2003), team members were fairly satisfied with their jobs and most were not experiencing high levels of burnout. Compared with two local CMHTs, assertive outreach staff reported a higher level of personal accomplishment, replicating the Pan-London study finding. Another common finding was that the assertive outreach staff rated low support from senior staff in the service as a greater source of stress than did CMHT staff. Team members also identified dual diagnosis as an unmet training need.

Newcastle and North Tyneside patients were more likely to contact patients to be white (86% v. 45%) or living alone (68% v. 52%). Most surprisingly, they had significantly higher levels of alcohol misuse or dependency (31% v. 16%) and drug misuse or dependency (40% v. 20%). This reflects the fact that the Newcastle and North Tyneside service may be managing a more severely ill patient group, with 93% having experience of compulsory admission and 70% having had an in-patient admission lasting more than 6 months. Using the mean MARC severity score (Huxley et al, 2000), assertive outreach patients in Newcastle and North Tyneside had significantly more severe problems than the sample of local CMHT patients on enhanced CPA (7.4 v. 3.4; t=6.35, d.f.=83, P<0.01; mean difference=4.0, 95% CI 2.7–5.3).

Wright et al (2003) have suggested that the London teams are assertive community treatment-like teams, but that the US assertive community treatment model may not easily translate to the UK context. The Newcastle and North Tyneside data contrast with both the London data and data from the UK700 study (Burns et al, 1999) in terms of the strong focus on patient engagement. Longitudinal studies are needed to determine whether this will actually enhance engagement and whether that improves outcome.


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**Treating maternal depression?**

Cooper et al (2003) reported a randomised trial with mothers with post-partum depression that compared routine primary care, non-directive counselling, cognitive-behavioural therapy (CBT) and psycho-dynamic therapy and found that psychological therapy improved maternal mood in the short term but the long-term effect was no better than spontaneous remission.
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The trial was generally well done and the procedures reasonably described. However, the researchers did not, from a cognitive–behavioural perspective, treat maternal depression. Cooper et al describe that treatment used cognitive–behavioural techniques but focused not on depression but on the management of mother–infant interactions.

Several randomised placebo-controlled trials have shown that CBT—when done properly—is an effective treatment for post-partum depression (Holden et al., 1989; Appleby et al., 1997; Chabrol et al., 2002) and for major depressive disorders (Hollon et al., 2002). There is an important relationship between post-partum depression and mother–infant interactions but it is not, by any means, the entirety or even the essence of post-partum depression. Although it is advisable to customise CBT to patients’ circumstances, exclusive use of one focus, such as mother–child interactions, is not a test of the therapy. If the goal is to change depression, one should treat depression. Thus, the title is inaccurate and the discussion of the lack of effect of CBT for maternal depression is misleading.


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Authors’ reply: There are many cognitive–behavioural therapies, with the precise form of the CBT shaped to the nature and context of the particular disorder. So, for example, CBT for panic disorder and CBT for bulimia nervosa (Hawton et al., 1989), although sharing a basic orientation and broad therapeutic principles, are very different from one another. The form of CBT in which we were interested had as its principal focus the mother–infant relationship and aspects of infant management. The reason for this was quite clear. It is well established that many forms of treatment for post-partum depression, including counselling (Holden et al., 1989), interpersonal psychotherapy (O’Hara et al., 2000), ‘cognitive–behavioural counselling’ (Appleby et al., 1997) and fluoxetine (Appleby et al., 1997), have significant antidepressant effects, but it has not been established that any of these interventions has an impact on the quality of the mother–infant relationship and child developmental progress, both known to be compromised in the context of post-partum depression. (The evidence for the efficacy of CBT in this context is, incidentally, less certain. Indeed, none of the three studies cited by Professor McGrath and colleagues in support of this form of treatment delivered an orthodox CBT; and one, in fact, was not a study of CBT at all, but of non-directive counselling.) We were interested in determining whether treatment that addressed the maternal role, as part of a wider supportive therapeutic relationship, would have wider benefits. The form of CBT we investigated was shaped by these concerns, and the discussion refers explicitly to this treatment and is, therefore, wholly apposite.

In several respects the findings of our trial were not what we had expected and were, to us, disappointing. However, the data were what they were, and it was our job to try to understand them. When the first trials comparing CBT with interpersonal psychotherapy for major depression were published in the 1980s, British clinical psychology revered with the chunterings of the CBT faithful whose instinctive reaction to the equivalence conclusion was to query the propriety of the trial CBT therapists. With time, a more mature position was evolved. The findings of our study, along with the broad failure of the trials of preventive treatments for post-partum depression, would seem to us to be cause for pause and reflection, rather than instinctive defensiveness.


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Cognitive–behavioural therapy as a treatment for psychosis

McKenna (2003) comments that Sensky et al (2000), in their trial of cognitive–behavioural therapy (CBT) v. befriending for the treatment of schizophrenia, found no advantage of CBT over befriending at the end of the 9-month intervention period. In his view, they were therefore not justified in making the claim that CBT is effective in treating negative as well as positive symptoms in schizophrenia. This assertion fails to recognise the different mechanisms by which CBT and drugs may benefit psychotic symptoms. While drugs are likely to produce a (relatively) immediate effect in altering neurotransmitter pathways, CBT (as is the case with other psychological therapies) is postulated to alter attachment-related memory (Gabbard, 2000) and develop an understanding of the illness. Cognitive–behavioural therapy utilises skills which, if successful, can be maintained by the patient long after therapy has ended. This would explain why Sensky et al (2000) witnessed a non-significant difference between the control and intervention groups at the end of the intervention period but a significant continued improvement in those receiving CBT (and not in those receiving befriending) at 9-month follow-up. It would not be expected that drugs would maintain a benefit 9 months after being stopped. Preliminary results of a 5-year follow-up of the cohort of patients in this study indicate that these gains in the CBT group have been maintained (D. Turkington, personal communication, 2001).
As a result of the distrust of psychological approaches, studies of CBT (e.g. Kuipers et al, 1997; Sensky et al, 2000) have invariably recruited patients whose symptoms are ‘resistant’ to medication. The fact that these studies have still shown significant improvement over either a control intervention or routine care is testament to the greater benefits that might be demonstrated if the patients enrolled in research were representative of those in clinical practice targeted for psychological intervention.

In any case, surely the question is not which is more effective, but how both pharmacological and psychological approaches could be combined for greatest effect.

Declaration of interest
The author has received grants from the Wolfson Foundation and the Association of Physicians of Great Britain and Northern Ireland for research into the durability of cognitive–behavioural therapy for the treatment of schizophrenia.


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Thank you for the debate on CBT and schizophrenia (Turkington/McKenna, 2003). I would like to make the following points.

First, CBT is not a single treatment – it contains many complex components and skills, and therapist variables must be an essential issue for careful evaluation as with all psychological therapies.

Second, befriending fares significantly better than ‘treatment as usual’ in much CBT research. McKenna dismisses this as placebo or ‘special treatment’. The fact of such significant improvement from befriending says something very serious about treatment as usual. Why should those suffering from psychosis not receive special treatment? The finding points to the need for more consideration of the (poorly termed) ‘non-specific factors in psychotherapy’ – factors clearly not treated as sufficiently important in basic care in psychosis (Paley & Shapiro, 2002).

Third, in the Sensky et al (2000) trial quoted, CBT patients maintained their (significant) clinical improvement at follow-up, whereas the befriending controls fell back towards previous levels. It seems that CBT gives the patients a thinking structure to help manage some of their symptoms in the longer term.

Fourth, many people believe that you cannot treat persons with psychosis as if they were suffering from something such as diabetes, for which a single remedy like insulin might be sufficient. McKenna’s pronouncement on randomised controlled trials is, therefore, open to serious questioning. The need adapted approach is the antithesis of the randomised controlled trial method. In the former, the treatment is individualised and intentionally different (qualitatively and quantitatively) from one case to another and may well change over time. A randomised controlled trial, equally intentionally, eliminates individuality in the treatment. Because the idea of relationships can be especially disturbing to patients with psychosis, psychological therapies can be seen by patients as threatening; therefore, the therapy has to be very carefully “administered” – individually and flexibly.

Fifth, there are other outcome measurements at least as important as psychiatric symptoms. The experience of treatment is very important, as well as quality of life measurements. Turkington emphasises the high take-up rate of CBT, far higher than uptake of medication in psychosis.

Sixth, thank goodness for CBT, just one of several ways for practitioners to re-discover some tools that enable them to relate to patients with psychosis. McCabe et al (2002) show how uncomfortable ordinary psychiatrists are without such tools when engaging with patients when the latter want to discuss symptoms.

Seventh, CBT and psychodynamic approaches overlap to a degree, at least as practised by Turkington (Martindale, 1998; Turkington & Siddle, 1998). Much has changed in psychodynamic therapy since the flawed studies of old. Modern psychodynamic approaches to psychosis have a much more flexible technique in engaging patients, and a greater and broader appreciation of mental mechanisms in psychosis.

Finally, relationship approaches in psychosis need encouragement, support and research. All psychiatrists need basic training in engaging with patients with psychosis. Research indicates that befriending might be a good place to start, but it is clearly not so easy – as the outcome of ‘treatment as usual’ indicates.

Declaration of interest
B.M. is Chair of ISPS (International Society for the Psychological Treatments of Schizophrenia) UK, a network the main objective of which is to promote psychological approaches to psychosis (treatment, education and research).


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Efficacy of antidepressant medication
The debate between Parker and Anderson & Haddad (2003) neatly summarised contemporary thinking on the question of antidepressant effect. It was a pity, though, that they provided no discussion of any historical perspective. The wonderfully clear account provided by David Healy (2002), for instance, shows how the marketing tail of psychopharmaceuticals now often wags the entire dog. The process by which this came about has been gathering momentum since the early 1960s. Healy explores its various causes and corollaries
in detail. It is not, he argues, due to any uniquely pernicious qualities of drug companies since similar trends can be seen in relation to some other types of therapy.

If this additional, temporal dimension had been taken into account, one suspects that Gordon Parker might have placed greater emphasis on one of the factors that he identified as contributing to the current situation: namely that ‘depression’ is currently modelled as a single entity, varying only in severity (p. 102). The term ‘depression’ is thus semantically equivalent nowadays to ‘abdominal pain’, not to ‘appendicitis’ or ‘peptic ulcer’. If trials of an antacid, say, were undertaken on patients selected for ‘abdominal pain’ the results obtained would sometimes be favourable, sometimes not. Debate over antacid usefulness would exactly parallel that over the effectiveness of antidepressants.

How did we get into this situation? It seems likely that a lot of the blame can be laid at the door of DSM–III (American Psychiatric Association, 1980), which explicitly aimed for reliability of diagnosis. Unhappily, there was an implicit downside. The state of the art in psychiatry, when DSM–III was under development, was such that reliability could be attained only at the expense of validity. Partly as a consequence of choices that were made then, this problem still remains. It is no good blaming the failings of clinical trials, the machinations of drug companies, the uselessness of antidepressants or reporting bias, for our predicament. The main fault lies in the consequences of a bad choice of diagnostic system, made by our predecessors for what seemed, at the time, good reasons. The remedy must lie primarily in seeing DSM for the hindrance that it is, and one day replacing it with a system that separates the ‘peptic ulcers’ from the ‘appendicitides’.


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Integrity and bias in academic psychiatry

The illuminating discussion by Drs Healy and Thase (2003) focuses on the magnitude of the pharmaceutical industry’s influence on academic medicine. However, this discussion needs to be taken a step further, and evaluated in relation to patient care. From my perspective, the central question is: ‘Does the influence of the pharmaceutical industry on academia result in biased knowledge?’ Professionals are charged with serving the best interests of patients/clients. In order to accomplish this, professionals need unbiased knowledge that can lead to an accurate risk–benefit assessment and serve to guide clinical decisions. If available knowledge is biased, decisions will be affected and clients will suffer accordingly. The frequently touted disclosure of potential conflicts of interest in academic publications is a small step in addressing the much more difficult question of whether existing knowledge is biased. Recognising potential bias is an initial step towards assessing and removing it from the collective knowledge used to make decisions in practice. For example, registering clinical trials is an approach to reducing publication bias (Dickersin & Rennie, 2003). Meta-analysis is an approach to removing bias from expert reviews of the literature (Beaman, 1991), although expert reviews still retain influence in the formulation of some practice guidelines (e.g. American Psychiatric Association, 1997). As the field moves more towards the implementation of evidence-based practice guidelines, the importance of removing bias remains central to providing optimal clinical care. If the extensive financial arrangements between industry and academia resulted in no bias to knowledge, I would probably agree with Dr Thase that no new policies are necessary to ‘safeguard our integrity’ (p. 390). However a recent systematic review and meta-analysis of evidence bearing on this question found ‘strong and consistent evidence … that industry-sponsored research tends to draw pro-industry conclusions’ (Bekelman et al, 2003: p. 463). The question now becomes, ‘What safeguards should be implemented to remove this bias from the knowledge that guides clinical practice (cf. Bodenheimer, 2000)?’ Commitment to our patients’ well-being requires that we act from this integrity.


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Good practice in publication of clinical trial results

As the name implies, ghostwriting is often hard to detect, so Healy & Cartell (2003) have made a valuable contribution to our understanding of this important subject by their measurement and thoughtful analysis of the practice. It is also refreshing to see such a balanced account which discusses both the benefits and potential dangers of ghostwriting.

Perhaps their most alarming observation is that the papers sponsored by the manufacturer reported ‘universally positive results’, which implies the existence of considerable publication bias. Such distortions to the published literature probably exist across all therapeutic areas and have been shown to distort the outcomes of meta-analyses (Tramer et al, 1997) and therefore to have serious implications for evidence-based medicine (Melander et al, 2003).

Readers may be interested to know that guidelines have recently been published which call on pharmaceutical companies to endeavour to publish results of all clinical trials of marketed products (Wager et al, 2003). The guidelines also provide recommendations to ensure that professional medical (ghost)writers are used appropriately so that their contribution can be beneficial rather than harmful. The Good Publication Practice (GPP) for pharmaceutical companies guidelines have been publicly endorsed by several drug companies and communications agencies. Further details are available at http://www.gpp-guidelines.org.

Declaration of interest

E.W. is an author of the GPP for pharmaceutical companies guidelines. He also makes a living as a freelance medical writer, which sometimes involves ghostwriting.


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One hundred years ago

Psychological medicine

Friday, August 1st. Dr. F. W. Mott (London) opened a discussion on Syphilis as a Cause of Insanity... A number of statistical data were cited by Dr. Mott from different observers and it was shown that the most recent observations concurred in assigning the first place among the etiological factors to syphilis. Juvenile general paralysis was almost invariably found to be a result of congenital syphilis and in 20 per cent. of the cases observed it was found that the fathers of the patients had had general paralysis. Dr. Mott concluded by adopting, for the purposes of raising a discussion, the thesis, “No syphilis, no general paralysis”.

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Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey
Mental health of refugees
D. Summerfield
Access the most recent version at DOI: 10.1192/bjp.183.5.459-a

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