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Editorial

Current emphasis on intriguing gains in our understanding of child psychopathology, though of major importance, should not weaken our attention as mental health professionals to "old" issues that tend to be ignored both because they render us helpless despite our increasing knowledge and because they seem so hard to remedy. I refer to those children who are subjected to severe disadvantaged conditions, both materially and psychologically, with parents who provide grossly inadequate or severely abusive care to them. These children may be considered for placement in foster care. Although there have been arguments against long-term foster care and in favour of short-term care, long-term foster care remains a persistent and important form of substitute care. The question must then arise as to whether permanent foster care can provide adequate alternatives. Practitioners are sometimes confronted with the detrimental effects of "oscillation" of children in and out of care, and reluctance as to the decision to place the child into permanent foster family care. Minty, in this issue's Annotation, provides an overview of the outcomes of long-term foster family care. Contrary to much current prejudice, Minty shows that there is ample evidence that long-term foster family care can provide a satisfactory upbringing for most children who lack adequate care in their birth families—provided that foster care begins early in life and continues at least until the late teens. Large-scale longitudinal studies have found that where psychosocial development is disturbed and educational progress deficient, these inadequacies are largely attributable to the social factors impinging on children before admission to, and discharge from, family foster care. Nevertheless, there is room for much improvement in the educational attainments of foster children, and there needs to be better training in the safe caring of children who have previously been seriously abused. Also, it is clear from Minty's overview that more methodologically sound research is needed to unravel the mechanisms that hamper or facilitate the healthy development of these children.

Another classical problem dealt with by Elliott in this issue's Practioner Review is school refusal. School refusal is not a psychiatric diagnosis but a sign or symptom that may be an isolated problem, or that may be associated with a multitude of possible diagnoses, including simple or social phobia, separation anxiety disorder, conduct disorder, or depression. Elliott claims that progress in clinical practice with regards to school refusal has been hampered by conceptual confusion. This review considers the distinction between school refusal and truancy, and school phobia and separation anxiety disorder. Elliott clearly points out that school refusal should not be considered to be a unitary syndrome but rather one that is heterogeneous and multi-causal. Consequently treatment strategies should be tailored to individual needs. Despite the fact that behavioural and cognitive behavioural approaches are widely accepted as central to the treatment of school refusal, treatments utilising a range of approaches seem more successful. Depending on its function, school refusal should be approached by treatment strategies that seem the most promising. True as this may be, it is alarming to realise that there seem to be so few controlled studies of the efficacy of treatments of school refusal. Knowledge of "what works" largely stems from clinical experience, and we know from meta-analytic studies of the efficacy of treatments in everyday clinical practice that it is hard to demonstrate the beneficial effects of nonstandardised treatments.

One of the most important prerequisites for advancing our field is the continuous development and testing of assessment procedures and diagnostic systems that are applicable across cultures. International collaboration is possible only when methodologies are available that can be widely shared for clinical, communicative, training, and research purposes. Although not all linguistic and cultural nuances are translatable, assessment procedures should be able to capture variations in psychopathology that are important within and between diverse cultures. To judge the cross-cultural value of assessment procedures, it is necessary to evaluate them in multiple cultures. The Child Behavior Checklist (CBCL) developed by Achenbach in the United States is a widely used instrument which has been translated at this moment into 58 languages. As some readers of this Journal will know, I have devoted quite some research to the cross-cultural testing of the CBCL. Any serious attempts to improve our understanding of the cross-cultural applicability of an instrument such as the CBCL are to be applauded. The paper by Hartman et al., in which the cross-informant model of the CBCL and the teacher version, the TRF, was evaluated by using Confirmatory Factor Analysis, is such an attempt. However, the strong claims of the authors concerning the construct validity seem to be based on grounds that are not so solid. It is a good custom of the editors of this Journal that each editor processes the papers that are allocated to him independently of the other editors. In this way it can be assured that papers that may create a conflict of interest are processed by an editor who is free of any prejudice. This procedure also creates the possibility for the editor whose turn it is to write the editorial to give his opinion on papers that he has not processed. The Hartman et al. paper is one that touches upon an area of my interest and I take the liberty to discuss a number of issues, arising from this paper, which I think are important. In this paper the syndrome scales of the CBCL, called cross-informant constructs because they were derived from data from parents, teachers, and adolescents' self-reports from large clinical samples, were tested in samples derived from 7 different countries and totalling 13,226 children

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whose parents provided information, and 8893 children whose teachers provided information. However, except for two samples, the samples were derived from the general population, whereas Achenbach derived his scales from clinical samples. He also discarded problem items with frequencies lower than 5% in his analyses. General population samples have much lower prevalences of problems than clinical samples, rendering many items with low frequencies. This may have influenced the analyses the authors used. This is especially problematic since the authors did not report how the samples were derived and how representative they are. For example, the Norwegian sample had a very low completion rate, making the sample unrepresentative of the general population, with a possible over-representation of subjects with few problems. In this way, the authors tested the original CBCL scales in samples in such a way that an adequate fit of the scales to their data was already unlikely to be present in advance. Another interesting issue is raised by the authors when they interpret the discrepancies between CBCL scale scores and DSM-III-R diagnoses based on parent interviews to be a result of insufficient construct differentiation of the CBCL. The authors may be right in that we need to improve our assessment methodologies, including the CBCL, but why not ask the same question with respect to the construct validity and construct differentiation of the DSM? For instance, cannot the enormous overlap between DSM anxiety disorders and depression, or between ADHD and Conduct Disorder, be considered equally well as a lack of construct differentiation? Also the authors rightly state that the CBCL and TRF were never intended to replace a clinical diagnosis. As with physical measures such as body temperature or blood pressure, behavioural questionnaire data may be used as one of many sources of data that need to be integrated by the clinician into an overall picture of the patient, including a clinical diagnosis. The authors state in their discussion that there is a tendency in clinical practice to assume that syndrome dimensions generated from the CBCL are indeed clinical ones. But what is the evidence for this statement? And what does it have to do with the psychometric properties of an instrument? Every carefully developed assessment or diagnostic procedure deserves to be used skilfully, and those who use it should at least be expected to have knowledge about its properties, including its limitations, but it is equally important that it should be tested in proper ways before coming up with strong claims about its value.

This issue contains three papers based on epidemiological studies. The first is a study by Horwood and Fergusson, who report from their Christchurch, New Zealand birth cohort. The authors investigated the relationship between maternal labour force participation and child academic achievement. The good news for working mothers is that the authors found small but significant associations between the extent of maternal labour force participation and the performance of the child on a number of standardised cognitive tests, as well as success in school leaving examinations. However, adjustment of a number of factors that are related to maternal labour force participation including maternal education, socioeconomic status, ethnicity, family composition, and child IQ, which meant that children from working mothers tended to come from relatively advantaged backgrounds, reduced the association between maternal labour force participation and child academic achievement to a nonsignificant level. The authors conclude that mothers in their sample had organised their labour force participation in such a way that once due allowance was made for family and social background, there was little consistent evidence to suggest that the extent of maternal labour force participation had either a beneficial or detrimental effect on levels of child academic achievement.

The second epidemiological study is the one by Dunn, Deater-Deckard, and Pickering, who analysed data from the Avon Longitudinal Study of Pregnancy and Childhood (ALSPAC), a large-scale longitudinal birth cohort study. The authors accessed the links between sibling relationships and mother–partner and parent–child relationships. They found that individual differences in sibling relationship quality were related to mother–partner affection and hostility assessed 4 years earlier, to contemporary parent–child negativity, and to indices of social adversity. Interestingly, they found that in stepfather families, mother–partner hostility was unrelated to parent–child negativity and sibling relationship quality.

The third epidemiological study is the one by Chadwick et al., who investigated the relationship between hyperactivity and reading disability. The findings suggest that hyperactivity and reading disability follow relatively independent developmental pathways, in the sense that one condition is unlikely to lead to the development of the other. The implication is that effective interventions directed at one of the conditions should not necessarily be expected to lead to improvements in the other. Rather than hoping that exclusively clinical or educational approaches will eventually succeed in meeting the needs of hyperactive and disruptive children with limited educational attainments, the findings underline the importance of recognising that successful intervention is likely to require appropriate and effective input of both kinds.

Two papers from the same Canadian research group, Boyle et al. and Hundert et al., describe a school-based prevention-intervention programme. The first paper concerns the methodological issues, and the second paper gives a report of the results. During the past decade, there has been growing interest in using schools for delivering mental health prevention programmes. This interest has stimulated a number of school-based evaluation studies that have demonstrated very modest beneficial effects over follow-up periods from 1 month to 2 years. Is this the best we can do? The answer is far from clear. A variety of methodological problems in design, sampling, measurement, and analysis characterise these studies and make it impossible to take away a coherent message about the prospects of prevention in the schools. Furthermore, much needs to be done on the programme side—at both theoretical and empirical levels—to identify the mechanisms that might allow programmes to achieve their objectives. Methodological problems can be addressed in future studies by adhering to well-published standards. The conceptual problems of what to do, with whom, and when are far more challenging.

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There is growing evidence that children exposed to a wide variety of traumatic events can develop Post-Traumatic Stress Disorder (PTSD). At any one time the potential number of children who may be suffering or are at risk of developing PTSD is enormous. However, comparatively few children receive appropriate interventions and many are not correctly identified. The study by Stallard et al. evaluated the effectiveness of a short self-report to identify children with PTSD. The results suggest that children with PTSD can be more accurately identified, thereby ensuring that limited mental health resources are focused upon those who need it the most.

Numerous previous studies, largely carried out in specialist centres, have suggested that children with epilepsy are more dependent upon their parents who show over-involvement towards them, and that this is of clinical significance. The study of Hodes et al., carried out with children with epilepsy who attended the paediatric department of a district general hospital, has two clinically relevant findings. First, that maternal over-involvement towards a child with epilepsy may be higher than that to siblings, but this may have little significance for the child's psychological adjustment. Second, amongst children with epilepsy who do not have major medical or developmental impairments, there may still be a small group who have significant difficulties. This was the group of children showing antisocial behaviour on the parental behavioural questionnaires together with educational difficulties sufficiently severe to have led to a statutory assessment by the educational authority, and whose mothers express high levels of criticism towards them. It would be appropriate to consider whether early intervention would be helpful for this group of children and their families.

The last two articles pertain to autistic spectrum disorders. The paper by Folstein et al. is an attempt to identify the distinct features of the broader autism phenotype that may result from the individual genes contributing to this condition. The last paper, by Hermelin et al., is both a single psychiatric case study and an interesting account of human creative skill and workmanship in general. Did you know, for instance, that both Cezanne and Monet were myopic? This paper describes and shows the paintings by a visually impaired and cognitively handicapped savant artist, Richard, who despite his autism and multiple physical handicaps from childhood onwards, has a remarkable talent for painting and an extremely good visual memory. Richard goes to his local library and looks at landscape photographs in books and magazines. He then goes home and after days, weeks, or even months, produces a picture from memory based on a photograph he has previously studied. In this case study, the paintings are compared with the models from which they were derived. The paintings transform perceptual reality, and are not a precise reflection of the models. Interestingly, there is nothing threatening in the paintings, and they show a happy, positive, and harmonious view of the world.

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