Correspondence

EDITED BY TOM FAHY

Confidential Inquiry into Suicide and Homicide by People with Mental Illness

Sir: Morgan (1997) offers welcome support to the National Confidential Inquiry into Suicide and Homicide by People with Mental Illness but expresses concern that the identification of suicides following inquest will lead to delays that will compromise the collection of complete and reliable data. Although we considered this possibility when planning the new notification system, there has been no evidence for it – consultants appear to have little difficulty in completing our questionnaires. Nevertheless, we agree that an early multi-disciplinary review following a likely suicide would benefit the Inquiry as well as local services.

The present way of identifying suicides (Appleby *et al*, 1997) was introduced because most eligible cases were not notified by the previous 'voluntary' system, and the aims of the Inquiry were therefore undermined. The use of public health records derived from inquest verdicts has allowed us to collect a comprehensive national sample, using an objective definition of suicide. The majority of inquests take place within a few months of a death; a great deal of suicide research accepts this delay, and its quality does not appear to be compromised. There may even be an advantage in asking mental health service staff about their care once their initial reactions to a death and their anxiety about an imminent inquiry have been allowed to settle.

Returns from the first years of the new Inquiry suggest that we shall collect detailed data on almost 1000 cases annually, and we are grateful to consultants who take the time to complete our questionnaires. They can be assured that the information they submit will provide a sound basis for our recommendations on clinical practice and training.

Appleby, L., Shaw, J. & Amos, T. (1997) National Confidential Inquiry into Suicide and Homicide by People with Mental Illness. *British Journal of Psychiatry*, **170**, 101–102.

Morgan, H. G. (1997) National Confidential Inquiry into Suicide and Homicide by Mentally Ill People (letter). *British Journal of Psychiatry*, **170**, 579–580.

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Psychiatry and the Church

Sir: I read with interest Archbishop Carey's editorial (Carey, 1997) and agree with Sims (from whom Carey quotes) that for too long psychiatry has avoided the spiritual realm. However, I would point out that the Church does not have a monopoly on addressing this area. If the individual is considered from a developmental point of view, organised religion as a psychosocial construct certainly has a role to play in promoting stability, validating and confirming an individual at their particular stage of development, and providing a socially legitimate world view. The Church is excellent at helping people in fulfilling their need to belong (Maslow, 1971) and is becoming better at addressing self-esteem needs. Similarly, it promotes conscientious/conformist self-sense (Loevinger, 1976) and Kohlberg's (1981) conventional moral sense. What it does not do is encourage growth from each stage of development (with respect to needs, self-sense and moral sense) to higher levels. For example, it does not promote the fulfilment of the individual's need for self-actualisation/self-transcendence. It could be argued that by making existence at a lower level more comfortable, it is actually inhibitory to further development.

There are certainly religions that do promote such growth and an increasing number of individuals at higher developmental levels, either within organised religious structures or who have made their progress alone. Wilber (1983) distinguishes between religions promoting vertical growth, which he calls 'authentic', and those that provide a lateral expansion or consolidation at a particular level of development, which he calls 'legitimate'. It is religion of the latter sort that Archbishop Carey describes.

Psychiatry needs to become a discipline capable of providing a service in the future. However, far from linking itself to an organisation which legitimately caters for the needs of an individual and society at a given developmental level, it should remain flexible enough to be of value to people at all levels of development, from the most regressed, psychotic individual at the pre-personal end of the spectrum, to the rare but increasing number of people who are suffering with disorders of mind at above 'normal' levels of development. While I welcome the cooperation between Christianity and psychiatry that Archbishop Carey proposes, I would be keen to ensure that this was not a strictly monogamous relationship, but that links are made between psychiatry and other potentially helpful psychosocial constructs, both legitimate and authentic.

Carey, G. (1997) Towards wholeness: transcending the barriers between religion and society. *British Journal of Psychiatry*, **170**, 396–397.

Kohlberg, L. (1981) *The Philosophy of Moral Development*. San Francisco, CA: Harper and Row.

Loevinger, J. (1976) *Ego Development*. San Francisco, CA: Jossey-Bass.

Maslow, A. (1971) *The Farther Reaches of Human Nature*. New York: Viking.

Wilber, K. (1983) *Eye to Eye: The Quest for the New Paradigm*. Boston, MA: Shambhala.

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Paroxetine withdrawal syndrome in a neonate

Sir: We report a suspected case of neonatal withdrawal syndrome after maternal use of paroxetine throughout the third trimester of pregnancy.

A 36-year-old woman had been treated with clomipramine for several years. She

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was commenced on paroxetine 30 mg during the sixth month of pregnancy, because of increased depressive symptoms, restlessness and decreased appetite.

A male neonate (4160 g) was delivered vaginally at 39 weeks' gestation after a normal pregnancy. The delivery was without complications. The newborn initially appeared alert (Apgar 9-10-10). At the age of 12 hours he was transferred to the neonatal department for observation because of increased respiratory rate (80 per minute) and jitteriness. During the next hours he developed increased muscle tone and tremor. When 'pulled-to-sit', there was a stiff flex in the elbows and the head was flexed forwards. There were no convulsions. The child was bottle-fed with formula without problems. C-reactive protein, haemoglobin, blood gases, blood glucose, electrolytes, ionised calcium and ultrasound of the brain were all normal. The symptoms decreased successively during the third and fourth day and the child was discharged at the age of four days with a normal neurological status except for sustained jitteriness. A follow-up at the age of four weeks showed a fully normal neurological status.

The serum paroxetine level in the child was 68 nmol/l at the age of one day, 75 nmol/l at two days and 23 nmol/l at three days of age. No serum or breast milk paroxetine concentrations were available from the mother, who chose not to breast-feed because of the drug treatment and earlier negative experience with breast-feeding. The mother's serum paroxetine concentration was 185 nmol/l 25 days after delivery, during continued treatment with 30 mg paroxetine per day.

We have been able to locate only two earlier reports of neonatal toxicity after maternal treatment with SSRIs; one for fluoxetine (Spencer, 1993) and one for sertraline (Kent & Laidlaw, 1995), with similar findings as in the present case. Poor neonatal adaptation with respiratory difficulties, cyanosis on feeding and jitteriness, were also associated with third-trimester exposure to fluoxetine in a recent study (Chambers *et al*, 1996). It is usually recommended that treatment with tricyclic antidepressants should be stopped or the dosage decreased before delivery to avoid neonatal withdrawal syndrome (Briggs *et al*, 1994). As very little information is available for the newer antidepressants, including SSRIs, they are not recommended during pregnancy. The increased popularity of these drugs in the treatment of various psychiatric disorders will, however, eventually lead even to pregnant women being exposed. The risk of neonatal withdrawal syndrome should in such cases be acknowledged, and dose reduction before delivery considered.

Briggs, G. G., Freeman, R. K. & Yaffe, S. J. (eds) (1994) *Drugs in Pregnancy and Lactation* (4th edn), pp. 192–195. Baltimore, MD: Williams & Wilkins.

Chambers, C. D., Johnson, K. A., Dick, L. M., *et al* (1996) Birth outcomes in pregnant women taking fluoxetine. *New England Journal of Medicine*, **335**, 1010–1015.

Kent, L. S. W. & Laidlaw, J. D. D. (1995) Suspected congenital sertraline dependence (letter). *British Journal of Psychiatry*, **167**, 412–413.

Spencer, M. J. (1993) Fluoxetine hydrochloride (Prozac) toxicity in a neonate. *Pediatrics*, **92**, 721–722.

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Subjective quality of life in schizophrenia

Sir: Franz *et al* (1997) discuss in their paper that atypical neuroleptics may have subjective quality of life advantages over conventional antipsychotics. Although their assertion may well prove to be true in the future, once we have more robust studies on this subject, I am not sure their conclusions are justified by the results of their study.

They applied the Munich Quality of Life Dimensions List (MLDL-GI) to a group of in-patients on day 10 of admission. They compared scores in those receiving conventional antipsychotics with those on atypical antipsychotics, and found the latter to have a significantly higher general quality of life. My first point is that they do not seem to have administered a baseline MLDL-GI (i.e. prior to commencing antipsychotics), so we do not know whether the higher scores in the group receiving atypical antipsychotics can be attributed to the medication. It could be possible that this group had higher scores to begin with. Second, the four subscales of the MLDL-GI include physical domain, mental domain, social life and everyday life. Although it may be valid to measure the physical and mental domain of in-patients 10 days after admission, I am not sure about the validity of measuring social life and everyday life in a group of in-patients recently admitted to hospital. Surely, social and everyday life must be measured within the context of the patient's own home and environment.

Franz, M., Lis, S., Plüddemann, K., *et al* (1997) Conventional versus atypical neuroleptics: subjective quality of life in schizophrenic patients. *British Journal of Psychiatry*, **170**, 422–425.

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Author's reply: The study to which Dr Agarwal refers was an open pilot study based on a non-experimental cross-sectional design. Assessment of subjective quality of life (SQoL) at baseline would be beyond the scope of such an explorative study. Restrictions to open studies have been discussed in the paper and the findings are, therefore, tested using an experimental double-blind repeated-measures design in a study that has just started. We definitely agree that different patients have different individual levels of satisfaction, which may influence their SQoL ratings.

The quality of life inventory is constructed in such a way that it can be used in whatever circumstances patients live. Hospital wards sometimes offer more of a 'home' to patients than the outside situation. Therefore, it is our opinion that validity of the assessment of SQoL in the domains of social life and everyday life does not depend on the place the patient is staying at the time. A patient always has a social and an everyday life. There are fellow patients, the hospital staff or visitors within the hospital, or there are friends and relatives back home. The assessment becomes valid if it really reflects only SQoL and no other dimension within this area of concern. In our study, patients were asked to assess their SQoL in different aspects of their present interpersonal contacts (social life subscale). Whether or not the assessment in this situation is *representative* of their predominant way of living is another question. The purpose of the study, however, was to investigate the present SQoL of newly admitted in-patients, subsequent to a psychotic exacerbation and taking different kinds of antipsychotic medication, within an exploratory research design.

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