LETTERS TO THE EDITOR

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The psychiatrist in primary care: let's look before we leap.

Sir – Dr Phelan in her review "The psychiatrist in primary care: let's look before we leap"[1] mentions three models of collaboration between general practitioners and psychiatrists. I would like to draw your attention to a variant as practised in the Scottish Borders over 25 years where 100% of psychiatrists collaborate with GPs.

Dingleton Hospital serves a mostly rural, scattered population of 104,000, there is no "Out-Patient Department" and all referrals go to the multidisciplinary sector team. Patients are seen for the most part in their own homes, preferably with a co-therapist. Only very small numbers are seen in the psychiatric hospital or GP surgery. Liaison takes place at regular meetings between the teams and GPs at the local health centres.

This home assessment and home treatment service has many advantages. There is improved access to care, with Dingleton's failure to attend rate running at about 5%. Patients often perceive it to be less stigmatising to be seen in the privacy of their own homes and patients' transport difficulties in this rural area are also overcome in this way. Home assessment with a co-therapist facilitates a holistic approach with improved opportunities for investigation and intervention in psycho-social and family factors. The GPs have access to the multidisciplinary team on a regular basis, with opportunities for collaboration, education, training and support.

Some of the disadvantages to collaboration models cited in Dr Phelan's article do not apply in the Scottish Borders. Because the system has evolved in this way over a period of time, administration and record keeping is not an issue. Accommodation problems are overcome by seeing people in their homes. Time spent in transit can be kept to a minimum by careful planning of visits. Hand free dictaphones and mobile phones can be used while travelling and time between joint work can be used to review cases.

Although the service at Dingleton has not yet been evaluated in the Scottish Borders, Burns et al[2,3] evaluated the Dingleton model in an urban setting. They found no differences in clinical or social functioning outcome but a substantial reduction in in-patient care and reduced total cost for a home based service. They believe that adequate investment in funding expanded teams able to operate in a truly multidisciplinary manner, would prove cost effective in the medium term.

Thus while appreciating Dr Phelan's caution about widespread introduction of psychiatric services in primary care settings, I would suggest the Dingleton model as one that could be extended to Ireland in both rural and urban areas. Given the Irish system where there are many single-handed GPs it is unlikely to be cost effective for multidiciplinary teams to visit every small practice on a regular basis. However, a negotiated time limited attachment may be applicable.

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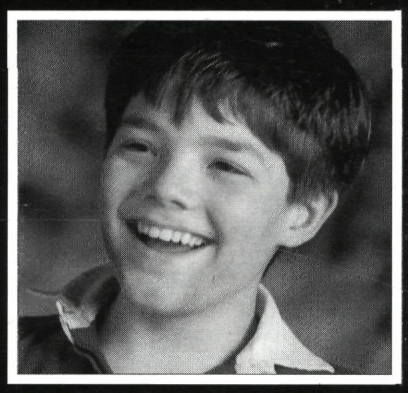
Author's reply to 'Commentary on *A sceptical reflection on the diagnosis of multiple personality disorder*'

Sir – I am grateful that Dr Putnam, one of North America's most eminent contributors to the multiple personality disorder (MPD) debate, should give me the opportunity to elaborate on my earlier paper in the journal.[1,2] Dr. Putnam begins by noting that multiple personality disorder (MPD) has a long history. So, of course, have demonic possession and exorcism, but these are also chimera.

Mistakenly, he says that I note a continuity between early 17th. century reports and modern cases. This is not the case. Modern cases are quite different. For example, earlier reports describe cases where the number of personalities were relatively few (often "dual personalities") and a reported history of childhood sexual abuse rarely reported.

Putnam says that my paper is in the tradition of prior critiques, perhaps encouraging a yawn, but the reality is that there are few, developed, sceptical critiques of MPD, hardly enough to warrant the categorisation "tradition". A recent and distinguished exception is to be found in North *et al.*[3]

He takes me to task for basing my speculations about the incidence and prevalence of MPD on straw-polls (letters published in the Bulletins of the Royal College of Psychiatrists and the British Psychological Society, circulated to 6000 psychiatrists and psychologists) and correspondence with colleagues. However, by his own admission there are, as yet, no published data to resolve arguments



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about incidence and prevalence.

I have, however, carried out a questionnaire survey of 400, randomly sampled, UK based, clinical psychologists and all UK based members of the Society of Clinical Psychiatrists. Only 15 respondents reported ever diagnosing or treating MPD. The total number of patients was 53, and 30 of these were diagnosed by just two clinicians.[4] Further findings from this survey of attitudes towards and experience of MPD in Britain are currently being analysed prior to publication.

It remains my conviction that MPD remains, almost exclusively, a North American phenomenon and I have already made some suggestions[5] about why this might be the case.

It is hardly a counter-argument to refer to Hacking[6] whose paper was based on putative cases in Britain more than 120 years ago. Dr Putnam does not cite another paper published in the same year, by the same author (7, p. 841)), where MPD is described as "strictly American with Canadian branch plants."

Dr Putnam goes on to say that "modern cases (*sic*) have been published in leading British psychiatric journals (*sic*)." However, to substantiate this he cites a single paper[8] which describes, in the most perfunctory fashion, a single and very tentative case.

It is disingenuous of Dr Putnam to lament that sceptics find enquiries about dissociative symptoms any more "leading" than standard questions about the presence of other pathognomic phenomenology. Firstly, "leading" questions are always to be deplored no matter what the differential diagnosis. I would have exactly the same misgivings about asking directly "Do you hear voices?" as I do about enquiries as to whether "there is another part of you that might like to talk to me."

More importantly, it is widely known that the diagnostic procedures involved in MPD are frequently of an emotional intensity and theatricality that makes the observer blanch. Kluft has described diagnostic interviews which last more than six hours before the first "signs" are elicited. Dr Putnam states that he does not believe that asking about hallucinations, obsessions or suicidal ideation could induce these symptoms in suggestible patients. I must disagree. The force of the epithet "suggestible" is to alert the diagnostician to this very possibility. It is commonplace that suggestible patients will report all manner of symptoms, somatic and psychological, given the demand characteristics of directive interviewing. This is the very raison d'être of the placebo effect. This is why diagnostic interviewing is a highly skilled, professional activity rather than a simple matter of question and answer.

The directive and frankly manipulative approach of believers in MPD produces "symptoms" as assuredly as believers in repression of chronic childhood sexual assaults, ritualistic satanic abuse and alien abduction provoke fantastic confabulations. A host of current and forthcoming publications testify to this disturbing epidemic.[9-11]

I do indeed believe that there seems to be a strong correlation between belief in MPD and in ritualistic satanic abuse. This is a falsifiable hypothesis awaiting investigation. However, it is not an hypothesis based on idle fancy. For example, a recent paper[12] derives from personal experience in a dissociative disorders unit in the US. where the consultant "presented a resumé that was long on expertise in the area of dissociative disorders, particularly multiple personality disorder created by satanic cult abuse ... the nurse manager had begun to sit in on [the consultant's] abreactive sessions ... and was alarmed at the coercive and leading nature of these therapy sessions." Patients have since retracted their accounts of abuse and abandoned their MPD symptomatology; lawsuits are pending which include claims of negligent diagnostic practices.

Dr Putnam says that I distorted what he said at the Amsterdam conference by implying that "diagnosis be made based on transient phenomena." I made no reference to diagnosis and referred only to the shifting grounds of definition of multiple "personalities". Where once we spoke of complex and integrated personalities we are now talking about more fleeting mood states.

Finally, Dr. Putnam refers to a number of studies which he says show significant central and autonomic nervous system differences between MPD patients and simulating control subjects.

It might seem that we are, at last, on firmer ground when biological markers are being sought. However, North et al. (pp.63-4) caution that "Even if laboratory evidence such as EEG markers of MPD becomes available ... We are brought back to the basic question: are these physiologic phenomena the essence of the mechanisms producing distinct personalities in MPD, or are they a by-product - through ordinary physiologic responses - of the extreme emotional displays of severe mood instabilities seen in patients tested for MPD?"

Putnam, Zahn and Post,[13] cited by Dr. Putnam, compared 9 MPD patients with 5 simulating control subjects on skin conductance differences between the right and left sides of the body. However, there was no difference between the differentiation of the alter egos produced by some controls and the most fragmented "reals".

Dr. Putnam also cites Miller *et al*[14] who looked at optical differences in five prominent areas of vision (visual acuity, refraction, eye muscle balance, ocular physiology and peripheral vision) in 20 MPD patients and 20 simulators. Miller *et al*, however, are sensibly cautious about their findings. They found no significant differences in acuity (near vision), the cylinder of the manifest refraction, ocular physiology or pupil size. The only significant differences were in measures of acuity (far vision) and visual fields. They go on to note that "individuals with MPD *may* (orig. ital.) experience some differences in visual functioning ... however, the results of the individual opthalmological measures are not consistent [across studies]." As Miller[15] has previously found, most consistency was obtained on subjective opthalmological measures.

Finally, Miller *et al* observe that left unanswered is the question of whether even such equivocal differences are specific to MPD. No comparisons with other psychiatric groups have yet been made. Nor has anyone even speculated about the processes which might underpin such visual differences.

I conclude by regretting that Dr Putnam thinks my sceptical accounts are on such a "low plane" but am grateful that at last he has acknowledged their existence.

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