Correspondence

Polypharmacy: how bad are we really?

The February edition of *The Psychiatrist* features a number of articles about psychotrophic polypharmacy. Taylor concludes that ‘rates of antipsychotic polypharmacy seem not to have changed’ while Langan & Shajahan conclude that ‘polypharmacy is an increasingly encountered clinical scenario’ without providing any evidence for this. Both authors assert that polypharmacy is by and large very undesirable with little evidence backing its use, with the possible exception of using aripiprazole as co-therapy with clozapine in order to reduce patients’ weight. We work in the Trust (now Health Board) in which Tungaraza et al did their research into polypharmacy and concluded that ‘only a third of individuals were on one psychotropic medication’. This implies poor adherence with National Institute for Health and Clinical Excellence (NICE) guidelines on schizophrenia which suggest that polypharmacy is best avoided unless there are exceptional circumstances and clozapine has been offered. We would like to explore the results of this last study as well as its underlying presumptions.

First, is it the patients? It is surprising that this is the first community study looking at polypharmacy and we obviously applaud Tungaraza et al for having conducted it. We also agree with the general sentiment that polypharmacy is by and large undesirable. However, the patient group they investigated is on the whole quite an ill cohort. The Schizophrenia Service in the old North East Wales Trust where Tungaraza et al conducted the study is moderately recovery focused. The standard of primary care is high and many people with good outcome and responsive schizophrenic illnesses are looked after in primary care, mostly on antipsychotic monotherapy. The patients in secondary care often include people who used to live in hospital settings, and have complex illnesses and problems that are often treatment resistant. They would all fall into the remit of having a severe and enduring mental illness as prescribed by the National Service Framework for Wales. In other words, these are patients with complex problems and significant comorbidity. Achim et al put the combined comorbidity of anxiety-type disorders at a staggering 50.1%. Dernovsek & Sprah remind us that 40% of people with chronic psychotic disorders have clinical levels of depression and 60% have anxiety symptoms. In a sample we examined, the rate of active symptoms of an anxiety disorder was 10%. These patients need treatment for their depressive and anxiety disorders as well as for their schizophrenia, which almost always requires additional medication on top of the antipsychotic. In summary, the patients that are seen in community care today are a cohort of patients with complex and often treatment-resistant problems and with high levels of comorbidity.

Second, is it the guidelines? Guidance is only guidance, so there is an expectation that exceptions may occur. The main problem with guidance, however, is that it is only as good as the evidence that it is based on. Lack of evidence for efficacy is not the same as evidence for lack of efficacy. Because something has a poor research base does not automatically make it unreasonable or ineffective. We agree that there have not been many large-scale studies looking at polypharmacy, but there have been some studies that suggest that polypharmacy might be useful in limited situations and circumstances. Mortimer reaffirms that ‘amisulpride has the best evidence as an affective adjunct to clozapine treatment’. The other problem with evidence-based research that primarily considers randomised controlled trials is that it always looks at an average. This does not take into account the fact that although some patients will have a good effect from an intervention, others will have no effect from a particular intervention even if the overall effect size might be average. This means that to get an average effect size we need some people who had particularly good effects and others who had no effect. Although we admit that we often do not know who is going to respond particularly well, it is clearly necessary to find inventive solutions for people whose illness will otherwise remain treatment resistant. Additionally, the recent update of the NICE guidelines for schizophrenia take into account the increasing amount of evidence that suggests that second-generation antipsychotics are not a homogeneous group and some of them are clearly more effective than others. This evidence is emerging and has been changing the way in which psychiatrists practice all around the world.

Third, is it the drugs? Karunakaran et al have shown that clozapine/ aripiprazole combinations can be a useful regime to allow people on clozapine to reduce their clozapine dose without a loss of efficacy. Similar studies have shown such effects for amisulpride and quetiapine. The service that Tungaraza et al researched in North Wales has a specialist clozapine clinic and a high number of patients on clozapine (143 in February 2010). Many of them are enabled to reduce their clozapine dose and thus their clozapine-related side-effects by introducing a second antipsychotic. We question whether this should be seen as good practice rather than condemned as polypharmacy.

In conclusion, rather than lamenting that only a third of patients studied in North East Wales were on monotherapy, we think it would be more appropriate to applaud the fact that no patient was on more than two antipsychotics. Most of the patients on two antipsychotics would have been on clozapine and either aripiprazole or amisulpride, which is used in order to reduce side-effects caused by clozapine. Additional psycho- tropic medication would primarily include antidepressants used to treat depression and anxiety disorders in our patients with schizophrenia or mood stabilisers in bipolar affective disorder, both following current NICE guidelines. This means that we have followed NICE guidelines even if it means using polypharmacy. We therefore feel that in many of the cases that sound like undesirable polypharmacy there may actually be very good reasons in accordance with guidance why two or three psychotrophic drugs are being used. This is in order to benefit patients whose side-effect profile can be improved and their debilitating anxiety or depressive disorders treated on top of the treatment for their schizophrenic illness. We would therefore like to see a more balanced view with regard to
polypharmacy in a patient group that is often non-responsive to medication and usually has complex comorbidities.

Furthermore, we would dispute the notion that Taylor suggested: that non-medical prescribers may improve the situation. We have concerns which are rather in contrast to this. Non-medical prescribers are more likely to follow guidance but if guidance changes or is flawed, as we have seen with the NICE guidelines for schizophrenia, non-medical prescribers are more likely to lack the flexibility to respond adequately to these challenges and may therefore contribute to suboptimal treatment rather than improve it. Lastly, we wholeheartedly embrace the recommendations that Langen & Shajahan put forward, which ask for the regular review of all instances of polypharmacy including clear documentation as to why polypharmacy is continuously used.


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Let’s not throw the baby out with the bath water

Tyler et al’s study on the effectiveness of crisis resolution and home treatment teams (CRHTs) is a good addition to the debate on the evidence base of these teams. The authors concluded that the introduction of CRHTs in Cardiff was associated with an increase in compulsory admission, a decrease in informal admission and bed days, and an increase in the number of suicides in the area covered by CRHTs. In as much as the authors can be commended in their fairly robust appraisal of the research methodology employed, nonetheless it is hard to overlook the major deficiencies in the study design.

The findings, but for the increased rate of suicides, are not new, and need not reflect negatively on CRHTs. The authors highlighted that none of the victims of suicide were under the care of the CRHT at the time of their death.

The often-cited North Islington Study also showed that compulsory admission was not significantly reduced; however, in recent years a number of possible explanations for this finding have emerged. It is highly likely that a sizeable proportion of the patients who were compulsorily admitted were not only severely ill, but lacking in insight or capacity to consent to a treatment plan. Gould et al’s study on patients presenting with acute onset of first-episode psychosis concluded that in this group of patients, although living in an area in which alternatives to admission were well developed, compulsory admission was still high.

Crisis resolution and home treatment teams exist within complex local systems and politics and it is inevitable that other key services such as the traditional community mental health team, in-patient service, mental health liaison team, primary care gateway service, assertive outreach and early intervention team in psychosis will play key roles in its effectiveness. An interesting enquiry is whether such specialist teams working jointly with CRHTs will be able to prevent compulsory in-patient admissions for these severely ill patients more effectively than CRHT alone.

A Cochrane review continues to gather increasing long-term evidence to support the implementation of the CRHT worldwide. The evidence for reducing informal admission, bed usage and patient satisfaction has been replicated in various studies. Crisis resolution and home treatment teams should not be seen as a government-ensured innovation, but rather a viable and acceptable approach to treating people with severe mental illness. Evidence suggests that improvements in outcome of CRHTs are most convincing where psychiatrists have embraced this development and use their informal power to support them. Let’s not throw the baby out with the bath water.

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Confusing title and misleading assumptions

The title and the aim of the study by Tyler et al state that they had made a controlled comparison of two crisis resolution and home treatment teams (CRHTs). However, reading through the
The second CRHT was not in existence in the two time periods when the data were collected.

There are inaccuracies in the reporting; in the results section the authors report duration of bed use and refer to Table 2 which is occupied bed days. The duration of bed use and number of bed days are two different measures. Also, numbers do not add up in Table 2, however they do add up in Table 1.

In summary, the study reports no statistically significant difference in number of admissions or number of bed days following introduction of a CRHT when compared with an area without the team. However, raw figures demonstrate a decrease in informal admissions and bed days, and an increase in formal admissions in the area where there is a crisis team.

The authors make assumptions that the increase in compulsory admissions following the introduction of a CRHT was because some patients who would otherwise have been admitted to the hospital and then detained under Section 5(2) of the Mental Health Act were taken on by the CRHT and then getting admitted through mental health assessments and on a section. This assumption is not supported by the data-set or anecdotal evidence.

The study also found that there is an increase in suicide in the catchment area where there is a CRHT. However, none of these suicides happened when the patients were under the CRHT. As it stands, it is difficult to explain that the increase in suicide is somehow connected to the introduction of the CRHT.

It is safe to assume that in Cardiff as the experience of the team grows and the teams get more embedded they will have a significant effect on both number of admissions and bed usage as demonstrated by the National Audit Office report.2


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Authors’ reply Dr Mahadun & Sadiq are right on both counts. The title ‘A controlled comparison of the introduction of a crisis resolution and home treatment team’ should be the proper title of the article. The top two lines of data in Table 2 are also incorrect, and should read as shown here.

These data illustrate an effect of the crisis resolution team (CRT) overall in reducing bed days. We agree that the interpretation of the data cannot provide a causal pathway between the experience of seeing a CRT and then having a higher risk of being admitted compulsorily, as we were not following the experience of individual patients through the care system. However, it is a reasonable hypothesis to posit that the increase in compulsory admissions following the introduction of the CRT was a direct consequence of the change in service provision across the trust. The same conclusion might be made about the change in suicide rates, but of course we stress that this was not a significant difference. The conclusion we are putting forward, and this was not one we were expecting when we started the study, is that the service configuration that follows the introduction of a CRT is one that tends to limit admissions and may possibly be directly associated with more compulsory admissions and more suicides. This is an important hypothesis to test, but we agree it cannot be confirmed from our data.

Drs Ogunremi & Talat argue from the position of enthusiasts for the CRT policy and we do not disagree with their opinion that it is a ‘viable and acceptable approach to treating people with severe mental illness’. But all policies have to be tested and evaluated, and clearly all your correspondents would agree that if a CRT, for whatever reasons, makes decisions that lead to greater compulsion and more suicides in either the shorter or longer term, their implementation should be questioned. In this context it could also be argued that a reduction in bed usage is probably a poor outcome measure; quality of life, patient satisfaction and clinical improvement over a reasonable period (e.g. probably about a year to cover all aspects of an illness episode) are much preferred.

Declaration of interest
J.M. is currently a consultant in one of the Cardiff crisis resolution teams.

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Improving physical health monitoring in psychiatry – change we need?

Gonzalez et al highlight the very important issue of routine blood testing of patients on antipsychotics, which currently is under-monitored in a psychiatric setting, particularly so in out-patients. However, the audit was conducted between 2004 and 2005, and it might not represent the current practice in UK. But physical health monitoring of patients with mental health problems still remains unsatisfactory. Some studies in 1986 and 2004 reported recording of physical examination carried out on admission by psychiatric trainees to be ‘uniformly poor’ to ‘variable’.2 The age-adjusted annual death rates from all causes among individuals with a psychiatric diagnosis is two to four times higher than in the general population.3 This makes it even more pertinent for us to take extra measures in order to provide the best care for our patients.
In 2009, we completed an audit with colleagues at North Derbyshire Mental Health Services NHS Trust. The results illustrated that physical examination on admission to an in-patient unit increased from 67 to 83% by the end of the audit cycle. The reasons for not examining patients varied from ‘transferred from medical ward’ to ‘team to review tomorrow’. We encouraged the consultant-led teams to take more responsibility in ensuring that a complete physical examination (including investigations such as baseline bloods and electrocardiograms) is done for every patient admitted to the unit, and also recommended quick and easy access to physical health equipment, especially out of hours.

While I appreciate the emphasis of the Royal College of Psychiatrists on increasing the awareness of physical illnesses in our client group and the importance of their detection, I believe actions speak louder than words. Our under-performance in this area is due to problems at multiple levels. Training in psychiatry has become completely detached from medicine. We need to increase psychiatric trainees’ exposure to medicine by incorporating physical examination in the MRCPsych curriculum nationally and possibly offering a compulsory rotation in medicine during core training. We also need to change the ethos within psychiatric teams (in-patient and community based) by encouraging psychiatric nurses to also improve their medical skills.

It can be quite tricky in out-patients to address physical health problems while also managing mental health issues. Like other services, why can’t we have a dedicated nurse at the out-patient clinic who records blood pressure, measures height, weight, hip and waist circumference, and does all the routine blood tests for every patient, before they go in to see the doctor?


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Attitudes to ECT — a nebulous concept with important implications

Kinnair et al.8 raise some important points regarding training and teaching students on electroconvulsive therapy (ECT). Some of the questions used, particularly in assessing students’ attitudes to ECT, have been used in similar studies9,10 and would therefore carry some face validity. Clearly, it is important to consider the sequence of teaching events in any instructional design of a teaching block; however, I would disagree with some of the authors’ conclusions. With such a descriptive study design, the use of binary variables (yes/no answers) and the absence of P-values, one cannot infer any significant differences between Groups A, B, C and D with regard to changes in knowledge of ECT. The relatively smaller sample sizes of these groups compared with the baseline sample would make a Type I error more likely, that is any differences seen could be due to chance.

Intuitively, one would expect either Group B or C to do better with their follow-up knowledge questions, simply based on constructivist theory (i.e. building on previously attained knowledge). In Group B, a certain amount of knowledge will be acquired from simply watching an ECT procedure. The authors did not state how soon after the lecture and/or witnessed ECT event, students were asked to complete the questionnaire. One cannot therefore assume a limited benefit (in terms of knowledge obtained) from watching ECT before receiving a lecture, as this could equally be due to having the lecture closer to the questionnaire.

I find the absence of any tables to explain the results of their attitudes questions somewhat disappointing. Previous research in this area has shown that medical students’ attitudes to ECT can be improved by receiving a didactic lecture on ECT,4 as well as observing an ECT application (either live or a pre-recorded video)5, so it is not surprising that Group B showed improved attitudes on two of the questions compared with Group D. It would have been interesting to know how many of those students who had seen either One Flew over the Cuckoo’s Nest or Beautiful Mind belonged to Groups A or D, which could explain the different response with regard to question 10 – ‘I would agree to have ECT if I was depressed’.

Better knowledge of ECT, particularly self-perceived knowledge, does not necessarily imply better attitudes to ECT.6 If we want to attract more students to our profession, further research in this area is essential to help unveil some of the secrets behind students’ negative perceptions, attitudes and prejudices with regard to ECT.


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Concerns over professional boundaries remain unresolved

The Executive Committee of the Spirituality and Psychiatry Special Interest Group (SPSIG) of the Royal College of Psychiatrists has made a rather late response to Harold Koenig’s editorial11 published in this journal in 2008. We were co-signatories to a letter1 that was highly critical of some of
Koenig’s proposals. These were that a spiritual history should be taken from all patients, even where the patient is resistant to this; that patients’ healthy spiritual or religious beliefs should be supported and unhealthy beliefs should be challenged; and that under some circumstances it is appropriate to pray with patients. Although we fully accept that it is sometimes appropriate to explore spiritual or religious issues with patients, we remain seriously concerned that these more controversial practices breach fundamental professional boundaries. Furthermore, the College appears to be lending tacit support for them.4

Although our letter3 has been referenced in a number of publications by members of the SPSIG Executive Committee (e.g. their recent book),5 our concerns over boundary violations remain unanswered. Indeed, Larry Culliford6 has rather exacerbated our concerns by suggesting that boundary breaches might a good thing; that this might have spiritual benefits for clinicians; and that boundaries are in any case illusory.

The General Medical Council position on these matters is clear. Their supplementary guidance on personal beliefs7 states:

You should not normally discuss your personal beliefs with patients unless those beliefs are directly relevant to the patient’s care. You must not impose your beliefs on patients, or cause distress by the inappropriate or insensitive expression of religious, political or other beliefs or views. Equally, you must not put pressure on patients to discuss or justify their beliefs (or the absence of them).

In our opinion, it is obvious that Koenig’s contentious recommendations are not compatible with this guidance. Although Dein et al3 acknowledge the risk of boundary breaches, and advocate extreme caution in praying with patients, they do not reject the practice. Indeed, it is implicitly left to the individual clinician to decide whether to pray or not. We can think of no example of a permissible practice in one-to-one clinical interviewing that is acknowledged to be hazardous to patients to this extent. We cannot understand why SPSIG does not simply state that prayer with patients in clinical settings is unacceptable. We feel that it would be helpful if they explained.

Declaration of interest
The authors have a range of personal convictions, including atheist, Buddhist, Methodist, Roman Catholic and non-denominational faith.


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Correction

Controlled comparison of two crisis resolution and home treatment teams. Psychiatr 2010; 34: 50–4. The title of this paper should read: A controlled comparison of the introduction of a crisis resolution and home treatment team.

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