

Correspondence

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Craddock and Mynors-Wallis's assault on thinking

The validity and utility of psychiatric diagnoses have long been a bone of contention between and within different professional and patient groups. This was clearly shown by the nearly 70 rapid responses to a 2001 *BMJ* article that proposed that post-traumatic stress disorder was a social construct with little clinical utility.¹ The responses were emotive and polarised, with an equal proportion of patients and professionals in each camp: those who felt diagnoses were important and life-changing, and those who felt outraged and negated by the medicalisation of social suffering. In their recent editorial Craddock and Mynors-Wallis² frame this diagnostic debate in terms of 'benefits and limitations'; possible 'disadvantages' are acknowledged but mention of potential harms is conspicuously absent.

They advocate 'embracing complexity', but for the rest of their article this does not ring true. They reel off the standard list of apparent advantages to diagnosis – providing reassurance and reducing blame, shame and stigma – but without reference to research findings. (Nowhere in their paper is any patient-led or collaborative research cited.) Also conspicuously absent in their list is the necessity of a diagnosis to guide treatment. Is this a tacit acknowledgement that there is little evidence to support such a claim and that, in mental healthcare at least, 'common factors' linked to the therapeutic alliance, alongside extra-therapeutic factors, explain the majority of treatment variance?³ In spite of this, they then go on to assert 'there are no issues about diagnosis (or indeed treatments) that are unique to psychiatry' (for the counter-argument see Bracken *et al*³ and related correspondence).

Their erroneous linkage between diagnosis and stigma reduction stands out as particularly misleading. There is now an abundance of evidence, including a comprehensive review published last year in this journal,⁴ that biomedical framing of mental illness tends to increase personal and social stigma and public desire for distance.

The authors may counter that a diagnosis does not imply biological causality, and they seem to endorse the standard biopsychosocial frame of reference. The problem is, as Roland Littlewood⁵ points out, it is more or less impossible to hold a 'personalistic' view of the self as agentic and intentional while at the same time subscribing to a 'naturalistic' view of being a product of biology, or even of the environment. One position always elides into the other. If this is true for professionals, it is certainly true for patients. And the dominant cultural understanding of diagnosis is that of biology, as it is with *de facto* psychiatric practice.⁶

Craddock and Mynors-Wallis seem to want to be reasonable; identifying themselves, with other psychiatrists, as 'reflective and tolerant of strongly opposing views and ideologies'. First, however, they resort to an unsubstantiated moral and emotive appeal to their position: 'This can be to our patients' disadvantage if we allow these views [i.e. critical of standard diagnostic practices] to be unopposed

by suggesting that our patients are somehow less deserving of a psychiatric diagnosis than a physical diagnosis'. Then, just in case we are still equivocating, using the College's Good Psychiatric Practice to bring us into line (as if this too was some ahistorical and acultural document), they pronounce: 'This [use of standardised diagnosis] is not an issue of personal choice for a practitioner. It is a professional responsibility to the patient'. Their penultimate reference (entitled 'Time to end the distinction between mental and neurological illnesses') betrays their own ideological foray.

Of course, if diagnosis is understood in the broader sense of a thoroughgoing, descriptive and summative attempt at understanding a patient's struggles, respectful of personal meaning and unblinded to issues of power and social context (the latter often being harder to change than biology, in which it may then of course be reflected⁷), then we too might endorse Craddock and Mynors-Wallis's position. But in terms of a reverence to standardised manuals (whether DSM or ICD) that lack true nosological validity, even by their own standards, and whose utility is at best questionable,⁸ and which in effect serve to obscure key psychosocial antecedents,⁷ we would also argue that our patients deserve better.

There is little space for wider critique (for this, see Timimi⁸) and discussion of alternatives here, but if mature science is comfortable with dissent and debate (and indeed sees this as necessary for progression) this editorial seems a misplaced attempt to close down discussion – first through unsubstantiated emotive appeal, then through the threat of professional censure – in order to maintain a façade of professional consensus. While we might wonder what lies behind such a move, we would advocate a more far-reaching attempt at embracing complexity. In particular, as we have argued elsewhere,³ in attending to issues of power, meaning, social context and the therapeutic alliance, alongside but not reduced to biology, we have much to offer the rest of medicine, which is also beginning to grapple with related issues.^{9,10}

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- 2 Craddock N, Mynors-Wallis L. Psychiatric diagnosis: impersonal, imperfect and important. *Br J Psychiatry* 2014; **204**: 93–5.
- 3 Bracken P, Thomas P, Timimi S, Asen E, Behr G, Beuster C, et al. Psychiatry beyond the current paradigm. *Br J Psychiatry* 2012; **201**: 430–4.
- 4 Angermeyer MC, Holzinger A, Carta MG, Schomerus G. Biogenetic explanations and public acceptance of mental illness: systematic review of population studies. *Br J Psychiatry* 2011; **199**: 367–72.
- 5 Littlewood R. *Pathologies of the West: An Anthropology of Mental Illness in Europe and America*. Continuum International Publishing Group, 2002.
- 6 Barrett RJ. *The Psychiatric Team and the Social Definition of Schizophrenia: An Anthropological Study of Person and Illness*. Cambridge University Press, 1996.
- 7 Krieger N. 'Bodies count', and body counts: social epidemiology and embodying inequality. *Epidemiologic Reviews* 2004; **26**: 92–103.
- 8 Timimi S. No more psychiatric labels: campaign to abolish psychiatric diagnostic systems such as ICD and DSM. *Self & Society* 2013; **40**: 6–14.
- 9 Das A. The 'rest of medicine' and psychiatry: why paradigms would differ. *Br J Psychiatry* 2013; **202**: 463.
- 10 Sharpe M. Psychological medicine and the future of psychiatry. *Br J Psychiatry* 2014; **204**: 91–2.

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Authors' reply: The letter from Rodger *et al* uses our editorial to rehearse the well-worn arguments of the small group of so-called 'critical psychiatrists' who are active and vocal in criticising core aspects of the practice of psychiatry as a medical subspecialty underpinned by science. The views expressed in the letter are mainly tangential to the views we expressed in our editorial and the authors have made assumptions and accusations that are unsupported by our text.

We are very keen to encourage informed and constructive debate to advance patient care and mental health. However, it is important to make a distinction between the freedom that is properly enjoyed in academic debate and the responsibilities that come with professional practice. At present, those who work as psychiatrists are expected to practise in accordance with evidence-based standards. The standards we adhere to will of course change over time as the evidence base develops. This is expected by patients and colleagues and required by regulators.

We continue to believe that our patients are best served by seeing psychiatrists who are trained to make a thorough assessment, come to a diagnosis and shared formulation with the patient of their problems and use this to draw up an evidence-based management plan. It seems strange to us that this should be surprising, contentious or upsetting to the authors of the letter.

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Concerns regarding an evaluation of MTFC-A for adolescents in English care

We are writing to highlight concerns regarding conclusions offered by Green *et al* in their evaluation of Multidimensional Treatment Foster Care for Adolescents (MTFC-A) relative to usual care for at-risk youth in English foster care.¹ We commend the authors for undertaking an independent review of MTFC-A. However, we offer some observations to help contextualise the efficacy of the evaluation with respect to the primary conclusion that MTFC-A did not result in better outcomes than usual care.

Green *et al*'s evaluation employed a two-arm, single-blinded (assessor) randomised controlled trial embedded within an observational quasi-experimental case-control study. An intent-to-treat (ITT) analysis was employed specific to the MTFC-A versus usual care comparison. The authors state that the study was intended to be powered at $\beta=0.80$ to detect half a standard deviation difference between ITT and usual care (with a target n of 130), and was powered $\beta=0.95$ to detect the same effect between ITT and usual care in the quasi-experimental study (with a target n of 90). However, the target allocation for the trial was not met. The trial randomly allocated only 34 participants ($n=20$ MTFC-A and $n=14$ usual care). Based on these numbers, we estimate the study was actually powered at $\beta=0.29$ in the ITT analysis to detect half a standard deviation difference between conditions assuming equal variances, and at $\beta=0.28$ assuming unequal variances.

Substantive conclusions therefore seem to be based on a substantially underpowered design (as far as we can tell from the detail presented in the original manuscript). Further, the quasi-experimental arm was described as a case-control design. However, it was not a matched case-control design. This is evident from multiple baseline differences between groups, some of which remained after an intensive set of propensity-score

weights was applied and after elimination of cases with probability of assignment to MTFC-A above 0.95 and below 0.05. Depending on the distribution of assignment probabilities, this may have resulted in relatively limited 'data trimming' in order to attain desired allocation probabilities near 0.50. The observed differences included not only age but also the primary outcome scores.

Notwithstanding concerns regarding statistical power for the trial, the authors reported intervention by baseline risk interactions in the only adequately powered arm of the study (see Table 5). Given prior demonstration of MTFC-A intervention by baseline risk interactions,² these results may have been more appropriately presented as a hypothesised replication. Statistical power is also a concern for the reported analyses of offending; $\beta=0.034$ to detect the observed ITT odds ratio of 1.24 using an allocation of 20 and 14 cases, and $\beta=0.031$ in the quasi-experimental arm to detect the observed ITT odds ratio of 1.07 with 93 and 92 cases. Interpretation of effects should therefore be treated with caution.

We raise one additional point of clarification regarding prior MTFC-A implementations. The authors state that the context of intervention in the UK differs significantly from that in the originating US studies, since 'these were focused on convicted delinquent youth where the alternative [to MTFC-A] was incarceration', thereby concluding that the 'control condition in the US studies approximated [...] to juvenile custody'. Actually, similar to the usual care condition in the Green *et al* study, the standard control condition in US MTFC-A studies is group care,³ not incarceration.

We offer these points by way of lending interpretation to the efficacy of Green *et al*'s results and to suggest caution in accepting the conclusion that MTFC-A may not result in better outcomes than usual care among at-risk adolescents in English care.

- 1 Green JM, Biehal N, Roberts C, Dixon J, Kay C, Parry E, et al. Multidimensional Treatment Foster Care for Adolescents in English care: randomised trial and observational cohort evaluation. *Br J Psychiatry* 2014; **204**: 214-21.
- 2 Leve LD, Chamberlain P, Smith DK, Harold GT. Multidimensional treatment foster care as an intervention for juvenile justice girls in out-of-home care. In *Delinquent Girls: Contexts, Relationships, and Adaptation* (eds S Miller, L Leve, P Kerig). Springer Press, 2011.
- 3 Chamberlain P. *Treating Chronic Juvenile Offenders: Advances Made through the Oregon Multidimensional Treatment Foster Care Model*. American Psychological Association, 2003.

Conflict of interest: The authors have collaborated with US colleagues on projects using the MTFC-A programme.

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Authors' reply: Harold & DeGarmo correctly refer to points regarding sample size and power that we already made in the discussion section of our paper. Despite this, we did point to the strengths of the study in the representativeness of the cohort within a real-world implementation setting, the fact that the study was conducted independently of treatment originators and UK implementation team, careful attention to triangulation and masked rating of primary outcome data (something often not undertaken in this kind of context), and the low attrition rates to endpoint. We stated that the convergence of findings from our mixed-method design and the confidence intervals of the outcome estimations gave some confidence to inferences from the results.

Harold & DeGarmo also question whether there was indeed a difference in the standard control condition (usual care) for participants in the US and UK studies. There are certainly likely to be differences in the nature and uses of group care between the two countries, given the differences in their child-welfare and juvenile-justice systems. However, the point we were making is that, in the USA, the MTFC programme for adolescents has been principally found to be successful when targeted at young offenders, in studies that have used a variety of measures of recorded reoffending to assess its effectiveness.^{1–3} This emphasis on the effectiveness of MTFC-A with young offenders is also clear from the programme developers' own website (www.mtfc.com). By contrast, the participants in our study were young people with complex emotional and behavioural difficulties, 93% of whom were in care because of abuse or neglect and less than a third of whom had a recent criminal conviction. The differences between the populations served by MTFC-A are clearly evident in an article comparing outcomes for high-risk adolescent girls written by the programme developers in the USA and their English colleagues⁴ and may perhaps partly explain why the results of the English evaluation were less positive than those in the USA.

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- 2 Chamberlain P, Reid JB. Comparison of two community alternatives to incarceration for chronic juvenile offenders. *J Consult Clin Psychol* 1998; **66**: 624–33.
- 3 Leve L, Chamberlain P, Reid JB. Intervention outcomes for girls referred from juvenile justice: effects on delinquency. *J Consult Clin Psychol* 2005; **75**: 1181–4.
- 4 Rhoades K, Chamberlain P, Roberts R, Leve L. MTFC for high risk adolescent girls: a comparison of outcomes in England and the United States. *J Child Adolesc Subst Abuse* 2013; **22**: 435–49.

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Are we reinforcing the anti-medical model?

The results of Penttilä *et al*'s meta-analysis emphasised the importance of the duration of untreated psychosis (DUP) in long-term recovery from schizophreniform illness.¹ Timely initiation of effective treatment has been demonstrated to improve outcome, but the modality of treatment is currently under much debate. Robust evidence exists for the efficacy of antipsychotic medication² but recent studies have proposed psychological interventions, specifically cognitive-behavioural therapy (CBT), as an alternative first-line treatment.

In a recent randomised controlled trial, CBT was used as a single intervention, instead of conventional antipsychotic treatment.³ To our complete surprise, one of the exclusion criteria was treatment with antipsychotic drugs. We wonder how ethical approval was granted, despite Tiihonen *et al*'s robust demonstration of reduced mortality over a considerable follow-up period for patients receiving antipsychotic medication.⁴ We feel that this will set a dangerous precedent of offering psychological treatment as an alternative to evidence-based treatment. In a clinical setting, adherence to drug treatment is already a significant issue and there

is potential to reinforce the idea that antipsychotic medication is harmful and unnecessary. We feel that this would further disadvantage an already vulnerable group of patients.

This issue has recently received a fair degree of coverage in the media, with articles such as Freeman & Freeman's piece in *The Guardian* fuelling long-held popular beliefs that antipsychotics are ineffective and in fact damaging to health.⁵ Given the well-documented drawbacks of antipsychotic drugs, it is understandable that patients and professionals will invest hope in non-drug alternatives. However, a large meta-analysis with over 3000 participants shows at best a small effect size for CBT.⁶ In reference to Penttilä *et al*'s paper, we would be interested to read subgroup analyses of specific first-line treatments and wonder if outcomes would differ between modalities.

While we would endorse any treatment, drug or non-drug based, that is proven to reduce DUP, it is vital that we do not lose sight of the fact that antipsychotics are the only evidence-based first-line therapy in psychotic illness.

- 1 Penttilä M, Jääskeläinen E, Hirvonen N, Isohanni M, Miettunen J. Duration of untreated psychosis as predictor of long-term outcome in schizophrenia: systematic review and meta-analysis. *Br J Psychiatry* 2014; **205**: 88–94.
- 2 Leucht S, Arbter D, Engel RR, Kissling W, Davis JM. How effective are second-generation antipsychotic drugs? A meta-analysis of placebo-controlled trials. *Mol Psychiatry* 2009; **14**: 429–47.
- 3 Morrison AP, Hutton P, Wardle M, Spencer H, Barratt S, Brabban A, et al. Cognitive therapy for people with a schizophrenia spectrum diagnosis not taking antipsychotic medication: an exploratory trial. *Psychol Med* 2012; **42**: 1049–56.
- 4 Tiihonen J, Lannqvist J, Wahlbeck K, Klaukka T, Niskanen L, Tanskanen A, et al. 11-year follow-up of mortality in patients with schizophrenia: a population-based cohort study (FIN11 study). *Lancet* 2009; **374**: 620–7.
- 5 Freeman D, Freeman J. At last, a promising alternative to antipsychotics for schizophrenia. *The Guardian*, 7 March 2014.
- 6 Jauhar S, McKenna PJ, Radua J, Fung E, Salvador R, Laws KR. Cognitive-behavioural therapy for the symptoms of schizophrenia: systematic review and meta-analysis with examination of potential bias. *Br J Psychiatry* 2014; **204**: 20–9.

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Author's reply: Dr Bindman and Dr Kripalani have suggested an analysis of the association between DUP and outcomes in subgroups by specific first-line treatment modalities. Unfortunately, it was not possible to analyse this in our meta-analysis, since none of the original studies had used only one treatment modality, but a combination of them in the early phases of treatment. As Bindman & Kripalani point out, and based on current knowledge of the efficacy of treatments in the early phase of schizophrenia, it would not be ethical to study treatment without antipsychotic medication in a first-episode clinical sample.¹ Also, DUP is usually defined as ending at the initiation of antipsychotic medication, which in clinical practice usually occurs about the same time as other treatment modalities begin; therefore, the included studies give only a little information on the effects of different treatments. However, it is interesting to note that de Haan *et al*² investigated the effect of delay in intensive psychosocial treatment by comparing this effect with delay in treatment with antipsychotic medication; and found that delay in psychosocial treatment may be a more important predictor of negative symptoms than delay in antipsychotic treatment.

The discussion about the possible effects of antipsychotics has been rather intense recently. However, the current guidelines for treatment of psychosis and schizophrenia clearly indicate that

antipsychotic medications are effective and recommended treatment for active psychotic symptoms,¹ though there is not so much evidence for the long term (i.e. several years of antipsychotic treatments³). Additionally, the clinical use of these medications is not always straightforward because of their known side-effects and the fact that, in all psychiatric disorders and other illnesses in medicine, there are always patients who do not want to take the recommended treatment. This seems to have been the case in the trial pointed out by Bindman & Kripalani.⁴ When considering the long-term effects of antipsychotics, it is evident that the long-term treatment of psychosis needs to be developed further.

We agree that it would be dangerous to see different treatments as alternatives to each other, and it has been shown that in psychiatry a combination of different treatments is, in general, more effective than any of them alone.⁵ Psychotherapy in the early phase of illness could be effective not only in preventing psychosis at prodromal phase, but also in enhancing adherence to antipsychotic medication.¹ Current treatment guidelines do not suggest that treatment of first-episode psychosis should include only antipsychotic medication without psychosocial treatment, but rather state that medication is one of the cornerstones of psychosis treatment. We believe there is still a lot to do in developing both medication and psychosocial treatments for schizophrenia, and hopefully active research can support this development.

- 1 National Institute for Health and Care Excellence. *Psychosis and Schizophrenia in Adults: Treatment and Management* (Clinical Guideline 178). NICE, 2014.
- 2 de Haan L, Linszen DH, Lenior ME, de Win ED, Gorsira R. Duration of untreated psychosis and outcome of schizophrenia: delay in intensive psychosocial treatment versus delay in treatment with antipsychotic medication. *Schizophr Bull* 2003; **29**: 341–8.
- 3 Harrow M, Jobe TH, Faull RN. Does treatment of schizophrenia with antipsychotic medications eliminate or reduce psychosis? A 20-year multi-follow-up study. *Psychol Med* 2014; **44**: 3007–16.
- 4 Morrison AP, Turkington D, Pyle M, Spencer H, Brabban A, Dunn G, et al. Cognitive therapy for people with schizophrenia spectrum disorders not taking antipsychotic drugs: a single-blind randomised controlled trial. *Lancet* 2014; **383**: 1395–403.
- 5 Cuijpers P, van Straten A, Warmerdam L, Andersson G. Psychotherapy versus the combination of psychotherapy and pharmacotherapy in the treatment of depression: a meta-analysis. *Depress Anxiety* 2009; **26**: 279–88.

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Electronic monitoring of forensic patients

Tully *et al* raise important questions about the introduction of electronic monitoring of forensic patients.¹ Incidents of absconding by forensic patients can give rise to calls for increased security and surveillance. As the authors point out, adoption of electronic monitoring as a panacea for these problems is short-sighted. Tully *et al* cover many of the concerns about electronic monitoring but one area is missing: that the evidence we have from electronic monitoring in the criminal justice sector is primarily of its effects on recidivism and absence without leave during use; evidence is very limited on the effects after its use.

In other words, electronic monitoring must eventually cease. Is the use of electronic monitoring during community reintegration actually preparing the patient for greater freedom and their rehabilitation, or simply delaying reoffending? Criminal justice experience with electronic monitoring focuses almost entirely on

its effectiveness during use, such as on bail or as an alternative to incarceration, usually combined with home detention. Electronic monitoring combined with home detention is superior to imprisonment in these studies, but we already know that non-custodial responses to crime in general have superior outcomes to incarceration (see, for example, Wermink *et al*²).

We know very little about outcomes after the use of electronic monitoring. Although the use of global positioning satellite (GPS) technology might improve the person's performance in following rules, it is not clear that this sort of rule following encourages the person in the ultimate tasks of forensic rehabilitation. Does it improve the therapeutic alliance to help the person make the life changes necessary to recover from illness and illness-related offending? Or does electronic monitoring seem a physical manifestation of distrust and create distance between the patient and the treatment team? If the only way that a person can safely have community contact is to wear an ankle bracelet, isn't it questionable whether they are ready for that level of community contact? Electronic monitoring may allow the person more apparent personal freedom than their clinical risk would otherwise allow. As Tully *et al* point out, adoption of the GPS technology may seem appealing, but its costs and effects are not clear and neither is its impact on therapeutic and community engagement. Short-term reductions in absence without leave might give the appearance of progress that the patient has not actually achieved. Long-term outcome is equally as important as short-term adherence.

- 1 Tully J, Hearn D, Fahy T. Can electronic monitoring (GPS 'tracking') enhance risk management in psychiatry? *Br J Psychiatry* 2014; **205**: 83–5.
- 2 Wermink H, Blokland A, Nieuwbeerta P, Nagin D, Tollenaar N. Comparing the effects of community service and short-term imprisonment on recidivism: a matched samples approach. *J Exp Criminol* 2010; **6**: 325–49.

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Tully and colleagues¹ justify the introduction of electronic monitoring of medium secure patients without indication of the size of the problem of absconding or the incidence of serious harm other than to reference an article in *The Sun* newspaper, which is neither informative nor free of bias.

Decisions around leave for patients detained within a medium secure unit are clearly complex. Consideration should always be given to the risk of absconding and associated risks if the patient were to abscond. Thus, patients who are at high risk of absconding and a serious risk to the public would not receive leave, whether they were tagged or not. Another factor is the clinical team's trust in that patient to use leave appropriately. Tagging patients would be a very clear indicator of a lack of such trust.

The suggestion that patients enter into electronic monitoring with consent is questionable: many patients in our experience abide by suggestions of their clinical team in order to progress through the system. Given that there is yet to be a strong argument that tagging is necessary and primarily in the patient's best interest (as opposed to a matter of public protection), can one justify this coercion? We would be very interested to know the process in which patients' perspectives were taken into account and whether this has altered the intervention.

Electronic monitoring would inform the clinical team if the patient were to breach the conditions of their leave in terms of approximate location and time of leave; however, it would not inform the team as to what that patient was doing with their leave and would not necessarily prevent serious incidents occurring, as

suggested. The use of a device whose main purpose has been pioneered by the criminal justice system seems to take us closer to making our hospitals prisons. A recent report published by the Criminal Justice Joint Inspection reiterates their 2008 findings that enforcement thresholds were not sufficiently stringent.² With notable problems implementing this system within the criminal justice system, is it justifiable to implement it within the forensic services, given the cost of such a system?³

Given the recent concerns about certain international security companies, the provision of such tags also raises ethical issues. Confidentiality must also be considered – would said companies have access to patient names and locations? The comparison of electronic monitoring with other uses of technology within psychiatry, such as mood monitoring via text message, is bizarre. The principles approach⁴ gives us a framework in terms of judging whether an intervention respects autonomy, beneficence, non-maleficence and justice. Debate of these principles will exceed the remit of this letter; however, it is worthwhile considering autonomy and beneficence in particular relating to the patient: we suggest that there is a breach in both. The weighing of these principles will not be easy and it will be a matter of debate whether the principle of justice will outweigh the former.

As the authors state, robust research in this area is needed, and we look forward to reviewing the evidence.

- 1 Tully J, Hearn D, Fahy T. Can electronic monitoring (GPS 'tracking') enhance risk management in psychiatry? *Br J Psychiatry* 2014; **205**: 83–5.
- 2 HM Inspectors of Probation. *It's Complicated: The Management of Electronically Monitored Curfews*. Criminal Justice Joint Inspection, 2012.
- 3 Shaw D. Satellites used to track mentally-ill violent criminals. *BBC News*, 25 August 2010.
- 4 Gillon R. *Philosophical Medical Ethics*. Wiley, 1985.

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Authors' reply: We had hoped that our article would stimulate a balanced discussion about this complex issue. We entirely agree with the view expressed in both letters that trust and therapeutic alliance between the patient and the treating team are critical components of the recovery process. We do not believe, however, that use of electronic monitoring necessarily indicates a lack of trust. It was envisioned that the device be used primarily for patients in the initial stages of taking leave as part of their clinical pathway towards discharge into the community. Our clinical experience, supported by as yet unpublished data, confirms that this has been the case in our service. In these circumstances, electronic monitoring may even help to further develop a trusting relationship between the wearer and the team, by granting earlier and more frequent leave and by allowing the patient to demonstrate avoidance of exclusion zones when on unescorted leave. There must be a balance between trust and therapeutic optimism in our treatment of our patients. Furthermore, viewing trust as being simply 'present' or 'absent' would be a naive approach in forensic services. These questions are being explored in quantitative and qualitative research of electronic monitoring in our service.

Both letters raise concerns about granting of leave for high-risk patients. Watson *et al* point out that decisions surrounding leave are complex, a view that we share. However, the implied view in both letters that patients can be discretely classified into high

risk for absconding or not is again overly simplistic. Clinical impression alone in risk assessment has been shown to be unreliable and validated risk assessment tools have been shown to be more useful in identifying individuals at low rather than high risk.¹ No validated tool for the assessment of absconding risk yet exists, though we are currently working on developing one. Risk management, therefore, involves a component of positive risk-taking aided by creative management strategies. We propose that electronic monitoring is such a strategy.

Watson *et al* are liberal in their use of the term 'coercion'. A policy was put in place whereby patients were informed that use of electronic monitoring was optional and if they chose to decline to wear the device, their leave would be risk assessed as per normal procedure. Consent is another complex issue in psychiatry and can be defined in degrees, rather than as a binary concept.² It is true that patients' decisions about consent to electronic monitoring are likely to be influenced by their wish to move more quickly towards leave and discharge. This has parallels with consent to medication and engagement in psychotherapies and occupational activities, particularly in the forensic setting.

Watson *et al* express concern about forensic services being closely aligned with the prison system. We believe that the use of secure units with locked wards and secure perimeters represents a level of coercion much more closely aligned to this system than does electronic monitoring. Any strategy that can help minimise the amount of time spent in such units would then surely be a welcome development for those concerned about patient liberty and overall progress. Far from making our units more like prisons, one of the key aims of our strategy was to allow for engagement in community leave and activities at the earliest possible stage. As Simpson & Penney point out, electronic monitoring may allow the person more apparent personal freedom than their clinical risk would otherwise allow.

The article referenced in *The Sun* was chosen as an example of media coverage of such absconding events. That such reports are often sensationalised or biased is one of the many challenges facing mental health services and patients. Media coverage of absconding events leads to reputational damage for services and can undermine the confidence of the community. We cannot and should not ignore community attitudes towards system breaches, especially as clinicians will be held to account when they occur. Another of our aims is therefore to reduce the frequency of these incidents, for the protection of the public and the reputation of our service.

Watson *et al* are correct in saying that electronic monitoring cannot directly prevent violent incidents. We believed that this was self-evident and therefore we did not address this issue in our article. Regarding costs, a cost-benefit analysis is currently underway. As our article states, our service was acutely aware of the important ethical considerations and we sought legal and ethical advice. A commentary addressing legal and ethical issues in more depth is currently being prepared. The questions Simpson & Penney raise about reoffending, recovery and longer-term outcomes are valid and we hope to address these in our future research.

- 1 Fazel S, Singh JP, Doll H. Use of risk assessment instruments to predict violence and antisocial behaviour in 73 samples involving 24 827 people: systematic review and meta-analysis. *BMJ* 2012; **345**: e4692.
- 2 Konow J. Coercion and consent. *J Inst Theor Econ* 2014; **170**: 49–74.

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