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Predicting violent offences by released prisoners

For a pejorative term without proven clinical utility, psychopathy has generated some very catchy sayings. Some bear little relationship to the research that generated them. 'Treatment makes psychopaths worse' is one (see Rice *et al*[1]). I fear that without urgent corrective action, 'Risk assessment doesn't work for psychopaths' (see Coid *et al*[2]) will be another.

Coid *et al* compared the ability of three structured risk assessment instruments – the Violence Risk Assessment Guide (VRAG), the Historical, Clinical, Risk Management-20 (HCR-20) and the Offender Group Reconviction Scale-II (OGRS-II) – to predict violent offences by released prisoners in different diagnostic groups. They defined one such group, 'psychopathic personality', using a score of over 30 on the Psychopathy Checklist-Revised (PCL-R). For most instruments and groups, Coid *et al* found moderate levels of predictive accuracy. For the 5.7% of the sample scoring over 30 on the PCL-R, however, no risk assessment instruments performed better than flipping a coin. The authors see major implications for risk assessment. They state that new actuarial tools may be required.

A better conclusion would be that if you define a group using a high score on one instrument that predicts violence, other such instruments will struggle to predict violence in that group. Originally designed to measure a psychological construct, psychopathy, the PCL-R has proved to be one of several instruments that consistently predict violence better than chance (area under the curve (AUC) 0.65–0.75; see Singh *et al*[3]). The VRAG and the HCR-20 are others. The other instruments could only have successfully predicted violence among Coid *et al*'s 'psychopathic personalities' if structured risk assessment instruments could be applied serially with increasing success.

We know that they cannot. When Seto[4] combined the results of using instruments sequentially to predict serious offending, also in ex-prisoners, he did no better than he had using one instrument alone. These data, and others suggesting the particular items on a scale are less important than the constructs, such as past behaviour and substance use, that the items represent,[5] have led some to suspect that a ceiling effect may apply to the prediction of violence in psychiatric and other populations.[6] Efforts to improve the accuracy of structured risk assessment instruments are probably better directed at reducing the quantity of missing data than at adding new instruments.[7]

I have a wager for Coid *et al*: try the process in reverse. Select the 5.7% of the sample with the highest HCR or VRAG scores and test whether the PCL-R is predictive in these groups. My five pounds says it will not be, and for the same reason. More is not necessarily better. Or, once you have squeezed the fruit, there usually isn't much point squeezing it again.

1 Rice M, Harris G, Cormier C. An evaluation of a maximum security therapeutic community for psychopaths and other mentally disordered offenders. *Law Hum Behav* 1992; **16**: 399–412.

2 Coid J, Ullrich S, Kallis C. Predicting future violence among individuals with psychopathy. *Br J Psychiatry* 2013; **203**: 387–8.

3 Singh J, Grann M, Fazel S. A comparative study of violence risk assessment tools: a systematic review and metaregression analysis of 68 studies involving 25,980 participants. *Clin Psychol Rev* 2011; **31**: 499–513.

4 Seto M. Is more better? Combining actuarial risk scales to predict recidivism among adult sex offenders. *Psychol Assess* 2005; **17**: 156–67.

5 Kroner D, Mills J, Reddon J. A coffee can, factor analysis and prediction of antisocial behavior: the structure of criminal risk. *Int J Law Psychiatry* 2005; **28**: 360–74.

6 Buchanan A. Risk of violence by psychiatric patients: beyond the ''actuarial versus clinical'' assessment debate. *Psychiatr Serv* 2008; **59**: 184–90.

7 Harris G, Rice M. Actuarial assessment of risk among sex offenders. *Ann N Y Acad Sci* 2003; **989**: 198–210.

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Authors' reply: In the introduction of his letter, Buchanan refers to psychopathy as a 'pejorative term' but later categorises it as a risk assessment instrument. It is neither. Psychopathy as a psychiatric syndrome was first described by a general psychiatrist[1] and further developed into a diagnostic construct operationalised with the PCL-R.[2] It is retained within dissocial personality disorder in ICD-10 and as an alternative model of antisocial personality disorder in DSM-5. The PCL-R is recognised internationally as the gold standard for assessment of psychopathy. Proficiency in its use should be a core competency for clinicians who work with offenders. Sadly, many are not adequately trained and struggle to comprehend why their treatments usually fail with these individuals and sometimes make their behaviour worse.

Buchanan may have misunderstood Seto's[3] method. The instruments were applied simultaneously, not sequentially. However, he is right that sequential screening does not improve accuracy. We would suggest a better reference for an explanation.[4] We would also emphasise that risk assessment instruments are no more than screening instruments. Most importantly, there is currently no evidence base to demonstrate that routine clinical use of these screens can prevent violence, despite mandatory use in some UK services.

With regard to the 'glass ceiling' effect that we have previously investigated,[5,6] reducing missing data will achieve little to break through this. Trigger factors precede many violent events. They may occur in the context of static and dynamic risk factors which have predictive efficacy. But trigger factors are causal, can occur within seconds to trigger violence and, most importantly, are not predictable.

Finally: the wager. There is no purpose in doing this if psychopathy is a personality construct. Furthermore, we have previously shown that few PCL-R items are predictive.[5] But we did rise to the challenge of Buchanan and tested the predictive accuracy of the VRAG, OGRS and HCR-20 in high-risk groups defined by these instruments. Using 32 as the HCR-20 cut-off and 27 for VRAG to be as close as possible to Buchanan's 5.7%, we estimated AUCs for VRAG and OGRS in the same HCR-20 high-risk group, and AUCs for HCR-20 and OGRS in the corresponding VRAG high-risk group. In the VRAG high-risk group, the HCR-20 showed a low AUC of 0.44 (95% CI

0.28–0.61). The OGRS was a more respectable 0.69 (95% CI 0.53–0.85). In the HCR-20 high-risk group, AUC for VRAG was 0.67 (95% CI 0.54–0.81) and OGRS 0.68 (95% CI 0.64–0.81).

Perhaps there would be mileage in squeezing the fruit again in Buchanan's next study?

1 Cleckley H. *The Mask of Sanity: An Attempt to Reinterpret the So-Called Psychopathic Personality*. Mosby, 1941.

2 Hare RD. *The Hare Psychopathy Checklist—Revised* (2nd edn). Multi-Health Systems, 2003.

3 Seto M. Is more better? Combining actuarial risk scales to predict recidivism among adult sex offenders. *Psychol Assess* 2005; **17**: 156–67.

4 Leon G. Assessing the validity and reliability of diagnostic and screening tests. In *Epidemiology* (3rd edn): 71–94. Elsevier Saunders, 2004.

5 Coid JW, Yang M, Ullrich S, Zhang T, Sizmur S, Farrington D, et al. Most items in structured risk assessment instruments do not predict violence. *J Forens Psychiatry Psychol* 2011; **22**: 3–21.

6 Liu YY, Yang M, Ramsay M, Li XS, Coid JW. A comparison of logistic regression, classification and regression tree, and neural networks models in predicting violent re-offending. *J Quant Criminol* 2011; **27**: 547–73.

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Effectiveness of methadone treatment for heroin addiction

Regarding Byford *et al*'s paper,[1] the authors present an analysis of the results of the Randomised Injectable Opiate Treatment Trial (RIOTT).[2] Participants of RIOTT were very few in number – fewer than 45 individuals in each of the three arms of the study (injectable heroin, injectable methadone and 'optimised' oral methadone). It required 3 full years at 3 sites to screen 301 volunteers, of whom 127 (40%) began the trial and only 89 completed the 26-week treatment protocol.

All of the participants had been receiving 'conventional' methadone treatment for more than 6 months and continued 'to inject "street" heroin regularly'. On average, they had had over four prior treatment episodes. Accordingly, it is reasonable to assume that the overriding motivation of those who volunteered was the hope of receiving injectable opiates, and it is likely that participant bias may have had a substantial impact on outcomes. Indeed, it is revealing that among those assigned to receive optimised oral methadone, 7 (17%) never began the trial and of the remaining 35 only 24 were still enrolled 26 weeks later.

Some of the reported findings seem to underscore the severe limitations that must be kept in mind in drawing even the most tentative conclusions. For example, although the oral methadone group claimed to have committed roughly three times as many crimes as the intravenous methadone group (mean 21 *v.* 7 crimes), the latter group spent 15 times more nights in prison (mean 6.1 *v.* 0.4). Surely provision of oral methadone did not somehow make patients more successful in their criminal pursuits.

Perhaps inevitably, the limited ability to extrapolate has been ignored in the wider distribution of the findings. Thus, one report (which refers readers seeking more information to the Press Officer of King's College London, with which the principal author and five of the seven co-authors are affiliated) had the unqualified headline: 'Injectable opioid treatment for chronic heroin addiction more cost-effective than oral methadone', and claimed that 'total cost savings of providing injectable opiate treatment for this chronic group in England could be between £29 and £59 million per year'.[3]

The criticisms noted above must not detract from the bottom-line, common sense, conclusion with regard to injectable opioid treatment: in the interests of addicts as well as the general community, it is essential that those who respond poorly to treatment (any treatment) be provided information on and referral to the broadest possible array of alternative services.

1 Byford S, Barrett B, Metrebian N, Groshkova T, Cary M, Charles V, et al. Cost-effectiveness of injectable opioid treatment v. oral methadone for chronic heroin addiction. *Br J Psychiatry* 2013; **203**: 341–9.

2 Strang J, Metrebian N, Lintzeris N, Potts L, Carnwath T, Mayet S, et al. Supervised injectable heroin or injectable methadone versus optimized oral methadone as treatment for chronic heroin addicts in England after persistent failure in orthodox treatment (RIOTT): a randomized trial. *Lancet* 2010; **375**: 1885–95.

3 King's College London. *Injectable opioid treatment for chronic heroin addiction more cost-effective than oral methadone*. King's College London, 2013; 1 October (http://www.kcl.ac.uk/iop/news/records/2013/October/Injectable-opioid-treatment-for-chronic-heroin-addiction-more-cost-effective-than-oral-methadone.aspx).

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Authors' reply: Newman rightly draws attention to the effectiveness of appropriately delivered methadone treatment for many people with heroin addiction worldwide over the past half-century. Our economic evaluation[1] and the preceding report on the main findings from the RIOTT trial[2] should not be considered an attack on the value of oral methadone to the majority who show substantial benefit from this treatment.[3,4] Rather the RIOTT trial needs to be recognised for what it was – an investigation of effectiveness and cost-effectiveness of alternative treatments in a subgroup of the treatment population with severe and chronic addiction who were not responding to oral methadone maintenance treatment.

It is also appropriate to inject a note of caution about the potential influence of expectations on trial participants. This limitation is inherent in any trial where the patient has a preference for which treatment arm they may be assigned to, and Newman is right that this has the potential to be a pronounced influence in the addiction treatment field. In fact, aware of this potential, we gathered some data from patients on their expectations and experiences of treatment within the trial, and this has recently been reported separately.[5]

Newman notes the modest sample size in this trial (total of 127 participants). This is a particular challenge in a field where treatment is intensive and expensive, and in countries which do not have a tradition of funding large treatment trials in the addictions field. We would nevertheless point out that the sample size was calculated in advance by the applicants for the original research award and was judged to be adequate to detect the expected effect size as defined in the protocol.[2]

Newman highlights a further limitation of sample size in this highly variable population, using the example of criminal activity. Although the oral methadone group reported committing a much higher number of crimes than the injectable methadone group, the latter group spent more nights in prison. However, the total number of participants spending any time in prison ($n = 6$; 5%) is extremely small relative to the number reporting any criminal activity ($n = 50$; 42%), so it would be inappropriate to try and come to any comparative conclusions.