

Response to comments by Nigel Williams and Nick Cowen

Carly Lightowlers and Jon Shute

We would like to thank Williams and Cowen for engaging with our modest paper in such a considered and detailed manner; it is always flattering to be taken seriously. We are pleased that their article reinforces our basic message that there were demonstrable associations between area-level deprivation and likelihood of being involved in the Greater Manchester riots. We disagree, however, with aspects of their analytic approach, interpretation, and stated policy implications; and with what we feel is a misrepresentation of our position. We would, therefore, like to begin by restating that position before addressing the substantive areas of disagreement.

Our own paper highlighted a clear area deprivation-riot participation link, while being clear about the limits of inference both in terms of simplistic causal attribution ('deprivation was neither necessary nor sufficient for explaining involvement in the riots') and the assumption that area-level characteristics could be attributed in both a descriptive and explanatory sense to individuals, that is, the 'ecological fallacy'. We also offered a brief account of some of the established reasons why multiply deprived communities tend to experience an excess of offences, and also produce an excess of offenders as measured by criminal justice data; and pointed to the potential role of policing and sentencing in the (re-)production of the observed relationships. In doing so - and consistent with our motivations for conducting the analyses at the time of the riots - we felt we were putting both structure and criminal justice process back into a public discourse that focussed on 'uncontextualised models of individual choice and moral irresponsibility', and where the primary political response favoured tougher community policing and exemplary sentencing regardless of the possibility that they could be counter-productive. In sum, we were careful not to go beyond the available data while suggesting that progressive and constructive responses needed to be sensitive to the broader evidence-base in a way that went well beyond 'get tough' policies and rhetoric.

Seen in this light, we have some difficulties accepting key aspects of Williams & Cowen's response to our paper:

1. Analytic approach: One purpose of a composite area-level index such as IMD is to try and capture a broader-based picture of local conditions than is offered by any single measure. This being the case, decomposition of the composite seems a curious thing to attempt,

particularly in light of the fact that is very high collinearity between constituent dimensions (see Table 1 below and Williams and Cowen's own recognition of this fact p32) that renders most forms of statistical analysis redundant, or at least highly problematic. For these reasons, this form of analysis has been moribund in quantitative criminology for some time (see Land et al 1990; McCall et al 2010). While we retain deep reservations about the logic and substance of Williams and Cowen's statistical analyses, we will therefore refrain from further comment here; preferring instead to emphasise our basic agreement on the obvious presence of an area deprivation-riot participation link.

	1	2	3	4	5	6	7	8	9	10
IMD (1)	1									
income (2)	.97	1								
employment (3)	.95	.94	1							
health & disability (4)	.94	.90	.88	1						
education, skills & training (5)	.92	.90	.90	.82	1					
barriers to housing services (6)	.19	.14	.09	.21	.08	1				
crime & disorder (7)	.61	.49	.44	.53	.44	.19	1			
living environment (8)	.55	.51	.43	.52	.44	-.06	.34	1		
income deprivation (children) (9)	.90	.89	.86	.84	.96	.13	.47	.44	1	
income deprivation (older people) (10)	.91	.92	.83	.88	.80	.19	.48	.57	.82	1

NOTE: all correlations significant at ** $p < .01$

Table 1: Inter-correlations (Spearman's rho) of area-level IMD score and constituent items

2. Interpretation: We offered no speculation as to the individual-level motivations and thought-processes of rioters, not because they are not relevant or necessary for a satisfactory explanation of the riots, but because to do so would have involved going well beyond our data. Seen in this light, much of Williams and Cowen's discussion demonstrates the ecological fallacy, and in so doing risks reinforcing an already over-individualised public discourse with faulty reasoning extrapolated from an apparently authoritative statistical engagement.

We feel this basic problem is compounded by two others. First, a clear preference for a 'rational choice', *homo economicus*, model of human nature that reduces structural context to a set of set of conditions modifying situational calculations of benefit, and that precludes the possibility of non-instrumental, expressive reasons for participation such as feelings of anger, humiliation, and disenfranchisement (see esp. chapter 7 of Young 2008). The possibility that the riots could be interpreted, *inter alia*, as an incoherent 'protest' against difficult life

conditions (including adversarial policing) cannot figure in this account. Second, and relatedly, Williams and Cowen confuse general and special forms of deterrence: the former relating to the effect of deterrent messages on the decision-making of the population at large; the latter relating to the effect of actual criminal justice sanctions on the individual offender. The comments in our paper were a caution against exemplary punishment and so related to special deterrence: the labelling effects demonstrated in British longitudinal studies of crime and development; and Gendreau et al's (1999) meta-analysis of 50 studies showing that relative to both shorter carceral sentences and community sentences, longer carceral sentences *increase* the chances of recidivism. Williams and Cowen 's paper, however, deny this literature with respect to a single statistical bulletin issued by the sentencing administrator (Ministry of Justice) and focus their argument on general deterrence, specifically, the likely role of perceived lack of policing effectiveness in promoting situational decisions to offend/participate in the riots. We do not doubt that this latter effect occurred, but disagree that it follows that more high visibility policing will prevent a by-definition unpredictable and rare phenomenon (rioting) from reoccurring; and without both considerable financial as well as social cost. The conceptual and empirical literatures on procedural justice and policing legitimacy (e.g., Tyler 2003), legal socialisation (Fagan & Tyler, 2008) and criminal sanction 'defiance' (Sherman 1993) clearly illustrate the importance of policing by consent. The fact that deficits in community policing may have been at the heart of the original disturbances in Tottenham (as well as in the recent past, e.g., Brixton 1981) points to the potential counter productivity of heavy-handed responses in contexts of deep social exclusion.

In sum, we find Williams and Cowen 's paper highly stimulating but their analyses to be based in faulty logic, their interpretation to be based in a rather limited and partial view of human nature, and their recommendations to assume an uncritically positive role of criminal justice. We enjoyed engaging with their comments but remain unconvinced that the associations uncovered in our analyses can be interpreted in the manner they suggest, and that the best response to the riots is (further) repressive policing and longer prison sentences. With a recent HMIC (Her Majesty's Inspectorate of Constabulary, December 2011) report mooting the future use of plastic bullets and water cannons to police riots, and Ministry of Justice figures (MoJ, February 2012) confirming the average prison sentence handed down to date being nearly four times as long as for comparable offences in 2010 (14.2 vs. 3.7 months), Williams and Cowen are likely to see their recommendations realised. But at what cost?

References

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