Misrepresenting as misconceptions: A comment on group-based trajectory modeling[1]

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The recent special issue of *JRCD* included a review of group-based trajectory modeling (GBTM) in life course research (Nagin 2016). Although there is plenty of valuable information in that article, I was disappointed that Nagin used this platform to mischaracterize rather than respond to the key controversies surrounding this method.

Nagin argued that the published critiques of the uses of GBTM in criminology reflect two fundamental misconceptions (2016: 364-5): The first supposed misconception is the assumption that the groups generated by the method are real entities rather than statistical constructs or approximations. The second is that the trajectories are immutable. However, contrary to what Nagin claims, there has not been serious disagreements about either one of those points. Instead the typical objections to the applications of GBTM have been precisely the opposite: *Because* the groups are only approximations of a more complex reality, using GBTM to identify the correct number of subpopulations is unreliable (Bauer 2007, Osgood 2005, Sampson et al. 2004, Skardhamar 2010). The main argument in my own work was explicitly directed at how GBTM was *interpreted* as providing support for Moffitt's taxonomic theory, concluding that GBTM studies are largely uninformative with respect to that theory (Skardhamar 2010: 315).

It appears that the real point of contention has to do with the way in which GBTM has been applied in the literature. Criticisms of the uses of GBTM in criminology are warranted by the fact that, despite typically being accompanied by appropriate caveats, the number and distinctness of groups has been the focal concern in the literature (e.g. D'Unger, Land, McCall et al. 1998, McDermott and Nagin 2001, Piquero 2008). This is particularly evident in Moffitt's review of a decade's empirical evidence for her taxonomic theory, where it is repeatedly stated that GBTM provides an objective tool for assessing the number of subpopulations in the sample (Moffitt 2006, pages 576, 579, 581 and 585). It is clear to anyone following this literature that much of the evidence claiming to find support for the taxonomic theory, or versions of that theory, is based on GBTM studies (Moffitt 2006). Nagin would do better to target his critical remarks at those studies rather than the critics pointing out the problem.

If the latent strata have no direct interpretation, it is hard to see why the number of strata would be a finding at all. Indeed, Nagin himself has pedagogically compared a discrete distribution to the way in which a histogram approximates a continuous distribution (Nagin 2005: 47). To extend the analogy, the number of bars in a histogram has little substantive meaning, although we certainly can discuss how many bars would best summarize the overall distribution. In this sense, finding a high-rate group is tantamount to nothing more than detecting an upper percentile of a distribution. There is little doubt that an upper percentile exists in all distributions, but this obvious fact does not constitutes meaningful support for any theory.

Moreover, Nagin should take responsibility for contributing to these misconceptions about the interpretation of the latent strata. While he has stated many times that GBTM should be understood as an approximation of a more complex reality and warned against reification of groups, he has *also* claimed that GBTM can be used for *testing for the existence of groups* and *test taxonomic theories*

(Nagin 1999: 140; Nagin 2005: 2, 13; Nagin and Odgers 2010: 111). Moreover, Nagin's recent essay maintains that GBTM can be used to *test* for the presence of subgroups, such as those proposed by Moffitt (Nagin 2016: 362). If the groups are not "real", it is hard to see what he means by testing for their presence. Since finding an upper percentile of a distribution is consistent with most theories, it is hard to see how GBTM can be a test of any theory. Nagin should explain under what conditions this can be considered a test.

Let me be clear, I firmly believe in the utility of GBTM as a method of analysis as long as it is applied and interpreted appropriately (Haviland, Nagin, Rosenbaum and Tremblay 2008; Skardhamar and Savolainen 2014). What I propose is that the field recognizes the limitations of the method and moves away from untenable claims regarding the correct number and shapes of trajectories.

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