Extensive exploration is a prerequisite for developing sound theories: a reply to the commentaries
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Extensive exploration is a prerequisite for developing sound theories
Dries Peters

Alcoholism is assumed to be a biopsychosocial disorder and disentangling causes is a daunting task. Although typology studies are seen as useful, researchers are hindered by the complexity of the statistics involved. When introducing another potentially fruitful statistical technique, as I have done in my research report\(^1\), one might expect many to remain on the sidelines. Therefore I appreciate that the three commentators have examined my approach to this complex problem critically, and am grateful for their constructive remarks and their commendations. I will now respond to the comments of Drs Valentine\(^2\), Zucker\(^3\) and Babor\(^4\).

From my own experience in taxonomic studies with statistical grouping methods I am convinced that obtaining a classification in the biological domain is astonishingly simple compared to obtaining one in the psychopathological domain. In this respect I can sympathize with Dr Valentine’s statement that classifying alcoholics cannot be the same as classifying insects. There are too many influences such as culture, social class, fashion, availability and financial resources which may mould alcohol abuse and dependence in various idiosyncratic ways. However, it is also plausible that there are generalities such as the alcohol dependence syndrome, or prototypical subtypes reflecting relatively distinct causal pathways. In the interpretation of the MDS space, Dr Valentine emphasizes typology-irrelevant moulding factors of alcoholism, i.e. culture, age, and gender. However, the specific constellation of these factors combined with indicators of psychopathology strongly suggest a meaningful prototypical/dimensional grouping in the MDS space, and not only final states of subcultural abusive drinking patterns.

The failure to find a categorical grouping of alcoholics implies, as Dr Valentine clearly illustrates, that treatment matching can certainly not be a simple operation and will require a good deal of clinical judgement.

Dr Zucker noted similarities between the MDS classification and his own typology of alcoholism. He remarks that “issues of comorbidity and life course ‘location’ emerge as key identifiers … even when the item structure marks them only in a very rudimentary way”, and concludes that these effects therefore must be very robust. I agree fully, but also want to stress that these effects did not emerge when dissecting cluster techniques such as k-means or average-linkage were used. One of the qualities of MDS is that this technique is robust for unbalanced representations of symptoms, as one should expect in a bootstrapping study.

My study used a cross-sectional design with a representative sample of those alcoholics who sought treatment, and of course it has certain limitations, as indicated by Dr Zucker. The “early starters/early remitters” and the “late onset alcoholics” will certainly be under-represented (5% was younger than 25, and 14% started drinking in a problematic way after the age of 45). A study with a longitudinal design in a general population sample as conducted by Dr Zucker\(^5\) would be better suited to examining such groups.

Dr Zucker also calls attention to issues concerning the “rarer forms of the disorder”. Unfortunately, due to technical reasons (mis specification as a consequence of a lack of knowledge of the relevant symptoms, and limited reliability of the psychopathological item domain), rarer forms of alcoholism—if captured—have nearly no opportunity of surfacing as groups in an exploratory taxonomic study unless an extremely large sample is used. Even more important is the question as to whether a taxonomic approach is appropriate for identifying truly rare forms of alcoholism. Rare forms are, presumably, the result of idiosyncratic causal mechanisms, and in these cases a clinical approach might be more appropriate.

Dr Babor gives two reasons why my study could have failed to find a two-type classification. The first one concerns “suspect” reliability of symptoms compared to scales. However, the symptoms in my study were up to psychometric standard, and the limited reliability of symptoms compared to scales was compensated by using a great many of them. Moreover, scales have their problems too: there is a strong likelihood that the fractional heterogeneity incorporated in any scale (not heterogeneity considered from a reliability perspective, but from a validity perspective) will function as error in a taxonomic study (see also the method section in the research report\(^1\).) Taken together the difference between a symptom and a scale approach will probably be too small to prevent a detection of a clear categorical two-type classification.
The other potential reason suggested by Dr Babor is an "absence of underlying typological characteristics" in my study. The principal difference between the item domain in the study of Dr Babor and in my study concerns the "putative etiological factors" which I excluded deliberately. As it is unclear how forms of alcoholism are related to causes, one can consider it premature to include aspects of them in taxonomic studies since it limits the possibilities of aetiological research and can lead to premature closures. If the typology obtained and its included aetiology are not convincingly confirmed then there is no path for subsequent development. Dr Cloninger's type I/type II classification could be considered an example: despite more than a decade of conflicting results (more negative than positive) from confirmatory research, no adjustment of the theory has been made, and—given the fact that causes are "fixed by design"—it is hard to imagine research strategies which can lead to further development of this theory: it seems to be heads or tails, but despite all attempts to replicate, the issue remains undecided and no one seems to have any alternative other than to try again (the recent replication by Cloninger's group of their original cross-fostering study is—again—counterbalanced by the "negative" finding in a recent study with data from the same Swedish registration system as used by Cloninger's group: genetic risk ratio of "age of onset of alcohol abuse" in monozygotic twins was the same as in dizygotic twins.)

If one chose to take the risk of including causes in the item domain then the strategy used by Dr Babor seems preferable: he incorporated a wider range of aetiological indicators than found in the type I/type II classification of Dr Cloninger. As these indicators are assumed to be more directly—and in combination more strongly—related to an assumed causal process, it could have lead to a more pronounced grouping in Dr Babor's study than I found in my study. This reasoning underlies Dr Babor's argument, that an absence of aetiological typological characteristics could have prevented the surfacing of the two-type classification in my study. However—as far as I know—in none of the two-type typology studies has a test of arbitrariness (e.g.,) or a test for a boundary region of rareness (see my study) been conducted.

Drs Babor and Zucker rightly stress the importance of theory and validation research and these remain the cardinal issues. This emphasis in taxonomic studies should also be seen from a historical perspective. In the early phase of taxonomic studies by statistical grouping methods only heuristic methods were available, and the evaluation of the obtained classification was sought primarily in validation research. As an additional strategy to improve classifications obtainable by heuristic methods, stress was laid on theory. This often resulted in exploration that was prematurely cancelled, in misspecified theory and non-optimal classification systems, which together lead to superfluous confirmatory research. It gave taxonomic studies with statistical grouping methods a bad name. One strategy which I have followed, in my approach to the typology problem, is to emphasize again the need for a substantial exploratory phase, since it is here that the research questions are solved by stating them correctly. It would not be an extreme position to assert that extensive exploration is a prerequisite for developing sound theories.

Certainly the most exciting development in statistics is in methods for exploration, and I think that a generous application of such methods can be of real benefit for developing sound theories of addictions. One of the most characteristic aspects of modern exploratory statistical methods is the incorporation of confirmatory steps, blurring the classic distinction between an exploratory and a confirmatory phase. This is recognizable in my study, which—although still an exploratory one—contains quite a number of confirmatory checks. When one contrasts these newer techniques with older procedures, one can see the progress made in statistics and perhaps also value the results obtainable by using them.

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