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Do the same social mechanisms explain bullying in different classrooms? A commentary on Madhavi & Smith and Atria, Strohmeier, & Spiel

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This paper reviews two studies on between-classroom variation in bullying by Mahdavi and Smith and by Atria, Strohmeier, and Spiel in this issue. Both studies demonstrated large variability between classrooms in the prevalence of bullying and the distribution of participant roles. The authors of the original studies consider this variability at odds with the influential participant roles and scapegoating approaches to bullying. In contrast, this review proposes that the essence of these theoretical approaches is to explain variance, both within and between classrooms, and over time. More explicit theorizing concerning determinants of between-classroom variance and assessment of classroom-level variables implicated by this theorizing are called for.

The two papers concerning bullying in this issue have a lot in common. Both address variance between classrooms in bullying with cross-sectional designs and both refer to the influential participant role approach (Salmivalli, Lagerspetz, Bjorkqvist, Osterman, & Kaukiainen, 1996). The most striking resemblance between both studies to me, however, is their ambition to falsify a theoretical approach. Atria, Strohmeier, and Spiel (this issue) aim to falsify the basic tenets of the participant role approach by demonstrating a degree of between-class variability that they believe the participant-role approach cannot accommodate. Madhavi and Smith (this issue) aim to falsify the so-called scapegoating hypothesis by demonstrating that a considerable proportion of classrooms have no scapegoat. This

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thoughtful Popperian—typically European?—attempt to design "critical experiments" is a relief amidst an epidemic of salami-sliced, data driven, atheoretical papers.

Notwithstanding the laudability of these attempts at falsification, I do doubt whether both studies achieved their aims, or rather falsified extreme interpretations of the theories they discuss that few proponents of those theories would agree with to begin with. I believe both studies suggest important explicitations of assumptions underlying the theories they confront, rather than falsify those theories themselves. Allow me to explain this rather abstract criticism for each study separately.

Madhavi and Smith (this issue) criticize both the participant role approach and the scapegoating hypothesis. Concerning participant roles, Madhavi and Smith criticize an alleged lack of attention to group phenomena underlying this approach. They argue that participant roles are primarily individual characteristics, in that "there is no reason to suppose that one person being in a certain role ... strongly influences the probability of a classmate being in that role" (p. 354).

I frankly do not understand that interpretation of participant roles. As I understand it, the very foundation of the participant role approach is that the likelihood that one person will be in a certain position (bully, victim, etc.) depends largely on other children's roles. The bully only bullies if he is supported, the victim is only victimized in the absence of a defender. In fact, there would simply not be a victim if there were many defenders and few bully supporters. Thus, in my interpretation, participant roles are—by definition—results of group processes, not the individual characteristics Madhavi and Smith ascribe them to be.

This divergence of interpretations of participant roles has far-reaching consequences for the relevance of their findings. Madhavi and Smith aim to demonstrate that participant roles are not individual characteristics, and they believe demonstrating this has implications for participant roles theory. But what if participant role theory already presupposes the very point they are trying to make here? In my view, their findings then provide support for the group phenomenon of participant roles. And very relevant and compelling support too.

A similar difference in interpretation arises in their discussion of "scapegoating". Madhavi and Smith aim to falsify the hypothesis that scapegoating is inevitable, and they convincingly do so by showing that a large proportion of classrooms have no scapegoats. However, there may be—at least—two very different scapegoating hypotheses at stake here that are easily confounded.

A *radical* scapegoating hypothesis might indeed be that scapegoating is inevitable. I am aware that this hypothesis is held by quite a few parents and teachers, and can be a major obstacle in any attempt to prevent or reduce

bullying. Knowing the excellent anti-bullying work of Madhavi and Smith's group, I am sure that they often encounter this cynical hypothesis in practice. I do, however, doubt whether anyone would seriously defend this radical position scientifically.

The alternative *conditional* scapegoating hypothesis has much stronger scientific roots and is presented extensively in Madhavi and Smith's introduction. It states (quoted from their text, this issue): ''Fraczek (1996, pp. 81–82) argued that the term 'scapegoat' is used mainly as a name for a person and/or out-group to whom is ascribed, without sufficiently rational reason, guilt for the failure and unrealized expectations of a social group .... The same phenomenon is observed in a classroom or school where some child is, temporarily or permanently, and without sufficient rational reason, blamed for various in-group failures'' (p. 355). This conditional hypothesis is fundamentally different in stating that scapegoating does *not* occur in every classroom, but only if certain conditions are met: There have to be group failure, unrealized expectations, or in-group failure. Thus, this conditional hypothesis would predict some classes to have scapegoats and others not to, just as the findings by Madhavi and Smith suggest.

In this sense, scapegoating would in fact be a phenomenon very much in line with a participant role approach: in a group configuration with one or two weaker individuals and a larger number of individuals wanting/trying to belong to a powerful subgroup, scapegoating might result *from this distribution of participants*. That is, not from individual characteristics themselves, but from their configuration in the group and the resulting group failure.

Atria et al. (this issue) argue that the participant roles approach does not accommodate variability between classrooms. They argue that large between-classroom variability falsifies purportedly generally made assumptions that bullying occurs at comparable rates in all classrooms, and that similar group mechanisms evoke and maintain bullying in all those classrooms. They demonstrate remarkable differences between classrooms in rates of bullying roles. The magnitude of the variability between classrooms they find is indeed impressive.

In their view of the bullying literature, no one seriously considers between-classroom variation. I was surprised to read this, and—doubting my own impression of this literature—read important participant role texts again to see whether I was mistaken. I agree with Atria et al. that there is little explicit referral to such differences (e.g., Salmivalli et al.'s classical 1996 text does provide a participant role distribution, but no indicator—*SD* or variance—of between-classroom variance in these distributions). Thus, it does indeed appear that the magnitude of the differences between classrooms has not been given enough attention.

However, participant role theory *itself* describes how different group configurations lead to different rates of bullying. Thus, the whole theory

seems to me to be based on the assumption that there are meaningful differences between classrooms, not on the alleged similarity Atria et al. criticize. The main issue, then, would be to formulate more explicit hypotheses about the sources of between-classroom variation that can very well be derived from the participant roles approach that was designed to do so.

Taken together, both studies provide important additions to the literature by demonstrating that classrooms with few or no victims or scapegoats exist, and that victimization and scapegoating are by no means inevitable. The practical importance of this falsification of an all too frequently held cynical folk psychology concerning bullying is evident.

I do not, however, believe the studies to have falsified either the scapegoating or the participant roles approach to bullying, but rather to have directed our attention to a lot of classroom variance that these theories may help explain. I wonder whether the very assumptions both studies aim to dispute are in fact made so frequently, and whether they are as fundamental to current theorizing on bullying as is proposed. Are these studies indeed shaking the very foundations of current theorizing about bullying, or are they rather addressing contextual variability in the applicability of theories that are themselves not falsified by their findings? I believe the latter to be the case.

Current theorizing on bullying describes a number of mechanisms that may cause or increase the likelihood of bullying, being victimized, and taking on various other social roles in the bullying process. Such theories do, however, to the best of my knowledge, not propose that these mechanisms occur with comparable frequencies in every single classroom. In fact, part of these theories—admittedly sometimes more implicitly than explicitly—concerns an outline of the very circumstances required to get the bullying mechanism going. Thus, at least in theory, they aim to explain the very variation in bullying between classrooms that both present studies describe. Finding variation in bullying between classrooms may support as well as falsify those theories, depending on whether the variance in bullying found in these studies can be explained by theoretically relevant classroom-level characteristics (climate, teacher, group composition), group level (social relations of whole group, friendships), and child-level characteristics (e.g., aggressiveness, temperament, etc.).

The main issue, therefore, does not seem to me to be whether variance between classrooms exists, but whether this variance can be predicted correctly by current theories. Neither study attempted to explain variance between classrooms by regressing this variance on theoretically relevant classroom characteristics, even though very relevant explanatory variables are suggested by the authors, such as the existence and effectiveness of school anti-bullying policies or interventions and the extent and sanctioning

of violence in the wider community. It would be interesting to see whether those variables do indeed explain part of the variance between classrooms found by Madhavi and Smith and by Atria et al.

Recent developments in applied data analysis have made it much more feasible to study the variance between individuals and between classrooms that both studies refer to. Multi-level analyses allow the very questions the studies pose to be answered in a straight-forward manner. As long as the above independent variables at individual and classroom level have been measured, variance can be decomposed into variance between individuals, variance between classrooms, and variance between individuals depending on the classroom they are in, and each of these three kinds of variance can be regressed on the suggested explanatory factors. Such analyses may long have required advanced statistical knowledge and user-unfriendly specialized software, but they have recently become much more accessible. SPSS versions 12 and up include extensive multi-level analysis possibilities, explained hands-on in excellent textbooks (e.g., Hox, 2002). Many of the challenging questions posed in both texts can thus be answered empirically, perhaps even with data already obtained by many research groups. For example, one may assess whether scapegoating depends on the group phenomena of in-group failure, on individual child characteristics, or on—as I would expect—an explosive combination of in-group failure and a specific individual child with certain catalysing characteristics.

Reflecting on the theories discussed in these papers and the design of most studies in bullying research to date, there appears to be a discrepancy between the static, cross-sectional[1] nature of most studies and the process-oriented nature of predominant theories. Whereas theories describe how and why certain group processes evolve, studies aiming to test those theories generally do not assess the very processes the theories are about, but merely assess one—outcome? intermediate?—time slice. Theories about bullying are formulated in terms of changes in one thing causing changes in other: In-group failure leads to scapegoating, then when a scapegoat is found, some children benefit, become less bullied, etc., and are thus reinforced in their role. When more defenders are present, less victimization follows, etc. Cross-sectional studies can not test such process hypotheses.

Trying to reconstruct a developmental process from its state at a single point in time requires a lot of inference, is inconclusive, and is prone to misinterpretation. To make a somewhat crude comparison: It is a bit like reconstructing the script for a stage play with only one picture of the closing scene as one's source of information. This may be a suitable approach for historians, as history cannot be repeated. We bullying researchers, are, however, in a much more privileged position: The bullying process is—like it

[1]Or longitudinal with infrequent measurement occasions.

or not—taking place in most schools, all of the time, and it is accessible to detailed, microgenetic studies of its development. A promising first step would be to go longitudinal with frequent repeated measurements of child, classroom, and contextual change factors. One might then, for example, assess whether change in classroom composition does indeed change prevalence of bullying (Atria et al.) and emergence of scapegoating (Madhavi & Smith), or whether a scapegoat remains a scapegoat in a different classroom.

In such studies, we will need to assess not only changes in bullying, victimization, and roles, but become much more focused on measuring the very processes believed to cause those changes. Madhavi and Smith give interesting examples of such mechanisms by introducing children's social reasoning concerning changes in the classroom. Structural assessment of such processes is very possible (e.g., Camodeca, & Goossens, 2005; Orobio de Castro, Merk, Koops, Veerman, & Bosch, 2005; Orobio de Castro, Veerman, Koops, Bosch, & Monshouwer, 2002; or even in vivo, Hubbard et al., 2004; Lochman & Dodge, 1998). Including such processes in longitudinal studies will enable much more direct tests of the hypotheses under study through mediation analyses. For example, concerning scapegoating one might demonstrate that bullying of a single scapegoat increases when classroom composition changes *because* more children consider bullying the victim a means to avoid becoming a victim themselves.

Ultimately, we will need to study these processes with longitudinal experimental designs to be able to demonstrate causality (Lacourse et al., 2002). There is great potential here. Most bullying research groups in Europe that I know—including the groups in this issue—are conducting bullying interventions. This provides a powerful tool not only to describe but also to influence the bullying process. Careful manipulation of this process may result in significant gains in our understanding of the causal mechanisms underlying the bullying process.

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