Letters to the Editor

Expanding the Focus and Also the Detail of Attention to Social Influences on Youth Violence

As she is prone to do, my colleague Nancy Guerra provides an insightful critical reminder of the importance of broadening our research emphasis to improve practical impact. She points out that we are making little headway to help affect youth violence, and it may be due to some constriction in focus. In her letter “Can we make violent behavior less adaptive for youth?”, Prof. Guerra suggests three important gaps that characterize much of the research and related preventive efforts, gaps that may leave important controlling influences and therefore important prevention opportunities out of our models. I applaud her voicing this rarely heard reminder to locate development within the social and political ecologies and to emphasize this as urgent for impact to be substantial. However, we should be thoughtful in how we can best scientifically study and be helpful and not simply exchange one constriction for another. It is likely that there is not a key waiting to unlock this complex social problem, but, rather, there is much to gain if these areas of work suggested by her are given as much consideration as those currently dominating our individual difference focus.

First, as she notes, in some settings and situations, violence can be an adaptive response, meaning it may provide safety, security, status, material goods and other rewards that motivate and/or sustain individuals. She indicates violence may be motivated by desperate need and limited alternatives (taking up drug dealing in a gang for money and safety) or by superficial pettiness that abides in some way in most of us (bullying for amusement). What may be more controversial is whether in fact, as she asserts, “violence works,” meaning, if traced in terms of benefit beyond the immediate moment and single perspective of the youth, it would show reliable gains in status, protection, and other immediate rewards. These might also relate to long-term benefits including enhanced capability, satisfaction, lower violence, and other important indicators of well-being. It may be that the answer will differ for short-term adaptation but not for long-term effectiveness [for a discussion of this distinction, see Tolan, Guerra, & Montiani-Klovahl, 1997]. Even short-term benefits may not be what seem to be there at first glance. Economic analyses suggest the income
gain of drug sales for most gang members is temporary and illusive, even if street lore is that it is a quick and easy way to make money. Also, gang involvement carries risk for arrest and violence from others that may counter any gain from use of violence. Thus, whether understood within a reinforcement model, a group norms model, or as an evolutionary force, all would predict lessening violence over time. Thoughtful theoretically sound formulations are needed that integrate the child as manager of his or her own development, political understanding as a developmental process, and understanding of how immediate compared to longer-term contingencies within local social settings (neighborhoods, communities) affect youth violence. This is not only important to permit informative scientific tests of alternative propositions but also to help move forward on how the challenge of attending to social-structural characteristics in prevention can be done. It may be that connecting the misunderstanding youth brings in evaluating risk and benefits, the real insights they and their family have about opportunity and social justice, and the limited opportunity to know about and access other resources for status, safety, and access to conventional adult roles, status, and rewards can provide the links in this that are political to the personal causal chain. I expect it will provide little suggestion that violence works or that it is a thought-based choice. I have hope that such complex linking can suggest more powerful preventive options like the ones Prof. Guerra seeks.

Second, she suggests that violence may be as much a product of the situation or community norms as any individual proclivity. This means that we should be examining how differences in social and economic resources, norms about status, property, use of violence, self-protection, and structural characteristics that can determine access to necessary developmental resources explain violence rather than presuming it is best understood as affecting susceptible individuals. This approach has had considerable influence in criminology without much translation into developmental and social psychological studies of motivation related to violence. The rich tracking of criminal actions as rational decisions, particularly within a given setting or set of potential options has provided organized explanation of alarming trends that individual difference-oriented research cannot clarify [see Fagan, Wilkinson, & Davies, 2007]. Similarly, there has been considerable attention to how time and place can help explain crime and that suggests important opportunities for prevention, particularly if intersected with social and developmental approaches [for one such analysis, see Horney, Tolan, & Wesibourd, 2012]. While Prof. Guerra points toward more attention to these issues in our research, it is important to note this is contrary to the developmental science agenda that is increasingly focused within individuals with neurobiological processes as the presumed critical interest. The exciting possibilities gained from longitudinal and neurobiologically focused research seems to have diminished appreciation for and use of population or place-based comparisons, which are needed to validly compare how setting and social condition differences affect violence. Forgoing one focus for another is not progress; what is needed are concerted efforts to connect the setting and social-structural characteristics to individual difference and neurobiological processes.
In her third alternative focus, she denotes the promise of positive youth development or how youth succeed. This is a nascent area conceptually with almost no connection to mainstream developmental and social research. Launched as an alternative to risk-focused research and with the hope of making risk-oriented programming outmoded, there is risk of exchanging one set of blinders for another which has such great promise because it simply has not been tested well. There are burgeoning thoughtful attempts to study positive youth development with one important initial finding being that positive development and problems in functioning are not exclusive or opposites. There is urgent need to determine empirically how access to resources, positive character traits, and important processes such as social connection and meaningful engagement with civic life might prevent violence, but we also need to know how these relate to risk factors and preventive efforts that have proven to have some impact already. I would second Prof. Guerra’s statement about the promise of focusing on how we can support youth to increase conditions for successful effective development. I would add, however, that my enthusiasm rests on recognizing that there is considerable scientific understanding that may precede sound application to violence prevention that can make more difference than what we have learned to date. As the initial forays into understanding positive youth development are suggesting, it will be a more complicated picture than simply that health-promoting and development-supporting efforts will take care of risk. Perhaps what is needed is not only concentrated and thoughtful study that relates what setting supports and youth capabilities help minimize risk but also study of how these might coincide with problems such as use of violence, mental illness, low school engagement, and limited attention to health management. As with her other points, Prof. Guerra points to important and bold challenges for scientists interested in affecting youth violence. Yet, the boldness may be a very reasonable and important reminder to rebalance our theoretical and empirical emphasis (as well as the resulting actions) to persons within situations with as much attention to population and setting variations in social circumstances as to individual variations in susceptibility among a given population or those within a given setting.

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References


**Conceptual Analysis and Empirical Facts**

In his erudite commentary, Racine criticized my use of conceptual analysis in an explication of one prominent theory on infant imitation. In the present letter, since I commented elsewhere on these claims [Paulus, 2011], I will not focus on Racine’s claims regarding infant imitation, such as the claim following Tissaw [2007] that “some psychological concepts, such as ‘intention,’ seem intrinsic to the ability to imitate because if one does not intend to imitate then one is presumably not imitating” [Racine, 2012, p. 132]. Rather, I will focus on the method and will defend the use of conceptual and empirical analyses as put forward in my article [Paulus, 2012].

Racine’s [2012] analysis centers on the use of the concept “rational imitation.” In particular, he scrutinizes whether or not this concept is correctly applied to describe infants’ behavior, and, based on a brief evaluation, he concludes that this is not the case. I am not sure if this analysis captures the core of the concept because he focuses on whether “imitation necessarily involves rationality” (p. 132). Yet, I do not think that anybody claimed that imitation is necessarily and always based on this kind of rationality. On a side note, my co-authors and I never claimed that motor resonance is “logically intrinsic to imitation” [Racine, 2012, p. 134]. The claim is, rather, that imitation could in some instances be based on a rational evaluation of the other’s actions. That is, the concept of imitation could have been applied correctly by the proponents of rational imitation as “rational” applied to only some instances of imitative behavior, designating a putative mechanism.

More importantly, however, is that proponents of rational imitation could simply respond to Racine that they agree that “rational imitation” is not the best wording to describe their empirical findings. Nevertheless, they could insist on their interpretation that infants’ behavior in these tasks [e.g., the re-enactment of the head action in Gergely, Bekkering, & Kiraly, 2002] is a “selective, inferential process that involves evaluation of the rationality of the means in relation to the constraints of the situation” [Gergely et al., 2002, p. 755]. Indeed, these proponents could state that, given they are not sure how to best label this behavior, we could use a temporary placeholder and call it *ABCDing*. 
Clearly, from a developmental psychology point of view, this would not change a lot as the authors would still claim that sophisticated reasoning abilities subserve infants’ re-enactment of observed actions. Whether we might label this behavior imitation or not might be a secondary question. As developmental psychologists, we are still confronted with the task to assess whether or not we think that this interpretation of the data is warranted and likely given the current state of knowledge on infants’ competencies.

What would be the consequence of such a replacement of terms for conceptual analysis? Racine’s notion of conceptual analysis would be of very limited help as it could only reveal if the concept of ABCD is correctly applied in the respective situations. However, as this concept was introduced with respect to these phenomena, its use is correct by definition.

Nevertheless, a developmental psychologist would still be interested in exploring the preconditions that must be met by the infant to engage in ABCDing and to reflect on whether or not the theory is well-grounded. That is, we could still apply the kind of analysis suggested in my article [Paulus, 2012]. We would still ask which kind of abilities are implicated in the claim that infants’ re-enactment of the observed behavior is “a selective, inferential process that involves evaluation of the rationality of the means in relation to the constraints of the situation” [Gergely et al., 2002, p. 755]. And we could still wonder whether or not we have evidence that infants indeed possess these abilities that are necessary to engage in such complex considerations [Paulus, 2012].

This point also renders Racine’s [2012] objection of a conflation of conceptual and empirical issues in my analysis as not warranted. The first step in my approach is a conceptual analysis of a particular theory. Here, the conceptual implications that remained implicit at the current level of formulation of the theory are explicated and spelled out [Brandom, 1994]. This kind of analysis remains on a purely conceptual level and does not involve empirical issues. It corresponds to the deduction of hypotheses from a given theory, with the small difference that it does not focus on theoretical predictions of future behaviors (typical of empirical hypotheses). Rather, it “looks back” (as described by Carpendale and Müller’s [2012] commentary in a stance resembling Kant’s [1781/1998] transcendental analysis) and tries to elucidate the conceptual commitments one makes when accepting this theory.

The second step then looks at the empirical evidence. Are the commitments met or not? Do infants possess these abilities at all that are necessary to engage in the hypothesized way of reasoning? Surely, one could directly examine these questions by empirical work, but here one can also rely on already acquired knowledge to estimate whether the commitments are warranted or not. This kind of analysis corresponds to the empirical test of a hypothesis.

In sum, contrary to Racine’s objection, this form of analysis clearly distinguishes between conceptual and empirical issues. Yet, for an analysis of the strengths and limitations of empirical
theories, it is necessary to relate conceptual analysis and empirical facts to each other in the same vein as we relate theoretically derived hypotheses and empirical findings to each other.

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References


Clarifying Conceptual Analysis and Empirical Facts: A Reply to Paulus

In a recent *Human Development* Letter to the Editor, Paulus [2012a] expressed doubts about the utility of my commentary on his recent use of conceptual analysis to criticize Gergely and Csibra’s so-called rational imitation explanation of preverbal imitative abilities [Paulus, 2012b; Racine, 2012]. However, by not providing the context for the points that he disputes, Paulus has changed or at least oversimplified my arguments in his letter in ways that weaken their force. Therefore, I write this reply to clarify important issues that Paulus raises in his letter. I argue
essentially that Paulus is repeating the premises contained in his initial article while not defending those premises from the concerns I raise in my commentary. I conclude by reemphasizing the point that my analysis supports the very sort of critique that Paulus wishes to make of researchers like Gergely, Csibra, Tomasello and colleagues.

It is helpful that, in his reply, Paulus points to his recent article [Paulus, 2011] where he notes his comments on the issue of whether intention is intrinsic to imitation. I had not previously read this article, and I believe the differences between Paulus’ approach and conclusions and mine are anticipated in his 2011 piece. Therefore, I will spend some time on his presentation of these views to show where we clearly agree and on other areas where we clearly disagree, which in my view still bedevil the conclusions he draws in his target article and his Letter to the Editor.

Paulus [2011] mentions Tissaw’s research in this area, work that I was surprised not to find in Paulus’ [2012b] target article. Although it is reassuring to know that Paulus is familiar with Tissaw’s work, I do not find his discussion of it satisfying. For one thing, whereas Paulus [2011] claims he does not “entirely agree” with Tissaw’s account (p. 853), the disagreement in Paulus’ view turns on the issue of whether one needs to be consciously aware of one’s intention to imitate in order for it to count as imitation. In my commentary, I claimed that the concept of imitation is logically related to the concept of intention and that if one did not intend to imitate, it would not count as imitation. Paulus would surely agree that nowhere was I making a hypothesis about conscious processes or the lack thereof; it is an empirical matter of whether infants (or for that matter, of whether adults) are in some cases aware of their intentions when engaging in a particular instance of imitative behavior. This is because there is nothing intrinsic to (logically required by) the concept of imitation that would mean that imitators should or should not engage in conscious processes when they imitate. Intention also is not always a conscious process; sometimes we reflect on our intentions, sometimes we do not.

Paulus does not attempt to explain in his reply how or why imitation can be unintended and yet still count as imitation. Instead, he cites Paulus [2011] where he distinguishes a so-called behavior-based definition of imitation, which he considers an ordinary language definition arrived at through reflections on Wittgensteinian methods of conceptual analysis and an intention-based definition attributed to Tomasello and colleagues [2005] among others. According to Paulus [2011], this behavior-based definition is satisfied “in all cases when infants show the same behavior a model has performed in front of them and as a consequence of the particular action the model has performed (and not as a consequence of any other behavior)” (p. 850). However, to do so in the right circumstance is what we mean by intending to imitate. To put it differently, one cannot copy the actions of another by accident and have it count as imitation. As long as one is not being forced to do so and it is not occurring by happenstance, it is intentional. Why does Paulus not call this intentional? I suspect it is because he does not agree with the empirical explanations given by, for
example, Tomasello and colleagues [2005] or Gergely and Csibra, which to Paulus seem too “high level.” But this is to confuse conceptual and empirical issues.

I urged in my commentary that an intrinsic part of the meaning of the concept, in its ordinary sense, is the fact that the actor intends the action. I have suggested that Paulus inadvertently agrees with me, but if he wishes to dispute this, one way would be to show how a behavior can be unintended and still qualify as imitation. Ironically, this logical point is reflected in the distinctions made by Tomasello and others with respect to imitation versus stimulus enhancement or emulation that Paulus [2011] criticizes. This criticism on the part of Paulus is illegitimate because the categories used by Tomasello et al. [2005] are sensible. Stimulus enhancement and emulation do not count as imitation because there is no intention to imitate. The problem in Tomasello et al.’s [2005] account from my point of view, however, is its sheer generality; Tomasello and colleagues join Gergely and Csibra in making the logical mistake that rationality is intrinsic to imitation [Racine, 2012]. And, they mistakenly claim to have discovered this empirically. My brief conceptual analysis in my commentary on Paulus [2012b] attempted to show that rationality is not intrinsic to imitation. What is wrong with my analysis as presented? Paulus’ reply does not address this and instead points to Paulus [2011]. But Paulus [2011] confuses this issue by making it one of whether agents are consciously aware of their intentions or not. Perhaps the issue is that like Gergely, Csibra, Tomasello and colleagues, Paulus also thinks of intentions as things in the head. From a Wittgensteinian sort of analysis, even though it is rather the other way around, we attribute certain capabilities to particular agents as a consequence of the things that they do in the right sorts of situations.

In developmental and comparative investigations of imitation, a problem is that the logical relations between intention and imitation in cases like these are overlooked and turned into empirical questions about whether agents who imitate really intend to imitate. However, if an ordinary use is in play, that question makes no sense. Given that researchers typically wish to make claims about imitation and intention in an ordinary (non-technical) sense, then that is the sense in play. That is, they are trying to explain familiar things. So, what is the underlying issue here? As Paulus [2011] insightfully recognizes, what he calls intention-based definitions are nothing more than a causal empirical account in disguise. And the problem, as Paulus [2011] also recognizes, is that the definitional and the empirical are being conflated.

Is it unfair, then, that I charge Paulus [2012b] with conflating conceptual and empirical issues? I think not. After diagnosing this problem, Paulus [2011] goes on to claim, “Clever experimental designs are necessary to determine in every given situation if the behavior was, for example, subserved by intention-reading or motor resonance” (p. 852). However, if my ordinary language read of imitative behavior is correct, such behavior is intrinsically intentional and this matter cannot be solved empirically; it becomes an instance of what I called in my commentary a rich-lean debate, which turns on a lack of attention to the conceptual nexus of the psychological
concept in question. To put it differently, whether imitation seems best supported by low-level (lean) motor resonance circuits (as per the suggestion of Paulus and colleagues) or high-level (rich) conceptual reasoning abilities (in the manner of Tomasello and colleagues or Gergely and Csibra) does not change the meaning of the behavior (i.e., how the concept is used). Am I mistaken about this? Paulus does not say. However, as Paulus [2011] has already diagnosed, the problem in Tomasello et al.’s [2005] intention-reading definition is that they have taken their putative cause and stipulated it as a new definition. I think we are now in a better position to understand the conceptual-empirical conflation in Paulus [2012b] that is still present in his recent letter [Paulus, 2012a].

Paulus [2012a] misleadingly contends that I remarked that he and his colleagues claimed that motor resonance is “logically intrinsic to imitation” (p. 1); what I stated was that motor resonance was no more logically intrinsic to imitation than was the principle of rational action championed by Gergely and Csibra (or, for that matter, the shared intentionality concept championed by Tomasello and colleagues). In this section of my commentary, I was essentially chastising Paulus for using Hackerian methods against his competitors but not applying them to his own position. To repeat an underlying issue in my view: Paulus [2012b] is relying on a form of conceptual analysis that is derived from Brandom and is at odds with the Hackerian analysis used in the conclusion of Paulus’ article. What Paulus could have done in his reply – but did not do – would have been to respond to my claim and justify the use of Brandom as a form of conceptual analysis. This would not have been an easy task because Brandom is a theoretician who has drawn on Wittgenstein but does not advocate a form of conceptual analysis. Perhaps Paulus could have just said that this does not matter, which he does with my attempt to clarify rational imitation when he claims that Gergely, Bekkering, and Kiraly [2002] could concede that their phenomenon of interest may not relate to imitation and instead call it an investigation of ABCDing [Paulus, 2012a, pp. 1–2], and then Paulus could defend his own idiosyncratic form of ABCDing conceptual analysis. Paulus did not do this either.

What I tried to show in my commentary is that Paulus’ form of analysis, whatever its merits and derivation, mixed up empirical and conceptual issues. Paulus does not rebut any of this in his reply, but rather repeats his position from his target article with the added claim that “for an analysis of the strengths and limitations of empirical theories, it is necessary to relate conceptual analysis and empirical facts to each other in the same vein as we relate theoretically derived hypotheses and empirical findings to each other” [Paulus, 2012b, p. 3]. This general way of thinking is part of the problem, at least with respect to performing useful conceptual analyses. Yes, the researcher has the burden of relating conceptual and empirical matters. However, as Wittgenstein and more recently Hacker have emphasized, in typical cases conceptual matters antecede, logically, matters of fact. It is thus misleading, if not a case of wholesale misunderstanding, to claim that the relation of empirical and conceptual matters is of a piece with theoretically derived hypotheses and findings that support or refute such hypotheses. The conceptual point is that we have to know what we are
Talking about before we theorize about and test it. When our research concerns ordinary psychological concepts like intention, rationality, and imitation that have clear uses, wittingly or otherwise we begin such investigations parasitic upon these uses. If not, we cannot claim to be saying anything about intention, rationality, or imitation. Such is the peril of the concept $ABCD$ that Paulus, in a surprisingly non-Hackerian manner, puts forth in his reply.

Of course, we can and should set down new definitions of technical concepts when we have reason to need them, but if these new concepts contain, to use Paulus’ example of a “selective, inferential process that involves evaluation of the rationality of the means in relation to the constraints of the situation” [Gergely et al., 2002, p. 755, as cited in Paulus, 2012b, p. 1], a familiar concept like “rational,” that has a clear and prior meaning, it becomes confusing. As I attempted to elucidate in my commentary, not only is nothing to be gained by such agnosticism, but it will also add to the confusion because we still have to evaluate the claim in light of a familiar and prior network of concepts (such as “rational”).

As for the remainder of Paulus’ claims in his Letter to the Editor, it suggests to me at least three possibilities: (a) I was not clear enough in my commentary on his target article, (b) Paulus did not understand many of my criticisms, or (c) Paulus is more intent on disputing my claims by repeating his position than defending his position in light of my commentary. It is always challenging to present a relatively unfamiliar method for approaching a problem in a short commentary, and perhaps I could have been clearer in my comments. However, Paulus [2011] uses Wittgenstein and Hacker more thoroughly than Paulus [2012b], and, therefore, I have to conclude that Paulus is familiar enough with these arguments to have understood most of my critical remarks. What I was attempting to do in my commentary was to show how conceptual work could resolve issues of rich-lean interpretation by holding fast, at least initially, to ordinary uses of concepts. It is ironic, then, that in his reply to my work, Paulus has defended the very positions of which he is critical in his 2011 and 2012b articles on imitation. To wit, he claims no one would say imitation necessarily involves rationality [Paulus, 2012b, p. 1], which may well be true. However, if one is explaining imitation through rational means in an unqualified manner – for it is surely the case that Gergely and Csibra do not claim that they mean this as only one possible mechanism, since their point is that imitation occurs by the rational processes they defend – then the logical question is whether rationality is intrinsic to imitation. What I was attempting to show Paulus in my commentary was that an analysis of the concepts “rational” and “imitation” could lead him to the very place he wanted to go. Surprisingly, Paulus does not want to go there.

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References


