Developmental Cognitive Neuroscience 17 (2016) 131–137

Contents lists available at ScienceDirect

Developmental Cognitive Neuroscience

journal homepage: http://www.elsevier.com/locate/dcn

Commentary

The audacity of specificity: Moving adolescent developmental neuroscience towards more powerful scientific paradigms and translatable models

Jennifer H. Pfeifer[a,b,*], Nicholas B. Allen[a,b,*]

[a] Department of Psychology, University of Oregon, United States
[b] Center for Translational Neuroscience, Prevention Science Institute, University of Oregon, United States

ARTICLE INFO

Article history:
Available online 23 December 2015

Keywords:
Precision
Translational neuroscience
Models
Dual-systems
Social reorientation

In this issue of *Developmental Cognitive Neuroscience*, two articles revisit a pair of seminal models that have permeated developmental neuroscience research focused on adolescence. Shulman and colleagues (2016) "review, reappraise, and reaffirm" research relevant to dual-systems models of adolescent development, while Nelson and colleagues (2016) "expand and update" their proposal regarding the social reorientation model of adolescence and its underlying neural circuitry. The present commentary aims to complement these efforts with a constructive critique that leads to concrete steps we believe can, and should, be taken to improve our models and maximize cumulative scientific progress in the field. We propose here that for adolescent developmental neuroscience to be truly meaningful – and by this we mean precise enough to not only make accurate and testable research predictions, but also be translatable into prevention, intervention, and policy programs that will significantly improve developmental outcomes for adolescents – we need to refocus our priorities and enable our scientific models to evolve.

Nearly two decades ago, Eysenck (1997) described the range of scientific methodologies appropriate to different stages of psychological research:

> "Science begins with a hunch, acquired through observation and induction, which is clearly a preparadigmatic position. If the hunch seems to work, psychologists construct small-scale hypotheses, for which they seek verification. If such verification is forthcoming in sufficient quantity, the level of theory is reached, and one may then consider the demands of falsification. . . The point between hypothesis and theory would seem to mark the advent of a paradigm. . . when the ordinary business of science takes over, that is, the large-scale testing of deductions from the theory, and the attempt to explain anomalies in terms of the theory's apparent failure" (pp. 1225–1226).

We believe that many of our models in adolescent developmental neuroscience, and the resultant research, are persisting in a *verification* stage, where we primarily focus on supportive evidence that is consistent with the model in question. Indeed, the task is so complex that this is no small achievement, and it is not surprising that the field registers some satisfaction at having models that explain a wide range of phenomena. However, greater progress will be achieved if we progress to a more *falsification* oriented approach, where we (i) rigorously examine and account for inconsistent evidence, and (ii) put our models at strong risk of falsification based on more precise predictions.

A precise prediction that is supported by data provides much stronger evidence for a model than does a less precise prediction.

* Corresponding authors at: Department of Psychology, 1227 University of Oregon, Eugene, OR 97403-1227, United States.Acknowledgements: The authors would like to thank members of the fall 2015 graduate seminar on Adolescence at the University of Oregon who provided valuable input on a draft of this manuscript. JHP was funded by P50 DA035763 and R01 MH107418. NBA was funded by R01 MH107418.
E-mail addresses: jpfeifer@uoregon.edu (J.H. Pfeifer), nallen3@uoregon.edu (N.B. Allen).

http://dx.doi.org/10.1016/j.dcn.2015.12.012

In other words, the degree of logical support for a model is greater given the rarity of the observation absent the theory – what Salmon (1984) has called a "damn strange coincidence" and Meehl (1978) has referred to as a "risky prediction." The importance of this kind of precision goes beyond scientific progress and model building (although that is reason enough). One of the great challenges that our field, along with many others, struggles with is finding strong translational applications of our work – ones that can really have an impact at both the population and individual level (Allen and Dahl, 2015). However, for this admittedly lofty goal to ever be achieved we have to have models that make predictions that are sufficiently precise and robust that we can prescribe public policy and clinical innovations that have real impact.

We are well aware that is much easier to sit on the sidelines and encourage others to do better than it is develop models and put them to the test. We have had our own attempts at theorizing and building models, with varying degrees of success, and we know that it is hard and exacting work (e.g., Allen and Badcock, 2003; Davey et al., 2008; Pfeifer and Peake, 2012). In this respect we would like to make it unambiguous that the work represented in the target articles is a brave and necessary part of the scientific process. The authors and their ideas have our respect and admiration. Also, it is fair to note that we are not proposing an alternative model here, but we do believe that the approach we describe herein is important in addition to, and support of, the process of model building and refinement.

1. A precision approach for adolescent developmental neuroscience: PECANS

In recent years, a number of reviews surveying the evidence regarding influential models of adolescent brain and behavioral development (Pfeifer and Allen, 2012; and others, e.g., Bjork et al., 2012; Crone and Dahl, 2012; Telzer, 2016) have noted sets of findings that do not conform to model predictions. It is tempting for supporters of these models to push these inconsistencies to the side, and for both sides to create a qualitative "box-score tally" of studies that do or do not provide support. For example, Shulman and colleagues (2016) list nine articles that show adolescents engage the striatum to a greater extent than both children and adults, four articles that find the opposite pattern, and four more that fail to demonstrate any age differences (p. 20). As is common in qualitative reviews, this list is then summarized as revealing "considerable evidence" in support of dual-systems models, while a "handful of studies" find the opposite pattern or no differences whatsoever. They then go on to explain this inconsistency in terms of separating out reward anticipation from receipt – three of those nine supporting studies are listed to demonstrate that adolescents engage the striatum "consistently" more than adults during reward receipt. Meanwhile, they suggest there is a tendency to see increased striatum during anticipation only when the cue reliably predicts greater likelihood of reward (referencing two studies that observe this, and two that do not).

Regardless of whether such lists generated by qualitative reviews (including both the Shulman and Nelson papers in this issue, but definitely not limited to them) are intended to be comprehensive or illustrative, we propose that it is well past time for us all to move beyond qualitative box-score tallies, and engage in more precise assessment of how robustly the evidence supports or contradicts the models. A recently published quantitative meta-analysis (Silverman et al., 2015) observed that across 26 studies, adolescents activated a number of regions more than adults during reward processing, including ventral and dorsal striatum, insula, amygdala, anterior and posterior cingulate cortex, orbitofrontal cortex, and frontal poles. This is an essential step away from

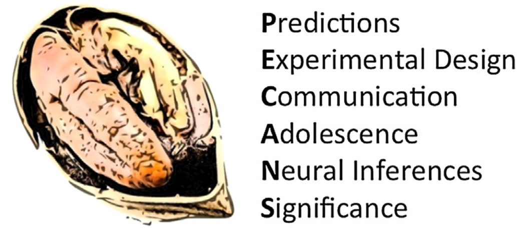

Fig. 1. The PECANS checklist, a mnemonic to enhance precision in adolescent developmental neuroscience.

box-score tallies. We applaud this work and will be the first to note that this kind of an assessment is much stronger and more satisfying, even in terms of *verifying* dual-systems or imbalance models. However, Meehl (1997) suggested that ideally, model assessment will be influenced by *both* the precision of prediction and extent to which observed data are close to those predictions.

As we have written about previously (Pfeifer and Allen, 2012), dual-systems and imbalance models in particular are applied so widely that they can be (and are) used to explain the vast majority of adverse outcomes in adolescence (and beyond; e.g., risk taking, anxiety, depression, violence, substance abuse, borderline personality, and indeed adolescence itself). Theories that include description of specific social brain systems, such as those of Nelson and colleagues (2016), have been less susceptible to this type of indiscriminant application – not because they are inherently less vulnerable to it, but mainly because to date there has been significantly less specific research informed by the predictions of these models. We suspect that as research interest in this subfield continues to intensify, the social reorientation model and other similar theories will rapidly face similar challenges in specificity. This is foreshadowed by the efforts of Nelson and colleagues (2016) to expand the social reorientation model beyond adolescence, and illustrate potential tension between deep precision and broad application just as experienced in previous years by dual-systems and related models.

In addition to a lack of precision in the applications of these models, there is a corresponding lack of audacity in the ways we (ourselves included) have tested these models. For a field so concerned about adolescent risk, we all have played it remarkably safe! There are many contributing factors to our collective risk aversion, not least of which include the need in new areas of study to build foundational knowledge bases from which to turn our hunches into hypotheses (as described by Eysenck, 1997), as well as the expense and difficulty of conducting adolescent developmental neuroscience research, particularly longitudinal and ecologically valid assessments. As such, we suggest that the field will be best served by taking the following concrete steps towards "audacious specificity" in the following domains (see Fig. 1): **P**rediction, **E**xperimental Design, **C**ommunication, **A**dolescence (Developmentally Meaningful Indicators), **N**eural Inferences, and **S**ignificance (Ecological and/or Translational Outcomes).

Precision in the first three areas (prediction, experimental design, and communication) is simply good scientific practice across disciplines. Additionally, accomplishing precision in these areas also requires precision in the last three areas (adolescent development, neural inferences, and significant outcomes) that are more particular to the field. In many cases, issues that are presented below with respect to one area actually contribute to others as well. Together, these six keywords provide a guiding mnemonic, following in the tradition of utilizing the PICO checklist to guide evidence-based medicine, which has been credited with improving both research practice and research synthesis through systematic reviews and meta-analyses. (The PICO checklist is a method used to frame and answer a clinical question – the mnemonic stands for