‘The Scylla of nativism …’

Stack and Lewis [2008] make several interesting suggestions in their review article on infants’ understanding of mental states. They claim that: (1) we should be cautious about rich interpretations of results from infant cognition research, particularly when conclusions are based only on looking times; (2) we should employ a rich variety of measures, tasks and new technologies, including techniques for eye-tracking; (3) associationist explanations for the results reported by Onishi and Baillargeon [2005], and by Surian, Caldi, and Sperber [2007], are at present just hypotheses open to empirical tests and they are not very convincing; (4) such findings should be interpreted as demonstrating that infants can understand actions and perceptions within non-shared environments, without invoking meta-representational skills, and (5) studies looking at the effect of different levels of engagements ‘lead us away from a simple nativist perspective’. Their article is critical of ‘adultocentric’ views of infant competences and it repeatedly warns the readers of the need to avoid the Scylla of nativism, namely ‘the appeal of a neatly packaged set of a priori skills’ that ‘lures us away from a focus on how such abilities actually emerge’ [p. 231].

We agree with Stack and Lewis on points 1–3, particularly on the need to be cautious in data interpretation and to employ a variety of tasks, paradigms, and measures that, hopefully, would provide converging evidence. At the same time, we believe that such cautionary remarks apply to every scientific investigation and every experimental method; there is nothing about infant looking-time methods that is any more problematic in this regard than, for example, using verbal instructions, stories, and questions in experiments with older children. Indeed, the looking-time methodology has been one of the more intensively studied and discussed methods since Fantz’ [1963] pioneering work on newborn form perception. One of the contemporary leaders in examining existing and developing novel looking-time methods is, in fact, Baillargeon and her students.
As for point 4, as scientists, we want to discover the correct explanation for human social intelligence and how our social intelligence in infancy produces development. In this vein, we believe that an early meta-representational capacity plays an important role in driving social development. In particular, we believe that a ToMM currently provides the best cognitive explanation of infants’ ability to take into account the effect of agents’ past perceptions on their future actions. Indeed, this view stands out for having predicted the sorts of findings that Baillargeon’s bold investigations point the way to. The other positions that Stack and Lewis refer to as associationism and ‘the interactional account’ made no such predictions, and either have to find a way to be consistent with such findings, or hope to deflate them. Currently, there is a surge of such attempts (Stack and Lewis being a case in point). It is undoubtedly good for the field to have been shaken out of the scientific complacency into which it had sunk.

As for their last point, since Stack and Lewis refer repeatedly to nativism and associationism as ‘Scylla and Charybdis,’ they apparently want us to associate these two theoretical positions with catastrophic shipwreck and disastrous failures. This rhetoric is surprising, given that both theoretical views have for centuries provided inspiration for both theoretical advance and empirical research. While associationism has been recently revived by current connectionist modeling, nativism has encouraged a new respect for the cognitive powers of infants and animals and prompted surprising discoveries [e.g., Schlottmann, Surian, & Ray, in press].

We are puzzled by the grossly oversimplified view of nativism assumed by the authors. It is hard to even recognize current or past thinking in this mold. For example, focus has never been on skills; it is obvious to everyone (even viewers of Bambi’s first steps) that where an infant has any ability to act in a structured way in the world, their performance initially lacks skill. (Infants’ lack of skills is exactly what has made the measurement of looking time a key method.) Instead what has been under discussion is the innate possession of abstract concepts [Leslie, 1987] and principles [Gelman, 1990]; that is, the focus is on competence, not on performance [Surian & Leslie, 1999]. This is a crucial point because, while some representational components of the infant’s cognitive systems are thought, on these views, to be innate, the skills that allow infants and children to exploit such representations typically require processes of learning, tuning, and practice.

There is at present no nativist theory claiming that the skill to reason about others’ belief is present at birth. It may be worth clarifying here that the term innate is used in nativist theories to express the idea that a certain concept or principle is not derived from experience; it is not meant to express the idea that they are present and available at n days from conception or at birth [Samuel,
Some nativist proposals claim that concepts such as *believe* and *pretend* are innate, and are core components of the human social competence, but other important processing components, such as inhibitory functions, may well develop in response to practice and social interaction. To posit a given representational basis for development does not diminish our interest in how the ability to efficiently exploit such representational structures develops; and we chart its development not only in young infants and toddlers, but also in school-age children. Indeed, Leslie and colleagues, rather than ‘deny the need to identify clear developmental pathways’ [p. 233] for false belief understanding, have shown how this pathway extends beyond the usual 3- to 4-year-old shift to include further shifts, one around 6 years and another at some yet-to-be-determined time between 9 years and adulthood [Friedman & Leslie, 2005; Leslie, Friedman & German, 2004; Leslie, German, & Polizzi, 2005].

Nativism is not and never has been the enemy of developmental pathways, as Stack and Lewis suggest, and, despite the constant repetition of such rhetoric, it never will be. It does, however, place boundaries on extreme accounts in which *everything* is learned and everything is learned in the same way by the same mechanism [Siegal & Surian, 2007]. It does conflict with the view that mind-brain capacities, utterly unlike other anatomical organs, develop purely as the result of individual learning experiences. At the same time, the developmental processes of gene expression (epigenesis) and of functional maturation are entirely compatible with the existence of learning processes. Psychological development depends on all of these [Meristo, et al., 2007; Surian & Siegal, 2008].

Stack and Lewis point to a way out of the ‘doomed lines of argument’ [p. 229] of nativism and associationism. They see this third way in the view proposed by Liszkowsky, Moll, Tomasello, and others, a view that emphasizes shared intentionality, social interaction, and social engagement in children’s and infants’ acquisition of social cognition. Tomasello and colleagues have greatly enriched our appreciation of the intricate interactive and communicative phenomena we need to explain in the study of early social competences and learning. However, when we turn to the business of cognitive explanation, we still confront the traditional problems in understanding the underlying cognitive mechanisms. In this vein, we note that Leslie’s [1987] account of early pretend play was the first to highlight the basic social-interactive and communicative nature of even early pretense, and the first to offer a single simple mechanism that linked solitary ‘first person’ pretend with ‘third-person’ pretense recognition with ‘first person plural’ shared pretense [Friedman & Leslie, 2007]. Why, we wonder, are Stack and Lewis less keen on this account even though it emphasized ‘shared intentionality, social interaction, and social engagement’?
In sum: (1) like everyone else, we are in favor of using a variety of appropriate empirical methods but we do not think that rhetoric will help much; (2) nativist hypotheses are not 'anti-developmental', indeed there are a number of ways in which such models can explain developmental changes; (3) while late competencies and developmental changes do not necessarily confute nativist hypotheses, early competencies do not necessarily rule out the role of learning. Our guess is that a full developmental explanation for the ontogenesis of social cognition will encompass specific innate representational capacities, processing constraints, and learning mechanisms.

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The all-encompassing evidence of ethnotheories

Comments on Sara Harkness’ essay review of my book Cultures of Infancy

Cultural, cross-cultural psychologists and anthropologists share an interest in understanding cultural conceptions of diverse domains of human life. Sara Harkness and I share the interest in understanding cultural conceptions of parental ideas about socialization and children’s development. What I learned from Sara’s essay review of my book Cultures of Infancy is that researchers and scientists also live within their own – maybe disciplinary? – ethnotheories and that these ethnotheories guide their perceptions and their information processing. Sara Harkness obviously looks through the lens of an anthropologist who is interested in unique cultural phenomena. I look through the lens of an evolutionary based cross-cultural psychologists interested in similarities as well as differences. Both lenses seem to lead to very different perceptions. Sara Harkness classified my conceptual approach in to the Individualism/Collectivism camp (‘the individualism/collectivism school of thought’). She defined, as the essence of this framework, a unidimensional dichotomy, with only two types of selves. In line with others [e.g., Killen, McGlothlin, & Lee-Kim, 2002; Neff, 2003] she described this framework as oversimplifying cultural complexities.

My view or lens is that I do not follow the individualism/collectivism framework, although it has opened up a new and rich tradition of thinking about cross-cultural differences [Hofstede, 2001; Triandis, 1995]. Accordingly, I state on page 33 of Cultures of Infancy, that the conception of prototypes that I propose does not ‘...represent a binary conception, as it conceives of independence and interdependence as two independent dimensions’. I follow and extend the frameworks proposed by Markus and Kitayama [1991] and Kagitcibasi [2007]. The psychological prototypes that I propose are based on prototypical environments, urban, highly educated, middle-class families and rural low-educated farming families (not traditionally living families in general). They differ with respect to the prevalence of the developmental organizer. The developmental organizer of the independent self is autonomy – autonomy also defines the mode and patterns of relatedness; the developmental organizer of the interdependent self is relatedness – relatedness also defines the mode and patterns of autonomy [cf. Keller, Demuth, & Yovsi, 2008]. The sociodemographic foundation of cultural models follows the Whiting tradition [the model of
psychocultural research, Whiting, 1977] as further furnished with an evolutionary framework. Resulting is not a ‘theory of sociodemographic determinism,’ [p. 416, Harkness’ lens], but a conception of developmental pathways that are geared to different socialization goals as adaptations to different environments (Keller lens).

The prototypes are not uniform of course, they show also variations within the models, but it is mainly variability in terms of autonomy or variability in terms of relatedness. In some respects, the prototypes may represent mutually exclusive views on particular psychological phenomena. The symbiotic mother-infant relationship, e.g., the cultural norm in the interdependent model, may constitute a serious incidence of psychopathology in the independent model. However, this is not equivalent with a unidimensional scale with two endpoints – both express conceptions of relatedness, yet with different perspectives on the self.

Part of the overall conception that I presented in my book is the variability that is constituted by the multiple combinations of autonomy and relatedness, beyond the prototypes, to which I devote a whole chapter [chapter 6, pp. 161–217]. I started with Kagitcibasi’s [2007] framework that there is a third prototype, which I come to question on the basis of our data. The autonomous related model is not a prototype, since there is much more variability in meaning possible due to the quantitative and qualitative combinations of autonomy and relatedness. Here again, the different lenses become obvious. I do not understand these variations of autonomy and relatedness as ‘misfits’ [Harkness, p. 416] but as a fascinating scenario for the study of cultural, historical, and generational change as well as consequences of culture contact. Therefore a larger number of samples is presented in the book with respect to these combinations than with respect to the prototypes of independence and interdependence – representing different cultural environments as well as historical epochs. Migration is another domain of study for these questions, where we recently started empirical research.

I think the basic differences in the lenses start with the definition of culture: Sara Harkness talked in her essay review about the cultures of India and those of Sub-Saharan Africa without further specifying the defining criteria except the geographical region. In my model, culture is the representation of the living environment in terms of sociodemographic and social characteristics, as expressed in shared ideas and shared practices [Greenfield & Keller, 2004; Keller, 2003]. This offers the possibility of defining cultural environments within countries, societies, and regions. In our research program we have the chance to compare families with the same language, religion, and cultural history, like rural and urban Nso or rural and urban Gujarati Indians that differ with
respect to formal education and the related economic condition. Consequently, the fertility patterns change and the socialization strategies of these cultural samples clearly differ.

In general our research strategy starts with identifying sociodemographic contexts, which are ethnographically detailed. We ‘assume’ that these contexts are associated with particular cultural models as emphases put on autonomy, relatedness, and combinations of the two dimensions. These assumptions are tested with qualitative and quantitative methodology. The shared ideas are analyzed with respect to socialization goals and parenting ethnotheories. The behavioral contexts are described and the interactional behaviors are analyzed. All these domains are assessed with different methodologies in close cooperation with local collaborators. Not only were answers found in our research programs, but also many questions arose, e.g., with respect to the development of mirror self-recognition. This has led to a new research project with a combined cross-sectional longitudinal design, in order to better understand the effects of familiarity in urban and rural Indian, urban and rural Cameroonian, and German (only urban) children [Kärtner, Keller, & Yovsi, 2008; Kärtner, Keller, Yovis, & Chaudhary, 2008].

In any case it seems to be necessary that people with different lenses need to communicate with each other more intensively in order not to accumulate misunderstandings but to jointly contribute to the exciting field of cultural/cross-cultural variations of general human themes.

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