

COMMENT

Embracing Complexity: Rethinking the Relation Between Play and Learning: Comment on Lillard et al. (2013)

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Lillard et al. (2013) concluded that pretend play is not causally related to child outcomes and charged that the field is subject to a *play ethos*, whereby research is tainted by a bias to find positive effects of play on child development. In this commentary, we embrace their call for a more solidly scientific approach to questions in this important area of study while offering 2 critiques of their analysis. First, we urge researchers to take a more holistic approach to the body of evidence on play and learning, rather than relying on piecemeal criticisms of individual studies, since positive effects of play on learning emerge despite the use of a variety of methods, contents, and experimental conditions. Second, we consider how best to study this topic in the future and propose moving away from traditional empirical approaches to more complicated statistical models and methods that will allow us to embrace the full variety and complexity of playful learning.

Keywords: pretend play, learning, cognitive development, social development

Imagination is everything. It is the preview of life's coming attractions.

—Albert Einstein

A person might be able to play without being creative, but he sure can't be creative without playing.

—Hanks and Parry, *Wake Up Your Creative Genius*

Play is ubiquitous across the animal kingdom. Children play, monkeys play, goats play, and even rats play. Play has been linked to positive social behaviors in animals (Pellis & Pellis, 2009) and is related to distinct biological markers in brain development (Byers & Walker, 1995; Panksepp, 2007). Indeed, the absence of play in childhood has been used as a diagnostic criterion for autism (American Psychiatric Association, 2000).

Why is play so common? What is its role in development? The past 20 years of developmental research has seen renewed interest in these questions. Following theoretical leads from Lev Vygotsky (1978) and Jean Piaget (1962), many now argue that play is important not only for encouraging the development of socially relevant skills like self-regulation and empathy but also for assisting in both academic and social learning (see Hirsh-Pasek, Golinkoff, Berk, & Singer, 2009, for review).

In their target article, Lillard et al. (2013) offered a critical eye on this body of research. They focused tightly on one area within

the play literature—pretend play—to ask whether and how this particular form of play is related to child outcomes in various areas of learning, including literacy, problem solving, and intelligence. Their review went beyond a mere recap of available findings, digging deeply into the methods sections of the literature to offer a detailed critique of the field's collected wisdom. The authors used this literature to evaluate three competing hypotheses: (a) pretend play causes developmental outcomes, (b) pretend play is a sufficient but not necessary variable in these outcomes (equipollency), and (c) pretend play is an epiphenomenon that goes along with but has no direct relation to child outcomes.

On the basis of their review, Lillard et al. (2013) concluded that pretend play is not causally related to child outcomes and that any appearance of a relation is merely an epiphenomenon. Further, and perhaps more important, they warned against overinterpreting the findings in this body of data because many of the studies purporting to demonstrate a strong association between play and child outcomes are flawed. For example, some studies used nonmasked experimenters in ways that did not allow researchers to avoid experimenter bias. Others relied on correlational data without subjecting their studies to randomized, controlled designs. And many were conducted in ways that confuse whether play itself was causally responsible for the observed effect or whether the effect was due to a confounded variable such as an increased amount of adult attention.

On the basis of these problems and others, Lillard et al. (2013) charged that this nascent field has fallen victim to a *play ethos* (Smith, 1988), whereby experimenter bias implicitly distorts not only the main conclusions of this field but the very foundation on which these conclusions are based. The play ethos occurs because researchers in this area tend to believe strongly, a priori, in the

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value of play. Hence the research they conduct is tainted by a bias to find positive effects of play on child development, even when such effects may not exist. Researchers in the grip of the play ethos may overinterpret correlational data as causal without critically assessing the evidence in a more rigorous and empirical way. One of the major contributions of the target article is to expose these biases and to call for a more solidly scientific approach to questions in this important area of study.

But if the field suffers from a play ethos, as Lillard et al. (2013) would have us believe, their examination of the flaws in the literature has swung the pendulum too far in the opposite direction. Because individual studies have methodological problems, they concluded that there is no compelling evidence for a meaningful relation between pretend play and positive developmental outcomes. They distorted the lack of strong evidence into an argument for there being no evidence for playful learning—a conclusion that we believe to be unwarranted at this point in the history of the field.

This comment aims to move the pendulum back toward the middle, acknowledging the presence of flaws in the research while at the same time arguing for the need to take seriously the positive contributions that pretend play, and play in general, might make in development. Our comments fall into two main categories: a plea to look at the body of the evidence taken as a whole and a call to reframe our research questions to allow us to explain rather than dismiss the variability in this literature. We conclude with some positive suggestions for a way forward for this field of research.

The Sum of Its Parts

To date, many of the studies claiming to reveal a relation between play and learning have left open the possibility that the reported relations between play and developmental outcomes are a matter of experimental sloppiness. Throughout their review, Lillard et al. (2013) highlighted cases in which this relation disappeared as soon as other factors were controlled. For example, several studies reported to have found that training children in play increased their creativity relative to that of children who engaged in other activities like music (e.g., Dansky, 1980; Feitelson & Ross, 1973). But when amount of adult contact was controlled, as in Christie (1983), all groups showed increases in creativity and the performance of the children in the play group did not improve more than that of children in the other groups (see also Moore & Russ, 2008).

Such cases provide examples of the kinds of biases and fallacious reasoning that exist within the literature—the play ethos—and for such cases we believe that the authors' critiques are justified. As Lillard et al. (2013) repeatedly argued, it is essential to replicate these results with unbiased testers and appropriate controls. Here, they have identified a bias that exists throughout the field and that could lead to erroneous conclusions across wide range of studies.

However, many of Lillard et al.'s (2013) critiques have a very different flavor. Throughout the article, the authors denounced or downgraded a host of positive findings for having possible alternative explanations. For example, positive effects of play on executive function from the Tools of the Mind program (Diamond & Lee, 2011) were criticized on the grounds that children had to make a plan for their play in advance. A study that found positive

effects of play on narrative development (Baumer, Ferholt, & Lecusay, 2005) was discounted even though it used blinded experimenters because the training was administered by “one full-time classroom teacher per condition—the teachers might have differed in narrative skill teaching effectiveness outside the intervention” (p. 21). The many studies finding positive effects of play on language learning were criticized because language is a later-developing skill, due to “later-maturing vocal control” (p. 18). Lillard et al. (2013) used the existence of such problems to argue that because we cannot trust the results of a variety of individual studies for a variety of different reasons, we should conclude that play is not causally related to learning outcomes.

We absolutely agree that alternative explanations exist for every published study, and it is through considering these kinds of alternative explanations that we are able to design further experiments and make scientific progress. But the trouble with this approach is that the criticisms of Lillard et al. (2013) differ depending on which study is currently under the microscope. Their critique of the literature takes this litany of disparate flaws as reason enough to reject the possibility of a causal relation between play and developmental outcomes.

We draw a different conclusion from these same data. Specifically, we would urge Lillard et al. (2013) and their readers to engage in a more holistic approach to the body of findings that emerge despite changes in task content and context. It is undeniable that every study will have its flaws or alternative explanations. But when each study has a different flaw or alternative explanation, as in the cases under discussion, the critique leveled at the entire field becomes less compelling. Unlike in cases of broad bias, the nonoverlapping concerns leveled against a large number of studies suggest no overarching bias in interpretation. Thus, rather than emphasizing the inconclusiveness of these data, the similar patterns of outcomes that survive this variability might teach us something profound.

Put simply, we agree with Lillard et al.'s (2013) observation that there are different problems across different studies done in different contexts with different experimental conditions. But this fact strongly suggests that the most fitting explanation of the data writ large is that play, or something strongly linked to play, is related to child outcomes. This conclusion takes a Bayesian perspective on the body of available evidence: We can draw positive conclusions from a variety of observations, even after discounting the flaws that might be evident in each.

The literature on parenting provides an example of the success of this kind of approach in the developmental literature. We cannot randomly assign children to have a certain kind of parent, so any study in this area falls prey to criticism for not adopting the gold standard of lab-based research. In addition, biological parents share, on average, half of their genes with any child, leaving open the possibility that any effects of parenting style are an artifact of inheritance. But the fact that research on parenting is plagued by complexity does not necessarily mean that parenting style makes no difference to child outcomes, or that we can draw no conclusions about the developmental effects of having different kinds of parents (see Collins, Maccoby, Steinberg, Hetherington, & Bornstein, 2000; Darling & Steinberg, 1993). Rather, our knowledge about the impact of parenting on child outcomes relies on a survey of the collective body of evidence and not necessarily on the security of the data from any individual study.

Similarly, to gain a full picture of the relations between play and learning, we must consider the body of evidence as a whole, not necessarily as the sum of its parts. Determining what effects play may have on learning should be seen as an inclusive exercise that explores the phenomenon of playful learning across multiple contexts and multiple contents. If across these diverse platforms we find associations between play and child outcomes that persist despite this variability, then the simplest and most parsimonious explanation might be that play indeed has some pivotal role in promoting healthy development.

Reframing the Question

The gold standard against which we evaluate the quality of studies is the double-blind, random-assignment method that does everything possible to control for any possible sources of variance other than the one(s) under investigation. There is much to admire about this approach; it is only through deliberate control of all of the independent variables that we can safely infer causation rather than correlation. This is the lens through which Lillard et al. (2013) viewed the studies that they reviewed, and it is through this approach that they are able to issue their powerful call for more research in this area to address the flaws in previous studies.

We agree with this call. However, we would add that what we need is not simply more and better studies, but different ones. We need to move away from the kinds of methods that have traditionally been used to study pretend play and to fundamentally change how we approach this topic. Why? The simple answer to this question is that play is complex, and complex constructs demand complex research designs and analyses. Working within the framework of the double-blind, random-assignment, controlled experiment in the case of play thus has its own flaws and may not allow us to adequately explicate the relations between play and development.

As a parallel, consider the literature on the associations between childcare and development. Researchers in this area began by hotly debating whether and how the quality of childcare was causally related to developmental outcomes. But there are many possible measures of quality, which are correlated but nonoverlapping, making it difficult to isolate which factor or factors are necessary for a variety of outcomes. Looking only at any single input measure might not reveal a statistically significant relation, even though, in the aggregate, childcare quality has a strong influence on child outcomes (see NICHD Early Child Care Research Network, 2003, 2005). In response to this issue, researchers in this area began to shift their research questions from asking about simple causality to examining multiple paths to outcomes with an eye to development over time and in context. Influenced by the work of Urie Bronfenbrenner (1979), scientists started to ask how they could explain and work with variability rather than merely control for it. The result was that predictor variables replaced independent variables, and *t* tests and analyses of variance gave way to structural equation modeling, multiple regressions, and growth curve models. Researchers can now ask which factors feed into the latent variable of quality care and can evaluate how each of the components alone and in concert carve paths for various learning outcomes over time.

We suggest that play shares this structure, in which many variables come together in complicated ways to influence child

outcomes. Thus, traditional approaches to empirical research, which force us to frame our research questions in narrow terms, will be inadequate to understanding the full scope of play's role in development. These approaches make (at least) two mistaken assumptions. First, they assume that we can clearly define the construct of pretend play. But the issue of defining play is one that has plagued researchers for many years. Play is a family-resemblance construct that includes object play, physical play, social play, and pretend play, among others. Even if we focus on one of these types of play, as Lillard et al. (2013) have done, definitions are still elusive. Pretend play is defined not by necessary and sufficient conditions but by a set of diverse criteria like flexibility, positive affect, intrinsic motivation, nonliteral action, and non-goal-directed behavior (Krasnor & Pepler, 1980; Sutton-Smith & Kelly-Byrne, 1984). No single ingredient surfaces as crucial for picking out pretend play or play in general. Treating pretend play as a unitary construct, and expecting strong degrees of consistency across a variety of study designs and outcome measures, may thus represent a flawed approach.

The second assumption of traditional approaches to empirical research is that it is possible to control all extraneous variables in order to focus only on the contributions of the construct of interest. Here, the problems are even more troubling. Even if we could define play accurately and concisely, attempting to view this construct in isolation runs the risk of overcontrolling, making artificial, or even removing the very factors we are hoping to study. Consider that play, pretend or otherwise, necessarily recruits a number of other psychological mechanisms like attention, joy, and active focus (see Chi, 2009; Fisher, Hirsh-Pasek, Golinkoff, Singer, & Berk, 2011). It is likely that these other psychological mechanisms are doing some of the work of keeping children engaged in the task at hand, hence supporting increased learning outcomes. But it is also certain that these other psychological mechanisms are an integral part of what it means for a given situation to be playful. Attempting to control them or equalize them across conditions, as a traditional approach insists that we must, would mean creating an artificial situation that no longer reflects the construct in question. Trying to study the impact of play alone may mean that we can no longer study the impact of play.

In the case of play, then, we can neither tightly define nor control our constructs, as traditional empirical approaches demand. Indeed, these two issues are endemic to the type of construct that play is and are therefore present no matter how our questions are framed. The implication is that we need to find ways to embrace the complexity, as it may be impossible to tame this construct to fit into our traditional notions of narrowly defined variables. If we are to make real progress in this foggy area, we will need to reframe our questions, moving away from "Does pretend play *cause* child outcomes?" toward "How much of the variance in child outcomes is attributable to play, above and beyond other factors?"

What is the best way to take up this challenge as we move forward in this field? In the final pages of their review, Lillard et al. (2013) made a number of suggestions along these lines, which we expand on briefly. The scientific study of play demands methods and models that are more sophisticated than those currently in use, so that we can untangle the many components of the play scenario that could contribute to optimal development. As we work toward this goal, some of the issues of definitions and covariates

that we raised above will become clearer. For instance, we suspect that many of the results demonstrating an added value for play will emerge only in those contexts where children are active, engaged, and learning something that is meaningful to them. These variables, which are necessary but not sufficient ingredients for play, may be doing the work of improving learning in play situations. As a result, other environments that recruit these mechanisms might also encourage learning (Weisberg, Hirsh-Pasek, & Golinkoff, 2012). The new approach to studying play should prepare us to embrace rather than to reject such complexity.

If Lillard et al. (2013) and we are right that more complex models can help us to discern which mechanisms link play and learning, we will still be left with the key question considered in the target article: Is play merely an epiphenomenon, or is play really related to learning? That is, does play just increase joy and attention to task in ways that foster better social and academic skills, such that other activities could do so just as well, or is play somehow uniquely well-suited to creating successful learning environments? This question is a serious one that deserves careful thought. We would argue that, if play turns out to support just the kinds of behaviors that promote good learning, then we should conclude that play is related to learning, regardless of which individual ingredients of play are doing the work.

Conclusion

Play is ubiquitous among our species and many others. This fact alone does not guarantee that play will serve any particular developmental purpose, yet the question of why animals and humans play deserves our most serious consideration. In their review, Lillard et al. (2013) offered us a superb analysis of the available literature and a strong call for better research on links between play and learning in the future. Their meticulous review of the research on pretend play and learning outcomes illustrates the myriad methodological problems that haunt this literature, including the *play ethos*. Lillard et al. did a great service to the field to reveal these shortcomings and to call for future research that will address them and move the discussion forward.

We are sympathetic to Lillard et al.'s (2013) claim that we need better research in this important domain of inquiry. As we heed their advice, though, we must be careful not to throw the proverbial baby out with the bathwater. In their approach to this literature, Lillard et al. are in some danger of dismissing a large body of research that might well provide us with a window into the role that play might exert on development. As we argued above, the field of play research when considered in the aggregate suggests that there are some links between play and learning, regardless of the flaws present in any individual study. These links should be better studied in order to be better understood.

In order to do so, however, we must move beyond simple questions like "Does play cause learning?" to embrace more complex questions and more sophisticated statistical analyses. We need to rethink how we want to operationalize and measure play in order to make progress in this area, which is rife with complexity and ambiguity. Researchers in this area might also consider reformulating the ways in which they think about different categories of play. For instance, there might be a middle ground between free play and direct instruction called *guided play*, which is child directed but driven toward a learning goal (Fisher et al., 2011;

Hirsh-Pasek et al., 2009; Klahr, Zimmerman, & Jirout, 2011). Although it might not fit squarely into whatever definition of play is currently in use, it is possible that guided play is more tightly related to outcomes than any type of free play.

In general, we suspect that play of a variety of types does have a real role in learning and that better research spotlighting the interactive forces that emerge in play will offer some resolution to current debates on these issues. We also suspect that play is not a unique contributor to learning. Rather, play is a sufficient condition for learning that unites some of the most powerful social, academic, and emotional tools that humans bring to bear on the problems that they encounter (see Buchsbaum, Bridgers, Weisberg, & Gopnik, 2012; Seligman, Railton, Baumeister, & Sripada, 2012). Without the flowering of play and imagination in childhood, humans might be seriously disadvantaged in areas that rely on creative potential, unable to move beyond "what is" to consider "what might be."

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