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COMMENT

Where's the Beef? A Commentary on Ferguson and Donnellan

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To make a scientific contribution, a reanalysis must be firmly rooted in the identification of a clearly superior methodological innovation over the original research. By contrast, a reanalysis rooted in dissatisfaction with previous results will necessarily be biased and can only obscure scientific discoveries. The reanalysis published by Ferguson and Donnellan began with a highly selective literature review, failed to justify its methodological choices, and interpreted its results in an incautious way. Notwithstanding these problems, the Ferguson and Donnellan article leaves intact the core contributions of the original article: that there is no evidence of a positive effect of infant viewing on subsequent development and that it is well worth conducting more research on the effects—including potentially adverse effects—of infant TV, DVD, and video viewing.

Keywords: mass media, television, language development, child development, reanalysis

The garden of science has many useful species: theoretical modeling, descriptive studies, tests of hypotheses using observational data, natural experiments, randomized trials, meta-analyses, and so on. To this richly productive ecosystem, Ferguson and Donnellan proposed to contribute a reanalysis. While a well-conducted reanalysis motivated by clear methodological improvements can enhance scientific understanding of an issue, the reanalysis offered by Ferguson and Donnellan fell well short of the ideal in its introduction, its methodological choices, and its interpretation. The result was a reanalysis that produced more heat than light.

Motivation

The article by Christopher Ferguson and Brent Donnellan proposed a reanalysis of an article published in 2007 (Zimmerman, Christakis, & Meltzoff, 2007a) that presented results of a survey of parents of children ages 2–24 months. One motivation for the reanalysis was concern over possible publication bias, with the implication that the original article might not have been published had the results been different. How credible is this claim?

The original article noted that concrete claims had repeatedly been made on behalf of the effectiveness of baby videos in promoting child development, and yet no research had at that point been conducted that would support those claims. The purpose of the original article was to contribute some correlational evidence about the face value of those marketing claims. This article became notable because it addressed an issue not previously studied. As

such, the results would have been published whether they had shown a positive association or a negative association. Ferguson and Donnellan went on to cite an article claiming that “almost all articles . . . in the behavioral sciences publish results which confirm the a priori hypotheses of the authors” (p. XXX). However, the original study that they reanalyzed had no a priori hypothesis. Thus, as a descriptive study in an understudied area, the article was not a good candidate for charges of publication bias. The reasons for this reanalysis, thus, are murky, and the reader would have been better served by a clear-cut rationale for conducting the reanalysis.

Ferguson and Donnellan appeared to be more concerned with the negative findings. In their introduction, they claimed that “other research fails to support a clear negative association between media exposure and cognitive outcomes” (p. XXX) and cited three articles, all of which, indeed, failed to find a clear negative association. Yet these three articles in turn cited other research studies, not cited by Ferguson and Donnellan, that did show negative associations. These articles included three experimental studies finding a negative association of TV viewing and language development (Carew & Clarke-Stewart, 1980; Gottfried, 1984; Nelson, 1973), a case-control study that found language delays with early viewing (Chonchaiya & Pruksananonda, 2008), and a longitudinal analysis that tested the effects of different content on cognitive development, finding negative effects for violent content and no effects for educational content (Zimmerman & Christakis, 2007).

Much has changed since the original article was published. More is now known about causal pathways and why a negative association between television and language development might occur. Language development depends crucially on parent–child interaction (Zimmerman et al., 2009) and even on play (Christakis, Zimmerman, & Garrison, 2007), both of which television seems to displace (Christakis et al., 2009; Schmidt, Pempek, Kirkorian, Lund, & Anderson, 2008). In addition, other work in this area has

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been done. In a randomized experimental study in which DVDs were distributed to parents of children between the ages of 12 and 25 months, researchers found no main effect of the DVD distribution but did find that language scores were significantly lower for children whose viewing of *Baby Einstein* began at earlier ages (Richert, Robb, Fender, & Wartella, 2010). Many other studies have been published in this area. The point here is not to provide a detailed literature review but only to point out that many important articles and insights were not included in Ferguson and Donnellan's highly selective review of the literature. Thus, although a brief literature review may be appropriate for a reanalysis, a literature review that presents the literature only on one side of a debate is not.

Methodology

In their reanalysis, Ferguson and Donnellan made a number of methodological choices, which were defended unevenly. There were differences in the reanalysis from the original analysis at both the conceptual and analytical level, but did these rise to the level of a significant and undeniable improvement over the original, or were they simply replacing one reasonable set of investigator choices with a different, and perhaps less reasonable, set?

AQ: 3 At the conceptual level, Ferguson and Donnellan placed more faith than did the original article in unadjusted bivariate analyses claiming they suggested *benefits* of viewing (p. XXX). This was clearly a major difference, but not for the better. In the presence of known confounders of the relationship between video viewing and language development—age, parental income, and gender, to name just three—a bivariate analysis tells us nothing. It is an example of why multivariate regression was invented (Morgan & Winship, 2007). The importance that Ferguson and Donnellan attached to bivariate analyses, mentioned several times, subtracted from the value of the reanalysis.

AQ: 4 The other changes involved the analytical methodology, including the choice of potential confounding variables, the use of normed versus raw language scores, and the decision to use the square root of television exposure. None of these choices was adequately justified. For example, the use of raw language scores was justified in part (p. XXX) by a citation to the article in which the norms were estimated (Fenson et al., 2000). Other citations were to textbooks (p. XXX), with no explanation of the underlying reason for the purported bias. Meanwhile, the use of percentiles has been explicitly defended in the literature (Fenson et al., 2000). To be sure, this work acknowledged a limitation to the effect that the norms skew away from the lower socioeconomic spectrum. Yet the data in the Zimmerman, Christakis, and Meltzoff article (2007a) included relatively few participants at the low end of the socioeconomic spectrum. Accordingly, the concern about the percentile language norms—however valid or invalid it may be on the face of it—was less an issue here than it might have been otherwise.

On the other hand, there is, in fact, a compelling reason to use age-normed scores. Age is highly associated with both television viewing at young ages and with language development. It is accordingly very important that age be properly controlled in any analysis of television viewing and language. But the relationship of age and each of these variables is nonlinear, so that simply including age (e.g., in months) would capture only the linear association,

while completely missing the nonlinear parts of the relationship. Including age as a covariate in an analysis with the raw language scores as the dependent variable would accordingly result in substantial residual confounding by age. There are two ways of dealing with this problem. One is to use a sophisticated transformation of the age variable, such as a third-order polynomial. The other is to use age-normalized language scores. The second approach was adopted in the original article.

Suppose that one did not adequately control for age—what would be the result? Age is positively correlated with language development and positively correlated with video viewing, so failing to adequately control for age would introduce a positive bias into the estimated association of language development and video viewing. If the true effect of video viewing on language development were negative, then that bias would make the measured effect smaller, zero, or even positive, depending on its strength. The Ferguson and Donnellan effects were exactly what one would expect from the inadequate control of the confounding effects of age, and exactly what one would expect from replacing a nonlinear control for age with a linear effect only.

Another novel choice made by Ferguson and Donnellan was to use the square root of video viewing instead of video viewing itself (p. XXX). The rationale was that the data in their raw form are skewed, yet as even Ferguson and Donnellan acknowledged, linear regression can handle independent variables with a skewed distribution (p. XXX). Accordingly, the fact that television viewing is not normally distributed in the real world does not in any way justify a transformation of the viewing variable. The two highest values imply video viewing of approximately 4 hr a day. Ferguson and Donnellan questioned these high values (p. XXX); yet other work on television viewing in children has suggested that there is nothing surprising about data in which the 99th percentile of TV viewing is 4 hr a day (Rideout, Vandewater, & Wartella, 2003). But if the problem is benign, the cure is not. Ferguson and Donnellan's square-root transformation—or any transformation—replaced observed data points with different data points and, in this sense, introduced measurement error into a key variable. Because measurement error is known to bias results toward the null, any transformation should be done only after careful consideration of the consequences. Such consideration did not occur here. One article was cited in which a square-root transformation of viewing hours was used (p. XXX), but not cited were the hundreds of articles in which the untransformed report of viewing hours was used.

The point here is not (primarily) to justify the original methods as necessarily superior to those of Ferguson and Donnellan. Rather the point is that Ferguson and Donnellan have failed to make the case that a reanalysis was warranted on the basis of methodology alone.

For some of their methodological choices, they made an unpersuasive case; for others, they made no case at all; and for those choices with an obvious bias in the direction of their outcome, they made no defense.

Can the Ferguson and Donnellan results really be taken as evidence of methodological-flexibility bias? The original study reported a negative association between video viewing and percentiles of language development. That observation has been made, and nothing about this reanalysis called it into question. Instead, a study was conducted that failed to observe any consistent association between the square root of video viewing and raw language scores. What can we conclude from this finding?

Interpretation

The attack is not only scientifically unmotivated, it is also aiming at the wrong target. Ferguson and Donnellan took issue with the University of Washington press release (p. XXX)—which was not part of the original science—and seem to be annoyed by the press coverage of the article—which was even further from the original science. Ferguson and Donnellan stated, in the first line of their Discussion, that “Zimmerman et al. (2007a) concluded that there was evidence of a ‘large negative association’ between exposure to baby media and vocabulary acquisition” (p. XXX). That statement is misleading. The conclusion of the original study—presented in the abstract—was that more research was required to understand its results, and the final paragraph of the study explicitly called for a randomized, controlled trial to get to the bottom of the effects of baby DVD viewing on language development. The Discussion section of the original article carefully and appropriately discussed not only the limitations of the research but also the multiple possible causal mechanisms that might have produced it, some of which did not imply that baby DVD viewing in fact is bad for language development.

Thus, it could be argued that the implications of the Ferguson and Donnellan reanalysis were not that different from those of the original article, namely, that these data provided no proof of the marketing claims made on behalf of baby videos. The original study was careful to avoid making claims that infant viewing is necessarily harmful. In the same spirit, the Ferguson and Donnellan article can in no way be interpreted as suggesting that infant viewing is necessarily innocuous. Neither article provided a definitive answer to the real question: Will watching baby videos help or hurt my child’s development? This question continues to be urgent. Not only do young children watch a lot of television and videos, but they often do so because of their parents’ belief that it will help their development (Rideout et al., 2003; Zimmerman, Christakis, & Meltzoff, 2007b).

In this context, it is a relief that now at least two peer-reviewed experiments, not cited by Ferguson and Donnellan, have begun to answer that question (Richert et al., 2010; Robb, Richert, & Wartella, 2009). In fact, a scientific consensus is beginning to emerge around the effects of infant video viewing, a consensus with two parts. First, while it might be theoretically possible for infant viewing of a video to produce some kind of learning (Barr, Muentener, Garcia, Fujimoto, & Chavez, 2007), there is no evidence to date of any beneficial effects on child development of infant viewing of TV, videos, or DVDs. Second, language and other forms of development depend crucially on interactions, and not just stimulus. To the extent that viewing of DVDs crowds out interactions that infants would otherwise be having with their siblings, adult caregivers, or environment—at least at high levels—this viewing may slow development.

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