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Challenging the Link Between Early Childhood Television Exposure and Later Attention Problems: A Multiverse Analysis

Matthew T. McBee1

Rebecca J. Brand2

Wallace Dixon1

1Department of Psychology, East Tennessee State University, Johnson City, TN.

2Department of Psychology, Villanova University, Villanova, PA 19085

**Abstract**

The claim that early childhood television exposure causes later attention problems (Christakis, Zimmerman, DiGiuseppe, & McCarty, 2004) seems to remain strongly held by the popular media as well as by researchers in the field. Using the same NLSY-79 dataset (*n* = 2,108), we conducted a multiverse analysis to examine the degree to which the finding reported by Christakis et al. was robust to analytic choices. We evaluated 888 analytic models, including linear regression, logistic regression, and two forms of propensity score analysis. Only **166** models (**18.7**%) yielded a statistically significant relationship between early TV exposure and later attention problems, with most models estimating a trivially small effect size. We conclude that the evidence for the harmful effect of early TV use is weak and inconsistent. All data and code necessary to reproduce our analysis is available online via github (<https://github.com/mcbeem/TVAttention>) and as a Docker container (<https://hub.docker.com/repository/docker/mmcbee/rstudio_tvattention>). Preprint [link].

**Keywords**: media, TV, ADHD, attention development, multiverse analysis, specification curve, computational reproducibility, garden of forking paths

Challenging the Link Between Early Childhood Television Exposure and Later Attention Problems: A Multiverse Analysis

Psychological science is capable of having a broad and deep impact on human lives. In developmental psychology in particular, there is a sense of relevance, indeed urgency, to many of its questions: Is it helpful or harmful to grow up multilingual? Do vaccines cause autism? Does screen time cause attention deficits? Research in this field has the potential to reveal which behaviors, products, and choices are harmful to development and which give children the best chance to grow up happy and healthy. The stakes are high; it is crucial that scientists get it right.

Once an erroneous finding has been disseminated via the media, it is nearly impossible to correct the public understanding of the issue. Take, for example, the supposed link between vaccines and autism. Even 20 years after Andrew Wakefield’s fraudulent 1998 report (Wakefield et al., retracted) implied a link between the MMR vaccine and autism, and despite numerous findings that such a link does not exist (Committee to Review Adverse Effects of Vaccines, 2012), a substantial proportion of the public still believes that it does (Oliver & Wood, 2014). Cases like this illustrate how important it is that researchers make their best attempt to disseminate accurate findings, and how crucial it is that we engage in and disseminate replication attempts, especially for high-impact findings. We follow Nature Editors (2016) and others in arguing that doing so is a duty to our profession.

Over a decade ago, Christakis and colleagues (Christakis, Zimmerman, DiGiuseppe, & McCarty, 2004) published a paper claiming a relationship between television exposure in toddlerhood and subsequent attention problems at school age. Although longitudinal in nature and including a variety of control variables, the lack of randomized manipulation of TV use made it inappropriate to draw strong causal conclusions from these data. In spite of the careful language used in the paper, subsequent presentations by the lead author discussing this work blur the lines between associations and definitive causal links. For example, in his TEDxRainier talk, **Christakis (2011)** said:

And we, we tested this some years ago, and what we found was that for the more television children watched before age three, the more likely they were to actually have attentional problems at school age. Specifically, for each hour that they watched before the age of three, their chances of having attentional problems was increased by about ten percent. So a child who watched two hours of TV a day before age three would be twenty percent more likely to have attention problems compared to a child who watched none (7:21 to 7:46).

This statement does not admit any epistemic (or statistical) uncertainty about the nature of this estimate, nor of any limitations to the evidence supporting the claim. It is presented as a scientific fact.

Unsurprisingly, popular media was much less careful in raising alarm about the potential harm of television exposure on children. Using Google search in April 2018 for “Does TV cause attention problems,” the first six hits all claim a link between TV and attention problems. One hit (WebMD) uses blatantly causal language in its headline (“Toddler TV Time Can Cause Attention Problems”) and another published by whitedot.org (Lotus, 2018) quotes Christakis as saying “TV ‘rewires’ an infant’s brain,” and says his study shows that “TV watching is a cause [of ADHD].” Although findings described on these pages are wildly overstated and out of date, we can find no retractions or contemporary updates associated with them.

Christakis’s story was attractive to the public for many reasons. Since the late 1990s, there has been a growing public interest in how early experiences impact later development (e.g., Nash, 1997). But as far back as the 1950s, new media technologies have been greeted with fear and skepticism (Goode & Ben-Yehuda, 2010; Markey & Ferguson, 2017), or sometimes even a moral panic about the harm they are supposedly wreaking on children (e.g., **Twenge, 2017**). Further, there is an intuitive face validity to the claim that TV harms children’s attention. Infants’ attention is undergoing rapid development (Ruff & Rothbart, 1996), and the stimulating pace of screen media rarely resembles the slower pace of real life. Recommendations from the American Academy of Pediatrics that children avoid television viewing were based on the notion that TV was inherently harmful for young children (AAP, 1999; Children and Media, 2018); they also reinforced it by lending credibility and weight to the claim.

All in all, parents of children born in the last 20 years had ample reason to believe that TV-watching caused attention disorders. Yet recent research indicates that this may not be true, or at least is much more nuanced than initially thought. A re-analysis of the data set used by Christakis et al. (2004) indicated that the finding was not robust in the face of certain small changes in analytic parameters (Foster & Watkins, 2010). A subsequent meta-analysis on screen media use and attention problems indicated that their relationship is only weak to moderate, but was unable to clarify the direction of causality or the potential that this estimate is contaminated with confounding bias (Nikkelen, Valkenberg, Huizinga, & Bushman, 2014). A recent review came to a similar conclusion (Kostyrka-Allchorne, Cooper, & Simpson, 2017). As is often the case, however, the paper making a claim of a causal link (Christakis et al., 2004) has 1008 citations in Google Scholar at the time of this writing, while the more methodologically sound critique (Foster & Watkins, 2010) has 76 citations and the meta-analysis (Nikkelen et al., 2014) has only 54. Not only the general public, but substantial portions of the research community, is still left with the message that TV causes attention problems..

Our in this paper ; see also **Orben, Dienlin, & Przybylski, 2019**data analysis . While many of these decisions can be made in a principled manner in accordance with known best practices, some of them are arbitrary or, at the least, ambiguous (King & Zeng, 2007). For example, should a researcher employ an efficient but assumption-leveraged linear model, or would propensity score analysis be a safer bet? This decision is driven by the researcher’s sense of the risk and reward landscape at play, as well as the desired balance between competing but largely obscured priorities – a preferred position with respect to a bias / efficiency tradeoff when neither bias nor efficiency can be evaluated directly. As such, these decisions are often resolved by the researcher’s idiosyncratic preferences, knowledge and comfort of methods, or what the researcher perceives the audience to expect.

The set of all possible analytic choicesis known as the At the terminus of each decision tree lies a result.; rendering any particular conclusion suspect.results vary across analytic methodsIf most defensible models reach a similar positive conclusion about the presence and magnitude of some effect, then the claim about that effect becomes more credible. Conversely, if most models produce null effects, or if the results vary wildly across models, then the claim made by one particular positive analysis must be viewed as less credible.

In this paper, we present a multiverse analysis of Christakis et al.’s (2004) original claim, using the same NLSY79 dataset, prepared in the same manner as was documented in the 2004 paper. We then subjected this data set to a wide variety of analyses across three general types: propensity score analysis, linear regression, and logistic regression. The models varied across many dimensions which are discussed in detail later. The selection of covariates (adjustment variables) is an important issue, common to all analysis approaches, that we discuss in detail in the next section.

**Method**

**Data**

As in Christakis et al. (2004), data for the present investigation were obtained from the National Longitudinal Survey of Youth 1979 (NLSY-79), available via the NLS Investigator web interface (2018). Child data came from the NLSY79 Child and Young Adult dataset. Information on the mothers of these children came from the original NLSY79 dataset. These datasets were merged via a common ID code variable allowing mother and child data to be linked. We initially downloaded 340 variables from the Child and Young Adult dataset and 40 variables from the NLSY79 dataset(NLSY, 2018). Our project’s Github page (under “Documentation”) presents a spreadsheet mapping our analysis variables to the variable codes and labels from the NLSY dataset. Our raw and processed analysis datasets as well as our analysis code are disclosed on this site, allowing interested readers to replicate or extend our analysis.

Our variable selection process was based on the one reported in the original paper. As per Christakis et al. (2004), we selected three cohorts of children who were approximately 7 years old during the three “index years” of 1996, 1998, and 2000. Our baseline variable selections matched the original study to the extent possible given the brief description in the original paper, which did not report ID codes for the selected variables. In most cases, we could unambiguously identify variables by searching the NLSY data by question text or question title.

**Selection of cases.** We followed the original paper’s criteria for sample selection. For each index year (1996, 1998, and 2000), including those children whose ages at index were between 6 years 9 months and 8 years 9 months.­ Children with severe vision or hearing impairment, as well as those with severe emotional disturbances or orthopedic disabilities were excluded. A total of 2,108 cases were extracted that met these conditions.

**Variables.** As in the original study, our measure of attention was the standardized score on the hyperactivity subscale of the five-item Behavior Problems Index (BPI), which was standardized to an IQ-like metric (M = 100, SD = 15) within sex, as per the original study. However, we also retained the raw attention scores which were unadjusted for sex. The five items addressed children’s ability to concentrate and pay attention, as well as their confusion, impulsivity, obsessions, and restlessness or inability to sit still.

Television use was calculated as in the original study. Items measuring hours per day of television watched by the child on both weekdays and weekends days were converted to average hours of TV by multiplying weekday hours per day by five, adding to this weekend hours per day multiplied by two, and dividing by seven. We took this measurement from three and two waves prior to the index year, such that TV was measured at approximately age 1.5 and age 3, though the exact age of each child during these waves could vary to some extent.

It was necessary to correct some out-of-range values prior to analysis. We set any BMI value outside the range of 13-22 (based on CDC norms) to missing, and truncated the following variables to the top of their ranges: TV use in average hours per day exceeding 16 (following the original study), highest grade completed exceeding 24 (as this would imply more than eight years of post-graduate education), and annual income of $839,078 (the NLSY description of this variable includes a comment that this value is probably untrustworthy).

The file “variable name propagation spreadsheet.xlsx” on the project github page (under “Documentation”) provides a crosswalk from our substantive, conceptual variable names to NLSY alphanumeric variable names. The analysis code is the canonical description of how the variables were constructed and should resolve any vagueness or ambiguity in the preceding description.

**Selection of covariates.** The goal of each of our models was to estimate the causal effect of early TV on mid-childhood attention as accurately as possible. Since this data was collected via an observational longitudinal design, confounding is a near-certainty. Causal inference from observational data, in theory, possible if the proper set of covariates are incorporated into the analysis such that all confounding paths are blocked (Rohrer, 2018). To this end, our models employed two different sets of covariates. Most of these variables are based on survey questions that were repeatedly administered on a biennial basis. We were interested in these variables as potential confounders (common causes) of both early childhood TV exposure and mid-childhood attention deficits or hyperactivity. Thus, nearly all of these variables were selected from survey administrations contemporaneous with the TV exposure observation. Two exceptions were maternal self-esteem, which was asked only in 1987, and maternal depression (CES-D), which was assessed only in 1992. Depending on the cohort, depression could have been assessed up to four years before birth or the same year the child was born; and self-esteem from one to five years before birth. In spite of this problem of timing, we included these two variables because the original paper did. In any case, we expected a moderate degree of stability over time in these constructs (Lovibond, 1998; Trzesniewski, Donnellan, & Robins, 2003), which may ameliorate some concern about the timing of their measurement. We hope that including these covariates reduced confounding bias that would otherwise render the estimates uninterpretable, though we seriously doubt that we have completely eliminated it (**Westfall & Yarkoni, 2016**).

*Original covariates.* The first set of covariates was identical to those employed in the original study. They included the following: cohort (year in which the child’s attention was assessed: 1996, 1998, or 2000), the child’s age when attention was assessed (typically 93 months, but varied between 81 and 105 months), child’s race, child’s sex, the number of children of the mother living in the household, mother’s highest grade completed, the cognitive simulation and emotional support of the home (measured between ages 1 and 3), binary indicators of maternal alcohol use and cigarette smoking during pregnancy, a binary indicator of whether the child’s father lived in the household, maternal self-esteem as assessed by the Rosenberg Self-Esteem Scale in 1987, maternal depression as measured by the CES-D in 1992, child’s gestational age at birth (centered at term), and an urbanicity indicator variable in the form of the four levels of the Statistical Metropolitan Sampling Area classification. Where applicable, all of these were extracted from the first wave of data availability to avoid conditioning on post-treatment variables, since they could have potentially biased our estimates if they were mediators or colliders (in other words, endogenous; Montgomery, Nyhan, & Torres, 2018; Rohrer, 2018).

*Expanded covariates*. The expanded covariate list included all of the original covariate set with the following additions, which we suspected to be plausible confounders for TV use and childhood attention. We added family income, the partner or spouse’s highest level of educational attainment, an indicator variable for low birth weight (less than 2500 grams or 5 lbs 8 oz), and an indicator that the child suffers from a health condition that limited their school and play activities[[1]](#footnote-1). Rather than a continuous gestational age at birth variable, we created a binary indicator of pre-term delivery (child born before 37 weeks of gestation), as we suspected this would better capture the relevant information in this variable.

Finally, we created a variable assessing infants’ temperament. According to the NLSY website (NLSY Temperament, 2018), the temperament scale included items taken from work by Mary Rothbart, Joseph Campos, and Jerome Kagan. We chose the six available items that represented aspects of difficult temperament, as defined by Rothbart and Bates (2006), which included irritability, high-intensity affect, and negative mood. These items included assessments of how often the child cries when seeing a stranger, how often she is afraid of dogs or cats, how often she cries with doctors or nurses, how often the caregiver has trouble calming the child, and how often the child cries compared to others. Our temperament variable was the mean of these items, each of which was represented on a 5-point scale.

We view temperament as potentially a key confounding variable that could influence both early childhood TV use and mid-childhood attention deficits and doubted that a credible causal effect could be identified without controlling for it. Temperament includes the ability to regulate one’s own attention (Posner & Rothbart, 2018; Smith et al., 1997; Thomas, Chess, & Birch, 1968), and as one would predict, certain temperament dimensions predict children’s later attention problems (Auerbach et al., 2008; Gurevitz, Geva, Varon, & Leitner, 2014; Sullivan et al. 2015). In addition, those infants with difficult temperaments may be shown more screen media than other children, as a way to keep them calm and engaged (Brand, Dixon, & Hardesty, 2011). In support of this claim, parents’ perception of infants’ energy level (Nabi & Krcmar, 2016), poor self-regulation (Radesky et al., 2014), and fussiness (Thompson et al., 2013) all predict TV use. In short, we suspected temperament to be an important confounder that was not included in Christakis et al.’s analysis.

At the direction of reviewers, who expressed a concern that the behaviors incorporated into our temperament variable might simply be an earlier manifestation of attention deficits, we performed an exploratory factor analysis. A two-factor model with varimax rotation exhibited clean simple structure separating attention from temperament items, and in which the largest absolute standardized cross-loading was 0.133. The correlation between factors was *r* = -0.114. We therefore concluded that attention and temperament were highly distinct variables.

**Analytic approaches.** Our models considered two different outcomes (raw attention vs the within-sex standardized attention scores used in the original analysis), measured TV use at approximately 1.5 and three years of age, and incorporated the two different sets of covariates designated above. Additional features specific to each model are described in the next section.

**Propensity Score Analyses.** Given the nature of the data set, we believed that propensity score analysis was the most defensible choice for estimating the causal effect of TV watching on later attention problems. A propensity score is the probability of being in the treatment group (in this case, the group being shown a large amount of TV), conditional on a variety of baseline characteristics (such as mother’s education, household income, and child temperament; cf. Austin, 2011). Once the propensity scores have been estimated, they can be applied via a non-parametric technique such as matching, weighting, or stratification in order to produce a virtual sample that is balanced in expectation on all of the covariates that were included in the propensity score model. In this manner, propensity score analysis approximates a randomized experiment with respect to the measured covariates included in the analysis (Rosenbaum & Rubin, 1983). Unlike a true experiment, however, propensity score methods do not balance on unobserved or omitted covariates.

Within the propensity score family of analyses, there are still many decisions to be made in the garden of forking paths. One of the most important of these is how to incorporate the propensity scores into the inference. We used two such methods in our multiverse analysis: inverse probability of treatment weighting (IPTW) and stratification. Further, because propensity score analysis requires dichotomizing the predictor variable, we ran analyses using six different percentile cut points to define the high and low TV groups as follows:

* Below 20th percentile / Above 80th percentile
* Below 30th percentile / Above 70th percentile
* Below 40th percentile / Above 60th percentile
* Below 50th percentile / Above 50th percentile
* Below 60th percentile / Above 60th percentile
* Below 70th percentile / Above 70th percentile

In all the propensity score analyses, we used boosted classification trees (as implemented in the *twang* package (v. 1.5, Ridgeway, McCaffrey, Morral, Griffin, and Burgette, 2017) to estimate the propensity scores, using bagging and cross-validation to prevent overfitting. Missing data on covariates is handled automatically by the classification tree approach, in that the missingness is treated as informative and propensity scores can be estimated for cases with missing covariate values.

*Inverse probability of treatment weighting*. In this method, the propensity scores are used to construct IPTW weights which, when applied to the data, equalize the distribution of propensity scores between the treatment and control groups – and by implication, also equalize the distribution of all of the covariates that were included in the propensity score model (Guo & Fraser, 2015). We because the effect of TV exposure might plausibly be different for those who watched a lot of TV versus those who watched an average amountWe ran analyses both with and without the inclusion of sample weights to correct for the design-based oversampling of certain demographic groups. In some analyses, we identified the four covariates with the largest residual imbalance statistics and gave those covariates an additional regression adjustment. These conditions were fully crossed, with 6 (TV cutpoints) x 2 (outcomes) x 2 (TV ages) x 2 (covariate sets) x 2 (treatment effects) x 2 (sample weights) x 2 (doubly-robust) yielding 384 IPTW propensity score models. The *survey* package (v. 3.35-1; **Lumley, 2004, 2019**) was used to estimate the treatment effect after applying IPTW weights.

*Stratification*. An alternative inferential strategy using propensity scores is to stratify on them, calculate a treatment effect specific to each stratum, then combine those stratum-specific estimates to produce the average treatment effect (ATE, Guo & Fraser, 2015). These models were computed for five different numbers of strata (4, 5, 6, 7, or 8), which were fully crossed with 6 (TV cutpoints) x 2 (outcomes) x 2 (TV ages) x 2 (covariate sets) yielding 240 stratification propensity score models. Neither sample weights nor the doubly-robust approach could be implemented in the stratification models, nor could these models estimate the average treatment effect for the treated. We used the *PSAgraphics* package (v 2.1.1; Helmreich & Pruzek, 2009) to perform the stratified analysis and calculated *p*-values for the treatment effect estimates using the normal approximation.

**Linear Regression.** Linear regression models are less robust than propensity score models yet offer substantially enhanced efficiency (in the form of smaller standard errors) and increased statistical power. These models allowed us to detect and more precisely estimate weak effects that could have remained hidden in the noise of the propensity score models, albeit with more risk of exposure to systematic bias due to assumption violations. Linear regression models do not require the TV exposure variable to be dichotomized, but they do require researchers to specify an appropriate functional form for the relationship between TV exposure and attention. We fit regression models of polynomial order one (e.g. linear), two, and three. The ­*p*­-value for the effect of TV on attention in such models was calculated via a joint *F* test comparing a model containing the TV variables plus all covariates against a baseline model containing only covariates. The point estimate for the TV effect in the poly-2 and -3 models was computed by linearizing the slope (via computation of a partial derivative) at the median TV exposure. This estimate and its associated standard error were calculated using the *multcomp* package (v. 1.4-10, **Hothon, Bretz, & Westfall, 2008**) via the specification of custom contrast coefficients. We fit models both with and without sample weights, again using the *survey* package to perform the weighted analysis. Finally, we performed our analysis using both listwise deletion of cases with missing values and using multiple imputation (as implemented by the *mice* package, v. 3.6.0; van Buuren & Groothius-Oudshoorn, 2011). However, sample weights could not be combined with multiple imputation, so these conditions were not fully crossed. Using listwise deletion, we fit 2 (outcomes) x 2 (TV ages) x 2 (covariate sets) x 3 (polynomial functional forms) x 2 (sample weights) = 48 models. Using multiple imputation, we fit 2 (outcomes) x 2 (TV ages) x 2 (covariate sets) x 3 (polynomial functional forms) = 24 models. A total of 72 linear regression models were fit to the data.

**Logistic Regression.** Finally, as a close replication attempt of the original study, we analyzed the data set using logistic regression, in spite of our belief that this approach is unjustified given the continuous and apparently linear nature of the response variable. Christakis et al. (2004) divided the continuous attention/behavior problems scale into typical and problematic levels of attention deficit based on a *z =* 1.2 cut point (corresponding to a score on the within-sex standardized attention score of 120). The authors argued that using this cut point yielded a rate of problematic attention similar to its incidence in the population. But would their conclusions regarding TV’s effects have been the same if they had used a different cut point? We defined multiple dichotomous outcome variables by varying the standardized attention cutpoint from 110 to 130. We produced percentile-equivalent cutpoints on the raw attention outcome. Like the linear regression models, we also fit models with and without sample weights, and also with and without multiple imputation of missing data (though these could not be combined). With listwise deletion, this yielded 21 (attention cutpoints) x 2 (outcomes) x 2 (TV ages) x 2 (covariate sets) x 2 (sample weights) = 336 models. Using multiple imputation, we fit 21 (attention cutpoints) x 2 (outcomes) x 2 (TV ages) x 2 (covariate sets) = 168 model, for a total of 504 logistic regression models. However, because of sparseness on the attention outcome (particularly the raw version), frequently the imposition of two adjacent cutoffs (e.g, 121 and 122) would produce identical categorizations of the outcome and therefore redundant results. After purging these redundancies, we were left with 192 unique logistic regression models. We used the *survey* and *mice* packages to incorporate sample weights and to perform multiple imputation.

In total, we fit 888 non-redundant models to the data, including 384 IPTW propensity score, 192 stratified propensity score, 72 linear regression, and 192 logistic regression models.

**Results**

Space limitations prevented us from displaying any results from any specific model in this paper. However, the github repository “Results” directory contains a subdirectory for every analysis conducted, which includes descriptive statistics, diagnostic tables and plots, and formatted model results, which were produced using the *stargazer* package (v. 5.2.1; Hlavac, 2015). Figures may be browsed on the github site. However, tables will appear as unrendered HTML code unless the link to the table is prefixed with <http://htmlpreview.github.com/?>. The easiest way to examine the results in detail is to download our Github repository to your local computer; once downloaded, rendered tables will open in your web browser.

**Descriptive statistics**

Tables 1 and 2 provide descriptive statistics for the continuous and categorical variables. Figure 1 displays histograms of TV use measured at ages 1.5 and 3. Figure 2 displays a set of scatterplots displaying the relationship between TV consumption and within-sex standardized attention measured at age 7.

[ppp]

**Discussion**

The broad goal of this paper was to determine the model dependency of the claim that early TV watching causes attentional problems (Christakis et al., 2004). In other words, to what degree does finding a relationship depend on the analysis model used? As only 6 out of 82 analyses suggested a causal relationship, all with tiny effect sizes, we assert that the claim is highly model-dependent.

Indeed, starting with the most straightforward method of visualizing the relationship -- a simple scatterplot with TV watching and attention problems as shown in Figure 1 – suggests that the purported relationship is illusory. This plot reveals essentially a flat line when TV use is measured at age 1.5 and a mildly s-shaped curvilinear relationship between TV and attention when TV use is measured at age 3 (top right panel). This curvilinear relationship, such as it is, is nearly dampened out of existence by the introduction of covariates (bottom right panel).

Other methods of estimating the relationship only further strengthen our opinion that no relationship exists in these data between TV and attention. Specifically, none of the propensity score analysis variants using the within-sex standardized outcome, nor any analyses using linear regression, provided any evidence for the claim. Only four of the propensity score models using the raw attention score, and two of 42 logistic regressions corresponding closely to the original choice of cutpoint, produced statistically significant results. Given the tiny minority of analytic paths that produced a significant result, we think the most reasonable conclusion is that there is *no causal effect* of hours of TV watching on attention problems, at least as defined by scores in the Behavior Problems Index.

We also examined the role of temperament in connection with TV watching and later attention problems. Our hunch at the outset of this project was that any relationship between early TV-watching and later attention problems might be the result of the third variable of temperament. In fact, however, there was little sign of a meaningful relationship between TV and attention to be explained. We did find the predicted link between children’s temperament and their early TV-viewing to some degree. Specifically, temperament emerged as a moderately important predictor in the propensity score models for TV use at age 3 but not at age 1.5. However, as our follow-up simple regression shows, in practical terms this is very small effect.

In some ways, the field has already moved beyond the broad-brush claims from the original paper: that hours of TV (in general) cause attention problems (for everyone). As Kostyrka-Allchorne et al. put it, it seems foolish to expect that screen time “as an undifferentiated activity,” (2017, p. 52) predicts much of anything. Recent research about screen media use in children has gotten more precise – investigating the specific effects of violent content, fantastical content, pace of scene-change, and the viewer’s voluntary control of the action, among other factors (Huber et al., 2018).

In one such line of work, Lillard and Peterson (2011) found that certain cartoons appear to temporarily attenuate children’s executive functioning, including planning and delay of gratification. The culprit in these cartoons was first thought to be the fast pace of the scene changes, but subsequent work suggested that the fantastical content seemed to be the cause (Lillard, Drell, Richey, Boguszewski, & Smith, 2015). Two things are notable about this line of research, however. The first is that it (and similar lines of research) was founded on the desire to locate a *mechanism* for the purported negative effect of TV – an effect that our multiverse analysis suggests is probably nonexistent. Second, while the experimental approach taken by Lillard and colleagues is thoughtful and well-controlled, the negative effects on executive functions are short-term in nature, and the authors note that it is unclear whether this might lead to long-term deficits. It seems equally plausible that fantastical content exercises the executive functions in much the same way as running exercises the leg muscles. Leg strength may be drained after a sprint, but improved in the long term. There is even some evidence that fantastical components to stories and problems lead to *improved* learning in preschoolers (Weisberg, Hirsh-Pasek, Golinkoff, & McCandliss, 2014). In any event, it would seem premature to suggest that children be shielded from fantastical content, which is such a rich part of childhood.

In summary, the multiverse analysis presented in this paper used a large, nationally representative dataset to ask the same question in 82 different ways: Is there any reason to believe that TV watching in early childhood causes attention problems in later childhood? In only six of these 82 analyses was the answer “yes.” Thus, we think the true answer is likely “no.” In fact, screen media may not be all that special. It may be just one more part of life that has the power to entertain, teach, confuse, distract, or inspire.

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Table 1

Marginal descriptive statistics for continuous variables

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Variable | Valid n | Mean | Std Dev | Min | Max |
|  |  |  |  |  |  |
| Age (yrs) when attention was measured | 2108 | 7.75 | 0.61 | 6.75 | 8.75 |
| Annual family income (thousands) | 1958 | 33.42 | 24.53 | 0 | 189.92 |
| Attention (raw) | 2108 | 2.64 | 0.39 | 1 | 3 |
| Attention within-sex SS | 2075 | 101.25 | 13.79 | 83 | 136 |
| CES-D Depression score (1992) | 2089 | 46.97 | 7.87 | 32.3 | 79.9 |
| Cognitive stimulation of home age 1-3 | 1907 | 97.61 | 16.15 | 11.1 | 148.2 |
| Emotional support of home age 1-3 | 1765 | 97.99 | 16.58 | 31.6 | 124.7 |
| Gestational age at birth | 1960 | -1.41 | 1.96 | -14 | 7 |
| Mother's age at birth | 2108 | 28.48 | 2.62 | 23 | 36 |
| Mother's years of schooling | 2095 | 12.95 | 2.48 | 0 | 20 |
| Number of children in household | 2097 | 1.64 | 1.2 | 0 | 7 |
| Partner's years of schooling | 1757 | 13.28 | 2.7 | 1 | 20 |
| Rosenberg self-esteem score (1987) | 2040 | 45.07 | 8.4 | 23.5 | 59.7 |
| Temperament | 1961 | 2.01 | 0.69 | 1 | 5 |
| TV hours per day age 1.5 | 1993 | 2.23 | 3.07 | 0 | 16 |
| TV hours per day age 3 | 2023 | 3.68 | 3.12 | 0 | 16 |
|  |  |  |  |  |  |



Table 2

Marginal descriptive statistics for categorical variables

|  |  |  |  |
| --- | --- | --- | --- |
| Variable | Value | n | Percent |
|  |  |  |  |
| Maternal alcohol use in pregnancy | No | 1050 | 49.81% |
|  | Yes | 932 | 44.21% |
|  | (missing) | 126 | 5.98% |
|  |  |  |  |
| Cohort (interview wave when attention was assessed) | 1996 | 829 | 39.33% |
| 1998 | 796 | 37.76% |
|  | 2000 | 483 | 22.91% |
|  |  |  |  |
| Father absent from household | No | 1681 | 79.74% |
|  | Yes | 399 | 18.93% |
|  | (missing) | 28 | 1.33% |
|  |  |  |  |
| Child sex | Female | 1034 | 49.05% |
|  | Male | 1074 | 50.95% |
|  |  |  |  |
| Low birth weight (< 2500g) | No | 1812 | 85.96% |
|  | Yes | 138 | 6.55% |
|  | (missing) | 158 | 7.50% |
|  |  |  |  |
| Health condition that limits school or play | No | 1917 | 90.94% |
| Yes | 122 | 5.79% |
|  | (missing) | 69 | 3.27% |
|  |  |  |  |
| Premature birth | No | 1744 | 82.73% |
|  | Yes | 216 | 10.25% |
|  | (missing) | 148 | 7.02% |
|  |  |  |  |
| Child race | Black | 572 | 27.13% |
|  | Hispanic | 397 | 18.83% |
|  | White | 1139 | 54.03% |
|  |  |  |  |
| Maternal smoking in pregnancy | No | 1447 | 68.64% |
|  | Yes | 528 | 25.05% |
|  | (missing) | 133 | 6.31% |
|  |  |  |  |
| Standard metropolitan statistical area (urbanicty) | Not in SMSA | 382 | 18.12% |
| SMSA; central city unknown | 680 | 32.26% |
|  | SMSA; in central city | 302 | 14.33% |
|  | SMSA; not central city | 639 | 30.31% |
|  | (missing) | 105 | 4.98% |
|  |  |  |  |







Table 3

Logistic regression results by attention cutpoint and missing data treatment

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Listwise | | |  | Multiple imputation | | |
| Attention cutpoint |  | Non-sig | Sig | Proportion sig |  | Non-sig | Sig | Proportion sig |
|  |  |  |  |  |  |  |  |  |
| 110 |  | 8 | 0 | 0.000 |  | 4 | 0 | 0.000 |
| 111 |  | 8 | 0 | 0.000 |  | 4 | 0 | 0.000 |
| 112 |  | 8 | 0 | 0.000 |  | 4 | 0 | 0.000 |
| 113 |  | 8 | 0 | 0.000 |  | 4 | 0 | 0.000 |
| 114 |  | 8 | 0 | 0.000 |  | 4 | 0 | 0.000 |
| 115 |  | 7 | 1 | 0.125 |  | 4 | 0 | 0.000 |
| 116 |  | 7 | 1 | 0.125 |  | 4 | 0 | 0.000 |
| 117 |  | 6 | 2 | 0.250 |  | 4 | 0 | 0.000 |
| 118 |  | 6 | 2 | 0.250 |  | 4 | 0 | 0.000 |
| 119 |  | 5 | 3 | 0.375 |  | 3 | 1 | 0.250 |
| 120 |  | 3 | 5 | 0.625 |  | 2 | 2 | 0.500 |
| 121 |  | 3 | 5 | 0.625 |  | 2 | 2 | 0.500 |
| 122 |  | 3 | 5 | 0.625 |  | 2 | 2 | 0.500 |
| 123 |  | 1 | 7 | 0.875 |  | 2 | 2 | 0.500 |
| 124 |  | 1 | 7 | 0.875 |  | 2 | 2 | 0.500 |
| 125 |  | 0 | 8 | 1.000 |  | 2 | 2 | 0.500 |
| 126 |  | 0 | 8 | 1.000 |  | 2 | 2 | 0.500 |
| 127 |  | 0 | 8 | 1.000 |  | 2 | 2 | 0.500 |
| 128 |  | 0 | 8 | 1.000 |  | 3 | 1 | 0.250 |
| 129 |  | 0 | 8 | 1.000 |  | 3 | 1 | 0.250 |
| 130 |  | 0 | 8 | 1.000 |  | 3 | 1 | 0.250 |
|  |  |  |  |  |  |  |  |  |

Note: *Non-sig*: the number of models using the specified attention cutpoint and missing data treatment that did not yield statistical significance. *Sig*: the number of models that yielded statistical significance.

Figure 1

Density plots for TV consumption at age 1.5 and age 3

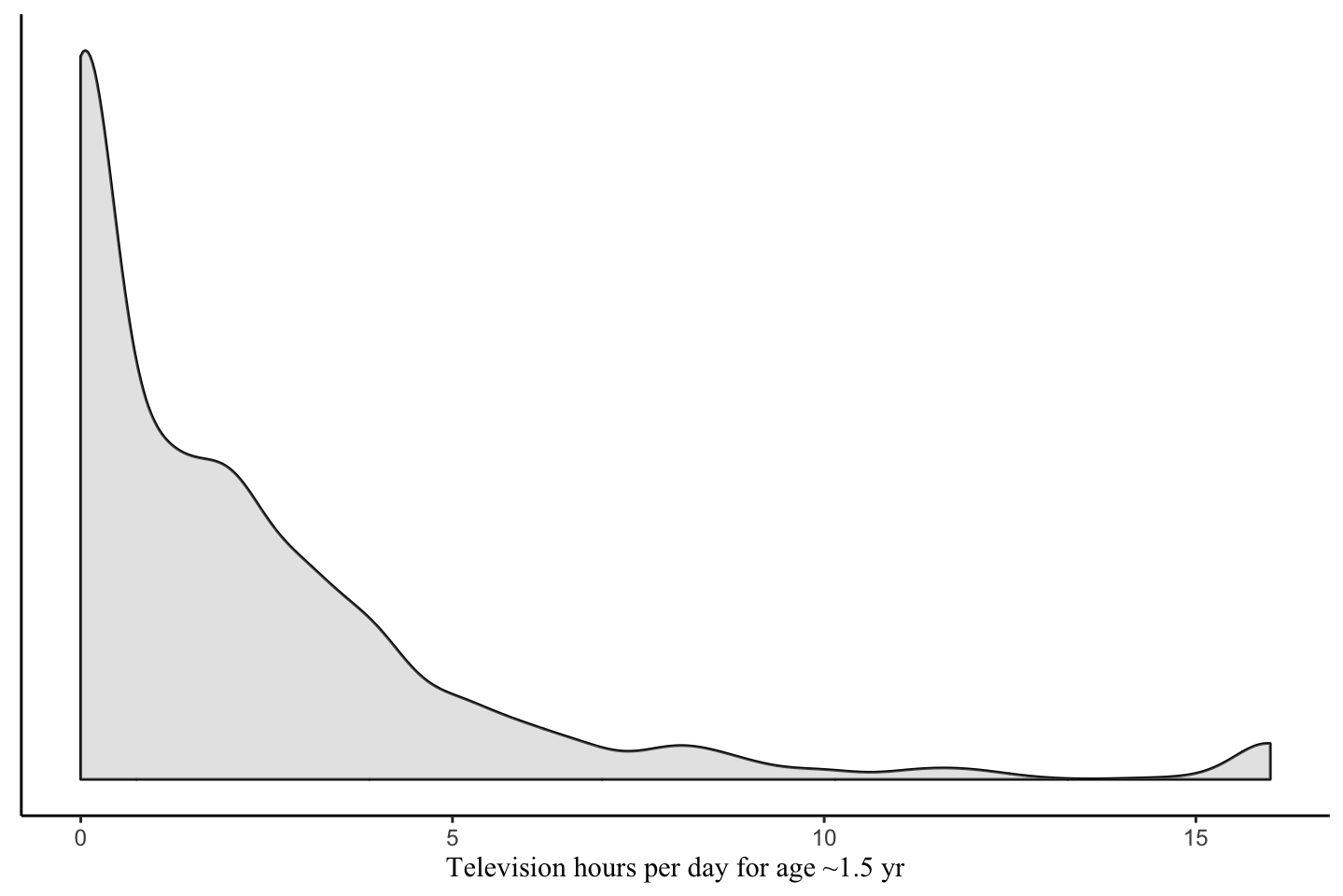
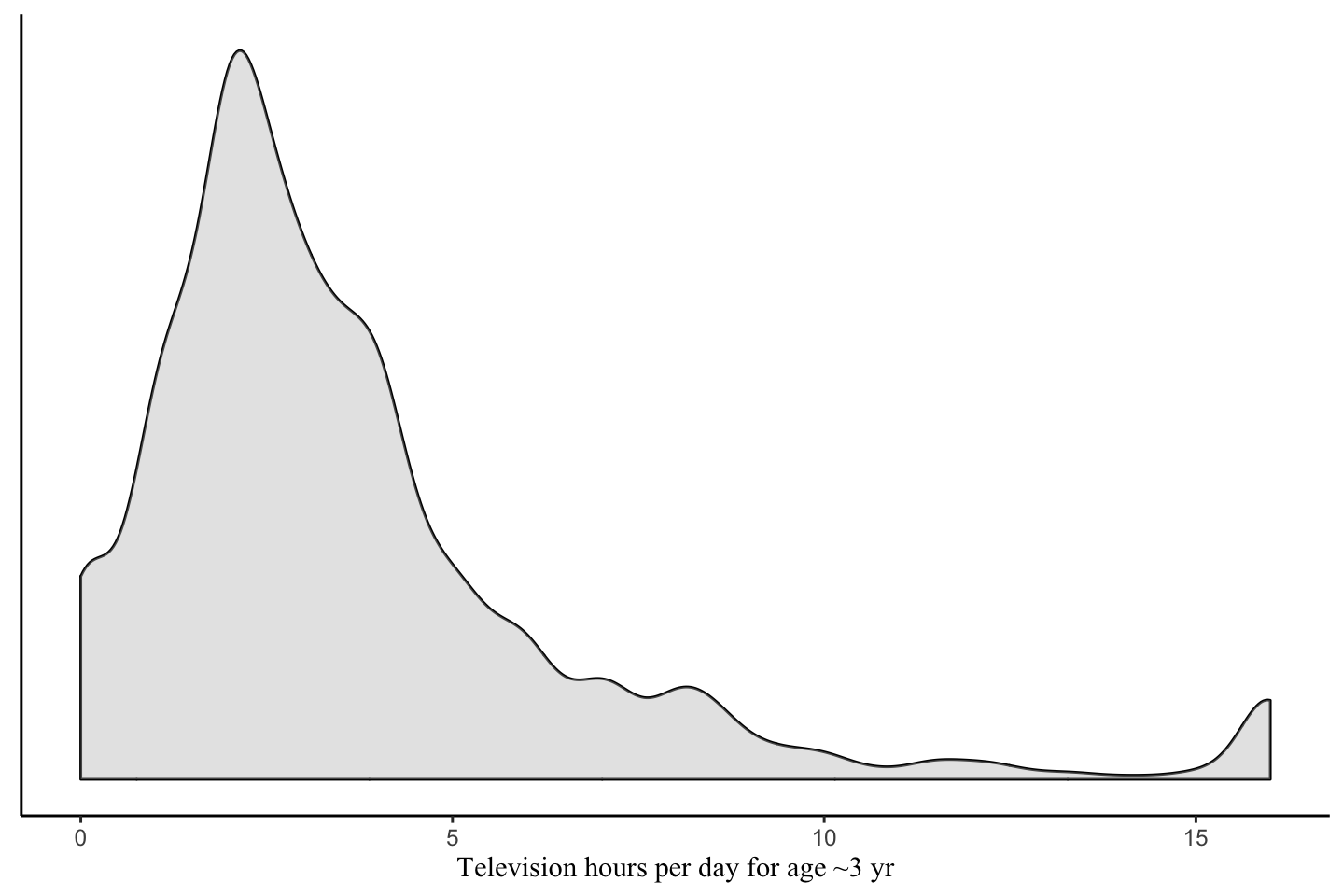


Figure 2

Scatterplots between early childhood TV use and standardized within-sex attention score at age 7. *Left column*: TV measured at age ~1.5. *Right column*: TV measured at age ~3. *Top row*: raw data. *Bottom row*: adjusted (residualized) attention score with effect of covariates removed. Bottom panel: red ‘x’ points are adjusted based on imputed covariate values. Solid blue smoothing line fits to non-missing data only; red dashed smoothing line fit all data (including imputed values).

A close up of a map

Description automatically generated

Note: point locations are slightly jittered to reduce overplotting

Figure 3

A picture containing screenshot

Description automatically generatedIPTW propensity score model results summary

Figure 4

A close up of a map

Description automatically generatedStratification propensity score model results summary

Figure 5

Regression model results

A screenshot of a cell phone

Description automatically generated

Figure 6

Logistic regression results summary

A screenshot of a cell phone

Description automatically generated

Figure 7

*p­­*-value summary for all models

A close up of a map

Description automatically generated

Figure 8

IPTW propensity score model post-mortem

A close up of a map

Description automatically generated

Figure 9

Logistic regression post-mortem

A screenshot of a computer

Description automatically generated

1. A prior version of this analysis also included the child’s body mass index (BMI), but we removed that variable at the direction of a reviewer, who was concerned about potential endogeneity. [↑](#footnote-ref-1)