The Effect of Media on Children:

a methodological assessment from a social epidemiologist

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ABSTRACT

In an effort to help advance it, this report considers the accumulating body of media research with children from a social epidemiological perspective. I am specially concerned with comparing and contrasting the types of research designs, standard measures and modeling frameworks used in media studies to those used in (social) epidemiology and prevention research more generally.

Overall, the state of research on the effect of various media on outcomes in children is robust and promises to be on par with the epidemiological and biomedical research on the relationship between cigarette smoking and lung cancer. Despite enormous difficulties, measures used in media research, be they exposure or outcome, appear well-conceived and credible, though more work on their evolution and validity in young children and field studies is suggested. The principal measurement deficiency lies in the measurement of potential confounders and effect modifiers. With respect to research designs, media research is again robust due to its use of lab-based experiments and field surveys. Principal deficiencies include neglect of effect-modification by recognized social groupings, contextual influences and social interaction. Substantial practical benefits may come from the implementation and evaluation of intervention programs, especially group-randomized trials. As for analyses, the principal weakness appears to be in extending causal inference into observational field studies.

I. INTRODUCTION

This report considers media effects research, principally conducted by academic communication and (developmental) psychology scholars, from the perspective of a social epidemiological methodologist somewhat unfamiliar with the literature. The goal is to offer a fresh perspective on the accumulating body of research so as to advance it. To this end, this report is especially concerned with comparing and contrasting the types of research designs, standard measures and modeling frameworks used in media studies with children to those used in (social) epidemiology and prevention research more generally. I do not consider any direct physiological media effects that may be associated with, say, electromagnetic fields, or injuries related to mechanical accidents or sitting positions.

A comment on social epidemiology may be helpful. Epidemiology is the study of the distribution and determinants of states of health in populations. As currently practiced, it is largely concerned with biological relationships between exposures and disease. Methodologically, it relies primarily on observational designs. *Social* epidemiology has been defined as the branch of epidemiology that considers how social interactions and collective human activities affect health (Oakes and Kaufman 2006). In other words, social epidemiology is about how a society's innumerable social arrangements, past and present, yield differential exposures and thus differences in health outcomes between the persons who comprise the population. Relatively new to the scene, social epidemiology is different from the bulk of traditional epidemiologic practice which tends to operate with a model or paradigm based on the fictitious Robinson Crusoe (Oakes 2005b). Recall that this character is someone in an environment devoid of social context, whose health depends only on biological relationships and the vicissitudes of island weather. Social interactions are central to social epidemiology, however. Indeed, it is the incorporation of purposive human

interaction and agency (i.e., social coordination and conflict) that links social epidemiology to the social sciences and raises enormous methodological obstacles to inference; obstacles that leading social scientists have long sought to overcome.

Further, it seems worth being clear and stating that methodological research is largely concerned with studying the logic of, and improving techniques for, scientific inference. The broad objective is to learn what conclusions can and cannot be drawn given specified combinations of assumptions and data (Manski 1995). Social epidemiological methodology is naturally the study of methods in and for social epidemiology (Oakes and Kaufman 2006).

The limitations of this review must be emphasized. First, this report's author is not a media researcher and, despite efforts and careful scholarly examination of the accumulated literature, it is all but certain that several nuances and scholarly associations have been missed or misinterpreted. Second, given the literally thousands of media effect studies published over the last fifty years, this report is based largely on (the many) existing reviews of media research (principally: Anderson and Bushman 2002a; Anderson and Bushman 2002b; Bushman and Anderson 2001; Carnagey and Anderson 2004; Cook et al. 1983; Felson 1996; Gentile and Anderson *In Press;* Roberts et al. 2004; Savage 2004; Schmidt et al. 2005; Van Evra 2004). While many seemingly important studies were examined 'in the original,' the strengths and gaps discussed below are necessarily filtered through the work of previous media research reviews. Third, it is stressed that when it comes to contrasts and comparisons between media research and epidemiologic/prevention research, the later is presented in its ideal form. Readers should not imagine that all or even most epidemiologists are adopting and incorporating the methodological insights offered by a relatively small number of methodologists. Epidemiology as a discipline is far from perfect and is facing its own crises (Poole and Rothman 1998; Savitz 1997; Shy 1997); it too may benefit from an

outsider's perspective, perhaps one from a media research scholar. Fourth, in contrast to the approach of some behavioral or psychological scholars aiming to model processes, the perspective offered here is one of 'treatment effects' designs and methods, as is consistent with most epidemiology and biomedical science. Finally and obviously, another epidemiological methodologist would surely identify and discuss different issues.

Some limitations may also be strengths. The outsider's perspective offered here may illuminate strengths and weaknesses that media effects scholars have failed to notice. Although a partial reliance on existing reviews may miss subtle nuances of particular studies that review authors filtered out or themselves missed, a review of reviews may speak volumes about a discipline's self-assessment. That is, even if not true in primary studies, gaps in review articles indicate disciplinary gaps in the identification and reporting of important research elements. Review articles, especially those addressing methodological issues, appear to have a powerful influence on future work.

The remainder of this report is organized into four major sections: (II) a very brief overview of principal findings, especially with respect to children; (III) an overview of the state of methodology in media effects research, including summary of identified gaps; (III) discussion of four identified gaps; and (IV) conclusions and recommendations.

II. THE STATE OF MEDIA EFFECTS RESEARCH (with children)

The media effects literature is impressive in both scope and depth. Numerous credible studies have documented at least short-term effects of various media forms (e.g., television, video games, internet) on a wide variety of outcomes including cognitive development, aggression, violence, sleep disturbances, and physical activity (eg,

Anderson and Bushman 2002b; Anderson and Pempek 2005; Escobar-Chaves et al. 2005; Funk 2005; Paik and Comstock 1994; Savage 2004; Wright et al. 2001).

Exposure to media, both directly and indirectly, is ubiquitous. Very recent research shows even very young children in the US are directly exposed to media. Vandewater et al. (2005a) found that 35% of children live in households where the television is 'always on.' The recent *Zero to Six* study found that 73% of children under the age of six watch TV daily. For those under two who watch TV, the mean time for watching is about 1.4 hours per day. Moreover, about 50% of children live in homes with three or more TVs (Rideout et al. 2003). African American children and children from less educated parents use media more frequently (Anand and Krosnic 2005).

Media effects are understood to be both harmful and helpful. While there are too few studies of very young children and media, including television, it seems clear that negative effects are plausible while few positive ones are (Anderson and Pempek 2005). Yet if children can learn negative things from the media, then they should be able to learn positive things too. Early studies of *Sesame Street* showed positive (ie, protective) effects related to alphabet, numbers, body parts, and shapes, with stronger effects for those in lower socioeconomic strata (Fisch 2002). And positive effects from *Dora the Explorer* and *Blues Clues* television programs are also observable (Schmidt et al. 2005).

Substantial progress has been made on the possible/probable theoretical and conceptual mechanisms for observed effects. Perspectives appear largely based on theories of socialization and learning, often associated with, or derivative of, Bandura's social learning theory (SLT) (Bandura 1985). More recent theoretical advances seem similarly focused and include aspects of situational framing and nascent notions of feedback loops. Other exciting work considers symbols and symbol appreciation (DeLoache 1989; Deloache 2004; DeLoache 2005). Additionally, scholars are improving theory for understanding the interactive/participatory nature of (often violent) video

games and outcomes (Carnagey and Anderson 2005; Funk 2005; Uhlmann and Swanson 2004).

Some have recently commented on the dearth of theoretical work for understanding media phenomena in very young children. This is true, but it seems that there is sufficient existing work, if not critical mass, to enable such advances. A major paradigm shift does not seem necessary as the basic science – that is, the micro- or cognitive-level aspects of when, where, and how learning (good or bad) from television and related media occur – seems well documented or on its way. For purposes here, at least, such work is critical as there can be no doubt about the inextricable link between theory and methodology; methodology is a mess without theoretically derived testable hypotheses.

III. MEDIA EFFECTS RESEARCH METHODOLOGY

The media research literature's methodology appears robust as investigators are employing a variety of measures, designs and models to specific problems. Overall, media effects research appears healthy and likely to bear more fruit without a major paradigm shift. Indeed, in contrast to some (see Feguson 2002), I find the media effects literature to be on par with the epidemiological and biomedical literatures addressing cigarette smoking and lung cancer: incontrovertible causal results from experimental data may be absent but an abundance of credible observational research and plausible mechanisms permits strong conclusions to be made. The media effects literature has more work to do, but it appear to be on the right track.

Exposure (ie, media) measurement appears fairly sophisticated as investigators clearly appreciate the limitations of both self-reports *and* "objective" measurement. Classifying content of exposure (eg, good or bad) is recognized as challenging, especially in a multimedia multitasking world, but it seems many media researchers are

aware of these challenges and are proceeding with caution and increasing rigor (see Vandewater and Lee 2006). Thus, it appears that little new or extra attention need be directed here. The only meaningful comment may be that few seem to truly appreciate that media exposure is now like air or water: ubiquitous, ever-evolving and not easily coded as data for a given analysis. No one is unexposed and the fact is there are both benefits and risks associated with exposure. Much like environmental epidemiology's struggle to analyze the effects of ambient toxins (eg, smog), media researchers must contend with seemingly intractable pathways and complex if not intractable causal relationships that include necessary, sufficient, and surely preemptive causal structures. The upshot is that at this point measurement obstacles are more conceptual than practical or empirical. Progress would seem to require that the conceptual apparatus come before advances to actual exposure measurement.

As mentioned above, outcome measures are diverse and include (1) psychometric scales tapping latent measures such as IQ, attention, and propensity for violence; (2) extant behaviors such as violence and social interaction; and (3) even health mediators and outcomes such as physical activity and obesity. No shift appears needed here either.

The broad literature is replete with behavioral-lab experiments, many sample surveys, a few case studies and qualitative investigations. Some cohort (ie, panel or longitudinal) designs have been implemented and examined. Adopted statistical models are fairly mainstream (e.g., ANOVA, logistic regression) but not routinely misused. Overall, employed research designs and statistical methods appear adequate.

Of course no literature, including that on the health effects of smoking, is perfect and complete. With respect to media effects research, I detect – hopefully not with too much errors – the following interrelated gaps or areas in need of increased attention.

(1) The media effects literature appears to be overlooking how the effect of various media (eg, TV, iPods, violent games) on whatever outcome may differ by persons of typically recognized social groups;

(2) The media effects literature appears to be overlooking multilevel confounding and/or modification by so-called contextual effects or related social interactions;

(3) The media effects literature pays too little attention to the nuances of and prospects for causal inference in observational designs;

(4) Media effects research has conducted too few field interventions (experiments or quasi-experiments) to test theories about the effects of media on various outcomes;

(5) Finally, while many are surely aware of the problem, it does not appear as if the majority of the media effect scholars discussing the innovative forces of media providers and capital markets. Little identified research is addressing the enormous sea change of media exposure the ongoing wireless broadband revolution is bringing and, it seems, will continue to bring at a faster rate. Few scholars are discussing the future of media effects research in the next 10 or 20 years, and thus exposure measurement seems lacking, already. There seems to be a shortage of discussion on how the subdiscipline might keep up with advertisers who are clearly discussing strategies to overcome the current market saturation of ads (ie, ubiquity of media stimuli). The concern is that media effects research may be left looking backwards and thus suffer a relevancy problem. However, it is not at all clear how to address this problem in an empirical science

that depends on existing data. Social epidemiology faces no similar problem. Accordingly, nothing more will be said about this here.

VII. FOUR METHODOLOGICAL GAPS

1. Individual-level effect modification

The media effects literature appears to be overlooking how the effect of various media (eg, TV, iPods, violent games) on whatever outcome may differ by persons of typically recognized social groups. No identified research addresses how, say, TV violence is differentially processed and/or acted upon by a Native American child versus an African American child, versus a Mexican immigrant child versus a wealthy suburban white child. It appears as if age and sex are the only measures across which effects are routinely presumed to vary. In her review, Oliver (2002) observes the paucity of research on effect modification but also notes several exceptions. Included would be Felson (1996) and others who note differential effects with respect to exposure to media violence and the differential effects of the positive impacts of *Sesame Street* by socioeconomic status (Fisch 2002). Nevertheless, Oliver concludes “By acknowledging the importance of what the audience brings to the viewing situation, media effects research stands to move beyond seeing unexplained variance as only a nuisance…” (p 520). I concur.

It is not obvious why such analyses are not a prominent component of the literature. One explanation may rest with the biology of human development. It is reasonable to wonder about differential effects by social-group in the very young. It could be that before the age of three, or thereabouts, development and media effects are remarkably uniform due to developmental and genetic imperatives. This is to say that very young Latino children learn and are affected by media in the same way as American Indian or Caucasian children, for example. This could be the case. Indeed,

without strong evidence, such work need not (i.e., *should not*) attribute differences, if detected, to genes or other non-mutable characteristics (see Shonkoff and Phillips 2000, chapter 2). Rather, attribution may be given to the differential "frames" or worldviews of such children due to their familial socialization. It is simply not clear how early socialization begins to have substantial effects on learning and behavior.

Relatedly, the media effects literature appears to have paid little attention to the statistical hurdles analysts must overcome to confidently claim support for or against such an effect. The lessons learned in social epidemiology may be of use in both these cases. Regression-based interaction effects are called "effect modifiers" in epidemiology (Rothman and Greenland 1998). The later term seems superior since it conveys an intuition about the phenomena. Additionally, it implies that an analysis is focused on one particular exposure, which may be altered across a small number (eg, two) identifiable groups. Regardless of name, the idea that the magnitude of exposure effects vary across populations is straightforward, except perhaps when considering continuous or multilevel measures.

Effect modification may be examined through stratified analyses, such as by age, race, IQ, proclivity for violence and so forth. An investigator may estimate and then compare effect estimates from the two sub-population analyses. If different, one may conclude that some characteristic of the subpopulation(s) modifies the overall or average effect. Within a regression framework, effect modification is typically estimated by including a regressor that is the product of one's principal exposure measure and, say, some demographic characteristic such as race/ethnicity. If the estimated coefficient for this product measure is meaningful (ie, large and precise) then evidence of effect modification is often said to exist.

It seems worth pointing out that the estimation of effect modifiers requires abundant high-quality data from strong research designs. Among others, epidemiologists

have advanced our understanding of when and why presumably strong modifiers are not observed in some analyses. Among other concerns, the issues of statistical power (ie, the ability to detect an effect) are not straightforward (Greenland 1993). Additionally, there is a close but distinct relationship between effect-modification and confounding (Greenland and Morgenstern 2001; Greenland and Robins 1986).

Overall, it is not clear whether the lack of attention to effect-modification is a function of interest, appreciation, or pure measurement deficiencies. As to the latter concern, the literature does not seem overly concerned with the measurement of potential effect-modifiers (and/or confounders) such as race, SES and related sociological and difficult to measure constructs (Kaufman 1999; Kaufman and Cooper 2001; Oakes 2005a; Oakes and Kaufman 2006; Oakes and Rossi 2003).

2. Multilevel inquiry, context effects and social interactions

It seems that the media effects literature is not considering the physical and social environments/contexts in which exposures and effects occur. Unlike the aforementioned discussion of individual-level effect modification the issue here is the contexts above and beyond personal-level characteristics (Diez-Roux 1998; Oakes 2004b). Such contexts may include family or childcare or school characteristics, friendship networks, neighborhood SES, degree of racial residential segregation, or peer-group norms.

Given the appreciation for understanding such contexts in early childhood development (see Shonkoff and Phillips 2000), it seems likely that such contexts play some role in the effect of media on a wide variety of outcomes. Presumably, the effect of media on children growing in a safe, supportive, and intellectually challenging home will differ from those growing up in disadvantaged homes. Vandewater (2004) makes this same point with respect to families and offers a useful example: the effect of television

use on obesity might be negligible for a child of a family that values physical activity but profound for a child in a household prone to lethargy. In her 2005 paper (Vandewater et al. 2005b), she finds that parental rules mediate media exposure, a finding consistent with prior work (see Fisch 2002). The critical insight here is that contexts shape, mediate and/or modify effects; surely such background characteristics may confound simple analyses too. Increasing attention to the "upstream" or higher-level context effects may illuminate new areas of media effects research.

Social epidemiologists are currently embracing the so-called multilevel regression model in hopes of disentangling the effects of individual characteristics from those attributable from contexts (see Diez-Roux 2003). For example, there has been a surge of research into how neighborhood environments affect health risks above and beyond the characteristics of individuals within given neighborhoods. Questions being addressed include the effect of being more wealthy in a relatively poor neighborhood on cardiovascular disease, the effect of neighborhood poverty on American Indian infant mortality, and the effect of 'social capital' on healthcare access and delivery.

Notice that these analyses are multilevel, which is to say that there is nesting of units. For example persons are typically nested within neighborhoods, children in schools, and patients in clinics. This means that there are measures (ie, variables) at both the level of an individual and the context, at least. Examples include individual income and neighborhood poverty, or one's race and the percentage white in a school. Multilevel nesting is particularly interesting to study since it addresses how social influences 'get into' people and how people act together – the so-called micro-macro and macro-micro transitions (Coleman 1990; Oakes and Kaufman 2006).

While exciting, it is important to mention that some methodologists are cautioning investigators from drawing overly strong inferences from observational data. In short, it does not seem possible to identify the desired multilevel effect parameters from

observational design data. Much like the earlier conclusions of social scientists, broad and specific cautions are growing more abundant (Durlauf 2004; Oakes 2004a; Oakes 2004b; Oakes *In Press;* Sobel 2006a).

Much related to multilevel or context effects are dynamic effects. Little attention has been paid to appreciating or modeling the often dynamic effects of social interactions on media-related outcomes. Few persons are exposed to stimuli – be it a television or a virus – in isolation. Within economics and both social and infectious disease epidemiology there is growing interest in understanding, modeling and estimating the (dynamic) effects of social interactions associated with exposures on both individual and group outcomes. The idea is that the behavior of one is a function of the behavior of others. Such "interaction-based models" are at the forefront research in economics (eg, game theory) and infectious disease if not social epidemiology (see Durlauf 2001; Durlauf 2004; Durlauf and Young 2001). While a simple idea, the methodological hurdles one must overcome to identify desired parameters is enormous if not too often impossible (Blume and Durlauf 2006).

Related is recent work on the too often hidden causal assumption called the Stable Unit Treatment Value Assumption, or "SUTVA" for short. The basic idea here is that in conventional models of cause and effect, the treatment one receives has no "spill-over" effects on others, an invariance assumption. The often used example is that giving your boss an aspirin for her headache has no impact on you. Of course, this assumption is often violated in the real-world, especially with respect to social scientific investigations. Accordingly, scholars are aiming to more deeply examine the implications of relaxing it. In statistics, the seminal work is by Cox and Rubin (Cox 1958; Rubin 1980). In epidemiology, it is Halloran and Struchiner (1995). In sociology, it is the work of Sobel (2006a; 2006b) that is leading the way. It seems media research would benefit from understanding if not appreciating the issues in this line of inquiry.

3. Causal inference in observational designs

Despite its strong foundation in and use of experimental designs, the media effects literature seems to pay little extant attention to the nuances of causal inference, particularly with respect to counterfactual reasoning. This deficit tends to hinder the understanding of non-experimental designs and even sampling plans that may be useful for media effects research. Relatedly, the media effects literature appears to undervalue and under-exploit methods for inferring cause in observational designs such as cross-sectional surveys. It seems as if such studies are dismissed as mere correlational studies. Cutting edge approaches to inferring cause in observational designs do not appear to being implemented. And there appears to be an overly optimistic impression of longitudinal (observational) designs; the severe problems of simultaneity do not appear to be fully appreciated.

Appreciation for the nuances of causal inference in non-experimental settings is critical since there may be ethical and/or practical constraints (eg, money) on experimental intervention. Epidemiology, at least ideally, is well versed in causal inference outside experiments. Classic discussions include those by Susser (1973) and Rothman (1974; 1993); more recent efforts include the work of Kaufman (Kaufman and Cooper 1999; Kaufman et al. 2003) and Maldonado and Greenland (Maldonado and Greenland 2002).

Many scholars are expressing a growing interest in and understanding of the counterfactual framework, which is merely a tool for conducting thought experiments that often illuminate problems and prospects analysts face in the “real” world. The genesis of the framework is often attributed to the philosopher Hume (1711-1776), but many contemporary philosophers and scientists have extended, clarified, and refined the original idea (Hausman 1998). Borrowing heavily from Oakes and Johnson’s (2006) discussion, I now offer a very brief introduction.

Within the counterfactual framework, a causal effect is ascertained through a comparison of "potential outcomes" that would have been observed under different exposures for the same unit (Little and Rubin 2000). For example, to calculate the causal effect of television exposure on the health of a toddler named Jane, we must simultaneously observe Jane's health under two conditions: one in which she was not exposed to, say, media violence and one in which she was. If it were possible to observe both of these situations (i.e., all potential outcomes), then it would be easy to calculate the desired causal effect: the effect would be the difference (or any contrast) of the two outcomes under the two scenarios. Of course it is not possible to simultaneously observe Jane's outcome under both conditions: she was either exposed to certain media or she was not. This fact – that we are missing observable data for one of the potential outcomes – is so important that it has been called the "fundamental problem of causal inference" (Holland 1986). To be clear, the *unobservable* data in the above scenario is called the "counterfactual," since it is counter to fact (Winship and Morgan 1999).

It is now easier to see the problem of the missing "potential outcomes" data. Specifically, a "target" group of children can only be observed under one exposure condition at a given point in time. This means *at least* one of the above exposure conditions and subsequent outcome frequencies is unobservable or counterfactual. Thus, in order to estimate a causal contrast we must obtain valid and observable *substitute* quantities for the desired counterfactual quantities (Maldonado and Greenland 2002). This substitution step is the crux of the causal contrast, the counterfactual framework, and causal inference more generally. If substitutes are *exchangeable* [1] with

[1] Exchangeability is a key concept that is related to confounding and elemental aspects of probability theory (De Fineti, Bruno. 1974. *Theory of Probability: A Critical Introductory Treatment, 2 Vols*. Translated by A. Smith. London: Wiley, Greenland, Sander and James M. Robins. 1986. "Identifiability, exchangeability, and epidemiological confounding." *Int J Epidemiol* 15:413-9.). Technically, exchangeability is a property of a joint distribution of a sequence of random variables that are unchanged by an arbitrary permutation of the sequence (Dodge, Yadolah. 2003. *The Oxford Dictionary*

targets, we are in good shape; if not, we suffer confounded effects which may be misleading at best.

Given the wide availability and easy access to statistical software, it is all too easy to make improper comparisons. I stress that it is critical for social epidemiologists and media researcher to appreciate that exchangeability implies that an observed counterfactual substitute *could have been* treated/exposed just as the unobservable counterfactual could (theoretically) have been. Indeed, estimates of causal effects in observational studies *presume* that the exposure of interest *could have happened* to anyone in the data (Rosenbaum 2002). If this is not possible, the substitution is improper and no causal inference may follow.

Analytic methods related to the counterfactual framework have been advanced; two seem worthy of mention. The first is propensity score matching . The core idea here is to match exposed and unexposed subjects (eg, kids) on their probability of being exposed… to various kinds of media. This probability, which is called the propensity score, is often estimated through logistic regression by regressing exposures (yes/no) on covariates/predictors. The advantage of this methodology is the mitigation of 'specification of models by p-values' and other aspects of sampling variability. In propensity score methods one models exposure and then uses such estimates to calculate effects; this procedure minimizes p-value specification concerns. Overall, propensity score matching aims to mimic experimental procedures with data from

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of Statistical Terms, Edited by T. I. S. Institute. London: Oxford University Press.). In simple terms, exchangeability is a term that connotes equality or substitution without penalty or change. As used here, groups are exchangeable if their substitution yields no impact on effect estimates. If substituting an observed control/comparison group for an unobservable counterfactual group (hypothetically!) yields the same effect estimate, then the observed and counterfactual groups are exchangeable. If the substitution yields a different effect estimate, then the groups are not exchangeable and estimated effects are confounded.

observational designs. See Oakes and Johnson (2006) for an accessible and thorough treatment.

The second approach is called instrumental variable analysis (IV). Unlike conventional regression models and propensity score matching procedures, the fundamental objective in IV analyses is to overcome confounding by *unmeasured/unmeasurable* variables. This is important since correlation between exposures/treatments and model disturbances yield biased effect estimates. IV analyses aim to mimic natural experiments by finding a measure/variable related to exposure but not related to the outcome measure (actually, model disturbances or errors). Such variables are called *instruments*. When good instruments are available, IV analyses can yield very useful effect estimates. The trouble is finding a good IV for poor ones can yield substantially biased results. For more, see discussion by Greenland (Greenland 2000), Glymour (Glymour 2006) and perhaps Angrist et al (1996).

Since experiments cannot always be implemented and some rightly express concern about the external validity of them, observational designs will continue to be the bread and butter of applied research. Accordingly. it seems that media effects researchers would benefit from an increased understanding of the contemporary causal inference literature. This is not to say that social epidemiologist or others have the answers: the concepts and issues remain slippery and very complex. Indeed, other more subtle issues such as simultaneity bias and redundant/over-determined causes must eventually be addressed (see Lewis 1973; Lieberson 1985).

4. Field experiments / Interventions

Media effects research has conducted too few field interventions (experiments or quasi-experiments) to test theories about the (negative) effects of media on various outcomes. Field research is, not surprisingly, dominated by observational surveys while

media-related experiments are conducted in behavioral labs. Methods for field experiments, especially group randomized trials, appear largely overlooked in the subdiscipline. Yet the public relevancy of media research may depend on such experiments, especially in this age of “evidence-based” medicine and perhaps policy. Regardless, the fact is that ultimately proposed/desired policy changes must be tested in the messy world of field experiments. Indeed, policy change itself may be viewed as an uncontrolled experiment (Campbell 1973).

With little exception, it is well known that most interventions and social program experiments fail to achieve desired results. Based on vast personal experience and an extensive review of the literature, the eminent program evaluator Peter Rossi formulated the ”Iron Law” of evaluation: *The expected value of any net impact assessment of any large scale social program is zero* (Rossi 1987).[2] It is rarely clear whether such failures are the result of ineffective interventions or methodologic obstacles (e.g., measurement error, poor design). But all seasoned program evaluators know that effective interventions are difficult to dream up and more difficult deliver under practical constraints. Nevertheless, given the evidence I tend to agree with Shonkoff and Phillips who write:

> The general question of whether early childhood programs can make a difference has been asked and answered in the affirmative innumerable times. The generic question is no longer worthy of further investigation. The central research priority for the early childhood field is to address more important sets of questions about how different types of interventions influence specific outcomes for children and families, who face differential opportunities and vulnerabilities (Shonkoff and Phillips 2000, p 379)

[2] Rossi (1987) also coined the “Brass Law” of evaluation: *The more social programs are designed to change individuals, the more likely the net impact of the program will be zero.* His point, of course, was that it is very difficult to change people, especially in isolation.

One often overlooked design for field experiments is the group randomized trial (GRT), sometimes called a place- or cluster-randomized trial (see Boruch et al. 2004; Hannan 2006; Murray 1998). The fundamental idea here is that whole groups (eg, cities, schools, states) are randomly assigned to experimental conditions, though data collection may occur at any level (eg, individual). There may be several reasons why assignment by group is either necessary or beneficial. Often discussed are structural constraints such as media delivery systems, concern with between-subject contamination (ie, talking), or perhaps ethical concerns such as not wanting to "single-out" certain children in front of their peers. But there is another reason that is far too infrequently appreciated: group randomized trials are the only experimental design that does not suffer violations of SUTVA (discussed above). Indeed, group randomized trials may encourage within group social interactions, collective action, norm changes and the like (see Wagenaar et al. 2000). It is for this reason that have I called it the *canonical* design for social epidemiology (Oakes 2004b).

Of course GRTs are not always feasible and certainly have their drawbacks. First and foremost, they are extremely expensive designs to execute. Second, GRTs also suffer the Iron Law (Sorensen et al. 1998; Susser 1995). Third, there are somewhat unique ethical concerns about informed consent (Oakes 2002). Nevertheless, I believe GRTs are important and media effects research would benefit from their implementation.

VII. CONCLUSIONS AND RECOMMENDATIONS

This brief report considered the methodology of media effects research with children from the perspective of a social epidemiologist somewhat unfamiliar with the nuances of the subdiscipline. The goal was to help advance ongoing and future work by identifying methodological gaps, if any.

The overarching conclusion is that media effects research is on firm methodological ground and should continue to yield inferentially robust results. While neither perfect nor without shortcomings, it is clear that scholars working within the subdiscipline appreciate obstacles and there appears to be a strong foundation from which advances will be made, especially with respect to exposure and outcomes measurement. There does not appear to be a need for a change in research paradigms. Identified gaps include insufficient attention to effect modifiers, contextual effects, causal inference in observational studies, and field experiments, especially group randomized trials.

A difficult problem concerns the complexity and ubiquity of the exposure, media. While many are surely aware of the problem of choice or self-selection, it does not appear as if the majority of the media effect scholars are discussing the innovative forces of media providers and capital markets. Little identified research is addressing the enormous sea change of media exposure the ongoing wireless broadband revolution is bringing and, it seems, will continue to bring at a faster rate. Related is the enormous change in the socioeconomic fabric of daily life in the United States, which appears to lead many parents to employ media (eg, televisions, games) as a childcare provider. Shonkoff and Phillips put it bluntly

> …[P]rofound social and economic transformations are making it exceedingly difficult for parents to strike a healthy balance between spending time with their children, securing their economic needs, and protecting them from the many risks beyond the home that may have adverse impact on their health and development (Shonkoff and Phillips 2000, p 393)

Since the stakes are sky high and impact of this (uncontrolled experiment) are unknown the importance of rigorous media effects research with children cannot be overestimated.

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