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**Movie Smoking and Teen Smoking Behavior: A Critical Methodological and Meta-Analytic Review**

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### **Abstract**

Policy makers and some scholars have expressed concerns regarding potential links between youth viewing smoking in movies and smoking in real life. Some advocacy groups have expressed the view that causal links between movie smoking and youth smoking definitively exist. However, research on actual smoking behaviors is relatively narrow, correlational, and tend to produce very small effect sizes. The current paper reviews the methodology of movie smoking studies and analyzes their results meta-analytically. A total of twelve independent datasets were analyzed which involved youth smoking behaviors as outcome (total participants = 70,788). Meta-analytic results suggested that, due to the large sample size of most studies, results often achieved “statistical significance” despite producing trivial effect sizes. Further, even these trivial effects may be due to widespread methodological limitations including demand characteristics and potential researcher expectancy effects. Recommendations are made for improving this research field.

**Key words:** Movies; Smoking; Mass Media; Adolescents

## Introduction

In recent years, several fields of media effects including video game violence (McCarthy et al., 2016; Sauer, Drummond & Nova, 2015), “sexy media” and teen sexual behavior (Ferguson, Nielsen & Markey, 2017) and “thin ideal” media effects (Holmstrom 2004; Whyte, Newman & Voss, 2016) have seen concerns that effects may be more modest or complex than had previously been believed. One issue that has received some attention in recent years regards whether watching actors in movies engage in smoking may prompt increases in teen smoking behavior. This issue has obvious public health implications, given the clear, detrimental impact of smoking on mortality and morbidity, particularly later in life. Thus, efforts to reduce the initiation of smoking, which often occurs in teen years, could have significant positive public health impacts. If movie smoking contributes to teen smoking then reducing smoking in movies would result in reduced teen smoking.

The issue is complicated given free speech concerns. In countries such as the United States, regulation and legislative efforts face a relatively high evidence bar for documenting harm caused by media in order to carve out a particular media issue as unprotected speech. In the United States, actual advertisements for cigarettes and tobacco was one such example, with regulations restricting where such advertisements can be placed (e.g. Public Health Cigarette Smoking Act; *Discount Tobacco City & Lottery v. FDA*, 2012).

Although direct advertisements for tobacco are, thus, restricted, characters smoking in fictional media depictions remains protected speech. Some advocacy efforts have sought to change this situation. For instance, the Centers for Disease Control (CDC; 2017) have released a statement directly attributing movie smoking as a cause of youth smoking. In 2016, a lawsuit was filed that sought to pressure the movie industry to automatically give any movie with

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smoking behavior an R-rating (Gardner, 2016). In defending the lawsuit, the lawyers involved stated “The only movie content that has ever been scientifically proven to kill kids by the hundreds of thousands is tobacco imagery.” The court was not convinced by this argument and the lawsuit was struck down. However, this commentary indicates that this field of research is being used by policy makers to claim not only that smoking in movies is a risk factor for adolescent smoking, but that it can be specifically attributed to specific deaths. The lawsuit alleged that 200,000 youths taking up smoking and 64,000 deaths each year could be attributed directly to movie smoking. These claims appear to be consistent with those of the CDC (2017) which claimed “Giving an R rating to future movies with smoking would be expected to reduce the number of teen smokers by nearly 1 in 5 (18%), preventing up to 1 million deaths from smoking among children alive today.”

Thus, policy makers, including the CDC are making broad causal claims regarding the impact of movie smoking and promising sweeping benefits by restricting the same. However, what evidence is available to support these claims?

### **Movie Smoking Research**

It is important to understand that the causal claims repeated by the CDC and other groups such as the US Surgeon General (US Department of Health and Human services, 2014) are based on data that is both correlational in nature and often extrapolated from the raw data rather than demonstrated within it. It is understandable that direct experimental data is not available: it would be both illegal and unethical to provide minors access to cigarettes as part of an experimental manipulation. This limitation alone makes it difficult to make causal attributions, when the majority of evidence on behaviors is correlational in nature. It is understandable that this state of affairs is frustrating insofar as it may be impossible to move beyond correlational

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data to make causal inferences for public health policy. However, wanting data to imply causal inferences does not make it capable of doing so, although in some cases, such as with smoking effects on mortality, overwhelmingly strong effect sizes bring policymakers as close as possible.

Most movie smoking studies are longitudinal or correlational epidemiological studies, with large sample sizes numbering in the thousands. Typically, such studies rely on self-reporting, with questionnaires geared toward teen's movie exposure as well as their self-reported tobacco use. Other variables such as socioeconomic status, parental environment and gender are routinely controlled. These designs are fairly basic (better reliance on multiple data reporters, controlling for genetics, etc., would improve upon these basic designs), but meet the general expectations of a good multivariate design (Savage, 2004).

That said, these designs may also experience some systematic problems. For instance, the self-report nature of the surveys involved may create false positive results due to both single-responder bias (Baumrind, Larzelere & Cowan, 2002) as well as mischievous responding. Single-responder bias can create false positives with both the predictor and outcome variable are provided by the same respondent, creating response sets. This can be worsened in situations in which the survey contents make the hypotheses of the study obvious, creating demand characteristics. This may occur whether or not the surveys ask about movie smoking specifically. Asking youth about movie watching habits then about their smoking habits, whether concurrently or longitudinally, raises the potential for demand characteristics. Some respondents may not remember the movie questions longitudinally (such as a year later), but others may, which may be all that is required for demand characteristics to cause spurious correlations. Mischievous responding occurs when participants purposefully answer falsely in order to give amusing responses. Past analyses have revealed that mischievous responding can

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cause false-positive results in social science research (Robinson-Cimpian, 2014). This is because mischievous responding tends to occur with more extreme answers, creating predictable correlations between extreme responding. For instance, Fan et al. (2006) document the case of false positives occurring between self-reported adoptee status and artificial limbs on one sample. Many studies, by asking about movie viewing, then smoking (either concurrently or a year later), likely set up demand characteristics that could cause spurious effects. Thus, analyzing results cautiously is warranted.

Some studies have examined general media use and smoking behavior in youth, with mixed results (e.g. Gutschoven, & Van den Bulck, 2005; Shi & Mao, 2011.) However, such studies may not always capture the key variable, namely actual exposure to smoking role models or normalization of smoking behaviors onscreen. Some studies use independent self-report measures, often involving teens noting the movies they watch and these being evaluated for smoking incidents either by researchers or teens themselves (e.g. Choi et al., 2012; Farrelly et al., 2012). However, many studies use a standardized approach developed at Dartmouth College (henceforth called the Dartmouth method) in which participants indicate whether they have seen a set of movies from a database that has been rated for smoking content using a standardized protocol (e.g. Wills et al., 2008).

Although some studies represent entirely independent databases, many published studies are pulled from a small number of large databases, indicating that many published articles are not independent, drawing results from the same samples. The first of these is a nationally representative sample of 6,522 youth (henceforth called the 6,522 study, e.g. Wills et al., 2008). The second of these are multiple articles drawn from samples of schools in New Hampshire and Vermont. These samples, have three cohorts, one from roughly 1993 (Sargent et al, 2007), one

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from 1999 (Dalton et al., 2009) and one from roughly 2012 (Primack et al., 2012). Although each cohort is independent, more than one article has been drawn from each of these cohorts. Henceforth these cohorts are referred to as the NH/VT studies. The third is a large sample of over 16000 youth drawn from six European Countries (6 Countries, e.g. Morgenstern et al., 2013). The fourth is a sample of youth from Schleswig-Holstein (e.g. Sargent & Hanewinkel, 2009). Articles from these four sets of data make up the wealth of articles in this field with comparatively few published articles independent of these four sources of data.

### **The Current Study**

Studies of movie smoking and youth smoking behavior have had an influential impact on public policy claims about movie smoking effects. These claims have been extrapolated to government reports attributing movie smoking to specific estimated deaths. However, reasonable concerns remain about the quality and effects seen in the research evidence base. Thus, the time is right for a methodological and meta-analytic review.

## **Methods**

### **Selection of Studies**

Identification of relevant studies involved a search of the PsycINFO, MedLine and Digital Dissertations databases using the search terms ("movie\*" OR "media" OR "television") AND ("smok\*" OR "cigarette\*") AND (Youth OR adolescent\* OR child\*) as subject searches. In addition, recent reviews of the movie smoking literature were examined for articles that may have been missed in the literature search. Included studies had to meet the following criteria:

1. Each study had to measure the influence of some form of media on an outcome related to smoking behavior. Outcomes typically included initiation of smoking or frequency of smoking. As our analysis was concerned with behavioral

outcomes, studies which looked at smoking attitudes or intent to have smoking in the future were not included. Media variables generally involved smoking seen in movies or television. General time spent on media was not included as a predictor variable given such studies had the potential to underestimate effects if media exposure did not capture smoking in media exposure.

2. Each study had to present statistical outcomes or data that could be meaningfully converted into effect size “ $r$ ”.
3. Participants in the study had to be below age 18 at least at Time 1 (in longitudinal analyses). Longitudinal analyses that extended into adulthood were included so long as the initial assessment took place during childhood or adolescence.
4. A given sample was included only once in the meta-analyses to maintain independence. Some samples, including longitudinal studies, may produce multiple publications, but only one such study was included in the current analysis. This became a particularly pressing issue for this field given the plethora of articles produced from a relatively small number of databases. If a database produced multiple independent cohorts (i.e. independent samples) each cohort was included as an independent sample. In each case, the most conservative estimates of effect were included.

The initial search (carried out in October, 2016) returned approximately 448 hits, the majority of which were either non-empirical, considered general media use rather than media smoking specifically or were with college student samples or otherwise did not meet the inclusion criteria above. Employing the inclusion criteria, the final search netted 32 published papers. However, only six of these did not come from the four big databases mentioned earlier.

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Allowing for three independent cohorts from the New Hampshire/Vermont data base, this left 12 independent controlled effect size estimates, with total participants  $n = 70,778$ . Each article was assessed by two raters, each blinded to the other's ratings for inclusion. Krippendorff's alpha (a method of calculating inter-rater reliability that allows for categorical data) reliability on the inclusion decision was .84, with discrepancies then resolved by consensus of all researchers. This process was completed by May 2017. The list of studies along with effect size estimates is presented in an online table at:

<http://www.christopherjferguson.com/Moviesandsmokingdata.xlsx>. Details on data extracted from each article are described below under effect size estimates and moderator analyses. A PRISMA chart is provided as Figure 1.

[Figure 1 about here]

### **Effect Size Estimates**

In line with recent innovations related to meta-analyses of multivariate analyses, the current paper makes use of effect sizes in the metric of  $r$  which are based upon multivariate analyses resulting in standardized regression coefficients (betas). Many meta-analyses in prior years had relied upon bivariate  $r$  in the hopes that using  $r$  rather than betas would result in more homogeneous analyses. However, due to the fact that most studies vary widely in measurement, analytics and sample, recent analyses have revealed that bivariate  $r$ s are no more homogeneous than are betas (Ferguson, 2015; Furuya-Kanamori & Doi, 2016)), thus removing the primary argument for meta-analyses relying on  $r$ . By contrast, reasons for a preference for betas in meta-analysis are numerous, primarily given the concern that bivariate  $r$  may return spuriously high effect size estimates that do not reflect real correlations once important factors are controlled (Pratt et al., 2010; Savage & Yancey, 2008). Use of betas make more sense theoretically, given

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that most multivariate analyses include theoretically relevant controls. As such, this study employs betas as effect size estimates.

In cases where articles presented more than one effect size estimate, they were aggregated for an average effect size. More critically for the current article, in numerous cases, a single dataset may have produced multiple overlapping articles. This was particularly true for the four big datasets addressed earlier in the paper. In these cases, to preserve independence, each dataset was included only once in the meta-analysis unless (as the NH/VT studies did) they included multiple independent cohorts. Some manuscripts presented multiple competing statistical models with different effect size estimates, particularly for multivariate analyses. When this occurred, the most conservative model was used as the effect size estimate for the controlled analyses. Given the question of how much variance remains for movie or media smoking effects, once other factors are well-controlled, this approach was viewed as valuable.

Several moderators were considered as potentially important for the current article. Study year was considered as a moderator along with the age of participants. Whether articles appeared to endorse government regulation or censorship or other public policy efforts likely to restrict speech was also coded. This was considered one potential test for researcher biases, which have proven to influence effects sizes in other realms of media effects (e.g. Ferguson, 2015; Ferguson, Nielsen & Markey, 2017). Studies were also coded for whether they had used the Dartmouth method for analyzing movie smoking. Potential moderators regarding the influence of demand characteristics and reliability/mischievous responding checks were also considered, but there appeared to be no variance among studies on either of these issues (i.e. no studies attempted to control for these issues). More positively, studies uniformly made attempts to control for reasonable 3<sup>rd</sup> variables.

## Analysis

The Comprehensive Meta-Analysis (CMA) software program was used to fit random effects models. The potential for publication bias was assessed using the Tandem Procedure which looks for concordance among several funnel-plot related tests for bias. This procedure is an empirically demonstrated, conservative estimating procedure for assessing publication bias, with low Type I error rates.

## Results

### Main Results

Across all studies, using random effects modeling, results suggested a statistically significant association between movie/media smoking and teen smoking behaviors ( $p < .001$ ). However, this effect size was trivial in nature ( $r = .076$ ; 95% CI = .039, .113). Heterogeneity among the studies was high  $Q(11) = 246.625$ ,  $I^2 = 95.54$ . A forest plot is provided in Figure 2. The Tandem Procedure did not suggest publication bias was a major issue for this field.

[Figure 2 about here]

### Moderator Analyses

Meta-regression suggested a significant negative correlation between study year and effect size, such that effect sizes declined in more recent studies ( $z = -9.81$ ;  $p < .001$ ). Likewise, age at first assessment was inversely related to effect size ( $z = -9.31$ ;  $p < .001$ ). Endorsement of censorship was unrelated to effect size, although effect sizes were higher for studies using the Dartmouth method for movie smoking ( $r = .111$ ) than those using other methods ( $r = .029$ .) It is worth noting that an inverse correlation existed among study year and Dartmouth method ( $r = -.537$ ) which may indicate a decline effect (Schooler, 2011), that newer studies are not replicating earlier studies using the Dartmouth method.

### **Odds Ratio Analyses**

The original planned analyses were followed by exploratory reanalysis using odds ratios (ORs) as effect sizes. Such analyses may provide greater illumination regarding the increased risk for smoking associated with movie smoking exposure. OR data were available for 10 of the included studies. Meta-analysis using random effects model revealed a small increase in risk associated with movie smoking (OR = 1.371, 95% CI: 1.189, 1.581). This value is below that suggested by Ferguson (2009) as a minimal threshold for clinical interpretation, as effects in this range are highly prone to false positive results.

### **Methodological Issues**

Two issues had been considered as potential moderators but could not be analyzed given the ubiquitous nature of the problem areas throughout the field. First, potential demand characteristics appear to be nearly universal in the field given the obviousness of the hypotheses given the degree to which smoking and/or media exposure questions in predictor and outcome variables are closely paired. It is likely that any reasonable youth could ascertain the purpose of these studies and have their responses influenced. Similarly, studies in the current sample generally failed to include checks for unreliable or mischievous responding. Both of these problems, which could result in spurious effect sizes, appear to be endemic to the literature on this topic.

### **Discussion**

Recent years have seen increasingly concerns regarding the impact of movie and other media smoking on teen smoking behaviors. Although such evidence is correlational nature, it has led to court cases, calls for public policy and attributions to specific numbers or proportions of deaths (e.g. CDC, 2017; Gardner, 2016; US Department of Health and Human services, 2014).

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Many anti-smoking advocates imply that the research evidence is conclusive. The current meta-analysis examined results across multivariate studies examining the impact of smoking in media on teen smoking in real life. Although due to the high power of meta-analysis, a statistically significant effect was found, the size of this effect is trivial and more parsimoniously explained by systemic methodological flaws in the research rather than real effects, or perhaps consistent with the “crud factor” view of trivial effects in social science research (Meehl, 1991). As such, the current body of literature is unable to support the contention that exposure to smoking in media is a risk factor for smoking in real life. A more generous interpretation would be that there is a real overlap in variance (which may or may not be causal) between movie and real-life smoking of less than half a percent. However, this figure does not translate to proportions of real deaths attributable to movies (i.e. it is not possible to conclude that half a percent of all habitual smoking would be prevented were movie smoking eliminated.) It is difficult to translate such a small effect into coherent policy recommendations. This small overlapping variance remains correlational, even if real. It may be that genetic predisposition, or other factors, may explain such a small proportion of variance, meaning that policy directed at such a correlation may have little impact. This is the danger in translating correlational research into prescriptive, causally-based policy. We observe that courts have been skeptical of media effects claims and blaming popular culture for societal ills tends to come at a credibility cost. If we assume that there is some tiny but real overlap in predictive variance between smoking in movies and real-life smoking, we leave it to policy makers to decide if instituting policy on this matter is worth the potential costs to credibility, time and resources.

In recent years it has become common for scholars to argue that trivial effects can be meaningful when extended over large populations. However, this line of argument is

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problematic for several reasons. First, trivial effects are often not real. That is, they are often explained better as artifacts of social science designs, noise or the “crud factor” of social science (Meehl, 1991). Second, effects which are produced as an element of variations within individuals over time or between groups of individuals cannot be extrapolated to populations. That is, knowing how much variance in a given individual’s behavior can be explained by a predictor does not tell us what proportion of the population is influenced to some detrimental degree (i.e. an  $r^2$  of say .01 does not mean that 1% of a population will be direly influenced.) Third, it cannot be assumed that effects observed over a given time frame will accumulate and become more powerful over longer time frames, particularly when observed longitudinal effects are weak and no evidence supports claims of increasing effects over time. Last, such claims were often based on comparisons to important medical findings which have since been discredited as statistically flawed (Ferguson, 2009.) Thus, we remain confident that the best current interpretation of the evidence is that exposure to movie smoking has little to no impact on smoking in the real world.

The small effect size ( $r = .076$ ) found between movie smoking exposure and smoking behaviors raises two issues. First, the practical significance of such results has been disputed in other areas related to media effects (Block & Crain, 2007; Ferguson, 2009). Typical arguments defending trivial effects tend to compare them to medical effect sizes, and argue that small effects spread throughout a population can be meaningful. However, these arguments have been criticized as problematic. First, it has been more recently recognized that the comparisons with medical effects depended on flawed statistics that mistakenly deflated the effect size of the medical effects (Block & Crain, 2007; Ferguson, 2009). Further, the types of analyses that produce within-participant variable effects cannot be extrapolated to populations in such a way

as to imply that a certain percentage (or any) of that population will experience a critical outcome.

The second issue is that effect sizes of such small size may be artifacts of the study methodology, demand characteristics, single-responder bias, researcher expectancy effects and researcher degrees of freedom, representing Type I error results. Thus, considerable caution is advised when interpreting effect sizes in the trivial range as indicating any kind of real effect (Ferguson, 2009).

Of particular concern is the failure of studies in this field to adequately consider how response bias may have influenced the relatively tiny effect sizes seen in most studies. In many respects, studies in this field have impressive designs, even compared to other media effects fields such as video game violence or thin-ideal media. Scholars had developed an interesting standardized approach for assessing movie smoking (the Dartmouth method), and generally used sophisticated multivariate designs, which are to be commended. However, results from these studies are difficult to interpret given the absence of reliability checks and the demand characteristics of almost all studies. Study authors also appear relatively unconcerned about the trivial nature of effect sizes found in studies, tending to focus on statistical significance as a binary outcome rather than engaging in a more cautious interpretation of effect sizes. Given the systematic methodological weaknesses of the extant studies, the most parsimonious explanation for these trivial effects is noise rather than real effects in the population.

The inverse correlation between effect size and study year could suggest that this field is experiencing a decline effect (Schooler, 2011). In other words, early studies may have provided unusually high effect sizes that are not necessarily replicating well in more recent studies.

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Involving greater preregistration of study design and analyses plans could help the field distinguish real effects versus those which might originate from researcher bias.

That the Dartmouth method of analyzing movie smoking produced higher effect sizes is another potential issue of interest. From a correlational study, it unclear whether this indicates that the Dartmouth method is superior to other methods or has a particular flaw. Complicating this observation is the fact that this method is conflated with specific research groups that are highly prolific in this field. Indeed, the field of media smoking appears to be particularly narrow and dominated by a small group of researchers. One single researcher appeared as coauthor on 25 of the 32 (78.1%) individual articles in this field. For any research field to be so conclusively dominated by a single individual, no matter how they may act in good faith, is an issue of concern. Such an individual or group of individuals can have an outweighed influence on the field, both through published papers and via the peer review process such that the field reflects that individual's research and perspective rather than an objective truth. That is particularly true in the absence of preregistration of studies.

Thus, it is difficult to say whether the Dartmouth method studies are necessarily better or worse in quality than other studies, only that, at least in terms of effect size, the Dartmouth method is has not replicated across other approaches. We see two possible explanations. First, the Dartmouth method really is superior to other approaches. In this case it should be adopted as a field standard. Second, the Dartmouth method is conflated with scholars who are particular active, in good faith, in advocating for movie smoking effects and does not replicate well. The issue of researcher biases related to spuriously high effects has been documented in other media effects fields (Ferguson, 2015). The latter explanation may be consistent with the decline effect observed in more recent studies. Unfortunately, neither the Dartmouth method nor independent

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studies employed the types of methods or controls we might have hoped to see in regard to limiting potentially spurious effects. We also suspect that this field has not yet developed a healthy atmosphere of skeptical “tire-kicking” which would allow for a testing of whether researcher expectancies (both pro-effects and more skeptical) are related to effect sizes. An analysis of preregistered studies versus those that are not preregistered could also be revealing.

### **Suggestions for Improvement**

#### **Open Science**

At present, the field is limited by significant potential for false positive results, not only given the presence of demand characteristics, but also due to the potential for researcher expectancy biases given our observation the field (like many in media effects) is arguably in confirmation rather than falsification mode. Opinions reinforcing government regulation, censorship or other public policy positions potentially restricting speech were endorsed in articles stemming from 7 of the 12 datasets included in the current analysis (58.3%). This suggests a fairly heavy degree of researcher involvement in advocacy efforts. This can create false positive results due to researcher degrees of freedom. One means for reducing this potential is through the use of preregistration, particularly of measures and data analysis plans. Preregistration may help the field avoid the undue influence of researcher expectancy effects which currently appear to be significant.

#### **Demand Characteristics and Other Sources of Bias**

The field needs to do more to reduce sources of bias in survey methodology. At present, bias due to demand characteristics, single-responder bias and mischievous or unreliable responding appear to be significant. Demand characteristics can be reduced by including distractor questionnaires in the methodology. Manipulation checks for unreliable or mischievous

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responding are needed. Single-responder bias can be reduced by getting other-report data, although we acknowledge this may be more difficult for this field as, undoubtedly, many teens hide smoking behavior from parents.

The current analysis did not find evidence for publication bias, although this observation is tempered by several caveats. First, the Tandem procedure is a very conservative estimator for publication bias and Type II error is possible. Second, funnel-plot based publication bias measures are best at detecting bias among smaller, larger effect studies. The current field comprises mainly large-n, small effect studies. Variation between such studies is often minimal, reducing the effectiveness of funnel-plot based analyses. Third, effect sizes across studies were universally low, providing little power for funnel-plot based analyses. Overall, publication bias among large-n, small effect studies can be more difficult to detect. Thus, publication bias can't be entirely ruled out. Few studies put much effort into interpretation of effect sizes which raises one concern that achieving "statistical significance" remains a primary benchmark rather than cautious interpretation of effect sizes.

### **Avoiding Alarmist Statements**

Perhaps the greatest concern we had in reviewing this field of study was the degree to which the field has been making alarmist claims that are unsupported by the current data. Most concerning have been claims that specific deaths can be directly attributed to movie smoking. These attributions make the errors both of mistaking correlation from causation, and extrapolating beyond the available data. To be frank, we were surprised to see such unscientific claims being made, particularly by organizations such as the Centers for Disease Control and the US Surgeon General. These claims are deeply misleading to the general public and run the risk of doing great damage to the credibility of these organizations. These claims should be removed

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from documents produced by these groups for the public and an improved system of peer review implemented to reduce the potential for further such problematic claims.

Scholars, likewise, are advised to be cautious in making attributional claims about movie smoking as a cause for real-life smoking. At present, data is equivocal on even the reliability of a correlation. Unfortunately, as with other media effects field, scholars may be tempted to speak beyond the available, limited data. Ultimately, claims that go beyond the data do a disservice to the public. Failing to inform the public regarding the weak nature of effect sizes or methodological limitations reduces the informed nature of any decisions on policy made based on such data. Failing to be entirely forthcoming regarding the nature of the data can also risk damaging the credibility of a research field should its limitations be pointed out by third parties or should earlier results prove to be difficult to replicate under more rigorous conditions as has happened to multiple fields in social science. We understand that finding the balance between caution in making claims of harm, and good-faith efforts to be of service to the public is difficult to find. However, we observe that within media effects fields, the rush to warn has too often come too quickly, and at the cost of honest communication of research data to the public and policy makers. We believe a more cautions and honest reckoning of the data will be of best service to all.

### **Public Policy**

At present, it does not appear that the current pool of data is sufficient to provide the scaffolding for public policy efforts directed at the movie industry. This may change with further, refined studies. Until such time, it is not recommended that policy changes, particularly those which may impact free speech rights, be altered based on the current pool of research results.

### **Conclusions**

The results of this meta-analysis do not support the claim that depictions of smoking in movies impact the smoking behavior of youths. The twelve datasets that comprise the meta-analysis yielded a near-zero effect size. While the observed correlation was statistically significant it was too small to support the hypothesis that a practically significant relationship might exist between exposure to smoking in movies and youth smoking.

Reducing tobacco consumption is a worthy cause, but film censorship does not appear to be a viable course of action. Correlation does not imply causation, but at the same time causation does require correlation. Future research might uncover a causal relationship that this study did not, but we remain highly skeptical.

### **Authorship**

CJF conceptualized this study. CJF and RMLN were involved in study selection and initial coding. CJF and PM were involved in effect size extraction and analysis. All authors were involved in writing the final manuscript and approve of the final submitted manuscript.

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**Figure Caption**

Figure 1: PRISMA diagram

Figure 2: Forest plot of included studies

Figure 1:



### PRISMA 2009 Flow Diagram

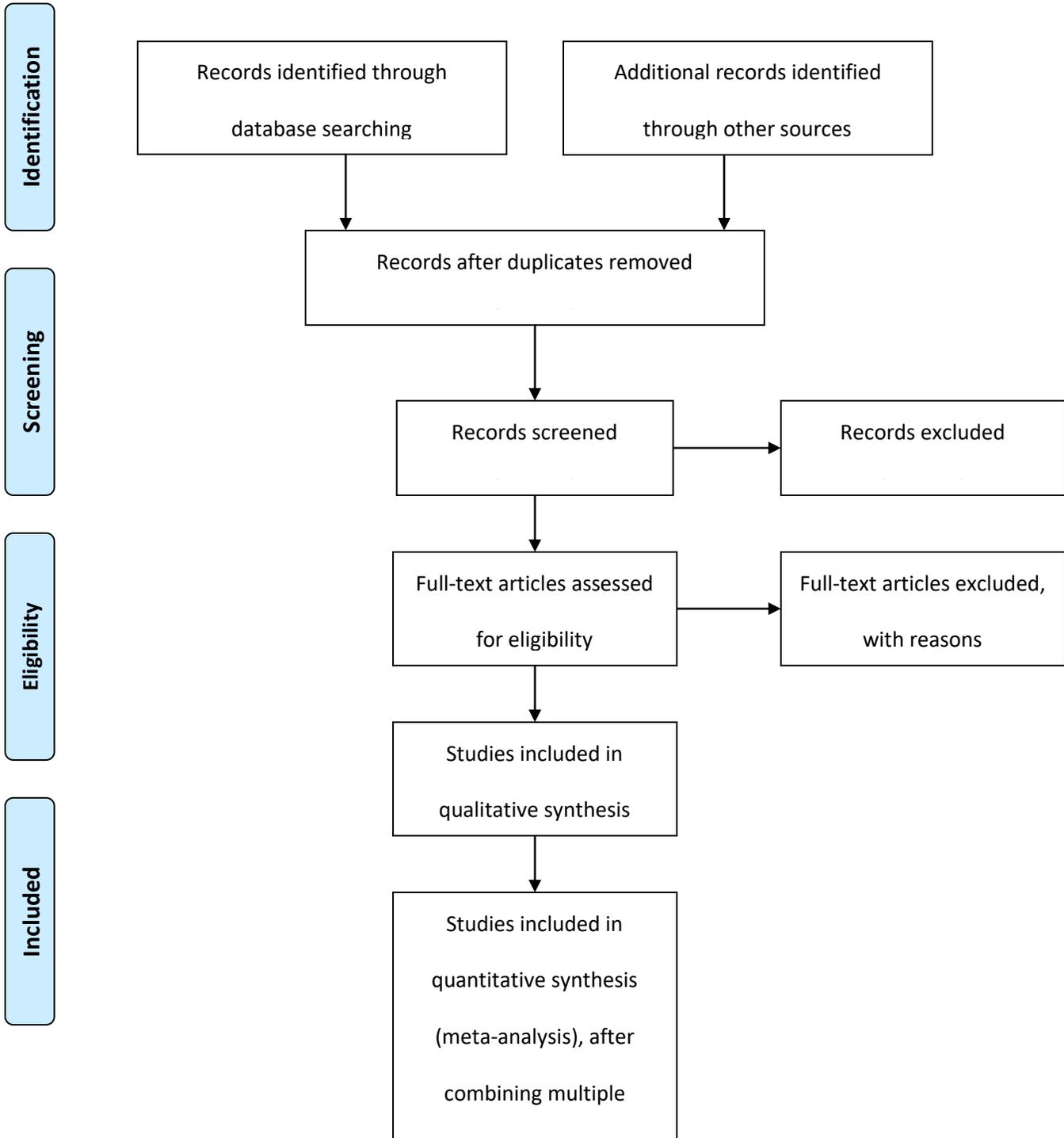


Figure 2:

