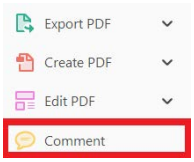
WILEY

USING e-ANNOTATION TOOLS FOR ELECTRONIC PROOF CORRECTION

Required software to e-Annotate PDFs: Adobe Acrobat Professional or Adobe Reader (version 11 or above). (Note that this document uses screenshots from Adobe Reader DC.)
The latest version of Acrobat Reader can be downloaded for free at: http://get.adobe.com/reader/

Once you have Acrobat Reader open on your computer, click on the Comment tab (right-hand panel or under the Tools menu).



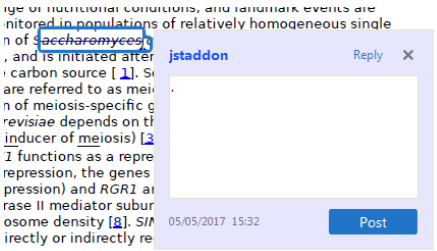
This will open up a ribbon panel at the top of the document. Using a tool will place a comment in the right-hand panel. The tools you will use for annotating your proof are shown below:

1. Replace (Ins) Tool – for replacing text.

Strikes a line through text and opens up a text box where replacement text can be entered.

How to use it:

- Highlight a word or sentence.
- Click on .
- Type the replacement text into the blue box that appears.

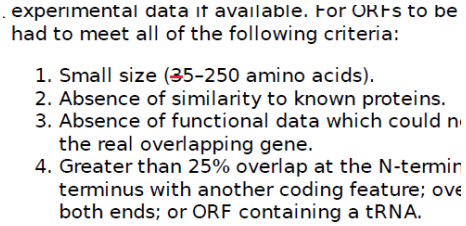


2. Strikethrough (Del) Tool – for deleting text.

Strikes a red line through text that is to be deleted.

How to use it:

- Highlight a word or sentence.
- Click on .
- The text will be struck out in red.

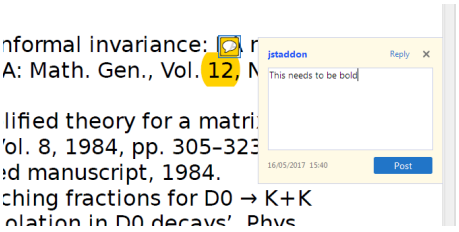


3. Commenting Tool – for highlighting a section to be changed to bold or italic or for general comments.

Use these 2 tools to highlight the text where a comment is then made.

How to use it:

- Click on .
- Click and drag over the text you need to highlight for the comment you will add.
- Click on .
- Click close to the text you just highlighted.
- Type any instructions regarding the text to be altered into the box that appears.

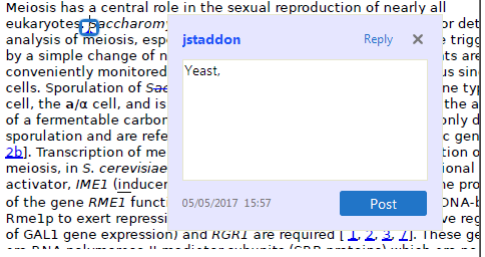


4. Insert Tool – for inserting missing text at specific points in the text.

Marks an insertion point in the text and opens up a text box where comments can be entered.

How to use it:

- Click on .
- Click at the point in the proof where the comment should be inserted.
- Type the comment into the box that appears.



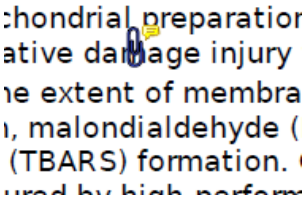
5. Attach File Tool – for inserting large amounts of text or replacement figures.

Inserts an icon linking to the attached file in the appropriate place in the text.

How to use it:

- Click on .
- Click on the proof to where you'd like the attached file to be linked.
- Select the file to be attached from your computer or network.
- Select the colour and type of icon that will appear in the proof. Click OK.

The attachment appears in the right-hand panel.

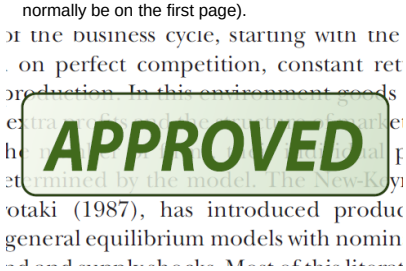


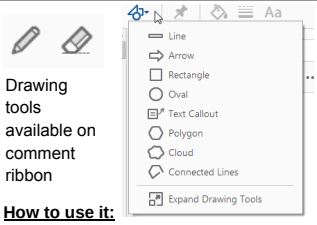
6. Add stamp Tool – for approving a proof if no corrections are required.

Inserts a selected stamp onto an appropriate place in the proof.

How to use it:

- Click on .
- Select the stamp you want to use. (The Approved stamp is usually available directly in the menu that appears. Others are shown under *Dynamic*, *Sign Here*, *Standard Business*).
- Fill in any details and then click on the proof where you'd like the stamp to appear. (Where a proof is to be approved as it is, this would normally be on the first page).

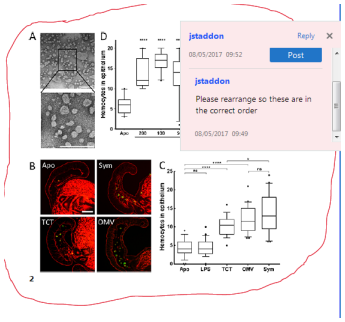




Drawing tools available on comment ribbon

7. Drawing Markups Tools – for drawing shapes, lines, and freeform annotations on proofs and commenting on these marks.

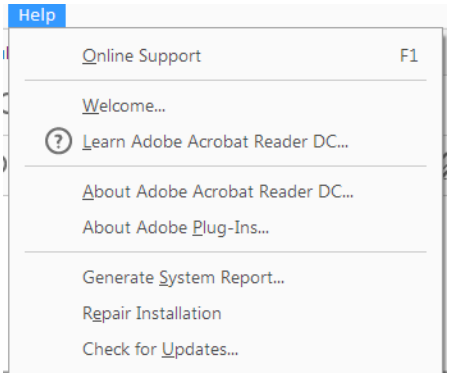
Allows shapes, lines, and freeform annotations to be drawn on proofs and for comments to be made on these marks.



How to use it:

- Click on one of the shapes in the Drawing Markups section.
- Click on the proof at the relevant point and draw the selected shape with the cursor.
- To add a comment to the drawn shape, right-click on shape and select *Open Pop-up Note*.
- Type any text in the red box that appears.

For further information on how to annotate proofs, click on the Help menu to reveal a list of further options:



Author Query Form

WILEY

Journal: SLTB

Article: 12517

Dear Author,

During the copyediting of your manuscript the following queries arose.

Please refer to the query reference callout numbers in the page proofs and respond to each by marking the necessary comments using the PDF annotation tools.

Please remember illegible or unclear comments and corrections may delay publication.

Many thanks for your assistance.

Query reference	Query	Remarks
1	**AUTHOR: Please provide qualifications of author "Christopher J. Ferguson".**	
2	**AUTHOR: Please confirm that given names (blue) and surnames/family names (vermilion) have been identified correctly.**	
3	**AUTHOR: Please check that authors and their affiliations are correct.**	
4	**AUTHOR: Please supply an abstract for this paper, structured using the following headings: Background, Aims, Materials & Methods, Results, Discussion, Conclusion.**	
5	**AUTHOR: Please check the placement of the statement "An early version . . . in April, 2018" is it okay.**	
6	**AUTHOR: Figure 1 was not cited in the text. An attempt has been made to insert the figure into a relevant point in the text - please check that this is OK. If not, please provide clear guidance on where it should be cited in the text.**	
7	**AUTHOR: Please suggest whether the term "pre/post analyses" can be changed to "before/after analyses" in the sentence "These typically involved pre/post analyses. . .. . .viewed within the community".**	

8	**AUTHOR: Please suggest whether the term "pre/post" can be changed to "before/after" in the sentence "As noted above, aggregate designs. . . . . suicide-themed television show".**	
9	**AUTHOR: The sentence "As noted results for this type of analysis are highly divergent" seems to be incomplete. Please check.**	
10	**AUTHOR: Please suggest whether the term "non-empirical" can be changed to "nonempirical" as per the dictionary in the sentence "The initial search (carried out in September 2017). . . . . did not meet the inclusion criteria above".**	
11	**AUTHOR: Ferguson, 2015 has not been included in the Reference List, please supply full publication details.**	
12	**AUTHOR: Please suggest whether the term "non-significant" can be changed to "nonsignificant" as per the dictionary in the sentence "The overall effect size was near zero (r = .034 with correction for publication bias) and non-significant."**	
13	**AUTHOR: Please suggest whether the term "pre-post" can be changed to "before and after" in the sentence "It may be worth examining better ways to analyze pre-post aggregate data based on raw number of suicides".**	
14	**AUTHOR: Please check the hierarchy of heading levels.**	
15	**AUTHOR: Please provide the volume number, page range for reference Thompson et al. (2018).**	

© 2018 The American Association of Suicidology
DOI: 10.1111/sltb.12517

13 Reasons Why Not: A Methodological and Meta-Analytic Review of Evidence Regarding Suicide Contagion by Fictional Media

CHRISTOPHER J. FERGUSON, XXX

For decades, policymakers and suicide prevention advocates have questioned whether exposure to media with suicide themes, whether television, movies, or music, could increase suicide risk among youth. To date, no clear picture has emerged, with data inconsistent. Two broad forms of data consider the issue, namely society-level aggregate data, and data from smaller correlational and experimental studies. The current article examined the evidence for suicide contagion by fictional media with a methodological and meta-analytic review. Results suggest that current data do not support the theory that suicide contagion by fictional media occurs. It is recommended that individuals exercise caution in public statements linking suicide-themed fictional media to suicide contagion as data may not be able to support such claims.

In the summer of 2017, the release of the popular and critically praised television show *13 Reasons Why* caused significant public consternation. The television show portrays the suicide and aftermath of a teenage girl who documents her motives for the suicide in a series of messages to other teens. Many members of the mental health and suicide prevention community criticized the show for allegedly glorifying suicide and increasing suicide risk among teen viewers. The Society for the Prevention of Teen Suicide (2017) released a statement claiming, "Unfortunately, the media tends to glamorize and sensationalize suicide." The National Association of School Psychologists (2017) released a statement noting, "We do not recommend that vulnerable youth, especially those who have any degree of suicide ideation, watch this series. Its powerful storytelling may lead impressionable viewers to romanticize the choices made by the characters and/or develop revenge fantasies." Numerous other clinicians, educators, and suicide prevention advocates followed suit, expressing concerns about the show (Figure 1).

Concerns about *13 Reasons Why* mirror historical apprehensions regarding the potential impact of suicide-themed media on youth. A family brought a lawsuit against performer Ozzy Osbourne in the 1980s, claiming his song *Suicide Solution* (which did not, in fact, advocate suicide) contributed to their son's suicide. The lawsuit was dismissed in 1988 (History.com, 2018). Other 1980s rock and metal bands such as *Judas Priest* also faced lawsuits or controversy related to alleged suicide-themed lyrics (Rohter, 1990). All these

CHRISTOPHER J. FERGUSON, Stetson University, DeLand, FL, USA.

An early version of these analyses was presented at the American Psychological Association's Technology, Mind and Society conference in April, 2018.

Address correspondence to Christopher J. Ferguson, Department of Psychology, Stetson University, 421 N. Woodland Blvd., DeLand, FL 32729; E-mail: CJFerguson1111@aol.com

UNCORRECTED PROOF

SLTB | Journal Code | 12517 | Manuscript No. | WILEY | Dispatch: 25.9.18 | No. of pages: 8 | CE: Durga Devi S | PE: Muthamilselvi S.

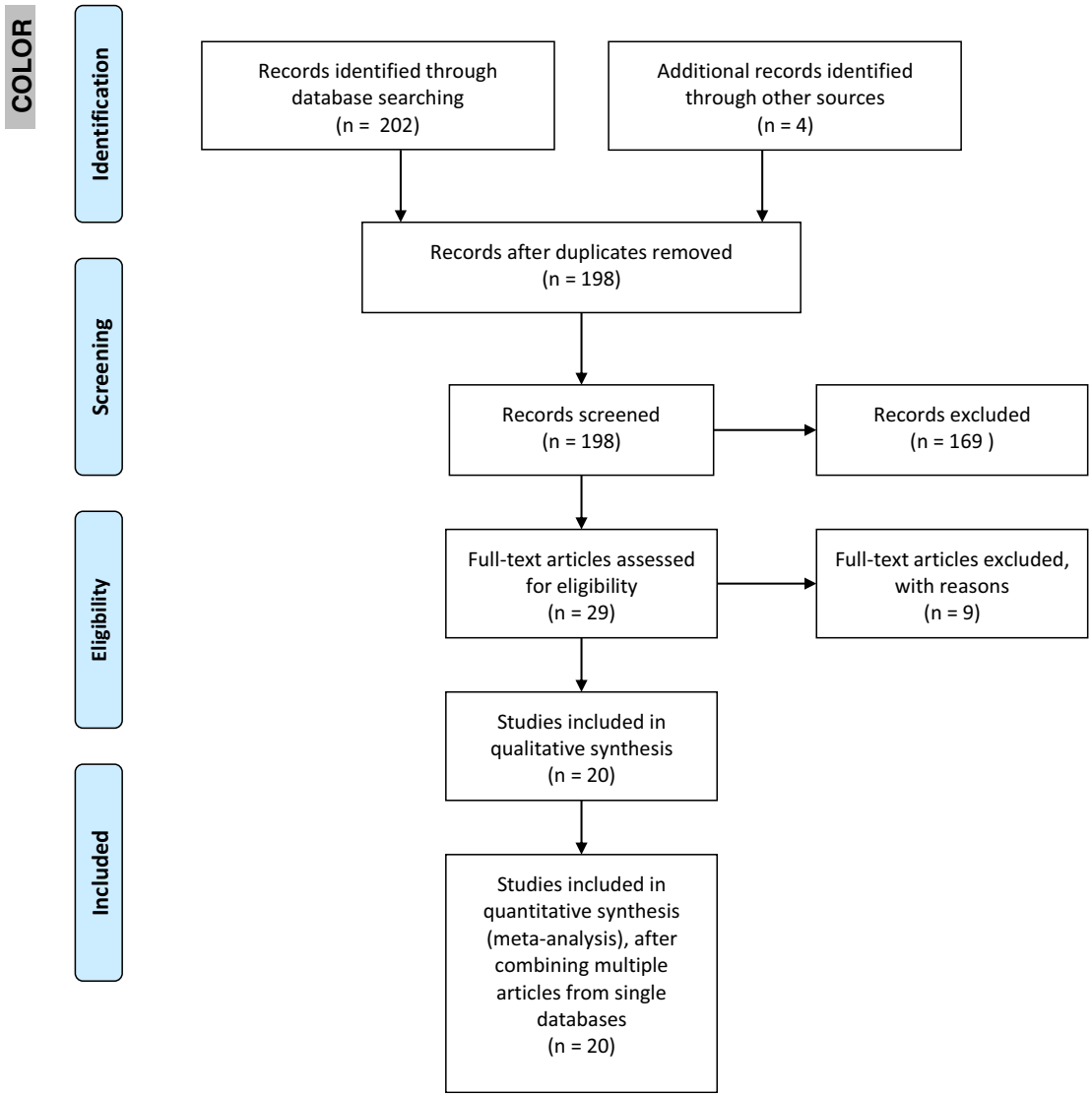


Figure 1. PRISMA Flow Diagram.

Proof

phenomena appear related to the Werther Effect, named after the lead character of Goethe's 1774 novel that was thought to have provoked a series of copycat suicides throughout Europe (Hittner, 2005).

Nonetheless, the majority of these societal-level concerns have progressed mainly through speculation. For instance, most of the public statements by professional groups, advocacy groups and individual counselors, educators, and advocates were not connected to a larger base of empirical data. This leaves open the question as to the degree that concerns about suicide contagion by fictional media are connected to peer-reviewed data.

Studies on suicide contagion by fictional media can be conceptualized as coming from two distinct evidence bases, each with their benefits and pitfalls. The first of these are population aggregate studies. These typically involved pre/post analyses of suicide

rates in a municipality before and after a particular television show with suicide themes is viewed within the community. The second are more traditional psychological studies, most of which may be survey based, asking participants about their history viewing certain shows or listening to certain songs and correlating these with surveys of mood or suicidal thoughts. Some studies may employ short-term experiments to examine how randomized exposure to different media influences mood or suicide acceptance. In the following section, each of these forms of studies is briefly reviewed.

Aggregate designs

8 As noted above, aggregate designs examine suicide rates pre/post the airing of a particularly popular suicide-themed television show. Such research designs appear to have been particularly popular during 1980s when the limited availability of multiple channels likely made it more realistic to isolate single television shows. Such designs have obvious appeal as they appear to be able to examine the direct impacts of a particular show. They also avoid demand characteristics that may be common for survey-based research.

That said, such designs have significant limitations. First, particularly in an environment in which pressure to publish findings is prevalent, false-positive results could reify ecological fallacies. Simkin, Hawton, Whitehead, Fagg, and Eagle (1995) note that such studies cannot affirm that suicide victims actually had seen the show in question and that this is an assumption. Unsound methodologies might accidentally create false-positive results based on fairly random fluctuations in suicide rates. Second, the description of analyses in many of the older studies is arguably vague. For example, Gould and Shaffer (1986) appear to use independent sample t tests (as well as Mann–Whitney *U* alternatives) to assess before/after differences, treating time blocks rather than individuals as the unit of analysis. This statistical analytic approach could create a higher amount of unreliability in the analyses, which may explain why effect sizes in this field tend to be particularly heterogeneous.

As noted results for this type of analysis 9 are highly divergent. Some studies appear to document fairly strong suicide contagion effects (e.g. Gould & Shaffer, 1986; Stack, Gundlach, & Reeves, 1994), whereas others have claimed no effects for fictional media suicide contagion (Phillips & Paight, 1987; Simkin et al., 1995). Thus, a consistent body of evidence does not appear to emerge from this group of studies. Interestingly, after the mid-90s, this type of study appears to have declined altogether, perhaps related to increased difficulties in isolating the effect of single media examples, given increased diversification of the media environment.

Traditional psychology studies

More traditional studies examine limited samples of individuals, often by conducting surveys of media use and suicidal thoughts or depression. Although the topic matter lends itself well to survey-based research, some experiments may consider short-term impact on milder variables such as positive beliefs about suicide or mood (Till et al., 2011). Such studies may naturally vary quite widely in quality. As one major issue, the pairing of questions related to media to questions related to mood or suicide may lead to hypothesis guessing/demand characteristics and false-positive results. It is possible, particularly in correlational studies, that bivariate correlations may be a poor index of actual effect sizes due to potential third variables. In most cases, theoretically important third variables may explain any bivariate correlation between two variables and, as such, theoretically derived multivariate analyses are often perceived as more valuable than are bivariate analyses (Furuya-Kanamori & Doi, 2016; Savage & Yancey, 2008). For example, it may be possible that females harbor more suicidal thoughts and greater depression and are also more drawn to storylines such as those in *13 Reasons Why*. Thus, controlling for gender effects would be critical. Other variables

UNCORRECTED PROOF

related to personality, such as neuroticism, family environment, and even genetics (e.g. Schwartz & Beaver, 2016), could be important to consider.

As with aggregate studies, evidence from traditional psychological studies appears to be mixed. Some studies do show correlations between suicide-themed media and increased suicide risk or decreased mood (Martin, Clarke, & Pearce, 1993; Stack, Kral, & Borowski, 2014). However, other studies have not replicated this effect (Lacourse, Claes, & Villeneuve, 2001; Till et al., 2011). Thus, as with aggregate data, a descriptive look at the evidence base does not reveal a consistent set of evidence for effects, one way or another.

The current study

Given that individual studies demonstrate considerable heterogeneity in regard to the degree that they provide evidence for suicide contagion by fictional media, it may be worth examining the issue from a meta-analytic perspective. Although meta-analysis does not necessary negate heterogeneity between studies, it can sometimes provide a sense for the trajectory of a field as well as whether some methodological factors may influence effect sizes. To date, this field appears not to have been subjected to meta-analytic review. Therefore, the current article will provide a methodological and meta-analytic summary of the field of fictional media suicide contagion to date.

METHODS

Selection of studies

Identification of relevant studies involved a search of the PsycINFO, MedLine, and Digital Dissertations databases using the search terms ("film* OR movie* OR television OR music") AND (suicide)) AND (Youth OR adolescent* OR child*) as subject searches. In addition, recent reviews of the fictional media suicide contagion 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 suicide behavior, suicide ideation, or depressed mood. Media variables generally involved suicide-themed television and movies as well as heavy metal music with suicide themes. 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 suicide themes in media specifically.
2 Each study had to present statistical outcomes or data that could be meaningfully converted into effect size "*r*."
3 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. In each case, the most conservative estimates of effect were included.

The initial search (carried out in September 2017) returned approximately 202 hits, the majority of which were either non-empirical, considered general media use rather than media suicide specifically or otherwise did not meet the inclusion criteria above. Employing the inclusion criteria, the final search netted 20 published papers. However, it is worth noting that two papers utilized the same sample, however, with different forms of media, namely television versus heavy metal (Till, Tran, Voracek, & Niederkrotenthaler, 2016; Till, Tran, Voracek, Sonneck, & Niederkrotenthaler, 2014). When calculating overall effect sizes, this sample was not included twice to maintain independence. Total participants n = 12,912. The list of studies along with effect size estimates is presented in an online table at: http://www.christopherjferguson.com/Suic

UNCORRECTED PROOF

ideContagion.xlsx. Details on data extracted from each article are described below under effect size estimates and moderator analyses.

Effect size estimates

In line with recent innovations related to meta-analyses of multivariate analyses, the current paper makes uses 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 makes more sense theoretically, given 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. 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 media 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, as was the type of study (aggregate or traditional research study). Whether traditional research studies controlled for third variables was also coded as was the apparent presence absence of demand characteristics as evidence by explicit attempts to reduce them. Studies were also coded for whether they consider TV/movies or heavy metal music as media predictors.

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

Overall results of the meta-analysis are presented in Table 1. As indicated, overall results did not support a relationship between fictional media portrayals of suicide and suicide behaviors, thoughts or depressed mood among consumers. The overall effect size was near zero ($r = .034$ with correction for publication bias) and non-significant. Slightly more evidence was found among aggregate studies ($r = .101$) than among traditional studies ($r = .019$) although this was tempered by the potential for publication bias which would reduce the effect sizes, with estimated correction to $r = .077$ for aggregate studies. In this case, the Tandem Procedure returned inconclusive results. As the Tandem Procedure tends to be quite conservative in detecting publication bias, high potential for publication bias is likely and the lower estimate may be better to use.

The estimate for bivariate effects was much higher ($r = .234$) than for better controlled effect sizes ($r = -.018$), although the bivariate effects likewise appeared to be inflated by publication bias ($r = .049$ when

UNCORRECTED PROOF

TABLE 1
Effect Size Estimates For Outcomes Related to Fictional Media and Suicide-Related Outcomes

Predictor	k	r	r_x	p-value	95% C.I.	I^2	PBias
All studies	23	.060	.034	.104	−0.038, 0.105	90.87	Yes
Aggregate studies	9	.101	.077	.016	−0.013, 0.165	81.93	Prob
Traditional studies	15	.019		.694	−0.076, 0.114	89.96	No
Bivariate	8	.234	.049	.001	−0.081, 0.177	82.74	Yes
Controlled	16	−.018		.673	−0.102, 0.066	92.73	No
TV/Movies	14	.020		.691	−0.078, 0.117	94.16	No
Heavy Metal	10	.099	.023	.015	−0.063, 0.107	65.50	Yes

controlled for publication bias). These results highlight the critical value in considering controlled rather than bivariate effects when examining media impacts.

Lastly, effects were slightly higher for heavy metal music (r = .099) than for television/movies (r = .020). However, heavy metal research likewise appeared to be impacted by publication bias with effect sizes greatly reduced when this was considered (r = .023).

Meta-regression also revealed a declining trend for effect sizes across study years ($Q(1) = 63.76$, $p < .001$. This indicates that evidence for effects has gotten weaker in more recent studies.

In all cases, between-study heterogeneity was quite large. Indeed, effect sizes vary considerably in the field. Thus, although meta-analytic results can confirm lack of clear evidence for effects, cautious should be used in interpreting aggregated effect sizes as "true" population effect sizes.

DISCUSSION

Whether fictional media depicting suicides contribute to youth or adult suicides or depression continues to be hotly debated in the public. As the controversy over *13 Reasons Why* demonstrated, concerns persist that fictional media suicides may prompt imitative behaviors among media consumers. The current meta-analysis examined the existing research in this field for potential evidence. Overall, results suggested that the current research evidence cannot support the belief that fictional media with suicide themes lead to a suicide contagion among viewers. Although some inter-study heterogeneity existed, better controlled effect sizes suggest that the impact of suicide-themed fictional media on viewer suicide contagion is minimal. Likewise, meta-regression revealed declining effect sizes across study year, suggesting that evidence has weakened over time. This may reflect the "decline effect" in which initial research results prove difficult to replicate over time.

It is important to note that issues of publication bias appeared to be common in the field. Although uncorrected effect sizes were nonetheless weak, even these may be exacerbated by publication bias. Thus, as with other media effects fields, the issue of suicide contagion would likely benefit from a renewed emphasis on preregistered studies that reduce the potential for researcher expectancy effects, as well as a commitment to publishing null studies where they exist.

One methodological issue was difficult to examine, as it was nearly universal. This was the potential for demand characteristics in traditional psychology studies (both correlational and experimental). Although demand characteristics did not appear to result in high effect sizes among most studies in this area, it remains possible that some individual studies might be influenced by demand characteristics. There are likely straightforward ways to

INCORRECTED PROOF

reduce demand characteristics in traditional studies, such as through the use of distractor items, surveys, or procedures to make hypothesis guessing more difficult for participants. Attention to demand characteristics on other areas of media effects appears to result in reduced effect sizes (e.g. Whyte, Newman, & Voss, 2016). Thus, it should be something to consider here.

The role of aggregate studies as evidence appears to be more complex. From an examination of these studies, details were often a bit unclear on how data were analyzed and whether the most appropriate data analytic tools were employed. Time points were typically used as units of analyses, which would appear to leave studies poorly powered and unreliable as number of time points was typically too few for time series analyses. It may be worth examining better ways to analyze pre-post aggregate data based on raw number of suicides. Such data may also be particularly prone to a combination of ecological fallacy and publication bias. It may be that there are some incidents in which suicides appear to rise after one show, but do not after another, or even appear to decline (indeed, the full record of aggregate studies supports this pattern.) However, studies that find an effect may be more exciting and more publishable, potentially distorting the academic record (which did, in fact, appear to have some potential issues with publication bias.)

These data from heavy metal music are also interesting in light of debates regarding the impact of such music going back to 1980s. Current evidence suggests that the negative impacts of heavy metal music on listeners are minimal. Although the effect sizes were slightly higher than for other forms of media, this was reduced once publication bias was considered. This finding appears to comport reasonably well with other recent evidence suggesting heavy metal music is not harmful to or may even be beneficial to fans of this genre (e.g. Sharman & Dingle, 2015; Thompson, Geeves, & Olson, 2018).

Results from this study also critically highlight the importance of using controlled, theoretically relevant, multivariate analyses when examining media effects. An overreliance on bivariate correlations can result in a significant overestimation and misinterpretation of media effects. Analyses, including those in meta-analysis, should focus on controlled effect sizes rather than bivariate.

Limitations

As with any study, this one has limitations. First, a meta-analysis is only as good as the studies included within it. As noted above, the aggregate studies, in particular, may not always have been designed or analyzed in the best possible way and this could influence results from some studies. Related, demand characteristics were present in almost all traditional studies. Second, although this meta-analysis attempts to correct for publication bias, such attempts could over or underestimate the amount of actual publication bias. Nonetheless, the adjustment estimates are likely closer to the population effect sizes than are the raw effect sizes which more clearly may be the product of publication bias. Third, the current meta-analysis did not include unpublished studies. However, as unpublished studies are not indexed, finding such studies can be a fraught process and may increase, rather than decrease bias in some circumstances.

Concluding statements

At present, evidence is not able to support the contention that fictional depictions of suicide lead to suicide contagion in viewers. Until such time as more sophisticated studies with fewer demand characteristics and preregistered designs become available, it is suggested that newsmakers and advocates refrain from making causal attributions regarding suicide-themed shows such as *13 Reasons Why* or heavy metal music.

UNCORRECTED PROOF

REFERENCES

Furuya-Kanamori, F., & Doi, S. (2016). Angry birds, angry children and angry meta-analysts. *Perspectives on Psychological Science*, *11*(3), 408–414. https://doi.org/10.1177/1745691616635599

Gould, M., & Shaffer, D. (1986). The impact of suicide in television movies. *The New England Journal of Medicine*, *315*, 690–694. https://doi.org/10.1056/NEJM198609113151107

History.com. (2018). *An Ozzy Osbourne fan commits suicide*. Retrieved from: http://www.history.com/this-day-in-history/an-ozzy-osbourne-fan-commits-suicide

Hittner, J. B. (2005). How robust is the Werther effect? A re-examination of the suggestion-imitation model of suicide. *Mortality*, *10*(3), 193–200. https://doi.org/10.1080/13576270500178112.

Lacourse, E., Claes, M., & Villeneuve, M. (2001). Heavy metal music and adolescent suicidal risk. *Journal Of Youth And Adolescence*, *30*(3), 321–332. https://doi.org/10.1023/A:1010492128537.

Martin, G., Clarke, M., & Pearce, C. (1993). Adolescent suicide: Music preference as an indicator of vulnerability. *Journal Of The American Academy Of Child & Adolescent Psychiatry*, *32*(3), 530–535. https://doi.org/10.1097/00004583-199305000-00007.

National Association of School Psychologists. (2017). "*13 Reasons Why*" *Netflix Series: Considerations for Educators*. Retrieved from: https://www.nasponline.org/resources-and-publications/resources/school-safety-and-crisis/preventing-youth-suicide/13-reasons-why-netflix-series-considerations-for-educators

Phillips, D. P., & Paight, D. J. (1987). The impact of televised movies about suicide: A replicative study. *The New England Journal Of Medicine*, *317*(13), 809–811. https://doi.org/10.1056/NEJM198709243171306.

Pratt, T., Cullen, F., Sellers, C., Winfree, T., Madensen, T., Daigle, L., et al. (2010). The empirical status of social learning theory: a meta-analysis. *Justice Quarterly*, *27*, 765–802. https://doi.org/10.1080/07418820903379610

Rohter, L. (1990). 2 Families Sue Heavy-Metal Band As Having Driven Sons to Suicide. New York Times. Retrieved from: http://www.nytimes.com/1990/07/17/arts/2-families-sue-heavy-metal-band-as-having-driven-sons-to-suicide.html

Savage, J., & Yancey, C. (2008). The effects of media violence exposure on criminal aggression: A meta-analysis. *Criminal Justice and Behavior*, *35*, 1123–1136.

Schwartz, J. A., & Beaver, K. M. (2016). Revisiting the association between television viewing in adolescence and contact with the criminal justice system in adulthood. *Journal Of Interpersonal Violence*, *31*(14), 2387–2411. https://doi.org/10.1177/0886260515576970.

Sharman, L., & Dingle, G. A. (2015). Extreme metal music and anger processing. *Frontiers in Human Neuroscience*, *9*, 272. https://doi.org/10.3389/fnhum.2015.00272.

Simkin, S., Hawton, K., Whitehead, L., Fagg, J., & Eagle, M. (1995). Media influence on parasuicide: A study of the effects of a television drama portrayal of paracetamol self-poisoning. *The British Journal Of Psychiatry*, *167*(6), 754–759. https://doi.org/10.1192/bjp.167.6.754.

Society for the Prevention of Teen Suicide. (2017). *13 Reasons Why*. Retrieved from: http://www.sptsusa.org/13-reasons-why/

Stack, S., Gundlach, J., & Reeves, J. L. (1994). The heavy metal subculture and suicide. *Suicide And Life-Threatening Behavior*, *24*(1), 15–23.

Stack, S., Kral, M., & Borowski, T. (2014). Exposure to suicide movies and suicide attempts: A research note. *Sociological Focus*, *47*, 61–70. https://doi.org/10.1080/00380237.2014.856707

Thompson, W., Geeves, A., & Olson, K. (2018). Who enjoys listening to violent music and why? *Psychology of Popular Media Culture*, *????*, ????–????.

Till, B., Niederkrotenthaler, T., Herberth, A., Voracek, M., Sonneck, G., & Vitouch, P. (2011). Coping and film reception: A study on the impact of film dramas and the mediating effects of emotional modes of film reception and coping strategies. *Journal Of Media Psychology: Theories, Methods, And Applications*, *23*(3), 149–160. https://doi.org/10.1027/1864-1105/a000048.

Till, B., Tran, U. S., Voracek, M., & Niederkrotenthaler, T. (2016). Music and suicidality: A study on associations between music preferences and risk factors of suicide. *Omega: Journal Of Death And Dying*, *72*(4), 340–356. https://doi.org/10.1177/0030222815575284

Till, B., Tran, U. S., Voracek, M., Sonneck, G., & Niederkrotenthaler, T. (2014). Associations between film preferences and risk factors for suicide: An online survey. *PLoS ONE*, *9*(7).

Whyte, C., Newman, L. S., & Voss, D. (2016). A confound-free test of the effects of thin-ideal media images on body satisfaction. *Journal Of Social And Clinical Psychology*, *35*(10), 822–839. https://doi.org/10.1521/jscp.2016.35.10.822.

Manuscript Received: March 4, 2018
Revision Accepted: July 14, 2018

UNCORRECTED PROOFS