Psychology of Popular Media Culture
2013, Vol. 2, No. 1, 20–37


2160-4134/13/$12.00 DOI: 10.1037/a0030766

# In the Eye of the Beholder: Thin-Ideal Media Affects Some, but Not Most, Viewers in a Meta-Analytic Review of Body Dissatisfaction in Women and Men

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The issue of thin-ideal (or muscularity ideal for males) media effects on viewers continues to be debated and discussed within the scientific community. Many scholars have concluded that thin-ideal media can have an appreciable effect on viewers. More recently several scholars have contested this issue suggesting that media effects may be small to negligible or limited to groups of individuals already at risk for body dissatisfaction. The current meta-analysis, the most comprehensive to date with 204 studies, sought to examine the effects of thin or muscular media ideals on men and women in experimental, correlational, and longitudinal studies. Outcomes included general body dissatisfaction, restrictive eating, and symptoms of eating disorders. Results indicate little evidence for media effects in males. Effects were minimal for most females as well although some evidence suggested that women with preexisting body dissatisfaction may be primed by media ideals, particularly in experimental studies. Little evidence emerged for ethnic differences or differences across media types. However, some evidence emerged that publication bias issues may be inflating effect size estimates in some areas of study. Further, contrary to expectations, effect sizes were generally smaller for child samples than for adult and college student samples. Taken together, it is concluded that media effects are generally minimal and limited to those with preexisting body dissatisfaction. The evidence further did not support substantive links between media use and eating disorder symptoms.

*Keywords:* body dissatisfaction, body image, eating disorders, mass media

*Supplemental materials:* http://dx.doi.org/10.1037/a0030766.supp

Body dissatisfaction involves subjective disapproval of one's own body shape or form and the belief that it is unattractive to others. Body dissatisfaction is considered to be quite common among Western societies, particularly among women. Prevalence rates for body dissatisfaction suggest 40% to 50% of women experience some level of body dissatisfaction (Bearman, Presnell, & Martinez, 2006; Monteath & McCabe, 1997), with estimates for males considerably lower, and with gender discrepancies increasing over the 20th century (Feingold & Mazzella, 1998). Body dissatisfaction is considered one risk factor for the development of serious eating disorders (Stice & Shaw, 2002). Researchers are interested in multiple contributors to body dissatisfaction, ranging from genetic to social to parental to peer influences. It is important to note that body dissatisfaction, like most psychological issues, is viewed as multidetermined and multidimensional, and few researchers consider a single risk factor in a theoretical vacuum (Levine & Murnen, 2009; Striegel-Moore & Bulik, 2007). Nonetheless, it is reasonable to suggest that much consideration has focused on the media as a potential cause of body dissatisfaction in both females and males.

Many scholars conclude that links between body dissatisfaction and media are clear, with considerable strength and consistency (e.g., Anschutz, Engels, Becker, & van Strien, 2008; Brown & Dittmar, 2005; Harrison, Taylor, & Marske, 2006; Thomsen, Weber, & Beth Brown, 2002). A number of studies exist that

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provide evidence for links between thin-ideal media and body dissatisfaction (e.g., Brown & Dittmar, 2005; Cash, Cash, & Butters, 1983; Harper & Tiggerman, 2008; Hawkins, Richards, Granley, & Stein, 2004; Wegner, Hartmann, & Geist, 2000). Among male participants, the focus is on muscularity rather than thinness although this manuscript uses "thin ideal" language in the main for ease of communication. At the same time, there are also studies that provide evidence against such links (Cusumano & Thompson, 1997; Ferguson, Munoz, Contreras, & Velasquez, 2011; Hayes & Tantleff-Dunn, 2010; Martin & Kennedy, 1993; Thornton & Maurice, 1997; Tiggemann, 2006). Nonetheless, it may be the case that a few null studies might be expected owing to chance, methodological errors, Type II errors, and so forth. Grabe, Ward, and Hyde (2008) essentially argue for this in suggesting that the majority of studies find effects, with only a minority of null studies. However, an alternate explanation bears considering: that many studies, perhaps the majority, do not easily fit within binary categories of "does/does not support the media effects hypothesis."

Research from the media effects view most often portrays the influence of thin or muscular ideal media from a perspective of social learning and cognition (e.g., Lamb & Peterson, 2012) or social comparison (Gibbons & Buunk, 1999). Social learning and cognitive perspectives tend to focus on imitation and the development of cognitive scripts about how one should act. By comparison, social comparison theories suggest that individuals have a tendency to compare themselves with others on important qualities, in this case including sexual attractiveness. Other scholars have critiqued these theoretical perspectives as failing to recognize the active role of media consumers in shaping media (Ferguson et al., 2011; Gill, 2012). Thus debates seem to often focus on whether viewers are active participants in the media process or whether the media has direct and unavoidable influences.

## Methodological Issues That May Influence Effect Sizes

A number of methodological issues have the potential to either spuriously inflate or deflate effect size estimates from individual studies on media and body dissatisfaction, and these are outlined briefly below.

### Measurement Error

Measurement error is a common issue in many fields, which often truncates effect size estimates, but in some cases may artificially inflate them instead. Generally there is a risk that unreliable measures may truncate effect size estimates, leading to Type II errors. However, in some fields such as the related area of media and aggression, the use of unstandardized measures of aggression has been found to inflate effect size estimates (Ferguson & Kilburn, 2009). However, unlike the field of aggression, where the use of unstandardized and controversial outcome measures has been a highly problematic issue (e.g., Ritter & Eslea, 2005; Savage, 2004), the field of body dissatisfaction and media has benefitted from a stable of well validated standardized clinical measures, thus reducing the impact of this issue for this field.

### Demand Characteristics

Demand characteristics occur when participants in a study are able to guess the purpose of the study or are given subtle, even unconscious, clues as to the behavior expected of them from the experimenter (Orne, 1962). Demand characteristics may be the product of several sources.

First, close or obvious pairing of the independent and dependent variables set up the potential for demand characteristics. Many studies closely paired either experimental media conditions or survey measures of media exposure with measures of body dissatisfaction or related outcomes (indeed it was difficult to find studies that did not). Under such conditions, hypothesis guessing, particularly when using undergraduate students who may be well-informed of both the media effects hypothesis and experimental procedures used in psychology, may be a relatively simple matter. Although fairly uncommon, some studies actually informed participants of the hypotheses in advance (e.g., Champion & Furnham, 1999).[1] By contrast, some studies did use sophisticated methods for reducing demand characteristics such as embedding outcome measures among distracter mea-

[1] This is based only on those reports that acknowledged revealing the hypotheses in advance to participants. It is possible this detail may not have been mentioned in some reports.

sures, or placing the media exposures among other distracter tasks.

### Introduction of Confounds

The introduction of confounds in experimental research, or the failure to control for important variables in correlational research, can result in spuriously high effect size estimates. For instance, in the field of video game violence, recent research has illuminated that many studies failed to match video game conditions carefully, introducing levels of competitiveness (Adachi & Willoughby, 2011) or complexity of controls (Przybylski, Rigby, & Ryan, 2010) as confounds. Controlling for these issues resulted in null effects for violent content as a variable.

In the field of body dissatisfaction and media, although some studies contrast thin-ideal models with average (or heavy) models, many others use nonhuman objects as controls (e.g., Hawkins et al., 2004; Mills, Polivy, Herman, & Tiggeman, 2002). The use of nonhuman objects as controls arguably fails to isolate the thin-ideal media variable. It may be simply that participants find images of young women to be threatening to their body image, whether those women are thin-ideal or not.

### Single Respondent Bias

Single responder bias occurs in correlational/longitudinal studies when the same respondent provides data for both the predictor and outcome variables. This has been observed to inflate effect sizes, presumably as a single respondent can enter a response set (Baumrind, Larzelere, & Cowan, 2002). Unlike the issue of measurement error, single respondent bias was nearly ubiquitous across body dissatisfaction studies, so much so that testing it as a moderator in meta-analysis would not be possible.

## Meta-Analytic Results

At present, several meta-analyses using differing approaches have examined body dissatisfaction issues (Grabe et al., 2008; Groesz, Levine, & Murnen, 2002; Holmstrom, 2004; Want, 2009), with effects ranging between $r = .08$ and $r = .17$. From these analyses, we can generally assert that the general effect size for media exposure on body dissatisfaction is likely within this range. Such effects are small (Cohen, 1992) and are below the $r = .2$ cutoff for practical significance suggested by some researchers (e.g., Ferguson, 2009; Franzblau, 1958; Lipsey, 1998).

In some cases errors may have inflated the effect size estimates. For example, some of the effect size estimates from Grabe et al. (2008) appear to have used only significant outcomes from some studies when nonsignificant outcomes were also available. It is important to note that the original articles in question tended to highlight significant findings, making it difficult for meta-analytic scholars to notice and properly extract the nonsignificant findings. Further the number of errors was fairly small in a sample of 77 articles.[2] Nonetheless, even a small number of errors that tended to favor statistically significant outcomes might inflate effect sizes. Further, Grabe et al. (2008), when extracting effects from experimental studies, focused on comparisons between experiments and nonhuman object controls where available, and did not use controls consisting of average weight females. The lead author (Grabe, 2011, personal communication) argued cogently that this was more externally valid; average sized models not typically being used in the media. Although this has merit, this comes at the expense of internal validity; that in isolating the thin-media ideal, comparisons between thin and averaged sized models would have been of greater value than thin models and objects. To be clear, the intent is not to be overly critical of Grabe et al. (2008), which remains a fine and important meta-analysis, rather to highlight how methodological issues can influence meta-analyses. This is likely an issue across all meta-analyses (see Ferguson & Heene, in press for discussion), not an issue specific to Grabe et al. (2008).

There are other limitations of the previous analyses that are worth noting. First, most of the previous meta-analyses were fairly small regarding number of studies, capturing only subsets of the research field. Holmstrom (2004) included 34 studies, Groesz et al. (2002) included 25, Want (2009) included 47 focused on experimental studies, and Grabe et al. included

[2] A list of the identified discrepancies is available on request.

77. Although examining subsets of studies can have value, there is also worth in examining an entire research field.

Second, previous analyses only tangentially considered the issue of publication bias. It is now understood that publication bias is a common problem not necessarily alleviated by the inclusion of unpublished studies. Although some forms of unpublished studies such as doctoral dissertations are indexed, many are not (that is to say, not available in a searchable database such as PsycINFO or Digital Dissertations). Nonindexed unpublished studies can be difficult to locate for multiple reasons, leading to selection bias in attempts to locate them, often favoring established positions over failed replications (See Ferguson & Brannick, 2012 for discussion). Publication bias, or the tendency to publish statistically significant findings at the expense of null studies, can result in spuriously high effect sizes in meta-analysis. The meta-analysis conducted by Grabe et al. (2008) did show some evidence of publication bias, with published data having significantly larger effect sizes than unpublished data.[3] The other meta-analyses conducted no formal tests of publication bias.

Third, no meta-analyses have considered the effects of media across ethnic groups or across genders. It is possible to consider that males and females or members of differing ethnicities may be differentially influenced by media.

Fourth, with the exception of Want (2009), little attention has been focused on studying specific methodological issues that may inadvertently influence effect sizes. These include the issues noted earlier in the article that may provoke demand characteristics or introduce unintended confounds.

Fifth, little attention in meta-analyses has focused on the potential for the influence of media on body dissatisfaction to be limited to specific groups, such as individuals with preexisting body dissatisfaction issues or other vulnerabilities (although see Groesz et al., 2002 for previous discussion). Vulnerable individuals include individuals with preexisting personality traits or body dissatisfaction issues. The conditions experienced by these individuals may make them more prone to experiencing body dissatisfaction or being primed by media reminders regarding preexisting body dissatisfaction. It is possible that a minority of individuals may be prone to making media comparisons that increase body dissatisfaction, whereas the majority of individuals do not. Some recent research has suggested this may be the case (Roberts & Good, 2010; see also Ferguson, Winegard, & Winegard, 2010 for a review).

Lastly, with the exception of Grabe et al., 2008, previous meta-analyses have not contrasted the effects for body dissatisfaction with arguably more serious eating disorder symptoms. This may be due to the observation that studies of media and eating disorders are relatively uncommon in relation to body dissatisfaction (Striegel-Moore & Bulik, 2007). Although in the minority, there are at present some studies that do examine the influence of media on eating disorder symptoms (although as Striegel-Moore & Bulik, 2007 note, rarely on full-blown syndromes). Thus it is arguably of some value to contrast effects on nonclinical body dissatisfaction and clinical symptoms of eating disorders.

## The Current Study

The current meta-analysis will focus on three main sets of questions. First, whether evidence is sufficient to support the existence of strong general population-level effects. Second, whether evidence exists for subgroup effects, particularly for individuals with predispositions for body dissatisfaction. Third, whether methodological issues can be identified that may explain the discrepancies witnessed between individual studies.

## Methods

### Study Selection and Categorization

PsycINFO and Digital Dissertations were searched for all articles indexed that included the following search terms: ([body image] or [body dissatisfaction] or [eating]) and ([media] or [computer game] or [TV] or [magazine]). This method was supplanted by reviewing the study inclusion lists of other recent meta-

[3] Their Table 5 (p. 470) seems to suggest the opposite, that unpublished studies have larger effects than published studies. However, this was a misprint as confirmed by the first author of the original meta-analysis (Grabe, personal communication, April, 2010).

analytic reviews (Grabe et al., 2008; Groesz et al., 2002; Holmstrom, 2004; Want, 2009). Aside from doctoral dissertations, unpublished articles were not included out of concern for selection bias problems owing to the nonindexed nature of these sources (Cook et al., 1993; Egger & Smith, 1998; Ferguson & Brannick, 2012). Articles were included if they met the following criteria, which were fairly broad:

(1) Articles had to be empirical articles providing enough data to calculate an effect size "r."

(2) Outcomes involved behaviors, cognitions, or emotional responses related to body dissatisfaction, disordered eating, or restrictive eating.

(3) The study provided some measurement, either experimental or correlational, of exposure to thin (or muscular) ideal media.

A total of 204 individual manuscripts were included in the current analyses of which 49 (24%) were unpublished dissertations or theses indexed on PsycINFO or Digital Dissertations. Together these articles included 443 independent effect size estimates. These effect size estimates refer to differing study outcomes within manuscripts or subgroup analyses and thus are independent sample groups. A list of the included articles is included in supplemental Appendix A.

## Effect Size Calculation

Pearson's *r*, a flexible and easily interpreted index of effect size, was used as the effect size estimate in this study. Although some scholars do prefer the effect size *d*, particularly for experimental outcomes, it is argued here that *r* is robust, easier to communicate to nonstatisticians, and the interpretation of results is unlikely to hinge on the selection of *r* or *d*. Correlation coefficients were transformed to Fisher's *z*, weighted, averaged, and transformed back to a pooled *r*, denoted $r_u$ (a meta-analytic effect size estimate, uncorrected for potential publication bias). In the case in which a study reported nonsignificant results but failed to provide statistical information (e.g., *F* value), the effect size was calculated using the provided means and standard deviations. In the event of multiple measures for the same construct occurring within a study (i.e., multiple dependent or independent measures), simple mean correlations were computed.

One issue that has arisen in meta-analysis is the issue of control variables in correlational or longitudinal studies. Meta-analyses generally rely on bivariate *r*, as this preserves the homogeneity assumption of meta-analysis. However, it has been observed that reliance on bivariate *r* may cause spuriously high effect size estimates, particularly in correlational research (Baumrind et al., 2002). This occurs because bivariate *r* effect size estimates do not include statistical controls for confounding predictor variables even when certain predictor variables are identified as important control variables by the field.

However, using controlled effect size estimates such as partial *r* or standardized regression coefficients is considered problematic, as individual studies may consider differing numbers of control variables, which reduces the integrity of the homogeneity assumption. This has been a difficult methodological issue to resolve. Although Baumrind et al. (2002) suggest examining better controlled effect size estimates may be desirable, this approach has not been universally accepted. With this in mind, for correlational and longitudinal studies, two sets of effect size estimates were calculated; one for bivariate *r*, one for controlled effects. A list of individual study-level effect sizes is presented as supplemental Appendix B.

## Interpretation of Effect Sizes

Capitalizing on considerable power, the results of meta-analyses are almost invariably "statistically significant" although the interpretation of the same can be difficult when effect sizes are very small or trivial. As such Ferguson and Brannick (2012) have advised against focusing on statistical significance and instead on conservative interpretation of effect sizes.

With that in mind, the effect sizes seen in meta-analytic results will be considered according to two metrics. The first of these will be Cohen's threshold of $r = .1$, below which effects are considered "trivial." Ferguson (2009) further argued for a threshold of $r = .20$ for practical significance, noting that smaller effects are more prone to publication bias and Type I error. Thus the current analysis adopts the threshold of $r = .1$, below which results are trivial, and $r = .2$, above which results are of particular importance.

## Statistical and Publication Bias Analyses

In accordance with recommendations of Hunter and Schmidt (2004), random effects models were used and are presented in the tables. Finalized effect size estimates did not differ substantially between random and fixed effect size estimates.

The proper analysis of publication bias has sometimes been a source of controversy. Although publication bias analysis is generally regarded as important, proper means of controlling for publication bias have not always been widely agreed upon owing to both Type I and Type II error issues with many publication bias procedures. To improve on previous methods, Ferguson and Brannick devised a Tandem Procedure for publication bias analyses, which reduces Type I and Type II error issues. The Tandem Procedure focuses on general agreement between publication bias measures, particularly related to a low Orwin's Fail-safe *N* (OFSN) indicating fragility of the results, significance for either the rank correlation or Egger's regression, and significance for Trim and Fill. This method was found to be reliable and valid in both simulation and analyses of actual data sets. Ferguson and Brannick discuss the Tandem Procedure in detail, although the components are noted briefly below:

(a) Orwin's OFSN lower than k (number of included studies). This index is used as a measure of the frailty of the field to unpublished null effects.

(b) Significance for either Begg and Mazumdar's rank correlation test or Egger's Regression, both of which examine the relationship between effect size and sample size.

(c) Significance for Duvall and Tweedie's Trim and Fill. If Trim and Fill is significant and corrects for publication bias, this is represented by the $r_+$ value in the tables. If $r_+$ is blank in the tables, no correction for publication bias was identified.

## Moderators

The current analysis considered the potential influence of several moderators based on questions raised by the limitations of previous meta-analyses as well as by the identified methodological issues noted earlier in the article (demand characteristics, control stimuli, etc.). Included studies were coded for the following moderators:

**Outcome type.** Outcomes were coded separately for five types of outcomes: those related to body dissatisfaction (BD), anorexia nervosa symptoms (AN), bulimia nervosa symptoms (BN), general eating disorder symptoms (EDNOS), and restrictive eating (RE). Symptoms related to AN or BN were typically operationalized as corresponding to clinically relevant subscales of measures such as the Eating Disorders Inventory. EDNOS was operationalized as involving scales of behavior that were aggregates of AN and BN, or were not specific to AN or BN. Thus EDNOS is a more general category than AN or BN despite some overlap. Restrictive eating was operationalized as involving purposeful reduction of calories to lose weight, but in the absence of further symptoms significant for an eating disorder diagnosis.

**Media type.** The majority of studies examined one or more of three types of media, namely magazines and print media, TV and visual media, and music videos. Too few studies considered other media such as video games to be substantively examined in meta-analysis. Effect sizes were calculated separately for these media types to examine whether some forms of media may have more or less impact than others.

**Preexisting susceptibility to body dissatisfaction.** Although most studies did not consider differences between individuals with preexisting susceptibility, a minority of studies did. In such studies preexisting susceptibility was operationalized through high prescores (i.e., one or more standard deviations above the mean, thus approximately 15%–16% of the general population) on body dissatisfaction surveys before experimental manipulation.

**Ethnicity.** In their analysis of ethnic differences in body dissatisfaction, Roberts, Cash, Feingold, and Johnson (2006) found that African American women, as compared with Caucasian women, had higher body esteem. The authors concluded that trends in body esteem among different ethnicities were rather complex, perhaps not entirely fitting with traditional sociocultural models of body dissatisfaction. To date, however, no meta-analyses have examined for ethnic differences in the propensity for media effects. Thus ethnicity will be considered as a moderator in the current analysis.

**Age.** Most discussions of media effects research suggest that children are particularly prone to negative outcomes of media exposure (APA, 2007). From this it might reasonably be expected that the effects of media exposure would be higher among children and perhaps teens in particular relative to adults, particularly in longitudinal research. As such age will be considered as a moderator variable.

**Best practices.** Earlier in the article, several methodological concerns were observed that potentially influence effect size estimates. It is possible to compare and contrast studies that control well for these methodological issues against those that do not. As such the following best practices criteria were adopted:

(1) The researchers used a cover story in an attempt to disguise the nature of the study.

(2) Well validated outcome measures were used.

(3) Efforts were made to "decouple" the independent and dependent variables so as to reduce demand characteristics. In correlational studies, this typically involved imbedding the questionnaires of interest among distracter questions to prevent hypothesis guessing. In experimental studies, this typically involved distracter tasks to reduce hypothesis guessing.

(4) In experimental studies, the study protocol included no directions to attend to the thinness, attractiveness, and so forth, of the model-related stimuli.

(5) In experimental studies, control stimuli were selected to avoid the possible introduction of confounds other than thin-ideal media. Studies using non–thin-ideal figures as controls rather than nonhuman objects were thus considered to meet this criterion.

Studies that met all criteria were considered "best practices." The best practices criteria and decision rubric were tested for consistency using a trained graduate student who was blind to the author's ratings of best practices. The trained student rated a random sample of articles from the general sample ($n = 22$, just more than 10%). Absolute agreement on "best practices" was 91%, indicating strong interrater reliability of the decision rubric.

**Publication status.** In addition to the Tandem Procedure explained earlier for publication bias analyses, publication status (published vs. dissertation) will be considered as a potential moderator.

**Continuous moderators.** In addition to the categorical moderators noted above, two continuous moderators will also be considered. Continuous moderator variables are those that provide data on a ratio or interval scale rather than as categorical variables. The first of these is the number of controls used in correlational and longitudinal studies. Consistent with Baumrind et al. (2002), it is expected the greater number of controls used will reduce effect size estimates.[4] Second, exposure duration will be considered as a moderator for experimental studies. Some previous work (Holmstrom, 2004; Want, 2009) has suggested that exposure duration is associated with reduced effect sizes, somewhat the opposite of expected. Thus it bears examining in a larger pool of studies. Continuous moderators will be examined using metaregression techniques.

## Results

Main results are presented in a series of tables. Most of the tables involve a common set of statistics and these are briefly noted here. The statistic k refers to the number of studies. The basic effect size estimate from the regression is represented as $r_u$ and, in the case Trim and Fill indicated an adjustment, $r_+$. The $r_+$ statistic is left blank when no publication bias was indicated. The 95% confidence interval for the effect size is also presented. The effect size homogeneity and $I^2$ statistics are presented as tests for potential moderator effects. The final columns represent the individual tests of publication bias used in the Tandem Procedure, and the final decision on publication bias.

Given that results were examined for publication bias, results that are both corrected ($r_+$) and uncorrected ($r_u$) are presented in each of the tables. The index represents the Trim and Fill adjusted *r* value correcting for publication bias

[4] Technically, Baumrind et al. (2002) would likely argue that the number of controls may not matter if authors employ poorly chosen controls that one would not expect to be correlated with the outcome or predictor variables. Thus there may be greater value in identifying certain *critical* controls and controlling for these, rather than quantity of controls. However, it is not clear that the science of media effects on body dissatisfaction has identified a consistent understanding of such controls. Peer effects may arguably be one critical control (Ferguson et al., 2011) although relatively few articles controlled for this.

where publication bias was indicated. This value thus provides an estimate of the total population of studies, reducing effect size inflation owing to publication bias. Where presented in the tables for correlational and longitudinal studies, the corrected $r_+$ is for controlled rather than bivariate outcomes. Where publication bias was not evident, $r_+$ is left blank, as this value is identical to uncorrected $r_u$. The use of the terms corrected ($r_+$) and uncorrected ($r_u$) are to be distinguished from controlled or partial *r* outcomes in correlational or longitudinal studies, which represent effect size estimates that control for potential "third" variables that may explain correlations between media use and body dissatisfaction (e.g., age, peer influences, family influences, etc.).

The first set of results considers media effects across genders and across outcome measures for experimental, correlational and longitudinal designs. Several sets of specific outcome measures were considered. The majority of studies considered body dissatisfaction, with a smaller number of studies considering anorexic symptoms (AN), bulimic symptoms (BN), general eating disorder symptoms (EDNOS), and restrictive eating (RE). Meta-analytic results are presented separately for outcome, thus preventing individual samples from being represented more than once in the analysis and preserving the assumption of independence. For some outcomes, too few studies existed for meta-analysis and these are not discussed. All tables include heterogeneity data including $I^2$, which indicates the between-study heterogeneity that is not likely due to chance.

In experimental studies involving women, exposure to thin-ideal media was related to body dissatisfaction, BN and RE, but not more general eating disorder symptoms. However, effect sizes were generally in the small range. For men, only data on body dissatisfaction were available, and the association between media exposure[5] and body dissatisfaction in men was negligible. Publication bias was evident, particularly in studies of body dissatisfaction. The correction for publication bias would reduce the effect size near to trivial values ($r = .11$ for body dissatisfaction and $-.03$ for EDNOS symptoms). These results are noted in Table 1.

Among correlational studies, little evidence emerged for strong associations between media exposure to thin-ideals (or muscular ideals for men) and body-related outcomes in either men or women. The only exception was for RE in women, where a small relationship was observed. Publication bias did not seem to be a major concern among correlational studies. These results are presented in Table 2.

Longitudinal studies likewise provided little evidence for a long-term association between media exposure and body dissatisfaction in either males or females. A slight relationship between media ideals and general eating disorder symptoms was noted for females, although this relationship was very small. These results are presented in Table 3.

## Bivariate Versus Controlled Effect Size Estimates

Both controlled effect size estimates and bivariate *r* are presented for correlational and longitudinal studies in Tables 2 and 3. As Baumrind et al. (2002) suggested, bivariate effect size estimates were slightly higher than better controlled estimates. However, most of these were variations of less than $r = .04$ and only one was as high as $r = .06$ in difference. It was felt that these minor differences would not significantly influence the interpretation of the results. Thus, subsequent Tables report only the better controlled effect sizes estimates and not bivariate *r* in the spirit of the recommendations of Baumrind et al. (2002).

## Moderator Variables

Many outcome categories in the meta-analysis, particularly for experimental and correlational studies of females, indicated significant heterogeneity. Such heterogeneity indicates that the deviation between effect sizes is greater than would be expected by chance. This may indicate inconsistencies between study results that may be due to moderator effects. Given that most studies were for the BD outcome, and that effects for males were generally negligible and relatively low in heterogeneity, moderator analyses were focused on body dis-

[5] Media exposures in studies with male participants most often involved depictions of thin muscular male ideals. This is usually referred to as muscularity and differs in some respect from the "thin ideal" for women. Nonetheless the language of "thin ideal" is used throughout this article for ease of communication.

Table 1
*Meta-Analytic Results for Main Analysis in Experimental Studies*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Females | | | | | | | | | | |
| BD | 140 | .11 | .17 | .13, .20 | $\chi^2(139) = 450.34, p < .001$ | 69.13 | 84 | $p < .05$ | NS | Yes |
| BN | 4 | | .15 | .01, .28 | $\chi^2(3) = 3.69$, NS | 18.68 | 3 | NS | NS | No |
| EDNOS | 7 | −.03 | .02 | −.10, .14 | $\chi^2(6) = 7.54$, NS | 20.46 | 0 | $p < .01$ | $p < .05$ | Yes |
| RE | 18 | | .15 | .07, .22 | $\chi^2(17) = 34.37, p < .01$ | 50.54 | 6 | NS | NS | No |
| Males | | | | | | | | | | |
| BD | 19 | | .07 | −.01, .15 | $\chi^2(18) = 36.68, p < .01$ | 49.55 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive; BD = body dissatisfaction; BN = bulimia nervosa symptoms; EDNOS = symptoms of eating disorders not tied to specific diagnosis; RE = restrictive eating.

satisfaction outcomes among women. It should be noted that several moderator categories have only a very small number of studies. They are presented here to document the current status of the field, but should be interpreted with great caution, as results based on only a small number of observations could prove unreliable.

**Outcome type.** Generally effect size estimates were similar across study types and outcome types. Among experimental studies of women, outcomes related to general eating disorder symptoms were particularly low, as indicated on Table 1. By contrast, as indicated in Table 2, RE-related outcomes were a little higher than other outcomes in correlational studies involving women. However, outcomes were generally higher for experimental studies involving women than they were for correlational or longitudinal studies, regardless of outcome type.

**Media type.** Effect size estimates across TV, magazines, and music videos are presented in Table 4. Once again the pattern in which higher effects are seen in experimental studies is visible. However, little differences emerged across media type.

**Preexisting susceptibility to body dissatisfaction.** Effect sizes for women with high and low preexisting susceptibility to body dissatisfaction are presented in Table 5. As can be seen, the effects for women with high preexisting susceptibility ($r = .26$) were much greater than for women with low susceptibility ($r = .07$).

Table 2
*Meta-Analytic Results for Main Analysis in Cross-Sectional/Correlational Studies*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Females | | | | | | | | | | |
| BD | 93 | | .05 (.07) | .03, .08 | $\chi^2(92) = 216.80, p < .001$ | 57.57 | 0 | NS | NS | No |
| BN | 25 | | .06 (.07) | .02, .10 | $\chi^2(24) = 54.54, p < .001$ | 56.00 | 0 | NS | NS | No |
| AN | 16 | | .08 (.12) | .02, .13 | $\chi^2(15) = 41.52, p < .001$ | 63.87 | 0 | NS | NS | No |
| EDNOS | 21 | | .06 (.10) | −.01, .13 | $\chi^2(20) = 93.54, p < .001$ | 78.62 | 0 | NS | NS | No |
| RE | 10 | | .14 (.16) | .06, .21 | $\chi^2(9) = 30.35, p < .001$ | 70.35 | 6 | NS | NS | No |
| Males | | | | | | | | | | |
| BD | 24 | | .07 (.07) | .04, .10 | $\chi^2(23) = 16.28$, NS | 0.00 | 0 | NS | NS | No |
| BN | 5 | | .04 (.07) | −.01, .08 | $\chi^2(4) = 2.50$, NS | 0.00 | 0 | NS | NS | No |
| EDNOS | 4 | .06 | .09 (.12) | −.03, .21 | $\chi^2(3) = 7.63, p < .05$ | 60.65 | 0 | NS | $p < .05$ | Yes |
| RE | 11 | | .03 (.04) | −.02, .08 | $\chi^2(10) = 7.78$, NS | 0.00 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive; BD = body dissatisfaction; BN = bulimia nervosa symptoms; AN = anorexia nervosa symptoms; EDNOS = symptoms of eating disorders not tied to specific diagnosis; RE = restrictive eating. Numbers in parentheses under $r_u$ indicate effect size estimate for bivariate correlations.

Table 3
*Meta-Analytic Results for Main Analysis in Prospective/Longitudinal Studies*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Females | | | | | | | | | | |
| BD | 16 | | .03 (.09) | .01, .07 | $\chi^2(15) = 18.17$, NS | 17.46 | 0 | NS | NS | No |
| EDNOS | 6 | | .11 (.15) | −.01, .22 | $\chi^2(5) = 21.69, p < .001$ | 76.95 | 0 | NS | NS | No |
| Males | | | | | | | | | | |
| BD | 8 | .03 | .04 (.10) | .01, .08 | $\chi^2(7) = 5.84$, NS | 0.00 | 0 | NS | $p < .05$ | Yes |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive; BD = body dissatisfaction; EDNOS = symptoms of eating disorders not tied to specific diagnosis. Numbers in parentheses under $r_u$ indicate effect size estimate for bivariate correlations.

**Ethnicity.** Effect sizes across ethnicities are presented in Table 6. In experimental research Caucasian women appeared to be more likely to respond to thin-media ideals ($r = .20$) than African American women ($r = .06$). There were too few studies of other ethnicities to allow for further comparisons. However, in correlational studies, little evidence for ethnic differences emerged.

**Age.** Among experimental studies, effects were strongest for older participants and weaker for preteens and children in particular. Among correlational studies, strongest effects ($r = .16$) were seen for young children, although effects quickly dropped off in preteen and teen years. Furthermore weakest effects were seen in longitudinal studies of children ($r = .03$). These results are presented in Table 7.

**Best practices.** Among experimental studies, higher effects were seen for studies with weaker methodologies (i.e., demand characteristics, lower internal validity, $r = .18$) as opposed to those with best practice designs ($r = .10$). Relatively little difference was seen for correlational studies, suggesting methodological issues have had less impact on effect sizes in this realm. There were not enough "best practice" longitudinal designs to allow for a meaningful comparison. These results are presented in Table 8.

**Publication bias.** Using the Tandem Procedure, publication bias did not seem to be an issue for most subgroups in the current analysis.

Table 4
*Meta-Analytic Results for Media Type as Moderator for Female Body Dissatisfaction*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Experiments | | | | | | | | | | |
| Magazines | 111 | | .15 | .11, .19 | $\chi^2(110) = 381.70, p < .001$ | 70.18 | 58 | NS | NS | No |
| Television | 25 | .13 | .19 | .12, .26 | $\chi^2(24) = 62.70, p < .001$ | 61.72 | 20 | $p < .05$ | NS | Yes |
| Correlational | | | | | | | | | | |
| Magazines | 38 | | .07 | .03, .11 | $\chi^2(37) = 141.24, p < .001$ | 73.80 | 0 | NS | NS | No |
| Television | 38 | | .05 | .02, .07 | $\chi^2(37) = 54.03, p < .05$ | 31.52 | 0 | $p < .05$ | NS | No |
| Music videos | 8 | | .05 | −.01, .10 | $\chi^2(7) = 9.49$, NS | 26.30 | 0 | $p < .05$ | NS | No |
| Prospective/ Longitudinal | | | | | | | | | | |
| Magazines | 5 | | .05 | .01, .09 | $\chi^2(4) = 3.59$, NS | 0.00 | 0 | NS | NS | No |
| Television | 9 | .00 | .03 | −.02, .08 | $\chi^2(8) = 12.10$, NS | 33.92 | 0 | $p < .01$ | $p < .05$ | Yes |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive.

Table 5
*Meta-Analytic Results for Pre-Existing Body Dissatisfaction as Moderator for Media Effects on Women's Body Dissatisfaction*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Experiments | | | | | | | | | | |
| High BD | 33 | | .26 | .20, .33 | $\chi^2(32) = 66.66, p < .001$ | 52.00 | 53 | NS | NS | No |
| Low BD | 25 | | .07 | −.01, .14 | $\chi^2(24) = 42.05, p < .001$ | 42.93 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive.

The exception, as can be seen from the various tables, was for experimental studies, particularly regarding women's body dissatisfaction. Subgroup analyses suggested that nonbest practice (see Table 8) experimental studies of Caucasian college students (see Table 7) were particularly prone to publication bias. This observation was further confirmed in the consideration of publication status as a moderator variable in which published experimental studies were shown to have higher effect sizes ($r = .19$) than unpublished dissertations ($r = .08$). These results are presented in Table 9.

**Continuous moderators.** Metaregression techniques found that effect size in correlational studies was inversely related to the number of control variables (i.e., other variables such as family, peer, or personality influences that might explain correlational links between media use and body dissatisfaction), ($Q_{model} = 16.49$, $Z = -4.06$, $p < .001$). However, this was not found for longitudinal studies. The relationship between exposure duration in experimental studies and effect size was not significant, although the trend was in the direction seen in previous analyses (Holmstrom, 2004; Want, 2009). These results are presented in Table 10.

## Discussion

Past meta-analytic reviews have not agreed regarding the impact of thin-ideal media on body dissatisfaction. The current meta-analytic review sought to examine several main issues in an effort to help address past discrepancies in opinion. These included whether evidence existed to support the position that thin-ideal media had strong and general population-wide effects, whether evidence existed for more specific subgroup effects, particularly for individuals predisposed to body dissatisfaction, and whether methodological issues could be identified to explain discrepancies between studies in this realm. More than 200 studies were included in the current meta-analysis providing the most comprehensive synthesis of this

Table 6
*Meta-Analytic Results for Ethnicity as Moderator for Media Effects on Women's Body Dissatisfaction*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Experiments | | | | | | | | | | |
| Caucasian | 29 | .15 | .20 | .14, .25 | $\chi^2(28) = 46.80, p < .01$ | 40.17 | 26 | $p < .001$ | $p < .01$ | Yes |
| African | 3 | | .06 | −.07, .18 | $\chi^2(2) = 0.53$, NS | 0.00 | 0 | NS | NS | No |
| Correlational | | | | | | | | | | |
| Caucasian | 10 | | .09 | .02, .15 | $\chi^2(9) = 24.47, p < .01$ | 63.22 | 0 | NS | NS | No |
| African | 4 | | .09 | −.13, .29 | $\chi^2(3) = 12.06, p < .01$ | 75.12 | 0 | NS | $p < .05$ | No |
| Asian | 8 | | .07 | .01, .14 | $\chi^2(7) = 5.36$, NS | 0.00 | 0 | NS | NS | No |
| Hispanics | 5 | | .04 | .00, .09 | $\chi^2(4) = 1.88$, NS | 0.00 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive.

Table 7
*Meta-Analytic Results for Age as Moderator for Media Effects on Women's Body Dissatisfaction*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Experiments | | | | | | | | | | |
| Adults | 12 | | .17 | .09, .24 | $\chi^2(11) = 12.07$, NS | 8.88 | 9 | NS | NS | No |
| College student | 110 | .11 | .17 | .13, .21 | $\chi^2(109) = 405.87, p < .001$ | 73.14 | 70 | $p < .05$ | NS | Yes |
| Teen | 12 | | .18 | .10, .25 | $\chi^2(11) = 26.47, p < .01$ | 58.44 | 9 | NS | NS | No |
| PreTeen | 3 | .06 | .10 | −.02, .21 | $\chi^2(2) = 0.55$, NS | 0.00 | 0 | $p < .05$ | NS | Yes |
| Child | 3 | | .05 | −.08, .18 | $\chi^2(2) = 1.02$, NS | 0.00 | 0 | NS | NS | No |
| Correlational | | | | | | | | | | |
| Adults | 4 | | .10 | .03, .16 | $\chi^2(3) = 1.65$, NS | 0.00 | 0 | NS | NS | No |
| College student | 53 | | .07 | .04, .10 | $\chi^2(52) = 99.19, p < .01$ | 47.58 | 0 | NS | NS | No |
| Teen | 28 | | .02 | −.01, .06 | $\chi^2(27) = 89.22, p < .001$ | 69.74 | 0 | NS | NS | No |
| PreTeen | 5 | | .06 | −.02, .13 | $\chi^2(4) = 2.61$, NS | 0.00 | 0 | $p < .05$ | $p < .01$ | No |
| Child | 3 | | .16 | .02, .24 | $\chi^2(2) = 2.52$, NS | 20.73 | 0 | NS | NS | No |
| Longitudinal | | | | | | | | | | |
| College student | 3 | | .09 | −.01, .20 | $\chi^2(2) = 0.38$, NS | 0.00 | 0 | NS | NS | No |
| Teen | 9 | | .03 | −.01, .07 | $\chi^2(8) = 10.89$, NS | 26.55 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive; Adults = non-college student adults.

research field to date. The implications of this study's results will be discussed, with the discussion divided among the three main purposes of this study.

## Is There Evidence for Strong, General, Population-Level Effects?

In regards to male respondents and exposure to muscular ideals, the answer appears to be "no." It is important to qualify this statement with the observation that most studies were with heterosexual male college students and studies of other populations such as gay men or nonstudents were comparatively few and thus generalizability to these groups is limited. However, no effect size outcomes for males, whether from longitudinal, correlational or experimental, nor whatever the outcome measures, surpassed even the relatively low $r = .10$ threshold for "trivial" effects. Although there are some individual studies that do suggest males may respond to media ideals, particularly regarding muscularity rather than thinness (e.g., Agliata & Tantleff-Dunn, 2004), on balance the research field did not provide sufficient evidence to endorse the belief in pervasive widespread effects on men.

The results for females were more qualified. The strongest support for media effects on fe-

Table 8
*Best Practices Analysis for Outcomes Related to Women's Body Dissatisfaction*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Experiments | | | | | | | | | | |
| YesBP | 25 | | .10 | .03, .17 | $\chi^2(24) = 49.88, p < .001$ | 51.89 | 0 | NS | NS | No |
| NoBP | 115 | .12 | .18 | .14, .22 | $\chi^2(114) = 390.50, p < .001$ | 70.81 | 85 | $p < .01$ | NS | Yes |
| Correlational | | | | | | | | | | |
| YesBP | 4 | | .04 | −.03, .12) | $\chi^2(3) = 0.70$, NS | 0.00 | 0 | NS | NS | No |
| NoBP | 89 | | .05 | .03, .08 | $\chi^2(88) = 216.09, p < .001$ | 59.28 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive; YesBP = best practices; NoBP = not best practices.

Table 9
*Publication Status as Moderator for Outcomes Related to Women's Body Dissatisfaction*

| Effect sizes | k | $r_+$ | $r_u$ | CI | Homogeneity test | $I^2$ | OFSN | RCT | RT | Bias? |
|---|---|---|---|---|---|---|---|---|---|---|
| Experiments | | | | | | | | | | |
| Published | 110 | .13 | .19 | .15, .23 | $\chi^2(109) = 349.06, p < .001$ | 68.77 | 93 | $p < .05$ | NS | Yes |
| Dissertation | 30 | | .08 | .02, .15 | $\chi^2(29) = 81.36, p < .001$ | 64.35 | 85 | NS | NS | No |
| Correlational | | | | | | | | | | |
| Published | 66 | | .05 | .02, .07 | $\chi^2(65) = 153.13, p < .001$ | 57.55 | 0 | NS | NS | No |
| Dissertation | 27 | | .08 | .03, .13 | $\chi^2(26) = 58.95, p < .001$ | 55.89 | 0 | NS | NS | No |

*Note*. k = number of independent effect sizes; $r_+$ = pooled correlation coefficient (corrected); $r_u$ = uncorrected effect size estimate; CI = 95% confidence interval for uncorrected effect size; $I^2$ = % of between study heterogeneity not due to chance; OFSN = Orwin's Fail-safe N; RCT = significance of Begg and Mazumdar's rank correlation test; RT = significance of Egger's regression; NS = non-significant; Inc = inconclusive.

males was seen for experimental studies, with effect sizes similar to those found in previous analyses of general effects (e.g., Grabe et al., 2008; Want, 2009). However, effects from correlational and longitudinal studies were not as strong, in most cases below the trivial cutoff. The effects even for experimental studies were small, and below the $r = .20$ threshold for practical significance, although mainly above the $r = .10$ threshold for trivial effects.

On balance the argument that media effects on body dissatisfaction are widespread, strong, and population-wide is not supported by the available evidence for either males or females. The scholarly community may benefit from greater emphasis on applying conservative language when discussing this issue with policymakers and the general public, noting that for the "average" man or woman, boy or girl, media effects in this realm are negligible. However, the absence of general effects does not mean an absence of effects on specific subgroups of viewers. This discussion now turns its attention to subgroup outcomes.

## Are Women With Preexisting Proclivities Toward Body Dissatisfaction Influenced by Media Thin-Ideals?

The number of studies that actually examines the issue of subgroup influences as opposed to general influences is relatively few, so few that this issue could not be addressed with males. Further, most of the studies that examined this issue were experimental, limiting the data on this hypothesis to experimental studies.

However, the data across experimental studies support the assertion of Roberts and Good (2010). The influence of media on women with preexisting body dissatisfaction issues or other proclivities (such as neuroticism) were both nontrivial and above the threshold for practical significance. By contrast, effects for women low in such preexisting concerns were negligible. Generally, preexisting concerns were defined across studies by scores between 1 to 2 standard deviations above the mean on outcome variables. This represents a proportion of women between a low of 2.2% through a high of 15.8%. From this it may be argued that

Table 10
*Meta-Regression Results for Continuous Moderators on Women's Body Dissatisfaction*

| Moderator | *Z*-value | *p*-value | Q (model) | Q (residual) | *p*-value (model) |
|---|---|---|---|---|---|
| Number of controls (correlational) | −4.06 | .001* | 16.49 | 200.31 | .001* |
| Number of controls (longitudinal) | −.28 | >.05 | 0.08 | 18.09 | >.05 |
| Exposure duration (experimental) | −1.46 | >.05 | 2.14 | 311.40 | >.05 |

* Denotes statistical significance.

there is greater value in divesting media effects research from the general effects view and examining ways in which specific subgroups of at-risk individuals may be influenced by, or interact with, the media.

## Do Methodological Issues Influence Effect Size Estimates?

As to the issue of publication bias, the tendency for a research field to "prefer" statistically significant articles over those with null effects, the use of the Tandem Procedure (Ferguson & Brannick, 2012) here suggested that some subsets of body dissatisfaction research showed evidence for publication bias, but others did not. However, this analysis of subgroups found that publication bias was particularly an issue for experimental studies involving Caucasian college student women, which consists of the largest single group of studies. Thus, although as noted earlier, such experimental studies provided the best evidence for effects, these effects may be inflated through the process of publication bias. Given that larger sample studies did not differ noticeably in methodology from smaller sample studies, the alternate hypothesis of a small-study effect (which can be misinterpreted as publication bias, although is in and of itself a potential issue for the interpretation of effect sizes) appears less likely than "true" publication bias. This is not out of step with the results of Grabe et al. (2008) who also found evidence for publication bias in this field.

This issue of null studies and what to do with them is certainly nothing unique to the field of body dissatisfaction. Given the vagaries of null hypothesis significant testing, and the continued focus on statistical significance despite its known limitations (e.g., Cohen, 1994; Ferguson, 2009; Loftus, 1996), the utility of null studies remains controversial. Focus on the reporting of statistical significance may impede the publication of null studies (Hubbard & Armstrong, 1997), particularly in "hot" or controversial research field (Ioannidis, 2005).

As to other methodological issues, issues such as demand characteristics or the introduction of confounds in experimental studies may also cause an inflation of effect size estimates. Results of the current analysis suggest this is more than an idle concern, as studies implying more "best practices" rigorous controls produced weaker effect sizes than those that did not. The issue of demand characteristics is always a difficult one. The use of greater distracter measures and distracter tasks may go some way toward alleviating these issues. By contrast, the issue of experimental comparison controls may be more controversial. Many studies compare thin-ideal media against nonhuman objects rather than non–thin-ideal media portrayals of humans. With a comparison of thin-ideal humans with nonhuman objects, we cannot ascertain whether any effects are due to the "thin ideal" or simply the presence of another human. For instance, would sexually attractive women who are otherwise of average weight have an impact on women's body dissatisfaction? It is not possible to know whether thinness or sexual appeal is the key variable without careful controls.

## Ethnic Differences

Previous research has indicated that African American women, relative to Caucasian women, appear to have fewer body dissatisfaction issues, although these differences may have narrowed somewhat in recent years (Roberts et al., 2006). The current meta-analysis is the first to examine differences across ethnicities in regards to susceptibility to media effects. Relatively few studies contrasted ethnic groups in terms of media effects on body dissatisfaction, although enough did to examine the issue meta-analytically albeit with some caution given the low number of studies for some ethnic categories. Among experimental studies, Caucasian women demonstrated greater susceptibility to media effects than did African American women, although the effects for Caucasian women were still small. However, in correlational studies, effects were minimal across all ethnic groups. The relatively low number of effect sizes specific to particular ethnic groups highlights the continued need for high-quality studies of effects that specifically consider particular ethnic groups.

## Implications for Policy and Science

Results of this analysis hold several important implications for science and public policy. The first of these is an emphasis on greater caution in public statements regarding the potential for negative effects of media. In light of

the negligible evidence for general effects, statements from the APA's (2007) position statement on sexualization of girls such as "In the emotional domain, sexualization and objectification undermine confidence in and comfort with one's own body, leading to a host of negative emotional consequences, such as shame, anxiety, and even self-disgust" (p. 3) and "Several studies (on both teenage and adult women) have found associations between exposure to narrow representations of female beauty (e.g., the 'thin ideal') and disordered eating attitudes and symptoms. Research also links exposure to sexualized female ideals with lower self-esteem, negative mood, and depressive symptoms among adolescent girls and women. In addition to mental health consequences of sexualization, research suggests that girls' and women's physical health may also be negatively affected, albeit indirectly" (p. 3) appear to overstate the data. Faraday (2010) suggests the APA's policy statement may fall into a pattern he refers to as the "girl crisis movement" which, though well meaning in intent to promote the welfare of girls and women, may inadvertently patronize girls by portraying them as particularly susceptible to societal harm despite evidence for considerable strengths. It is not the intent of this article to suggest we should not remain alert to societal sexism, or issues of particular value to girls or women, but rather to suggest that the discrepancies between public discourse and actual data on media effects might be understood in the context of well-meaning, but nonscientific, public advocacy.

The APA (2007) statement perhaps unintentionally raises an interesting question, due to the conflation of "thin" and "sexualized" ideals in their policy statement. Would "thin" models have less impact if they were otherwise sexually unattractive? Would sexually attractive models have less impact on body dissatisfaction so long as their weight falls within World Health Organization recommended guidelines rather than being rail thin? This conflation of thin ideal and sexual ideal has not yet been well-addressed in the literature.

At the same time media effects cannot be entirely dismissed, as the data supported the position of Roberts and Good (2010) that a subset of women with preexisting body dissatisfaction issues can potentially have those issues primed by media portrayals of thin ideals. With this in mind, it may be wise for the scholarly community to focus to a greater extent on subgroup analyses of at-risk individuals rather than more general effects. This may require as well some shift from traditional sociocultural models of body dissatisfaction to more of a diathesis-stress model (Ferguson et al., 2011). The notion that body dissatisfaction is something that is modeled through the activation of cognitive schema formed through repetitive exposure to media images does not appear to be bearing enough significant fruit. It is important to note that even for the subset of vulnerable women, the available evidence is not sufficient to indicate thin-ideal media is a *root cause* of body dissatisfaction, rather that thoughts of body dissatisfaction may be primed more easily in women with such predispositions. Interventions that focus on the cognitions and social environments that contribute to these predispositions may bear greater fruit.

Discussions of a link between media and clinical eating disorder symptoms may, in particular, need to be rethought. Although such views are often stated with great conviction (Roberts & Good, 2010), comparatively few studies examine this issue and, by and large, offer little support for this position. This is not a trivial point, for an overfocus on media effects has the potential to distract policymakers and society from other more critical factors involved in eating disorder symptomology. Given a limited economic climate, the investiture of research funding and clinical attention into a false path can carry with it significant costs.

## Limitations

As with all studies, this one has its limitations. Chief among these was the relative lack of studies outside of the area of body dissatisfaction in women. There would have been value in meta-analytically examining the moderator variables for body dissatisfaction among the other outcome variables, as well as for men. Yet the lack of sufficient studies made this impossible for the present analysis. Second, moderator analyses were developed from previous research; however, it is always possible that important potential moderators may have been missed. In addition, several of the moderators were categorical rather than continuous, which limits the utility of quantitative metare-

gression techniques in evaluating them. Further, it should be noted that some moderator categories were based on very small numbers of studies. Such results should be considered carefully, as results based on a small number of observations could prove unreliable. They are presented here to indicate the current status of the field, but should be interpreted with caution. Lastly, any meta-analysis must be alert for selection bias issues, which have the potential to influence effect size estimates. The present article is the most comprehensive to date on the topic of body dissatisfaction and used only indexed articles and dissertations in order to reduce selection bias. Thus it is felt, for the present analysis, selection bias is unlikely to be a serious limitation.

## Concluding Statements

The potential impact of thin-ideal media (or muscular-ideal media for men) on body dissatisfaction remains an issue of intense public interest and scholarly debate. Results from the current study suggest that public discussion of this issue, including by the APA, have often exceeded the available data. Effects of thin-ideal media appear to be limited to a subgroup of women with preexisting body dissatisfaction susceptibility. The effects of thin or body ideal media on men and most other women are negligible. Furthermore, systematic methodological issues particularly related to demand characteristics and weak experimental controls may have the potential to inflate effect size estimates. It is recommended that future statements by the scholarly community reflect a greater degree of caution in proclaiming links between media and body dissatisfaction or eating disorders. It is hoped that this article will contribute positively to discussions of this topic.

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Received March 9, 2012
Revision received October 5, 2012
Accepted October 9, 2012 ■