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Effects of Weapons on Aggressive Thoughts, Angry Feelings, Hostile Appraisals, and Aggressive Behavior: A Meta-Analytic Review of the Weapons Effect Literature

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Abstract

Guns are associated with aggression. A landmark 1967 study showed that simply seeing a gun can increase aggression—called the "weapons effect." This meta-analysis integrates the findings of weapons effect studies conducted from 1967 to 2017. It includes 162 effect-size estimates from 78 independent studies involving 7,668 participants. The theoretical framework used to explain the weapons effect was the General Aggression Model (GAM), which proposes three routes to aggression—cognitive, affective, and arousal. The GAM also proposes that hostile appraisals can facilitate aggression. As predicted by the GAM, the mere presence of weapons increased aggressive thoughts, hostile appraisals, and aggression, suggesting a cognitive route from weapons to aggression. Weapons did not significantly increase angry feelings. Only one study tested the effects of weapons on arousal. These findings also contribute to the debate about social priming by showing that incidental exposure to a stimulus (weapon) can affect subsequent related behavior (aggression).

Keywords
weapons effect, aggression, General Aggression Model, social priming

> "Guns not only permit violence, they can stimulate it as well. The finger pulls the trigger, but the trigger may also be pulling the finger."
>
> —Leonard Berkowitz (1968, p. 22)

Obviously, using a gun can increase aggression and violence, but can just seeing a gun increase aggression? In 1967, Leonard Berkowitz and Anthony LePage conducted a randomized experiment to find out. Male college students were tested in pairs, but one of them was actually an accomplice of the experimenter who was pretending to be another participant. They evaluated each other's performance on a task (e.g., listing ideas a used car salesperson might use to sell more cars). The "evaluations" were the number of stressful electrical shocks given, which ranged from 1 to 10. First, the accomplice evaluated the participant's performance by using either seven shocks (provocation condition) or no shocks (no provocation condition). Next, the participant "evaluated" the accomplice's performance. The number of electrical shocks the participant chose for the accomplice was used to measure aggression. The participant was seated at a table that had a shotgun and a revolver on it, or badminton rackets and shuttlecocks. The items on the table were described as part of another study that another experimenter had supposedly forgotten to put away. There was also a control condition with no items on the table. The experimenter told participants to ignore the items on the table, but apparently they could not. Provoked participants who saw the guns were more aggressive than the other participants. Berkowitz and LePage called this effect the "weapons effect." Mere exposure to weapons such as guns can increase aggression.

In later experiments, similar results were obtained when pictures of guns were used instead of actual guns (Leyens & Parke, 1975). Several field experiments tested the weapons effect outside of the lab using rifles placed in racks in the back windows of a pickup truck driven by an accomplice who refused to move when a traffic light turned green, and

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using horn honking as the aggression measure (e.g., Turner, Layton, & Simons, 1975).

A prior meta-analysis, published in 1990, integrated the findings from weapons effect studies (Carlson, Marcus-Newhall, & Miller, 1990). Our meta-analysis provides a significant and much-needed update to this important but decades old meta-analysis, four methodological and two theoretical. This meta-analysis makes four important methodological improvements over the 1990 meta-analysis. First, the sheer number of studies integrated is over 5 times more in this meta-analysis. The 1990 meta-analysis included 31 effect-size estimates, whereas this meta-analysis includes 162 effect-size estimates. Second, the 1990 meta-analysis excluded unpublished studies. It is well documented that studies reporting statistically significant results are more likely to be published than are studies reporting nonsignificant results. This "prejudice against the null hypothesis" seems pervasive (Greenwald, 1975; Kepes, Banks, McDaniel, & Whetzel, 2012). In meta-analysis, the conditional publication of studies with significant results is called the "file drawer problem" (Rosenthal, 1979). The most extreme version of this problem would result if only 1 out of 20 studies conducted was published and the remaining 19 studies were located in researchers' file drawers (or garbage cans), assuming the .05 significance level is used. If publication bias is a problem, then the studies included in a meta-analysis may represent a biased subset of the total number of studies conducted on the topic. That is why we collected as many unpublished studies as possible for the present meta-analysis.

Third, we conducted a comprehensive sensitivity analysis to assess the robustness of our mean effect sizes. The robustness of published results in the social sciences, including social psychology, has been questioned (Ferguson & Heene, 2012; Fiedler, 2011; Ioannidis, 2012; Kepes & McDaniel, 2013; Open Science Collaboration, 2015; Pashler & Wagenmakers, 2012; Simmons, Nelson, & Simonsohn, 2011; Yong, 2012). Publication bias is currently one of the phenomena that has been shown to have adversely affected our published meta-analytic results (e.g., Banks, Kepes, & McDaniel, 2015; Ferguson & Brannick, 2011) and, thus, distorted our cumulative knowledge (Kepes & McDaniel, 2013). To assess the robustness of our results, we followed "best practice" recommendations (e.g., Greenhouse & Iyengar, 2009; Kepes, Bushman, & Anderson, 2017; Kepes, McDaniel, Brannick, & Banks, 2013) and conducted a comprehensive sensitivity analysis on the effect sizes at the study level to evaluate the robustness of the results for each individual distribution. We used seven publication bias methods, each of which is capable of estimating a "for publication bias adjusted" mean effect. Because between-studies heterogeneity is known to have adverse effects on publication bias methods (as well as the "basic" or naïve meta-analytic methods; for example, Borenstein, Hedges, Higgins, & Rothstein, 2009; Kepes & McDaniel, 2015; Terrin, Schmid, Lau, & Olkin, 2003), we performed all these analyses at the subgroup level to control for moderating effects. Fourth, we accounted for heterogeneity due to outliers by performing all analyses with and without identified outliers. Extreme outliers can have a profound effect on meta-analytic results, and we wanted to make sure our findings were not unduly influenced by outliers. Therefore, we feel strongly that our meta-analytic study is not only an update on the almost 30-year-old Carlson et al. (1990) meta-analysis, which is in desperate need of an update, but also a methodological template for other meta-analyses. To obtain more robust and accurate meta-analytic results, we suggest that future meta-analyses should follow the procedures we have outlined in this meta-analysis.

Two other very important advances of this meta-analysis over the 1990 Carson et al. meta-analysis are theoretical advances. First, the 1990 meta-analysis did not directly compare the magnitude of the weapons effect for provoked and nonprovoked participants. Berkowitz and LePage (1967) only found a weapons effect for provoked participants. This meta-analysis tests whether weapons have a greater effect on provoked participants than on nonprovoked participants. Second, the 1990 Carson et al. meta-analysis only tested the effects of weapons on aggressive behavior. The present meta-analysis also tests the effects of weapons on aggressive thoughts, angry feelings, and hostile appraisals. This extension is important because it sheds light on *why* weapons increase aggression. For example, the most common explanation of the weapons effect is that weapons prime aggressive thoughts. Our meta-analysis directly tests this hypothesis. The theoretical foundation for this meta-analysis is the General Aggression Model (GAM; Anderson & Bushman, 2002), which is described next.

GAM

The GAM provides a useful framework for understanding the weapons effect (see Figure 1). The GAM subsumes other models that have been used to explain the weapons effect, such as those based on classical conditioning, operant conditioning, and priming (e.g., Berkowitz, 1974, 1982, 1983). In the GAM, two types of input variables can influence aggression: personal and situational. Personal variables include anything the individual brings to the situation (e.g., gender, age, genetic predispositions, personality traits and other individual differences, attitudes, beliefs, values). This meta-analysis focuses on three personal variables—gender, age, and whether participants were college students or not. Some critics have argued that college students, who are often recruited from Introductory Psychology participant pools, are not representative of "real people" (e.g., Oakes, 1972; Sears, 1986). We tested whether the weapons effect occurs for males and females, for participants of different ages, and for student and nonstudent samples.

Situational variables include all external factors that can influence aggression (e.g., aggressive cues such as weapons,

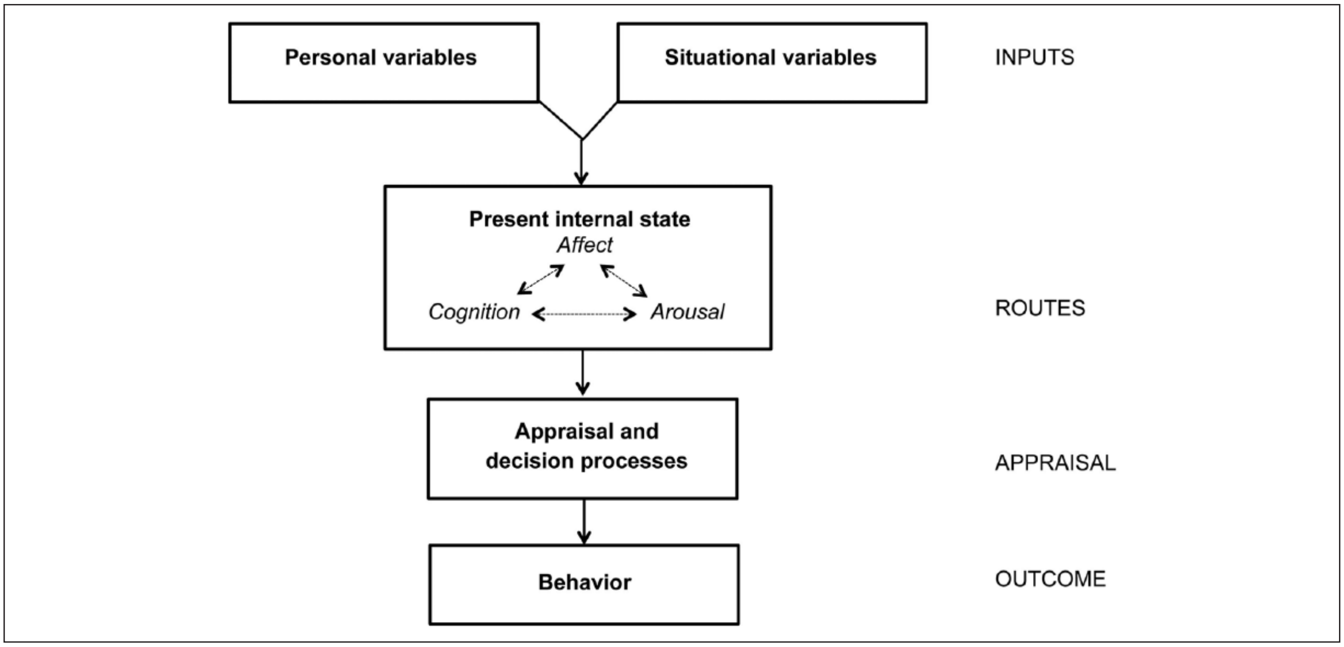


Figure 1. The General Aggression Model (GAM).
Source. Anderson and Bushman (2002), Krahé (2013).

violent media exposure, provocation, frustration, alcohol, hot temperatures, crowding). This meta-analysis focuses on two situational variables—exposure to weapons and provocation. Specifically, we test whether the weapons effect occurs for provoked and nonprovoked individuals. When provoked, individuals become physiologically aroused and ready to attack others. Thus, provoked individuals might be particularly prone to react aggressively when primed with weapons.

According to the GAM (Anderson & Bushman, 2002), personal and situational factors influence one's internal state, which can include aggressive cognition, aggressive affect, and physiological arousal levels. Thus, there are three possible routes to aggression—through aggressive cognition, aggressive affect, and physiological arousal. However, these routes are not mutually exclusive or even independent, as indicated by the dashed lines in Figure 1. For example, someone who has aggressive ideas might also feel angry and have elevated blood pressure. This meta-analysis examines the effects of weapons on aggressive cognition and aggressive affect. Unfortunately, only one study examined the influence of weapons on self-reported arousal (De Oca & Black, 2013). In that study, participants rated threatening items (e.g., weapons) to be more arousing than nonthreatening items (e.g., trees, food, couches). We could find no studies that tested the effects of weapons on physiological arousal (e.g., heart rate, blood pressure, skin conductance).

According to the GAM, internal states can influence appraisal and decision processes. First, there is an immediate initial appraisal of whether the situation is dangerous, threatening, or warrants aggression. This initial appraisal might lead directly to an automatic or impulsive behavior, or it might lead to a reappraisal. If the initial appraisal is judged to be unsatisfactory and if the person has sufficient time and cognitive resources, reappraisal occurs (Barlett & Anderson, 2011). During reappraisal, the person considers alternative explanations of the situation and alternative behavioral options. When the appraisal is judged to be satisfactory, or when time or resources become insufficient, the appraisal process terminates and the person engages in the behavior, which completes one cycle. This meta-analysis examined the influence of weapons on hostile appraisals of others. Note that in the GAM, hostile appraisals are more proximal to aggressive behavior than are internal states. Thus, hostile appraisals might have a stronger influence on aggression than internal states.

The types of appraisals and decisions people make can influence their behavior. The primary outcome variable in our meta-analysis was aggressive behavior. Most researchers define aggression as any behavior intended to harm another person who wants to avoid being harmed (Baron & Richardson, 1994).

Moderators

In addition to theoretical outcomes and moderators encompassed by the GAM, we also considered several study characteristics that might influence the magnitude of the weapons effect, including publication status (i.e., published in a peer-reviewed journal vs. unpublished), the year the study was conducted (to test whether the magnitude of the weapons effect has changed over time), and whether the researchers used a between-participants design or a within-participants design. We coded several characteristics about the weapons used in studies (i.e., actual weapon vs. photo of weapon, real weapon vs. toy weapon, type of weapon).

In addition to these moderators, we examine the study setting. Laboratory experiments have been criticized because they are conducted in artificial settings, with unrealistic measures, and unrepresentative samples—mainly college students (for a review, see Anderson & Bushman, 1997). Moreover, participants in laboratory experiments can become suspicious about being deceived, which can contaminate the results. Field studies overcome these criticisms; yet, they are not without their own shortcomings. For instance, another important difference between laboratory experiments and field studies is control over possible confounding variables. Previous research has shown that aggression effects tend to be larger in the lab where conditions are more tightly controlled than in the field (Anderson & Bushman, 1997). In this meta-analysis, we coded whether the study was conducted in a laboratory or field setting.

Overview

The primary purpose of this meta-analysis was to examine the effects of the mere presence of weapons on aggressive thoughts, angry feelings, hostile appraisals, and aggressive behavior. Although violent media (e.g., television programs, movies, video games, the Internet) also include weapons, studies that examined the effects of violent media on aggression were not included in this meta-analysis. Numerous meta-analyses have already shown that exposure to violent media can increase aggressive thoughts, angry feelings, physiological arousal, hostile appraisals, and aggressive behavior (e.g., Anderson et al., 2010; Bushman, 2016; Bushman & Huesmann, 2006; Greitemeyer & Mügge, 2014). We were interested in a more basic question: Can the mere presence of a weapon—that is not being used by one person to injure or kill another person—increase aggression? We predicted that the mere presence of weapons would increase aggression.

In an attempt to understand *why* weapons might increase aggression, we considered their effects on aggressive thoughts, angry feelings, and hostile appraisals using the GAM as a theoretical guide. Based on previous research (e.g., Anderson, Benjamin, & Bartholow, 1998), we predicted a cognitive route between exposure to weapons and aggression. Specifically, we predicted that weapons would prime or activate aggressive thoughts and increase hostile appraisals.

Because provoked individuals become physiologically aroused and ready to attack others, they might be particularly prone to react angrily and aggressively when primed with weapons. Thus, we predicted a stronger weapons effect among provoked participants than among nonprovoked participants.

We also examined several possible moderators of the weapons effect, including the gender and age of participants, whether participants were college students or not, and study characteristics (i.e., publication status, year study was conducted, between- or within-participants design, laboratory vs. field setting, type of weapons exposed to). However, we made no predictions about these moderators.

Method

Literature Search

To locate relevant studies, we searched the PsycINFO, PsycARTICLES, MEDLINE and SocINDEX, Google Scholar, and Dissertation Abstracts International from 1967 (the year the first weapons effect study was published by Berkowitz & LePage) through 2016. A thorough search was conducted to be sure that no relevant studies were excluded. We used the search terms (*gun** OR *firearm** OR *weapon**) AND (*aggress** OR *violen**). The asterisk allows terms to have all possible endings (e.g., the term *aggress** will retrieve studies that used the terms *aggress, aggressed, aggressor, aggressive*, and *aggression*). Thus, the article had to include the term *gun** or *firearm** or *weapon**, plus the term *aggress** or *violen**. We also searched Social Science Citation Index for any article that cited the original weapon's effect study by Berkowitz and LePage (1967).

Five additional steps were taken to obtain any studies we might have missed. First, we searched the reference sections of relevant meta-analytic reviews (Anderson & Bushman, 1997; Bettencourt & Kernahan, 1997; Bettencourt & Miller, 1996; Bettencourt, Talley, Benjamin, & Valentine, 2006; Bushman & Anderson, 1998; Carlson et al., 1990) and narrative reviews (Berkowitz, 1971; Toch & Lizotte, 1992; Turner & Leyens, 1992; Turner, Simons, Berkowitz, & Frodi, 1977). Second, we searched the reference sections of all retrieved studies. Third, we contacted all researchers who had conducted a weapons effect study from our list of retrieved studies, and requested from them any published and unpublished weapons effect studies. Fourth, we searched the proceedings for eight relevant conferences for unpublished studies: (a) American Psychological Association (APA), (b) Association for Psychological Science (APS), (c) European Association of Social Psychology, (d) International Society for Research on Aggression, (e) Society of Australasian Social Psychologists, (f) Society of Experimental Social Psychology, (g) Society for Personality and Social Psychology (Division 8 of APA), and (h) Society for the Psychological Study of Social Issues. Fifth, we sent an announcement requesting unpublished and published weapons effect studies to seven listserves: (a) European Association of Social Psychology, (b) International Society for Research on Aggression, (c) Society of Australasian Social Psychologists, (d) Society of Experimental Social Psychology, (e) Society for Personality and Social Psychology (Division 8 of APA), (f) Society for the Psychological Study of Social Issues, and (g) Society for the Study of Peace, Conflict and Violence: Peace Psychology Division (Division 48 of APA).

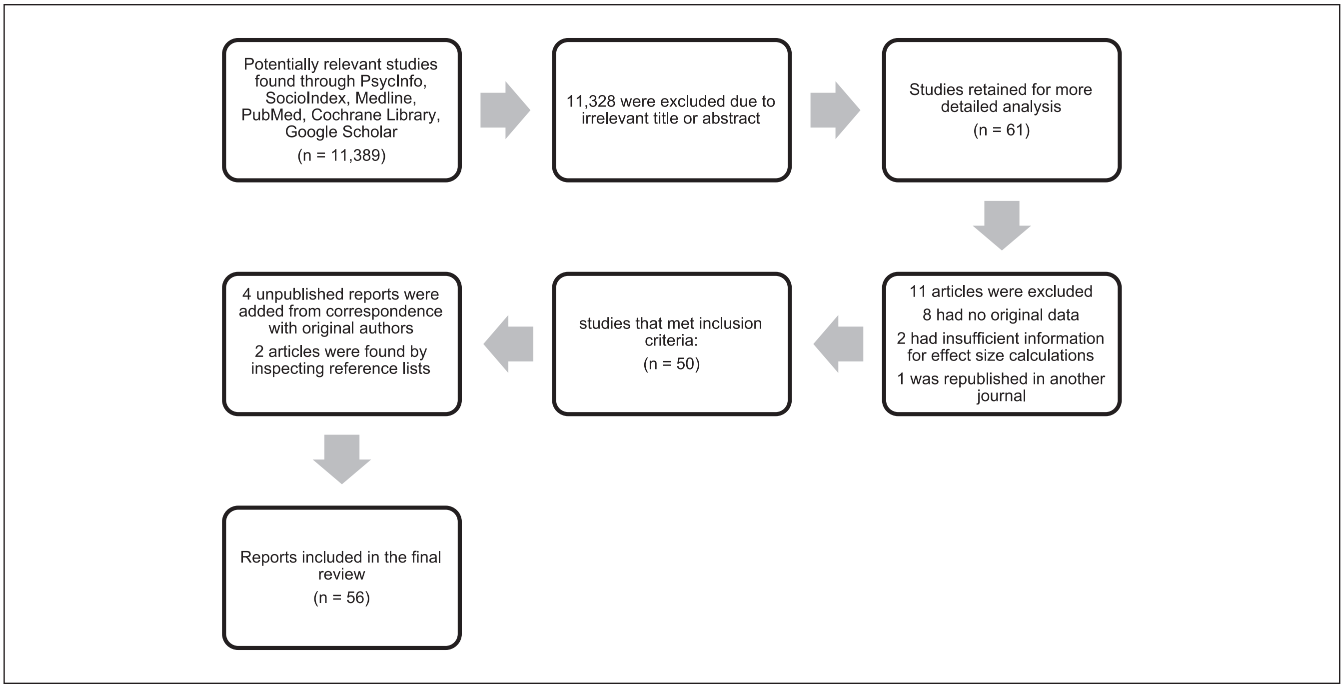


Figure 2. PRISMA flowchart of literature search and inclusion/exclusion decisions.

This thorough search yielded 11,389 articles, but not all were relevant to this meta-analysis. To determine whether articles were relevant, we read their titles, abstracts, or both. Unpublished studies, dissertations, and conference papers were also included in the database to address potential publication bias (i.e., the "file drawer problem," Rosenthal, 1979). We found 13 unpublished research reports from master's theses, doctoral dissertations, conference proceedings, and personal communication. These 13 reports yielded 16 studies and 40 tests of the weapons effect. A PRISMA flow chart of the literature search and study coding is shown in Figure 2.

Inclusion Criteria

Two inclusion criteria were used. First, a study needed to include a weapons condition (e.g., guns, knives, swords, hand grenades) and a no weapons (control) condition (e.g., nonviolent objects such as badminton rackets, flowers, eating utensils, nothing at all). Some studies used a between-participants design, where participants were randomly assigned to weapons or no weapons conditions. Other studies used a within-participants design, where participants were exposed to both the weapons and no weapons conditions in a random order. The weapons could be real weapons or toy weapons, physically present or shown in photographs. Second, a study needed to include a measure of aggressive cognition, aggressive affect, physiological arousal, hostile appraisal, or aggressive behavior. Some studies included more than one measure.

Two articles reported identical analyses from the same set of data (Simons & Turner, 1975, 1976). The more recent of the two articles was included in this meta-analysis because it provided a more complete set of analyses.

The final sample included 56 research reports that described 78 independent studies involving 7,668 participants. We computed 162 effect-size estimates from these 56 research reports.

Moderators

Type of weapons. We coded whether the weapons were guns, knives, or a mixture of various weapons (e.g., guns, knives, swords, grenades, clubs). Most studies used either guns or knives exclusively, although some used both guns and knives (e.g., Blanchette, 2006; Sulikowski & Burke, 2014). Studies using mixtures of weapons were varied. For example, one study used guns, clubs, and swords as stimuli (Anderson et al., 1998). We coded whether participants were exposed to actual weapons or photos of weapons. We also coded whether participants were exposed to real weapons or toy weapons. Studies using mixtures of toy weapons also varied, including toy guns, daggers, bazookas, and so on (e.g., Goff, 1995; Mendoza, 1972).

Type of outcome. This meta-analysis examined four types of outcomes: (a) aggressive cognition, (b) aggressive affect, (c) hostile appraisals, and (d) aggressive behavior. Only one study investigated the effect of weapons on physiological arousal, so we could not include it as an outcome. That study found that participants rated weapons as more arousing than nonthreatening objects such as trees and food (De Oca & Black, 2013). Although some researchers have included other outcomes, there were not a sufficient number of these other outcomes to include in our meta-analysis. For example, one study found that testosterone levels increased more in men who handled a gun than in men who handled a nonviolent toy (Klinesmith, Kasser, & McAndrew, 2006). Testosterone has repeatedly been linked to aggression in research studies (Archer, 1988; Sapolsky, 1998). Next, we describe prototypical ways of measuring each outcome.

Aggressive cognition is most often measured using reaction times to aggressive and nonaggressive words (e.g., Anderson et al., 1998; Bartholow & Heinz, 2006). In other studies, participants completed word fragments by filling in missing letters to form words as quickly as possible (e.g., Benjamin, Crosby, & Bushman, 2016). For example, the word fragment K I _ _ can be completed to form a nonaggressive word (e.g., KISS, KIND, KITE), or can be completed to form an aggressive word (e.g., KILL, KICK).

Aggressive affect is most often measured using mood scales. For example, participants rate how they felt "right now" using a list of adjectives, including some that measure aggressive affect (e.g., angry, furious, irritable; for example, Anderson, Anderson, & Deuser, 1996). Although weapons can also influence other emotions (e.g., anxiety, empathy), no studies included in this meta-analysis examined other emotions.

Hostile appraisals are measured in several different ways. For example, some studies have measured primary or automatic appraisals by speed of fist clenching (e.g., da Gloria, Duda, Pahlavan, & Bonnet, 1989) and by speed of identification of weapons versus neutral objects (e.g., De Oca & Black, 2013; Sulikowski & Burke, 2014). Other studies have measured secondary or controlled reappraisal by having participants indicate how disagreeable, hostile, and angry they thought a target person was (Epstein, 1980; Holbrook et al., 2014). Unfortunately, there were not enough studies to examine primary and secondary appraisals separately.

In laboratory experiments, aggression has most typically been measured by electric shocks (e.g., number, intensity, duration) given to an accomplice pretending to be another participant. Other studies have used other aversive stimuli to measure aggression, such as noise blasts (Epstein, 1980; Lindsay & Anderson, 2000) or extremely spicy hot sauce (Klinesmith et al., 2006). Nonphysical measures of aggression have included negative evaluations of experimenters or accomplices (Fischer, Kelm, & Rose, 1969). In field experiments involving adults, aggression has been measured either by the number of horn honks at an accomplice who is stalled at a traffic light (Halderman & Jackson, 1979; Turner et al., 1975) or the number of wet sponges thrown at a victim (Simons, Fenn, Layton, & Turner, 1976). In field experiments involving children, aggression has been measured using behaviors observed in interactions with other children, such as pushing, shoving, kicking, and hitting (e.g., Turner & Goldsmith, 1976).

Provocation. For each study, we coded whether participants were provoked or not. Like Berkowitz and LePage (1967), several researchers used electric shocks to provoke participants (e.g., Berkowitz & LePage, 1967; Frodi, 1975; Turner & Simons, 1974). Other researchers have used other unpleasant stimuli to provoke participants, such as noise blasts (e.g., Bartholow, Anderson, Carnagey, & Benjamin, 2005; da Gloria et al., 1989) or personal insults (e.g., Caprara, Renzi, Amolini, D'Imperio, & Travaglia, 1984).

Participant gender, age, and college student status. To test for potential gender differences in the weapons effects, we examined males and females separately. Most weapons effect experiments included only male participants (e.g., Berkowitz & LePage, 1967; Buss, Booker, & Buss, 1972; Klinesmith et al., 2006; Turner & Simons, 1974). However, a number of other experiments included both male and female participants (e.g., Caprara et al., 1984; da Gloria et al., 1989; Lindsay & Anderson, 2000), and one experiment included only female participants (Gallina & Fass, 2014). Because males are typically more physically aggressive and more likely to use weapons than females, weapons might have greater effects on males than on females (Caprara et al., 1984).

To test for potential age differences, we coded the average age of participants in each study. Although most weapons effect studies have used adult participants, some have used children or adolescents.

Because some critics have argued that college students, who are often recruited from Introductory Psychology participant pools, are not representative of "real people" (e.g., Oakes, 1972; Sears, 1986), we also examined college students and other participants separately. Thus, we not only tested the potential for the weapons effect to change as a function of age, but we also tested whether college students (where the average age is typically between 18 and 21) differed from noncollege students. By doing so, we hoped to gain further insight into the generalizability of the weapons effect as a function of age as well as across college student and noncollege student samples.

Study characteristics. We coded whether the study used a between- or a within-subjects design. We coded whether the study was published in a peer-reviewed journal or not. We also coded the year on the research report to determine whether the magnitude of the weapons effect has changed over time. Finally, we coded whether the study was conducted in a laboratory or field setting.

Intercoder Reliability

Two independent judges coded all studies included in the meta-analysis. There was 100% agreement on all coded characteristics.

Analysis Strategy

We used Cohen's d as the effect-size estimate, which gives the number of standard deviations between the weapons and no weapons conditions. When means, standard deviations, and sample sizes were not reported, we contacted the authors and requested the missing data. Otherwise, we estimated Cohen's d from test statistics using standard formulas (Borenstein et al., 2009). Each effect size was weighted by the inverse of its variance, which is the optimal weight (Hedges & Vevea, 1998).

We used random-effects (RE) meta-analytic procedures, which assume that effect sizes differ from population means by both subject-level sampling error and study-level variability (Borenstein, Hedges, & Rothstein, 2007). In contrast, fixed-effects (FE) models assume only subject-level sampling error. RE models are more conservative than FE models, but they require fewer statistical assumptions and allow for generalizations to a broader set of studies than only the ones included in the meta-analysis (Hunter & Schmidt, 2004).

For studies that reported multiple effect sizes, we used a shifting unit of analysis approach (Cooper, 1998). Each statistical test was coded as if it were independent. For example, suppose male and female participants in one study were exposed to guns versus neutral objects (control), and were provoked or not. After exposure, participants completed a measure of aggression (e.g., number of electric shocks given to an accomplice). In this study, four effect-size estimates would be coded (i.e., provoked/males, unprovoked/males, provoked/females, unprovoked/females). For the overall effect, the four effect-size estimates would be averaged so that the study provides only one effect-size estimate. For an analysis examining the effects of weapons on provoked versus unprovoked participants, the study would provide two effect-size estimates (i.e., provoked vs. unprovoked, combining males and females). For an analysis testing for gender differences in the weapons effect, the study would also provide two effect-size estimates (i.e., males vs. females, combining provoked and unprovoked conditions). The shifting unit of analysis was also used for research reports that included more than one outcome. The shifting unit of analysis retains as much data as possible without violating the independence assumption that underlies the validity of meta-analytic procedures.

Finally, to assess the robustness of our results to publication bias and outliers, we conducted a comprehensive sensitivity analysis (Greenhouse & Iyengar, 2009; Kepes et al., 2013) to determine the trustworthiness of our obtained results (Kepes & McDaniel, 2013).

Results

There was a significant effect of weapons on aggressive cognition, affect, appraisals, and behavior when the effects for these outcomes were combined (k = 78, d = 0.30, 95% CI = [0.23, 0.37]). Overall, there appears to be a weapons effect for these aggressive outcomes.

Table 1 contains the results of the initial naïve meta-analysis for the different outcomes and different moderator variables. By "naïve," we mean the meta-analytic mean effect without any "adjustment" for potential biases (Copas & Shi, 2000). In the sensitivity analysis section, we discuss the impact of publication bias and outliers on these naïve estimates. For each analysis, we display the name of the analyzed distribution, the associated number of samples (k), and individual observations (N), and the mean effect size and corresponding 95% confidence interval. We also report Q-tests for differences between classes of each moderator. However, many of these Q-tests are underpowered.

Outcome Variables

As can be seen in Table 1, weapons increased aggressive thoughts, hostile appraisals, and aggressive behavior. However, weapons did not significantly increase angry feelings. The confidence interval for angry feelings includes zero, probably due to low statistical power. As Table 1 shows, the magnitude of the weapons effect was not the same for all outcome variables. Specifically, the magnitude of the weapons effect on hostile appraisals was significantly larger than for all other outcomes. To increase statistical power, we combined the outcomes when examining the possible presence moderators of the weapons effect. Table 1 contains the results for the moderators.

Moderator Variables

The magnitude of the weapons effect did not differ for provoked and unprovoked participants, and was significant for both groups. Although the magnitude of the weapons effect was twice as large for lab studies than for field studies (probably due to tighter control of possible confounds), the difference was not significant (probably due to low statistical power). The magnitude of the weapons effect did not differ for between-participants designs and within-subjects designs, and was significant for both types of designs. Although the effect was more than twice as large for photos of weapons than for actual weapons, the difference was nonsignificant (probably due to low statistical power). As we discuss below, this difference disappears when corrections are made for publication bias. The magnitude of the weapons effect did not differ for real and toy weapons, although this moderator is confounded with age of participants.

The magnitude of the weapons effect did not depend on whether the weapons were guns, knives, or a mixture of various weapons (e.g., guns, knives, swords, grenades). All of the 95% CIs excluded zero.

In terms of publication status, the weapons effect was larger for published studies than for unpublished studies. The confidence interval for published studies excluded zero, whereas the confidence interval for unpublished studies included zero.

Table 1. Effect-Size Estimates and CIs for the Categories of the Moderator Variables Coded.

Variable and class	Between classes effect (Q_b)	*k*	*N*	*d* and [95% CI]
Outcome variable	Q_b (3) = 8.69, $p < .04$			
Cognition		19	3,543	0.27_b [0.20, 0.34]
Affect		7	953	0.14_b [−0.12, 0.39]
Appraisal		22	1,424	0.46_a [0.32, 0.59]
Behavior		38	2,382	0.21_b [0.04, 0.37]
Provocation level	Q_b (1) = 0.37, $p < .55$			
None/low		58	5,713	0.27 [0.20, 0.35]
High		32	1,719	0.35 [0.12, 0.57]
Research setting	Q_b (1) = 1.92, $p < .17$			
Lab		64	6,768	0.33 [0.25, 0.40]
Field		14	900	0.15 [−0.08, 0.39]
Research design	Q_b (1) = 1.36, $p < .25$			
Between		50	5,684	0.26 [0.16, 0.37]
Within		28	1,984	0.35 [0.25, 0.45]
Photos vs. actual	Q_b (1) = 3.61, $p < .06$			
Photos of weapons		43	5,230	0.36 [0.27, 0.44]
Actual weapons		22	1,847	0.14 [−0.07, 0.35]
Real vs. toy	Q_b (1) = 0.41, $p < .53$			
Real weapons		65	7,077	0.31 [0.23, 0.39]
Toy weapons		13	591	0.25 [0.07, 0.42]
Weapon type	Q_b (2) = 2.65, $p < .27$			
Guns		60	5,498	0.26_a [0.17, 0.36]
Knives		6	808	0.27_a [0.10, 0.44]
Mixed		14	1,362	0.39_a [0.27, 0.50]
Publication status	Q_b (1) = 6.59, $p < .02$			
Published		62	5,267	0.36 [0.27, 0.44]
Unpublished		16	2,401	0.14 [0.00, 0.28]
Participant gender	Q_b (1) = 0.43, $p < .52$			
Males		6	3,55	0.46 [0.17, 0.77]
Females		6	391	0.34 [0.08, 0.60]
Participant college student status	Q_b (1) = 0.40, $p < .53$			
College student		52	4,201	0.32 [0.22, 0.41]
Nonstudent		26	3,467	0.27 [0.17, 0.38]

Note. Comparisons are for weapons versus nonweapon conditions. Positive effects indicate greater effects when exposed to weapons. Subscripts refer to differences between effect-size estimates within each category. Effect-size estimates with different subscripts are significantly different at the .05 significance level. CI = confidence interval.

Only a limited number of studies allowed for analyzable direct comparisons of the weapons effect between male and female participants. Of the six studies that allowed for such comparisons, there was no significant gender effect, although the effect size for males tended to be higher than for females. Both confidence intervals excluded zero; they also overlapped.

In order to address the question of whether the weapons effect generalized beyond college or university samples, we ran two analyses. One was a metaregression of mean age of participants on the size of the weapons effect. The other analysis was a direct comparison of college and noncollege student samples on the magnitude of the weapons effect. The mean age of participants did not significantly influence the magnitude of the weapons effect ($b = 0.006$, 95% CI = [−0.004, 0.016], $z = 1.19$, $p < .24$). The magnitude of the weapons effect also did not differ for studies that used college student samples and studies that used nonstudent samples (see Table 1). Both confidence intervals excluded zero.

We also tested whether the magnitude of the weapons effect has changed over time using publication year as a moderator variable. There was a significant positive relation between publication year and the magnitude of the weapons effect ($b = 0.006$, 95% CI = [0.001, 0.010], $z = 2.50$, $p < .02$), which suggests that the magnitude of the weapons effect has increased over time.

Sensitivity Analysis

As noted earlier, we conducted a comprehensive sensitivity analysis to assess the robustness of our results (Greenhouse & Iyengar, 2009; Kepes et al., 2017; Kepes et al., 2013). All sensitivity analyses were conducted in *R* using the metafor (Viechtbauer, 2015) and meta packages (Schwarzer, 2007) and with the recommended RE estimation model. First, we calculated the naïve observed meta-analytic mean estimate ($\bar{d}_o$) for each distribution as well as the associated statistics (e.g., 95% confidence interval, 90% prediction interval, heterogeneity indices). Next, we conducted a one-sample-removed analyses to examine the influence of each individual effect size on the obtained meta-analytic results (Borenstein et al., 2009; Kepes et al., 2013). Then, we assessed the potential for publication bias in each individual meta-analytic distribution. Following best practice recommendations (Kepes et al., 2012; Kepes & McDaniel, 2015), we used a comprehensive battery of methods, including trim-and-fill (Duval, 2005), cumulative meta-analysis (Kepes et al., 2012), selection models (Vevea & Woods, 2005), and PET-PEESE (precision-effect test, precision-effect estimate with standard error; Stanley & Doucouliagos, 2014). After these assessments, we used a multivariate battery of influence diagnostics to identify potential outliers (Viechtbauer, 2015; Viechtbauer & Cheung, 2010). We then deleted any identified outlier(s) and re-ran all analyses. Thus, we performed all analyses twice, once on the original distributions and once on the distribution without identified outlier(s).

For all methods, we used the recommended methodological and statistical options. For example, we implemented trim-and-fill with the recommended FE model and the L_0 estimator (Duval, 2005; Kepes et al., 2012), and also used the RE trim-and-fill model with the same estimator to assess the robustness of the obtained results from the recommended FE model (Moreno et al., 2009). In addition to the regular cumulative meta-analysis by precision (Kepes et al., 2012), we report the cumulative meta-analytic mean of the five most precise effect sizes (for a similar approach, see Stanley, Jarrell, & Doucouliagos, 2010). To implement the selection models, we use a priori models (e.g., Hedges & Vevea, 2005) with *p* value cut-points to model moderate and severe instances of publication bias as recommended (see Vevea & Woods, 2005). Finally, we note that all methods become less stable with small-sample sizes (i.e., small distributions), partly due to second order sampling error and low statistical power (Kepes et al., 2012; Schmidt & Hunter, 2015; Sterne et al., 2011). Most publication bias assessment methods should not be used with distributions containing less than 10 effect sizes, including funnel plot- and regression-based methods (Kepes et al., 2012; Sterne et al., 2011). Therefore, we urge extreme caution when interpreting results from distributions with less than 10 effect sizes.

Once we ran all analyses, rather than relying on a single mean estimate, we examined the range of results (i.e., the mean effect-size estimates, taking publication bias and outliers into consideration) to triangulate the location of the "true" mean effect-size estimate (Kepes et al., 2012). This approach is recommended to advance the methodological rigor of our sciences (Kepes et al., 2017; Orlitzky, 2012), and is aligned with customer-centric reporting of scientific evidence (Aguinis et al., 2010) and evidence-based practice (Kepes, Bennett, & McDaniel, 2014; Kepes & McDaniel, 2013). The results of these analyses are displayed in Table 2 (the bottom panel displays the results without the identified outliers). Columns 1 to 3 report the name of the analyzed distribution as well as the associated number of samples (*k*) and individual observations (*N*). Columns 4 to 6 display the naïve meta-analytic results, including the naïve observed meta-analytic mean ($\bar{d}_o$) and the associated 95% confidence (95% CI) and 90% prediction intervals (90% PI). Columns 7 to 9 display distinct assessments of heterogeneity, Cochran's *Q* statistic, I^2, and tau (τ). Column 10 shows the results of our one-sample-removed analysis (osr; minimum, maximum, and median $\bar{d}_o$). Columns 11 to 18 display the results from the trim-and-fill analyses; for the recommended FE and RE models. For each model, we report the side of the funnel plot on which the imputed effect sizes are located (FPS), the number of the imputed effect sizes (*ik*), the trim-and-fill adjusted mean effect-size estimates (t&f$_{FE}$ $\bar{d}_o$ or t&f$_{RE}$ $\bar{d}_o$) as well as the associated 90% CI. Column 19 reports the cumulative mean for the five most precise samples (pr_5 $\bar{d}_o$); columns 20 and 21 the results from the moderate (sm$_m$ $\bar{d}_o$) and severe selection (sm$_s$ $\bar{d}_o$) models; and column 22 the result of the PET-PEESE (pp $\bar{d}_o$) analysis. Finally, although not discussed due to space considerations, we have included the forest plots that display the contour-enhanced funnel plots with trim-and-fill imputations (using the recommended FE model with the L_0 estimator) as well as the cumulative meta-analyses by precision in the appendix (for interpretation guidelines, see Kepes et al., 2012).

Publication bias. Publication bias seems to have affected many of the naïve meta-analytic mean estimates to a noticeable degree (i.e., by more than 20%; Kepes & McDaniel, 2015). For instance, for studies with a "within-subjects design" (k = 28), the naïve meta-analytic mean ($\bar{d}_o$ = 0.35) estimate seems to be overestimated by between 0.05 (sm$_m$ $\bar{d}_o$ = 0.30, Δ = 0.05 or 14%) and 0.17 (sm$_s$ $\bar{d}_o$ = 0.18, Δ = 0.17 or 49%). However, the estimate from the severe selection model (sm$_s$ $\bar{d}_o$) could be an outlier as it does not converge well with the results of the other methods. Yet, even excluding this estimate from consideration, the degree of overestimation, on average, appears to be around 0.08 or 23% (Δ = 0.10 or 27% when including sms $\bar{d}_o$), which can be considered "moderate" (Kepes et al., 2012). Thus, taking publication bias into consideration and triangulating the "true" mean effect size for within-participants designs, it is likely to be around 0.25 instead of 0.35. Interestingly, this estimate (i.e., around 0.25) is very close to the naïve mean estimate for "between-participants designs" ($\bar{d}_o$ = 0.26, k = 50). Even more intriguing is the finding that the naïve mean for between-participants designs appears to be relatively unaffected by publication bias because all but one of the publication bias assessment methods indicate that the naïve mean is fairly robust to the

Table 2. Meta-Analytic and Publication Bias Results.

	Meta-analysis									Publication bias analyses											
										Trim-and-fill											
										FE trim-and-fill				RE trim-and-fill				CMA	Selection models		PET-PEESE
Distribution	k	N	$\bar{d}_o$	95% CI	90% PI	Q	I^2	τ	osr $\bar{d}_o$	FPS	ik	t&f_{FE} $\bar{d}_o$	t&f_{FE} 95% CI	FPS	ik	t&f_{RE} $\bar{d}_o$	t&f_{RE} 95% CI	pr_5 $\bar{d}_o$	sm$_m$ $\bar{d}_o$	sm$_s$ $\bar{d}_o$	pp $\bar{d}_o$
Original distributions																					
Outcome variable																					
-Cognition	19	3,543	.27	[0.20, 0.34]	.13, .42	25.70	29.96	.08	.26, .29; .27	L	6	.22	[0.15, 0.30]	L	6	.22	[0.15, 0.30]	.25	.25	.24	.20
-Affect	7	953	.14	[−0.12, 0.39]	−.33, .60	14.67	59.09	.25	.05, .21; .12	L	1	.04	[−0.23, 0.32]		0	.14	[−0.12, 0.39]	.15	.07	n/a	.02
-Appraisal	22	1,424	.46	[0.32, 0.59]	.06, .85	53.70	60.90	.23	.43, .49; .46	L	4	.38	[0.25, 0.51]	L	4	.38	[0.24, 0.51]	.42	.41	.33	.41
-Behavior	38	2,382	.21	[0.04, 0.37]	−.46, .87	100.97	63.36	.04	.18, .23; .21		0	.21	[0.04, 0.37]		0	.21	[0.04, 0.37]	.11	.09	n/a	.11
Provocation level[a]																					
-None/low	58	5,713	.27	[0.20, 0.35]	−.10, .64	169.02	66.28	.22	.26, .29; .27		0	.27	[0.20, 0.35]		0	.27	[0.20, 0.35]	.19	.19	n/a	.29
-High	32	1,719	.35	[0.12, 0.57]	−.56, 1.26	123.39	74.88	.54	.30, .39; .35	L	2	.26	[0.02, 0.50]		0	.35	[0.12, 0.57]	.17	.21	n/a	-.10
Setting																					
-Lab	64	6,768	.33	[0.25, 0.40]	−.02, .68	165.53	61.94	.21	.32, .34; .33	L	5	.28	[0.20, 0.36]	L	1	.32	[0.25, 0.40]	.19	.27	n/a	.28
-Field	14	900	.15	[−0.08, 0.39]	−.44, .75	35.78	63.67	.34	.10, .21; .16		0	.15	[−0.08, 0.39]		0	.15	[−0.08, 0.39]	−.06	.05	n/a	.12
Design																					
-Between-participants design	50	5,684	.26	[0.16, 0.37]	−.17, .69	121.21	59.57	.26	.25, .28; .26	L	1	.25	[0.15, 0.36]		0	.26	[0.16, 0.37]	.28	.18	n/a	.26
-Within-participants design	28	1,984	.35	[0.25, 0.45]	.01, .69	83.43	67.64	.20	.33, .37; .35	L	5	.26	[0.15, 0.36]	L	3	.30	[0.19, 0.40]	.24	.30	.18	.25
Photos vs. actual																					
-Images of weapons	43	5,230	.36	[0.27, 0.44]	.02, .69	121.74	65.50	.20	.34, .37; .36	L	9	.27	[0.18, 0.35]	L	8	.27	[0.19, 0.35]	.19	.32	.25	.24
-Actual weapons	22	1,847	.14	[−0.07, 0.35]	−.48, .77	61.44	65.82	.37	.09, .18; .14	R	5	.32	[0.11, 0.54]	R	2	.22	[0.01, 0.43]	.15	.17	n/a	.30
Real vs. toy																					
-Real weapons	65	7,077	.31	[0.23, 0.39]	−.07, .69	185.05	65.41	.23	.30, .32; .31	L	3	.28	[0.20, 0.36]		0	.31	[0.23, 0.39]	.19	.24	n/a	.27
-Toy weapons	13	591	.25	[0.07, 0.42]	−.10, .60	20.23	40.68	.19	.22, .32; .24		0	.25	[0.07, 0.42]	L	3	.18	[0.01, 0.35]	.19	.18	n/a	.29
Weapon type																					
-Guns	60	5,498	.26	[0.17, 0.36]	−.19, .72	167.59	64.80	.27	.25, .28; .26	L	5	.21	[0.11, 0.31]	L	5	.21	[0.10, 0.31]	.23	.17	n/a	.22
-Knives	6	808	.27	[0.10, 0.44]	.04, .51	7.26	31.11	.12	.21, .33; .26	R	2	.34	[0.13, 0.54]	R	1	.29	[0.12, 0.46]	.29	.29	.27	.41
-Mixed	14	1,362	.39	[0.27, 0.50]	.10, .68	34.88	62.73	.17	.36, .42; .39	L	5	.26	[0.14, 0.39]		0	.39	[0.27, 0.50]	.32	.36	.34	.13
Publication status																					
-Published	62	5,267	.36	[0.27, 0.44]	−.02, .73	157.30	61.22	.22	.34, .37; .35	L	7	.29	[0.20, 0.37]	L	1	.35	[0.27, 0.43]	.24	.29	n/a	.28
-Unpublished	16	2,401	.14	[0.00, 0.28]	−.23, .52	41.90	64.20	.22	12, .18; .14	R	3	.22	[0.07, 0.37]	R	2	.19	[0.04, 0.34]	.23	.08	n/a	.33
Participant gender																					
-Male	6	355	.47	[0.17, 0.77]	−.06, 1.00	14.24	64.88	.28	.32, .60; .48	L	2	.26	[−0.06, 0.57]		0	.47	[0.17, 0.77]	.51	.41	.36	.05
-Female	6	391	.34	[0.08, 0.60]	−.12, .79	14.19	64.77	.24	.25, .43; .35	L	1	.24	[−0.03, 0.51]		0	.34	[0.08, 0.60]	.38	.29	.25	.03
Participant college student status																					
-College student	52	4,201	.32	[0.22, 0.41]	−.11, .75	152.94	66.65	.26	.30, .33; .32		0	.32	[0.22, 0.41]	R	2	.34	[0.24, 0.44]	.22	.24	n/a	.29
-Nonstudent	26	3,467	.27	[0.17, 0.38]	−.02, .56	51.41	51.37	.17	.25, .30; .27	L	3	.23	[0.12, 0.34]	L	3	.23	[0.12, 0.34]	.25	.22	n/a	.24
Distributions without identified outliers																					
Outcome variable																					
-Cognition	19	*No outlier(s) identified.*																			
-Affect	7	*No outlier(s) identified.*																			
-Appraisal	22	*No outlier(s) identified.*																			
-Behavior	37	*No outlier(s) identified.*																			

(continued)

Table 2. (continued)

	Meta-analysis									Publication bias analyses											
										Trim-and-fill											
										FE trim-and-fill				RE trim-and-fill				CMA	Selection models		PET-PEESE
Distribution	*k*	*N*	$\bar{d}_o$	95% CI	90% PI	*Q*	I^2	τ	osr $\bar{d}_o$	FPS	*ik*	$t\&f_{FE}$ $\bar{d}_o$	$t\&f_{FE}$ 95% CI	FPS	*ik*	$t\&f_{RE}$ $\bar{d}_o$	$t\&f_{RE}$ 95% CI	pr_5 $\bar{d}_o$	sm_m $\bar{d}_o$	sm_s $\bar{d}_o$	*pp* $\bar{d}_o$
Provocation level[a]																					
-None/low	58	*No outlier(s) identified.*																			
-High	32	*No outlier(s) identified.*																			
Setting																					
-Lab	64	*No outlier(s) identified.*																			
-Field	14	*No outlier(s) identified.*																			
Design																					
-Between-participants design	50	*No outlier(s) identified.*																			
-Within-participants design	28	*No outlier(s) identified.*																			
Photos vs. actual																					
-Images of weapons	43	*No outlier(s) identified.*																			
-Actual weapons	22	*No outlier(s) identified.*																			
Real vs. toy																					
-Real weapons	65	*No outlier(s) identified.*																			
-Toy weapons	11	483	.30	[0.15, 0.45]	.18, .43	8.35	.0	.0	.28, .34; .29	L	2	.26	[0.12, 0.41]	L	2	.26	[0.12, 0.41]	.30	.26	.22	.19
Weapon type																					
-Guns	60	*No outlier(s) identified.*																			
-Knives	4	444	.29	[0.13, 0.45]	.16, .42	1.87	.0	.0	.23, .30; .31	R	2	.33	[0.18, 0.47]	R	2	.33	[0.18, 0.47]	n/a	.27	.28	.36
-Mixed	14	*No outlier(s) identified.*																			
Publication status																					
-Published	62	*No outlier(s) identified.*																			
-Unpublished	16	*No outlier(s) identified.*																			
Participant gender																					
-Male	4	91	.47	[0.21, 0.74]	.25, .70	1.65	.0	.0	.41, .53; .48		0	.47	[0.21, 0.74]		0	.47	[0.21, 0.74]	n/a	.44	.40	.74
-Female	5	247	.43	[0.15, 0.71]	.01, .85	7.52	46.81	.21	.34, .48; .45		0	.43	[0.15, 0.71]		0	.43	[0.15, 0.71]	.43	.40	.38	.51
Participant college student status																					
-College student	52	*No outlier(s) identified.*																			
-Nonstudent	24	*No outlier(s) identified.*																			

Note. *k* = number of effect sizes in the analyzed distribution; *N* = meta-analytic sample size; $\bar{d}_o$ = random-effects weighted standardized mean difference; 90% PI = 90% prediction interval; *Q* = weighted sum of squared deviations from $\bar{d}_o$; I^2 = ratio of true heterogeneity to total variation; τ = between-sample standard deviation; osr = one sample removed, including the minimum and maximum effect size and the median $\bar{d}_o$; Trim-and-fill = trim-and-fill analysis; FPS = funnel plot side (i.e., side of the funnel plot where samples were imputed; L = left, R = right); *ik* = number of trim-and-fill effect sizes imputed; $t\&f_{FE}$ $\bar{d}_o$ = fixed-effects trim-and-fill adjusted $\bar{d}_o$; $t\&f_{FE}$ 95% CI = fixed-effects trim-and-fill adjusted 95% confidence interval; $t\&f_{RE}$ $\bar{d}_o$ = random-effects trim-and-fill adjusted $\bar{d}_o$; $t\&f_{RE}$ 95% CI = random-effects trim-and-fill adjusted 95% confidence interval; CMA = cumulative meta-analysis; pr_5 $\bar{d}_o$ = random-effects weighted standardized mean difference of the five most precise effect sizes; sm_m $\bar{d}_o$ = one-tailed moderate selection model's adjusted $\bar{d}_o$; sm_s $\bar{d}_o$ = one-tailed severe selection model's adjusted $\bar{d}_o$; PET-PEESE = precision-effect test-precision effect estimate with standard error; PET-PEESE $\bar{r}_o$ = PET-PEESE adjusted $\bar{d}_o$; n/a = not applicable (the selection model yielded a nonsensical result due to an inflated variance estimate).

[a] Two studies, Dienstbier et al. (1998) and Page and O'Neal (1977), collapsed their data across provocation levels. Consequently, those studies were removed from the provocation level subgroups.

influence of publication bias. Therefore, it seems as if the design, within or between participants, does not have a real influence on the magnitude of the mean estimate, which is likely to be around 0.25 once publication bias is taken into consideration.

The results for other distributions are similar. For example, the naïve mean for the "photos of weapons" distribution (k = 43) was estimated to be 0.36. Yet all publication bias assessment methods indicate that this is overestimated by between 0.04 (sm_{m} $\bar{d}_o$ = 0.32, Δ = 0.04 or 11%) and 0.17 (pr_5 $\bar{d}_o$ = 0.19, Δ = 0.17 or 47%). On average, the degree of overestimation seems to be around 0.10 (or 29%), which is clearly noticeable and can be considered "moderate" (Kepes et al., 2012). Thus, the "true" underlying mean for the effect for "photos of weapons" is likely to be around 0.26. By contrast, the naïve mean for "actual weapons" ($\bar{d}_o$ = 0.14, k = 22) seems to be underestimated as all publication bias assessment methods yield larger magnitude mean estimates, varying between 0.15 (pr_5 $\bar{d}_o$ = 0.15, Δ = 0.01 or 7%) and 0.32 ($\text{t\&f}_{FE}$ $\bar{d}_o$ = 0.32, Δ = 0.18 or 129%). On average, the underestimation seems to be around 0.09 (or 66%), which means that the "true" underlying mean for the effect of actual weapons is likely to be around 0.23, very close to the likely location of the "true" underlying mean for "photos of weapons."

Taken together, some distributions seem to be adversely affected by publication bias, leading to overestimates as well as underestimates of the "true" underlying mean effect size. However, the results for other distributions indicate that the potential distorting effect of publication bias is likely to be unknown at the moment. As an example, for the outcome variable "behavior" (k = 38, $\bar{d}_o$ = 0.21), two publication bias assessment methods (both trim-and-fill models) suggest that the naïve mean estimate of 0.21 is "free" of publication bias and thus robust. Yet the other methods indicate that the naïve mean could be "severely" overestimated (i.e., overestimated by at least 40% [pr_5 $\bar{d}_o$ = 0.15, Δ = 0.10 or 48%; sm_m $\bar{d}_o$ = 0.09, Δ = 0.12 or 57%; pp = .15, Δ = 0.10 or 48%]; Kepes & McDaniel, 2015). Thus, the naïve mean for aggressive behavior may not be robust and, if so, the degree of robustness is currently unknown because the methods do not converge on a narrow range around the potentially "true" underlying mean.

Outliers. Although publication bias seems to have affected the majority of naïve mean effect-size estimates, our results indicate that outliers tended to have no noticeable effect on the naïve meta-analytic results. Out of the 23 analyzed original distributions, the comprehensive multivariate battery of influence diagnostics identified outliers in only four (4/23 = 17%). However, some of those four distributions were noticeably affected by the identified outliers. For instance, the influence diagnostics identified two outliers in the "toy weapons" distribution, reducing the number of effect sizes from 13 to 11. In turn, the naïve mean was estimated to be 0.30, an increase of 0.05 or 20% (before outlier removal: $\bar{d}_o$ = 0.25). Furthermore, all but one (i.e., PET-PEESE) of the publication bias assessment methods yielded larger magnitude adjusted mean estimates. An inspection of these results indicates that the "true" underlying mean is likely to be between 0.25, the original obtained naïve mean, and .30, the naïve mean after the removal of the identified outliers.

In sum, publication bias and/or outliers seem to have adversely affected some of the naïve meta-analytic mean estimates. For the most part, after taking publication bias and, if necessary, outliers into consideration, our results tended to be fairly robust and aligned with the interpretation that the weapons effect is "real" and of similar magnitude to other important effects in the social sciences (Bosco, Aguinis, Singh, Field, & Pierce, 2015; Richard, Bond, & Stokes-Zoota, 2003). Thus, we can have substantial confidence in the reported results.

Discussion

The results of our meta-analysis show that merely seeing a weapon can increase aggressive thoughts, hostile appraisals, and aggressive behavior. Our findings extend previous efforts to review the weapons effect literature (e.g., Carlson et al., 1990). In particular, the obtained results not only provide additional evidence that the mere presence of weapons can increase aggressive behavior, but more importantly provide insights into why weapons increase aggression. Based on the GAM, there are three possible routes to aggression—a cognitive route, an affective route, and an arousal route. The weapons effect appears to use the cognitive route. Our findings indicate that merely seeing a weapon primes or activates aggressive thoughts in memory. This might partially explain why seeing weapons can increase aggressive behavior. People who have aggressive thoughts active in their minds might be more likely to act on those thoughts and behave in an aggressive manner. Our findings also show that the mere presence of weapons can increase hostile appraisals. Although there was a smaller number of studies for which hostile appraisals were the outcome variable (k = 22), the effect was statistically significant and moderate in size. The mere presence of weapons can cause people to believe other people are aggressive and will respond in an aggressive manner in ambiguous situations. This hostile perception of others should increase the likelihood of aggression.

It is important to note that the weapons effect is quite robust. The effects occurred inside and outside the lab, for many different kinds of weapons (e.g., guns, knives, spears, swords, hand grenades), for actual weapons and photos of weapons, for males and females, for college students and for nonstudents, and for people of all ages, regardless of whether they were provoked.

The weapons effect was also robust to a comprehensive sensitivity analysis that examined the role of both publication bias and outliers. The results from the sensitivity analysis suggest that the weapons effect was larger in published studies than in unpublished studies, even after taking publication bias into consideration (see Table 2). It is worth noting that the weapons effect was significant in published studies but not in unpublished studies (see Table 1). However, the 95% CI overlapped (published: 95% CI = [0.27, 0.44]; unpublished: 95% CI = [−0.01, 0.29]), indicating that the two mean estimates are not statistically significantly different from each other. After taking publication bias into consideration, the differences between published and unpublished studies tended to be smaller in magnitude but remained noticeable. Thus, our results clearly indicate that the literature on the weapons effect, like so many other literature areas in the social sciences (e.g., Banks et al., 2015), has been affected by publication bias. Still, we can conclude that despite this adverse effect, the evidence indicates that the weapons effect is real and noticeable.

It is also worth noting that the weapons effect tended to increase slightly over time. That particular finding is important given that many of the studies included in the meta-analysis, especially the earlier studies, utilized small samples. This is worth further comment for at least two reasons. First, initial reports based on smaller underpowered samples are particularly vulnerable to being nonreplicable in subsequent studies (Ioannidis, 2005; Trikalinos & Ioannidis, 2005). In other words, initial findings may be inflated, and a decline effect may be observed subsequently. Second, many of the early failures to replicate the weapons effect were themselves based on relatively small samples that would be arguably underpowered. More recent published and unpublished studies have been based on larger samples than earlier studies, and arguably the reported effects have been more stable.

These assertions are supported by supplemental analyses we conducted. As can be seen from the cumulative meta-analyses by year of publication in our supplemental materials (Kepes et al., 2012), many effects are relatively stable across time, providing credence for the assertion that the decline effect is not of major concern for the weapons effect literature. Furthermore, for some distributions (e.g., outcome variable: Cognition [k = 19], and provocation level: None/low [k = 58]), the effect seems to get stronger over time. Our supplemental analyses also suggest that more recent studies have tended to use relatively large samples, which tend to get published regardless of the obtained effect-size magnitude (as large-sample studies tend to obtain statistically significant results due to their sample size). In support of this view, several of the most precise samples (i.e., large samples) in our cumulative meta-analyses by precision (see our appendix) were published relatively recently, whereas many imprecise samples (i.e., small samples) were published some time ago. When examining the forest plots depicting the cumulative meta-analyses by year in our supplemental materials for drift, we once again urge extreme caution when the distribution is relatively small, especially when it contains less than 10 effect sizes.

Theoretical Implications

Although the obtained findings are consistent with the GAM, the GAM is not the only model that can explain these findings. The findings from this meta-analysis are also consistent with other models that have been used to explain the weapons effect, such as those based on classical conditioning, operant conditioning, and priming (e.g., Berkowitz, 1974, 1982, 1983). For example, Berkowitz (1974) used a stimulus-response learning model to explain the weapons effect. According to Berkowitz (1974), weapons become associated with aggression through their frequent pairing with aggression in the mass media and in everyday life. Once that association is made, the mere presence of weapons can elicit aggressive responses when individuals are exposed to them. Other scholars have argued that operant conditioning served as an alternative explanation for both successful and unsuccessful instances of the weapons effect (Ellis, Weinir, & Miller, 1971). According to this perspective, participants process not only the presence of the weapon but also the likelihood of reinforcement or punishment when behaving aggressively toward another person. Researchers were already discussing the potential for weapons to serve as cognitive cues that primed aggression as early as the 1970s (e.g., Turner et al., 1977). However, cognitive priming theories specific to aggression did not fully develop until the late 1980s and early 1990s. One example is the cognitive neoassociation theory (Berkowitz, 1990), which proposes that aggressive thoughts are linked together in memory, forming an associative network. Once an aggressive thought is processed or stimulated, activation spreads through the network and primes associated thoughts as well. Thus, seeing a weapon can prime or activate other aggressive thoughts in memory. Having aggressive thoughts accessible in memory can increase the likelihood of aggressive behavior.

Most recently, the Situated Information Model has been used to explain the weapons effect (Engelhardt & Bartholow, 2013). According to this model, exposure to a weapon will lead to an increased accessibility of aggressive thoughts that may lead to aggression if individuals attribute any arousal activated by the weapon to internal processes rather than the weapon itself. This model has been used to explain some of the early failures to replicate the weapons effect, such as when individuals have been made aware of experimental hypotheses. If individuals are aware that the weapon can influence their behavior, they presumably make external attributions for any arousal that occurs from exposure to a weapon, thus leading to an inhibition of aggression. On the other hand, if individuals are unaware that weapons can influence their behavior, they presumably make internal attributions for any arousal that occurs from exposure to a weapon, thus leading to a facilitation of aggression. This theory could not be tested in this meta-analysis, because not enough studies have measured physiological responses to seeing weapons. It is also worth noting that the GAM subsumes many of these other theories, which is why we used it as the theoretical foundation for this meta-analysis.

More generally, these findings shed light on the controversy regarding social priming effects (Benjamin & Bushman, 2016). As scholars have observed (e.g., Molden, 2014), social psychologists have accepted as a given that mere exposure to any of a number of social stimuli will facilitate changes in thoughts, attitudes, and behavioral outcomes, often outside of conscious awareness or control. However, over the past several years, there have been a number of noteworthy failures to replicate highly cited social priming experiments (e.g., Pashler, Coburn, & Harris, 2012; Shanks et al., 2013), leading some scholars to express serious doubts about social priming research as it is currently conducted (Kahneman, 2012). In the case of social cues that should facilitate aggressive outcomes, we would want to consider if the initial observed effects are consistently replicated over time. Based on our findings from the available published and unpublished literature, it appears that weapons function as social primes insofar as they facilitate aggressive thoughts, hostile appraisals, and aggressive behavior. Those findings are consistent with research on other social stimuli that have been demonstrated to facilitate aggression, such as violent video games (Anderson et al., 2010). Of course, this meta-analysis cannot conclusively settle the social priming debate, but social priming does appear to occur with weapons. For other examples of social priming, see the recent special issue on the topic published in *Current Opinion in Psychology* (Strack & Schwarz, 2016).

Practical Implications

The findings from this meta-analysis have important practical implications, especially for societies in which guns are highly visible. For example, the United States (U.S.) is the most heavily armed society in the world, with about 90 guns for every 100 citizens (MacInnis, 2007). Although the U.S. is only about 4% of the world's population (Schlessinger, 2013), U.S. citizens possess about 31% of the world's guns. Guns are easily purchased in the United States with little oversight or regulation. With so many guns around, the weapons effect might have a significant effect on the thoughts, feelings, appraisals, and behavior of many U.S. citizens.

Understanding the weapons effect can help parents with children. Research has shown that children are naturally curious about guns, have difficulty distinguishing a real gun from a toy gun, are prone to handle guns, and can shoot themselves or others with guns (American Academy of Pediatrics, 2013). About 35% of American homes with children have at least one gun (Schuster, Franke, Bastian, Sor, & Halfon, 2000). In only 39% of these homes are guns locked up, unloaded, and separate from ammunition, as recommended by the American Academy of Pediatrics (AAP). Some of these guns are in plain sight, such as in glass cabinets, on racks, or on shelves. To reduce the weapons effect, parents can keep guns out of sight of family members. As the English writer John Heywood said, "Out of sight out of mind." Our meta-analysis found that even toy guns can produce a significant weapons effect. Thus, parents can also give their children toys other than guns.

Although our meta-analysis did not examine weapons in the mass media, there are plenty of weapons present in the mass media. For example, a recent study found that the amount of gun violence in movies rated PG-13 (for viewers 13 and older) has more than doubled since the rating was introduced in 1985 (Bushman, Jamieson, Weitz, & Romer, 2013). In fact, since 2012, there have been significantly more acts of gun violence in PG-13 movies than in R-rated movies (for viewers 17 and older; Bushman et al., 2013). Importantly, the increase in gun violence in PG-13 movies has continued in more recent years (Romer, Jamieson, & Jamieson, 2017). Thus, parents can also reduce exposure to guns in the mass media by actively monitoring the media their children consume.

Limitations and Future Research

Like all meta-analyses, this one has limitations. Relatively few studies measured aggressive affective outcomes (i.e., anger), and although the overall estimated effect size was not negligible, it was not significant. Of those studies, most only examined the effect of weapons on anger in nonprovoking situations. It is conceivable that the mere presence of weapons on anger is stronger under provoking circumstances than nonprovoking circumstances. Future research should include measures of anger for both provoked and nonprovoked individuals. Thus, the findings regarding the influence of weapons on anger are inconclusive. Furthermore, almost no research has examined the effects of weapons on physiological arousal. The one experiment we are aware of (De Oca & Black, 2013) is suggestive but was conducted using a small sample for the purpose of selecting stimulus materials for a subsequent experiment.

Although recent research on the weapons effect has included both male and female participants, very few researchers have directly tested for possible differences in the size of the weapons effect between males and females. That may be due to researchers failing to report null findings when conducting initial tests of gender as a potential moderator, or due to the tendency for contemporary participant pools to be disproportionately female, making such comparisons difficult to test. The few studies available in which gender was a moderator suggest that although the weapons effect might be larger for males than females, the difference was nonsignificant. Thus, we cannot definitively say whether there are gender differences in the weapons effect.

With very few exceptions, researchers have left unexamined the potential moderating role of individual differences in aggressive personality traits (e.g., Caprara et al., 1984) or life experience with weapons (e.g., Bartholow et al., 2005; Korb, 2016). Thus, we were unable to include such individual differences in this meta-analysis. It is also worth noting that the vast majority of the weapons effect research has been conducted using samples from the United States and Europe. Only one study was conducted in the Middle Eastern sample (Mahjoob, Leyens, & Yzerbyt, 1992), and only two studies were conducted on an Asian sample (Guo, Egan, & Zhang, 2016; Zhang, Tian, Zhang, & Rodkin, 2016). Additional research on the weapons effect utilizing participants from a wider variety of cultures would allow for greater confidence in the generalizablilty of the weapons effect, much like cross-cultural research has done for other risk factors for aggression, such as violent video games (e.g., Anderson et al., 2010).

With regard to our sensitivity analysis, we also need to acknowledge some limitations. First, some of our methods (e.g., contour-enhanced funnel plot, trim-and-fill, PET-PEESE) tend to attribute the degree of asymmetry in the funnel plot to publication bias (Duval, 2005; Kepes et al., 2012; Moreno et al., 2009). Asymmetry in a funnel plot asymmetry can also be caused by other heterogeneous influences, such as moderators and outliers. To account for such factors, we formed subgroups based on the moderators we identified in our review. In addition, we accounted for outliers by using a thorough battery of multivariate influence diagnostics to identify potential outliers (Viechtbauer, 2015; Viechtbauer & Cheung, 2010), deleting them from the distribution, and then re-analyzing the entire distribution without the identified outlier(s). Consequently, our approach minimized the influence of heterogeneous effects on the reported results. Still, to alleviate additional concerns regarding such influences, we used the contour-enhanced funnel plot (see our supplemental materials) to differentiate between publication bias and other causes of funnel plot asymmetry, especially the small-sample bias (Kepes et al., 2012; Sterne et al., 2011). Regardless of the reason for asymmetry, it can bias naïve meta-analytic results. In addition, some of our publication bias methods (e.g., selection models) are less affected by heterogeneous influences and should thus be relatively robust regardless of whether such influences are present or not (Kepes et al., 2012; Vevea & Woods, 2005). Also, for most analyzed distributions (i.e. moderator-based subgroups), most publication bias methods tended to converge on a fairly narrow range of results. Although we acknowledge that not all methods always converged, the vast majority of the methods tended to provide highly confirmatory results, especially after outliers were removed (if they were present). Put differently, by using several publication bias assessment methods, we estimated “multiple reference points” to triangulate (Jick, 1979) the location of the “true” underlying mean effect. Given that the methods generally converged on a narrow range of possible locations for the “true” mean, we can have confidence in our obtained results and the associated conclusions (Kepes & McDaniel, 2013).

Finally, we wish to reiterate an important caveat we mentioned earlier. Just as it is for other statistical methods, sample size is of utmost importance when determining the confidence one can have in results obtained from meta-analytic or publication bias methods. Prior research has recommended to apply such methods, especially funnel plot-based publication bias methods, only to meta-analytic distributions with at least 10 effect sizes (Kepes et al., 2012; Sterne et al., 2011). Very few, but some (e.g., 4/23 [17%], before outlier removal) of the distributions we analyzed contained less than the recommended minimum of 10 effect sizes; yet we still applied all methods to them. We did this primarily for transparency reasons. Also, if one feels comfortable reporting a naïve meta-analytic mean and related results (e.g., 95% CI), one should also feel comfortable reporting the results of publication bias and related methods (Kepes, Banks, & Oh, 2014). However, we once again urge caution when interpreting the results of the naïve meta-analysis and publication bias analyses with distributions containing few effect sizes.

Recommendations for Future Meta-Analyses

We also have recommendations for future meta-analyses. Our results showed that both outliers and publication bias can have distorting effects on naïve meta-analytic results. Hence, suggestions regarding the irrelevance of outlier and publication bias analyses (Aguinis et al., 2011; Dalton, Aguinis, Dalton, Bosco, & Pierce, 2012) seem to be a mere urban myth. Furthermore, we found that publication bias had a much stronger adverse effect on our obtained results than outliers did. This is what previous research found as well (Kepes & McDaniel, 2015). Yet it may not be that way in other literature areas. Furthermore, although the adverse effects of publication bias were much stronger and more widespread than the effects of outliers, the latter did have a noticeable distorting effect on some meta-analytic results.

Taken together, aligned with prior research from other literature areas in the social sciences (e.g., Banks et al., 2015; Kepes, Banks, & Oh, 2014), our results point to the quite obvious conclusion that the published literature on the weapons effect has been affected by publication bias. We do not know whether other areas in social psychology are affected as well and, if so, the degree to which they are. Therefore, we suggest that future meta-analytic studies assess the robustness of their results by using the comprehensive battery of publication bias methods recommended in the literature and used in this article (Kepes et al., 2012; Kepes & McDaniel, 2015). That way, one can have much greater confidence in the robustness of meta-analytic results. Given the current “crisis of confidence” in many of the psychological sciences (Pashler & Wagenmakers, 2012), such a comprehensive assessment may be more important than ever to ensure that our results and the associated conclusions are trustworthy (Kepes, Bennett, & McDaniel, 2014; Kepes & McDaniel, 2013).

Conclusion

In conclusion, the weapons effect is alive and well today. Indeed, it may even be increasing over time. The National Rifle Association correctly notes, “Guns don’t kill people; people kill people.” But guns are not just neutral stimuli either. Merely seeing a gun can make people more aggressive. As Professor Len Berkowitz noted, although the finger pulls the trigger of a gun, “the trigger may also be pulling the finger.”

Appendix

Interpretation for Contour-Enhanced Funnel Plots

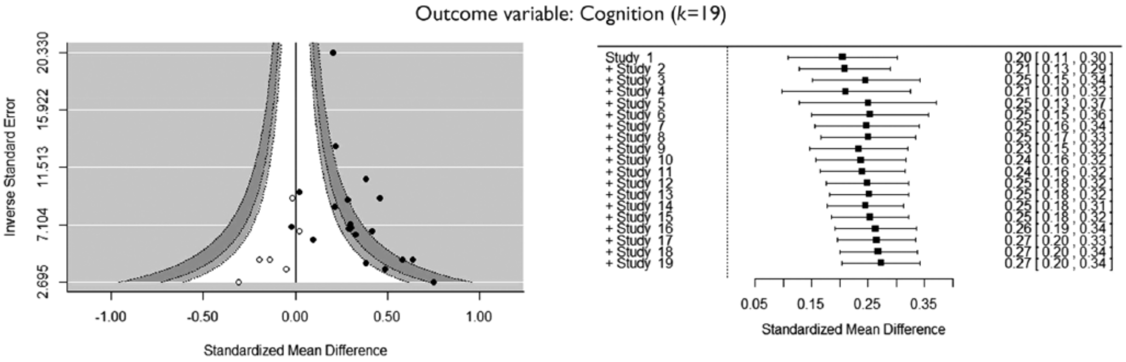
Funnel plots display the precision (i.e., inverse of the standard error) of an effect size (e.g., a correlation or a standardized mean difference) against its magnitude. Precision is typically shown on the vertical axis (*Y*-axis), and the effect-size magnitude on the horizontal axis (*X*-axis). Samples with large magnitude effect sizes are plotted on the right side of the *X*-axis, and samples with small magnitude effect sizes on the left side. Because effect sizes from larger samples tend to be more precise (i.e., they are less affected by sampling error) than effect sizes from smaller samples, they tend to cluster at the top of the funnel plot around the population mean. By contrast, smaller samples are usually spread throughout the bottom of the funnel plot because these samples tend to contain more sampling error, resulting in their effect sizes deviating to a greater degree from the population mean.

If the distribution is symmetrical, publication bias is likely to be absent. By contrast, if small-sample studies with insignificant effect sizes are not included in the funnel plot, but large-sample studies with statistically significant effect sizes are, the distribution will be asymmetrical. Causes for asymmetry include publication bias and the small-sample bias. The contour-enhanced funnel plot helps distinguish publication bias from the small-sample bias and related causes of funnel plot asymmetry by incorporating contour lines that correspond to values of statistical significance (e.g., $p < .05$ and $p < .1$). If the funnel plot distribution is asymmetric and the “missing” effect sizes are located in areas of statistical insignificance (e.g., $p > .1$), credence is provided to the possibility that funnel plot asymmetry is due to publication bias. By contrast, if the distribution is asymmetric because of missing samples in areas of statistical significance (i.e., the light gray area in the contour-enhanced funnel plot represented by $p < .05$), the small-sample bias could be present (for an alternative view, see Kepes, Banks, & Oh, 2014). For more detailed information see Kepes, Banks, McDaniel, & Whetzel, (2012; see also Kepes & McDaniel, 2015; Peters, Sutton, Jones, Abrams, & Rushton, 2008; Sterne et al., 2011).

Interpretation for the Forest Plots Displaying the Cumulative Meta-Analyses by Precision

To obtain the plots, the averages study effect sizes were sorted from most precise to least precise and entered into the meta-analysis one at a time in an iterative manner. The lines around the plotted means are the 95% confidence intervals for the meta-analytic means. The numbers on the right of each forest plot represent the weighted meta-analytic mean estimate following each iteration and its corresponding confidence interval. A drift from smaller to larger cumulative meta-analytic means is consistent with an inference of statistically insignificant correlations from smaller sample size studies being suppressed (i.e., publication bias). For more detailed information, see Kepes et al. (2012); see also, for example, Kepes et al. (2014) and Kepes and McDaniel (2015).

Funnel plots (with FE trim and fill imputations) and forest plots (displaying the cumulative meta-analysis by precision) Figures A1 - A23.

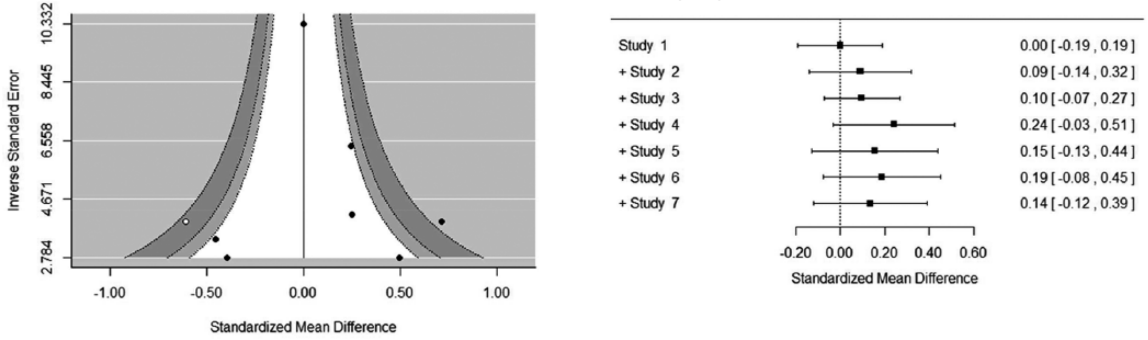


Outcome variable: Cognition, without identified outliers (*k*=19)

Figure A1

(no outlier[s] identified) (no outlier[s] identified)

Outcome variable: Affect (*k*=7)



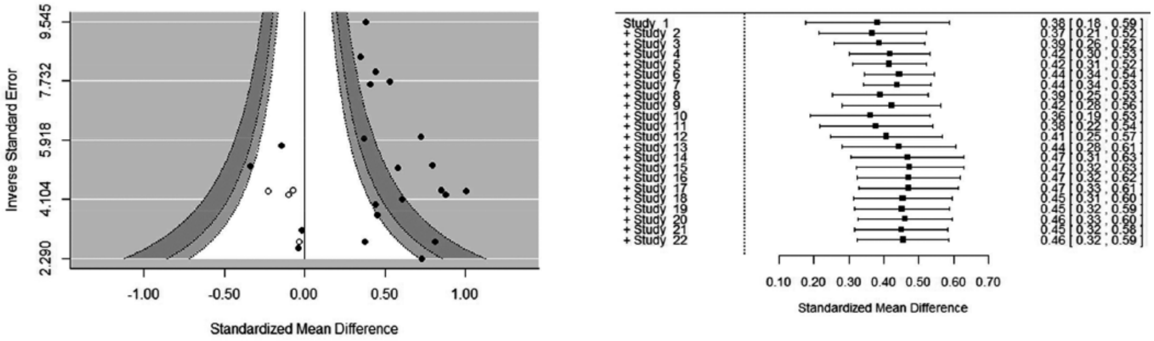
Outcome variable: Affect, without identified outliers (*k*=7)

Figure A2

(no outlier[s] identified)

(no outlier[s] identified)

Outcome variable: Appraisal (*k*=22)



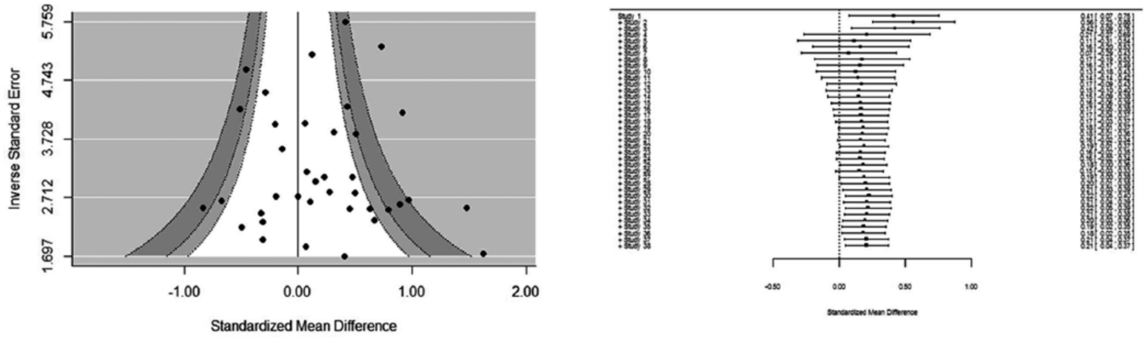
Outcome variable: Appraisal, without identified outliers (*k*=22)

Figure A3

(no outlier[s] identified)

(no outlier[s] identified)

Outcome variable: Behavior (*k*=38)



Outcome variable: Behavior, without identified outliers (*k*=38)

Figure A4

(no outlier[s] identified)

(no outlier[s] identified)

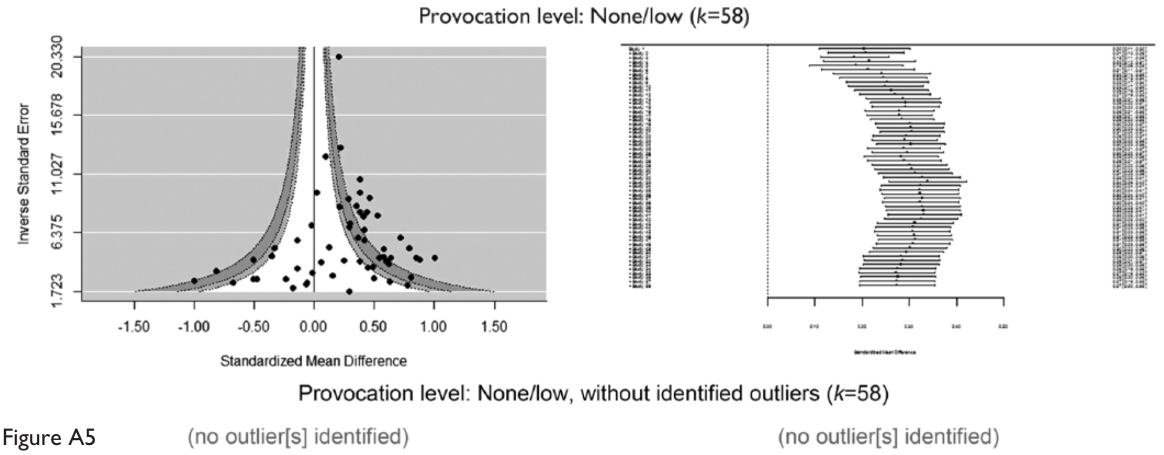


Figure A5

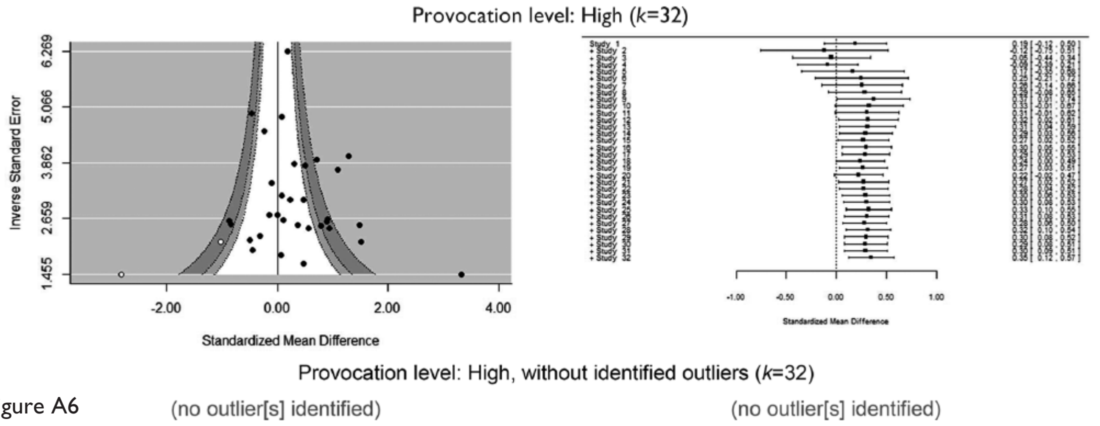


Figure A6

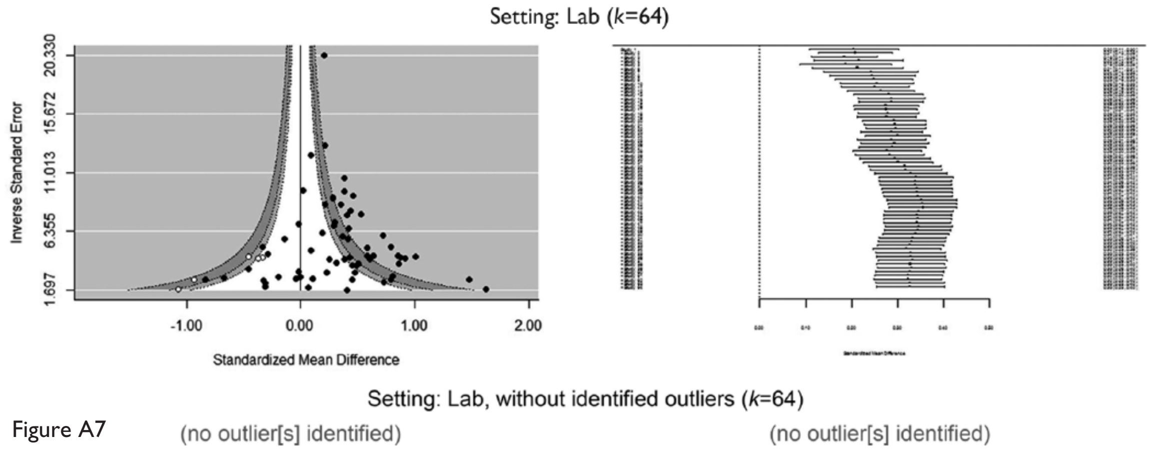


Figure A7

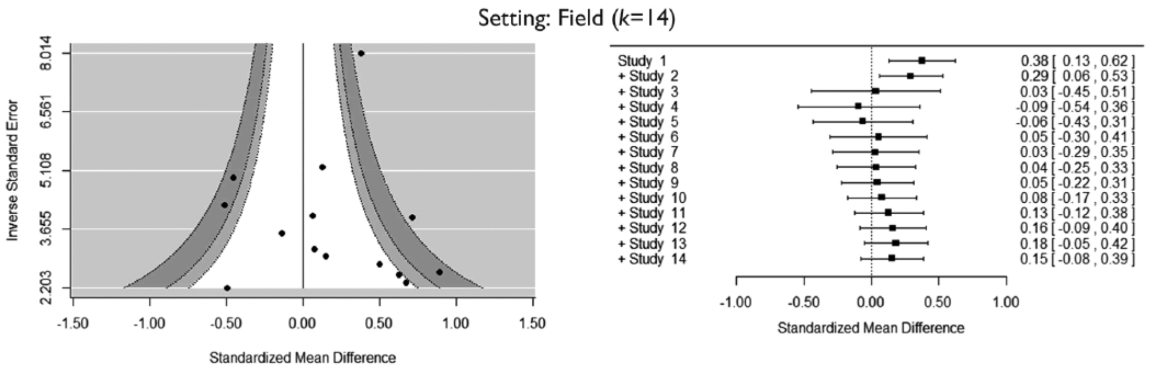


Figure A8

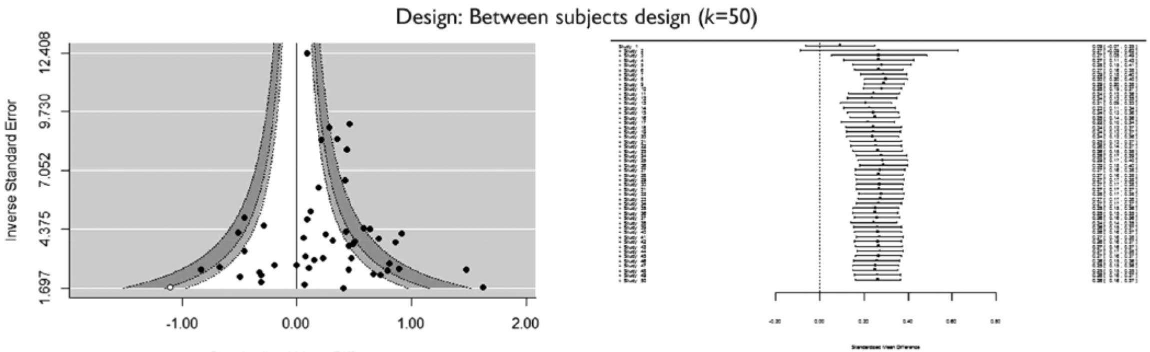


Figure A9

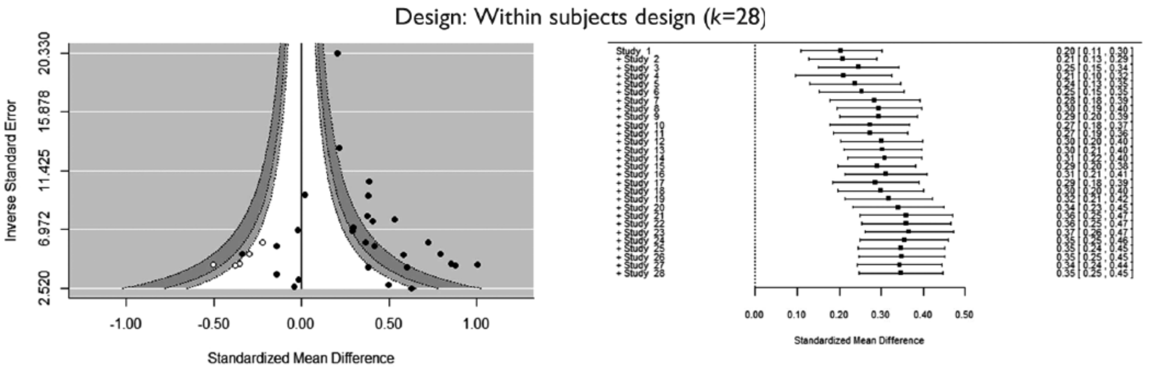
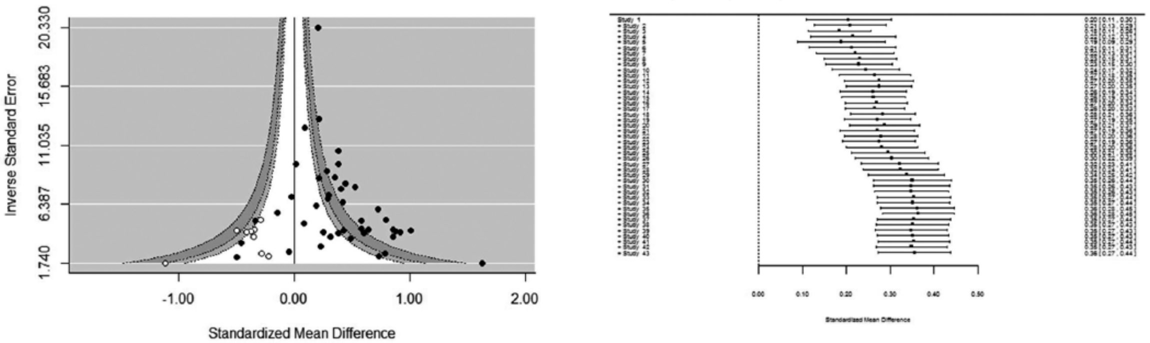


Figure A10

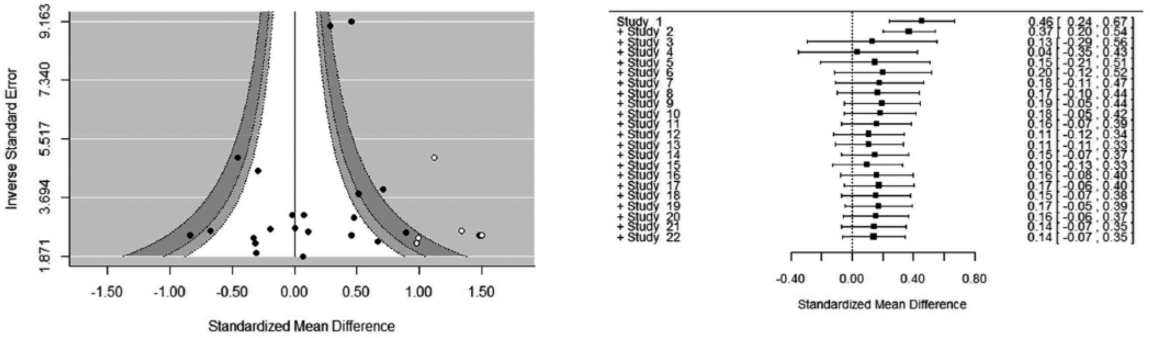
Photos vs. actual: Photos of weapons (*k*=43)



Photos vs. actual: Photos of weapons, without identified outliers (*k*=43)

Figure A11 (no outlier[s] identified) (no outlier[s] identified)

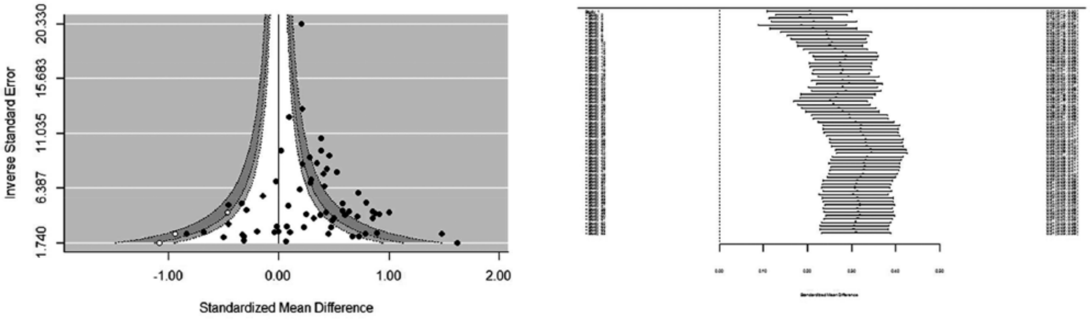
Photos vs. actual: Actual weapons (*k*=22)



Photos vs. actual: Actual weapons, without identified outliers (*k*=22)

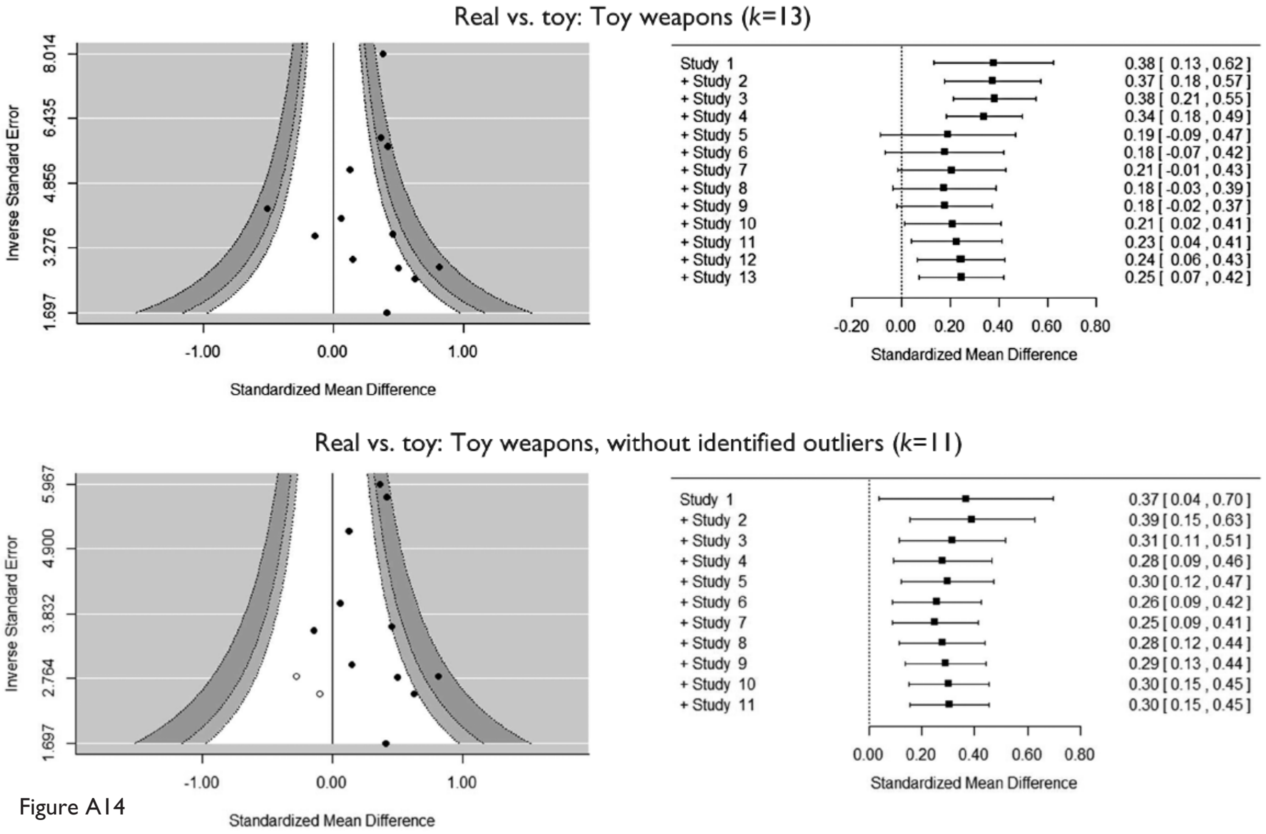
Figure A12 (no outlier[s] identified) (no outlier[s] identified)

Real vs. toy: Real weapons (*k*=65)



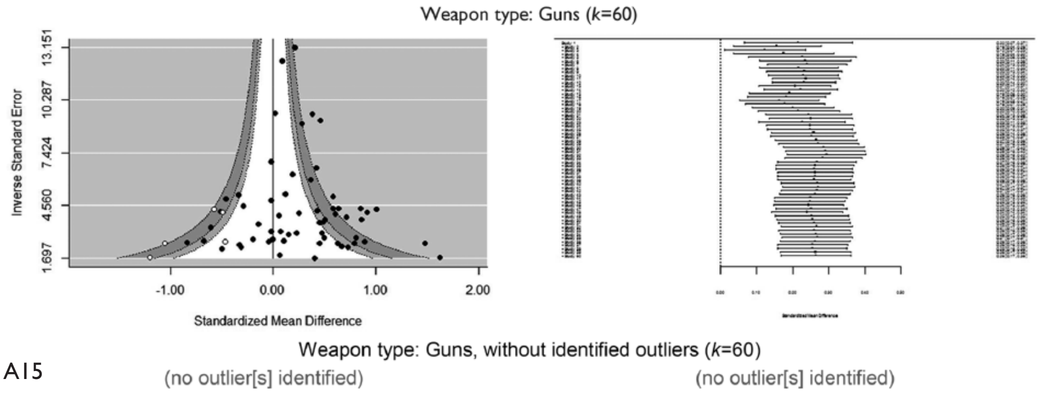
Real vs. toy: Real weapons, without identified outliers (*k*=65)

Figure A13 (no outlier[s] identified) (no outlier[s] identified)


Real vs. toy: Toy weapons (k=13)

Real vs. toy: Toy weapons, without identified outliers (k=11)

Figure A14

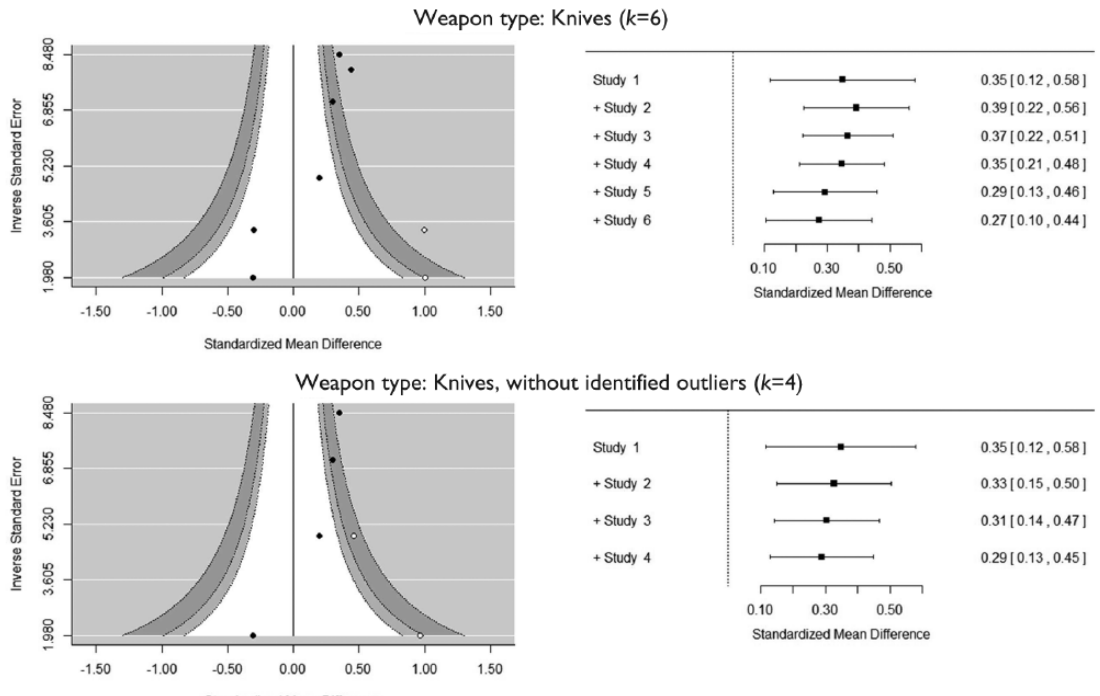

Weapon type: Guns (k=60)

Weapon type: Guns, without identified outliers (k=60)

(no outlier[s] identified)

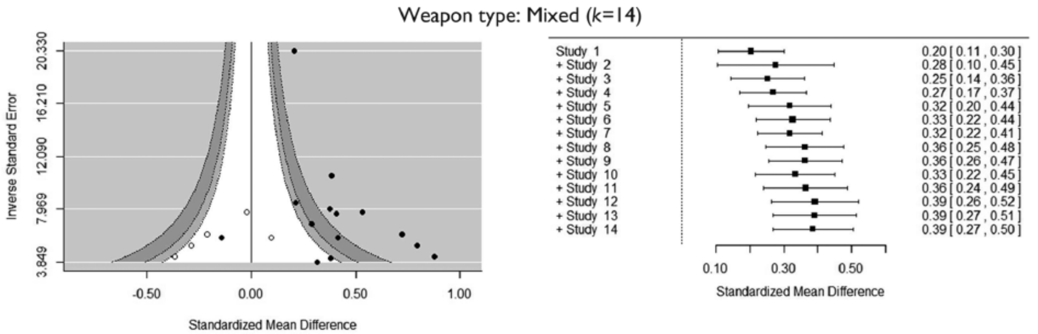
(no outlier[s] identified)

Figure A15


Weapon type: Knives (*k*=6)

Weapon type: Knives, without identified outliers (*k*=4)

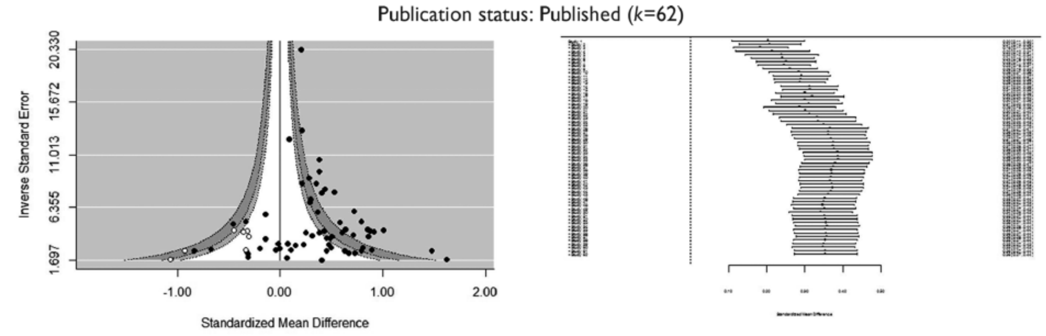
Figure A16


Weapon type: Mixed (*k*=14)

Weapon type: Mixed, without identified outliers (*k*=14)

(no outlier[s] identified) (no outlier[s] identified)

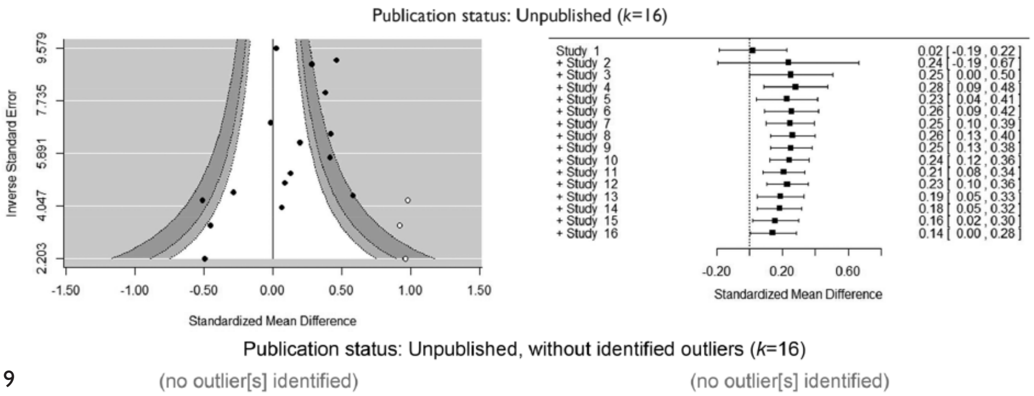
Figure A17


Publication status: Published (*k*=62)

Publication status: Published, without identified outliers (*k*=62)

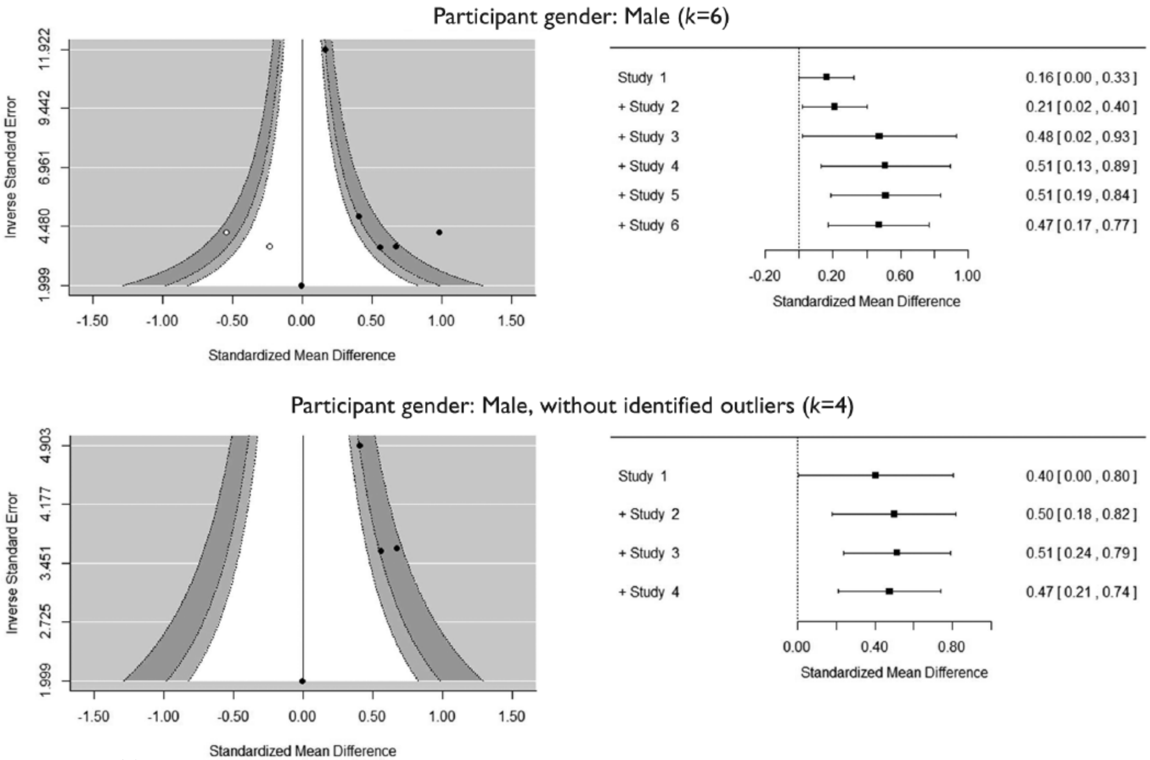
(no outlier[s] identified) (no outlier[s] identified)

Figure A18


Publication status: Unpublished (*k*=16)

Publication status: Unpublished, without identified outliers (*k*=16)

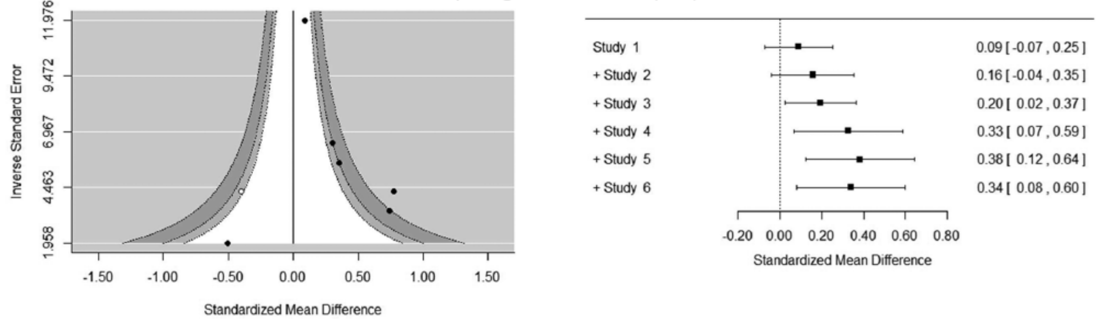
Figure A19


Participant gender: Male (*k*=6)

Participant gender: Male, without identified outliers (*k*=4)

Figure A20

Participant gender: Female (*k*=6)



Participant gender: Female, without identified outliers (k=5)

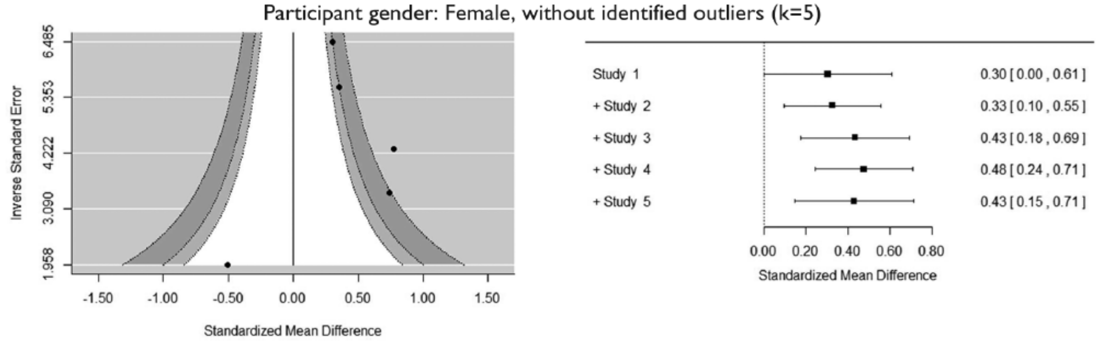
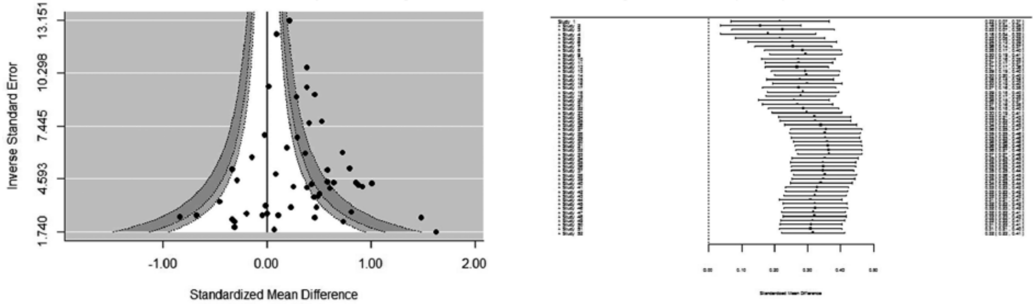


Figure A21

Participant college student status: Colleague student (*k*=52)

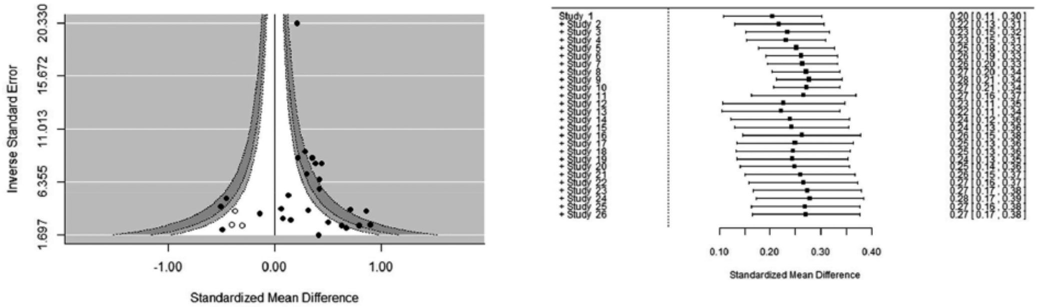


Participant college student status: Colleague student, without identified outliers (*k*=52)

(no outlier[s] identified) (no outlier[s] identified)

Figure A22

Participant college student status: Nonstudent (*k*=26)



Participant college student status: Nonstudent, without identified outliers (*k*=26)

(no outlier[s] identified) (no outlier[s] identified)

Figure A23

Authors' Note

The data for the meta-analysis can be obtained from the first author.

Acknowledgments

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Declaration of Conflicting Interests

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Supplemental Material

Supplementary material is available online with this article.

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