PARTICIPANT RECRUITMENT METHOD AND RAPE PREVALENCE RATES: A META-ANALYSIS

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ABSTRACT

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This project used meta-analysis and meta-regression to examine sources of variability in rape prevalence estimates. In addition to exploring previously-identified sources of variation, this study makes a unique contribution to the literature by evaluating participant recruitment method as a previously-unexplored source of potential variation. The research questions addressed in this study were: 1) How much variation in sexual assault prevalence rates is observed in research studies, 2) Does the method through which participants are recruited for research studies predict variation in rape prevalence estimates, and 3) Are other sample or study methodology-related variables predictive of variation in rape prevalence estimates? Meta-regression techniques were used to regress study-level covariates onto obtained prevalence rates to identify and quantify these sources of variation. Findings from this study will help researchers more fully understand the implications of their sample and methodology-related choices for study design and participant recruitment, and improve the clarity with which researchers, practitioners, and other stakeholders can interpret rape prevalence estimates.

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INTRODUCTION

Research has consistently shown that rape is a serious public health concern with long-lasting consequences for its victims (Amstadter, McCauley, Ruggiero, Resnick, & Kilpatrick, 2008; Breiding et al., 2014). National-scale studies have found that 11.5%-36.1% of women have experienced at least one completed rape over the course of their lifetimes (Breiding et al., 2014; Brener, McMahon, Warren, & Douglas, 1999; Kilpatrick, Resnick, Ruggiero, Conoscenti, & McCauley, 2007; Merrill et al., 1998; Tjaden & Thoennes, 2000). While alarmingly frequent at either end of this range, the notable variability in prevalence estimates raises questions regarding why victimization rates differ so greatly across studies.

This variation in prevalence findings could reflect differences in individuals' risks of being sexually victimized or differences in how studies assess that victimization. Meta-analysis is an effective way to understand the consistency, or lack thereof, in research findings (Borenstein, Hedges, Higgins, & Rothstein, 2009; Lipsey & Wilson, 2001; Schmidt & Hunter, 1977), and it is therefore a promising way to explore what factors affect rape prevalence estimates. This paper discusses the evolution of rape victimization research, summarizes the currently-recognized sources of variation in rape prevalence findings, and presents the results of a meta-regression that considered participant recruitment method as an additional source of potential variation.

LITERATURE REVIEW

Defining Scope and Clarifying Terms

This project was limited in scope to instances of rape, which is typically defined as unwanted penetration of the victim by the perpetrator through the use of force, threats of force, or when the victim is otherwise unable to consent due to intoxication or incapacitation[1] (See Campbell & Townsend, 2011, for common definitions). This penetration could include vaginal, oral, or anal penetration by another person's sex organ, mouth, fingers, or a foreign object. This project focused only on victimization rates for completed rapes, not attempted rapes. This project was also specific to rape perpetrated against women, as women are the victims in the vast majority of rapes (Breiding et al., 2014; Catalano, Smith, Snyder, & Rand, 2009; Sinozich & Langton, 2014; Tjaden & Thoennes, 2000); correspondingly, female pronouns are used to refer to rape victims throughout this paper.

It is important to clarify the distinction between rape incidence and rape prevalence rates. The incidence of an event refers to the number of new *instances* of an event that occurred in a given period of time, typically 6 months to 1 year. One person can contribute multiple data points to an incidence rate if they experienced multiple events during the specified time period. Prevalence of an event, in contrast, refers to the number of *people* experiencing the effects of a condition during a given period of time. Because the effects of rape are understood to be long-lasting, prevalence of rape is typically measured by life stage (e.g. childhood, adolescence, adulthood), or over the course of one's entire life (Bachar & Koss, 2001; Koss, 1993). This project was specific to rape prevalence rates, but this literature review discusses the evolution of rape research in the assessment of both incidence and prevalence rates.

[1] Much of the literature discussed has also explored unwanted sexual experiences other than penetration (e.g., unwanted touching). Though this is a valuable avenue of research, it is outside the scope of the proposed study.

Early Victimization Estimates

Prior to the 1970's, rape victimization rates were assessed through official crime statistics, reflecting only crimes that had been reported to the police. Annual incidence rates were compiled through the FBI's Uniform Crime Report (UCR), and the resulting statistics led to the conclusion that rape was a relatively rare occurrence, affecting only .02-.04% of women each year (Shorter, 1977). Any rapes not reported to law enforcement agencies were not captured in these statistics, leading some scholars to question whether these estimates were underestimating the true frequency of rape (Koss, 1993; Koss, Gidycz, & Wisniewski, 1987; Russell, 1982).

The National Crime Survey (NCS), instituted in 1972, was designed to provide an incidence estimate inclusive of both reported and unreported incidents of rape. This national, random household survey was the first attempt to directly ask people about their victimization experiences, and it found an annual incidence rate 6.5 times greater than that reported by the UCR (BJS, 1987). While the NCS established that self-report methods provide a fuller picture of rape than do estimates relying solely on police reports, the NCS was criticized as ineffective at screening for interpersonal crimes, including rape (Koss et al., 1987; Penick & Owens, 1976; Russell, 1982). The primary critique was that the NCS screening questions intended to prompt disclosure of rape experiences were imprecise; participants were only asked directly about rape if they responded affirmatively to a question about whether anyone had attacked them. This broadly worded question about attacks did not cue all respondents to disclose their rape experiences, and these victims therefore remained uncounted by NCS estimates (Koss, 1996). Thus, the NCS, too, was thought to underestimate the true occurrence of rape.

The limitations of the NCS suggested that how a project asks about rape has the potential to impact what it will capture and what it will miss. In the early 1980's, social scientists Mary

Koss (1982, 1987) and Diana Russell (1982) set out to explore what methods best encourage victims to disclose their rape experiences. Both Koss and Russell rejected the idea that the most effective way to capture the occurrence of rape was to ask women directly whether they had ever experienced "rape." Their concern was that asking in this way required women to not only have experienced the event, but to define their experience in those terms (Koss et al., 1987; Russell, 1982). If women have a preconceived notion of what constitutes "real" rape (Estrich, 1987), however, and their experience diverges substantially from that stereotype, Koss and Russell worried that they may be hesitant to endorse survey items asking whether they have been raped.

Both Koss and Russell addressed this issue by creating measures that used behaviorally-specific, rather than label-based, screening questions (Koss, 1982; Russell, 1982). Behaviorally-specific screening questions describe the act being assessed and ask whether respondents have experienced that act (e.g. "have you had sexual intercourse when you didn't want to because a man threatened or used some degree of physical force (twisting your arm, holding you down, etc.) to make you?[2]"). Both Russell (1982) and Koss (1982, 1987) employed this strategy to assess the annual incidence of rape in such a way that their findings could be directly compared to those put forth by the NCS. Russell (1982) interviewed a community sample of adult women and found an annual incidence rate of 3.5%, 7 times greater than the incidence rate reported by the NCS when they surveyed the same city four years prior (BJS, 1977). Koss et al. (1987) surveyed a national, random sample of college women and found an annual incidence rate 10-15 times greater than what had been reported by the NCS among similarly aged women (BJS, 1984). Together, these studies established that the incidence of rape was far greater than what had previously been detected by the NCS.

[2] This question is from the Sexual Experiences Survey (Koss & Oros, 1982).

Russell (1982) and Koss (1982, 1987) also advocated measuring rape prevalence, in addition to rape incidence, rates. When measuring rape incidence, a person who has not been victimized in the last 6 months or 1 year is counted as a non-victim. Some researchers have suggested that this short-term measurement minimizes the long-lasting psychological and physical consequences of rape, underestimating the full population of women adversely affected by rape at a given moment in time (Bachar & Koss, 2001). Russell (1982) measured the prevalence of rape over women's lifetimes and found that 24% of women in her sample had experienced a completed rape. Koss (1987) assessed the prevalence of rape in adolescence or adulthood and found that 15.4% of her sample had experienced a completed rape since age 14. This move toward measuring rape prevalence, in addition to incidence, was another important contribution Russell (1982) and Koss (1982, 1987) made to the field.

Contemporary Victimization Estimates

Many contemporary studies have continued to use behaviorally-specific questions to measure both rape incidence and rape prevalence. Nationally representative studies have found that 11.5%-18% of women in the United States have been raped at some point in their lifetimes (Breiding et al., 2014; Kilpatrick et al., 2007; Tjaden & Thoennes, 2000). Other non-nationally representative studies measuring rape only during adolescence and adulthood have found rape prevalence rates between 9.4%-30% (Botta & Pingree, 1997; Cleere & Lynn, 2013; Clements & Ogle, 2009; Koss, Figueredo, Bell, Tharan, & Tromp, 1996; Littleton, Axsom, Breitkopf, & Berenson, 2006; Littleton, Breitkopf, & Berenson, 2008; Orchowski, Untied, & Gidycz, 2013). Studies have found annual incidence rates ranging from .06%-1.6% in the general population (Breiding et al., 2014; Sinozich & Langton, 2014) to 1.7%-4.7% among undergraduate women (Fisher, Cullen, & Turner, 2000; Mohler-Kuo, Dowdall, Koss, & Wechsler, 2004).

While contemporary victimization estimates are substantially higher than the early estimates offered by the NCS (BJS 1984, 1987), there is notable variability from study to study. Some of this variation may be attributable to varying victimization risks in study samples, while other variation may be due to methodological differences between studies.

Variations in study sample. There are a variety of factors that make certain groups of people particularly vulnerable to rape. These elements may be identity-related (e.g. victim characteristics) or context-related (e.g. when/where the sample is drawn), but regardless, have the potential to impact the prevalence of rape victimization reported in a sample. What is known about these factors' abilities to predict prevalence rate variation is summarized in the following section.

Race/ethnicity. Research exploring variability in sexual victimization rates among different racial/ethnic groups have reported mixed results. Some studies have reported that non-Hispanic white women have a higher victimization rate than women of other races and ethnicities (Koss et al., 1987), while other studies have found that African American women are at greater risk than white women (Gross, Winslett, Roberts, & Gohm, 2006). Still other studies have found no difference between Black non-Hispanic and white non-Hispanic women (Breiding et al., 2014). It appears that racial/ethnic differences in victimization rates could be at least partially dependent on the perpetration tactics measured, with one national study finding that white college women were more likely than women of other races to have been raped while intoxicated, but less likely to report experiencing rape perpetrated through physical force (Mohler-Kuo et al., 2004).

There are two trends, however, that have consistently emerged. First, Hispanic women have reported less sexual victimization than non-Hispanic women in multiple large-scale studies

(Sinozich & Langton, 2014; Tjaden & Thoennes, 2000). Data reported by Breiding et al. (2014) also supported this disparity, though they did not test for statistically significant differences. Multiple studies have also found that American Indian and Alaskan Native women experience particularly high rates of sexual victimization (Koss et al., 1987; Tjaden & Thoennes, 2000). Again, the national study data reported by Breiding et al. (2014) supports this conclusion.

These findings suggest that a sample made up of large numbers of American Indian or Alaskan Native women may report a particularly high rape prevalence rate. Similarly, a sample comprised of many Hispanic women may report a rape prevalence rate lower than has been found in non-Hispanic samples. It is less clear whether samples whose racial diversity differs in other ways will see systematic variation in rape prevalence rates.

Age. Age has consistently emerged as an important risk factor for sexual victimization. National studies have estimated that 40.4%-61.6% of all rape victims experience their first rape before age 18 (Breiding et al., 2014; Kilpatrick, Edmunds, & Seymour, 1992; Tjaden & Thoennes, 2000) and 78.7% experience their first rape before the age of 25 (Breiding et al., 2014; Kilpatrick et al., 1992). Rape incidence measures aim to capture victimizations that have occurred relatively recently, therefore a higher proportion of participants in this high-risk age range could be expected to increase the victimizations reported in rape incidence studies. The role of participant age in rape prevalence studies, however, is less clear. Older participants would have had more years in which to experience victimization, and may therefore report higher rape prevalence rates, but it is also possible that the time elapsed between their highest-risk time periods and study participation could make them less likely to remember or report victimizations that have occurred.

College/community sample. There is little disagreement that college-aged individuals are at particularly high risk of sexual violence, but it remains unclear whether enrollment at a college or university adds additional risk (i.e., above and beyond the age risk, as noted above). Unfortunately, few studies have directly compared victimization rates among similarly-aged college and community samples. While many studies have found higher rates of rape in college samples than are generally found in community samples (Breiding et al., 2014; C. P. Krebs, Lindquist, Warner, Fisher, & Martin, 2009), and some have made claims that the party culture of many universities contributes to increased rates of rape among college students (Fisher, Sloan, Cullen, & Lu, 1998), it is difficult to disentangle the increased risk that comes from being college *aged* versus being enrolled in college. Contrary to previous assertions that college students are at a greater risk than non-students, a recent study that directly compared 18-24 year old students and non-students found that the non-student sample experienced rape at 1.2 times greater a rate than did 18-24 year old enrolled students (Sinozich & Langton, 2014). This study, however, did not ask explicitly about rapes that occurred when the victim was under the influence of alcohol or drugs or otherwise incapacitated ("incapacitated rape"). Incapacitated rape has been found to occur frequently among college students (Kilpatrick et al., 2007; C. P. Krebs, Lindquist, Warner, Fisher, & Martin, 2007; Mohler-Kuo et al., 2004); therefore, a study that compares student and non-student rape rates without including incapacitated rape in its measurement may be underestimating the occurrence of rape among college students. Whether studies that measure rape in college versus community samples should expect to find systematically different victimization rates is, as of yet, an unanswered question.

Study year. One last difference that must be considered when comparing study samples is whether differences in reported rates of victimization could be due to changes in victimization

rates over time. Since the late 1990's, activists and educators have engaged in concerted rape prevention efforts (Rozee & Koss, 2001). If these efforts have been successful, research should show a decrease in victimization rates since that time. Some studies have supported this, showing marked decreases in rape incidence rates since the mid-1990's (Finkelhor, Shattuck, Turner, & Hamby, 2013; White & Lauritsen, 2012). Studies of lifetime prevalence rates, however, have not mirrored this decline (e.g., Breiding et al., 2014), and it remains to be seen whether the reported decrease in incidence rates will eventually be reflected in longer-term prevalence rates, as well.

Variations in study methodology. Campbell and Adams (2009) succinctly summarize the possibility of methodological variability in rape prevalence rates, asking "How does what we study, and how we study it, influence who participates and what they tell us?" (p. 395). The next section of this paper will summarize extant literature on potential methodological sources of variation in rape victimization estimates.

Data collection method. Rape prevalence research primarily relies on victim self-report methods to gather prevalence data (Koss, 1996; Reddy et al., 2006; Rozee & Koss, 2001). Within the self-report category there are further distinctions between in-person interviews, phone-based interviews, automated telephonic interviews (also known as automated telephonic data collection, or ATDC), paper-and-pencil surveys, and computer-based surveys. As the method of data collection may feasibly influence both whether trauma victims participate in a study and what they disclose (Rosenbaum & Langhinrichsen-Rohling, 2006), many researchers see comparing disclosure rates found through different data collection methods as a fruitful avenue of study (DiLillo, DeGue, Kras, Di Loreto-Colgan, & Nash, 2006; Rosenbaum & Langhinrichsen-Rohling, 2006).

Existing studies have produced mixed findings regarding whether certain data collection methods systematically detect higher prevalence rates of rape than others. While some studies have found that less personal data collection methods, such as automated telephonic data collection, produce highest rates of rape disclosure (Reddy et al., 2006), other studies have found that participants disclose most fully during in-person interviews (Basile, 2002; Martin, Anderson, Romans, Mullen, & O'Shea, 1993). Many recent studies that compare web-based to paper-and-pencil style surveys have found that web-based surveys elicit greater disclosure on sensitive topics (Booth-Kewley, Larson, & Miyoshi, 2007; Kays, Gathercoal, & Buhrow, 2012; Weisband & Kiesler, 1996), but other studies have found that prevalence rates do not vary between these conditions (DiLillo et al., 2006).

Participants may also respond to data collection methods differently based on their age, race, or other demographic differences. Marginalized communities, for example, may carry historical distrust of scientific research and be particularly receptive to data collection methods, such as interviews, that prioritize rapport building (Campbell, Adams, Wasco, Ahrens, & Sefl, 2010; Ryen, 2003). Participants' familiarity with computers may also impact their disclosure on web-based surveys, potentially leading to reduced disclosure among older, poorer, or less educated respondents (Simmons & Bobo, 2015). Additionally, studies that gather data telephonically may under-represent low-income and/or other populations that may not have landlines (Rozee & Koss, 2001; Shield & Rehm, 2012; Simmons & Bobo, 2015).

Screening questions. Victimization screening questions "communicate to the respondent the kind of incidents the interviewer wishes to have recalled" (Koss, 1993, p. 207). Screening questions are typically separated into two categories: gate screening questions and behaviorally-specific screening questions (Fisher et al., 2000; Koss, 1993). The difference between gate and

behaviorally-specific screening questions has been described as the difference between a wide and an inverted funnel (Wyatt & Peters, 1986), in that gate screening questions start broad and get more specific if victimization is disclosed, compared to behaviorally-specific screening questions which use multiple narrow questions to arrive at an overarching label. A gate question may ask, for example, whether there was "ever a time when you were forced to have sex against your will, or were raped?" (Moore, Nord, & Peterson, 1989). In contrast, a behaviorally-specific screening questions does not "ask simply if a respondent 'has been raped' but rather describes a victimization incident in graphic language that covers the elements of a criminal offense" (Fisher & Cullen, 2000, p. 337).

Gate screening questions and behaviorally-specific screening questions offer different advantages. Gate screening questions are less time intensive for most participants to respond to, as non-victims must only answer a single question (Kilpatrick et al., 1985; Wyatt & Peters, 1986). Gate screening questions are also often less graphic (Fisher & Cullen, 2000; Koss, 1992), and may be preferred by researchers uncomfortable with exposing non-victims to detailed questions about possible assaults. Many researchers have advocated for the use of behaviorally-specific screening questions, however, arguing that gate screening questions are ineffective at stimulating recall of all intended events and may therefore systematically under-estimate rape prevalence (Fisher & Cullen, 2000; Koss, 1993, 1996; Rozee & Koss, 2001).

Consistent with this argument, repeated studies have found that behaviorally-specific screening questions detect higher rates of rape than do gate screening questions (Campbell & Townsend, 2011; Crowell & Burgess, 1996; Fisher et al., 2000; Kilpatrick, 2004; Koss, 1993). To explore and clarify this difference, Fisher et al. (2000) conducted a research project evaluating the degree to which type of screening question influenced rape incidence estimates.

Her team conducted telephone interviews with two random samples of college women. The content of the interviews was nearly identical except that one assessment used behaviorally-specific screening questions and the other used gate screening questions, and this methodological difference may have had a substantial impact on disclosure rates. The percentage of women who reported completed rape in the sample responding to behaviorally-specific screening questions was 11 times greater than the percentage who reported completed rape in the study using gate screening questions (Fisher et al., 2000). The study authors concluded that given the similarity in all other aspects of the study methods, the disparate rates of victimization "most likely stem from the wide range of behaviorally-specific screen questions" asked to one group and not the other (Fisher et al., 2000, p. 13).

Assessment scope. A rape victimization assessment can vary in scope based on the perpetration tactics, time period, and perpetrator characteristics included in the measurement. Regarding perpetration tactics, rape assessments typically either limit their scope to instances of force and threats of force only (e.g. Kilpatrick et al., 1992; Sinozich & Langton, 2014; Tjaden & Thoennes, 2000), or expand their scope to include rapes perpetrated by force, threats of force, and victim intoxication/incapacitation (e.g. Cleere & Lynn, 2013; Kilpatrick et al., 2007; Koss et al., 1987). While logic dictates that studies that include multiple perpetration tactics would find higher rates of victimization than studies that measure only one, this issue has been understudied empirically.

The time frame specified in a victimization measure also reflects variation in assessment scope. A frequently used victimization measure developed by Koss and colleagues' (1987), for example, assesses adolescent and adult victimization prevalence by asking about a participant's experiences since age 14. This measure could be expected to identify a different prevalence

estimate than would a measure asking only about participants' experiences since the age of 18. To date, however, no study has systematically compared the differences associated with a study's victimization age range.

Finally, assessment scope can be limited by the characteristics of the perpetrator. Some rape assessments ask only about perpetrators with a certain relationship to the victim, such as rapes committed by an intimate partner (e.g. Bell, Busch-Armendariz, Sanchez, & Tekippe, 2008). Other assessments exclude female perpetrators from their analysis and ask only about assaults committed by male offenders (e.g. Forbes & Adams-Curtis, 2001). There has not yet been a comparison of victimization rates based on these differences.

Key Gaps and Future Directions

Since the early 1980's, a large body of research has established that rape is a crime perpetrated against a great many women. A precise estimate of victimization, however, is elusive, as there is notable variation in rape prevalence findings. Koss (1993) stated, "The search for the causes of variation among prevalence estimates logically begins with a description of the processes that must occur for an instance of rape to be captured in the findings of a victimization study" (p. 204). Substantial energy has been directed toward studying many of those processes, and the literature review above describes the findings such research has produced.

However in the two decades since Koss called for an exploration of methodological processes that may impact victimization rates, one issue that has not yet been examined is participant recruitment method. Recruitment efforts introduce potential participants to a study, and may influence who decides to participate or what information they share. In rape victimization research, there are four commonly used participant recruitment methods. The first of those methods is "sign-up recruitment," in which researchers advertise a study and wait to be

contacted by potential participants. The second method, "captive audience recruitment," is characterized by recruiting a group gathered for another purpose, such as students attending a class. Though not required, entire group participation is the goal of this recruitment method and participation rates often exceed 90% (Bridgeland, Duane, & Stewart, 2001; Forbes & Adams-Curtis, 2001; Koss et al., 1987). The third method, "unsolicited mailing recruitment" involves sending a measure to unsuspecting people and asking them to complete and return it. Finally, "random digit dialing recruitment" entails calling randomly identified potential participants to solicit their participation.

These four methods differ in important ways. Studies using the sign-up recruitment method create a scenario in which the study is advertised to a wide range of potential participants and participation of any specific individual is not assumed; rather, potential participants are in a position to "opt-in" to the study. In contrast, studies that use captive audience, unsolicited mailing, or random digit dial recruitment methods are asking specific people to take part in the study, creating a scenario in which those individuals must "opt-out" of participation. Relatedly, the dynamics of participant refusal differ between recruitment methods, as well. Individuals who do not want to participate in a study using sign-up or unsolicited mailing recruitment methods are able to decline to participate passively by not signing up or not returning the survey that was mailed to them. People recruited for studies using captive audience or random digit dial recruitment methods, on the other hand, must actively communicate their non-participation to the study facilitator. One can imagine these dynamics of consent and refusal being especially impactful in rape-related research. If rape victims have different preferences for research participation than non-victims, recruitment methods that provide easy options for non-participation may impact the proportion of rape victims who will participate in a given study.

These recruitment methods also offer potential participants different amounts of information about the study when soliciting their participation. The nature of the unsolicited mailing recruitment method means that the potential participant has the full content of the study available to them when deciding whether or not to participate. They are able to look through the measurement instrument, or even start filling it out, and decide against participation if they do not like what they see. In contrast, people recruited through the other three methods must rely solely on the study description given by the researcher when deciding whether or not to participate. Some research has found that rape victims choose to participate in rape-related research out of a desire to share their stories and help other women (Campbell & Adams, 2009; Campbell et al., 2010), opening up the possibility that rape victims may frequently volunteer for studies they know are about rape and may therefore be disproportionately represented in the sample. This has not yet been tested, however, and it is equally plausible that rape victims might decline to participate in rape research more frequently than do non-victims.

Participant recruitment methods also vary based on the amount of contact potential participants have with study facilitators during recruitment. Participants recruited through sign-up or unsolicited mailing methods have no contact with researchers during recruitment; rather, they leave their contact information (sign-up) or receive a survey (unsolicited mailing) without direct interaction with the study facilitator. Individuals recruited through captive audience methods generally have minimal contact with a study facilitator, as would be the case if a researcher spoke briefly to a class of students before handing out a survey. Those recruited through random digit dial methods have substantial contact with the study facilitators at recruitment, as the study facilitators are calling them directly and asking them to participate. Some research has suggested that increased feelings of anonymity increase rape victims'

willingness to disclose their victimization (Reddy et al., 2006), but other research has suggested that rape victims disclose most fully in conditions that offer increased contact between participant and researcher (Basile, 2002).

Table 1.
Recruitment Method Differences

Characteristics	Sign-up	Captive Audience	Unsolicit. Mailing	Random Digit Dial
Selection	Opt-in	Opt-out	Opt-out	Opt-out
Refusal	Passive	Active	Passive	Active
Access to full study at recruitment	No	No	Yes	No
Contact with researchers at recruitment	No	No/Minimal	No	Yes

Table 1 summarizes these differences and demonstrates that no two participant recruitment methods are identical in all categories. It is unknown whether the use of one participant recruitment method over another impacts study findings. Specifically in studies that report on rape prevalence, it is not known whether the method used to recruit participants predicts the proportion of rape victims who decide to participate in, or disclose their victimization during, the study. The lack of research exploring this potential source of variation has left an important gap in knowledge, and it is this gap addressed by the current study.

METHODS

Methodology and Research Questions

To examine this topic, a meta-analysis and meta-regression were conducted to examine whether participant recruitment method and other study-level covariates are associated with variation in rape prevalence rates. The study was guided by the following research questions:

RQ1: How much variation in sexual assault prevalence rates is observed in research studies?

RQ2: Does the method through which participants are recruited for research studies predict variation in rape prevalence estimates?

RQ3: Are other sample or study methodology related variables predictive of variation in rape prevalence estimates?

Sample

Target population. The target population for this meta-analysis was all peer-reviewed studies conducted in the United States that were published in English after January 1, 1980 and reported the prevalence of rape experienced by women during adolescence or adulthood.

Database selection. After consultation with the Michigan State University Psychology Reference Librarian and carrying out a feasibility assessment, this study was carried out using the Proquest database. Not only does Proquest offer excellent coverage of interdisciplinary journals (e.g., psychological, sociological, medical, military, etc.) but it also facilitates searching within both PsychInfo and PsychArticles at the same time, whereas a number of other databases are unable to search both simultaneously. Proquest also offers a variety of options regarding where in the article search terms could be located, as well as a methodological search restriction.

Search terms and restrictions. *Search term* restrictions were implemented, such that "rape" or "sexual assault" had to appear anywhere in the article other than full text[3]. Results were also restricted by *language* to those written in English, and by *date* to include only articles published after 1/1/1980. *Methodology* was limited to "empirical studies" only. Finally, results were limited to *peer-reviewed* articles. Figure 1 demonstrates the culling of articles returned at each step.

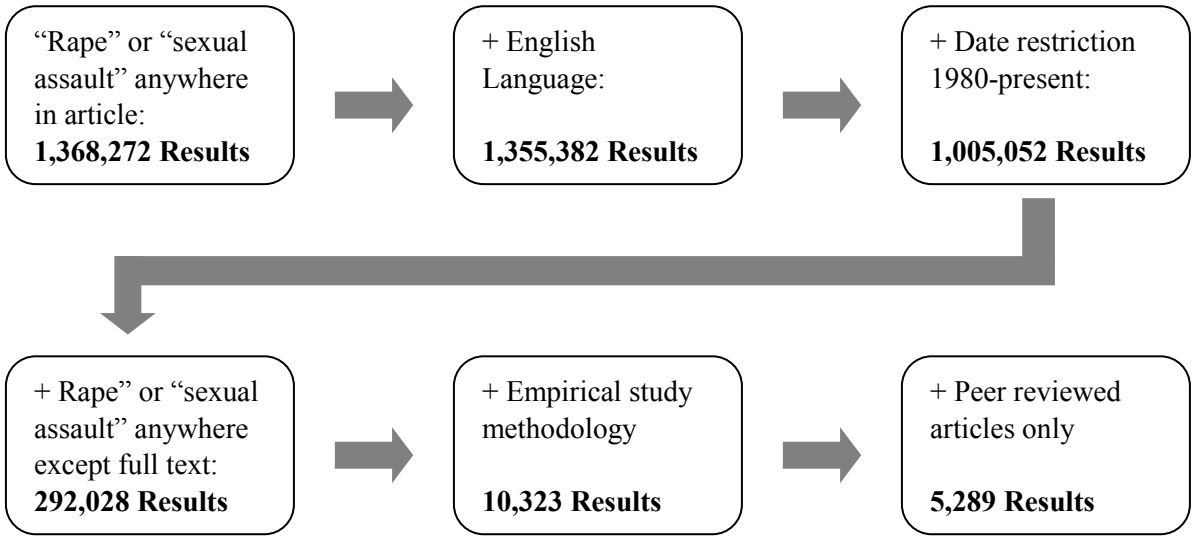

Figure 1. Progressive Restriction of Search Terms and Corresponding Results

There are trade-offs to the peer-review restriction that must be acknowledged. Restricting a meta-analysis to peer-reviewed articles can introduce publication bias, in which studies with an unfavorable result are not published, and the results of a meta-analysis relying only on published literature are therefore biased (Borenstein et al., 2009). Publication bias is most frequently related to inflated effect sizes, as smaller, non-significant results are the most likely to remain

[3] The "anywhere but full text" search option requires that search terms appear in the article title, abstract, or key words.

unpublished (Borenstein et al., 2009; Dickersin & Min, 1993; Egger & Smith, 1998). The suppression of non-significant effects was not of major concern in this study, as prevalence statistics are not evaluated in terms of significance (i.e., a prevalence rate of 20% is no more or less significant than a prevalence rate of 40%). If, however, sexual assault studies are considered more or less publishable depending on their identified prevalence rate, publication bias could still impact the direction or magnitude of study findings. To this author's knowledge, no such trend has been identified in the literature, but such bias remains a possibility. Though it would have been ideal to include all relevant published and unpublished literature in this study, there was a pressing need to reduce the number of articles to a more manageable number, and with that goal, the peer-review restriction was implemented. The resulting sampling frame was 5,289 articles.

Procedures

Developing inclusion and exclusion criteria. With the goal of identifying a final sample of articles that report rape prevalence data experienced by women in the United States during adolescence or adulthood, the following inclusion criteria were developed:

1. The study must provide rape victimization prevalence data, or must provide data such that victimization prevalence rate can be calculated.
2. Rape victimization data must have been gathered in such a way that it is distinguishable from other crimes (e.g., domestic violence, stalking).
3. Victimization status of participants must not have been known by researchers at the time of participant recruitment.
4. Rape victimization data must have been gathered directly from potential victims.
5. Rape victimization data must be specific to female participants, or must be presented in such a way that rape prevalence can be separated by gender.

6. Participants' mean age must be over 18, and there must be no indication that minors needed parental consent to participate.
7. The victimization prevalence rate must be specific to experiences of rape during adolescence/adulthood (ages 14 and older).[4]
8. The sample must not have been selected based on the expectation that they experience rape at an unusually high rate (e.g., psychiatric in-patients, homeless women in substance abuse treatment programs, etc.).
9. The study must have been conducted in the United States.
10. The study must present a prevalence rate specific to penetrative (oral/anal/vaginal) rape.

Exclusion codes (presented in Table 2) were applied to each excluded article to track the reason each article would not be included in the meta-analysis. In general, there were four main types of exclusion codes. Studies could be excluded for prevalence-related reasons, meaning that the article did not ask individuals about their rape experiences in a way that allows for the calculation of prevalence data (e.g., the study specifically recruited rape victims). Studies could also be excluded due to the makeup of their sample if the study sample was drawn from populations outside the scope of this proposed project (e.g., male victims). Additionally, studies could be excluded if they only measured types of victimization that were outside the scope of this proposed project (e.g., childhood victimization). Finally, studies could be excluded if information needed to conduct the proposed analysis was missing from the article or if the article

[4] While inclusion rule 6 and inclusion rule 7 both refer to age, it is important to note that inclusion rule 6 refers to the participant's age *at the time of participation in the study* and inclusion rule 7 refers to participant age *at the time of victimization.*

indicated that other study related characteristics were outside the scope of the proposed project (e.g., the provided prevalence rate was of all unwanted sexual contacts).

Table 2.
Exclusion Codes and Explanations

Prevalence Related Exclusion Codes	
Exclusion Code	Explanation
ND-V/P (No Data on Victimization/Prevalence)	Studies that do not collect rape victimization prevalence data will not have the effect size necessary to be included in the meta-analysis.
PERP (Perpetrator)	The study asked questions or focused exclusively on perpetrators/perpetration, not victims/victimization.
HRP (High Risk Population)	Studies in which the sample was selected based on the belief that the rape prevalence rate among that group would be substantially above average will be excluded. Inclusion of these studies would be expected to increase within-group variance to the point that identification of group differences would be extremely difficult.
AVS (All Victim Sample)	Any sample in which participants were recruited on the basis of their victimization status would have a victimization prevalence rate of 100%. Because all research questions in this project involve understanding the factors that affect victimization prevalence in a sample, samples in which the prevalence rate is guaranteed to be 100% will not contribute toward answering these questions.
SDC (Secondary Data Collection)	A primary research question involves understanding how participant recruitment impacts prevalence findings, therefore studies that do not recruit participants (but rather present prevalence information based on secondary records such as police reports) must be excluded.
Sample Related Exclusion Codes	
Exclusion Code	Explanation
MVS (Male Victim Sample)	This project examines only the prevalence of rape among women, therefore any studies comprised solely of male victims or, or where data is collected in such a way that male and female victims cannot be separated, will be excluded.
NAS (Non-Adult Sample)	Only samples comprised of adult women will be included in this study, therefore any studies specific to minors (e.g., middle school students, high school students) will be excluded. Studies conducted primarily with minors often have to attain parental consent during participant recruitment, and such a requirement has the potential to substantially impact who participates and therefore serves as a confounding variable when attempting to explore the predictive ability of recruitment method on prevalence rates.

Table 2. (cont'd)	
NUSA (Non-USA)	Because rape prevalence rates can vary widely between countries, this project only measures the prevalence of rape among women in the United States. Therefore, I will exclude studies that are conducted outside of the United States.
Victimization Experience Related Exclusion Codes	
Exclusion Code	Explanation
LVE (Lifetime Victimization Experiences)	This study is specific to rape experienced during adolescence or adulthood. Studies that measure rape over the lifetime (e.g., "Has anyone ever…") diverge substantially from this focus and will therefore be excluded.
CVE (Childhood Victimization Experiences)	This study is specific to rape experienced during adolescence or adulthood. Childhood victimization is outside scope of this project, and therefore any articles that only report data about childhood victimization experiences will be excluded.
TLSS (Time, Location, Situation Specific)	Some research studies measure rape in specific contexts only, such as on a date, occurring at work, or while on a college campus. Because the proposed study is comparing measurements of all rapes occurring in adolescence or adulthood, these situation-specific assessments should be excluded.
Study Related Exclusion Codes	
Exclusion code	Explanation
DI (Definitional Inconsistency)	As has been previously discussed, prevalence estimates that include any unwanted sexual contact (e.g., forced kissing) would be expected to be substantially greater than those specific to the upper end of the rape spectrum. The acts included at the upper end of that spectrum are typically vaginal, oral, or anal penetration by a perpetrator's sex organ, hands, or any foreign object. Studies that measure only sexual assault outside of this definition (e.g., unwanted groping, kissing, etc.) or that collect data in such a way that those categories cannot be separated, will be excluded. This code will also be applied to rape definitions that only ask about female perpetrators.
DUP (Duplicate)	Because each sample can only be included once in a meta-analysis and multiple articles may be published on one study, I will need to exclude publications using data that has been presented in an already included article. I will evaluate articles for inclusion beginning with the oldest articles (i.e., 1980 to present) and will therefore include the first publication of a data set that provides all necessary information (i.e., that has not been excluded for any other reason). Additional articles published on already included data will receive this exclusion code.

Applying exclusion codes. Articles were first evaluated through a read of their title and abstract. There were times when these two pieces of information were sufficient to decide that an article should be excluded. The article titled "Dissimulation in Phallometric Testing of Rapists' Sexual Preferences," for example, strongly implies that the article is about perpetrators, not victims, and should therefore be excluded. The article's abstract confirmed this suspicion, ("Sexual preferences of 38 rapists were assessed…") and the article was excluded without a read of the full text. If there was any doubt as to an article's suitability for inclusion, however, the full text of the article was read to determine its inclusion status. Because the exclusion codes were not mutually exclusive, a process was developed (presented in Appendix A) for systematically prioritizing and applying the exclusion codes.

After applying the inclusion criteria and corresponding exclusion codes to all 5,281 articles in the initial sample and resolving any uncertainties with the project's committee chair, it was determined that 78 articles met the study's inclusion criteria and could be included in the meta-analysis.

Measures

The following section describes the operationalization of variables that were coded for inclusion in the meta-analysis and meta-regression.

Effect size. The proportion of rape victims in a sample was the effect size in this meta-analysis. This proportion may have been given as a percentage (e.g., "18% of the women in our sample had been raped") or as a proportion (e.g., "We screened 500 women for rape and contacted the 103 women who reported experiences that met our definition of rape"). Prevalence rate was recorded as a proportion rounded to the thousandth decimal place (e.g., .206).

Covariate analysis. Included articles were coded for a number of variables, including participant recruitment method and other variables discussed in rape prevalence literature as factors that may impact rape prevalence rates. Due to lack of variability in the final sample or extensive missing data, not all relevant variables could be included in the final analysis (see Appendix B for explanation of these variables' exclusion). The following section describes the operationalization of the variables included in the final analysis.

Recruitment method. Prior to coding the full sample of included articles, the goal of the project was to compare sign-up recruitment, captive audience recruitment, unsolicited mailing recruitment, and random digit dial recruitment in this meta-analysis. However, only one of the 78 included studies recruited through random digit dialing, therefore that category was excluded from the recruitment method meta-regression. Operational definitions for the remaining three recruitment methods included in the analysis can be found below in Table 3.

Table 3.
Participant Recruitment Method Operational Definitions

Recruitment Method	Operational Definition
Sign-Up Recruitment	To be categorized using as sign-up recruitment, studies: • Must advertise their study in public places and either provide space for potential participants to leave their information or provide information contact information for potential participants to contact study facilitators. • May advertise their studies with paper or online advertisements, but must not provide access to the full study at recruitment.
Captive Audience Recruitment	To be categorized as using captive audience recruitment, studies: • Must recruit participants from a pre-existing group of people (e.g., a classroom, a Church group, etc.). • Must aim to get full-group participation. • Must assume participation until people state that they do not want to participate. • Must administer the victimization measure at the time of recruitment.

Table 3. (cont'd)	
Unsolicited Mailing Recruitment	To be categorized as using unsolicited mailing recruitment, studies: • Must send the victimization measure to people who have not previously signed up to participate in the study. • May send the victimization measure through postal mail or email.

Mean participant age. As was discussed in the literature review, it is unclear how participant age may relate to victimization rates in rape prevalence studies. To explore this variable, mean participant age was included as a covariate in the analysis. This variable was entered to one decimal place (e.g., 20.6) whenever possible.

Sample source. Discerning how victimization levels vary depending on the source participants were drawn from (e.g., college, community) has been another source of tension within rape prevalence literature (Fisher et al., 1998; Kilpatrick et al., 2007; Sinozich & Langton, 2014). In this analysis, sample source was defined as a categorical variable with three categories: college, community, and military. Studies that drew their sample from a mix of source categories (e.g., part community and part college student sample) were coded as missing for this variable.

Publication year. To evaluate the possibility that victimization rates or their disclosure have changed over time, publication year was recorded as a continuous integer variable in the analysis. The original coding plan was to include data collection year, rather that publication year, in the analysis; however, 52 of the 78 included studies did not specify the year(s) of data collection, therefore publication year was used as a proxy variable.

Screening questions. Studies were coded dichotomously as using either "behaviorally-specific" or "gate" screening questions to assess participants' histories of sexual victimization. When the distinction was not overtly stated in the article, articles were coded as using behaviorally-specific screening questions if they explicitly described the discrete acts and tactics

that constitute rape. Articles reporting on screening questions that did not explicitly describe the acts and tactics constituting rape were coded as having used gate screening questions.

Perpetration tactics. Included articles were also coded for the perpetration tactics included in their victimization assessment questions. Perpetration tactics was coded as a dichotomous variable based on whether the assessment scope was limited to force/threats of force only ("force only") or included victim intoxication/incapacitation ("force/incapacitation").

Victimization age cutoff. As was discussed previously in the inclusion/exclusion section of this document, this meta-analysis was restricted to studies that asked women about rape victimization during adolescence or adulthood. The exact age given as a lower bound for adolescence (e.g., "Since the age of 14…", "Since the age of 18…") was recorded as the study's victimization age cutoff. This variable was entered as a continuous integer variable.

Data Entry

Prior to data analysis, information on all relevant variables was recorded onto computerized coding sheets. A randomly selected 20% of the paper records were checked against the original articles for error and no error was identified (100% accuracy). Data were then entered from the computerized coding sheets into the software program that would be used for analysis. After this second phase of data entry was complete, a different 20% of articles were randomly selected and accuracy was compared between the software data entry screen and the computerized records. Again, minimal error was identified (98.9% accuracy). Identified errors were corrected and the data was deemed ready for analysis.

Analysis

Data analysis was conducted using Comprehensive Meta-Analysis Version 3 (Borenstein, Hedges, Higgins, & Rothstein, 2013) and employing a random-effects model. Random-effects

models are recommended when sample or study differences suggest that there is true variation from study to study (Borenstein et al., 2009), as was the case with the studies included in this analysis. Research Question 1 ("How much variation in rape prevalence rates is observed in research studies?") was addressed in this main meta-analysis. Research Questions 2 ("Does the method through which participants are recruited for research studies predict variation in rape prevalence estimates?") and Research Question 3 ("Are other sample or study methodology related variables predictive of variation in rape prevalence estimates?") were addressed through a meta-regression that accompanied the meta-analysis. The meta-regression also used a random-effects model, as this is the recommended course of action when the covariates are expected to explain some, but not all, of the variation in effects (Borenstein et al., 2009).

The effect size for this study was the proportion of rape victims in a sample, therefore the summary effect size estimates the proportion of rape victims one would find in a full population of rape prevalence studies. Prevalence rates were calculated from the sample size (i.e., number of female participants in the study who provided victimization data) and events (i.e., the number of those participants classified as rape victims according to study criteria). All analyses used two-tailed tests with an alpha level of .05. Because many of the observed proportions were expected to fall below .2 or above .8 and the standard error is compressed as the proportion nears 0 or 1, Lipsey and Wilson's (2001) recommendation to use a logit transformation of the raw proportion was followed. The intercepts and regression coefficients were inverse log-transformed after the analysis was completed for ease of interpretation.

The analysis included four separate models. The first model did not include any covariates and was used only to assess heterogeneity in the final sample without any explanatory predictors. The second model included participant recruitment method as the sole covariate, with

sign-up recruitment as the reference group. The third model included main effects for all included covariates (recruitment method, sample source, screening question, perpetration tactics, mean participant age, victimization age cutoff, and publication year). The final model included all previously-included covariates as well as the interaction between perpetration tactics and sample source. The interaction term was mean centered in Model 4, and the other continuous variables were centered (either around the mean or another meaningful value) in all models to facilitate interpretability of the intercept.

RESULTS

Characteristics of Included Studies

Seventy-eight studies were included in the final meta-analysis. The most common recruitment method was sign-up recruitment (k = 46), followed by captive audience recruitment (k = 14) and unsolicited mailing recruitment (k = 11), with the remaining studies either not reporting their recruitment method (k = 2) or reporting a type of recruitment method outside the scope of this project (k =5). The studies were primarily drawn from college samples (k = 58), with a sizeable minority drawn from community (k = 15) and military (k = 4) samples, and one study drawing from a mixed college/community sample. Most studies used behaviorally-specific questions (k = 76) compared to gate screening question (k = 2)[5]. Studies that included intoxication as a perpetration tactic (k = 60) were more common than studies that restricted perpetration tactics to use of force (k = 18). The modal victimization age cutoff was 14 (k = 64) with the remaining studies (k = 12) reporting an age cutoff ranging from 15 to 18. Mean participant age ranged from 18.1 to 45.3, with a mean of 22.7 (SD = 6.3). Publication year ranged from 1987 to 2016 (k = 3 studies published in 1980's, k = 18 studies published in 1990's, k = 29 studies published in 2000's, and k = 28 studies published in 2010's). Table 1 (Appendix C) summarizes these characteristics by article.

Meta-Analysis Results

The rape prevalence rate across the 78 studies ranged from 5.3% to 48.9%. The pooled rape prevalence rate across studies was 17.8% (95% CI [16.1%, 19.7%]; see Appendix D for forest plot). The included studies had a large amount of heterogeneity, $Q(77) = 2071.23$, $p <$

[5] Though the distribution of studies between these categories was quite skewed, this variable was included due to its substantial theoretical relevance. Presentation of the final model will include a summary of changes in the model when studies using gate screening questions and the corresponding variable were excluded.

.001, and inconsistency, I^2 = 96.28%. Variation in true effects was also substantial (T = .529, T^2 = .279), which supports the utility of the meta-regression discussed in the next section.

Sensitivity and publication bias analyses were also conducted on this full sample of studies. The sensitivity analysis leaves out one study at a time and assesses the pooled effect size to determine whether any one study contributes unduly to the summary effect size. Exclusion of any individual study resulted in only a marginal change in the effect size (mean prevalence rate varied from 17.5% to 18.1%). There was no jump of more than .1% between effect sizes when analyzed in this way, indicating that no one study had an undue effect on the summary effect size.

Traditional publication bias analysis looks for missing studies with small sample sizes and effect sizes, as these are the studies most likely to remain unpublished and therefore be missing from a meta-analysis. Because this study was utilizing an effect size not based on statistical significance, and therefore it was unknown from which side of the distribution studies might be missing from, both sides of the funnel plot were examined for potential asymmetry.

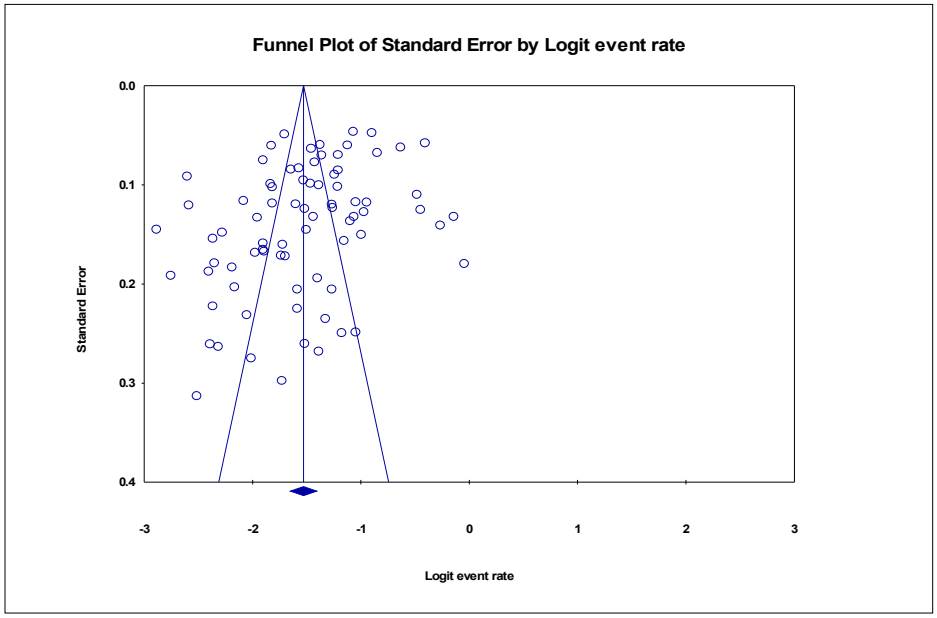

Figure 2. Funnel Plot of Standard Error and Logit Event Rate

Visual examination of the funnel plot (Figure 2, above) suggested the results were skewed toward smaller effect sizes. This subjective interpretation is supported by Begg and Mazumdar's Rank Correlation Test. The rank order correlation used in this test was -.15 (recommended one-tailed p-value = .03). Duval and Tweedie's Trim and Fill method produced similar findings, identifying an uneven distribution of studies that would require 17 studies on the right side of the distribution to symmetrize fully. In most meta-analyses, publication bias skews the effect size to the right of the distribution, indicating that studies with larger effect sizes and larger sample sizes are more likely to be published. That pattern is reversed in this meta-analysis, in which there are a disproportionately small number of studies with large effect sizes.

Though these tests identified asymmetry, they do not indicate the reasons for the asymmetry. It is possible that sexual assault studies are less likely to be published if they report a high rate of victimization in their sample. It is also possible that a highly predictive variable has an uneven distribution of studies in each category, such that more studies are in the category associated with lower prevalence rates. Sample source is one such potential variable: 53 of the 78 studies in this meta-analysis were based off of college student, compared to community or military, samples. If college student samples tended to have significantly lower prevalence rates than community or military samples, the skewed distribution of effect sizes could simply reflect that variation. This issue is further addressed in the subsequent meta-regression, which was carried out with the goal of identifying study-level predictors of effect size variation.

Meta-Regression Results

In order to ensure that all meta-regression models are comparing the same set of studies, the Comprehensive Meta-Analysis software program restricts the meta-regression sample to studies that have data on all included covariates. This process left 66 studies for the meta-

regression analysis[6]. Studies were excluded in this regard due to not providing information about their recruitment method (k = 1), mixing multiple recruitment methods (k = 1), using a recruitment method outside the scope of this study (k = 5), or lacking participant age information (k = 3). Additionally, studies were excluded from the regression if, after consultation with the project's committee chair, they seemed to be substantially impacted by a moderator that had not been proposed for study and was not reflected in a sufficient number of studies to include as a covariate. Studies involving alcohol administration (k = 3) were excluded from the meta-regression for this reason. In these three studies, participants consumed (or were led to believe they consumed) alcoholic beverages in a laboratory setting, with the goal of testing how lowered inhibitions due to alcohol affect perceptions of risk. Alcohol administration is advertised as a component of these studies, and participants are generally required to be regular drinkers in order to be eligible for participation. These potential biasing factors (i.e., the requirement to be a regular drinker and the required interest in consuming alcohol as part of study participation) would need to be captured as a covariate in order to contribute positively to the meta-regression; however, the extremely skewed distribution of studies including this potential covariate (75 : 3) led to the decision to exclude these studies from the meta-regression[7].

Model 1, which included only the intercept, is essentially a meta-analysis of these 66 studies (Borenstein et al., 2009). The pooled rape prevalence rate across these 66 studies was

[6] Studies that were excluded from the meta-regression due to missing data were retained for inclusion in the meta-analysis.

[7] In order to fully understand the impact of removing these studies, the meta-regression models were re-run with the alcohol administration studies included. The model that included alcohol administration studies and an 'alcohol administration' covariate had slightly worse model fit statistics ($Q = 571.70$, $I^2 = 90.03$, $T = .3421$) than the model that excluded these studies ($Q = 532.97$, $I^2 = 89.68$, $T = .3337$). The proportion of variance explained was similar between models, with an R^2 analog of .55 in the model that excluded the three 'alcohol administration' studies and an R^2 analog of .56 in the model that included 'alcohol administration' studies and the accompanying covariate. Due to the skewed distribution of the studies between categories, the lack of improvement in model fit, and the relative lack of theoretical importance of this variable, I excluded the three 'alcohol administration' studies from my analysis.

16.7% (95% CI [15.1%, 18.6%]. This sample has a slightly smaller summary effect size than did the full meta-analysis. This was to be expected, as the three studies excluded due to alcohol administration had substantially higher effect sizes than did most other studies, as did two of the studies excluded for their use of snowball recruitment strategies. The remaining 66 studies included in the meta-regression retained significant heterogeneity, $Q(65) = 1533$, $p < .001$, and inconsistency, $I^2 = 95.76\%$. Variation in true effects remained high ($T = .4962$, $T^2 = .2462$).

A test of Model 2, which included recruitment method as the sole covariate, yielded a *Q*-value of 5.46 with 2 degrees of freedom and a corresponding significance level of $p = .07$. Since recruitment method was the only covariate in this model, the *Q*-value for the model is identical to the omnibus test for the recruitment method covariate. In this case, the non-significant p-value indicates that we cannot reject the null hypothesis that all coefficients (in this case, the recruitment method omnibus coefficient) are actually zero. While the recruitment method variable trends toward significance, we cannot reject the null hypothesis that the true effect size is the same regardless of recruitment method. While the captive audience recruitment to sign-up recruitment comparison reflected this trend toward significance only ($p < .1$), the unsolicited mailing recruitment to sign-up recruitment comparison did reach statistical significance ($p<.05$), with studies using unsolicited mailing recruitment methods reporting a 1.4 times higher prevalence rate than studies using sign-up recruitment (OR = 1.406, 95% CI [1.0058, 1.9662]. The R^2 analog of .05 suggests that the model explains 5% of the variance in true effects, though because the full model did not reach statistical significance we cannot reject the null hypothesis that the model actually explains no variation. Correspondingly, the I^2 statistic for this model is identical to that of Model 1 ($I^2 = 95.35\%$), though the variation in true effects was slightly lower ($T = .4838$, $T^2 = .2341$).

The next model, Model 3, added screening questions, sample source, perpetration tactics, mean participant age, victimization age cutoff, and publication year to the analysis. The *Q*-value for this third model was 66.10 with 9 degrees of freedom and an associated significance of $p < .001$, signifying that we can reject the null hypothesis that all covariate coefficients are zero. In other words, we can conclude that at least one of the covariates is related to the effect size (Borenstein et al., 2009). The significant goodness of fit test ($Q = 657.79$, $df = 56$, $p < .001$) indicates that there is unexplained variation left in the model. The I^2 value of 91.49% communicates that 91.49% of the variation in observed effects is due to variation in true effects rather than sampling error. Correspondingly, the *T* statistic was .3685, indicating a large standard deviation of true effects. The R^2 analog for this model was .45, signifying that 45% of the variance is explained by the covariates.

In this larger model, recruitment method was not a significant predictor of variation in rape prevalence rate, nor was victimization age cutoff or publication year. Screening question, sample source, perpetration tactics, and mean participant age all predicted significant variation in studies' rape prevalence rates with the other covariates held constant. Consistent with previous literature, screening questions were a significant predictor of rape prevalence rate (*OR* = - .3583, *SE* = .2967, $p < .001$), suggesting that studies using gate screening questions reported rape prevalence rates approximately 65% lower than studies using behaviorally-specific questions. Due to the small number of studies using gate screening questions ($k = 2$) compared to behaviorally-specific screening questions ($k = 64$), this result should be interpreted cautiously, but it is worth noting that the direction and magnitude of the coefficient is well supported by other empirical studies (Fisher & Cullen, 2000; Koss et al., 1987; Rozee & Koss, 2001). Additionally, the model was re-run without the two studies that used gate screening questions

and the accompanying covariate and the only change in the model was a reduction in proportion of variance explained. The omnibus test for sample source was also significant (Q = 11.69, df = 2, $p < .01$). Looking at sample source by category with college students (k = 50) as the reference group, there was not a significant difference between community samples (k = 12) and college samples (OR = .9357, SE = .2106, n.s.), but there was a significant difference between military samples (k = 4) and college samples (OR = 2.1151, SE = .2748, $p < .001$), such that studies using military samples had an average prevalence rate 2.1 times higher than studies reporting on college student samples. Perpetration tactics significantly predicted prevalence rates in this full model as well (OR = 1.4521, SE = .1271, $p < .01$), with studies that asked about rape via incapacitation in addition to rape via force finding prevalence rates nearly 45% higher than studies that restricted their inquiry to rape committed via physical force. Finally, mean participant age significantly predicted studies' prevalence findings (OR = 1.0321, SE = .0134, $p < .05$), with each one year increase in a study's mean participant age increasing the odds that participants in that study would report rape victimization by approximately 3%.

Finally, Model 4 included all previously-tested covariates as well as the interaction between sample source and perpetration tactics. Ideally, all three sample sources would have been tested in this interaction analysis, however the small number of military studies (k = 4) precluded meaningful interaction analysis; therefore, the sole interaction term was Community Sample X Perpetration Tactics. Similar to the main effects model, the significant Q-value for this model indicated that at least one of the covariates was related the effect size (Q = 91.80, df = 10, $p < .001$.). Also similar to the main effects model, the goodness of fit statistics confirmed that unexplained variance remained (Q = 532.97, df = 55, $p < .001$). This model fit statistics for the interaction model indicated better fit than the main effects model (I^2 = 89.68%, T = .3337). The

R^2 analog for this model was .55, indicating that 55% of the variance is explained by the included covariates.

The Community Sample X Perpetration Tactics interaction predicted significant variation in the effect size (*OR* = .277, *SE* = .3528, $p < .001$). This interaction was further explored in SPSS by grouping the data according to sample source and then running a regression with the rape prevalence rate as the dependent variable and perpetration tactics as the independent variable. While perpetration tactics was not predictive of prevalence rate variation in studies based on community samples, it did predict significant variation in studies based on college samples ($F(1, 56) = 14.46$, $p < .001$), with college studies that included incapacitation as a perpetration tactic finding significantly higher prevalence rates than college studies that only assessed rape via physical force.

With this interaction included, the community sample to college sample comparison that had been non-significant in the main effects model became significantly predictive in this model (*OR* = 1.8848, *SE* = .2714, $p < .05$), with studies based on community samples reporting 1.8 times greater prevalence rates than studies based on college student samples when perpetration tactics was held constant. The other variables that were non-significant in the main effects model (recruitment method, victimization age cutoff, and publication year) remained non-significant in the interaction model, and the variables that were significant in the main effects model remained significant in the interaction model, as well. The military sample to college sample comparison decreased slightly in magnitude (*OR* = 1.8015, *SE* = .2608, $p < .05$), with military samples detecting an average prevalence rate 1.8 times greater than studies using college samples. Screening questions (*OR* = .2707, *SE* = .2816, $p < .001$), perpetration tactics (*OR* = 1.8201, *SE* =

.1326, $p < .001$), and mean participant age (*OR* = 1.0545, *SE* = .0136, $p < .001$) all increased in magnitude with this additional covariate held constant.

DISCUSSION

Summary and Discussion of Findings

Across the 78 studies included in the meta-analysis, approximately 18% of female participants reported a history of rape as an adolescent or adult. In interpreting this finding, it is important to remember that the vast majority of studies included in this meta-analysis used convenience sampling methods and were not carried out with the goal of attaining a generalizable prevalence rate. This summary effect size should therefore not be interpreted to mean that 18% of women in the general population have experienced rape in adolescence or adulthood. Instead, the summary effect size confirms that researchers can expect to find a sizeable number of rape victims in a variety of contexts and using a variety of methods.

Research Question 1 asked how much variation is observed among these prevalence rates, and the results are based on the full meta-analytic sample of 78 studies. Studies included in this meta-analysis reported rape prevalence rates as low as 5.3% and as high as 48.9%. Statistics capturing heterogeneity and inconsistency confirm the significance of this spread, and lead to the conclusion that there is a great deal of variation in this collection of studies. This finding supports the underlying purpose of this project and the subsequent research questions that explore potential sources of this variation.

Research Question 2 and Research Question 3 examined the factors that predict variation in rape prevalence rates, and are therefore based on the meta-regression sample of 66 studies. Research Question 2 asked whether participant recruitment method could explain a significant amount of variation in rape prevalence rates. When examined as the sole predictor, this covariate trended toward but did not reach statistical significance ($p = .07$), and it did not approach statistical significance in models that included other relevant variables. What should one take

away from these non-significant findings? First, the trend toward significance in the bivariate model but not the multivariate model suggests that recruitment method may be correlated with another predictor that was better able to explain variation in prevalence rates; this possibility will be explored in greater depth shortly. Second, because recruitment method had never before been examined as a potential predictor of rape prevalence rate variation, any findings, null or otherwise, can inform future research. Researchers frequently make methodological decisions in an attempt to attain an unbiased sample (e.g., White & Humphrey, 1997; Tansill, Edwards, Kearns, Gidycz & Calhoun, 2012), and prior to this meta-regression, researchers have not known whether recruitment method may be one such biasing factor. In other words, researchers have not known whether a study that used sign-up recruitment would uncover a different prevalence rate than an identical study that used captive audience recruitment or unsolicited mailing recruitment. This meta-regression provides initial evidence that researchers can choose the recruitment method that best meets their needs without concern that it will bias their rape prevalence findings. It should be noted, however, that this evidence is based on a comparison of sign-up recruitment, captive audience recruitment, and unsolicited mailing recruitment only, and should not be taken to suggest that other recruitment methods are equally comparable.

Research Question 3 asked whether other sample or study methodology related variables predict variation in rape prevalence rates, and both the main effects and full meta-regression models revealed a number of significant predictors. Consistent with previous literature, studies using behaviorally-specific screening questions found significantly higher prevalence rates than studies that used gate screening questions. This difference has been replicated in multiple studies (Fisher et al., 2000; Koss et al., 1987), but (to this author's knowledge) this is the first study to demonstrate that the difference emerges in the context of a meta-analysis as well as that of

primary studies. Krebs (2014) suggests that the superiority of behaviorally-specific questions compared to gate screening questions is nearing universal consensus in the rape measurement field. This meta-regression supports that conclusion, both in terms of the higher prevalence rate reported by studies using behaviorally-specific questions and the vast majority of researchers who chose to employ behaviorally-specific questions in their studies.

Mean participant age, too, was a significant predictor of prevalence rate variation. While adolescence and young adulthood have long been recognized in the literature as periods of particularly high risk (Breiding et al., 2014; Tjaden & Thoennes, 2000), previous literature has less clearly established the impact that a study's mean participant age may have on the reported prevalence rate. When, as was the case with this meta-analysis, prevalence is defined as the presence of any rape victimization in adolescence or adulthood, older participants may be expected to report victimization at higher rates than younger participants, as they would have had more time to accumulate such experiences. Conversely, it is possible that older participants might report such victimizations less frequently, since more years have elapsed since their period of highest risk. In the final model, each additional year increase in a study's mean participant age increased the odds that participants in that study would report rape victimization by 5%, indicating that studies with older participants tend to find higher rape prevalence rates. When attempting to compare prevalence rates between samples, therefore, researchers should be aware that even moderate differences in mean participant age could significantly impact reported prevalence rates. It should be pointed out, however, that the mean age of participants across all studies was relatively young ($M = 22.7$, $SD = 6.3$), and it is possible that the positive linear relationship seen in this analysis would not continue past a certain point.

As expected, perpetration tactics (whether the assessment included rape perpetrated through incapacitation or was limited to rape perpetrated through physical force) was also a significant predictor of prevalence rate variation, with studies that included incapacitation in their assessments finding significantly higher prevalence rates than those that restricted their assessments to force. An assessment of rape that includes two possible perpetration tactics (physical force *and* intoxication) should logically be expected to find a higher prevalence rate than an assessment that includes only one; few research studies, however, have quantified this difference. In a notable exception, Biere and Davis-Siegel (2014) compared the FBI's recently expanded definition of rape to a previously-used, more restrictive, definition and found that 40% of sexual assaults had been missed by the more restrictive definition. This meta-regression suggests the same key conclusion: more restrictive definitions underestimate rape prevalence. In the final model, studies that included incapacitation in their perpetration tactics found rape prevalence rates approximately 1.8 times higher than studies that restricted their scope to rapes perpetrated through physical force. These findings underscore the importance of including the full range of perpetration tactics in studies intended to assess rape prevalence.

Finally, in both the main effects and full models, sample source was a significant predictor of rape prevalence rate variation. When visually examining the data during analysis, the author noticed what appeared to be a relationship between sample source and recruitment method. Though this relationship was not anticipated at the outset of the study, it does seem logical, in hindsight, that different recruitment methods might be appropriate for college, community, and military samples. Analysis in SPSS confirmed a significant correlation (r = .416, $p < .001$). Based on the behavior of the variables in the multivariate model, it is likely that

recruitment method's trend toward significance in the bivariate model was due to its correlation with sample source.

While a previous study has compared victimization rates of college students and similarly aged non-students (Sinozich & Langton, 2014), the analysis of sample source in this study examined a broader question: do studies that measure rape prevalence among college students tend to find substantially different prevalence rates than studies carried out with community or military samples? In the main effects model, studies based on military samples tended to find prevalence rates over two times greater than studies carried out with college samples, but there was no significant difference between college and community samples. However, once a variable capturing the interaction between sample source (college vs. community sample) and perpetration tactics (force only vs. force plus incapacitation) was included in the model, both community and military samples differed significantly from college samples. With this interaction term included in the final model, prevalence rates were identified as approximately 80% higher among studies utilizing community or military samples compared to college student samples.

If not accounted for in research, this interaction has the potential to skew conclusions regarding the relative risk of college and community samples. Whether a study includes incapacitation in its rape measurement is likely to impact college and community samples differently, with college samples more heavily impacted by a measurement restricted to force than community samples. For example, researchers attempting to measure the relative risk of rape among college and non-college women may find comparable levels of risk if the measurement is restricted to rape perpetrated through force, but higher relative prevalence rates for college students if incapacitation is included in the measurement. Future research could

clarify this relationship by asking college and community samples to report separately on a) their experiences with physically forced rape, and b) their experiences with incapacitated rape. These figures could be compared to clarify the relative victimization rate between college and community samples when incapacitation is or is not included in the victimization measure.

Limitations

Unexpected problems were encountered in implementing the sampling frame and coding procedures that limit the generalizability of this meta-analysis. Most notably, missing data led to a number of relevant covariates not being included in the meta-regression. These covariates, including participant race, data collection method, and perpetrator gender, could have impacted not only the variance explained by the model but also the explanatory power of other predictors. Data collection method, in particular, is a variable that has been found to substantially impact prevalence findings (see Krebs, 2014, for a review), and its lack of inclusion is an important caveat to these findings. Unfortunately, many studies did not clearly communicate whether they had collected data through an online or written survey, and once those two categories were collapsed there was insufficient variability to include this variable in the analysis.

In addition to the unexpected challenge of missing data, intentional decisions made regarding the sampling frame of this meta-analysis place limits on the conclusions that can be drawn. First, though there was no evidence that the peer-review restriction inflated the summary effect size, limiting included studies to only those in the peer-reviewed literature did reduce the number of potential studies by half. While this reduction was necessary in order to limit the scope of the project, it must be acknowledged that additional studies could have impacted the findings, and the magnitude and direction of that potential impact is unknown. Secondly, while the decision to limit studies to those that measure rape victimization during adolescence and

adulthood was intentional, an unintended consequence of that sampling decision may have been increasing the number of college student samples relative to the number of community and military samples. While evaluating articles for inclusion, multiple studies were identified that collected rape victimization data through healthcare screenings. The majority of these studies, often carried out among community samples, assessed lifetime victimization rather than victimization during adolescence/adulthood and were therefore excluded. Measuring adolescent and adult victimization separately from childhood victimization may be methodologically prudent for research and some targeted interventions, but for many community interventions, the distinction may be cumbersome and unnecessary. The choice to restrict measurement to adolescent and adult victimization may have reduced the number of community samples relative to college student samples, and therefore limited this meta-analysis' ability to identify distinctions between those groups.

Future Directions

Though this study found that participant recruitment method does not impact a study's reported rape prevalence rate, this should not be taken to mean that participant recruitment is entirely unimportant. Recruitment practices vary greatly, for example, in regards to what information potential participants have access to when they decide whether or not to participate. Particularly within the sign-up method of recruitment, there is great variation in the transparency of recruitment language. Research has repeatedly shown that participating in rape research is a positive experience for the vast majority of participants, including rape victims (Becker-Blease & Freyd, 2006; Black, Kresnow, Simon, Arias, & Shelly, 2006; Campbell, Adams, Wasco, Ahrens, & Sefl, 2010), however many of those studies were completely transparent in their recruitment efforts. It is possible that the degree to which potential participants know they will be

asked about their rape histories may impact their experiences of participation. There are many unanswered questions regarding how the research community recruits participants, and this meta-analysis looked at only one small piece of that picture.

Additionally, there is much yet to be learned about the variables that this meta-regression identified as significant predictors of rape prevalence rate variation. This is particularly true in regard to the interaction between sample source (college vs. community samples) and perpetration tactics (force only vs. force plus incapacitation). Comparing the victimization rate between different samples is a common area of inquiry (e.g., Tromp, Koss, Figueredo, & Tharan, 1995; Sinozich & Lanton, 2014), but little research has evaluated if and how methodological decisions impact college and community samples differently (see Fisher & Cullen, 2000, for an exception). This meta-regression found that whether or not a study included incapacitation as a perpetration tactic impacted college and community samples to varying degrees, with more similar rates between the groups when the study assessed only rapes perpetrated through force. What other methodological variables might impact the reported prevalence rates of college and community samples differently? While racial makeup of a study's sample and data collection method were not included in this meta-regression due to a large amount of missing data, they may be worth examining in the future for possible interactions with a study's sample source. It is possible, for example, that racial diversity of a sample may predict prevalence rate variation among community but not college samples. Similarly, self-administered surveys may be the most effective way of encouraging disclosure among college students but not community respondents. That the inclusion of incapacitation as a perpetration tactic predicted prevalence differently in college versus community samples should encourage this line of questioning in future research.

Finally, the previously-discussed inconsistency with which many variables were reported will hopefully inform researchers' publication practices. Omission of relevant study variables such as the year data was collected, the racial diversity of a study's sample, or the method through which survey data was collected from published articles has implications not only for a reader's understanding of the original study but also for that article's potential inclusion in future meta-regressions. Authors cannot be expected to include every detail of their sample or methods in their publications, but they should think carefully about what variables might be relevant to researchers evaluating the literature as a whole. Meta-analysis and meta-regression have the potential to clarify a great deal in sexual assault research, but these methods are only as useful as the information provided in primary studies. Researchers should ideally view this meta-analysis as encouragement to report on not only the variables immediately relevant to their interests but also on study level variables that could facilitate future meta-analytic work.

APPENDICES

Appendix A

Applying Inclusion and Exclusion Codes for Rape Prevalence Meta-Analysis

1. Does the study ask a group of people whether they have experienced some form of sexual violation? Y / N

a. If yes, *move to question 2*.

b. If no, which statement is most accurate:

i. The study doesn't ask about victims/victimization, only about perpetrators/perpetration. *Mark as 0, PERP.*

ii. The researchers knew something about people's victimization history before asking them to participate in <u>any part</u> of the study (e.g., a study where they recruited people from rape crisis centers, emergency rooms, or through their contact with police). *Mark as 0, AVS.*

iii. The study isn't actually asking real people anything. It is getting data from existing records, such as police reports, medical records, or crime statistics. *Mark as 0, SDC.*

iv. The study doesn't ask people whether they themselves have been victimized, it asks only about their attitudes or beliefs (like who is responsible in a hypothetical scenario about rape), or their non-victimization experiences (e.g., how much they drink or whether they know a rape victim) *Mark as 0, ND-V/P.*

2. Is the sample either made up of people who are all at least 18 or who you can reasonably assume did not need parental consent to participate (e.g., college students)? Y/N

a. If yes, *move to question 3*.

b. If no (e.g., the survey is given to middle school or high school students) *Mark 0, NAS.*

3. Is the study either specific to adolescent/adult victimization or does it separate childhood victimization experiences from adolescent/adult victimization experiences?

a. If yes, move to question 4.

b. If no, which statement is most accurate:

i. The study only asks about victimization during childhood. *Mark as 0, CVE.*

ii. The study asks whether someone has ever experienced victimization. *Mark as 0, LVM.*

4. Does the study ask only women about their victimization experiences or does it separate prevalence data by gender? Y/N

a. If yes, move to question 5.

b. If no, which statement is most accurate:

i. The study asks only men about their victimization experiences or collected the data in such a way that male and female victimization cannot be separated. *Mark as 0, MVS.*

ii. The study asks both men and women about their victimization experiences and collected the data in such a way that prevalence rates could be separated by gender, but prevalence rates are not separated by gender in the published article. *Mark as 2 and write note detailing the missing information that would need to be gathered in order to use this article.*

5. Does the study ask about rape or about acts that meet the current FBI definition of rape (vaginal or anal penetration by any body part or object, or oral penetration by a sex organ) or does it separate the data such that prevalence of these acts can be differentiated from prevalence of other types of sexual victimization?

a. If yes, move *to question 6.*

b. If no, which statement is most accurate:

i. The study asks about sexual victimization in such a way that the prevalence of rape/acts meeting the current FBI definition of rape cannot be separated from other types of victimization (e.g., "have you had any unwanted sexual experiences"). *Mark 0, DI.*

ii. The study collected data in such a way that rape prevalence could be separated from other types of sexual victimization, but prevalence rates are not separated in the published article. *Mark as 2 and write note detailing the missing information that would need to be gathered in order to use this article.*

6. Is the study asking about all rape experiences (i.e., all times, perpetrators, places, etc.) that have been experienced since adolescence/adulthood? Y/N

a. If yes, *move to question 7.*

b. If no (e.g., asks only about assaults "on a date," "since entering the military," "while you were drinking," "In the last year," or "While you've been at college"), *Mark 0 TLSS.*

7. Did the study take place in the United States? Y/N

a. If yes, *move to question 8.*

b. If no, *Mark as 0, NUSA*

8. Did the study referenced in this article state its recruitment method and use one of the four recruitment methods included in the proposed meta-analysis (sign-up, captive audience, unsolicited mailing, or random digit dial)? Y/N

a. If yes, *mark as 1*.

b. If no, *mark as 0, IRI.*

Appendix B

Explanation of Proposed Covariates Unable to be Included in Meta-Regression

A number of variables were put forth in the thesis proposal that were unable to be included in the final meta-regression. Those variables, and their accompanying reasons that they were not included in the meta-regression, are presented below.

1. **Participant race:** Participant race was not reported consistently in the sample of included studies. Six studies were missing information on that variable entirely, meaning that including this variable would have led to those studies being dropped from all meta-regression analysis. There was a great deal of variation among studies that did report on this variable, with some including Latina/Hispanic as a race and others as a separate ethnicity category. Ultimately, data on this variable was too inconsistent and unreliable to include.
2. **Data collection method:** The vast majority of studies included in this meta-analysis used surveys for data collection, rather than telephone or in-person interviews. While the intent was to further differentiate between pencil/paper and internet surveys, this data was not reported reliably across articles, with many articles stating they administered a survey without clarifying the format used for administration.
3. **Data collection year:** The majority of studies included in the meta-analysis did not report the year(s) of data collection. Publication year was included in the meta-regression as a proxy variable.

Perpetrator gender: Included articles did not reliably report whether they restricted their assessment scope to male perpetrators or whether they included female perpetrators in their victimization measure, as well. Though some articles specified that they restricted their inquiry

to male perpetrators, many did not, and it would have been inaccurate to assume that those articles all included female perpetrators in their measurement.

Appendix C

Table of Covariate Values for Studies Included in Rape Prevalence Rate Meta-Analysis

Table 4.
Covariate Values for Studies Included in Rape Prevalence Rate Meta-Analysis

Study (with publication year)	Prevalence Rate	Recruitment Method	Sample Source	Screening Question	Perpetration Tactics	Mean Age	Vict. Age Cutoff
Barnett (1987)	10.3%	Sign-up	College	Behaviorally-Specific	Force	19.0	14
Koss (1987)	15.4%	Captive Audience	College	Behaviorally-Specific	Incapacitation	21.4	14
Jenkins (1987)	13.0%	Captive Audience	College	Behaviorally-Specific	Force	19.0	14
Koss (1991)	13.9%	Unsolicited Mailing	Community	Behaviorally-Specific	Incapacitation	36.5	14
Copenhaver (1991)	17.0%	Unsolicited Mailing	College	Behaviorally-Specific	Incapacitation	20.1	14
Reilly (1992)	8.6%	Captive Audience	College	Behaviorally-Specific	Force	19.9	14
Walch (1992) Sample 1	15.5%	Captive Audience	College	Behaviorally-Specific	Incapacitation	23.1	18
Walch (1992) Sample 2	7.5%	Captive Audience	Community	Behaviorally-Specific	Incapacitation	35	18

Table 4. (cont'd)

Pihlgren (1993)	18.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.6	14
Gidycz (1993)	13.8%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.7	14
Tromp 1995 Sample 1	30%	Unsolicited Mailing	Community	Behaviorally-Specific	Incapacitation	36.6	14
Tromp 1995 Sample 2	29%	Unsolicited Mailing	Community	Behaviorally-Specific	Incapacitation	40.5	14
Abbey (1996)	23.0%	Captive Audience	College	Behaviorally-Specific	Incapacitation	25.9	14
Layman (1996)	14.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.0	14
Botta (1997)	20.0%	Unsolicited Mailing	College	Behaviorally-Specific	Incapacitation	19.0	14
Breitenbecher (1998)	22.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.0	14
Stepakoff (1998)	8.7%	Sign-up	College	Behaviorally-Specific	Incapacitation	20	17
Breitenbecher & Scarce (1999)	27.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	20.6	14

Table 4. (cont'd)

Merill (1999)	34.7%	Captive Audience	Military	Behaviorally-Specific	Force	20.3	14
Moore (1999)	20.0%	Captive Audience	College	Behaviorally-Specific	Force	20.3	14
Breitenbecher (1999)	24.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.7	14
Humphrey & White (2000)	13.0%	Captive Audience	College	Behaviorally-Specific	Force	18.3	14
Kalof (2000)	22.1%	Unsolicited Mailing	College	Behaviorally-Specific	Incapacitation	21.0	18
Marx et al. (2000)	9.0%	Sign-up	College	Behaviorally-Specific	Force	19.5	14
Breitenbecher & Scarce (2001)	26.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	20.9	14
Messman-Moore & Long (2002)	25.7%	Sign-up	Community	Behaviorally-Specific	Incapacitation	37.4	17
Testa (2003)	17.2%	N/A	Community	Behaviorally-Specific	Incapacitation	23.8	14
Smith & Frieze, 2003 (Sample 1)	11.8%	Missing	College	Behaviorally-Specific	Incapacitation	19.4	14

Table 4. (cont'd)

Smith & Frieze 2003 (Sample 2)	23.6%	Missing	College	Behaviorally-Specific	Incapacitation	Missing	14
Vanzile-Tamsen et al. (2005)	18.2%	Unsolicited Mailing	Community	Behaviorally-Specific	Incapacitation	24	14
Brown et al. (2005)	12.2%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.2	17
Littleton & Breitkopf (2006)	20.4%	Sign-up	College	Behaviorally-Specific	Incapacitation	Missing	14
Yuan et al. (2006)	14.0%	N/A	Community	Behaviorally-Specific	Force	41	18
Schumm et al. (2006)	23.0%	Captive Audience	Community	Behaviorally-Specific	Force	21.7	16
Schultz et al. (2006)	48.9%	Unsolicited Mailing	Military	Behaviorally-Specific	Force	45.3	14
Stander et al. (2007)	25.6%	Captive Audience	Military	Behaviorally-Specific	Incapacitation	19.7	14
Stoner et al., (2007)	38.2%	Sign-up	Community	Behaviorally-Specific	Force	24.6	16
Turchik et al. (2007)	12.4%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.9	14

Table 4. (cont'd)

Benson et al., (2007)	13.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.3	14
Pumphrey-Gordon & Gross (2007)	18.%	Sign-up	College	Behaviorally-Specific	Incapacitation	22.4	14
Winslett & Gross (2008)	15.1%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.8	14
Campbell et al. (2008)	39.0%	Captive Audience	Military	Behaviorally-Specific	Force	40.6	18
Littleton et al. (2008)	16.2%	Captive Audience	Community	Behaviorally-Specific	Incapacitation	27.0	14
Breitenbecher (2008)	26.0%	Unsolicited Mailing	College	Behaviorally-Specific	Incapacitation	21.4	14
Gidycz et al. (2008)	9.3%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.8	14
Littleton, Axsom et al. (2009)	20.2%	Sign-up	College	Behaviorally-Specific	Incapacitation	21.7	14
Clements & Ogle (2009)	10.1%	Sign-up	College	Behaviorally-Specific	Force	19.0	14
Edwards et al. (2009)	7.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.7	14

Table 4. (cont'd)

Littleton, Tabernik et al., (2009)	21.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	22.6	14
Segal (2009)	22.0%	N/A	College	Behaviorally-Specific	Incapacitation	24.4	14
Lawyer et al., 2009)	13.1%	Sign-up	College	Behaviorally-Specific	Incapacitation	20.1	14
Franklin (2010)	11.4%	Sign-up	College	Behaviorally-Specific	Force	20.1	14
Messman-Moore et al. (2010)	17.8%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.8	14
Yeater et al. (2010)	17.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.5	14
Peterson & Muehlenhard (2011)	6.9%	Sign-up	College	Gate	Incapacitation	19.2	14
Walker & Messman-Moore (2011)	16.8%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.7	14
Lehavot & Simoni (2011)	40.0%	N/A	Community	Behaviorally-Specific	Incapacitation	33.8	14
Munro et al. (2012)	5.3%	Sign-up	Community	Gate	Force	25.6	16

Table 4. (cont'd)

Orchowski & Gidycz (2012)	8.3%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.5	14
Amacker & Littleton (2013)	19.8%	Sign-up	College	Behaviorally-Specific	Incapacitation	21.3	14
Messman-Moore et al., (2013)	19.2%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.8	14
Walsh, Messman-Moore et al. (2013)	22.9%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.7	14
Untied et al., (2013)	8.4%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.2	14
Cleere & Lynn (2013)	15.2	Sign-up	College	Behaviorally-Specific	Incapacitation	19.0	14
Nelson & Lepore (2013)	24.6%	Captive Audience	Community	Behaviorally-Specific	Force	23.4	16
Walsh, DiLillo et al. (2013)	22.4%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.7	18
Schry & White (2013)	18.8%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.4	14
Franklin (2013)	8.6%	Sign-up	College	Behaviorally-Specific	Force	20.7	14

Table 4. (cont'd)

Hequembourg et al. (2013)	43.4%	N/A	Community	Behaviorally-Specific	Incapacitation	24.5	14
Turchik & Hassija (2014)	27.5%	Sign-up	College	Behaviorally-Specific	Incapacitation	18.9	16
Jordan et al., (2014)	11.1%	Unsolicited Mailing	College	Behaviorally-Specific	Force	18.5	14
Parkhill et al. (2014)	46.5%	Sign-up	Community	Behaviorally-Specific	Incapacitation	25.3	14
Littleton et al., (2014)	18.9	Sign-up	College	Behaviorally-Specific	Incapacitation	19.3	14
Hollander (2014)	25.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	21.1	14
Carey et al. (2015)	6.0%	Unsolicited mailing	College	Behaviorally-Specific	Force	18.1	14
Osman (2016) Sample 1	15.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.1	14
Osman (2016) Sample 2	28.0%	Sign-up	College	Behaviorally-Specific	Incapacitation	19.2	14
Schry & White (2016)	19.4%	Sign-up	College	Behaviorally-Specific	Incapacitation	Missing	14

Appendix D

Forest Plot of Studies Included in Rape Prevalence Rate Meta-Analysis

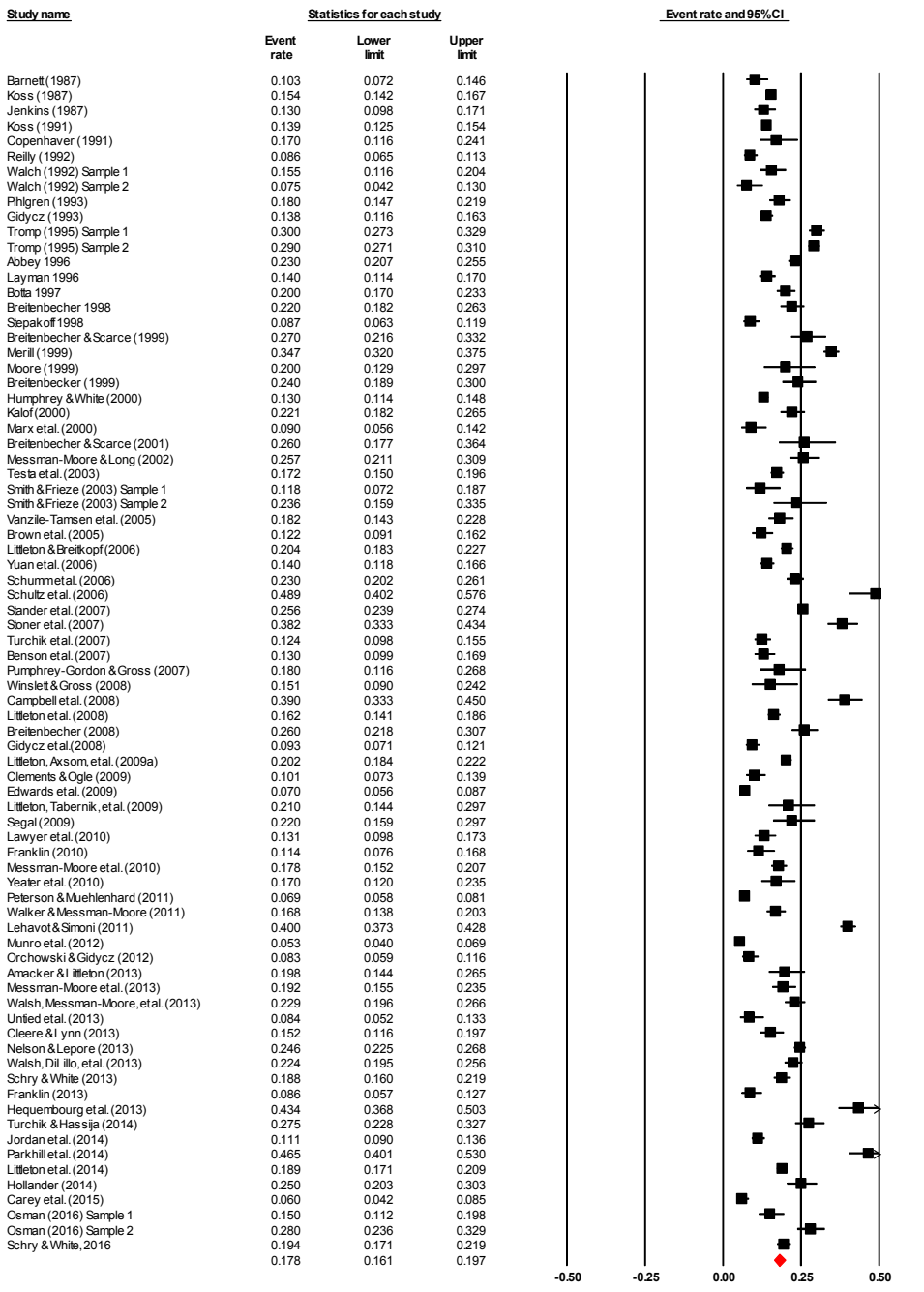

Figure 3. Forest Plot of Studies Included in Rape Prevalence Rate Meta-Analysis

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