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Crime and Justice Coordinating Group

Juvenile Curfew Effects on Criminal Behavior and Victimization

David B. Wilson, Charlotte Gill, Ajima Olaghere and Dave McClure

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Corresponding author	David B. Wilson, PhD Department of Criminology, Law and Society George Mason University 4400 University Drive, MS 4F4 Fairfax, VA 22030 USA E-mail: dwilsonb@gmu.edu Full list of author information is available at the end of the article

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The Campbell Collaboration
P.O. Box 4404 Nydalen
0403 Oslo, Norway
www.campbellcollaboration.org

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Synopsis/Plain Language Summary

The Campbell review in brief

The evidence suggests that juvenile curfews do not reduce crime or victimization.

What is this review about?

Curfews restrict youth below a certain – usually 17 or 18 – from public places during nighttime. For example, the Prince George's County, Maryland, curfew ordinance restricts youth younger than 17 from public places between 10 P.M. and 5 A.M. on weekdays and between midnight and 5 A.M. on weekends. Sanctions range from a fine that increases with each offense, community service, and restrictions on a youth's driver's license. Close to three quarters of US cities have curfews, which are also used in Iceland.

A juvenile curfew has common sense appeal: keep youth at home during the late night and early morning hours and you will prevent them from committing a crime or being a victim of a crime. In addition, the potential for fines or other sanctions deter youth from being out in a public place during curfew hours.

Juvenile curfews have received numerous legal challenges. The constitutional basis for infringing the rights of youth rests on the assumption that they reduce juvenile crime and victimization.

This review synthesizes the evidence on the effectiveness of juvenile curfews in reducing criminal behavior and victimization among youth.

What are the main findings of this review?

What studies are included?

Included studies test the effect of an official state or local policy intended to restrict or otherwise penalize a juvenile's presence outside the home during certain times of day. This must have been a general preventive measure directed at all youth within a certain age range and not a sanction imposed on a specific youth.

Twelve quantitative evaluations of the effects of curfews on youth criminal behavior or victimization are included in the review.

Do curfews reduce crime and victimization?

The pattern of evidence suggests that juvenile curfews are ineffective at reducing crime and victimization. The average effect on juvenile crime during curfew hours was slightly positive - that is a slight increase in crime - and close to zero for crime during all hours. Both effects were not significant. Similarly, juvenile victimization also appeared unaffected by the imposition of a curfew ordinance.

However, all the studies in the review suffer from some limitations that make it difficult to draw firm conclusions. Nonetheless, the lack of any credible evidence in their favour suggests that any effect is likely to be small at best and that curfews are unlikely to be a meaningful solution to juvenile crime and disorder.

Other studies have suggested curfews may be ineffective as juvenile crime is concentrated in hours before and after school, and that under-resourced police forces focus on more urgent demands than enforcing curfews.

What do the results mean?

Contrary to popular belief, the evidence suggests that juvenile curfews do not produce the expected benefits. The study designs used in this research make it difficult to draw clear conclusions, so more research is needed to replicate the findings. However, many of the biases likely to occur in existing studies would make it more, rather than less, likely that we would conclude curfews are effective. For example, most of these studies were conducted during a time when crime was dropping throughout the United States. Therefore, our findings suggest that either curfews don't have any effect on crime, or the effect is too small to be identified in the research available.

How up to date is this review?

The search for this review was updated in March 2014, and the review published in March 2016.

What is the Campbell Collaboration?

The Campbell Collaboration is an international, voluntary, non-profit research network that publishes systematic reviews. We summarise and evaluate the quality of evidence about programmes in social and behavioural sciences. Our aim is to help people make better choices and better policy decisions.

About this summary

This summary was prepared by Howard White (Campbell Collaboration) and is based on the Campbell Systematic Review 2016:0X 'Juvenile Curfew Effects on Criminal Behavior and Victimization: A Systematic Review' by David B. Wilson, Charlotte Gill, Ajima Olaghere, and Dave McClure. Anne Mellbye (R-BUP) designed the summary, which was edited and produced by Tanya Kristiansen (Campbell Collaboration).

Executive Summary/Abstract

BACKGROUND

A juvenile curfew has a common sense appeal: keeping youth at home during the late night and early morning hours will prevent them from committing a crime or becoming a victim of a crime. This appeal has led to the popularity of curfews, at least within the United States and Iceland. However, prior reviews have questioned the effectiveness of curfews.

OBJECTIVES

The aim of this review was to synthesize the evidence on the effectiveness of juvenile curfews in reducing criminal behavior and victimization among youth.

SEARCH METHODS

The systematic search was conducted between January 20, 2014 and March 5, 2014. The search strategy yielded 7,349 titles and abstracts. The initial screening identified 100 of these as potentially relevant and in need of a full text review for study of eligibility. Fifteen documents representing 12 unique studies were found to be eligible and then coded.

SELECTION CRITERIA

To be eligible, a study must have tested the effect of an official state or local policy intended to restrict or otherwise penalize a juvenile's presence outside the home during certain times of day. This must have been a general preventive measure directed at all youth within a certain age range and not a sanction imposed on a specific youth. All quantitative research designs were eligible. An eligible study must have assessed the effect of a curfew on either juvenile criminal behavior or juvenile victimization. The manuscript, published or unpublished, must have been written in English and reported on data collected after 1959.

DATA COLLECTION AND ANALYSIS

The typical evaluation design of an eligible study was a variant on an interrupted

time-series. To accommodate these designs, the effect size used in this synthesis was the percent change in the crime or victimization rate during the period of time with a curfew relative to a baseline period, adjusting for any overall linear time trend. The outcomes of interest included crime and victimization, which were categorized by time of day (curfew hours, non-curfew hours, or all hours) and offender or victim age (juvenile or adult). The effects during non-curfew hours and the effects for adults served as control outcomes; that is, outcomes that should be unaffected by a curfew.

RESULTS

The pattern of evidence suggests that juvenile curfews are ineffective at reducing crime and victimization. The mean effect size for juvenile crime during curfew hours was slightly positive (reflecting a slight increase in crime), whereas it was essentially zero for crime during all hours. Both effects were nonsignificant. Similarly, juvenile victimization also appeared unaffected by the imposition of a curfew ordinance.

AUTHORS' CONCLUSIONS

The evidence suggests that juvenile curfews are ineffective at reducing crime or victimization. This is not, however, a conclusive finding. The observational nature of the research designs creates potential sources of bias and, as such, the findings need additional replication. However, many of the most plausible biases should have increased the likelihood of finding an effect. In particular, most of the studies reviewed were conducted during a time period when crime was decreasing throughout the United States. Thus, it appears that juvenile curfews either have no effect on crime and victimization or the effect is too small to be reliably detected with the data available.

1 Background

On July 4, 2011, more than 80 youth were involved in a series of violent altercations in downtown Silver Spring, Maryland, USA. This community is in Montgomery County, Maryland, near the northern tip of Washington, DC. These youth were from several different gangs, according to police (Laris & Morse, 2011), and came to downtown Silver Spring from the adjoining jurisdictions of Washington, DC, and Prince George's County, Maryland, both of which have juvenile curfew ordinances. The altercations took place over a two-hour period and resulted in the stabbing of one participant. Calls for a juvenile curfew grew from this incident, along with another incident, a "flash mob" theft, that occurred elsewhere in the county later in the summer. This incident involved a large number of youth descending on a convenience store in the early hours of the morning and collectively shoplifting whatever they could carry (Jouvenal & Morse, 2011).

A *Washington Post* editorial argued for the proposed juvenile curfew but stated that, "contrary to the hyperbole that has marked Montgomery County's debate about setting a youth curfew, the proposal is neither a draconian infringement of teen rights nor a miraculous cure-all to juvenile crime. It is merely a common-sense approach that police believe would be a useful tool in protecting public safety" (Editorial, 2011). As the editorial implied, some individuals and groups strongly supported the curfew proposal whereas others strongly opposed it. Two of the authors of this systematic review attended a community event in Montgomery County where the proposed curfew bill was discussed and can attest to the conviction that some members of the public have regarding the common sense value of a curfew for reducing youth crime and the potential for victimization. Ultimately, however, the proposed curfew was not adopted.

The focus of this systematic review is to examine whether this common sense notion about the effectiveness of a juvenile curfew is supported by statistical evidence. As will be shown, the findings show no clear pattern of benefit for a juvenile curfew in terms of reducing crime or victimization.

1.1 OVERVIEW OF JUVENILE CURFEWS

Juvenile curfews encompass a variety of restricted activities and sanctions implemented with the intention of controlling delinquency and increasing public safety. Curfews are built upon the assumption that "restricting the hours when young people may be in public should limit their opportunities to commit crimes or become victims" (McDowall, 2000, p. 59). With this underlying logic, such policies can take a variety of forms, including variations in targeted age groups, hours of operation, exceptions to the policy, and sanctions for violations (e.g., Ford, 1994, p. 1679; Ruefle & Reynolds, 1995).

Typically, curfews are directed at all youth under the age of 18 and are enforced during the late evening through to the early morning hours, although it is also common for a curfew to be restricted only to those under 17. The hours of enforcement often differ between weekdays and weekends, or holidays. For example, the Prince George's County, Maryland, curfew ordinance restricts youth younger than 17 from public places between 10 P.M. and 5 A.M. on weekdays and between midnight and 5 A.M. on weekends. Exemptions are also common, such as for youth accompanied by a parent or guardian, youth returning home from a place of employment, and youth traveling to or from a religious event. Sanctions can range from a fine that increases with each offense, community service, and restrictions on a youth's driver's license. It would be atypical for a curfew offense to result in some form of detention, but such a sanction is often permissible under the law.

There are other forms of curfew ordinances that restrict certain activities rather than the general public movement of juveniles. An example is graduated driver licensing laws that prohibit youth aged 16 to 18 from driving at night or carrying more than a set number of passengers (Foss & Evenson, 1999; Hartling et al., 2004). Another variation is a curfew specific to an adjudicated juvenile and is part of her sentence or condition of probation.

For the most part, curfews that apply to all youth of a certain age are a United States phenomenon with a history dating back over one hundred years (see Adams, 2003 for a brief history). A survey conducted in 1994 showed that 77% of American cities with populations of 200,000 or greater had a juvenile curfew policy (Ruefle & Reynolds, 1995). Similarly, a 1995 survey showed that 73% of the 200 largest American cities reported having a curfew ordinance (Ruefle & Reynolds, 1996). A study by the U.S. Conference of Mayors (1997) showed that 70 percent of the mayors, or an appropriate representative, from 272 cities with a curfew believed that their policy was effective. Bannister et al. (2001) suggest that most jurisdictions that impose a curfew consider it to be effective at reducing juvenile delinquency. Curfew policies have also found support among the public, particularly residents of jurisdictions in which they are used. Nelson (as cited in Ruefle & Reynolds, 1996) found that 92 percent of 300 adult residents of Cincinnati, Ohio supported the city's curfew, and 72 percent reported that it made them feel safer. Fisher (as cited in

Ruefle & Reynolds, 1996) also found majority support for a proposed curfew in Mobile, Alabama. Good (2006) reports that 96 percent of surveyed residents in cities with curfew policies viewed the laws as "very or somewhat effective for combating juvenile crime in their communities," and 93 percent considered curfew enforcement a good use of police resources.

Iceland passed a Child Protection Act in 2002 that included a curfew that affects all children aged 16 or younger (Curfew, n.d.). In the United Kingdom, the Anti-social Behaviour Act of 2003 included a curfew element that allowed police officers to take youth under the age of 16 home if found unsupervised on the streets between 9 P.M. and 6 A.M. (Smithson & Flint, 2006). However, this element of the Act was ruled illegal by the High Court and was only in effect for a short period of time. Yet, curfew orders for individual juveniles adjudicated for a crime are used in Britain. In the 1990s, there was growing demand for juvenile curfews in Canada, but none has been implemented given that they would violate the rights of youths as defined by the Canadian Charter of Rights (Howe & Covell, 2001). We have been unable to identify any countries other than the United States and Iceland that have general juvenile curfews which apply to all youth of certain ages within a given jurisdiction (see also, Curfew, n.d.).

1.2 HOW CURFEWS MIGHT AFFECT CRIME

The logic supporting the effectiveness of juvenile curfews is simple: keep youth home during the late evening and early morning hours and you will prevent them from committing a crime and becoming a victim of a crime (Adams, 2003; Levesque, 2014; McDowall, 2000). Reduced opportunity to commit crimes should translate into fewer crimes. Furthermore, the potential for fines or other sanctions is presumed to deter youth from being out in a public place during curfew hours. It is also argued that curfews provide police with a useful tool for managing youth in public places during the curfew hours. Finally, curfews may make it easier for parents to enforce a rule for when a youth must be home in the evening (Ruefle & Reynolds, 1995).

The soundness of the above logic has been questioned. An argument against the effectiveness of curfews is that most crimes by juveniles, particularly those against persons, are committed in the hours before and after school (Gottfredson & Soule, 2005). Thus, any possible effect on juvenile crime is constrained by the small proportion of juvenile crime occurring during curfew hours. Additionally, it is rare for curfew ordinances to be associated with increased law enforcement resources, reducing the likelihood of effective enforcement. Studies have shown that over-stretched and under-resourced police departments may forego the enforcement of a curfew law in favor of focusing on more urgent demands (McDowall, 2000, p. 59; Bannister et al., 2001, p. 237; see also Reynolds et al., 2000; Watzman, 1994). Thus, having a curfew ordinance in place does not always translate into vigorous enforcement, further limiting any potential effect.

1.3 LEGAL CHALLENGES

Juvenile curfews have received numerous legal challenges. Bast and Reynolds (2003) present a detailed discussion of four legal cases brought against a curfew ordinance within the United States. Two of these curfew ordinances were upheld and two were struck down. A common basis for these challenges was that the curfew ordinance violated the civil rights of adolescents (most of whom are not adjudicated delinquents) by restricting their freedom of movement or other individual liberties (Ford, 1994, p. 1694; see also Bannister et al., 2001; Cole, 2003; Fried, 2001; Simpson & Simpson, 1993; Watzman, 1994; White, 1996). As discussed above, attempts to institute juvenile curfews in the United Kingdom and Canada failed on the basis that they would violate the rights of youth. The U.S. Supreme Court has yet to review the constitutionality of juvenile curfews. Watzman (1994) argues, however, that the more exceptions for "acceptable activities" (for example, legitimate employment) a given policy provides, the more likely it is to survive a challenge on constitutional grounds.

Fried (2001) argued that an important legal consideration in the debate over the constitutionality of juvenile curfews is their effectiveness. The legal justification for restricting the rights of juveniles rests on the state's interest in protecting juveniles from victimization and in reducing juvenile crime. If juvenile curfews are found to be ineffective in furthering these interests, then legal challenges may become more successful within the U.S. context. However, as argued by Bast and Reynolds (2003), the certainty of the evidence on effectiveness needed for a legal justification is likely to be low, only needing to establish a reasonable expectation of positive benefits.

1.4 PRIOR REVIEWS

Prior reviews have questioned the effectiveness of curfews for preventing crime and victimization. The most comprehensive review was by Adams (2003; see also, 2007). He identified ten relevant studies that measured criminal offending or victimization using a research design that allowed for at least a pre-post comparison of outcomes. Most of these studies reported no change in crime rates as a result of the curfew ordinance. According to Adams, where changes were observed, they were just as likely to reflect an increase in crime as a decrease. He also noted that curfew enforcement rarely resulted in the detection of serious offenses. McDowall (2000) drew similar conclusions from an examination of six evaluations of curfew policies, stating that the body of research showed little to no preventive effect, with the most promising studies indicating no more than a modest crime reduction attributable to the curfew. McDowall noted that all of the research designs in these studies had substantial weaknesses.

1.5 CONTRIBUTION OF THIS REVIEW

Juvenile curfews affect a large percentage of juveniles within the United States and their use is based on the assumption that they reduce juvenile crime and victimization. The constitutional basis for infringing on the rights of youth also rests on this assumption. Prior reviews, such as that of Adams (2003), have questioned the effectiveness of juvenile curfews. This review will update prior reviews with newer studies and apply meta-analytic techniques to the evidence, something not done in prior reviews.

2 Objectives

The aim of this review was to synthesize the evidence on the effectiveness of juvenile curfews in reducing juvenile criminal behavior and juvenile victimization. The type of curfew of interest was a general, civilian curfew that affects all youth of a specified age within a given jurisdiction.

3 Methods

3.1 CHANGES TO METHODS FROM THE PROTOCOL

A few changes from the methods detailed in the protocol were necessary given the complexities of this review. However, these changes were consistent with the objectives of the review.

The eligibility criteria were modified in two ways. First, the text was edited to improve clarity and readability. Second, we relaxed the research design criteria to include simple pre-post studies, that is, studies with only a single crime rate estimate for both the pre- and post-curfew periods. Because these studies are at high risk of bias, they were kept separate in analyses and forest plots, and were not used in our overall assessment of the effects of juvenile curfews. The purpose of including these studies was to fully document all identifiable quantitative estimates of the effects of curfews on juvenile crime and victimization.

The protocol stated that effect sizes were to be coded as percent change in the crime rate, ideally adjusted for any time trend. The details of how these computations were to be carried out were not specified. The methods that were developed during the course of this review remain consistent with the goals of the protocol and are detailed in Section 3.4.2.

One item was added to the risk of bias assessment that addressed maturation bias, or change over time. Natural change over time confounded with the adoption of a curfew ordinance is an important potential source of bias. Studies with too few baseline time points given the type of data were judged as at high risk for this type of bias.

Finally, we dropped an item from the risk of bias assessment: the items addressing statistical concerns related to the appropriateness of the statistical analysis given the data. Because our synthesis of findings relied almost exclusively on descriptive data and not on inferential models provided by the authors of the included studies, this item was not informative in assessing the credibility of the effect sizes that were the basis of our review. Furthermore, we were unable to develop clear guidelines for assessing the appropriateness of the statistical models used across the eligible studies.

3.2 CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

3.2.1 Type of curfew ordinance evaluated

Eligible studies must have tested the effect of an official state or local policy intended to restrict or otherwise penalize a juvenile's presence outside the home during certain times of day. The curfew policy must have been a general preventive measure directed at all youth within a certain age range. Thus, we excluded studies that examined curfews imposed as part of a specific sentence or probation condition for an individual youth. The curfew ordinance must have been passed as a measure to improve public safety through the specific targeting of juvenile crime and delinquency; military or riot curfews directed at all citizens were excluded. Also excluded were ordinances that solely placed restrictions on youth driving. Curfew policies that included exemptions for certain types of legitimate activity, such as employment, were eligible, as most curfews have such exemptions.

3.2.2 Types of research designs

All quantitative evaluation designs were eligible. We expected that most studies of juvenile curfews would have been conducted at the macro- or aggregate-level (i.e., comparing crime and delinquency rates in jurisdictions where policies are imposed to rates in the same jurisdiction prior to the policy, or to comparable jurisdictions that do not impose a curfew). Such designs are often called ecological studies (Hingson, Howland, Koepsell, & Cummings, 2001). This expectation stemmed from the nature of the intervention. Curfews are imposed categorically on all juveniles living within a specific geographic area. As such, randomized controlled trials at the individual-level are not possible. Although randomized controlled trials at the jurisdiction (or geographic) level were eligible, we anticipated that these were highly unlikely to exist given the legal nature of curfews. Jurisdictions legally cannot agree to a study where a random process determined whether they would pass or not pass a juvenile curfew ordinance.

The strongest study design we expected to find was an interrupted time-series, including designs with multiple series across different cities (i.e., panel studies). For our purpose, an interrupted time series involves multiple measurements of the outcome both before and after the curfew ordinance went into effect. Interrupted time-series designs are often considered one of the stronger quasi-experimental designs (e.g., Shadish, Cook, and Campbell, 2002). The primary threat to internal validity for this design is a historical artifact - that is, some other change coinciding with the imposition of the curfew that might account for the change in crime rates. A variant on this is a study with multiple interrupted time-series with one or more series serving as a comparison, such as a community that did not implement a curfew. These have clear advantages over a simple single time-series in that broad-scale historical effects can be statistically controlled for given the staggered timing of the start of the curfew laws. These designs also account for regression to the mean (see Kline, 2012).

Micro-level studies at the individual level were eligible. For example, a study might compare youth living in various jurisdictions, some with and some without a curfew. Similarly, non-equivalent comparison group designs at the macro-level that contrast crime rates for communities with and without curfews were eligible.

A simple pre-post analysis at the macro-level is similar to an interrupted time-series, but lacks the necessary multiple pre- and post-measurement time points (i.e., they don't have a series of observations for each time period). These studies are at high risk of bias given that there is no assessment of the underlying trend in the crime rate (i.e., whether crime was already increasing or decreasing before the start of a curfew). These designs were eligible, but were kept separate in all analyses and figures and did not weigh into our overall assessment of the effectiveness of juvenile curfews. We included them to fully document the statistical evidence on this topic. For a study to be included in this synthesis, it must have been possible to compute an index of the effect of the curfew on crime or victimization.

3.2.3 Types of participants

Juvenile curfews do not have participants in the typical sense. To be eligible, the curfew ordinance must have applied to youth within a specified age range within a given jurisdiction.

3.2.4 Types of outcome measures

Eligible studies had to have measured and reported data on at least one of the two primary outcomes of interest: juvenile crime or juvenile victimization (where a youth is the victim of a crime). Outcome data may have been based on official records (arrests, charges, convictions, etc.) or self-reported measures. These outcomes may have reflected crime only during the curfew hours or crime at any time of day. For example, a study may have reported the total juvenile arrest rate by month with this rate reflecting both curfew and non-curfew hours combined. The logic of accepting the latter as a valid primary outcome is that curfews are intended to reduce juvenile offending overall, and not just displace crimes from the curfew to non-curfew hours. Similarly, victimization data might reflect victimization during curfew hours or victimization at any time of day.

The secondary outcomes examined in this review are better conceptualized as control outcomes. That is, outcomes that should be unaffected by a curfew ordinance. These include adult crime rates and juvenile crime rates during non-curfew hours. No other outcome measures were coded as part of this review.

3.2.5 Types of settings

Eligible studies could have been conducted in any country and published in any form. Given the linguistic limitations of the authors, only English language publications were eligible. We worked with our international contacts within the Campbell Collaboration to learn which countries were likely to have used and evaluated juvenile curfew ordinances so that we could target our searches appropriately. As discussed in the Background section of this review (Section 1), only the United States and Iceland have or currently make use of the type of curfews eligible for this review. We were unable to identify any Icelandic studies. Thus, the English language restriction is unlikely to have resulted in the exclusion of any study (i.e., we are not aware of any other eligible study reported in a language other than English).

We restricted this review to studies using data collected after 1959. The generalizability of any study conducted prior to 1960 to the present is questionable given the markedly different current social and legal context.

3.3 SEARCH METHODS FOR IDENTIFICATION OF STUDIES

The systematic search was conducted between January 20, 2014 and March 5, 2014. Two categories of keywords were developed for this search. The first category lists key terms and synonyms related to juvenile curfew policies. The second category addresses measured study outcomes, including terms such as *crime*, *delinquency*, *arrest*, etc. The intention of separating the terms in this manner was to include all the potentially relevant results while simultaneously excluding the large bodies of literature on parenting and adolescent development from non-criminological disciplines. These two sets of keywords were combined with the Boolean operator, "AND."

1. Intervention of interest

CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*)

2. Outcomes of interest

CRIM* or DELINQUEN* or ARREST* or DETAIN* or DETENTION or "CALL* FOR SERVICE*" or OFFEND* or ADJUDICAT* or STATUS or VICTIM* or SAFE* or FEAR* or DRUG* or ALCOHOL* or LOITER* or STEAL* or STOLE* or THEFT or JOYRIDE or JOY-RIDE or "JOY RIDE" or VANDAL* or GANG or VIOLEN* or ASSAULT or FIGHT*

The specific search string used was adjusted for each database. For example, searches were initially run using only the "intervention of interest" keywords. If the search yield returned was over 1,000, then the search was restricted using the "outcomes of interest" keywords. Furthermore, databases differ in terms of the sophistication of the searches that are possible and we took advantage of these differences.

The following electronic databases were searched: AIC – Australian Institute of Criminology; ASSIA – Applied Social Science Index and Abstracts; CINCH (the Australian Criminology Database) via Informit; Criminal Justice Abstracts; EconLit; First Search – Dissertation Abstracts; Google Scholar; HeinOnline; Jill Dando Institute of Crime Science (JDI) via OVID; NCJRS (National Criminal Justice Reference Service); Policy Archive; PolicyFile; Criminal Justice Periodicals (now ProQuest Criminal Justice); Dissertations & Theses: Full Text; Evidence-Based Resources from the Joanna Briggs Institute; PubMed; PsycINFO; Public Affairs Information Service; RAND Documents; Social Sciences Citation Index; Social Services Abstracts; Sociological Abstracts; Social Science Research Network (SSRN); Worldwide Political Science Abstracts.

The following organizational websites were searched for potential grey literature studies: Association of Chief Police Officers (ACPO); Association of Chief Police Officers of Scotland (ACPOS); Association of Police Authorities (APA); Australian Research Council Centre of Excellence in Policing and Security (CEPS); Canadian Police Research Centre; Her Majesty's Inspectorate of Constabulary (HMIC); Home Office (UK); Medline/Embase; Ministry of Justice (UK); National Council for Crime Prevention (Sweden); National Institute of Justice; Office of Juvenile Justice and Delinquency Prevention; Scottish Institute for Policing Research SIPR; U.S. state juvenile justice agencies and court services.

Section 13 provides detailed notes regarding the search of each of these databases and websites, including the keywords used and yield.

3.4 DATA COLLECTION AND ANALYSIS

3.4.1 Data extraction and management

In addition to extracting effect size data as described in the next section, we coded descriptive information about the location of the research, the nature of the curfew, and the methodological characteristics of the study (see 'Coding Manual' in Section 14). All descriptive study information was double-coded and the first author resolved any differences. The first author coded effect sizes and all of the data and computations relevant to each effect size are presented in Section 9. Double-coding of effect sizes was not feasible given the complexity of these computations. However, a second coder verified the accuracy of the data used in these computations. At the time of coding, the second coder was a doctoral candidate with over three years of experience in working with systematic reviews and data extraction.

3.4.2 Measure of Treatment Effect

The most common research design was a macro-level examination of crime or victimization counts over time. Typical effect sizes, such as the standardized mean difference and the odds ratio, cannot be computed from the data generated by these designs. However, it is possible to use percent change in the crime or victimization count as the effect size.

A simple comparison of the relative pre-and post-crime count (or rate) associated with a curfew may be confounded with an underlying upward or downward trend in crime. For example, most of the studies included in this review were conducted in the United States during the late 1990s or early 2000s. This time period coincides with what is often referred to as the “great crime drop,” during which almost all categories of crime decreased across the country. Because of this, it was desirable to have a measure of the effect of a curfew that accounted for this underlying trend.

Five basic design types were used across the eligible studies: (1) an interrupted time-series with 26 or more time points (months or years), some before and some after the curfew ordinance went into effect; (2) a short interrupted time-series with 4 to 6 time points (e.g., years), with only 2 or 3 time points before and after the start of the curfew ordinance; (3) a simple pre-post comparison of counts (or rates) for one year before and one year after the adoption of a curfew; (4) regression analysis of crime rates for multiple cities or counties across multiple years; and (5) an individual level logistic regression analysis of juveniles geo-coded into locations with and without a curfew. Each of these designs requires a different method of effect estimation. The first step was to estimate the difference in the logged rate or count, ideally adjusted for any time trend. These logged rate differences were then converted into a percent difference, as explained below.

For studies that used an interrupted time-series design, percent change was estimated using a negative binomial model on the crime or victimization counts over time. Although count data such as these can be estimated with a Poisson model, crime data are typically over-dispersed and, as such, more appropriately handled with a negative binomial model. Both Poisson and negative binomial regression models produce comparable coefficients, but the latter produces larger standard errors unless the data are not over-dispersed. Data were extracted from tables (if available) or figures, and inputted into the statistical software program ‘R’. PlotDigitizer (see http://plotdigitizer.sourceforge.net) was used to extract data from figures. Data that reflected rates and not counts were converted into approximate counts using a constant population estimate before running the negative binomial model. This conversion does not affect the estimated effect, but does produce more accurate standard errors. The model estimated included a term for time (month or year) and a dummy code for whether the curfew policy was in effect (1=yes, 0=no). The coefficient for the curfew dummy reflects the difference in the log number of crimes for the post versus pre periods adjusting for any linear time trend.

The negative binomial model could not be used for short time-series (e.g., 4 to 6 time points) as there were insufficient data to estimate the over-dispersion parameter. However, these studies have sufficient data to estimate the curfew effect using a Poisson model. Given that the variance of the Poisson distribution equals the mean, a Poisson-based model can estimate the standard error of the coefficient even with only four data points, two prior to the curfew and two after the curfew. The standard errors from a Poisson model on these data are likely to be negatively biased (i.e., too small) given that these standard errors do not take into account any over-dispersion. Furthermore, the limited number of time points affects the precision of the estimate of the linear time trend. These potential biases with the estimates from these studies were considered when interpreting the results. As with the negative binomial, the coefficient for the curfew dummy variable reflected the difference in the log number of crimes for the post- versus pre-curfew period, adjusting for any linear time trend.

Studies that provided data on the rate or count of crimes or victimization for some period of time (usually one year) before and after the adoption of a juvenile curfew provided the weakest evidence on the effectiveness of the curfew. With such data it is not possible to estimate the underlying linear trend in the crime rate. Percent change, however, was computed as the difference in the two counts divided by the baseline value. We made use of the Poisson distribution for estimating the standard error. This made the assumption that these counts were Poisson distributed, as with the short time-series studies. The method used was adapted from Ng and Tang (2005). Assuming that the two time periods are equal (which they were in all cases), the standard error for the difference in the logged counts was computed as

$$se = \sqrt{\frac{1}{x_1} + \frac{1}{x_2}}$$

where x_1 and x_2 are the crime counts during for pre and post periods, respectively.

The fourth design type used OLS regression to model the logged arrest rates for multiple cities over multiple years. Some or all of the selected cities experienced a change in curfew policy during the years examined. Because the dependent variable was logged, the regression coefficient for the curfew indicator variable is directly interpretable as the difference in the logged crime rates, post- versus pre-curfew, adjusted for the linear effect of year and any other covariates in the model. As such, the coefficient associated with the curfew was coded as the effect size. The standard error reported in the study for this coefficient was also used as the standard error used to compute the inverse variance weight. Note also that the percent change in rates is directly comparable to the percent change in counts, given that the former is simply the latter divided by a constant (the population).

Finally, a single study reported on a logistic regression model based on individual level data. This study produced an odds ratio as the estimate of the effect of a juvenile residing in a jurisdiction with a curfew, relative to a juvenile residing in a jurisdiction without a curfew. Converting an odds ratio into a percent change relative to a baserate requires knowing the baserate, which was not provided in this study. However, applying the same conversion that was used for the logged differences to the logged odds ratio produces a percent change in the odds relative to the baseline odds. This was done in this case to improve comparability across forest plots. The effect sizes from this study were not, however, combined with effect sizes from other studies.

Although different methods were used to compute the effect size across these different design types, the effects are comparable in that they all reflect percent change in crime counts or rates, with the exception of the logistic regression on individual data. The latter was treated separately in all analyses. Although the metric is the same across these different estimation methods, that is, percent change, the effect estimate do vary in their ability to isolate the curfew effect from other potential sources of change. As such, estimates that adjust for a linear time trend were handled separately from those that do not. The estimates based on multicity data over numerous years include covariates, such as population size. These covariates do not affect the metric of the effect size (it remains an index of percent change associated with the introduction of the curfew policy) but do remove potential sources of bias.

Meta-analyses were performed on the logged difference effect sizes generated by the above methods. However, the goal was to represent the effects as percent change. The standard equation for converting a logged difference in a count or rate into a percentage change is

$$\%\, change = 100 * (e^{logged\ difference} - 1)\ .$$

Because we coded the curfew dummy variable as 1 (indicating that the curfew was in effect) and 0 (indicating that it was not), using the above equation computes the percentage change relative to the post curfew rate and not the pre-curfew rate. To correct for this, the difference in the logged counts was flipped by negation. Thus, the conversion used was

$$\%\, change = 100 * (e^{-\,logged\ difference} - 1)\ .$$

To demonstrate, assume a baseline crime count during the non-curfew period as 100 and a crime rate during the curfew period as 80. This reflects a 20 percent drop in crime: (100-80)/100 = .20 or 20%. The difference in the logged values as is ln(100) - ln(80) = .22. Using the first equation above produces a result of 100*(exp(.22)-1) = 25%. This is the percentage change relative to the curfew rate, or (80-100)/80 = .25 or 25%. However, using the second equation produces the desired value: 100*(exp(-.22)-1) = -20%. (Technical note: the difference between two logged values is a percentage change around the intercept or average of the logged values. In the example used here, the average of the logged values of 100 and

80 is 4.49, the exponent of which is 89.44. Thus, the drop in the crime rate from 100 to 80 reflects a 22% change relative to a crime rate of 89.44%.)

3.4.3 Moderator analysis

We had proposed conducting moderator analyses that examined the pattern of evidence for different types of curfew policies, if possible. However, the number of studies with meaningfully different policies was too small to allow for any such analyses.

3.4.4 Assessment of risk of bias

The assessment of risk of bias in the study designs eligible for this study was challenging (see Hingson et al., 2001; Hartling et al., 2004). The typical quality assessment tools used in Cochrane and Campbell systematic reviews are designed for non-ecological studies that are implemented at the individual level or other units that can be assigned randomly to conditions (e.g., small geographic regions such as crime hot spots). The potential sources of bias that we were most concerned about across these studies and tried to assess were:

1. Historical artifacts: anything that might be confounded with the intervention, such as other community or police initiatives directed at reducing crime.

2. Measurement confounds: any changes in the way data were collected over time that might bias the findings, such as a changes in police recording practices.

3. Selection bias: non-comparability of comparison communities at baseline, if relevant.

4. Regression to the mean: selection of curfew locations or imposition of curfew ordinance based on a short-term spike in crime rates (e.g., a recent ‘crime wave’).

5. Outcome reporting bias: selected outcomes were reported in the study, meaning the study explicitly mentions having measured an outcome, such as juvenile crime rate during curfew hours, but does not provide results for the outcome that would allow for the computation of an effect size.

6. Maturation bias: data inadequate to properly adjust for any underlying time trend (i.e., time period too short for the design used). A study was judged at risk for maturation bias if it used monthly data with fewer than 24 months both pre-curfew and post-curfew, or if it used yearly data with fewer than five pre-curfew years.

3.4.5 Unit of analysis issues

Multiple reports or manuscripts based on the same study or data were treated as a single entity. We selected the most complete reference as the primary document for coding. Other documents were used only if they provided unique information of relevance to this review. Section 7.1 lists all references used during the coding process. No meta-analytic synthesis included two effects from the same study sample.

3.4.6 Assessment of reporting biases

The number of effect sizes for each outcome of interest was too low for formal assessment of publication selection bias such as through the use of the Duval and Tweedie (2000) trim and fill method. However, we did search for and found unpublished studies.

3.4.7 Data synthesis

Random-effects inverse-variance weighted meta-analysis was used to synthesize effect sizes across studies. Analyses were performed on the difference in the logged counts or rates. The method-of-moments estimator of the random effects variance component, τ^2, was used. Final results were converted into percent change, as discussed in Section 3.4.2. These analyses were performed using the *metafor* package in R, Version 1.9-5 (Viechtbauer, 2010). All R code used in the analyses and for producing the forest plots is presented in Section 12.

4 Results

4.1 DESCRIPTION OF STUDIES

4.1.1 Results of the systematic search

The search strategy yielded 7,349 references. Removal of duplicates reduced this to 6,499. A pre-eligibility screening of titles and abstracts identified 100 references potentially relevant to this review, including review articles or other pieces with relevant background information, such as legal discussions. Two coders examined these 100 documents in detail for eligibility. Fifteen documents representing 12 unique studies satisfied our eligibility criteria. As is often the case, several studies that were initially coded as eligible were later recoded as ineligible upon closer inspection. These studies and others that were near misses on eligibility are listed in Section 7.2.

Of the 85 references coded an ineligible, 42 were excluded as simply not relevant. That is, the publication did not examine any aspect of a juvenile curfew ordinance. Thirty studies were categorized as background articles, including review articles, editorials, or descriptions of curfew policies. Ten studies examined some aspect of a curfew other than its impact on juvenile crime or victimization, such as the differences in the characteristics of curfew violators versus non-violators or parental attitudes towards curfews. Finally, two were excluded because of insufficient information to compute an effect size and one had data collected prior to 1959.

4.1.2 Description of included studies

Tables 1 and 2 provide descriptive information on the included studies. As shown in Table 1, most of these studies were published in journal articles (9 of 12). Two were published as technical reports and one was a Master's thesis. The publication dates ranged from 1999 through to 2012; 10 of the 12 studies being published between 1999 and 2003, inclusively. All of these studies were conducted in the United States. These studies were based on data collected between 1980 and 2004, inclusively, with only two studies (Kline 2012 and McDowall et al. 2000) using data prior to 1990.

Most of the studies examined a nocturnal curfew, as shown in Table 2, although two studies examined a daytime curfew during school hours. The age range affected by the curfew was typically 17 and under although a few only affected youth 16 and under or 15 and under. It is interesting to note that some curfew ordinances exclude children under the ages of 11 or 12. Presumably a curfew is unneeded for such young children given that police would intervene if they found an unaccompanied child less than 11 years of age out in public regardless of the existence of a curfew ordinance (at least within the United States).

Seven of the twelve studies used some form of interrupted time series design (see Table 2). These designs involved comparing the crime rate for some number of months or years prior to the curfew implementation to some number of months or years after the curfew implementation. These have been differentiated in Table 2 as either "ITS" or "Short ITS". The latter are interrupted time-series designs with fewer than 10 total observational units (i.e., months or years). This distinction affected how effect sizes were estimated. The latter are also less able to effectively control for any time trend unrelated to the curfew.

It is worth noting that Cole (2003) used an ABAB interrupted time series design. This study used data from Washington, DC, where the curfew law was ruled unconstitutional after having been in effect for a period of 13 months. However, the ruling was later reversed and the curfew was reinstated a little over two years later. Taking advantage of this 'turning on-and-off' of the curfew, Cole compared the juvenile arrest rate across four time periods that alternated from a non-curfew period to a curfew period.

Two studies (Moscovitz et al., 2000; Rodabough & Young, 2002) collected short time-series type data, but collapsed the data such that there was only a single pre-curfew and post-curfew rate. Moscovitz and colleagues compared emergency medical services transportation of juveniles who were victims of an assault during a 3-month period in the year before a curfew went into effect with the same period after a curfew went into effect. Rodabough and Young compared juvenile arrest and victimization data for two one-year time periods: one year before the curfew was implemented and one year after the curfew was implemented. In Table 2, these two studies are categorized as pre-post designs.

Finally, three studies (Gius, 2011; Kline, 2012; McDowall et al., 2000) used a regression-based design. Gius (2011) used 1997 data from the National Longitudinal Survey of Youth (NLSY) that included the state and county of residence for each youth. This allowed for an examination of the effect of living in an area with a curfew based on self-reported criminal behavior of youth and, as such, reflects a cross-sectional analysis.

The Kline (2012) and McDowall et al. (2000) papers made use of city level data over several years (panel data). Essentially, these are multiple interrupted time-series analyzed in a single regression model. Kline (2012) analyzed a sample of 54 cities (with a 1990 population greater than 180,000 that began enforcing a curfew law during the years from 1980 through 2004. The regression models used logged arrest rates from the FBI's Uniform Crime Report (UCR). The regression model used an ‘event-study’ design to estimate the impact of curfew enactment. Kline (2012) claims that this model also accounts for any confounders related to a city enacting a curfew in response to an increase in arrests and any general time trends.

The McDowall et al. (2000) study used a similar design to Kline (2012). McDowall and colleagues included 11 years of FBI UCR arrest data for youths 17 and younger, and 19 years of homicide victimization data. Two sets of analyses were performed, one on 52 counties and the other on 12 city-counties. The former included counties where a change in the curfew law may have affected only a portion of the county. The latter included only the subset of cities or counties for which any change in a curfew law affected the entire city or county. We used only the latter as it is a more direct test of the effect of implementing a curfew, although the results for both sets of analyses lead to the same conclusion. As with the Kline (2012) study, McDowall et al. (2000) used the logged arrest rate as the dependent variable.

4.2 RISK OF BIAS IN INCLUDED STUDIES

All of the studies included in this synthesis are observational and therefore subject to risk of bias in terms of making a causal inference about the effects of a curfew. The risk of bias ratings are shown in Table 3. Arguably the four methodologically strongest studies were Cole (2003), Kline (2012), McDowall et al. (2000), and Roman and Moore (2003; also Gouvis, 2000). All used some variant of an interrupted time-series design of sufficient length to rule out many threats to their internal validity (see Shadish et al., 2002), such as a maturational effect. However, Cole (2003) was coded as at risk of regression to the mean, along with Fivella (200), Males and Macallair (1999), and Sutphen and Ford (2001) given a mention in their articles that the passage of the curfew ordinance was related to concern over high crime rates. This concern, however, may also be an issue for other studies, but was not mentioned by the authors.

The most serious issue across these studies was the possibility of a maturational bias, with seven of the twelve studies judged as at risk. This assessment was based on the inadequate number of data points over time. All three of the studies categorized as short interrupted time-series designs and both of the pre-post designs had too few baseline observations to adequately assess whether any decrease in crime associated with the start of the curfew was part of an existing change over time. In addition, two of the longer interrupted time-series designs were also judged as at risk of a maturation bias. These two studies (Fivella, 2000; Mazerolle et al., 1999) used monthly data and had only 26 and 23 months of data respectively, which was only one year before and one year after the start of the curfew. Only four studies were judged to have a time-series of sufficient length to adequately control for the potential bias: Cole (2003), Kline (2012), McDowall et al. (2000), and Roman et al. (2003). Cole (2003) and Roman et al. (2003) used monthly data with multiple years of baseline data, whereas Kline (2012) and McDowall et al. (2000) used yearly data across multiple cities.

None of the studies was assessed as being at risk of measurement confounding. Many made use of FBI UCR data that maintains consistency in how they are gathered over time. Furthermore, no studies made mention of concerns regarding the measure of crime or victimization.

A potential threat to the validity of these studies is that another intervention, incident, or policy change is confounded with the start of the curfew ordinance. This is difficult to assess and requires knowledge of local conditions. Three studies (Fivella, 2000; Kline, 2012; Males, 1999) make mention of a possible historical artifact, such as the start of midnight basketball programs. However, other studies may have similar problems, potentially without the awareness of the researchers.

For selection bias to be an issue there must be a comparison condition. Only the three regression-based studies are at risk for this threat (Gius, 2011; Kline, 2012; McDowall et al., 2000). Given the observational nature of all three of these studies, it is possible that the regression models omitted an important variable. However, the Kline (2012) and McDowall et al. (2000) studies included the baseline crime rates in the models, reducing the potential magnitude of any selection bias. For the Gius (2011) study, several characteristics of the juveniles were included in the model to adjust for risk of delinquency. Furthermore, in this study the juveniles do not self-select into curfew and non-curfew areas.

In our assessment, the three regression-based studies (Gius, 2011; Kline, 2012; McDowall et al, 2000), along with the interrupted time-series studies by Cole (2003) and Roman and Moore (2003), provided the strongest assessment of the effects of curfews. However, all studies in this synthesis are subject to at least some risk of bias.

4.3 SYNTHESIS OF RESULTS

Twenty-four effect sizes were computed across the 12 studies. These effects represent both criminal behavior and victimization. effects were coded for both juveniles and adults during curfew hours, non-curfew hours, and all hours. Where possible, effects were adjusted for a time trend. All coded effects are shown in Table 4 and mean effects are shown in Table 5.

4.3.1 Effect on crime

A primary purpose of a curfew is to reduce the number of crimes committed by juveniles. The pattern of evidence across these studies suggests that curfews do not accomplish this goal. The strongest evidence comes from the effects adjusted for a time trend for the outcomes of juvenile crime during curfew hours and during all hours (see Figures 1 and 2). Two studies provided effect sizes for the effects of curfews on juvenile crime during curfew hours. Both studies found a small increase in crime with an overall mean percent change of 9.5% (95% confidence interval of -9.1% to 24.9%, p = 0.292). This mean effect is not statistically significant and has a fairly large confidence interval. Eight studies provided estimates of the percent change in juvenile crime during all hours (curfew and non-curfew). The overall mean was very close to zero (0.5% change in juvenile arrests with a 95% confidence interval of -9.8% to 8.1%, p = 0.915) with five studies reporting small increases in crime, one a near zero effect, and two a decrease in crime. One of these effects (Fivella, 2000) was substantial, representing a 46% decrease in juvenile arrests across both curfew and non-curfew hours. This is a clear outlier. Given that a relatively small proportion of juvenile crime occurs during curfew hours, this large effect is only possible if crime dropped substantially during both curfew and non-curfew hours. Removing this effect from the meta-analysis, however, produces roughly the same result, although with substantially less heterogeneity (see Table 5).

The two effects from Gius (2011) are based on individual level data and, as such, could not meaningfully be combined with effects from other studies. The logged odds ratios were converted to percent change in odds to facilitate comparison with the other results. No mean effect size was computed given that these two effects are from the same study. Counter-intuitively, the effect for self-reported arrests was lower than the effect for self-reported criminal behavior (whether resulting in an arrest or not) (see Figure 3). It is not clear why a curfew ordinance would produce a decrease in arrests and not in criminal behavior more generally.

Figure 4 provides a single estimate from Rodabough and Young (2002) for juvenile arrests during curfew hours. This effect, while in the desired direction and statistically significant, is based on simple pre-post data and does not account for any underlying crime drop over time. Thus, it is at a high risk of bias.

Two studies (Kline, 2012; Fivella, 2000) provided estimates of the effects of juvenile curfews on adult crime during all hours (curfew and non-curfew). These effects are a type of control outcome. There is no theoretical reason to expect a juvenile curfew to have an effect on adult rates of crime. Both of these effects are negative (a decrease in crime) with an overall mean percent change of -9% (see Figure 5, 95% confidence interval of -25.3% to 5.3%, p = 0.230). While this effect is not statistically significant, it represents a larger effect than any of the mean effects for juvenile crime rates. Thus, any observed changes in juvenile crime of comparable magnitude might easily be the result of some process other than the curfew that is affecting all crime, whether committed by juveniles or adults.

4.3.2 Effects on victimization

Another goal of juvenile curfews is to reduce juvenile victimization. As with crime, the evidence does not support the effectiveness of curfews in accomplishing this. Figures 6 through 8 show these effects. Across these Figures there is no clear pattern of positive or negative change in victimization.

Figure 6 shows two effects and the mean effect for juvenile victimization during all hours adjusted for a time trend. One effect reflects a positive change and the other a small negative change. The mean effect is close to zero and not statistically significant (a 0.6% decrease in victimization with a 95% confidence interval of -21.7% to 16.9%, $p = .953$). These two studies provide the more credible estimates of curfew effects on victimization given that they are adjusted for any time trend.

The effects shown in Figure 7 are from the two studies that used a simple pre-post design. Three effects are from Moscovitz et al. (2000), one for juvenile victimization during curfew hours, another for during non-curfew hours, and one for all hours. The Rodabough and Young (2002) study provided a single effect of juvenile victimization during all hours. Because three of these four effects are from the same study, a mean effect was not computed (this is also true for the effects shown in Figure 8). The pattern of effects in Figure 7 is consistent with the more credible effects shown in Figure 6.

Finally, Figure 8 shows adult victimization effects. All three of these effects are from Moscovitz et al. (2000). We would not expect a juvenile curfew to have an effect on adult victimization. Thus, these effects help establish natural variation in victimization and are of comparable magnitude to the effects observed for juveniles, further supporting to the conclusion that curfews are ineffective at reducing victimization.

5 Discussion

5.1 SUMMARY OF MAIN RESULTS

The evidence across the 12 studies synthesized in this review suggests that a curfew reduces neither juvenile criminal behavior nor juvenile victimization. However, we caution that all the studies in the review suffer from some limitations that make it difficult to draw firm conclusions. Our finding of no effect may mean that juvenile curfews truly have no impact on crime, or that any impact they have is too small to be reliably detected given the statistical power of the studies, or that the findings are biased. Nonetheless, while we cannot firmly conclude that juvenile curfews have no effect on crime, the lack of any credible evidence in their favor suggests that any effect is likely to be small at best and that curfews are unlikely to be a meaningful solution to juvenile crime and disorder. Our findings are consistent with the conclusions of the prior review by Adams (2003) and the study by Reynolds et al. (2000), the latter an excluded study whose effect size we could not calculate, but was otherwise eligible for inclusion.

Many of the potential biases of concern across these studies should have increased the likelihood of finding an effect, such as the general downward trend in crime during the mid-1990s forward, the introduction of other police or community actions to reduce crime, or implementing a curfew following a recent “spike” in crime. The most likely of these is the general drop in crime during the mid-1990s forward given that only two studies included data prior to the early 1990s. Thus, many of these studies should have seen a drop in juvenile crime simply as a result of this underlying trend. Although It is possible for biases to mask beneficial effects, but this seems less likely in this research context.

The well-established finding that only a small percentage of juvenile offending occurs during curfew hours reinforces this conclusion. Even if a curfew eliminated juvenile crime during curfew hours, the effect on total juvenile crime would be small. However, even those studies that specifically examined juvenile crime during curfew hours failed to find a crime reduction effect attributable to a curfew.

Juvenile victimization also appears to be unaffected by a curfew. There was less evidence available for examining victimization, with effect sizes from two credible studies and two questionable ones. However, in the absence of a crime reduction effect, any effect on victimization seems unlikely. All of these studies were conducted in the United States and as such may not generalize to other countries where youth activity patterns in the evening hours may differ substantially.

5.2 QUALITY OF THE EVIDENCE

This literature has significant methodological weaknesses and is vulnerable to several potential threats to internal validity. The nature of the intervention does not allow for randomization, at least at the level of whether or not a jurisdiction has passed into law a juvenile curfew ordinance. It would be possible to experimentally manipulate different levels of curfew enforcement in ways similar to several randomized studies in the policing literature. However, that would address the question of enforcement and not the effect of the ordinance itself. It is the latter that was the focus of this review.

Most eligible studies used a variant on an interrupted time-series design. This can be a strong design in terms of internal validity if it has a sufficiently long series of observations both before and after the start of the intervention and if there are no competing interventions starting around the same time as the intervention of interest exist (see Shadish, et al. 2002). A long series of observations helps establish any long term upward or downward trend in the outcome, such as dropping crime rates, and also any seasonal variations that might be confounded with the intervention, such as an increase in crime during summer months. A long series also allows for an assessment of regression to the mean. In a time-series, this would appear as a sudden increase (or decrease) in the outcome of interest immediately prior to the start of the intervention and a return to baseline levels thereafter. In curfew evaluations, this might occur if a city passes a curfew ordinance in response to a recent ‘crime wave’.

In assessing risk-of-bias, we judged all but two of the interrupted time-series designs as too short to adequately disentangle a time trend effect from a curfew effect. It is interesting to note, however, that this vulnerability should have biased the results in the direction of finding a beneficial crime reduction effect of a curfew, given that most of these studies were conducted during a period of consistently decreasing crime rates. Furthermore, the most credible and least credible designs produced roughly similar results. That is, we would have expected the most likely biases to produce crime reduction effects. This strengthens the likelihood that the findings reflect the absence of an effect of a curfew rather than methodological biases across the studies.

5.3 LIMITATIONS AND POTENTIAL BIASES IN THE REVIEW PROCESS

The method of estimating the percent change index that was used as the effect size for this synthesis failed to account for any autocorrelation in the data, which is the tendency of observations close in time to be more highly similar than those farther apart in time. The consequence is that the standard errors for the effect sizes were likely to be too small. The percent change index, however, is unaffected by this potential issue. Adjusting the standard errors for autocorrelation would have involved using an ARIMA model or the Newey-West estimator for robust standard errors. Only two of the twelve studies had data that would have allowed for ARIMA modelling and we are unaware of a Newey-West estimator for Poisson or negative binomial models. Further, assuming that these count data were normally distributed would have traded one problem for another. Also, a few of the studies, such as McDowall et al. (2000), found little evidence for autocorrelation in their data. The conclusions for this review, however, are unlikely to be affected by this statistical problem. Underestimating the size of the standard errors biases things in the direction of finding statistically significant effects, not the other way around. Considering that we did not find statistically significant effects, standard errors robust to autocorrelation would have also led to the overall conclusion that juvenile curfews are unlikely to be effective.

5.4 AGREEMENTS AND DISAGREEMENTS WITH OTHER STUDIES OR REVIEWS

Prior reviews of juvenile curfews have also concluded that they are not effective. For example, Reynolds et al. (2000) resolved that the imposition of a youth curfew has no bearing on victimization, juvenile victimization, and juvenile arrests. Furthermore, Adams (2003) concluded that, “overall, the weight of the scientific evidence, based on ten studies with weak to moderately rigorous designs, fails to support the argument that curfews reduce crime and criminal victimization” (p. 155). His review is the most comprehensive prior review in the literature and is widely cited by others as evidence that curfews are not effective. Our updated review of the evidence arrives at the same conclusion.

6 Authors' Conclusions

Juvenile curfews appeal to common sense notions of how to prevent crime and victimization. We experienced the strength of some individuals' belief in the effectiveness of a curfew at a community event in Montgomery County, Maryland, when the county was considering the adoption of a juvenile curfew. The empirical evidence, however, runs counter to common sense. Any effect of a curfew on crime, either positive or negative, is likely to be small at best.

6.1 IMPLICATIONS FOR PRACTICE AND POLICY

A majority of large cities within the United States have a juvenile curfew ordinance. The value of these ordinances is questionable. A policymaker in a jurisdiction without a curfew should not consider one as a solution to youth crime. A curfew may appeal to common sense, but is unlikely, given the evidence, to provide meaningful benefits. This conclusion, however, is restricted to the context of the United States. In many European countries, there are fewer cultural and legal barriers to teenage youth participating in a city's night life. This greater freedom of movement in the late night hours has unclear implications for effects of a curfew in such a context if one were to be implemented.

The evidence presented in this review also raises questions regarding the constitutionality of juvenile curfews within the United States. As discussed in the introduction to this systematic review, the imposition of curfews on the rights of an individual youth and his or her family, has been justified by states on the grounds that doing so protects youth and reduces youth crime. The lack of strong evidence to support this claim provides an avenue for legal challenges of existing curfew ordinances. However, the strength of the evidence presented here may be insufficient for a meaningful legal challenge.

6.2 IMPLICATIONS FOR RESEARCH

Additional high quality evaluations are needed. Although the pattern of evidence across these studies is fairly clear, only four of these twelve studies were judged to be methodologically credible. The remaining studies had clear methodological weaknesses. Future studies need to ensure that they have an adequately long series of data points to control for any underlying change in the crime rate unrelated to the start of a curfew. Furthermore, several of the existing studies could be improved through the application of more sophisticated statistical modelling and through obtaining additional pre and post data. Finally, any jurisdiction that repeals their curfew laws creates an opportunity for a ABA time-series design (control period, treatment period, control period) that can help establish any causal connection between a curfew and juvenile criminal behavior and victimization.

7 References

7.1 REFERENCES TO INCLUDED STUDIES

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8 Information about This Review

8.1 REVIEW AUTHORS

Lead review author:

The lead author is the person who develops and co-ordinates the review team, discusses and assigns roles for individual members of the review team, liaises with the editorial base and takes responsibility for the on-going updates of the review.

Name:	David B. Wilson
Title:	Professor
Affiliation:	George Mason University
Address:	4400 University Drive, MS4F4
City, State, Province or County:	Fairfax, Virginia
Postal Code:	22030
Country:	USA
Phone:	+001 (703) 993-4701
Mobile:	
Email:	dwilsonb@gmu.edu

Co-author(s):

Name:	Charlotte Gill
Title:	Assistant Professor
Affiliation:	George Mason University
Address:	4400 University Drive, MS 6D12
City, State, Province or County:	Fairfax, Virginia
Postal Code:	22030
Country:	USA
Phone:	+001 (703) 993-6085
Mobile:	
Email:	cgill9@gmu.edu

Co-author(s):

Name:	Ajima Olaghere
Title:	Research Associate
Affiliation:	George Mason University
Address:	4400 University Drive, MS4F4
City, State, Province or County:	Fairfax, Virginia
Postal Code:	22030
Country:	USA
Phone:	+001 (703) 993-8315
Mobile:	
Email:	aolagher@gmu.edu

Co-author(s):

Name:	Dave McClure
Title:	Research Associate
Affiliation:	The Urban Institute (Justice Policy Center)
Address:	2100 M Street, NW
City, State, Province or County:	Washington, DC
Postal Code:	20037
Country:	USA
Phone:	+001 (202) 261-5358
Mobile:	
Email:	DMcClure@urban.org

8.2 ROLES AND RESPONSIBILITIES

Please give brief description of content and methodological expertise within the review team. The recommended optimal review team composition includes at least one person on the review team who has content expertise, at least one person who has methodological expertise and at least one person who has statistical expertise. It is also recommended to have one person with information retrieval expertise.

Who is responsible for the below areas? Please list their names:

- Content: David B. Wilson, Charlotte Gill, Ajima Olaghere, Dave McClure
- Systematic review methods: David B. Wilson
- Statistical analysis: David B. Wilson

- Information retrieval: Ajima Olaghere

8.3 SOURCES OF SUPPORT

None.

8.4 DECLARATIONS OF INTEREST

We have no conflicts of interest related to juvenile curfew policies or to any of the studies reviewed as part of the synthesis.

8.5 PLANS FOR UPDATING THE REVIEW

We will consider updating the review in four years if there are any new studies.

8.6 AUTHOR DECLARATION

Authors' responsibilities

By completing this form, you accept responsibility for maintaining the review in light of new evidence, comments and criticisms, and other developments, and updating the review at least once every five years, or, if requested, transferring responsibility for maintaining the review to others as agreed with the Coordinating Group. If an update is not submitted according to agreed plans, or if we are unable to contact you for an extended period, the relevant Coordinating Group has the right to propose the update to alternative authors.

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9 Tables

Table 1: Study location, years data collected, and publication type

Author/Year	Location	Years Data Collected	Publication Type
Cole (2003)	Washington, DC, USA	1993-2001	Journal
Fivella (2000)	Concord, California, USA	1994-1996	Thesis
Gius (2011)	Nationally representative sample, USA	1997	Journal
Kline (2012)	54 Cities, USA	1980-2004	Journal
Males & Macallair (1999)	Monrovia, California, USA	1992-1997	Journal
Males (2000)	Vernon, Connecticut, USA	1990-1998	Journal
Mazerolle et al. (1999)	Cincinnati, Ohio, USA	1997-1998	Tech. Report
McDowall et al. (2000)	12 Cities, USA	1985-1996	Journal
Moscovitz et al. (2000)	Washington, DC, USA	1995	Journal
Rodabough & Young (2002)	Waco, Texas, USA	1993-2001	Journal
Roman & Moore (2003)	Prince George's County, Maryland, USA	1992-1999	Tech. Report
Sutphen & Ford (2001)	Unnamed mid-southern city, USA	1992-1998	Journal

Table 2: Curfew ordinance characteristics and study design information

Author/Year	Curfew Type	Curfew Hours (weekday/weekend)	Ages Affected	Unit of Analysis	Number of Units	Design
Cole (2003)	Nocturnal	11pm-6am/1am-6am	0-16	Month	96	ITS
Fivella (2000)	Nocturnal	midnight-5am	0-17	Month	26	ITS
Gius (2011)	Nocturnal		Variable	Juveniles	4688	Regression
Kline (2012)	Nocturnal		Variable	Year/City	25/54	Regression
Males & Macallair (1999)	School hours	8:30am-1:30pm	12-17	Year	4	Short ITS
Males (2000)	Nocturnal	11pm-6am	0-17	Year	4	Short ITS
Mazerolle et al. (1999)	School hours	7:30am-3pm	7-17	Month	23	ITS
McDowall et al. (2000)	Nocturnal		Variable	Year/City	12/12	Regression
Moscovitz et al. (2000)	Nocturnal	11pm-6am/1am-6am	0-16	Month	6	Pre-post
Rodabough & Young (2002)	Nocturnal	11pm-6am/midnight-6am	0-15	Year	10	Pre-post
Roman & Moore (2003	Nocturnal	10pm-5am/midnight-5am	12-17	Month	87	ITS
Sutphen & Ford (2001)	Nocturnal	11pm-5am	0-17	Year	6	Short ITS

Table 3: Risk of Bias

Author/Year	Historical Artifact	Measurement Confounds	Selection Bias	Maturation Bias	Regression to Mean
Cole (2003)	No	No	N/A	No	Yes
Fivella (2000)	Yes	No	N/A	Yes	Yes
Gius (2011)	No	No	Yes	N/A	No
Kline (2012)	Yes	No	Yes	No	No
Males & Macallair (1999)	Yes	No	N/A	Yes	Yes
Males (2000)	No	No	N/A	Yes	No
Mazerolle et al. (1999)	No	No	N/A	Yes	No
McDowall et al. (2000)	No	No	Yes	No	No
Moscovitz et al. (2000)	No	No	N/A	Yes	No
Rodabough & Young (2002)	No	No	N/A	Yes	No
Roman & Moore (2003)	No	No	N/A	No	No
Sutphen & Ford (2001)	No	No	N/A	Yes	Yes

Table 4: All computed effect sizes

Author/Year	Outcome	Hours	Unit of Analysis	Number of Units	Effect Size Estimation Method	Adjusted for Time Trend	Effect Size	Standard Error	Percent Change
Cole (2003)	Juvenile arrests	All	Month	96	Neg. Binomial	Yes	0.061	0.026	5.9
Fivella (2000)	Juvenile arrests	All	Month	26	Neg. Binomial	Yes	-0.381	0.123	-46.4
Fivella (2000)	Adult arrest	All	Month	26	Neg. Binomial	Yes	-0.167	0.070	-18.2
Gius (2011)	Juvenile crimes (self-report)	All	Juveniles	4688	Logistic	No	-0.055	0.044	N/A
Gius (2011)	Juvenile arrests (self-report)	All	Juveniles	4688	Logistic	No	-0.231	0.062	N/A
Kline (2012)	Juvenile arrests	All	Year/City	25/54	OLS-logged outcome	Yes	-0.110	0.049	-11.6
Kline (2012)	Adult arrest	All	Year/City	25/54	OLS-logged outcome	Yes	-0.023	0.047	-2.3
Males & Macallair (1999)	Juvenile arrests	Curfew	Year	4	Poisson	Yes	0.114	0.117	10.8
Males & Macallair (1999)	Juvenile arrests	All	Year	4	Poisson	Yes	0.002	0.061	0.2
Males (2000)	Juvenile arrests	All	Year	4	Poisson	Yes	0.067	0.044	6.5
Mazerolle et al. (1999)	Juvenile arrests	Curfew	Month	23	Neg. Binomial	Yes	0.103	0.531	9.8
McDowall et al. (2000)	Juvenile homicide victim	All	Year	12/12	OLS-logged outcome	Yes	-0.042	0.117	-4.2
McDowall et al. (2000)	Juvenile arrests	All	Year	12/12	OLS-logged outcome	Yes	0.051	0.198	5.0
Moscovitz et al. (2000)	Juvenile victim of assault	Curfew	Month	6	Rate difference	No	0.182	0.303	16.7
Moscovitz et al. (2000)	Adult victim of assault	Curfew	Month	6	Rate difference	No	-0.177	0.166	-19.4
Moscovitz et al. (2000)	Juvenile victim of assault	Non-curfew	Month	6	Rate difference	No	-0.061	0.174	-6.3
Moscovitz et al. (2000)	Adult victim of assault	Non-curfew	Month	6	Rate difference	No	0.135	0.150	12.6
Moscovitz et al. (2000)	Juvenile victim of assault	All	Month	6	Rate difference	No	0.000	0.151	0.0
Moscovitz et al. (2000)	Adult victim of assault	All	Month	6	Rate difference	No	-0.006	0.111	-0.6
Rodabough & Young (2002)	Juvenile arrests	Curfew	Year	10	Rate difference	No	-0.251	0.097	-28.5
Rodabough & Young (2002)	Juvenile victimization	Curfew	Year	10	Rate difference	No	0.058	0.085	5.6
Roman et al. (2003)	Juvenile arrests	Curfew	Month	87	Neg. Binomial	Yes	0.072	0.165	6.9
Roman et al. (2003)	Juvenile victimization	All	Month	87	Neg. Binomial	Yes	0.077	0.177	7.4
Sutphen & Ford (2001)	Juvenile arrests	All	Year	6	Neg. Binomial	Yes	0.181	0.104	16.6

Table 5: Random effects mean percent change and related statistics

Outcome	Number of Studies	Mean Percent Change	95% Confidence Interval Lower	95% Confidence Interval Upper	Tau^2	z	p	Q	p
Juvenile crime, curfew hours	2	9.5%	-9.1%	24.9%	0.0000	1.05	0.295	0.04	0.834
Juvenile crime, all hours	8	-0.5%	-9.8%	8.1%	0.0088	-0.11	0.915	23.64	0.001
Juvenile crime, all hours (outlier removed*)	7	2.7%	-4.2%	9.2%	0.0036	0.78	0.436	12.50	0.052
Adult crime, all hours	2	-9.0%	-25.3%	5.3%	0.0068	-1.2	0.230	2.93	0.087
Juvenile victimization	2	-0.6%	-21.7%	16.9%	0.0000	-0.06	0.953	0.31	0.577

* Fivella (2000) dropped

10 Figures

Figure 1: Juvenile crime during curfew hours -- adjusted for time trend

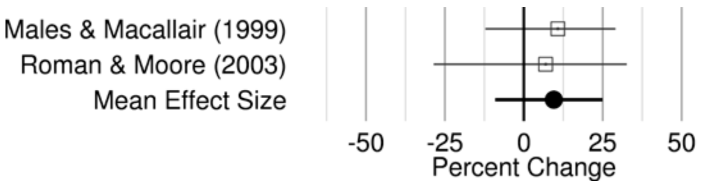


Figure 2: Juvenile crime during all hours -- adjusted for time trend

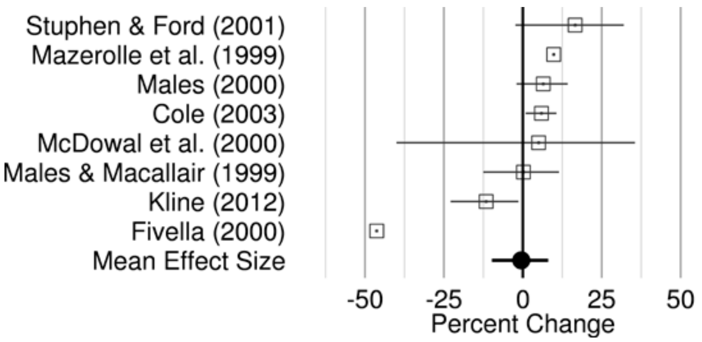


Figure 3: Juvenile crime during all hours -- individual level data

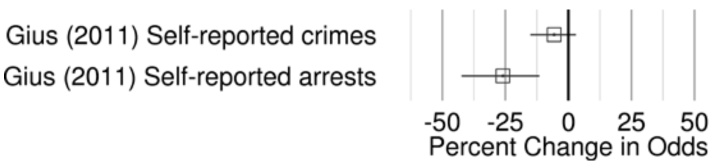


Figure 4: Juvenile crime during curfew hours -- not adjusted for time trend

Figure 5: Adult crime during all hours -- adjusted for time trend

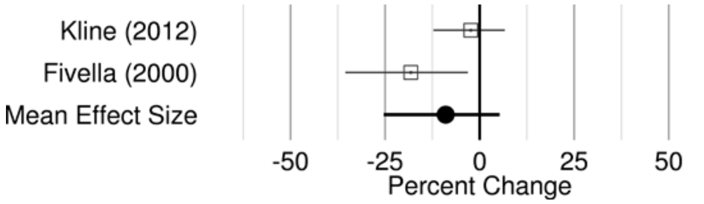


Figure 6: Juvenile victimization during all hours -- adjusted for time trend

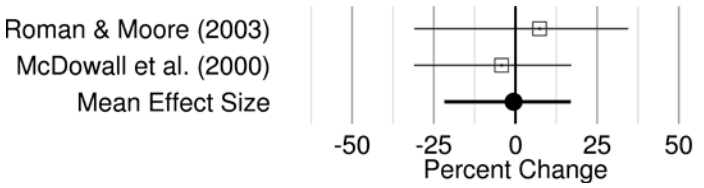


Figure 7: Juvenile victimization -- not adjusted for time trend

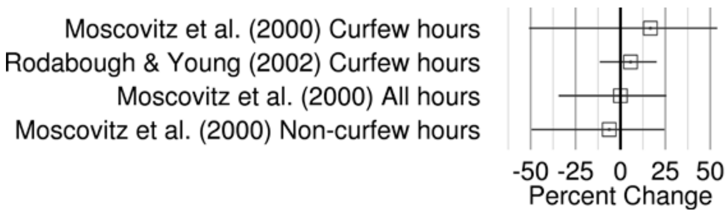
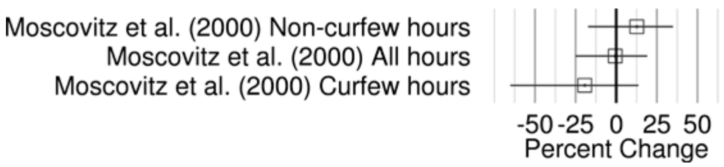


Figure 8: Adult victimization -- not adjusted for time trend



11 R Code for Computation of Effect Sizes

The calculations for all of the effect sizes were done using the statistical/mathematical program R. There is a subset for each study indexed by a reference identifier and by the author(s) and year. The goal was to compute an index of effect that would be comparable across studies and suitable for meta-analysis (i.e., it needed to have a standard error). As discussed in the methods section, this was accomplished in most cases by fitting a negative binomial or Poisson regression model that regressed the crime count on a time indicator (typically months or years) and a dummy variable indicating whether the curfew policy was or was not in effect. Poisson regression was used only when too few data points were available to estimate the over-dispersion parameter, that is, when there were only 2 pre curfew and 2 post curfew observations.

11.1 REFERENCE ID 24 AND 26: COLE (2003)

Data were extracted from Figure 1 on page 223 of Cole (2003) using PlotDigitizer. These data represent the total number of juvenile arrests by month for the period October 1, 1993 through September 30, 2001. This study is an ABAB time-series design with four phases: no curfew, curfew, no curfew, and curfew. The curfew law was enacted in July 1995 but ruled unconstitutional and therefore stopped in October of 1996. It was reinstated after the original court decision was overturned in January of 1999. The outcome represents juvenile arrests for a given month during both curfew and non-curfew hours.

```
require(plyr)

## Loading required package: plyr

require(MASS)

## Loading required package: MASS

ref24 <- data.frame(
  month = c(1:96),
  phase  = c(rep(1,21),rep(2,16),rep(3,34),rep(4,25)),
  curfew = c(rep(0,21),rep(1,16),rep(0,34),rep(1,25)),
```

```
  juvenile = c(
     284.204, 293.341, 264.898, 240.629, 274.818, 334.058, 326.491,
     330.065, 305.513, 275.118, 329.906, 295.897, 326.464, 251.810,
     251.899, 284.838, 262.030, 273.881, 233.329, 301.336, 312.977,
     231.296, 268.619, 261.466, 290.856, 303.331, 236.476, 275.463,
     296.080, 337.575, 310.798, 265.451, 282.514, 255.745, 284.921,
     314.732, 273.751, 254.494, 235.658, 264.416, 246.831, 277.677,
     256.750, 239.370, 239.110, 248.874, 219.177, 203.888, 225.130,
     221.321, 168.034, 165.478, 218.024, 225.085, 194.763, 223.310,
     207.188, 213.609, 198.317, 190.124, 200.308, 202.552, 186.423,
     169.679, 169.414, 171.657, 181.213, 204.127, 201.986, 182.938,
     168.066, 146.304, 171.513, 185.034, 150.327, 161.967, 151.689,
     184.623, 150.124, 179.925, 175.074, 180.035, 173.513, 173.248,
     190.951, 136.409, 134.270, 152.591, 152.331, 215.956, 144.711,
     183.288, 169.667, 160.013, 152.864, 152.813
    )
  )
# make correction to arrest counts and convert to integers
ref24$juvenile <- round(ref24$juvenile - 2,0)
# published statistics of juvenile arrest by phase
# phase 1: n = 21, mean = 286.43, sd = 31.18, min = 231, max = 334
# phase 2: n = 16, mean = 278.13, sd = 28.09, min = 229, max = 336
# phase 3: n = 34, mean = 208.56, sd = 30.24, min = 163, max = 275
# phase 4: n = 25, mean = 163.28, sd = 18.62, min = 134, max = 213

# Compare stats on estimated values from PlotDigitizer to the
# above values
ddply(ref24, "phase", summarize,
      N = length(juvenile),
      mean = mean(juvenile),
      sd = sd(juvenile),
      min = min(juvenile),
      max = max(juvenile))

##   phase  N  mean    sd min max
## 1     1 21 286.2 31.09 231 332
## 2     2 16 278.6 28.48 229 336
## 3     3 34 209.0 30.79 163 276
## 4     4 25 163.2 19.35 132 214

# values are reasonably close
# generate negative binomial model
ref24.nb <- glm.nb(juvenile ~ month + curfew ,
```

```r
                      data = ref24, link = log)
summary(ref24.nb)
```

```
##
## Call:
## glm.nb(formula = juvenile ~ month + curfew, data = ref24, link = log,
##     init.theta = 114.4129449)
##
## Deviance Residuals:
##     Min       1Q   Median       3Q      Max
## -2.3321  -0.7005   0.0589   0.7057   2.5378
##
## Coefficients:
##              Estimate Std. Error z value Pr(>|z|)
## (Intercept)  5.780132   0.023076   250.5   <2e-16 ***
## month       -0.008500   0.000472   -18.0   <2e-16 ***
## curfew       0.060740   0.026459     2.3    0.022 *
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(114.4) family taken to be 1)
##
##     Null deviance: 453.846  on 95  degrees of freedom
## Residual deviance:  95.473  on 93  degrees of freedom
## AIC: 900.5
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  114.4
##           Std. Err.:  24.9
##
##  2 x log-likelihood:  -892.5
```

```r
# effect size, standard error, and variance
results <- data.frame( "es" = ref24.nb$coefficients[3],
                       "v"   =       vcov(ref24.nb)[3,3]  )
# display results
results
```

```
##             es          v
## curfew 0.06074 0.0007001
```

11.2 REFERENCE ID 36: FIVELLA (2000)

Data were extracted from Figure 3 on page 43 of Fivella (2000) using PlotDigitizer. These data present the total number of arrests per month for juveniles and adults, respectively, for a total of 26 months, 13 pre curfew and 13 post curfew.

```
require(MASS)
ref36 <- data.frame(
      month = c(1:26),
      curfew = c(rep(0,13),rep(1,13)),
      arrests.juv = c(
  83.3536,  86.7558,  97.8129,  110.571,
  111.422,  123.329,  104.617,  123.329,
  134.386,  137.789,  96.9623,  107.169,
  137.789,  68.0437,  103.767,  92.7096,
  70.5954,  96.9623,  118.226,  92.7096,
  109.721,  97.8129,  106.318,  68.0437,
  85.0547,  80.8019),
      arrests.adult = c(
  510.328,  511.179,  549.453,  534.143,
  466.100,  574.970,  592.831,  551.154,
  636.209,  636.209,  597.934,  567.315,
  662.576,  609.842,  611.543,  540.948,
  485.662,  498.420,  566.464,  475.456,
  477.157,  481.409,  560.510,  538.396,
  586.877,  605.589)
)
ref36$arrests.juv <- round(ref36$arrests.juv,0)
ref36$arrests.adult <- round(ref36$arrests.adult,0)

# generate the negative binomial models
ref36juv.nb <- glm.nb(arrests.juv ~ month + curfew,
                      data = ref36, link = log)
ref36adult.nb <- glm.nb(arrests.adult ~ month + curfew,
                        data = ref36, link = log)
summary(ref36juv.nb)

##
## Call:
## glm.nb(formula = arrests.juv ~ month + curfew, data = ref36,
##     link = log, init.theta = 69.19740825)
##
## Deviance Residuals:
##    Min      1Q  Median      3Q     Max
```

```
## -2.080  -1.052   0.196   0.725   1.776
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  4.61549    0.07178   64.30   <2e-16 ***
## month        0.01419    0.00818    1.73   0.0829 .
## curfew      -0.38048    0.12279   -3.10   0.0019 **
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(69.2) family taken to be 1)
##
##     Null deviance: 39.896  on 25  degrees of freedom
## Residual deviance: 26.401  on 23  degrees of freedom
## AIC: 225.3
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  69.2
##           Std. Err.:  32.8
##
##  2 x log-likelihood:  -217.3
```

```
summary(ref36adult.nb)
```

```
##
## Call:
## glm.nb(formula = arrests.adult ~ month + curfew, data = ref36,
##     link = log, init.theta = 164.7992709)
##
## Deviance Residuals:
##     Min       1Q   Median       3Q      Max
## -1.9405  -0.6293   0.0642   0.5796   2.0207
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  6.27633    0.04088  153.53   <2e-16 ***
## month        0.00926    0.00465    1.99    0.046 *
## curfew      -0.16706    0.06977   -2.39    0.017 *
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(164.8) family taken to be 1)
```

```
##
##     Null deviance: 32.140  on 25  degrees of freedom
## Residual deviance: 26.114  on 23  degrees of freedom
## AIC: 284.3
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  164.8
##           Std. Err.:  59.5
##
##  2 x log-likelihood:  -276.3
```

```
# extract the effect size data
es <- c(ref36juv.nb$coefficients[3],
        ref36adult.nb$coefficients[3])
v <-  c(vcov(ref36juv.nb)[3,3],
        vcov(ref36adult.nb)[3,3])
results <- as.data.frame(cbind(es,v))
row.names(results) <- c("juvenile","adult")
# display results
results
```

```
##                es        v
## juvenile -0.3805 0.015078
## adult    -0.1671 0.004867
```

11.3 REFERENCE ID 39: GIUS (2011)

Gius used geocoded individual level data from the National Longitudinal Survey of Youth. Two models were estimated, one with self-reported criminal activity as the dependent variable and the other with self-reported arrest as the dependent variable. Both were dichotomous indicators of crime. Results are from tables 1 and 2.

```
# data from tables 1 and 2
ref39 <- data.frame(
          es = c(-0.055, -0.231),
          t = c(-1.263, -3.705)
          )
row.names(ref39) <- c("criminal activity","arrests")
# compute se and v for each effect size
ref39$se <- ref39$es/ref39$t
ref39$v <- ref39$se^2
```

```
# display results
ref39

##                        es      t      se        v
## criminal activity -0.055 -1.263 0.04355 0.001896
## arrests           -0.231 -3.705 0.06235 0.003887
```

11.4 REFERENCE ID 40: GOUVIS (2003)

Goivis (2003) and Roman and Moore (2003) [ID 80] are based on the evaluation of the Prince George's County Youth Curfew. Gouvis (2003) reports the youth victimization outcomes, whereas Roman and Moore (2003) report the crime outcomes. This study is referenced as Roman and Moore in tables and figures.

Figure 1 on page 11 provides the time series for the victimization rate by age group (12 to 16, 17 to 21, and 22 to 25) by month for a total of 87 months. The data for this figure were extracted using PlotDigitizer. Only the data for the 12 to 16 year olds were extracted because of the difficulty in determining the actual position of the lines on the graph for the other two groups.

The figure represents the rates per 1,000 youth in the population per month. To get accurate standard errors, we need the raw counts. These can be approximated by rescaling the values. Table 1 shows that the total number of victimizations for 12 to 16 year olds during curfew hours was 1,132 and during non-curfew hours was 9,322. The series was rescaled to total the sum of these values.

```
require(MASS)
require(psych)

## Loading required package: psych

# data from figure 1
ref40 <- data.frame(
                  month = 1:87,
                  curfew = c(rep(0,54),rep(1,33)),
                  victim_rate = c(
                     3.3616354,3.6399617,2.8296463,4.1066756,3.3413622,
                     5.131214,3.8261027,6.353723,3.303111,2.5377977,
                     4.29171,4.264116,4.2455845,1.5009087,0.9875489,
                     2.2555485,3.964442,6.69,5.6727986,6.931801,
                     3.22446,2.2252364,3.682209,5.3641114,1.7197342,
                     0.9814287,2.4294531,2.4288833,2.9141285,2.8956459,
                     4.1096625,2.885542,4.3334684,2.164661,2.1281352,
                     3.099244,2.1359768,1.1907687,1.8919786,2.602137,
                     3.060472,3.7526848,4.4628596,5.173083,4.2188454,
```

```
                    3.2736208,1.8604946,2.0938516,2.3452353,2.3266711,
                    1.1745119,2.082608,3.2697306,5.572373,6.2826123,
                    2.5752554,3.483368,1.8363476,2.0787342,2.0781643,
                    1.8527142,1.8611253,2.0855014,1.6350892,2.5612285,
                    4.1621723,4.386581,3.423281,2.0461595,2.072614,
                    1.8381014,0.91986865,1.5760765,1.8184793,2.7265918,
                    2.9509516,4.0570507,5.6399674,5.414485,5.171024,
                    1.7785516,2.020922,1.7864094,3.3783398,2.9189303,
                    1.775752,1.5592825)
                    )
# make slight adjustment so mean and sd agree with Table 2
m.victim <- mean(ref40$victim_rate)
ref40$victim_rate <- ref40$victim_rate - (m.victim-3.10)
describe(ref40$victim_rate,skew=FALSE)

##   vars  n mean   sd median trimmed  mad  min  max range   se
## 1    1 87  3.1 1.42   2.88    2.98 1.47 0.92 6.93  6.01 0.15

# rescale victim rates back into raw counts
sum(ref40$victim_rate)

## [1] 269.7

ref40$victim_counts <- ref40$victim_rate*((1132+9322)/sum(ref40$victim_rate))
sum(ref40$victim_counts)

## [1] 10454

ref40$victim_counts <- round(ref40$victim_counts,0)
# compute negative binomial model
ref40.nb <- glm.nb(victim_counts ~ month + curfew,
                   data = ref40, link = log)
summary(ref40.nb)

##
## Call:
## glm.nb(formula = victim_counts ~ month + curfew, data = ref40,
##     link = log, init.theta = 5.562777215)
##
## Deviance Residuals:
##    Min      1Q  Median      3Q     Max
## -2.408  -0.816  -0.256   0.439   1.962
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  4.99922    0.11073   45.15   <2e-16 ***
```

```
## month       -0.00559    0.00342   -1.64     0.10
## curfew       0.07667    0.17695    0.43     0.66
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(5.563) family taken to be 1)
##
##     Null deviance: 95.346  on 86  degrees of freedom
## Residual deviance: 89.416  on 84  degrees of freedom
## AIC: 930.7
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  5.563
##           Std. Err.:  0.861
##
##  2 x log-likelihood:  -922.654
```

```
# effect size, standard error, and variance
results <- data.frame( "es" = ref40.nb$coefficients[3],
                       "v"   =      vcov(ref40.nb)[3,3]  )
# display results
results
```

```
##             es       v
## curfew 0.07667 0.03131
```

11.5 REFERENCE ID 52: KLINE (2011)

Kline (2011) used data from 54 cities with populations greater than 180,000 in 1992 that also enacted a curfew ordinance sometime between 1980 and 2004. City level UCR arrests for both adults and juveniles were used on the outcomes of interest. Ordinary least squares (OLS) was used to fit the models using the log of arrest as the dependent variable.

```
# results for total crimes from tables 3 and 5
ref52 <- data.frame("es" = c(-.110,-.023),
                    "se" = c(.049,.047))
rownames(ref52) <- c("juveniles","adults")
ref52$v <- ref52$se^2
# display results
ref52
```

```
##                es     se        v
## juveniles -0.110 0.049 0.002401
## adults    -0.023 0.047 0.002209
```

11.6 REFERENCE ID 63: MALES (2000)

Males (2000) compared the Part I index offenses for two pre-curfew periods (1990/91 and 1992/93) with two post-curfew periods (1995/96 and 1997/98) for Vernon, Connecticut. Values represent arrest rates per 100,000 based on UCR data (these are not specific to juveniles). These arrests may have occurred at any time, during curfew or non-curfew hours. Vernon had a population of roughly 30,000 during this time period. Data are also presented for comparison cities in Connecticut of similar size that did not have juvenile curfews.

```
# data from table 1
ref63 <- data.frame( "years" = c(1:4) ,
                     "curfew" = c(0,0,1,1),
                     "arrests" = c(3288, 2521, 2282, 2223)
                     )
# estimate Poisson model
ref63.poisson <- glm(arrests ~ curfew + years, data=ref63,
                     family=poisson(link=log))
summary(ref63.poisson)

##
## Call:
## glm(formula = arrests ~ curfew + years, family = poisson(link = log),
##     data = ref63)
##
## Deviance Residuals:
##     1      2      3      4
##  2.67  -2.95  -3.09   3.28
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)   8.2116     0.0317  259.14  < 2e-16 ***
## curfew        0.0668     0.0442    1.51     0.13
## years        -0.1605     0.0198   -8.12  4.5e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for poisson family taken to be 1)
##
##     Null deviance: 267.65  on 3  degrees of freedom
```

```
## Residual deviance:  36.12  on 1  degrees of freedom
## AIC: 80.84
##
## Number of Fisher Scoring iterations: 3

# extract effects
results <- data.frame( es = ref63.poisson$coefficients[2],
                       v  = vcov(ref63.poisson)[2,2] )
# display results
results

##              es        v
## curfew 0.06681 0.001955
```

11.7 REFERENCE ID: 65: MCDOWALL ET AL. (2000)

McDowall et al. (2000) "used panel data from a sample of cities and counties to examine the effects of curfew laws on youth crime rates. The analysis estimated the impact of new and revised laws on juvenile homicide victimizations (1976 to 1995) and on juvenile arrests for a variety of offenses (1985 to 1996)." Two sets of analyses were performed, one on 52 counties and the other on 12 city-counties. The former included counties where a change in the curfew law may have affected only a portion of the county. The latter included only the subset of cities or counties for which any change in a curfew law affected the entire city or county. Our analyses are based on the latter as that provides the more sensitive test of any effect of a curfew, albeit with less statistical power.

The data were analyzed using OLS estimates on log-differenced data. Thus, the coefficients represent percentage change associated with the curfew. McDowall et al. estimated for both a new law and a revised law, both compared to no law. Only the former were used here. We averaged the effects across the different offense types to arrive at an overall effect size for offending. This was a weighted average using the inverse of the squared standard error of the coefficient as the weight. The standard error was the square-root of the mean variance. This is likely to be a conservative estimate of the standard error (i.e., it assumes a high degree of covariance among these estimates).

```
library(metafor)

## Loading 'metafor' package (version 1.9-5). For an overview
## and introduction to the package please type: help(metafor).

# data from table 3
ref65victim <- data.frame( "Model" = "Homicide Victim",
                                     "es" = -.0416 ,
```

```
                                    "se" = .1168 )
ref65arrests       <- data.frame( "Model" = c("Homicide","Rape", "Robbery",
                                 "Agg Assault", "Burglary", "Larceny",
                                 "Moter V Theft", "Simple Assault",
                                 "Vandalism", "Weapon"),
                     "es" = c(.3619,-.1856,  .0499, .1047, -.0366,
                             -.1980, .1463, -.0481, .2002, -.1822),
                     "se" = c(.1577, .2937,  .1273, .1947,  .1756,
                              .1798, .1054,  .2716, .2365,  .1498)
                     )
# compute variances
ref65victim$v <- ref65victim$se^2
ref65arrests$v <- ref65arrests$se^2
# compute weighted mean for arrests
arrests_es <- weighted.mean(ref65arrests$es,1/ref65arrests$v)
# estimated SE for ES
arrests_se <- sqrt(mean(ref65arrests$se^2))
arrests_v <-  arrests_se^2
# combine results
arrests <- data.frame( "Model" = "Arrests",
                       "es" = arrests_es ,
                       "se" = arrests_se,
                       "v" = arrests_v)
ref65 <- rbind(ref65victim,arrests)
# display results
ref65

##             Model      es     se        v
## 1 Homicide Victim -0.0416 0.1168 0.01364
## 2         Arrests  0.0512 0.1979 0.03917
```

11.8 REFERENCE ID 68: MOSCOVITZ ET AL. (2000)

Moscovitz et al. (2000) conducted "a retrospective comparative cohort study of injuries to subjects 13 to 20 years in Washington, DC. The intervention observed was the implementation of the Juvenile Curfew Act of 1995. Data were collected for a three-month period from July 17, 1995 to October 17, 1995, when the curfew was in effect and a corresponding period in 1994 when there was no curfew."

Data were based on Washington, DC, Fire Department Medical Information System records. Data were extracted on injuries from assault and motor vehicle collisions. We used the assault data as a measure of victimization.

```
# function for computing Poisson based standard error for rates
v.Rates <- function (rate1,rate2) {
  v <- 1/rate2 + 1/rate1
  return(v)
}
# function for computing poisson based effect size for rates
effect.Rates <- function (rate1,rate2) {
  effect <- log(rate2) - log(rate1)
  return(effect)
}
# data from table 1
ref68 <- data.frame(
  "age" = c(rep(c("13-16","17-20"),3)),
   "outcome" = c(rep("curfew hours",2),
                 rep("noncurfew hours",2),
                 rep("all hours",2)),
   "yr1994" = c(20,80,68,83,88,163),
   "yr1995" = c(24,67,64,95,88,162)
  )
# compute effect sizes
ref68$effect <- effect.Rates(ref68$yr1994,ref68$yr1995)
ref68$v <- v.Rates(ref68$yr1994,ref68$yr1995)

# display results
ref68

##     age         outcome yr1994 yr1995    effect       v
## 1 13-16    curfew hours     20     24  0.182322 0.09167
## 2 17-20    curfew hours     80     67 -0.177334 0.02743
## 3 13-16 noncurfew hours     68     64 -0.060625 0.03033
## 4 17-20 noncurfew hours     83     95  0.135036 0.02257
## 5 13-16       all hours     88     88  0.000000 0.02273
## 6 17-20       all hours    163    162 -0.006154 0.01231
```

11.9 REFERENCE ID 79: RODABOUGH AND YOUNG (2002)

Rodabough and Young (2002) compared juvenile arrest and victimization data for two one-year time periods: one year before the curfew was implemented and one year after the curfew was implemented. Thus, this study is a simple pre-post study based on the rates of arrests and victimizations. These data were restricted to curfew hours for both time periods. Data were extracted from tables 10 and 12.

```
# function for computing Poisson based standard error for rates
v.Rates <- function (rate1,rate2) {
```

```
  v <- 1/rate2 + 1/rate1
  return(v)
}
# function for computing poisson based effect size for rates
effect.Rates <- function (rate1,rate2) {
  effect <- log(rate2/rate1)
  return(effect)
}
# juvenile arrests
v1 <-  v.Rates(243,189)
es1 <- effect.Rates(243,189)

# juveniles as victims
v2 <- v.Rates(268,284)
es2 <- effect.Rates(268,284)

# create final data frame
ref79effects <- cbind(c(es1,es2),c(v1,v2))
colnames(ref79effects) <- c("es","v")
ref79effects <- as.data.frame(ref79effects)
rownames(ref79effects) <- c("Juvenile Arrests","Juvenile Victims")

# display results
ref79effects

##                        es        v
## Juvenile Arrests -0.25131 0.009406
## Juvenile Victims  0.05799 0.007252
```

11.10 REFERENCE ID 80: ROMAN AND MOORE (2003)

Roman and Moore (2003) and Gouvis (2003) [ID 40] are based on the evaluation of the Prince George's County Youth Curfew. Gouvis (2003) reports the youth victimization outcomes, whereas Roman and Moore (2003) report the crime outcomes.

Figure 1 on page 12 provides the time series for total arrests during curfew hours for 12 to 16 year old youths by month for a total of 87 months. The data for this figure were extracted using PlotDigitizer and is shown below, along with a negative binomial estimate of the effect of curfew.

The figure represents the rates per 1,000 youth in the population per month. To get accurate standard errors, we need the raw counts. These can be approximated by rescaling the values. Table 1 shows that the total number of arrests during curfew hours for 12 to 16 year olds as 3,385. Thus, the raw counts across the series need to sum to this total. This is accomplished through a simple rescaling of the rates back into raw counts. However, these counts are adjusted for changes in the population over time.

```
require(MASS)
require(psych)
# data from figure 1
ref80 <- data.frame(
                  month = 1:87,
                  curfew = c(rep(0,54),rep(1,33)),
                  arrest_rates = c(
  10.3896, 11.1688, 8.77423, 12.0983, 13.1170,
  10.7524, 11.2322, 19.3466, 12.4312, 5.84500,
  6.08524, 9.31954, 9.05085, 6.77603, 10.7587,
  12.4661, 6.95780, 18.3657, 16.0909, 23.2173,
  12.1102, 9.89533, 8.09959, 10.0764, 4.38838,
  5.37712, 8.79098, 10.4683, 8.28341, 14.0326,
  18.8537, 24.3933, 8.97477, 6.81983, 7.44932,
  9.09666, 7.66023, 5.26570, 4.33825, 14.0096,
  12.7529, 14.6098, 10.6285, 11.8268, 7.78559,
  4.94195, 5.69117, 5.87146, 5.45302, 5.45379,
  4.79576, 5.66466, 7.31219, 11.7440, 10.5471,
  12.6435, 7.13529, 8.42335, 6.80728, 4.23318,
  7.01831, 3.75551, 3.48674, 7.91863, 5.58398,
  10.9440, 15.9447, 9.89740, 7.20350, 5.55754,
  6.96543, 5.55895, 6.90692, 4.81175, 4.78253,
  9.33422, 7.71815, 10.8925, 9.54581, 17.1513,
  7.03228, 4.36836, 6.34503, 6.34568, 6.34637,
  2.72433, 5.59924))
# make slight adjustment so mean and sd agree with Table 2
m.arrests <- mean(ref80$arrest_rate)
ref80$arrest_rate <- ref80$arrest_rate - (m.arrests-9.09)
describe(ref80$arrest_rate,skew=FALSE)

##    vars  n mean  sd median trimmed  mad  min   max range   se
## 1     1 87 9.09 4.3   8.03    8.54 3.75 2.66 24.33 21.67 0.46

# rescale arrest rates back into raw counts
sum(ref80$arrest_rates)

## [1] 796.7

ref80$arrest_counts <- ref80$arrest_rates*(3385/sum(ref80$arrest_rates))
sum(ref80$arrest_counts)
```

```
## [1] 3385

ref80$arrest_counts <- round(ref80$arrest_counts,0)
# compute negative binomial model
ref80.nb <- glm.nb(arrest_counts ~ month + curfew,
                   data = ref80, link = log)
summary(ref80.nb)

##
## Call:
## glm.nb(formula = arrest_counts ~ month + curfew, data = ref80,
##     link = log, init.theta = 7.321777224)
##
## Deviance Residuals:
##    Min      1Q  Median      3Q     Max
## -1.780  -0.830  -0.211   0.390   2.781
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  3.97760    0.10195   39.01   <2e-16 ***
## month       -0.00814    0.00317   -2.57     0.01 *
## curfew       0.07168    0.16483    0.43     0.66
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(7.322) family taken to be 1)
##
##     Null deviance: 104.079  on 86  degrees of freedom
## Residual deviance:  87.678  on 84  degrees of freedom
## AIC: 722.2
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  7.32
##           Std. Err.:  1.30
##
##  2 x log-likelihood:  -714.17

# effect size, standard error, and variance
results <- data.frame( "es" = ref80.nb$coefficients[3],
                       "v"   =       vcov(ref80.nb)[3,3]  )
# display results
results
```

```
##             es       v
## curfew 0.07168 0.02717
```

11.11 REFERENCE ID 92: SUTPHEN & FORD (2001)

Sutphen and Ford (2001) examined data on juvenile arrests for a three-year period prior to the implementation of a curfew and after the implementation of the curfew. Years examined were 1992 through 1998. The curfew was implemented mid-year in 1995. As such, data for 1995 is not used in the estimate of the curfew effect. Data represent all juvenile arrests during curfew and noncurfew hours per 10,000 juveniles in the population per year. The estimated population of juveniles was roughly 34,000. Thus, the raw event rates can be approximated by multiplying the provided rates by 3.4

```
require(MASS)
require(psych)
# raw data from table 1
ref92 <- data.frame( "year" = c(1992, 1993, 1994, 1995, 1996, 1997, 1998),
                     "curfew"=  c( 0,    0,    0,    1,    1,    1,    1),
                     "arrests" = c(630, 541, 586, 599, 594, 600, 518))
# drop year 1995, curfew was implemented mid year
ref92 <- ref92[-c(4),]
# rescale to approximate raw counts
ref92$arrests <- round(ref92$arrests * 3.4, 0)
ref92

##   year curfew arrests
## 1 1992      0    2142
## 2 1993      0    1839
## 3 1994      0    1992
## 5 1996      1    2020
## 6 1997      1    2040
## 7 1998      1    1761

# estimate negative binomial model
ref92.nb <- glm.nb(arrests ~ year + curfew, data = ref92, link = log)
summary(ref92.nb)

##
## Call:
## glm.nb(formula = arrests ~ year + curfew, data = ref92, link = log,
##     init.theta = 549.8620843)
##
## Deviance Residuals:
##      1       2       3       5       6       7
```

```
## 0.454  -1.612   1.105  -0.216   1.075  -0.891
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept) 111.1327    48.0853    2.31    0.021 *
## year         -0.0520     0.0241   -2.15    0.031 *
## curfew        0.1812     0.1042    1.74    0.082 .
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(549.9) family taken to be 1)
##
##     Null deviance: 11.108  on 5  degrees of freedom
## Residual deviance:  6.022  on 3  degrees of freedom
## AIC: 79.66
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  550
##           Std. Err.:  408
##
##  2 x log-likelihood:  -71.66
```

```
# extract the effect size data
es <- ref92.nb$coefficients[3]
v  <- vcov(ref92.nb)[3,3]
ref92results <- as.data.frame(cbind(es,v))
# display results
ref92results
```

```
##            es       v
## curfew 0.1812 0.01087
```

11.12 REFERENCE ID 112: MAZEROLLE ET AL. (1999)

Mazerolle et al. (1999) evaluated the effect of the Cincinnati, Ohio, daytime curfew that precluded children from ages 6 to 18 from being in public places other than school during school hours.

The evaluation examined juvenile calls for service and juvenile arrests for selected crimes for the year before (10 months) and one year after (14 months for calls for service and 13 months for arrest data) the daytime curfew went into effect. Only calls for service and arrests within 1,000 feet of fifteen public schools were included.

Arrest data were based on the UCR Index and focused on five specific crime categories: aggravated felony assault, misdemeanor assault, drugs, vandalism, and weapons offenses. PlotDigitizer was used to extract data for calls for service from Figures I.B.1 through I.B.15 and juvenile arrests data from Figure II.B.1 through II.B.15. Separate negative binomial models were run for the total calls for service and juvenile arrests across the 15 schools.

```
require(MASS)
# raw data
ref112cfs <- data.frame( "schools" =  c("aiken","clark",
                          "hughes","scpa","taft","walnut",
                          "western","withrow","woodward","bloom","dater",
                          "jacobs","peoples","porter","shroder"),
                "m1" = c(16,9,47,44,50,2,19,8,28,42,13,7,1,17,8),
                "m2" = c(17,9,33,50,60,7,24,9,25,53,14,5,5,25,10),
                "m3" = c(27,4,51,68,57,10,15,11,35,41,10,5,1,30,11),
                "m4" = c(17,11,55,61,66,10,10,4,23,45,18,4,4,33,13),
                "m5" = c(24,7,42,46,50,17,20,13,14,52,11,5,1,20,7),
                "m6" = c(17,1,45,53,42,5,9,3,16,40,6,5,1,33,8),
                "m7" = c(7,7,36,65,52,19,9,6,9,39,9,5,2,34,12),
                "m8" = c(5,0,51,67,42,3,11,2,13,36,6,3,1,41,5),
                "m9" = c(39,2,43,72,59,9,16,20,37,46,9,7,9,30,16),
                "m10" = c(35,0,44,74,69,1,26,0,22,57,11,5,0,20,0),
                "m11" = c(20,2,42,52,33,2,15,4,25,40,10,4,2,25,13),
                "m12" = c(17,1,43,51,43,10,16,11,14,37,6,8,11,29,7),
                "m13" = c(23,6,68,71,56,16,23,5,17,40,11,9,3,26,10),
                "m14" = c(28,5,49,37,62,15,13,14,10,46,11,4,6,24,11),
                "m15" = c(23,4,41,46,73,7,15,11,20,45,12,8,9,40,8),
                "m16" = c(14,3,42,62,78,8,8,8,13,43,5,5,6,31,10),
                "m17" = c(24,3,46,73,90,7,19,11,12,46,14,13,4,21,8),
                "m18" = c(7,2,36,78,54,1,9,7,14,35,7,7,7,21,8),
                "m19" = c(6,4,35,90,63,5,8,4,1,41,8,6,1,40,6),
                "m20" = c(2,6,37,68,65,5,5,5,6,57,7,9,1,38,2),
                "m21" = c(24,1,50,65,117,5,31,28,19,54,7,12,13,29,7),
                "m22" = c(29,7,70,65,103,7,26,18,27,35,9,11,16,26,10),
                "m23" = c(14,3,52,69,82,5,19,17,23,37,2,4,9,19,9),
                "m24" = c(17,1,35,54,0,10,16,23,18,40,13,4,11,18,7))
ref112arrests <- data.frame( "schools" =  c("aiken","clark",
                          "hughes","scpa","taft","walnut",
                          "western","withrow","woodward","bloom","dater",
                          "jacobs","peoples","porter","shroder"),
                "m1"  = c(66,0,3,0,11,0,6,5,40,3,11,0,0,8,1),
                "m2"  = c(4,0,5,0,8,2,9,3,35,2,13,1,2,1,1),
                "m3"  = c(31,3,2,1,15,4,14,6,33,4,9,6,7,6,3),
```

```r
                "m4"  = c(10,1,0,0,9,2,10,1,22,1,10,5,1,5,8),
                "m5"  = c(19,3,5,0,21,5,3,10,10,5,11,3,5,7,4),
                "m6"  = c(2,0,0,0,6,0,0,2,14,2,3,1,0,2,0),
                "m7"  = c(0,0,3,1,10,0,3,0,1,1,4,0,2,3,0),
                "m8"  = c(0,0,1,2,15,1,0,1,11,3,0,0,0,0,2),
                "m9"  = c(41,0,6,2,13,3,18,34,49,3,5,4,3,1,5),
                "m10" = c(77,0,2,1,10,1,15,12,27,6,4,5,24,0,16),
                "m11" = c(62,0,4,2,18,1,12,12,28,4,0,8,5,0,3),
                "m12" = c(18,0,3,0,11,3,10,19,10,3,0,3,16,0,7),
                "m13" = c(40,0,2,4,2,0,17,10,10,6,5,1,1,0,6),
                "m14" = c(44,3,2,0,14,0,5,21,18,6,12,2,12,1,8),
                "m15" = c(32,1,0,0,14,7,14,10,25,11,8,2,16,2,2),
                "m16" = c(26,0,6,11,8,2,8,7,17,3,4,3,5,1,1),
                "m17" = c(32,0,5,0,14,2,16,7,10,4,1,0,4,1,6),
                "m18" = c(0,0,3,1,11,0,4,0,1,2,0,6,0,0,1),
                "m19" = c(4,0,3,0,8,0,0,0,2,2,1,3,0,0,2),
                "m20" = c(0,0,2,1,14,1,0,1,0,0,0,0,0,0,0),
                "m21" = c(33,0,4,6,24,3,52,16,18,17,6,3,38,1,7),
                "m22" = c(32,1,19,0,19,1,23,29,12,3,6,1,18,2,7),
                "m23" = c(2,0,1,1,13,1,5,10,14,0,1,0,6,3,1),
                "m24" = c(NA,NA,NA,NA,NA,NA,NA,NA,NA,NA,NA,NA,NA,NA,NA))
# sum counts across schools
cfs     <- ref112cfs[-c(1)]
arrests <- ref112arrests[-c(1)]
cfstotals <- as.data.frame(apply(cfs,2,sum))
arreststotals <- as.data.frame(apply(arrests,2,sum))
colnames(cfstotals) <- "cfs"
colnames(arreststotals) <- "arrests"
# create curfew dummy variable
curfew <- data.frame( "curfew" = c(rep(0,10),rep(1,14)) , "month" = rep(1:24) )
# combine data
ref112 <- cbind(curfew,cfstotals,arreststotals)
ref112

##     curfew month cfs arrests
## m1       0     1 311     154
## m2       0     2 346      86
## m3       0     3 376     144
## m4       0     4 374      85
## m5       0     5 329     111
## m6       0     6 284      32
## m7       0     7 311      28
## m8       0     8 286      36
## m9       0     9 414     187
```

```
## m10      0    10 364     200
## m11      1    11 289     159
## m12      1    12 304     103
## m13      1    13 384     104
## m14      1    14 335     148
## m15      1    15 362     144
## m16      1    16 336     102
## m17      1    17 391     102
## m18      1    18 293      29
## m19      1    19 318      25
## m20      1    20 313      19
## m21      1    21 462     228
## m22      1    22 459     173
## m23      1    23 364      58
## m24      1    24 267      NA
```

```
# compute negative binomial model
ref112cfs.nb <- glm.nb(cfs ~ month + curfew,
                       data = ref112, link = log)
ref112arrests.nb <- glm.nb(arrests ~ month + curfew,
                       data = ref112, link = log)
summary(ref112cfs.nb)
```

```
##
## Call:
## glm.nb(formula = cfs ~ month + curfew, data = ref112, link = log,
##     init.theta = 57.82055431)
##
## Deviance Residuals:
##    Min      1Q  Median      3Q     Max
## -2.136  -0.755  -0.108   0.807   1.875
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  5.78032    0.06326   91.38   <2e-16 ***
## month        0.00856    0.00807    1.06     0.29
## curfew      -0.07765    0.11339   -0.68     0.49
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(57.82) family taken to be 1)
##
##     Null deviance: 25.272  on 23  degrees of freedom
## Residual deviance: 23.974  on 21  degrees of freedom
```

```
## AIC: 262.5
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  57.8
##           Std. Err.:  19.4
##
##  2 x log-likelihood:  -254.5
```

```
summary(ref112arrests.nb)
```

```
##
## Call:
## glm.nb(formula = arrests ~ month + curfew, data = ref112, link = log,
##     init.theta = 2.457934563)
##
## Deviance Residuals:
##     Min       1Q   Median       3Q      Max
## -2.0370  -1.0927  -0.0865   0.5156   1.4137
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  4.71489    0.29866   15.79   <2e-16 ***
## month       -0.00853    0.03967   -0.22     0.83
## curfew       0.10299    0.53076    0.19     0.85
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for Negative Binomial(2.458) family taken to be 1)
##
##     Null deviance: 24.575  on 22  degrees of freedom
## Residual deviance: 24.519  on 20  degrees of freedom
##   (1 observation deleted due to missingness)
## AIC: 261.1
##
## Number of Fisher Scoring iterations: 1
##
##
##               Theta:  2.458
##           Std. Err.:  0.704
##
##  2 x log-likelihood:  -253.053
```

```
# effect size, standard error, and variance
results <- data.frame( "es" = c(ref112cfs.nb$coefficients[3],
                                ref112arrests.nb$coefficients[3]),
                       "v"   = c(vcov(ref112cfs.nb)[3,3],
                               vcov(ref112arrests.nb)[3,3])
                               )
rownames(results) <- c("cfs","arrests")
# display results
results

##               es       v
## cfs     -0.07765 0.01286
## arrests  0.10299 0.28171
```

11.13 REFERENCE ID 113 & 111: MALES

Males (1999) analyzed data from Monrovia, California. "Two measures are available from monthly police department tabulations to analyze crime trends: arrests and crimes reported to police. Monrovia's monthly police reports from January 1992 through July 1997 are the basis of analysis (Monrovia Police Department). These sources provide crimes reported to police and arrests by offense and age. The January 1992--September 1994 period provides thirty-three months of pre-curfew data. October 1994, the month the curfew was implemented, is a transition month. The November 1994--July 1997 period provides thirty-three months of post-curfew data. Arrests and crimes reported to police are compared for these periods. Arrest rates are calculated for ages 12-17 (juveniles subject to curfew) and 18-69 (adults) from both census and California Department of Finance population enumerations and intercensal estimates. Crime figures for Monrovia and for neighboring cities also are available from the state Criminal Justice Statistics Center in California Criminal Justice Profiles, Los Angeles County (1990-95 and 1996, 1997 updates)."

The curfew was only enforced during the school year. This study provides data separately for the school year and summer months. Thus, we have used the school year juvenile arrests as the estimate of the curfews effect.

Part 1 crimes reported to police were used as they provided an estimate of the time trend. Arrest data were collapsed to a simple pre and post total count.

```
# data from table A-1
ref113 <- data.frame(
  year = c(1,2,3,4),
  curfew = c(0,0,1,1),
  part1.curfew.hrs = c(410,391,380,293),
  part1.schl.yr = c(1205,1045,882,792),
  part1.summer = c(362,296,279,206)
```

```r
  )
ref113$part1.schl.yr.tot <- ref113$part1.curfew.hrs+
                            ref113$part1.schl.yr
ref113

##   year curfew part1.curfew.hrs part1.schl.yr part1.summer
## 1    1      0              410          1205          362
## 2    2      0              391          1045          296
## 3    3      1              380           882          279
## 4    4      1              293           792          206
##   part1.schl.yr.tot
## 1              1615
## 2              1436
## 3              1262
## 4              1085

# estimate Poisson models
ref113.curfew.hrs <- glm(part1.curfew.hrs   ~ year + curfew,
                                       data=ref113,
                                       family=poisson(link=log))
ref113.schl.yr    <- glm(part1.schl.yr.tot ~ year + curfew,
                                       data=ref113,
                                       family=poisson(link=log))
summary(ref113.curfew.hrs)

##
## Call:
## glm(formula = part1.curfew.hrs ~ year + curfew, family = poisson(link = log),
##     data = ref113)
##
## Deviance Residuals:
##      1       2       3       4
## -0.939   0.993   1.007  -1.104
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)   6.2062     0.0842   73.68   <2e-16 ***
## year         -0.1441     0.0522   -2.76   0.0058 **
## curfew        0.1140     0.1168    0.98   0.3290
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for poisson family taken to be 1)
##
```

```
##     Null deviance: 22.8583  on 3  degrees of freedom
## Residual deviance:  4.0995  on 1  degrees of freedom
## AIC: 41.06
##
## Number of Fisher Scoring iterations: 3
```

```
summary(ref113.schl.yr)
```

```
##
## Call:
## glm(formula = part1.schl.yr.tot ~ year + curfew, family = poisson(link = log),
##     data = ref113)
##
## Deviance Residuals:
##      1       2       3       4
## -0.276   0.294   0.314  -0.336
##
## Coefficients:
##             Estimate Std. Error z value Pr(>|z|)
## (Intercept)  7.52604    0.04393  171.33  < 2e-16 ***
## year        -0.13209    0.02728   -4.84  1.3e-06 ***
## curfew       0.00185    0.06108    0.03     0.98
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## (Dispersion parameter for poisson family taken to be 1)
##
##     Null deviance: 115.94586  on 3  degrees of freedom
## Residual deviance:   0.37367  on 1  degrees of freedom
## AIC: 42.51
##
## Number of Fisher Scoring iterations: 3
```

```
# extract effects
curfew.hrs <- data.frame( es = ref113.curfew.hrs$coefficients[3],
                          v  = vcov(ref113.curfew.hrs)[3,3] )
schl.yr    <- data.frame( es = ref113.schl.yr$coefficients[3],
                          v  = vcov(ref113.schl.yr)[3,3] )
results <- rbind(curfew.hrs,schl.yr)
row.names(results) <- c("curfew hours","school year")
# display results
results
```

```
##                    es        v
## curfew hours 0.114034 0.013646
## school year  0.001853 0.003731
```

12 R Code for Analyses and Forest Plots

```
# R code for analyzing juvenile curfew effect size data
# Written by David B. Wilson
# February 2015

setwd('~/new/juvcurfew/data/R')

## load various libraries
library(metafor)
library(ggplot2)
library(plyr)

###
### create some useful functions
###

logDiff2Percent <- function(x) { -(exp(-x)-1)*100 }

addsum <- function(df) {
	df <- df[order(df$es),]
	ma <- rma.uni(es,sei=se,data=df,method="DL")
	newrow <- data.frame(authorlbl = "Mean Effect Size",
		es = as.numeric(ma$b) ,
		se = as.numeric(ma$se),
		lower = as.numeric(ma$ci.lb),
		upper = as.numeric(ma$ci.ub),
		sum = 1 ,
		es_percent = logDiff2Percent(as.numeric(ma$b)) ,
		lower_percent = logDiff2Percent(as.numeric(ma$ci.lb)) ,
		upper_percent = logDiff2Percent(as.numeric(ma$ci.ub))
		)
	newdf <- rbind.fill(newrow,df)
	newdf$xorder <- 1:nrow(newdf)
	newdf <- newdf[order(newdf$xorder),]
	newdf$authorlbl <- factor(newdf$authorlbl)
	authorlbl <- newdf$authorlbl
	newdf$xorder <- factor(newdf$xorder, labels = authorlbl)
	return(newdf)
}

xOrder <- function(df) {
	df <- df[order(df$es),]
	df$xorder <- 1:nrow(df)
	authorlbl <- df$authorlbl
	df$xorder <- factor(df$xorder, labels = authorlbl)
	return(df)
}

theme_fplot <- function(asratio,textsize) {
	theme(
		legend.position = "none",
		aspect.ratio = asratio,
		panel.border = element_blank(),
		axis.ticks = element_blank(),
```

```
              axis.text.x = element_text(size=textsize,colour="black"),
              axis.text.y = element_text(size=textsize,colour="black"),
              axis.title.x = element_text(size=textsize),
              panel.grid.major = element_line(colour="grey80"),
              panel.grid.minor = element_line(colour="grey90"),
              panel.grid.major.y = element_blank(),
              panel.grid.minor.y = element_blank(),
              panel.background = element_blank()
              )
}

forestPlot <- function(plotdata,eslabel,asratio,textsize,ticlbls,limits) {
       p <- ggplot(plotdata, aes(x=es_percent,
       xmax=upper_percent,
       xmin=lower_percent,
       y=xorder ,
       size=factor(sum), shape=factor(sum), colour=factor(sum)))
       p <- p + geom_point()
       p <- p + geom_point(size=(((plotdata$sum+3)/3)*textsize/4))
       p <- p + scale_shape_manual(values=c(0,16))
       p <- p + geom_errorbarh(height=0)
       p <- p + scale_size_manual(values=c(.5,1))
       p <- p + geom_vline(xintercept = 0, lty=1,size=.75, colour="black")
       p <- p + scale_x_continuous(breaks=ticlbls, limits=limits)
       p <- p + scale_colour_manual(values=c("grey50","black"))
       p <- p + ylab("") + xlab(eslabel)
       p <- p + theme_fplot(asratio,textsize)
       return(p)
}

###
### Read CSV data that was exported from FileMaker database
### this is effect size level data (one record per effect size)
###
jcdata <- read.csv("~/new/juvcurfew/data/filemaker/es.csv",header=FALSE)
colnames(jcdata) <- c("studyid","esid","authorlbl","escoder","dvlabel","dvtype",
              "dvcrime","datasrc","sample","hours","uoa","n","model",
              "adj4time","es","v","se")
jcdata$uniqueid <- jcdata$studyid + jcdata$esid/100
# number of effect sizes per study
as.data.frame(table(jcdata$authorlbl))
# compute 95% confidence interval
jcdata$lower <- jcdata$es - jcdata$se*1.96
jcdata$upper <- jcdata$es + jcdata$se*1.96
# convert logged difference in rates/counts to percent change
# including the 95% confidence interval
jcdata$es_percent <- logDiff2Percent(jcdata$es)
jcdata$lower_percent <- logDiff2Percent(jcdata$lower)
jcdata$upper_percent <- logDiff2Percent(jcdata$upper)

###
### Create subsets, one for each analysis and forest plot
###

# create dummry to indicate if a summary row (needed for forest plot)
jcdata$sum <- 0

# crime; juveniles; curfew hours; adjusted for time trend
jcdata1 <- subset(jcdata, jcdata$sample=="Juveniles" &
              jcdata$hours=="Curfew" &
              jcdata$dvcrime=="Yes" &
              jcdata$adj4time=="Yes")

# crime; juveniles; all hours; adjusted for time trend
jcdata2 <- subset(jcdata, jcdata$sample=="Juveniles" &
              jcdata$hours=="Both" &
              jcdata$dvcrime=="Yes" &
              jcdata$adj4time=="Yes")
```

```
# crime; adults; all hours; adjusted for time trend
jcdata3 <- subset(jcdata, jcdata$sample=="Adults" &
              jcdata$hours=="Both" &
              jcdata$dvcrime=="Yes" &
              jcdata$adj4time=="Yes")

# crime; juveniles; curfew hours; not adjusted for time trend
jcdata4 <- subset(jcdata, jcdata$sample=="Juveniles" &
              jcdata$hours=="Curfew" &
              jcdata$dvcrime=="Yes" &
              jcdata$adj4time=="No")

# crime; juveniles; all hours, individual level
jcdata5 <- subset(jcdata, jcdata$sample=="Juveniles" &
              jcdata$hours=="Both" &
              jcdata$dvcrime=="Yes" &
              jcdata$adj4time=="No" &
              jcdata$model=="Logistic regression")

# victimization; juveniles; all hours; adjusted for time trend
jcdata6 <- subset(jcdata, jcdata$sample=="Juveniles" &
              jcdata$hours=="Both" &
              jcdata$dvcrime=="No" &
              jcdata$adj4time=="Yes")

# victimization; juveniles; any hours; no adjustment to time trend
jcdata7 <- subset(jcdata, jcdata$sample=="Juveniles" &
              jcdata$dvcrime=="No" &
              jcdata$adj4time=="No")

# victimization; adults; any hours; no adjustment to time trend
jcdata8 <- subset(jcdata, jcdata$sample=="Adults" &
              jcdata$dvcrime=="No" &
              jcdata$adj4time=="No")

# makes sure all effects were used and used only once
duplicated(rbind(jcdata1,jcdata2,jcdata3,jcdata4,
jcdata5,jcdata6,jcdata7,jcdata8))
length(rbind(jcdata1,jcdata2,jcdata3,jcdata4,
jcdata5,jcdata6,jcdata7,jcdata8))
length(jcdata)

# generate mean effects
model1 <- rma.uni(es, sei=se, data=jcdata1, method="DL")
model2 <- rma.uni(es, sei=se, data=jcdata2, method="DL")
# drop outlier
model2b <- rma.uni(es, sei=se, data=jcdata2[- grep("Fivella",
              jcdata2$authorlbl),], method="DL")
model3 <- rma.uni(es, sei=se, data=jcdata3, method="DL")
model6 <- rma.uni(es, sei=se, data=jcdata6, method="DL")
model1
model2
model3
model6

# create results table
row4Table <- function(df,rowlbl) {
data.frame(model = rowlbl,
              k = as.numeric(df$k),
              es = logDiff2Percent(as.numeric(df$b)),
              lower = logDiff2Percent(as.numeric(df$ci.lb)),
              upper = logDiff2Percent(as.numeric(df$ci.ub)),
              tau2 = as.numeric(df$tau2),
              z = as.numeric(df$zval),
              zp = as.numeric(df$pval),
              q = as.numeric(df$QE),
              qp = as.numeric(df$QEp))
       }
       results <- rbind(row4Table(model1,"Juveniles, Curfew Hours"),
```

```
        row4Table(model2,"Juveniles, All Hours"),
        row4Table(model2b," (outlier dropped)"),
        row4Table(model3,"Adults, All Hours"),
        row4Table(model6,"Juveniles, Victimizations")
)
results$es <- round(results$es,digits=1)
results$lower <- round(results$lower,digits=1)
results$upper <- round(results$upper,digits=1)
results$tau2 <- round(results$tau2,digits=4)
results$z <- round(results$z,digits=2)
results$zp <- round(results$zp,digits=3)
results$q <- round(results$q,digits=2)
results$qp <- round(results$qp,digits=3)
results

# add summary row to data tables (mean effect size
jcdata1 <- addsum(jcdata1)
jcdata2 <- addsum(jcdata2)
jcdata3 <- addsum(jcdata3)
jcdata6 <- addsum(jcdata6)

# create variable to order effect for forest plots without
# a mean effect
jcdata4 <- xOrder(jcdata4)
jcdata5 <- xOrder(jcdata5)
jcdata7 <- xOrder(jcdata7)
jcdata8 <- xOrder(jcdata8)

## create and save plots
forestPlot(jcdata1,"Percent Change",.25,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="juv_curfew_hours_adj.png",dpi=600)
forestPlot(jcdata2,"Percent Change",.6,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="juv_all_hours_adj.png",dpi=600)
forestPlot(jcdata3,"Percent Change",.25,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="adult_all_hours_adj.png",dpi=600)
forestPlot(jcdata4,"Percent Change",.25,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="juv_curfew_hours_not_adj.png",dpi=600)
forestPlot(jcdata5,"Percent Change in Odds",.25,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="juv_all_hours_micro_level.png",dpi=600)
forestPlot(jcdata6,"Percent Change",.25,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="juv_victim_all_hours_adj.png",dpi=600)
forestPlot(jcdata7,"Percent Change",.55,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-65,65))
ggsave(file="juv_victim_all_hours_not_adj.png",dpi=600)
forestPlot(jcdata8,"Percent Change",.35,16,
        ticlbls=c(-50,-25,0,25,50),limits=c(-75,75))
ggsave(file="adult_victim_all_hours_not_adj.png",dpi=600)

## save workspace iamge
save.image("c2-juvcurfew-analysis.RData")
```

13 Search Notes

13.1 DATABASES

AIC – Australian Institute of Criminology

Date: February 4, 2014
Time: 7:32 PM
Final search string:
CURFEW
Hits: 2
Notes: simple search in Humanities & Social Sciences Collection

ASSIA – Applied Social Science Index and Abstracts

Date: January 20, 2014
Time: 7:59 AM
Final search string: CURFEW
Hits: 75
Notes: command line used for search

CINCH (the Australian Criminology Database via Informit)

Date: March 5, 2014
Time: 13:10 (AEST – Australian Eastern Standard Time)
Final search string: juvenile* OR young* OR youth* OR minor* OR child* OR kid* OR teen* OR adolescen* OR pubescent*) AND (curfew)
Hits: 22
Notes: Any field; date range 1970–2014

Criminal Justice Abstracts

Date: January 20, 2014
Time: 8:43 PM
Final search string: CURFEW
Hits: 265

EconLit

Database: EconLit
Date: January 20, 2014
Time: 8:57 PM
Final search string: CURFEW
Hits: 6

First Search - Dissertation Abstracts

Date: January 20, 2014
Time: 9:07 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*)

Hits: 23

Google Scholar

Date: January 20, 2014
Time: 9:32 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN*) and (CRIM* or DELINQUEN* or ARREST*)
Hits: 1,220
Notes:

- Selected searches only for pages written in English under Google Scholar settings.
- Stopped at page 24; Google Scholar stopped working because it does not allow automated traffic, which can be caused by meta-searching
- Resumed download on 1/21/14, starting at page 24. Stopped again at page 35; Google Scholar would not process request for next page of references to download.
- Resumed download on 1/28/14; starting on page 35. Stopped at page 44 (downloaded).
- Resumed download on 2/1/14; starting on page 45; completed search, 50 pages total.

HeinOnline

Date: January 20, 2014
Time: 10:12 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN*) and (CRIM* or DELINQUEN* or ARREST*)
Hits:1,125
Notes: "Full Text" was selected as the location to execute the search

Jill Dando Institute of Crime Science (JDI)

Date: January 21, 2014
Time: 8:54 AM
Final search string: CURFEW
Hits: 18
Notes: the entire UCL site was searched, not just the Jill Dando Institute; there was no separate search mechanism for JDI.

NCJRS (National Criminal Justice Reference Service)

Date: January 21, 2014
Time: 9:47 AM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*)
Hits: 331
Notes: used command line

Policy Archive

Date: February 5, 2014
Time: 5:02 PM
Final search string: (via Google) site: www.policyarchive.org juvenile
Hits: 6
Notes:

- Search mechanism for PolicyArchive was not working; used Google instead to search the site.
- Could not save references to Zotero; browsed references from Google
- Conducted search gain on March 3, 2014 to browse references and the search returned 3,640 results, only six of which were from the www.policyarchive.org. None of the entries were relevant.
- Hits amended to reflect this new, updated yield.

PolicyFile

Date: January 21, 2014
Time: 1:37 PM
Final search string:
CURFEW and (JUVENILE*) and DETENTION
Hits: 1,065

Criminal Justice Periodicals (now ProQuest Criminal Justice)

Date: January 29, 2014
Time: 5:00 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*) and CRIM*
Hits: 1,173
Notes:

- Used command line; zotero froze on second page of 100 references. Reloaded page and started download again but reduced reference listing to 50 items per page. Most likely will have duplicates because of this.
- Zotero froze again with 50 items listed; reloaded page and reduced item listing to 20 items per page. Last item downloaded was #150. On new page with 20 items per page listed (#141-160), checked to see if items 141-150 were already downloaded and the time stamp. These references were accounted for in Zotero and included the same time stamp. The page now with 20 items listed was downloaded again and items #141-150 were deselected.
- Stopped downloading at page 9, "Known translator issues" error; references were not downloading into Zotero. Same issues for pages 10 and 11. Will retry later.
- Resumed search on January 30, 2014 at 7:29 PM: received "Known translator issues" error message again; moved to export remaining references and import into Zotero

Dissertations & Theses: Full Text

Date: January 29, 2014
Time: 5:58 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*) and CRIM*
Hits (final): 10,416
Notes:

- Used command line
- Using entire original search string, received 12,932 hits
- Using this modified version, received 10,866hits
- The first 1,000 references were downloaded from the above final number of hits.
- Encountered "Known Translator Issues" error again; exported first 1,000 (sorted by relevance by database, a default setting) references

into RIS format and then imported into Zotero, 200 references at a time.

Evidence-Based Resources from the Joanna Briggs Institute

Date: February 4, 2014
Time: 7:22 PM
Final search string: CURFEW
Hits: 0
Notes:

- Searched under Research tab > Registered titles
- Discovered an OVID database for JDI; outsourced search to colleagues with access to the JDI database via OVID, results below:

Database: Joanna Bri1 (via Ovid)
Date: March 5, 2014
Time: 13:30 (AEST – Australian Eastern Standard Time)
Final search string: juvenile* OR young* OR youth* OR minor* OR child* OR kid* OR teen* OR adolescen* OR pubescent*) AND (curfew)
Sub-searches:

1. (juvenile* or young* or youth* or minor* or child* or kid* or teen* or adolescen* or pubescent*).mp. [mp=text, heading word, subject area node, title] = 1894 results
2. curfew.mp. [mp=text, heading word, subject area node, title] = 0 results
   1 and 2 = 0
   Hits: 0

PubMed

Date: January 30, 2014
Time: 9:05 PM
Final search string: CURFEW
Hits: 43

PsycINFO

Date: January 30, 2014
Time: 9:15 PM
Final search string: CURFEW
Hits: 70

Public Affairs Information Service

Date: January 30, 2014
Time: 9:33 PM
Final search string: CURFEW
Hits: 75

RAND Documents

Date: January 30, 2014
Time: 9:41 PM
Final search string: CURFEW
Hits: 6
Notes:
Two references are PDFs, both entitled "Los Angeles..." and were not downloaded into Zotero, but can be viewed under the snapshot: "Search the RAND Website."

Social Sciences Citation Index

Date: February 1, 2014

Time: 8:38 PM
Final search string: TS=(CURFEW)
Hits: 130
Notes: advanced search

Social Services Abstracts
Date: February 1, 2014
Time: 9:40 PM
Final search string: CURFEW
Hits: 25
Notes: command line search

Sociological Abstracts
Date: February 1, 2014
Time: 9:51 PM
Final search string: CURFEW
Hits: 113

SSRN – Social Science Research Network
Date: February 1, 2014
Time: 10:13 PM
Final search string: CURFEW
Hits: 13

Worldwide Political Science Abstracts
Date: February 1, 2014
Time: 10:39 PM
Final search string: CURFEW
Hits: 99
Notes: command line search

13.2 GREY LITERATURE

Association of Chief Police Officers ACPO
Date: Monday, January 20, 2014
Time: 7:13 PM
Final search string: JUVENILE
Hits: 1
Notes: Case study section currently under construction.

Association of Chief Police Officers of Scotland ACPOS
Date: February 2, 2014
Time: 9:17 AM
Final search string: CURFEW
Hits: 11
Notes: on April 1, 2013 ACPOS merged with the Scottish Crime and Drug Enforcement Agency (SCDEA) to form Police Scotland

Association of Police Authorities APA
Date: February 2, 2014
Time: 9:31 AM
Final search string: (via Google): curfew site:www.apa.police.uk
Hits: 0

Australian Research Council Centre of Excellence in Policing and Security (CEPS)

Date: February 2, 2014
Time: 9:34 AM
Final search string: CURFEW
Hits: 1

Canadian Police Research Centre

Date: February 2, 2014
Time: 9:57 AM
Final search string: CURFEW
Hits: 0
Notes: CPRC does not have its own website

Her Majesty's Inspectorate of Constabulary HMIC

Date: February 2, 2014
Time: 5:29 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*)
Hits: 14
Notes: searched within "Publications"

Home Office (UK)

Date: February 2, 2014
Time: 6:44 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*)
Hits: 272
Notes: searched within publications

Medline/Embase

Date: February 1, 2014
Time: 10:54 PM
Final search string: CURFEW
Hits: 43

Ministry of Justice (UK)

Date: February 2, 2014
Time: 6:41 PM
Final search string:
CURFEW and (JUVENILE* or YOUNG* or YOUTH* or MINOR* or CHILD* or KID* or TEEN* or ADOLESCEN* or PUBESCENT*)
Hits: 84
Notes: searched within publications

National Council for Crime Prevention (Sweden)

Date: February 2, 2014
Time: 6:48 PM
Final search string: CURFEW
Hits: 0

National Institute of Justice

Date: February 2, 2014
Time: 6:57 PM
Final search string: CURFEW
Hits: 3

Notes: searched within publications (all publications)

Office of Juvenile Justice and Delinquency Prevention

Date: February 2, 2014
Time: 7:06 PM
Final search string: CURFEW
Hits: 3
Notes: searched within publications

Scottish Institute for Policing Research (SIPR)

Date: February 2, 2014
Time: 7:18 PM
Final search string: CURFEW
Hits: 12
Notes: the snapshot does not capture the list of retrieved items from the search. Manually entered items into Zotero

U.S. state juvenile justice agencies and court services

Various state juvenile justice agencies and court services. Used the National Center for State Courts website to link to various agency websites: http://www.ncsc.org/Topics/Children-Families-and-Elders/Juvenile-Justice-and-Delinquency/State-Links.aspx

Date: February 4, 2014 (started) - February 11, 2014 (finished)
Time: 8:50 PM
Total hits (all 50 states): 11,359

More details available from the authors.

Hits (per state):

AL:	0	KS:	705	NY:	165	WV:	36
AK:	4	KY:	13	NC:	184	WI:	13
AR:	3	LA:	3	ND:	87	WY:	8
AZ:	7,563	ME:	9	OH:	258		
CA:	49	MD:	82	OK:	0		
CO:	61	MA:	16	OR:	24		
CT:	32	MI:	110	PA:	0		
D.C.:	121	MN:	24	RI:	13		
DE:	4	MS:	13	SC:	3		
FL:	183	MO:	10	SD:	39		
GA:	3	MT:	0	TN:	45		
HI:	18	NE:	106	TX:	154		
ID:	3	NV:	0	UT:	35		
IL:	79	NH:	6	VT:	0		
IN:	195	NJ:	68	VA:	481		
IA:	28	NM:	1	WA:	302		

Notes:

AL: No search mechanism for juvenile justice and court services websites. Searched respective websites using Google.

AK: No observable studies related to youth curfews

AR: Hits only from Arkansas Department of Human Services

AZ: Browsed through fist two pages; material irrelevant and duplicates

CA: Hit yield combined from courts website (n=27) and Department of Corrections and Rehabilitation (n=22). Browsed first two pages, no observable studies related to youth curfews.

CO: Browsed first two pages, no observable studies related to youth curfews.

CT: (Judiciary) Browsed first and the only two pages, no relevant studies about juvenile curfews, only case law. (DCF) Browsed first and only page, which did not show any studies on youth curfews. Hit yield combined from judiciary website (n=29) and department of children and families (n=3).

D.C.: Browsed first and the only two pages, no relevant studies about juvenile curfews

DE: Hits only from Delaware state courts website; browsed first and only page, which did not show any studies on youth curfews.

FL: Browsed first two pages for judiciary and department of juvenile justice; no observable and relevant studies on youth curfews. Hit yield combined from judiciary website (n=169) and DJJ website (n=14).

GA: Hits only from Georgia Department of Juvenile Justice; none relevant studies on youth curfews.

HI: Browsed all hits; no observable studies related to youth curfews.

ID: Hits only from the Idaho Department of Juvenile Corrections; none relevant studies on youth curfews

IL: Hits only from Illinois State Courts website; browsed first two pages, no relevant studies on youth curfews, primarily case law.

IN: Hit yield combined from court website (n=194) and Family and Social Services Administration (n=1). Browsed first two pages of court website; no relevant studies related to youth curfews.

IA: Hit yield combined from judiciary website (n=27) and Department of Human Services (n=1). Browsed all hits, no observable studies related to youth curfews.

KS: Hit yield combined from Kansas courts websites (n=543) and the Kansas Department of Corrections (n=162). Browsed first two pages of each site; no studies related to youth curfews, mainly office documents and policies.

KY: Hits only from Kentucky Department of Juvenile Justice; browsed hits, none related to studies on youth curfews.

LA: Hits only from the LA Office of Youth Services, Office of Juvenile Justice; only three section headings were retrieved that are policy/operation statements.

ME: Hits only from the Maine Dept. of Corrections. Browsed through all 9 items, none relevant studies related to youth curfews.

MD: Hit yield combined from MD Dept. of Juvenile Justice (n=16) and MD Courts (n=66). Browsed first and only page of Google results and first two pages of MD Courts search results. No observable studies related to youth curfews.

MA: Hits only from the Department of Health and Human Services. Browsed all hits and no observable studies related to youth curfews.

MI: Hits only from the Michigan Courts website. Browsed first two pages and no observable studies related to youth curfews.

MN: Hit yield combined from MN Courts website (n=22) and the MN Dept. of Corrections (n=2); no observable studies related to youth curfews, instead overviews, handbooks, and policy manuals.

MS: Hits only from the Mississippi courts website. Browsed all hits and no observable studies related to youth curfews.

MO: Hit yield combined from MO courts website (n=2) and Dept. of Social Services (n=8). No observable studies related to youth curfews.

MT: n/a; no hits returned.

NE: Hit yield combined from NE court website (n=8) and the Dept. of Health and Human Services (n=98). Browsed first and only page on court website. Browsed first two pages of Health and Human Services website, no observable studies related to youth curfews.

NV: n/a; no hits returned.

NH: Hits only from the NH Dept. of Health and Human Services. Browsed all titles, none relevant to youth curfews.

NJ: Hits only from NJ courts; browsed through first two pages, no observable studies related to youth curfews.

NM: Single hit from Dept. of Children, Youth and Families and related to adoption.

NY: Hit yield combined from NY courts and NY Administration for Children's Services (Dept. of Juvenile Justice merged with this Dept.). Browsed the first page and the first two pages of ACS and NY courts, respectively; did not observe any relevant studies on youth curfews.

NC: Hit yield combined from NC courts (n=61) and NC Dept. of Public Safety (n=123). Browsed through first two pages and did not see any studies relevant to youth curfews.

ND: Hit yield only from ND courts. Browsed first two pages and did not see any studies relevant to youth curfews

OH: Hit yield combined from OH courts (n=232) and Dept. of Youth Services (n=68). Browsed first two pages on each site and did not see any studies relevant to youth curfews.

OK: n/a; no hits received

OR: Hit yield only from Oregon courts; browsed entire yield, no studies relevant to youth curfews.

PA: n/a; no hits received

RI: Hit yield combined from RI courts (n=8) and Dept. of Public Safety (n=5). Browsed through hits and did not see studies relevant to youth curfews.

SC: All three hits were not a study on youth curfews, instead one presentation, program manual, and a reintegration program.

SD: Hit yield combined from SD courts (n=12) and DOC (n=27). Browsed first two pages of each site, no studies relevant to youth curfews.

TN: Hit yield combined from TN Commission on Youth and Children (n=11) and TN courts (n=34). Browsed all hits on each site, no studies relevant to youth curfews.

TX: Hit yield combined from TX Dept. of Juvenile Justice (n=22) and TN courts (n=132). Browsed first two pages, did not see any studies relevant to youth curfews.

UT: Hit yield combined from Commission on Criminal and Juvenile Justice (n=13) and UT courts (n=22). Browsed all results from each search; no studies relevant to youth curfews

VT: n/a; no hits returned.

VA: Hit yield combined from VA courts (n=36) and Dept. of Criminal Justice Services (n=445). Browsed first two pages and did not see any studies relevant to youth curfews.

WA: Hit yield combined from WA courts (n=301) and Commission on Juvenile Justice (n=1). Browsed first two pages of WA courts hits and the one hit from Google did not produce any studies relevant to youth curfews; mostly information on sentencing reform acts.

WV: Hit yield from WV courts only; browsed through first two pages, no studies relevant to youth curfews.

WI: Hits from WI courts only; browsed through first and only two pages, no studies relevant to youth curfews.

WY: Hit yield combined from WY courts (n=6) and the Dept. of Family Services (n=2). Browsed through all results and did not see any studies relevant to youth curfews.

14 Coding Manual

A FileMakerPro database was created based on the coding manual below. Coding was done directly into FileMakerPro.

Study Level Coding

Use one study level code sheet for each study. If multiple documents report on the results from the same study, identify one document as primary and use its document ID as the StudyID below. Record document IDs for the related documents in the CrossRef# fields.

Identifying Information

1. Study (document) identifier [studyid] _____
2. Cross reference document identifier [crossref1] _____
3. Cross reference document identifier [crossref2] _____
4. Cross reference document identifier [crossref3] _____
5. Coder's initials [scoder] _____
6. Date coded [sdate] ___ - __ - __

General Study Information

7. Author and Year [author] _________________________________
8. Funder (e.g., NIJ) [funder] _________________________________
9. Geographical location of study [slocale] _________________________________
10. Geography (1=single site; 2=multiple sites; 9=cannot tell) [A study evaluating the effects of a curfew in a single city, county, or country would be coded as “1”, whereas a study examining multiple non-contiguous cities or counties would be coded as “2”.]

[sites] _____

11. Country [country] _______________________________
12. Date range for data collection [startdate] ___ - __ - __

[donedate] ___ - __ - __

13. Publication Type (primary document if multiple used) [pubtype] _____

   1. Book 2. Book Chapter
   3. Journal (peer reviewed) 4. Federal Gov't Report
   5. State/Local Gov't Report 6. Dissertation/Thesis
   7. Unpublished (tech report, conference paper)

Information on the Curfew Policy

14. Description of policy____

____

____

15. Curfew type [curfewtype] _____

1. Nocturnal (i.e., nighttime)
2. School hours

For the next four items, use military time (e.g., 1830 for 6:30, 0:00 for midnight, etc.). If missing, code as 9999. If not applicable, code as 8888. Not applicable would be suitable for a study evaluating multiple jurisdictions with different start and stop times.

16. Weekday curfew begins [curfewstart1] _____
17. Weekday curfew ends [curfewend1] _____
18. Weekend curfew begins [curfewstart2] _____
19. Weekend curfew ends [curfewend2] _____

18. Youngest age affected by this curfew [curfewage1] _____
19. Oldest age affected by this curfew [curfewage2] _____

20. Level at which curfew implemented [curfewsize] _____

1. Neighborhood
2. City
3. County
4. Metropolitan area
5. State
6. Country

21. Number distinct jurisdictions included in curfew (this reflects the entities on which data was collected) [curfewn] _____
22. Nature of geographic areas [areatype] _____

1. Urban
2. Rural
3. Surburban
4. Metropolitan (urban/suburban)
5. Other mixed

24. Noted implementation problems ____

Information on Comparison Communities

25. Study included comparison area(s) (i.e., areas that didn't have a curfew at any point during the study) (0 = no; 1 = yes) [compgrp] _____

26. Study used an historical comparison within the same area that implemented a curfew (i.e., baseline data for a jurisdiction that implemented a curfew) (0 = no; 1 = yes) [comptime] _____

27. Nature of comparison geographic areas (8 if "no" to item 25) [compsize] _____

1. Neighborhood
2. City
3. County
4. Metropolitan area
5. State
6. Country

28. Number of comparison areas (this reflects the entities on which data was collected) [compn] _____

29. Nature of geographic areas [areatypec] _____

1. Urban
2. Rural
3. Surburban
4. Metropolitan (urban/suburban)
5. Other mixed

Study Methodology

30. Design Type [designtype] _____

1. Interrupted time-series (single series)
2. Interrupted time-series (multiple treatment series)
3. Interrupted time-series (with comparison series)
4. Interrupted time-series (single series, ABAB design)
5. Individual level data (non-equivalent comparison design)
6. Pre-post rates (one reate per; one reate post)
7. Randomized controlled trial (true experiment)

Notes: Items 1-4 are for times-series with multiple crime counts or rates prior to the start of a curfew ordinance and multiple crime counts or rates after the start of a curfew ordinance. The counts or rates may be for weeks, months, or years. 6 is similar to a time-series but is with only 1 data points before and after the start of the curfew. 5 is for studies of individual level data on juveniles with youth living in different jurisdictions. 7 is for studies with multiple geographic areas with crime rates measured both before and after the curfew policy and data are analyzed in a single model, such as an OLS regression.

31. Unit of analysis (this reflects the unit-of-analysis for the data used in computing effect sizes) [uoa] _____

1. Week
2. Month
3. Year
4. Year by City
5. Individual (juvenile)

32. Number of units (9999 if not reported) [numunits] _____

33. Analysis type [analysistype] _____

1. OLS regression
2. ARIMA
3. Other regression
4. Simple comparison of rates
5. Other

34. Analysis adjusted for covraites (other than baseline measure of outcome) (9999 if not reported; 8888 if varying intervals) [interval] _____

35. Baseline characteristics measures ____

36. Analysis type [analysisyype] _____

1. OLS regression
2. ARIMA
3. Other regression
4. Simple comparison of rates
5. Other

37. Analysis adjusted for covariates (1=yes; 0=no) [covariates] _____

Assessment of Risk and Bias

38. Any noted or apparent historical artifacts (1=yes; 0=no) [histart] _____

39. Measurement confounds (change in measure over time) (1=yes; 0=no) [measart] _____

40. Selection of curfew area based on high baseline crime rate (1=yes; 0=no; 8=n/a) [selart] _____

41. Appropriate statistical analysis for design (1=yes; 0=no) (1=yes; 0=no; 8=n/a) [statapp] _____

42. Time series too short (fewer than 50 time points total) (1=yes; 0=no; 8=n/a) [tooshort] _____

43. Cannot determine crime trend over time (e.g., one pre and one post measurement) (1=yes; 0=no; 8=n/a) [notimecntrl] _____

[44-45 for non-equivalent comparison groups only]

44. Non-trivial baseline differences (statistically significant baseline differences or baseline differences that are potentially substantively meaningful – i.e., $d > .10$) (1=yes; 0=no) [basediff] _____

45. Statistical analysis failed to adjust for measured baseline differences (1=yes; 0=no) [noadjust] _____

Effect Size Coding

Use one effect size level coding sheet per effect size. A study may have multiple effect sizes. Number these sequentially using the ESID field.

Identifying Information

1. Study (document) identifier [studyid] _____
2. Effect identifier [esid] _____
3. Coder's initials [ecoder] _____
4. Date coded [edate] ___ - __ - __

Outcome Type

5. Label for outcome____[dvlabel]
6. Outcome type [dvtype] _____
   1. Juvenile arrests
   2. Juvenile crimes
   3. Juvenile victimization
   4. Adult arrests
   5. Adult victimization
7. Is this a measure of crime? (0 = no; 1 = yes) [dvcrime] ____
8. Source of data [datasrc] _____
   1. Official record
   2. Self-report (juvenile self-report of crime)
   3. Citizen report
9. Population on which outcome is measured [sample] _____
   1. Juveniles
   2. Adults

Sample size information

9. Unit-of-analysis [uoa] _____
   1. Week
   2. Month
   3. Year
   4. Year by City
   5. Individual (juvenile)

10. Number of units [n] _____

Effect Data

All computations were done in R. See appendix XX for complete documentation on each computation.

11. Method of estimating effect size [model] _____

    1. Negative binomial
    2. Poisson
    3. Logistic regression
    4. OLS regression (logged outcome)
    5. OLS regression
    6. Logged relative rate

12. Effect size model adjusted effect for linear time trend. (0 = no; 1 = yes) [adj4time] _____

13. Effect size [es] _____

14. Effect size variance [v] _____

15. Effect size standard error [se] _____

16. Page number for effect size data [es_page] _____

17. Effect size notes [esnotes]

____

____

____

THE CAMPBELL COLLABORATION

The Campbell Collaboration
info@campbellcollaboration.org
Phone: (+47) 23 25 50 00

Mailing address:
P.O. Box 4004, Nydalen
N-0403 Oslo, Norway

Visiting address:
Pilestredet Park 7
(Entrance from Stensberggata)

Website:
www.campbellcollaboration.org

THE CAMPBELL COLLABORATION

About this review

Curfews restrict youth below a certain – usually 17 or 18 – from public places during night time. A juvenile curfew has common sense appeal: keep youth at home during the late night and early morning hours and you will prevent them from committing a crime or being a victim of a crime. In addition, the potential for fines or other sanctions deter youth from being out in a public place during curfew hours.

Juvenile curfews have received numerous legal challenges. The constitutional basis for infringing the rights of youth rests on the assumption that they reduce juvenile crime and victimization.

This review synthesizes the evidence on the effectiveness of juvenile curfews in reducing criminal behavior and victimization among youth.The review summarizes findings from 12 studies.

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