SYSTEMATIC REVIEW

Youth Exposure to Endemic Community Gun Violence: A Systematic Review

Pilar Bancalari[1] · Marni Sommer[1] · Sonali Rajan[2,3]

Abstract

Community gun violence persists as a daily reality for many youth in low-income urban communities. While most gun violence research has focused on the direct victims of firearm homicide, exploration into the broader public health repercussions of community gun violence on youth has lagged. This systematic review aimed to synthesize and critically assess the state of evidence on indirect exposure to community gun violence among low-income urban youth in the U.S. PubMed, Web of Science, ProQuest, and SCOPUS were searched for peer-reviewed articles exploring the scope, risk factors, and impacts of community gun violence exposure on this population. Of the 143 studies identified and screened, 13 studies were ultimately included. The broad themes emerging include (1) a lack of consensus regarding the range of experiences that constitute community gun violence, (2) exposure to violence involving a firearm as distinct from that with other weapons, (3) a need to conceptualize multiple dimensions of gun violence exposure, (4) differential impacts of exposure to community gun violence across developmental stages, and (5) how indirect gun violence exposure uniquely contributes to cycles of community violence. Future research must move toward a consistent typology, multidimensional conceptualization, and developmental- and context-specific examination of community gun violence exposure.

Introduction

The public health impact resulting from gun violence has historically been operationalized by quantifying physical injuries and deaths resulting from gunshots (Mitchell et al., 2019). Less explored are the physiological and psychosocial consequences of chronic and indirect exposure to community gun violence among the populations who are disproportionately impacted—primarily low-income urban communities of color. Indirect exposure to community gun violence poses broad threats to youth development and wellbeing. These threats warrant a shift in the paradigm of gun violence exposure to encompass indirect experiences as distinct exposures to be comprehensively studied and prevented. While the effects of direct gun violence victimization have been described extensively throughout the literature, the distinct repercussions of indirect exposure on youth represent an overlooked externality of endemic gun violence in the United States (U.S.), meriting separate analysis. The aim of this review is to summarize and critically appraise the state of evidence on indirect exposure to community gun violence among low-income urban youth in the U.S. Although there is no standardized definition, for the purposes of this review, indirect exposure to community gun violence will refer to witnessing gunfire or hearing gunshots in public places such as streets, parks, and schools, knowing a friend or family member who has been shot or carries a gun, or being aware of gun violence in one's community.

✉ Pilar Bancalari
pilarbancalari@gmail.com

1   Department of Sociomedical Sciences, Mailman School of Public Health, Columbia University, 722 West 168th Street, New York, NY 10032, USA

2   Department of Health and Behavior Studies, Teachers College, Columbia University, 525 West 120th Street, New York, NY 10027, USA

3   Department of Epidemiology, Mailman School of Public Health, Columbia University, 722 West 168th Street, New York, NY 10032, USA

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Scope of Indirect Exposure to Community Gun Violence

Gun violence has become an inescapable reality across the U.S., where there are more civilian firearms per capita than in any other country (Furman, 2018). While a vast body of literature exists on the influence of community violence on youth, the meaning of "community" varies considerably from study to study. The term "community violence" is sometimes employed to differentiate violence that occurs in homes (domestic violence) from violence occurring in public spaces, such as schools, streets, parks, and stores. In other instances, community violence refers to all violence occurring within a community, whether inside homes, between strangers, or across street blocks.

Although there is extensive research describing the psychological and social consequences associated with community violence exposure on youth, there are few studies on the impacts of indirect exposure to violence involving a gun. Nonetheless, a great deal of the literature on exposure to community violence of any type includes measures that assess direct victimization and indirect exposure to gun violence (Goldstick et al., 2017; McGee et al., 2017). Direct gun violence exposure is defined as being threatened, injured, or killed with a firearm. Indirect gun violence exposure is sometimes defined as witnessing violence involving a firearm, yet some studies have adopted broader definitions encompassing the experiences of hearing gunshots, learning of gun violence, knowing someone who has been victimized, or seeing violence in the media (Stein, et al., 2003a, 2003b). While estimates vary, indirect exposure to gun violence is consistently more common than direct victimization (Stein, et al., 2003a, 2003b). For example, data from the National Survey of Children's Exposure to Violence estimates that 17% of 14–17-year-old youth have heard gunshots or seen someone shot (Finkelhor et al., 2015). Among urban African American 6–7-year-olds in Detroit, 84% had witnessed gunfire, 26% had witnessed a shooting, and 14% had seen a dead body. Rates of exposure to community gun violence vary considerably between populations and regions, with certain subgroups being at higher risk of exposure and subsequent sequalae.

Populations Placed At-Risk

The effects of gun violence exposure follow gendered and age-moderated patterns, although these remain poorly understood (Cooley-Strickland et al., 2009). Indirect exposure to gun violence is highest among Black, urban, male, and low-income adolescents (Mitchell et al., 2019). Latinx and Native American youth are also more likely to witness community violence than their White counterparts (Mitchell et al., 2019). While gun violence certainly occurs in rural areas, studies show that urban-dwelling youth experience higher rates of exposure to firearm-related violence (Slovak, 2002; Turner et al., 2019). Males are more likely than females to have been exposed to community violence, including gun violence, across populations (Finkelhor et al., 2015).

A large body of evidence has demonstrated that gun homicides are often concentrated within small geographical "hot spots" and networks of people (Braga et al., 2010). Racial disparities in gun violence exposure are echoed by rates of firearm victimization, with Black youth 0–19 years old being nine times as likely to die by gun homicide than their White counterparts (Centers for Disease Control & Prevention, 2018). Neighborhood poverty, often rooted in structural racism, is a strong predictor of crime as chronic disinvestment, blight, and community disorder fuel crime in certain areas, thus increasing residents' vulnerability to gun violence exposure (Kim, 2019).

Psychosocial Sequelae

At the same time, evidence suggests that indirect exposure to community violence results in symptomology that is distinct from that resulting from direct exposure (Foster & Brooks-Gunn, 2009). For example, a study of preschool children found that witnessing violence was associated with internalizing symptoms (e.g., depression, anxiety, post-traumatic stress), while direct victimization led to externalizing problems (e.g., aggression, conduct disorder, impulsivity) (Stein et al., 2001). Exposure to community gun violence has been linked to distress, anxiety, depression, anger, withdrawal, post-traumatic stress, substance use, desensitization to violence, and academic difficulties, yet the particular risk factors and pathways underlying these associations are not well-explained (Cooley-Strickland et al., 2009; Luthar & Goldstein, 2004).

Physiological effects have also been documented as a result of exposure to violence involving firearms. Two studies including children from San Juan, Puerto Rico and Hartford, Connecticut showed that exposure to gun violence significantly increased odds of asthma, after controlling for socioeconomic status, prematurity, air pollution, and exposure to tobacco smoke (Ramratnam et al., 2015; Rosas-Salazar et al., 2016). While still incompletely understood, hypothalamic-pituitary-adrenocortical (HPA) axis and immune response dysregulation resulting from psychosocial distress have been proposed as the biological mechanisms underpinning this association (Rosenberg et al., 2014). Witnessing gun violence during adolescence has also been linked to hypertension in adulthood, in line with extensive research demonstrating the harmful effects of early trauma on health across the life course (Ford & Browning, 2014).

Developmental Impacts

Research suggests that youth exposed to gun violence during early childhood (birth through age 8), may be placed at risk for the most severe developmental consequences. Early childhood trauma is known to disrupt neurodevelopment via physiological dysregulation and learned maladaptive coping (Cooley-Strickland et al., 2009). A biopsychosocial model points toward four mechanisms of risk and resilience underpinning the relationship between childhood trauma exposure and psychopathology: information processing biases that heighten threat perception, maladaptive learning mechanisms, heightened emotional reactivity, and emotional dysregulation (McLaughlin & Lambert, 2017). Moreover, early trauma has been associated with a host of adverse health outcomes later in life, many of which are the leading causes of morbidity and mortality in the U.S. (Petruccelli et al., 2019). The concept of allostatic load, or the strain resulting from the body's effort to maintain homeostasis, is useful in understanding how chronic stress leads to physiological 'wear-and-tear' via repeated activation of the neural, neuroendocrine, and immune systems during threatening situations (McEwen, 1998). Children and adolescents living in neighborhoods with high rates of gun violence likely also face a multitude of social adversities in addition to violence exposure such as structural poverty and neglect (Cooley-Strickland et al., 2009). Yet contextual factors such as familial or school support and the presence of a stable caregiver can serve as protective factors, buffering the deleterious impacts of gun violence exposure on development (Foster & Brooks-Gunn, 2009; Luthar & Goldstein, 2004). A recent systematic review argues that expansion of the definition of gun violence exposure as an adverse childhood experience (ACE) is warranted given its known effects on youth health outcomes, their wellbeing, and persistent research gaps (Rajan et al., 2019).

Current Study

Indirect exposure to community gun violence among youth poses an overlooked and understudied externality of endemic gun violence in the U.S, thus hindering our ability to effectively prevent and also respond to such experiences. The primary purpose of this review is to explore the scientific literature on the prevalence and physiological and psychosocial effects of indirect exposure to community gun violence among urban[1] low-income youth of color (Black and Latinx)

ages 2–18 years old in the U.S. This review identifies subsets of urban youth—by age, gender, and race—that are particularly vulnerable to the adverse impacts of community gun violence, and explores associated risk and protective factors. Finally, gaps in research to date on indirect exposure to community gun violence are described and future research directions proposed.

Methods

Search Strategy

This review was limited to urban youth ages 2 to 18 years old in the U.S., with special attention given to populations placed at-risk including low-income Black and Latinx youth. Indirect gun violence was operationalized based on existing literature and included seeing or hearing gunfire, knowing someone who has been shot or carries a gun, and being aware of gun violence in one's community (Kennedy & Ceballo, 2014; Mitchell et al., 2019; Overstreet, 2000; Rajan et al., 2019; Turner et al., 2019). The databases used included PubMed, Web of Science (core collection), ProQuest, and SCOPUS. An exploratory search using scoping keywords based on an initial scan of the literature (*indirect, community violence, exposure to violence, firearm, gun, youth, urban, mental health, trauma, childhood adversity,* and *adverse childhood experiences*) was conducted to harvest terminology for a comprehensive search strategy. The following combined search string was found to yield the most relevant results across databases: ("indirect" OR "exposure") AND ("community") AND ("gun*" OR "firearm*") AND ("violence" OR "crime") AND ("youth" OR "child*" OR "adolescent*"). This search string was used to identify scientific literature on indirect exposure to community gun violence among the population of interest across the four aforementioned databases.

Screening

Search results were imported into the systematic review management tool Covidence (*Covidence Systematic Review Software*). One reviewer screened study titles and abstracts for relevance after which two reviewers independently screened full texts based on selection criteria. These same two reviewers discussed and resolved three discordant votes. Inclusion criteria included peer-reviewed articles in English published in 1995 and thereafter on the scope of community

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[1] Large central metro counties are defined by the National Center for Health Statistics (NCHS) as counties in metropolitan statistical areas (MSAs) of one million or more population that either (1) contain the entire population of the largest principal city of the MSA, (2) are completely contained within the largest principal city of the MSA,

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Footnote 1 (continued)

or (3) contain at least 250,000 residents of any principal city in the MSA.

Fig. 1 PRISMA flow diagram of literature search, screening, reasons for exclusion, and results of review
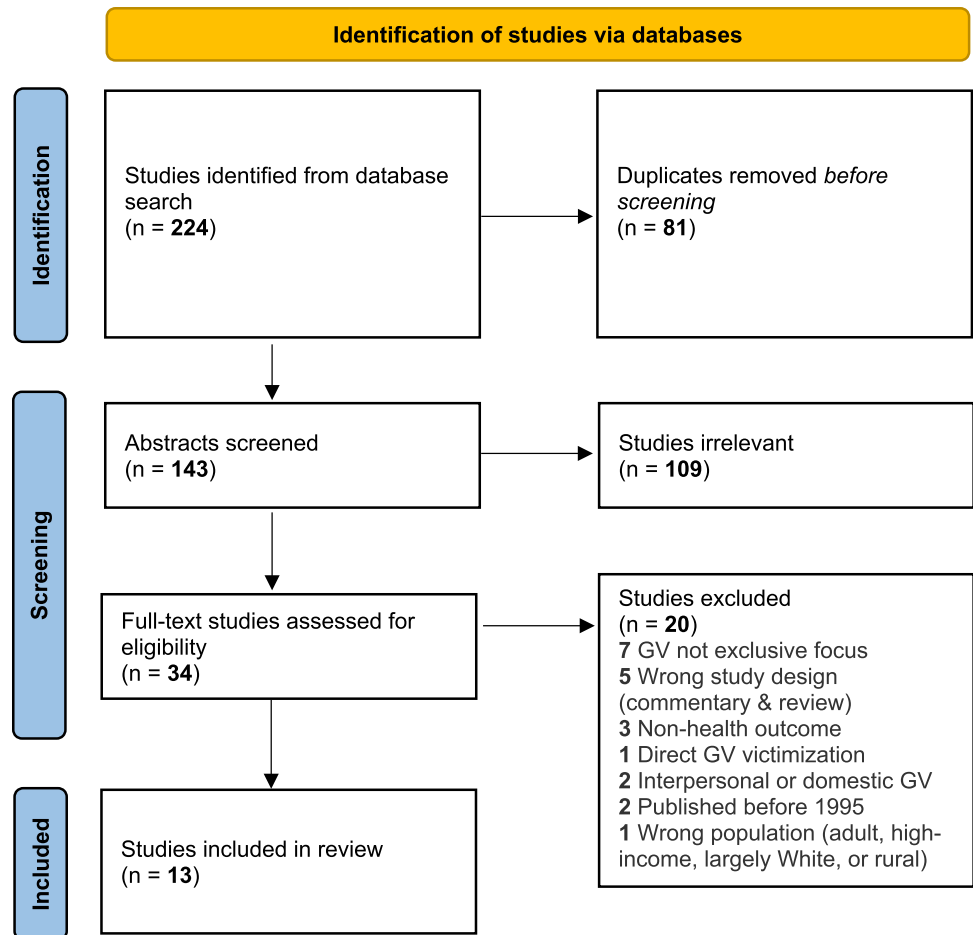
gun violence, defined as firearm assaults occurring in public spaces, among low-income children and adolescents ages 2 to 18 years old living in urban areas. Studies regarding direct gun violence victimization, legal intervention, domestic violence, non-firearm-related violence, media or video game violence, suicides by firearm, and adult, rural, largely White non-Latinx, high-income, or international populations were excluded. Although studies concerning non-gun related violence were excluded from the formal review, evidence applicable to community gun violence exposure was used to complement the review findings. For the purposes of this review, youth wellbeing was defined as an optimal state of physical, social, cognitive, and psychological health at both the individual and environmental levels (Pollard & Lee, 2003).

Thematic and Quality Assessment

Relevant study characteristics including the study setting, sample size, population demographics, violence exposure conceptualization, primary outcomes, major findings, identified research gaps, and recommendations were extracted from each study. Major patterns and themes across the

reviewed studies were then identified based on an inductive analysis of the extracted data. A quality assessment was conducted using a quality appraisal checklist adapted from the Graphical Appraisal Tool for Epidemiological Studies frame (Jackson, 2006). The dimensions of quality that were assessed included sample selection, outcome measurement, analyses, and other potential threats to internal and external validity.

Results

The search strategy yielded 224 studies, of which 81 duplicates were removed, leaving 143 studies in the abstract screening (Fig. 1). Of those screened, 34 full-text studies were assessed for eligibility by the study authors. Thirteen studies were included in the final review: 6 cross-sectional, 3 longitudinal, 2 geospatial mapping, and 2 case–control (Table 1). Although no qualitative studies exploring the impacts of indirect exposure to gun violence were identified, a qualitative study of repeated exposure to violent crime among low-income youth of color in an urban community in the Northeast provided valuable context for the review

Table 1 Summary of relevant findings from literature review

Title, Author(s), Year (Design)	Location (sample size), demographics	Violence exposure conceptualization	Outcome(s)	Relevant findings	Gaps in Research	Recommendations	Quality appraisal score*
A multivariate analysis of gun violence among urban youth: The impact of direct victimization, indirect victimization, and victimization among peers, McGee et al. (2017) (Cross-sectional survey)	Hampton Roads, VA (n = 500), Lower socioeconomic African American youth ages 12–18	Threatened with a gun, shot at with a gun, seen others attacked with a gun, seen others threatened with a gun, seen a dead body, and been to parties where guns were fired (domains: community, family, school; Level of exposure: peer victimization, direct victimization, and indirect victimization)	Gun related delinquency	Direct gun victimization predicts gun related delinquency, and this association is strong among African American adolescents	Lack of research accounting for domains of gun violence exposure and differentiating between direct, indirect and peer victimization. Limited longitudinal data exploring these relationships	Because gun violence exposure is an antecedent to gun violence perpetration, adolescents gun violence exposure and victimization should be prevented as a strategy to prevent future gun violence	++
Childhood Trauma Exposure and Gun Violence Risk Factors among Victims of Gun Violence, Wamser-Nanney et al. (2019) (Cross-sectional survey)	New Orleans, LA (n = 72), Patients 18–55 years old admitted to Level I trauma center due to gunshot injury from community violence (not domestic violence)	Community violence exposure was defined as witnessing someone die suddenly or be badly injured	Gun ownership, gun carrying, gun arrests, impulsivity, perceptions regarding violence (hypothesized predictors of GV victimization)	Childhood trauma (exposure to domestic and community violence) is a risk factor for gun violence involvement	Limited research specific to gun violence and its associated risk factors	Research with larger sample sizes to tease apart different trauma ecologies on specific risk factors and associations with gun violence	+
Community Gun Violence as a Social Determinant of Elementary School Achievement, Bergen-Cico et al. (2018) (Geospatial mapping)	Syracuse, NY (n = 2127), Third grade elementary school children	Police department data for confirmed gunshots	Standardized NY state test scores for English Language Arts (ELA) and math	Higher levels of gun violence within school catchment areas were significantly associated with higher rates of ELA and math failure	Few studies have assessed the geo-spatial relationship between exposure to CGV near schools and youth academic achievement	Indirect exposure to CGV may be an important social determinant of academic performance among elementary school children	+
Effects of exposure to violence with a weapon during adolescence on adult hypertension, Ford & Browning (2014) (Longitudinal)	U.S. (n = 7971), Male and females from National Study of Adolescent Health ages 11–17 years at wave 1	Saw someone stab or shoot another person	Hypertension	Males who had witnessed violence and females who had been victimized in the past year had increased odds of hypertension, controlling for BMI, smoking, alcohol abuse and depression	Limited research into the potential mechanisms underlying exposure to violence with a weapon and hypertension in adulthood	Adolescents should be screened for exposure to serious violence and referred appropriately and research should target violence prevention	++

Table 1 (continued)

Title, Author(s), Year (Design)	Location (sample size), demographics	Violence exposure conceptualization	Outcome(s)	Relevant findings	Gaps in Research	Recommendations	Quality appraisal score*
Exposure to gun violence and asthma among children in Puerto Rico, Ramratnam et al. (2015) (Case–control)	San Juan, PR (n=466), Children ages 9–14 years	Lifetime exposure to gun violence: having heard a gunshot more than once	Asthma	Gun violence exposure was associated with asthma in children after adjusting for SES, parental asthma, exposure to tobacco smoke, prematurity and residential proximity to a major road	No studies on impact of indirect exposure to gun violence and physiological outcomes such as asthma	Longitudinal research into exposure to violence involving firearms and asthma, as well as the potential biological mechanisms linking these	++
Firearm Violence Exposure and Serious Violent Behavior, Bigenheimer, Brennan and Earls (2005) (Longitudinal)	Chicago, IL (n=1517), Adolescents aged 12–15 years	Had been shot or shot at, or had seen someone shot or shot at	Serious violence perpetration	Exposure to firearm violence increased likelihood of adolescent perpetrating serious violence in subsequent 2 years	Lack of longitudinal studies and studies accounting for personal characteristics and environmental factors that influence exposure to community violence and violence perpetration	Identify adolescents exposed to firearm violence to prevent future violence perpetration	++
Firearm Violence Exposure and Suicidal Ideation Among Young Adults Experiencing Homelessness, Hsu et al. (2020) (Cross-sectional survey)	Los Angeles, CA, Denver, CO, Houston, TX, Phoenix, AZ, New York City, NY, San Jose, CA and St. Louis, MO (n=1426), Young adults experiencing homelessness	Being shot at and shot by, seeing someone being injured or killed with a gun, shooting a gun at another individual	Suicidal ideation	45% had experienced direct or indirect gun violence; vast majority of homeless youth who reported being perpetrators had also experienced victimization; direct and indirect gun violence victimization was not associated with suicidal ideation	Literature among homeless youth has overlooked potential role of violence perpetration in suicide risk	Firearm violence perpetration is a risk factor for suicide risk and should be screened for by providers	++
Gun Exposure Among Black American Youth Residing in Low-Income Urban Environments, Quimby et al. (2018) (Cross-sectional survey)	Chicago, IL (n=185), Urban Black American adolescents from low-income public schools	Knowing someone with a gun, presence of a gun at home or relative's home, have ever held a gun	Internalizing (anxiety, depression and trauma) and externalizing (beliefs about aggression and delinquency) symptoms	Almost half of adolescents in sample were exposed to guns, most often outside of their homes. Gun exposure was associated with externalizing symptoms but knowing someone who owns a gun with both internalizing and externalizing symptoms	Few studies have looked at impact of presence of guns in adolescents' lives on psychological and behavioral outcomes. Limited data on differential impacts of gun exposure across genders	Distinguish between gun violence victimization, gun carrying and exposure to guns in order to tease apart outcomes and appropriately tailor interventions	++

Table 1 (continued)

Title, Author(s), Year (Design)	Location (sample size), demographics	Violence exposure conceptualization	Outcome(s)	Relevant findings	Gaps in Research	Recommendations	Quality appraisal score*
Gun Violence Exposure and Posttraumatic Symptoms Among Children and Youth, Turner et al. (2019) (Cross-sectional survey)	Boston, MA, Philadelphia, PA and rural areas (n = 630), Children ages 2–17 years	Witnessing gun violence: (1) Saw someone threaten another person with a gun, (2) saw someone hurt another person with a gun on purpose, and (3) saw someone shooting a gun in a public place (on the streets, parking lots, or stores); Hearing gun violence: (1) heard (but not seen) a gun being shot in a public place like the streets, parking lots, or stores	Child victimization, polyvictimization, and posttraumatic symptoms	Direct gun violence exposure, witnessing gun violence and hearing gunshots were significantly associated with other forms of violence victimization. Polyvictimization was strongly associated with posttraumatic symptoms in older youth (10–17 years old) and hearing and witnessing gun violence were related to both posttraumatic symptoms among younger children (2–9 years old)	Although rates of indirect exposure to gun violence are increasingly available, there is less evidence on its psychological impacts. Limited research has not distinguished exposure to gun violence from other forms of violence and types of gun violence exposure	Mental health professionals and trauma-informed services should be aware that indirect gun violence exposure (hearing and seeing gun violence in their neighborhoods) can induce posttraumatic stress symptoms in children	+
Gun Violence, African Ancestry, and Asthma: A Case-Control Study in Puerto Rican Children, Rosas-Salazar et al. (2016) (Case–control)	San Juan, PR and Hartford, CT (n = 472), Racially & ethnically diverse male juvenile offenders 14–19 years old at baseline	Hearing gunshots more than once	Asthma and total IgE	Gun violence exposure moderates the relationship between African ancestry and asthma and atopy in Puerto Rican children	Mechanisms underpinning the association between exposure to violence and asthma are poorly understood	Further longitudinal research into the impact of African ancestry and frequent gun violence exposure and asthma among children from racial and ethnic minorities	++

Table 1 (continued)

Title, Author(s), Year (Design)	Location (sample size), demographics	Violence exposure conceptualization	Outcome(s)	Relevant findings	Gaps in Research	Recommendations	Quality appraisal score*
Gun- and Non-Gun-Related Violence Exposure and Risk for Subsequent Gun Carrying Among Male Juvenile Offenders, Beardslee et al. (2018) (Longitudinal)	Maricopa, AZ and Philadelphia, PA (n = 1170), 1–15 year old children and youth	Being shot or shot at, or seen others being shot or shot at	Gun carrying	Adolescent offenders were more likely to carry a gun after exposure to gun violence but not after exposure to non-gun violence	Longitudinal analyses have not isolated within-individua changes in violence exposure and gun carrying across multiple assessments and few studies have focused specifically on male adolescents with a history of serious criminal offense or controlled for pre-existing and time-stable factors that account for associations between violence exposure and gun carrying	Gun violence prevention interventions should target adolescent boys who experience or witness gun violence and those in communities with high rates of gun violence	++
Invisible wounds: Community exposure to gun homicides and adolescents' mental health and behavioral outcomes, Leibbrand et al. (2020)(Geospatial analysis)	20 large cities across U.S. (n = 2823), 9–12 Grade students	Physical distance from adolescents' home or school addresses to gun homicide	Externalizing behavior problems, anxiety, and depression	Proximity to gun homicide was associated with significantly worse depression and anxiety symptoms among girls and anxiety symptoms among boys	Lack of studies on the impact of gun violence on community member who witness or hear about it, and whether effects vary by individual characteristics such as gender or proximity to event	There is a need to consider the ways in which local gun violence impacts adolescents' short- and long-term mental health beyond those physically impacted	++

Table 1 (continued)

Title, Author(s), Year (Design)	Location (sample size), demographics	Violence exposure conceptualization	Outcome(s)	Relevant findings	Gaps in Research	Recommendations	Quality appraisal score*
Understanding the Impact of Seeing Gun Violence and Hearing Gunshots in Public Places: Findings From the Youth Firearm Risk and Safety Study, Mitchell et al. (2019) (Cross-sectional Survey)	Boston, MA, Philadelphia, PA and rural areas of eastern TN (n=630), Children	Directly seeing gun violence and hearing gunshots in public places such as streets, parking lots, or stores	Protective action, level of fear, sadness and upset resulting from indirect gun violence exposure	Forty-one percent of youth reported indirect exposure to gun violence, with 32% reporting exposure in the last year. Older and urban youth had higher levels of exposure than younger and non-urban youth. Most were exposed from hearing gunshots, whereas fewer saw gun violence in public places. Almost 60% of youth reported being very or extremely distressed (fear, anger, sadness, and generalized upset) due to indirect gun violence, with youth living in urban areas, females and younger children being more likely to experience high fear	Included only English-speaking participants and indirect gun violence exposure survey questions measure different experiences and are thus, not meant to be read as a scale	High rates of indirect exposure to gun violence point toward need for community-level programs to promote youth safety and well-being, such as educational programs for youth who have seen gun violence or heard gunshots. Bolstering children's sense of safety and reducing community disorder may buffer the impacts of gun violence exposure	+

*Each study underwent a quality appraisal using an adapted checklist from the National Institute for Health and Care Excellence. The composite scores are defined as follows:

++ All or most of the checklist criteria have been fulfilled, where they have not been fulfilled the conclusions are very unlikely to alter

+ Some of the checklist criteria have been fulfilled, where they have not been fulfilled, or not adequately described, the conclusions are unlikely to alter

− Few or no checklist criteria have been fulfilled and the conclusions are likely or very likely to alter

findings (Opara et al., 2020). Additionally, three review papers (Garbarino et al., 2002; Kennedy & Ceballo, 2014; Rajan et al., 2019) and two commentaries (Furman, 2018; Schaechter & Alvarez, 2016) were excluded from this specific study, but used to frame the present analyses.

Major themes emerging from the reviewed studies span (1) inconsistent operationalization of community gun violence exposure, (2) indirect exposure to gun violence as distinct from other forms of violence, (3) the need for a multidimensional conceptualization of exposure to community gun violence, (4) differential impacts of community gun violence exposure across developmental stages, and (5) how community gun violence exposure among youth contributes to future perpetration and perpetuates cycles of violence. The quality of studies was fairly good, with almost all studies fulfilling most or all of the criteria (Table 2). The dimensions where the reviewed studies scored lower were largely due to non-probability sampling limiting the generalizability of study findings, lack of confounder adjustment, and self-reported outcome measures leading to potential bias. Most studies selected explanatory variables based on theoretical basis, included adequate sample sizes and representative demographic makeups, and used appropriate analytical methods. All but one study were scored as internally valid, while more than a third lacked external validity because of non-probability sampling methods or non-generalizable population samples (e.g., male juvenile offenders, homeless young adults).

Inconsistent Operationalization of Community Gun Violence

The set of experiences which comprise exposure to community gun violence remain ill-defined, limiting the utility of the growing body of evidence in this area (Kennedy & Ceballo, 2014). In terms of indirect exposure, there appear to be three broad categories examined throughout the literature. First, much of the research on indirect exposure to gun violence is concerned with experiences of seeing gunfire or hearing gunshots (Mitchell et al., 2019; Turner et al., 2019), although a substantial portion is limited solely to experiences of witnessing gunfire (Bingenheimer, 2005; Hsu et al., 2020; Wamser-Nanney et al., 2019). A smaller number of studies consider knowing a close friend or family member who has been shot or carries a firearm to be indirect exposures to gun violence (Quimby et al., 2018). A study of low-income Latinx and Black adults not included in this review found that having a victim of gun violence in one's social network exacerbates mental health symptoms (Smith et al., 2020). Another supplemental study found that youth exposure to family/friend homicide was significantly related to trauma symptoms and poly-victimization (Turner et al., 2018). Knowing a victim of gun violence is often considered

a separate exposure from witnessing, as the cognitive and emotional implications following exposure may be vastly different. Cognitive processing of the event is moderated by the child's age, extent of knowledge, relational proximity to the victim, and outcome of the incident (e.g., injury or death).

Few studies have explored the impact of a child's general awareness of and perception of gun violence in their neighborhood. Perceptions of neighborhood safety may be an important mediator between exposure to community violence and developmental outcomes in youth (Overstreet & Mazza, 2003). Some even posit that the mere presence of a gun induces distress, exacting a psychological toll over time by disrupting youth's sense of safety (Quimby et al., 2018). A study of low-income Black youth in Chicago found that merely knowing someone who owns a gun was associated with both internalizing (anxiety, depression, and trauma) and externalizing (aggression and delinquency) symptoms (Quimby et al., 2018). The same study found that gun exposure (not gun violence) was related to reduced trauma symptoms in girls. This outcome is hypothesized to be a result of a regained sense of control that the presence of guns may offer girls who often lack other forms of power (Quimby et al., 2018). The idea that merely hearing about community gun violence may be detrimental to youth mental health is in line with the criterion for a PTSD diagnosis, which emphasizes *perceived* threat of a traumatic event and subjective distress rather than direct experiences (Kennedy & Ceballo, 2014).

The broad scope of experiences that may fall under the umbrella of gun violence exposure has yet to be fully considered throughout the literature. For example, the impacts of indirect exposure to unintentional and self-inflicted gun violence (i.e., suicide) on youth are rarely discussed. Only one of the reviewed studies looked at the experience of being threatened with a firearm (McGee et al., 2017). While gun violence perpetration has been linked to negative psychosocial outcomes, these associations remain tenuous given the extensive overlap between gun violence victims and perpetrators (Hsu et al., 2020). Moreover, youth who are indirectly exposed to community gun violence often also experience domestic violence and child maltreatment due to shared contextual factors (Turner et al., 2019). A persistent methodological challenge in distinguishing community from domestic violence is that both often stem from disputes between family or friends (Scott, 1999). Nonetheless, witnessing violence has been more strongly linked to violence perpetration, while victimization is correlated with emotional dysregulation and poor social adjustment (Guerra et al., 2003; Schwartz & Proctor, 2000). Figure 2 integrates definitions across the literature to develop a typology of gun violence exposure ranging from direct victimization to community exposure.
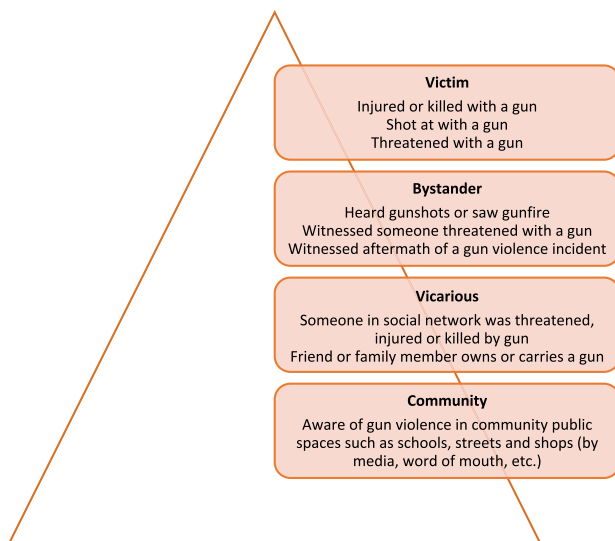
Fig. 2 A typology of gun violence exposure. Categories may overlap (e.g., encountered a crime scene where a known person was injured or killed with a gun)

Firearm Violence: A Distinctly Lethal Threat

Although numerous studies use measures of community violence exposure that distinguish between subscales of personal victimization and witnessing violence, many existing studies fail to differentiate between exposure to *gun-related* violence as opposed to other forms of violence (Turner et al., 2019). Firearms are one of the only weapons designed specifically to threaten, seriously injure, or kill. Consequently, violence with a gun represents a more profound threat to safety than that with other weapons. Additionally, gun violence exposure is especially far-reaching relative to other forms of violence, with gunshots resounding across multiple street blocks, making indirect exposure a particularly pertinent public health concern. The nature of community gun violence is such that a child does not need to be directly exposed to suffer its consequences. Rather, gun violence ripples across a child's ecosystem, through a diminished sense of safety, parental distress, and broader neighborhood disorder (Kim, 2019; Sharkey et al., 2012).

Community violence poses a lower immediate threat than direct adverse experiences such as domestic violence and physical/sexual abuse. However, community violence involving a firearm likely triggers a stronger stress response and disturbance to physiological regulatory systems given its level of threat relative to other weapons (McLaughlin & Sheridan, 2016). An analysis among a national sample of adolescents demonstrated that those who feared injury or death during an incident were about 9 times more likely to

develop depressive symptoms (Zinzow et al., 2009). The same study found that witnessing community violence with a weapon was associated with an increased risk of depression. A study using data from the National Crime Victimization Survey found that victims of violence involving a firearm were more likely to experience severe distress and functional impairment than victims of non-firearm-related violence (Kagawa et al., 2020). Another study of urban preschoolers found that handling of parents' firearms in the home was correlated with more aggressive behavior, suggesting gun exposure may contribute to externalizing behaviors from a young age (Hardy et al., 1996). Indirect exposure to gun violence conceivably leads to greater perceived threat appraisal among youth due to the erratic nature of gunfire and high risk of injury or death if exposed.

Dimensions of Gun Violence Exposure

Recent research into the conceptualization of community violence exposure has raised the importance of more precisely defining and studying the various dimensions of exposure and their moderating effects on psychosocial outcomes. The child maltreatment and trauma fields overall have shown that dimensions of exposure, such as frequency, degree of injury, chronicity, and age at first report contribute to notable differences in developmental outcomes. This more nuanced conceptualization has led to a more precise understanding of the moderators and consequences of different traumatic events. The body of literature on community violence exposure, and more specifically indirect gun violence exposure, has lagged in this theoretical shift. Emerging evidence supports the integration of the following dimensions of community violence exposure into its conceptualization: type, severity, physical proximity, relational proximity, and chronicity of exposure (Kennedy & Ceballo, 2014). These dimensions interact with each other and with individual, familial, and community-level characteristics to produce a distinct set of symptoms among exposed youth.

Urban youth are often exposed to many different forms of violence, with about 3 in 5 children experiencing either physical assault, sexual victimization, maltreatment, property victimization, or witnessing violence (Finkelhor et al., 2015). The contextual factors of violence exposure lead to variable outcomes among youth, with certain forms of violence being associated with distinct psychopathologies. However, questions remain as to whether to classify severity on some 'objective' scale of intensity (i.e., getting shot is worse than seeing someone get shot) or based on the *perceived* severity of exposure. Whether a child feared for their own life, that of a loved one, or witnessed gunfire among

strangers may be critically important to their interpretation and cognitive appraisal of the event.

Studies of community violence exposure have demonstrated that physical proximity to violence moderates the severity of resulting symptoms (Scott, 1999). For example, a study of a sniper attack in a schoolyard, predating this review's timeframe, revealed that children directly victimized experienced the most severe symptoms, followed by children present in the schoolyard, children who heard gunfire from the classrooms, and finally, those who were not present but had knowledge of the event (Pynoos, 1987). Similarly, a more recent geospatial analysis of community exposure to gun violence in 20 large cities across the U.S. found that geographical proximity between gun homicides and adolescents' homes or schools was associated with significantly worse anxiety and depression symptoms (Leibbrand et al., 2020).

In addition to physical proximity, relational proximity has been shown to moderate the effects of community gun violence exposure on children and adolescents. Youth who witness known individuals being shot are at higher risk of experiencing traumatic symptoms as a result. Among a national household probability sample of adolescents, those who witnessed incidents of violence involving a known, as opposed to unknown, non-relative were more likely to develop PTSD (Zinzow et al., 2009). Furthermore, studies examining the effects of network exposure to gun homicides demonstrate that relational proximity is significantly linked to gun violence victimization (Papachristos & Wildeman, 2014). These findings indicate the importance of accounting for relationship to victim when determining the severity and potential consequences of exposure to gun violence among youth. For example, a multivariate analysis of gun violence among urban youth used a scoring system to account for level of exposure, with higher scores indicating a closer relationship to the victim (McGee et al., 2017).

The lack of longitudinal studies documenting the effects of cumulative exposure to local gun violence represents an important gap in the literature (Beardslee et al., 2018; Lee et al., 2020; McGee et al., 2017). Longitudinal data would allow for better differentiation between the effects of acute versus accumulated exposures. For example, externalizing outcomes predominantly observed among exposed adolescents, as opposed to younger children, are hypothesized to be a result of desensitization to violence over time (Bingenheimer, 2005; Quimby et al., 2018). Youth repeatedly exposed over long periods of time are prone to developing normative beliefs about community violence and subsequently, maladaptive cognitive frameworks and coping behaviors. Furthermore, most youth impacted by gun violence are chronically exposed throughout their childhoods warranting more longitudinal research among high-risk populations (Scott, 1999).

Gun Violence Exposure across Developmental Stages

The effects of exposure to local gun violence on youth vary substantially across developmental stages. Although younger children may experience more profound psychological disturbances, older children are at higher risk of experiencing cumulative exposures over time. Research suggests that younger children exhibit more internalizing symptoms (i.e., depression, anxiety, and/or intrusive thoughts) while older youth present externalizing behaviors (i.e., aggression) as coping mechanisms or ways to regain control over an environment they perceive as unsafe (Quimby et al., 2018).

Traumatic experiences, including witnessing violence, during early childhood interfere with healthy development, and can induce affective, somatic, behavioral, cognitive, relational, and self-esteem-related impairments (Berkowitz, 2003; van der Kolk, 2005). Although infants and toddlers cannot yet conceptualize firearms and do not necessarily grasp their lethality, they are likely to mimic their parent's or caregiver's response and are therefore, more adversely impacted by parental distress (Al'Uqdah et al., 2015; Schaechter & Alvarez, 2016). For example, a study of 160 children 0 to 11 years old found that maternal distress partially explained the relationship between community violence exposure and behavioral (internalizing and externalizing) problems in children, when controlling for socioeconomic status and family aggression (Linares et al., 2001). Recently, attention has been drawn toward the role of maladaptive learning, particularly fear conditioning, in producing psychopathologies in children exposed to adversity early on (McLaughlin & Sheridan, 2016).

Young children may also somaticize emotional distress resulting from exposure to violence. A study of 268 African-American 6–8-year-old children in the Detroit area found that witnessing community violence predicted PTSD symptoms, difficulty sleeping and headaches while controlling for depression, anxiety, child abuse, domestic violence, and life stress (Bailey et al., 2005). A systematic review assessing literature on the link between exposure to community violence and physical health outcomes found the strongest evidence for positive associations with cardiovascular (blood pressure) and sleep outcomes (Wright et al., 2016). The potential mechanisms underlying the association between gun violence exposure and physiological outcomes include dysregulation of the hypothalamic-pituitary-adrenocortical axis induced by psychosocial distress (Rosas-Salazar et al., 2016). Furthermore, threat appraisal (how youth evaluate

what is "at stake" in a situation) has been proposed as an important mediating, and in some cases moderating, process determining the impact of gun violence exposure on physiological outcomes (Wright et al., 2016).

It is hypothesized that youth who become desensitized to community violence over time are, in turn, more likely to experience externalizing problems (Kennedy & Ceballo, 2014). According to *pathological adaptation theory*, youth who are chronically exposed to community violence develop coping mechanisms that minimize psychological distress but increase propensity for aggressive behavior (Ng-Mak et al., 2004). Violence victimization is strongly linked to violence perpetration among adolescents (Guerra et al., 2003). Qualitative studies elucidate the mental models that youth develop as a result of chronic exposure to community violence (Opara et al., 2020). This environment often cultivates a sense of helplessness and even fatalism in adolescents, which, when paired with poor executive functioning, can lead to greater risk-taking and create added vulnerabilities for marginalized youth, including anger, dissociation, post-traumatic stress symptoms, and aggression (Quimby et al., 2018). Despite this pattern of desensitization and ensuing externalizing behavior, adolescents may still experience internalizing symptoms following exposure. Among a national sample of adolescents who witnessed community violence, the prevalence of PTSD and major depressive episodes was estimated at 7% and 11%, respectively (Zinzow et al., 2009).

Vulnerable Populations and Cycles of Violence

A public health approach demands not only the mitigation of the direct impacts of community gun violence on youth, but also strategic efforts toward addressing its root causes. This requires acknowledging, for example, that gun violence perpetrators are frequently also victims of gun violence, both comprising overlapping subpopulations placed at risk (Abt, 2019; Hsu et al., 2020). Among youth considered "delinquent", incidents of exposure to community violence are often indistinguishable from violence involvement and deviant peer affiliation. However, research suggests that adolescent behavior in and of itself is not considered a risk factor for exposure. Rather, family and neighborhood context determine adolescents' violence involvement, both as perpetrators and victims (Halliday-Boykins & Graham, 2001). Youth often carry guns as a response to prior exposure to violence and a subsequently low sense of safety in their community (McGee et al., 2017). Gun violence perpetration on its own is, however, considered a risk factor for psychosocial sequelae (Hsu et al., 2020).

Discussion

Exposure to community gun violence is a serious, yet far less understood threat to youth development and wellbeing. Its impacts on youth are far-reaching, however the full scope of exposure is difficult to measure for many reasons. The studies reviewed in this work reveal that there is a wide variation in how researchers define and categorize exposure to gun violence, ranging from solely considering direct victimization to accounting for exposure to gun violence through media or news outlets. The mere awareness of gun violence in one's neighborhood is credibly harmful in and of itself, as youth are forced to constantly cope with its looming threat. This review found that while there is a plethora of evidence on the effects of community violence on children and adolescents, only a small portion of the literature focuses exclusively on the impacts of indirect exposure to violence involving a firearm. The limited evidence available suggests that gun violence hampers healthy brain development among children in ways distinct from other forms of violence, resulting in both short- and long-term psychological and physiological sequalae. Lastly, there is a need to integrate models of poly-victimization and acknowledge that exposure to community gun violence is often one of many stressors impacting the well-being of low-income youth of color (Gorman-Smith & Tolan, 1998).

Improving the detection and assessment of exposure to community gun violence is a critical first step in addressing its effects (Voisin, 2007). Recent work has been done to develop comprehensive gun violence risk tools. For example, preliminary results from a prospective cohort study demonstrate that the SaFETy (Serious fighting, Friend weapon carrying, community Environment, and firearm Threats) screening tool is a feasible and valid method of identifying youth at high risk of future firearm violence (victimization, perpetration, firearm injury, or firearm death) (Goldstick et al., 2017). The questionnaire includes items assessing interpersonal violence, community violence exposure, mental health, substance use, and peer influence. A related methodological challenge in collecting this data is parents' demonstrated propensity toward underestimating children's exposure to violence (Zimmerman & Pogarsky, 2011). Researchers attribute this to trivialization of child-reported events, inadequate supervision, child desensitization to violence, and poor parent–child communication due to mistrust or fear of parental restrictions of autonomy. Further research should aim to develop gun violence risk tools that capture a range of gun violence exposures from knowing a friend who carries a gun to being directly threatened or injured, along with the relevant dimensions of exposure. Risk stratification would allow for a more targeted and efficient allocation of resources toward those youth most at risk

of gun violence exposure, victimization, and perpetration. Moreover, documenting the various dimensions of community gun violence may offer insight into the spaces where youth are at highest risk of exposure and interventions that might be most effective.

The nation's current reckoning with systemic racism highlights the importance of positioning community gun violence within a context of structural inequity. Geospatial analyses have shed light on the intersecting social determinants of community gun violence. For example, an analysis of neighborhood trauma in Syracuse, New York revealed a syndemic of lead exposure, community violence, and poor academic outcomes (Lane et al., 2017). Public health researchers and practitioners are tasked with identifying the ways in which community violence is rooted in systemic oppression, and anti-Black policies such as segregation, police brutality, and income inequality (Quimby et al., 2018; Roundtable on Population Health Improvement et al., 2017). Neighborhood crime is often used as a justification for continued neglect and disinvestment in low-income communities of color, contrasting with the massive mobilization of recent years to prevent gun violence impacting more affluent, White children. Community gun violence feeds into cycles of violence by demoralizing youth, inducing gun carrying, and fostering community disorder. The body of evidence proposes that depersonalized exposure is more likely to increase aggression and lead to future gun violence perpetration than more direct forms of gun violence victimization.

Protective factors and patterns of resilience that buffer the negative impacts of exposure to chronic community violence among youth are equally important (Kennedy & Ceballo, 2014). Identified domains of resilience include personal resources (e.g., temperament, social skills, and social engagement), family support, and community buffers (e.g., teachers, neighbors, institutional structures) (Copeland-Linder et al., 2010). Interventions that bolster resilience across ecologies, including parental monitoring, community cohesion, school safety, youth empowerment, and access to mental health services, may reduce distress associated with exposure to community gun violence (Luthar & Goldstein, 2004). Given the interconnectedness of gun violence with other issues such as poverty and domestic violence, wrap-around services within schools can be particularly effective in supporting exposed youth (Copeland-Linder et al., 2010). Family-based counseling that aims to strengthen emotional cohesion and enhance monitoring is a promising intervention to reduce the psychological trauma resulting from exposure to community gun violence (Luthar & Goldstein, 2004). School-based screening and group therapy—most often Cognitive Behavioral Therapy—is the most common intervention employed to reduce internalizing symptoms among youth after exposure to community gun violence (Ali-Saleh Darawshy et al., 2020; Saltzman et al., 2001;

Stein, et al., 2003a, 2003b). Youth living in neighborhoods with high rates of gun violence should be routinely screened for exposure in schools and health care facilities and referred to social and psychological services accordingly. This is particularly important to identify students who may be exhibiting externalizing behaviors such as aggression and mislabeled as "problematic"(Voisin & Berringer, 2015). While there are many interventions to mitigate the psychological sequelae resulting from exposure to community gun violence, very few aim to reduce exposure by preventing community violence (Ali-Saleh Darawshy et al., 2020). Moreover, these interventions are often not made readily available to the communities who need these interventions the most. Notably, a classroom-based intervention implemented in two Colombian cities with high rates of community violence was effective in curbing aggression among elementary school students through social-emotional learning, parent workshops, home visits, and extracurricular peer groups (Chaux et al. 2017). Violence prevention curricula and community-based deterrence programs in addition to systemic reforms (e.g., criminal justice, gun accessibility, poverty alleviation) have the potential to reduce exposure and interrupt cycles of violence among youth although more rigorous research is needed to establish these relationships (Ali-Saleh Darawshy et al., 2020).

This review has limitations that warrant consideration when interpreting the study's findings. While selection criteria were limited to studies focused on urban youth, it is important to note that rural youth are also exposed to gun violence, though perhaps through different modalities. This review excluded additional forms of gun violence, including suicide, accidental shootings, and police violence, all of which have distinct, yet significant, impacts on exposed youth. This review was limited to studies of populations in the U.S., which has a unique gun violence landscape due to the second amendment, lobbying by interest groups, the proliferation and cultural symbolism of firearms, and its hyper-politicization at the national level. Studies that explored exposure to community violence and may have touched on gun violence as one of many exposures were excluded from the formal review. Furthermore, studies that do not include indirect exposure to gun violence as a separate exposure and where indirect exposure may have been analyzed in conjunction with direct victimization were excluded, thus limiting the body of knowledge from which conclusions were drawn. Finally, this review did not include studies predating 1995 and thus, could have missed important data published beforehand.

This review notes patterns of methodologic limitations across the emerging body of research on indirect exposure to community gun violence among youth. First, most of the reviewed studies rely on self-reported measures of community gun violence exposure, subjecting the findings to

recall and other similar forms of bias. In the case of young children, parents are often asked to report experiences with exposure to gun violence on their child's behalf, which can result in underreporting. Secondly, a large portion of the community gun violence literature is cross-sectional in nature, limiting the potential to draw causal links between community gun violence exposure and health outcomes among children and youth. This limitation is particularly pertinent in terms of linking exposure to gun violence with developmental outcomes and documenting the impact of cumulative exposure into adolescence. Additionally, no qualitative studies exploring youth perspectives on exposure to community gun violence and its effects on their sense of safety and wellbeing were found. Qualitative data are needed to better understand the lived experiences of children and youth who are exposed to community gun violence and how it may impact their perceptions of violence, day-to-day functioning, and sense of wellbeing. Lastly, a persistent methodological challenge in establishing causal links between exposure to community gun violence and health outcomes is in controlling for all potential confounders as the broader social ecosystem is a strong determinant of both neighborhood violence and health status.

Conclusion

While the field of research on gun violence has benefited recently from increased funding and public attention, the public health implications of youth exposure to community gun violence remain poorly understood. Beyond reviewing the emerging body of evidence, this review brings attention to the far-reaching impact of indirect exposure to gun violence among youth and puts forth a novel conceptualization of community gun violence that contributes to propelling the field forward in examining this critical, yet understudied issue. This review found that exposure to community gun violence is prevalent among low-income youth of color in the U.S., disrupts cognitive, emotional and social development, and contributes to ongoing community violence. These findings point toward the importance of expanding the scope of violence exposure assessments and distinguishing between different forms of violence exposure. A theoretical shift within the community gun violence literature is warranted, from broad definitions of exposure to a more nuanced, multi-dimensional conceptualization which accounts for type, severity, physical proximity, relational proximity, and chronicity. In order to build a comprehensive understanding of the scope, risk factors and consequences of community gun violence, each of its relevant constructs must be systematically defined and potential moderating characteristics accounted for. Intrapersonal and contextual factors that merit further investigation include childhood traumas, ongoing stressors, and coping resources. Future literature and corresponding analyses should differentiate between types of exposures, integrate a more exhaustive examination of the contextual factors surrounding gun violence and consider their implications for primary prevention strategies.

Appendix

See Table 2.

Table 2 Quality appraisal table depicting scores across various dimensions of quality of studies included in review

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system, setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking –)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
A multivariate analysis of gun violence among urban youth: The impact of direct victimization, indirect victimization, and victimization among peers, McGee et al. (2017) (Cross-sectional survey)	++	++	– (Sample is not generalizable to population given sampling method used)	NA	++	NA	++	++	++	++	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Childhood Trauma Exposure and Gun Violence Risk Factors among Victims of Gun Violence, Wamser-Nanney et al. (2019) (Cross-sectional survey)	++	++	++	NA	++	NA	+ (Confounders found to be unrelated to outcomes and excluded from analysis; other forms of trauma were not assessed)	++	++	+	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Community Gun Violence as a Social Determinant of Elementary School Achievement, Bergen-Cico et al. (2018) (Geospatial mapping)	++	++	++	NA	++	NA	− (No confounder adjustment)	++	++	++	++	NA	++

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the eligible population or area well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Effects of exposure to violence with a weapon during adolescence on adult hypertension, Ford & Browning, (2014) (Longitudinal)	++	++	++	NA	++	NA	++	++	+(Potential for measurement error and different sampling frames and composition for blood pressure collection)	++	++	++	+(Blood pressure change not assessed; cumulative violence exposure during childhood and adolescence not captured)

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Exposure to gun violence and asthma among children in Puerto Rico, Ramratnam et al. (2015) (Case–control)	++	++	++	++	++	+ (Self-reported violence exposure)	++	+ (Children in Puerto Rico)	++	++	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Firearm Violence Exposure and Serious Violent Behavior, Bigenheimer, Brennan and Earls (2005) (Longitudinal)	++	++	+ (Differential attrition)	+	++	+ (Self-reported violence exposure may have led to misclassification)	++	++	+ (Self-reported outcome variables)	++	++	++	++ (5-year follow-up period)

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Firearm Violence Exposure and Suicidal Ideation Among Young Adults Experiencing Homelessness, Hsu et al. (2020) (Cross-sectional survey)	++	++	++	NA	++	NA	++	++	+ (Computer-assisted self-administered survey)	++	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Gun Exposure Among Black American Youth Residing in Low-Income Urban Environments, Quimby et al. (2018) (Cross-sectional survey)	++	++	++	NA	++	NA	+ (Only controlled for neighborhood stressors)	++	+ (Self-reported outcomes measures)	+ (Low reponse frequency)	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Gun Violence Exposure and Posttraumatic Symptoms Among Children and Youth, Turner et al. (2019) (Cross-sectional survey)	++	+ (Not a probability based sample)	++	NA	++	NA	++	++	+ (Self-administered survey; parents reported violence exposure for younger children which may have led to under or overreporting)	++	NR	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Gun Violence, African Ancestry, and Asthma: A Case–Control Study in Puerto Rican Children, Rosas-Salazar et al. (2016) (Case–control)	++	++	++	+ (Potential for selection bias)	++	NA	++	+ (Puerto Rican Children)	+ (Child's self-reported exposure to gun violence)	++	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the eligible population or area well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Gun- and Non-Gun-Related Violence Exposure and Risk for Subsequent Gun Carrying Among Male Juvenile Offenders, Beardslee et al. (2018) (Longitudinal)	++	++	+ (Limited data on participant recruitment)	NA	++	NA	++	++	++	++ (Sample retention was relatively high)	++	++	++ (3 year follow-up period)

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised? How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Invisible wounds: Community exposure to gun homicides and adolescents' mental health and behavioral outcomes, Leibbrand et al. (2020) (Geospatial analysis)	++	+ (Over-sampled unmarried parents)	++	++	++	NA	++	++	+ (Some measures were youth-reported and some parent-reported)	+ (Missing data; differential attrition)	++ (Self-reported measures with high reliability & validity)	NA	+ (6-year gap between outcome measurements)

Table 2 (continued)

Title, Author(s), Year (Design)	1.1 Is the source population or source area well described? Was the country (e.g. developed or non-developed, type of health care system), setting (primary schools, community centres etc.), location (urban, rural), population demographics etc. adequately described?	1.2 Is the eligible population or area representative of the source population or area? Was the recruitment of individuals, clusters or areas well defined (e.g. advertisement, birth register)? Was the eligible population representative of the source? Were important groups underrepresented?	1.3 Do the selected participants or areas represent the eligible population or area? Was the method of selection of participants from the eligible population well described? What % of selected individuals or clusters agreed to participate? Were there any sources of bias? Were the inclusion or exclusion criteria explicit and appropriate?	2.1 Selection of exposure (and comparison) group. How was selection bias minimised?	2.2 Was the selection of explanatory variables based on a sound theoretical basis? How sound was the theoretical basis for selecting the explanatory variables?	2.3 Was the contamination acceptably low? Did any in the comparison group receive the exposure? If so, was it sufficient to cause important bias?	2.4 How well were likely confounding factors identified and controlled? Were there likely to be other confounding factors not considered or appropriately adjusted for? Was this sufficient to cause important bias?	2.5 Is the setting applicable to the US? Did the setting differ significantly from the US?	3.1 Were the outcome measures and procedures reliable? Were outcome measures subjective or objective (e.g. biochemically validated nicotine levels ++ vs self-reported smoking −)? How reliable were outcome measures (e.g. inter- or intra-rater reliability scores)? Was there any indication that measures had been validated (e.g. validated against a gold standard measure or assessed for content validity)?	3.2 Were the outcome measurements complete? Were all or most of the study participants who met the defined study outcome definitions likely to have been identified?	3.3 Were all the important outcomes assessed? Were all the important benefits and harms assessed? Was it possible to determine the overall balance of benefits and harms of the intervention versus comparison?	3.4 Was there a similar follow-up time in exposure and comparison groups? If groups are followed for different lengths of time, then more events are likely to occur in the group followed-up for longer distorting the comparison Analyses can be adjusted to allow for differences in length of follow-up (e.g. using person-years)	3.5 Was follow-up time meaningful? Was follow-up long enough to assess long-term benefits and harms? Was it too long, e.g. participants lost to follow-up?
Understanding the Impact of Seeing Gun Violence and Hearing Gunshots in Public Places: Findings From the Youth Firearm Risk and Safety Study, Mitchell et al. (2019) (Cross-sectional Survey)	++	++	++	NA	++	NA	− (No confounder adjustment)	++	+ (Parent and child-reported measures)	++	++	NA	NA

Table 2 (continued)

Title, Author(s), Year (Design)	4.1 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (i.e. it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	4.2 Were multiple explanatory variables considered in the analyses? Were there sufficient explanatory variables considered in the analysis?	4.3 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for?	4.6 Was the precision of association given or calculable? Is association meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CIs wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	5.1 Are the study results internally valid (i.e. unbiased? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design?	5.2 Are the findings generalisable to the source population (i.e. externally valid)? Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications	Composite score
A multivariate analysis of gun violence among urban youth: The impact of direct victimization, indirect victimization, and victimization among peers, McGee et al. (2017) (Cross-sectional survey)	++	++	++	++	++	+ (Study findings are not generalisable because of sampling method used)	++
Childhood Trauma Exposure and Gun Violence Risk Factors among Victims of Gun Violence, Wamser-Nanney et al. (2019) (Cross-sectional survey)	− (Small sample size; insufficient power)	++	++	+ (Underpowered study;serves as preliminary exploratory analysis)	+ (Potential covariates found to be unassociated with outcomes variables and thus not inlcuded in analysis)	++	+
Community Gun Violence as a Social Determinant of Elementary School Achievement, Bergen-Cico et al. (2018) (Geospatial mapping)	− (Small sample size)	−	++	++	− (Potential confounders not controlled for)	++	+

Table 2 (continued)

Title, Author(s), Year (Design)	4.1 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (i.e. it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	4.2 Were multiple explanatory variables considered in the analyses? Were there sufficient explanatory variables considered in the analysis?	4.3 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for?	4.6 Was the precision of association given or calculable? Is association meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CIs wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	5.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design?	5.2 Are the findings generalisable to the source population (i.e. externally valid)? Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications	Composite score
Effects of exposure to violence with a weapon during adolescence on adult hypertension, Ford & Browning (2014) (Longitudinal)	++	++	++	++	++	++	++
Exposure to gun violence and asthma among children in Puerto Rico, Ramratnam et al. (2015) (Case–control)	++	++	++	++	++	+	++
Firearm Violence Exposure and Serious Violent Behavior, Bigenheimer, Brennan and Earls (2005) (Longitudinal)	++	++	++	++	++	++	++
Firearm Violence Exposure and Suicidal Ideation Among Young Adults Experiencing Homelessness, Hsu et al. (2020) (Cross-sectional survey)	++	++	++	++	++	++	++

Table 2 (continued)

Title, Author(s), Year (Design)	4.1 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (i.e. it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	4.2 Were multiple explanatory variables considered in the analyses? Were there sufficient explanatory variables considered in the analysis?	4.3 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for?	4.6 Was the precision of association given or calculable? Is association meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CIs wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	5.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design?	5.2 Are the findings generalisable to the source population (i.e. externally valid)? Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications	Composite score
Gun Exposure Among Black American Youth Residing in Low-Income Urban Environments, Quimby et al. (2018) (Cross-sectional survey)	NR	++	++	+	++	+ (Sample characteristics differed from state averages)	++
Gun Violence Exposure and Posttraumatic Symptoms Among Children and Youth, Turner et al. (2019) (Cross-sectional survey)	NR (small sub-sample sizes)	++	++	++	++	+ (Not based on probability samples)	+
Gun Violence, African Ancestry, and Asthma: A Case–Control Study in Puerto Rican Children, Rosas-Salazar et al. (2016) (Case–control)	++	++	++	++	++	++	++
Gun- and Non-Gun-Related Violence Exposure and Risk for Subsequent Gun Carrying Among Male Juvenile Offenders, Beardslee et al. (2018) (Longitudinal)	++	++	++	++	++	++ (Limited to male juvenile offenders)	++

Table 2 (continued)

Title, Author(s), Year (Design)	4.1 Was the study sufficiently powered to detect an intervention effect (if one exists)? A power of 0.8 (i.e. it is likely to see an effect of a given size if one exists, 80% of the time) is the conventionally accepted standard Is a power calculation presented? If not, what is the expected effect size? Is the sample size adequate?	4.2 Were multiple explanatory variables considered in the analyses? Were there sufficient explanatory variables considered in the analysis?	4.3 Were the analytical methods appropriate? Were important differences in follow-up time and likely confounders adjusted for?	4.6 Was the precision of association given or calculable? Is association meaningful? Were confidence intervals or p values for effect estimates given or possible to calculate? Were CIs wide or were they sufficiently precise to aid decision-making? If precision is lacking, is this because the study is under-powered?	5.1 Are the study results internally valid (i.e. unbiased)? How well did the study minimise sources of bias (i.e. adjusting for potential confounders)? Were there significant flaws in the study design?	5.2 Are the findings generalisable to the source population (i.e. externally valid)? Are there sufficient details given about the study to determine if the findings are generalisable to the source population? Consider: participants, interventions and comparisons, outcomes, resource and policy implications	Composite score
Invisible wounds: Community exposure to gun homicides and adolescents' mental health and behavioral outcomes, Leibbrand et al. (2020) (Geospatial analysis)	++	++	++	++	++	++	++
Understanding the Impact of Seeing Gun Violence and Hearing Gunshots in Public Places: Findings From the Youth Firearm Risk and Safety Study, Mitchell et al. (2019) (Cross-sectional Survey)	++	++	++	++	++	+ (Small subgroup sample sizes)	+

Not applicable (NA) Should be reserved for those study design aspects that are not applicable given the study design under review (for example, allocation concealment would not be applicable for case–control studies)

Each study is then awarded an overall study quality grading for internal validity (IV) and a separate one for external validity (EV):

++ All or most of the checklist criteria have been fulfilled, where they have not been fulfilled the conclusions are very unlikely to alter

+ Some of the checklist criteria have been fulfilled, where they have not been fulfilled, or not adequately described, the conclusions are unlikely to alter

– Few or no checklist criteria have been fulfilled and the conclusions are likely or very likely to alter

Declarations

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