

## ANALYTICAL ESSAY

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# Exposure to Violence as Explanatory Variable: Meaning, Measurement, and Theoretical Implications of Different Indicators

ŞULE YAYLACI

*University of Pennsylvania, USA*

AND

CHRISTOPHER G. PRICE

*University of Wisconsin–Madison, USA*

The study of intra-state violence has been a main focus of scholars since the end of the Cold War, and in recent years particular attention has been paid to the consequences of civil wars on future political, social, and economic development. Yet, understanding the consequences of political violence requires a clear working definition of what we mean when we say that someone was “exposed to” or was “a victim of” violence. Researchers use disparate measures ranging from living in a country that is categorized as a civil war case, despite living hundreds of miles away from areas of conflict, to being displaced and losing most of one’s family members in attacks. In this essay, we offer conceptual clarification for various forms of victimization and indirect forms of exposure, present examples of works using these different measurement strategies, and examine how different measures affect findings using a sample of articles. We conclude with recommendations about indicators researchers can choose from and suggest that future research should probe further into the use of subjective measures of exposure.

El estudio de la violencia intraestatal ha sido uno de los principales focos de atención de los académicos desde el final de la Guerra Fría, y, en los últimos años, se ha prestado especial atención a las consecuencias de las guerras civiles en el futuro desarrollo político, social y económico. Sin embargo, comprender las consecuencias de la violencia política requiere una definición clara de lo que queremos decir cuando afirmamos que alguien ha estado «expuesto» a la violencia o ha sido «víctima» de la misma. Los investigadores utilizan medidas dispares que van desde el hecho de vivir en un país catalogado como caso de guerra civil, aunque se viva a cientos de kilómetros de las zonas de conflicto, hasta ser desplazado y perder a la mayoría de los miembros de la familia en ataques. En este ensayo, ofrecemos una aclaración conceptual de las diversas formas de victimización y de las formas indirectas de exposición, presentamos ejemplos de trabajos que utilizan estas diferentes estrategias de medición y analizamos cómo afectan las diferentes medidas de los hallazgos utilizando una selección de

artículos. Concluimos con recomendaciones sobre los indicadores que los investigadores pueden elegir y sugerimos que las investigaciones futuras profundicen en el uso de medidas subjetivas de exposición.

L'étude de la violence intraétatique constitue la principale préoccupation des chercheurs depuis la fin de la guerre froide. Ces dernières années, une attention particulière a été accordée aux conséquences des guerres civiles sur le développement politique, social et économique. Pourtant, la compréhension des conséquences de la violence politique nécessite une définition de travail claire de ce que l'on entend derrière quelque'un « d'exposé à des » ou qui a été « la victime de » violences. Les chercheurs emploient des mesures différentes, qui vont du fait de vivre dans un pays classé comme un cas de guerre civile, bien que ce soit à des centaines de kilomètres des zones de conflit, au déplacement et à la perte d'une grande partie de sa famille dans des attaques. Dans cet article, nous proposons une clarification conceptuelle de diverses formes de victimisation et de formes indirectes d'exposition, présentons des exemples de travaux utilisant ces différentes stratégies de mesure et analysons les effets du recours à différentes mesures sur les résultats à l'aide d'un échantillon d'articles. Nous concluons sur des recommandations concernant les indicateurs pouvant être choisis par les chercheurs avant de suggérer que les travaux de recherches ultérieurs s'intéressent plus avant à l'utilisation de mesures subjectives de l'exposition.

**Keywords:** civil war, exposure to violence, victimization, direct and indirect exposure, contextual exposure

**Palabras clave:** Guerra civil, exposición a la violencia, victimización, exposición directa e indirecta, exposición contextual

**Mots clés:** guerre civile, exposition à la violence, victimisation, exposition directe et indirecte, exposition contextuelle

## Introduction

The past two decades have seen a dramatic expansion in research on civil wars. This research addresses a number of questions, including the causes (e.g., [Fearon and Laitin 2003](#)), dynamics (e.g., [Christia 2012](#); [Balcells 2017](#)), and termination of conflict (e.g., [Walter 1997](#)). A particularly vibrant area in this line of research is examining the consequences of conflict on social, political, and economic development (see [Davenport et al. 2019](#) for a review). While the consequences of civil wars on social capital and development prospects may be less destructive than originally expected, systematizing findings across cases and thus understanding the effects of war has been difficult ([Bauer et al. 2016](#), 250–51; [Price and Yaylaci 2021](#), 297–300). Although there are less tractable methodological and ethical challenges in this line of research ([Davenport and Ball 2002](#), 446–48; [Wood 2006](#), 373–74; [Subotić 2020](#), 7–9), an addressable issue is the very different ways in which authors conceptualize and measure exposure to violence. Indicators range from living in a country that is categorized as a civil war case, despite living hundreds of miles away from areas of conflict, to being displaced and losing most of one's family members in attacks. This diversity poses a problem for scholars seeking to understand the effects of exposure to violence.

Given the diversity among indicators of exposure to violence, we ask: What are the conceptual differences between different measures of victimization? Is being injured more traumatic than witnessing a killing? How about experiencing multiple forms of victimization? Individuals who were incarcerated due to some affinity with the rebellion also lost their homes and family members, experienced torture and

injury, and witnessed others being killed. How can researchers factor in intensity of exposure? Do findings vary based on the way authors operationalize victimization? What can researchers do to deal with inevitable issues of measurement error? How should researchers and practitioners best take these differences into account when measuring exposure to violence? Answering these questions is an important contribution not only to the advancement of conflict scholarship, but also to using this scholarship to provide policy advice for peace-building efforts.

We start by clarifying the concepts of exposure to violence and victimization, distinguishing between direct and indirect exposure as well as individual-level and contextual exposure. Next, we discuss alternative methods of operationalizing these concepts as well as challenges to matching these with theory. Using a sample of articles on the effects of exposure to violence on prosocial outcomes in the civil war literature, we examine how sensitive these findings are to different indicators.<sup>1</sup>

Our review provides three insights and recommendations. First, differences in how authors operationalize victimization may be critical in explaining divergent findings. We suggest opting for using multiple indicators or an index over single indicators as a way to resolve this issue. Second, personal and collective exposure to violence imply different mechanisms, helping to explain why alternative indicators provide different results. We urge scholars to match their choice of indicator for measuring exposure to violence with the causal mechanisms they propose in their theory. Third, using subjective measures of insecurity to tap personal perceptions of threat is an important and promising approach that future studies should incorporate, particularly when trying to integrate the insights from qualitative research with quantitative findings. We believe that these recommendations will improve our understanding of what civil wars leave behind, thus helping design better policy interventions for transitional justice and enduring peace in post-conflict societies.

### Exposure to Violence and Victimization

Understanding the consequences of political violence, and specifically the political effects of civil war violence, requires a clear working definition of what it means to “be exposed to” or “be a victim of” violence.

Exposure to violence is a complex concept as civil war violence varies in form and intensity, across geographies and groups, even in the same conflict (Kalyvas 2006, 202–209; Balcells 2017, 5–7; Gutiérrez-Sanín and Wood 2017, 20–22). As an example, imagine three individuals exposed to the same violent incident in different ways. The first individual was at the location of the attack and injured, the second lives in the same city but in a different location than the attacks, and the third one lives 500 kilometers away from the origin of attack but watches it on television. While all three individuals were “exposed” to this event, how do we account for the very different nature of their experiences? The first individual was directly and personally victimized by the attack, the second one was affected only by vicarious threat, and the third individual was exposed to the attack solely via news. Do these differences in how these three individuals experienced the incident predict how their social and political behaviors will change in the future?

Often, the terms “direct” and “indirect” exposure are used to differentiate between varying individual-level experiences with violence, yet there is little consensus in the conflict literature as to where the boundaries between direct and indirect exposure lie. Here, bringing in victimization as a concept can help us distinguish between different types of exposure. Victimization is defined as the intentional act or threat of physical or psychological harm (Zimmerman and Posick 2016, 178), where physical harm could be in the form of death, disappearance, injury, rape,

<sup>1</sup> By social consequences, we broadly refer to social capital, prosocial behaviors such as altruism and cooperation, and trust, tolerance, and attitudes toward peace.

Table 1. Forms of exposure to violence

Direct		Indirect	
Individual level		Aggregate level	
Primary victimization		Secondary victimization	Contextual exposure
Personal victimization (e.g., bodily harm, displacement)	Material victimization (e.g., damage to property or livelihood)	For example, victimization of family members, friends, or acquaintances	For example, residing in a place that was bombed at the time of the explosion

displacement, or illness, and psychological harm includes effects such as post-traumatic stress disorder (PTSD), as well as changes in cognition or conditioned behavior as a result of witnessing or vicariously experiencing effects of violence.

Direct exposure, known as primary victimization in social psychology, is sometimes used only to refer to effects of violence on the self, by personal victimization (McAllister 2004, 138; Werner 2016, 3; although others use more expansive definitions of direct violence, such as Lyall, Blair, and Imai 2013, 687). Primary victimization can also be further divided between physical and property victimization, such as damages to one's home or livelihood (e.g., Hartinger-Saunders et al. 2011, 5), a distinction we see in studies on the effects of civil war violence (e.g., Becchetti, Conzo, and Romeo 2014, 288).<sup>2</sup>

People living in a locality that was subject to a violent attack may not have been personally victimized but they may know someone who was. Indirect forms of exposure, also sometimes called secondary victimization, refer to the effects of violent acts on those who are socially connected to victims, whether as family members, friends, neighbors, or coworkers (Ruback and Thompson 2001, 134; Condry 2010, 219–22; Schmid and Muldoon 2015, 78).<sup>3</sup>

Individuals may also be indirectly exposed to violence via the “context” they live in. Even if individuals themselves are not socially connected to the affected people, they may experience second-hand trauma by learning about the experiences of others (see Miller and Izzo 2010), a form of indirect exposure that we refer to as contextual exposure. Contextual exposure is a vicarious experience that can have threat and trauma implications for the individual and is more of an aggregate-level concept. Table 1 summarizes these different but not mutually exclusive forms of exposure to violence.

We should note that inasmuch as victimization as a concept taps direct and indirect experiences of conflict extending from being “exposed to violence” for the most part, people's interpretations of “being a victim” may go beyond these strict definitions. For example, Cairns et al.'s (2003) comparison of experiences of conflict and perceived victimhood in Northern Ireland shows that a large number of people did not actually have the direct or indirect experiences of the conflict yet claim to be victims. On the other end, many who do have these experiences did not consider themselves as victims. Social connotations associated with victimhood and material and immaterial costs and benefits of victimhood may also play a role

<sup>2</sup>In this essay, our focus is on civilian victimization, but we recognize that there are not hard boundaries between civilians and armed groups, and that armed group members are also victims of conflict (Krystalli 2021).

<sup>3</sup>Please note that the terms “direct” and “indirect” are used to refer to different notions than Balcells (2011, 399–400) who discusses direct and indirect violence against civilians by combatants. By directness of violence, Balcells refers to production of violence and degree of intimacy between combatant and victim. We use “direct” to refer to personal exposure to violence.

(Ferguson, Burgess, and Hollywood 2010, 878–79). Krystalli (2021, 129) adds that “an array of actors—state agencies and their officials, human rights NGOs and their reports, international organizations and their consultants, researchers and their questions, victims’ associations and their advocacy—continually construct, contest, accord, and perform the category of ‘victim.’”<sup>4</sup> While perceived victimhood may not fully overlap with formal definitions, these definitions are essential for precision in measurement.

*Measuring Victimization and Exposure to Violence*

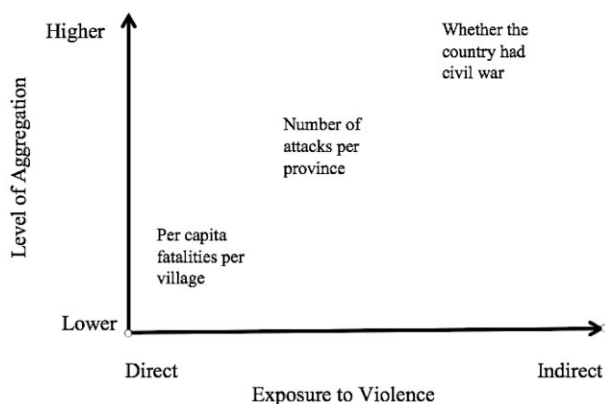
The severity of conflict experiences varies significantly, and so do attempts to measure these experiences. In conflict studies, indicators of victimization can range from an answer to single questions such as “have you or family been injured in the conflict?” (e.g., Aguilar, Balcells, and Cebolla-Boado 2011, 1406; Grosjean 2014, 434; Hazlett 2020, 850; Werner and Lambsdorff 2020, 865) to indices combining further types of victimization. The single question “have you or your family been injured in the conflict?” is an oft-used instrument and taps some forms of both primary and secondary victimization, rendering it difficult to distinguish the effects of being personally victimized and victimization by loss of a loved one. It also focuses on a single dimension of physical “injury” in victimization, but victimization can take several forms as discussed above such as displacement or psychological trauma. The complexity of victimhood demands more complex instruments such as indices.

Many victimization indices contain only information about direct (primary) victimization, with some focusing solely on physical and psychological victimization of household members (e.g., Voors et al. 2012, 945), and others adding the material costs of conflict, such as the loss of property and livelihood (e.g., Kunovich and Hodson 1999, 655; Cassar, Grosjean, and Whitt 2013, 298; Vélez et al. 2016, 18). Some indices also include secondary effects such as the loss of family or friends (e.g., Bellows and Miguel 2009, 1148; Mironova and Whitt 2018, 756; also see Bauer et al. 2014, 49–50; Mironova and Whitt 2016, 658).

Measures of contextual exposure are often aggregated indicators, such as the sum of fatalities or events within a specified geographic or political unit. Country case studies looking at subnational variation may aggregate at the local level, ranging from small units of aggregation such as neighborhood, village, or district to much larger units such as a province or state. Some time-series cross-sectional survey studies aggregate these results at the country level, often using binary variables indicating presence of war (Miguel, Saiegh, and Satyanath 2011, 64; Hutchison 2014, 807; Peffley, Hutchison, and Shamir 2015, 822; Tir and Singh 2015, 485). These works consider exposure to civil war as tantamount to being from a country that suffered from civil war.

Scholars may choose to work with aggregate indicators of exposure to violence as a proxy for individual-level exposure. This may be because they do not have access to individual-level data or that they believe that asking individuals to “judge” whether they were victimized by answering a set of questions may be *subjective* as they are self-reported, memory-based, and retrospective (Child and Nikolova 2020, 152). To attain a more *objective* indicator, some scholars opt for using an instrument that matches place of residence with georeferenced data on attacks (e.g., Getmansky and Zeitzoff 2014; Barceló 2021). While such “objective” indicators are appealing as aggregated data seem more reliable, it is not necessarily the case; as we discuss below, data on attacks or fatalities from the same context may be incompatible. Aggregated data also come with the cost of missing important individual-level variations in exposure.

<sup>4</sup> See Bar-Tal et al. (2009) for a detailed discussion on perceived collective victimhood, and Ibáñez and Velásquez (2009) for difficulty of identifying victims in the example of forced displaced households.



**Figure 1.** Level of aggregation and exposure to violence.

*Notes:* This graph plots a rough sketch of examples for how higher levels of aggregation likely capture more indirect effects of violence assuming that level of victimization is held constant. Lower levels of aggregation (e.g., village) are most likely to capture direct forms of exposure to violence. However, when aggregated at higher levels, the chances that a high number of fatalities or events mean that each household was directly exposed to violence in some form decrease. The purpose here is to show how alternative aggregate indicators correlate on the spectrum of directness of exposure to violence.

With regard to the link between aggregated indicators and experiences of conflict, we contend that there is something of a linear relationship between the level of aggregation and the directness of individual-level exposure, as shown in figure 1. For a given country, at lower levels of aggregation (i.e., the smaller the second-level unit above individual), higher values indicate higher chances of individuals being directly victimized. However, when aggregation is done at a higher level, chances of higher values of exposure capturing direct exposure for any individual go down.

For example, if the number of attacks counted for a village of a few hundred residents is high, it means that personal loss is likely to be pervasive for the residents of that village. In contrast, if the same number of attacks were counted at the provincial level of a few thousand residents spread over a few hundred miles, it is harder to make presumptions about direct effects on the residents in that province. Assuming that these would be the same runs the risk of committing the ecological fallacy, unless violence is evenly distributed over a given territory. This is essential to bear in mind as contextual exposure likely impinges on society and politics through different causal mechanisms, a point we expand on later in this essay.

The review so far encapsulates theoretically important and methodologically imposed differences in experiences captured by the catch-all term of “exposure to violence.” The extent to which these experiences are tapped by individual-level and contextual-level indicators of exposure to violence brings about questions on validity. Below, we unravel the connections between experiences with violence and the instruments used in empirical works. Variations in the operationalization of exposure to violence may also impinge on the empirical outcomes. We ask: Are findings sensitive to different ways to operationalize exposure to violence?

As a first step to answer this question, we searched for published work in the civil war literature that focuses on the effect of violence, and as an outcome variable we picked prosocial outcomes. To this end, we looked closely at papers that employ alternative instruments to operationalize exposure to violence for the same outcome variable. Some of these studies use different individual-level indicators of exposure to violence, such as direct or indirect exposure as discussed above. Others employ



individual and contextual indicators of violence within the same study, often as robustness checks. We leverage these different operationalization strategies—direct and indirect, individual and contextual—to make “within-study comparisons” and graphically present how these different indicators change findings. We also choose some studies that look at the effect of violence exposure on the same outcome variable in different country cases.<sup>5</sup> Having coded their indicators of exposure to violence by level of aggregation—individual or contextual level, we group these studies by the outcome variable to observe how exposure at different levels changes the findings. Then, we briefly discuss studies that explicitly focus on different contextual indicators of exposure to violence. Taken together, these comparisons shed light on how different indicators of violence—whether direct or indirect, at different levels of aggregation, and based on different contexts—affect findings and what the implications are for researchers’ choice of operationalization. In this endeavor, our goal is to find comparable examples for illustrative purposes, rather than creating an exhaustive list of research that looks at the impact of exposure to violence on prosocial outcomes.

#### *Comparison of Different Victimization Indicators*

Victimization takes many forms, and different personal experiences with violence, whether direct, indirect, or a compounded experience—more than one form of exposure—would seem likely to result in different attitudinal and behavioral effects on individuals (e.g., [Werner 2016](#), 3). Many of the victimization indices attempt to capture effects of PTSD, a result of exposure to traumatic stressors, in particular personal victimization ([Letica-Crepulja et al. 2011](#), 710). Besides personal victimization, a variety of experiences such as displacement, torture, death, or disappearance of close family members or friends are all known to be potential causes of traumatic stress syndromes ([Husain et al. 2011](#), 527–29; [Matheson et al. 2019](#), 2; also see [Wilson and Raphael 2013](#)).

Which of these experiences is more traumatic? Some argue that injury is more traumatic than material losses ([Becchetti, Conzo, and Romeo 2014](#), 288; [Hazlett 2020](#), 850; [Werner and Lambsdorff 2020](#), 865), because injury is a direct threat to life while property losses are recoverable, or that injuries have lasting impact, especially if they cause disability or lasting scars.<sup>6</sup> While at first sight this comparison between direct personal harm and material losses may seem sound—consider the difference between losing a limb and having a single bullet damage your house—there are challenges to this view. Material losses can threaten one’s life, and material losses and bodily harm are not necessarily mutually exclusive.<sup>7</sup> As an example of the former, consider this testimony from a lawyer in Syria:

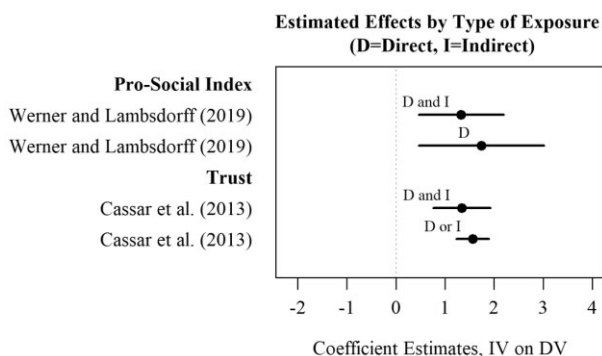
On April 24, they laid siege to Daraa city. [. . .] Soldiers would raid houses and spill the cooking oil on the floor. They’d spoil the food that people had stored in their winter pantries so they had nothing to eat. They’d shoot at water tanks so there was no water left. ([Pearlman 2017](#), 101).

Residents of these homes suffer from lack of access to food, and displaced peoples carry a heavy mental burden. In many occasions, when houses are raided, people are also hurt. Pearlman writes that “[c]ountless Palestinian families were forced to watch as Israeli troops stormed their homes in the middle of the night, ransacked

<sup>5</sup> If a paper used an outcome variable that was not used by any other paper with a clear focus on the effect of exposure to violence, we did not use it in our comparisons. The works we present as examples are a subset of a much wider universe of articles on exposure to civil war violence.

<sup>6</sup> The appeal of precision when focusing on direct physical harm in the form of injury as a measure of victimization is also noted ([Hazlett 2020](#)).

<sup>7</sup> We would like to thank reviewer 2 for bringing this point to our attention.



**Figure 2.** Effects of direct and indirect exposure to violence, same study.

*Notes:* This figure plots a sample of within-study comparisons of the effect sizes of different victimization indicators on various outcome variables in the civil war literature. In Werner and Lambsdorff's study, D (direct exposure) is operationalized by being personally injured. Direct and indirect exposure (D and I) is operationalized as individuals who were injured or had a family member injured. The coefficients come from table D.5 in Supplementary Material in Werner and Lambsdorff's article. In Cassar, Grosjean, and Whitt's study, the comparison was between direct or indirect, and direct and indirect, where the latter captures a more intense form of victimization. We are using the coefficients of the interaction term for the same village reported in panel A, model 4 on page 302. The dependent variable in Cassar, Grosjean, and Whitt's study is the amount sent in a trust game.

their belongings, and beat their relatives bloody" (Pearlman 2003, 39). Material damages often come with additional physical and psychological harm.

Beyond the challenges of comparing the trauma of bodily harm to material losses, other issues present themselves when we consider the gamut of wartime experiences. Is death of a family member or displacement the most impactful factor in one's postwar attitudes and behavior? Is witnessing death of a third party more traumatic than getting injured? Answers are difficult to come by partly because there are not sufficient systematic empirical analyses and partly because the effects are complex, contingent on dynamics and repertoire of violence as well as context, which renders it difficult to compare the effect of one form of victimization to the other.

In most accounts of violence, war survivors are shown to experience more than one wartime event as mentioned above, and these effects are likely to accumulate (Schaal and Elbert 2006, 99–103; Husain et al. 2011, 527–29; also see DePaul 1999; King and Meernik 2017). For example, during the Second Intifada, a Palestinian woman born in 1933 reported the loss of a cousin, incarceration of her child, and living in fear day by day for her and her family members' lives (Pearlman 2003, 12–13). In intractable conflicts, such experiences are indeed common. Research also shows that personal victimization and indirect forms of exposure (whether witnessing or secondary victimization) are correlated (Zimmerman and Posick 2016, 182). Figure 2 shows how the compounding of various types of individual-level exposure to violence within the same study changes the magnitude of estimated coefficients; the more forms of trauma an individual is exposed to, the stronger and more adverse seem to be the effects. For example, Cassar, Grosjean, and Whitt (2013, 302, 305) report the difference between experiencing either injury or loss of a family member and experiencing both, and they find that those who experience both lose more trust in their fellow villagers than those exposed to only one form.

Precisely because of the vagueness of boundaries between direct and indirect exposure and the fact that many, if not most, respondents experience more than one form of trauma, many scholars opt for indices (e.g., Voors et al. 2012, 945;

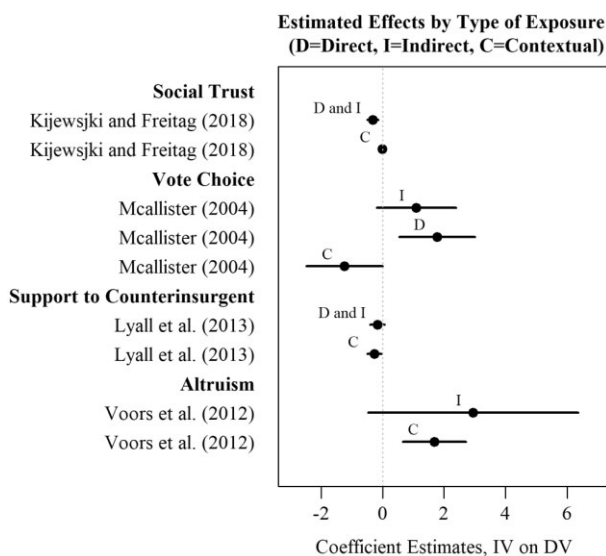


Silva and Mace 2014, 2; Mironova and Whitt 2016, 662; Kijewski and Freitag 2018, 724). Indices have a number of advantages. Indexing allows a cumulative measure of war experiences, which is often calculated by addition of the number of different experiences, sidestepping issues with assigning weights to certain experiences that can raise a number of questions. Indices can also be used to collect discrete responses in sensitive contexts. For example, using an index that asks “how many of the following occurred,” rather than directly asking about specific types of personal victimization in post-war contexts where the provision of aid may depend on giving the “right” answer or where certain types of victimization may violate social norms (Wood 2006, 383; Cohen and Hoover Green 2012, 446), helps researchers identify the distribution of specific types of violence (Traummüller, Kijewski, and Freitag 2019, 2016). Moreover, using general indices that ask “how many of these experiences apply to you” avoids asking questions about specific forms of violence that may seem to beg a detailed follow-up from a respondent, raising ethical concerns over the possibility of re-traumatization. In works that focus on more general questions on the effects of violence, (e.g., “How do civil war experiences shape social trust in Kosovo?” Kijewski and Freitag 2018, 717), using an index may be the best strategy. While this approach does not obviate the need for researchers using in-person survey or interview techniques to take measures to address the risks of re-traumatization, this approach would appear to generate less need for respondents to identify or explain specific types of victimization.

Yet, for research where the question may demand information on a specific type of victimization (e.g., what are the effects of being abducted into the Lord’s Resistance Army? Blattman 2009, 235), understanding the effects of distinct types and frequency of violence may be crucial notwithstanding the challenges of getting a thorough account of all these experiences and disentangling the effect of each. For researchers conflicted between focusing on the effects of specific types of victimization and wider wartime experiences, the best strategy may be to have a list of questions that can be used to construct indices as well as examining specific types of victimization. We provide an example of this type of list while discussing challenges with efficient methods of measurement in the Discussion section.

#### *Comparison of Individual and Contextual Effects*

Does direct exposure via injury or other forms of primary victimization affect people differently than contextual exposure, such as living in a locality that had a violent attack? Figure 3 shows that while individual and contextual exposures tend to have similar effects in terms of direction (sign of coefficients), there are differences in the magnitude of effects (coefficients), even within the same research study. A study on social trust in Kosovo points to a divergence between the effects of individual and contextual exposure: experiencing victimization, either directly or indirectly through personal harm or harm to family members and close friends, diminishes social trust robustly, while contextual exposure as operationalized by the number of events causing loss of life in a municipality less so (Kijewski and Freitag 2018, 731). McAllister (2004, 138) finds that contextual exposure to political violence increases the Catholic vote for Sinn Féin, the party historically associated with the Irish Republican Army (IRA) in Northern Ireland, while individual-level exposure (injury to friends or family) has the opposite effect (see figure 3). Lyall, Blair, and Imai (2013, 703) examine the effects of violent incidents committed by both US-led security forces and the Taliban on civilian support for these actors and find that individuals report slightly less willingness to cooperate with counterinsurgents if they were living in a community (contextual) where a violent incident by counterinsurgents took place than if they were directly victimized. In Uganda, Voors et al. (2012, 952) find that both household victimization (operationalized with an index of experiences, including death) and community-level exposure (operationalized as the share of

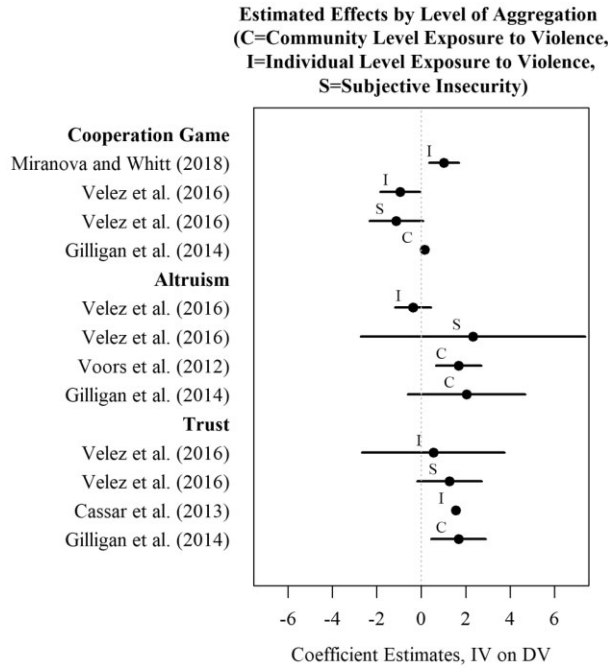


**Figure 3.** Effects of exposure to violence by type of exposure, within the same study.

*Notes:* This figure plots comparison of the effects (coefficients) of individual-level versus contextual exposure within the same study, and the works chosen are from the civil war literature and not an exhaustive list. In individual-level exposure, we distinguish between direct and indirect forms. Direct exposure (D) refers to primary victimization—effects of violent acts on the self—including bodily injury or material losses. Indirect exposure (I) refers to secondary victimization—the effects of violent acts on those who are socially connected to victims, whether as family members, friends, neighbors, or coworkers. Contextual exposure (C) refers to violence in the locality individuals live in such as municipality, village, or city. Where authors mix the type of exposure measured, we use “and” to indicate all types of exposure being measured. Social trust is operationalized by differences in the belief that someone would return a wallet with its contents; vote choice refers to Catholic vote for Sinn Féin; support for counterinsurgents is willingness to support international security forces in Afghanistan on a Likert scale; and altruism is operationalized with a dictator game where the receiving group’s identity is unknown.

war-related deaths in a community) have the same positive effect on altruism toward neighbors. Given the small size of many of these communities in both the cases of Afghanistan and Uganda, the individual-level and contextual exposure indicators are likely closely linked, and it is likely that in these studies they capture the same effect, especially in communities with a high share of war-related deaths. Similarly, Nussio, Rettberg, and Ugarriza (2015, 346) found no difference between “victims”—those who are directly affected by war—and “nonvictims” in Colombian Civil War with respect to their attitudes toward transitional justice mechanisms. As possible explanations, the politics of being categorized as victim/nonvictim that we discuss above and the proximity between victims and perpetrators are considered. Just as likely is that “nonvictims,” although not directly affected, may have been indirectly exposed to violence (e.g., lost family members) or contextual effects from living through war may have caused significant trauma.

Figure 3 shows that individual-level exposure (D and I) tends to have wider error margins, which may imply differences in individuals’ subjective interpretation of the same type of victimization experiences, something we discuss below. Given a mix of statistical models, the figure does not allow for a direct comparison of effect sizes between studies but does highlight that even in studies using the same design and methods, there are differences in effect. These differences lend credence to the



**Figure 4.** Effects of exposure to violence on prosocial behavior, same outcome variable across different studies.

*Notes:* This figure plots a comparison of different studies with the same outcome variable using similar measurement tools in operationalizing the dependent variable. We focus on cooperation, trust, and altruism as they tap the latent variable of prosocial behavior. Coefficients from Velez’s and Gilligan, Pasquale, and Samii’s studies are rescaled via linear transformations for presentation purposes; the relationship is now between exposure to violence and trust game outcomes (10s and 100s in local currency returned, respectively). In this graph, only studies that utilized behavioral games are presented for ease of comparison. For altruism, here we are only reporting coefficients from studies that measured altruism broadly, not parochial altruism, with a dictator game where the identity of the other group is unknown. Voors et al. is an exception who used social choice experiments, similar to a dictator game. The indicator we report for trust is the amount sent in trust games. For space economy, we only included the coefficients that are most pertinent to the main argument in each study, and if there are multiple dependent variables (such as in trust games—amount sent and received) and if the results are the same for all, we just used the first one authors present. Velez et al.’s study controls for subjective insecurity at the individual level (marked as S), so some of the effects of victimization are absorbed by that.

idea that the theoretical mechanisms differ between different types of exposure, which is an important aspect of measurement we expand on in discussion below.

Besides within-study comparisons between the effects of individual-level and contextual exposure, we can also see how findings differ between studies analyzing the same outcome variable but operationalizing exposure at different levels of analysis. Figure 4 shows a sample of such studies, and again we can see that contextual exposure has distinct effects from individual-level exposure, whether direct or indirect, and tends to depict a more positive picture on the social consequences of civil war violence than individual-level exposure.

The first cluster in figure 4 presents three studies that used public good games to measure the effect of civil war on “cooperation” with others for a common

goal—an essential prosocial behavior. They report contrasting findings for personal victimization and contextual exposure: Vélez et al. (2016, 6) and Mironova and Whitt (2018, 758) show that individual-level wartime victimization undermines cooperation in Colombia and Kosovo, respectively, while Gilligan, Pasquale, and Samii (2014, 608), using community-level exposure to conflict in Nepal, find that communities affected by violence see increased cooperation. We may infer that while directly victimized people may be less trusting (Kunovich and Hodson 1999, 657–58; Cassar, Grosjean, and Whitt 2013, 301; Rohner, Thoenig, and Zilibotti 2013, 230–31; Becchetti, Conzo, and Romeo 2014, 294) and thus less cooperative, they are often in the minority and may not be driving the average community effects. In contrast, the majority of individuals who are exposed to violence indirectly through contextual effects may grow to be more cooperative, given the same shared experiences.

Figure 4 shows the differences in reported coefficients from other studies that look at altruism and trust using the same measurement tools, and they reveal similar patterns. While altruism and trust seem to be positively affected by community-level exposure, the effect of individual-level exposure on these prosocial behaviors tends to be more negative.

Similar divergent results can be observed in studies on the effect of exposure to violence on political participation. In Uganda, Blattman (2009, 236) finds that those who were abducted by the Lord's Resistance Army are significantly more likely to vote and be a community mobilizer than peers who were not. Looking at the same case but with a sample of ordinary citizens, De Luca and Verpoorten (2015a, 119–20) use district-level event counts as an indicator of contextual exposure. They find a statistically significant increase in local informal political participation at the individual level, but not for turning out to vote in national elections. They explain the discrepancy with reference to the loss of confidence in formal, but not communal institutions. The difference may be due to the sample of abductees in Blattman's paper. Processes of armed group socialization, a critical part of the combatant experience, might explain why we see positive growth in his study, yet the mixed result in De Luca and Verpoorten's study (2015b, 672) of the same case, which includes victims who were not socialized into the armed group (also see Daly 2018). The contrast between De Luca and Verpoorten's and Blattman's finding about the effect of exposure to violence on voting in part speaks to differences between contextual and individual-level exposure, as well as differences between individual-level experiences of violence.

Regarding the differences between individual-level exposure and contextual exposure, another important point concerns the subjectivity and objectivity of the measures. Subjectivity in the self-reports of being exposed to violence may pose a challenge to reliability of the indicators as discussed above. Such accounts can have recall bias, and recent research shows that "measurement error in the retrospective victimization measure" may be correlated with some of the outcome measures such as civic engagement and trust (Child and Nikolova 2020, 153). Thus, objective indicators, often in the form of georeferenced data on attacks or casualties, may be appealing to sidestep such measurement errors. Objective indicators are by nature aggregated, and they can be problematic, especially if the goal is to connect individual experiences with individual-level outcomes. Individuals from the same bombed province may have utterly distinct experiences, which generates measurement noise due to within-unit variance in these indicators.

Child and Nikolova (2020, 156–57) compared subjective and objective measures of victimization during World War II (WWII) and found that while the former is associated with more positive effect on social attitudes, the latter is associated with more negative effects. They argue that endogenous misreporting may be the reason why subjective measures do not show similar effects to objective measures. While this finding may cast some shadow on studies using survey-based measures of

victimization, we are cautious to make any strong inferences from this comparison. We are of the view that the findings of Child and Nikolova may itself also be a by-product of subjective victimization indicators. The authors use a binary response to the question of “whether the respondent, or any of his/her parents or grandparents, were physically injured or killed during World War II.” The objective measure comes from data on conflict during WWII at the electoral district or some other local administrative division level (primary sampling unit), and the measure itself is a sum of attacks and operations in the respective unit. As we discuss above, binary responses fail to capture the nature and severity of individual-level exposure. This question further suffers from not being able to capture the other forms of indirect exposure or secondary victimization, casting shadow on the validity of measure. Perhaps more importantly, it is looking at an interstate war at a global scale, which is hardly comparable to intergroup conflict oft-observed in civil wars.

While the concerns about recall bias are serious, we are not ardent proponents of using what may be regarded as more “objective” indicators for reliability purposes, especially given issues we raise below about inconsistencies of these objective indicators. In an inquiry about microlevel effects of violence, indicators of individual-level exposure should fare better using a list that gauges a wider range of experiences for accuracy than “objective” ones, a point we cover further in the Discussion section.

*Comparison of Different Aggregate Indicators and Notes on Measuring Contextual Exposure*

Aggregated indicators of exposure can take many forms, such as fatalities or event counts, and can illuminate contextual effects of exposure to violence. For example, three studies from Israel on violent attacks and support for right-wing parties share a common dependent variable and allow us to see how the level of aggregation matters. Berrebi and Klor (2006, 899–925) use annual death counts at the national level, Berrebi and Klor (2008, 280, 286) use terror fatalities at the locality level, and Getmansky and Zeitsoff (2014, 590, 596) use whether a given community is within Palestinian rocket range. Importantly, all three studies report increase in right-wing party support, despite the differences in levels of aggregation and measurement tools. We also see that contextual exposure, whether measured with fatalities in Israel or event counts in Colombia, increases support for peace negotiations (Gould and Klor 2010, 1466; Tellez 2019, 1064). Sensitivity analysis in other studies on civil war violence further shows that decisions on the use of aggregated event counts rather than aggregated fatalities do not fundamentally alter the results (Rohner, Thoenig, and Zilibotti 2013, 231; De Luca and Verpoorten 2015b, 675).

Terrorism studies report slightly different findings, depending on whether authors use fatalities or event counts (e.g., Gassebner, Jong-A-Pin, and Mierau 2008; Young 2019).<sup>8</sup> Young (2019, 332) shows that using fatalities as an indicator of terrorism does not yield as many significant results as using a count of attacks. Peffley, Hutchison, and Shamir (2015, 828) echo Young and report that the effect of fatalities is smaller than the effect of the count of terrorist attacks on political tolerance. Attacks may engender fear, and even in the absence of fatalities they may lead to behavioral or attitudinal change. Studies show that direct exposure is not necessary for feeling threatened in the cases of national trauma (e.g., Bleich, Gelkopf, and Solomon 2003, 618–20). Those who live outside of the location of attacks may be exposed to the news of terrorism through media and may experience the trauma vicariously (Davis and Macdonald 2004, 68; Balcells and Torrats-Espinosa 2018, 10625).

It is also worth pointing out that aggregated indicators of exposure may have drastically different counts of both events and fatalities. Kalyvas (2006, 38–41) notes that most studies of civil war suffer from a bias toward urban areas, a problem

<sup>8</sup> We are referring to terrorism literature because many insurgencies deploy terrorist tactics and some insurgent attacks are considered as acts of terrorism (Stanton 2013).



exacerbated by the challenges of navigating contested zones during wartime. While many authors focus on fatalities, considered to be the easiest indicator of violence to verify in civil war, even these numbers vary widely and are subject to both over- and undercounting (see Krause 2013; Krüger et al. 2013). For instance, Davenport and Ball (2002, 441) show that there is an order of magnitude difference in the number of events reported by human rights organizations and newspapers reporting on violence during the Guatemalan Genocide. They note that each of these organizations reports based on its expected audience, with newspapers focusing on violence in urban areas but not rural ones, human rights organizations underreporting single fatality incidents while capturing multifatality incidents, and interviews showing more recent events due to recency bias (Davenport and Ball 2002, 428). While there are representative samples of lethal violence in some cases, such as Spiegel and Salama's (2000, 2205–2206) epidemiological work estimating fatalities in Kosovo during the 1999 war, it often takes years of comparison and multiple systems' estimation across different datasets to verify these findings (Krüger and Ball 2014, 6; Spagat 2014, 1).

Beyond the impact of violence on reporting during war, there are issues of motivated recall which occur after conflict. Individual understandings of why violence occurred are subject to framing processes, where the meaning of an event is altered due to the opinions of others (Shesterinina 2016, 417). Given the rise in criminal culpability for wartime violence, armed actors have incentives to obscure their responsibility or downplay the magnitude of violence during wartime. And where post-war orders are dominated by one side, civilians may avoid challenging dominant narratives or report types of violence they believe make them more likely to gain access to needed post-war help (Cohen and Hoover Green 2012, 454).

When looking at contextual exposure, researchers should also be wary about the type of fatalities, particularly as civilian casualties and security force casualties may have different psychological and political effects. For example, Criado (2017, 204–208) notes that there is a different salience to civilian victims and the assassination of politicians than when members of security forces fall victim to terrorist violence (also see Yaylacı and Bakmer 2019, 566–72). Balcells and Torrats-Espinosa (2018, 10624) similarly show that while civilian casualties increase electoral participation significantly, combatant casualties do not. To understand the reason behind distinct effects of these casualties, consider a bombing attack that targets civilians and an armed attack targeting soldiers. The former elicits mortality salience, as individuals may believe that they can be next, regardless of their civilian status, a common feature in civil wars featuring mass killings (see De la Calle 2017; Krcmaric 2018 for civilian targeting). The latter, however, may not pose any personal threat to the civilians, particularly if it takes place as part of a purely military operation well away from civilian life. Personal fear and threat perceptions seem likely to differ across these two scenarios, perhaps with distinctions along the lines of personal and sociotropic threat (Huddy et al. 2002, 486–90; Hetherington and Suhay 2011, 551; Belge 2016, 285–86). Because the cognitive and psychological repercussions of the type of casualty differ, they may be associated with different attitudinal and behavioral responses. We thus suggest that civil war researchers differentiate between the targets of attacks when trying to understand the effects.

Finally, we would like to draw attention to higher-level contextual effects. While in many cases contextual effects may only be felt at the local level, some attacks may have nation-wide effect, being unprecedented in scale, scope, target, or violation of local norms. For example, in Peru, even though Shining Path had engaged in numerous mass killings between 1984 and 1992, it was the Tarata bombing<sup>9</sup> that

<sup>9</sup> It was the biggest and most impactful attack of Shining Path in its armed warfare in Lima. Car bombings occurred on Tarata Street, which is replete with financial businesses located in Miraflores—one of the most upscale districts in the capital city Lima. Twenty-five people died and 155 were injured. One can easily think of similar cases such as the Enniskillen bombing in Northern Ireland or the 9/11 attacks in the United States, where the effects of the attack were felt well outside of the immediate environs.



shifted the government's counterinsurgency response and the public perception of the threat (TRC 2004, 140). In the case of such attacks that are unprecedented in scale or method, feelings of insecurity are not exclusive to those who were directly exposed or victimized, and the effects of such attacks may be felt on a much broader scale (Bleich, Gelpkopf, and Solomon 2003, 615–20; Rubin et al. 2005, 610–12). Those who live outside of the origin of attacks may be exposed to the news of the violent attack through media, and while the trauma they experience is vicarious and not direct, it may still be pervasive. For example, while most Americans did not directly experience the attacks of September 11, 38 percent of Americans recalled the attack as the most important event in the past year of their life in 2002 (Hartig and Doherty 2021).<sup>10</sup>

The nation-wide effects of unprecedented attacks can be categorized as a type of contextual effect; however, these types of events differ in the sense that gauging their effects is not easy using common quantitative indicators such as casualty or event counts. For example, the Tarata bombing was not exceptional in terms of the casualties; its uniqueness lies in the fact that it was in a previously untargeted and a very central section of the capital city of Peru. Similarly, if the 9/11 attacks in the United States were coded with event counts, they would only represent somewhere between one and four events, a small number in comparison to the annual event tolls within other countries, or in comparison to annual white supremacist terror attacks within the United States. Identifying the differential effects of these particularly salient attacks is an essential step in further research on the effects of exposure to violence. Researchers should thus take into account the qualitative differences between attacks, particularly where they violate long-standing norms or expectations, and consider the possibility of widespread effects in using contextual indicators of exposure to violence.

Contextual indicators can also be binary, presence or absence of conflict, as mentioned above, which may be appealing for research utilizing time-series cross-national data. In this case, we suggest at least distinguishing between territorial and governmental wars (Gleditsch et al. 2002).<sup>11</sup> Uppsala/PRIO Armed Conflicts project define governmental conflicts as those that concern “the type of political system, the replacement of the central government, or the change of its composition while Territorial conflicts involve demands for secession or autonomy” (Gleditsch et al. 2002, 619). Governmental wars attack the government, the regime, and its ideology. The institutions and the actors are often targeted, crippling the state. Because ideological conflicts often do not feature a strong collective identity threat (as the threat is to the government, not to a defining feature of the nation), collective threat framing will be weak. Ethnic territorial wars attack the territorial integrity of a nation and existence of the politically powerful ethnic collective. These wars also differ in geography. In territorial wars, the war is restricted to a certain area. In governmental wars where wars are not restricted to a geography, violence is likely to be spread across the country and more likely to reach to the capital (Buhaug and Gates 2002, 421–22). Thus, in territorial and governmental wars, both the nature of the collective threat and the geographic distribution of violence will be different, which will affect individual perceptions of conflict and violence. We hence suggest attending to the differences in character of the wars when using binary variables to capture “exposure to violence.”

<sup>10</sup> Hartig, Hannah, and Carroll Doherty. “Two Decades Later, the Enduring Legacy of 9/11.” Last edited September 2, 2021, accessed October 1, 2021, <https://www.pewresearch.org/politics/2021/09/02/two-decades-later-the-enduring-legacy-of-9-11/>.

<sup>11</sup> Uppsala/PRIO Armed Conflicts dataset shows that since the WWII, virtually two-thirds of all intrastate conflicts since 1946 have been challenges to the central government, the remaining being classified as territorial disputes (Gleditsch et al. 2002). Findings attest to the importance of this categorization: Buhaug (2006), for instance, shows that country size and ethnic fractionalization increase the likelihood of territorial conflicts, but not the likelihood of governmental conflicts.

Finally, we would like to note that even though highly aggregated indicators may jeopardize catching nuances at the subnational exposure to violence, the extent to which it can do so depends on the geographies of political violence. For example, per “People on War Project” data, more than 80 percent of people in Abkhazia report living in the area where the war took place; in Afghanistan 79 percent, and in Bosnia-Herzegovina 61 percent lived in the war zone ([International Committee of the Red Cross 1999](#)). In contrast, in Turkey at most 10–15 percent lived in the war zone, and thus the effectiveness of a binary measure of war presence to capture war experiences will vary significantly between Turkey and Bosnia.

### Discussion

In the review above, theoretical as well as empirical issues associated with measuring exposure to violence and victimization were raised. In this section, we will discuss two main issues that emerged from the review: first, best practices in operationalizing exposure to violence and, second, how to apply indicators at different levels to theoretical mechanisms.

On the first issue, challenges in accurately measuring exposure to violence abound, given breaks in the historical record, social desirability biases, and issues with the accuracy of memory, particularly of traumatic events, and even the most “objective” figures of violence, such as fatalities, can be contentious ([Krüger et al. 2013](#), 248–49; [Krause 2013](#), 268–69; [Seybolt, Aronson, and Fischhoff 2013](#), 4–5; [Davenport and Ball 2002](#), 434). These problems are not limited to quantitative studies, as many authors writing on ethnographic methods note the challenges in interpreting incomplete or outright falsehoods from their research subjects (e.g., [Fujii 2010](#)). Using ethnographic techniques may provide enough metadata in the form of nonnarrative hints of falsehood, such as evasions and silences, to help the researcher identify outright falsehoods. However, interpreting metadata requires repeated interactions and a level of local knowledge that are often absent in shorter or less reflective field research ([Fujii 2010](#), 240; [Yaylacı 2020](#), 1–3). Given that many models of qualitative inference focus on identifying “high leverage” observations which while few in number provide direct evidence of a particular causal process, this implies that even a small number of poorly interpreted interviews may give misleading findings ([Brady and Collier 2004](#), 209). In summary, all conflict researchers face challenges with accurately measuring victimization, regardless of level of analysis or methodological approach. We provide a guideline below in the hopes of alleviating these challenges.

On the second issue, indicators at the individual and communal level would seem to naturally imply different types of causal mechanisms because contextual exposure, or living in a violent area, is not the same as primary victimization, where an individual is directly harmed. While some works try and compare across these different wartime experiences, we do not know if the fears induced by living in a violent zone are different than the trauma of injury or losing a loved one. This is particularly important given the growing body of work addressing the consequences of civil conflict, as well as the inherent ethical and methodological challenge in gathering information on victimization. We will explore this issue in more depth below.

#### *Choosing Better Indicators*

Despite all the challenges awaiting researchers, more accurate indicators of exposure are possible. Regarding individual-level indicators, we believe that indices are powerful tools in capturing a wide array of experiences and may also help in distinguishing between direct and indirect exposure. Although both political science and psychology research show that vicarious experiences of violence are trauma inducing (e.g., [Eriksson et al. 2001](#), 206; [May and Wisco 2016](#), 234–35; [Haer, Scharpf, and Hecker 2021](#), 43), capturing the qualitative variations within each and severity of the experience are essential, as different forms of direct

**Table 2.** Direct experiences

a. Personal/bodily experiences			
During the conflict, have you ever been:	No	Yes, once	Yes, more than once
incarcerated? held in captivity? held in detention?			
forced to hide? ambushed?			
threatened by security forces or rebels?			
robbed? subject to extortion?			
left without food/shelter?			
kidnapped?			
forcibly separated from family?			
sexually assaulted?			
attacked with a weapon?			
physically injured? <sup>12</sup> subject to beating to the body? tortured?			
forced to migrate? <sup>13</sup> forced to evacuate your house under dangerous conditions?			
not permitted to give corpses a proper burial?			
fallen ill without access to medical care due to conflict?			
caught up in/witnessed bombing?			
caught up in/witnessed riot?			
caught up in/witnessed shooting?			
caught up in/witnessed aerial strike?			
b. Material losses			
Have you suffered any war damage to/confiscation of:	No	Yes, partially	Yes, complete damage
your home?			
car/truck/tractor? farming equipment?			
shop/store (if you had one)?			
household items (e.g., furniture, appliances)?			
livestock? crops?			

exposure are shown to have distinct and even opposite prosocial impacts. For example, Vélez et al. (2016, 10) show that witnessing a homicide increases cooperation, while displacement diminishes it.

Building primarily on Schmid and Muldoon's (2015, 81) list of items to gauge political conflict exposure in Northern Ireland, Balcells' (2012, 324–27) measures of victimization in the Spanish Civil War, Kunovich and Hodson's (1999, 664–65) property damage and violence indices, and other sources, we put together a list that can be used by researchers (tables 2 and 3). The list has items to gauge direct and indirect exposure, and for each item we propose ways to ask about the severity of victimization. Depending on the research study's goals, an index can be created

<sup>12</sup> Alternative/additional ways to measure intensity of injury may be to offer the following choices:

Yes, but no lasting physical harm.

Yes, I still have scars.

Yes, I became handicapped.

<sup>13</sup> Alternative/additional ways to measure intensity of displacement may be to offer the following choices: Yes, within the same village. Yes, to a nearby village/town/city. Yes, to a town/city that we had no familiarity with.

Table 3. Indirect experiences

Have any of your household members or close family members/friends ever been <sup>14</sup> :	No	Yes, once	Yes, more than once
killed?			
disappeared?			
kidnapped?			
incarcerated? held in captivity? held in detention?			
ambushed? forced to hide?			
threatened by security forces or rebels?			
left without food/shelter?			
separated from family?			
sexually assaulted?			
physically injured?			
forced to migrate?			
caught up in/witnessed bombing?			
caught up in/witnessed riot?			
caught up in/witnessed shooting?			
caught up in/witnessed aerial strike?			
had war damage on property (house, car, livestock, shop, etc.)?			

*Note:* The same list can be asked for close friends or other relatives.

with weights or tailored to measure severity. Clustered items can be asked separately. For sensitive questions, such as having being subject to sexual violence, researchers can incorporate the questions as a list experiment, which reduces ethical concerns about deliberately probing for traumatic experiences.

The items in the offered list are not necessarily observed in all types of civil conflict. Armed groups' repertoire of violence varies based on institutions for behavioral control, including command structure, preferences, and incentives for restraint. For example, communist rebels commit less sexual violence than noncommunist rebels (Hoover Green 2016, 626; see also Hoover Green 2018). Similarly, wars vary in the frequency of forced displacement or civilian casualties; according to the data from the "People on War Project," in all the war settings surveyed, about one-third of the population were forced to leave but these numbers are much higher in Somalia, at 63 percent, and in Afghanistan, at 83 percent (International Committee of the Red Cross 1999). The items suggested can be tailored according to the specifics of the case, to capture case-relevant violence against civilians.

For aggregated data, aggregating at lower units of analysis such as village or municipality is ideal unless violence was observed across all geographies in a uniform manner. If a study focuses on the impact of losing a family member and if death toll was high and observed across different territories, then aggregation at a higher level would not jeopardize the data quality and measurement accuracy. For example, in El Salvador, 33 percent lost somebody in their immediate family in the war while in Cambodia this number is 79 percent. Aggregating higher levels in Cambodia would then pose less risk to inference than in El Salvador. Once such trade-offs are established, researchers may benefit from disaggregated datasets such as the Armed Conflict Location and Event Data Project (Raleigh et al. 2010).

Even when ostensibly valid and reliable measures of violence are obtained, individual interpretations of the same kind of victimization may be different. Gutiérrez-Sanín and Wood (2017, 23–25) note that there are marked differences in how armed organizations select victims and the methods of violence they use, and that these are as important as the frequency of violence in discussing civil war violence.

<sup>14</sup>Follow-up questions can list family members such as father, mother, children, wife, grandparents, uncle, aunt, and cousin.

For example, whether one lost a family member in a random bombing event versus in a face-to-face attack by a particular ethnic group would matter in how individuals assign blame and how they perceive the event.

The method and repertoire of violence may be particularly important for why individuals believe that they may become victims. Individuals may be targeted *selectively* based on a behavior that armed groups seek to “punish,” *collectively* due to a group identity they have, or *indiscriminately*, based on little but luck (Price and Yaylaci 2022, 3–6; Steele 2018, 812–13; Valentino, Huth, and Balch-Lindsay 2004, 377–79). These differences should be considered when trying to account for different subjective reactions to the same frequency or method of violence, as individuals are likely to come to at least some opinion on what, if any, reasons are acceptable for the use of violence.

In particular, collective targeting, where individuals are selected as victims due to their membership in social categories (Gutiérrez-Sanín and Wood 2017, 21; Schwartz and Straus 2018, 228; Steele 2018, 811–12), is likely to have very different impacts than indiscriminate violence, where individuals are “collateral damage” and become victims by random chance.<sup>15</sup> As an example of these differences, consider Steele’s description of displacement in Colombia:

If a person is selectively targeted or faces indiscriminate violence, the decision to stay in or leave a community does not depend on others who are selectively or indiscriminately targeted; the likelihood that the person will suffer direct violence will not change whether or not others stay. In contrast, when armed groups collectively target, the decision to stay or leave of any given individual who shares the targeted trait depends on the response of everyone else similarly targeted (Steele 2017, 27).

While clearer conceptualizations of what is meant by victimization will help in reconciling divergent findings on the effects of violence, works that focus only on frequency are still liable to miss important effects caused by other elements of an armed organization’s pattern of violence.

A final concern is that exposure to violence may systematically covary with other variables, some of which are exceedingly hard to measure. Many authors have pointed out that the use of violence is related to an armed organization’s presence and control within a given area (Kalyvas 2006, 147–50; Balcells 2017, 6–7). For all measures, this means that victimization may depend on armed organizations’ control, and thus effects of victimization may be confounded by processes which lead to both control and violence (Hoover Green 2016, 620–21; also see Hoover Green 2018). For example, Arjona (2016, 11–12) notes that local institutions are important in predicting both control and the use of violence by armed organizations in Colombia. Studies that compare localities but do not take this hard-to-measure variable into account may thus be confounded, as the strength of local institutions is liable to predict both which localities are targeted with violence and the strength of postwar institutions and prosocial behaviors. Designs using aggregate measures of violence are particularly likely to be affected by this problem, as even the inclusion of district or municipality fixed effects may not capture granular variation on control (Kalyvas, Shapiro, and Masoud 2008, 417).

#### *Theoretical Mechanisms behind Different Measurements of Exposure to Violence*

Our review above indicates that contextual exposure may be associated with different outcomes than direct individual exposure. This does not signal a discrepancy; instead, it indicates the different impacts that violence can have on individuals and societies. As discussed above, collective exposure impinges on individual attitudes and behavior differently than direct individual exposure (figures 3 and 4). When

<sup>15</sup>We would like to thank reviewer 2 for bringing this aspect into our attention.

most members in a village are victimized, the aggregated and individual-level indicators may not exhibit a striking difference (e.g., [Voors et al. 2012](#), 952); however, when violence is not geographically concentrated, it is plausible that the trauma of direct victimization may be linked to a different set of emotional and cognitive mechanisms than being exposed to violence contextually.

Studies explaining an individual-level outcome variable, such as voting or group membership, often propose a causal mechanism that captures cognitive or psychological reactions to violence. In the case of direct exposure to violence, individual reactions to stress and concomitantly individual understandings of threat and insecurity can explain changes observed in victims' attitudes and behavior, whether negatively via PTSD or positively via post-traumatic growth (PTG) ([Tedeschi and Calhoun 2004](#), 2–3). [Blattman \(2009, 244\)](#), for example, argues that victims use voting as a coping mechanism to overcome trauma, which links victimization and higher turnout.

In the case of contextual exposure, the effects observed are for aggregated indicators, and posited causal mechanisms should take this into account. To illustrate, if the indicator is total count of events, the mechanism needs to tap the effect of the changes in the larger social environment, as well as on individual attitudes or behavior. For example, [De Luca and Verpoorten \(2015b, 671\)](#) offer the decreasing rate of interaction frequency between individuals caused by safety-seeking behaviors as a mechanism for the declining trust in districts with more violent events. Similarly, [Tellez \(2019, 1054\)](#) invokes the threat felt by community members who live in targeted zones and the resulting fear response and personal security concerns as a mechanism to explain their higher desire for peace, rather than a specific personal response to the trauma of loss, injury, or displacement. Another example of this is [Gallego \(2018, 602\)](#) who analyzes voter turnout using municipality as the unit of analysis and proposes strategic interaction between armed groups as the causal mechanism rather than psychological factors. He contends that when a non-state armed group does not have territorial control—often the case when the main strategy is mobility and guerilla warfare—the group resorts to intimidation and sabotage of the elections, which leads to a decline in voter turnout.

Mechanisms operating on different levels could be one reason for observing distinct effects between contextual and individual-level exposure, but the limitations of objective measures treating all events or injuries equally could be another. The role of subjective insecurities is a recently proposed explanation for observed variation in outcomes as a result of similar exposures to violence, presuming that “individuals experiencing similar conditions of violence in their environment may develop different perceptions of insecurity” ([Vélez et al. 2016, 2](#)). [Vélez et al. \(2016, 12\)](#) show that subjective insecurity mediates the effect of victimization on prosocial behavior (cf. [Voors et al. 2012](#)). Even though individuals are from the same village, they vary in their perception of security, and their subjective insecurity can explain mixed findings of a positive effect on trust and altruism but a negative effect on cooperation. Subjective perceptions may affect long-term outcomes even more so ([Balcells 2012, 314](#)). Including subjective insecurities also allows for works that include the competitive efforts of armed actors to make meaning of violent actions, a deeply political and social process that can be observed in many conflicts ([Petersen 2011, 80–85](#); [Shesterinina 2016, 411–13](#)), but often receives little attention in works that examine the effects of violence using only the number or lethality of violent incidents in a given locale.

Incorporating mechanisms that operate at the contextual level, such as subjective insecurities and threat framing, with works looking at the individual effects of violence would help address three important issues. First, given that most works use a subnational approach, it would help researchers understand how their findings may differ based on case-specific dynamics ([Reno 2019, 448](#)). Second, it would allow better understanding of the conditions under which armed groups can reframe their actions, and a better sense of when certain types of large-scale attacks may



have national consequences. This point appears particularly important for peace building, where the ability to control a narrative may be easier and less costly than an ability to control violence. Third, it would help synthesize the different quantitative and qualitative approaches to questions on the effects of violence. Qualitative work, with an attention to causal processes and the lived experience, seems particularly useful in helping quantitative scholars better explain how differences in measurement lead to differing estimations of effect.

### Conclusion

Given that civil wars have been the most common form of political violence since 1945, understanding their consequences on political and social processes is critical for efforts to rebuild and create lasting peace. While there have been many excellent works looking at these outcomes, one challenge is the variety of ways scholars actually measure exposure to wartime violence, a practice that is fraught with ethical and practical issues separate from those of identifying what type of measures to use. Most attempts to measure these experiences are prone to nonrandom error, particularly due to recall and survival biases. These biases seem particularly important in discussing indirect and contextual exposure, which rely on an accurate recall of location and emotional state, in contrast to direct exposures that likely leave lasting scars. Scholars may also face further trade-offs between construct validity and theoretical fit of the indicators: Indicators that are more likely to be accurately measured (e.g., total fatalities in a locality) do not always match the level theory is developed (e.g., microlevel effects of violence).

While issues of measurement are an important concern, our review problematizes the choice behind operationalization, and we advance three main recommendations. First, based on the findings of others that civil war victimization is most often cumulative, we argue that indices are better at capturing the multiple traumas individuals endure, and they are malleable depending on the goals of the research study. We provide a list of questions on direct and indirect experiences with violence that can be used by researchers in constructing new indexes depending on their focus.

Second, we urge scholars to be mindful of the implications of their proposed theoretical mechanism on their operationalization of exposure to violence, particularly when aggregated indicators are used. If the indicators used are at a high level of aggregation or largely contextual, individual cognitive or psychological mechanisms such as PTSD or PTG may be a poor fit; conversely, indicators that look at direct individual-level victimization would seem better suited to measuring individual-level mechanisms. Lastly, our review also highlights the idea that a better understanding of the meaning attached by armed groups and civilians to violence is necessary to advance our understanding of the consequences of civil war violence. Studies in Colombia (Vélez et al. 2016, 2) and Israel (Hirsch-Hoefler et al. 2016, 848, 853) present subjective insecurities as a promising dimension in understanding the social consequences of civil war violence, and one that allows for greater attention to the efforts by armed actors to frame their actions, an inherently political process (among many, Petersen 2011, 80–85; Shesterinina 2016, 411–13). We suggest that in tandem with the use of indices, subjective reactions should be gauged by the researchers and practitioners, to better understand the lasting individual and communal effects of civil war violence.

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