

Historical Legacies of Political Violence

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Summary

Legacies of political violence are long-term changes in social behavior and attitudes, which are attributable—at least in part—to historical episodes of political conflict and contention. These legacies can potentially reshape the subsequent political and social order. Their catalysts can range from armed conflict, mass repression, and genocide to oppressive institutions and interpersonal violence. The lasting effects of violence include changes in political participation and preferences, intergroup relations, economic activity and growth, and public health outcomes. Estimating these effects presents a methodological challenge, due to selection, posttreatment bias, and the difficulty of isolating specific mechanisms. These challenges are particularly acute given the long time span inherent in studying historical legacies, where effects may be measured generations or centuries after the precipitating event. Understanding these legacies requires distinguishing between *persistence* mechanisms, where effects of violence continue within an individual directly exposed to violence through trauma, and the secondary *transmission* of effects between individuals through family socialization, community and peer influences, institutionalization, and epigenetic and evolutionary changes. Research on this subject remains nascent—across many disciplines—and inconclusive on whether violence fosters mostly negative or positive forms of social and political change.

Keywords: political violence, historical legacy, persistence, intergenerational transmission, trauma, contention, civil conflict

Subjects: Contentious Politics and Political Violence, Groups and Identities, History and Politics, Political Behavior, Political Institutions, World Politics

What Makes a Legacy?

At its core, literature on armed conflict and violence seeks to understand contentious interactions between two or more political actors. These actors may be government affiliates (e.g., armies, security services, law enforcement, pro-government militias), their political challengers (e.g., opposition parties, social movements, insurgents), groups not aligned with either side (e.g., self-defense militias, peacekeepers), or unarmed civilians. The interactions between groups are, to varying degrees, coercive—involving the threat or use of physical harm by one side in order to compel a change in the behavior or expressed attitudes of the other or to deter the opposing side (or whoever else is watching) from embarking on an unwanted course of action. Research on political violence typically asks how the contentious actions of one side might influence those of

another and under what conditions these dynamics are likely to escalate or remain contained. A growing body of research on the legacies of political violence—the subject of this review—further asks how long these effects might last, to whom they might extend, and why.

Broadly defined, a legacy is “a long-lasting effect of an event or process.”¹ There is no consensus on how long it takes for an “effect” to become a “legacy,” with timescales ranging from several years (Jakupcak et al., 2007; Schwarz & Kowalski, 1991) to several generations (Lupu & Peisakhin, 2017), and even centuries (Nunn & Wantchekon, 2011; Voigtlander & Voth, 2012).² Some studies consider the impact of an event or process on individuals and groups not directly exposed to it (Dinas & Fouka, 2018), while others consider lasting effects on immediate participants, victims, and eyewitnesses (Grossman, Manekin, & Miodownik, 2015). This research program has spanned multiple disciplines with highly visible publications reaching top journals in political science (Balcells, 2012; Blattman, 2009; Charnysh & Finkel, 2017), economics (Charnysh, 2019; Nunn, 2009; Voigtlander & Voth, 2012), sociology (King, Messner, & Baller, 2009; Messner, Baller, & Zevenbergen, 2005; Pettigrew et al., 2008), history (Henderson, 1991), psychology (Bauer, Cassar, Chytilová, & Henrich, 2013; Kirmayer, Gone, & Moses, 2014; Nisbett, 1993; Yehuda et al., 1994), and public health (Johnson & Thompson, 2008). This article reviews the literature’s main findings to date and the theoretical and methodological challenges scholars in this area regularly face. It places emphases on the cohesiveness of this research program and the barriers to the accumulation of knowledge. There is an emerging consensus that past violence has a durable influence on politics and society, and researchers have made significant progress in empirically estimating the size and direction of long-term effects. However, scholars have struggled to explain how these effects emerge, why they persist as long as they do, and why they vary over space and time.

What Sorts of Legacies Exist?

A typical empirical study on the legacy of violence examines the impact of some “treatment” (e.g., exposure to violence) at time t on a political, social, economic, or health outcome at time $t + \Delta t$, where Δt is some interval of time. The following overview of the literature’s main results is organized by treatment type, the effects of these treatments on attitudes and behavior, and the methodologies and evidence past studies have used.

Types of Exposure

The main categories of treatment include exposure to (a) two-sided violence in the context of armed conflict; (b) one-sided violence such as genocide and mass repression; (c) institutions that violate personal integrity and suppress individual liberties; and (d) crime, sexual and gender-based violence, and other interpersonal violence. “Exposure” has a slightly different meaning in each case.

Legacy of Armed Conflict

Research on the legacy of armed conflict has focused primarily on exposure to violence by nonstate actors. This includes violence by rebels in a civil war (Blattman, 2009; Cassar, Grosjean, & Whitt, 2013; Weintraub, Vargas, & Flores, 2015), terrorist attacks (Canetti-Nisim, Halperin, Sharvit, & Hobfoll, 2009; Getmansky & Zeitzoff, 2014; Gould & Klor, 2010; Montalvo, 2011), and violence between ethnic groups (Hager, Krakowski, & Schaub, 2019; Michalopoulos & Papaioannou, 2016). A smaller subset of this literature examines the effects of wartime violence by government forces (Balcells, 2012; Fouka & Voth, 2019; Miguel & Roland, 2006; Rozenas, Schutte, & Zhukov, 2017). Still, many studies either do not differentiate by actor at all or combine the two sides' violence into general measures of exposure (Bauer et al., 2013; Couttenier, Preotu, Rohner, & Thoenig, 2016; Voors et al., 2012).

There is a further distinction in this literature between research focusing on the effects of specific wartime tactics—like rocket strikes (Getmansky & Zeitzoff, 2014), strategic bombing (Miguel & Roland, 2006), and torture (Johnson & Thompson, 2008)—and research that does not disaggregate by tactics at all and instead examines more general exposure to conflict (Bar-Tal, 2001; Bellows & Miguel, 2006; Blattman, 2009).

The counterfactual to “use of tactic A,” of course, is not necessarily “absence of violence.” Combatants make tactical choices based on a wide array of considerations, including doctrine (Posen, 1986), operational intelligence (Leites & Wolff, 1970), technology (Lyall & Wilson, 2009), battlefield conditions (Kramer, 2005), normative constraints (Merom, 2003), and expectations of how targets might react (Carter, 2016). Research on the short-term dynamics of violence has long explored why different tactics (e.g., selective arrests versus indiscriminate artillery shelling) can have different behavioral effects (Kalyvas, 2006; Lyall, 2009) and why different actors can respond to the same tactics in different ways (Toft & Zhukov, 2015). While scholars of long-term legacies recognize that testing the effect of one tactic in isolation obscures the full range of choices available to perpetrators of violence, surprisingly few studies compare effects across a broad repertoire of tactics or analyze interactions between them (e.g., Charnysh, 2015; Grossman et al., 2015).

Legacy of Mass Repression and Genocide

A growing number of studies examine the legacy of one-sided violence against civilians, both inside and outside the context of war. The bulk of this research investigates repression and mass killings by governments, although a smaller number of studies consider violence by nonstate actors, like anti-Jewish pogroms (Voigtländer & Voth, 2012), lynchings (King et al., 2009), or warfare in hunter-gatherer societies (Bowles, 2009). Unlike research on the aftereffects of armed conflict, which typically studies exposure to multiple incidents of violence over the full duration of war or another protracted period of time (e.g., Voors et al., 2012), many studies in this category focus on a single defining campaign of mass violence.

Literature in this group has devoted much of its attention to the long-term effects of repression in totalitarian regimes. These include China's Cultural Revolution (Wang, 2019), Stalin's Great Terror (Zhukov & Talibova, 2018), and the Ukrainian famine of 1932 (Rozenas & Zhukov, 2019). Other studies have considered the legacy of state-led forcible displacement, such as the deportation of Crimean Tatars (Lupu & Peisakhin, 2017), the resettlement of families from western Ukraine (Rozenas et al., 2017), and mass relocations of ethnic and religious diasporas (Akbulut-Yuksel & Yuksel, 2015; Alpan, 2012; Braun & Kvasnicka, 2014; Charnysh, 2019; Dinas & Fouka, 2018; Schutte, 2019). Some have examined legacies of more recent mass atrocities, like the Rwandan genocide (Rogall & Yanagizawa-Drott, 2013).

The most thoroughly researched episode of mass violence is the Holocaust. Multiple studies have investigated socialization of Holocaust trauma across the generations following initially affected family members (Lev-Wiesel, 2007; Palgi, Shrira, & Ben-Ezra, 2015; Wayne & Zhukov, 2019; Yehuda, Bell, Bierer, & Schmeidler, 2008; Yehuda et al., 1994). Others have looked beyond the targeted population and examined the impact of the Holocaust on communities that were not directly victimized by it, like Charnysh and Finkel's (2017) study of death camp property acquisition by local residents.

Research on the legacies of mass repression and genocide cleaves between work that views these legacies as gradated and variable across individuals and groups (Charnysh & Finkel, 2017; Lupu & Peisakhin, 2017; Voors et al., 2012) and those that view mass repression as all-encompassing within a society (Antoniou, Dinas, & Kosmidis, 2017; Ó Gráda, 2007; Tyner & Rice, 2016). The latter take the experience of mass repression to be collectively shared, including among individuals not directly exposed to harm. This divide is revealing of an undertheorized question in the literature: Whether the lessons of violence diffuse across individuals (e.g., from parent to child, or neighbor to neighbor), or whether they are broadcast across society in a way that makes individual exposure to treatment virtually indistinguishable. The second of these perspectives suggests a trauma threshold, where the effects of genocide, famine, or other mass violence are so great that all those who survive can be assumed to exceed some level of treatment exposure, indistinguishable across societal subgroups.

Legacy of Institutions

A separate branch of the literature turns its focus away from episodic exposure and instead examines prolonged exposure to institutions that systematically exploit or discriminate against specific social groups or otherwise violate personal integrity and freedoms. These include slavery (Acharya, Blackwell, & Sen, 2016b; Nunn & Wantchekon, 2011), feudalism (Dower, Finkel, Gehlbach, & Nafziger, 2018), foreign occupation (Acemoglu, Davide, Simon, & Robinson, 2011), colonialism (Mamdani, 2001; Nunn, 2008), communism (Pop-Eleches & Tucker, 2017), and authoritarianism (Falleti, 2011). Similar to literature on the effects of violent events, this research conceptualizes "legacy" as a "long-term causal relationship between past institutions and practices and those of the present" (Beissinger & Kotkin, 2014, p. 11).

In most applications, what separates past and present is some historical rupture, like emancipation, decolonization, partition, or regime collapse.³

What “exposure” means in an institutional setting is more complicated than in the case of violence, where the focal events are situated in a particular place and time. Conversely, it is difficult to trace the long-term effects of institutions to a single incident or experience.

Institutions operate through broader channels, by structuring social interactions and promoting and enforcing norms and standards of behavior (Lowes, Nunn, Robinson, & Weigel, 2017). The more diffuse nature of institutional exposure does not imply an absence of variation. Among studies that attempt to quantify the intensity of exposure, the most common approaches rely on duration (e.g., how long a given territory spent living under a regime), geographic location (e.g., relative to a historical border or port), and population statistics (e.g., size or proportion of population affected).

The first approach—duration of exposure—assumes that opportunities for institutions to shape attitudes and behavior, as well as to supplant preexisting ones, increase with longevity (e.g., Bowles, 2009; Fontana, Nannicini, & Tabellini, 2018; King et al., 2009; Messner et al., 2005; Miguel & Roland, 2006). This assumption does not require uniformity of exposure over time, since institutions can undergo many reforms that are potentially consequential for citizens’ behavior (e.g., de-Stalinization in the USSR, Open Door Policy in China). Accounting for such phase shifts typically entails disaggregating the full period of exposure into a series of shorter spells (e.g., Pop-Eleches & Tucker, 2017, pp. 51–53).

The second approach—geographic location—exploits spatial discontinuities in institutional exposure (e.g., Banerjee, Iyer, & Somanathan, 2005; Dell, 2010; Iyer, 2010). These discontinuities often occur around an administrative border, separating locations that saw the introduction of a given institution at an earlier point in time, from those where the same institutions emerged later or not at all. Some studies in this category use border discontinuities as a natural experiment, with an assumption that the demarcation of these boundaries was “as if” random (Mattingly, 2017; Peisakhin, 2012). However, because borders are typically endogenous to politics (Alesina & Spolaore, 1997), arbitrarily drawn boundaries can be quite difficult to find in most contexts.

The third approach—population statistics—has the advantage of capturing institutional exposure in a relatively direct way (Rozenas & Zhukov, 2019; Zhukov & Talibova, 2018). While the first two approaches assume that exposure varies over time and space, this one assumes that—even at the same location and time—some individuals may have been affected more directly than others. Examples of population-based exposure measures include the proportion of each U.S. county’s population that was enslaved (Acharya et al., 2016b), the proportion of serfs in a Russian district’s population (Dower et al., 2018), and shipping statistics on the transatlantic slave trade (Nunn, 2008). Where official statistics are unavailable—or unreliable—researchers have used a variety of statistical techniques to estimate the size of the affected population (e.g., Ball, Asher, Sulmont, & Manrique, 2003; Wolowyna et al., 2016). Even so, not all types of institutional exposure lend themselves to such a direct form of measurement. Furthermore, institutional treatment effects are not always proportional to the share of the population exposed and can assume a variety of functional forms, from declining returns to nonmonotonic threshold effects (Zhukov, Davenport, & Kostyuk, 2019).

Legacy of Interpersonal Violence

At the opposite extreme from institutions and organized mass violence by states are experiences of smaller scale, everyday violence between citizens. Interpersonal violence and crime routinely occur under any political system, in peacetime and in war (Bateson, 2012; Corbacho, Philipp, & Ruiz-Vega, 2015; Romero, Magaloni, & Díaz-Cayeros, 2016). Yet armed conflicts can create permissive conditions for such violence to unfold, either as a byproduct of state collapse and lawlessness or as part of a campaign of coordinated acts that specially groomed combatants perform en masse (e.g., wartime rape, Cohen, 2013a; Wood, 2009).

Although interpersonal violence is empirically pervasive, it accounts for a relatively small share of the legacy literature, particularly within political science. There are several potential explanations for this gap. First, there is a widespread perception that the “treatment” is not as overtly political as in armed conflict, repression, or institutions. While coercive violence by rebel groups or government forces typically has an implicit political purpose (e.g., to compel obedience or deter opposition), the same cannot be said for many violent crimes, where the perpetrator’s political agenda is not a primary motivation behind the physical act of violence (Agnew, 2005). Second, the literature typically conceptualizes exposure to such violence (and any subsequent effect) as occurring at the individual level in a highly localized manner (Bateson, 2012). Relatively few studies examine the effects of community-level exposure to crime (Couttenier et al., 2016; King et al., 2009). Third, crime literature nearly always takes a narrow temporal scope, estimating effects within the span of a few years or a single generation.

While extant literature on the legacies of crime and interpersonal violence is limited, there is latent demand for such work for at least three reasons. First, crime is hardly devoid of political content. Crime often emerges from a power vacuum created by an absence of state enforcement (Skaperdas, 2001). The quality of this enforcement—along with other political factors, like electoral representation—affects victims’ willingness to report crimes (Baum, Cohen, & Zhukov, 2018; Iyer, Mani, Mishra, & Topalova, 2012). If victims attribute crime to the inability or unwillingness of political authorities to stop it, exposure can have significant downstream implications for political attitudes (Lee, Porter, & Comfort, 2014; Romero et al., 2016). For this reason, among others, crime often becomes a subject of politicization in electoral campaigns (Daxecker & Prins, 2016; Scheingold, 1992; Sears, Lau, Tyler, & Allen, 1980; Stephens-Dougan, 2016; Valentino, Hutchings, & White, 2002). Further, criminal law itself is a product of political decisions to criminalize some types of behavior but not others (Cohen, 1996; Matza, 1969).

Second, the critique that most crimes directly affect fewer people than macrolevel phenomena like war or genocide also applies to individual acts of violence by armed groups in war and individual arrests by the secret police. In most cases, the cumulative impact of exposure to these incidents (e.g., effect of living in a high-crime area on voting) is a more theoretically informative quantity of interest than the individual-level effect of a single incident. Moreover, even some individual crimes can have community-level effects. For example, hate crimes and pogroms explicitly target individuals according to group-level characteristics like ethnicity or religious affiliation (Haider-Markel, 2002; Jenness, 2018). Other high-casualty events, like school

shootings, can reach community-level exposure through media coverage or social networks connecting victims to relatives and friends (Joslyn & Haider-Markel, 2018; Newman & Hartman, 2017).

Third, the nonpersistence of these effects over longer periods of time is a (mostly untested) empirical claim. It rests on the assumption that interpersonal violence is not the pervasive “treatment” that many presume to exist in mass repression or war, and therefore its effects are unlikely to carry over from parent to child or neighbor to neighbor. Yet not all crimes are episodic, nonpolitical events, and whether their effects will necessarily be limited to immediate victims remains an open question.

One category of interpersonal violence that has attracted significant scholarly attention is sexual and gender-based violence, including wartime rape. Sexual violence can produce lasting trauma that persists within individual victims and their families and transmits to children and across family and social networks (Cohen, 2016; Follette Polusny, Bechtle, & Naugle, 1996; Ullman, Filipas, Townsend, & Starzynski, 2005). Studies on the persistence of trauma from sexual violence are prevalent in psychology and medical research (Follette et al., 1996; Smith & Freyd, 2013; Ullman et al., 2005). Political scientists, in turn, have studied the pervasiveness of sexual violence during armed conflict (Cohen, 2013a, 2013b, 2016; Wood, 2009) and in peacetime (Baum et al., 2018). Yet relatively little scholarship has examined the effect of sexual violence on the political attitudes of directly and indirectly affected individuals. There remains a considerable gap in the political science literature’s exploration of how communities perceive such violence, how they process it through socialized memory, and how this socialization affects the legacy of sexual and gender-based violence (Lonsway & Fitzgerald, 1994; Suarez & Gadalla, 2010).

Types of Effects

The long-term effects of these disparate forms of exposure fall into four categories, comprising effects on (a) political behavior and attitudes, (b) social behavior and attitudes, (c) economic conditions, and (d) public health outcomes, like PTSD.

Political Behavior and Attitudes

Because the treatments to which individuals and communities may become exposed are often political in their intended effects, much of the literature asks basic questions of political efficacy. These include matters of behavioral compliance and attitudinal shifts, where effects generally fall on one of four dimensions: changes in behavior and attitudes toward the specific perpetrator (e.g., loyal vs. disloyal), changes in political preferences or ideology (e.g., left wing vs. right wing), behavioral effects on political participation (e.g., vote vs. abstain), and changes in institutional trust.

Loyalty to Perpetrator

Loyalty, broadly defined, is a feeling of positive attachment to an organization or group (Hirschman, 1970). It can manifest as an attitudinal stance (e.g., approval of an incumbent's performance) and as a behavioral response (e.g., voting for an incumbent). Within the legacy literature, the object of (dis)loyalty is typically the actor whom victims hold most responsible for violence or institutional exposure or that actor's allies and political successors. Postexposure, the dominant empirical pattern appears to be a decline in affinity toward perpetrators of violence. Numerous studies of vote choice have found that historical exposure to incumbents' violence reduces the latter's local vote margin (Rozenas et al., 2017) and increases electoral support for the opposition (Costalli & Ruggeri, 2015; Fontana et al., 2018). Exposure to violence can also lead to a rejection of the perpetrator's political or national identity by victimized groups (Balcells, 2012; Lupu & Peisakhin, 2017)—which has implications not only for loyalty but also for broader preferences and ideology.

Other studies have yielded more ambiguous results—where the effect on (dis)loyalty is null or context dependent. Barceló (2018), for example, finds no clear evidence that police brutality against the Catalan secessionist movement in Spain affected support for separatism. Using survey evidence from Bosnia and India, respectively, Hadzic (2018) and Schutte (2019) find no evidence that reminders of past victimization affect policy preferences or desire for retaliation. Rozenas and Zhukov (2019) and Wang (2018) both show that the effect of past repression is conditional on the political opportunity structure in which postrepression behavior unfolds—increasing opposition at times when the perpetrator is out of power, and increasing loyalty (e.g., through preference falsification, Greif & Tadelis, 2010; Kuran, 1995) when the perpetrator can credibly threaten to resume violence. In a similar vein, Daly (2019) found that belligerents who emerge from a conflict in a militarily dominant position tend to attract more votes, despite their use of atrocities during war.

Political Ideology

A separate subset of this literature looks at the legacy of violence and institutions on broader political preferences and ideology, typically constructed as falling on a continuum from left to right. For example, a host of studies has found that exposure to violence increases support for right-wing or nationalist political movements, parties, and policies, likely because of the association of some right-wing groups with more hawkish stances on national security and exclusionary policies toward out-groups (Belmonte & Rochlitz, 2019; Canetti et al., 2018; Charnysh & Finkel, 2017; Christian & Rössel, 2017; Grossman et al., 2015; Zeitzoff, 2014). Such effects seem particularly profound following exposure to terrorism (Berrebi & Klor, 2006; Berrebi, Esteban, & Klor, 2008; Kibris, 2011).

The literature on institutional exposure has also found enduring effects of communism on voting, including some seemingly contradictory patterns, like right-wing voters' support for communist parties' successors (Belmonte & Rochlitz, 2019; Bustikova & Kitschelt, 2009; Ishiyama, 2009). Cross-national variation in these voting patterns appears to depend, at least in part, on whether opportunity structures are sufficiently permissive for the radical right and other ideological

parties to emerge in such settings (Beichelt & Minkenberg, 2002; Minkenberg, 2002). Outside the post-communist context, there is evidence that institutional exposure affects not only historical victims but also the beneficiaries of those institutions. In a study of the electoral legacies of slavery, for example, Acharya et al. (2016b) found that white residents of southern counties in the United States with high shares of slaves in 1860 are more likely to hold conservative political views and express racial resentment toward African Americans. Separately, Weaver (2017) attributes the mobilization of support for Radical Reconstruction in the postbellum United States to Union veterans in the North, who were keen to back the political measures necessary to preserve their victory.

Political Participation

A third set of political effects concerns participation, particularly voter turnout. Here, findings are decidedly mixed. In a pair of seminal studies on Sierra Leone and Uganda, Bellows and Miguel (2009) and Blattman (2009) found that exposure to rebel violence can lead to substantial increases in voter registration and turnout. Subsequent studies on civil conflicts in other regions have found contrary results (e.g., Gallego, 2018), as has research on the legacy of mass repression. Zhukov and Talibova (2018), for example, found that communities exposed to Stalinist repression have systematically lower rates of electoral turnout in contemporary Russia under Vladimir Putin—echoing Rozenas and Zhukov's (2019) observation that the long-term effect of repression depends on whether a credible threat of renewed retribution exists.

Hadzic and Tavits (2019) find countervailing, gendered effects of past violence on political participation: While exposure to violence increases participation among men, it reduces it among women. These patterns, in turn, affect post-conflict political representation by placing greater emphasis on stereotypically “masculine” traits, like aggression, dominance, and decisiveness. In another study, Hadzic and Tavits (2020) find that while women are more empowered to run in post-conflict elections, the emphasis on traditionally male traits and security reduces voter support for female candidates.

Institutional Trust

Finally, several studies have examined the effects of exposure on trust in public institutions more broadly. For example, Nunn and Wantchekon (2011) find that individuals belonging to ethnic groups most exposed to Africa's slave trade exhibited less trust in their local government in the early 2000s. In China, Wang (2019) finds that exposure to violence during the Cultural Revolution erodes trust in political rulers. In the United States, Lee et al. (2014) find a negative relationship between the incarceration of family members or partners and trust in police and government. Grosjean (2014a) uses nationally representative survey data from 35 European countries to show that any type of wartime victimization is associated with a lower perceived legitimacy and effectiveness of national institutions. These seemingly negative effects run counter to the view—popular among some historians and anthropologists—that war can foster societal transitions that facilitate the development of new state institutions and strengthen existing ones (Carneiro, 1970; Diamond, 1997; Tilly, 1985).⁴

Social Behavior and Attitudes

Beyond politics, the literature has established that violence can have a more general effect on individuals' interactions with other members of society, including members of one's own group (in-group) and members of outside groups (out-groups). Many of the studies in this category have been interested in whether prosocial, altruistic responses are more or less common than antisocial or exclusionary ones.

There is growing evidence that exposure to violence can, under some conditions, lead to more prosocial behavior and attitudes. For example, Voors et al. (2012) run a field experiment in rural Burundi and find that individuals exposed to violence display more altruistic behavior toward their neighbors. In a survey of children and adults in Sierra Leone and the Republic of Georgia, Bauer et al. (2013) similarly find a strong relationship between exposure to war—particularly early in life—and people's egalitarian motivations. Gilligan, Pasquale, and Samii (2014) find that communities more exposed to violence during the Maoist rebellion in Nepal display more trust and higher levels of collective action. More generally, Bauer et al. (2016) conduct a meta-analysis of cross-national survey evidence to show that, across a wide spectrum of cases, people exposed to war violence go on to behave more cooperatively and altruistically. To account for these findings, many studies invoke "post-traumatic growth," a psychological phenomenon where traumatic events promote an increased sense of compassion and a greater sense of personal resilience and strength (Tedeschi & Calhoun, 2004).

Most of these prosocial attitudes, however, appear to be confined to members of one's own ethnic or social group. With respect to outside groups, the impact of exposure often runs in the opposite direction. For example, several studies have shown that political violence can have a negative effect on intergroup trust and increase support for physical separation (Beber, Roessler, & Scacco, 2014; Canetti-Nisim et al., 2009; Rohner, Thoenig, & Zilibotti, 2013). These negative effects are especially pronounced in regions where opposing groups intermix (Cassar et al., 2013). Lupu and Peisakhin (2017) similarly find that victimization leads to positive within-ethnic-group identification but more hostile attitudes toward out-groups—particularly if individuals associate that out-group with the original perpetrators of violence.

Other studies have yielded more optimistic results and found that some prosocial attitudes may carry over to relationships with outside groups, particularly refugees. Hartman and Morse (2018) find that individuals who experienced violence during the Liberian civil war became more accepting and willing to host out-group refugees. Dinas and Fouka (2018) find that descendants of Greeks forcibly displaced from Turkey exhibited greater sympathy for refugees from the Syrian civil war. Wayne and Zhukov (2019) find similar attitudes toward refugees in a survey of Holocaust survivors and their families. A common thread in these findings appears to be individuals' perception that out-group members' experiences of victimization are substantively similar to their own.⁵

Aggregate Economic Effects

Many of these societal impacts have implications for future economic activity and growth, leading a substantial subset of the literature to closely investigate the economic legacy of violence and institutions. One of the most voluminous of these literatures examines long-term economic outcomes in postwar settings. While most studies agree that war creates a temporary drop in income, there is disagreement over the long-term impact of war-related destruction on growth. Davis and Weinstein (2002) find that, at a minimum, war does not inhibit future development: Using Allied bombing of Japanese cities during World War II as a “shock” to relative city size, they find that destruction preceded an extremely powerful pattern of recovery, with most cities returning to their prior relative position within about 15 years. Similarly, Miguel and Roland (2006) find that the U.S. bombing campaign in Vietnam did not have a negative impact on subsequent local poverty rates, consumption, infrastructure, literacy, or population density.

Other studies have painted a more pessimistic picture. Besley and Reynal-Querol (2014) find a negative correlation between historical exposure to conflict in Africa and future economic development. In an influential piece, Nunn (2008) finds a strong negative relationship between African countries’ exposure to the transatlantic slave trade and current economic performance. Acemoglu, Hassan, and Robinson (2011) similarly show that Russian cities that experienced the Holocaust most intensely have grown less and have had worse economic outcomes after the collapse of the Soviet Union.

The literature has pointed to several channels through which these negative effects may emerge. Exposure to violence tends to discourage savings and potentially drags down local and foreign investment in affected regions (Abadie & Gardeazabal, 2003; Voors et al., 2012). In mixed communities, conflict can reduce individuals’ willingness to partake in trade with out-group members (Cassar et al., 2013; Rohner et al., 2013). Another channel is education. Exposure to conflict tends to disrupt schooling and reduces enrollment among school-age children (Leon, 2012; Shemyakina, 2011). Finally, war can stifle future growth through physical destruction and unexploded ordinance. In a study of the consequences of the U.S. bombing campaign in Cambodia, Lin (2016) finds that bombing in highly fertile land caused a long-term decline in rice production and an increased dependence on subsistence farming.

Public Health Outcomes

Siloed from much of the political science literature is a separate scholarship focusing on medical and public health outcomes. Much of this research considers the development of post-traumatic stress disorder (PTSD) in individuals directly affected by violence (Jakupcak et al., 2007; Johnson & Thompson, 2008) or secondarily exposed through social networks at the time of a violent event (Johnson & Thompson, 2008; Schwarz & Kowalski, 1991). Other studies examine the lasting effects of PTSD, and health risk factors for children and grandchildren of victims (Yehuda et al., 1994, 2008).

A common assumption in public health legacies scholarship is that the strength of PTSD's transmission (across individuals) or persistence (over time) is tied to the severity of initial exposure.⁶ This severity is not limited to physical proximity; social networks can similarly amplify an event's impact. In the wake of a school shooting, for example, Schwarz and Kowalski (1991) find that PTSD symptoms correlated with both physical nearness and producing or witnessing strong emotional responses from others.

Other work has focused on the intergenerational persistence of trauma. Past research on Holocaust survivors, in particular, has suggested that the unique magnitude of genocidal violence is especially likely to manifest across family and community networks, like from mother to child (Yehuda et al., 2008). This persistence can leave palpable biological markings, producing shifts in cortisol levels among children and grandchildren of Holocaust survivors decades after initial exposure (Yehuda et al., 1994). It is unclear if those severely affected by other forms of violence exhibit similar patterns or how severity and the transmission (or persistence) of health outcomes relate.

Unique among the legacies literature is public health scholarship on mediating factors that reduce the effects of violence exposure. A common theme is the importance of strong community and family ties to those processing the trauma. Johnson and Thompson (2008) reviewed a body of literature on PTSD development in survivors of trauma, finding that mental preparedness, religious beliefs, and social and family support can reduce PTSD symptoms in those exposed to violence. Chen and Koenig (2006) note that increased religiosity may also be a byproduct of trauma, providing a frame to process difficult incidents and community ties. In Johnson and Thompson (2008) and Schwarz and Kowalski (1991), a lack of processing (or "decompression") increases the likelihood of lasting effects from violence. Social acknowledgement of recovery from suffering, as well as the avoidance of "negative processing" through the spread of antisocial or depressive behaviors, also reduce the impact of trauma (Johnson & Thompson, 2008). Conversely, network ties can also *increase* the spread of negative externalities from violence.

How PTSD relates to other political, social, and economic outcomes represents an open question, and there is a gap between the findings of public health scholars and other legacies of conflict work in understanding this relationship. PTSD is associated with long-term attitudinal changes and shifts in behavior patterns. Jakupcak et al. (2007) report that U.S. veterans of conflict in Afghanistan and Iraq who exhibited more PTSD symptoms saw an increase in anger and hostility and were more likely to endorse aggression, independent of combat exposure. Transmission of these affects across a community and through generations maps onto the attitudinal and behavioral changes found in other scholarship. These shifts might occur in interaction with or because of PTSD.

Methodology

What sorts of evidence have past studies used to establish these long-term behavioral and attitudinal effects? While a portion of the literature has relied on qualitative methods, like interviews (Lev-Wiesel, 2007) and comparative case studies (Baranovicé, 2001; Dannruther,

2007; Falleti, 2011; Janmaat, 2007; Zajda, 2007), the bulk of contemporary research on historical legacies has been quantitative. The data typically come from surveys (Grosjean, 2014a; Lupu & Peisakhin, 2017), conflict data sets (Besley & Reynal-Querol, 2014; Weintraub et al., 2015), archival sources (Dower et al., 2018; Nunn & Wantchekon, 2011), administrative records (e.g., election results) (Getmansky & Zeitzoff, 2014; Voigtländer & Voth, 2012), or some combination thereof (Charnysh, 2015; Rozenas & Zhukov, 2019). Although some are cross-national in scope (Bauer et al., 2016; Grosjean, 2014a; Michalopoulos & Papaioannou, 2016), most of these data sets are microlevel, capturing variation across individual survey respondents (Balcells, 2012; Hartman & Morse, 2018), or subnational geographic units (Costalli & Ruggeri, 2015; Fouka & Voth, 2019). Most of what is known about legacies of violence comes from the analysis of these data.

Long-term effects are difficult to estimate for two reasons. First, historical exposure to treatment is typically not assigned randomly. If, for example, a government repressed a community because it saw that community as disloyal, a negative relationship between past repression and future loyalty—whether observed through statistical analysis, structured interviews, or comparative case studies—may simply reflect preexisting political preferences rather than a “causal effect” of repression. Second, even if treatment were randomly assigned, a potentially long period of time separates treatment from observed outcomes, raising the prospect of posttreatment bias. This bias occurs because observed outcomes may result not only from the initial exposure but also from individuals’ experiences in the intervening period. Because these experiences may themselves be consequences of exposure (e.g., decisions to seek treatment for PTSD or immigrate to a different country), efforts to take these postexposure factors into account can confound estimates of the initial treatment’s causal effect. These two problems pose significant challenges for both quantitative and qualitative research on historical legacies.

Nonrandom Treatment Assignment

Much of the literature’s intellectual effort has been oriented toward estimating the “main effect” of exposure on some political, social, economic, or health outcome. The challenge is that exposure typically does not occur at random. Exposure is usually the product of strategic decision-making, intended to elicit a particular response from a particular subset of the population. As a result, raw estimates of this effect (e.g., differences in average outcomes across exposed and nonexposed communities) will be biased.

Experiments and Quasi-Experiments

The “gold standard” for circumventing these problems would be an experiment, where subjects are randomly assigned to “exposure” (treatment) and “no exposure” (control) groups. Because randomly victimizing participants is patently unethical, researchers have utilized a series of alternative designs.

First among these are “natural experiments,” where the level of exposure appears to be “as if” random. For example, Miguel and Roland (2006) exploit the arbitrarily drawn nature of the border between North and South Vietnam as a source of exogenous geographic variation in U.S.

bombing. To study the legacy of colonial rule in East Asia, Mattingly (2017) examines discontinuities along the western border of the Japanese colony of Manchukuo, which the Japanese had drawn with the deliberate aim of creating new divisions where none had previously existed. Montalvo (2011) takes a similar approach with respect to the timing of Madrid terrorist attacks, which occurred after some Spanish voters had cast their ballots in national elections but before others had done so. The difficulty with such research designs is that historical examples of “arbitrarily drawn” borders and unanticipated temporal shocks can be difficult to find in practice and claims of “as if” randomization are sometimes at odds with historical evidence (e.g., see Kocher & Monteiro’s, 2016 critique of Ferwerda & Miller, 2014).

Another common approach uses “lab-in-the-field” experimental game protocols to gauge trust and other types of cooperative and prosocial behavior and then evaluates differences in observed strategies across subjects exposed to different levels of violence. Voors et al. (2012), for example, run a series of games to measure risk, time, and social preferences among individuals in a random sample of Burundian villages and compare subjects’ observed behavior across sampling units with historically higher versus lower levels of violence. Bauer et al. (2013) and Cassar et al. (2013) run variants of dictator-type games, in which participants choose between different ways of allocating goods between themselves and an anonymous partner—randomly varying whether that partner is an in-group or out-group member. Other experimental protocols randomly assign subjects to different emotional primes (e.g., anger, sadness), prior to gameplay (Zeitzoff, 2014).

Other studies have used priming experiments, which randomly assign respondents to receive a stimulus (e.g., an interviewer may ask subjects to consider the perspectives of out-group members), and compares the effects of this treatment across individuals with different preexisting levels of exposure (Dinas & Fouka, 2018; Hartman & Morse, 2018; Wayne & Zhukov, 2019).⁷ While this research design ensures that the experimental treatment assignment is randomized, the same is not true for these studies’ observational measures of exposure to violence—whose effect remains the quantity of primary theoretical interest. Survey respondents more exposed to violence—based on self-reporting or community-level measures—may be systematically different on multiple dimensions besides exposure (e.g., they may be older, poorer, less educated). These confounding factors preclude causal interpretation of the “exposure effect” or any interaction between exposure and treatment.

To reduce these inferential challenges, some studies have sought to show that their sample is balanced in terms of observable covariates and that the share of exposed and nonexposed respondents does not differ between treatment and control groups (Dinas & Fouka, 2018). In a similar vein, Wayne and Zhukov (2019) create a reweighted version of their data set, where the level of self-reported exposure is weakly unconfounded by observable preexposure factors. Such adjustment-based solutions, however, are no substitute for true randomization of exposure, which—as previously noted—is both unethical and infeasible in an experimental context.

Observational Causal Inference

The alternative to experiments is causal inference with observational data, of which there are multiple varieties, including instrumental variables, regression discontinuity, difference-in-differences, and synthetic controls. The first set of these, instrumental variable designs, exploit exogenous variation in exposure due to some third variable (i.e., the instrument). Examples of instruments for exposure have included soldiers' combat eligibility (Grossman et al., 2015), crop suitability (Acharya et al., 2016b), historical incidence of serfdom (Dower et al., 2018), historical settlement of Scots-Irish migrants (Grosjean, 2014b), religious polarization (Dower et al., 2018), weather shocks (Rozenas & Zhukov, 2019), distance to sulfur mines (Wang, 2019), and proximity to railroads (Zhukov & Talibova, 2018) and historical road crossings (Grosjean, 2014b).

The validity of this approach rests on four assumptions: (a) the instrument strongly predicts exposure, (b) the instrument's effect on exposure is monotonic, (c) the instrument is exogenous to exposure and the outcome, and (d) the instrument only affects the outcome through its impact on exposure. While the first two conditions are relatively straightforward to test, the other two are not. The last identifying assumption in particular—that the error term in the structural equation is orthogonal to the instrument (the exclusion restriction)—is fundamentally untestable.

While it is impossible to conclusively rule out all potential violations of exclusion restrictions, there are things one can do to better establish their plausibility, including placebo tests, statistical tests of specific alternative pathways, qualitative evidence from secondary sources, and complementing instrumental variable results with those from a second, parallel estimation strategy that does not rely on such assumptions (e.g., Rozenas et al., 2017; Rozenas & Zhukov, 2019). Most studies, unfortunately, do not go this far. One review of articles employing instrumental variables methods in top political science journals (Sovey & Green, 2011) found that an overwhelming majority of articles published since 1985—and almost half since 2003—offered no justification for exclusion restrictions at all. Very few present a battery of tests similar to those outlined in this section.

A conceptually similar approach is the regression discontinuity design (RDD), which exploits exogenous variation in exposure due to a threshold value on a “forcing” variable, such that all observations above the threshold are exposed and those below are not. In cases where the assignment mechanism is not deterministic (fuzzy RDD), identifying assumptions are similar to those for instrumental variables: The forcing variable's effect on exposure must be monotonic, and the forcing variable must affect the outcome only through exposure. RDD is a favored estimation strategy in “natural experiment” studies that examine geographic variation across administrative boundaries (Mattingly, 2017; Rozenas et al., 2017). The forcing variable in such cases typically is distance to the boundary, and the threshold is where that distance is zero. As with instrumental variables, however, the exclusion restriction is fundamentally untestable.

A third estimation strategy is difference-in-differences (D-in-D), which requires data on two groups (exposed, not exposed) at two times (before and after exposure). A D-in-D analysis compares changes over time within the exposed group to differences over time in the nonexposed

group. This approach makes no assumptions about unconfoundedness across groups at the same time (or within the exposed group over time) and exploits the presence of multiple nonexposed groups to adjust for unmeasured confounders. Montalvo (2011), for example, uses D-in-D to estimate differences in voting—before versus after terrorist attacks—among resident Spanish voters, compared to nonresident voters. This within-group variation, strictly speaking, need not be over time and can be over any other salient dimension. For example, Fouka and Voth (2019) use D-in-D to show that tension in Greco-German relations in 2008–2014 had a systematically larger effect in areas that suffered German reprisals during World War II.

The key assumption behind D-in-D is that of parallel trends. According to this assumption, outcomes in the exposed and nonexposed groups would need to have followed the same trajectory in the absence of exposure. This assumption is difficult to test and is usually shown by comparing trends in preexposure data.

Where the parallel trends assumption is unreasonable—and particularly where the number of exposed units is low, and/or the nonexposed units cannot reproduce the counterfactual of interest—synthetic controls can offer a potential alternative. Synthetic control units are convex combinations of multiple nonexposed units, which—once combined—approximate the preexposure characteristics of the exposed unit. After constructing this unit, a researcher compares observed postexposure outcomes in the exposed unit with postexposure outcomes in the synthetic control unit. For example, Abadie and Gardeazabal (2003) uses a weighted combination of two Spanish regions to mimic the economic evolution of Basque country in the absence of terrorism. In his study of the Madrid bombings, Montalvo (2011) uses provinces containing nonexposed voters as the basis for a synthetic comparison group. Because the weights used in the construction of synthetic controls are selected to minimize imbalance on a set of observable preexposure covariates, the credibility of this identification strategy rests on an assumption of no unmeasured confounders—which, like many of the other assumptions discussed here, can be difficult to defend.

Posttreatment Bias

A second major set of methodological challenges stems from the fact that, in many applications, a lengthy period of time separates initial exposure from observed effects. For example, if one wishes to study the effect of Holocaust exposure on the future political attitudes of survivors (e.g., Wayne & Zhukov, 2019), one must contend with the fact that survivors have had many experiences in the intervening 75 years, some of which may have impacted their political beliefs. Quite a few of these experiences (e.g., moving to a different city, embarking on a particular profession, joining community organizations) may themselves be consequences of exposure, as victims seek to cope with or overcome their trauma. Some of these experiences may be informative for understanding the mechanisms behind long-term effects. Yet incorporating posttreatment information into one's analysis does not always help illuminate these channels and is quite likely to bias estimates of exposure's effect.

Conditioning on posttreatment information can induce confounding because—after such conditioning—the treatment (exposed) and control (nonexposed) groups no longer have potential outcomes equivalent in expectation. If posttreatment variables are themselves consequences of exposure (e.g., if the treatment is “conflict” and the posttreatment variable is “displacement” or “migration”), the effects of the latter will be difficult to distinguish from the effects of the former. If a posttreatment variable is endogenous to the outcome (e.g., if the outcome is “political attitudes” and posttreatment variable is “member of a conservative political party”), conditioning on it can further induce spurious correlations between the treatment and the outcome (Acharya, Blackwell, & Sen, 2016a). The direction of the bias may be either toward zero (underestimating the effect) or away from zero (overestimating it), depending on one’s model. There are some situations where posttreatment bias should be avoidable, by simply not conditioning on any postexposure information. If one assumes that treatment assignment is ignorable given a set of observed preexposure covariates, then additional adjustments for postexposure variables should be unnecessary (Rosenbaum, 1984). Yet even in cases where this assumption is plausible, as in experimental research with randomized treatment assignment, scholars routinely engage in posttreatment conditioning. In a review, Montgomery, Nyhan, and Torres (2018) find that 46.7% of experimental studies published in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics* from 2012 to 2014 conditioned on posttreatment information. In many other cases, including most observational research, the conditional ignorability assumption is more problematic. If treatment assignment is not ignorable conditional on observed pretreatment covariates alone (e.g., where available preexposure information is scarce), but is ignorable only if one also considers a set of unobserved pretreatment covariates, then adjustment for only the observed pretreatment variables will yield biased estimates (Rosenbaum, 1984). No simple solution exists for this problem (see Imai, Keele, & Yamamoto, 2010), but in practice scholars have tended to err on the side of conditioning on more covariates rather than less, even if those covariates are posttreatment. Acharya et al. (2016a, p. 512) find that “two-thirds of empirical papers in political science—published in the *American Political Science Review*, *American Journal of Political Science*, and *World Politics* between 2010 and 2015—that make causal claims condition on post-treatment variables.” Yet unless these posttreatment variables are plausible surrogates for unobserved pretreatment variables (Rosenbaum, 1984, pp. 662–663), such a strategy does little to address the problem of unobserved pretreatment confounders and only aggravates the problem of posttreatment bias.

These challenges complicate estimation of the effect of exposure and limit our understanding of the mechanisms behind the effect. In light of these challenges, some scholars have turned their attention to estimating average controlled direct effects (ACDE): The effect of exposure when mediating variables are held constant at a particular level (Acharya et al., 2016a). One such approach is sequential-g estimation (Joffe & Greene, 2009; Vansteelandt, 2009), which transforms the dependent variable by removing from it the effect of postexposure variables and then estimates the effect of exposure on this demediated outcome. Another is telescopic matching (Blackwell & Strezhnev, 2018), which uses nonparametric matching to impute counterfactual outcomes for fixed values of each mediating variable, and then uses these imputations to help estimate the direct effect of exposure while holding the mediating variables constant. While these estimation strategies cannot definitively establish the primacy of a hypothesized causal pathway,

a nonzero ACDE implies that the effect of exposure is not driven exclusively by one particular mechanism (Acharya et al., 2016a)—enabling scholars to rule out specific alternative explanations of their findings.

Why Do Historical Legacies Exist?

Asking about the origin of historical legacies is akin to asking why history exists (Nunn, 2009). Despite the literature’s efforts to establish that violence has a measurably persistent effect on attitudes and behavior, an important question remains: Why? No consensus exists as to what transmits and maintains these legacies. Relatively little is known about the process of aggregation: How multiple individuals’ vectors of exposure combine into lasting cultural products, shared narratives, and institutional and biological change.

The mechanisms behind violence’s lasting effects generally fall into two categories: mechanisms of persistence and mechanisms of transmission. Mechanisms of persistence focus on the first-order relationship between exposure to a violent act (or to information about it) and the attitudes and behavior of directly exposed individuals—how individuals process their experiences, what lessons they draw, and how durable these lessons are in shaping individuals’ preferences and choices. Mechanisms of transmission address the diffusion of these lessons to second- and third-order individuals, elevating the first-order effect into a broader social “legacy.” While the literature often uses these terms interchangeably, persistence and transmission are conceptually and empirically distinct. If persistence explains the durability of effects within individuals over time, transmission explains the communication of these effects across individuals.

Research on violent legacies often assumes that transmission and persistence operate via the same channels, and that their expected effects are the same across first-, second-, and third-order individuals—in kind, if not in degree. Yet second- and third-order exposed individuals by definition have no direct lived experience of the original traumatic event or process. There are reasons to doubt that the behavioral lessons of a violent event would persist for the same reasons within victims as within their children, neighbors, or associates (Charnysh, 2015). As discussed in the section *Transmission Beyond First-Order Individuals*, processes of transmission and persistence are indeed synergistic for some mechanisms. In many cases, however, a logical distinction between the two pathways is possible.⁸

Persistence Within Individuals

The term “persistence” has taken on multiple meanings in the literature. First is a continuance of violence’s effect within an exposed individual over the course of that individual’s lifespan. Second is a continuance of an effect across generations, within the family or household of the exposed individual. Third is continuance of the effect within a broader community or social group. Because the second and third meanings require some interpersonal transmission to operate, only the first of these meanings represents persistence in its purest form.

While narrow, this definition does not entirely exclude the possibility of social and environmental influences, insofar as they might push a person to reflect or reconsider one's experiences. Nor does it necessarily limit the scope of inquiry to individuals who were directly exposed, since secondhand or thirdhand knowledge of an event also has the potential to shape attitudes and behavior. Yet if one is to discuss persistence as a mechanism separate from transmission, the theoretical focus must remain on how long (and why) an effect lasts within an individual following direct or indirect exposure, rather than the process by which individuals communicate their experiences to others.

Trauma

One mechanism of persistence that often takes central stage in research on individual exposure to violence is trauma and the attitudinal and biological effects of PTSD.⁹ For example, Grossman et al. (2015) suggest that PTSD and the psychological distress of combat may explain why combat exposure leads to negative attitudes toward peace resolution. Getmansky and Zeitzoff (2014) similarly argue that the threat of rocket attacks tied to the projected range of rockets increases PTSD levels, irrespective of actual rocket hits—leading to increased hawkish, right-wing voting behavior.

Exposure to trauma may create lasting biological or psychological change (Yehuda et al., 1994) and can yield a variety of processing and coping responses. Psychological distress and an existential fear of violence can contribute to persistent feelings of vulnerability and threat (Canetti-Nisim et al., 2009). This psychological state may lead to long-term increases in hostile attitudes and behaviors toward perceived outgroups (Canetti-Nisim et al., 2009). Coping can be individualized, manifesting itself through security-seeking behavior, a reinforcement of in-group affinity, and increases in long-term aggressive behavior (Jakupcak et al., 2007). It can also be shared, as victims (or perpetrators) talk through memories of the event with others, and share narratives (Voigtländer & Voth, 2012) and attitudes (Grossman et al., 2015).

Importantly, PTSD can arise following both direct and indirect exposure to violence, as individuals learn of the harrowing experiences of others (Johnson & Thompson, 2008; Schwarz & Kowalski, 1991). This “secondhand” trauma may help explain why individuals who did not personally experience violence may still experience some of its effects. On its own, however, trauma is not a mechanism of transmission. It cannot explain why some second-order individuals learn of the traumatic event, but not others, or why trauma can cascade through a community, becoming a fixture of a group’s shared identity and outlook. Trauma explains persistence, not interpersonal transmission.

Transmission Beyond First-Order Individuals

Why do individuals who were not originally exposed to violence come to behave and think like individuals who were? One can think of this formative process as a type of informational diffusion, where an initial act of violence conveys a signal (e.g., about an actor’s ability or willingness to hurt), those who receive the signal transmit it to other parties, and the perceived

“lessons” of violence gradually coalesce into shared attitudes and local norms of political behavior. Transmission channels can vary from relatively small social networks (e.g., family, community) to broad informational campaigns (e.g., history textbooks, propaganda).

In a sense, transmission mechanisms explain the persistence of effects beyond first-order individuals—within family, community, or society. Where the distinction between transmission and within-individual persistence becomes blurry is in cases where this process generates feedback loops, with the firsthand sender of a signal becoming a third- or fourth-hand receiver of the same signal. Such feedback can potentially reinforce one’s acquired attitudes and behavior, or it may spark a reassessment—depending, in part, on whether the new signal is at odds with the original (Nyhan & Reifler, 2010). Due to the centrality of interpersonal communication to this feedback process, these borderline cases fall into the “transmission” category in the current discussion—although the two mechanisms can interact in potentially complex ways.

Family Socialization

Central to much of the current theorizing about the transmission of legacies is family socialization: children adopting the behavioral patterns, anxiety, or attitudes of their parents. At the root of this literature is social learning theory, which posits that individuals learn through observation and imitation (Bandura, 1969). According to this view, a child’s political socialization begins early, before formal education. Sapiro (2004) makes a case for close study of childhood socialization, noting that children have the cognitive capacity to process political arguments by age 4 or 5. By the first year of primary school, children already exhibit consistent, structured political orientations influenced by their environment, with more developed orientations associated with higher levels of social capital (Van Deth, Abendschön, & Vollmar, 2011). Yet measuring children’s political attitudes vis-à-vis parental attitudes invites new measurement challenges since not all children are willing or able to participate in traditional forms of political discourse (Pauliina, Kallio, & Häkli, 2011).

How does parental exposure to violence transmit to a child? The primary vehicles for family socialization are within-family discussions of politics and the gradual inculcation of shared values that frame political decisions (Jennings & Niemi, 1968). Openness to political learning and attitudinal change tends to decline with age, leading to a crystallization of political beliefs as individuals leave young adulthood (Stoker & Jennings, 2008). While children’s independence and broader attitudes toward parents may mediate the strength of socialization (Miklikowska, 2016), political views tend to be much more closely aligned between children and their parents than even economic status (Jennings, Stoker, & Bowers, 2009). Most of the evidence for these claims, notably, comes from survey data on the United States and examines political views more broadly, rather than the legacy of violence in particular.

More recent work has added nuance to assumptions about socialization, suggesting that children and parents are bi-directional transmitters of political attitudes, depending on the strength of family bonds like trust and empathy (Miklikowska, 2016). This broader sense of family responsibility and concern for one’s parents or children appears to be a critical condition for family socialization. For example, Palgi et al. (2015) find that the continued salience of Holocaust

memories across generations, from survivors to their children and grandchildren, depends on levels of family involvement—defined as empathizing with other generations and tying personal success to redemption from victimhood narratives. Research on the children of Holocaust survivors has challenged expectations of a linear decay of effects across generations, revealing significant variation in the strength and nature of transmission. While some phenomena, like “psychic numbing” and denial, can weaken parent-to-child transmission, others, like the inculcation of guilt, anger, and respect, may strengthen it (Prince, 1985; Solkoff, 1992).

Community and Peer Influences

Outside the household, community and peer influences play important roles in the transmission and persistence of legacies of violence. Community-based socialization is a “process through which actors adopt the norms and rules of a given community” (Checkel, 2017, p. 592). This process can entail the construction of meaning through performative acts, group bonding over shared experience and attitudes, or shared culpability and trauma. As with family socialization, the effects of group-based identity formation depend in part on the age of individuals on the receiving end, with children and adolescents being particularly susceptible to socialization (Bauer et al., 2013). The resulting collective memory, “at least partly constructed through explicit acts of story- and symbol-creation” (Sapiro, 2004, p. 10), becomes part of people’s lived experiences, irrespective of the originating violence.

This socialization often comes in reinforcement of a shared narrative recounting a defining traumatic event in a population. For example, Dinas and Fouka (2018) find that priming Greek respondents to think about forced displacement during the Asia Minor catastrophe tends to increase sympathy and donations for Syrian migrants in Greece. These narratives can calcify divisions along a particular political fault line, such as left-right divisions in the Spanish Civil War (Balcells, 2012). As Rees, Allpress, and Brown (2013) note, shared narratives can be primed to manifest as a form of moral shame, such as memories of Nazism increasing support for a German Turkish minority, or image shame, such as the Iraq War increasing social distance in the United Kingdom toward Pakistani immigrants.

More ominously, shared narratives can aid in the mobilization of popular support for violence and repression. Voigtländer and Voth (2012) show that localities with centuries-long traditions of anti-Semitic attitudes and pogroms in Germany reliably predict violence against Jews in the 1920s and votes for the Nazi Party. Looking at a much shorter timeframe, Bateson (2017, p. 635) argues that wartime socialization of civilians in Guatemala helped popularize “the idea that repression is a necessary and effective way to provide security.” Research on the “culture of honor” or “culture of violence” in the U.S. South similarly associates local violent proclivities with Scots-Ulster settlement patterns (Grosjean, 2014b; Messner et al., 2005; Nisbett, 1993). Others note the endurance of “the colonial mentality” manifest through fights over “correct” histories, displays of collective emotion, or as internalized oppression (Volpato & Licata, 2010; Antoniou et al., 2017). All of these arguments underscore the role of collective trauma and a

perpetuation of shared memories through public regurgitation of narratives and symbols or through continued structural violence (Fouka & Voth, 2019; Kirmayer et al., 2014; Rozenas & Zhukov, 2019).

Other community-level determinants of interpersonal transmission include changes in social capital. Social capital refers to aspects of interpersonal relations—“social networks and the norms of reciprocity and trustworthiness that arise from them” (Putnam, 2001, p. 19)—that facilitate coordination and cooperation for mutual benefit. To the extent that exposure to conflict and violence can change the relative strength of social network ties and patterns of community engagement, it can affect the transmission of the lessons of this violence to second- and third-order individuals. The direction of this effect is contested. Some find that conflict increases social capital (Bellows & Miguel, 2006; Hopfensitz & Miquel-Florensa, 2014). Others find that, within localities, violence decreases trust and willingness to engage interpersonally (Cassar et al., 2013; Nunn & Wantchekon, 2011; Wood, 2006) and stifles civic engagement (Zhukov & Talibova, 2018). A third possibility is that conflict can both decrease trust and increase civic engagement (Grosjean, 2014a). Guiso, Sapienza, and Zingales (2016) find that social capital endowments can transmit over long periods of time, creating individual-level persistence in attitudes like feelings of self-efficacy from childhood.

Institutionalization

Violent legacies can become embedded in formal institutions and structures, like borders, written histories, and memorials. These institutions can influence attitudes and behavior in a manner similar to family and community socialization by transmitting information about some defining experience to otherwise nonexposed persons and imposing durable norms of behavior that constrain individual choice. The key distinction is that—unlike the “narrow-casting” of family and community socialization—institutionalization can reach a broader, potentially national, audience.

Borders

The imposition of physical borders—as a result of violent contention or geopolitics—can have long-term effects on attitudes, continued violence, and economic development. By restricting physical movement and communication between communities on each side of a divide, borders can contribute to a gradual divergence of attitudes and behavior among otherwise proximate groups. As such, borders can create new legacies of their own, helping to construct new identities and new distinctions between in-group and out-group. For example, Peisakhin (2012) shows that the areas of Ukraine formerly within the Austria-Hungarian Empire held more negative views toward Russia in the early 2000s, suggesting that institutional legacies of the empire served to reinforce out-group attitudes. Michalopoulos and Papaioannou (2016) show that ethnic groups divided by the partition of Africa are more likely to experience violence and suffer from lower levels of economic development.

Written Histories

The politicization of history is an institutionalized parallel to community narrative building but on a national scale. History classrooms are a central forum for the communication of political legacies to new generations. For societies where large portions of the population were likely complicit in past violence—enthusiastically or not—official interpretations of historical events can span the gamut from denunciation and reflection (e.g., denazification in Germany) to sanitization and rehabilitation (e.g., treatment of Stalin in contemporary Russian textbooks). Schooling can increase or limit social awareness of past victimization and contribute to the construction of shared narratives and identities. The official (re)interpretation of history in the aftermath of violence is therefore a powerful institutional tool for the transmission of attitudinal and behavioral lessons. Unsurprisingly, contemporary fights to reassess official histories have emerged in Bosnia-Herzegovina (Baranovicé, 2001), Ireland (Henderson, 2005), Israel (Podeh, 2000), Japan (Bukh, 2007), Ukraine (Janmaat, 2007), Russia (Zajda, 2007), Rwanda (King, 2013), and many other contexts. Within political science, however, this mechanism of transmission has largely eluded systematic empirical study.

Memorialization and Propaganda

A related mechanism to historical education is memorialization, which seeks to affect the national consciousness by amplifying (or countering) a particular interpretation of the past. An example is the use of war memorials and monuments to political leaders as permanent markers of memory and history (Johnson, 1995; Koonz, 1994; Mayo, 1988; Savage, 1994). Such symbols can shape collective memory by declaring publicly which groups and ideas should be considered central to national identity, legitimizing (or disavowing) a movement's political successors and reiterating (or rejecting) a group's historical enmities (Forest & Johnson, 2011; Koonz, 1994). That said, the existence of state propaganda does not reduce the public to passive recipients of officially sanctioned visions, the imposition of which can also spark backlash and contention (Forest & Johnson, 2002; Huang, 2018; Landrieu, 2018).

Monuments and acts ritualized into regularly occurring memorials form a similarly institutionalized form of narrative imposition, physically projecting a historical vision onto a shared political landscape. Fights over the treatment of places of violence often run parallel to contested political narratives over past action, such as the treatment of concentration camps in postwar Europe (Koonz, 1994) or narratives of Israeli history (Canetti et al., 2018; Zerubavel, 1994).

Physical monuments may also embody shared sentiments or provoke controversy when these narratives are contested. Savage (1994) notes how changing political power in the U.S. South at the end of the Reconstruction era brought a wave of monuments memorializing the “Lost Cause” mythology of the Confederacy. Contemporary debates over Confederate monuments parallel these contested political narratives over past violence (Johnson, 1995; Landrieu, 2018). Likewise, the treatment of Soviet monuments (Forest & Johnson, 2011), as well as the toppling of statues during political upheavals, evoke similarly physical displays of changing historical political narratives.

Other memorializing comes in the form of ritual acts, like commemorative ceremonies, which reinforce shared out-group narratives. For example, Voigtländer and Voth (2012) associate commemorative memorializing, such as anti-Semitic “Passion Plays” and children’s games’ association, with later support for the Nazi Party.

The impact of propaganda on the formation of public attitudes has been a matter of some debate (Tucker et al., 2018). While some have argued that propaganda and misinformation can have deleterious effects on political knowledge and polarization (Sunstein, 2018), others have found the effects to be more limited (Allcott & Gentzkow, 2017) or context dependent (Huang, 2018). The lack of consensus on this question reflects a general shortage of empirical work on propaganda’s impact on violent legacies and highlights a demand for future rigorous investigation of this topic.

Epigenetics and Evolutionary Pressures

Largely missing from political science literature on legacies of violence is the potential transmission of violence’s impact through epigenetic changes (i.e., changes in the expression of genes due to environmental influences) and evolutionary processes (i.e., selection pressures favoring certain traits and strategies).

While it is well established that exposure to violence can produce lasting biological change (Yehuda et al., 1994), relatively little is known about whether these changes might drive potential attitudinal and behavioral variation in subsequent generations. Some social scientists have hinted at the possibility of genetic inheritance, like the “culture of honor” associated with Scots-Ulster ancestry (Grosjean, 2014b; Nisbett, 1993). Smith et al. (2012) use a large pool of data from the Minnesota Twin Study and attribute 60–70% of variation in political attributes to genetics or in-utero environmental factors. Proximate to this line of inquiry is Bowles (2009), who uses mortality data and warfare prevalence from prehistoric human development to model the emergence of prosocial, group-centric behaviors. Another mechanism in this category is a parochial altruism that favors one’s own family, ethnic, linguistic, or social group and punishes norm violators (Bernhard, Fischbacher, & Fehr, 2006). Research connecting in-group social psychology with the evolution of political attitudes is limited, however. The more specific effect of violence exposure on the immediate offspring of affected individuals remains an open empirical question.

State of the Literature

The benefits of understanding how violence—as a defining causal agent of historical change—affects social and political outcomes is enormous. There is mounting evidence that exposure to past violence has durable, potentially intergenerational effects on behavior and attitudes, and scholars have made significant strides in building a research program around a demanding set of theoretical problems. Yet many challenges and unanswered questions remain. There are difficulties this line of inquiry continues to face in four areas: measurement of exposure, generalizability, mechanisms of transmission and persistence, and lessons for policy.

The Meaning of Exposure

Research shows that one does not need to directly experience violence to feel its effects (Novick, 2000). Plausible counterfactuals for exposure to violence can be hard to find, particularly as the scale of violence grows. In a global war like World War II—which affected life on virtually every continent—imputing a set of “nonexposed” counterfactuals is exceedingly difficult. The same is true when an entire population is subject to repression or institutional change, or if systemic violence eliminates an affected group entirely. Moreover, at higher levels of violence, direct participation or victimization may be unnecessary to experience its effects. Yet even in such extreme cases, variation still exists in the timing, nature, and severity of individual experiences.

These complexities have implications not only for the counterfactuals one uses to estimate violence’s impact (e.g., some violence vs. none, or a lot vs. some) but also for the measurement of individual exposure. By excluding second- and third-order exposure from the “treated” group, one potentially underestimates the true scope of violence’s impact in society. Yet expanding this measure to include second- and third-order exposure—itself a legacy of violence—entails some nontrivial estimation challenges and obscures fundamental experiential differences between active participants and bystanders, victims, and witnesses.

Measurement of exposure to violent events is nearly universally tied to physical nearness to violence. While this assumption may hold for those who directly experienced violence, second- and third-order exposure—through socialization or other channels of diffusion—may not operate through physical proximity. For example, traditional and social media can transmit information about violence and its effects in manner largely unbounded by geography (Bryant, 2016; Eriksson, 2015). It is unclear whether such modes of transmission can produce lasting trauma in the absence of physical proximity. One’s receptivity to such information may also be endogenous to exposure, as some individuals turn to social media as a means to cope with trauma, while others opt out (Eriksson, 2015; Salzmann-Erikson & Hiçdurmaz, 2016).

An alternative to measuring exposure by proximity is survey measurement of self-reported event memories or PTSD or measurement of biological changes after the fact (Bowles, 2009; Yehuda et al., 1994, 2008). Like physical proximity, these ex-postmeasures do not directly capture violence exposure. They are also subject to confounding by any postviolence or secondary experiences that an individual might have had. As the meaning of exposure becomes more individualized and multidimensional, it becomes harder to separate the various forces acting to reshape attitudes and behavior. The consequences of these measurement concerns for legacies of violence research depend on the framing of exposure and its subsequent theorized impact on the individual.

Generalizability

The literature has tended to tie historical legacies to specific geographic contexts. Whether the patterns observed in particular national or subnational cases can carry over to other violent contexts is generally unclear. The Holocaust remains a dominant case in the literature, albeit one that is difficult to separate from the historical experience of the Second World War in Europe. Research on colonialism, with some exceptions (Mattingly, 2017), centers on sub-Saharan Africa.

Research on institutional legacies outside of colonialism tends to focus on Eastern Europe (communism) and the United States (slavery). Relatively little work has emerged on Central America, Central Asia, or the Pacific. Methodological efforts have focused on establishing internal validity in a manner that often places a premium on deep knowledge of a particular case (e.g., by developing creative instrumental variable and regression discontinuity designs). The drawback is that such efforts necessarily entail a trade-off between the credibility of microlevel results and the ability to generalize beyond them.

This problem is not unique to the literature on legacies of violence and is quite pervasive in subnational conflict research more broadly (see a review in Zhukov et al., 2019). As standardized data on local violence and political behavior become increasingly available (e.g., x-sub, grid.prio), cross-national comparisons and meta-analyses will become more feasible. The greater challenge will be in the development of estimation strategies that are equally applicable across countries and contexts.

Transmission and Persistence

While scholarship has begun to take seriously mechanisms of legacy formation, large gaps remain in understanding why some effects perpetuate while others decay. Much remains unknown about the relative influence of family-, community-, and national-level socialization on this process and how these transmission mechanisms interact. The literature has yet to take stock of why some social networks and information channels facilitate adaptation and change, while others suppress these effects from taking hold. Nor is it clear why the persistence of these attitudinal and behavioral effects varies among individuals with direct experiences of violence and among those with only second- or third-degree knowledge of an event. Many studies conflate mechanisms of transmission and persistence or minimize conceptual distinctions between them. While this approach may be justified in some applications, it has stymied theoretical development in others. Without a more refined understanding of transmission and persistence mechanisms, it will be difficult to construct a satisfying answer to the aggregation puzzle: Why and how individual exposure scales up to community- and national-level effects. When a violent effect moves from traumatizing one or several individuals to tens of thousands, can such an outcome result solely from interactions between networked individuals, or does it require a broader campaign to increase education and awareness? Answering such questions will require a great deal of theoretical brush-clearing, while also overcoming formidable challenges of measurement and estimation.

Implications for Policy

Violence is a recurring feature of political life in many countries around the world. Yet social science literature on legacies of violence has been somewhat reluctant to consider how its insights might help foster resilient communities or restore violence-ridden locales. Research has shown that exposure to this violence can have lasting political, social, and biological effects—some positive (Bateson, 2012; Blattman, 2009) and others decidedly not (Grosjean, 2014a; Yehuda

et al., 1994). The literature further suggests that certain structural and contextual factors, like strong social network ties and the threat of resumed violence, can mitigate and even reverse some of these effects (Johnson & Thompson, 2008; Rozenas & Zhukov, 2019).

Whether violence leads to lasting legacies of cooperation or division has profound implications for the design of public policy, the distribution of resources, and efforts to mitigate future conflict. The continued political salience of historical violence is also potentially informative for localized political campaigning and outreach, as incumbents and challengers tailor their messages to different groups of constituents. If violence indeed reduces trust and increases out-group prejudice (Beber et al., 2014; Mackie, Devos, & Smith, 2000; Zeitzoff, 2014), post-conflict communities may be particularly susceptible to nationalist mobilization, and populist, exclusionary appeals. If—as research suggests (Costalli & Ruggeri, 2015)—violence creates openings for new political entrepreneurs and political realignments, one might further expect politics in such communities to be quite volatile and ripe for disruption.

The intergenerational persistence of violence's effects poses difficult questions for efforts to rebuild social and political trust. The facilitation of interpersonal communication between perpetrators and victims—a cornerstone of restorative justice—has a different meaning for an event's direct participants than it might for second- and third-generation descendants. As shared experiences of violence coalesce into group narratives and identities over time, efforts at reconciliation—particularly those that challenge these narratives and identities—may threaten to undermine in-group cohesion, creating political risk for those involved. The aggregation of individual experiences into group-level trauma places upward pressure on the kinds of reformatory actions and policy concessions that victimized groups might consider necessary for reconciliation.

For many of the reasons outlined in this article, empirical research on legacies of violence is not yet in a position to yield clear and practical lessons for policy. Many of the literature's most intriguing findings—like backlash effects or the phenomenon of “post-traumatic growth”—are uncertain and context dependent. Estimating the direction and magnitude of these effects in observational settings is difficult due to the nonrandom, potentially endogenous nature of exposure. Adjustment-based efforts to establish causality have limitations of their own, and the assumptions behind many identification strategies can be difficult to defend. Even where evidence of causality is more compelling, the mere existence of an effect does not always yield clues about why the effect exists. When one further considers the long time horizons involved, and the host of postexposure factors that might confound one's analysis, it becomes difficult to view any particular result as “conclusive” enough to produce actionable policy prescriptions.

The same, of course, can be said for many other lines of inquiry in social science. There is no universally credible estimation strategy, and the search for one often produces local estimates that—however rigorous—are difficult to generalize to other contexts.

Further, even if all the technical challenges discussed here are ultimately resolved, others will surely arise. This does not mean that drawing policy lessons is a futile exercise, particularly given the frequency of the phenomenon at hand and the potential of this research to shed light on its social, political, and economic impact. Yet the default setting in such an exercise should be humility and caution rather than certainty and conviction.

Conclusion

Research on legacies of violence draws from a wide range of disciplinary backgrounds, methodologies, and contexts. It offers promising opportunities for cross-disciplinary and mixed-methods analysis, and raises new, difficult, and beguiling questions that are likely to keep researchers busy for some time. Like any new research tradition, this one is not without pitfalls and obstacles.

These problems are not insurmountable. Many of them stem from underspecification of scope conditions and mechanisms, in a literature where theoretical development has tended to lag behind empirical innovation. Due to the relative novelty of this field, and the inherent difficulty of identifying long-term effects, scholars and reviewers have generally prioritized a high empirical standard of evidence over theoretical depth. It is hard to explain *why* these legacies exist while so many doubts linger over *whether* they exist. The good news is that demand for such theoretical development is increasing as evidence on the *whether* question continues to accumulate. The remarkable strength and durability of these effects compels scholars to better understand how and why legacies of violence emerge, spread, and persist.

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Notes

1. “legacy, n. and adj <<https://www.oed.com/view/Entry/107006?rskey=JTLwJG&result=1>>.” *Oxford English Dictionary*. September 2019. Oxford University Press.
2. This conceptualization fits within Wittenberg’s framework for conceptualizing legacy type arguments: “To summarize, researchers construct legacy-type arguments with three components: an outcome that is not fully explainable from causes contemporaneous with that outcome, a cause or correlate that existed prior to the outcome, and potential (or at least speculative) links between the antecedent and the outcome. Whether the existence of a legacy has been established in a study depends, of course, on the quality of that study’s argument.” See Wittenberg, 2015, p. 369.

3. Separating the legacy of institutional exposure into distinct historical phases (e.g., before, during, and after) poses some methodological challenges. Often, the literature organizes these historical “moments” into distinct “pretreatment,” “treatment” and “posttreatment” periods, like a quasi-experiment. Of course, in a long-run historical legacy, there is rarely a clear and observable pretreatment baseline against which one might make comparisons (e.g., local attitudinal and behavioral patterns prior to colonization). The imputation of such a baseline presents numerous difficulties in large, nebulous timespans (e.g., colonialism and decolonization are gradual, decades-long processes). Furthermore, data availability on this baseline may be tied to the treatment itself (e.g., record-keeping by colonial administrations).
4. It is worth noting that the literature on war and state-building advances a different set of theoretical mechanisms (e.g., in the case of Tilly, 1985, the state’s need to raise revenue for war making) and generally examines societies at earlier stages of political development. The fact that the two sets of findings are at odds, however, suggests that some of the conditions conducive to state-capacity-building may also be detrimental to subsequent state-society relations.
5. Another explanation is that wartime experience imparts new skills, some of which have the potential to change attitudes. Jha and Wilkinson (2012) find that in South Asia communities with greater World War II combat experience acquired strong organizational skills, which helped facilitate economic codependency between groups and “safe havens” from ethnic cleansing during the partition of India.
6. Although the terms are often used interchangeably, we define *transmission* as conveyance of effects from individual to individual and *persistence* as continuity of effects within individuals over time.
7. The validity of priming experiments rests, in part, on subjects’ unawareness that a manipulation is taking place. Whether these conditions actually hold in practice can be established through funnel questionnaires and other tests of conscious awareness (Doyen, Klein, Pichon, & Cleeremans, 2012).
8. There are competing, often opposing, delineations of transmission and persistence mechanisms in the literature. This review focuses on the point at which a mechanism affects an individual’s attitudes and behavior to distinguish these competing frames—persistence for first-order exposed individuals, and transmission of effects to new secondarily exposed individuals. Persistence becomes the continuance of violence’s effects within initially exposed individuals over time. Transmission is the spread of effects beyond these first-order exposed individuals across space. This is different from, for instance, Charnysh’s (2015) preferred framework, which considers transmission across generations (e.g., through shared family and community narratives, textbooks, memorials, and mythologizing) to be a form of persistence. However, the two approaches are not fundamentally at odds. In the current reading, mechanisms of interpersonal transmission allow the effects of violence to *persist* in the aggregate and in the long run.
9. As the section Public Health Outcomes notes, the development of PTSD is itself a consequence of exposure. Yet because individuals cope with their trauma in a variety of ways, the experience of living with PTSD can also drive downstream variation in behavior and attitudes.

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