Contrary to the title of the introduction to the 28 July special issue, "Violence: no silver bullet," there is indeed a "silver bullet" for violence—or at least an evidence-based, tested, and testable model of violence development predictive of effective measures of prevention and intervention. It is criminologist Lonnie Athens' 1992 identification of a four-stage process of violent socialization ("violentization") common to all of the incarcerated violent criminals he interviewed, but incomplete or absent in nonviolent individuals with experience of violence who were interviewed as controls (*1*).

The four stages that Athens identified, typically traversed in childhood and early adolescence, consist of (i) brutalization (being violently subjugated physically and/or psychologically, experiencing valued primary group members being violently subjugated, being violently coached); (ii) belligerency (taking stock of brutalization in the light of violent coaching, culminating in a qualified resolution to respond to further serious provocation with serious violence); (iii) defensive violent performances, which, if successful, culminate in (iv) virulency (responding to the empowering social trepidation and violent notoriety evoked by successful violent performances with a more fundamental, unqualified resolution to use unprovoked violence).

Athens' violentization model implies that many different strategies of prevention and intervention can successfully prevent violent behavior by diverting candidates from proceeding further through the stages of the violentization process. Protecting children from brutalization should be the single most effective public health measure, because individuals who are not brutalized do not need to make the further choices that lead to violent outcomes.

Violent behavior is a consequence of violent socialization. Soldiers and police officers learn it through institutional, truncated forms of violentization and apply it defensively in our behalf. Any theory of the etiology of violent behavior should explain professional violence as well as criminal. Athens' model does. It deserves to be included in any discussion of violence and "silver bullets."

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Conflict and Resolution in Primates—All Too Human?

In his review "Primates—a natural heritage of conflict resolution" ("Violence" special issue, 28 Jul., p. 586), Frans de Waal provides three examples of conflict resolution involving third parties: policing and pacification, triadic reconciliation, and third-party mediation. He suggests that all of these interactions might be variations on a basic behavioral mechanism. Indeed, such a mechanism, known as the triangle, has been described in models of human family dynamics (*1*). If a relationship between two individuals is disturbed, enlisting a third party may defuse the disturbance, thus activating a triangle (two interactants and one intervener).

Relationships among three individuals have emergent behavioral properties (*2, 3*). Triangles should be more stable than relationships between two people because the multiple interconnecting relationships provide pathways for anxiety to shift among individuals. As long as a low-stress situation can be maintained, two individuals can minimize the costs of stress and maintain a stable relationship. How-

ever, when situations arise that cause disruption, this may destabilize the relationship, with an attendant increase in anxiety. If involving a third party sufficiently defuses the disturbance between the original opponents, the social environment may have a predictable structure (*2*).

De Waal says that "[c]onflict in valuable relationships induces greater anxiety, which in turn creates a greater need for calming [postconflict] contact with the opponent." It seems possible that a disruption of a valuable relationship may create a need for resolution by third-party intervention, as has been predicted for human relationships. The triangle may also be a little-recognized way of resolving conflicts in nonhuman animals.

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By focusing on the reconciliation of aggression in primates, de Waal highlights an important aspect of animal behavior, but neglects another major aspect of the story. Conflict reconciliation is common in some species in intragroup aggression, but rare, often nonexistent, in intergroup conflicts.

Aggression between social groups is frequent in many species of nonhuman primates—such aggression is often violent and seldom followed by reconciliation. This is especially true in macaques such as the rhesus, in which overt fighting is common. Jane Goodall summarized aggressive behavior in chimpanzees by noting, "Chimpanzees in small compatible groups may maintain peaceful relationships for hours or days. Nevertheless, they can be easily aroused to sudden violence...particularly directed at individuals of neighboring social groups" (*1*). In the classic literature of ethology, Konrad Lorenz described a similar behavior in social groups of Norway rats: "Members of a clan assist each other in fights against strangers, and thus a smaller clan is at a disadvantage in fights against a larger one"(*2*).

Many animal species show aggressive xenophobia against nongroup individuals (*3*). Such behavior, which is not discussed by de Waal, is rarely reconciled peacefully. He appears to reject the ethological concept of innate aggressive tendencies in favor of innate social reconciliation, but can we have one without the other?

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Response

Southwick's comments about intergroup violence are on the mark. Such violence does occur in many animals and is usually not embedded in the same social matrix existing within social groups. Near the end of my review, I conclude as much about the Relational Model, stating that "[c]ertain forms of aggression, such as warfare and random shootings, fall outside this framework" (p. 590). Nevertheless, there are intriguing indications that reconciliation does occasionally occur after confrontations between primate groups, in-

cluding rhesus monkeys (*1*). This issue deserves further study.

However, I must disagree with Southwick's conclusion that I reject innate aggressiveness. Animals would have had no need for mechanisms to control aggression, and to repair the damage it inflicts on social relationships, if open conflict had not been pervasive. Attention to conflict resolution needs to go hand in hand with attention to the sources of aggression, genetic and otherwise.

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Cause of Seal Die-off in 1988 Is Still Under Debate

Peter Ross and his colleagues review in their letter (15 Sept., p. 1878) their findings on immune disruption by polychlorinated biphenyls (PCBs) in harbor seals, a rare example of controlled experimental research much needed in marine mammal toxicology. There is no question that PCBs can pose threats to humans and wildlife. Their letter was prompted by my comment (Letters, 16 June, p. 1965) that a 1988 die-off of seals in Europe was due to a morbillivirus epizootic. This contrasted with a report by Jocelyn Kaiser (News Focus, 21 Apr., p. 424) that implied PCB exposure was the underlying factor spurring this epizootic. I cited evidence that this generalization was inaccurate. Ross and colleagues note that my points were technically correct, but that I did not consider a broader "weight of evidence." They say that "[c]urrent scientific consensus supports the idea that PCBs played a contributory role in the event."

There seems to be, however, no such scientific consensus. This is evident in recent proceedings from two major interdisciplinary, international scientific workshops on marine mammals and contaminants (*1, 2*). The summary chapter (*3*) from a workshop in Norway sponsored by the International Whaling Commission noted the capacity of morbilliviruses to produce high mortality in immunologically naïve populations, well documented in terrestrial mammals before the synthesis of PCBs, and concluded, "at this stage it is unclear whether contaminants had a role in morbillivirus epizootics in marine mammals." The plenary chapter on morbilliviruses in marine mammals (*4*) stated, "given the lethal effects of morbilliviruses, it is...unlikely that organochlorine toxicity had anything other than a marginal effect on mortality" and "there is no evidence that [PCBs] have affected mortality or morbidity due to morbilliviral infection." This review concluded, however, that further investigation is warranted.

The group report on immunotoxicology (*5*) from a second workshop sponsored by the U.S. Marine Mammal Commission also noted an insufficiency of evidence to establish a cause-and-effect relation between exposure to environmental contaminants and injury to the immune system that might affect marine mammal populations. In a recent independent review (*6*), J. R. Geraci and colleagues observed that during the European seal die-off "there was much speculation that the deaths were the result of pollution, and this remained an obsession for the media," but they concluded that the evidence for a causal relation or compounding effect between contaminants and susceptibility to disease in marine mammals was inconclusive.

Thus, much current thinking emphasizes the virulence of morbillivirus infections rather than major roles for PCBs. This has been underscored by this year's die-off of Caspian seals, ascribed to a morbillivirus, with any role for organochlorines currently downplayed (News of the Week, "Canine virus blamed in Caspian seal deaths" by R. Stone, 22 Sept., p. 2017). A precautionary approach certainly should be taken in human and wildlife health issues, and a "weight of evidence" evaluation has a role in environmental management decisions. However, from a scientific standpoint, application of such an approach to the contributory role of PCBs in the severity of the 1988 seal die-off has generated a hypothesis that is difficult to test. Several attempts thus far have failed to find support over an alternative that fits within the bounds of existing data on morbillivirus infections. Lack of consensus has resulted.

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