

How Government Reactions to Violence Worsen Social Welfare: Evidence from Peru

Renard Sexton Emory University
Rachel L. Wellhausen University of Texas at Austin
Michael G. Findley University of Texas at Austin

Abstract: *Dissident violence inflicts many costs on society, but some of the longest-lasting consequences for civilians may be indirect, due to the government's response. We explore how government policy responses affect social welfare, specifically through budgetary shifts. Using subnational violence and budgeting data for Peru, we demonstrate that attacks on soldiers during the budget negotiation period drive a shift from local social services, especially health, to defense. One soldier fatality implies a 0.13 percentage point reduction in the local health budget share (2008–12). Health budget cuts due to a single soldier fatality result in 76 predicted additional infant deaths 2 years later. We show that the effect on health budgeting operates through decreases in women's use of health facilities and postnatal services. We offer evidence that Peru's coercive response indirectly harms civilians due to butter-to-guns budgetary shifts. Our results identify a budgetary mechanism that translates dissident violence into a deterioration in social welfare.*

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/HUZGPJ>.

At the conclusion of its 27-year civil war, Angola's 2003 national budget allocated 78% of government resources to public services. Angola's health infrastructure was decimated, due not only to the direct effects of violence but also to "meager resources allocated to the sector" throughout the war (Selassie 2002). Plans for 2003 included restoring health services that had been chronically underprovided: acquiring and distributing essential medicines, training health workers in rural areas, and upgrading training for nurses working in hospitals (All Africa 2002).

In Mali, in the midst of a violent insurgency, donors have sent targeted food and humanitarian aid, and some governments have provided military equipment and support (Metherell 2013). But general donor reluctance in the face of conflict has affected Mali's budget. In 2012, the budget was cut 20%, such that ministries including health

were "just ticking over . . . paying salaries and investing nothing" (All Africa 2012).

Both Angola and Mali belong to a set of states that have long faced excruciating decisions about how to allocate their limited resources in times of violence. These decisions are especially difficult because the governments in both states are constrained by constitutional balanced budget provisions, not to mention informal constraints. We demonstrate how shifting budget allocations in response to dissident violence can erode social welfare outcomes, as states rob civilian goods such as health in favor of other priorities. That civilians face indirect costs when violence siphons government resources away from social services compounds its tragedy.

Most research linking civilians and violence demonstrates that civilians suffer dramatically at the hands of government and rebel forces and, under some

Renard Sexton is Assistant Professor, Department of Political Science, Emory University, 1555 Dickey Drive, Atlanta, GA 30322 (renard.sexton@emory.edu). Rachel L. Wellhausen is Associate Professor, Department of Government, University of Texas at Austin, 158 W. 21st Street, Stop A1800, Austin, TX 78712-1704 (rwellhausen@utexas.edu). Michael G. Findley is Professor, Department of Government, University of Texas at Austin, 158 W. 21st Street, Stop A1800, Austin, TX 78712-1704 (mikefindley@utexas.edu).

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conditions, are targeted (e.g., see Downes 2008; Eck and Hultman 2007; Kalyvas 2006; Wood 2010). Scholarship on noncombat effects typically focus on cross-national correlations between violence and social outcomes such as health or education (e.g., see Ghobarah, Huth, and Russett 2003; Iqbal 2010; Lai and Thyne 2007). We look to another potential source of negative noncombat effects: the government's response to violence. In so doing, we (1) identify particular welfare effects of government responses to violence, together with the mechanisms generating the effects; (2) employ a credible identification strategy to isolate an independent effect of government response on social welfare; and (3) use fine-grained data to carry out that strategy.

Governments can and do respond to acts of dissident violence through both combat and noncombat means. They may respond with inducement strategies and spend more on social services, in an attempt to win the hearts and minds of the people.¹ In stark contrast, governments might respond to violence with coercive strategies, which have been documented in contexts as diverse as the United States, Europe, and many developing countries (Kalyvas 2004, 2006).² In this article, we argue that choosing coercion can require a resource-constrained government to cannibalize spending on social services in favor of other priorities—shifting resources from “butter” to “guns.” We show that such budget shifts cause short- and long-term deterioration in civilian welfare, including increasing loss of life, particularly at the local level, where cuts are most proximate to the services provided to people in need.

To investigate the link between a government's coercive response and welfare outcomes, we use recently collected World Bank data on budget allocations and spending (*World Bank BOOST Data for Peru* 2016) together with novel subnational data on violence. Peru is a particularly useful context in which to explore the effects of dissident violence on subnational budgeting shocks. In the period for which we have violence data (2008–11), Peru remains the site of government-targeted violence largely in the context of the transformed Shining Path's drug activity. In this period, Peru faced 88 dissident attacks that killed 86 soldiers spread across seven of its 24 departments. Crucially, Peru has a highly centralized budgeting process, and, like Angola, Mali, and many other countries

worldwide, Peru's constitution requires the budget to be balanced (and this provision is followed; Elkins 2016). As such, any attempt by the Peruvian government to increase coercive activities requires boosting defense-related budgets at the expense of other sectors. These characteristics allow us to isolate how attacks influence the national government's budget and its disbursements to the departments. We then use USAID's Demographic and Health Survey (DHS) data (Rutstein and Rojas 2006) to trace effects on infant mortality and women's health facility use (Latin American Public Opinion Project [LAPOP] 2016). In short, we are able to connect dissident violence and poor longer-term health outcomes via the pathway of government budgeting. Peru's status as a middle-income democracy suggests our results can speak to the implications of budgeting for an important segment of the world.

To identify the budgeting pathway, we must confront the possibility that dissidents may respond to government spending at the same time that the government responds to dissident behavior. Further, underlying factors such as economic growth, opposition politics, or foreign aid may affect cycles of dissident violence as well as government spending.³ To address these issues, we take advantage of Peru's clearly demarcated national budget discussion period (September–November), which does not allow for congressional budget changes before September 1 or after November 30. We explain how Shining Path attacks—largely random relative to the budget period—studiously avoid civilian casualties, which allows us to explore the budgetary channel without having to contend simultaneously with direct effects of violence.⁴

We demonstrate that the Peruvian government's response to soldiers killed in the budget period cannibalizes discretionary social spending in the departments in which soldier fatalities take place, primarily pulling from health budgets, to the significant detriment of infant health. In reduced form, using data from 2008 to 2012, one soldier killed implies a shift of 0.13% out of the local health budget, resulting in a predicted 76 additional infant deaths 2 years later. Women's reports of health care usage, including postnatal care, decrease in the wake of budget shifts. We find evidence that the government moves resources from health to defense spending especially when violence is unexpected. We also find evidence of smaller shifts with respect to other discretionary social spending on culture and environment. We rule out that these channels

¹Even a government pursuing this strategy may not have the resources available to succeed, especially if it faces sharp budget constraints.

²A government could choose to deliberately rob social services resources to punish civilians it perceives to be supporting the opposition. We are skeptical that democratic governments, in particular, would make this choice; we provide evidence to rule out this motivation in our context.

³If such factors increase the overall budget pie, normatively problematic outcomes may be mitigated. Our findings reinforce that new funds are also subject to distributional conflicts.

⁴We conduct additional tests on civilian perceptions of changing security to verify that the exclusion restriction is not violated.

directly impact infant health, but these shifts may imply longer-term negative environmental or social effects.

This article theorizes and identifies endogenous government responses to anti-government violence, thereby providing a more complete explanation of the welfare effects of those responses, with a focus on the crucial area of health. From a policy perspective, our argument highlights a role for international donors to supplement otherwise routine social spending in times of violent conflict.

Downstream Consequences of Violence

Violence, by definition, is costly and tragic, and it has negative downstream consequences on broader social welfare. Evidence indicates that war can lead to deteriorations in public health in interstate conflicts (Iqbal 2010), including higher adult mortality (Li and Wen 2005), fewer disability-adjusted life-years due to infectious disease (Ghobarah, Huth, and Russett 2003), increased infant mortality and worse access to potable water (Gates et al. 2012), and negative public health consequences accruing disproportionately to women over men (Plumper and Neumayer 2006). In Peru specifically, scholars have found that the war with the Shining Path led to negative women's health outcomes (Grimard and Laszlo 2013), negative education outcomes (León 2012), and lower monthly earnings (Galdo 2013).

We theorize and test the causal chain that explains correlations between violence and longer-term negative social welfare outcomes. When a dissident group uses violence, a government must decide how to respond to the violence amid the broader set of governance activities in which a sovereign state engages.⁵ A government could choose to ignore violence. Alternatively, a government could invest more in social services to win the hearts and minds of the people, as Colombia has done in some regions in recent years to combat the FARC. Such responses should not necessarily generate worse long-term social welfare outcomes. However, we frequently observe that governments respond to attacks on their forces through increased security and military measures designed to fight dissidents. State adoption of coercive strategies is rooted in a rationalist logic, in which costs imposed on citizens intimidate them and deter their undesirable behavior (Leites and Wolf 1970; Schelling 1966; Shultz 1979).

⁵Some literature on terrorism in particular examines how governments choose their responses (e.g., Thomas 2014), although that line of inquiry does not consider downstream effects of government choices.

Coercive strategies may have been successful historically, but they appear to be decreasingly effective (Lyall and Wilson 2009). Nonetheless, we expect that in a state where dissident violence is long-standing and the government has historically responded through other coercive strategies, dissident attacks on government forces likely generate a coercive response. This expectation is supported by the reality that governments with sticky military doctrines and standard operating procedures tend to respond to acts of violence in very similar ways over time, especially in asymmetric conflicts in which institutions are quite stable and learning is slow (Van Evera 2003, Findley and Edwards 2007).

However, budget dynamics shape the government's ability to fund a coercive response. Finite budgets, especially in developing, conflict-affected countries that face formal and informal hard budget constraints, force governments to make difficult butter-to-guns trade-offs. At the interstate level, government choices can vary based on regime type (Carter and Palmer 2015), partisan orientation (Whitten and Williams 2011), or the size of the economy (Palmer 1990). Some scholarship focuses on trade-offs at the national level (Iqbal 2010; Mintz 1993; Powell 1993; Skaperdas and Syropoulos 2001), with limited scholarship focused on middle-income countries (e.g., Antonakis 1999). While it is possible that a government chooses a coercive strategy that cuts social services but does not shift freed resources into defense, we are doubtful of the usefulness of such a punishment mechanism, especially for a democratically elected government. Cuts in services could punish civilians who support dissidents, and the prospect of increased future funding could induce civilians to change their behavior and withdraw their support, but this process would surely threaten the government's electoral success (Kalyvas 2006). In the Peruvian context on which we focus, we provide evidence that the government acts consistently with a butter-to-guns approach rather than a punishment motivation. Yet with either motivation, an unstated but necessary condition facilitates changes to the budget: The budget must be open to renegotiation.⁶

We aim to link robbed social service budgets to subnational social welfare, but welfare outcomes are, of course, a product of many different factors, from environmental dynamics to human decisions. Is there baseline evidence that government spending can be efficacious? We focus on the effect of government spending on health

⁶Given the extensive literature on bureaucratic incentives, we see it as unlikely that subnational units would deliberately choose not to spend their budgeted funds in order to enact their own coercive strategies. In the Peruvian context, 91% of allocated health budgets are spent (2008–12).

outcomes, both for comparability to the large literatures on health effects of violence and because of high variability in health outcomes. Building on a review of literature in health economics, Farahani, Subramanian, and Canning (2010) estimate the effects of cross-sectional, public health spending on mortality outcomes in India, finding that a 10% increase in subnational health spending decreases the probability of death in the state by 2%, with those effects applied primarily to women, youth, and the elderly. We posit that the opposite may also be true: Lower health spending may be associated with higher mortality rates. If lower health spending is associated with higher defense spending, we see a plausible chain from anti-government violence, to the government's response, to eroding health outcomes. Indeed, in the study of interstate conflict, Iqbal (2010) shows the plausibility that government spending on defense has adverse effects on public health. In order to isolate the budget mechanism driving this chain, we endogenize the government response to dissident violence.

Theory

A government that faces unexpected dissident violence may find it difficult to quickly increase defense budgets in response. For example, 27 national constitutions include balanced budget requirements, with 22 of these in developing or middle-income countries, including Angola, Mali, and our setting of Peru (Elkins 2016). Given the incentives driving sovereign lending, foreign aid, state-to-state finance, and other potential external sources of finance, it is difficult to access external funds to address immediate security threats. Raising new funds from domestic sources is both politically difficult and rarely possible in the short term. Facing formal or informal budget constraints, the government can turn to budget reallocations, boosting defense allocations to localities where new anti-government violence suggests pre-existing allocations were insufficient.

Given domestic political constraints, it is most realistic that a locality's discretionary budgets are robbed as its defense budget is increased, especially as immediate responses to violence generate rewards for local incumbents.⁷ The baseline division of national resources across subnational localities is surely path-dependent, making it relatively impervious to short-term shocks. Additionally,

⁷In Appendix D.1 in the supporting information (SI), we find that dissident violence during the budgetary period just before an election in Peru is correlated with worse incumbent performance. SI Appendix D.2 provides evidence that increasing defense spending in the wake of violence is popular.

it is (at minimum) unlikely for a politician to consent to a budget allocation that does not provide her locality with any direct benefits while also robbing her locality's resources. In our context, peaceful localities or localities with long-standing violence would not derive direct, short-term benefits from increased military action against unexpected security threats elsewhere.

We leverage the reality that, while immediate shifts out of discretionary budgets would be more effective in stopping an emerging threat, in many contexts, budgets can only be reallocated when under negotiation. Because, in practice, social services make up the majority of discretionary budgets across the developing world, our hypotheses focus on the link between the timing of budget negotiations and butter-to-guns transfers.

H1: When unexpected, anti-government violence occurs in a given locality during budget negotiations, defense budgeting/spending in that locality increases while social services budgeting/spending decreases.

Our expectations in Hypothesis 1 imply that anti-government violence during the budgetary period drives an increase in legislative debate on defense issues, including subnational appropriations and the broader fight against dissidents. Our arguments imply that politicians are motivated to make at least some of this debate public and not just behind closed doors.

H2: Anti-government violence during budget negotiations leads to greater legislative debate on defense issues.

As noted, we focus on shifts out of social services budgets in Hypothesis 1 because social services dominate discretionary spending in developing countries. In our setting of Peru, budgets for social services, including health, environment, and culture, make up more than 90% of discretionary spending (*World Bank BOOST Data for Peru 2016*). Analyses of these three budgets together as well as each in isolation should provide support for our theory.

In extending our theory, we focus on the health budget, which alone accounts for 54% of Peruvian discretionary spending. Normatively, adverse consequences of reduced health services would come at a serious cost for citizens who rely on government provision, especially the most vulnerable (Plumper and Neumayer 2006). Infant mortality is the most commonly used indicator in work linking public spending to social welfare, which allows our results to better compare to other empirical studies (Farahani, Subramanian, and Canning 2010). Thus, we hypothesize and test that cuts in local health budgets

lead to worsening infant mortality in localities facing butter-to-guns transfers.⁸ Our theory implies that women's reduced access to health services, and particularly postnatal care, drives worsening infant mortality.

H3: Decreases in local health budgets/spending lead to women's lower access to health services, including postnatal services, and increased infant mortality.

Taken together, our hypotheses establish the full causal chain under investigation. When budget negotiations make it possible, the government robs social services budgets and reallocates resources to defense in those localities experiencing unexpected anti-government violence. Robbing the health budget, in particular, has measurable adverse consequences for social welfare.

Note that this causal chain rests on several priors implicit in our discussion above. First, our theory requires that budgetary shifts at the national level have binding effects on subnational allocations. Second, budget negotiations must be time-delimited. Third, we must be able to account for any direct effects of violence on civilians. Ideally for our purposes, dissidents target government forces but do not directly hurt civilians. Fourth, anti-government violence must be exogenous to the timing of budget negotiations. Fifth, the government must be taking actions in line with a coercive strategy for our expectations to hold. In our research design below, we discuss how the situation in Peru fits these criteria.

Research Design

We use the timing of soldier fatalities in Peru to instrument for government budgets and thus identify the causal effect of budget shifts (Hypothesis 1) on welfare outcomes (Hypothesis 3). Our two-stage approach differs from a traditional instrumental variable analysis in that both the first and second stages of the analysis are theoretically meaningful.

Case Selection

We choose to study subnational effects in Peru for a number of reasons. First, our causal mechanism should be clearest in the presence of hard budget constraints. Usefully, Peru has a constitutional balanced budget provi-

⁸We do not have compelling expectations that the absence of evidence of downstream effects of environment and culture budget cuts in our time window means that there are no effects, nor do we have appropriate subnational indicators to consider.

sion, which constrains both the national government's and subnational departments' ability to run deficits. How credible is that budget constraint? In the period for which we have data (2008–12), the budget constraint is hard; Peru averaged a national budget surplus of 0.96% of GDP.⁹ The budget surplus corresponds with high levels of GDP growth in the period: Although growth fell to 1% in 2009, it averaged 7.5% in 2008 and 2010–12. Note that if GDP growth allowed Peru to delay contentious budgetary trade-offs, it would be more difficult for us to find support for our theory.

Like other democracies, Peru faces informal constraints on budget allocations, especially in the short run. Peru is a unitary democracy, and Peru's budgeting is centralized via a national budget that allocates resources to Peru's departments.¹⁰ Congress has the ability to reallocate budgets, per democratic practices, but reallocation is a product of legislative bargaining. Each of the 24 departments receives roughly the same amount of government funding per capita; threatening this arrangement would be difficult.¹¹ Spending is implemented through the ministries in Lima and their regional offices in each department.¹² This bureaucratic structure makes us confident that budget allocations are not fungible across departments. However, it is possible that the defense budget is more fungible because it is implemented through the military chain of command rather than regional ministerial offices.¹³

Our identification strategy relies on the fact that Peru has a time-constrained budget process. The key player in the budget process is the National Office for the Public Budget (DNPP) within the Ministry of Economics and Finance (MEF). The DNPP independently produces a draft budget between April and July each year. Congressional review takes place during a strict window from September through November. It is in this window that members

⁹Peru had a deficit only in 2009, of 1.14% of GDP, and a maximum surplus of 2.12% of GDP in 2008 (World Development Indicators).

¹⁰Thus, despite some recent decentralization, Peru is more of a unified government that has "substantial strategic powers for all parts of the budget process" (Hallerberg and Marier 2004), not one in which federalism disrupts subnational fiscal discipline (Wibbels 2000).

¹¹Peru's one special administrative unit, Callao (56 square miles), effectively has a departmental government; we exclude it when data are missing.

¹²Some nonprogrammatic funds, in particular related to mining, are transmitted directly to local governments.

¹³Despite our best efforts, we are not able to link outcomes of military spending, like troop placement and movements, to changes in particular departments' budgets. Such data are not available for public collection.

of Congress have the ability to reallocate resources, via line-item changes.¹⁴ The budget is required to be finalized by November 30. At this point, there is nothing a member of Congress can do to reallocate budgets other than wait until the following year's budget period. Thus, the September through November window is key to our identification strategy. The president signs the budget law in early December.¹⁵

Sadly, Peru has experienced persistent dissident violence, although, crucially, this violence has not targeted civilians in the period for which we have data. Peru has faced violence from Shining Path (*Sendero Luminoso*) since the group was founded in 1980 as a Communist, Mao-inspired militant group. The Shining Path once carried out terrorist attacks against civilians in addition to fighting the Peruvian military, police, and paramilitary units.¹⁶ However, when the leader of the Shining Path, Abimael Guzman, was captured in 1992, the organization lost a great deal of its political and military strength, and it was further weakened when Guzman's successor, Oscar Ramirez, was captured in 1999. Since the early 2000s, the Shining Path has morphed into a much smaller cocaine production and smuggling organization, albeit one that continues to espouse Marxist-Leninist political rhetoric.

Government military forces continue to engage armed Shining Path guerrillas and drug runners, with the express goal of eradicating what the government characterizes as a "subversive" (and not terrorist) organization. The crucial point for our purposes is that this violence does not include civilian targets and thus does not have a direct effect on civilian welfare. We exploit contemporary acts of violence by the Shining Path against government soldiers, in particular, instances when government

soldiers are killed by Shining Path affiliates. During the period for which we have violence data (2008–11), there were 86 soldiers killed by Shining Path attacks, of which 35 fatalities occurred during budget periods. Soldier fatalities per department-year range from 0 to 22, and fatalities in the budget period range from 0 to 12. Fatalities took place in seven of Peru's 24 departments.¹⁷ Our analysis leverages variation in soldier fatalities and budgeting over time in those seven departments, as well as the lack of variation in the remaining departments that did not experience soldier fatalities in the study period. Does other violence in Peru undermine our strategy? The military was occasionally deployed in response to clashes between local communities and mining corporations in the study period. However, protestors have not targeted soldiers, and there have been no resultant soldier fatalities. We demonstrate in SI Appendix B.4 that mining protests, in or out of the budget period, do not affect our results.

To leverage soldier fatalities, we must satisfy an important identifying assumption: Dissidents do not strategically choose to attack based on the budget negotiation timeline. If insurgents in a given department chose to defer their attacks to December, for example, in years with below average expected defense appropriations for their department, our analysis would overestimate the effects. It would inappropriately attribute budgetary changes to violence rather than some other underlying political process. To test for this kind of strategic behavior, we look at the average number of dissident attacks that result in soldier fatalities by month and see whether there is a significant increase or decrease at the beginning (September) or end (November) of the budget negotiations. SI Appendix A provides evidence that there is no sorting around these months.¹⁸

Estimation Strategy

We intend for our approach to be internally valid and also provide results that apply to the active and inactive parts of Peru. In a counterfactual world in which the Shining Path

¹⁴The congressional budget committee was eliminated in 1993. Since then, Congress's power is limited to line-item changes to the draft budget that reallocate funds.

¹⁵The president does have the ability to independently appropriate funds outside of the formal budget through the use of *decretos de urgencia* (urgent decrees) that bypass Congress. Presidents typically use these for issues like generic economic stimulus as well as public emergencies and safety. We do not find evidence that presidents have used extra-budgetary *decretos de urgencia* to put more funds into defense in response to dissident violence. Nor do we have evidence that presidents have spent these funds to alleviate downstream butter-to-guns effects. Empirically, *decretos de urgencia* have not washed out budgetary trade-offs, nor have they established a hearts-and-minds strategy alongside Peru's long-standing coercive strategy.

¹⁶Peru has long pursued coercive strategies, in part since coercion was successful against minor dissidents in the 1960s and also due to the heavy influence of the military on politics. In this article, we focus on the fact that Peru continues to follow a coercive strategy and that this strategy generates motivations for butter-to-guns shifts. For more on the origins of Peru's choice of coercion, see Mauzeri (1991), McClintock (1984), and Taylor (1998).

¹⁷Departments affected by Shining Path violence experienced on average one soldier fatality during the budget period (standard deviation of 2.4), and an average of 2.5 total fatalities per year (standard deviation of 4.7). Soldier fatalities were recorded in Apurimac, Ayacucho, Cusco, Huancavelica, Huanuco, Junin, and San Martin departments during the study period.

¹⁸The small difference between December and September–November is driven by a particularly bloody December 2010 in Junin department, where clashes killed eight soldiers in quick succession. Our regression results are unchanged by excluding Junin in 2010 from the analysis. Thus, we do not believe that there is evidence for strategically timed violence by the Shining Path biasing the estimates.

had a national campaign of violence against government forces, the average effects that we estimate would apply throughout the country. Therefore, we examine whether there are systematic differences across the kinds of departments that did and did not experience soldier fatalities. SI Appendix B.8 shows that department size, defense budget share, health budget share, or infant mortality do not significantly vary across “treated” and “untreated” departments. However, departments with soldier fatalities are substantially poorer in terms of GDP per capita.¹⁹ Accordingly, we control for GDP per capita in all our regressions. Additionally, although overall departmental budget execution is similar between treated and untreated departments, we include it to rule out the alternative explanation that weak departmental government capacity drives both violence and poor health services.

We use a two-way fixed effects specification that accounts for departments and years. We model fixed departmental effects by demeaning the series and fixed year effects with a set of dummy variables (Wooldridge 2010). This approach leverages variation from the mean in our dependent and independent variables within departments over time and across departments in a given year, but without estimating all the dummy variables that would otherwise be required. Though we have only 24 departments and a short panel, we are confident in our use of this approach. Wooldridge (2010, 303) establishes that fixed effects estimators are unbiased and consistent for any number of fixed effects.²⁰ In addition, we cluster our standard errors at the department level, which accounts for within-unit correlation of errors (including serial correlation; Arellano 1987).²¹ Our results are maintained after using a bias-reduced linearization, mitigating the concern that clustered robust standard errors may be insufficiently conservative (SI Appendix B.1; Angrist and Pischke 2008; McCaffrey and Bell 2003). Results are also robust to first-differences and lagged dependent variable specifications (SI Appendix B.3). Finally, we implement an alternative approach to evaluating the joint statistical significance of our results using nonparametric combination (NPC; Caughey, Dafoe, and Seawright 2017). NPC helps to account for our relatively small sample size by allowing us to effectively borrow power across outcomes. With NPC, we gain additional confidence in the joint

¹⁹This difference in GDP per capita could limit generalization if a wealthy department might be able to generate private spending to offset deleterious budget trade-offs.

²⁰That is, they are unbiased with respect to the main parameter of interest; we do not interpret the coefficients on covariates.

²¹Using the Lagrange multiplier test, we find that, conditional on time and unit fixed effects, there is minimal autocorrelation (SI Appendix C).

significance of all five outcomes in our theory (combined $p < .01$; SI Appendix B.2).

For the first stage we estimate, using OLS:

$$X_{it} = \gamma_1 Z_{it-1} + \gamma_2 W_{it-1} + \gamma_3 G_{it} + \gamma_4 Year_t + \epsilon_{it},$$

where X is the budget share of interest in department i in year t . Z is the count of soldier fatalities in the department in last year’s budget period (September–November), W is the count of soldier fatalities in last year’s pre-budget period (January–August), G is the vector of covariates, $Year$ is a vector of year dummies, and ϵ is the error term. Department fixed effects are absorbed through demeaning.

We then fit the second stage, using OLS:

$$Y_{it+1} = \beta_1 \hat{X}_{it} + \beta_2 W_{it-1} + \beta_3 G_{it} + \beta_4 Year_t + \epsilon_{it},$$

where Y is the infant mortality rate in department i in year $t + 1$, and \hat{X} is the fitted values from the first stage.²²

In order for the two-stage estimates to be valid, we must satisfy the exclusion restriction: Dissident violence that kills government soldiers (the exogenous variable in the first stage) cannot affect infant mortality (the outcome of the second stage) through any channel other than government budgets and spending. We substantiate this below by ruling out the key alternative channel that local insecurity could affect citizens’ use of health services.

We provide evidence on observable implications of our theory, per Hypothesis 2, by analyzing the content of debates in the Peruvian Congress that are proximate to fatal attacks. For each of the fatal attacks that took place in the budgetary period, we compare the content of the debate for the two sessions before the attack (during the preceding 1–2 weeks) and one session immediately after the attack (on average, 2 days later). We estimate the probability that Congress increases discussions of defense allocations and the broader Shining Path conflict in the session after a budget-period attack, relative to preceding sessions. We also test whether effects scale with the number of soldier fatalities. Regressions have year and event fixed effects and clustered standard errors.

Data

Our violence and budget data cover 2008–11, and our health outcome data cover 2009–12.²³ To measure anti-government dissident violence, we introduce new data from Peru’s *Defensoria del Pueblo*, an independent government agency in Peru that tracks social and political

²²In SI Appendix B.5, we report results using a hurdle model.

²³See SI Appendix Table 1 for summary statistics.

violence. Importantly, this includes not only open-source data but also data collected directly via local *Defensoria* offices in each of Peru's departments. *Defensoria* reports include incident descriptions on "subversive" violence by dissidents from the Shining Path. We code the number of soldier fatalities in these incidents by department-month to form our key explanatory variable. Additionally, the Peruvian National Police (PNP) and Peru's National Intelligence Directorate (DNI) collect and publish aggregate figures on terrorist violence, which we use to further validate our data.²⁴ We find that discrepancies between data sources are restricted to nonviolent events.²⁵ Fortunately for our analysis, we are focused on violent events that produce soldier fatalities, which are the most heavily reported and least likely to be missed.²⁶

Our data on the budget debates of the Peruvian Congress come from the public releases of everything that was stated on the record during the plenary sessions of the congress (*Diario de los Debates*). We reviewed all the plenary sessions during the budgetary periods of 2008–12, in which there were about six plenary sessions per month during the budget debate period (September–November), or one every 5 days. For our quantitative analysis, we focus on the two plenary sessions before a fatal attack and the session immediately after. On average, the preceding sessions were 2–11 days before and post-attack sessions were 2 days after. We coded several pieces of information: (1) Did the session mention funding for the military or defense more generally (often referred to as the "armed forces" or "national defense")? (2) Did the session mention soldiers killed in the Shining Path conflict (often referred to as the "terrorists" or "narco-terrorists")? (3) Did the session mention relationships between defense and social services budgets?

Data on subnational government budgeting and spending are from the *World Bank BOOST Data for Peru* (2016). These are annual data for each of Peru's departments; they include budget allocations and final spending for sectors including defense, health, environment, and culture. On average across the departments, 94% of defense allocations and 91% of health budget allocations are spent. We present results using both budget allocations and actual spending levels, which are useful for

understanding predicted effects.²⁷ We divide each sectoral budget by the overall departmental budget to compute the budget share dedicated to the sector. Government budget execution is computed from the overall budgetary and executed figures by budget line; when we include this as a covariate, we exclude the outcome budget categories.²⁸

Department-level deaths of infants under 1 year old per 1,000 live births is our second-stage dependent variable, which is a standard operationalization of health outcomes and thus increases the comparability of our analyses to other scholarship. Data are from annual waves of USAID's Demographic and Health Survey (DHS) in Peru (INEI 2009–12). We use the standard World Health Organization procedures for measuring infant mortality to compute the rate in each department-year, based on the DHS data. It is unfortunate, but statistically useful, that infant mortality rates are relatively high and varied across Peruvian departments. This means that even with the small DHS samples per department, we can obtain reasonably precise estimates. We also use data from DHS surveys on women's use of health facilities and postnatal services.²⁹

Results

We begin by showing the first-stage effects of the count of soldier fatalities in the budget period on budget shares and spending for overall social services, health in particular, and defense, with a focus on unexpected violence (Hypothesis 1). Expected violence takes place in departments where there were already soldier fatalities during the 9 months before the budgetary period, whereas unexpected violence occurs in departments without such pre-budget period fatalities. We present the results operationalizing pre-budget period violence as a dummy variable to ease interpretation of the interaction term and because we think that for members of Congress, expectations of violence are binary in nature (was there previous violence or not?).³⁰

Table 1 shows that when unexpected soldier fatalities occur in the budget period, the defense budget share increases as the social services share falls, especially the

²⁴The Global Terrorism Database is the only international resource for Peru that codes violence against civilians; it reports two incidents of terrorism against civilian targets during the period under study, with zero reported civilian casualties.

²⁵For example, for nonviolent incidents in 2008, the *Defensoria* reports 31, the DNI reports 247, and the PNP 50.

²⁶Even if there were to be undercounting in the *Defensoria* data we use, this would bias our estimates toward zero.

²⁷For 15 of the 100 department-year observations, there is no defense spending reported in the data. Here, we assume that this means there is zero defense spending and support this assumption in SI Appendix B.6.

²⁸For example, when the health budget is the outcome, we compute government execution from all lines except for health.

²⁹See SI Appendix Table 1 for summary statistics.

³⁰The results are robust to using counts.

TABLE 1 Unexpected Budget-Period Soldier Fatalities Decrease Total Social Services and Health-Sector Budget Shares/Spending, and Increase Defense Budget Shares/Spending

Dependent Variable	Social Services		Health		Defense	
	Budget (1)	Spending (2)	Budget (3)	Spending (4)	Budget (5)	Spending (6)
Budget period fatalities (count)	-.26* (.11)	-.32*** (.08)	-.18** (.06)	-.20** (.06)	.10* (.05)	.11* (.05)
Pre-budget fatalities (dummy)	-.32 (1.46)	-.24 (1.38)	-.81 (1.17)	-1.07 (1.15)	-1.24 (.75)	-1.25 (.72)
Budget × Pre-budget fatalities	.19 (.33)	.21 (.29)	.39* (.14)	.41* (.16)	-.20 (.11)	-.20 (.11)
Dept. GDP per capita	-3.39 (11.03)	-1.75 (9.84)	6.81 (9.25)	7.99 (8.74)	-4.56 (7.14)	-4.27 (6.83)
Dept. budget execution	-6.12 (13.27)	.88 (7.25)	-2.12 (6.71)	-2.32 (6.71)	-3.69 (7.74)	-3.72 (7.56)
N	96	96	96	96	96	96
Departments	24	24	24	24	24	24

Note: Department and year fixed effects are not shown; robust standard errors clustered by department.

*p < .05, **p < .01, ***p < .001.

health share. On average, when there were no soldier fatalities earlier in the year in the department, each fatality drives a 0.10% increase in defense share and a 0.26% drop in combined social services, with a 0.18% drop for health specifically. From a baseline of a 12.3% average budget share for health, this translates into approximately a 1.5% decrease in a department's health budget share per soldier fatality in the budget period. When fatalities occur before the budgetary period, however, these effects disappear.³¹ The results for spending are nearly identical to those for budget shares, consistent with the fact that an average 91% of health and 94% of defense budgets are actually spent.

To provide evidence on Hypothesis 2, we analyze 80 sessions of budget debates in the Peruvian Congress.³² We find evidence of increased support for adding resources to the anti-Shining Path fight after soldier fatalities (Table 2). Recall that we compare debates in the two sessions before a fatal attack (occurring during the preceding 1–2 weeks) and the one session after a fatal attack (on average, 2 days later), conditional on the attack's being in the budget period. In a session after a fatal attack, Congress is over three times more likely to discuss fatalities from the conflict with Shining Path in general (from 14% to 50%) and five times more likely to discuss defense spending

(from 11% to 57%) as compared to sessions immediately prior. The effect scales with the number of fatalities suffered (columns 3 and 4). For example, using the estimates from column 4, when one soldier is killed the marginal effect is a 32% increase, but with five fatalities the probability increase is 67%.³³

Throughout the debates, the members making the most forceful arguments in favor of defense spending came from the rightist "Popular Force" Fujimorist party (before 2012, "Force 2011"). Partisanship appears a more compelling explanation for who speaks up than military ties, as the 3% of members of Congress from 2011 to 2016 (4 of 130) with military backgrounds were not more likely to speak on the record for defense spending. Additionally, those members who spoke generally represented Lima, and not the departments in which unexpected anti-government violence had taken place. This fits with our expectations that it is easier for a politician to publicly choose a coercive strategy if her constituency is less cognizant of its drawbacks.

Consider a session after a 2009 attack. A prominent general argued that the Shining Path could "easily become as powerful as the FARC" in Colombia if Peru does not do more to "confront them and stop them from growing." The minister of defense argued that "the military would have to increase spending in order to defeat the Shining Path militants," and he specifically asked for a budget

³¹In a placebo test, soldier fatalities in December do not affect budgets set in the previous period (SI Appendix D.4).

³²A session is a single day's worth of debate. In general, there are three sessions per week while Congress is meeting.

³³We use the delta method to estimate linear marginal effects.

TABLE 2 Budget-Period Soldier Fatalities Increase the Likelihood of Congressional Debate on Fatalities and Defense Spending

Dependent Variable	Congressional Debate Explicitly References:			
	Fatalities (1)	Defense Spending (2)	Fatalities (3)	Defense Spending (4)
Session is after fatal attack	.36* (.13)	.46** (.14)	.14 (.16)	.24 (.17)
Soldier fatalities (count)			-.01 (.01)	-.02 (.01)
Session after × Fatalities			.09** (.03)	.09* (.03)
N (sessions)	42	42	42	42
Fatal attacks	14	14	14	14

Note: A session is a day of debate in Congress. “Session is after fatal attack” is a dummy variable that indicates whether the session is before or after the attack. We compare the session after an attack with the two preceding sessions. Standard errors are clustered by attack. Effects are unchanged (in significance or magnitude) with time/unit fixed effects.

*p < .05, **p < .01.

increase of USD 43 million.³⁴ Several dozen right-wing members of Congress supported such an increase. In the same debate, an increase in military pensions was also under consideration. Members supporting the anti-Shining Path—motivated increase supported the pension increase as well. However, one right-wing member, Representative Arias-Schreiber of Ucayali department, noted on the record that increasing military pensions could imply future trade-offs in the budget. However, Arias-Schreiber addressed pensions in isolation, and he did not say that increasing funding for anti-Shining Path operations would imply budgetary trade-offs.³⁵ Thus, even the one member who publicly considered trade-offs did not connect them to the use of coercive force.³⁶

Implications of Health Budget Cuts

We now consider the two-stage relationship that captures the extended effects of health budget and spending changes on downstream health outcomes. We use soldier fatalities in the budget period ($t-1$) as an instrument for health budget share as well as spending.³⁷ The results in Table 3 show that for combined social services spending (column 1), for each 1% reduction there is an infant

³⁴ *Diario de los Debates* April 22, 2009.

³⁵ *Diario de los Debates*, April 22, 2009. Arias-Schreiber is in the right-wing Popular Force party, a retired admiral, and from a department that did not experience attacks, all suggesting he would be likely to support defense increases.

³⁶ See SI Appendix E for more discussion.

³⁷ The first-stage regression is reported in SI Appendix D.5.

TABLE 3 Soldier Fatalities Indirectly Increase Infant Mortality

Dependent Variable	Social Services (Combined) Infant Mortality		Health Infant Mortality	
	(1)	(2)	(3)	(4)
(Instrumented) budget	-15.1* (7.6)		-24.0* (12.1)	
(Instrumented) spending		-13.9* (6.0)		-21.1* (10.0)
N	72	72	72	72
Departments	24	24	24	24
First-stage F-statistic	18.1	33.7	16.0	13.8
Controls	Yes	Yes	Yes	Yes

Note: Controls: department GDP per capita, department budget execution. Department and year fixed effects are not shown; robust standard errors clustered by department.

*p < .05.

mortality increase of about 15 per 1,000 live births; for the health budget (column 3), the effect size is 24 per 1,000 live births. The corresponding estimates for combined social services and health spending are 14 and 21 per 1,000 live births. For context, average infant mortality across all of Peru during our period of study is 18 per 1,000 live births (UNData 2018). For those in the bottom quintile of income, the average infant mortality rate is 45

TABLE 4 Soldier Fatalities in the Previous Year’s Budget Period Reduce Women’s Usage of Local General and Postnatal Health Services

Dependent Variable	Health Facility Postnatal	
	(1)	(2)
Budget period fatalities (count)	-.024* (.008)	-.004* (.001)
Pre-budget fatalities (dummy)	-.006 (.006)	.000 (.001)
Department GDP per capita	.484 (.378)	.080 (.180)
Department budget execution	-.941** (.338)	-.155 (.145)
N	96	96
Departments	24	24

Note: Health Facility: Among all women, visit to health facility in past 12 months (DHS). Postnatal: Postnatal visits to government health facilities attended by a licensed health officer (doctor, obstetrician, or nurse) for births in last 12 months (DHS). Department and year fixed effects are not shown; standard errors clustered by department.
*p < .05, **p < .01.

per 1,000 live births; among the richest quintile, it is 5 per 1,000. A way to think about the effect sizes is that health cuts are essentially moving people from the average of 18 to 39–42 per 1,000 live births.³⁸

Tracing the pathway from an individual soldier fatality during the budget period to infant mortality illustrates stark consequences. On average, one soldier fatality causes a 0.13 percentage point reduction in the local health budget (SI Appendix D.5, Table 18 first stage 2SLS estimates). We multiply this by our estimate that a 1% reduction in departmental health spending increases infant mortality by 24 per 1,000 live births (Table 3, column 3). We then multiply by 24,500, the average number of live births in a department-year. One soldier fatality results in an estimated 76 additional infant deaths 2 years later.

Mechanism: Women’s Usage of Health Services

We turn to women’s usage of health services to substantiate the link between budgeting and infant mortality. Table 4 provides evidence that the government’s response worsens women’s usage of general health services and postnatal services. We estimate the effect of a soldier fatality in the budget period on the share of women

³⁸See again SI Appendix D.3.

TABLE 5 Reductions in Women’s Usage of Local General and Postnatal Health Services Increase Infant Mortality in t+1

Dependent Variable	Infant Mortality (t+1)	
	(1)	(2)
(Instrumented) health facility visits	-2,012 (1,287)	
(Instrumented) postnatal visits		-162** (38)
N	72	72
Departments	24	24
First-stage F-statistic	6.8	31.9
Controls	Yes	Yes

Note: Controls: department GDP per capita, department budget execution. Health facility visits indicate among all women, visit to health facility in past 12 months (DHS). Postnatal visits indicates visit to government health facilities attended by a licensed health officer (doctor, obstetrician, or nurse) for births in last 12 months (DHS). Department and year fixed effects are not shown; robust standard errors clustered by department.
**p < .01.

who report using services in the following year. For each soldier fatality in the budget period, overall women’s visits to health facilities decrease by 2.4% (column 1). One soldier fatality in the budget period results in a 0.4% decrease in the share of new mothers who report postnatal care attended by a medical professional (column 2).

Table 5 completes the causal chain by showing the adverse effects of the butter-to-guns trade-off on infant mortality. We use budget-period fatalities (t–1) to predict women’s health visits (t) and the relationship with infant mortality (t+1, the infant’s first year).³⁹ For a 1% decrease in women’s usage of health services, infant mortality increases by 20 per 1,000 live births (column 1). This represents a doubling of Peru’s national average of 18 per 1,000 live births. For postnatal care, the effect sizes are significant but smaller: For 1% in lost usage, infant mortality increases by 1.6 per 1,000 live births.⁴⁰ In SI Appendix D.3, we show that the increased infant mortality estimated in Table 5 is disproportionately felt by the poorest women.

³⁹Health budget shares do not explain infant mortality in the previous year (SI Appendix D.4).

⁴⁰Note that these are not independent, as some women using fewer health services are also new mothers. In SI Appendix D.6, we show that men’s health facility usage is not as sensitive. Also, we acknowledge that the effects in column (1) may be impacted by a weak instruments problem since the first-stage F-statistic is below 10.

TABLE 6 Survey Evidence Suggests That Soldier Fatalities Do Not Affect Citizens' Perceptions of Security

Dependent Variable	“Security Is a Problem” (1)	“Terrorism Is a Problem” (2)	“Crime Is a Problem” (3)
Budget-period fatalities	.002 (.002)	-.002 (.001)	-.004 (.004)
N	46	46	46
Departments	23	23	23
Controls	Yes	Yes	Yes

Note: Controls: pre-budget fatalities, department GDP per capita, department budget execution. Department and year fixed effects are not shown; robust standard errors clustered by department.

Women’s reduced usage of health services appears to result from reduced access, as clinics cut hours or shutter doors. To substantiate, we must rule out the alternative that women suffer from increased insecurity, which reduces their access to health services at previous rates even if clinics remain open. By ruling out this alternative, we also help satisfy the exclusion restriction on which our identification strategy relies: Any effect of soldier fatalities on health outcomes must operate through the budget and not some other mechanism. We use survey data examining whether soldier fatalities affect respondents’ perceptions of security (LAPOP 2016).⁴¹ Soldier fatalities in the budget period have no significant effects on respondents’ propensity to report that security, terrorism, or crime are problems in the following year (Table 6). This finding is consistent with our expectation that because Shining Path violence is directed against government targets, it does not affect civilians’ perceived security.

Finally, we consider whether a worsening disease vector might cause increased infant mortality, rather than decreased access to health services. Cuts to health or environmental budgets or spending could directly affect the spread of disease, which increases infant mortality even if women’s usage of health services is uninterrupted. A direct link between health resources and disease would challenge the pathway via health access. In SI Appendix B.7, we replicate the analysis in Table 3 using subnational data on under-5 admissions for acute respiratory problems and acute diarrhea as dependent variables (Ministry of Environment of Peru 2017). We find that (instrumented) health, environment, and culture budgets or spending has no effect on these disease outcomes.

⁴¹ See SI Appendix F.

This implies, first, that the causal link between health resources and infant mortality does not trace through these two diseases. Second, the results indicate that cuts to environment and culture spending are unlikely to represent exclusion restriction violations. In sum, the identified increase in infant mortality appears to come from reduced health services access via health budget cuts rather than insecurity, non-health social spending reductions, or the spread of disease.

Why Decrease Health Budgets?

Given the detrimental health effects, can we explain why the Peruvian Congress implements butter-to-guns trade-offs? First, recall that we found no evidence in the congressional record that the government responds to violence with a “hearts-and-minds” strategy. Nor do the budget data show an increase in social services resources or health resources around violence.

The government could cut local health resources in order to deliberately punish civilians for (perceived) collusion. We are skeptical that the democratically elected government would punish parts of the electorate, however, and find no empirical support for this motivation. Recall that our identification strategy leverages unexpected anti-government violence in previously peaceful departments. We find significant cuts to health resources in these departments compared to other departments (while accounting for variation in long-standing violence). Departments with surprising violent activity by the Shining Path are unlikely to have long-term civilian collusion with the Shining Path. Additionally, the congressional record provides no evidence that cuts to health resources are motivated by punishment. In our view, the evidence leans toward the explanation that butter-to-guns transfers take place because the Peruvian government must rob discretionary budgets to fund its coercive responses. Such a dynamic is similar to the one identified in Dell and Querubin (2018): Butter-to-guns transfers generate short-term sacrifices, intended to eliminate violent long-term attacks. To the extent that increased infant mortality makes it more difficult to eliminate long-run violence, Peru’s choices are inefficient.⁴²

We support our intuitions by testing which motivation is consistent with the government’s treatment of other non-health discretionary budgets. A punishment-motivated government would target sectors most likely to

⁴²The existing evidence regarding spending to win over hearts and minds is mixed at best (Zürcher 2017); in some cases, these projects generate additional insurgent violence (Crost, Felter, and Johnston 2014; Sexton 2016).

inflict pain on civilians who support the Shining Path, for which health is a compelling candidate. Sectors without clear links to downstream suffering would not be useful targets. In contrast, a government making butter-to-guns trade-offs to fund its coercive response would be motivated to rob any discretionary resources. Thus, consistent with Hypothesis 1, we expect that budget-period soldier fatalities also trigger significant declines in less immediately salient budgets. Results in SI Appendix B.9 show that budget-period fatalities also drive decreases in budgets for environment and culture, two discretionary sectors highly unlikely to be targeted by a government seeking to punish civilians. These results support Hypothesis 1 and undermine the conjecture that Peru's government is punishment motivated. We expect that shifts out of environment and culture would cause negative downstream effects, which would be testable with appropriate department-level data and estimates of the length of time it would take for the effects of reduced budgets to take hold. Future research on the downstream effects of violence-driven budgeting changes on environmental outcomes is particularly important.

Conclusion

We use evidence from Peru to show that short-term security concerns in the aftermath of dissident attacks that kill government soldiers can provoke budgetary realignments away from social services. These transfers have negative effects for downstream social welfare. On average, a single soldier killed in a department, at a time when the national government can reallocate budgets, generates around 76 additional infant deaths 2 years later. We show that the increase in infant mortality is caused by a reduction in women's reported usage of general health services and postnatal services in particular. Further, we find evidence that butter-to-guns trade-offs underlie the identified negative welfare effects of the Peruvian government's coercive responses to anti-government violence, although politicians unsurprisingly do not explicitly acknowledge these transfers or their negative downstream effects in public congressional debates.

Ideally, domestic governments would internalize these indirect effects of budget cuts to social welfare. Even when political realities dictate that spending on defense increase, emergency support to critical social spending could be repackaged as a required corollary. But given the reality of social spending cuts by budget-constrained governments, we see a role for international actors to counter these potentially avoidable deleterious social welfare effects. International donors could help fill in gaps in

otherwise standard budgetary areas, rather than focusing solely on humanitarian assistance. Additionally, donors might pressure governments to recognize the long-term welfare costs of coercive responses to violence.

To what extent are these results specific to Peru? Similar effects are most likely to exist in places where dissident violence against the government intersects with reasonably binding budget constraints. Sub-Saharan African states including Angola, Mali, Burkina Faso, Ivory Coast, Mauritania, and Niger fall in this category. In Latin America, El Salvador and Nicaragua are relevant. Morocco, Egypt, Gabon, Ghana, Serbia, and Ukraine are other states with constitutional balanced budget provisions that may sometimes be binding, especially in the face of sudden security threats. In addition to the 22 countries with formal balanced budget constraints, there are 71 developing democracies (countries with real GDP per capita less than \$20,000 and Polity scores above zero) where informal constraints from legislative bargaining could generate the kind of dynamic we observe if exposed to unexpected dissident or terrorist violence. We expect that shifts in government priorities in response to violence affect nondefense spending and downstream social welfare outcomes even in places without hard budget constraints, although those effects may be masked or delayed by deficit spending.

Ultimately, we demonstrate that long-term effects of violence can be indirect and depend critically on government decisions regarding how to react. We draw attention to the pivotal role of the government in mediating the effects of violence on broader social welfare via a budgeting pathway. This pathway is worth serious future investigation because it suggests that broader negative welfare effects of conflict are not inevitable.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Summary Statistics

Appendix A: Attacks and Budget Negotiations

Appendix B: Robustness Tests

Appendix C: Addressing Serial Correlation

Appendix D: Additional Results

Appendix E: Notes on Congressional Debate

Appendix F: LAPOP Survey Sample by Department