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

June 1, 2018

Contents:

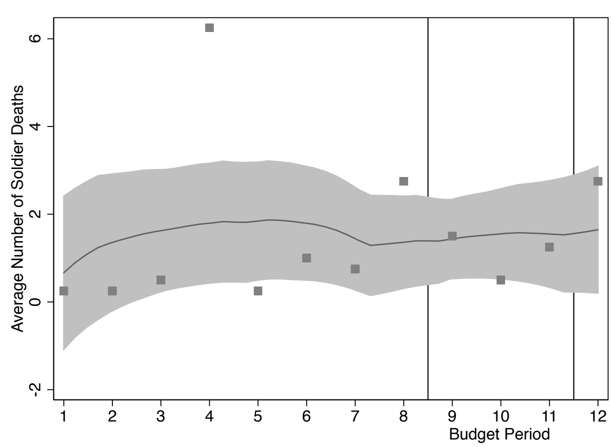
Summary Statistics

Table 1: Summary Statistics

Variable	Obs.	Mean	Std. Dev.
Infant Mortality (per 1,000 live births)	96	17.9	26.2
Health Budget Share	100	12.6	5.6
Defense Budget Share	100	1.78	3.1
Health Spending Share	100	12.0	4.8
Defense Spending Share	100	1.86	3.31
Soldiers killed in Budget Period	100	0.3	1.5
Soldiers killed in December	100	0.3	0.2
Soldiers killed in Budget Period (de-trended)	100	0.22	1.45
Women's Health Access	46	0.6	0.2
Incumbent party vote change 2006 to 2011	24	-0.1	0.1
Environmental Budget Share	75	2.2	1.1
Cultural Budget Share	75	1.7	1.1
National Pride (LAPOP)	69	0.62	0.16
Security biggest problem (LAPOP)	69	0.01	0.03
Terrorism biggest problem (LAPOP)	69	0.01	0.03

A. Attacks and Budget Negotiations

Figure 1: Average number of dissident attacks that kill soldiers, by month



B. Robustness Tests

B.1 Bias-Reduced Linearization

In the following table we show that our main results are robust to using a Bias-reduced Linearization (BRL), as recommended by Bell and McCaffrey (2002) in studies with few clusters.

Table 2: Robustness: Biased-reduced Linearization

	DV: Defense budget share (1)	DV: Health budget share (2)
Soldiers killed (count)	.11	-.18*
	(.08)	(.09)
Soldiers killed pre-budget? (dummy)	-1.29	-.83
	(.76)	(1.22)
Soldiers killed (budget) *	-.21*	.39**
Soldier pre-budget? (dummy)	(.15)	(.14)
Department GDP per capita	-5.14	6.74
	(6.97)	(9.89)
Department Budget Execution	-.20	-1.47
	(8.26)	(6.87)
N	96	96
Departments	24	24

Department and Year fixed effects, SE clustered by department.
* p<0.05, ** p<0.01

B.2 Non-Parametric Combination

As articulated by Caughey, Dafoe and Seawright (2017), non-parametric combination is an empirical approach that leverages multiple observable implications of a theory to test the combined statistical significance. For studies with limited clusters or units such as ours, it has the advantage of pooling the statistical power across outcomes. We employ the authors' R package *NPC* to implement this method across 5 outcome variables (health, defense and environmental spending, as well as health facility visits and licensed postnatal attention). To do so we dichotimize our treatment variable (zero soldier fatalities in the budget period v. greater than zero fatalities in the budget period), as the NPC is best defined with a binary independent variable. As recommended by Caughey et al (2017), we use the 'product' combination function (Fisher's chi-square combination).

Table 3 shows that combining the five outcomes we are able to convincingly show combined statistical significance of our estimates, even with the reduced statistical power associated with the dichotimized treatment variable. This contributes additional evidence that the results we show in the main text are reliable.

Table 3: Non-parametric combination of results

Outcome	**P-value**
Defense Budget Share	0.12
Health Budget Share	0.15
Environmental Budget Share	0.03
Health Facility Visit	0.01
Licensed Postnatal Attention	0.06
Combined	**0.01**
N	96
Departments	24

NPC (Caughey et al 2017) using NPC package in R
'product' combination function with two-sided test.

B.3 LDV and First Differences

As a robustness test, we show in the following section that our estimates are largely consistent when using two alternative specifications: lagged dependent variables and first differences. This is even as we may be concerned about Nickell Bias, due to the short panel. Nonetheless, the estimates in Tables 4 and 5 make us more confident in the results.

Table 4: Robustness: LDV

	DV: Defense budget share (1)	DV: Health budget share (2)
Soldiers killed (count)	.10*	-.25*
	(.05)	(.10)
Soldiers killed pre-budget? (dummy)	-1.21	-.56
	(.77)	(1.27)
Soldiers killed (budget) *	-.17	.45*
Soldier pre-budget? (dummy)	(.17)	(.17)
Department GDP per capita	-5.04	4.93
	(7.19)	(9.14)
Department Budget Execution	-3.40	-2.73
	(7.77)	(6.37)
Lag Budget share	-.05	.13
	(.18)	(.12)
N	96	96
Departments	24	24

Department and Year fixed effects, SE clustered by department.
* $p<0.05$, ** $p<0.01$

Table 5: Robustness: First differences

	Diff Def budget share (1)	Diff Health budget share (2)
Diff: Soldiers killed (count)	.10*	-.18*
	(.05)	(.08)
Diff: Soldiers killed pre-budget? (dummy)	-1.47***	1.19
	(.43)	(1.04)
Diff: Soldiers killed (budget) *	-.42**	.03
Diff: Soldier pre-budget? (dummy)	(.16)	(.15)
Department GDP per capita	-.16	1.41*
	(.66)	(.70)
Diff: Department Budget Execution	5.20	4.53
	(8.79)	(4.93)
N	72	72
Departments	24	24

Year fixed effects, SE clustered by department.
* p<0.05, ** p<0.01

B.4 Including Mining Conflict as a Control

During the period of study, a second source of violence in Peru was community-based conflict in the extractives sector. Readers may be concerned that the estimates of the effects of Shining Path terrorism on budgeting decisions could be confounded by this violence. In the following table we show that main effects are not changed by including mining violence.

Table 6: Accounting for mining-sector violence does not change results

	DV: Health budget share (1)	Defense budget share (2)
Soldiers killed (count)	-.13*	.10*
	(.06)	(.05)
Soldiers killed pre-budget? (dummy)	-.72	-1.23
	(1.29)	(.73)
Soldiers killed (budget) *	.26	-.26*
Soldier pre-budget? (dummy)	(.16)	(.12)
Department GDP per capita	5.60	-4.44
	(9.36)	(6.60)
Mining protest during	.76*	-.09
budget period (logged count)	(.35)	(.36)
Mining protest outside	.67	.28
budget period (logged count)	(.35)	(.30)
Department Budget Execution	-2.48	.03
	(7.16)	(8.09)
N	96	96
Departments	24	24

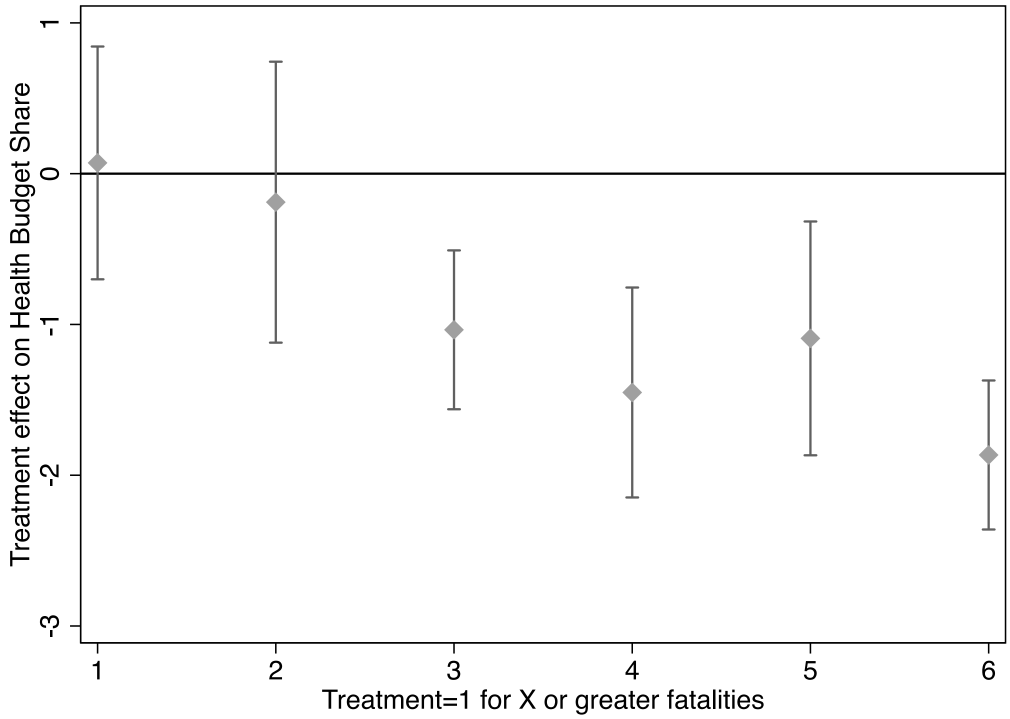
Department and Year fixed effects, SE clustered by department.

DV: Infant mortality in year before budget realized.

B.5: Binary Treatment with Varying Cutoffs

By using OLS to estimate the effect of dissident violence on budgeting and then health outcomes, we are imposing stringent linearity assumptions on the relationship between the intensity of violence and the outcomes of interest. In order to observe the effects in a more flexible and visually interpretable fashion, below we estimate a set of hurdle-type models that include a binary treatment with a moving cut-off. As see in Figure 2, the effect appears to take hold at three or more soldier fatalities, with the one and two (or greater) fatality cut-offs not producing an effect that is statistically distinguishable from zero. The effect gets progressively larger as the cutpoint is moved higher, indicating that there is a monotonic, if not linear, increase in the effect of greater numbers of fatalities on health budget cuts.

Figure 2: Treatment effects with a treatment dummy with varying cutpoints



B.6: Robustness to Potential Underreporting in Defense Spending Data

In the BOOST data on defense spending in Peru there are some idiosyncrasies that might make us worry that there is underreporting in a few instances. First, we know that many governments are touchy about revealing disaggregated data on national security spending, particularly if it could reveal sensitive strategic or tactical information to foes. Second, we noted that there are 19 of 100 observations in our dataset that report zero defense spending in a department-year: four of which are actively reported as zeros, 15 passively reported as zeros (no expenditures categorized as defense in that department-year).

In Section 5 we report the estimates using the data with zeros included for department-years with no defense spending reported in the BOOST datB. Below we employ multiple imputation using predictive mean matching to show that the results are robust to this potential underreporting. Table 7 below shows that the results remain consistent with the main results shown above, with each soldier fatality driving an increase in department defense budget of close to 4 million Soles and spending of nearly 6 million Soles.

Table 7: Soldiers killed during budget period and defense share

	Defense Budget (Soles)		Defense Spending (Soles)	
	(1)	(2)	(3)	(4)
Number of Soldiers Killed	3.78*	3.42*	5.80+	5.61+
	(1.65)	(1.22)	(3.42)	(2.90)
High Defense Budget (t-1)	30.2	32.6	23.3	25.1
	(26.3)	(28.6)	(24.9)	(25.1)
Number of Soldiers Killed *	9.6	14.1	4.9	7.27
High Defense Budget (t-1)	(23.5)	(28.7)	(22.7)	(25.2)
De-trended?	No	Yes	No	Yes
N	97	97	97	97
Clusters	25	25	25	25

* p<0.05, + p<0.1 Dept & Year fixed effects, SE clustered by dept.

Empty defense budget lines replaced with multiple imputation.

B.7: No Exclusion Restriction Violation from Environment or Culture Cuts

We may be concerned that cuts to the environment or culture budget could have an independent effect on infant mortality. It could be that cuts to environment spending would result in more early childhood illness, which would violate our exclusion restriction by worsening infant mortality independent of access to treatment at health clinics. We use data from Peru's Ministry of Environment (2016) on environmental health outcomes, specifically 1) under-5 admissions for acute respiratory problems and 2) under-5 admissions for acute diarrhea. These two variables help us understand whether cuts to environment are having a direct impact on infant health. We find that cuts in health, environmental or culture budgeting/spending have no effect on these two outcomes. This indicates that it is reductions in care, rather than morbidity, that are driving the infant mortality effects.

Table 8: No effects of budgeting on child morbidity

	Acute Diarrhea (under 5)			Acute Respiratory (under 5)		
	(1)	(2)	(3)	(4)	(5)	(6)
(Instrumented) health budget	-.00			-.06		
	(.00)			(.06)		
(Instrumented) enviro budget		-.00			-.27	
		(.00)			(.24)	
(Instrumented) culture budget			-.00			-.69
			(.00)			(1.20)
N	72	72	72	72	72	72
Departments	24	24	24	24	24	24
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Controls: Dept. GDP per capita, Dept. budget execution.
Department and Year fixed effects, robust SE clustered by department.

B.8: Only GDP per capita is significantly related to treatment status

Table 9 notes several potentially important dimensions that could affect our contention that could complicate our argument about treatment status. We find that only GDP per capita in the department has a significant, negative, effect on the likelihood of a soldier fatality. We note that the difference in GDP per capita can limit generalization if, for example, a wealthy department might be able to generate private spending to offset the budget effects we identify. Indeed, our argument leads to a policy implication that to avoid deleterious health effects, departments would do well to find funding for health services that do not come through traditional central-government budgets.

Table 9: Departments with soldier fatalities are poorer but otherwise similar

	GDP per Capita (ln) (1)	Population (100K) (2)	Defense Budget Share (3)	Health Budget Share (4)	Infant Mortality (5)
Department has soldier fatality	-.69***	-.38	-.91	1.11	1.87
	(.18)	(.44)	(.69)	(1.16)	(5.18)
N	24	24	24	24	24

*** p<0.01

B.9: Soldier fatalities reduce other social services.

Table 10: Soldier fatalities in the budget period reduce other social services sector budget shares.

Dependent variable:	Environment budget share (1)	Culture budget share (2)
Budget-period fatalities (count)	-.04**	-.03
	(.02)	(.03)
Pre-budget fatalities (dummy)	-1.94	-.85
	(1.83)	(2.52)
Dept. GDP per capita	.15	-.09
	(.22)	(.19)
Dept. budget execution	2.12	1.03
	(1.59)	(3.29)
N	96	96
Departments	24	24

Department and Year fixed effects, robust SE clustered by department.
** $p<0.05$

C. Serial Correlation

Given the small number of clusters in our analysis (24 departments), the presence of serial correlation may generate concerns about efficiency and appropriately characterizing the variance. Although Arellano (1987) shows that with fixed effects and clustered standard errors the estimator is unbiased and consistent, the relatively few clusters means that the asymptotics cannot be assumed to fully take hold. We use the Lagrange multiplier test to verify that this is not a serious problem.

As seen in Table 11, when only accounting for common shocks, using just time fixed effects, there remains serial correlation of errors. The chi-squared test rejects the null of no serial correlation with p value < 0.0001. However, after including department fixed effects, in Table 12 the serial correlation of errors disappears. Model 2 reflects the main specification we use in the paper, and we feel confident that autocorrelation is not rendering the results incorrect. The two-way fixed effects plus controls and clustered standard errors is verified in this case as appropriate estimation strategy.

Table 11: Model 1 (year fixed effects): LaGrange Multiplier Autocorrelation Test

Outcome	Health	Defense
Residuals	Residuals	
L.Residuals	-0.19	0.05
	(0.2)	(0.13)
LM test statistic (N*R^2)	23.3	28.8
P-value	0.00	0.00

Vector of X_{it} included in the auxiliary regression.

Table 12: Model 2 (year and department fixed effects): LaGrange Multiplier Autocorrelation Test

Outcome	Health	Defense
Residuals	Residuals	
L.Residuals	0.02	-0.01
	(0.11)	(0.11)
LM test statistic (N*R^2)	1.02	0.49
P-value	0.31	0.48

Vector of X_{it} included in the auxiliary regression.

D. Additional Results

D.1 Timing of Soldier Fatalities and Incumbent Vote Share

In the following table we provide some evidence that government soldiers being killed in recent insurgent attacks hurts the reelection chances of members of Congress from the department, while cuts to the health budget do not. The election in question took place in April 2011, and we look at violence from the budget period of 2010 (September to November), as compared to casualties from the pre-budget period (January to August). The outcome is the change in incumbent party vote share from 2006 to 2011.

Table 13 indicates that soldiers killed in the run-up to the election reduce incumbent vote share by 5 percentage points, where as casualties farther back in time do not. Similarly, the health budget share appears to not be related to vote share changes.

Table 13: Only recent soldier fatalities reduce incumbent party vote share (2011 election)

	(1)	(2)	(3)
Soldier fatalities (count, $<$ 6 mo. to election)	-.05+		-.05+
	(.03)		(.03)
Soldier fatalities (count, $\geq$ 6 mo. to election)	.01		.01
	(.02)		(.02)
Department GDP per capita	-.02	-.01	-.01
	(.02)	(.02)	(.02)
Health budget share		.00	.00
		(.01)	(.01)
N	24	24	24
Departments	24	24	24

Robust standard errors. + p<0.1, ** p<0.05, *** p<0.01.

D.2 Rally Around the Flag?

In Table 14 we provide additional suggestive evidence, using the LAPOP question on national pride to see if fatal attacks on government soldiers provokes a 'rally around the flag' effect. We again see that citizens respond to recent attacks, as opposed to ones that took place longer before. We interpret this as providing some evidence in favor of public support for immediately increasing defense spending in the face of insurgent violence.

Table 14: Soldier fatalities increase national pride, briefly

	(1)
Soldier fatalities (count, Sept-Nov budget period)	.06**
	(.02)
Soldier fatalities (count, Jan-Aug)	-.00
	(.00)
Department GDP per capita	-.05
	(.44)
N	69
Departments	23

Department and Year fixed effects, SE clustered by department.
** $p<0.05$

D.3 The Poor Bear the Burden of Health Budget Shifts

Our estimates in Table 15 suggest that, among women, the burden of reduced budgeting for health is primarily borne by the poorest. Using data from the Latin American Public Opinion Project (LAPOP), we are able to show the effects of budget-period soldier fatalities on health facility usage by women of different income strata.[1] We calculate income quartiles by department, based on survey respondents' reporting of their monthly household income in 200 Sole bandwiths. As we see in Table 15, women in the top quartile of household income do not report a significant reduction in health visits. However, women in the bottom quartile are impacted quite dramatically and negatively, with reported access reduced by more than twice the average effect: a 7% drop.

Table 15: Soldier fatalities in budget period hurt poor women's health access, in particular

Dependent variable:	Reported Health Visits	
	Top 25%	Bottom 25%
	(1)	(2)
Soldier fatalities in budget period (count)	.04 (.03)	-.07*** (.01)
Departments	23	23
N	46	46

Women's reported health visits (count) per year. Respondents are asked to report their approximate monthly household income in 200 Sole bandwidths, which we use to compute percentiles. (LAPOP) Department and Year fixed effects, SE clustered by department.

*** p<0.01.

A test of difference in coefficients shows (1) and (2) are significantly different from zero with p<.01.

[1] Unfortunately, the DHS does not report comparable income statistics, so we cannot make a direct connection to local infant mortality.

D.4 Placebo Test Results

Another way to provide evidence in support of our hypothesized budgetary pathway is through placebo tests. First, if the budget must be passed by 30 November, then soldiers killed in attacks in December cannot significantly affect the budget. Thus, to test the accuracy of our identification strategy, we regress health budget shares on soldier fatalities in December (See Table 16), as well as soldier fatalities during the budget period and controls. We continue to have strong significant effects on soldier fatalities in the budget period, while soldier fatalities in December have no effect, providing important evidence that the budgetary pathway is driving the observed effects.

Table 16: Placebo: Soldier fatalities in December (after budget period) have no effect on health budget share or women's health facility visits

Dependent variable:	Health Budget Share	Women's Health Visits
	(1)	(2)
Soldier fatalities (count, December)	.06	.00
	(.69)	(.12)
Soldier fatalities (count, budget period)	-.13*	-.03***
	(.06)	(.01)
Department GDP per capita	4.40	.31
	(8.55)	(.44)
Soldier fatalities pre-budget period (count)	.12	-.01
	(.07)	(.01)
N	96	96
Departments	24	24

Department and Year fixed effects, SE clustered by department.

* p<0.1, *** p<0.01.

Second, we conduct a placebo test that takes into account the full two stages of our estimation. As before, we instrument health budget share in t with soldiers killed in the budget period in year $t-1$. We test whether our instrumented health budget share can explain infant mortality in year $t-1$. If our identifying assumptions hold, this exercise should lead to null results. In Table 17, we indeed find null results whether we regress the health budget share or actual spending on the instrumented health share measure. Thus, these placebo tests provide additional evidence in favor of our estimates.

Table 17: Placebo: Health budget does not affect last year's infant mortality

	DV: Infant Mortality (t-1)	
	(1)	(2)
(Instrumented) Health budget	-21.62	
	(206.34)	
(Instrumented) Health spending		-95.52
		(1868.26)
N	72	72
Departments	24	24
First Stage F-stat	64.1	56.1
Controls	Yes	Yes

Department and Year fixed effects, SE clustered by department.

DV: Infant mortality in year before budget realized

D.5 First Stage of 2SLS

Here we present the first stage regressions for the estimates shown in Table 4 of the Results section.

Table 18: First stage estimates

	Social Services (combined)		Health	
Dependent variable:	Budget (1)	Spending (2)	Budget (3)	Spending (4)
Budget period fatalities (count)	-.29+	-.32*	-.18+	-.21+
	(.16)	(.14)	(.11)	(.11)
N	72	72	72	72
Departments	24	24	24	24
First Stage F-stat	117	108	182	193
First Stage F-stat (no FE)	10.1	9.8	9.8	8.5
Controls	Yes	Yes	Yes	Yes

Controls: Dept. GDP per capita, Dept. budget execution.

Department and Year fixed effects, robust SE clustered by department.

* p<0.05 + p<0.1

D.6 Gender Dimensions of Health Access

In the following table we separate the effects of dissident attacks during the budget period on health service access by women and men, based on LAPOP survey data. In short, women greatly reduce their access, whereas the impacts for men are very small.

Table 19: Disaggregating health access effects by gender

Dependent variable:	Women's health access (1)	Men's health access (2)
Budget period fatalities (count)	-.03*	-.004
	(.01)	(.02)
N	46	46
Departments	23	23
Controls	Yes	Yes

Outcomes from LAPOP survey. Controls: Dept. GDP per capita, Dept. budget execution. Department and Year fixed effects, robust SE clustered by department.

* $p<0.05$ + $p<0.1$

E. Notes on Congressional Debate

One of the critical questions about the congressional debate evidence referenced in the main text is whether members of Congress acknowledge that there are budgetary tradeoffs associated with boosting defense spending. In reviewing the text, such budgetary tradeoffs were only openly discussed in one public debate, during the Plenary Debate of November 28, 2013. One member, Rep. Carlos Tubino Arias-Schreiber of “Popular Force” from Ucayali department, questioned whether increasing military pensions (that would primarily benefit lower rank members) would imply future budgetary tradeoffs.[2] He did not, however, question whether increasing funding for current operations against Shining Path dissidents would imply future tradeoffs. In the course of the debate, several dozen members articulated their support for increasing defense spending on military pensions and increasing defense spending on current operations against the Shining Path; these members did not mention budgetary tradeoffs with regard to either issue. Thus, the evidence strongly suggests that members of Congress do not publicly acknowledge tradeoffs. From a political perspective, it makes sense that politicians would not want to publicly acknowledge that increases in defense spending come at a cost to other spending from which constituents directly benefit.

Additionally, partisanship is strongly associated with who makes interventions in favor of increased defense budgets. Time and again, the most forceful denunciations of anti-government attacks and notes in favor of defense spending came from members of “Popular Force,” the rightist political party associated with Keiko Fujimori (before 2012, “Force 2011”). Partisanship is a more compelling explanation than military experience, as only 3% of members of Congress from 2011-2016 (4 of 130) had military backgrounds, and they were not more likely than others to speak for defense spending. Nevertheless, many “Popular Force” members speaking for greater defense spending represented the Lima department, meaning that they did not come from the subnational locations in which budgetary tradeoffs would be most visible. This reinforces the argument that politicians are more likely to choose a coercive strategy if those most in favor of a coercive strategy are less cognizant of its drawbacks.

[2] Tubino is a retired admiral, and the Ucayali department did not experience soldier fatalities in the period under study.

F. LAPOP Survey Sample by Department

Three waves of the LAPOP survey are included in our sample period, with the following departmental coverage. All departments are included, with the exception of Madre de Dios, Peru's smallest department, which is not covered in the survey.

Table 20: Number of LAPOP survey respondents by department during study period

Department	**N**
Amazonas	72
Ancash	177
Apurimac	51
Arequipa	191
Ayacucho	116
Cajamarca	218
Callao	165
Cusco	173
Huancavelica	77
Huanuco	119
Ica	88
Junin	181
La Libertad	253
Lambayeque	195
Lima	1467
Loreto	141
Moquegua	35
Pasco	24
Piura	260
Puno	222
San Martin	125
Tacna	43
Tumbes	37
Ucayali	70