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THE POLITICAL SCAR OF EPIDEMICS*

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Epidemic exposure in an individual's 'impressionable years' (ages 18 to 25) has a persistent negative effect on confidence in political institutions and leaders. This loss of trust is associated with epidemic-induced economic difficulties, such as lower income and unemployment later in life. It is observed for political institutions and leaders only and does not carry over to other institutions and individuals. A key exception is a strong negative effect on confidence in public health systems. This suggests that the distrust in political institutions and leaders is associated with the (in)effectiveness of a government's healthcare-related response to epidemics. We show that the loss of political trust is largest for individuals who experienced epidemics under weak governments with low policymaking capacity, and confirm that weak governments in fact took longer to introduce policy interventions in response to COVID-19. We report evidence that the epidemic-induced loss of political trust discourages electoral participation in the long term.

Epidemics are stress tests for governments. Public officials and institutions face the challenge of assembling information and mounting interventions against a rapidly spreading, potentially fatal disease. They must communicate that information, describe their policies and convince the public of their trustworthiness. Fukuyama (2020) argued that keys to success in dealing with COVID-19 are 'whether citizens trust their leaders, and whether those leaders preside over a competent and effective state'. By way of example, Rothstein (2020) ascribed greater early success at containing COVID-19 in the Nordic countries than Italy to greater trust in government.

Trust in government is not a given, however; there is reason to ask how epidemic exposure itself affects such trust. On the one hand, there is the 'rally "round the flag hypothesis"'. Trust in and support for political institutions and leaders tend to rise in the wake of disasters (Mueller, 1970). On the other hand, trust in government may decline because public institutions and those charged with their operation fail to prevent or contain the epidemic. In both cases, moreover, the persistence of the effect is unclear.

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Here we provide the first large-scale evidence on the effects of epidemics on political trust.[1] We use novel data on trust and confidence in governments, elections and national leaders from the 2006–18 Gallup World Polls fielded in up to 140 countries annually, together with data on the incidence of epidemics since 1970 as tabulated in the EM-DAT International Disasters Database. We show that exposure to epidemics, specifically when an individual at the time of exposure is in his or her 'impressionable years' (ages 18 to 25) during which attitudes and outlooks are indelibly formed, durably shapes confidence in governments, elections and leaders.

Our empirical strategy exploits within-country-year between-cohort variation. We ask whether cohorts of individuals exposed to epidemics during their impressionable years display less political trust than other cohorts surveyed in the same country and same year. The estimated effect is substantial: an individual with the highest exposure to an epidemic (relative to zero exposure) is 5.1 percentage points less likely to have confidence in the national government, 7.2 percentage points less likely to have confidence in the honesty of elections and 6.2 percentage points less likely to approve of the performance of the national leader. These effects represent the average treatment values for the remainder of life. They are up to four times larger for age groups that are close to their impressionable years and decay only gradually as individuals age.

We obtain these estimates controlling for country, year, cohort and age fixed effects, as well as for country-by-year fixed effects in our more demanding specifications. We further address identification concerns in four ways. First, we show that a country's epidemic experience has no analogous impact on political trust for individuals who are older or younger at the time of exposure; the effect is specific to the exposure in the impressionable years. Second, we show that our epidemic exposure has no impact on social trust and trust in a variety of non-political institutions; the impact is specific to political institutions and leaders. Third, by creating an event-study setting around the dates since early 2000s when a pandemic was declared by the World Health Organisation, we show that countries with and without a pandemic shock share a common trend in levels of trust in the pre-event window: that the divergence starts only after the shock. We also confirm that our impressionable-year results carry over when we employ this alternative pandemic dataset. Finally, to verify that we are capturing the effects of epidemic exposure and perceptions of the adequacy of the associated public-policy response, as distinct from general health conditions, we compare the effects of communicable and non-communicable disease exposure and show that our results hold only for the former.

Our results are not driven by other observable economic, social and political exposures that individuals may have simultaneously experienced in their impressionable years (such as growth and stability of the economy, inflation, GDP per capita, internal conflict, external conflict, corruption scandals, democratic accountability, revolutions, assassinations, purges, riots, anti-government demonstrations). A test proposed by Oster (2019) indicates that our results are unlikely to be driven by the unobserved variation potentially related to omitted factors. Our estimates are robust to different measures of epidemic exposure (such as a population-unadjusted treatment variable or various threshold dummies for high exposures) and across a variety of specification checks (excluding potentially bad controls, multiple hypothesis testing, ruling out the importance of

[1] There is some evidence on other political impacts of epidemics and containment efforts. Flückiger *et al.* (2019) showed that the intensity of the West African Ebola is associated with greater trust in government. (We return to this study below.) Campante *et al.* (2020) found that heightened concern about Ebola led to lower voter turnout in the United States. Amat *et al.* (2020) showed that, following the COVID-19 outbreak in Spain, citizens expressed a stronger preference for technocratic governance and strong leadership. Another body of research examines the impact of trust in government on epidemics and containment efforts. Marlow *et al.* (2007) showed that trust in government is a predictor of flu vaccine acceptance by mothers in the United States.

influential observations, constructing a dependent variable based on principal component analysis). As a falsification exercise, we present results focusing on our baseline sample, but randomly allocating each individual to a different country where they could have spent their impressionable years. We find no effect.

Finally, we provide evidence that epidemic exposure alters not just reported political attitudes, but also actual political behaviour: respondents with epidemic exposure in their impressionable years are less likely to have voted in recent national elections, more likely to have taken part in lawful/peaceful public demonstrations and more likely to have signed a petition.

The second part of the paper explores the mechanisms behind our results. We show that these negative attitudinal changes are accompanied by negative changes in economic outcomes: epidemic exposure in an individual's impressionable years is associated with lower subsequent incomes and a lower likelihood of employment later in life. We show that individuals exposed to epidemics in their impressionable years are less likely to have confidence in public health systems, suggesting that the perceived adequacy of health-related government interventions during epidemics is important for trust in government generally.

We investigate whether an effective and timely policy response at the time of the epidemic matters for how citizens adjust their political trust. In the absence of an international dataset on policy reactions to past epidemics, we make this point in two steps. First, we validate the conjecture that the prior strength of a government positively predicts the speed of its policy response to the recent COVID-19 pandemic. Second, we show that, when individuals experience epidemics under weak governments, the negative impact on trust is larger and more persistent. This is consistent with the idea that governments lacking unity and legislative capacity are also less capable of reacting effectively to national health crises, producing a more substantial long-term decline in their citizens' political trust.

Section 1 reviews kindred literatures. Sections 2 through 4 describe our data, empirical strategy and model. Sections 5 and 6 present the baseline results and mechanism at play, after which Section 7 concludes.

1. Literature

Our analysis connects up to several literatures. First, there is work in economics on the determinants and correlates of trust. Contributions here (e.g., Alesina and La Ferrara, 2000; Nunn and Wantchekon, 2011) tend to focus on social trust (trust in other individuals, both in-group and out-group trust) rather than trust in political institutions and leaders. There are also a few studies of trust in political institutions and leaders (Becker *et al.*, 2016; Algan *et al.*, 2017; Dustmann *et al.*, 2017), but these tend to focus either on the impact of political circumstances long past or of relatively recent economic variables, such as growth and unemployment. Ours is the first study to present global evidence for the adverse impact on trust of health-related concerns and to consider the long-term impact of health crises experienced at an early stage of an individual's life cycle.

Second, there is the literature on the 'impressionable years'. A seminal study pointing to the importance of this stage of the life cycle in durably shaping attitudes and values is the repeated survey of women who attended Bennington College between 1935 and 1939 (Newcomb, 1943; Newcomb *et al.*, 1967), among whom beliefs and values formed then remained stable for long periods. An early statement of the resulting hypothesis is Dawson and Prewitt (1969); Krosnick and Alwin (1989), among others, then pinpointed the impressionable years as running from ages 18 to 25.

In terms of applications, using survey data from Chile, Etchegaray *et al.* (2018) showed that individuals in their impressionable years in periods of political repression have a greater tendency to withhold their opinions, compared to those who grew up in less repressive times. Farzanegan and Gholipour (2021) found that Iranians experiencing the Iran-Iraq War in their impressionable years are more likely to prioritise a strong defence. In our paper, we control for many aspects of economic, social and political experience during an individual's impressionable years to establish that our results are not spuriously driven by the factors detected in these previous studies. Our contribution is not only to add evidence for yet another adverse shock (i.e., epidemics), but also to document its persistence in the long term and to identify a novel mechanism (i.e., government policy (in)effectiveness) through which individuals update their beliefs.

Third, there is the recent pandemic-related literature. Flückiger *et al.* (2019) focused on the Ebola outbreak in West Africa from 2013 to 2016 and showed that state legitimacy—proxied by trust in central government (parliament and president) and police—increased disproportionately in regions with higher exposure to the epidemic. The authors further showed that the effects are more pronounced in areas where governments responded more successfully to the epidemic. Aasve *et al.* (2021) used the approach of Algan *et al.* (2017) to study the impact of the 1918–9 Spanish flu pandemic on social trust. Analysing the General Social Survey for the United States, they found that individuals whose families emigrated to the United States from a country with many Spanish flu victims display less trust in other people. Fetzer *et al.* (2021) used an experimental research design to establish that individuals' beliefs about pandemic risk factors associated with COVID-19 are causally related to their economic anxieties. In contrast to the single-epidemic focus of these and other recent studies, ours is the first (to the best of our knowledge) to bring large-scale international evidence and generalise the impact of a large set of historical epidemic episodes on individual beliefs and behaviour.

Finally, there is our own work (Eichengreen *et al.*, 2021), where we investigate whether exposure to previous epidemics affected young people's trust in science and scientists. An obvious difference between the two papers is the focus, science and scientists versus politics and politicians. Another important difference lies in the channels or mechanisms linking epidemic exposure to distrust in distinct political and scientific spheres. Here, where we show that epidemic exposure during early stages of life matters for political trust, the mechanism is the (lack of) effective and timely government policy response. In our companion paper, where we demonstrate that epidemic exposure reduces trust in scientists and in the benefits of their work, the mechanism is lack of consistent scientific communication during past epidemics. The adequacy of the public-policy response and problems of scientific communication are entirely different mechanisms. These two papers also differ in terms of illustrating how distrust translates into changes in actual behaviour in the respective spheres. While we show in our companion paper that epidemic-induced scientific distrust translates into negative views towards vaccines and lower rates of child vaccination, we report evidence in the current paper that individuals with lower political trust after past epidemics reduce their electoral participation and prefer voicing their opinions via alternative means (such as attending demonstrations and signing petitions).

2. Data

Our principal data sources are the 2006–18 Gallup World Polls (GWPs) and the EM-DAT International Disasters Database. GWPs are nationally representative surveys fielded annually from 2006 in about 150 countries, with responses from approximately 1,000 individuals in each

country. Our full sample (depending on outcome variables) covers some 750,000 respondents in 142 countries.

The outcome variables come from questions asked of all Gallup respondents about their confidence in the national government, their confidence in the honesty of elections and their evaluation of the job performance of the incumbent leader. (*i*) 'In (this country), do you have confidence in each of the following, or not: . . . How about the national government?' (*ii*) 'In (this country), do you have confidence in each of the following, or not: . . . How about the honesty of elections?' (*iii*) 'Do you approve or disapprove of the job performance of the leadership of this country?'

GWP provides information on respondents' age, gender, educational attainment, marital status, religion, urban/rural residence, labour market status and income.

Data on worldwide epidemic occurrence and its effects are drawn from the EM-DAT International Disasters Database from 1970 to the present. These data are compiled from UN agencies, non-governmental organisations, insurance companies, research institutes, press agencies and other sources. The database includes epidemics (viral, bacterial, parasitic, fungal and prion) meeting one or more of the following criteria:

(*a*) 10 or more deaths;
(*b*) 100 or more individuals affected;
(*c*) declaration of a state of emergency;
(*d*) calls for international assistance.

Our dataset includes 47 epidemics and pandemics since 1970. This includes large outbreaks of cholera, Ebola and H1N1 and also more limited epidemics. Averaged across available years, H1N1, Ebola, dysentery, measles, meningitis, cholera, yellow fever, diarrhoeal syndromes, Marburg virus and pneumonia were the top-ten diseases causing epidemic mortality worldwide. Many of these epidemics and pandemics affected multiple countries. Note that the EM-DAT International Disasters Database does not include data on non-communicable diseases. We employ separate data on non-communicable diseases below.

We provide the full country-year-epidemic list in Online Appendix E. One hundred and thirty-seven countries experienced at least one epidemic, so measured, since 1970. This includes 51 countries in Africa, 40 in Asia, 22 in the Americas, 19 in Europe and 5 in Oceania.[2] Each epidemic is tagged with the country where it took place. When an epidemic affects several countries, the database contains separate entries for each. EM-DAT provides information on the start and end dates of the epidemic, the number of deaths and the number of individuals affected, where the number of individuals affected is how many require assistance with basic survival needs such as food, water, shelter, sanitation and immediate medical treatment during the period of emergency. We aggregate all epidemic-related information in this database at the country-year level and merge it with Gallup World Polls.

[2] Manipulation of case/death numbers by national authorities could be an issue, especially if such manipulation is associated with relevant country characteristics (such as trust in national authorities, etc.). Most plausible is that countries with ex ante low political trust underreport individuals affected by epidemic events. Measurement bias would therefore indicate a positive relationship between political trust and case/death numbers. It follows that we may potentially be underestimating the negative effect of epidemics on political trust. In any case, the estimations using *incidence* rather than *intensity* of epidemics (Online Appendix Table B11) are unlikely to suffer from this form of mismeasurement error, as it is much more difficult to hide the incidence of an epidemic than the case/death numbers. Additionally, thanks to a referee who noticed 16 observations in Online Appendix E where death numbers seem to exceed affected people, we have confirmed that our results do not depend on the exclusion of these few inconsistent observations.

In robustness checks, we also employ a disaggregated panel dataset on communicable as well as non-communicable diseases from the Institute for Health Metrics and Evaluation and a dataset on recent WHO-declared epidemics from Ma *et al.* (2020).[3]

3. Empirical Model

To assess the effect of past epidemic exposure on confidence in government, elections and political leaders, we estimate the specification

$$\begin{aligned} Y_{i,c,t,a,b} = {} & \beta_1 \text{ Exposure to epidemic (18–25)}_{icb} + \beta_2 X_i \\ & + \beta_3 \text{ People affected contemporaneously}_{ct-1} + \beta_4 C_c \\ & + \beta_5 T_t + \beta_6 A_a + \beta_7 B_b + \beta_8 C_c \times \text{Age} + \varepsilon_{ict}, \end{aligned} \tag{1}$$

where $Y_{i,c,t,a,b}$ is a dummy variable for whether or not respondent i of age a and birth year b in country c at time t approves or has confidence in an aspect of their country's political institutions or leadership. Responses to all three questions are coded as dummy variables, with one representing a positive answer and zero otherwise. We estimate linear probability models for ease of interpretation.

To measure *Exposure to epidemic (18–25)*, we calculate for each respondent the number of persons affected by an epidemic as a share of the population, averaged over the eight years when the respondent was aged 18 to 25, consistent with the 'impressionable years' hypothesis. The vector of individual controls X_i includes indicator variables for urban residence and the presence of children under the age of 15 in the household, and dummy variables for gender, marital status, employment status, religion, educational attainment and within-country-year income deciles. *People affected contemporaneously* controls for whether or not the individual is also exposed to an epidemic at the time surveyed. This is also calculated as the number of individuals affected by an epidemic as a share of the population in the country of residence in the year immediately prior to the interview. This variable is lagged to ensure that the independent variable is realised before the dependent variable.

We also control for income before taxes in both log and log-squared form. Prior epidemic exposure may possibly affect an individual's responses by affecting his or her subsequent income. But, by controlling for household income separately, we can rule out that prior exposure affects an individual's responses solely via this income channel. A sense of the relative importance of this and other channels can be gained by comparing specifications with and without this income variable.

We include fixed effects at the levels of country (C_c), year (T_t) and age (A_a). The country dummies control for time-invariant variation in the outcome variable caused by factors that vary cross-nationally. Year dummies capture the impact of global shocks that affect all countries simultaneously. Age dummies control for the variation in the outcome variable caused by factors that are heterogeneous across (but homogeneous within) age groups. We also include country-specific age trends ($C_c \times$ Age) and cohort fixed effects (B_b). A fully saturated specification also includes country-year fixed effects, which account for possible omitted country features

[3] To explore underlying mechanisms, we use data from Google Trends, the European Center for Disease Prevention Control, the Johns Hopkins Coronavirus Resource Center and the Oxford COVID-19 Government Response Tracker. Online Appendix D summarises the additional data sources. Online Appendix Table A1 shows descriptive statistics for the outcome variables, country characteristics and individual characteristics.

that may change with time (such as GDP per capita, population, political regime, etc.). In this case we drop contemporaneous epidemic exposure, because it is perfectly correlated with the country-year dummies. We cluster SEs by country and use sample weights provided by Gallup to make the data representative at the country level. Finally, we limit our sample to individuals born in the same country in which they were interviewed by Gallup.[4]

4. Threats to Identification

One can imagine several potential threats to identification. First, estimates could be driven by factors that are specific to each cohort, since our treatment categorises individuals in each country by year of birth. Some cohorts could have cohort-specific attitudes toward political institutions and leaders or be more or less trusting than others in general. Individuals born in the late 1940s and early 1950s may vest less trust in political institutions and leaders, for example, because they experienced the widespread protests against political repression in the late 1960s, their impressionable years. We therefore include dummies for year of birth so as to compare the individuals only within the same birth cohort.[5]

Second, independent of cohort, individuals may exhibit differential behaviour across the life cycle. They may become more (or less) trusting as they age, for example. Political views and ideologies may change from more liberal when young to more conservative when older (Niemi and Sobieszek, 1977). Age-specific factors may also matter if different generations were exposed to epidemics with different probabilities; given advances in science and improvements in national healthcare systems, one might anticipate that epidemics are less likely to be experienced by younger generations. We therefore include a full set of age-group dummies, which eliminates any influence on our outcome variables of purely age-related and generational effects.

Generational *trends* in political attitudes could also be heterogeneous across countries. Some national cultures may be more flexible and open to change in individual values and beliefs, leading to larger differences across generations. We therefore include country-specific age trends.

Third, an omitted variable that varies across countries and years can bias estimates even when conventional country and year fixed effects are included separately. This issue arises, for example, when we observe individuals' attitudes toward national political institutions and leaders. Because the identity of those leaders and the structure of those institutions may change, it can be difficult to separate these shifts in identity and structure from the treatment (i.e., the epidemic). For instance, even when approval of a leader declines following an epidemic, we may not capture this effect if the epidemic simultaneously triggers a change in the identity of the leader, bringing in someone for whom approval levels are higher. We address this by including dummies for

[4] We cannot guarantee that these individuals spent all of their impressionable years in their country of birth, but any measurement error arising from this concern only stacks the cards against us by lowering the precision of our estimates. Furthermore, to the extent that large epidemics push individuals to migrate to other countries not affected by the same epidemic, survivorship bias may lead us to underestimate the true negative effect of a past epidemic experience. We also estimated models using two alternative weightings: one that adjusts the previous weights using time-varying country population and another that adjusts the previous weights using time-invariant country population (averaged across available years). The results remain qualitatively unchanged.

[5] Including these dummies biases our estimates downward if epidemics are correlated across countries and affect them simultaneously. In this case, any common effect of an epidemic on a specific cohort will be subsumed by these cohort-specific dummies, and our treatment will pick up variation in past epidemics only when they strike countries at different points in time. This empirical design cannot be considered as staggered since epidemic exposure across subsequent cohorts is not permanent. The fact that a cohort of individuals born in year X in country A had epidemic exposure during their impressionable years does not necessarily mean that the following cohort born in year $X + 1$ (or $X + 2$, etc.) in the same country A will also face a similar exposure during their impressionable years.

each country-year pair. This eliminates all heterogeneity in our outcome variables traceable to country-specific time-varying factors, such as changes in the government or leader. Thus, the treatment only compares individuals within the same country and survey year, ensuring that these individuals face the same political institutions and leaders. This mitigates concerns that the results are driven by other structural differences between countries that are repeatedly exposed to epidemics and those that are not.

Fourth, in any study of the impact of past experience on current outcomes, the underlying assumption is that the effect is persistent. This, after all, is the essence of the 'impressionable years' hypothesis. To the extent that this is not the case (because the effect has a relatively short half-life), our empirical strategy will be biased towards failing to reject the null of no effect. We explore this by tracing the impact of past epidemic exposure across different age groups and show that the effect persists for at least two decades while decaying only gradually as individuals age. Hence, the full-sample estimates represent the average treatment effect across the whole life cycle after the impressionable years.

Although we fully saturate our specifications with fixed effects, there could still be other past exposures correlated with epidemics. To address this, we control for observable economic, political and social factors in the country in question during the individual's impressionable years. Including these controls for other past conditions has no impact on the stability of our coefficients of interest. In addition, we use the methodology developed by Oster (2019). The results suggest that our findings are unlikely to be driven by unobserved variation.

5. Results

Table 1 reports estimates of (1). The dependent variables are a dummy indicating that the respondent 'has confidence in the national government' (panel A), that the respondent 'approves of the performance of the leadership of his or her country' (panel B), that the respondent 'has confidence in the honesty of elections' (panel C), the average of all three outcome variables (panel D) and the first principal component of responses (panel E). Column (1) reports estimates with country, year and age group fixed effects. Column (2) adds the logarithm of individual income and its square, demographic characteristics, within country-year income decile fixed effects and labour market controls. Column (3) adds country-specific age trends, while column (4) adds cohort fixed effects. Column (5) fully saturates the specification with country × year fixed effects, non-parametrically controlling for all potentially omitted variables that can vary across countries and years.

Column (1) shows a negative, statistically significant relationship between exposure to an epidemic in an individual's impressionable years and current confidence in the national government. Column (5) restricts all variation to within country-year observations The point estimates shrink (while remaining significant at the 1% level) because both treatment and control groups in this setting will have experienced the same epidemics, but at different points in the life cycle. Hence, to the extent that epidemics carry negative effects for other experience windows, we are only estimating the differential impact on individuals who were in their impressionable years (versus not) at the time of epidemic exposure.

In our preferred model (column (4)), an individual with the highest exposure (0.032, that is, *the number of people affected by an epidemic as a share of the population* in an individual's impressionable years) relative to individuals with no exposure has on average 5.1 percentage points (-1.592×0.032) less confidence in the national government after his or her

Table 1. *The Impact of Exposure to Epidemic (18–25) on Political Trust.*

	(1)	(2)	(3)	(4)	(5)
Panel A. Outcome: have confidence in national government					
Exposure to epidemic (18–25)	−1.073*	−0.924	−1.614***	−1.592***	−0.508**
	(0.594)	(0.576)	(0.265)	(0.262)	(0.219)
Observations	760,099	760,099	760,099	760,099	760,099
Panel B. Outcome: approval of the leader					
Exposure to epidemic (18–25)	−1.521***	−1.501***	−1.916***	−1.957***	−0.583***
	(0.380)	(0.369)	(0.326)	(0.330)	(0.118)
Observations	719,742	719,742	719,742	719,742	719,742
Panel C. Outcome: have confidence in honesty of elections					
Exposure to epidemic (18–25)	−1.643**	−1.481*	−2.226***	−2.258***	−1.181***
	(0.794)	(0.811)	(0.341)	(0.339)	(0.273)
Observations	736,679	736,679	736,679	736,679	736,679
Panel D. Outcome: average of all three outcome variables					
Exposure to epidemic (18–25)	−1.365**	−1.248**	−1.855***	−1.867***	−0.705***
	(0.565)	(0.539)	(0.264)	(0.264)	(0.154)
Observations	636,156	636,156	636,156	636,156	636,156
Panel E. Outcome: first principal component of responses					
Exposure to epidemic (18–25)	−4.672**	−4.269**	−6.361***	−6.400***	−2.378***
	(1.932)	(1.841)	(0.914)	(0.913)	(0.530)
Observations	636,156	636,156	636,156	636,156	636,156

Table 1. *Continued*

	(1)	(2)	(3)	(4)	(5)
Country fixed effects	Yes	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	Yes	No
Age group fixed effects	Yes	Yes	Yes	Yes	Yes
Labour market cont. & individual income	No	Yes	Yes	Yes	Yes
Demographic cont. & income decile fixed effects	No	Yes	Yes	Yes	Yes
Country × age trends	No	No	Yes	Yes	Yes
Cohort fixed effects	No	No	No	Yes	Yes
Country × year fixed effects	No	No	No	No	Yes

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%. The main three outcome variables come from the following questions. (*i*) 'In (this country), do you have confidence in each of the following, or not: . . . How about the national government?' (*ii*) 'Do you approve or disapprove of the job performance of the leadership of this country?' (*iii*) 'In (this country), do you have confidence in each of the following, or not: . . . How about the honesty of elections?'. *Exposure to epidemic (18–25)* corresponds to the number of persons affected by an epidemic as a share of the population, averaged over the eight years when the respondent was aged 18 to 25. Responses to all three outcome questions are coded as dummy variables, with one representing a positive answer, and linear probability models are estimated for ease of interpretation. For example, according to column (4), an individual with the highest *Exposure to epidemic (18–25)* relative to individuals with no exposure has on average 5.1 percentage points (-1.592×0.032) less confidence in the national government after their impressionable years. Demographic characteristics include a male dummy, a dummy for each age group, dummy variables for marital status (single, married), educational attainment (tertiary education, secondary education), religion dummies (Christian, Muslim and other religions), employment status (full-time employed, part-time employed, unemployed), a dummy variable for living in an urban area and presence of children in the household (any child under 15). Income decile fixed effects are constructed by grouping individuals into deciles based on their income relative to other individuals within the same country and year. Individual income includes all wages and salaries in the household, remittances from family members living elsewhere and all other sources before taxes. Gallup converts local income to international dollars using the World Bank's individual consumption PPP conversion factor, which makes it comparable across all countries. Results use the Gallup sampling weights and robust SEs are clustered at the country level.

Source: Gallup World Polls, 2006–18 and EM-DAT International Disaster Database, 1970–2017.

impressionable years.[6] Given that the mean level of this outcome variable is 50%, the effect is sizeable.

The second and third panels of the table report results for approval of the performance of the leader and confidence in the honesty of elections. The results on impressionable-year epidemic exposure have the same sign, statistical significance and magnitude (a 6.2-percentage-point decrease in approval of the political leader and a 7.2-percentage-point decrease in the honesty of elections, where the mean outcome levels are 51% in both cases). When we use the average and the first principal component of these variables (as a way of identifying their common element) in panels D and E, respectively, we again obtain very similar results.

5.1. *Do Impressionable-Year Effects Persist As Individuals Age?*

We investigate persistence by estimating our baseline specification on the subsample of older individuals immediately adjacent to their impressionable years (that is, ages 26 to 35) and then roll the age window forward in a series of separate estimates. This permits us to observe how the coefficients change as we increase the distance between the age in which impressionable individuals were exposed to epidemics and the age at which they were surveyed. If the effects are persistent then the estimated coefficient should not change substantially as distance increases between exposure and observation.

Figure 1, based on column (4) of Table 1, shows the effect of epidemic exposure on the outcome variables. The effects on the base subsample (i.e., 26–35) are up to four times larger than the point estimates for the full sample, confirming that the age groups closest to the experience window (i.e., 18–25) are disproportionately affected (compared to other age groups). For this base sample, the median time between the past experience window (median age of 21.5 years) and the subsample (median age of 30.5 years) is nine years, documenting the effect of past epidemics in the medium term.

When the model is re-estimated on successively older subsamples, the magnitude of the impact remains stable for the first six estimates following the base sample before decaying gradually. It comes close to vanishing only estimated on the subsample of individuals aged 36 to 45, at which point the median time distance between the experience window and the subsample is 19 years. Evidently, epidemic experience during the impressionable years has persistent effects on political trust that can remain for two decades of adult life.[7]

5.2. *Are the Results Unique to Impressionable Years?*

One could argue that our treatment effect can be influenced by the potential differential response in individuals who may have experienced the same epidemics, not during their impressionable years, but in other close-by experience windows. Since these individuals will be categorised as counterfactuals in our setting, their potential differential response may drive our estimates

[6] Because epidemics are rare events and our main independent variable of interest, *Exposure to epidemic (18–25)*, is skewed to the right, it may not be appropriate to use its SD or mean when discussing effect size.

[7] Additional analysis (available upon request) shows that our results are qualitatively the same when we include country-specific squared and cubed age trends as additional controls in our baseline specification. It is also possible that individuals dissatisfied with the country's political system immigrate, and thus that older age windows contain individuals who remain in the country because they are relatively more satisfied (compared to those who move out). We therefore use a survey question from Gallup World Polls that asks each individual whether or not they would like to move permanently to another country in the next 12 months. In unreported results (available upon request), we fail to find evidence supporting this possibility.

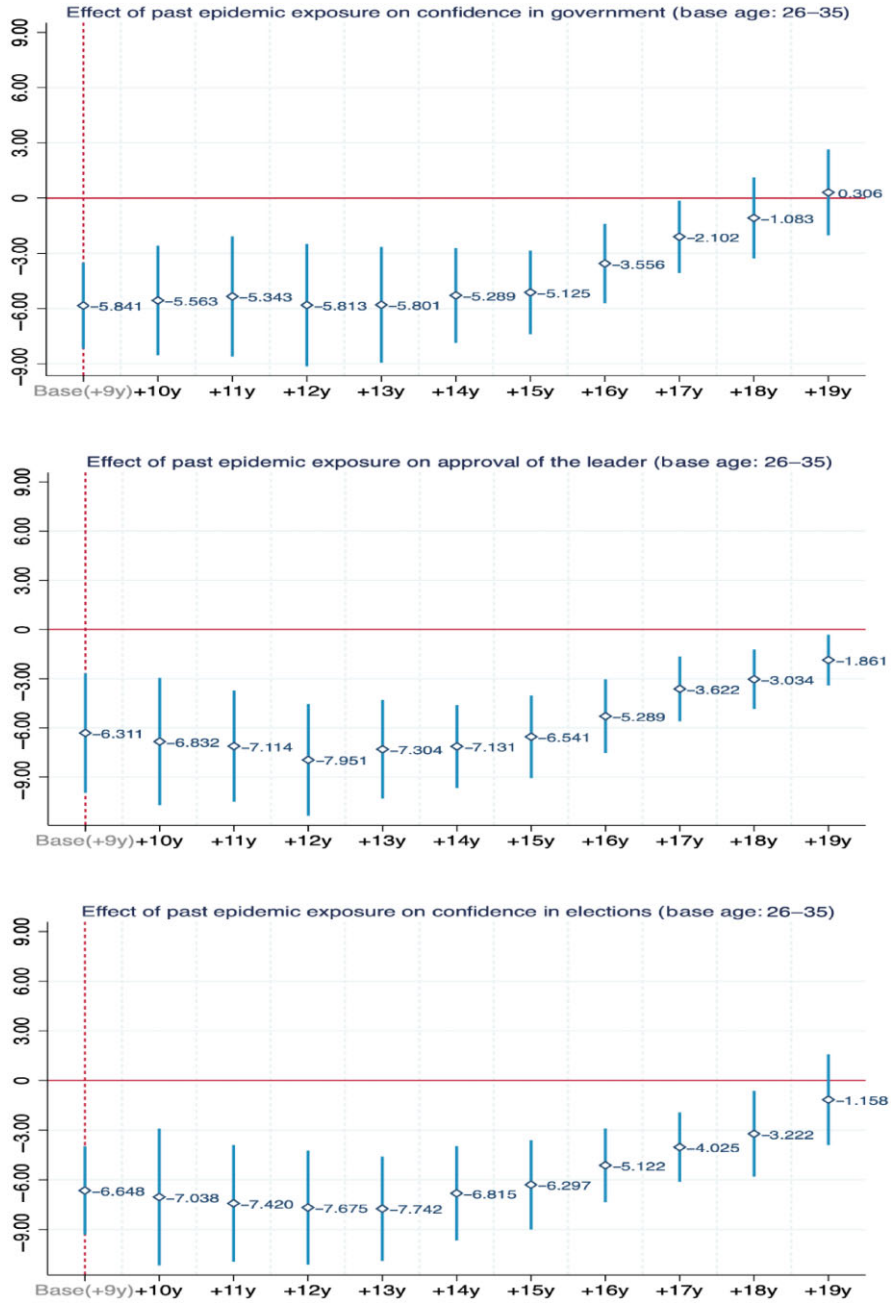

Fig. 1. *Effects of Epidemics in Impressionable Years over Subsamples with Rolling Age Windows.*
Notes: This figure shows the persistency of the effects on three main outcome variables by restricting the observations to the respondents who are in the 26–35 age range at the time of the survey (base sample) and then repeatedly rolling this age window forward by one year for each separate estimation. The specification is column (4) of Table 1 and only the estimated coefficient on *Exposure to epidemic (18–25)* is plotted. Confidence intervals are at the 95% significance level.
Source: Gallup World Polls, 2006–18 and EM-DAT International Disaster Database, 1970–2017.

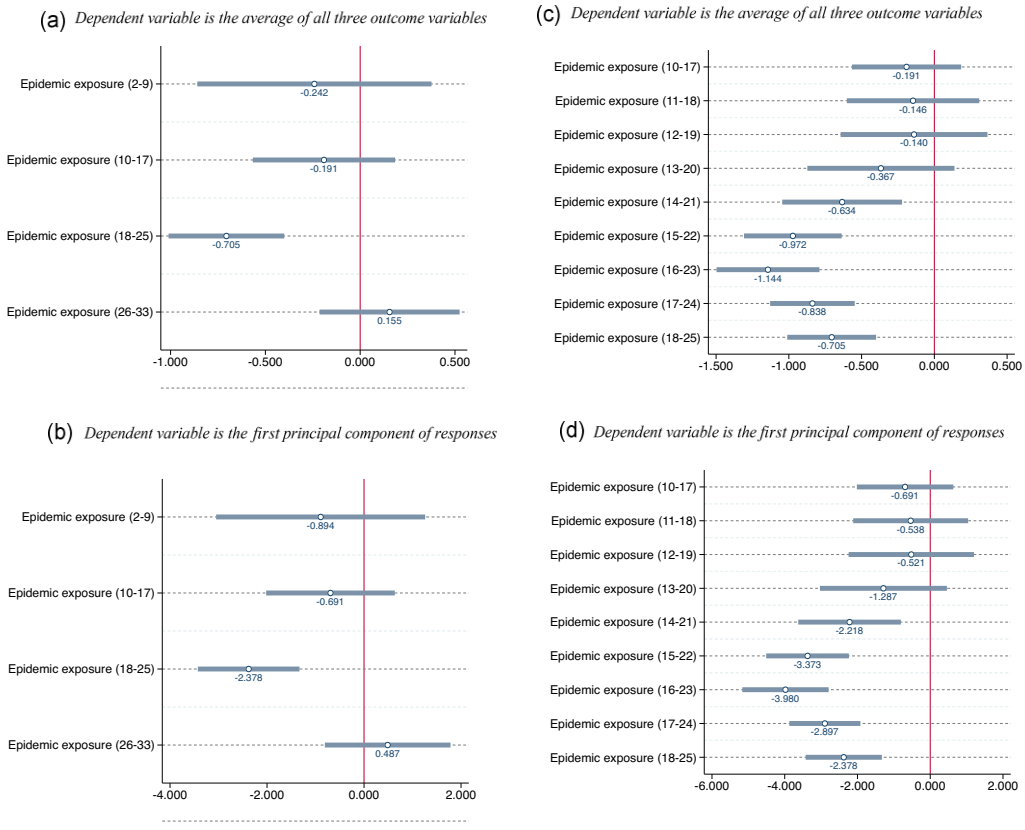

Fig. 2. *Effects of Epidemics in Alternative Treatment Years.*
Notes: This figure shows the treatment effect for various age bands. That is, we calculate for each individual the number of people affected by an epidemic as a share of the population, averaged over the eight years when the individual was 2–9, 10–17, 18–25 and 26–33 years old. Each point estimate comes from four separate models. The specification is column (5) of Table 1. Confidence intervals are at the 95% significance level. Results use the Gallup sampling weights and robust SEs are clustered at the country level.
Source: Gallup World Polls, 2006–18 and EM-DAT International Disaster Database, 1970–2017.

upwards or downwards. In order to check this possibility, we re-estimate our specification on these alternative windows.

Figure 2 shows the effect of exposure in successive eight-year age windows (analogous to the eight-year window of ages 18 to 25). We repeat the analysis only for the first four windows after birth to make sure that we have age-wise comparable samples across separate estimations. It is important to keep in mind that, as we check the later experience windows, respondents' ages at the time of the survey have to be restricted to those older than the corresponding experience window. The analysis focuses on two composite dependent variables: the average of the three outcome variables (panel (a)) and the first principal component of the responses (panel (b)).

In both cases, the negative effect is only evident when epidemic exposure occurs in the individual's impressionable years. This alleviates the concern that an individual who experiences the same epidemic a little earlier or later than the impressionable age window may produce a

differential response compared to an individual who has not experienced any epidemics at any of these windows.

In panels (c) and (d), we examine alternative experience windows, rolling them forward by one year each time from the ages of 10–17 to 18–25. We find that the effects increase in older age windows and reach their maximum during the ages of 16–23 before declining. This suggests that the impressionable ages during which young people are most responsive to epidemic experience could be slightly earlier than the conventional definition used in the previous literature.

5.3. *Additional Analysis and Robustness Checks*

Additional analyses, reported in the Online Appendix, document the robustness of our findings. These include (*i*) controlling in various ways for additional economic, social and political exposures that individuals may have experienced in their impressionable years; (*ii*) conducting an Oster (2019) omitted variable test; (*iii*) estimating models for placebo outcomes related to non-political institutional or social trust; (*iv*) restricting the analysis only to overlapping samples for alternative measures of political trust; (*v*) using an alternative dataset for epidemic events; (*vi*) confirming that countries experiencing pandemics exhibit the same pre-trends in terms of political trust as other countries; (*vii*) focusing on large epidemics; (*viii*) distinguishing between extensive and intensive margins of the treatment effects; (*ix*) comparing the effects of communicable versus non-communicable diseases during impressionable years; (*x*) conducting falsification analyses; (*xi*) implementing multiple hypothesis tests; (*xii*) excluding potential 'bad controls'; (*xiii*) experimenting with alternative treatment definitions; (*xiv*) ruling out influential observations; (*xv*) employing an alternative estimator to take into account the potential negative weights and the heterogeneous treatment effects and (*xvi*) providing additional evidence for the changes in individuals' political behaviour after past epidemic exposure.

6. Evidence on Mechanisms

Our finding of less trust in governments and leaders will be lent additional plausibility if not just self-reported survey responses, but also actual socioeconomic outcomes are negatively affected by impressionable-year epidemic exposure. Therefore, with the aim of exploring the potential economic channels behind the loss of trust documented in the previous section, we implement our strictest specification in Table 2, but substitute dependent variables with the log of the income level (in international dollars), employment status (a dummy indicating employment) and educational attainment (two dummies indicating different levels of education). We find that treated individuals have lower incomes later in life and are less likely to be employed post epidemic exposure. These impacts on economic outcomes may indeed constitute some of the channels through which epidemic exposure leads treated individuals to revise their views of political institutions in negative directions.[8]

Despite the null results documented previously on outcomes related to trust in non-political institutions, there exists an important exception. As reported in Online Appendix Table C1, we

[8] We also find that they are more likely to have graduated from university. Thus, a side effect of unfavourable labour market conditions appears to be heightened incentive for human capital accumulation in the form of tertiary education, given the decrease in the opportunity cost of time. One would not expect to see similar effects on lower levels of education, since such decisions are made before individuals experience the impressionable-year epidemic shock. Reassuringly, we find no such effect.

Table 2. *The Impact of Exposure to Epidemic (18–25) on Potential Economic Channels.*

	Outcome			
	Income (1)	Employed (2)	Tertiary education (3)	Secondary education (4)
Exposure to epidemic (18–25)	−1.634***	−0.946***	0.358**	−0.099
	(0.431)	(0.205)	(0.153)	(0.393)
Country fixed effects	No	No	No	No
Year fixed effects	No	No	No	No
Age group fixed effects	Yes	Yes	Yes	Yes
Labour cont. & individual income	No	No	No	No
Demog. cont. & inc. decile fixed effects	Yes	Yes	Yes	Yes
Country × age trends	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Country × year fixed effects	Yes	Yes	Yes	Yes
Observations	636,156	636,156	636,156	636,156
Adjusted R^2	0.959	0.337	0.181	0.222

Notes: ** significant at 5%; *** significant at 1%. The outcome variables are (*i*) log of the income level (in international dollars), (*ii*) a dummy indicating employment status with one representing a positive answer, (*iii*) a dummy indicating tertiary education status with one representing a positive answer, (*iv*) a dummy indicating secondary education status with one representing a positive answer. *Exposure to epidemic (18–25)* corresponds to the number of persons affected by an epidemic as a share of the population, averaged over the eight years when the respondent was aged 18 to 25. Linear probability models are estimated for binary outcome variables for ease of interpretation. For example, according to column (1), an individual with the highest *Exposure to epidemic (18–25)* relative to individuals with no exposure has on average 5.2 percentage points (−1.634 × 0.032) less income and, according to column (2), the same individual is on average 3 percentage points (−0.946 × 0.032) less likely to be employed after their impressionable years. Outcome variables are not included as controls in the same estimation. Demographic characteristics include a male dummy, a dummy for each age group, dummy variables for marital status (single, married), educational attainment (tertiary education, secondary education), religion dummies (Christian, Muslim and other religions), employment status (full-time employed, part-time employed, unemployed), a dummy variable for living in an urban area and presence of children in the household (any child under 15). Income decile fixed effects are constructed by grouping individuals into deciles based on their income relative to other individuals within the same country and year. Individual income includes all wages and salaries in the household, remittances from family members living elsewhere and all other sources before taxes. Gallup converts local income to international dollars using the World Bank's individual consumption PPP conversion factor, which makes it comparable across all countries. Results use the Gallup sampling weights and robust SEs are clustered at the country level.

Source: Gallup World Polls, 2006–18 and EM-DAT International Disaster Database, 1970–2017.

identify a negative relationship between individuals' impressionable-year exposure to epidemics and their trust in the country's healthcare system. This suggests that the loss of trust in political institutions may be related to the governments' healthcare-related policy responses during past epidemics.

Weak, unstable governments with limited legislative strength, limited unity and limited popular support are least able to mount effective responses to epidemics. If they are prone to disappointing their constituents, we would expect the effects we identify to be strongest when the government in office at the time of exposure is weak and unstable, other things equal. To explore this, we use International Country Risk Guide data on government strength. They measure, for the period since 1984, the unity of the government, its legislative strength and its popular support.

As a first step toward identifying the underlying mechanism, we exploit the recent COVID-19 setting and show in Online Appendix C that government strength is associated with a statistically significant improvement in policy response time (see Online Appendix Table C2 and Figures C1–C3). Given this, we conjecture that weak governments, so measured, also performed poorly during past epidemics, and that individuals in such settings downgrade their confidence

in government and trust in its leaders more severely as a result. Hence, in our second step, we calculate the average score for government strength in the individual's impressionable years. We then construct an indicator that takes the value of 1 for this past experience if the observation is in the bottom half/tercile/quartile of impressionable-year government strength index scores across all respondents. We include this variable categorically rather than in continuous form to limit the likelihood of a response to pandemic experience. We include this measure of impressionable-year government strength by itself in addition to interacting it with impressionable-year epidemic exposure to distinguish epidemic-specific and general effects.

This leads to the specification

$$\begin{aligned} Y_{i,c,t,a,b} = {} & \beta_{10}\,\text{Exposure to epidemic}_{icb} \times \text{Government strength}_{icb} \\ & + \beta_9\,\text{Government strength}_{icb} + \beta_0 + \beta_1 X_{ict} + \beta_2\,\text{Exposure to epidemic}_{\text{icb}} \\ & + \beta_3\,\text{Number of people affected}_{ct-1} + \beta_4 C_c + \beta_5 T_t + \beta_6 A_a + \beta_7 B_b \\ & + \beta_8 C_c \times \text{Age} + \varepsilon_{ict}. \end{aligned} \tag{2}$$

The results reported in Table 3 (reports estimates of (2)) suggest that the effect of exposure to an epidemic on political trust is more than twice as large if the epidemic is experienced under a weak government. These findings suggest that our effects are mostly driven by individuals that experienced epidemics under weak governments who are less able to mount effective responses to epidemics.[9]

Importantly, the point estimates for the weak government dummy itself are small and mostly insignificant. This suggests that we are identifying not a 'weak government effect' per se, but rather the effect of epidemic exposure in the presence of a weak government.[10]

7. Conclusion

We have shown that experiencing an epidemic can negatively affect an individual's confidence in political institutions and trust in political leaders. This negative effect is large, statistically significant and persistent. Its largest and most enduring impact is on the attitudes of individuals in their impressionable late-adolescent and early-adult years when experiencing an epidemic. It is limited to infectious or communicable diseases, where a government's response is especially important. It is the largest in settings where there already exist doubts about the strength and effectiveness of government.

The implications are unsettling. Imagine that more trust in government is important for containment, but that failure of containment harms trust in government. One can envisage a scenario where low levels of trust allow an epidemic to spread, and where the spread of the epidemic

[9] Similar mechanisms are identified by Flückiger *et al.* (2019) in the context of Ebola outbreak in West Africa. The authors showed that the effects of Ebola exposure on perceived state legitimacy are more pronounced in areas where governments responded relatively well to the epidemic.

[10] Online Appendix Figures B1–B3 show further evidence of the importance of government strength at the time of the epidemic. We again restrict the observations to the 26–35 age range and re-estimate (2) when rolling the age window forward. In each figure, the top panel shows the estimates for the total effect on individuals experiencing epidemics under weak governments, while the bottom panel shows the corresponding estimates for individuals experiencing epidemics under strong governments. For all outcomes, the negative impact on trust is larger and more persistent for respondents who experienced epidemics under weak governments. Again, this is consistent with the notion that these individuals became and remained more disenchanted with their country's political institutions and leaders, insofar as those institutions and leaders failed to adequately respond to the country-wide public health emergency.

Table 3. *The Role of Government Strength.*

	Outcome				
	Have confidence in national government (1)	Approval of the leader (2)	Have confidence in honesty of elections (3)	Average of all three outcome variables (4)	First principal component of responses (5)
Exposure to epidemic (18–25) × BelowMedianGov.Strength	−4.033***	−1.092	−2.987***	−2.471***	−8.609***
	(0.876)	(0.848)	(0.618)	(0.704)	(2.460)
Exposure to epidemic (18–25)	−0.235	−3.018***	−1.901**	−1.770*	−5.998*
	(1.038)	(1.044)	(0.832)	(0.988)	(3.446)
BelowMedianGov.Strength	0.014*	0.015*	−0.000	0.009	0.033
	(0.008)	(0.009)	(0.007)	(0.008)	(0.027)
Exposure to epidemic (18–25) × BottomTercileGov.Strength	−3.919***	−2.230***	−4.863***	−3.479***	−11.955***
	(0.719)	(0.628)	(0.559)	(0.565)	(1.964)
Exposure to epidemic (18–25)	−1.048	−2.514***	−1.183*	−1.560**	−5.377**
	(0.808)	(0.693)	(0.697)	(0.724)	(2.521)
BottomTercileGov.Strength	0.013*	0.023***	0.002	0.014*	0.051*
	(0.008)	(0.008)	(0.007)	(0.007)	(0.026)
Exposure to epidemic (18–25) × BottomQuartileGov.Strength	−3.578***	−2.027***	−4.643***	−3.184***	−10.924***
	(0.748)	(0.542)	(0.521)	(0.518)	(1.803)
Exposure to epidemic (18–25)	−1.289	−2.657***	−1.373*	−1.777**	−6.130**
	(0.889)	(0.640)	(0.800)	(0.784)	(2.724)
BottomQuartileGov.Strength	−0.000	0.010	−0.002	0.004	0.015
	(0.008)	(0.010)	(0.008)	(0.008)	(0.028)
Observations	422,522	394,322	412,050	358,771	358,771

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%. The specification is (2) and results reported in each column and panel come from separate models. The main three outcome variables come from the following questions. (*i*) 'In (this country), do you have confidence in each of the following, or not: . . . How about the national government?' (*ii*) 'Do you approve or disapprove of the job performance of the leadership of this country?' (*iii*) 'In (this country), do you have confidence in each of the following, or not: . . . How about the honesty of elections?'. *Exposure to epidemic (18–25)* corresponds to the number of persons affected by an epidemic as a share of the population, averaged over the eight years when the respondent was aged 18 to 25. *Government Strength*, included as a categorical variable, measures the unity of the government, its legislative strength and its popular support, averaged over the eight years when the respondent was aged 18 to 25. Responses to all three outcome questions are coded as dummy variables, with one representing a positive answer, and linear probability models are estimated for ease of interpretation. For example, according to the first panel in column (1), an individual with the highest *Exposure to epidemic (18–25)* who had a weak government in charge at the time (relative to individuals with no exposure or with similar exposure, but a strong government in charge) has on average 12.9 percentage points (−4.033 × 0.032) additional drop in confidence in the national government after their impressionable years. Demographic characteristics include a male dummy, a dummy for each age group, dummy variables for marital status (single, married), educational attainment (tertiary education, secondary education), religion dummies (Christian, Muslim and other religions), employment status (full-time employed, part-time employed, unemployed), a dummy variable for living in an urban area and presence of children in the household (any child under 15). Income decile fixed effects are constructed by grouping individuals into deciles based on their income relative to other individuals within the same country and year. Individual income includes all wages and salaries in the household, remittances from family members living elsewhere and all other sources before taxes. Gallup converts local income to international dollars using the World Bank's individual consumption PPP conversion factor, which makes it comparable across all countries. Results use the Gallup sampling weights and robust SEs are clustered at the country level.
Source: Gallup World Polls, 2006–18, EM-DAT International Disaster Database, 1970–2017 and the International Country Risk Guide (https://www.prsgroup.com/explore-our-products/icrg/).

reduces trust in government still further, hindering the ability of the authorities to contain future epidemics and address other social problems. As Schmitt (2020) puts it, 'lack of trust in government can be a circular, self-reinforcing phenomenon: Poor performance leads to deeper distrust, in turn leaving government in the hands of those with the least respect for it.'

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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