Armed Conflict Increases Elephant Poaching

Gabriel Englander

Between 2002 and 2014, more than 100 armed, intergroup conflicts began near elephant habitat in Africa and Asia. In the same period, many elephant populations have been decimated by poaching (Wittemyer et al., 2014; Chase et al., 2016; Thouless et al., 2016). In this chapter, I exploit variation over space and time in conflict onset to estimate the effect of conflict on elephant poaching.

Existing research has built strong suggestive evidence that conflict increases poaching. For example, poaching effort has been shown to increase during conflict when combatants use ivory to fund their operations (Hatton et al., 2001; Beyers et al., 2011). Researchers have also shown that anti-poaching enforcement decreases when park rangers are targeted by combatants or when international organizations withdraw from the conflict zone (Beyers et al., 2011; Dudley et al., 2002; Yamagiwa, 2003; Hanson et al., 2009). Most recently, Daskin and Pringle (2018) find an association between years of conflict and declining large wild herbivore populations in African protected areas.

One limitation of existing research is that both conflict and poaching are likely caused by factors that are unobservable or difficult to measure accurately, such as institutional quality (Dudley et al., 2002; Hanson et al., 2009; Blattman & Miguel, 2010; Gaynor et al., 2016). Omitting such variables from analysis biases estimates

I thank Peter Berck, Solomon Hsiang, Maximilian Auffhammer, Paula Ochiel, Kate Pennington, Edward Rubin, Derek Wolfson, Wenfeng Qiu, and Karl Dunkle Werner for constructive comments and discussion.

Supplementary Information The online version contains supplementary material available at (https://doi.org/10.1007/978-3-031-24823-8_13).

G. Englander (✉)
Development Research Group, World Bank, Washington, DC, USA
e-mail: aenglander@worldbank.org

D. Zilberman et al. (eds.), *Sustainable Resource Development in the 21st Century*, Natural Resource Management and Policy 57,
https://doi.org/10.1007/978-3-031-24823-8_13

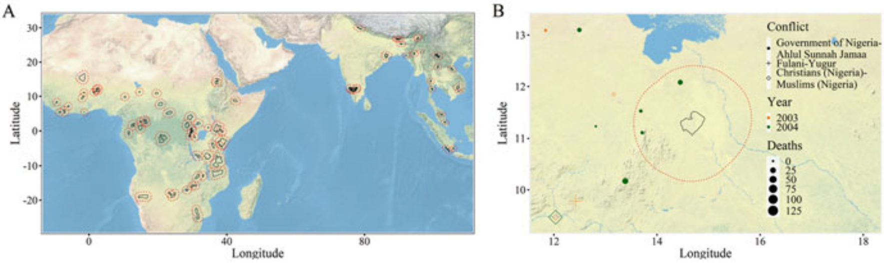

Fig. 1 **MIKE sites and data processing example**. **A** MIKE site boundaries (solid) and 100 km buffers (dashed). Some MIKE sites have multiple boundary polygons associated with them. The 100 km buffers were drawn around each boundary polygon and then combined by MIKE site. **B** Conflict onset calculation for Waza National Park, Cameroon, in 2004. The conflict between the Government of Nigeria and Ahlul Sunnah Jamaa is defined as beginning in 2004 because there were fewer than 25 battle deaths associated with this conflict in 2003 and more than 25 battle deaths associated with this conflict in 2004. Conflict onset for Waza National Park is defined as occurring in 2004 because at least one of the battles in the Government of Nigeria-Ahlul Sunnah Jamaa conflict in 2004 occurred within 100 km of Waza National Park

of the effect of conflict on poaching (Angrist & Pischke, 2008). Given that funding for anti-poaching enforcement is limited, understanding the causal effect of conflict on poaching would enable policymakers and conservation practitioners to better allocate funding among conservation priorities and respond when conflict occurs.

My regression models control for all time-invariant site characteristics, all location-invariant temporal effects, and flexible functions of temperature and precipitation. After controlling for these variables, the estimates are causal as long as the remaining variation in omitted variables is not correlated with both conflict onset and poaching (see the Methods section). I relax this assumption and test it indirectly using several different methods. Overall, this empirical approach—the best available given the nature of conflict and poaching—seems to yield estimates that are plausibly causal.

The Monitoring the Illegal Killing of Elephants (MIKE) program has operated since 2002 and includes data from 77 sites in 39 countries across Africa and Asia (Fig. 1a). MIKE's data collection methodology allows for a measure of poaching called the Proportion of Illegally Killed Elephants (PIKEs). Each year, each site's PIKE equals the number of observed poached elephant carcasses divided by the total number of observed elephant carcasses. PIKE is a relatively reliable measure of poaching because it is independent of surveyor effort and elephant population stock under an assumption discussed below. Intensive studies of a small number of MIKE sites find that PIKE accurately represents mortality patterns (Kahindi et al., 2010; Jachmann, 2012). Supplementary Table 1 provides the summary statistics of the MIKE data.

Conflict onset is a commonly used measure of conflict (Miguel et al., 2004; Blattman & Miguel, 2010; Bazzi & Blattman, 2014) and is the preferred measure in this work for several reasons. As opposed to measures of conflict intensity, such as the number of human deaths, using conflict onset in a regression framework requires no assumptions on the structure of its relationship with poaching. Onset events are discrete shocks to the incentives and resources available to potential poachers and anti-poaching authorities. This characteristic makes onset events arguably more exogenous with respect to poaching than measures of conflict intensity. It also gives conflict onset more statistical power to identify changes in poaching levels. For example, a new conflict will tend to induce greater variation in the behavior of park rangers than would a change in conflict intensity.

A conflict, defined by a unique pair of actors (e.g., Government of Nigeria vs. Ahlul Sunnah Jamaa), is active in a given year if 25 or more battle deaths were associated with it that year (Sundberg & Melander, 2013). I define a conflict to begin in a given year if there were fewer than 25 battle deaths in the previous year and 25 or more battle deaths in the current year. My results are robust to using different battle death thresholds to define onset events (Supplementary Fig. 1).

I connect conflict onset events to MIKE sites by drawing a buffer around each MIKE site and checking for each site–year whether a battle occurred within the buffer that belongs to a conflict that began that year. Figure 1b displays an example of this procedure for one site–year. Compared to all other conflict onset events in Africa and Asia between 2002 and 2014, onset events that occur close to MIKE sites are more likely to involve non-state actors killing civilians (Supplementary Table 2). This difference is consistent with rebel groups and terrorists exploiting local populations, in part by poaching their elephants (Christy & Stirton, 2015).

Results

Contemporaneous Effect

I find that the onset of a new conflict within 100 km of a MIKE site significantly increases contemporaneous PIKE in that MIKE site by 0.057 to 0.103 (Table 1). Relative to the average PIKE for the entire data (0.467), these estimates represent an increase in poaching of 12–22%. This result persists even when additionally controlling for site-specific trends (Column 2) or country-by-year indicator variables (Column 3). These results are robust to using different buffer distances to link onset events to MIKE sites, using different measures of poaching and different estimation procedures, using different measures of conflict, and using MIKE data between 2002 and 2017 without weather control variables (Supplementary Fig. 2 and Supplementary Tables 3–5, respectively). The estimate from the preferred specification in Table 1, Column 1 is more than 2.5 times larger than the estimated upper bound on bias from omitted variables (Altonji et al., 2005), indicating that unobservables correlated with conflict onset and poaching are not driving these results (see the Methods section).

Temporal Dynamics

Conflict onset has both an immediate and a persistent effect on poaching levels, exacerbating its negative impact (Fig. 2). In the years before conflict onset, poaching levels are relatively constant, indicating that fighters already present in the area are

Table 1 Conflict onset increases contemporaneous poaching

	Site and year fixed effects	With site trends	With country-by-year fixed effects
Conflict onset	0.103***	0.057**	0.082*
	(0.031)	(0.025)	(0.042)
R-squared	0.567	0.714	0.848

Coefficients represent the effect of conflict onset on contemporaneous poaching, where poaching is measured by PIKE. All regressions are estimated by ordinary least squares with 631 observations and include MIKE site fixed effects, year fixed effects, and third-order polynomials in temperature and precipitation as control variables (see the Methods section). Column 2 adds MIKE site-specific trends to the base specification. Column 3 adds country-by-year fixed effects to the base specification (which subsume the year fixed effects). Clustered standard errors at the country level are displayed in parentheses and are estimated by bootstrapping with replacement at the country level (1000 replications). ***$P < 0.01$; **$P < 0.05$; *$P < 0.1$.

not increasing poaching to fund an anticipated conflict (no reverse causality). At conflict onset, there is a spike in poaching. Relative to poaching in the year before onset, PIKE increases by 0.25, a more than 50% increase relative to its mean value. Poaching then slowly declines to baseline levels in the years following the onset event. These intuitive temporal dynamics provide further evidence that conflict onset has a causal effect on poaching.

PIKE Assumption and Reliability of PIKE Data

PIKE is independent of population stock and surveyor effort if, conditional on the number of poached and non-poached carcasses available to discover, the probability of finding a poached carcass equals the probability of finding a non-poached carcass (Burn et al., 2011; Hsiang & Sekar, 2016). Violations of this assumption that are uncorrelated with conflict onset induce classical measurement error, which would attenuate my estimates but not cause bias. However, my estimates would be biased if this assumption is systematically violated at conflict onset. For example, if fighters occupy part of a MIKE site and prevent rangers from surveying the area, the probability of detecting poached carcasses may decrease. In this case, my estimates are biased downward and conflict onset actually has an even larger effect on poaching. If, instead, conflict onset leads to improved intelligence gathering and poached carcass detection increases, I would overestimate the effect of conflict onset on poaching. Reassuringly, even if the probability of detecting a poached carcass becomes up to 35% higher at conflict onset (and is unchanged for all other observations in which conflict onset does not occur), the effect of conflict onset on PIKE would still be statistically significant at the 95% level after "correcting" for this bias and re-estimating the Column 1 regression of Table 1 (Supplementary Fig. 3).

Conflict onset also does not seem to affect the availability of poaching data (no selective attrition). While poaching data only exists for 631 out of 1078 possible site–year combinations, the conflict data is comprehensive. The proportion of site–years missing poaching data if conflict onset occurs is 39.4% and is 36.5% if conflict

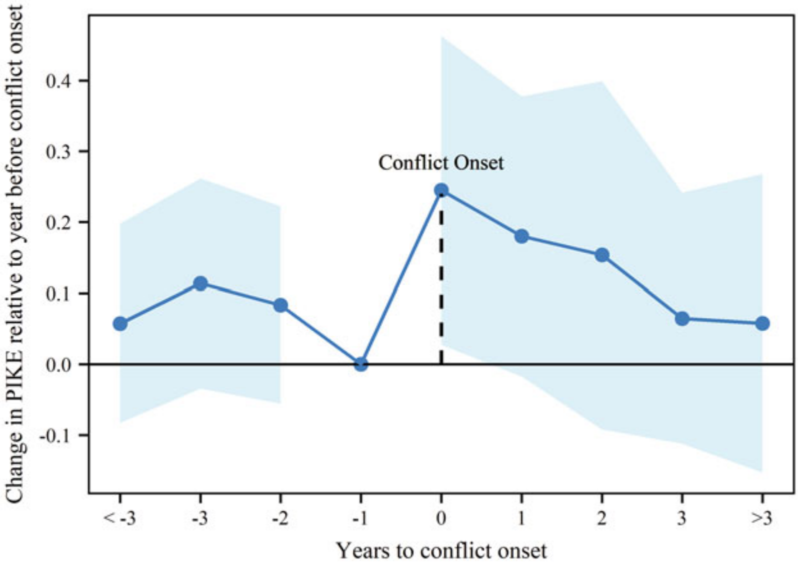

Fig. 2 Temporal dynamics of poaching with respect to conflict onset. Each point estimate represents the change in PIKE relative to the year before conflict onset (the omitted category). Regressors used as controls: site fixed effects, year fixed effects, and third-order polynomials in temperature and precipitation. Standard errors are estimated by cluster bootstrapping with replacement at the country level (1000 replications). 95% confidence intervals are displayed. N = 631

onset does not occur (p-value from a two-sided t-test equals 0.52). Furthermore, I find that conflict onset does not affect elephant natural mortality, providing indirect evidence that carcasses are classified accurately (Supplementary Table 6). To the extent that natural mortality carcass count is an indicator of surveyor effort (conditional on control variables), this null result also suggests that conflict onset does not affect surveyor effort.

Discussion

As poaching continues to threaten the survival of elephants in the wild, causal estimates of the drivers of poaching can help better allocate limited anti-poaching effort and funds. In this chapter, I find that conflict onset causes a substantial increase in poaching. This evidence supports previous appeals to governments and international conservation organizations to increase support for park rangers during periods of conflict, as rangers and associated law enforcement personnel can mitigate the negative effects of conflict on wildlife (Dudley et al., 2002; Yamagiwa, 2003; Beyers et al., 2011).

By using a similar approach as in Fig. 2, I estimate that ∼30% of poached carcasses in the MIKE data set are attributable to the contemporaneous and persistent effects of conflict (see the Methods section). By extrapolation, I calculate that conflict was responsible for the illegal killing of about 80,000 elephants in Africa and Asia between 2002 and 2014. For comparison, there are about 600,000 elephants remaining in the wild (Thouless et al., 2016; Sukumar, 2006).

Elephant poaching, and wildlife and habit conservation as a whole, are emotional, salient, and complex problems that could be better addressed with more empirical evidence on the causes of negative outcomes. While I cannot distinguish between the various channels through which conflict affects poaching, this work is nevertheless the first to present plausibly causal estimates of a driver of site-level poaching dynamics for any species. The wide spatial and temporal range of the data used to obtain these estimates supports their external validity. Future work on identifying channels through which conflict affects poaching will need to balance the use of micro-level data without limiting analysis to a small subset of locations and years.

Methods

Poaching Data

I use publicly available data on the numbers of carcasses found for each MIKE site and year (Convention on International Trade in Endangered Species, 2017). During the course of regular patrols, rangers and associated personnel record each elephant carcass observed and attempt to determine whether the elephant was poached (Burn et al., 2011). Thus, for each site–year, two values are recorded: the number of poached carcasses and the total number of carcasses, from which the number of non-poached carcasses (i.e., natural mortality) can be inferred. MIKE sites contain 30–40% of wild elephants (Convention on International Trade in Endangered Species, 2016). In constructing the poaching data I use in the regressions, I dropped three MIKE sites with only one observation. Because I include a separate indicator variable for each site in all regressions (“site fixed effects”), these three sites would not have contributed to my estimates.

Conflict Data

I use the publicly available Uppsala Conflict Data Program (UCDP) Georeferenced Event Dataset (Sundberg & Melander, 2013; Croicu & Sundberg, 2017). Each row of this dataset corresponds to an armed battle event and includes the day the battle occurred, GPS coordinates, estimated number of battle deaths, a news source, and the actors involved. The dataset uses conflict identifiers to group events by unique actor pairs. For example, Lord’s Resistance Army vs. Government of Uganda is one conflict, and Lord’s Resistance Army vs. civilians is a different conflict. In constructing the conflict data used in the analysis, I excluded battles where only the country in which the battle took place was known. Battles that occur within MIKE site boundaries are included when connecting onset events to MIKE sites. Conflicts with battles occurring outside MIKE site buffers may still assign onset status to a given MIKE site as long as at least one battle occurs within the MIKE site buffer.

Importance of Controlling for Temperature and Precipitation

As MIKE sites and their surrounding areas are primarily rural, variation in agricultural yields and wages could affect both poaching and the probability of conflict onset. Even if such data were available for all site–years, controlling for agricultural yields, for example, would be a "bad control" because conflict onset likely affects yields (Angrist & Pischke, 2008). Therefore, flexibly controlling for temperature and precipitation, which are not affected by conflict onset and poaching, is the best available approach. It is also important to control for precipitation because low precipitation levels can cause elephant mortality, which reduces PIKE by inflating its denominator (Dudley et al., 2001). Because low precipitation levels also increase conflict onset (Miguel et al., 2004), not controlling for precipitation would bias my estimates downward. None of my regression specifications yield statistically significant relationships between poaching and temperature or between poaching and precipitation. Nevertheless, it is important to control for temperature and precipitation because of their theoretical importance as potential determinants of both conflict onset and poaching.

Weather Data

I use publicly available data from the University of Delaware to control for third-order polynomials in temperature and precipitation (Matsuura & Willmott, 2015). This data provides cumulative monthly precipitation and mean monthly temperature data at a 0.5 degree resolution until 2014. I first calculate squared and cubed terms for each grid cell. Then, I spatially aggregate grid cells to the site level by weighting cell values by the proportion of area that they account for in a MIKE site and its buffer. Finally, I sum over months in the same year to obtain a third-order polynomial in cumulative annual precipitation for each site–year and weight monthly mean temperature by the days in a year that each month accounts for, to obtain a third-order polynomial in mean temperature for each site–year.

Regression Estimation

In my preferred specification in Table 1, Column 1, I estimate the following multivariate panel regression using ordinary least squares:

$$PIKE_{sct} = \beta Onset_{sct} + \gamma_s + \delta_t + \sum_{k=1}^{3} \alpha_k temp_{sct}^k + \sum_{k=1}^{3} \theta_k precip_{sct}^k + \epsilon_{sct}, \tag{1}$$

where s indexes site, c indexes country, t indexes year, γ_s are site fixed effects (separate indicator variable for each site), δ_t are year fixed effects (separate indicator variable for each year), and k indicates the term of the third-order polynomial in temperature and precipitation. The distribution of residuals from estimating this equation is approximately normal (Supplementary Fig. 4). The coefficient on conflict onset (β) is causally identified if $Onset_{sct}$ is uncorrelated in expectation with ϵ_{sct} (time-varying, within-site unobservable determinants of $PIKE_{sct}$).

Unobservable changes over time at particular sites that affect both poaching and conflict onset, such as a deterioration in local institutions, could violate this assumption. Table 1, Column 2 regression adds site-specific trends ($\gamma_s t$) to the controls in Eq. (1). The estimated effect in this specification is slightly smaller than in the preferred specification, but its statistical significance implies that these types of unobservable changes are not driving my results.

Time-varying, country-level shocks are another threat to the above assumption. For example, changes in political or economic conditions, such as a coup or export price shock, or changes in national anti-poaching policy, could simultaneously affect poaching and the probability of conflict onset. Table 1, Column 3 regression controls for all such confounders by replacing the year fixed effects in Eq. (1) with country-by-year fixed effects (δ_{ct}). This specification yields a similar estimate as Eq. (1), indicating that my results are not due to time-varying, country-level confounders.

MIKE sites in the same country may have serially correlated errors. I therefore estimate standard errors in all ordinary least squares regressions by cluster bootstrapping with replacement at the country level (1000 replications). Clustering at the country level allows the errors of sites in the same country to be arbitrarily correlated across all time periods but assumes the errors of sites in different countries are uncorrelated. I bootstrap instead of using the standard clustering formula because the small number of countries in my data (39) may make standard errors calculated by the formula too small (Cameron et al., 2008).

Upper Bound on Omitted Variables Bias

In case the assumption necessary for Eq. (1) to estimate a causal effect is violated, it is important to assess the extent to which my estimates are confounded by omitted variables. Altonji et al. (2005) provide a proof and method for estimating an upper bound on omitted variables bias given the following assumption: the relationship between conflict onset and observable determinants of PIKE (control variables) is at least as strong as the relationship between conflict onset and unobservable determinants of PIKE. This assumption is reasonable because of the strong predictive power of my control variables. The site fixed effects are especially relevant because some sites are more prone to conflict than others, for reasons that vary little over the study period. For example, 61% of sites have no conflict onset events in years with poaching data, while Virunga National Park has an onset event every year (results are robust to dropping these sites and re-estimating Eq. (1)).

I estimate the upper bound on omitted variables bias to be 0.041. My coefficient estimate is 0.103 (Table 1, Column 1) or 2.5 times greater than this upper bound. Therefore, my finding that conflict onset increases poaching is not driven by omitted variables bias.

Estimating Temporal Dynamics

An event study maps temporal dynamics of the dependent variable relative to the date of treatment (Jacobson et al., 1993). Figure 2 presents results from estimating the following regression by ordinary least squares:

$$
\begin{aligned}
PIKE_{sct} = & \sum_{y=-4}^{4\backslash\{-1\}} \beta_y Onset_{y,sct} + \gamma_s + \delta_t \\
& + \sum_{k=1}^{3} \alpha_k temp_{sct}^k + \sum_{k=1}^{3} \theta_k precip_{sct}^k + \epsilon_{sct},
\end{aligned}
\tag{2}
$$

where subscript y indexes time relative to the year of conflict onset. All other variables and subscripts are as defined for Eq. (1). For $y < 0$ ($y > 0$), $Onset_{y,sct} = 1$ if conflict onset occurs in y years (occurred y years ago) and equals 0 otherwise. $Onset_{0,sct} = 1$ for site–years with onset events and equals 0 otherwise.

For each observation, I calculate the number of years until the next conflict onset and the number of years since the most recent conflict onset (within the same MIKE site). This calculation is not affected by missing poaching data because the conflict data is comprehensive. I include indicator variables (the $Onset_{y,sct}$ terms) for observations that occur 3 years before conflict onset, 2 years before onset, year of onset, and 1, 2, and 3 years after onset. I group observations that occur four or more years before the next conflict onset into an additional indicator variable and do the same for observations that occur four or more years after the most recent conflict onset. Sites that never had conflict onset are not included in any of these indicator variables by definition. The year before conflict onset is the omitted category (including it would cause collinearity with site fixed effects).

Extrapolation

I first estimate a modified version of Eq. (2). Because I want to calculate the number of poached elephants attributable to conflict onset, I use poached carcass count as the dependent variable, add ln(natural mortality count + 1) as an additional control variable, and estimate Eq. (2) using a negative binomial regression with a log link function. I chose a negative binomial model instead of a Poisson model because of overdispersion in poached carcass counts (Supplementary Table 1). Supplementary Figure 5 plots the $Onset_{y,sct}$ coefficients and standard errors from this regression. I use this model to predict the number of poached carcasses in the data and to predict the number of poached carcasses if conflict onset did not occur (set $Onset_{y,sct} = 0$ if $y \geq 0$, then predict). The difference in these two predictions is 2092 (equal to 30% of the total poached carcasses in the MIKE data between 2002 and 2014). The interpretation of this difference is that there would have been 2092 fewer poached carcasses in the MIKE data if no conflict onset events had occurred.

I rely on estimates of the number of African elephants poached between 2010 and 2012 in order to extrapolate from the MIKE data to the total number of elephants poached in Africa and Asia between 2002 and 2014 (Wittemyer et al., 2014). Wittemyer et al. (2014) estimate that 100,891 African elephants were poached between 2010 and 2012 (average of empirical and model-based method in Table 1 of that paper). These estimates are the best available because there are no peer-reviewed, global estimates of the number of elephants poached each year.

There were 2743 poached carcasses discovered in MIKE's African sites between 2010 and 2012. Compared to Wittemyer et al. (2014), a poached carcass discovered at an African MIKE site in this period represents 36.8 poached carcasses ($= \frac{100{,}891}{2743}$). Given the strong assumption that this ratio is constant between 2002 and 2014 and holds for Asia as well, conflict onset was responsible for the illegal killing of 76,963 elephants between 2002 and 2014 ($= 2092 \times 36.8$). This rough extrapolation is meant to emphasize the important contribution of conflict to overall poaching levels.

References

Altonji, J. G., Elder, T. E., & Taber, C. R. (2005). Selection on observed and unobserved variables: Assessing the effectiveness of Catholic schools. *Journal of Political Economy, 113*(1), 151–184.

Angrist, J. D., & Pischke, J.-S. (2008). *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton: Princeton University Press.

Bazzi, S., & Blattman, C. (2014). Economic shocks and conflict: Evidence from commodity prices. *American Economic Journal: Macroeconomics, 6*(4), 1–38.

Beyers, R. L., Hart, J. A., Sinclair, A. R. E., Grossmann, F., Klinkenberg, B., & Dino, S. (2011). Resource wars and conflict ivory: The impact of civil conflict on elephants in the Democratic Republic of Congo—The Case of the Okapi Reserve. *PLoS ONE, 6*(11), e27129.

Blattman, C., & Miguel, E. (2010). Civil war. *Journal of Economic Literature, 48*(1), 3–57.

Burn, R. W., Underwood, F. M., & Blanc, J. (2011). Global trends and factors associated with the illegal killing of elephants: A hierarchical Bayesian analysis of carcass encounter data. *PLoS ONE, 6*(9), e24165.

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *Review of Economics and Statistics, 90*(3), 414–427.

Chase, M. J., Schlossberg, S., Griffin, C. R., Bouché, P. J. C., Djene, S. W., Elkan, P. W., Ferreira, S., Grossman, F., Kohi, E. M., Landen, K., et al. (2016). Continent-wide survey reveals massive decline in African savannah elephants." *PeerJ, 4*, e2354.

Christy, B., & Stirton, B. (2015). How killing elephants finances terror in Africa. *National Geographic, 12*. https://www.nationalgeographic.com/tracking-ivory/article.html.

Convention on International Trade in Endangered Species (2016). *Report on Monitoring the Illegal Killing of Elephants (MIKE)*. https://cites.org/sites/default/files/eng/cop/17/WorkingDocs/E-CoP17-57-05.pdf.

Convention on International Trade in Endangered Species (2017). *Numbers of carcasses found for each reporting site and year*. https://cites.org/eng/prog/mike/data_and_reports.

Croicu, M., & Sundberg, R. (2017). UCDP GED Codebook version 5.0. In *Department of Peace and Conflict Research, Uppsala University*. http://ucdp.uu.se/downloads/ged/ged50-rdata.zip.

Daskin, J. H., & Pringle, R. M. (2018). Warfare and wildlife declines in Africa's protected areas. *Nature, 553*, 328–332.

Dudley, J. P., Ginsberg, J. R., Plumptre, A. J., Hart, J. A., & Campos, L. C. (2002). Effects of War and Civil Strife on Wildlife and Wildlife Habitats. *Conservation Biology, 16*(2), 319–329.

Dudley, J. P., Criag, G. C., Gibson, D. St. C., Haynes, G., & Klimowicz, J. (2001). Drought mortality of bush elephants in Hwange National Park, Zimbabwe. *African Journal of Ecology, 39*(2), 187–194.

Gaynor, K. M., Fiorella, K. J., Gregory, G. H., Kurz, D. J., Seto, K. L., Withey, L. S., & Brashares, J. S. (2016). War and wildlife: Linking armed conflict to conservation. *Frontiers in Ecology and the Environment, 14*(10), 533–542.

Hanson, T., Brooks, T. M., Da Fonseca, G. a. B., Hoffmann, M., Lamoreux, J. F., Machlis, G., Mittermeier, C. G., Mittermeier, R. A., & Pilgrim, J. D. (2009). Warfare in Biodiversity Hotspots. *Conservation Biology, 23*(3), 578–587.
Hatton, J., Couto, M., & Oglethorpe, J. (2001). Biodiversity and war: A case study of Mozambique. In *Biodiversity Support Program, Washington, DC*.
Hsiang, S., & Sekar, N. (2016). Does Legalization Reduce Black Market Activity? Evidence from a Global Ivory Experiment and Elephant Poaching Data. In *National Bureau of Economic Research Working Paper 22314*.
Jachmann, H. (2012). Pilot study to validate PIKE-based inferences at site level. *Pachyderm, 52*, 72–87.
Jacobson, L. S., LaLonde, R. J., & Sullivan, D. G. (1993). Earnings losses of displaced workers. *American Economic Review, 83*(4), 685–709.
Kahindi, O., Wittemyer, G., King, J., Ihwagi, F., Omondi, P., & Douglas-Hamilton, I. (2010). Employing participatory surveys to monitor the illegal killing of elephants across diverse land uses in Laikipia-Samburu, Kenya. *African Journal of Ecology, 48*(4), 972–983.
Matsuura, K., & Willmott, C. J. (2015). Terrestrial Air Temperature: 1900–2014 Gridded Monthly Time Series. Version 4.01.. http://climate.geog.udel.edu/~climate/html_pages/Global2014/README.GlobalTsT2014.html. Accessed January 8, 2017.
Miguel, E., Satyanath, S., & Sergenti, E. (2004). Economic shocks and civil conflict: An instrumental variables approach. *Journal of Political Economy, 112*(4), 725–753.
Sukumar, R. (2006). A brief review of the status, distribution and biology of wild Asian elephants. *International Zoo Yearbook, 40*, 1–8.
Sundberg, R., & Melander, E. (2013). Introducing the UCDP georeferenced event dataset. *Journal of Peace Research, 50*(4), 523–532.
Thouless, C. R., Dublin, H. T., Blanc, J. J., Skinner, D. P., Daniel, T. E., Taylor, R. D., Maisels, F., Frederick, H. L., & Bouché, P. (2016). African Elephant Status Report 2016: An update from the African Elephant Database. In *Occasional Paper Series of the IUCN Species Survival Commission No. 60*.
Wittemyer, G., Northrup, J. M., Blanc, J., Douglas-Hamilton, I., Omondi, P., & Burnham, K. P. (2014). Illegal killing for ivory drives global decline in African elephants. *Proceedings. National Academy of Sciences. United States of America, 111*(36), 13117–13121.
Yamagiwa, J. (2003). Bushmeat poaching and the conservation crisis in Kahuzi-Biega National Park, Democratic Republic of the Congo. *Journal of Sustainable Forestry, 16*(3-4), 111–130.