A Global Experiment in Black Market Dynamics: the Effect of Legal Ivory Sales on Illegal Ivory Production

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Abstract

Black markets are estimated to represent a fifth of global economic activity, but their response to global policy is poorly understood because participants systematically hide their actions. It is widely hypothesized that relaxing global trade bans in illegal goods allows legal supplies to competitively displace illegal supplies, but a richer economic theory provides more ambiguous predictions. Here we evaluate the first major legalization experiment in an internationally banned market, where a monitoring system established before the experiment enables us to observe the behavior of illegal suppliers before and after. International trade of ivory was banned in 1989, with global poaching data collected by ecologists since 2003. A one-time legal sale of stocks in 2008 was designed as an experiment, but its global impact has not been evaluated. We find that international announcement of the legal ivory sale corresponds with an abrupt 38% increase in illegal ivory production across two continents, and a ten-fold increase in its trend. The recent increase in poaching apparently originated primarily with the experimental sale. More generally, these results suggest that changes to producer costs and/or consumer demand induced by legal sales may have larger effects than displacement of illegal production in some global black markets, implying that partial legalization of banned goods does not necessarily reduce black market activity.

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Societies frequently endeavor to ban the trade of products and services—like certain drugs, weapons, body parts of endangered animals, human organs, or prostitution—deemed to have unfavorable consequences for individuals or society at large. Black markets emerge in response to these bans, often leading to pernicious consequences; this unregulated economy is estimated to be 23% of global GDP [1]. It is thus unclear whether bans achieve a desired overall effect of minimizing adverse effects on populations [2]. Prevailing economic theory suggests that legalizing a regulated supply of a banned good (creating a "white market") is preferable to an outright ban if the legal supply is less harmful and can competitively displace the illegal supply [3, 4] (SOM Section A and Figure S1A). Alternative theories suggest the conditions required for competitive displacement do not occur for all goods [4, 5]. Most banned goods are at the center of a debate on the likely effects of legalization.

Such a debate is at the heart of ivory trade regulation. In 1989, the Convention on the International Trade of Endangered Species (CITES) banned the international trade of ivory, depressing black market demand for some years and resulting in an increase in wild African elephant (*Loxodonta africana*) populations [3, 6, 7]. By the mid-1990s, poaching for ivory had begun to climb again, possibly due to rising demand from economically empowered Asia [8]. Pro-trade groups suggested that governments could sell their own legal stockpiles of ivory—collected from elephants that died naturally—to Asian buyers, thereby competitively displacing black market ivory sales [6].

Opponents of legal ivory sales warned that the one-off sale could lead to a substantial increase—not decrease—in the poaching of elephants [9, 10]. Such an effect may occur for two reasons (SOM Section A). First, when a ban exists, it suppresses demand since some potential consumers are unlikely to buy banned substances [4, 11], perhaps due to social stigma or fear of punishment. Easing bans may thus bring these consumers back into the market [5]. Since legal supplies of ivory would be limited, this increased demand could spill over into the black market (Figure S1B), especially if consumers have difficulty distinguishing legal and illegal versions of the same good. Secondly, relaxing a ban may make it more difficult for authorities to distinguish illegal ivory from legal versions [9, 12], lowering the costs of smuggling or selling illegal ivory and thus increasing supply at a given price (Figure S1C).

After a legal one-time sale of ivory to Japan in 1999 whose effects were unmonitored, CITES gradually established 77 sites throughout Africa and Asia to monitor the effects of possible future sales. The primary data collected, an index called PIKE (Proportion of Illegally Killed Elephants), measures the fraction of detected elephant carcasses that were illegally killed [13, 14]. Then China, a major market for illegal ivory [15, 16], joined Japan in requesting the right to purchase ivory legally in

a one-time sale [10, 9, 17]; opponents stated that China's regulatory system was not stringent enough to prevent the conflation of legal and illegal ivory [9].

In July 2008, after years of debate [17, 18, 19], CITES approved China to join Japan as the second designated trading partner for legal ivory and declared that an "experimental" sale may proceed [17, 20]; in October-November of 2008, China and Japan purchased 62 and 45 tons of ivory, respectively, at auction [21, 22]; and in March-April 2009, the shipment of legal African ivory arrived in China and Japan [21], where the governments resell the ivory to citizens via domsetic markets (see SOM Section B-C and Table S1 for a detailed timeline).

The 2008 sale represents the first global experiment in the partial legalization of an international market that was otherwise banned worldwide. Here, we provide the first systematic evaluation of the effect of this experimental sale on illegal ivory production. The availability of baseline PIKE data and the abrupt decision to allow a legal ivory sale to China provide an unprecedented window into global black market dynamics. Thus, our analysis informs one of the most prominent debates in conservation science while shedding light on how partial legalization can affect the illegal production of globally banned goods more generally.

Because the sale announcement was abrupt, its impact on global poaching can be analyzed using an established statistical approach common in finance, known as an "event study" [23]. If factors that affect poaching at the local, national, and global level—such as local economic conditions or national patterns of corruption—do not change abruptly at the time of the sudden announcement of the sale, then any systematic and discontinuous change in poaching that coincides with the sale announcement can be causally attributed to the sale [24]. This is analogous to how analysts estimate the "treatment effect" of a new product announcement on firm stock value [23]. In this approach, a local population just before the announcement serves as the "control," and that same population just after the announcement serves as the "treatment." Our implementation of this method flexibly accounts for all constant and trending factors at the local, national, and global level that might affect poaching (SOM Section H), allowing us to examine whether there is a treatment effect of the sale announcement on PIKE.

Results

Illegal elephant killing

Examining annual PIKE data from Africa and Asia during 2003-2013 (N=562) [25], we find an abrupt, significant, permanent, and geographically widespread increase in poaching following the announcement of China's inclusion in the sale in 2008 ($p<0.01$). Figure 1A displays the distribution of PIKE each year across all sites after site-level averages are removed from the data (SOM Section H). Upward trends at the 95th centile are unchanged through the sale, but the rest of the distribution shifts upward abruptly in 2008. Figure 1B estimates changes in mean site-level PIKE (demeaned by site) relative to 2007, the year just before the sale announcement. Average PIKE before 2008 was 0.34 and increased abruptly to 0.50 for the period after the sale, an increase of 0.16 (± 0.026, $p<0.01$). This average increase is partially attributable to a clear discontinuous increase of PIKE by 0.13 (± 0.05, $p< 0.05$) in 2008, a 38% increase above pre-sale PIKE, and an increase in the annual trend of PIKE from 0.001 (± 0.02) yr^{-1} before the sale to 0.012 (± 0.013) yr^{-1} after the sale. There is no corresponding discontinuity in natural elephant mortality (SOM Section I and Figure S11). Notably, our finding contrasts with prior analyses that study globally aggregated PIKE statistics (instead of site-level PIKE) and concluded poaching declined after the sale—an artifact of selective site reporting and outliers (SOM Section J and Figure S12), patterns to which our analysis is robust.

The estimated size and significance of the 2008 discontinuity in PIKE is robust to assumptions that there (1) is no trend, (2) is a common secular trend, (3) is a trend that breaks in 2008, (4) are common trend breaks within each country, (5) are unique trend breaks at each site (Table 1 columns 1-5, SOM Section H). Trends are visible but statistically imprecise, making it difficult to determine which of (1)-(5) are correct. Narrowing the window of analysis to ± 3 or ± 2 years of the sale, a standard approach to ensuring estimated discontinuities are distinct from trends [23], recovers unchanged estimates, although uncertainty increases because of reduced sample size (Table 1 columns 6-7).

Since sites did not record data for all 12 months during the first year of surveillance and are thus incomparable to following years, data from 2002 is omitted form our main analysis (SOM Section G); however our results are robust to including 2002 in the analysis (SOM Section Section K, Figure S13 and Table S3). Additionally, our results are robust to excluding the first year of surveillance for all sites, even those starting after 2002 (Table S3). The data are well modeled by our linearized approach (Figures S8-S10), which outperforms commonly used nonlinear approaches that require discarding 32%

of the data (SOM Section H). Our result is robust to modeling illegal killings separately from natural mortality using either least squares or Poisson regression approaches (Table S4). Furthermore, our result is also robust to the inclusion of outliers and the use of an alternative CITES data file (Figure S14 and Table S2).

The size of the estimated discontinuous increase in PIKE is similar in both Africa and Asia (Table 1 columns 8-11), although the effect is significant in Africa (N=463, $p< 0.05$) but not in Asia (N=99), perhaps because there is less data available. Focusing within Africa, we decompose the average effect across the continent into partial contributions from each country (Figure 2, SOM Section M). Notably, the average increase in PIKE is not driven by outliers but is pervasive across Sub-Saharan Africa and is individually significant in 9 separate countries. One out of 26 countries has a significant decline in poaching at the 5-10% level, an outcome likely to occur due to random sampling.

Seizures of illegal ivory

We expect illegal production of ivory (poaching) to increase the illegal trade of ivory (smuggling). Thus, to provide corroborating evidence that the legal sale of ivory affected the rate of illegal elephant killing, we use the Elephant Trade Information System's (ETIS) global data on intercepted illegal ivory shipments during 1989 to 2011 as an imperfect proxy measure for illegal trade levels [22]. In addition, we analyzed previously published data on the approximate weight of ivory seizures made from 1996 to 2011 [26] (SOM Section L). Theory predicts that consumers must be exposed to trade in legal ivory before a legal sale can elevate black market sales. Therefore we expect smuggling levels are likely to change only in 2009—when delivery of legal ivory occurred—and adjust our analysis to reflect this later event date (SOM Sections A, L).

Analyzing ivory seizures across African countries, the primary exporters of concern, we find seizure rates rose 3.4 ($\pm$1.4, $p<0.05$) seizures yr^{-1} in 2009 and total shipment quantities seized rose 317.0 ($\pm$183.6, $p<0.1$) kg yr^{-1}, almost entirely because of a rise in raw ivory—consistent with the increase in seizures being related to the increase in poaching and not just an increase in ivory smuggling activity generally. These represent increases of 75% and 110%, respectively, above baseline seizure rates of 4.8 seizures yr^{-1} (Figure S15) and 288.0 kg seized yr^{-1} (Figure S16-S17). Analysis of global seizures are in the SOM, but are complicated by a Chinese redefinition of seizures in 2009 (Figure S18).

Alternative theories

In order to infer a causal link between the legal sale and abrupt changes in elephant poaching, we make the key assumption that there is no similarly discontinuous change in other confounding variables in 2008 that may have induced the abrupt change in PIKE. We test this assumption for salient covariates by implementing event studies for each.

Many observers argue that China's increased affluence and increased influence in African range states explain the upward secular trend in illegal elephant killings over the last two decades [6, 8, 22, 27]. Our analysis above does not address these hypotheses as we do not identify the factors driving the secular trend in PIKE. Nonetheless, it is useful to consider these factors as potential confounders to our analysis. We repeat event studies with sixteen covariates that are closely linked to the commonly proposed drivers of elephant poaching over the last decade [6, 8, 22, 27], as well as to the financial crisis, to see if any of them exhibit a discontinuity in 2008 mirroring that found in PIKE (Figure 3). We find no clear evidence of a discontinuous break in 2008 in any of these covariates that resembles the structure of the PIKE response (Figure 1) and seems likely to explain it (SOM Section N).

Discussion

We find that a singular legal ivory sale corresponds with an abrupt, significant, permanent, and geographically widespread increase in the production of illegal ivory through elephant poaching. The announcement of China's inclusion in the one-time sale of legal ivory in 2008 coincides with an abrupt and statistically significant increase in the illegal killing of elephants across Africa; the delivery of this legal ivory to Japan and China in 2009 coincides with a statistically significant increase in the number and total weight of seizures of illegal ivory in Africa. The sudden 2008 increase in poaching does not correspond with any abrupt and systemic change in China's or Japan's affluence or influence in elephant range states, as measured by numerous covariates.

Our results are most consistent with the theory that the legal sale of ivory triggered an increase in black market ivory production by increasing consumer demand and/or reducing the cost of supplying black market ivory, and these effects dominated any competitive displacement that occurred (Supplementary Figure S1). As we lack price data, we cannot determine if supply or demand responses spurred poaching (see SOM); however the data indicate illegal ivory suppliers anticipate opportunities to sell and/or smuggle illegal ivory, consistent with qualitative reports [7].

Qualitative evidence from China bolsters our systemic quantitative evidence that the one-time sale led to an increase in ivory production through either changes in supplier costs or consumer demand. Consistent with concerns prior to the sale [9], the circulation of legal ivory has lowered the cost of supplying illegal ivory (Figure S1C); for example, ID cards meant to certify the legal origin of carvings are instead used to launder illegal ivory [28]: "Ivory dealers in China are selling ivory carvings but retaining their ID cards to legitimize carvings made from smuggled ivory. The cards themselves now have value and are tradable in a secondary market." Further, inability of consumers to distinguish between legal and illegal ivory was possible due to a lack of public knowledge regarding different sources of ivory; a 2007 poll reported that 70 percent of Chinese consumers believed ivory was taken harmlessly from living elephants, suggesting the public would broadly be unable to understand the significance of legal versus illegal ivory [29]. This may have allowed demand for ivory to be affected by the 2008 sale, which was noted by a Chinese newspaper report [30] to have "stimulated new consumption instead of slowing down illegal ivory trades."

Our findings demonstrate that partial legalization of a banned good can increase illegal production of the good because the existence of white markets may influence the nature of black markets. However, the exact relationship between legalization and illegal production documented here is not universal. To predict a specific market's response to legalization, we need supply and demand functions for legal and illegal versions of a good, as well as their sensitivity to legalization itself (SOM Section A). Our findings are likely to extend to markets structurally similar to ivory markets, such as those for products from other slow-growing, slow-breeding, or low-population density species like rhinoceroses and tigers. These species are difficult or impossible to produce in large quantities at low cost [31], limiting the ability of legal supplies to displace illegal supplies. In contrast, crocodiles have been reared at low cost [32], allowing legal pelts to competitively displace illegal supply.

The insight that legalization does not always reduce black market activity may extend beyond the wildlife trade. For example, populations often consider relaxing bans on trade in drugs [33], prostitution [5], weapons [34], and human organs [35] with a focus on the potential competitive displacement of illegal production. Our results demonstrate that supply and/or demand in these black markets may respond directly and dynamically to partial legalization. Thus the total effect of partial legalization on illegal production is determined by whether legal production will displace more production of illegal goods than it induces.

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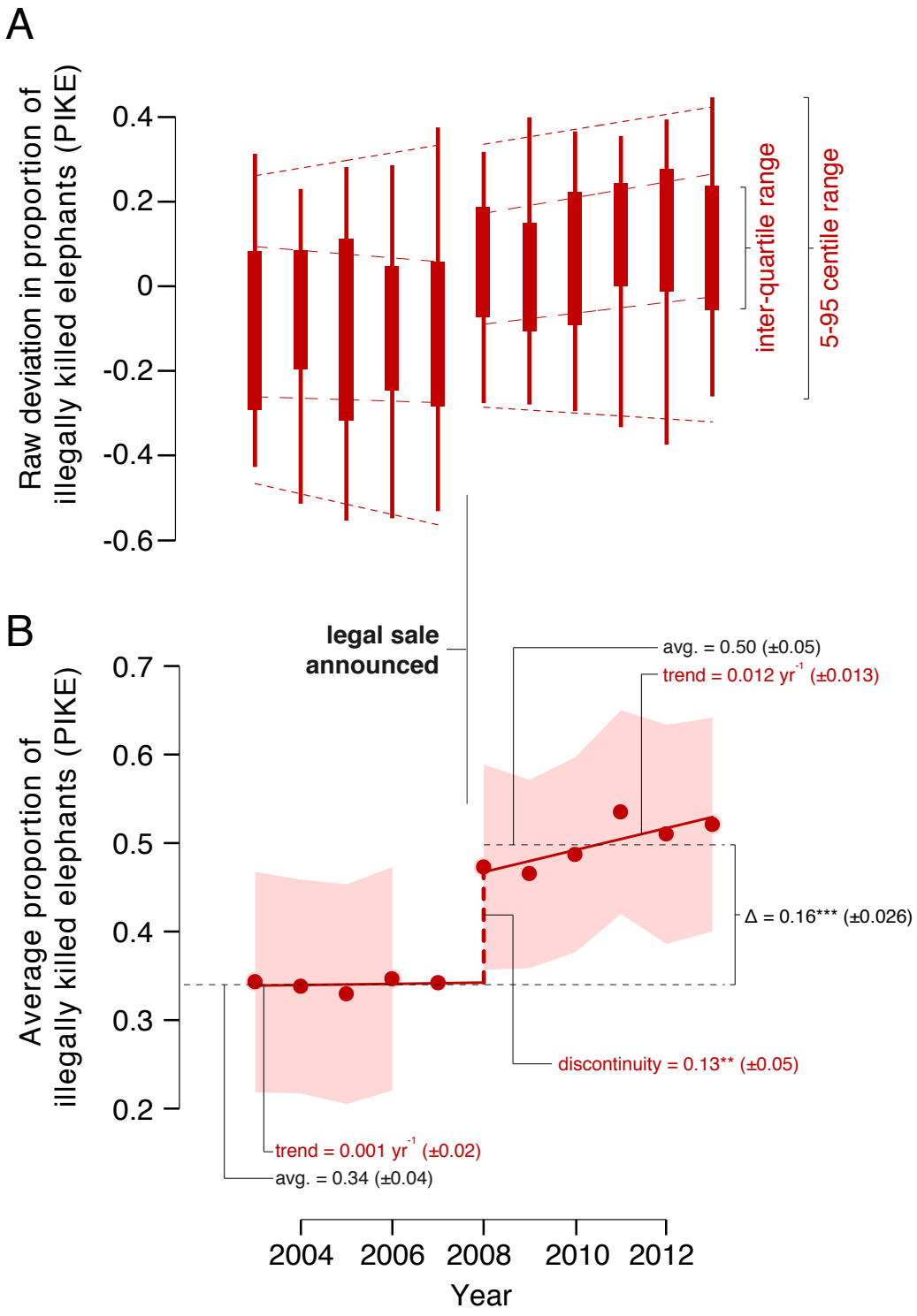


Figure 1: **Effect of legal ivory sale on PIKE (N=562).** (A) Raw annual distribution of dead elephants found to have been illegally killed (PIKE) after site-level averages are removed from the data. Sample includes all reporting countries. Boxes are inter-quartile ranges and whiskers contain the inner 90% of observations. Long-dash lines are trend in 25th and 75 quantiles before and after the 2008 sale announcement. Short-dash lines are same, but 5th and 95th quantiles. (B) Annual average site-level PIKE, adjusted for all constant site-specific differences (fixed effects, as in B), estimated relative to average pre-sale PIKE in 2007 (i.e., 2007 is the reference category). 95% confidence intervals (clustered by country) for pair-wise comparisons against average PIKE in 2007, immediately before the legal sale was announced in 2008. Dashed black lines mark average values before and after sale. Solid red lines mark trends in PIKE before and after sale. Dashed red line is the estimated discontinuity in PIKE contemporaneous with the sale announcement. All estimates report standard errors in parentheses.

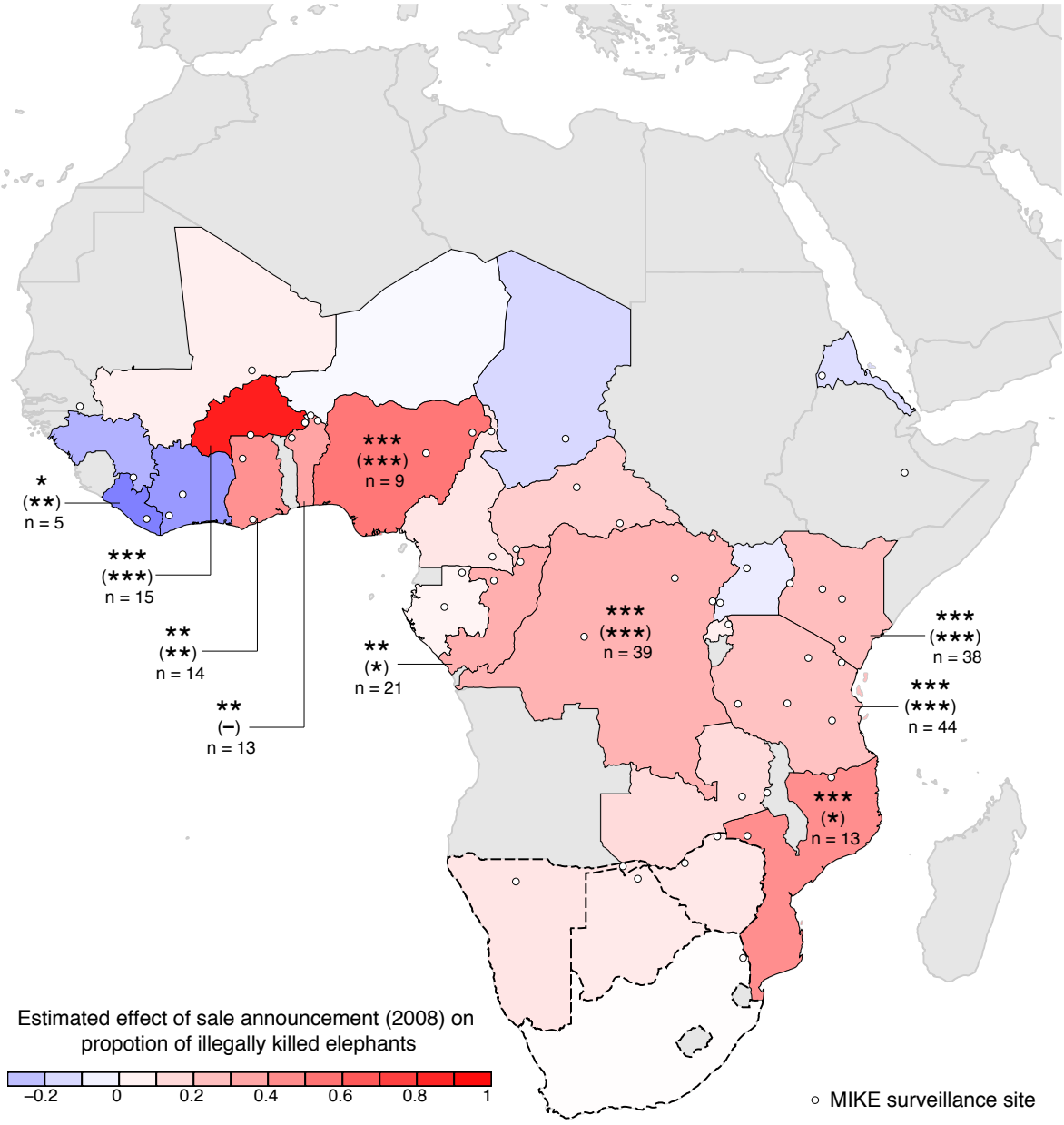


Figure 2: **Estimated change in elephant poaching across African range states following the announcement of the legal ivory sale.** The contribution of individual countries to the discontinuity shown in Figure 1B, with color indicating the average change in PIKE after the 2008 announcement across sites in each country (see SOM). Stars indicate that the estimated effect in a single country is significant (* $p<0.1$, ** $p<0.05$, *** $p<0.01$) under standard assumptions, stars in parentheses indicate whether estimates are significant using bootstrapped s.e. ('-' indicates s.e. are inestimable). MIKE surveillance sites marked with circles. Countries with insufficient data are grey. Countries supplying legal ivory for the 2008 sale are outlined with dashed lines.

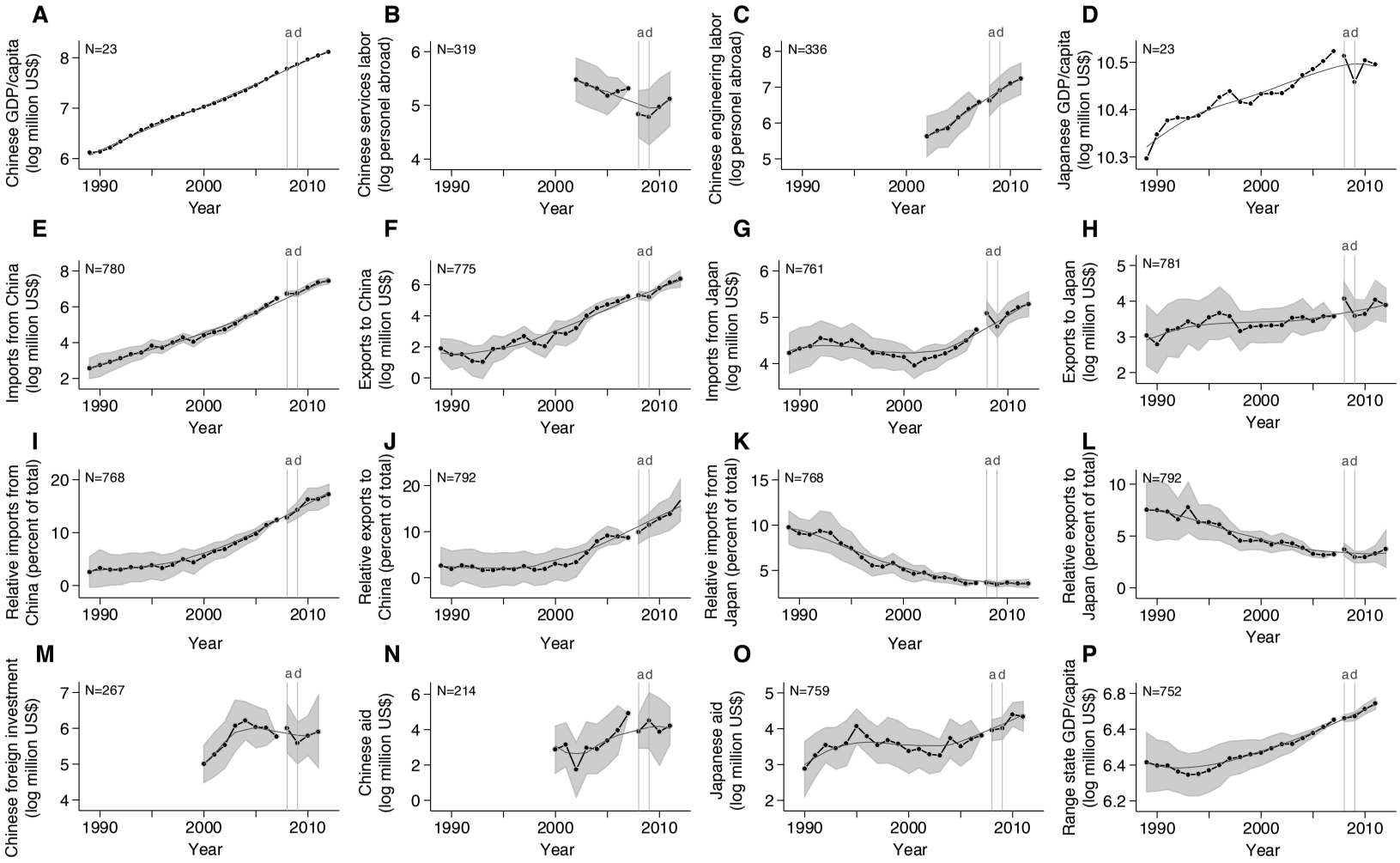


Figure 3: **Examining alternative hypotheses for the increase in illegal elephant killing.** Same as Figure 1B, except outcome data describe country-by-year level factors that might contribute to the abrupt 2008 increase in PIKE. "a" denotes the date of the sale announcement, "d" denotes the date that legal ivory was delivered. Thin light-grey line is a local nonlinear regression. (A) Chinese Gross Domestic Product (GDP) per capita. (B) Number of Chinese labor services personnel in range countries. (C) Same as B but engineering personnel. (D) Japanese GDP per capita. (E) Range state imports from China. (F) Range state exports to China. (G) Range state imports from Japan. (H) Range state exports to Japan. (I) Percentage of total imports to range states coming from China. (J) Percentage of exports from range states going to China. (K) Same as I, but Japan. (L) Same as J, but Japan. (M) Chinese foreign investment in range states. (N) Chinese foreign aid to range states. (O) Japanese foreign aid to range states. (P) GDP per capita in range states.

Table 1: **Estimated effect of legal ivory sale announcement on PIKE**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Countries in sample:	all	all	all	all	all	all	all	Africa	Africa	Asia	Asia
Years in sample:	2003-13	2003-13	2003-13	2003-13	2003-13	2004-11	2005-10	2003-13	2003-13	2003-13	2003-13
Effect of sale	0.159***	0.115**	0.129**	0.130**	0.143**	0.113	0.148	0.137**	0.139**	0.129	0.095
	(0.037)	(0.051)	(0.054)	(0.053)	(0.057)	(0.076)	(0.117)	(0.062)	(0.061)	(0.125)	(0.091)
Pre-2008 trend			0.001			-0.001	-0.035	0.005		-0.037	
			(0.016)			(0.039)	(0.074)	(0.018)		(0.057)	
Change in trend at 2008			0.011			0.018	0.048	0.004		0.062	
			(0.023)			(0.052)	(0.086)	(0.026)		(0.068)	
common linear trends		Y									
common trend break			Y			Y	Y	Y		Y	
country-specific trend breaks				Y					Y		Y
site-specific trend trends					Y						
Observations	562	562	562	562	562	301	206	463	463	99	99
R-squared	0.539	0.540	0.540	0.679	0.764	0.648	0.716	0.502	0.665	0.567	0.619

All models remove site-specific means by including site fixed effects. Standard errors clustered by country in parentheses: *** p<0.01, ** p<0.05, * p<0.1. "Effect of sale" is the estimated magnitude of the discontinuity at 2008. "Change in trend at 2008" is relative to the "Pre-2008 trend." Average trends not reported for models with country-specific or site-specific trend breaks.

Supplementary Online Materials

This Supplementary Materials Appendix contains eleven sections, with additional treatment of the following topics:

A Economic theory
B Timing of decisions related to the sale
C Chinese and Japanese contexts
D Data sources
E Discrepancies between two official versions of PIKE data
G 2002 data quality
F Outliers
H Methods
I Legal elephant mortality
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L Seizures
M Estimating heterogenous effects across countries
N Discontinuity findings in covariates

A Economic theory

Here we provide a model to make clear the precise parameters needed to predict a black market response to partial legalization. Supplementary Figure S1 presents a simple graphical model for black market dynamics following partial legalization.

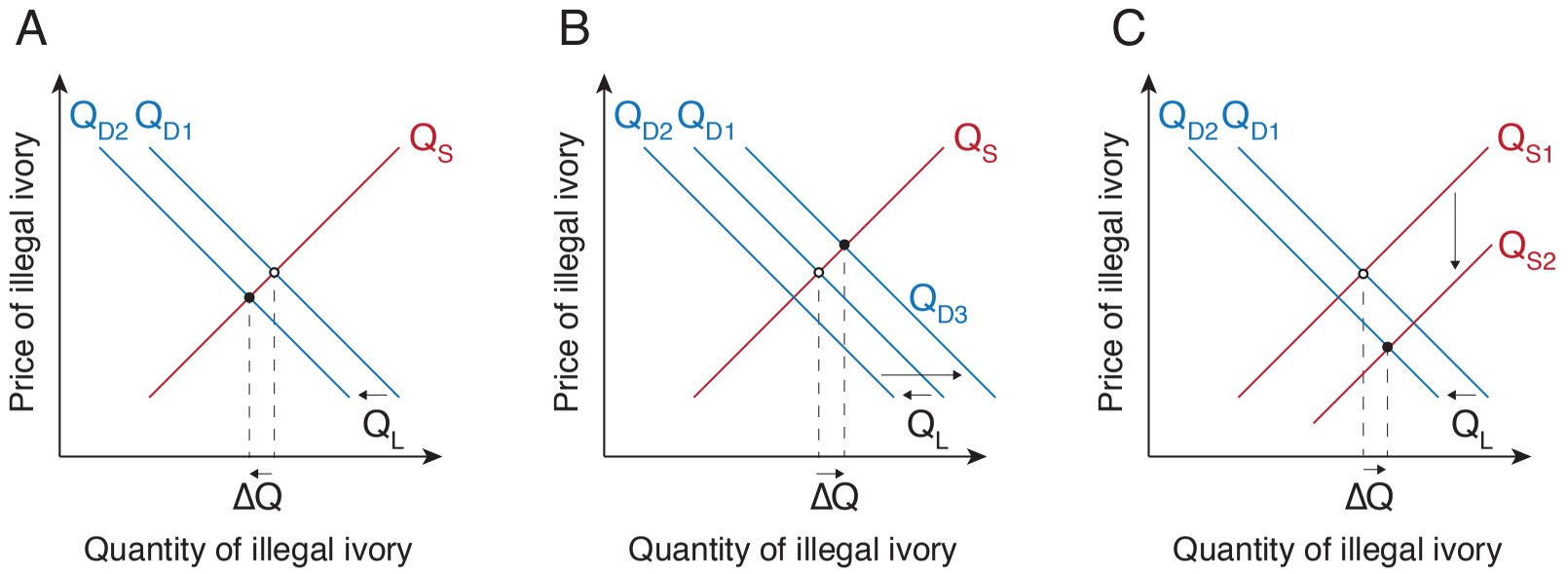
In a simple model where supply and demand do not directly respond to legalization, we must only consider

- Q_S = quantity supplied in black market
- Q_D = quantity demanded in black market
- P = black market price
- Q_L = quantity of legal supply sold by government in a white market

then the change in illegal ivory supplied to the black market is

$$\Delta Q = \left(\frac{\partial Q_S}{\partial P} - \frac{\partial Q_D}{\partial P} \right)^{-1} \left(-Q_L \frac{\partial Q_S}{\partial P} \right) \tag{1}$$

which we obtain by solving for the horizontal displacement in the equilibrium quantity in Figure S1A. So long as the supply curve is upward sloping and the demand curve is downward sloping, then ΔQ will be negative and the legal supply will competitively displace the illegal supply. The magnitude of



Supplementary Figure S1: **Potential responses of the ivory black market to a legal ivory sale.** Stylized supply (red) and demand (blue) functions for illegal ivory determine the quantity of illegal ivory (Q) and its price in the black market. The initial equilibrium is marked by the open circle. Then a legal sale of Q_L units of ivory occurs. The final equilibrium is marked by the closed circle. The net change in sales of illegal ivory caused by the legal sale is ΔQ (solved for in SOM). (A) Traditional theory suggests supply and demand do not respond to legalization directly; only that demand for illegal ivory shifts down by Q_L from Q_{D1} to Q_{D2}, displacing illegal ivory production by ΔQ. (B) If legalization directly increases demand, for example because some consumers mistakenly believe all ivory is suddenly *de facto* legal, then the demand function may shift to a new level Q_{D3} that could exceed initial demand, even though some demand is initially reduced as in A because it is met by legal supply. (C) If legalization lowers the cost of supplying illegal ivory, for example because smuggling may become easier, then the supply function will shift from Q_{S1} to Q_{S2} towards lower prices for a fixed quantity supplied. The legal sale continues to reduce demand as in A, however the net effect ΔQ of the policy on illegal ivory production may be positive if supply costs fall sufficiently. Demand responses (B) and supply responses (C) to legalization may happen simultaneously but are drawn separately for clarity.

displacement is less than or equal to the magnitude of the quantity sold legally

$$|\Delta Q| \leq Q_L$$

with the value for ΔQ depending on the slope of the supply and demand functions. If supply is inelastic, then $\frac{\partial Q_S}{\partial P} \approx 0$ and $\Delta Q \approx 0$ regardless of the quantity of legal supply sold by the government. If supply is perfectly elastic, then $\frac{\partial Q_S}{\partial P} \to \infty$ and $\Delta Q \to -Q_L$.

We propose that a more complete model should account for changes in supply and demand functions directly in response to partial legalization. Thus we assume a measure that describes the extent of legalization at a moment in time and supply and demand respond directly to changes in this variable:

- L = measure of (perceived) legality, $L = f(Q_L) \in [0, 1]$
- $\frac{\partial Q_S}{\partial L} > 0$, legalization makes smuggling and evasion easier
- $\frac{\partial Q_D}{\partial L} > 0$, bans reduce demand via stigma or fear of penalty

Combining these new assumptions, we can solve for total change in illegal supply exchanged in the black market induced by the legal sale. The three responses depicted in Supplementary Figure S1 all occur simultaneously, superimposed on one another in the black market. The net effect of these three responses on the quantity of black market sales is

$$\Delta Q = \left(\frac{\partial Q_S}{\partial P} - \frac{\partial Q_D}{\partial P}\right)^{-1} \Big(\underbrace{-Q_L \frac{\partial Q_S}{\partial P}}_{\substack{\text{Displacement} \\ \text{effect} \\ (-)}} + \underbrace{\frac{\partial Q_D}{\partial L}\frac{\partial Q_S}{\partial P}}_{\substack{\text{Demand} \\ \text{effect} \\ (+)}} - \underbrace{\frac{\partial Q_S}{\partial L}\frac{\partial Q_D}{\partial P}}_{\substack{\text{Supply} \\ \text{effect} \\ (+)}} \Big). \tag{2}$$

Because the "Demand effect" and the "Supply effect" are both positive, it is theoretically possible that the total change in black market sales ΔQ is positive if these terms together are larger than the "displacement effect". Identifying the sign and magnitude of ΔQ in the ivory black market is the contribution of the main text. Because we find that $\Delta Q > 0$, we conclude that the supply and demand effects are substantial and that focusing only on displacement effects (Equation 1) will lead to inaccurate predictions.

We stress in the main text that only this conceptual finding, i.e. supply and demand effects are important, is likely generalizable to black markets other than ivory. In order to make quantitative predictions regarding a non-ivory black market (or ivory black markets under other conditions), one would need measurements of all the values in Equation 2 to make predictions. Because these values will differ between markets, it is inaccurate to assume that the $\Delta Q = 30\%$ for ivory production that we estimate will be observable in other markets. Furthermore, if supply and/or demand in the ivory market respond nonlinearly to the perceived legality parameter L, then ΔQ may be very different for policies that differ in the amount of ivory legalized.

B Timing of decisions related to the sale

Our assessment of how the one-time sale affected elephant poaching hinges on when the black market became confident that stockpiled ivory from elephant range states would be sold to China. We believe illegal ivory traders were most likely uncertain about whether China would be included in the one-time sale until the 2008 announcement. The brief timeline in Supplementary Table S1 shows events relevant to the sale.

The timeline has two notable implications. First, the 2008 approval of China as a trading partner was abrupt and unexpected; many non-profit organizations were fighting hard to prevent further sales of ivory, especially to China [10, 9], and CITES even rejected China's approval in 2007. Secondly, the Chinese government was making some efforts at enforcement before the sale [17, 38], suggesting there was a regulatory system with which illegal ivory traders had to contend. The conditions were such that the 2008 approval and sale of legal ivory to China may have provided illegal ivory suppliers an abrupt opportunity to sell to markets to which they had limited access. Since the ivory market has reacted quickly to CITES decisions in the past [7], it is reasonable to expect the black market to respond quickly to China's inclusion in the 2008 sale.

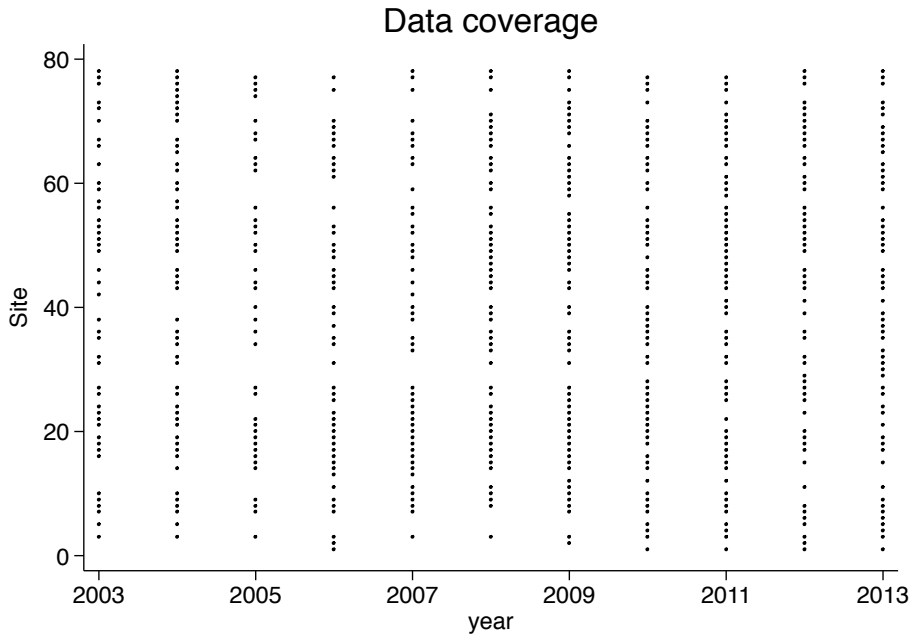
The timeline also demonstrates how our results are inconsistent with two additional hypotheses that have been previously proposed to explain the recent rise in elephant poaching. The first suggests that the Chinese government set an artificially high price for resale of its purchased ivory in its domestic market [28, 43], inducing additional poaching activity [28]. However, China's high domestic price was unexpectedly revealed in 2009 [28], after the abrupt increase in PIKE occurred in 2008. A second hypothesis argues that CITES' refusal to lift the moratorium on future legal ivory sales was responsible for the increase in poaching [42, 22]. Specifically, observers have suggested that the combination of (i) a 2007 CITES commitment to disallow future sales of ivory for nine years (from the time of the upcoming sale) from Botswana, Namibia, Zimbabwe, and South Africa and (ii) CITES' 2010 denial of a legal ivory sale by Tanzania and Zambia signalled to markets that legal ivory sales were unlikely in the near future. The timings of these refusals do not match the observed 2008 change in PIKE.

C Chinese and Japanese contexts

Because the legal sale of ivory to China and Japan happened simultaneously, we cannot easily distinguish between their effects. Nonetheless, there are numerous accounts beyond the scope of this analysis that suggest the legal ivory sales to China and Japan did not have identical effects on global black markets. The Japanese market may not have presented the same opportunities as the Chinese market to illegal traders. Japan is a much smaller country than China; the Japanese demand for ivory was mostly limited to its use for making hankos, or signature stamps [28]; and the Japanese market for ivory was already in decline [22]. For these reasons and other reasons, it is widely thought that the Chinese market and not the Japanese market is the primary driver of elephant poaching [44, 14, 45, 42, 28, 43]. In addition, while environmental advocates were not satisfied with Japanese controls on illegal ivory, it appears that there was agreement that Japanese regulatory efforts had progressed farther than Chinese efforts [9, 46], and perhaps presented less opportunity to launder illegal ivory after a legal sale.

Supplementary Table S1: A timeline of events related to the 2008 one-time sale

Year	Events
1989	The Convention on International Trade in Endangered Species (CITES) bans international trade in African elephant ivory (international trade in Asian elephant ivory already banned) [7].
1999	One-time sale of ivory from Botswana, Namibia, and Zimbabwe to Japan occurs [36].
2002	CITES approves ivory stocks held by Botswana, South Africa, and Namibia for another one-time sale. However, the sale is not to occur "before May 2004", and is contingent on the verification of "prospective importing countries." At this time, Japan is the only country to have ever been approved as a trading partner for ivory since the 1990 ban [37].
2004	China establishes an ivory registration and licensing system and other mechanisms to meet CITES requirements, hoping to be declared a trading partner for ivory [9].
2005	A CITES technical team visits China and declares the systems in place for registration and licensing to be "demonstrably effective" [38]. However, the CITES Standing Committee declares that China's status as a "destination for illicit trade in ivory" makes it inappropriate to declare China a trading partner [38].
2006	The CITES Standing Committee re-approves Japan as a trading partner, but also requests that any reasons for reviewing this decision be presented the following year [39]. The committee decides not to make any decision about China's status as a trading partner for the time being [39].
2007	CITES approves Zimbabwe's ivory stocks for a (thus unscheduled) upcoming one-time sale [40]. CITES confirms that Japan shall remain a trading partner for the one-time sale [41]. In a six to six vote, the CITES Standing Committee rejects China's request to be made a trading partner, declaring the topic will be further discussed in the next year [19]. CITES declares a nine-year moratorium on further sales of legal ivory from the four countries participating in current one-time sale; the moratorium is to begin the year of the impending sale [40].
2008	July: The CITES Standing Committee verifies ivory from Botswana, Namibia, South Africa, and Zimbabwe were of legal origin and are ready for sale [17]. July: The committee also approves China as a trading partner that may participate in the sale [17]. October/November: Ivory from African range states is auctioned off to China and Japan [21].
2009	Export of ivory from southern Africa occurs in February and March [21]. Final inspections of Chinese and Japanese shipments occur in March and April, respectively [21]. China announces domestic ivory price.[28]
2010	CITES rejects Tanzania's and Zambia's requests to allow one-time sales of their stockpiles of ivory [42].



Supplementary Figure S2: **Data coverage in sample.** Each point represents a unique observation in the sample for each MIKE site and year. MIKE sites are indexed with a unique identification number.

D Data Sources

The proportion illegally killed elephants (PIKE) is the standard index used by the Monitoring the Illegal Killing of Elephants (MIKE) program to track the severity of poaching in selected sites across the world [13, 14]. We use PIKE as our annual measure of poaching from 2003 until 2013, for which PIKE data is public [25]. PIKE data coverage is displayed in Figure S2. Observations from 2002 and three extreme outliers are dropped for reasons detailed below.

PIKE includes data from African savanna elephant (*Loxodonta africana*), African forest elephant (*Loxodonta africana cyclotis*), and Asian elephant (*Elephas maximus*) range countries. Since Asian elephants are also a Schedule I species and may be affected by the black market in African ivory, we have included Asian elephant range states in our analyses wherever data are available. We requested monthly PIKE data multiple times, which would have allowed us to better determine whether the increase in PIKE was due to the one-time sale, but were not permitted access to the data because it is not made publicly available by the elephant range states.

ETIS provides the world's largest collection of ivory seizure records [22], with data as far back as 1989. We used data on the number of seizures made globally that was available from June 26, 2012, when analysis for the 16th CITES Conference of Parties was conducted (Milliken, personal communication). In addition, we used data on the weights of seizures made globally from 1996 to 2011 [26]. In the latter case, data were made available in two forms: i) the year and exact weight (but not the country) of each seizure made between 1996 and 2011, and ii) the number of small (less than 10

kg), medium (between 10 kg and 100 kg), and large (100 kg or greater) seizures made in each country in each year. Using data from the former, we calculated the average small, medium, and large seizure made each year; we then multiplied these averages by the number of seizures made in each size class in each country each year and summed these approximations to estimate the total weight of ivory seizures made in each year in each country. Increases in seizures are thought to mean a corresponding increase in successfully smuggled ivory [47, 28, 48].

We drew our data on potential covariates measuring Chinese influence and presence in elephant range states from a variety of sources. We used GDP data from the World Bank's World DataBank [49]. For data on exports from and imports to elephant range states, we use the International Monetary Fund's Direction of Trade Statistics, taken from IHS Global Insight Web [50]. We used estimates of Chinese ODA-like (Overseas Development Assistance) and OOF-like (Other Official Flows) investments in African range states from AidData.org [51, 52]. For Japan's aid to elephant range states, we used total official flows (sum of ODA and OOF) from the OECD website [53]. Estimates of Chinese foreign direct investment were taken from the "actually used foreign direct and other investment"sections in Chinese Statistical Yearbooks 2002-2012 [54, 55]. We obtain the number of Chinese personnel in each elephant range state at year's end for labor services and engineering projects from the China Trade and External Economic Statistical Yearbooks 2006 [56], 2009 [57], 2012 [58] as a measure of the physical presence of Chinese citizens in countries reporting PIKE. We have no reason to believe any potential bias from this data, whether from press reports [51, 52] or China's self-reported statistics [54, 55, 56, 57, 58], would affect our ability to find patterns in these data relevant to our study.

E Discrepancies between two official versions of PIKE data

CITES made an Excel file of the official PIKE data (for 2002-2011) available through a Github site (https://Github.com/citesmike/mike_R_analyses/blob/master/pike_sc62.csv); this data was originally accessed May 7, 2013 and appears unchanged as recently as April 25, 2015. This file had the advantage of including raw data—that is, the number of total carcasses and number of illegal (poached) carcasses in each site. In July 2014, CITES published SC65 Inf. 1 [59], which, combined with previously published CITES documents [60], included all PIKE data from 2002-2013. This provided data we did not have previously: PIKE values for 2012-2013, as well as PIKE values from 2004-2011 which had only recently been reported to CITES (Julian Blanc, personal communication July 8, 2014). In addition, however, SC65 Inf. 1 also had several data values (for 18 site-years in total) that contradicted those from the Github file, listed in Table S2. Seventeen of these 18 discrepancies occurred at Asian MIKE sites. Despite repeated requests (July 8, 2014; July 21, 2014; August 18, 2014), CITES has not responded to explain those discrepancies or tell us which values are correct. CITES has also not (to our knowledge) made public an updated file of the raw data including the 2012-2013 data. We therefore use the latest data—those published in SC 65 Inf. 1—as the basis of our main analysis. Since the data provided in SC 65 Inf. 1 were presented as the PIKE value rounded to two decimal places and the total number of carcasses (and did not include the number of illegal carcasses), we inferred the number of illegal carcasses (where the Github file did not provide a value). For ten of the values in our final data set (where total carcasses were greater than 100), this required us to choose between

Supplementary Table S2: **Discrepancies between two official versions of PIKE data**

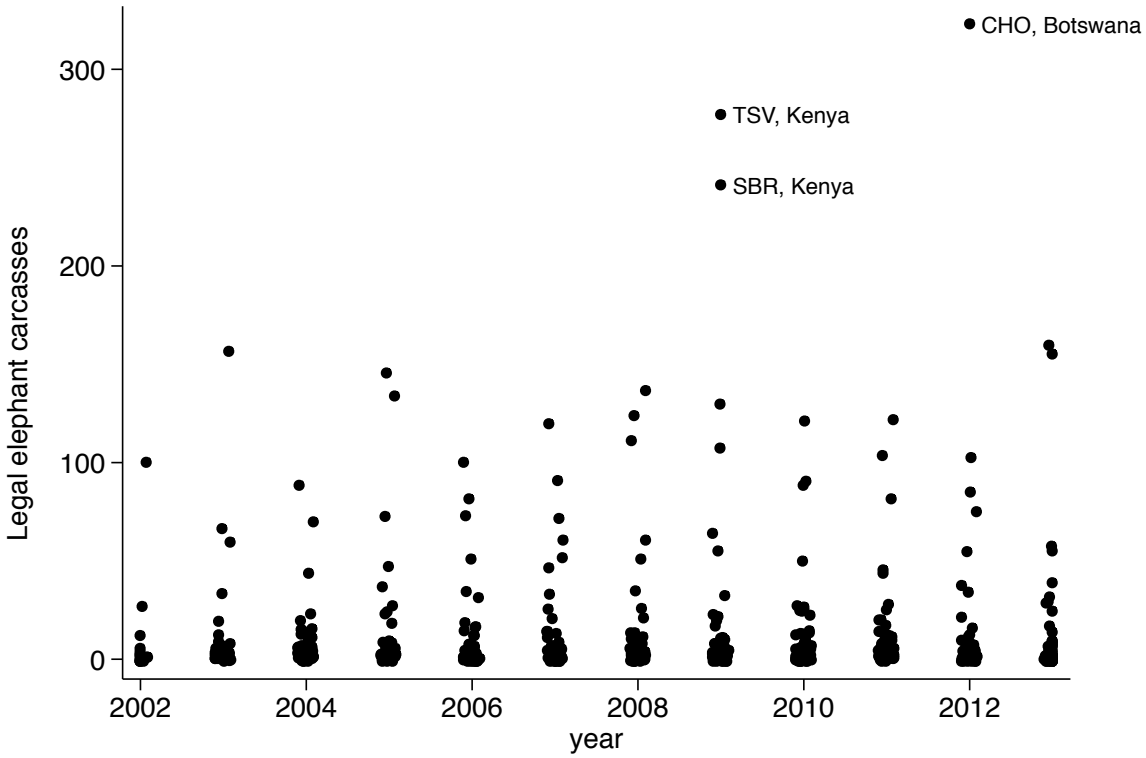
			SC65 Inf. 1 Data†		Github Data*	
Country	Site	Year	Total Carcasses	Illegal Carcasses	Total Carcasses	Illegal Carcasses
India	Chirang Ripu	2008	4	0	8	0
India	Chirang Ripu	2009	7	4	5	0
India	Deomali	2006	3	3	2	0
India	Dihing Patkai	2006	6	0	1	0
India	Dihing Patkai	2008	3	1	5	1
India	Dihing Patkai	2009	2	0	3	0
India	Eastern Dooars	2007	20	0	15	0
India	Eastern Dooars	2008	15	0	15	1
India	Eastern Dooars	2009	12	0	2	0
India	Garo Hills	2006	6	3	4	0
India	Garo Hills	2008	9	2	6	1
India	Garo Hills	2009	4	3	8	3
Malaysia	Kluang	2007	0	0	1	0
Malaysia	Kluang	2009	1	1	2	1
India	Mayurbhanj	2006	17	3	1	0
India	Mysore	2006	57	24	3	1
China	Xishuangbanna	2006	2	1	1	0
Nigeria	Yankari	2011	4	4	1	1

†Ref. [59]. *Accessed April 25, 2015.

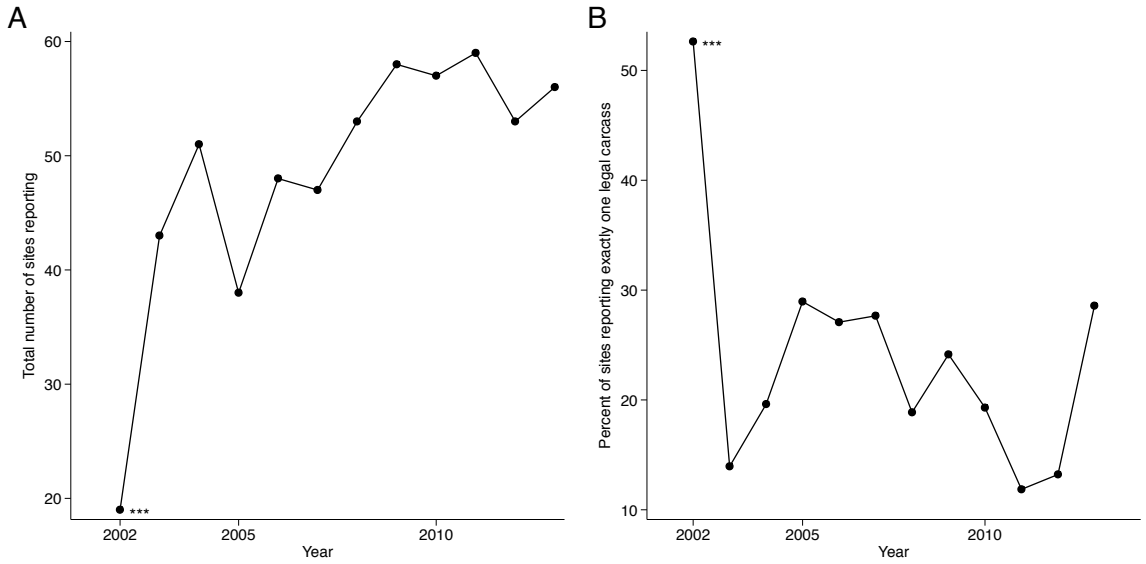
two plausible values for the number of illegal carcasses (either of which would have resulted in the same reported rounded PIKE value in SC Inf. 1). In these cases, we estimated the number of illegal carcasses to be that which would result in a PIKE value closest to the decimal reported in SC 65 Inf. 1. In addition, since CITES has not disclosed the reason for the conflict between the Github data and SC65 Inf. 1 data, it is plausible that the Github data are correct. We therefore re-run our analyses with the Github values wherever there was a conflict, and show in Section K that there is no change in our results using this alternative data source.

F Outliers

We drop three extreme and influential outliers (see Figure S3). In 2009, a drought in Kenya caused legal mortality at two sites (Tsavo and Samburu-Laikipia) to exceed 200 at each site. These represent 9.6σ and 11.1σ outliers. In 2012, one site in Botswana (Chobe) exhibited legal mortality above 300 (cause unknown), a 12.9σ outlier. These three observations represent 0.5% of our sample (after 2002 is dropped) but exert substantial influence on our estimates even though they provide little additional



Supplementary Figure S3: **Counts of legal elephant kills in range states (jittered).** Legal elephant kills in range states are used to normalize counts of illegally killed elephants in the PIKE measure, to account for the effort of surveyors and the changing likelihood of discovering any elephant carcass. In 2009, just following the delivery of legal ivory to China and Japan, two sites in Kenya reported discovering very high counts of legal elephant deaths (9.6σ and 11.1σ outliers) due to drought [13]. In 2012, one site in Botswana reported even higher legal mortality (12.9σ outlier). These three sites strongly influence aggregate PIKE statistics (Supplementary Figure S12) and have moderate influence on our baseline regression model. These three outliers are dropped from the sample in our main estimation.



Supplementary Figure S4: **PIKE data collection irregularities in 2002.** (A) The total number of MIKE sites reporting data in each year. The low number of reporting sites (19) in the first year of the program is unlikely to be explained by normal variability in data collection ($p < 0.01$). Moreover, twelve of the nineteen sites reported not collecting a complete year of data in the first year of operation [61]. (B) The percent of reporting sites that reported finding exactly one legal carcass in each year. The high fraction of sites (53%) reporting exactly one legal carcass and no illegal carcasses is unlikely to be explained by normal variability ($p < 0.01$).

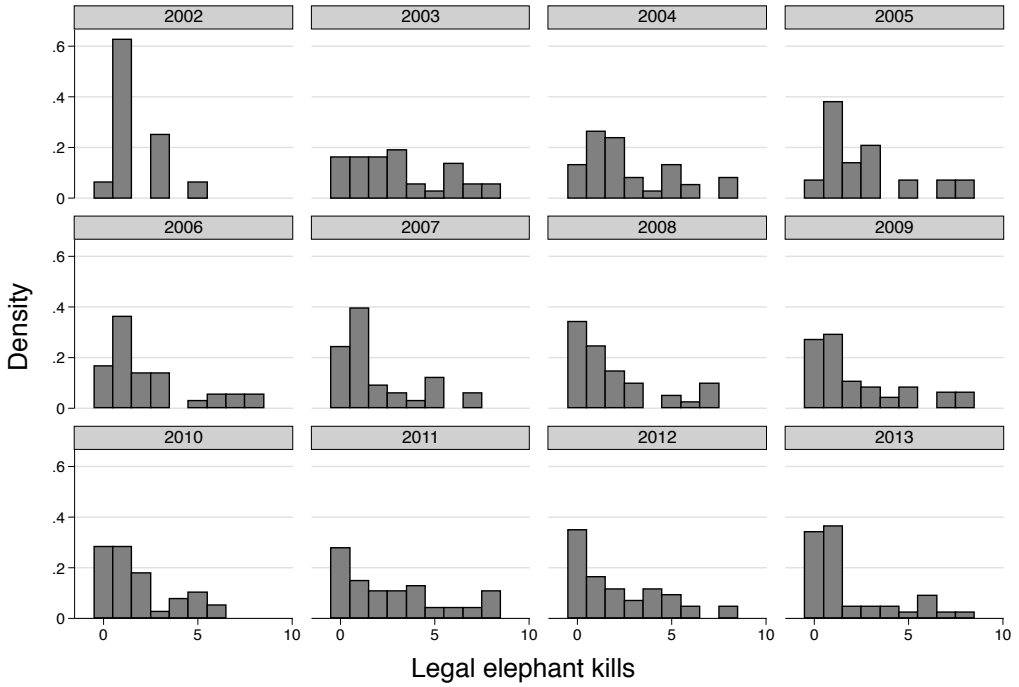
information on black market behavior, so they are dropped from our sample.

In Appendix J, we demonstrate that failing to drop these observations provides a highly misleading picture aggregate PIKE responses using a fully aggregated measure of PIKE that has been used by some prior analyses. Our approach is much less sensitive to these outliers, but they are removed from the sample nonetheless as they provide little additional information relative to the potential distortion they introduce.

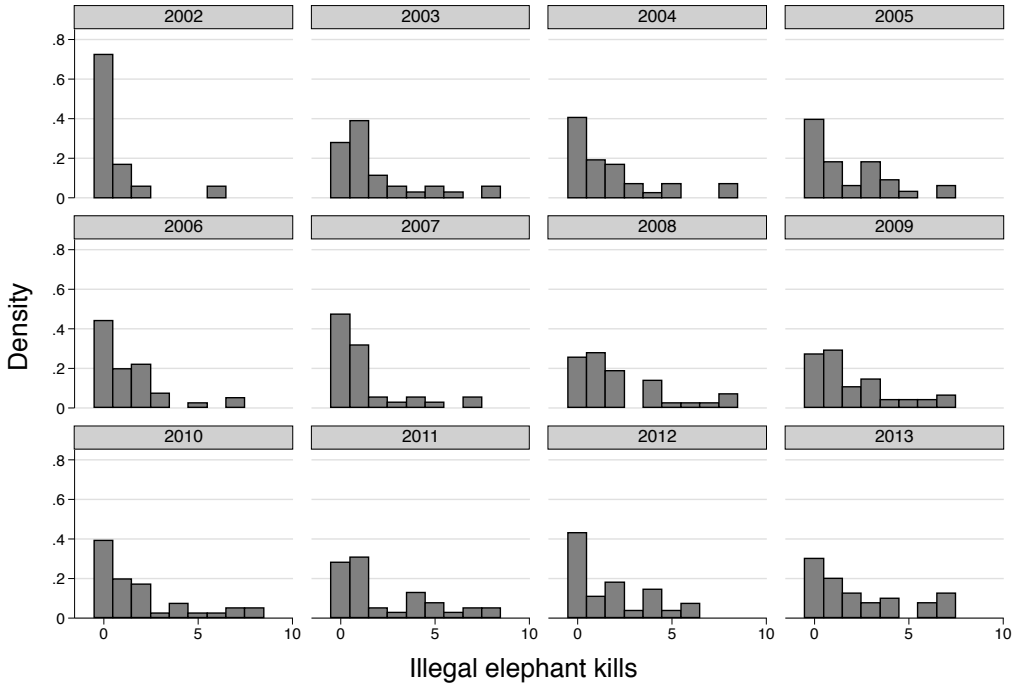
G 2002 data quality

Widespread surveillance of illegal elephant killing began with the deployment of the MIKE surveillance system in 2002. Because the system was being brought online during 2002, annual observations recorded for the year 2002 exhibit numerous properties that make them qualitatively different and statistically incomparable to observations in later years. To our knowledge, the reliability of annualized 2002 PIKE data has not been previously discussed or documented, so we do both here. Ultimately, because of concerns over both reliability and the failure of sites to sample a complete and comparable 12 month interval during 2002 leads us to drop 2002 observations from our main analysis. However, including 2002 in our analysis does not change our central conclusions, as demonstrated in Appendix K.

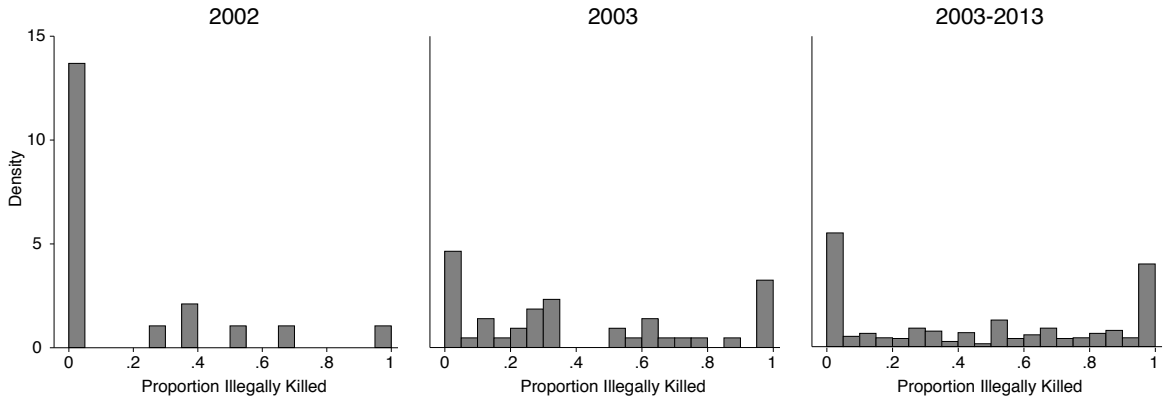
Issues arising from the 2002 PIKE data include:



Supplementary Figure S5: **Annual distribution of legal elephant carcass counts.** Histogram of the number of legal carcasses reported each year for sites reporting ten or fewer legal carcasses. Of sites reporting in 2002, an abnormally large fraction reported exactly one legal carcass and an unusually small number of sites reported zero.



Supplementary Figure S6: **Annual distribution of illegal elephant carcass counts.** Histogram of the number of illegal carcasses reported each year for sites reporting ten or fewer illegal carcasses. Of sites reporting in 2002, an abnormally large fraction reported zero illegal carcasses and an unusually small number of sites reported one.



Supplementary Figure S7: **Extreme and irregular distribution of PIKE in 2002.** Histogram of raw PIKE values in 2002 compared with 2003 and the average distribution across the remaining sample. The distribution of PIKE in 2002, the first year of data collection, differs dramatically from the immediately following year (2003), which exhibits a distribution similar to the overall sample (2003-2013).

- Only 19 sites report any data in 2002, shown in Figure S4A. During the remaining sample, an average of 51.1 sites reported data each year with a standard deviation of 6.6 sites reporting. The next lowest reporting year was 2005, when 43 sites reported data. The count of sites reporting in 2002 is thus a 4.9σ outlier. The low number of sites reporting is likely due to the lag required for many sites to be brought online after the start of the program.

- Of the 19 sites reporting in 2002, 12 began data collection after January. Thus, the annual carcass counts supplied by these sites do not represent a complete 12-month long total and thus cannot be compared to later years where 12-month totals are reported, as they will be systematically lower. Because elephant mortality and poaching patterns may exhibit strong seasonality that differs between regions, this introduces an unknown bias. We have requested monthly PIKE data by site to correct for this issue, but have been denied access on legal grounds (over email May 10, 2013, reiterated in phone conversation with Julian Blanc, 2013).

- Of the 19 sites reporting in 2002, 53% report exactly 1 legal carcass (Figure S4B). Across years, the average fraction of sites with exactly one legal carcass is 21% with a standard deviation of 6.4%. The next highest year is 29% of sites reporting one legal carcass. This means that 2002 is also a 4.9σ outlier in the number of sites reporting exactly one legal carcass, which is extremely unlikely to occur due to natural sampling error and variability ($p < 0.01$), even when accounting for a trend in these values. We do not know why a majority of sites report exactly one legal carcass and no illegal carcasses in the first year of the MIKE program when no other year exhibited such a pattern.

- The large fraction of sites reporting one legal carcass in 2002 seems additionally exceptional when compared to the number of sites reporting zero or two carcasses, in comparison to other years in the sample. Figure S5 displays the distribution of legal carcass counts across years. Across

other years, the number of sites reporting zero legal carcasses is generally close to the number of sites reporting one legal carcass, and moderately close to the number of sites reporting two legal carcasses. However, in 2002 there is an extremely large difference between the number of sites reporting one legal carcass and either two or zero legal carcasses.

- The distribution of illegal carcasses in 2002 seems similarly abnormal, with 13 of 19 sites reporting zero illegal carcasses and only two sites reporting one carcass. Figure S6 displays the distribution of illegal carcasses for each year. In other year, the number of sites reporting zero and one illegal carcass is always much closer than in 2002.

- The combined effect of abnormal reporting for both legal and illegal carcasses in 2002 generates a highly irregular distribution of PIKE across sites. Figure S7 compares the distribution of PIKE between 2002 and 2003, as well as the pooled sample of all other years. The large number of sites reporting zero illegal carcasses and one legal carcasses causes the mass of observations to stack at PIKE = 0. In the following year, the distribution shifts abruptly to have roughly equal mass at PIKE = 1, with roughly uniform mass between zero and one. This pattern persists for all subsequent periods, suggesting that the distribution in 2002 is an irregularity.

We do not know why the 2002 data is so qualitatively and quantitatively different from data in subsequent years when distributional patterns stabilized. Many survey programs exhibit large data irregularities in their first round, so perhaps the irregular patterns in 2002 are unsurprising. In many cases, it is observed that surveyors collect data differently or make errors during the initial rollout of a program, sometimes because training and experience accumulate with time. In other cases, there is political pressure to demonstrate that at least some survey data is collected in a first round, even if its quality may be compromised [62]. We do not know whether these or other factors contributed to observed irregularities in the first year of PIKE data. It is possible that the incomplete sampling of months in the first year of surveillance, combined with seasonality in legal and illegal mortality, explains most of the irregular pattern in 2002.

Regardless of the source of these irregularities, the data in 2002 is known to be incomparable to later years because of the uneven sampling of months [61], so we drop it from our main sample. Below we test whether our results hold if 2002 is included and find that its inclusion has limited effect. The main impact of including 2002 in the sample is that it increases our estimated sampling error (because irregular observations have large residuals) and it biases the pre-sale trend substantially.

H Methods

Sample

Obtaining raw annual PIKE data from 79 MIKE sites in 40 African and Asian countries during 2002-2013 [25]. In our main analysis we drop three outliers (Section F) and observations from 2002 (Section G). Six observations with zero total carcasses are assigned PIKE = 0 (otherwise PIKE = $\frac{0}{0}$). We drop two observations from sites that reported PIKE only one year in this remaining sample (changes over time within this site cannot be identified). This leaves us with 562 PIKE observations across 77 sites in 38 countries.

Core approach

It is widely understood that local economic conditions, geography, the presence of roads, the quality of law enforcement, and numerous other factors influence the intensity of poaching at specific sites. It is thus imperative that the influence of these factors are accounted for when examining global trends. In particular, because the sampling of sites is incomplete and often interrupted it is possible that changes in global trend estimates could be confounded by a change in the composition of sites reporting PIKE data. For example, if the representation of sites with weaker governance and higher poaching in the sample systematically increases over time, it would appear though there is a secular upward trend in PIKE if this factor is not accounted for. In principle, one could account for such a bias by "controlling" for covariates. However, it is generally impossible to explicitly to account for *all* potential factors that influence poaching, and any list of control variables in a regression model is open to the critique that it is incomplete and may have omitted key variables. This critique cannot be addressed until the omitted variable is collected and included in the model, at which point another potential omitted variable might be identified, raising concern yet again. The situation initially appears hopeless, and it appears even more hopeless when one considers that additively "controlling" for these variables in a standard regression model makes the strong assumptions that the effect of each control variable is fixed across locations–or in more sophisticated models that these effects satisfy slightly weaker distributional assumptions. Fortunately, there is an alternative approach that is simple, general, remarkably robust, and, most importantly, feasible.[63]

We conceptualize poaching levels as some unknown and potentially nonlinear function of various local, national, and global factors, some of which may be observable and others which might not be. For site i located within country j observed in year t, we could stylize this model as

$$PIKE_{ijt} = \underbrace{f_i(\ site_geography_i,\ local_econmic_conditions_i,\ national_institutions_j,\ culture_j,\ \text{etc.})}_{\text{site-specific effects}} + \underbrace{g(global_policy_t, oil_prices_t, \text{etc.})}_{\text{global trends}} + \underbrace{h(national_policies_{jt}, economic_trends_{jt}, \text{etc.})}_{\text{country trends}} + \epsilon_{ijt} \tag{3}$$

recognizing that the functions $f_i(.)$, $g(.)$, and $h(.)$ may be unknown. Furthermore, $f_i(.)$ may differ between locations, since it is possible that various factors interact in complex ways such that incremental changes in any one factor do not influence PIKE identically across different contexts. Models that explicitly "control" for various factors, such as income or governance, make assumptions about the form of these functions. If we do not make such assumptions, but instead assume that site-specific factors evolve very slowly, we can estimate $f_i(.)$ as the average PIKE observed during the period of observation in site i, known as a "site fixed effect." A site fixed effect non-parametrically captures all the site-specific factors that do not change during the period of observation but that may affect PIKE at a site [63]. Importantly, these parameters also capture any country-specific variables, such as national institutions, governance and culture. The simplest model that isolates these site-specific

factors from trending factors captured in $g(.)$ and $h(.)$ is

$$PIKE_{ijt} = \mu_i + \gamma_t + \epsilon_{ijt}. \tag{4}$$

The site-specific constants μ_i are site fixed effects that account for all constant local factors, such as baseline mortality, climate, socio-economic conditions, and levels of human-elephant conflict. These site fixed effects remove any bias in trends due to compositional changes in which sites report data in different years. The annual constants γ_t describe global changes in poaching $g(.)$ that are common to all sites. If global black markets adjust abruptly to the legal sale of ivory, this effect will be measured by a change in γ for years following the sale. ϵ is unexplained residual variation, which may include national changes $h(.)$ that are independent of global trends as well as various uncorrelated local factors that alter PIKE across years within a site.

After estimating Equation 4, we remove the site fixed effects μ_i from the data so only the demeaned data ($\gamma_t + \epsilon_{ijt}$) remains. We plot the distributions of this otherwise raw data in Figure 1A of the main text. After simply removing site-specific means, the discontinuous jump in the distribution of annual PIKE in 2008 is readily visible. Dashed lines depict trends in different centiles of the annual distributions of ϵ_{ijt}, which exhibit clear discontinuities except at the 95th centile. With only minimal data processing (i.e. demeaning the data by site), the effect of the experimental sale is apparent.

In Figure 1B of the main text we plot the estimates of γ_t, which capture all globally coherent shifts in PIKE in each year, represented by $g(.)$ in Equation 3[1]. If global policies or events cause systematic changes in PIKE around the world, this will result in a shift in the distribution of PIKE (Figure 1A) and a change in the mean of this distribution (Figure 1B).

Accounting for trends

Because some factors that influence poaching, such as the affluence of potential ivory consumers or the level of organization in poaching syndicates, might change over the interval of observation, we employ a second model that additionally allows us to control for trends in these covariates:

$$PIKE_{ijt} = \mu_i + \theta t + \beta \cdot post_event_t + \epsilon_{ijt} \tag{5}$$

where θ is a global trend and *post_event* is a dummy variable that is equal to one if the observation follows the announcement of China's inclusion in the sale (hereafter "the announcement"). Grouping observations based on whether they are before or after the sale is less flexible than the approach in Equation 4, but it is a standard approach [63] that allows us to retain statistical power when we account for various trends. The average effect of the legal sale on PIKE (i.e. the size of the discontinuity between 2007 and 2008) is described by the coefficient β listed in columns 1, 2, and 4 of Table 1 in the main text. A similar approach allowing for country or site specific linear trends (θ_j or θ_i) recovers nearly identical results.

[1] **This figure differs from similar figures in the literature that do account for site fixed effects [13].**

Accounting for trend breaks

Figure 1 indicates that in addition to the PIKE data exhibiting a trend and a discontinuity in 2008, the trends in PIKE themselves change in 2008 (except for the highest quantiles of the distribution). To model both the change in the intercept and slope of the PIKE trend, we use a "trend break" model which estimates a separate trend before and after the sale announcement in 2008. Specifically, we estimate:

$$PIKE_{ijt} = \mu_i + \theta_{(ij)}t + (\theta'_{(ij)}t + \beta) \cdot post_event_t + \epsilon_{ijt} \tag{6}$$

where θ' is the change in the trend that occurs after the sale event. Note that the total trend after the sale is $\theta + \theta'$. To increase the flexibility of our models, we also allow for both the pre-sale trend and the post-sale trend to differ either by country j or by site i. Results from estimating Equation 5 with a common trend break are shown in column 3 of Table 1. Estimates for the common pre-trend θ and the change in this common trend θ' are listed. Column 4 in Table 1 presents results from Equation 5 estimated with country-specific pre- and post-trends, although only the estimated effect β is listed because of display constraints and because it is the primary parameter of interest. Column 5 presents results with site-specific pre- and post trends, which, despite its high flexibility, still recovers significant results that mirror our main finding. Columns 6-11 in Table 1 also display results from these two models, applied to subsamples of the data.

Interpretation

The results in Figure 1 and regression results presented below describe outcomes from a global experiment, which are analyzed and interpreted in the framework of an "event study." Event study research designs are a standard econometric approach[23], used most commonly in finance, to measure the effect of an exogenous event on some dependent variable. Event studies are usually considered "quasi-experiments" because the analyst is usually incapable of experimentally manipulating the timing of the treatment, although in this case the timing of the legal sale was determined by CITES and originally portrayed as an actual experiment.

Event studies identify the causal effect of events on outcomes by measuring whether an outcome variable exhibits discontinuous behavior at the moment of treatment. So long as potentially confounding variables are continuous and "smooth" throughout the event, conditional on covariates, then any discontinuous behavior in the outcome at the time of the treatment can be attributed to the treatment event [23, 24]. For example, in standard applications analysts examine stock prices over time and evaluate whether abnormal returns jump discontinuously at the moment that a firm announces new information about profits. So long as other factors that affect stock returns do not also change discontinuously at the time of the announcement, then we can infer that the discontinuity observed at the moment of the announcement is attributable to the announcement. Potential confounding factors may be trending upward or downward at the time of the announcement, however so long as these trends are "smooth" then their effects on the outcome can be removed by de-trending the data (as in Equation 5-6). Moreover, such trends will not affect the estimated size of the discontinuity so long as they are appropriately accounted for with a sufficiently flexible trend structure. However, if confounders also change abruptly at the moment of treatment this may confound causal inference. In the example of

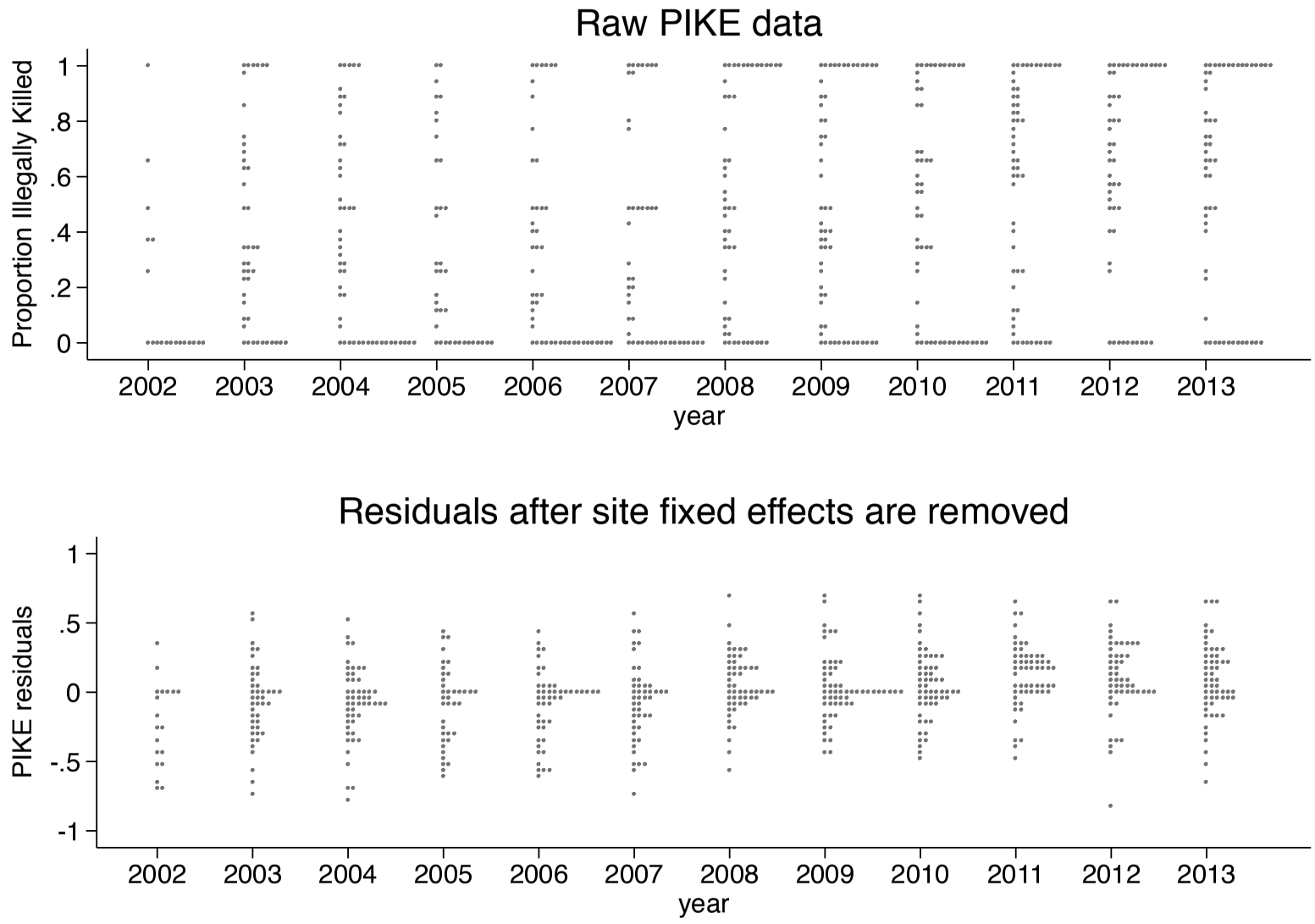
stock returns, if government regulation is announced at the same time that a firm announces information on profits, it will not be possible to determine whether an abrupt change in stock prices are due to the government announcement or the firm announcement. Because this assumption is critical for inference, we explicitly test it for a variety of plausibly important covariates in Section N. We do not find clear evidence of discontinuities among a variety of relevant covariates, although it is of course impossible to test all potential confounders around the globe. Importantly, any confounding event that would bias our estimates must (i) occur abruptly in 2008, (ii) affect PIKE around the globe in a coherent and immediate way that persists through 2013, (iii) not occur in other years between 2003-2013. We do not observe any obvious confounders related to the CITES timeline (Figure S1) or other global events, although it is always possible that future work may uncover an important potential confounder.

Checking assumptions of a linearized approach

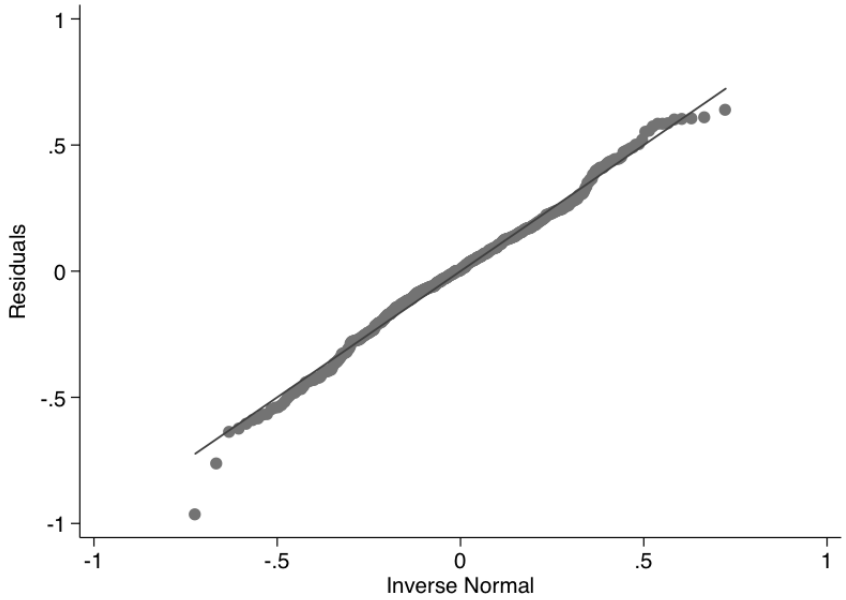
In the context of Equation 3, there are various approaches for ensuring that our estimates for the size of the discontinuity are not biased by different types of trends at the global, national, or site level. These are explained and evaluated below and in Section K. These approaches are able to account for changes in conditions around the world or other events that might alter the global distribution of PIKE. Our results are unchanged under these various alternative approaches.

Other analyses of the PIKE data have used a variety of alternative statistical models (e.g. [13]). Here, we opt for the simplest and most transparent model that adequately accounts for critical patterns in the data (Equation 4). This approach allows us to clearly see what is happening in the data and does not obscure what are relatively simple patterns with complex transformations. (We verify that our findings are robust to more complex approaches in Appendix K.) Earlier analyses of the PIKE data have focused on using nonlinear models that constrain PIKE to be bound between zero and one inclusive (the interval [0,1]), because PIKE values are constrained by construction. The sample distribution of PIKE is displayed in Figure S7, and the stacking of observations at zero and one are also conspicuous features that might be considered in a statistical model. Interestingly, however, linear statistical models perform remarkably well in this context and have multiple advantages to nonlinear approaches. Nonlinear approaches are substantially more difficult to interpret, generally require stronger assumptions regarding the data generating process, and do not allow all of the data to be used.

The key reason why linear models perform so well in this context is because individuals sites almost never exhibit variation in PIKE that spans the full range between zero and one. Sites with high poaching rates tend to always have high poaching rates, and visa versa for low or moderate poaching rates. Thus, variation in PIKE *within* a site tends to be modest and therefore well approximated by linear functions, as suggested by Taylor's Theorem. Our approach only draws inferences from *within-site* changes in PIKE because average site-level poaching rates are absorbed by the site fixed effect μ_i. That is, all remaining variations has mean zero for each site and inference is based on variations over time around this mean. Because variations within a site are much smaller than differences in average poaching rates between sites, Taylor's Theorem applies and linearization describes variation in the data well.



Supplementary Figure S8: **Removing site-specific averages from raw data.** All statistical models used in this analysis remove site-specific averages from the data using site fixed effects. This transformation removes average cross-site differences in reported poaching rates do to geography, culture, economics, surveillance, etc. It also removes biases due to the timing of site entry and exit from the sample. The raw distribution of PIKE is shown for each year in the top panel. Bottom panel shows the distribution of PIKE residuals after site averages are removed via regression on site fixed effects. Notably, the distribution of residuals is more approximately normal than the raw data.

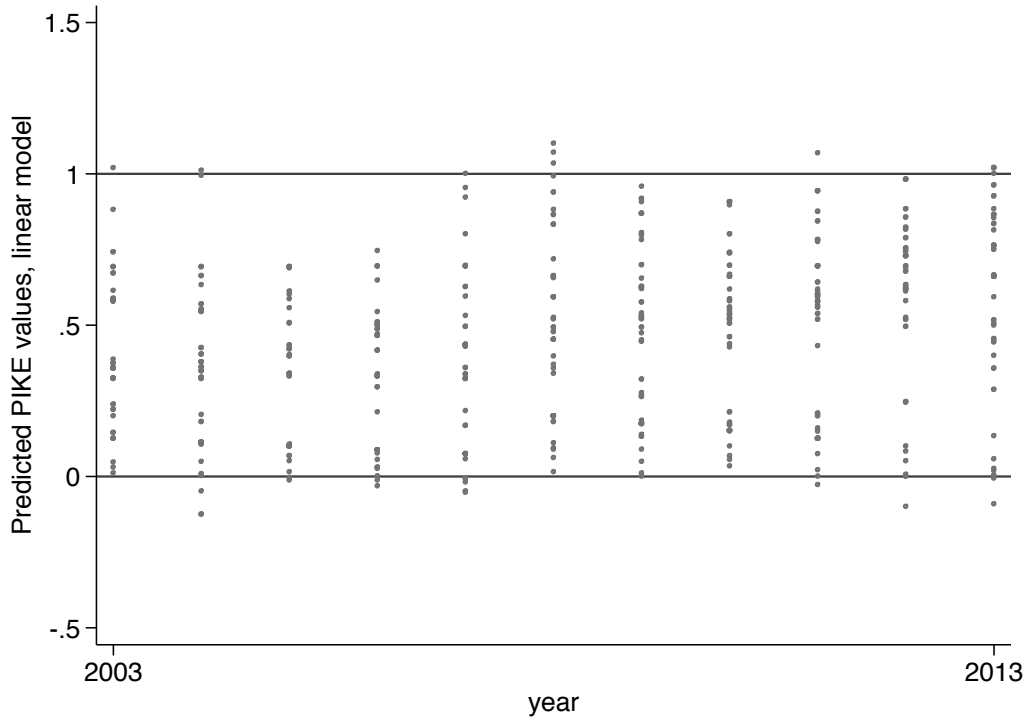


Supplementary Figure S9: **Testing for normality.** Q-Q plot for the residuals of our benchmark model (Equation 4), comparing the distribution of residuals against a normal distribution. Residual PIKE variation in ϵ_{ijt} appears to be well approximated by a normal distribution.

To see this, Figure S8 displays the raw annual distribution of PIKE (top panel) which at first glance appears to be strongly affected by bounds at zero and one. However, once site-specific averages (μ_i) are removed (bottom panel) the distribution of data in each year look approximately normal with a shifting mean[2]. This is why the remaining variation is well-modeled by $\gamma_t + \epsilon_{ijt}$. The variance across years appears to be relatively consistent (homoscedasticity) and is mean zero conditional on annual averages. If the disturbances ϵ_{ijt} were independent and identically distributed, then Ordinary Least Squares (OLS) would be the Best Linear Unbiased Estimator, as indicated by the Gauss-Markov Theorem, meaning that no other estimation procedure is able to use the available data more efficiently than OLS. It is unlikely that the disturbances ϵ_{ijt} are entirely independent across observations within each country, since country-level policies and conditions may change the spatial distribution of poaching within a country or the overall level of poaching within portions of a country or the serial correlation of poaching within a site or subregion. Thus our implementation of Equation 4 does not assume independence across observations within a country and instead non-parametrically estimates a block-diagonal variance-covariance matrix for residuals ϵ_{ijt} where errors for any pair of observations in the same country is not assumed to be zero. This approach is known as "clustering" observations by country [64]. Notably, this approach accounts for arbitrary forms of spatial and temporal correlation within each country and is robust to heteroscedasticity.

Normality of the residual ϵ_{ijt} is not essential for OLS to produce unbiased and efficient estimates of β. Nonetheless, checking that disturbances ϵ_{ijt} are normal is reassuring since it often suggests that

[2]Some stacking at zero occurs because some sites always report the same PIKE values (often either 0 or 1), so there is no within-site variation at those sites.



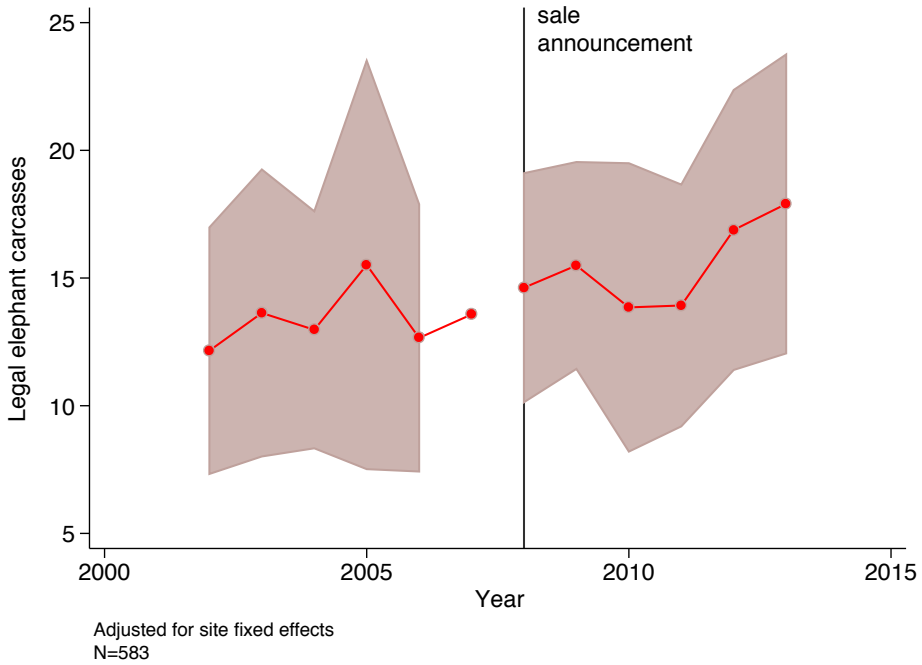
Supplementary Figure S10: **Checking linear model predictions.** Actual PIKE values are bound to lie between zero and one (inclusive) by construction. The linear models used here do not constrain predicted values to lie on this interval, so predicted values must be checked. Predicted observations using our complete model (Equation 6) are plotted for all observations in the sample. 94.7% of predicted values lie on the interval [0,1]. 30 out of 563 observations (5.3%) are outside this interval. Of these 30 predictions, the average distance from the [0,1] interval is 0.0367, with standard deviation 0.0379 and maximum 0.125. Alternatively, if a nonlinear logit fixed effects model that constrains predictions to [0,1] is used, no predictions lie outside these bounds but 32.1% of the sample must be dropped.

variation unexplained by a model is consistent with random noise and it is an assumption that is used to estimate critical values for uncertainty in finite samples. Figure S9 displays a Q-Q plot for our residual variation and indicates that it is indeed extremely close to normally distributed.

Issues arising with alternative nonlinear probability models

Linear probability models are useful because they are transparent, they allow us to non-parametrically account for a variety of spacio-temporal patterns, and they approximate the data well. The central drawback of linear probably models is that predicted probabilities can exceed 1 or fall below 0, which are not valid probabilities. Thus, an important check when using these models is to examine whether the model behaves poorly near these bounds. Figure S10 displays predicted values for our linear probability model. Out of 563 observations, only 30 are outside this interval. Of these 30 predictions, the average distance from the [0,1] interval is 0.0367, with standard deviation 0.0379 and maximum 0.125. Alternatively, if a nonlinear logit fixed effects model that constrains predictions to [0,1] is used,

no predictions lie outside these bounds but 32.1% of the sample is unusable in nonlinear fixed effects models and must be dropped. Below, in Section K, we explore alternative statistical models that do not drop this much data but which do implicitly constrain PIKE to values between 0 and 1. These alternative approaches do have other weaknesses, but none as severe as discarding 32% of the data. Importantly, under these alternative approaches, our results remain unchanged.



Supplementary Figure S11: **Effect of legal ivory sale on count of legal elephant carcasses.** Same as Figure 1B in the main text, but for count of elephant carcasses that are discovered and determined to have died of natural causes or were killed legally. No changes in legal carcasses are statistically significant.

I Legal elephant mortality

PIKE is the standard variable used in studies of elephant poaching because it standardizes the data based on the "discovery rate" of all elephant carcasses [13, 14]. This approach is useful because the discovery of carcasses by surveyors is an irregular process, and changes in surveyor behavior could in principle artificially alter counts of poached elephant carcasses that are discovered. However, because of this standardization, it is theoretically possible that our results are an artifact of a change in the number of legal elephant carcasses that are discovered at MIKE sites. PIKE is computed as

$$PIKE_{it} = \frac{illegal_carcasses_{it}}{legal_mortality_{it} + illegal_carcasses_{it}}$$

and thus an abrupt decline in $legal_mortality_{it}$ in 2008 could generate our main result artificially. To check that this is not the case, we apply Equation 4 to $legal_mortality_{it}$ exactly as it was applied to $PIKE_{it}$. The results are presented in Figure S11. There is no abrupt decline in legal mortality in 2008 and the difference between legal mortality rates before and after the sale are almost identical and are not statistically different. If anything, there is a small *increase* in legal mortality following the sale, which could slightly bias our main result towards zero by reducing post-sale PIKE. If

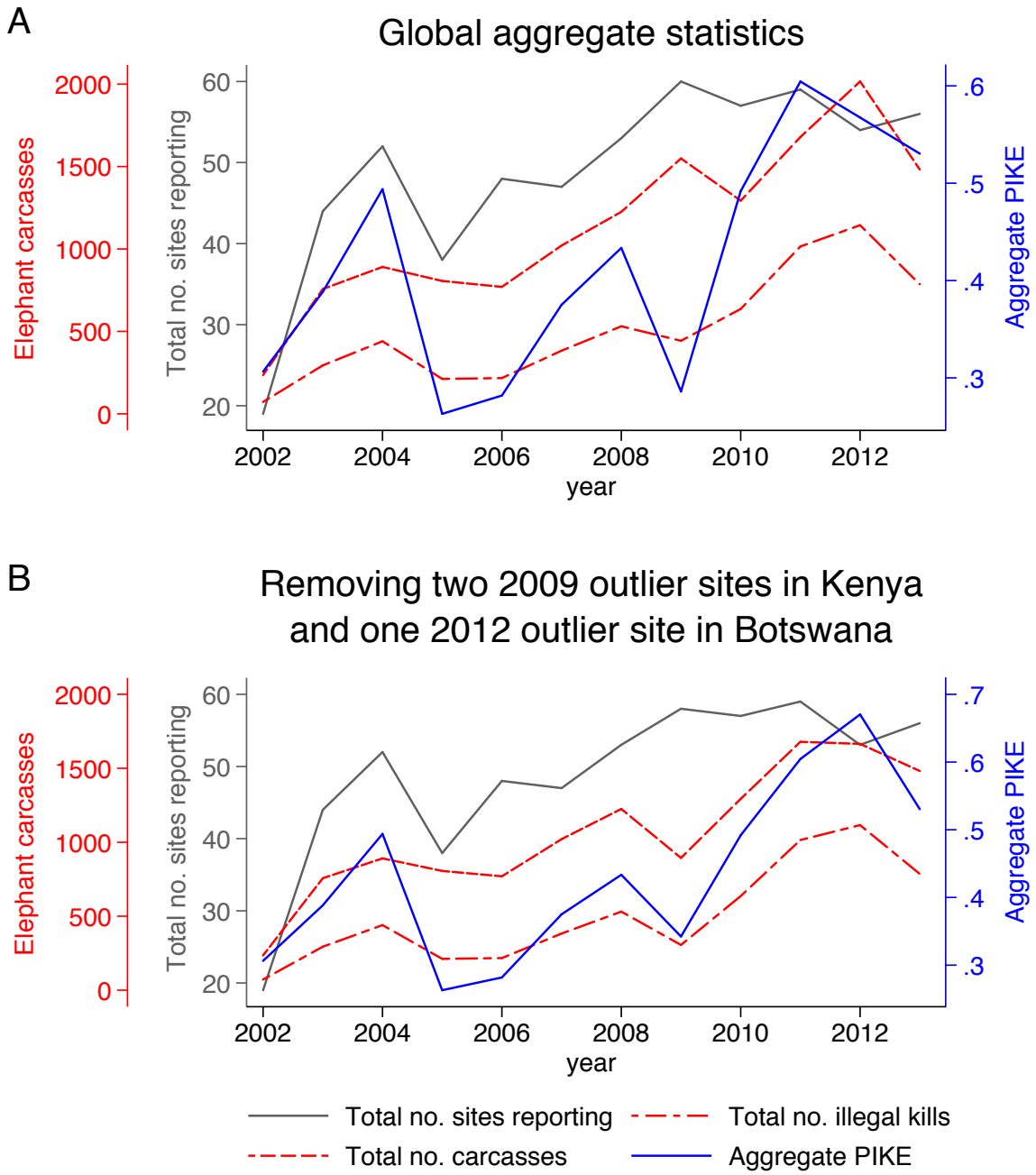
J Comparison to aggregate statistics

In the main text, we estimated the average effect of the legal ivory sale on PIKE in the average site or country. That is, the presented results were site-level or country-level effects representing local changes in expected PIKE, conditional on the sale. However, it would be possible to compute a globally aggregated PIKE measure and to examine whether it responds to the sale. Rather than computing a local measure of PIKE, one could sum illegal carcasses and total carcasses globally, dividing the two to construct an aggregate PIKE measure

$$Aggregate_PIKE_t = \frac{\sum_i illegal_carcasses_{it}}{\sum_i (legal_mortality_{it} + illegal_carcasses_{it})} \tag{7}$$

which is plotted as the blue line in Supplementary Figure S12A. It has been noted [13] that this aggregated measure declined in 2009, just after the legal sale was delivered to China, but it would be incorrect to infer that this observation is at odds with our main result. Most of the change in aggregate PIKE is driven by an increase in legal elephant mortality observed in 2009, since total illegal killing declined very modestly (Supplementary Figure S12A, red lines). Inspection of the data (Supplementary Figure S3) indicates that this increase in legal mortality is driven by two outlying observations from sites in Kenya that experienced extreme drought that year [13]. When those two outlying observations are removed, the sharp drop in PIKE essentially vanishes (Supplementary Figure S12B). These two outlying observations are removed in the analysis presented in the main text. However, we demonstrate in Section K that even if these outliers are included in our analysis, they are dramatically less influential because of the panel approach we adopt and do not generate a similar artifact in our estimates.

There are four additional reasons to prefer the panel approach we use (Equation 4 and 5), rather than examining aggregated global statistics (Equation 7). First, in the panel approach, local illegal killing is always normalized by local total mortality. In the aggregated data, measures of illegal killing are normalized by global total mortality, however global trends may not reflect idiosyncratic local events that increase the likelihood of observing illegal carcasses locally. Second, the panel approach allows us to account for unobserved local heterogeneity in both average poaching levels and local trends, which may be different across locations. Third, our panel of data is unbalanced, so some sites either stop reporting or begin reporting (or both) during the period of observation. If these sites differ from the average site, then their appearance or disappearance in the data will bias aggregate PIKE statistics but will not bias a panel model including site fixed effects. Fourth, the disaggregated approach allows us to examine spatial heterogeneity in effects, as is done in Figures 2 and S18.



Supplementary Figure S12: **Declines in 2009 aggregate PIKE are driven by two outlying observations.** The total number of sites reporting any elephant carcasses in MIKE data (grey line) used to compute aggregate PIKE statistics analyzed in prior studies. Global aggregate PIKE (blue line) is the total number of illegally killed elephant carcasses discovered (red dot-dashed line) divided by the total number of carcasses discovered (red dashed line). (A) Prior studies observed that aggregate PIKE declined sharply in 2009 following the legal sale of ivory. This drop does not reflect a correspondingly rapid drop in illegally killed elephants, but rather is driven by a rise in total carcasses. This abrupt rise in total carcasses is driven entirely by two sites in Kenya that report extremely high rates of *legal* elephant mortality in 2009 ($> 9\sigma$ outliers, see Supplementary Figure S3), which are dropped from our main analysis. (B) When these two outlier observations are removed from the sample (see Appendix F for details), aggregate total mortality falls (rather than rises) in 2009 and the associated drop in aggregate PIKE appears similar to background variability.

K Robustness of results

Narrowing the window of analysis

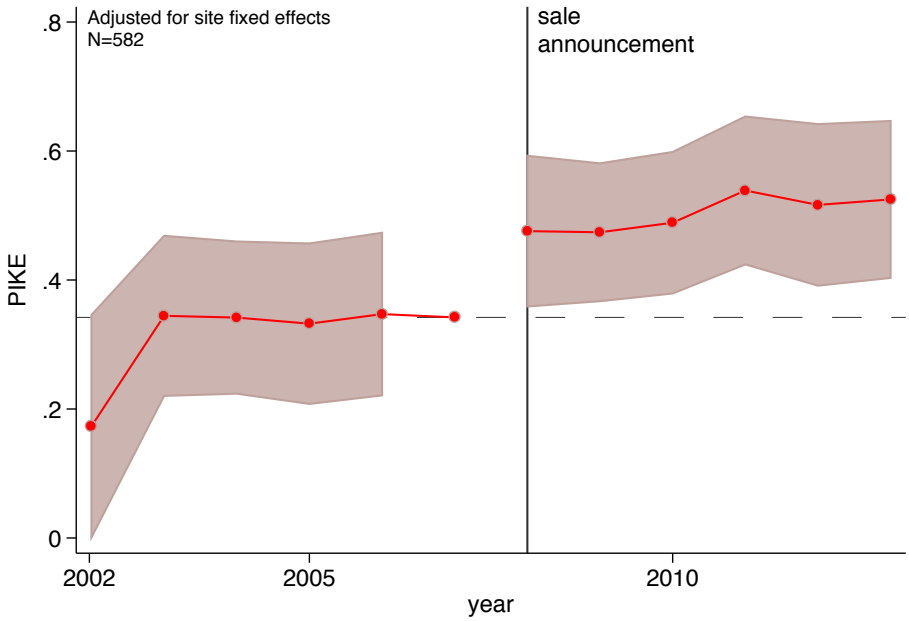
In addition to accounting for secular trends (Section H), we verified the robustness of our results by "zooming in" to the moment of the event, narrowing the window of years that we examine [23]. To do this, we repeat the analysis using Equation 5 with data from only 2 or 3 years immediately before and after the sale. This approach reduces the influence of observations far from the discontinuity, thus preventing trends from biasing our point estimates. The primary tradeoff of this approach is that uncertainty increases because the size of the sample is reduced substantially. Table 1 columns 6 and 7 present estimates using this approach. Consistent with our main finding, our point estimates remain essentially unchanged, although statistical uncertainty rises as expected. Because the size of standard errors increases, these estimates are no longer statistically significant, however the purpose of this test was to check for bias in the point estimates and so significance is irrelevant. When the sample is restricted to 3 years before and after the announcement, the size of the estimated discontinuity in PIKE is 0.11, while when the sample is restricted to 2 years before and after, the discontinuity is 0.15. These two estimates are very near and well within one standard error of the main estimate of 0.13, suggesting our main estimate is unlikely to be biased by trends unaccounted for in our main analysis.

Regional subsamples

Regional subsamples of the data recover estimates very similar to our main estimate, suggesting that our globally pooled model is broadly representative of regional patterns. Restricting our sample to only African countries obtains a similar 0.14 ($\pm$0.062) increase in PIKE from 2008 onwards ($p<0.05$, Table 1 row 8). Restricting the sample to Asian countries recovers an estimated increase in PIKE of 0.13 ($\pm$0.125, Table 1 row 10), but the variance of the estimate is larger, likely due to the smallness of this subsample (N=99), rending this increase not statistically significant on its own. Importantly, however, both estimates are almost identical to the pooled estimate 0.13 (Table 1 row 3), indicating that pooling is a reasonable approach.

Inclusion of 2002

As detailed in Section G, data from 2002 (the first year that the surveillance system was deployed) is not comparable to other years in the sample. This may have been related to inexperience among new surveyors and management systems and was most likely related to sites not surveying during the entire twelve months of the year. For these reasons, we omitted 2002 from our main analysis. Nonetheless, we document that our main conclusions are robust to including 2002 in our sample. In Table S3 we display our main estimate (column 1) and the analogous estimate when 2002 is included in the sample (column 2). The estimated size of the effect of the sale becomes somewhat smaller at 0.10; this estimate is not significantly different from the result in our main analysis and remains significant at the 10% level. Figure S13 displays why the size of the estimate changes. Because 2002 is a large negative outlier, it causes the pre-2008 trend to be much steeper (see pre-2008 trend estimate in columns 1-2 of Table S3). This causes the fitted trend to lie above the data in 2007, thereby reducing the estimated size of the discontinuity.



Supplementary Figure S13: **Results including 2002** Same as Figure 1B in the main text, except keeping data from 2002 in the sample.

It is possible that some of the difference between 2002 data and the remaining data is due to the surveillance network being new and the sites reporting in 2002 not reporting all elephant carcasses in a uniform way, perhaps due to inexperience. Should similar issues affect other sites that began reporting after 2002, it is possible that the first year of those records are biased in a similar way. Thus, we test whether our results are robust to eliminating the first year of data reported by *each* site, an alternative approach to simply dropping the year 2002 that treats time series from each site identically. Results from this alternative approach are shown in column 3 of Table S3; our results remain significant and unchanged, including the ten-fold change in the PIKE trend that occurred in 2008.

Outcome specification

Our main approach to estimating the effect of the legal sale is to use PIKE as our dependent variable, as in Equations 4-6. This approach follows the literature in ecology on the use of data from the field, where sampling may be strongly affected by natural factors and surveyor behavior, both of which are partially corrected for by normalizing illegal carcasses by the total count of carcasses found at each site [13]. Moreover, the modeling assumptions required to use our approach, such as the normality of PIKE residuals conditional on nonparametric controls, were each individually inspected in Section H. Nonetheless, we verify that our result is robust to an entirely different modeling approach, whereby we model the count of illegal carcasses discovered and do not assume the discovery rate for illegal carcasses is the same as that of legal carcasses (the implicit assumption adopted when using PIKE to correct for discovery rates).

First, we estimate the effect of the sale on $\ln(illegal_carcasses)$ at each site, controlling for the same

Supplementary Table S3: **Estimated effect of legal ivory sale on PIKE with including 2002 data**

	(1)	(2)	(3)
Years in sample	2003-2013	2002-2013	2002-2013
Effect of sale	0.129**	0.098*	0.107*
	(0.054)	(0.049)	(0.056)
Pre-2008 trend	0.001	0.014	0.012
	(0.016)	(0.015)	(0.018)
Change in trend after 2008	0.011	-0.002	0.001
	(0.023)	(0.022)	(0.026)
drop first obs. per site			Y
Observations	562	581	504
R-squared	0.540	0.531	0.578

Dependent variable is PIKE. All models include site fixed effects and a common trend with a kink in 2008. Standard errors clustered by country in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Sample is all countries. Column 1 includes our benchmark model and sample (same as column 3 in Table 1). Column 2 includes data from 2002, which is omitted from the benchmark model because of reporting irregularities probably related to the initial rollout of the surveillance program (see Appendix G). Column 3 is estimated using a restricted sample where the first observation in each site is dropped, an alternative approach to handling low quality data during surveillance rollout.

variables as in Equation 6 but introducing an additional control variable for $\ln(legal_carcasses + 1)$ at each site:

$$\ln(illegal_carcasses)_{ijt} = \phi \ln(legal_carcasses + 1)_{ijt} + \mu_i + \theta t + (\theta' t + \beta) \cdot post_event_t + \epsilon_{ijt}. \quad (8)$$

The coefficient on legal carcasses ϕ will be equal to unity if the ratio of illegal carcasses to legal carcasses is fixed, although it is not assumed *ex ante*. The coefficient β can be interpreted as the discontinuous change in the log of illegal carcasses that are discovered starting in 2008, controlling for the count of legal carcasses. Column 1 of Table S4 presents results using this alternative approach. The estimated effect of the sale $\beta = 0.377$ is large and statistically significant, indicating that poaching increased abruptly by roughly 46% in 2008. This estimate is very near the 38% increase in PIKE estimated using our benchmark approach (Figure 1).

Poisson regression

In column 2 of Table S4, we estimate a model with conditional mean similar to Equation 8, however instead of using OLS we assume that the count of illegal carcasses is distributed Poisson and estimate

Supplementary Table S4: **Estimated effect of legal ivory sale on illegal elephant killing using alternative specifications**

	(1)	(2)	(3)
Effect of sale	0.377**	0.422***	0.362***
	(0.181)	(0.069)	(0.070)
Pre-2008 trend	-0.005	0.028	0.033*
	(0.056)	(0.018)	(0.019)
Change in trend after 2008	0.088	0.084***	0.084***
	(0.077)	(0.021)	(0.022)
Poisson regression		Y	Y
control for ln(legal carcasses + 1)	Y	Y	
control 5th-order polynomial in legal carcasses			Y
Observations	562	532	535

Dependent variable is ln(illegal carcasses+1) in column 1 and count of illegal carcasses in columns 2-3. All coefficients are interpretable as log points. All models include site fixed effects and a common trend with a kink in 2008. Columns 1-2 control for ln(legal carcasses+1). Columns 2-3 estimate a Poisson fixed effects regression with a log link function. Column 3 controls for the count of legal carcasses using a fifth-order polynomial in legal carcass count. Standard errors clustered by country in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Sample is all countries, 2003-2013.

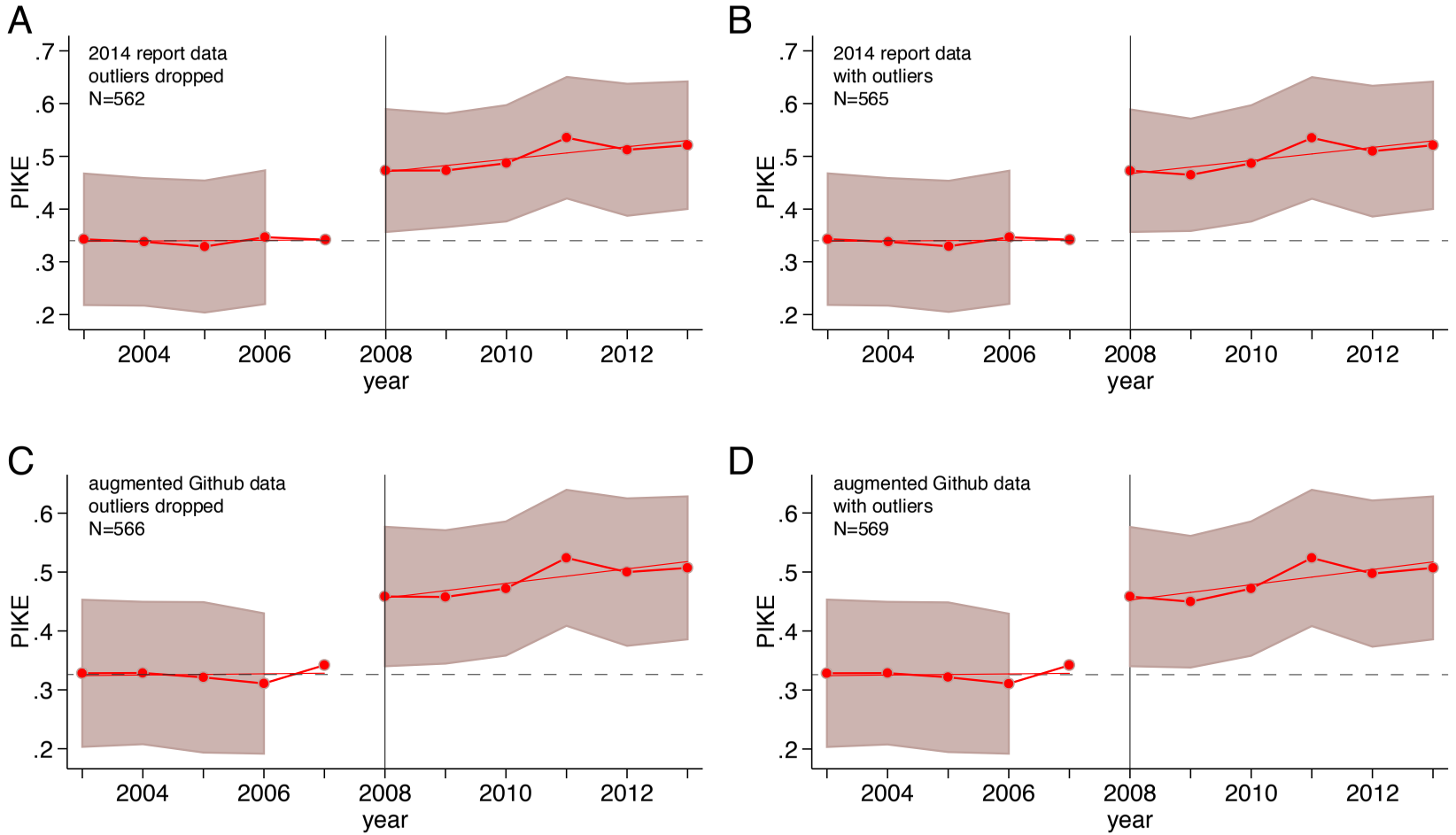
the parameters describing the conditional mean via maximum likelihood. We continue the recover our main result, although the estimated effect is slightly larger, indicating a jump in poaching by roughly 50%. Using this approach, the post-2008 change in the poaching trend by roughly 8%/yr is highly statistically significant.

In column 3 of Table S4, we estimate a similar model to column 2 but instead of controlling for the log of legal carcasses + 1, we control for the count of legal carcasses using a fifth-order polynomial. This alternative approach allows us to flexibly account for the number of legal carcasses discovered without requiring that we add one to the count of legal carcasses, which is a standard albeit *ad hoc* solution to the issue that the log of zero is not well defined (in cases where no legal carcasses are discovered). This alternative approach recovers nearly identical estimates.

Outliers and data file

As detailed in Section F, three extreme outlying observations were dropped from our analysis because of exceptionally high legal mortality rates. As shown in Section J, these outliers substantially bias aggregate PIKE statistics used in prior analyses. Here we check that inclusion of these outliers do not systematically bias our results. Panel A of Figure S14 replicates our main results (Figure 1B in the main text). In panel B, we include the three outliers that were dropped from the main analysis and find the results unchanged.

As noted in Section E, CITES provide two slightly different versions of their data via their Github



Supplementary Figure S14: **Effect of legal ivory sale on PIKE using alternative CITES data file and including outliers.** (A) Same as Figure 1B in the main text. (B) Same as A, except including the three outliers described in Section F. (C) Same as A except using publicly available data posted on Github for 2003-2011, instead of CITES report data. (D) Same as B, except using the Github data.

website [25] and in published CITES documents [60, 59]. Since the published data [59] were more recent and complete, we use these data in our main analysis. Here, to ensure that our results are not sensitive to the differences in the data file obtained from CITES, we implement our analysis using the Github data (for the years 2003-2011) augmented with data from SC65 Inf. 1 for 2012-2013 and site-years absent in the Github data. Panel C of Figure S14 replicates our main result using this alternative data file (compare to panel A). Using the Github data, we estimate that pre-sale PIKE was 0.326 ($\pm$0.04) compared to 0.340 ($\pm$0.04) using the report data. Post-sale values are 0.487 ($\pm$0.048) and 0.50 ($\pm$0.048), respectively. Our results do not change when we implement our analysis using the alternative CITES data file.

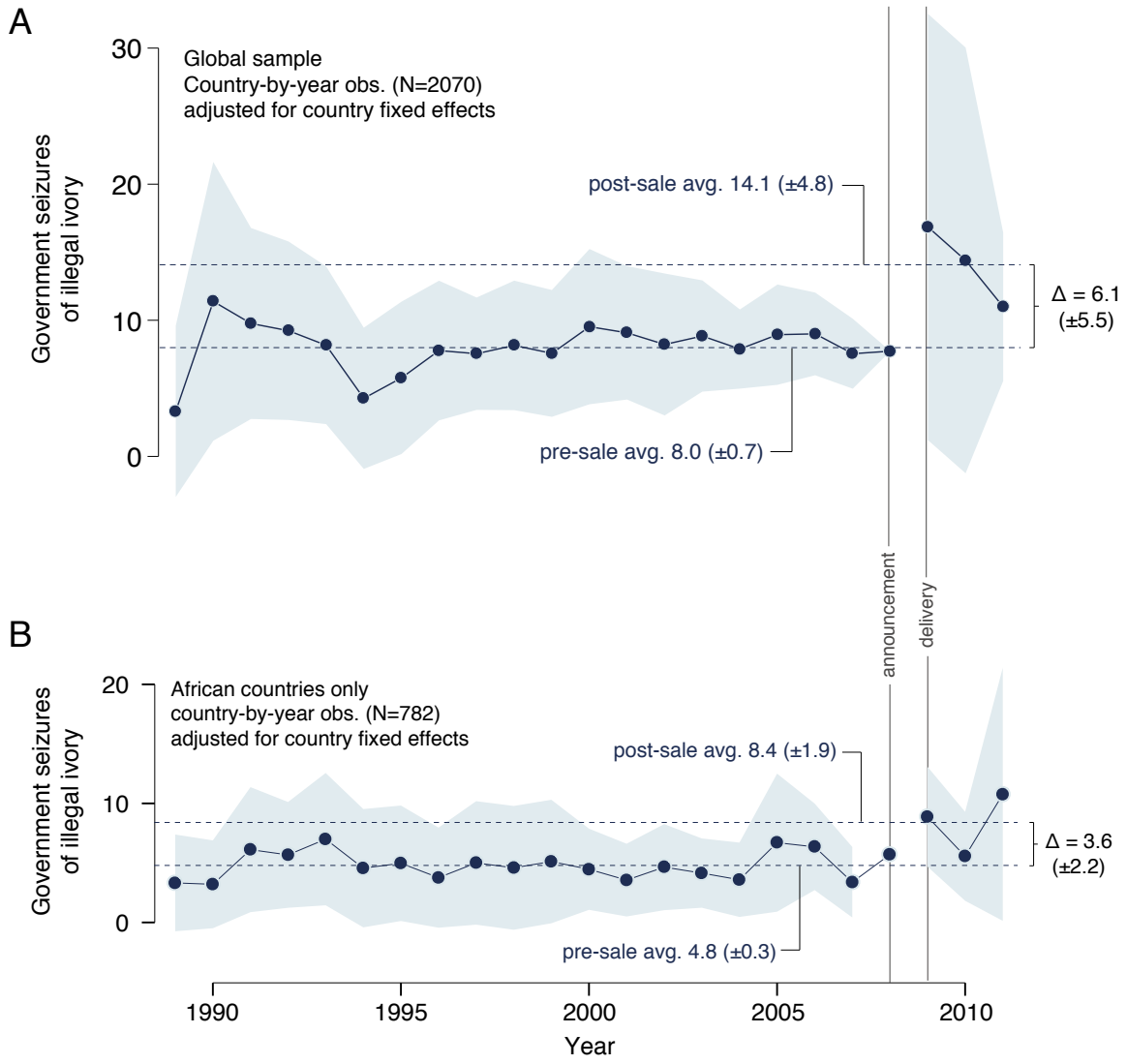
In Panel D of Figure S14, we use the Github data file and do not drop the three outlier observations (which remain outliers with the alternative data file). We obtain our main result.

L Ivory seizures

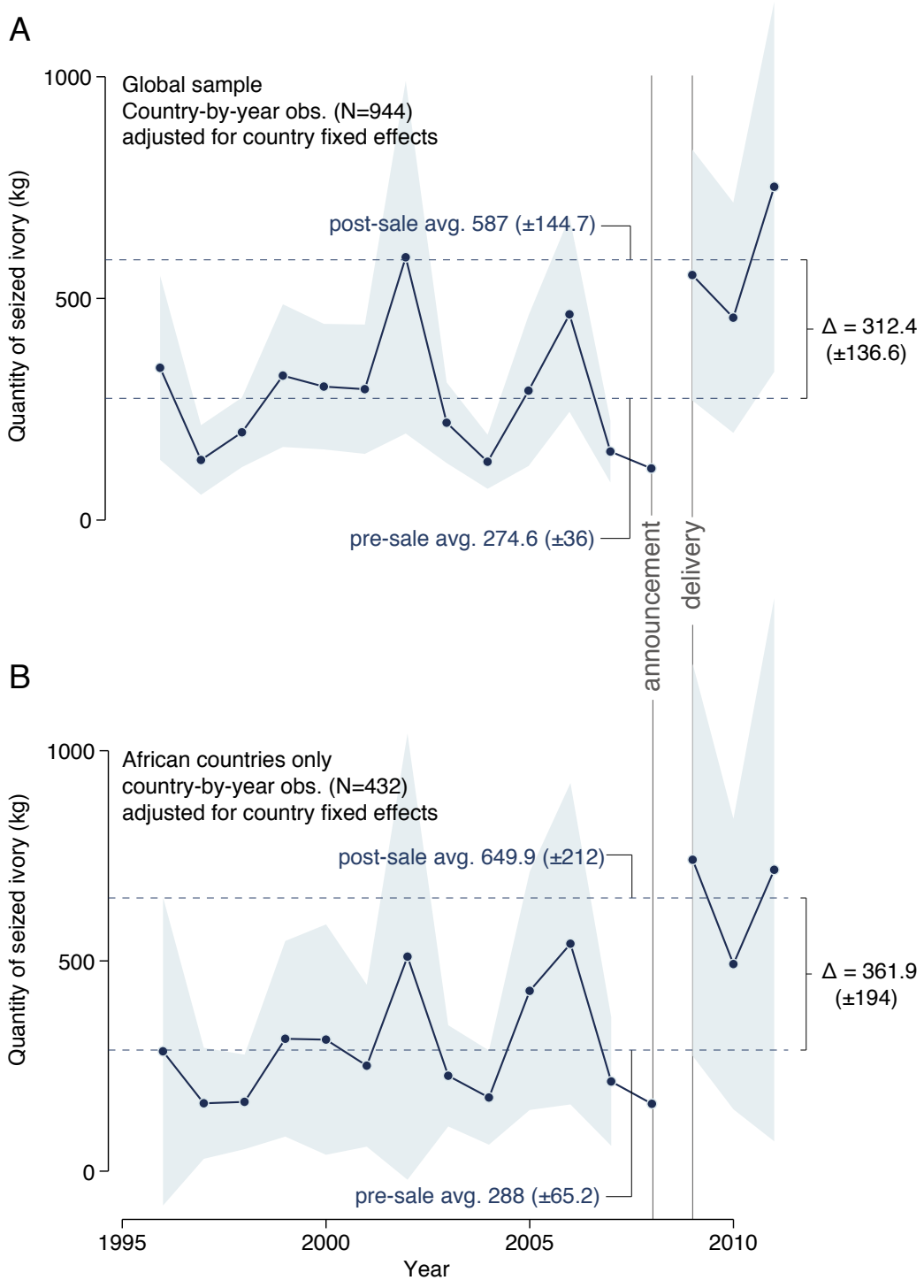
We examine whether rates of government seizure for illegal ivory respond to the legal sale by applying Equation 4 to global data. Seizures are considered an imperfect proxy measure for overall smuggling activity, but are interpreted cautiously because seizures depend strongly on surveillance effort. We adjust the date of the estimated discontinuity to 2009 because theory suggests that mechanisms leading to changes in consumption of illegal ivory can only occur once legal ivory is available on the market. If legal ivory were able to displace illegal ivory on the market, the resulting decrease in the smuggling of illegal ivory would be expected to occur after the delivery of ivory to China and Japan in 2009 (see Section A, Figure S1A). The two alternative mechanisms by which the availability of legal ivory could lead to increased consumption of illegal ivory also require that legal ivory be in circulation in the market. First, consumers might increase demand in response to an increase in their perceived legality of a good, which will not change until they actually observe legal versions of the good on the legal market (Section A, Figure S1B). Second, suppliers might venture to put more illegal ivory on the market because the circulation of legal ivory helps mask that of illegal ivory, reducing the cost of smuggling (Section A, Figure S1C). Since the delivery of legal ivory only occurred in 2009 (Section **??**), we may expect the most dramatic change in smuggling behavior– and, thus, seizures– to occur in this year.

demand for ivory sales will not respond to the legal sale until consumers observe legal ivory in circulation (recall Section A).

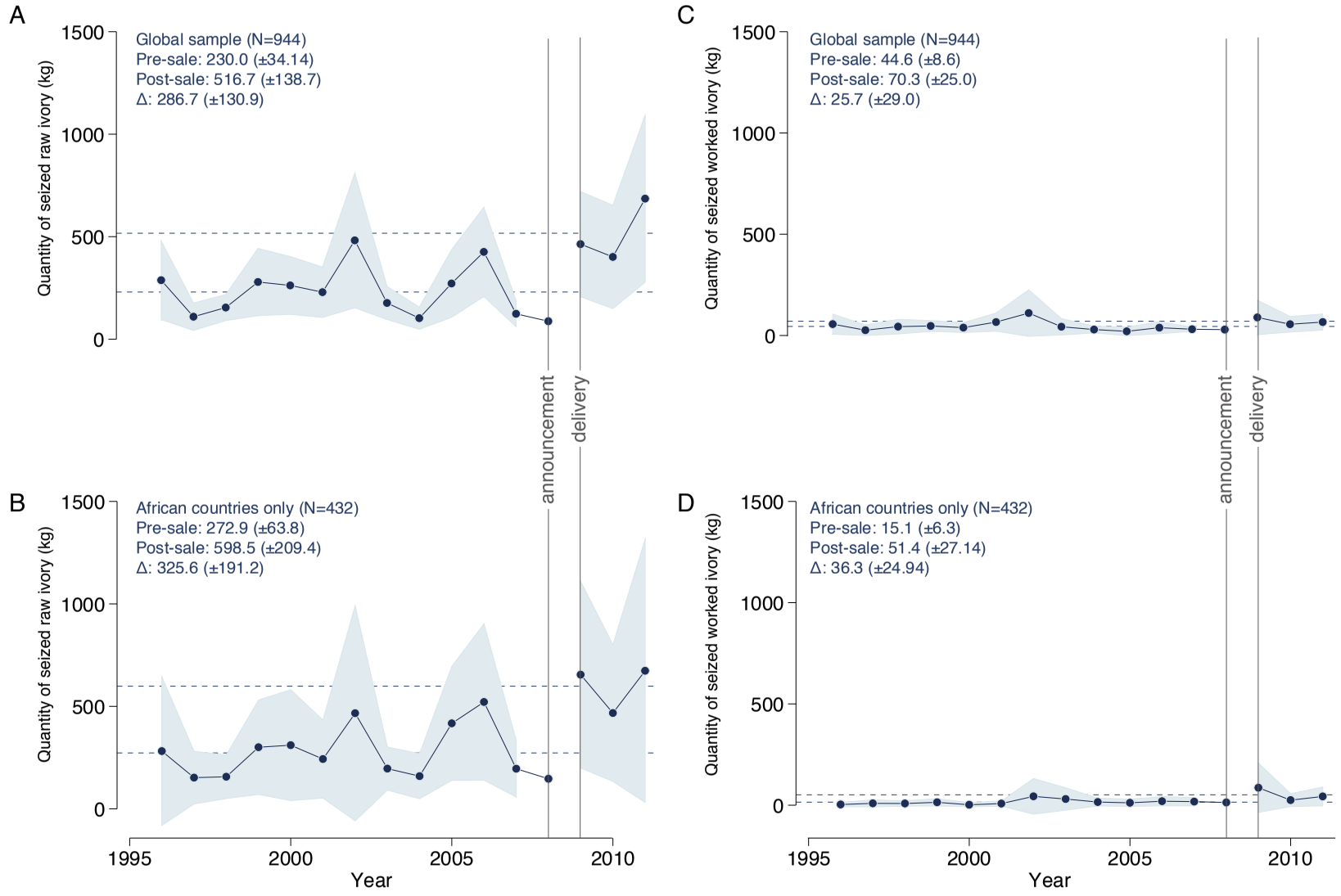
Applying Equation 4 to country-level data on seizure counts (total seizures) or total quantities (kg) recovers results that are shown in Supplementary Figures S15A and S16A, respectively. Globally, average seizure rates rose roughly 6.1 ($\pm$5.5) seizures yr^{-1} in 2009, although the higher value and high variance of this estimate is largely driven by China, whose reported seizures rose abruptly by 475 seizures yr^{-1} on average (Figure S18). While this result seems to corroborate our central finding that the legal sale to China caused a rise in PIKE, it should be interpreted cautiously since the quality of China's seizure data is unclear. For example, in 2009 China redefined seizures to include "personal effects", including small ivory trinkets, that had previously been excluded from their ETIS reports [22]. We thus strongly suggest that the seizures count data is interpreted with caution. The total mass



Supplementary Figure S15: **Effect of legal ivory sale on number of illegal ivory seizures globally.** (A) Same as Figure 1B in the main text, but for seizures globally. Comparisons are relative to 2008, the year just before legal ivory was physically delivered to purchasing countries. Dashed lines are average seizures per country before and after the sale. Standard errors in parentheses. (B) Same as A, but only for African countries.



Supplementary Figure S16: **Effect of legal ivory sale on illegal ivory seizures by weight globally.** (A) Same as Figure S15A, but for total mass of seized ivory. (B) Same as A, but only for African countries.



Supplementary Figure S17: **Effect of legal ivory sale on illegal ivory seizures by weight and whether raw or worked globally.** (A) Same as Figure S15A, but for total mass of seized raw ivory. (B) Same as A, but only for African countries. (C) Same as A, but for worked ivory. (D) Same as B, but for worked ivory.

of seized ivory at the country level rose by roughly 312.4 (±136.6) kg yr^{-1} in 2009, and this result is robust to removing China from the sample (see Section D for more on how the total weight of ivory seizures made each year in each country was estimated).

Smuggled ivory may be seized in any country, however any signal related to increased poaching will likely be strongest at the point of origin for the illegal ivory since all ivory must be smuggled out of the country where it originated. In the period studied, most poaching for ivory occurred in African range states [28]; just in 2010-2012, the number of African elephants killed by poachers was twice the total population of wild Asian elephants [65]. Thus, in an effort to increase the signal-to-noise ratio in our analysis, we focus on seizures in African countries. Even if all countries were to generate similar quantities of "noise" in their seizure data, focusing on African countries will increase signal-to-noise because the signal is likely strongest at the source of illegal production, before smuggling routes disperse contraband widely.

Restricting our sample to African countries, we apply Equation 4 and recover results shown in Figures S15B and S16B. Pre-sale average seizure rate rose from 4.8 seizures yr^{-1} to 8.4 seizures yr^{-1} in 2009. Pre-sale average seizure quantities rose from 288 (±65.2) kg yr^{-1} to 361.9 (±194.0) kg yr^{-1}. Applying an analog to Equation 5, we estimate statistically significant and similarly sized increases of 3.4 (±1.4, $p<0.05$) seizures yr^{-1} and 317.0 (±183.6, $p<0.1$) kg yr^{-1}. Roughly 90% of the increase in seized ivory is raw ivory, with the vast bulk of it seized in Africa (Figure S17), consistent with the increase in seizures being related to the increase in poaching and not just an increase in ivory smuggling activity generally. Accounting for uncertainty (95% C.I.), this suggests smuggling out of Africa may have increased 14-128% (using seizure rates) or −17-238% (using ivory mass seized). These ranges are large, in part because data on the precise weight of each seizure made in each country are not made publicly available (instead, weights of seizures were estimated based on their size class—see Section D); nonetheless, both ranges span our main estimate that poaching rates increased roughly 38% at the time of the sale. Importantly, however, changes in surveillance effort among African countries might have changed at the time of the sale, which could bias these estimates. Thus, we consider this finding corroborating evidence, but interpret it cautiously.

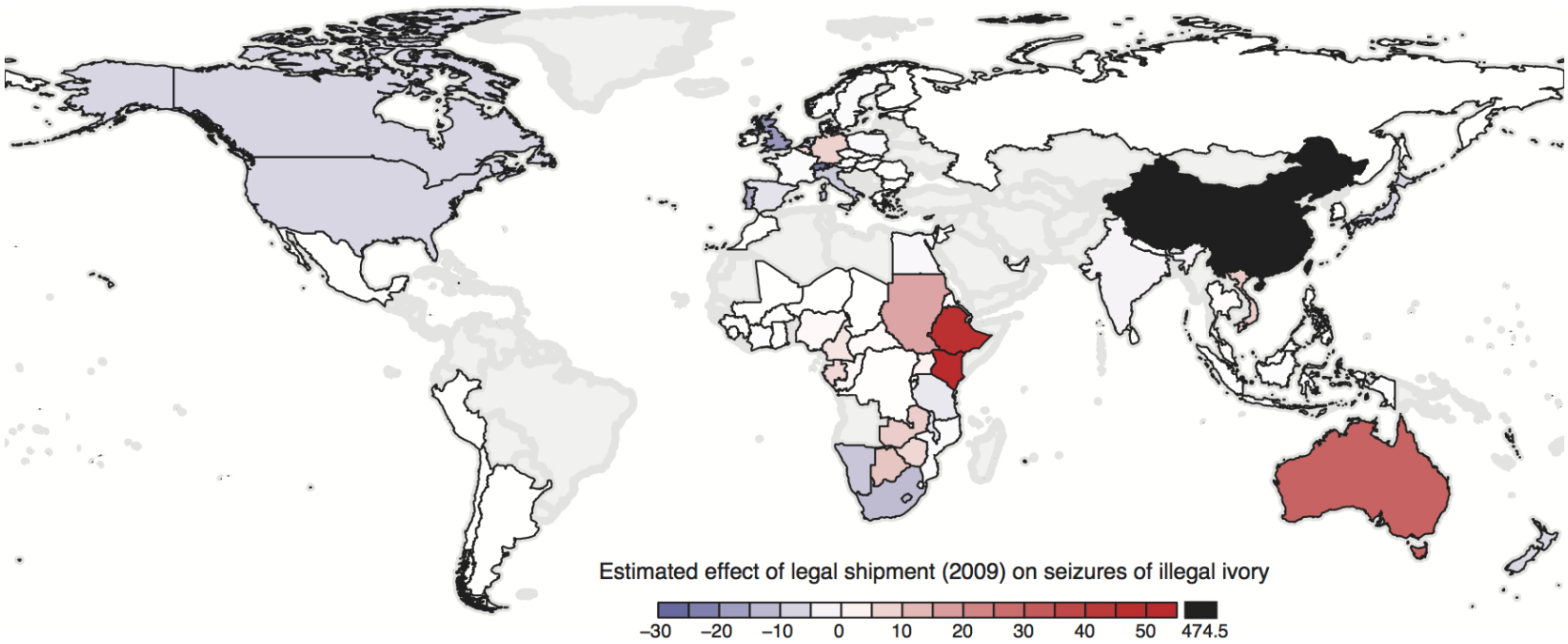
M Estimating heterogeneous effects across countries

To estimate the relative contribution of individual countries to parameter estimates in Figure 1 and Table 1, we apply a variant of Equation 4 that allows for heterogenous effects across countries. Specifically, to construct Figure 2 we estimate

$$PIKE_{ijt} = \mu_i + \beta_j * post_event_{jt} + \epsilon_{ijt} \tag{9}$$

where $post_event_{jt}$ is a post-event dummy that is country-specific (i.e. a post-event dummy is interacted with country-specific fixed effects) and β_j is the country-specific effect plotted in color. Variables are defined as in Section H.

Similar estimates can be obtained using seizures data, however site specific fixed effects μ_i are replaced with country fixed effects μ_j and the cutoff date is changed from 2008 to 2009, as in Section L. These results are shown in Supplementary Figure S18. The average positive effect in Africa is



Supplementary Figure S18: **Change in seizures of illegal ivory following the delivery of the legal ivory sale.** The contribution of individuals countries to the discontinuity shown in Supplementary Figure S15, with color indicating the average change in seizures after the 2009 delivery in each country, estimated using an analog of Equation 6. Countries with no data are grey.

somewhat diffuse, although it appears to be less widespread than the effect on PIKE. The small, negative and statistically insignificant effect of the sale on non-African and non-Asian countries (Table 1) appears to correspond with declines in seizures in North America and Europe. The reported seizure rate in China rose abruptly by 475 seizures yr^{-1} on average. As stated above: while this result seems to corroborate our central finding that the legal sale to China caused a rise in PIKE, it should be interpreted cautiously since the quality of China's seizure data is unclear (for example, in 2009 China redefined seizures to include "personal effects", including small ivory trinkets, that had previously been excluded from their ETIS reports [22]).

N Discontinuity findings in covariates

As described in Section H, our event study research design permits causal inference if other factors known to affect PIKE are "smooth"across the moment of the event – i.e. that they do not exhibit discontinuities similar to Figure 1B. While it is impossible to test this assumption for the universe of potential confounders, we analyze sixteen covariates that are closely linked to the commonly proposed drivers of elephant poaching over the last decade [6, 8, 42, 22, 27]. Applying Equation 4 but replacing PIKE with salient covariates, we examine Chinese affluence using per capita Gross Domestic Product (GDP), and we examine Chinese influence in elephant range states using trade measures (imports, exports, relative imports, relative exports), measures of China's financial influence in African elephant range states (foreign direct investment, foreign aid), and measures of China's physical presence in range states (service and engineering personnel) in Figure 3 (panels A-C, E, F, I, J, M, N). In addition, we repeat this analysis for Japanese GDP per capita, aid and trade data (Figure 3 G, H, K, L, O). We also examine GDP per capita of range states reporting PIKE since labor market conditions influence the opportunity cost of poaching (Figure 3P).

The 2008 one-time sale coincided with the year of the recent financial crisis, which could be argued to have had an effect ivory black markets. We have not seen specific mechanisms proposed by which this may have happened, and the per capita GDP of elephant range states is already included. One possibility is that the crisis reduced the volume of trade between Western nations and the elephant range states, enhancing China's *relative* importance as a trading partner, and thus the relative importance of ivory demand in China, in elephant range states. We therefore included the proportion of all range states imports and exports that came from and went to China, respectively (Figure 3 Panels I-J). We also repeated these analyses for Japan (Figure 3 Panels K-L).

We use the same approach for modeling the covariates as we did for PIKE. Analysis using Equation 4 produced Figure 3. None of the resulting covariates show the abrupt level increase in 2008 that is visible for the PIKE data (Figure 1A). Chinese services labor exhibits the only discontinuous level-shift in 2008 (Figure 1B), however the change is a decline in Chinese presence, suggesting this change is unlikely to explain the abrupt increase in PIKE. We also note that imports to and from Japan rose abruptly in 2008; however, unlike PIKE, both measures return to their trend in 2009. In sum, we believe the absence of visible discontinuities in Figure 3 resembling the striking discontinuity in Figure 1B is strong evidence that changes in Chinese and Japanese affluence and influence in elephant range states are not likely to be responsible for the discontinuous increase in poaching reported here.

Nonetheless, we also analyze the covariates using Equation 5; unlike with the PIKE data, which accommodate only linear trends, we are able to fit the covariates data with models accounting for a) linear common trends, b) quadratic common trends, c) cubic common trends, or d) linear country-specific trends. These analyses provide several instances which suggest a small, statistically significant, discontinuous increase in 2008 for the covariate analyzed. Based on these instances, the covariates can be placed in three categories:

1. Covariates for which the models accounting for country-specific linear trends indicate a statistically significant change in 2008, but the models accounting for polynomial trends do not:
   (a) Elephant range state imports from China (Panel E): 2008 effect = 0.449, $p<0.01$, n=780.
   (b) Percentage of elephant range state imports from China (Panel I): 2008 effect = 0.036, $p<0.01$, n=768.
   (c) Percentage of elephant range state exports to China (Panel J): 2008 effect = 0.043, $p<0.05$, n=792.
   (d) Percentage of elephant range state imports from Japan (Panel K): 2008 effect = 0.017, $p<0.01$, n=768.
   (e) Percentage of elephant range state exports to Japan (Panel L): 2008 effect = 0.012, $p<0.05$, n=792.
   (f) Chinese foreign direct and other investment in range states (Panel M): 2008 effect = -0.617, $p<0.10$, n=267.
2. Conversely, covariates in which only the models accounting for polynomial common trends indicate a significant change in 2008:
   (a) Number of Chinese personnel in range states for labor services (Panel B): 2008 effect = -0.486, $p<0.10$, n=319.
   (b) Elephant range state exports to Japan (Panel H): 2008 effect = 0.433, $p<0.05$, n=781.
3. Covariates for which the linear and polynomial models suggest a statistically significant increase in 2008:
   (a) Elephant range state imports from Japan (Panel G): 2008 effect = 0.829, $p<0.01$ for linear models; effect = 0.499, $p<0.01$ for polynomial models; n=761);
   (b) Japanese aid to elephant range states (Panel O): effect = 0.446, $p<0.05$ for linear models; 2008 effect = 0.560, $p<0.10$ for polynomial models; n=759.

For many of the covariates in category 1 (Panels I-M), Figure 3 indicates that the underlying trend is likely to be polynomial and not linear, suggesting the linear models fit are inappropriate. While R-squared and AIC measures suggest that country-specific linear trends models outperform polynomial models for all covariates, these improvements are likely a mechanical consequence of the large increase in predictors. Figure 3 does indicate a reasonably linear trend for imports from China (Panel E), but the discontinuity appears to be driven by a single point (2008), after which the data returns to trend.

In category 2, the number of Chinese personnel in range states for labor services (Panel B) does indicate a sharp discontinuity in 2008—but the change is a decrease, contradicting the prevailing hypothesis about how the presence of Chinese citizens in range states affects the ivory trade. The other category 2 covariate, exports to Japan from range states, appears to show an anomalous increase in 2008 before returning to trend. This fails to reflect the permanent increase we found in PIKE.

In the third category, range state imports from Japan (Panel G) present a case similar to that of exports to Japan (Panel H), and appear to be driven by an anomalous increase in 2008. Japanese aid (Panel O) shows no obvious discontinuity at 2008, and the apparent discontinuity may be driven by anomalously high aid provided to range states in 2010.

While none of the panels in Figure 3 present a strong case for an alternative explanation for the increase in PIKE, further exploration of some of the covariates, particularly how overall trade between China and the elephant range states may relate to the ivory trade, may be merited.