Trade Openness and the Illegal Ivory Trade

Diana Schoder

Haverford College

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Abstract

This paper examines how the illegal ivory trade—including poaching, smuggling, and stockpiling—responds to increases in trade openness. When a Trade and Investment Framework Agreement promotes legal trade between an ivory-exporting country and the United States, the impact on illegal ivory is unclear. Predictions about poaching activity and smuggling diverge, and the option to stockpile further complicates this scenario. A difference-in-differences estimation using a panel regression with synthetic controls and unique tests to analyze shifting markets and stockpiling behavior suggest that 1) trade openness motivates increased poaching, 2) this increased supply of ivory is, at least in part, stockpiled, while 3) seizures of ivory at the US border do not change. Secondary results show that poverty does stimulate poaching and that increases in legal trade mask a growing illegal trade.

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1 Introduction

The illegal wildlife trade encompasses an astonishing array of products, such as endangered orchids, shark fins, and tigers. However, it is best known for ivory—the product that has inspired everything from investigative documentaries to international legislation. Worth $8-10 billion annually, the trade has a close relationship with terrorism, human trafficking, narcotics, and weapons (United Nations Office on Drugs and Crime, 2014). And, it is pushing elephants to extinction. Elephant populations are declining at a rate of 8% a year—a significant loss for ecosystems across Africa and Asia, as well as for humans around the globe (Chase et al., 2016).

Given the rapidly unfolding consequences of ivory trafficking, it is critical for economists, policymakers, and conservationists to figure out solutions. Of course, the first step in developing a solution is to understand the problem. Because ivory is a black market good, the underlying mechanisms that allow the market to operate are hidden. Supply and demand structures are relatively unknown, and so are the factors that influence them.

Fortunately, economists can take advantage of quasi-natural experiments to gain insights into this trade. When an event disrupts the usual patterns of the ivory trade, there may be a shock to the number of illegally killed elephants and border seizures of ivory. If these changes occur after the event and only in countries affected by the event, then the event can shed light on how the illegal ivory trade works. Traditionally, economists have focused on ivory trade bans to better understand how white markets influence black markets (Bulte and van Kooten, 1999). However, there is much more to explore.

In this paper, I examine the effect of trade openness on the illegal ivory trade. More specifically, I test whether entering into a Trade and Investment Framework Agreement (TIFA) affects the number of elephants poached and border seizures per year. Because TIFAs aim to promote trade between the United States and other nations (U.S. Office of Trade Representative, 2016), they provide an abrupt shift in trade openness that allows for a quasi-natural experiment. After a trade agreement is in place, the increase in trade openness may create a supply shock in the ivory

market by changing the cost of smuggling in one of two ways: either increased scrutiny at the U.S. border would make ivory confiscation more likely and therefore make smuggling more costly, or the influx of legal trade would overwhelm Customs and allow more ivory to slip through, thereby decreasing the cost of smuggling. The increase in trade openness could also motivate more poaching. If the ivory-exporting country introduces more ivory into U.S. black markets, crime syndicates may expect that these increased ivory flows would enhance long-run U.S. demand for ivory. To meet this demand, poachers would need to increase their rates of poaching and augment their ivory stockpiles. I empirically analyze the effects of the TIFAs on ivory poaching and smuggling using a difference-in-differences estimation. Moreover, I use an original test to examine how stockpiling behavior changes. No previous studies have theoretically or empirically addressed these related questions.

When policymakers understand the role of trade openness in the illegal ivory trade, they can then design trade agreements that reduce poaching and trafficking. If trade openness deters the illegal ivory trade, then trade agreements may be an effective way to cut down on illegal ivory. However, my finding that trade openness increases poaching suggests that policymakers should consider providing additional conservation aid to accompany these agreements. I also find that the surplus of ivory is at least partially stockpiled, which indicates that policymakers should consider new ways to prevent stockpiling and to enforce laws against it. By investigating the connection between trade openness and the illegal ivory trade, this paper provides new information on how this illicit trade functions and how we can best halt it.

The next section of this paper will provide a more in-depth background on the illegal ivory trade and discuss the literature that motivates this study. Section 3 describes the data used, and Section 4 explains the methodological framework, results, and robustness checks. Finally, Section 5 further discusses the results and concludes with policy recommendations.

2 Background and Literature Review

Despite the significance of the illegal ivory trade, the relevant economics literature has notable gaps. This literature review discusses these gaps and how they have been addressed in the past—a history that has informed the knowledge we have and the tools available for moving forward. I first give an overview of the literature's focus on trade bans and how that connects to the questions raised in this paper. I then assess the data and methodology used to examine the illegal ivory trade. Finally, I delve into some recent literature that adds to the context of this study.

Most of the literature on the illegal ivory trade focuses on what happens when a country imposes or lifts a trade ban on ivory. Lifting a trade ban has two potential outcomes: it could foster a white market that displaces the black market, or the legal supply could mask a growing illegal trade. The trade ban literature focuses on determining which of these effects is stronger. As this literature began to develop, the nature of the effects of a trade ban remained ambiguous or inconclusive, often due to necessary but limiting assumptions[1]. Given this uncertainty, researchers and conservationists advocated for the precautionary principle, arguing that trade bans would be the safest option for elephant populations (Bulte and de Kooten, 1999). Years later, researchers expanded their studies to include the nuances of illegal trade. These new analyses—both theoretical and empirical—incorporated consumer stigma (Fischer, 2004), laundering (Fischer, 2004), shifting supply costs (Fischer (2004); Hsiang and Sekar (2016)), perceived rarity of endangered species (Hall et al., 2008), and shipping costs (Moyle, 2014). When considering these non-traditional factors, it is clear that trade bans drive down demand for illegal ivory and make supplying it more difficult. Most recently and most convincingly, a one-year partial ivory legalization experiment in China has spurred the illegal ivory trade (Hsiang and Sekar, 2016). During this experiment, the influx of legal ivory disguised the flow of illegal ivory, which enabled increased smuggling—a consequence of lifting trade bans

[1]These include the assumption that changing the quantity of ivory in circulation will not affect prices (Bulte and van Kooten, 1999), that an ivory speculator has no competing stockpilers (Bulte et al., 2003), and that the effect of law enforcement on poaching does not change after a trade ban is implemented (Heltberg, 2001).

that is now largely accepted.

Evidently, the illegal ivory trade is dynamic; demand changes with consumer perceptions of ivory, and supply responds to a variety of costs and opportunities. This dynamism suggests that, like legalization, other trade policies could alter the volume of smuggled ivory as well. This is especially true of policies encouraging an influx of goods, which could then mask a parallel flow of illegal goods. One example of this is Trade and Investment Framework Agreements (TIFAs), which aim to increase trade opportunities among countries. However, no economics literature has addressed the effect of trade openness on the illegal ivory trade. If trade agreements are analogous to lifting trade bans, then the increased magnitude of trade should allow more illegal ivory to slip through Customs. Outside of the economics literature, researchers have found that trade openness enhances states' abilities to intervene in drug trafficking within drug-producing countries, but not within drug-consuming countries (Bartilow and Eom, 2009). Although ivory and drugs have significant differences, the possibility that trade openness can motivate increased scrutiny of and intervention in trafficking remains intriguing.

Moreover, trade openness may impact concealment and evasion costs within the ivory supply chain (Basu, 2014). As these costs change, smuggling behavior would most likely change as well. For example, if trade openness creates an influx of legal trade that overwhelms U.S. Customs and Border Patrol, the U.S. may not be able to inspect every shipment as thoroughly as it once could. This decreases the cost of evasion and makes smuggling cheaper, thereby incentivizing further shipments of ivory. More concretely, Hsiang and Sekar (2016) share an anecdote about smuggling behavior after China's 2008 partial legalization experiment. Sanctioned shipments of ivory had ID cards that denoted their status as legal, but the ID cards were sold on a secondary market for shipments of illegal ivory. The cost of buying an ID card was offset by the profits from the increased smuggling success, which the ID cards all but guaranteed. So, this decrease in concealment costs facilitated the illegal ivory trade.

The option to stockpile complicates this picture. Crime syndicates have the option to sell or stockpile ivory. If the syndicates expect ivory prices to rise, they

will stockpile in order to increase their future profits (Kremer and Morcom, 2000). In fact, they may even subsidize poachers, speculating that elephant extinction will make their stockpiles more valuable (Bulte et al., 2003). Stockpiling also increases as global interest rates drop, an insight that, based on the predictions of Kremer and Morcom (2000), Moyle (2014) verifies using the three-year interest rate. Thus, even if trade agreements do not immediately increase the volume of illegal trade, they may lead to greater stockpiles to be used in future trade, and these stockpiles should grow most at a time of decreasing interest rates. When examining the impact of trade openness, this timing mismatch between the agreement and its effect is critical to understand.

As the content of this research expanded, the data and methodology available to study the illegal ivory trade have also evolved. Most of the early literature on trade bans is theoretical; after all, data on black markets do not usually exist. This theoretical work largely involves bioeconomic modeling, which takes into account not only the decisions of economic actors but also wildlife responses, such as rates of population growth. In the field's seminal paper, Kremer and Morcom (2000) create an open-access resource model for storable goods. Unlike models that do not allow for storage (Bulte and de Kooten, 1999), this model introduces more realism into the study of ivory: anyone can harvest ivory, and, because ivory is durable, the harvesters can factor stockpiling into their long-term decision-making. Using this model, the authors prove that survival and extinction equilibria exist for elephant populations. They conclude that governments can coordinate the survival equilibrium by implementing harsh antipoaching enforcement if they are credible, or by selling off government stockpiles to lower ivory prices and deter poaching if they are not credible. From this bioeconomic model, I utilize the insight that crime syndicates stockpile when they anticipate that ivory will be more valuable in the future. In their model, the value of ivory increases as it becomes increasingly scarce. I posit that crime syndicates also stockpile when they expect to encounter increased confiscations at the U.S. border. Rather than subjecting their ivory supply to these confiscations, they wait to release ivory from their stockpiles until they have a better chance of successfully smuggling the ivory.

However, the reliance of bioeconomic models on an extensive set of assumptions—such as poacher responsiveness to ivory prices and how elephant populations change over time—makes the results difficult to generalize. In the next wave of bioeconomic modeling, researchers use ecological data for the models' parameters. These numerical examples and simulations, however, often encounter methodological shortcomings.[2] More recently, researchers have been utilizing ecological data in empirical work. Hsiang and Sekar (2016) use the Proportion of Illegally Killed Elephants data as a proxy for poaching rates. Their work is grounded in the past theoretical work discussed above, but its data-driven results on China's partial legalization have been much needed. Empirical work is currently at the forefront of this research, and it appears to be the most promising route forward.

Although some characterize the illegal ivory trade as chaotic or driven by individual decisions—such as the argument that poaching is a direct product of poverty, which makes people desperate for any source of income (Missios, 2004)—I focus on the highly organized, rational behavior of crime syndicates (Basu, 2014). The actors who create the infrastructure of the illegal ivory trade and determine its volume are the ones who take trade policy into account. They may be able to recruit poachers based on the individual poachers' circumstances, but analyzing this mechanism is outside of the scope of this paper. Instead, I will control for poverty as one factor in poaching.

Over the past two decades, economists studying the illegal ivory trade have made significant advances. This progress, however, is far from complete. Recent data and methodology make this an exciting time to unearth new insights. The recognition of non-traditional factors in illegal trade, especially on the supply side, is paving the way for more creative thought. And, the gravity of the problem calls for a deeper understanding of these issues. Assessing the impact of trade agreements on the illegal ivory trade is the next step in building upon this literature, and its unique contribution will illuminate the inner workings of this illicit market for economists

[2]For example, Burton (1999) identifies a simulation that relies on data that only exists when profits are positive. When using the same model to simulate a period of negative profits, the resulting effect of profit on poaching effort may be distorted.

and conservationists alike.

3 Data

The original dataset used in this analysis is a compilation of data from an ivory wildlife seizure database, ecological monitoring data, and traditional economic sources. Because this study is investigating the impact of Trade and Investment Framework Agreements on the volume of poaching and smuggling, the countries used and time-frame developed reflect the overlap among trade, poaching, and smuggling data. The dataset thus covers the years from 2002 to 2010 for Kenya, Tanzania, Uganda, Mozambique, Thailand, Indonesia, and the Lao People's Democratic Republic.[3] To find out when each Trade and Investment Framework Agreement was signed, I located a scan of each original document, which states the day, month, and year of signing, on the U.S. Office of Trade Representative website.

More specifically, the Convention on International Trade in Endangered Species of Wild Flora and Fauna (CITES) has a public database on all trade in endangered species (Convention on International Trade in Endangered Species of Wild Flora and Fauna, 2016). From this database, I use the panel data on ivory seizures at the U.S. border. This data ranges from 1975 to 2010, and the ivory is measured in individual tusks, carvings, scraps, and pieces. I assume that, after controlling for the U.S. Customs budget, the ivory seized reflects a constant proportion of the ivory that gets through the U.S. border undetected, and so the ivory seized is a proxy for ivory smuggled. CITES has a sub-program called the Monitoring of the Illegal Killing of Elephants (MIKE), and they produce a database on poached elephants (Monitoring of the Killing of Elephants, 2016). From this panel data, which ranges from 2002 to 2015, I take the number of illegal carcasses found per year until 2010. This information was collected during ecological monitoring. Out of the countries recorded in each of these databases, I chose the seven that entered into TIFAs during the period that both databases were collecting information, resulting in the country

[3]When I run my analysis without countries that have 2008 TIFAs, the countries included are Mozambique, Thailand, Indonesia, and the Lao People's Democratic Republic.

selection noted above. The dependent variables for the main analysis are the ivory seized at the U.S. border and the number of elephants illegally killed. The ivory seized variable encompasses three measures: the number of ivory pieces seized per year, the number of seizure incidents per year, and whether or not there was a seizure in a given year.

The other data sources provide the necessary covariates. From the World Bank's World Development Indicators, I am using imports as a percentage of U.S GDP and exports as a percentage of the GDPs of each ivory-exporting country, which are both cross-sectional data (World Bank, 2016). From the U.S. Customs and Border Protection (CBP) website, I am using their budget statistics (budget per year in U.S. dollars) for years 1990 to 2015 (U.S. Customs and Border Patrol, 2016). The U.S. Customs budget controls for any potential changes in scrutiny at the U.S. border that could affect the number of ivory seizures. From the Federal Reserve Bank in St. Louis, I am using the 3-Month London Interbank Offered Rate (LIBOR), based on the U.S. Dollar (LIBOR, 2016). Because engagement in poaching often results from a lack of other options for obtaining income (Barrett and Arcese, 1998), I control for poverty using GDP per capita from the World Bank.

I do not have ivory prices, which means that I cannot account for changes to the price of ivory in motivating poaching or stockpiling. However, most ivory prices quoted in news articles or investigations are in dollars per kilogram. This is useful as a rough estimate, but much of the ivory from the CITES data is measured in carvings. The added value of the artistry would not be reflected in these estimates, which could skew the results. Reliable and nuanced ivory prices, as well as other currently unknown data, would thus be beneficial for future research.

4 Methodology and Results

4.1 Difference-in-Differences Estimation

My primary methodological framework is a difference-in-differences estimation using a panel regression with synthetic controls. The difference-in-differences estimation

tests how the number of elephants poached and ivory seizures change for a country that has entered into a TIFA in the time period after they sign the TIFA. Because these countries have no appropriate counterparts that would experience parallel trends, I create a synthetic country for each actual country using Stata's *synth* package (Abadie et al., 2010). The only other option for a control group is to choose an existing country, which would experience different conservation regulations, poaching enforcement, political stability, and involvement with crime syndicates. Without this desired consistency across countries, the assumption of parallel trends does not hold, and synthetic countries must take the place of actual countries.

Synthetic controls were introduced in 2010 (Abadie et al., 2010). Other researchers have since extended the methodology to allow for multiple treated observations that are treated in different time periods (Pieters et al. (2016); Kreif et al. (2016)). The variables for each synthetic country are weighted vectors of pre-intervention data. The countries that exist in both the CITES and MIKE data, excluding the ones that enter into an agreement during the same year as the synthetic country being developed, make up the "donor pool", or the repository of data that can be used to create the weighted vector. The synthetic variables include all variables that vary by country: seized ivory, illegally killed elephants, exports, and GDP per capita. I merge the rest—U.S. imports, the Customs budget, and Libor—into the synthetic data by year. This process creates seven control countries, one for each actual country, that expand the dataset to a total of fourteen countries.[4]

Additionally, I generate several variables needed to run the difference-in-differences analysis. The first, $treat_i$, indicates whether a Trade and Investment Framework Agreement (TIFA) is in place between the U.S. and each exporting country i. All non-synthetic variables are treated, and all synthetic controls are not treated. The second, $after_t$, indicates whether an observation takes place after the TIFA is signed (the post-intervention period). For example, if a country enters into a TIFA in 2005, all observations for that country and its synthetic control are marked as within the

[4]The most accurate synthetic controls—the ones that best fit the pre-intervention data of the treated groups—should have a root mean squared prediction error (measuring the fit between the data and the synthetic controls) less than three (Pieters et al., 2016); 48.81% of my synthetic controls meet this standard.

after period for all years after 2005. $Treatafter_{i,t}$ serves as an interaction term for countries that are part of the agreement after it is in place. Similarly, two variations on these variables, $afteron_t$ and $treatafteron_{i,t}$ include the year of the TIFA in the post-intervention period, and I use these as robustness checks.

I perform this analysis for the number of elephants poached and the three measures of ivory seizures according to this regression equation:

$$X_{i,t} = B_0 + B_1 Year_t + B_2 treat_i + B_3 after_t + B_4 treatafter_{i,t} + B_{5,\ldots,10} controls_{i,t} + \alpha_i + e_{i,t}$$

$X_{i,t}$ denotes elephants poached, number of ivory pieces seized, number of seizure incidents, or whether or not a seizure occurred in a given year. In each regression, the coefficient of interest is on $treatafter_{i,t}$, which shows the impact of increased trade openness for treated countries in the post-intervention period. For the Ivory Seizures regression, a significant increase in this coefficient would indicate that entering into a trade agreement motivates either increased scrutiny at the U.S. border or increased smuggling, assuming that ivory seizures are a constant proportion of ivory smuggled given the Customs budget. A positive coefficient would indicate increased poaching for the Elephants Poached regression.

Of course, these coefficients must also be interpreted in light of one another, shifting markets, and stockpiling effects in order to piece together a coherent narrative, as shown in Chart One. All panel regressions are OLS regressions with country fixed effects, α_i. The only exception is the regression for whether or not there was an ivory seizure in a given year, which is a logit panel regression used to accommodate the binary nature of the dependent variable.

My main results utilize a model that excludes countries with 2008 TIFAs, without an outlier observation, using all years after (not including) the TIFA year as the post-intervention period, and without clustered standard errors. These factors do not meaningfully change the results, as discussed in the robustness section.

Chart 1: Example Scenario Interpretations				
Elephants Poached	Ivory Seizures	Shifting Markets	Stockpiling	Interpretation
Increase	Increase	No change	No change	Trade openness motivates an increase in poaching, and the surplus ivory is caught in transit.
Increase	No change	Away from U.S.	No change	Trade openness motivates an increase in poaching, and the surplus ivory circulates outside of the U.S.
Increase	No change	No change	Increase	Trade openness motivates an increase in poaching, but the surplus ivory is stockpiled and not entered into circulation immediately.
No change	Increase	No change	Decrease	Trade openness motivates a release of ivory from stockpiles.
Decrease	No change	No change	No change	Trade openness motivates an increase in scrutiny at the U.S. border.
Decrease	Increase	Toward U.S.	No change	Trade openness motivates a shift in ivory toward the U.S. market.

After a country enters into a Trade and Investment Framework Agreement with the U.S., 8.4 more elephants are poached in that country each year relative to countries that did not enter into the agreement. This is statistically significant at the 1% level. However, the number of ivory pieces seized, the number of seizure incidents, and whether or not a seizure occurred is not significantly different than before the agreement or relative to a country not in the agreement. The increase in poaching without an increase in seizures lends itself to several interpretations: the surplus of ivory is easier to smuggle because the influx of legal trade masks the increase in illegal trade; the surplus of ivory is being re-routed to other countries; or the surplus of ivory is being stockpiled.

Table 1: Results

	(1) Elephants Poached	(2) Ivory Pieces Seized	(3) Number of Ivory Seizures	(4) Whether a Seizure Occurred
Year	0.33	-82.25	-0.25	-0.37
	(1.52)	(75.98)	(0.19)	(0.54)
After	-6.66	135.07	-0.35	1.62
	(4.34)	(216.79)	(0.55)	(1.47)
Treatafter	8.42***	146.34	-0.18	-0.95
	(3.10)	(154.81)	(0.40)	(1.32)
GDP/capita	-0.004**	0.09	0.0003	0.003**
	(0.00)	(0.08)	(0.00)	(0.00)
%Imports	0.06	50.09	0.24**	0.84**
	(0.93)	(46.28)	(0.12)	(0.36)
%Exports	0.16	-7.90	0.05***	0.42**
	(0.12)	(6.19)	(0.02)	(0.17)
Customs	0.000006	0.0001	0.0000009	-0.0000008
	(0.00)	(0.00)	(0.00)	(0.00)
constant	-681.53	164080.84	486.33	
	(3028.15)	(151429.51)	(383.80)	
r2	.15	.06	.12	
N	72.00	72.00	72.00	72.00

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1 also shows that a one dollar increase in GDP per capita causes a poaching decrease of .004 elephants per year, matching the literature's prediction that alleviating poverty makes people less likely to poach. Also, a 1% increase in U.S. imports as a percentage of U.S. GDP increases the number of ivory seizures by .24 seizures per year and increases the chance of a seizure occurring by 21.1%. As exports as a percentage of the exporting country's GDP rise by 1%, the chance of a seizure occurring increases by 10.5%. So, when the volume of legal trade between an ivory-exporting country and the U.S. increases, the U.S. may not catch significantly more pieces of ivory, but it does perform more seizures. However, entering into a Trade and Investment Framework Agreement does not significantly change the number of these seizures or the chances of being caught relative to comparable non-TIFA countries. These secondary results show that poverty does stimulate poaching and that increases in legal trade mask a growing illegal trade, which leads to increased seizures.

4.2 Shifting Markets

Because elephant poaching is increasing without additional seizures, there must be a surplus of ivory not entering the U.S. It is possible that the extra ivory is shifting markets (being disproportionately funneled away from the U.S). In addition to my main regressions, I test for shifting markets using an OLS panel regression. I construct a ratio of U.S. imports to non-U.S. imports[5] in which all imports come from the exporting countries used in the main regressions. None of these countries are synthetic, and the country fixed effects are for the exporting countries. This ratio tests for changes in the United States' share of illegal ivory imports over time:

$$U.S.ShareIvoryImports_{i,t} = B_0 + B_1 Year_t + B_2 After_t + B_3 CustomsBudget_t + \alpha_i + e_{i,t}$$

The ratio of U.S. imports of ivory to non-U.S. imports of ivory approaches zero as the U.S. share of the market declines, is one when they are equal, and is greater than one when the U.S. imports more than the rest of the world combined. If this ratio is significantly smaller after a Trade and Investment Framework Agreement, as represented by the coefficient on $After_t$, then the decrease suggests that illegal ivory is shifting to other markets. This is plausibly due to an expected increase in scrutiny at the U.S. border. Moreover, this regression takes into account the U.S. Customs budget as a proxy for changes to U.S. scrutiny so that these funding changes are not conflated with other changes in ivory seizures. I re-run the regression using $Afteron_t$, a variable that includes the year of the TIFA in the post-intervention period, and the results are comparable.

There is no evidence to suggest that the surplus of ivory is being re-routed away from the U.S. toward other countries. Unfortunately, China, the largest ivory market and most problematic country given its 2008 partial legalization experiment, has not reported most of its seizures to the CITES database. Using only ivory-exporting countries that did not have TIFAs in 2008 helps to remedy this lack of information.

[5]The non-U.S. countries are the United Arab Emirates, Australia, Denmark, Spain, the United Kingdom, Luxembourg, the Netherlands, Norway, New Zealand, Portugal, Qatar, Sweden, and South Africa.

Ultimately, without more accurate data on China, this analysis must be considered incomplete.

Table 2: Shifting Markets Results

	(1) Ratio	(2) Ratio
Year	0.025	0.006
	(0.07)	(0.07)
After	-0.137	
	(0.21)	
Customs	0.0000002	0.00000004
	(0.00)	(0.00)
Afteron		0.156
		(0.21)
constant	-50.966	-12.577
	(136.41)	(141.64)
r2	.07	.068
N	63.00	63.00

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable for both columns is the ratio of U.S. ivory seizures to non-U.S. ivory seizures. Column 1 excludes the TIFA year in the post-intervention period, and Column 2 includes the TIFA year in the post-intervention period.

4.3 Stockpiling

Because the surplus of ivory does not appear to shift markets, it is possible that the exporting countries stockpile the ivory. To test for this, I construct a ratio of tusks imported into the U.S. to non-tusk ivory imported into the U.S. All of the imports are from the exporting countries used in the main regressions, and I do not use any synthetic controls. Because tusks make up the vast majority of illegal ivory stockpiles (Sas-Rolfes et al., 2014), an increase in stockpiling—or the retention of tusks in exporting countries—should result in a decrease in the share of tusks crossing the U.S. border relative to carvings or other non-tusk items. To estimate the effect of trade openness on the share of tusks coming into the U.S., I run the

following regression with country fixed effects:

$$ShareOfTusks_{i,t} = B_0 + B_1 Year_t + B_2 After_t + B_3 VolumeOfIvoryImports_{i,t} + \alpha_i + e_{i,t}$$

If the number of tusks decreases relative to the non-tusk supply of ivory during the post-intervention period as represented by $After_t$, then countries are stockpiling their raw ivory. This regression includes the total volume of ivory imports as a control for whether an increase in tusks simply reflects a greater number of seizures in general.

The stockpiling analysis shows a .17 decrease in the ratio in the post-intervention period. So, out of every 100 ivory imports, the number of tusks seized by the U.S. drops by 17 after a country signs a trade agreement. This decline in raw ivory suggests that tusks are being stockpiled within the exporting countries.

Table 3: Stockpiling Results

Year	0.03*
	(0.01)
Volume of Imports	0.00002
	(0.00)
After	-0.17**
	(0.08)
constant	-58.75*
	(29.65)
r2	0.08
N	63.00

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the ratio of tusks to non-tusk items.

As corroboration, I also look at the three-year interest rate, which is known to negatively correlate with stockpiling. Libor alone is not enough to determine stockpiling, but it can still support this analysis. Moyle (2014) empirically confirmed the relationship between stockpiling and the interest rate. By comparing the quantity of raw ivory approved for sale to China to the actual state-sanctioned carving of raw ivory in China, Moyle creates a rough measure of stockpiling. This measure, as

theoretically predicted by Kremer and Morcom (2000), negatively correlates with the three-year interest rate. I look to this relationship as validation of my results; if stockpiling tests match the expected correlation, then the results will have a more convincing place within the literature. The general decrease in the three-year global interest rate after 2005 supports the idea that stockpiling is occurring during the post-intervention period.

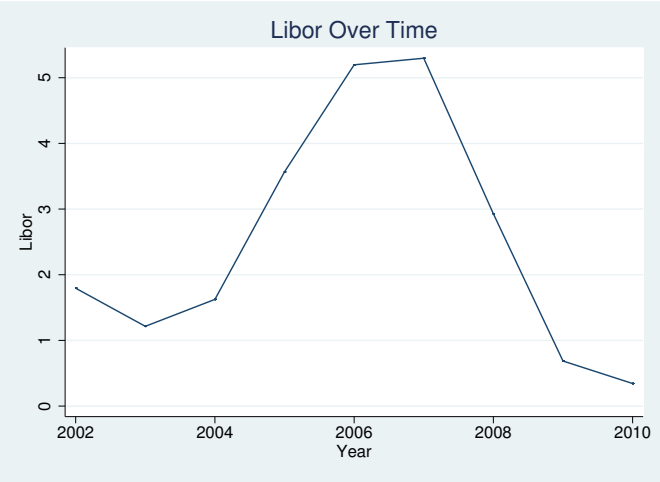

Figure 1: Libor Over Time

The shifting markets and stockpiling results support the conclusion that, after trade openness motivates increased poaching, the surplus ivory is not smuggled through U.S. Customs, caught at the U.S. border, or exported to other countries but held within the exporting country in a stockpile.

4.4 Robustness

To ensure the robustness of these results, I have performed several robustness checks. I run variations on the main regressions to address data, methodological, and conceptual concerns. In 2007, the U.S. imported over 3000 ivory pieces from Indonesia—an outlier that could skew the ivory seizure results. I run the regression for ivory pieces seized with and without the outlier to see whether the outlier is driving the results.

The poaching data extends through 2015, so I am able to run the poaching analysis with these extra years and examine longer-term effects. Because each TIFA occurs during the middle of the year, I run each regression with the post-intervention period including and excluding the year of the agreement. I also test models with and without clustered standard errors.

The outlier observation does not change the significance of the number of ivory pieces seized (see Table 5), but it does flip the difference-in-differences estimator from positive to negative. However, neither of these results are significantly different than zero, and so the conclusions remain the same.

Including the year of the TIFA in the post-intervention period does not significantly affect the poaching result (see Table 6). However, it does add significance to the number of ivory seizures (.596 at the 5% significance level) and whether a seizure occurred (1.486 at the 10% significance level). Unfortunately, because I do not have monthly data, it is impossible to tell whether these increases happened after the implementation of the TIFA or whether they anticipated the TIFA. And, even if the increased trade openness drove these results, a .596 yearly increase in number of seizures could not fully account for the additional 8 elephants poached. Without month-level data or more substantial evidence, using the years after the TIFA as the post-intervention period continues to be the most reliable path.

Extending the poaching data through 2015 (see Table 7) increases the number of elephants killed per year by 20 more than the results from the 2010 data. This result is significant at the 1% level. A country that entered into a trade agreement has 28 more elephants poached each year than an analogous country that did not enter into an agreement. Without the data extension for ivory seizures, however, it is unclear how this ivory is circulated, and so the primary results cannot speak to the larger ramifications of this increase in poaching.

Clustering standard errors results in a loss of significance for all results (see Table 8). Clustering standard errors fixes the problem of having standard errors that correlate within groups (in this case, by country) but not between groups. Having a small number of clusters, however, can lead to over-rejection of the null hypothesis. Because this study has fourteen clusters at most, clustering standard errors may not

actually be an appropriate robustness check.

Most importantly, I perform these analyses with and without the countries subject to 2008 TIFAs. China's 2008 partial ivory legalization experiment is likely to put upward pressure on poaching results for countries whose post-intervention periods perfectly align with this policy. China is not well represented in the CITES seizures database, and so statistically controlling for this change is not an option. Removing the three countries with 2008 TIFAs decreases the sample size, but the results are more accurate. Whether including or excluding the 2008 TIFA countries (See Table 4), all coefficients remain equally significant and retain their direction. The magnitude of the difference-in-differences estimator for Elephants Poached shrinks, but this most likely reflects the upward pressure that China's ivory legalization exerted on poaching during 2008. Because of China's apparent influence, all other models used exclude the 2008 TIFA countries.

5 Discussion and Conclusion

This study shows that trade openness motivates increased poaching and suggests that the surplus of ivory is stockpiled rather than entered into circulation. It also provides support for the idea that poverty encourages poaching and that an increase in legal trade can mask a growing illegal trade. Without additional insights into the strategies of poachers and crime syndicates, the reasoning behind this stockpiling remains an open question. If the crime syndicates expect increased scrutiny due to the trade agreement, then they would not want to waste their ivory supply by shipping it, knowing that the chances of confiscation are higher. They would maximize their profits by waiting until there is an opportunity for safer smuggling—and perhaps until the value of ivory increases due to its future scarcity. Verifying this potential explanation would require further research and analysis.

However, there must be a reason that trade openness spurs poaching even though the ivory cannot be immediately sold. Because the stockpiling analysis does not indicate how much ivory is being stockpiled versus successfully smuggled, it is also possible that greater quantities of ivory are slipping through the U.S. border unde-

tected. If the increased legal trade masks this increased ivory smuggling–even when taking into account the additional stockpiling—then there is less support for the idea that trade agreements lead to increased scrutiny.

The main caveat of these results is data reliability. Because much of this data deals with black markets, it is bound to be incomplete. For example, controlling for the Customs budget may indirectly measure scrutiny, but it cannot take into account the bribery that is rampant in the illegal ivory trade. Similarly, shifting markets and stockpiling are unobservable, but the tests in Subsection 4.3 come closer to observing this behavior than any literature has so far.

As ivory data continues to improve, researchers will be better able to understand illegal ivory markets and reach more nuanced conclusions. The recent surge of empirical data and methodology over the past fifteen years has paved the way for this analysis, but more accurate and more comprehensive data will reduce the ambiguity of future studies' conclusions. Even with these data limitations, new approaches, such as the stockpiling ratio analysis that this paper originated, can shed light on illicit activity.

Under certain conditions, these results are generalizable to other black markets. Other durable, storable, non-consumable goods with limited production (or reproduction) include weapons and timber. For example, countries that experience illegal logging may see deforestation intensify after signing a trade agreement, or trade openness may make weapons trafficking more successful. And, the new methodology for estimating stockpiling is generalizable to any good that is traded in multiple forms, of which a subset is more frequently stockpiled than the other forms.

This link between trade and poaching indicates that policymakers should consider pro-conservation amendments or built-in conservation aid to improve trade agreements between the US and ivory-exporting countries. If these conservation policies help to deter poaching, potentially by providing alternate sources of income while enhancing the enforcement necessary to find illegal stockpiles and the people who organize them, then they could be used to offset the effects of trade openness. And, better Customs training or increased Customs funding could further mitigate problems that arise from an overwhelming influx of legal (and illegal) trade.

Poaching has put elephant populations at risk of extinction, and the criminal activity behind ivory trafficking is a threat to humans as well. Understanding how the illegal ivory trade works can help economists, policymakers, and conservationists to create the policies necessary to dismantle this trade. Understanding the relationship between trade openness and the illegal ivory trade is an important step forward; however, it is only one facet of a complex and largely mysterious trade. Across disciplines, more work is needed to fully grasp the systems, structures, and strategies that allow the illegal ivory trade to continue to thrive.

6 References

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7 Appendix

Table 4: Results With and Without 2008 TIFAs

	(1) Elephants Poached	(2) Ivory Pieces Seized	(3) Number of Ivory Seizures	(4) Whether a Seizure Occurred	(5) Elephants Poached	(6) Ivory Pieces Seized	(7) Number of Ivory Seizures	(8) Whether a Seizure Occurred
main								
Year	3.80	-61.34	-0.16	-0.48	0.33	-82.25	-0.25	-0.37
	(2.74)	(43.95)	(0.13)	(0.33)	(1.52)	(75.98)	(0.19)	(0.54)
After	-17.64**	152.42	-0.28	0.97	-6.66	135.07	-0.35	1.62
	(7.78)	(112.65)	(0.34)	(0.92)	(4.34)	(216.79)	(0.55)	(1.47)
Treatafter	29.99***	100.37	0.13	0.48	8.42***	146.34	-0.18	-0.95
	(7.61)	(98.04)	(0.29)	(0.86)	(3.10)	(154.81)	(0.40)	(1.32)
GDP/capita	-0.01**	0.08	0.0003*	0.001	-0.004**	0.09	0.0003	0.003**
	(0.01)	(0.06)	(0.00)	(0.00)	(0.00)	(0.08)	(0.00)	(0.00)
%Imports	-0.98	48.73*	0.07	0.43**	0.06	50.09	0.24**	0.84**
	(1.69)	(26.95)	(0.08)	(0.21)	(0.93)	(46.28)	(0.12)	(0.36)
%Exports	-0.27	-5.53	0.04***	0.18**	0.16	-7.90	0.05***	0.42**
	(0.49)	(3.70)	(0.01)	(0.08)	(0.12)	(6.19)	(0.02)	(0.17)
Customs	0.000003	0.00006	0.0000007	0.000001	0.000006	0.0001	0.0000009	-0.0000008
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
constant	-7591.86	122254.38	312.68		-681.53	164080.84	486.33	
	(5468.43)	(87592.54)	(258.68)		(3028.15)	(151429.51)	(383.80)	
r2	.24	.05	.04		.15	.06	.12	
N	126.00	126.00	126.00	126.00	72.00	72.00	72.00	72.00

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1-4 include years that signed TIFAs in 2008. Columns 5-8 exclude these countries.

Table 5: Ivory Pieces Seized With and Without Outlier

	(1) With Outlier	(2) Without Outlier
Year	-52.46	2.37
	(54.97)	(5.60)
After	128.37	-12.31
	(136.94)	(14.70)
Treatafter	174.30	-1.11
	(178.86)	(13.07)
GDP/capita	-0.07	0.002
	(0.09)	(0.01)
%Imports	51.18	0.75
	(50.40)	(3.46)
%Exports	-4.41	0.05
	(5.50)	(0.52)
Customs	0.00008	-0.000007
	(0.00)	(0.00)
constant	104466.38	-4739.02
	(109440.03)	(11170.21)
r2	0.06	.01
N	126.00	125.00

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable for each regression is the number of ivory pieces seized. Column 1 includes Indonesia's 2007 outlier observation, and column 2 excludes this observation.

Table 6: Results Including and Excluding the TIFA Year in the Post-Intervention Period

	(1) Elephants Poached	(2) Ivory Pieces Seized	(3) Number of Ivory Seizures	(4) Whether a Seizure Occurred	(5) Elephants Poached	(6) Ivory Pieces Seized	(7) Number of Ivory Seizures	(8) Whether a Seizure Occurred
main								
Year	3.62	2.37	-0.16	-0.48	2.78	2.58	-0.18	-0.34
	(2.78)	(5.60)	(0.13)	(0.33)	(2.60)	(5.47)	(0.13)	(0.33)
After	-17.24**	-12.31	-0.28	0.97				
	(7.86)	(14.70)	(0.34)	(0.92)				
Treatafter	30.44***	-1.11	0.13	0.48				
	(7.70)	(13.07)	(0.29)	(0.86)				
GDP/capita	-0.01**	0.002	0.0003*	0.001	-0.01**	0.0004	0.0002	0.0008
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
%Imports	-0.84	0.75	0.07	0.43**	-0.76	2.25	0.08	0.32*
	(1.73)	(3.46)	(0.08)	(0.21)	(1.56)	(3.26)	(0.07)	(0.19)
%Exports	-0.26	0.05	0.04***	0.18**	-0.57	0.17	0.04***	0.19**
	(0.49)	(0.52)	(0.01)	(0.08)	(0.48)	(0.52)	(0.01)	(0.08)
Customs	0.000004	-0.000007	0.0000007	0.000001	-0.0000006	-0.000003	0.0000006	0.000002
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Afteron					-8.06	-20.71	-0.16	-0.74
					(7.17)	(13.98)	(0.32)	(0.81)
Treatafteron					35.52***	1.40	0.60**	1.49*
					(6.93)	(11.49)	(0.25)	(0.83)
constant	-7217.00	-4739.02	312.68		-5518.74	-5189.73	357.24	
	(5540.99)	(11170.21)	(258.68)		(5191.14)	(10907.36)	(250.02)	
r2	.24	.01	.04		.32	.02	.11	
N	125	125	125	125	125	125	125	125

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1-4 exclude the year of the TIFA in the post-intervention variables $After_t$ and $Treatafter_{i,t}$. Columns 5-8 include the year of the TIFA in the post intervention variables $Afteron_t$ and $Treatafteron_{i,t}$.

Table 7: Poaching Analysis Including Years Through 2015

	(1) Elephants Poached (With 2008 TIFAs)	(2) Elephants Poached (Without 2008 TIFAs)
Year	-4.06	0.63
	(2.73)	(1.82)
After	-26.42**	-22.80***
	(11.00)	(7.06)
Treatafter	67.06***	28.58***
	(10.23)	(6.88)
GDP/capita	-0.03***	-0.01***
	(0.01)	(0.00)
%Imports	4.27*	1.19
	(2.24)	(1.53)
%Exports	-0.33	0.23
	(0.43)	(0.21)
Customs	0.00004***	0.00001*
	(0.00)	(0.00)
constant	8046.05	-1291.65
	(5446.85)	(3635.83)
r2	.34	.31
N	196.00	112.00

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 2 extend the data years through 2015 for Elephants Poached, the dependent variable. Column 1 includes TIFAs signed in 2008, and Column 2 excludes these TIFAs.

Table 8: Results With Clustered Standard Errors

	(1) Elephants Poached	(2) Ivory Pieces Seized	(3) Number of Ivory Seizures	(4) Whether a Seizure Occurred
main				
Year	0.696	-63.92	-0.228	-0.371
	(0.380)	(70.57)	(0.210)	(0.543)
After	-7.755	116.1	-0.342	1.617
	(7.772)	(132.3)	(0.816)	(1.472)
Treatafter	8.878	298.5	-0.382	-0.955
	(5.292)	(328.4)	(0.455)	(1.322)
GDP/capita	-0.00767	-0.129	0.000117	0.00329*
	(0.00519)	(0.174)	(0.000274)	(0.00138)
%Imports	0.126	48.68	0.255	0.844*
	(0.324)	(50.26)	(0.186)	(0.358)
%Exports	0.163	-6.063	0.0412	0.420*
	(0.253)	(5.882)	(0.0353)	(0.165)
Customs	0.00000719	0.000138	0.00000101	-0.000000773
	(0.00000770)	(0.000172)	(0.00000150)	(0.00000246)
constant	-1407.4	127498.7	450.8	
	(771.6)	(140631.2)	(417.0)	
r2	0.170	0.0811	0.131	
N	72	72	72	72

Notes: Standard errors are in parentheses. Significance levels are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1-3 are panel regressions with country fixed effects and clustered standard errors. Column 4 is a panel logit regression without fixed effects and clustered standard errors.