

# Illegal Markets and Contemporary Slavery: Evidence from the Mahogany Trade in the Amazon\*

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**VERY PRELIMINARY - PLEASE DO NOT CIRCULATE**

## Abstract

In this paper, we revisit an episode of a market transition from legal to illegal and investigate how illegal markets could affect modern slavery. We exploit the Brazilian government's complete shutdown of the mahogany market in the late 90's using a quasi-experimental research design exploiting the natural variation in the occurrence of mahogany trees in Brazilian municipalities as a treatment indicator. Taking advantage of new administrative data on labor inspections from the Brazilian Ministry of Labor, we find that the shutdown of the mahogany market sharply increased the probability of labor inspections found slave labor in affected municipalities. To deal with the possibility that police actions could have been coordinated to attack large companies right after the law change, we rely on the fact that 100% of police operations in the locations in the sample resulted from local complaints rather than police investigations. Our results are robust to several robustness exercises.

**JEL codes:** J47, K32, K42.

**Key-words:** Modern slavery, deforestation, illegal markets

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# 1 Introduction

In 1948, the Universal Declaration of Human Rights determined that “slavery and the slave trade shall be prohibited in all their forms.”. Even though, in the 21st century, several countries have registered the now-called “contemporary forms of slavery” and struggle to combat forced labor. The International Labor Organization estimates that around 21 million people worldwide have their Human Rights violated and are coerced - often through the use of violence - to carry out forced labor. These workers are generally in a vulnerable situation, living in poor regions, and are recruited to work in sectors usually associated with illegal activities, such as lumber mills and charcoal mines. These illegal markets are often associated with the use of violence and much less security for workers. The most recent evidence on the effects of market illegality shows that it led to a higher increase in violence ([Chimeli and Soares, 2017](#)). However, there is little empirical evidence on how market illegality could change work relations and to what extent it can increase forced labor.

There are two main reasons illegal markets are more prone to using slave labor than legal ones. The first clear reason is that it is virtually impossible for an agent on an illegal market to formally employ anyone. Therefore, agents in this market resort to informal labor, where there are no institutions mediating employer-employee relationships. Since deforestation activities tend to occur in rural, poor, and isolated sites, workers generally have no bargaining power to set favorable work conditions. A second reason is that modern slavery often is based on violence and life threats, where perpetrators hold enslaved individuals as prisoners ([Sakamoto, 2006](#)). In addition to the empirical evidence that the shutdown of the mahogany market increased homicides found by [Chimeli and Soares \(2017\)](#), the authors present plenty of anecdotal evidence on how violence is used in the illegal mahogany market and how mahogany loggers have access to illegal weapons and often threaten any resistance to the logging. That is, in a market already familiar with the use of violence, the cost of committing another crime is much smaller than in a legal and regulated market.

In this paper, we revisit an episode of a market transition from legal to illegal and investigate how illegality affects modern slavery. We exploit the complete shutdown of the mahogany market by the Brazilian government in the late 90’s using a quasi-experimental research design that takes advantage of the natural variation in the occurrence of mahogany trees in Brazilian municipalities as a treatment indicator. Our difference-in-differences estimates show that the transition of the mahogany market to illegal increased the probability of labor inspections finding slave labor in municipalities inside the mahogany area. Event-study specifications reassure that the treatment variables are not capturing only distinct preexisting dynamics of slave labor in mahogany regions. Our results are robust to different specifications and alternative measures of contemporary slavery, enhancing the reliability of the baseline estimates. Importantly, to deal with the possibility that labor inspections could have been coordinated to target large companies right after the law change, we rely on the fact that all of the police operations in the sample resulted from local complaints rather than police investigations/intelligence. Moreover, we show that the effects on the number of enslaved workers start to increase two years before the effect on the number of labor inspections. This suggests that we are measuring changes in slavery prevalence rather than just changes in enforcement.

The paper proceeds as follows. Section 2 provides institutional context on slave labor

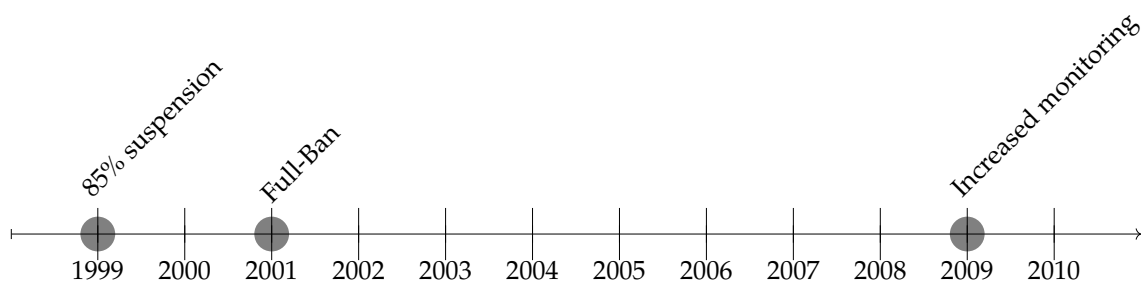
and the mahogany market in Brazil. The conceptual framework is presented in Section 3. Section 4 presents the data and discusses our definition of treatment and control municipalities. Section 5 presents our empirical strategy to estimate the effect of the transition of the mahogany market to illegal on contemporary slavery. Section 6 presents our main results, possible threats to identification, robustness checks, and the magnitude and interpretation of the results. Section 7 summarizes our findings and concludes.

## 2 Background

### 2.1 Mahogany market in Brazil

As Mahogany occurs mostly in the Amazon region, before the shutdown of the legal mahogany market in 2001, Brazil was one of its greater exporters. The country was responsible for 41% of all mahogany imports from 1989 to 2001 to the United States, according to the US Department of Agriculture. The intense exploration and risk of extinction raised concerns among environmentalists pushing for regulating the mahogany market in the country. In 1994, the government established that exporters should obtain a license and present detailed forest management plans before extracting the trees. The first regulation was followed by an establishment of export quotas in 1998 and by the creation of a working group to audit forest management plans, which eventually led to a suspension of 85% of all mahogany management plans in 1999. In 2001, the government determined the prohibition of mahogany extraction, transportation, and domestic or international trade, completely dissolving its legal market. Finally, in 2009, the Brazilian government sharply increased illegal deforestation monitoring, restricting access to rural credit in properties where illegal deforestation was detected. As shown by Assunção et al. (2020), this policy was responsible for a sharp reduction in deforestation from 2009 to 2011. Figure 1 presents a timeline of the main mahogany regulations and policy changes since 1999.

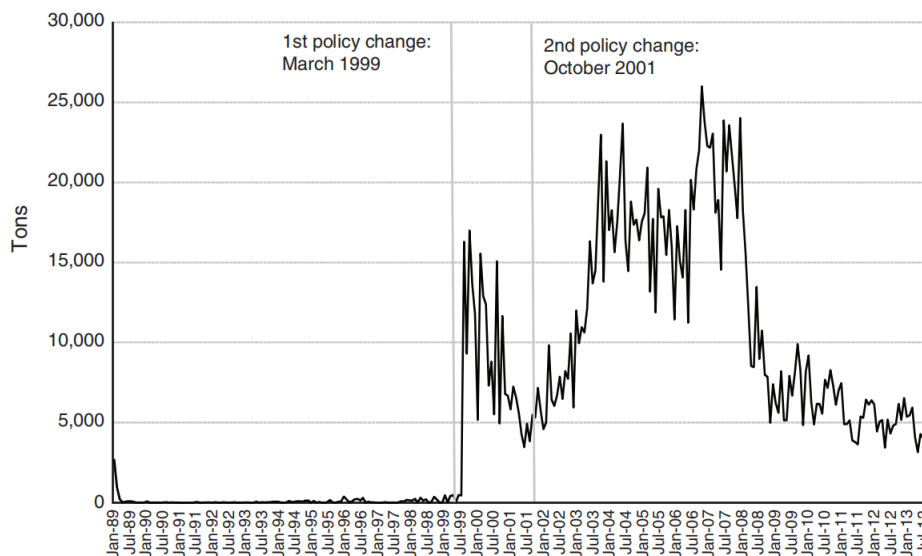
Figure 1: Time line of Mahogany Regulation



Despite becoming illegal, there is much evidence that the mahogany trade didn't stop after the prohibition. Chimeli and Soares (2017) shows how just after the ban, the exports of "other tropical timber species" increased in the country's total exports from almost zero, to the same level as the mahogany exports before the prohibition. That is, the exporters were able to relabel their products bypassing the fragile system of inspection at the ports, and exporting mahogany as other types of wood (Barreto et al., 2001; Blundell and Rodan, 2003; Chimeli and Boyd, 2010). In Figure A.2 we show the time trends of exports of "other tropical

timber species” before and after the shutdown of the legal mahogany market. The evidence in Figure A.2 is very appealing and shows how exports of “other timber tropical species” rose sharply from almost zero tons to 15 thousand tons just after the 85% suspension ban of 1999, and rose again just after the Full-Ban in 2001.

Figure 2: Trends in exports of “other timber species” before and after mahogany bans



Notes. Trends in total exports of “other timber species” constructed by [Chimeli and Soares \(2017\)](#).

Operating illegally, the mahogany market, as other illegal ones such as the drug market ([Reuter, 2009](#)), began to incorporate violence, the use of weapons, and other intimidation strategies in the now illegal logging areas. Indeed, using a difference-in-differences research design [Chimeli and Soares \(2017\)](#) documents a sharp increase in homicide rates in the mahogany area after the shutdown of the legal market.

## 2.2 Forced Labor in Brazil

Although several countries have abolished slavery, the use of forced labor is still recurrent, especially in developing regions. Thousands of people, mainly in rural areas, are still coerced to work by the use of violence or intimidation. According to the ILO Forced Labour Convention, 1930 (No. 29), forced or compulsory labor is: “all work or service which is exacted from any person under the threat of a penalty and for which the person has not offered himself or herself voluntarily.”

In Brazil, the crime of submitting someone to forced labor is covered by the Brazilian Penal Code since 1940, which establishes a penalty of 2 to 8 years in prison. However, there are few cases of slave labor discovered before the 1990s. With the disclosure of cases in the media by NGOs, such as the Pastoral Land Commission (CPT), there was increased investigation by the public authorities ([Costa and to Combat Forced Labour, 2009](#); [Phillips and Sakamoto, 2012](#)). In 1995, the Special Mobile Inspection Group (GEFM) was created by the Ministry of Labor and Employment. It works jointly with Federal Police agents,

prosecutors from the Public Ministry of Labor (MPT), representatives from the Brazilian Institute for the Environment and Renewable Natural Resources (Ibama), and the National Institute for Colonization and Agrarian Reform (Incra). The GEFM was internationally recognized as a good practice in the fight against slave labor, bringing together different institutions with the purpose of jointly monitoring complaints of forced labor.

Integration with environmental entities (i.e. Ibama and Incra) occurs in large part because most complaints of slave labor occur in rural regions. Since many of the workers are hired to clear native forests, the crime of subjecting these individuals to conditions akin to slavery is frequently accompanied by environmental crimes. One such crime is the clearing of the thick vegetation that recurs in areas that have been previously cleared of trees and converted to pasture. This is due to the dynamics of the crime of forced labor. *Gatos*, or "cats" in English, are the estate owners' contractors who approach vulnerable workers and offer them a contract to work in agricultural facilities typically located far from their cities of origin. The worker is forced to buy groceries and other needs for his family's maintenance "on credit," usually at prices higher than market rates. The worker is also charged for his tools, lodging, and food at the job site. Then, as a perverse "protection" against the induced debt, the employer starts withholding the workers' documents and, in some cases, forbidding them from leaving by using armed guards (Sakamoto, 2006; Costa and to Combat Forced Labour, 2009).

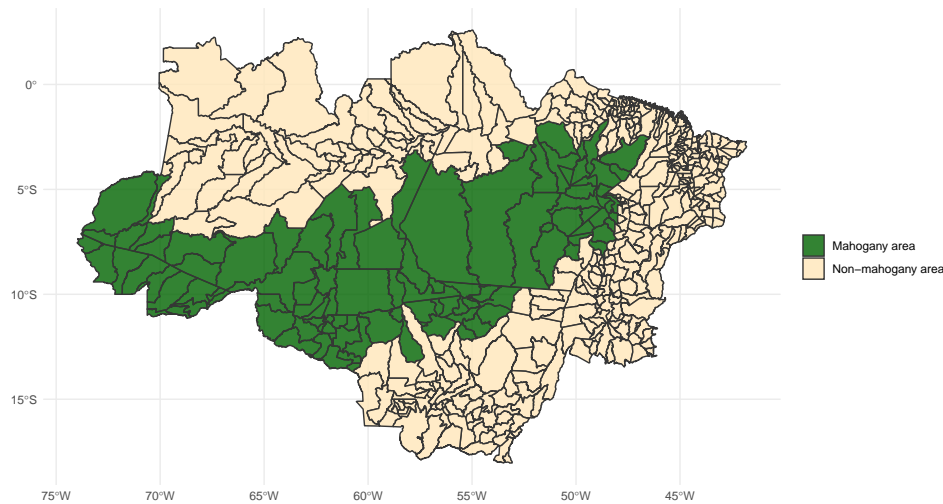
### 3 Conceptual Framework

As discussed in the previous section, many of the workers rescued from slave conditions were working in deforestation-related activities in rural areas, whether legal or illegal. This is not a particularity of Brazil, rather, this pattern emerges in several countries (Jackson and Sparks, 2020; Jackson et al., 2020), increasing the relevance of our study.

To facilitate the rest of the paper, in this section, we discuss and hypothesize the main reasons why an illegal deforestation market tends to rely more on slave labor than legal ones. The first clear reason is that it is virtually impossible for an agent on an illegal market to formally employ anyone. Therefore, agents in this market resort to informal labor, where there are no unions or government institutions mediating employer-employee relationships. Since deforestation activities tend to occur in rural, poor, and isolated sites, workers generally have no bargaining power to set favorable work conditions. A second reason is that modern slavery often is based on violence and life threats, where criminals hold enslaved individuals as prisoners (Sakamoto, 2006). In addition to the empirical evidence that the shutdown of the mahogany market increased homicides found by Chimeli and Soares (2017), the authors present plenty of anecdotal evidence on how violence is used in the illegal mahogany market and how mahogany loggers have access to illegal weapons and often threaten any resistance to the logging, making set for the exploitation of slave labor.

In summary, the context of illegal markets is usually much more violent, and the cost of committing another crime such as slavery is smaller for illegal agents. Naturally, this kind of violence is much less likely to happen in a legal and regulated environment.

Figure 3: Mahogany Municipalities



Notes. This figure plot the spatial distribution of the natural occurrence of Mahogany trees using [Chimeli and Soares \(2017\)](#) data.

## 4 Data

### 4.1 Mahogany Data

The data on the natural occurrence of Mahogany trees were obtained by [Chimeli and Soares \(2017\)](#). A municipality is considered treated if there is an intersection between its area and the area of the natural occurrence of mahogany. We also get baseline covariates for municipalities in our sample, such as population, GDP, and mortality indicators. The spatial distribution of "mahogany-municipalities" is presented in Figure A.1. Note that our sample includes only municipalities on the Legal Amazon to increase the comparability of treated and non-treated municipalities.

### 4.2 Forced Labor Data

The Ministry of Labor and Employment is the primary branch of the federal government in charge of inspecting for violations of labor laws. Data on labor inspections since 1995 were obtained directly from the Ministry using the Law of Access to Information. The data contains detailed information on labor inspections, such as the date of the inspection; the municipality; its nature (i.e., whether it was based on anonymous complaints or Ministry intelligence work); the number of workers freed in each inspection; their respective occupations; and information on the firms that were investigated. This data is our primary indicator of forced labor in Brazilian municipalities. Finally, we were able to obtain information on individual characteristics from the Ministry of Labor and Employment.

## 5 Empirical Strategy

To assess the impact of the ban on Mahogany trade on contemporary slavery, we exploit the natural variation of the presence of mahogany trees and the timing of the policy changes in a difference-in-difference (DD) research design. Note that in our study, all treated units are treated at the same time, so our estimates do not suffer from the drawbacks of DD estimators recently highlighted in the literature (Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021). Our DD specification is described in equation 1:

$$\begin{aligned} Forced\ Labor_{mst} = & \alpha + \beta_1 \times (D_{1999 \leq t \leq 2001} \times Mahog\ Area_m) \\ & + \beta_2 \times (D_{2002 \leq t \leq 2008} \times Mahog\ Area_m) \\ & + \beta_3 \times (D_{t \geq 2009} \times Mahog\ Area_m) + \theta_m + \gamma_{st} + \varepsilon_{mt} \end{aligned} \quad (1)$$

Where  $Forced\ Labor_{mst}$  is a dummy variable that equals to one if at least one slave had been freed in municipality  $m$ , state  $s$ , and year  $t$ .  $Mahog\ Area_m$  is a dummy variable that equals to one if the municipality  $m$  is under the mahogany area. Our parameters of interest are  $\beta_1, \beta_2$ , and  $\beta_3$ , which, under the parallel trends assumption, measures the impact of each of the three mahogany bans on our measure of forced labor.  $\theta_m$  are a set of municipality fixed-effects, while  $\gamma_{st}$  represent state-by-year dummies.  $\varepsilon_{mst}$  is an error term, clustered at the municipality-level (following Bertrand et al. (2004)). All regressions are weighted by population since small municipalities have noisy data. To check if treatment and control municipalities followed similar trends in contemporary slavery before the law change, we also estimate an event study specification as described in equation 2.

$$\begin{aligned} Forced\ Labor_{mst} = & \sum_{\tau=1995}^{1997} \beta_{\tau} Mahog\ Area_m \times I(\tau = t) + \sum_{z=1999}^{2013} \beta_z Mahog\ Area_m \times I(z = t) \\ & + \theta_m + \gamma_{st} + \varepsilon_{mst} \end{aligned} \quad (2)$$

Note that Equation 2 is a more flexible version of equation 1 that allows to empirically test the common trends assumption. The main difference now is that we have as many parameters of interest as years (minus one) in our panel. If the parallel trends assumption holds, we should expect that the coefficients for all  $\beta_{\tau}$  are not statistically different from zero.  $\beta_z$  coefficients measures the effect of the mahogany ban on contemporary slavery for each post-treatment year.

## 6 Results

### 6.1 Main Results

Our main results are presented in Table 1. Each column of Table 1 shows the estimated coefficients from a different specification of equation 1 where the dependent variable is a dummy that equals one if at least one SMIG labor inspection found slave labor in the municipality in a given year, as explained in section 5. Our main specification is presented in

column (2) and includes municipality fixed-effects and state-by-year fixed-effects. Our results show that all restriction policies on the mahogany market have statistically significant and positive effects on the probability of a labor inspection finding slave labor in affected municipalities. As shown in Table 1, results are robust across different specifications. Also, as shown in Table A.3 in the Appendix, results are also robust using unweighted regressions.

Table 1: Illegality of Mahogany Trade and Modern Slavery Indicator

	Modern Slavery Indicator			
	DD	DD	DD	Triple Diff. 1
	(1)	(2)	(3)	(4)
1st Policy (1999-2000) (Mahogany 85% Suspension)	0.0369** [0.0161]	0.0707*** [0.0237]	0.0718*** [0.0238]	0.124*** [0.0464]
2nd Policy (2001-2008) (Mahogany Full-Ban)	0.184*** [0.0432]	0.298*** [0.0581]	0.300*** [0.0614]	0.557*** [0.119]
3rd Policy (2009-2013) (Plus Deforestation Monitoring)	0.201*** [0.0757]	0.198*** [0.0668]	0.225*** [0.0650]	0.527*** [0.118]
Observations	11,932	11,932	11,913	11,932
R-squared	0.308	0.383	0.414	0.389
Municipality & Year FE	Yes	Yes	Yes	Yes
State×Year FE	No	Yes	Yes	Yes
Baseline Charac.×Year FE	No	No	Yes	No
<b>Interac. of Main treat with:</b>				
State percent in exp. before 1999	No	No	No	Yes

*Notes.* Standard errors shown in parentheses are clustered at the Municipality level. Column one presents Municipality and year fixed-effects. Column two introduces State×Year fixed-effects. In column three, the baseline municipality characteristics include: percent of area planted, child mortality, assassinations related to land conflicts (rate), per capita GDP (ln), fraction of GDP in agriculture (the latter 2 measured in 1996), and these controls are interacted with year fixed effects. Finally, Columns(4-5) present Triple Difference estimations and interact the main treatment variables respectively with: State percent in exportation before 1999 and Suspect state exportation after 1999. Column two is our baseline specification. (\*\* $p \leq 0.01$ , \*\* $p \leq 0.05$ , \* $p \leq 0.1$ ).

Overall, estimates suggest that the stricter the regulation, the stronger the increase in contemporary slavery. The full-ban policy point estimate (column (2)) indicates an increase of 30 p.p., relative to the mean, on the probability of a labor inspection found slave labor, while the point estimate for the 85% suspension of management plans suggests an increase of 7 p.p., relative to the mean, on slave labor. That is, the more illegal the market becomes, the higher the incentive for the agents on that market to exploit slave labor. The

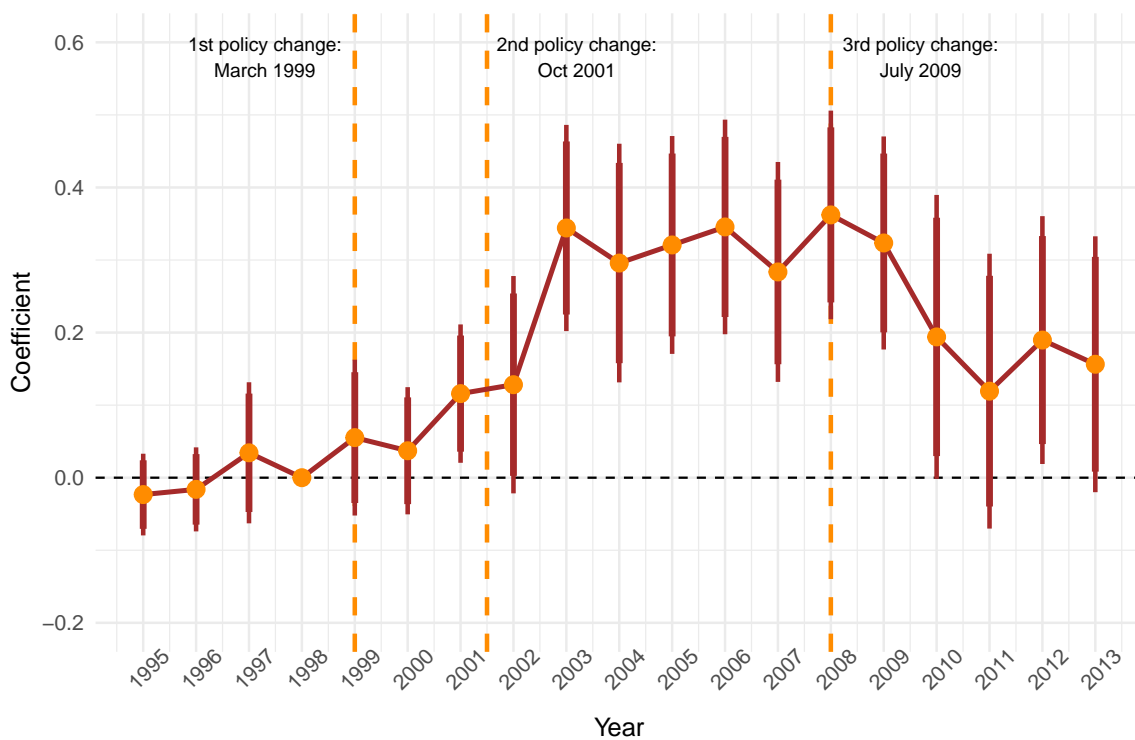


point estimate for the 2009 increase in deforestation satellite monitoring also has an impact on the probability of finding slave labor (22.5 p.p.). This effect is consistent with the fact that 100% of the labor inspections are based on denounces and joint operations with environmental regulatory agencies, implying that the more illegal deforestation is detected, the higher the chance of public agencies finding enslaved workers, leading to more denouncements and, consequently, to more labor inspections.

Finally, column (4) of Table 1 interacts our treatment indicators with a measure of mahogany activity before 1999 (the year where the first policy started). The idea of this triple-difference specification is to check if the effect of the policies are higher in regions where the mahogany exports were also higher before the first policy change. As expected, point estimates in column (4) suggest that the market size before prohibition and monitoring matters since bigger markets have more incentives to maintain the illegal activity after prohibition.

As discussed in section 5, the main assumption needed to interpret the results presented in Table 1 as the causal effect of illegal markets on contemporary slavery is the parallel trends assumption. In this study context, this means that municipalities inside and outside the mahogany area must have similar trends in our measure of contemporary slavery before the first policy change. To check if this assumption holds, Figure 4 plots the results from the estimation of the flexible event-study specification presented in equation 2. The coefficients from the interaction of the treatment indicator with the relative year dummies are plotted along with the 90% and 95% confidence intervals.

Figure 4: The effect of Mahogany prohibition on Modern Slavery



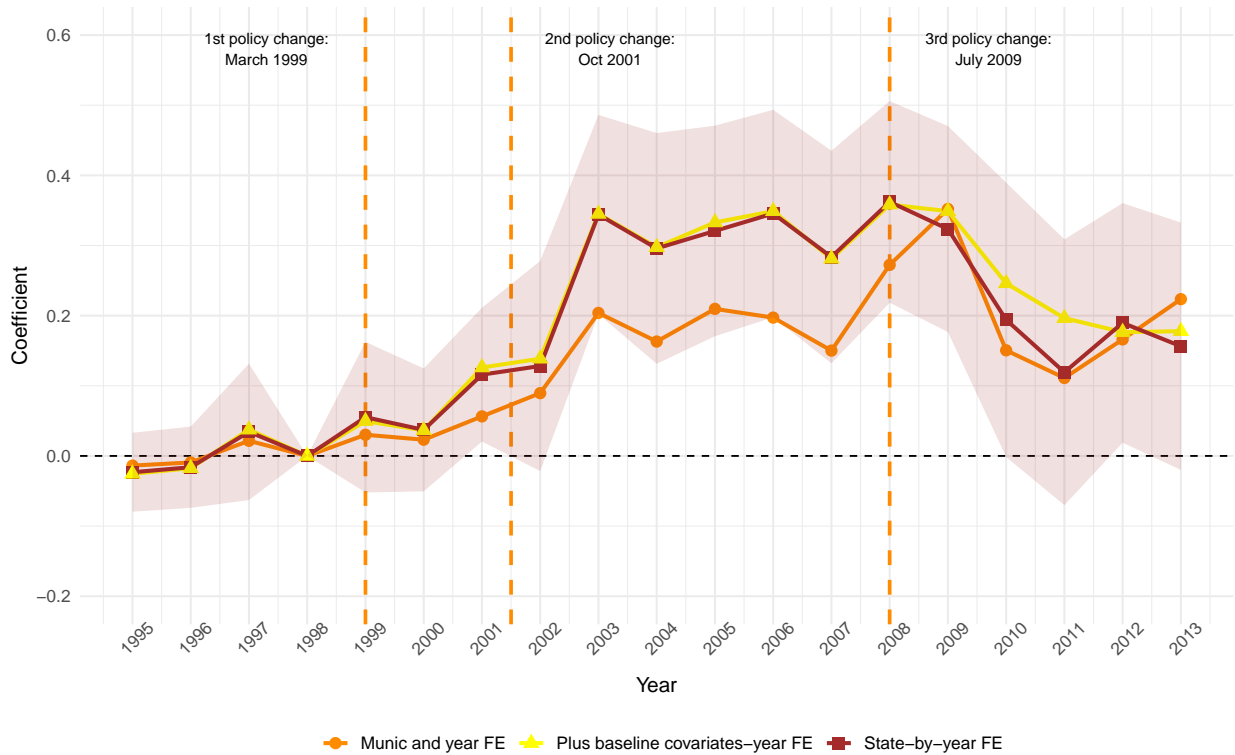
*Notes.* The figure plots the coefficients and 95% confidence intervals of the interaction term of the treatment indicator (being in the mahogany area) and yearly dummies from the regression specified in equation 2. The regression is weighted by the average population during the pre-treatment period. The dependent variable is a dummy variable that equals one if in that municipality-year at least one labor inspection found slave labor. Standard errors are clustered at the municipality level.

The results presented in Figure 4 are visually appealing and enhance the reliability of the findings in Table 1. The coefficients in the pre-treatment period are not statistically different from zero at conventional significant levels — the p-value from a Wald test on the joint nullity of pre-treatment coefficients is 0.246 — suggesting that the parallel trends assumption holds. Another advantage of this flexible specification is that it allows the researcher to see how the treatment effects evolve. The post-treatment coefficients are consistent with results in Table 1, becoming statistically significant just after the first policy change, where the higher coefficients correspond to the Full-Ban period.

## 6.2 Robustness Checks

In the previous section, we show how the transition of the mahogany market to illegal increased contemporary slavery in affected municipalities. This section presents results for several robustness exercises that aims to check the robustness of the main results.

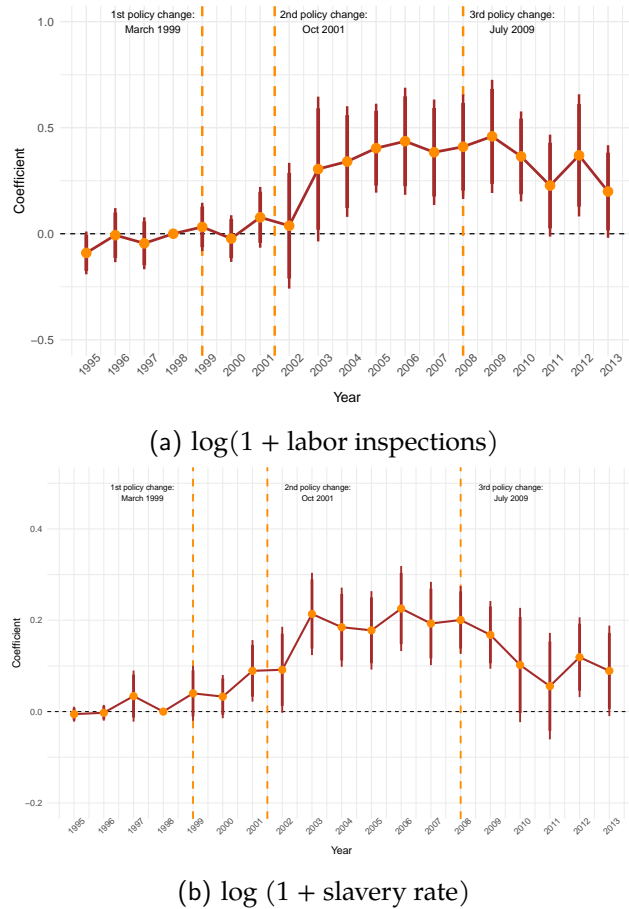
Figure 5: The effect of mahogany prohibition on Modern Slavery for different specifications



*Notes.* The figure plots the coefficients of the interaction term of the treatment indicator (being in the mahogany area) and yearly dummies from alternative specifications of equation 2. The shaded area corresponds to 95% confidence intervals of our baseline specification, that controls for state-by-year fixed-effects. The regression is weighted by the average population during the pre-treatment period. The dependent variable is a dummy variable that equals one if in that municipality-year at least one labor inspection found slave labor. Standard errors are clustered at the municipality level.

To check if our flexible event-study results are robust to different specifications, Figure 5 plots coefficients from the estimate of our baseline and alternative specifications of equation 2, controlling for the interaction of baseline covariates with time trends, and also only controlling for year and municipality fixed-effects. The baseline covariates used were the area of the municipality, the log of GDP per capita, the agriculture GDP and the number of political assassinations as a measure of violence. As can be seen in Figure 5, our event study results are robust to both two alternative specifications, which leads to coefficients inside the 95% confidence intervals boundary of our baseline results.

Figure 6: The effect of Mahogany prohibition on the number of labor inspections and Slavery Index



*Notes.* The figure plots the coefficients, and 95% confidence intervals of the interaction term of the treatment indicator (being in the mahogany area) and yearly dummies from the regression specified in equation 2. The regression is weighted by the average population on the pre-treatment period. The dependent variable in panel (a) is the log of one plus the number of labor inspections in municipality  $m$  and year  $t$ . Standard errors are clustered at the municipality level. The dependent variable in panel (b) is the log of one plus the ratio of the number of inspections that found slave labor to the total number of labor inspections in municipality  $m$  and year  $t$ . Standard errors are clustered at the municipality level.

Our measure of contemporary slavery is a dummy that equals one if a labor inspection in that year has freed at least one enslaved individual. In that sense, one can argue that post-treatment changes in the level of this variable might reflect an increase/decrease in the number of labor inspections rather than a variation in the use of slave labor. We adopt two strategies to check if this is the case. First we estimate our event-study specification using the log of one plus the number of labor inspections as the dependent variable, and display the results in panel (a) of Figure 6. As in Figure 4, results show no difference in trends in treatment and control municipalities before 1999's in the number of labor inspections. Meaningful differences between these two figures only appear after 1999. Whereas we estimate a positive and statistically significant effect of the mahogany regulation on slave labor after 2000, only in 2003/2004 we can detect a statistically significant effect on the number of labor inspections in treated municipalities. This timing difference suggests that at least

from 2000 to 2003, the turning of the mahogany market into illegal increased the probability of detecting slave labor even given the same level of labor inspections before 1999. Naturally, since 100% of labor inspections in our sample are determined by a denounce, after some point in time the number of labor inspections increased too — likely due to the consequential increase in the awareness of the authorities — which explains the statistically significant coefficients post 2003 for labor inspections, as can be seen Figure 6. Therefore, this exercise strongly suggests that we measure, indeed, slave labor rather than just changes in enforcement.

The second strategy is to build a new contemporary slavery variable to take into account enforcement levels. This is possible because not all labor inspections ended up founding slave labor. Indeed, in our sample of labor inspections, only 55% percent have freed at least one enslaved worker. Therefore we build an index that is equal to the ratio between the number of labor inspections that found slave labor in municipality  $m$  and year  $t$  and the total of labor inspections in that same municipality and year and used it as a dependent variable in our event-study specification. Results are displayed in panel (b) of Figure 6. As can be seen in Figure 6, results using this new variable are qualitatively identical to the ones using our Modern Slavery Indicator, suggesting that we are capturing an increase in forced labor, not just an increase in enforcement, enhancing the reliability of our baseline estimates.

Table 2: Mahogany Illegality and Forced Labor Outcomes

	Modern Slavery Indicator	Log (Modern Slavery)	Log (Enslaved Workers)	Labor inspection Indicator	Log (labor inspections)
	(1)	(2)	(3)	(4)	(5)
Placebo Periods (1995-1997)	-0.00168 [0.0331]	-0.00773 [0.153]	0.00475 [0.272]	-0.0280 [0.0494]	-0.174 [0.263]
1st Policy (1999-2000) (Mahogany 85% Suspension)	0.0694* [0.0360]	0.343* [0.176]	0.530* [0.313]	0.0591 [0.0416]	0.253 [0.227]
2nd Policy (2001-2008) (Mahogany Full-Ban)	0.297*** [0.0594]	1.613*** [0.328]	2.416*** [0.507]	0.217*** [0.0742]	1.262*** [0.425]
3rd Policy (2009-2013) (Plus Deforestation Monitoring)	0.197*** [0.0745]	1.052*** [0.377]	1.425*** [0.537]	0.239*** [0.0789]	1.332*** [0.430]
Observations	11,932	11,932	11,932	11,932	11,932
R-squared	0.383	0.403	0.389	0.373	0.398
Municipality & Year FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors shown in parentheses are clustered at the Municipality level. (\*\*\*)  $p \leq 0.01$ , (\*\*)  $p \leq 0.05$ , (\*)  $p \leq 0.1$

Our final exercise is an attempt to rule out the hypothesis that our results are driven by the definition of our dependent variable. Figure A.3 and Table 2 presents results for both the baseline and event-study like regressions using alternative measures of slave labor as dependent variables. As can be seen in Figure A.3, results are qualitative the same using the log of one plus the number of freed workers as the dependent variable. The same conclusion arises from analyzing the results displayed in Table 2, where column (1) shows our

baseline measure of modern slavery and each other column shows results from an alternative definition of the dependent variables used throughout the paper. Overall this exercise strongly indicates that our results are robust to different definitions of the dependent variables, enhancing the reliability of our baseline estimates. Finally, in Figure A.4 in the Appendix, we show that our estimates are robust to the exclusion of the 10 most populated municipalities, one-by-one, from the sample, ruling out that our results are driven by an outlier municipality.

## 7 Conclusion

In this paper, we revisit a unique episode of a market transition from legal to illegal and investigate how illegal markets could affect modern slavery. We exploit the complete shutdown of the mahogany market by the Brazilian government in the late 90's using a quasi-experimental research design using the natural variation in the occurrence of mahogany trees in Brazilian municipalities as a treatment indicator.

Our difference-in-differences estimates show that the transition of the mahogany market to illegal increased the probability of labor inspections finding slave labor in municipalities inside the mahogany area. Results hold taking account the number of slave operations, reinforcing that we are measuring changes in slavery prevalence rather than just changes in enforcement. Event-study specifications reassure that the treatment variables are not capturing only distinct preexisting dynamics of slave labor in mahogany regions. Our results are robust to different specifications and alternative measures of contemporary slavery, enhancing the reliability of the baseline estimates.

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# Online Appendix to “Illegal Markets and Contemporary Slavery: Evidence from the Mahogany Trade in the Amazon”

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<b>A Appendix</b>	<b>2</b>
A.1 Figures . . . . .	2
A.2 Tables . . . . .	6

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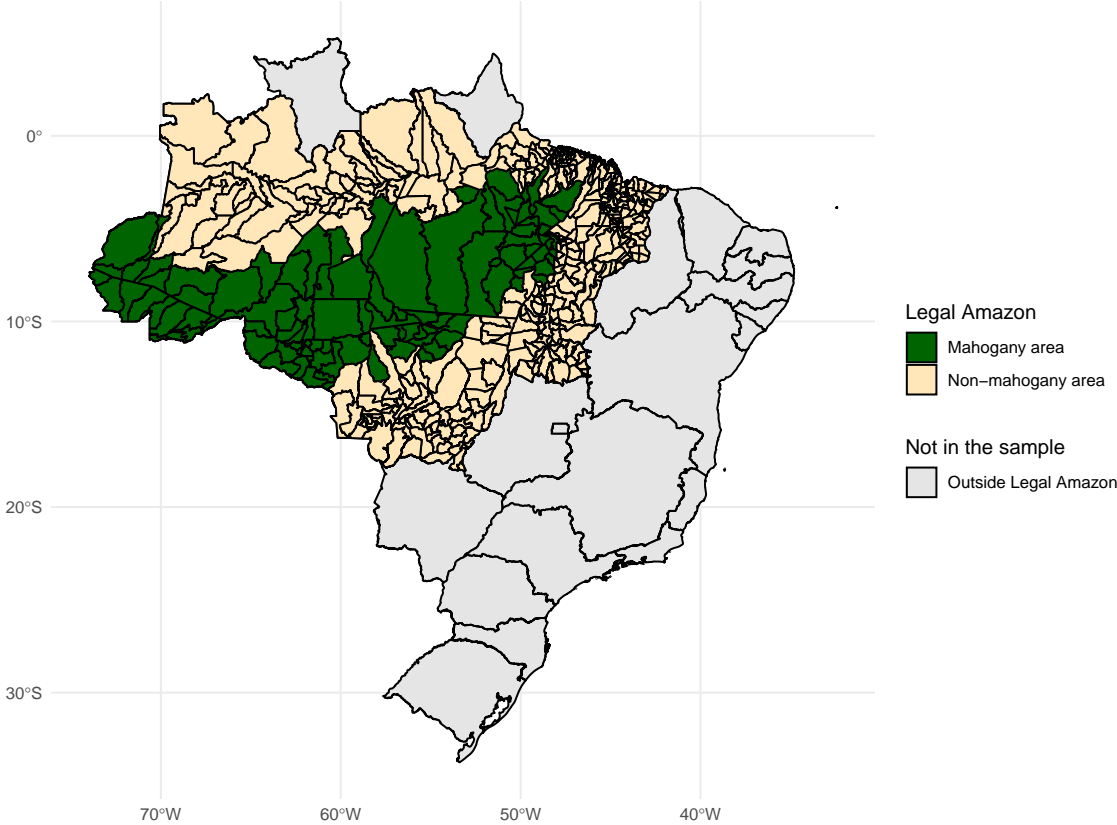
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# A Appendix

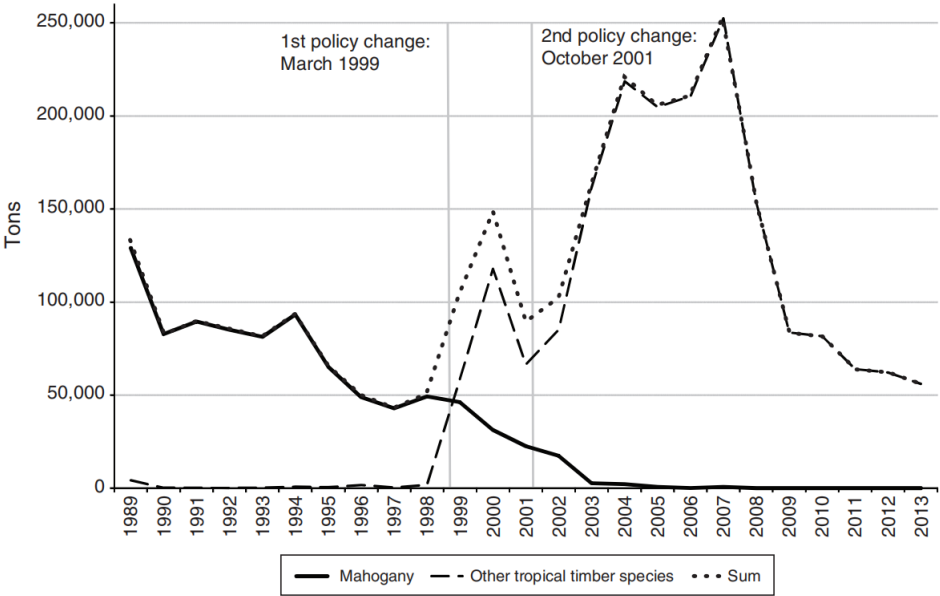
## A.1 Figures

Figure A.1: Municipalities in the Sample



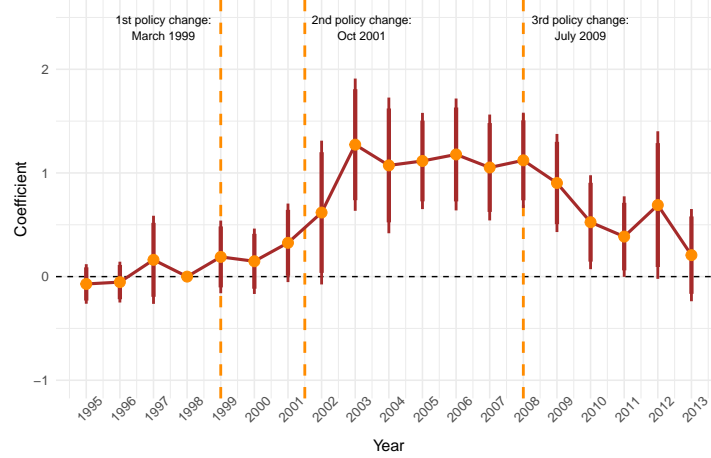
Notes. This figure plot all municipalities in our sample by treatment status.

Figure A.2: Trends in exports of mahogany and "other timber species"



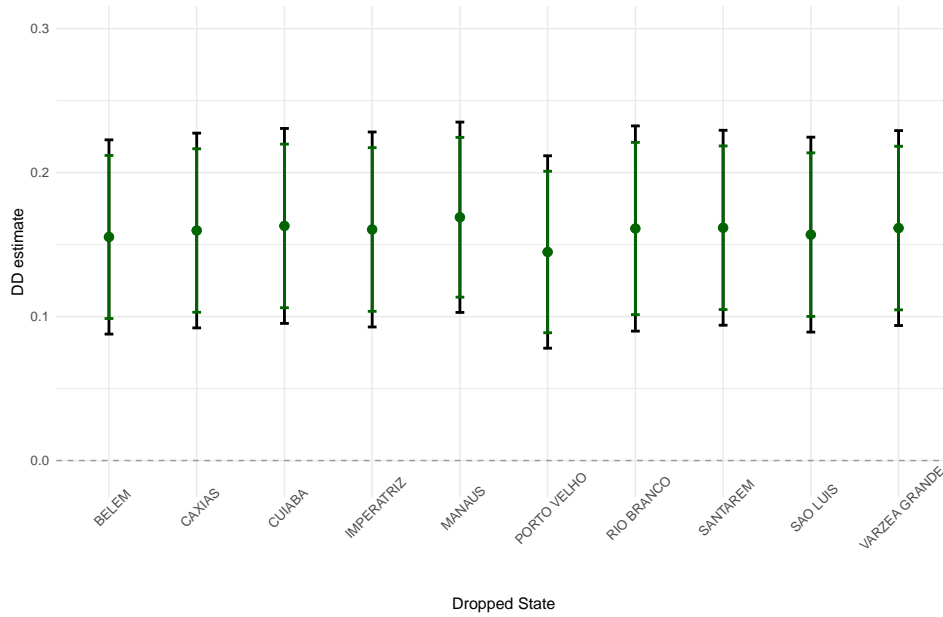
Notes. Trends in total exports of mahogany and "other timber species" constructed by [Chimeli and Soares \(2017\)](#).

Figure A.3: The effect of Mahogany prohibition on the number of freed workers



*Notes.* The figure plots the coefficients and 95% confidence intervals of the interaction term of the treatment indicator (being in the mahogany area) and yearly dummies from the regression specified in equation 2. The regression is weighted by the average population during the pre-treatment period. The dependent variable in the panel is the log of one plus the number of freed enslaved individuals in municipality  $m$  and year  $t$ . Standard errors are clustered at the municipality level.

Figure A.4: The effect of Mahogany prohibition on contemporary slavery -  
Leave-one-municipality-out



*Notes.* The figure plots the coefficients and 90% and 95% confidence intervals from difference-in-differences estimates described in 1, but grouping all policies in one post-treatment variable, for different sample restrictions, dropping, one-by-one, the 10 most populated municipalities from the sample. Standard errors shown in parentheses are clustered at Municipality level.

## A.2 Tables

Table A.1: Summary Statistics

<b>Panel A: Before 1999</b>					
Statistic	N	Mean	St. Dev.	Min	Max
Modern Slavery Indicator	3,140	0.007	0.083	0	1
# Labor Inspections	3,140	0.063	0.471	0	10
# Freed workers	3,140	0.483	8.073	0	220
Population	3,140	28,500.410	81,781.800	696	1,356,285
GDP per capita	1,256	2.353	2.337	0.289	31.209
Homicide rates	3,140	11.486	18.178	0.000	159.652
Pol. Deaths Rates	3,140	0.002	0.026	0.000	0.780
<b>Panel B: After 1999</b>					
Statistic	N	Mean	St. Dev.	Min	Max
Modern Slavery Indicator	8,792	0.064	0.245	0	1
# Labor Inspections	8,792	0.190	0.870	0	20
# Freed workers	8,792	2.446	21.849	0	1,113
Population	8,792	34,005.140	103,318.100	1,039	1,891,724
GDP per capita	6,908	3.721	3.902	0.641	62.183
Homicide rates	8,792	18.034	20.657	0.000	211.060
Pol. Deaths Rates	5,024	0.002	0.022	0.000	1.198

*Notes.* This table show summary statistics for the main variables mentioned in the paper. Panel A show statistics before the first mahogany ban, while Panel B shows statistics for the post treatment period.

Table A.2: The Effect of Mahogany-Bans on contemporary Slavery: DD estimates

	Modern Slavery Indicator			
	(1)	(2)	(3)	(4)
<b>Post</b>	0.160***	0.219***	0.162***	0.232***
	(0.034)	(0.044)	(0.034)	(0.045)
R <sup>2</sup>	0.300	0.374	0.348	0.405
Observations	11,932	11,932	11,913	11,913
Municipality FE	✓	✓	✓	✓
Year FE	✓		✓	✓
State-by-year FE		✓		✓
Baseline Charac. x Year FE			✓	✓

*Notes.* This table show results from difference-in-differences estimates described in 1, but grouping all policies in one post-treatment variable. Standard errors shown in parentheses are clustered at the Municipality level. Column one presents Municipality and year fixed-effects. Column two introduces State×Year fixed-effects. In column three, the baseline municipality characteristics include: percent of area planted, child mortality, assassinations related to land conflicts (rate), per capita GDP (ln), fraction of GDP in agriculture (the latter 2 measured in 1996), and these controls are interacted with year fixed effects. (\*\* $p \leq 0.01$ , \*\* $p \leq 0.05$ , \* $p \leq 0.1$ ).

Table A.3: Illegality of Mahogany Trade and Modern Slavery Indicator: Unweighted regressions

	Modern Slavery Indicator			
	DD	DD	DD	Triple Diff
	(1)	(2)	(3)	(4)
1st Policy (1999-2000) (Mahogany 85% Suspension)	0.0192** [0.00912]	0.0359*** [0.0133]	0.0353*** [0.0129]	0.105*** [0.0371]
2nd Policy (2001-2008) (Mahogany Full-Ban)	0.116*** [0.0193]	0.187*** [0.0255]	0.184*** [0.0256]	0.477*** [0.0588]
3rd Policy (2009-2013) (Plus Deforestation Monitoring)	0.111*** [0.0204]	0.150*** [0.0256]	0.145*** [0.0260]	0.437*** [0.0665]
Observations	11,932	11,932	11,913	11,932
R-squared	0.260	0.279	0.290	0.286
Municipality & Year FE	Yes	Yes	Yes	Yes
State×Year FE	No	Yes	Yes	Yes
Baseline Charac.×Year FE	No	No	Yes	No
<b>Interac. of Main treat with:</b>				
State percent in exp. before 1999	No	No	No	Yes

*Notes.* Unweighted estimates in all columns. Standard errors shown in parentheses are clustered at the Municipality level. Column one presents Municipality and year fixed-effects. Column two introduces State×Year fixed-effects. In column three, the baseline municipality characteristics include: percent of area planted, child mortality, assassinations related to land conflicts (rate), per capita GDP (ln), fraction of GDP in agriculture (the latter 2 measured in 1996), and these controls are interacted with year fixed effects. Finally, Columns(4-5) present Triple Difference estimations and interact the main treatment variables respectively with: State percent in exportation before 1999 and Suspect state exportation after 1999. Column two is our baseline specification. (\*\*\*)  $p \leq 0.01$ , (\*\*)  $p \leq 0.05$ , (\*)  $p \leq 0.1$ .