The Use of Violence in Illegal Markets: Evidence from Mahogany Trade in the Brazilian Amazon[*]

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Abstract

Agents operating in illegal markets cannot resort to the justice system to guarantee property rights. It is often argued that, in these contexts, violence is used to enforce previous agreements and to fight for market share. This argument plays a major role in the current debate on drug legislation, but lacks empirical support. This paper explores an episode of transition of a market from legal to illegal to provide evidence on the causal effect of illegality on systemic violence. Brazil has historically been the main world producer of big leaf mahogany (a tropical wood), but, starting in the 1990s, extraction and trade were restricted and eventually prohibited. First, we present evidence that large scale mahogany trade persisted after prohibition, through misclassification of mahogany exports as "other tropical timber species." Second, we document relative increases in violence after prohibition in areas with natural occurrence of mahogany. The increase in violence is particularly strong in states with higher share of mahogany exports before prohibition or with higher suspected illegal mahogany activity after prohibition. Trends in socioeconomic variables cannot account for this profile. We present the first documented experience of increase in violence following the transition of a market from legal to illegal.

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1. Introduction

Agents operating in illegal markets cannot resort to the justice system to uphold contracts, to guarantee property rights, or to seek protection from competitors' improper behaviors. It is often argued that, in these contexts, violence is used to enforce previous agreements and to fight for market share (for a case study, see Mieczkowski, 1990). This argument plays a major role in the current debate on the negative effects of the illegality of drug trade and the War on Drugs (see, for example, Nadelmann, 1989, Miron and Zwiebel, 1995, The Economist, 2001, and Keefer and Loayza, 2010). Historical episodes such as the American alcohol prohibition and the Opium Wars seem to support this view, but there is scant causal evidence on the effect of illegality on violence and skepticism is common (see Naylor, 2009, or discussion in Fagan and Chin, 1990, and Donohue et al, 2011). Weak states, unable to enforce the rule of law, may be prone to the development of illegal markets and to chronic violence, without any causal relationship necessarily existing between these two phenomena. In addition, violence may be associated with drugs because of the psychopharmacological and economic compulsive effects they have on users (Goldstein, 1985). Causal evidence on the effect of market illegality per se on violence is currently non-existent. Randomized experiments in this context seem virtually impossible and institutional transitions of markets from legal to illegal – which could be used as natural experiments – are extremely rare.

This paper explores a singular episode of transition of a market from legal to illegal to provide a first piece of causal evidence on the effect of market illegality on violence. Brazil has historically been the main world producer of big leaf mahogany, an extremely valuable tropical wood.[1] From the end of the 1990s to the early 2000s, the Brazilian government implemented a series of policies progressively restricting the extraction and trade of mahogany, culminating with prohibition in 2001. We use structural breaks, difference-in-difference, propensity score, and synthetic control strategies to show that mahogany extraction persisted and was associated with increased violence after prohibition. Our identification trusts on the timing of implementation of restrictions to the mahogany trade and on three pieces of information on

[1] Grogan et al (2002) claim that mahogany is one of the most valuable woods in the world, with the price per cubic meter for a high quality variety around US$ 1,200 in 2001. The area of natural occurrence of big leaf mahogany is restricted to Central America and to the South American region of the Amazon. The total Brazilian production of mahogany between 1971 and 2001 is estimated to have been of the order of US$ 4 billion, with 75% corresponding to exports to the US and Europe (Grogan et al, 2002).

the relevance of mahogany for a given region. We have data on the natural occurrence of mahogany in the Brazilian territory, on state level mahogany exports before prohibition, and on exports of "other tropical timber species" after prohibition.

We first follow and extend the work of Chimeli and Boyd (2010) and present evidence that large scale mahogany trade persisted after prohibition, through misclassification of mahogany exports as exports of "other tropical timber species." We show that, after prohibition, exports of the residual trade category "other tropical timber species" jumped from virtually zero to levels comparable to those of previous mahogany exports, representing an increase of 3,500% in a single month. At the same time, no other tropical species traditionally exported from the Amazon experienced a remotely similar increase in exports during the same period. We estimate endogenous structural breaks to the series of exports of "other tropical timber species" and find two breaks that closely match the dates of the two most important restrictions imposed on mahogany extraction: the suspension of 85% of the operating licenses on March 1999 and the complete shutdown of the market on October 2001.

Following, we document relative increases in violence after prohibition in areas with natural occurrence of mahogany. We reach remarkably similar results using three alternative empirical strategies: difference-in-difference, propensity score (matching and weighting), and synthetic control. The increase in violence in areas with natural occurrence of mahogany is particularly strong in states with higher share of mahogany exports before prohibition or with higher suspected illegal mahogany activity after prohibition, and is concentrated among prime-aged single men. These patterns reinforce the idea that the documented increase in violence is associated with the functioning of the illegal mahogany market. At the same time, socioeconomic conditions in the relevant areas improved along several dimensions, therefore, if anything, contributing to reduce violence. In fact, the increase in violence after prohibition does not seem to be related to changes in socioeconomic conditions, agricultural activity, urbanization, public security expenditures, or overall mortality profiles, and is not associated with pre-existing trends. The evidence presented here constitutes the first documented experience of increase in violence following the transition of a market from legal to illegal.

Our main result for the state of Pará, which accounted for 70% of the mahogany exports before prohibition, imply that the illegal mahogany market generated a total of 2080 deaths between 1999 and 2007. For the median municipality in the area of mahogany occurrence

(27,500 inhabitants), this amounts to 4.5 deaths per year. Depending on the hypothesis on the size of the mahogany market, our estimates point to one additional death per each US$ 212,000 to US$ 306,000 of illegal market size. Despite all of their limitations, this is the first time that numbers linking violent deaths directly to the size of an illegal market are estimated.

Our paper is related to the literature on illegal drugs and violence. There is a vast literature outside economics with case studies of prevalence of crime and violence among drug users and sellers (see papers in De La Rosa et al, 1990). In economics, most papers focus on the effect of enforcement on violence. Miron (1999 and 2001) explores US time series and cross-country data on enforcement of alcohol and drug policies and finds a positive correlation between enforcement and homicides. Medina and Martínez (2003) use variation in drug prohibition enforcement across Colombian municipalities and find no significant relationship between enforcement and violence. But none of these papers deals explicitly with the endogeneity of enforcement. Dell (2012), on the other hand, shows that plausibly exogenous increases in drug-trafficking repression in Mexico – identified from close elections between parties with different takes on drug policies – are associated with increased violence. Other papers look at the effect of illegal drugs production on local violence. Angrist and Kugler (2008) show that the shift of coca production from Bolivia and Peru to Colombia in the mid-1990s was accompanied by increased violence in coca growing areas, while Mejía and Restrepo (2011) show that increases in the demand for Colombian coca due to changes in repressive policies abroad are associated with relative increases in violence in areas adequate for cultivation.

The small literature that deals directly with the relationship between market illegality and crime and violence is the closest to our paper. Adda et al (2010) explore an episode of decriminalization of cannabis possession in a London borough between 2001 and 2002. They find that decriminalization was associated with increases in drug related offenses and reductions in other types of offenses (as police shifted resources towards non-drug related crimes). Owens (2011a) uses state level data and presents evidence that average increases in violence in the 1920s US were mostly driven by demographic trends, bearing almost no relationship with the criminalization of alcohol markets. Still, Owens (2011b) shows that criminalization of alcohol led to a change in the distribution of homicides towards ages 20 to 30, suggesting that indeed it was associated with the emergence of organized crime and systemic violence (partly offset by the reduction in homicides in other age-groups due to reduced consumption).

As Adda et al (2010) and Owens (2011a and 2011b), we use an institutional change that can be seen as a natural experiment on the effect of illegality. But, differently from Adda et al (2010), we analyze the complete shutdown of a market, rather than changes in the criminal status of consumers in a specific location. And, differently from both Adda et al (2010) and Owens (2011a and 2011b), we analyze the incidence of violence in a market unrelated to "vice" goods (drugs, alcohol, prostitution, etc), so that we immediately isolate systemic violence from violence that may be due to the consumption of the good itself (psychopharmacological) or to characteristics of consumers (economic compulsive). Also, our data and the characteristics of mahogany trade allow us to see the illegal market in operation, to identify the locations where violence should be occurring, and to link violence explicitly to the production side of the market. Therefore, our setting is more adequate for the analysis of the overall effect of prohibition on the incidence of systemic violence. The results suggest that prohibition, per se, is associated with increased violence.

The remainder of the paper is structured as follows. Section 2 presents a background of mahogany trade and policy in recent decades in Brazil. Section 3 presents the data used in the paper. Section 4 describes our empirical strategy. Section 5 presents the results related to mahogany trade after prohibition. Section 6 presents the results on prohibition of mahogany trade and violence. Finally, section 7 concludes the paper.

2. Background

2.1 Mahogany Policy in Brazil

Big leaf mahogany (*Swietenia macrophylla King*) is a native species of the Americas, originally ranging from Mexico to the Amazon region in South America. The durability, color and malleability of the timber from this tree are the main reasons for the high prices it fetches in international markets and have led to its intense exploration over the years. Most of the remaining big leaf mahogany trees are located in the Amazon forest, and Brazil was the largest exporter of the species prior to prohibition of production and trade by the local government in 2001. Brazilian production was mainly exported to the United States and European high-end furniture and construction markets. Data from the United States Department of Agriculture show that Brazil supplied 41% of all types of mahogany imports (including Asian and African species) to the US market between 1989 and 2001 (Bolivia and Peru followed with 25% and 15%, respectively).[2] This accounted for 43% of the total value of US mahogany imports. The Brazilian mahogany share of the US market reached a peak of 68% in 1992. The importance of Brazilian mahogany to the US market is further reflected in the evolution of the total volume of imports by the US, including all types of mahogany from around the world. As Brazilian authorities increased restrictions on the extraction of the species, the total volume of US imports decreased. Official US imports from around the world declined by 70% between 1989, when Brazil supplied 52% of the American market, and 2007, after the complete shutdown of the Brazilian mahogany market.

Exploration of Brazilian mahogany in the 1980s and 1990s, even under heavy regulation, contributed to increased concerns by domestic and international environmentalists who argued that continued extraction would soon lead to extinction. Although this statement has been disputed,[3] a series of stricter regulations were introduced by the Brazilian government starting in the early 1990s to curb extraction. These included: (i) export quotas limiting international sales to 150,000 m^3, 65,000 m^3 and 30,000 m^3 in 1990, 1998 and 2001 respectively; (ii) moratorium on the issuance of new forest management plans to back up mahogany extraction starting in July 1996; (iii) creation of a working group to audit forest management plans (required for mahogany extraction to take place), which led to the suspension of 85% of all management plans in March 1999; and (iv) prohibition of extraction, transportation and domestic or international trade of mahogany in October 2001. Finally, big leaf mahogany was listed on Appendix II of the United Nations Convention of International Trade of Endangered Species of Wild Fauna and Flora (CITES) in November 2002 (this regulation came into force in November 2003). Inclusion of a species in Appendix II of CITES requires careful monitoring of international trade by both the exporting and importing countries. This, in turn, might have reinforced the impetus for maintaining the more stringent outright prohibition already imposed by Brazilian authorities.[4] Institutionally, the two main restrictions were those introduced in

[2] Refer to the appendix for a discussion of USDA's Global Agricultural Trade System data.
[3] See Roozen (1998) and cited references.
[4] See IBAMA (1999), Grogan et al (2002), and Lentine et al (2003).

March 1999 – when 85% of the operating licenses were suspended – and October 2001 – when mahogany extraction was finally prohibited.

Despite tightened regulations, mounting evidence points to the continuing smuggling of big leaf mahogany formally exported under the guise of "other tropical timber species." In a recent article, Chimeli and Boyd (2010) analyze official export data to the United States and the European Union to show that Brazilian exports under the trade category "other tropical timber species" increased abruptly in 1999 and were sustained at volumes comparable to those of former exports of big leaf mahogany. They estimate structural breaks in the series for "other tropical timber species" and verify that these regime changes closely match regulatory changes in the big leaf mahogany market. An especially strong structural break takes place following the suspension of 85% of all forest management plans in March 1999. In section 5 below, we reproduce their results and expand on their analysis by formally testing whether the expansion in the exports of "other tropical species" is due not to smuggling, but rather to exports of substitute species or idiosyncratic changes in the market for these species. We continue to find support to the hypothesis that the drastic jump in exports of "other tropical species" is in fact smuggling of mahogany.[5]

But how can mahogany be smuggled out of the country through formal export channels? Selected timber species from the Amazon (mahogany, Brazilian cedar, ipe, and virola-balsa) have separate international trade codes that exporters are required to specify when they sell their product (Common Mercosur Nomenclature – NCM, chapter 44). In addition to these, there is an aggregate residual trade code that encompasses "other tropical timber species" (NCM 4407.29.90).

Exporters (or hired export companies) have to produce an invoice specifying the quantity and value of the transaction and have to fill out two export forms ("Registro de Exportação," or Export Registry, and "Declaração de Despacho de Exportação," or Declaration of Export Dispatch). Both these forms specify the NCM code of the exported good, and this is the point at which exporters have the opportunity to list mahogany as another species. Finally, an outsourced customs dispatcher is then responsible for presenting the cargo at the port.

[5] Theoretical discussions of contexts in which more stringent regulations may backfire can be found in Bulte and Van Kooten (1999) and Becker et al (2006). Glaeser and Shleifer (2001) provide a more general discussion of the relative advantages and disadvantages of quantity regulations.

While import tariffs are common in Brazil, the same is not true for export taxes. As a result, the likelihood of inspection at the port ("yellow light" or "red light" levels of monitoring) is much lower for exports than for imports. This gives exporters an opportunity to smuggle mahogany as a different species (which is subject to less stringent regulations).[6] Once mahogany is smuggled, the exporter is paid the invoice value through regular export procedures and the importer obtains a cargo complete with formal documentation.

Figure 1(a) presents the monthly series of exports of "other tropical timber species" from Brazil between 1989 and 2007, together with two vertical lines indicating the main restrictions to mahogany trade. Exports of "other tropical timber species" jump by 3,500% in September 1999, just a few months after 85% of the mahogany management plans were suspended. The figure also shows a further increase in this residual category of timber exports after October 2001, when mahogany extraction and export was finally prohibited.

Figure 1 (b) presents the same series of exports of "other tropical timber species," but on an annual basis, and plotted together with the exports of mahogany. It is clear that the declining trend of mahogany exports after the introduction of restrictions is accompanied by a rising trend in exports of "other tropical timber species."[7] In order to illustrate this point, the figure also presents the sum of the two series, which displays a more stable pattern. The aggregate series do suggest that legal mahogany exports were replaced by illegal exports under the guise of "other tropical timber species." Prior accounts by Blundell and Rodan (2003), Barreto and Souza (2001), and Gerson (2000) in the case of mahogany, and more recently by Nellemann (2012, p. 7) with respect to illegal logging in general, describe this same phenomenon. Apprehensions from as recently as early 2010 provide additional anecdotal evidence that Brazilian mahogany was systematically exported as species falling under the general category "other tropical timber species" (see, for example, Diário do Pará, 2010).

If we do believe that exports of "other tropical timber species" represent disguised mahogany in their entirety, Figure 1(b) would actually indicate an overall increase in exports

[6] In addition, identification of mahogany by physical inspection is extremely difficult and requires an expert with knowledge of mahogany and, additionally, of andiroba, cedar, and curupixá, species that can be easily mistaken by it. As recently as 2011, there are studies being conducted on the identification of mahogany based on equipment using infrared light (O Globo, 2011). These have as main objective the development of technologies to facilitate detection and reduce the illegal trade of mahogany.

[7] Some export of mahogany after prohibition in 2001 is registered, since exports from specimens extracted before prohibition were allowed under certain circumstances.

following prohibition. Though apparently counterintuitive, this is in line with the idea that quantity regulations may be optimal – as compared to taxation or prohibition – when private enforcement by competitors play an important role in detecting and denouncing violations. This point is developed in detail by Glaeser and Shleifer (2001) and is directly applicable to our setting, where monitoring by the state in the vast and inaccessible Amazon jungle is extremely difficult and legal market operators are essential in enforcing regulations. Without – or with a sufficiently small number – of legal market agents, private enforcement may lose its grip and the illegal market may instead expand and become more competitive.

A recent report on illegal logging by the United Nations and the INTERPOL claims that misreporting of species is prevalent and, in some instances, reaches volumes "up to 30 times greater than official volumes reported" (Nellemann 2012, p.13). In this perspective, the relative magnitudes of official mahogany exports before prohibition and of "other tropical timber species" after prohibition do not seem surprising. We may just be observing a regulatory change that was enough to create the incentives for this particular illegal market arrangement to develop.

In line with this logic, if we consider the sum of mahogany and "other tropical timber species" as indicating entirely mahogany exports, the increase in supply portrayed in the figure also led to a reduction in price. This is what should happen if the international demand for mahogany was stable during the period. There is a reduction in implicit prices (from value of exports) concomitant with the increase in supply after 1999. From the average changes in quantities and implicit prices, assuming a stable demand, we calculate a price-elasticity of -2.22.[8] Our argument does not require that all exports of "other tropical timber species" after 1999 are disguised mahogany exports, only that a substantial part of it is. Once the illegal exporting arrangement was set up, we cannot rule out the possibility that it would also be used to export other timber extracted illegally. Still, the quantities and implicit prices from the export data are broadly consistent with increased supply of a commodity with characteristics of a luxury good (high price-elasticity), such as mahogany. In any case, as long as a significant portion of exports of "other tropical timber species" are mahogany, our following discussion holds.

[8] We obtain this elasticity by using average implicit prices and quantities during the periods before and after the major restrictions to extraction were introduced (pre- and post-1999).

The existing evidence suggests that regulations that significantly reduced exports of big leaf mahogany and finally culminated into outright prohibition created an active illegal market. Grogran et al (2002) estimate the total value of mahogany exports between 1971 and 2001 to have been around US$ 4 billion. This value averages US$ 129 million per year, corresponding to 1.2% of the aggregate GDP of the state of Pará in 2000 (Pará accounted for more than 70% of total mahogany exports before prohibition). This highlights the relevance of this potential market to the local economy. Furthermore, the peculiar characteristics of the smuggling of Brazilian mahogany allow us to track down this illegal activity. This provides us with a unique opportunity to test the hypothesis that illegal markets are associated with increased violence.

2.2 Violence and the Illegal Mahogany Market

The role of violence in markets operating outside the scope of the legal system has received some theoretical attention. Donohue and Levitt (1998), for example, analyze the efficiency of allocations in these markets, arguing that it is directly related to the cost of fighting and the uncertainty regarding the outcome of a fight. Reuter (1985 and 2009) argues that these markets are typically organized in such a way that "firms" are small and short-lived, and tend to interact much more through competition than collusion. These would be consequences of lack of access to external credit markets, of the attention drawn by large firms, and of the difficulty and high risk of using violence to maintain centralized control, all of which would imply negative returns to firm size. He also argues that, in illegal markets, violence characterizes not only interactions between competitors, but within organizations, from labor disputes to reputation building and managerial successions.

In relation to the particular case of violence in connection with illegal logging, anecdotal evidence abounds both in Brazil and elsewhere (see, for example, Greenpeace, 2001 and 2004, Hance, 2010, and Nellemann, 2012). It is easy to find reports that discuss illegal logging as intrinsically related to the widespread use of violence.[9] News from private media outlets, non-governmental organizations, and official media document time and again the use of violence in the illegal mahogany market and give indications of the scale of this criminal activity.

In the Brazilian Amazon, protected timber species are stolen from private land (Comissão Pastoral da Terra, 2011), indigenous and conservation areas (Soares, 2003, O Liberal,

[9] For example, Hance's (2010) interview article on Indonesia is titled "Violence a part of the illegal timber trade, says kidnapped activist."

2002, and Universo Online, 2004), and public land (Soares, 2003, Universo Online, 2004, and Mendes, 2005). Intimidation-driven deals with indigenous tribe leaders are also commonly reported (Mendes, 2004). Whereas illegal extraction of mahogany already occurred before prohibition in association with fraudulent forest management plans, Mendes (2005) documents an increase in organized crime in Pará following prohibition.

In the process of extracting mahogany, loggers are said to resort to illegally obtained weapons and "threat execution of whoever may offer any resistance" (Soares, 2003). Threats and murders of rural workers, non-governmental organization leaders, and government officials attempting to disrupt the functioning of the illegal mahogany market have been widely publicized. For example, Adilson Prestes, a landless rural worker, was murdered by gun shots in the town of Novo Progresso, Pará, on July 3, 2004, allegedly for having denounced to local authorities extraction of mahogany in public and indigenous lands and a clandestine cemetery (Universo Online, 2004). At the other end of the political spectrum, a former president (2001-2002) of the Brazilian Institute for the Environment and Natural Resources (IBAMA) received death threats because of his role in the prohibition of mahogany extraction and trade (O Liberal, 2002). The investigative commission for bio-piracy of the Brazilian House of Representatives also documented death threats to two other IBAMA staff members in Pará (Câmara dos Deputados, 2005). According to Mendes (2005), organized crime in the region has two main activities: intensive exploration of mahogany and other species in public and protected land, and illegal sales of public land. In this process, loggers command a small army of men ready to perform acts of sabotage, intimidation, and murder of rural workers, union leaders, and human rights militants.

Reports from the media suggest a significant depth of the so-called "mahogany mafia," and a Federal Police officer has compared its market to that for narcotics (O Estado de São Paulo, 2002). While describing individuals involved in this trade, the same officer states that "we are not dealing with small transgressors, but a mafia (…). They use violence, move large fortunes and coerce the small guys." Allegations of this nature led to a formal arraignment by a prosecutor in the state of Pará. According to its report, the "mahogany mafia" – who illegally extracted the timber to be sold in international markets – built clandestine roads, bridges, and airstrips, assembled a "war arsenal," exploited slave labor, and owned 38 vehicles (mainly trucks), a ferry boat, and airplanes (Ministério Público do Estado do Pará, 2002). Influential politicians, indigenous tribe leaders, and public officials from federal and local governments have also been accused of involvement in the illegal mahogany market (Mendes, 2005).

In the following sections, we first test for structural breaks in the Brazilian exports of "other tropical timber species." We then compare the estimated breaks with the dates when Brazilian authorities imposed restrictions on the mahogany market. These regime changes can serve as benchmarks to test the effect of prohibition on violence.

3. Data

Mahogany Variables

In order to conduct our exercise, we need some indicator of the relevance of mahogany in certain areas of the country. We use different pieces of information to construct such indicator. First, Lentini et al (2003), based on Lamb (1966), provide a map indicating the area of natural occurrence of mahogany in the Brazilian territory (reproduced here in Appendix A.1). We superimpose this map on a map of the political division of Brazil into municipalities and create a dummy variable equal to 1 if a municipality is located within the area of natural occurrence of mahogany.[10]

Given the difficulty of access in the Brazilian Amazon, where the mahogany occurrence area is concentrated, the former variable may not be a very precise indicator of the economic relevance of mahogany in a certain region. For remote areas, with costly transportation, natural occurrence may not be enough to warrant profitable exploration. So we also construct variables trying to capture the economic relevance of mahogany in different regions. We use state level information on the total exports of mahogany (in kilograms) before prohibition, starting from 1989. Based on this information, we create a variable indicating the state share in total exports of mahogany before 1999. Exports can be done by a state that does not produce mahogany, in case an exporting company buys wood from a producing state. Still, more than 90% of mahogany exports come from the region of natural occurrence of mahogany, with more than

[10] For the state of Pará, the main producer of mahogany before prohibition, Greenpeace (2001) presents a map indicating locations of legal mahogany logging and locations where investigations uncovered illegal mahogany extraction. It is reassuring that these locations are all within the area of natural occurrence of mahogany indicated by our variable and imply an overall distribution of mahogany activity very similar to that suggested by the map from Groggan et al (2002).

70% coming from the state of Pará, which is typically identified as the main producer and the area where most of the illegal logging takes place (see, for example, Greenpeace, 2001).

Finally, we also use information on exports of "other tropical timber species" by state from 1989 to 2007. We use this information to present evidence on the continuing exploration and trade of mahogany after prohibition and as a proxy for the extent of illegal logging taking place in different states.

The data on exports of mahogany and other types of tropical timber come from the Brazilian Secretariat on International Trade, from the Ministry of Development, Industry and International Trade (from its "Análise das Informações de Comércio Exterior," or Analysis of Information on International Trade, available at aliceweb.desenvolvimento.gov.br). The two export categories we analyze in this paper are monthly exports in kilograms of mahogany and "other tropical timber species" for all exporting states of Brazil, from January 1989 to December 2007. To construct these series we take into account a change in export codes that took place in 1996. The precise strategy used to match the codes before and after 1996 is described in detail in Appendix A.2.

Outcome Variable

Our outcome variable, used as an indicator for the incidence of violence, is the homicide rate per 100,000 inhabitants. This variable is available yearly at the municipality level from the Brazilian Ministry of Health's integrated system of information (www.datasus.gov.br). Homicide rates are thought to have higher reporting than other types of violence (Soares, 2004), and the unified system of public health from the Brazilian government warrants a certain uniformity in definition across regions. The homicide data are available yearly since the early 1980s. Information on other types of crime and violence in Brazil is processed by state level police forces. The states from the Northern region of the country, where the mahogany area is located, do not have systematic data collection at the municipality level for these variables. This precludes the use of indicators of less extreme forms of violence, generated outside the health system, in our analysis.

Control Variables

The choice of control variables is guided by our main empirical concerns, which we discuss in detail in the next section. Our goal is to account for other relevant changes possibly taking place simultaneously and maybe determined by the prohibition of mahogany trade, and which may also affect the incidence of violence.

Few variables are available yearly at the municipality level, so we also use several state level controls. Most of the state level variables come from the Brazilian National Household Survey ("Pesquisa Nacional por Amostra de Domicílios") and were tabulated by the Institute for Applied Economic Research ("Instituto de Pesquisa Econômica Aplicada," available from www.ipeadata.gov.br), a think tank from the Brazilian government. These include: poverty rate, ratio of income per capita of the top 10% of the income distribution to the bottom 40%, percentage of households with more than 2 members per room, enrollment rate between ages 7 and 14, percentage of informal workers in the labor force, percentage of population living in households with access to treated water, percentage of population living in households with toilet connected to the public sewerage system, illiteracy rate in the population above 15, percentage of the labor force occupied in agriculture and fishing, and unemployment rate. We also use state level expenditures on public security per capita, obtained from the Brazilian Ministry of Finance.

At the municipality level, we have information on: (i) total area planted, from the municipal agricultural surveys from IBGE; (ii) mortality by cause of death, from the Brazilian Ministry of Health; (iii) number of deaths associated with land conflicts, collected by the "Comissão Pastoral da Terra," a catholic organization that monitors and tries to mediate land related conflicts in Brazil; and (iv) for a limited number of years, gdp per capita and fraction of gdp in agriculture, from the Brazilian national accounts.

Variables constructed from the PNAD are available under a consistent methodology since 1992, but for the years 1994 and 2000, when the survey did not take place. Municipality level gdp per capita is available only for 1996 and after 1999. Other state and municipality variables are available for all years between 1992 and 2007.

Given the creation of a large number of municipalities in Brazil in the early 1990s, and the fact that the policies we analyze were introduced in the end of the 1990s, we restrict our sample to the period between 1995 and 2007. Still, regressions including all controls lose part of the observations within this time interval.

Table 1 presents descriptive statistics for municipalities inside and outside the area of occurrence of mahogany, considering the three samples that are used throughout the paper: all

Brazil, only states with some natural occurrence of mahogany, and only the state of Pará. The table presents numbers on average homicide rate, gdp per capita, and fraction of gdp in agriculture, before and after the first main regulatory change (1999). The objective of the table is to characterize the differences between municipalities located in these areas of Brazil and to highlight the main challenges implicit in our empirical exercise.

Table 1 makes clear that mahogany regions were initially poorer and more dependent on agriculture than other regions of Brazil, independently of whether one considers the entire country or municipalities in the main mahogany producing states as the comparison group. When compared to Brazil as a whole, mahogany regions start with lower homicide rates in the pre-1999 period, but surpass the countrywide rate after 1999. Within mahogany occurring states, mahogany areas were slightly more violent before 1999, but this difference opens up widely afterwards. Interestingly, mahogany areas seem to have experienced socioeconomic improvements at faster rates than other areas in the country. All these patterns are particularly clear within the state of Pará. The table also shows that pre-1999 differences across mahogany and non-mahogany areas are much milder when we look at more homogeneous areas, either states with some mahogany occurrence or only the state of Pará, as opposed to comparing mahogany occurring areas to Brazil as a whole.

For illustrative purposes, Figure 2 plots the yearly homicide rates for the state of Pará, disaggregated by areas with and without occurrence of mahogany. The pattern of relative increase in violence within mahogany areas becomes clearer. The dates of the main restrictions to mahogany exploration do seem to be associated with relative increases in homicide rates in mahogany occurring areas. It is also important to notice that the evolution of homicides is almost identical between mahogany and non-mahogany areas before prohibition, but a gap opens up immediately after the first major restriction to logging in 1999. Table 1 and Figure 2 suggest that prohibition may indeed have had an effect on violence, but they also hint at the challenges implicit in our empirical exercise.

4. Empirical Strategy

4.1 Illegal Mahogany Trade after Prohibition: Structural Break Estimation

We first provide evidence that exports of mahogany continued after prohibition, through misclassification of mahogany exports as exports of "other tropical timber species." In order to do so, we show that the historical series of exports of "other tropical timber species" experienced huge increases in quantity (kilograms) following the introduction of the most severe restrictions on mahogany extraction and trade. To develop this argument formally, we follow Bai and Perron (1998) and test for endogenous structural breaks in the series and check whether the dates identified by the model match the timing of introduction of restrictions in the mahogany market. This same exercise was conducted for the aggregate monthly series for Brazil by Chimeli and Boyd (2010). Here we extend their analysis by formally controlling for exports of some selected species, in an attempt to account for the possibility of substitution of other timber types for mahogany and for widespread movements in the market for tropical woods. We also perform the same tests with yearly series (which tend to be less noisy than monthly series).

Consider a function with m structural breaks determining $m+1$ distinct regimes:

$$y_t = \delta_j + x_t'\beta + u_t \quad \text{with} \quad t = T_{j-1}+1,\cdots,T_j \quad \text{and} \quad j = 1,\cdots,m, \tag{1}$$

where y_t is the observed dependent variable, δ_j are regime specific averages (regime specific coefficients of regression of y_t on a vector of 1's), x_t and β are covariates and associated coefficients, u_t is the possibly autocorrelated and heteroskedastic disturbance at time t, and $T_1,\ldots, T_m$ are the break points to be estimated.

Estimation of these breakpoints initially requires calculation of the minimum sum of squared residuals for each admissible partition of the time domain:[11]

$$S_T(T_1,\cdots,T_m) = \min_{\delta} \sum_{i=1}^{m+1} \sum_{t=T_{i-1}+1}^{T_i} (y_t - \delta_i - x_t'\beta)^2. \tag{2}$$

Next, Bai and Perron (1998) use a dynamic programming algorithm to compute the minimum $S_T(T_1, \ldots, T_m)$ over all admissible partitions, yielding the estimated breakpoints

$$(\hat{T}_1,\cdots,\hat{T}_m) = \text{argmin}_{T_1,\cdots,T_m} S_T(T_1,\cdots,T_m). \tag{3}$$

Finally, to determine the number of breaks in the series, we employ a set of statistics derived by Bai and Perron (1998) to first test the null hypothesis of no breaks versus the alternative of $m = b$ breaks, and then to test the null hypothesis of l breaks against $l+1$ breaks.

We also conduct the same exercise controlling for exports of cedar, ipe and virola-balsa.

[11] By admissible partition of the time domain, we mean partitions $T_1,\ldots, T_m$ such that each regime lasts for no less than a given pre-determined time length h greater than the number of regressors in the model.

Cedar and ipe are imperfect substitutes for mahogany, mainly due to their durability, and jumps in their exports could reflect systematic changes in the demand for other timber types from the Amazon as a response to mahogany prohibition. Virola and balsa are less dense types of timber with very different uses from mahogany. We include virola-balsa exports as a control variable to capture overall movements in the international markets that might also be correlated with jumps in exports of "other tropical timber species."

4.2 Mahogany Prohibition and Violence: Diff-in-Diff, Propensity Score, and Synthetic Control

The dimensions of variation we explore to identify the causal effect of prohibition on violence are the timing of the institutional changes and the differential relevance of mahogany across different areas of the country. In principle, if the increase in homicides after prohibition is larger in mahogany occurring or producing areas, it could be attributed to prohibition.

The timing of the intervention considered here is unique for the entire country. So identification of the effect of prohibition comes from the heterogeneous response of different areas to prohibition, rather than from differential timing of treatment. Areas with no mahogany related activity should experience no significant changes in the incidence of violence due to prohibition, while areas with some type of mahogany activity should experience increases in violence.

Given the institutional discussion from section 2 and the evidence to be presented in the next section, we focus on two particular years as key moments in the tightening of regulations that eventually led to mahogany prohibition. First, we create a dummy variable equal to 1 for the interval between 1999 and 2001, capturing the first major step towards prohibition (suspension of 85% of the operating licenses for management plans). Following, we create a second dummy variable equal to 1 for 2002 and following years, corresponding to the final prohibition of mahogany trade instituted in October 2001.

We adopt three strategies to identify the effect of prohibition on violence: difference-in-difference, propensity score (matching and weighting), and synthetic control. Each of these strategies has different advantages and disadvantages, discussed in further detail below. The consistency of results across these three alternatives lends additional credibility to the results.

Difference-in-Difference

We start with the following difference-in-difference strategy:

$$Homicide_{it} = \alpha + \beta_1.(D_{1999 \le t \le 2001} \times Mahog\_Var_i) + \beta_2.(D_{t \ge 2002} \times Mahog\_Var_i) \qquad (4)$$

$$+ z_{it}'\gamma + \vartheta_i + \mu_t + \varepsilon_{it},$$

where $Homicide_{it}$ indicates the homicide rate for municipality *i* in year *t*; $D_{1999 \leq t \leq 2001}$ is a dummy variable equal to 1 for the years between 1999 and 2001; $D_{t \geq 2002}$ is a dummy variable equal to 1 for 2002 and all following years; $Mahog\_Var_i$ is some variable indicating the relevance of mahogany in municipality *i* (to be discussed in the next paragraph); z_{it} is a vector of control variables; ϑ_i is a municipality fixed-effect; μ_t is a year fixed-effect; ε_{it} is a random term; and α, β_1, β_2, and γ are parameters. Under the usual assumptions, $E[\varepsilon_{it} | D_{t \geq 1999}, D_{t \geq 2002}, Mahog\_Var_i, z_{it}, \vartheta_i, \mu_t] = 0$, and OLS estimation of the above equation provides unbiased estimates of the β's. In this hypothetical setting, the random term ε_{it} is not correlated with the independent variables, so OLS estimates of the β's indeed provide the parameters of interest: the causal impact of mahogany trade restrictions on homicide rates.

We use three pieces of data to identify the relevance of mahogany in a given area ($Mahog\_Var_i$). First, we use information on the area of natural occurrence of mahogany. From that, we create a dummy variable equal to 1 for municipalities that are in the mahogany occurring area. But natural occurrence of mahogany may not be enough for its trade to be an important activity, given that it may not be economically profitable to explore mahogany in remote and difficult to access areas. So we also use information on mahogany exports before prohibition and exports of "other tropical timber species" after prohibition. This information is available only at the state level, so we create two variables: one indicating the share of the state in aggregate mahogany exports between 1989 and 1998 (before prohibition), and another indicating the total amount of yearly "suspected mahogany exports," both before and after prohibition. The second variable is constructed simply by adding the series of mahogany and "other tropical timber species" exports, on the assumption that the latter represented illegal mahogany exports. The first variable gives a measure of the importance of mahogany to the local economy before prohibition, while the second gives an estimate of mahogany activity which includes the illegal period (in reality, we only use the post-prohibition period as treatment, but the pre-prohibition period is useful in some of our robustness exercises).

These variables capture export activity, but not necessarily extraction, since non-mahogany producing states can also be exporters (though, in reality, this is a rare event). They vary only at the state level, but they can be interacted with our dummy indicating areas of occurrence of mahogany to create a triple difference. This triple difference would compare not only areas with natural occurrence of mahogany to other areas, but also, within the area of natural occurrence of mahogany, those in states where mahogany was an important economic activity to those in states where it was not. So we create two additional treatment variables that are the products of one of the export variables (state share in mahogany exports before prohibition or "suspected mahogany exports" after prohibition) and the dummy variable for mahogany occurrence area. These treatments correspond to triple differences in timing of prohibition, natural mahogany occurrence, and relative importance of mahogany activity. We have therefore three treatment variables used in the paper, though we focus most of the discussion on the simple double difference.

In our context, there are two potential concerns with the difference-in-difference strategy: omitted variables and differential dynamic behavior of homicide rates. First, there may be other changes happening simultaneously to the prohibition of mahogany. In particular, prohibition has economic impacts that may indirectly affect the incidence of violence, through reduced income and worsened labor market opportunities, or through changes in the pattern of agricultural activity and violence in the agricultural frontier. In addition, some of the mahogany areas are remote regions of the country that may be going through modernization and increased urbanization. To address these concerns, we control for a large set of state characteristics: unemployment rate, percentage of informal workers, fraction of household with more than 2 members per room, inequality, fraction of labor force in agriculture, enrollment rate between ages 7 and 14, percentage of population in households with access to treated water, percentage of population in households with toilet connected to the public sewerage system, and illiteracy rate in the population above 15. We also control for municipality area planted and, in some specifications, municipality gdp per capita and fraction of municipal gdp in agriculture.

Since most of our controls vary only at the state level, in some specifications we resort to yearly mortality data, which we do have at the municipality level. We use as additional controls mortality from infectious diseases, neoplasms, heart and circulatory conditions, suicides, traffic accidents, and before age 5. Infectious diseases, neoplasms, and heart and circulatory conditions are intended to capture broad mortality and demographic trends at the municipality level, and also possibly changes in registration. Suicides and traffic accidents control for changes associated with modernization and urbanization, even though, with imperfect monitoring and registration, they may be misreported homicides. Finally, mortality under age five and from infectious diseases can be seen as controls for living conditions and local economic development.

Finally, ill-defined property rights in the Brazilian agricultural frontier are commonly associated with violent land disputes (see Alston et al, 2000, and Altson and Mueller, 2010). Since part of this agricultural frontier is located in the Brazilian Amazon, overlapping some of the mahogany area, one might be worried about confounding effects of land conflicts in our estimation. Therefore, when controlling for mortality patterns, we also include the number of deaths associated with land conflicts.

Given the heterogeneity across regions of Brazil discussed in the descriptive section, we conduct our analysis using three alternative samples. We start by looking at Brazil as a whole, then restrict the sample to municipalities in states with natural occurrence of mahogany, and then only to municipalities in the state of Pará. Treatment and control groups are more homogeneous within states with some mahogany and within Pará. Pará is also particularly relevant because it accounted for more than 70% of mahogany exports before prohibition. But, on the other hand, contamination of the control group is a potential problem over smaller areas, where spillovers of violence from mahogany to non-mahogany regions are more likely. So, given the relative strengths of the different samples, we keep all of them through most of the paper. This same concern motivates our use of propensity score matching and weighting as an additional robustness exercise.

The second issue commonly raised in the context of difference-in-difference exercises is the possibility of differential dynamic behavior of the dependent variable. In our context, this would amount to homicides in mahogany areas behaving differently already before prohibition. Figure 2 suggests that this was not the case for Pará, where most of illegal logging is supposed to take place, but we still explore this possibility by conducting various placebo exercises involving the pre-prohibition period. This concern also motivates our use of the synthetic control method as an additional robustness test.

There are a few remaining methodological issues in our difference-in-difference estimation that should be mentioned explicitly: (i) as the variance of homicide rates is directly related to population size, we weight all regressions by population size; and (ii) as the difference-in-difference strategy may lead to underestimation of standard errors due to autocorrelation in the residuals, we cluster standard errors at the municipality level in all specifications, allowing for an arbitrary structure of correlation over time (as suggested by Bertrand et al, 2004).

Propensity Score Matching and Weighting

Any remaining empirical concern in our difference-in-difference strategy would have to be associated with some notion of inadequacy of the various control groups used as counterfactuals. To address this concern, we use a propensity score strategy. First, using information from the pre-prohibition period, we estimate a probit of the probability of being in the mahogany area on the full set of municipality characteristics that we observe, including the homicide rate (area planted, mortality variables, homicide rate, gdp per capita, and fraction of gdp in agriculture, averaged over 1995-1998).[12] Following, once the predicted propensity score is obtained, we conducted a one-to-one nearest neighbor matching and also apply an inverse-probability weighted regression adjustment. In the one-to-one matching, we look at the dependent variable both as the homicide rate averaged over the post-prohibition period (either 1999-2007 or 2002-2007) and the change in homicide rate between the pre- and post-prohibition periods. In the inverse-probability weighted regression, we use as dependent variables the change in homicide rates between the post- and pre-prohibition periods, and as independent variables both the levels and changes in all the independent variables listed before (controlling for the initial level of homicides as well). The inverse probability weighted regressions have the additional advantage of being doubly robust, meaning that misspecification of either the regression equation or the propensity score separately do not harm consistency (Imbens and Wooldridge, 2009). In addition, since we run these equations in differences, the propensity score is robust to selection on unobservables, as long as these unobservables are time invariant (Abadie, 2005). We conduct the matching looking at municipalities in mahogany areas of the state of Pará, and compare them to municipalities without mahogany, but located in states with some occurrence of mahogany (excluding Pará).

[12] Though location in the mahogany area is obviously an intrinsic characteristic of municipalities, being therefore predetermined, this probit intends to capture characteristics of municipalities that are correlated to this geographic feature. This way, we can use the propensity score to build a control group that is, on the socioeconomic characteristics observed, as similar as possible to the municipalities in the mahogany area. Strictly, location in the mahogany area is not a random variable, so this equation is not precisely correct from a structural perspective (longitude and latitude, for example, would fully determine whether a municipality is in the mahogany area).

This choice of sample tries to increase comparability between treatment and potential controls, minimizing at the same time problems of contamination (for this reason, we exclude municipalities without mahogany in the state of Pará, since they are more likely to be affected by spillovers from mahogany). Results are similar if we use areas with and without mahogany in states with some mahogany occurrence.

Synthetic Control Method

Though the matching strategy is able to compare municipalities that were as similar as possible in the period before intervention – or weight municipalities in a regression setting by how similar they were in that period – it misses the specific dynamic of homicide rates before prohibition, since data has to be collapsed into pre and post prohibition periods. If the main concern related to comparability refers to the specific dynamics of the dependent variable before prohibition, a lot will be missed in the matching strategy. An alternative in this case is to apply the synthetic control method for comparative case studies proposed by Abadie and Gardeazabal (2003). In this case, we look at the area of mahogany occurrence as a single unit and try to match the behavior of its homicide rate in the pre-prohibition period by creating a weighted average of areas in a potential control group. Our pool of potential controls is composed of regional averages of areas without mahogany, in states that have some mahogany occurrence. Again, the goal is to start from a pool of potential controls that is as close as possible to the average of municipalities that are located in the mahogany occurrence area. Even though there is a substantial loss in the cross-sectional dimension here, since we are not able anymore to match individual municipalities, there is a gain in the time dimension, because we are able to use our controls to reproduce the dynamic behavior of homicides in the areas with mahogany occurrence as closely as possible. Together with the propensity score matching strategy discussed before, our synthetic control exercise addresses any remaining concerns stemming from the difference-in-difference strategy. In our view, the similarity of results across these diverse empirical strategies lends additional credibility to the analysis. In the results section, we explain in further detail how the samples for this exercise are constructed. To anticipate the main point, for comparability purposes, when constructing these samples we attempt to emulate the strategy of the propensity score exercises. The results are not sensitive to the specific sample chosen to construct the synthetic control.

5. Results: Illegal Mahogany Trade after Prohibition

The analysis in this section is related to a growing body of literature on detection of illegal activities, exemplified by Fisman and Wei (2004), Fisman et al (2009), and Della Vigna and La Ferrara (2010). Table 2 presents the estimated structural breaks for exports of "other tropical timber species" and Figure 3 plots the corresponding data. The results on the left portion of the table are based on monthly exports, while the right portion uses total annual exports. Whereas the number of observations is much larger when we use monthly information and our ability to pinpoint structural breaks improves, higher frequency data can be more volatile and mask longer run movements in the series. We therefore report our results for both series.

Table 2 presents estimated break dates (month of the year in parenthesis) followed by their corresponding confidence intervals. Since Bai and Perron's (1998) algorithm uses integers for dates, confidence intervals formed by time spans smaller than the time unit of the series are not reported and appear as NA. The top portion of Table 2 reports estimated structural breaks in models without controls. The first structural break for our monthly series occurs in August 1999, following suspension of 85% of all forest management plans for extraction of big leaf mahogany. The suspended management plans were located in the state of Pará and affected the largest producers of mahogany, who effectively lost their ability to legally extract and sell this resource. The structural break taking place between August and September 1999 corresponds to an increase of 3,500% in the Brazilian exports of "other tropical timber species" in a single month.[13] When we focus on the annual series, the first structural break occurs in 1998 and reflects the increase in exports taking place in August of the following year, as described above.

The second structural break occurs sometime between 2001 and 2003. This break is consistent with two institutional changes affecting the mahogany market: i) prohibition of extraction, transportation and trade of big leaf mahogany imposed by Brazilian authorities at the end of 2001, and ii) inclusion of big leaf mahogany in Appendix II of CITES at the end of

[13] Most of the exports of "other tropical timber species" came from the state of Pará, the largest producer of mahogany. Estimating structural breaks for exports of "other tropical timber species" from Pará only as opposed to aggregate exports as in Table 3 produces identical point estimates and minor variations in the confidence intervals for the monthly series. Estimates of break dates for the individual states where mahogany naturally occurs are available from the authors upon request.

2002, which might have signaled that prohibition was likely to be maintained. We therefore find initial support to the hypothesis that the drastic contraction of the formal mahogany market in 1999 and prohibition in late 2001 contributed to the flourishing of an illegal market in the main producing states.[14]

Next, we entertain the possibility that the increase in exports of "other tropical species" does not reflect illegal exports of mahogany, but instead exports of substitutes for the banned species. To do so, we first consider the exports of ipe and cedar, which are also extracted and historically exported from the Amazon region. Due to their durability, ipe and cedar are better imperfect substitutes for mahogany than the lower quality species that are included under the residual trade category "other tropical timber species." Therefore, variability in the exports of ipe and cedar could reflect systematic changes in the demand for substitute timber from the Amazon as a response to mahogany prohibition. The top two graphs in Figure 4 depict the time series for exports of these species along with the corresponding time trends (smoothing splines). Changes in exports of ipe and cedar are smooth and in no way resemble the drastic jump in exports of "other tropical species" around the time of major regulations affecting the mahogany market.

The trade category "other tropical timber species" is, in reality, meant to include a series of species of lower quality and value than mahogany. These species individually represent a smaller fraction of the total wood export volume and do not justify a separate trade category. Therefore, if no smuggling of more valuable species actually takes place under this residual category, we would expect trade patterns of "other tropical species" to resemble those of other less valued timber. Virola, balsa and conifers are less valuable timber types that have their own trade categories (virola and balsa are combined into one category) due to their large export volumes, and their export series can shed light on the functioning of the market where "other tropical timber species" should belong.

Virola and balsa come predominantly, but not exclusively, from the Amazon region. Their series can thus capture trade patterns for lower-valued woods from the Amazon. Conifers come predominantly, but not exclusively, from southern Brazil. The series for conifers is less

[14] When analyzing total Brazilian exports of "other tropical timber species" to the European Union and the United States for the time span ranging from January 1989 to December 2006, Chimeli and Boyd (2010) estimate one structural break in August 1999 and another sometime in the time span ranging from September 2002 to April 2004, depending on the consumer market and taking confidence intervals into account.

directly related to trade of Amazon species, but we include it in our discussion as it might capture overall trade patterns for lower-value woods from Brazil. The bottom two graphs in Figure 4 plot the aggregate exports (quantities) for virola-balsa and conifers. The smoothing splines again suggest a smoother and much different time trend than that observed in the case of "other tropical timber species". This suggests that no drastic change occurred in the market for less valued timber from Brazil, and lends further support to the hypothesis that the large structural breaks in the series for "other tropical timber species" actually reflect the flourishing of an illegal market for Brazilian mahogany.

To assess the alternative hypothesis discussed in the previous paragraphs formally, we control for the possibility that substitution and idiosyncratic changes in the market for timber are actually driving our structural break estimates. We do so by re-estimating the Bai and Perron structural breaks controlling for imperfect substitutes of mahogany (ipe and cedar) and for less valuable timber from the Amazon (virola-balsa). The bottom half of Table 2 presents the estimated structural breaks including controls and the bottom graphs in Figure 3 plot the corresponding regime change dates (vertical lines). When analyzing the annual (but not monthly) series, we drop the variable for ipe from our set of controls, since the minimum time interval between two structural breaks in Bai and Perron (1998) has to be greater than the number of regressors in the model. This means that, with annual data, the inclusion of three controls plus a constant would force the structural breaks to be at least five years apart. The results with controls are virtually identical to those without controls.[15]

Another way to try to detect the functioning of this illegal market would be to follow Fisman and Wei (2004) and look at data on imports of Brazilian timber from other countries. If imports in the US and Europe continued to be classified as mahogany, even as Brazilian exports were replaced by "other tropical timber species," there would be further support to the our main hypothesis. In Appendix A.3, we look at US import data. We observed a reduction in US imports of Brazilian mahogany concomitant with the reduction in official exports from Brazil, so the type of phenomenon noticed by Fisman and Wei (2004) is not happening. But we do

[15] If we ignore the problem alluded to above and estimate the model with annual series using our entire set of controls (three controls for the different types of timber plus the intercept), we artificially estimate the first break in 1997, five years before the second break in 2002. However, visual inspection of the series for "other tropical timber species" suggests that no unusual changes took place in 1997. Substituting Ipe for cedar in the estimation of structural breaks for the annual series produces results analogous to those presented in the table, with one structural break in 1998 and another in 2003, and no estimated confidence intervals in either case.

observe an increase in US imports of a residual category equivalent to "other tropical timber species." In addition, the prominent role played by Brazilian mahogany before prohibition among all US imports of tropical timber is assumed, after prohibition, by "other tropical timber species." This pattern, though only indirectly, also supports the idea that imports of this category in the US represented disguised mahogany.

The estimated regime changes and their timing, which closely match the main regulatory restrictions in the mahogany market, are the starting point for our subsequent analysis of the impact of illegality on violence. Before proceeding, however, it is instructive to inspect, in the main mahogany producing areas, the evolution of socioeconomic conditions that might be associated with violence. Figure 5 plots data for potential socioeconomic determinants of violent crime: gdp per capita, unemployment rate, poverty rate and enrollment rate between ages of 7 and 14 for the state of Pará, the main exporter of mahogany and "other tropical timber species."[16] None of these indicators suggest drastic changes that might be contributing to increase violence in the state. In fact, these figures indicate overall trends that would seem to contribute to reduce violence, such as reduced poverty and inequality and increased enrollment. This is especially clear after 2001, the period when mahogany prohibition was set in place and the illegal activity is supposed to have intensified.

To sum up, exports of "other tropical timber species" experienced discrete increases – significant both statistically and in terms of magnitude – soon after the main restrictions to mahogany trade were implemented. At the same time, no other type of wood displayed a remotely similar pattern of exports during the same period. In addition, socioeconomic conditions in the main producing areas, if anything, were improving over time. Together, this evidence indicates that an active illegal mahogany market appeared in the Brazilian Amazon starting in the end of 1990s, while no other obvious factor was contributing to increase violence in the region.

[16] Data for these socio-economic indicators, except for gross state product per capita, are based on the national household sample survey (PNAD) and do not contain information for 2000. Data for the year 2000 were interpolated for illustrative purposes.

6. Results: Mahogany Prohibition and Violence

6.1 Difference-in-Difference: Benchmark Results

Table 3 presents the results for our benchmark specification. The table contains three panels, each one for a different sample: all Brazilian municipalities in panel A, municipalities in states with some mahogany occurrence in panel B, and only municipalities in the state of Pará (which accounts for 70% of exports before prohibition) in panel C. All three panels have five columns in common containing coefficient estimates from variations of the specification in equation 4: (i) without the inclusion of controls in column 1; (ii) including all state level controls and municipality area planted in column 2; (iii) including controls and considering a single treatment dummy after 1999 in column 3; (iv) including controls and considering a single treatment dummy after 2002 (excludes the period between 1999 and 2001 from the sample) in column 4; and (v) including all municipality level mortality controls in column 5. The first five columns in the three panels of Table 3 present the most important results in the paper.

Columns 1, 2 and 5 illustrate the effect of different sets of controls in the estimation, while columns 3 and 4 show how results change if, instead of considering two consecutive treatment periods as in our benchmark specification, we consider a single treatment period. In addition, columns 6 and 7 in panels A and B use the triple difference strategy alluded to before: mahogany occurring area interacted with pre-prohibition share of exports and with treatment years, and mahogany occurring area interacted with "suspected mahogany exports" and with treatment years. Since the export data vary only at the state level, we cannot use the triple difference strategy when looking at data from Pará in panel C.

The results for all specifications in panel A show a significant effect of both treatment variables, reflecting the suspension of 85% of the management plans and the shutdown of the legal mahogany market. The coefficient on the first policy change (*treat 1999*) is always much smaller than that on the second one (*treat 2002*) and tends to be estimated less precisely, but, still, it is strongly significant in most cases. Areas in the region of natural occurrence of mahogany experienced some relative increase in homicide rates between 1999 and 2001, and then again more intensely after 2002. In addition, the triple difference estimates from columns 6 and 7 indicate that these increases were particularly strong in those states with higher share of mahogany exports before prohibition and with higher "suspected mahogany exports" after prohibition. This suggests that the strongest effects of prohibition were observed in areas that had natural occurrence of mahogany and where mahogany-related economic activity was most relevant (either legally before prohibition or illegally after prohibition). This pattern is consistent with the logic behind our identification strategy and the anecdotal evidence discussed in section 2. We leave the discussion on the quantitative impact of the estimated coefficients for later, but notice for now that the figures from columns 6 and 7 are not directly comparable to other numbers in the table, since the scale of the treatment variable in these cases is different.

In column 2, when we include our whole set of state level controls – accounting for a vast array of socioeconomic variables – and municipality area planted, the results become stronger than in column 1, both in terms of point estimates and statistical significance. This is not due to the reduction in sample size. If we run the same specification from column 1 with the sample from column 2, the results are very similar, but slightly weaker than those from column 1. In fact, the inclusion of controls strengthens the relationship between prohibition and violence, meaning that differential socioeconomic changes taking place in mahogany areas probably contributed to reduce local violence, as should be expected from Figure 5. In other words, our treatment variables do not seem to be capturing the effect of other unobserved changes related to socioeconomic conditions, urbanization, or agricultural activity. In column 2, when the controls are included, both treatment variables appear as statistically significant at the 1% level. This remains true when we consider each treatment separately, in columns 3 and 4, and when we consider the triple differences, in columns 6 and 7.

In column 5, we include controls for a number of mortality rates by cause of death, which we do observe yearly at the municipality level. These mortality rate categories capture changes in overall mortality and demographic trends (infectious diseases, heart and circulatory diseases, and neoplasms), in socioeconomic conditions (before age five and infectious diseases), and in urbanization and modernization (traffic accidents and suicides). We also control for deaths associated with land conflicts, to disentangle the violence we are identifying from that related to land conflicts in the agricultural frontier. Since police inefficiency and reporting problems may imply that a fraction of homicides are not registered by the health system as such, possibly being allocated to other causes of death, mortality by some of these causes may be partly endogenous to our exercise (some homicides might be registered as deaths due to traffic accidents, suicides, or even heart attacks, for example). Under this scenario, the mortality controls would not belong to the right-hand side of the equation, as pointed out, for example, by Angrist and Pischke (2009). Still, we conduct this exercise as an extreme test of our model, and possibly as a lower bound estimate of the effect of prohibition on violence. The inclusion of the mortality variables reduces the estimated treatment coefficients, bringing them closer to those observed in column 1. The effect of the second treatment variable remains positive and strongly significant, while, as in the first column, the first treatment (*treat 1999*) is statistically significant only at the 10% level.

Following, we restrict the sample in two different ways to deal with concerns related to the comparability of treatment and control groups. First, in panel B, we look at municipalities in states with some natural occurrence of mahogany, ending up with a sample of 628 municipalities. Apart from column 1, the results remain remarkably similar across the two samples, despite a reduction of 4,346 municipalities in the cross-sectional dimension of the panel. This indicates that heterogeneity across different areas of Brazil does not directly impact the results presented in panel A. Even within mahogany occurring states, mahogany occurring areas experienced significant increases in violence when compared to other areas.

Next, in panel C, we go one step further and restrict the sample to the state of Pará. Pará accounted for more than 70% of mahogany exports before prohibition and most of the illegal activity is presumed to take place there, so it deserves particular attention. In addition, Table 1 and Figure 2 show that there is much less municipality heterogeneity within Pará than across Brazil as a whole, so this exercise may further help diminish concerns related to heterogeneity between treatment and control. Results are typically of similar order of magnitude, but much larger, than those obtained before. The behavior of the coefficients as we move from the specifications in columns 1 to 5 is qualitatively similar to that from the previous panels in Table 3. Again, our previous results do not seem to be driven by unobserved heterogeneity across control and treatment groups. Looking only at the state of Pará, we still detect a statistically significant increase in homicide rates in mahogany occurring areas when compared to other areas. If anything, differences are starker within Pará than across Brazil as a whole.

Appendix Table A.4.1 conducts two additional exercises, which we mention briefly here. First, it computes standard errors for the specification from column 2 in Table 3 using the Driscoll-Kraay formula, which is robust to arbitrary forms of spatial correlation across panel units (in our case, municipalities; see details in Driscoll and Kraay, 1998). Results become stronger in terms of statistical significance when we use standard-errors robust to spatial correlation, indicating that cross-sectional correlation is not affecting the results in Table 3. We also include additional controls for gdp per capita (natural logarithm) and fraction of gdp in agriculture in the specification from column 2 in Table 3. These specifications are not displayed in the main text because they should be seen with caution, since we lose observations from years 1995, 1997, and 1998 due to data availability, being left with a single year in the pre-treatment period (1996). Still, results remain very close to those obtained before, typically falling in-between the coefficients from columns 2 and 5 in Table 3.

We conclude our discussion of the benchmark results analyzing the quantitative aspect of the estimated coefficients. To look at a more homogeneous set of municipalities and concentrate on the state that accounts for most of the production (Pará), consider the coefficients in column 5 from panel C. These can be immediately read as changes in homicide rates per 100,000 inhabitants after the mahogany restrictions, corresponding therefore to increases of, respectively, 6.6 between 1999 and 2001 and 16.6 after 2002. Comparing with the pre-1999 average homicide rate in mahogany areas of Pará, these estimated coefficients correspond to increases of roughly 48% between 1999 and 2001 and of 120% after 2002. Though apparently large, these numbers have to be seen in light of the experience of the state of Pará during this period, where the overall increase in homicide rates was well above 100%. In fact, this estimated coefficient explains only 70% of the differential increase in homicide rates across areas with and without mahogany between 1995 and 2007 (portrayed in Figure 2). Since the mahogany market is estimated to correspond roughly to 1% of the state GDP, it seems plausible that most of this spike in violence was indeed due to increased illegal logging and the context of violence that followed.

To give a more concrete meaning to the estimated coefficient, consider the median municipality in the mahogany occurring area of Pará (27,500 inhabitants). The estimated impact of the post-2002 treatment amounts to 4.5 additional homicides every year. From 1999 to 2007, the effect in Pará adds up to 2080 additional deaths due to illegal mahogany activity.

Bearing in mind the historical size of the mahogany market – US$ 129 million per year between 1971 and 2001 (Groggan et al, 2002) – and Pará's 70% share in it, the estimated coefficient for the prohibition period implies one additional death per each US$ 306,000 of illegal market. Considering the average level of suspected mahogany exports after prohibition (US$ 89 million, in 2000 values), the similar number amounts to one death per US$ 212,000 of illegal market size.[17] Despite all of their limitations, it is worth mentioning that this is the first time that numbers linking violent deaths directly to the size of an illegal market are estimated in the literature.

6.2 Difference-in-Difference: Placebo and Demographics

Despite the evidence from Figure 2 and the consistency of results across the different samples in Table 3, one might still be concerned that the treatment variables are capturing distinct dynamics of violence in mahogany regions. If this was the case, these dynamics should be present already before the restrictions to mahogany exploration and trade were imposed. To assess this possibility, we introduce variables that account for pre-intervention trends, or placebo interventions, in homicide rates. We use the three samples from Table 3 and include pre-1999 placebos in specifications with our simple double difference and also with the triple differences: (i) a dummy for 1997-1998 interacted with the dummy indicating mahogany areas for the double difference; (ii) a dummy for 1997-1998 interacted with the pre-1999 share of mahogany exports and with the mahogany area dummy, in the case of the triple difference using exports before prohibition; and (iii) an interaction of mahogany exports with a pre-1999 dummy and with the mahogany area dummy, in the case of the triple difference using "suspected mahogany exports." This last placebo is the only one that varies on a yearly basis. It can also be seen as a test against the alternative hypothesis that it is logging per se, not illegality, that is associated with violence. In effect, it is a test of whether mahogany activity was also associated with violence before it was illegal. The other two placebos simply try to detect whether homicides in mahogany areas were already behaving differently a couple of years before the restrictions to extraction were imposed.

Results are presented in Table 4. None of the pre-intervention placebos appear as statistically significant. The estimated coefficients on pre-intervention variables are very small in magnitude, while those on the treatments are again very similar to the coefficients from Table 3 (column 2). There is no evidence that our treatment variables are capturing differential dynamic behavior of homicide rates that were present before the introduction of restrictions to

[17] We obtain these numbers simply by dividing the market size of Pará (roughly 70% of the Brazilian market) by the average number of yearly deaths caused by prohibition (averaged over the post-2002 period).

mahogany trade. In this respect, columns 3 and 6 imply that, in mahogany occurring areas, mahogany exports were not correlated with violence before prohibition, but "suspected mahogany exports" were significantly correlated with violence after prohibition. This evidence rules out alternative stories associating mahogany extraction – irrespective of its legal status – to violence.

Our final difference-in-difference exercise presents one additional piece of evidence to lend support to the specific story outlined in section 2. In order to shed light on the nature of the increase in violence detected here, we break down homicide rates by demographic characteristics: gender, age, and marital status. For brevity, we consider only our simplest double difference treatment variable and run the specification from column 2 in Table 3 with the three different samples. Results are presented in Table 5, where first we break down homicide rates by gender and then, considering only males, break it down again by age group and marital status (in the latter case, since we do not have population by marital status, we simply divide homicide by marital status by the entire male population). Qualitative results are identical across the three samples: the increase in violence is fundamentally driven by violence against prime-aged single men. Point estimates in column 1, for example, are almost two times higher than the analogous coefficients from Table 3. Since this is also the group most likely to be involved in illegal activities and, more generally, crime, we believe that this result lends additional support to the specific hypothesis outlined in the paper. For example, looking at the coefficients for the second treatment variable in panel B, the increase in homicide rates is 13 times larger for men than for women, 8 times larger for prime-aged men than for other age groups, and 8 times larger for single men than for married men.

Men represent a larger fraction of homicides to start with, so it is also important to compare these effects with the average incidence of homicides across groups. Table 5 also reports the 1995 level of the dependent variable for the different groups considered and the proportional effect corresponding to the estimated coefficients. For panel B, proportional effects are more than 50% larger for men than for women, 35% larger for prime aged men than for men in other age groups, and almost 2 times larger for single men than for married men. Proportional effects are also larger for prime-aged single men in the other samples, though the specific magnitude varies across the samples and demographic groups considered.

In the following subsections, we conduct two final robustness exercises, trying to address any remaining concern related to the causal interpretation of our coefficients. We resort to methodologies that are very distinct from the difference-in-difference strategy in terms of implementation and that deal formally with the comparability of treatment and control groups. One of these methodologies – propensity score matching and weighting – addresses concerns about static differences between the two groups, which may be related to the evolution of the dependent variable over time. The other – synthetic control method – addresses concerns about dynamic differences in the behavior of the dependent variable before the treatment, which may also be related to its evolution after treatment. In both cases, the methodologies create artificial controls, judged to be adequate comparison groups according to some pre-specified metric: static differences in observable variables in the case of propensity score, and the behavior of the dependent variable in the pre-intervention period in the case of the synthetic control.

6.3 Propensity Score Matching and Weighting

To implement the propensity score, we first average all dependent and independent variables over the pre intervention period (1995-1998). We then estimate a probit of the probability that the municipality belongs to the mahogany area on all variables that can be observed at the municipality level: gdp per capita (natural logarithm, observed only in 1996), fraction of gdp in agriculture (observed only in 1996), fraction of area planted, child mortality, heart and circulatory diseases mortality, infectious diseases mortality, neoplasms mortality, suicide rate, mortality by traffic accidents, land conflict deaths, and also homicide rate (the dependent variable before treatment). Following, we use the propensity score in a one-to-one nearest neighbor matching comparison and in an inverse-probability weighted regression.

Since our goal is to minimize problems of heterogeneity and contamination across groups, we use an initial sample of treatment and pool of potential controls that already takes that into account. The evidence from the triple differences and from panel C of Table 3 suggests that the most important effect of prohibition was concentrated in Pará, which played a prominent role in the trade of mahogany. So we consider municipalities in the mahogany area within the state of Pará as our treatment group. As for the control group, our challenge is to select municipalities that were initially similar to the treatment group, and at the same time were less likely to be affected by the mahogany prohibition either directly or indirectly (through spillovers). We therefore consider a pool of potential controls composed of municipalities without mahogany, but located in states with some mahogany occurrence, excluding those in Pará (which may have suffered indirect impacts from what was happening in the large mahogany market in the state). This leaves us with a sample of 425 municipalities, corresponding to municipalities with mahogany in the state of Pará and municipalities without mahogany located in states with some mahogany occurrence (excluding Pará). In all cases we estimate the average treatment effect on the treated, meaning the impact of prohibition on municipalities of Pará that were located in the mahogany area.

Table 6 presents the results from our exercises using the propensity score. We consider two alternative treatment periods (post-1999 and post-2002), analogous to columns 3 and 4 of Table 3, and five different specifications. For purposes of comparison, the first row presents the result from a simple difference-in-difference regression, completely analogous to that estimated before, using the data collapsed in the before-after periods (just two observations in time), without using the propensity score. The second row conducts a simple one-to-one nearest neighbor matching comparison, where the dependent variable is the average homicide rate in the post intervention period. The third row repeats the same exercise, except that the dependent variable is now the change in average homicide rates between the post and the pre intervention periods. The fourth row presents results from an inverse-probability weighted regression, where the dependent variable is the change in homicide rates and the controls are the pre-intervention values of all variables included in the propensity score estimation (including the initial value of homicide rates). Finally, the fifth row presents the results from another inverse-probability weighted regression that includes as controls, in addition to those from the previous specification, the changes in average values of all independent variables between the pre- and post-intervention periods. The inverse probability weighted regressions have the additional advantage of being doubly robust, meaning that misspecification of either the regression equation or the propensity score separately do not harm consistency (Imbens and Wooldridge, 2009). In addition, since we run these equations in differences, the propensity score is robust to selection on unobservables, as long as these unobservables are time invariant (Abadie, 2005). For the interested reader, Appendix Tables A.4.2 and A.4.3 present the results from our propensity score estimation and for a mean difference test between treatment and control for the case of the nearest neighbor matching exercise.

With only one exception, all estimated treatment effects are positive and statistically significant. Though the coefficients vary in magnitude across specifications and treatments, they all fall within the range of those presented in Table 3. The average estimated impact across the propensity score specifications that consider the post-1999 treatment is 11.6, very close to the analogous estimates from column 3 in panels A and B from Table 3, and also close to the simple pre-post difference-in-difference from the first row of Table 6. When considering the post-2002 treatment, the average effect across the propensity score specifications is 19.1, in between the estimates from the analogous specification in column 4 (across the three panels) of Table 3, and slightly above the corresponding coefficient from the before-after exercise in the first row of Table 6. Also as in Table 3, all estimated effects for the post-2002 period are larger than the analogous effects for the post-1999 period.

Overall, qualitative results from the propensity score strategies are remarkably consistent, both quantitatively and qualitatively, with the results from the difference-in-difference strategy. Most importantly, the similarity of results across the various specifications indicates that heterogeneity across control and treatment groups before prohibition does not seem to be affecting the results presented in Table 3. This was already suggested by our previous estimates considering different samples, and it is confirmed here.

6.4 Synthetic Control Method

Our final exercise considers heterogeneity in the time dimension. We use the synthetic control method for case studies proposed by Abadie and Gardeazabal (2003). To keep this exercise as comparable as possible to the propensity score strategies from the last section, we look at the evolution of average homicides within the mahogany areas of the state of Pará, and create a synthetic control group from the areas without mahogany, but located in states with some mahogany occurrence (four states, corresponding to those that have mahogany occurrence, but that also have areas without mahogany, excluding Pará, are used to construct the synthetic control; results are very similar if we use the entire country to construct the synthetic control). The synthetic control is constructed to reproduce as closely as possible the behavior of homicide rates in the mahogany areas of Pará before 1999. The predictors we use to create the weights associated with the synthetic control are the yearly homicide rates before 1999, and all municipality variables averaged over the pre-intervention period (gdp per capita, fraction of gdp in agriculture, area planted, and all mortality variables).

The results from this exercise are presented in Figure 6, plotted as times series. For comparison, we also present the evolution of homicides in the areas of Pará without mahogany. The synthetic control closely matches the evolution of homicides in mahogany areas of Pará in the pre-treatment period. After that, the synthetic control displays a mildly upward trend. As a result, there is a gap in homicide rates between mahogany areas of Pará and the synthetic group that appears in 1999, and then widens up again in 2002. If we compute the average difference in homicide rates between the mahogany area of Pará and the synthetic control after 1999, it amounts to 17.3. If we make the same calculation for the period after 2002, it adds up to 22.1. Both these numbers fall precisely within the intervals estimated with the propensity score strategies (Table 6) and with the difference-in-difference approach (Table 3). Also importantly, Figure 6 shows that the evolution of homicides in the synthetic control after 1999 is not very much different from the evolution of homicides in areas without mahogany in the state of Pará. Once again, the evidence indicates that the results from the difference-in-difference strategy are not contaminated by problems of comparability across control and treatment groups.

7. Concluding Remarks

This paper presents evidence of the increase in violence in Brazilian regions with natural occurrence and trade of mahogany, following the introduction of increasingly restrictive regulations and eventual prohibition of mahogany exploration. Much has been said in the popular press and in the academic literature about the intrinsic association between market illegality and the use of violence. Still, there is very little if any direct causal evidence on this relationship. We present what we believe to be the first piece of evidence on the increase in violence following the complete shutdown of a legal market, and the subsequent appearance of an active illegal market. The increase in homicides we document is not related to changes in socioeconomic conditions, pre-existing trends in violence, or intrinsic characteristics of the good being traded or its consumers. Our evidence points to a causal effect of market illegality, per se, on the incidence of systemic violence.

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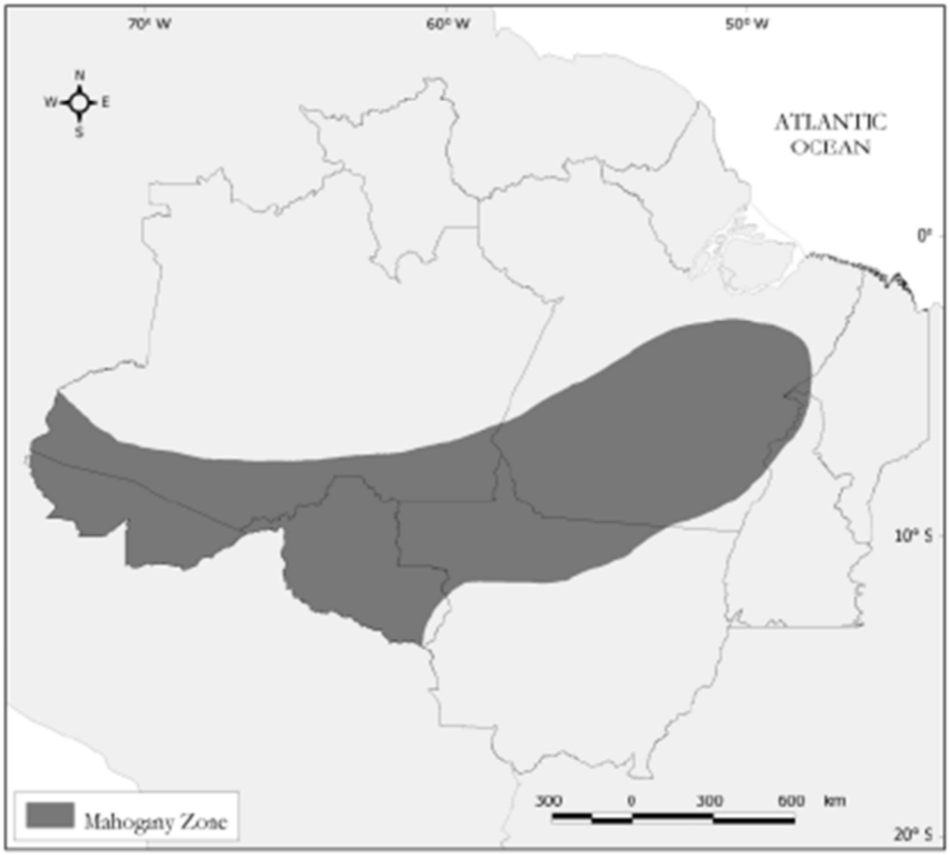
Appendix

A.1 Area of Natural Occurrence of Mahogany in Brazil, from Lentini et al (2004)



A.2 Construction of Export Series

Between January 1989 and December 1995, the Brazilian government used the Brazilian Merchandise Nomenclature (NBM) to code products internationally traded. In January 1996, Brazil started adopting the Mercosur Merchandise Nomenclature (NCM) also used by Argentina, Paraguay and Uruguay. Since most merchandise codes were either consolidated or expanded in the new classification system, MDIC then compiled a list to convert NBM into NCM codes. We used this list to construct our series.

International trade data were reported using both systems in 1996 and we used monthly data for this year to check for possible discrepancies associated with the two classification systems. The case of mahogany exports was straightforward (4407.24.10 in the NCM system corresponds to 4407.23.0102 and 4407.23.0201 in the NBM system) with no discrepancies in 1996. As for other tropical species, exports according to the NCM system (4407.29.90) do not match the summation of the corresponding NBM codes in 1996 (4407.21.0100, 4407.21.0200, 4407.21.9900, 4407.22.0100, 4407.22.0200, 4407.22.9900, 4407.23.0199 and 4407.23.0299). Exports of other tropical species in 1996 according to the NCM system were nil for all Brazilian states, whereas they were positive for parts of the year according to the NBM system. The states that had positive exports were Amazonas, Mato Grosso and Pará, all of them in the Amazon region and with parts of their territory overlapping the area where big leaf mahogany naturally occurs. Their joint exports totaled 1,595,578 Kg in 1996, corresponding to about 2.4% of the annual average for these states between 1989 and 2007. Visual inspection of the data suggests structural breaks in the exports of other tropical species starting in 1999. Since we build our series using the summation of NBM codes prior to 1997, we err on the safe side and make the test for structural breaks more stringent.

We used the same approach to build the series for cedar, ipe and virola-balsa. Cedar's codes are 4407.29.10 from 1996 through 2007 (NCM) and 4407.99.0199, 4407.99.0201 and 4407.99.0399 from 1989 through 1996 (NBM). By using 1996 as a validation year, we build the series using only code 4407.99.0201 for the earlier period. Ipe's codes are 4407.29.20 (NCM), and 4407.99.0199, 4407.99.0208 and 4407.99.0303 (NBM). We use only the last two NBM codes to build our ipe series. Virola-balsa's codes are 4407.24.90 (NCM) and 4407.23.0199, 4407.23.0299, 4407.99.0102, 4407.99.0205, 4407.99.0301 and 4407.99.0399 (NBM). We ignore NBM codes 4407.24.90 and 4407.99.0399 in the construction of our virola-balsa series.

A.3 Other Data on International Trade of Timber

An important branch of the literature on illegal international trade focuses on discrepancies in trade statistics reported by exporting and importing countries (see for example Fisman and Wei, 2004 and 2009, and Ferrantino et al, 2012). If no illegal trade takes place between countries A and B, quantities of a good exported from country A to country B, as reported by A, should be identical to quantities imported by country B from country A as reported by B. By analyzing systematic discrepancies between the data reported by trade partners and by examining tax and tariff structures that might give an incentive to misclassify a traded good in a given country, these studies attempt to unveil the magnitude and characteristics of illegal international trade.

One alternative would be to follow this literature and use data from the World Bank's World Integrated Trade Solution (WITS) database, which is derived from the United Nations' COMTRADE database. These use the Harmonized System (HS) at the 6 digit level. Unfortunately, however, the HS-6 level of aggregation prevents us from focusing exclusively on mahogany. Trade code 440723 refers to "Baboen, Mahogany, Imbuia and Balsa Wood". Furthermore, there is no clear equivalent to "other tropical species" among the available trade codes. In any case, the trade numbers for code 440723 for Brazil and two of its main importers of mahogany – US and UK – suggest reduced quantities until trade progressively declined to insignificant levels. Although consistent trade gaps exist throughout the period, with imports slightly larger than exports in most cases, suggesting some misreporting, there is no direct evidence of smuggling after mahogany prohibition.

Next, we explore alternative datasets on the international trade of timber involving Brazil and the United States to further investigate the hypothesis that mahogany is smuggled out of Brazil as "other tropical timber species." Data for US imports of timber can be obtained from the United States Department of Agriculture, Foreign Agricultural Service through their website (http://www.fas.usda.gov/gats/). The trade codes for the Brazilian NCM-8 and US HS-10 are similar, but not identical (in addition to the obvious two-digit level discrepancy). Furthermore, US data are reported in cubic meters, whereas Brazilian data are reported in kilograms. This difference in measurement may raise comparability problems for trade categories that include more than one species with different wood densities, such as "other tropical timber species". We should therefore use caution when comparing these two series.

Both the US import and the Brazilian export series for mahogany depict a very similar pattern, with declining trends converging to zero after restrictions started being imposed.[18] As for other tropical species, trade is virtually zero for several years and subsequently increases to significantly larger levels, except that the US data show a much smoother increase in imports than the Brazilian export series suggests.[19] It is therefore difficult to obtain deeper lessons from these datasets, other than that mahogany exports went from large levels to zero whereas the opposite happened to other tropical species, as could already be seen from the Brazilian export data alone. So the phenomenon identified by Fisman and Wei (2004) is not taking place here.

In an attempt to obtain further insight into the US-Brazil trade of mahogany, we explore a more aggregate data set that might nevertheless contain some useful information. We can retrieve an aggregate series for US imports of "hardwood lumber" from Brazil (mahogany is a hardwood, in contrast to conifers, for example, that are classified as softwood). The data are aggregated at a level that prevents clear inferences about smuggling of mahogany. Still, if mahogany was an important component of the US timber imports from Brazil, then the Brazilian data for mahogany exports to the US should be correlated with the US imports of Brazilian hardwood when the mahogany market was legal. Furthermore, if smuggling of mahogany as "other tropical timber species" indeed occurred, we would expect the Brazilian data on exports of "other tropical timber species" to the US to be correlated with the US imports of hardwood from Brazil after prohibition, but not before. In that case, we could conjecture that "other tropical timber species" would have assumed the role previously played by mahogany in the overall US imports of Brazilian hardwood. The correlation coefficients between US hardwood imports from Brazil, Brazilian mahogany and "other tropical timber species" exports to the US, as well as the corresponding p-values, appear in Table A.3.1.

For the period before the first estimated structural break (before 85% of the mahogany extraction licenses were cancelled, or January 1989 to July 1999), US imports of Brazilian hardwood were not significantly correlated with Brazilian exports of "other tropical timber species" to the US. This suggests that, before intervention in the mahogany market, the market

[18] The trade codes for mahogany in the US HS-10 nomenclature are 4407.21.0000, 4407230025, 4407230030, 4407.24.00.25, 4407.24.00.26, and 4407.24.00.30. The trade code for mahogany in the Mercosur common nomenclature (NCM – Brazilian series) is 4407.24.10.

[19] The trade codes for "other tropical" in the US HS-10 nomenclature are 4407.29.00.90, 4407.29.00.91, and 4407.29.00.95. The trade code for "other tropical timber species" in the Mercosur common nomenclature (NCM – Brazilian series) is 4407.29.90.

for other tropical species was quite different from that for hardwood, where mahogany belongs. Furthermore, US imports of hardwood were significantly correlated with Brazilian exports of mahogany at the 7% level and with a correlation coefficient of 0.17. This pattern is reversed in the post-intervention periods (August 1999 to December 2007 and November 2001 to December 2007). The correlation between US hardwood imports and Brazilian exports of other tropical species jumps to 0.35 and becomes significant at the 1% level after the formal prohibition of the Brazilian mahogany market (November 2001). During that same period, the correlation between US hardwood imports and Brazilian mahogany exports was negative, reflecting the fact that mahogany exports were declining and started happening only in some isolated cases (mahogany apprehended by the government and impounded cargoes that were subsequently authorized), eventually dropping to zero. Even if indirectly, the correlations presented in Table A.3.1 support the hypothesis that mahogany was smuggled as "other tropical timber species" from Brazil to the US after prohibition.

Finally, the United Nations Environment Program CITES trade database provides information on trade volumes for species listed in one of its appendices. This is the case for mahogany from Brazil. However, since these data are based on official statistics and do not cover species that are not threatened and could serve as a facade for illegal trade of an endangered species, we cannot learn much from the CITES data.

Table 1: Descriptive Statistics - Brazilian Municipalities, Averages of Selected Variables for the 1995-1998 and 1999-2007 Periods

	Municipalities Outside the Mahogany Area, in All Brazil (N=4,811)			Municip. Outside the Mahogany Area, in States with some Mahogany Occurrence (N=465)			Municipalities Outside the Mahogany Area, in the State of Pará (N=84)		
	Homicide Rate	GDP pc	% GDP in Agric	Homicide Rate	GDP pc	% GDP in Agric	Homicide Rate	GDP pc	% GDP in Agric
Pre-1999	25.28	6.12	18.1%	14.22	3.13	30.6%	12.83	2.78	30.0%
	(24.42)	(5.07)	(0.24)	(14.69)	(2.93)	(0.28)	(10.05)	(2.32)	(0.30)
Post-1999	26.64	7.18	9.4%	17.48	4.05	16.7%	16.05	3.23	12.1%
	(20.09)	(5.83)	(0.13)	(14.32)	(3.55)	(0.16)	(14.10)	(3.06)	(0.13)

	Municip. Inside the Mahogany Area (N=163)			Municipalities Inside the Mahogany Area, in the State of Pará (N=44)		
	Homicide Rate	GDP pc	% GDP in Agric	Homicide Rate	GDP pc	% GDP in Agric
Pre-1999	20.63	2.52	31.3%	13.81	1.92	37.8%
	(16.27)	(1.73)	(0.23)	(11.11)	(2.02)	(0.24)
Post-1999	30.78	4.23	21.1%	34.94	4.05	22.8%
	(19.50)	(2.47)	(0.15)	(20.74)	(3.80)	(0.16)

Obs.: Averages weighted by municipality population (standard errors in parenthesis). Variables are homicide rates per 100,000 inhabitants, gdp per capita in 2000 R$ (in thousands), and percentage of gdp in agriculture. Pre-1999 is the average from 1995 to 1998 for homicide rate, and 1996 for gdp per capita and fraction of gdp in agricutlure; Post-1999 is the average between 1999 and 2007.

Table 2: Bai and Perron Test Results, Breakpoints in Brazilian Exports of "Other Tropical Species" - 1989-2007

Monthly exports			Annual exports		
No Controls			**No Controls**		
Break Dates	95% Confidence Interval		Break Dates	95% Confidence Interval	
1999(8)	NA	NA	1998	NA	NA
2003(6)	2003(4)	2003(8)	2002	2001	2003
With Controls (Cedar, Ipe and Virola-Balsa)			**With Controls (Cedar and Virola-Balsa)**		
Break Dates	95% Confidence Interval		Break Dates	95% Confidence Interval	
1999(8)	NA	NA	1998	NA	NA
2003(6)	2003(5)	2003(9)	2002	2001	2003

Obs.: Variable is export of other tropical timber species. Series cover the period from 1989 to 2007. Table reports the results of the Bai and Perron (1998) structural break test, with estimated break dates and respective confidence intervals.

Table 3: Illegality of Mahogany Trade and Homicides, Brazilian Municipalities, 1995-2007, Difference-in-Difference Benchmark Results

	Panel A: Municipalities in All States						
	No Controls	Controls	Only One Treatment Period		+ Mort. Controls	Triple-Diff	
			Post 1999/ Pre 1999	Post 2002/ Pre 1999		State % in Exp. before 1999	Suspect. State Exp. after 1999
Vars	1	2	3	4	5	6	7
treat 1999	3.545*	6.140***	10.91***		3.686*	10.61***	0.0800***
	[2.139]	[2.297]	[2.537]		[2.180]	[3.795]	[0.0277]
treat 2002	11.22***	13.01***		12.63***	9.436***	34.39***	0.165***
	[3.917]	[2.999]		[2.987]	[3.025]	[6.813]	[0.0319]
state level controls and municipality area planted		X	X	X	X	X	X
mortality controls					X		
Observations	64,662	59,080	59,080	49,228	59,060	59,080	59,080
R-squared	0.734	0.786	0.785	0.781	0.806	0.787	0.787

	Panel B: Municipalities in Mahogany States						
	No Controls	Controls	Only One Treatment Period		+ Mort. Controls	Triple-Diff	
			Post 1999/ Pre 1999	Post 2002/ Pre 1999		State % in Exp. before 1999	Suspect. State Exp. after 1999
Vars	1	2	3	4	5	6	7
treat 1999	5.017**	5.546***	11.24***		3.663**	8.884**	0.0616**
	[2.171]	[2.091]	[2.707]		[1.649]	[3.745]	[0.0286]
treat 2002	7.935**	13.98***		14.20***	10.48***	32.85***	0.153***
	[3.132]	[3.396]		[3.496]	[3.126]	[6.993]	[0.0319]
state level controls and municipality area planted		X	X	X	X	X	X
mortality controls					X		
Observations	8,164	7,503	7,503	6,250	7,483	7,503	7,503
R-squared	0.703	0.72	0.716	0.725	0.746	0.729	0.729

	Panel C: Municipalities in Pará				
	No Controls	Controls	Only One Treatment Period		+ Mort. Controls
			Post 1999/ Pre 1999	Post 2002/ Pre 1999	
Vars	1	2	3	4	5
treat 1999	11.66***	11.46***	19.49***		6.597*
	[3.365]	[3.538]	[4.365]		[3.460]
treat 2002	20.80***	23.38***		23.00***	16.60***
	[5.313]	[5.063]		[5.169]	[5.081]
municipality area planted		X	X	X	X
mortality controls					X
Observations	1,664	1,629	1,629	1,250	1,609
R-squared	0.744	0.752	0.741	0.775	0.784

Obs.: Robust standard-errors in brackets (clustering at municipality); *** p<0.01, ** p<0.05, * p<0.1. Dependent variable is the homicide rate (per 100,000 inhabitants). All regressions include a constant, municipality and year dummies, and are weighted by population. Treatment variables are dummies=1 between 1999-2001 and after 2002 interacted with: dummy of mahogany occurring area (columns 1-5); state share in total pre-1999 mahogany exports x dummy of mahogany occurring area (column 6); sum of state exports of mahogany and "other tropical timber species" (which we call "suspected state exports after 1999") x dummy of mahogany occurring area (column 7). State controls are: ratio of top 10% to bottom 40% of income distrib, poverty rates, % of household with more than 2 members per room, primary enrollment rate btwn 7 and 14, informality in labor force, % pop with access to water, % pop with access to sewage, % labor force in agriculture, unemployment, illiteracy, and expenditures on public security p.c. (ln). Municipality controls are % of area planted, mortality by heart and circulatory diseases, neoplasms, infectious diseases, traffic accidents, suicides, child mortality, and assassinations related to land conflicts. Panel A includes municipalities in all states in Brazil, panel B includes only municipalities in states with some occurrence of mahogany, and panel C includes only municipalities in the state of Pará.

Table 4: Illegality of Mahogany Trade and Homicides, Testing for Pre-1999 Placebo Effects, Brazilian Municipalities, 1995-2007, Difference-in-Difference

	Municipalities in All States			Municipalities in Mahogany States			Municipalities in Pará
	Double-Diff	Triple Diff with State % in Product before 1999	Triple Diff with Suspected Mahog. Exp. after 1999	Double-Diff	Triple Diff with State % in Product before 1999	Triple Diff with Suspected Mahog. Exp. after 1999	Double-Diff
Vars	1	2	3	4	5	6	7
treat1	7.724**	11.97**	0.0348	5.831*	7.335*	0.000916	10.86***
	[3.510]	[4.805]	[0.0468]	[2.976]	[4.080]	[0.0450]	[3.787]
treat2	14.60***	35.76***	0.142***	14.27***	31.32***	0.122***	22.79***
	[3.405]	[6.656]	[0.0330]	[3.474]	[6.593]	[0.0309]	[4.890]
placebo	3.125	2.655	-0.105	0.579	-3.034	-0.142	-1.178
	[3.161]	[4.259]	[0.144]	[2.523]	[2.764]	[0.137]	[2.282]
Observatio	59,080	59,080	59,080	7,503	7,503	7,503	1,629
R-squared	0.786	0.787	0.787	0.72	0.729	0.729	0.752

Obs.: Robust standard-errors in brackets (clustering at municipality); *** p<0.01, ** p<0.05, * p<0.1. Dependent variable is the homicide rate (per 100,000 inhabitants). All regressions include a constant, municipality and year dummies, and are weighted by population. Treatment variables are dummies=1 between 1999-2001 and after 2002 interacted with: dummy of mahogany occurring area (columns 1, 4, and 7); state share in total pre-1999 mahogany exports x dummy of mahogany occurring area (columns 2 and 5); sum of state exports of mahogany and "other tropical timber species" (which we call "suspected state exports after 1999") x dummy of mahogany occurring area (columns 3 and 6). State controls are: ratio of top 10% to bottom 40% of income distrib, poverty rates, % of household with more than 2 members per room, primary enrollment rate btwn 7 and 14, informality in labor force, % pop with access to water, % pop with access to sewage, % labor force in agriculture, unemployment, illiteracy, and expenditures on public security p.c. (ln). Municipality control is % of area planted. Columns 1-3 include municipalities in all states in Brazil, columns 4-6 include only municipalities in states with some occurrence of mahogany, and column 7 includes only municipalities in the state of Pará. Pre-1999 placebos vary according to the treatment variable: (i) a dummy for 1997-1998 interacted with the treatment variables (mahogany occurring areas, pre-1999 share of mahogany exports, and the interaction of these two) for 1, 2, 4, 5, and 6; and (ii) yearly mahogany exports before 1999 interacted with a dummy of mahogay areas for columns 3 and 6.

Table 5: Illegality of Mahogany Trade and Homicides, Demographic Profile of Victims, Brazilian States, 1995-2007, Difference-in-Difference

	Panel A: Municipalities in All States					
	male	fem	male prime age	male other age	male single	male married
Vars	1	2	3	4	5	6
treat1	11.44***	0.619	18.63***	2.968***	9.200***	1.564**
	[4.052]	[0.757]	[6.933]	[1.150]	[3.408]	[0.608]
treat2	23.58***	1.113**	38.81***	5.055***	20.76***	2.844***
	[5.657]	[0.446]	[9.488]	[1.306]	[5.115]	[0.696]
1995 Level of Dep. Var.	42.38	4.04	70.02	8.62	26.65	4.41
Proportional Effect	56%	28%	55%	59%	78%	64%
Observations	59,080	59,080	59,060	59,060	59,080	59,080
R-squared	0.793	0.319	0.782	0.327	0.794	0.317

	Panel B: Municipalities in Mahogany States					
	male	fem	male prime age	male other age	male single	male married
Vars	1	2	3	4	5	6
treat1	10.17**	0.76	17.17***	2.278	8.685***	1.802***
	[3.951]	[0.786]	[6.636]	[1.421]	[3.301]	[0.646]
treat2	25.30***	1.833***	42.10***	5.396***	23.01***	2.732***
	[6.440]	[0.588]	[10.95]	[1.596]	[5.684]	[0.807]
1995 Level of Dep. Var.	25.63	2.96	42.16	7.27	15.40	3.39
Proportional Effect	99%	62%	100%	74%	149%	81%
Observations	7,503	7,503	7,483	7,483	7,503	7,503
R-squared	0.721	0.28	0.705	0.371	0.73	0.34

	Panel C: Municipalities in Pará					
	male	fem	male prime age	male other age	male single	male married
Vars	1	2	3	4	5	6
treat1	21.02***	1.685*	34.08***	7.146***	16.65***	4.253***
	[6.845]	[0.899]	[11.55]	[1.593]	[5.535]	[1.280]
treat2	42.19***	2.981***	71.21***	9.350***	38.74***	4.598***
	[9.750]	[0.780]	[17.17]	[1.809]	[9.019]	[1.341]
1995 Level of Dep. Var.	23.77	2.16	40.53	5.63	16.43	2.71
Proportional Effect	178%	138%	176%	166%	236%	170%
Observations	1,629	1,629	1,609	1,609	1,629	1,629
R-squared	0.737	0.36	0.73	0.465	0.738	0.413

Obs.: Robust standard-errors in brackets (clustering at municipality); *** p<0.01, ** p<0.05, * p<0.1. Dependent variable is the homicide rate (per 100,000 inhabitants). All regressions include a constant, municipality and year dummies, and are weighted by population. Treatment variable are dummies=1 between 1999-2001 and after 2002 interacted with a dummy for mahogany occurring area. State controls are: ratio of top 10% to bottom 40% of income distrib, poverty rates, % of household with more than 2 members per room, primary enrollment rate btwn 7 and 14, informality in labor force, % pop with access to water, % pop with access to sewage, % labor force in agriculture, unemployment, illiteracy, and expenditures on public security p.c. (ln). Municipality control is % of area planted. Panel A includes municipalities in all states in Brazil, panel B includes only municipalities in states with some occurrence of mahogany, and panel C includes only municipalities in the state of Pará.

Table 6: Average Treatment Effect on the Treated Estimated using the Propensity Score - Municipalities in Mahogany Areas of Pará and Municipalities outside the Mahogany Area, but located in States with Some Occurrence of Mahogany (excluding Pará) - Outcomes Measured as Averages after 1999 or after 2002

	Treat. Group: Municip. with Mahogany in Pará Control Group: Municip. without Mahogany in States with Mahog. Occurr., excluding Pará	
	Post-1999/Pre-1999 5	Post-2002/Pre-1999 6
Before-after Diff-in-Diff without Matching	12.92** [5.297]	16.83*** [5.983]
Nearest Neighrbor Match; Outcome: Avg Homic	11.85*** [3.565]	18.77*** [3.759]
Nearest Neighrbor Match; Outcome: Δ Avg Homic	18.78*** [4.701]	25.71*** [4.636]
Inverse-Probability Weighted Regression; Outcome: Δ Avg Homic; Controls: Pre-1999 Variables	9.825** [3.836]	18.12*** [3.233]
Inverse-Probability Weighted Regression; Outcome: Δ Avg Homic; Controls: Pre-1999 Variables & Changes	6.073 [4.800]	13.73*** [3.757]

Obs.: Propensity Score estimated from the equation presented in the Appendix Table A2. Details of the methodology explained in the text.

Table A.3.1: US Imports vs Brazilian Exports – Various Periods

	Corr(HW,OT)	Corr(HW,MH)	N
01/89 - 07/99	0.0271	0.1658	127
	[0.7623]	[0.0625]	
08/99 - 12/07	0.3037	0.0183	101
	[0.002]	[0.8559]	
11/01 - 12/07	0.3563	-0.1975	74
	[0.0018]	[0.0917]	

Obs.: p-values in brackets.

Table A.4.1: Illegality of Mahogany Trade and Homicides, Brazilian Municipalities, 1995-2007, Computing Standard Errors Robust to Spatial Correlation and Controling for GDP per capita and % of GDP in Agrictulture

	Municipalities in All States		Municipalities in Mahogany States		Municipalities in Pará	
	Spatial Corr. Robust (Driscoll-Kraay) Std Errors	Controls for GDP pc and % GDP in agric.	Spatial Corr. Robust (Driscoll-Kraay) Std Errors	Controls for GDP pc and % GDP in agric.	Spatial Corr. Robust (Driscoll-Kraay) Std Errors	Controls for GDP pc and % GDP in agric.
Vars	1	2	3	4	5	6
treat 1999	6.140***	7.161*	5.546***	3.505	11.46***	7.688**
	[2.031]	[4.172]	[1.470]	[2.846]	[1.269]	[3.463]
treat 2002	13.01***	12.68***	13.98***	10.69***	23.38***	18.84***
	[2.022]	[3.259]	[0.967]	[3.266]	[1.105]	[3.981]
Observations	59,080	44,264	7,503	5,622	1,629	1,248
R-squared	0.786	0.801	0.72	0.766	0.752	0.808

Obs.: Robust standard-errors in brackets (clustering at municipality); *** p<0.01, ** p<0.05, * p<0.1. Dependent variable is the homicide rate (per 100,000 inhabitants). All regressions include a constant, municipality and year dummies, and are weighted by population. Treatment variable are dummies=1 between 1999-2001 and after 2002 interacted with a dummy for mahogany occurring area. State controls are: ratio of top 10% to bottom 40% of income distrib, poverty rates, % of household with more than 2 members per room, primary enrollment rate btwn 7 and 14, informality in labor force, % pop with access to water, % pop with access to sewage, % labor force in agriculture, unemployment, illiteracy, and expenditures on public security p.c. (ln). Municipality control is % of area planted. Columns 1, 3, and 5 show Driscoll-Kraay standard errors (robust to spatial correlation). Columns 2, 4, and 6 control also for gdp per capita (ln) and % of gdp in agriculture. Columns 1 and 2 include municipalities in all states in Brazil, columns 3 and 4 include only municipalities in states with some occurrence of mahogany, and columns 5 and 6 include only municipalities in the state of Pará.

Table A.4.2: Propensity Score Probit Estimation, Municipalities in Mahogany Areas of Pará and Municipalities outside the Mahogany Area (but located in States with Some Occurrence of Mahogany, excluding Pará), Averages between 1995-1998

Vars	Dep. Var: Mahogany Area Dummy
ln_gdp_pc	-0.0189
	[0.194]
% gdp agric	-0.215
	[0.535]
% area plant	-2.659
	[2.503]
child mort	0.268***
	[0.0725]
heart mort	-1.708***
	[0.615]
infecc mort	-1.447*
	[0.821]
neop mort	-1.747
	[1.237]
suic	-4.603
	[3.974]
traff mort	1.444
	[1.004]
polit deaths	25.38***
	[9.203]
homicide	0.0185**
	[0.00865]
Observations	425

Obs: Robust standard-errors in brackets; *** p<0.01, ** p<0.05, * p<0.1. Dependent variable is a dummy indicating that a municipality is located in the mahogany occurrence area. Independent variables are averages between 1995 and 1998 of gdp per capita (ln, 1996), fraction of gdp in agriculture (1996), % of area planted, mortality by heart and circulatory diseases, neoplasms, infectious diseases, traffic accidents, suicides, child mortality, assassinations related to land conflicts, and homicide rate.

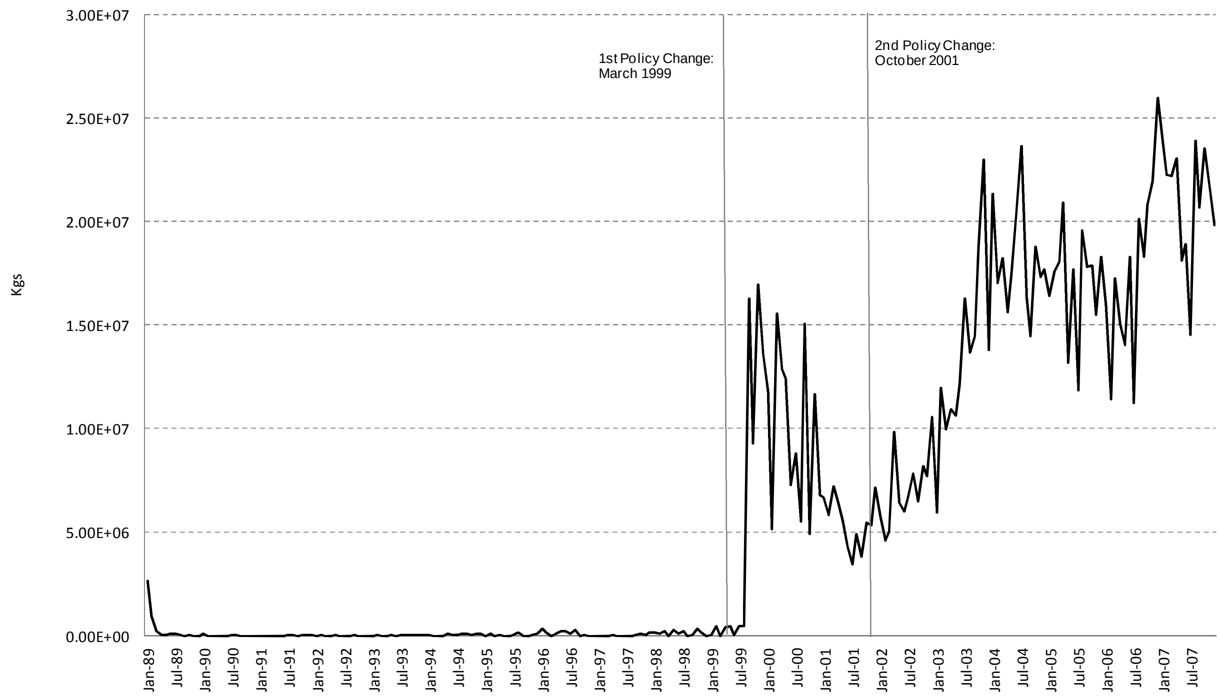
Table A.4.3: Mean-Differences Test for Nearest Neighbor Match on Pre-1999 Averages - Municipalities in Mahogany Areas of Pará Matched to Municipalities outside the Mahogany Area, but located in States with Some Occurrence of Mahogany (excluding Pará)

Variable	Unmatched/Matched	Mean		Diff. Treat x Control	
		Treated	Control	t	p>|t|
ln_gdp_pc	Unmatched	0.3505	0.3945	-0.41	0.68
	Matched	0.3505	0.4087	-0.4	0.69
% gdp agric	Unmatched	0.4960	0.4997	-0.12	0.91
	Matched	0.4960	0.4620	0.74	0.46
% area plant	Unmatched	0.0247	0.0437	-1.82	0.07
	Matched	0.0247	0.0223	0.38	0.70
child mort	Unmatched	2.9644	2.2828	2.05	0.04
	Matched	2.9644	2.2557	1.31	0.20
heart mort	Unmatched	0.2655	0.4723	-3.34	0.00
	Matched	0.2655	0.2783	-0.27	0.79
infecc mort	Unmatched	0.2673	0.3198	-1.34	0.18
	Matched	0.2673	0.2286	0.94	0.35
neop mort	Unmatched	0.0878	0.1463	-2.58	0.01
	Matched	0.0878	0.1150	-1.06	0.29
suic	Unmatched	0.0105	0.0197	-1.64	0.10
	Matched	0.0105	0.0061	1.24	0.22
traff mort	Unmatched	0.0829	0.0935	-0.48	0.63
	Matched	0.0829	0.1190	-1.35	0.18
polit deaths	Unmatched	0.0125	0.0012	5.33	0.00
	Matched	0.0125	0.0111	0.22	0.83
homicide	Unmatched	11.7260	8.5825	1.47	0.14
	Matched	11.7260	13.2440	-0.52	0.60

Obs.: Variables are averages between 1995 and 1998 of gdp per capita (ln, 1996), fraction of gdp in agriculture (1996), % of area planted, mortality by heart and circulatory diseases, neoplasms, infectious diseases, traffic accidents, suicides, child mortality, assassinations related to land conflicts, and homicide rate.

Figure 1: Exports of Mahogany and Other Tropical Timber Species

(a) Exports of Other Tropical Timber Species (in Kgs), Brazil, Monthly Data, 1989-2007



(b): Exports of Mahogany and Other Tropical Timber Species (in Kgs), Brazil, Annual Data, 1989-2007

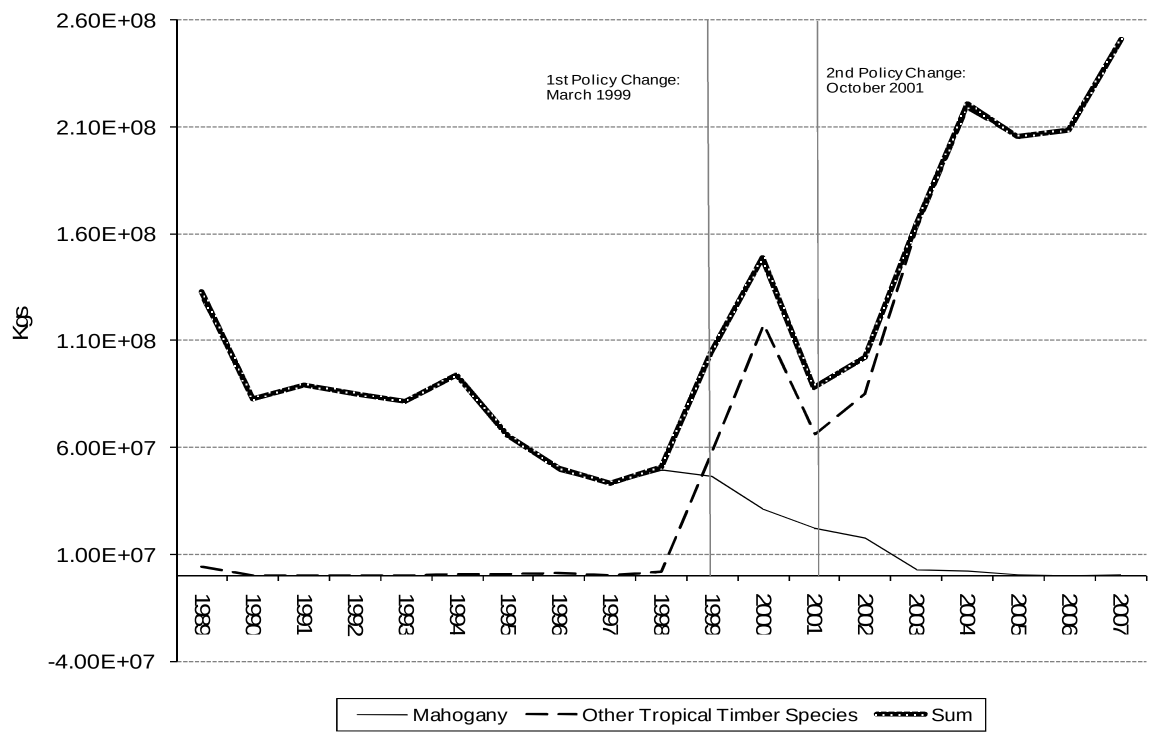


Figure 2: Homicide Rates in Municipalities Inside and Outside the Area of Natural Occurrence of Mahogany, Pará, Brazil, 1995-207

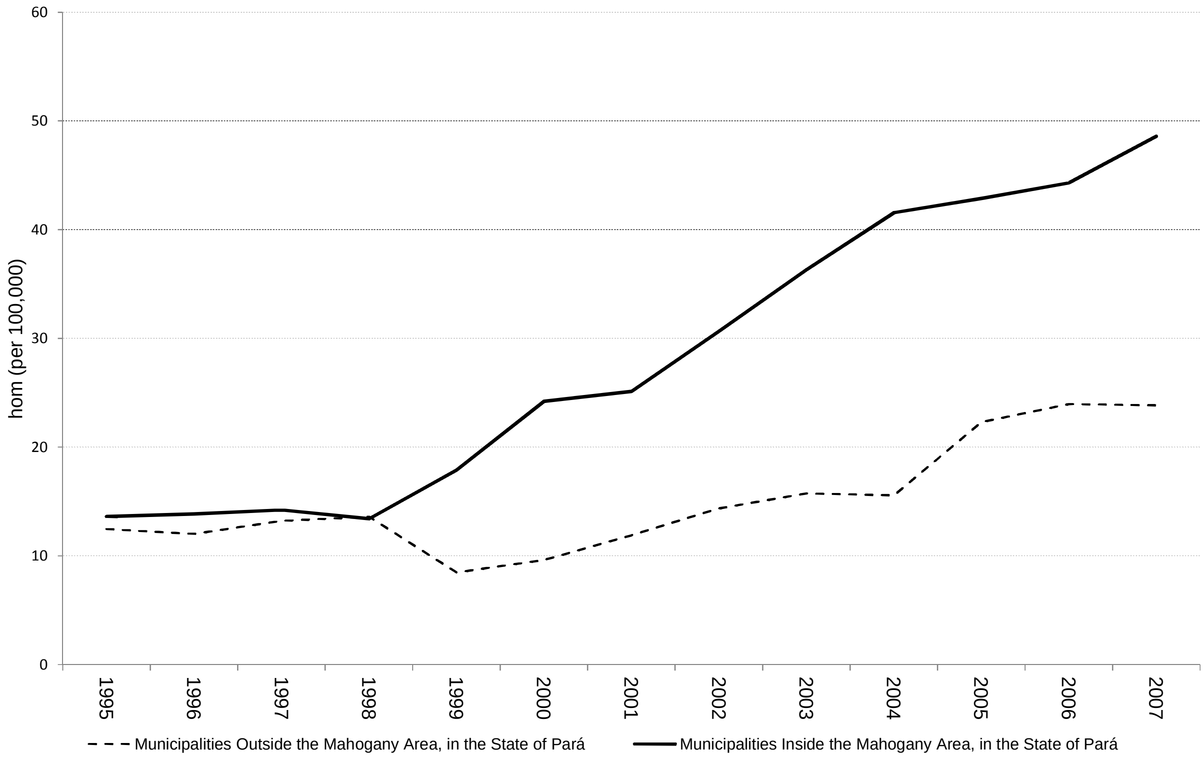


Figure 3: Estimated Structural Breaks, Exports of "Other Tropical Timber Species" from Brazil, 1989-2007 – Monthly and Annual Data

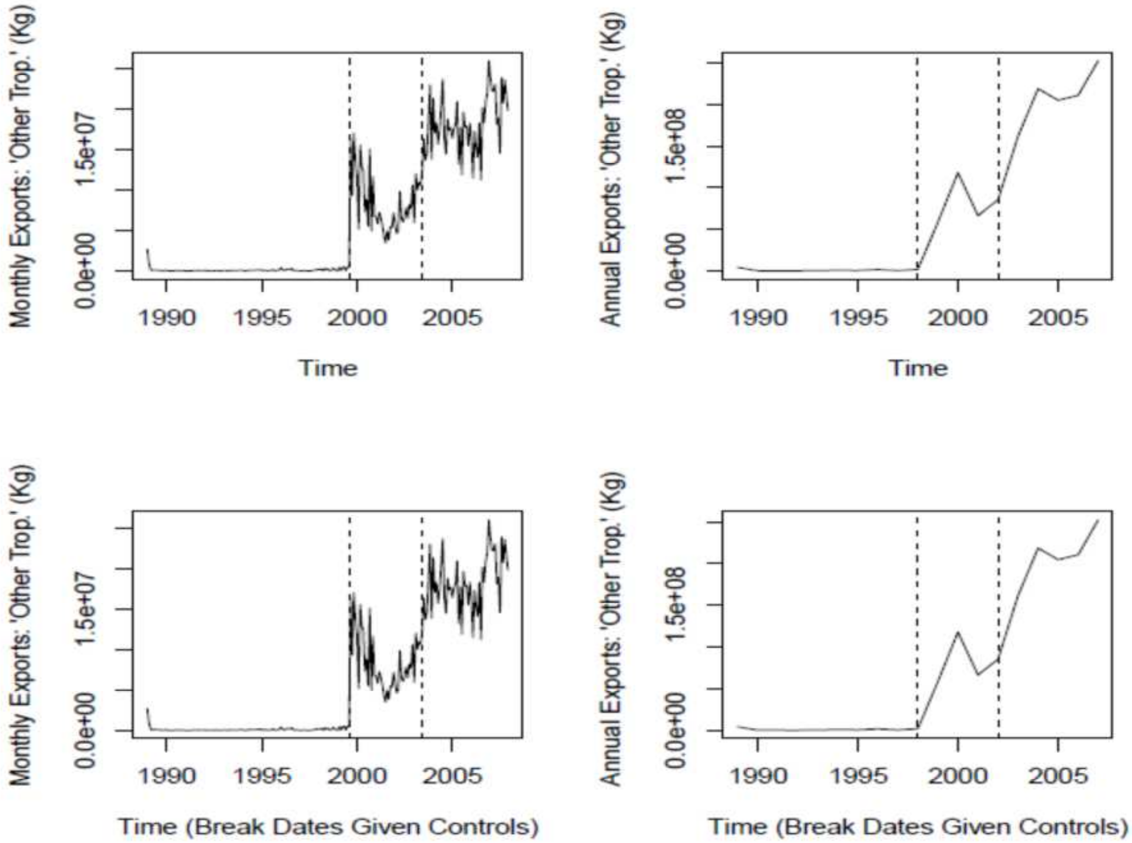


Figure 4: Brazilian Exports of Ipe, Cedar, Virola-Balsa and Conifers, 1989-2007 – Monthly Data

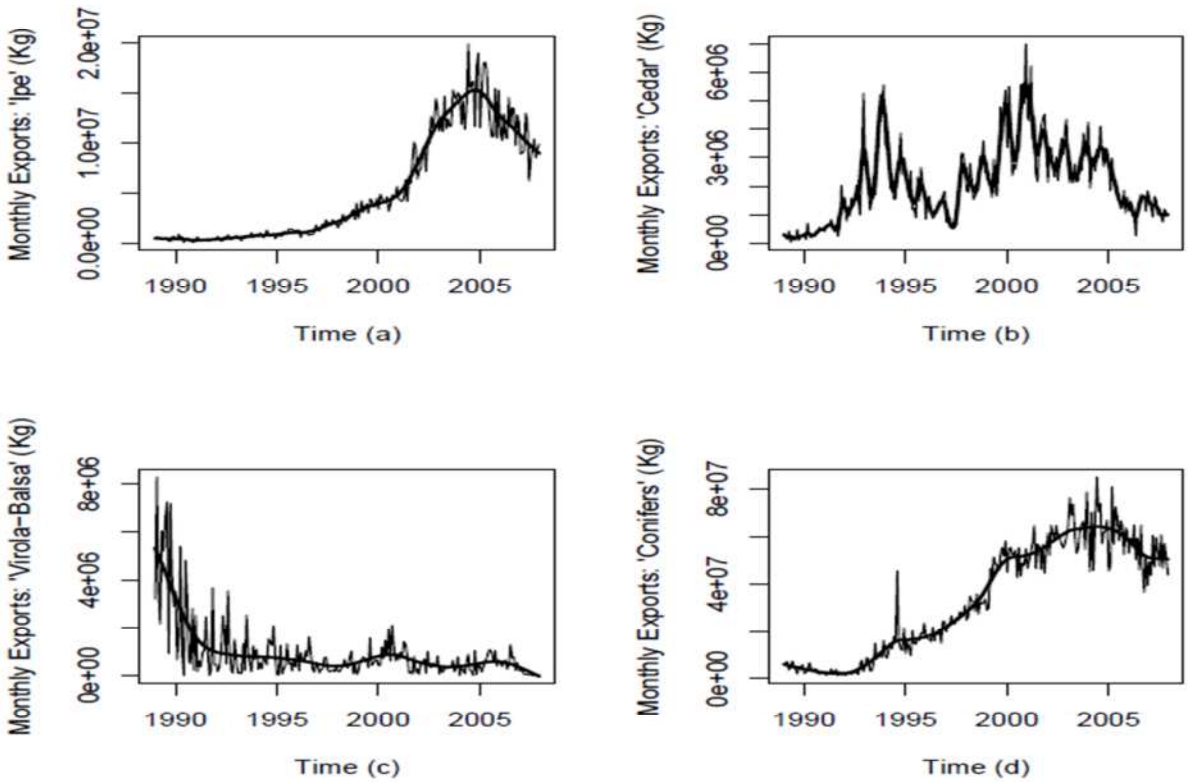
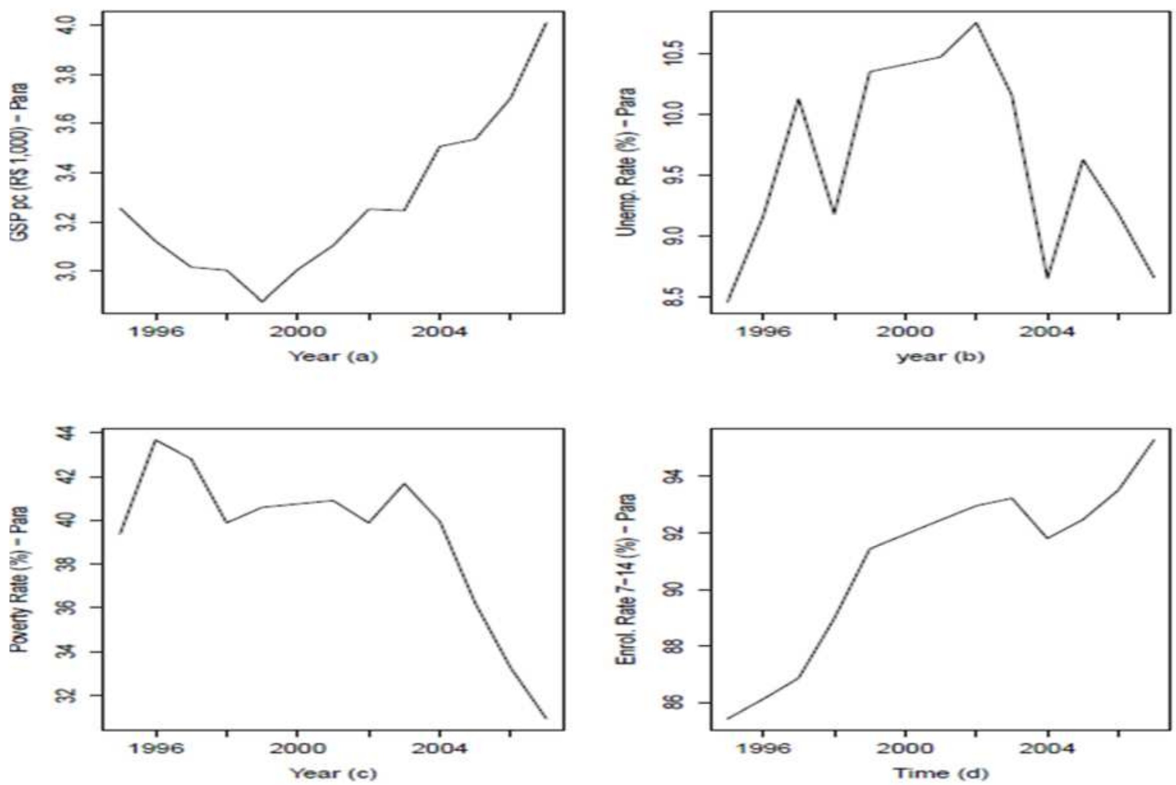


Figure 5: Socioeconomic Indicators for the State of Pará, 1995-2007 – Yearly Data



Obs: Data for 2000 interpolated because there was no Brazilian National Household Survey (PNAD) in that year.

Figure 6: Homicide Rates in the Synthetic Control and in Municipalities Inside and Outside the Area of Natural Occurrence of Mahogany, Pará, Brazil, 1995-2007