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Essays in Regulatory Economics

By

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Professor Peter Berck
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Abstract

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This dissertation consists of three essays. The objective of the essays is to study the impacts of different regulations on the behavior of regulated agents. The first two essays focus on the analysis of non-traditional regulatory policies that complement traditional regulations consisting of inspections and fines for plants that violate regulations. The third essay studies the impacts of the Minimum Legal Drinking Age regulation on alcohol and marijuana consumption.

The first essay of this dissertation analyzes the effects of disclosing information online and through the newspapers about Mexican gas stations that cheat the consumer by selling *chiquilitros* (liters that are less than a true liter). The information about gas stations that commit fraud is revealed through random inspections that the Consumer Protection Agency (PROFECO by its Spanish acronym) conducts on all gas stations in Mexico and is disclosed in PROFECO's website. Newspapers in different municipalities also publish reports with lists of gas stations that are reported by PROFECO as being in violation of regulations. Using data on inspection histories and local news reports, we estimate the impact of disclosing information online and through the newspapers on the probability of future regulatory compliance. Our findings show that disclosing information online significantly improves compliance with regulations. In contrast, newspaper reports are only effective at improving compliance rates for those gas stations that had been found in violation prior to the publication of the reports. One of the main reasons gas stations improve their behavior is that their sales are negatively affected as a result of bad publicity in the newspapers. Using a unique dataset with monthly gasoline sales at the gas station-level, we show that gas stations that were reported in the newspaper reports suffered a loss of sales of 2.2% to 2.4% in the month of the publication. The results suggest that public disclosure of firm's behavior through the media can serve as a complementary tool for inspections and fines in contexts were fines and sanctions are limited.

The second essay studies the impacts of self-policing policies to induce environmental audits. State-level statutes in most of the states of the US provide firms that engage in environmental self-audits and that self-report their environmental violations, with a variety of different regulatory rewards, including "immunity" from penalties and "privilege" for information contained in self-audits. These regulations have been controversial in the

environmental arena. Critics argue that they provide with incentives to polluters to reduce the level of care, increasing toxic emissions and inspection costs. Proponents argue instead that these regulations can effectively induce more care by polluting plants and lower EPA's enforcement costs. We find that, by encouraging self-auditing, privilege protections tend to reduce pollution and government enforcement activity; however, sweeping immunity protections, by reducing firms' pollution prevention incentives, raise toxic pollution and government inspection oversight. We conclude that self-policing policies that grant limited incentives to firms to self-audit are effective at reducing both toxic emissions and government enforcement effort, whereas those regulations that grant excessive protection by reducing the penalty from disclosed violations, increase both toxic emissions and enforcement costs.

The third essay estimates the causal effect of increased availability of alcohol on marijuana use. We exploit the Minimum Legal Drinking Age regulation that restricts the consumption of alcohol for people younger than 21 and compare alcohol and marijuana consumption in individuals just below and just above the age of 21. We show that both the probability and frequency of marijuana consumption decrease sharply at age 21, while the probability and frequency of alcohol intake increase, suggesting that marijuana and alcohol are substitutes. We further find that the substitution effect between alcohol and marijuana is stronger for blacks than whites and for women than men. Overall, our results suggest that policies designed to limit alcohol use have the unintended consequence of increasing marijuana use.

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Chapter 1

Who is Selling You *Chiquilitros* of Gasoline?

1.1 Introduction

Every year, the Mexican Consumer Protection Agency, PROFECO, inspects all gas stations in Mexico to make sure they supply gasoline according to federal standards. These inspections often reveal gas stations sell *chiquilitros* (little liters) of gasoline or liters that are less than a true liter. The selling of *chiquilitros* is detected when inspectors find differences between the actual volume dispensed and the volume registered in the pump's register. Cheating is done by altering the mechanic and electronic components of the gasoline dispensers.

Despite constant monitoring and sanctioning of gas stations, cheating practices are prevalent: 30% of inspections conducted during the first half of 2006 detected violations. Sanctions have not been enough to deter violations mainly because they are not always imposed and when they are, they are small relative to the gains from cheating. During 2006, more than 900 inspections detected violations but only in 278 of those cases did the courts impose fines.[1] In Mexico City, fines imposed on gas stations caught selling *chiquilitros* represented 37% of their annual estimated gains from cheating.[2]

In August of 2006, in an attempt to increase its regulatory powers, PROFECO started disclosing the inspection histories of all gas stations on its Internet site, where the outcomes of the most recent inspections are rated using a traffic color system indicating the severity of the violation. Following PROFECO's launching of their Internet tool, several local newspapers in various cities started publishing PROFECO's Internet lists of gas stations classified as violators.

This paper estimates the impact of disclosing the inspection histories of gas stations on the website and through newspaper publications on the probability of violating regulations. It also evaluates if gas stations reported in the newspapers suffered a decline on their sales at the month the reports were published and during the following months. Public disclosure in the context of the Mexican gasoline market is of particular importance since it is highly regulated by PEMEX, the national oil company. In the Mexican gasoline market, all gas stations are franchises of PEMEX, they have to display PEMEX logos and cannot freely set their prices.[3] The paper shows that consumers do use the information disclosed in the newspapers to discriminate across gas stations.

Policies that mandate disclosure of information have been applied in different markets and most of the literature shows that uncovering information to the public has positive impacts on welfare (Jin and Leslie, 2003; Ferraz and Finan, 2008; Cohen and Konar, 1997; Garcia, Sterner and Afsah, 2007; Khanna, Quimiu and Bojilova, 1998; Scorse, 2010; Hamilton, 1999). However, in some cases, information disclosure can lead to welfare losses. Dranove et al. (2003) show that disclosing hospital report cards in New York and Pennsylvania about coronary artery bypass graft surgery mortality rates lowered social welfare as low quality health care providers selected less sick patients in order to increase their ratings and more surgeries were conducted on healthier patients. Information disclosed as a result of public disclosure policies has also been

[1] Every time a gas station is found in violation PROFECO starts an administrative legal process against the gas station that usually gets resolved in the courts after long legal battles.
[2] Based on my own calculations.
[3] Gasoline price is set by the government and is adjusted according to fiscal needs. There is some price variation across municipalities. Municipalities that share a border with a US city adjust their price on a weekly basis according to the average gasoline price registered the previous week in the US contiguous city. The rest of the municipalities fall under the same price zone and its price is adjusted on a monthly basis.

criticized for being ineffective at persuading consumers (Weil et. al., 2006; Magat and Viscusi, 1992), which brings into question the cost effectiveness of such programs.

An open question in the literature is therefore, if consumers respond to the disclosure of firms' information by third parties and, if they do what is the magnitude of the response. Two obstacles have limited the empirical work in this area: 1) obtaining detailed micro-data on sales, profits or revenues at the firm-level is difficult and 2) markets usually adjust to information disclosed via prices or quality, which makes the analysis of consumer's reaction to information challenging. Past studies fail to fully identify the effects of information disclosure on product demand either because they lack direct measures of quantity demanded per firm or because they do not consider the effect that information disclosure has on prices (Jin and Leslie, 2003; Beach et. al., 2008 and Piggot and Marsh, 2004).

This paper makes four contributions to the existing literature on information disclosure impacts. First, by using a unique dataset with monthly gasoline sales at the gas-station-level and by focusing on a market where firms cannot freely adjust their prices, it identifies changes in gasoline demand as a result of changes in quality, induced by disclosure of information about firm's product quality, avoiding price endogeneity. Second, in contrast with past studies that analyze the impacts of information disclosure on inspection outcomes, it considers the joint determination of violations and inspections.[4] Third, it evaluates the impacts of two sources of information disclosure, Internet and newspapers, and demonstrates that they have different impacts. Finally, it is one of the few studies in a developing country that studies public disclosure mechanisms using detailed firm-level data on inspections, violations, sales and gas stations characteristics.

To identify the impacts of the launching of the website, I exploit the fact that the starting of the website was a sudden event and unanticipated by gas station owners and test for changes in the probability of violating regulations before and after the website started. Identification of the effects of newspaper reports comes from variation in timing of the publication of the first report in different municipalities. I focus on the first newspaper publication because most of the municipalities had only one newspaper publication during the period of study (2006-2008) and also because once the first newspaper report is released in a municipality, gas stations located in those municipalities may correct their behavior in response to the possibility of being exposed in future reports. I show that once municipality time-invariant characteristics are controlled for, the timing at which the first report is published in a given municipality is uncorrelated with time-varying characteristics of the gasoline market in the municipality, and can be considered for the most part as an exogenous source of variation.

To estimate the impacts of the website and newspaper reports on the probability of violating regulations I estimate a bivariate probit model with sample selection, where the probability of violation and the probability of inspection are jointly estimated taking into account that violations are only observed if an inspection occurred. To evaluate the impacts of newspaper reports on gasoline demand I estimate an OLS model with gas stations fixed effects on the volume of gasoline sold per month as a function of the number of times a gas station was reported in a given month. I also include lags of the newspaper reports variables to test its lasting effect.

The findings show that the website had negative and significant impacts on the probability of violating regulations. The website is estimated to have reduced the probability of violating

[4] For example, Jin and Leslie's (2003) study on the impacts of disclosure of restaurant hygiene score cards in Los Angeles does not address the impact of the number of inspections on restaurants' hygiene levels.

regulations by 20%. Larger gas stations were more likely to be found in violation, belonging to a chain and average years of schooling in the areas where gas stations are located were negatively associated with the probability of violation. I also find evidence of targeted enforcement by PROFECO: gas stations that were detected in violation in past inspections were more likely to be inspected in current periods and gas stations located in the same geographic area were more likely to be inspected during the same month.

Consistent with past findings (Scorse, 2010; Hamilton, 1999; Garcia, Sterner and Afsah, 2007), I observe the website and newspaper reports had larger impacts on gas stations that, before the first newspaper report was released in a given municipality, had poor compliance histories. Public disclosure online and through the newspapers is estimated to have each reduced the probability of violating regulations by 27% for those gas stations with a history of noncompliance before the first newspaper was published in a given municipality. Moreover, within the set of gas stations with a history of noncompliance before the reports were published, newspapers are estimated to have had similar impacts on the probability of violation, regardless of whether or not they appeared in the newspapers. Thus, gas stations with poor compliance histories may have improved their behavior to avoid potential market losses from being exposed in the news.

Results of the impacts of newspaper reports on gasoline sales show that gas stations that were reported in the newspapers lost between $498 and $539 per report. This amount represents a decline between 2.2% and 2.4% of gas stations monthly sales and a 15% increase in the average fine imposed on violators. The economic losses were temporary and only incurred at the month the newspapers were released. Hence, newspapers induced improvements in behavior possibly because they increased the costs of noncomplying with regulations.

Overall the paper shows that public disclosure mechanisms in a context where fines are limited and regulatory agencies are weak in terms of their capacities for fining violators can improve behavior and serve as a complementary mechanism to regulate firms.

The rest of the paper proceeds as follows: Section 1.2 reviews PROFECO's information disclosure program. Section 1.3 describes the possible impacts of disclosing information about gas stations inspection outcomes on the Internet and through the newspapers on the probability of violating regulations as well as on gasoline demand. Section 1.4 presents the data, data sources and descriptive statistics. Section 1.5 discusses the sources of identification and the empirical strategy for estimating the impacts of the website and the newspaper reports on the probability of violating regulations. Section 1.6 shows that newspaper reports have similar impacts on gas stations with a history of noncompliance before the first newspaper report was released, regardless of whether or not they appeared in the news. Section 1.7 estimates the impacts of newspaper reports on gasoline demand. In Section 1.8 I conduct a series of robustness checks on the estimation of the impacts of the website and newspaper publications on the probability of violation. Finally, Section 1.9 concludes.

1.2 PROFECO's Information Disclosure Policy

Since 2005, the Mexican government has taken several measures to prevent gasoline retailers from defrauding the consumer. In 2005, PROFECO's inspection policies were changed. The new rules, NOM-005-SCFI-2005, incorporated more accurate gasoline sampling techniques to detect the selling of *chiquilitros* and mandated the verification of the electronic components of the dispensers.[5] In August of 2006, PROFECO announced to the media its new website tool where consumers and interested parties in general can access the ratings and the inspection histories of each operating gas station since 2005. This announcement was unexpected by gas station retailers according to an interview I had with a PROFECO official. Several attempts to block the access to the website by hackers after it went live also confirm that the launching of the website was sudden and unanticipated by gas station owners (El Universal, 2006). In the empirical analysis I use this exogenous change in the regulation to estimate the new regulations' impact on the probability of cheating.

Every year PROFECO determines the calendar of inspections. PROFECO's goal is to inspect every gas station at least one time a year and ratings are generally updated every month. If a gas station was inspected since the previous month, the rating is changed to reflect the most recent inspection outcome. If no inspection has occurred in a gas station during the previous 180 days the rating is discarded and that gas station appears without rating. This rule PROFECO uses to discard ratings induces exogenous within city variation of exposure to ratings since length of exposure to a given rating is based on timing at which inspections occur, which is random for each gas station. This source of variation is exploited in the empirical analysis to compare gas stations that were caught in violation and were exposed in a newspaper report and gas stations that were found in violation but were not exposed in the news.

PROFECO’s ratings are assigned according to the severity of the violation detected. Red ratings are given when any of the following violations are detected: pumps that dispense less than 0.985 liters per alleged liter sold, price alterations, broken or altered security seals, adulterating the gasoline content (gasoline mixed with other substances), modifications of the electronic and mechanic components of the dispensers, or refusal to be inspected by the manager of the gas station.[6]

Yellow ratings are assigned when pumps are found to dispense between 0.995 and 0.985 liters per liter sold, pump leaks are detected or the gas station has pumps out of service, also when dispensers do not satisfy minimum technology standards according to Federal standards (NOM-005-SCFI-2005).[7] Green indicates that the inspection did not find any anomalies. Gas stations that have not been inspected recently are given no rating. More than 50% of the violations detected by PROFECO's inspectors involve the selling of *chiquilitros* or differences between volume dispensed and volume sold, as registered in the pumps register.

1.3 Possible impacts of media disclosure on the probability of violating regulations and on gasoline sales

Regulation of gas stations in Mexico is characterized by low fines and low monitoring frequencies. Before PROFECO's website disclosed gas stations, gas stations had on average 1 inspection per year. When inspectors detect violations in a pump they shut it down and file a legal process against the gas station that usually results in a fine. In Mexico City, fines assessed against gas stations found selling *chiquilitros* during the first half of the year 2006 amounted to

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[5] The old version of the norm NOM-005-SCFI-2005 only considered the verification of the mechanic components of the dispensers.

[6] Dispensing less than 0.985 liters per liter and rejection of inspection comprised 90% of red violations.

[7] The most common reason for assigning a yellow rating in my panel was dispensing between 0.995 and 0.985 liters per liter sold.

37% of their gains from cheating and,[8] during the same period, around 70% of all the inspections conducted by PROFECO before the implementation of the website did not find violations. This apparent "puzzle" where the net benefit of noncompliance is higher than zero and yet firms comply with regulations has been documented in other contexts such as the regulation of polluting plants in the United States and Canada (Harrington, 1988; Helland, 1998; Laplante, 1996).

Harrington (1988) offers a theoretical explanation of this "puzzle" by using a dynamic game in which regulators and firms interact over time. In Harrington's model, the regulator classifies firms into two groups, the good and the bad, according to their compliance histories. Firms that belong to the bad group are those found in violation. Firms in compliance are classified as good firms. High compliance rates are guaranteed by a targeted enforcement strategy, where firms that are in noncompliance face more frequent inspections and higher penalties. Firms with low gains from cheating always comply and firms with high gains from cheating always violate regulations. Firms in the middle move from compliance to noncompliance according to their inspection histories.

In accordance with Harrington's model, I should expect gas stations that were found in violation in previous inspections to be more likely to be inspected in current periods and also gas stations with higher gains from cheating to be more likely to violate regulations.
Public disclosure on the website and through the newspapers increases the expected costs of noncompliance for all gas stations since consumers can potentially use that information to discriminate across gas stations. Therefore, the launching of the website and the publication of newspaper reports are expected to decrease the probability of violating regulations across all firms.

Due to PROFECO's rule to update the ratings disclosed on its website, where gas stations that have not been inspected recently are given no rating, it is possible that disclosing inspection outcomes may have stronger effects on the group of firms that are more prone to violate regulations (the group of bad firms in Harrington's model). The reason is that those firms will be more likely to have active ratings in the website, as they are inspected more often, and consequently, may be more likely to appear in the news. Thus, I expect disclosure of inspection outcomes on the website and through the newspapers to have larger impacts on the group of gas stations that have a history of noncompliance.

Given that newspapers only published reports with gas stations in violation, the impact of media coverage on gasoline demand depends on the proportion of consumers that become informed after the information is released to the public and are willing to travel to other gas stations in order to avoid the risk of being defrauded.[9] If consumers' expected cost for avoiding being cheated on is lower than their expected benefit then I should expect to see significant decreases in gasoline demand in gas stations exposed in the news. Empirical research suggests that information revealed by the media has temporary effects on behavior, even if the information reveals bad news to the consumer (Beach et. al., 2008; Piggot and Marsh, 2004;

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[8] Gas stations that were found in severe violation for selling *chiquilitros* in Mexico City during the first half of 2006 altered on average 3.6 pumps and sold 30 milliliters less per liter dispensed. They sold 41,380 liters of gasoline per pump. Their monthly gains from cheating roughly amounted to $2,681, considering the price per liter of gasoline was $6 during the first half of 2006. In contrast, the average fine imposed on those gas stations was $6,000. Considering gas stations in Mexico City are inspected around 2 times a year, their annual gains from cheating amounted to $32,177, while the total sanction, assuming they were sanctioned every time they were inspected, was $12,000.

[9] In this paper I will only test the impact of exposure on the newspapers on gasoline sales. I do not test for gasoline demand responses to the website because I do not have the exact date at which PROFECO updates the website and also because the constant updating process of the ratings makes it difficult to track changes of demand over time.

Zivin and Neidell, 2009; Kiesel, 2010; Piggot and Marsh, 2004). In accordance to those findings I anticipate to find temporary impacts of newspaper publications on gasoline demand.

1.4 Data

The dataset is a monthly panel for the period 2006-2008 with data on the number of inspections, number of inspections that detected violations, monthly retailers' gasoline purchases to PEMEX, which is a proxy for sales,[10] gasoline price, total number of pumps, storage capacity, chain,[11] population and average years of schooling in the census block where the gas station is located. The sample has 96,588 observations comprising 2,683 gas stations over 36 months. Additionally, using geostatistical software, I created three variables to capture spatial externality effects: the number of competitors (gas stations) located within a radius of 1 km from each gas station, percentage of competitors within 1km of (euclidian) distance that were listed in a newspaper at a given month and percentage of competitors located within a radius of 1 km that were inspected at a given month. I focus my analysis on the 48 largest municipalities in Mexico, representing all states in the country. Table 1.1 presents summary statistics.[12]

In the sample, every gas station was inspected on average 1.4 times a year, 22% of the inspections found a violation, 36% of those inspections detected a severe violation and 56% of gas stations were found in violation at least one time from 2006-2008. On average, every month, 10% of gas stations in a municipality was inspected. Newspaper reports were compiled from the websites of local newspapers from each of the 48 municipalities. A total of 31 municipalities had at least one newspaper report during the period of study. Reports from newspapers with no website engines or with no search utilities are not included in the sample.[13] Newspaper reports vary in terms of the information provided, day of the week of publication and the section where they appeared. All of them had the characteristic that only reported violators, either by listing their ratings or by mentioning that those gas stations that were listed in the report were found in violation by PROFECO's inspectors and reported in PROFECO's website. In the reports, violators were identified by either listing their PEMEX ID, which is displayed at the gas station, or by the address of the gas station. In some cases the reports explicitly mentioned the ratings assigned by PROFECO (red or yellow) and in some other cases they only reported that those gas stations were found in violation and provided a list with the types of violations detected by PROFECO. In this paper I consider the total effect of newspaper publications without distinguishing between types of reports as I am interested in the overall response to information disclosure. A total of 49 reports were recorded for the period 2006-2008, with 138 gas stations reported. Of those gas stations, 119 had one single report, 17 were reported two times and 2 had three reports.

[10] The use of monthly purchases by gas station has the additional advantage that it is measured in complete liters and not in *chiquilitros*.
[11] In some cases multiple franchises belong to a single chain or owner. In my sample, 30% of gas stations belong to chains.
[12] Due to imperfect spatial matching and missing values from the census data, the variables from the census have fewer observations.
[13] Focusing on newspapers with website engines induces selection in terms of newspaper coverage as small papers are unlikely to have website engines. However, it is more likely that consumers and gas station owners react to information from large newspapers than to small newspapers, with circulation rates of less than 2,000. The range of circulation rates of the newspapers recorded in my sample is 2,140 to 292,618.

1.5 The impacts of media disclosure on violations

For estimating the impacts of disclosure of information on the website on the probability of violating regulations I exploit the fact that it was a sudden and unanticipated event for gas station owners, inducing an exogenous change in regulation. Figure 1.1 presents graphical evidence of the impacts of the website on the percentage of inspections that detected violations. The vertical line indicates the time at which the website was introduced. Although Figure 1.1 shows a decline on the average percentage of inspections that found violations after the introduction of the website, it may be misleading to attribute the improvement in inspection outcomes solely to the presence of the website given that violations are only observed if an inspection occurred. Changes in the number of inspections may also influence the probability that violations occur as Figure 1.2 shows. Figure 1.2 plots the number of inspections conducted before and after the website. The influence of inspections on the probability of violating regulations was particularly important in the Summer of 2007 (around month 20 in Figures 1.1 and 1.2) when the number of inspections registered a peak.

Identification of the newspaper impacts comes from variation in timing at which newspapers in different municipalities published the first newspaper report about gas stations in those municipalities with red or yellow ratings in PROFECO's website. I focus on the first newspaper publication for two reasons. First, most of the municipalities had only one newspaper publication.[14] Second, once the first newspaper report is released in a municipality, gas stations may correct their behavior in response to the possibility of being exposed in future reports.

To illustrate the relationship between violations, inspections and the time at which the first newspaper report was published I created two figures. Figures 1.3 and 1.4 plot the percentage of inspections that found violations and the number of inspections one year before and one year after the first time a local newspaper published a newspaper report as indicated by the vertical line, respectively.[15]

There is significant variation in timing of the publication of the first newspaper report across municipalities. Figure 1.4 plots the histogram of the first time newspapers published a report over time for all municipalities. Most of the reports appeared in the Summer of 2008 (around month 30), probably as a late response to the dissemination of the website. Another cluster of newspaper reports was registered around month 8, at the time the website was launched. The publication of the first newspaper report is correlated with the percentage of violators that are detected in the municipality, as shown in Figure 1.3.

In order to address the potential endogeneity of timing of the publication of the first newspaper report I estimated a duration model on the probability that a given municipality released a newspaper report for the first time as a function of time-varying gasoline market characteristics at those municipalities and a set of time-invariant municipality variables. The unit of observation for this analysis is a municipality at a given month. Market characteristics include percentage of gas stations in violation, percentage of gas stations inspected, total gasoline sales and number of complaints against gas stations in the municipality.[16] Results of the Cox

[14] This is due to the fact that most of the municipalities had their first newspaper report by the end of my panel.

[15] The horizontal axes of the graphs were normalized so that the time of the first newspaper publication is at zero. The graphs span over a one year window around the first time a newspaper published a report in a given municipality because most of the inspections (70%) were conducted within that time frame.

[16] Municipality characteristics include population, total number of gas stations, percentage of gas stations that belong to a chain, number of households with TV, number of households with access to piped water, electricity and sewage, total number of pumps and gasoline storage capacity, average years of schooling and number of households with PC.

proportional hazard model are presented in Table 1.2.[17] Column (1) of Table 1.2 shows the results of the duration model without municipality controls. Results of the duration model in column (1) indicate that the probability of publishing a report in a municipality, given that there has not been a publication in previous periods, is correlated with time-varying characteristics of the gasoline market, such as the percentage of gas stations found in violation, the number of complaints and total sales. According to these results an increase in the percentage of gas stations in the municipality found in violation at a given month increases the risk of a newspaper publication by 9%. Column (2) includes municipality time-invariant characteristics. When controlling for municipality characteristics, none of the time-varying market characteristics variables are significant, suggesting that most of the observable correlation between the first time newspapers published a report and the gasoline market characteristics can be explained by time-invariant municipality characteristics. The timing of publication of the first report can be considered for the most part a source of exogenous variation, once municipality characteristics that do not vary over time are controlled for.

The previous discussion suggests that in order to estimate the impacts of the website and newspaper publications on the probability of violating regulations one needs to control for the probability of being inspected at a given month and for municipality fixed effects. The model I estimate is a bivariate probit model with sample selection,[18] where the probability of inspecting a gas station at a given month is jointly estimated with the probability of finding a violation.

Let $Inspec^* = X'_{it}\beta \quad + \varepsilon_{1it}$ represent the benefit to the regulator of inspecting gas station *i*, where X is a vector of explanatory variables, which includes gas station characteristics and municipality fixed effects. Although the latent variable $Inspec^*$ is not directly observable, the indicator function $Inspec$ is observable:

$$Inspec = \begin{cases} 1 \; if \; Inspec^* > 0, \\ 0 \; if \; Inspec^* \leq 0. \end{cases} \tag{1}$$

Let $Viol^* = X'_{it}\delta + \varepsilon_{2it}$ denote the benefit gas station *i* obtains from violating regulations (tampering with the pumps). The latent variable $Viol^*$ is not observable but the indicator function $Viol$ is:

$$Viol = \begin{cases} 1 \; if \; Viol^* > 0, \\ 0 \; if \; Viol^* \leq 0. \end{cases} \tag{2}$$

The error terms ε_{1it} and ε_{2it} are assumed to have a bivariate normal distribution with correlation $Corr(\varepsilon_{1it}, \varepsilon_{2it}) = \rho$.

In a standard bivariate probit, four probabilities are estimated:

$$\begin{aligned} &1. Prob(Inspec = 1, Viol = 1) \\ &2. Prob(Inspec = 1, Viol = 0) \\ &3. Prob(Inspec = 0, Viol = 1) \\ &4. Prob(Inspec = 0, Viol = 0) \end{aligned}$$

[17] The Cox proportional hazard model is more flexible than other duration models since it does not require a parametric assumption about the time-dependence of the probability density function.

[18] I focus on the probability of violating regulations because most of the gas stations are inspected only one time in a given month which induces a large number of zeros on both, the number of inspections and the number of inspections that found violations.

In the presence of sample selection, only probabilities 1 and 2 are observed. Outcomes 3 and 4 are indistinguishable from each other. Thus, for estimating a bivariate probit with sample selection, the likelihood function is constructed using the following probabilities: $Prob(Inspec = 0)$, $Prob(Inspec = 1, Viol = 1)$ and $Prob(Inspec = 1, Viol = 0)$.

The specification I estimate for the violation latent variable is given by:

$$Prob(Viol_{it}) = \tau + \gamma_1 Violast_{it} + \gamma_2 website_t + \gamma_3 newspaper_{mt} + Z'\beta + \varepsilon_{it} \qquad (3)$$

where $Viol$ is a dummy variable that takes a value of one if an inspection on gas station *i* at month *t* discovered a violation (only observed if an inspection occurred during that month), τ is a trend variable, which controls for increasing detection of violations over time, $Violast$ is a dummy variable that takes a value of one if in the previous inspection gas station *i* was found in violation. This variable captures the influence of past enforcement on the current probability of violating regulations. Z is a set of covariates that includes gas stations characteristics such as number of pumps, storage capacity, population and average years of schooling at the census tract, a chain dummy and the number of competitors within 1km of distance. The variable $website$ is a dummy variable that takes a value of one for observations that occurred after August of 2006, when the website was introduced. $Newspaper$ is a dummy variable that takes a value of one at the month the first time a newspaper published a report in municipality *m* and stays on until the end of the panel.

The identifying assumption for estimating the effect of newspapers is that, conditioned on gas station characteristics and municipality fixed effects, the timing of the first newspaper publication is uncorrelated with either ε_{1it} or ε_{2it}, the unobservables that affect the probability of inspection and violation, respectively. As is the case with a standard Heckman model with sample selection, the model requires an exclusion restriction, a variable that is correlated with the probability of inspection and uncorrelated with ε_{2it}. I use the percentage of gas stations around gas station *i* that are inspected at month *t*. The larger the percentage of gas stations around gas station *i* that are inspected at month *t*, the higher the probability that gas station *i* is inspected during the same month. This is due to PROFECO's inspection policy that targets geographic areas. The geographic clustering of inspections responds to PROFECO's limited resources for inspecting gas stations and to their needs to reduce inspection costs and therefore is unlikely to be correlated with unobservables that influence the probability of violating regulations.

Table 1.3 presents results from the estimation of the bivariate probit model with sample selection for the probability of inspection (columns (1) to (3)) and the probability of violation (columns (4) to (6)). Three models are presented in Table 1.3: a reduced model (columns (1) and (4)), a base model with municipality fixed effects, gas stations characteristics and time trend (columns (2) and (5)) and a model that adds to the base model socioeconomic characteristics (columns (3) and (6)). Estimated marginal effects of the control variables on the probability of violation are presented in Table 1.4.[19]

First I discuss the estimation results of the probability of inspection presented in columns (1) to (3) of Table 1.3. The estimates of the website dummy and ρ, the correlation coefficient of the error terms in the equations of the probability of violation and the probability of inspection, are significant and positive for all models, which validates the econometric approach to jointly

[19] Marginal effects of the dummy variables are calculated as the difference between the estimated probability of violation when the dummy takes a value of one and the estimated probability of violation when the dummy takes a value of zero.

estimate the inspection and violation equations. Estimates in Table 1.3 show evidence of two types of PROFECO's inspection targeting strategies. First, the coefficient of the dummy of violation in the previous inspection is positive and significant, which indicates that violators are inspected more frequently. Second, inspections are geographically clustered as evidenced by the significant and positive coefficient of the *Competitors inspected* variable, possibly to decrease inspection costs. Other significant variables that determine inspections are *population* and *storage capacity*. Gas stations located in more populated areas are less likely to be inspected and larger gas stations are more likely to be inspected.

The marginal effects of the explanatory variables on the probability of violation show that the website had significant and negative impacts on the probability of violating regulations. Given that the percentage of inspections that detected violations before the website was launched was 30%, the website reduced the probability of violating regulations by 20%. Gas stations that were detected in violation in the previous inspection were more likely to be in violation in the current period. This result suggests that previous inspections and the sanctions imposed as a result of those inspections were not sufficient to deter violations. In accordance with one of the implications of Harrington's model of targeted enforcement that indicates that gas stations with higher gains from cheating are more likely to violate regulations, I find that gas stations with larger storage capacity were more likely to be found in violation. Reputation effects seem to have a strong impact on chains: belonging to a chain is associated with a 13% reduction in the likelihood of violating regulations, as proportion of sample violation rates (see Table 1.1).[20] Gas stations located in areas where the surrounding population has one more year of schooling are less likely to violate regulations by 3%. More educated citizens are more likely to file complaints and to exert pressure on the regulators to improve enforcement.

The estimate of the newspaper dummy, although negative, does not show significant effects on the probability of violating regulations. However, evidence from public disclosure programs suggest that disclosure policies are particularly effective on firms that, before the policy went into effect, were in poor compliance status (Scorse, 2010; Hamilton, 1999; Garcia, Sterner and Afsah, 2007).

To asses if public disclosure of gas stations' inspection outcomes on the website and through the newspapers had larger impacts on gas stations with a history of noncompliance, first I present graphical evidence on the impacts of newspaper publications on violation rates focusing on the set of gas stations that, before the first newspaper published a report in a municipality, were found in violation in at least one inspection. Hereafter, I will also refer to this set of gas stations as the set of violators. Figures 1.6 and 1.7 plot the percentage of inspections that detected violations and the number of inspections before and after a newspaper published a report, respectively. Figure 1.6 shows that, in the set of violators, the first newspaper report induced a sharp decline on the percentage of inspections that detected violations, which can be partly associated to the decline in inspections depicted in Figure 1.7.

To estimate the impacts of media disclosure on the probability of violating regulations in the set of violators I follow the same econometric strategy and estimate a bivariate probit model with sample selection. Tables 1.5 and 1.6 present the results of the estimations and the marginal effects of the variables on the probability of violation, respectively.

Overall, the impacts of the explanatory variables on the probability of inspection are similar to the ones obtained when using the complete sample: having being found in violation in the previous inspection increased the likelihood of an inspection. Also, gas stations located in the

[20] Some chains display their logos at the gas station and can be identified by consumers.

same geographic location were more likely to be inspected during the same month and the number of inspections increased after the website was launched. Contrary to the findings when focusing on the complete sample, the probability of inspection significantly decreased when newspapers started publishing reports.

The effects of most of the control variables on the probability of violation are also similar to the ones obtained when using the complete sample of gas stations. Larger gas stations were more likely to violate regulations, belonging to a chain and being located in areas with more educated people are associated both with a lower likelihood of violation. The website impacts are also negative and significant. Given that 47% of the inspections conducted on the set of violators detected violations before the newspapers started publishing reports, the website is estimated to have reduced the probability of violating regulations by 27%, a stronger impact than the estimated for the complete sample.

Two differences with respect to the estimates obtained when using the complete sample of gas stations are worth discussing. The first one is that the correlation coefficient ρ is not significant and that the Wald test of independence of equations cannot be statistically rejected in models where municipality and gas stations characteristics are included. Although in principle this would suggest conducting separate estimations for the probabilities of inspection and violation, results of separate regressions (not reported here) are similar to the ones presented in Tables 1.5 and 1.6 and, given that the number of inspections seem to have decreased after the first newspaper publication, controlling for the probability of inspection in the violation equation is necessary. The second and more important difference is that newspapers seem to have had negative and significant impacts on the probability of violating regulations. After the newspapers published the first report, the probability of violation decreased by 27% for gas stations located in municipalities with media reports and that had a history of noncompliance. Consistent with findings of other public disclosure programs (Scorse, 2010; Hamilton, 1999; Garcia, Sterner and Afsah, 2007), PROFECO's disclosure program and newspaper reports seem to have had stronger impacts on gas stations with a history of noncompliance.

The reason newspaper reports only induced changes in behavior for gas stations with a history of noncompliance may be explained by the fact that persistent violators are more likely to be reported in the news and to face bad publicity in the future, as they are inspected more frequently and exposed to bad ratings in the Internet for longer periods. It is also possible that violating gas stations become more aware of potential exposure to bad publicity once newspapers start publishing reports, whereas gas stations with good compliance histories underestimate the possibility of being media exposure.

Within the set of violators, there are gas stations that were not reported in the newspapers and gas stations that appeared in the newspapers. In the next section, I discuss the possible explanations of the difference in exposure to media in the set of violators and evaluate if disclosure of information on the website and through newspaper reports had different effects on those two types of gas stations.

1.6 Heterogeneous impacts of newspaper reports on gas stations with a history of noncompliance

Only 7.4% of gas stations that were found in violation before a newspaper published a report were exposed in the news. There are two possible explanations for why some violators were exposed in the news and some others were not: 1) at the moment the reporter consulted the web page some gas stations had ratings and some others did not, a variation that is, for the most part, exogenous due to PROFECO's rule for updating the ratings that discards ratings with old inspections and 2) the reporter arbitrarily selected which gas stations with red or yellow ratings to report. In order to check if there was a systematic selection of gas stations into the news, Table 1.7 shows the coefficient estimates of a set of OLS regressions of gas stations characteristics before the first newspaper was published on a dummy that takes a value of one for gas stations that were exposed in the newspapers in the first report. Column (1) shows the basic OLS estimates and Column (2) controls for municipality fixed effects. Column (1) of Table 1.7 reveals that gas stations that were exposed in the news do not systematically differ from gas stations that were not in the news, except in their average violation rates. Gas stations that were in the news were more likely to be found in violation before the newspaper published a report. However, controlling for municipality fixed effects removes most of the differences in violation rates between gas stations exposed and those not exposed, suggesting that within municipalities there was not a systematic selection of gas stations into the newspaper reports. Hence, selection into the newspaper reports seems to be mainly driven by PROFECO's rule to update the ratings.

To test if the first time a newspaper report had differentiated impacts for exposed and not exposed gas stations that had a history of noncompliance before the news, I follow the same procedure as in the previous sections and estimate a bivariate probit model with sample selection restricting the sample to the set of violators. The violation equation I estimate is given by

$$\mathrm{Prob}(\mathrm{Viol}_{it}) = \tau + \mathrm{Violast}_{it} + Z'\beta + \gamma_1 \mathrm{website}_t + \gamma_2 \mathrm{news}_{it} + \gamma_3 \mathrm{nonews}_{it} + \varepsilon_{it} \quad (4)$$

where *news* takes a value of one for gas stations that appeared in the first newspaper report and stays on until the end of the panel, *nonews* takes a value of one for gas stations that were in a municipality that published a newspaper but were not listed the first newspaper report. The matrix *Z* denotes the set of covariates and *Violast* is a dummy variable that takes a value of one if gas station *i* was found in violation in the previous inspection.

Table 1.8 presents the marginal effects estimates on the probability of violation. The impacts of the first newspaper report are negative and significant for both types of gas stations, those exposed and those not exposed. Although gas stations that did not appear in the newspapers seem to have reduced the probability of violating regulations more than gas stations that appeared in the newspaper reports, the (χ^2) tests of equality of coefficients, $\gamma_2 = \gamma_3$, cannot be rejected in any of the specifications. The estimates suggest that gas station owners of those gas stations detected in violation previous to the first newspaper publication reacted in a similar fashion to the threat of exposure in future reports, regardless of whether or not they were exposed in the first newspaper report, and improved their behavior. One possible explanation of this result is that violators anticipated potential market losses as a consequence of newspaper publications in future periods.

In the next section I evaluate if gas stations that were reported in the newspapers suffered from a decline in their market shares as a consequence of bad publicity.

1.7 The impacts of newspaper reports on gasoline demand

In the previous sections it was estimated that gas stations with a history of noncompliance before the first time a newspaper published a report in a municipality were less likely to violate regulations after the first report was published. Moreover, newspaper reports had similar impacts on the set of violators, regardless of whether or not they were reported in the newspapers, indicating that violators may have had anticipated the potential losses in market shares as a consequence of bad publicity and corrected their behavior possibly to avoid exposure in the newspapers in future reports.

To evaluate the impacts of newspaper reports on monthly gasoline sales I estimate the following equation for sales of gas station *i* at month *t*:

$$Sales_{it} = \alpha_i + m\tau + \beta_1 percnews_{it} + \beta_2 violdummy_{it} + \sum_{j=0}^{3} b_j \cdot news_{it-j} + \varepsilon_{it} \qquad (5)$$

where $Sales_{it}$ are purchases of regular and premium gasoline of gas station *i* at month *t*, which is a proxy for gasoline sales, α_i denote gas stations fixed effects, $m\tau$ is a vector of municipality by time fixed effects and ε_{it} is the disturbance term. The inclusion of municipality by time fixed effects captures changes over time in the gasoline market at the municipality-level that may influence the demand of gasoline in a particular gas station, such as new market entrants, weather or economic shocks. The variable $news_{it}$ represents the number of newspaper publications that reported gas station *i* at month *t*. I included 3 lags of the news variable in order to capture the duration of the impact of the newspaper release. The variable $percnews$ denotes the percentage of gas stations within 1km from gas station *i* that were reported in the news at month *t*. This variable intends to capture spatial externality effects of the publication of the newspaper reports. Suppose gas station *i* did not appear in the news at time *t*, the larger the percentage of competitors around gas station *i* that are reported in the news, the more likely consumers will switch to gas station *i* for consuming gasoline. Since every time a gas station is found in violation, altered pumps are shut down until the violations are corrected, I included the variable $violdummy_{it}$, that takes a value of one if gas station *i* was found in violation at time *t* to control for the effect that this type of sanction has on sales.

The main identifying assumption in equation 5 is that the number of newspaper reports is not correlated with the error term ε_{it}. Since newspapers only reported violators and the probability of violating regulations may be correlated with monthly sales, identification of the parameters in equation 5 requires to control for gas station characteristics that influence the probability of violating regulations. In Section 5, it was shown that the probability of violating regulations was correlated with storage capacity, belonging to a chain and years of schooling in the census block where gas stations are located. Thus, the inclusion of gas station fixed effects should control for time-invariant characteristics that influence the probability of violating regulations and also for those time-invariant characteristics that affect the likelihood of appearing the newspapers, such as location, gas stations ownership and municipality characteristics.

Since the inclusion of gas station fixed effects may not be sufficient to control for unobservables in the sales equation that may be correlated with the number of newspaper reports, I also estimate equation 5 on the sample of violators, those gas stations with a history of noncompliance before the first time a newspaper published a report. In Section 6 it was shown that, within the group of violators, gas stations that appeared in the first newspaper report were similar in their characteristics to gas stations that were not exposed in the media (see Table 1.7). Restricting the sample to the group of violators eliminates potential biases that may remain after the inclusion of fixed effects.

Results of the estimates of equation 5 are shown in Table 1.9. According to these estimates, newspaper reports had negative and significant impacts on gasoline sales. The impacts on the complete sample are similar to the impacts estimated on the sample of violators, which suggests that the inclusion of fixed effects controls for most of the characteristics that influence the number of times a given gas station was exposed in the news. Columns (2) and (4) of Table 1.9 present the results for the specifications that include lags of the number of newspaper reports. According to these results, the effect of newspaper publications decreases over time and is only significant at the month of the newspaper publication.

There are two possible explanations for the temporary impact on sales. One is that consumers expect that, as a result of being exposed in the news, gas stations will have incentives to correct their behavior and consequently, they punish exposed gas stations only for a short period of time, while they correct their anomalies. A second explanation is that consumers may perceive that the net gains from going to other gas stations decrease over time once they start going to other gas stations and internalize the costs of searching for alternative retailers.

Gas stations that were reported by the newspapers sold between 12,100 and 13,160 liters less at the month of the news publication, which amounts to selling between 2.2% and 2.4% liters less per month.[21] These estimates are lower bounds of the impacts of newspaper publications since gas stations in the news corrected their violations and consequently, purchased more gasoline once they were reported in the newspapers. Considering the average real price of gasoline for the period 2006-2009 was $0.75 dollars per liter, the average commercial margin was 5% and that gas stations in the news corrected their behavior, as a result of media exposure, gas stations reported in the news lost between $498 and $539 per newspaper report at the month of exposure.

To compare the decline in sales from newspaper exposure to the average fine, I use a set of gas stations in Mexico City, for which I was able to obtain data on fines. Gas stations in Mexico City that were caught in violation in the period of study were fined $3,620.[22] Considering those gas stations sold 929,477 liters of gasoline per month, gas stations exposed in the news lost $557 every time a report was released, which amounts to almost 2% of their monthly revenues. As a percentage of the fine, bad publicity in the form of newspaper reports represents 15%.

Why are gas stations correcting their violations as a consequence of newspapers publications if consumer's reaction is temporary and represents only a fraction of the fine? One possible explanation is that gas stations may fear continuous media exposure to contribute to the formation of bad reputation among consumers in which case, the impacts of repeated newspaper reports may be larger and last for longer periods.

1.8 Robustness Checks

In this section I perform robustness checks on the estimates of equation 3 on the complete sample and the sample of violators. Regarding the website impacts estimates, the first concern is that, although the estimates control for the probability of being inspected, the decline in violation rates that is observed after the website was introduced may be driven by the large increase in the number of inspections that started in the second quarter of 2007 (after month 19 in Figure 1.2). A second concern is that the results may be driven by a few outliers. For example, Figure 1.1 shows that before the website started there is a large percentage of inspections that detected violations

[21] In the sample of violators, gas stations sold on average 535.83 cubic meters per month previous to the publication of the news.
[22] The average fine was calculated for any type of violation. The amount of the fine depends on the severity of the violation. Fines are higher when gas stations are caught selling *chiquilitros*.

at month 4. Similarly, there is a large decline in violation rates at month 12, after the website started. To test if the estimates are robust to the omission of outliers I estimate a bivariate probit model with selection restricting the sample to those observations before April of 2007 (month 16 in Figures 1.1 and 1.2) and discarding the outliers of months 4 and 12.

Table 1.10 presents marginal effects estimates of the website dummy on the probability of violation. The estimations exclude the dummy variable that indicates if the gas station was found in violation the previous inspection, as when it was included the model did not converge. Also, it excludes the time trend as is not significant in any of the estimated models. Table 1.10 shows that estimates of the website are robust to the exclusion of outliers. Moreover, the magnitude of the impact of the website is similar to the ones obtained when considering the full sample.

Results presented in Section 5 showed that newspaper reports only had significant impacts on the set of gas stations that, previous to the publication of newspaper reports, were found in violation. One concern of those results is that many of the reports were released at the end of the panel (see Figure 1.5) and that the results may be driven by the few observations that remain after the release of the first newspaper report. In order to test if the estimated impacts of newspaper reports are robust to the exclusion of municipalities for which the first newspaper report was released at the end of the panel, I estimate equation 3 excluding those municipalities that had their first newspaper report after month 26. Estimates of the marginal effects on the probability of violation are presented in Table 1.11. The estimates of the newspaper publications are negative and significant for all specifications, suggesting that the effects are robust to the exclusion of municipalities that had their first report late in the period of study.

1.9 Conclusions

This paper contributes to the literature on public disclosure. It shows that public disclosure policies can complement traditional regulation mechanisms of inspections and fines in contexts where regulatory agencies have limited sanctioning capacities. The paper estimates the effects of disclosing information about gas stations that violate fuel supplying standards in Mexico on the probability of violation in future periods. Two types of media sources of information disclosure are discussed: Internet and newspapers. The results show that information disclosed online has significant and negative impacts on the probability of violation, whereas newspaper publications only have impacts on gas stations with a history of noncompliance before the newspapers published a report.

It is also shown that gas stations with a history of noncompliance before the first time a newspaper report was released corrected their behavior, regardless of whether or not they were listed in a report. Being reported in the newspapers as a violator has negative and temporary impacts on monthly gasoline sales. Thus, gas stations may correct their behavior in anticipation of the potential market losses due to bad publicity.

List of Tables

Table 1.1. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Violation dummy	11098	0.222	0.416	0	1
Inspection dummy	96588	0.115	0.319	0	1
Years of schooling	86580	10.240	2.215	0	15.17
Population (1000)	86580	2.786	1.819	0	11.736
Storage capacity (100 m3)	96588	1.828	0.886	0.2	11
Number of pumps (10)	96588	1.695	0.836	0.2	6
Number of competitors within 1km	96588	1.861	1.835	0	10
News dummy municipality	96588	0.400	0.490	0	1
News dummy gas station	96588	0.019	0.136	0	1
Newspaper reports	96588	0.002	0.043	0	2
Website dummy	96588	0.778	0.416	0	1
Chain dummy	96588	0.296	0.456	0	1
Competitors inspected (%)	96588	0.080	0.213	0	1
Total Sales (m3)	96588	537.767	395.850	0	3755.758
Price Magna (real pesos/liter)	93905	5.665	0.206	4.880	7.105
Price Premium (real pesos/liter)	93905	6.807	0.249	5.379	7.601

Table 1.2. Duration Model on the probability of publishing the first report

Variables	(1)	(2)
Complaints	0.892**	0.977
	[0.045]	[0.062]
Percentage gas stations in violation	1.095**	1.083
	[0.043]	[0.053]
Percentage gas stations inspected	1.006	1.015
	[0.012]	[0.010]
Sales	1.041***	0.986
	[0.012]	[0.022]
Municipality characteristics	N	Y
Observations	1308	1308

Note: Hazard ratios reported from a Cox proportional hazards model on time to first publication. The unit of observation is a municipality. Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

Table 1.3. Media Effects on the probability of inspection and violation

	Prob(Inspec=1)			Prob(Viol=1)		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Violation last inspection		0.093***	0.098***		0.397***	0.415***
		[0.022]	[0.023]		[0.049]	[0.054]
Competitors inspected	0.73***	0.640***	0.651***			
	[0.04]	[0.037]	[0.040]			
Storage capacity		0.012**	0.014**		0.074***	0.073***
		[0.006]	[0.007]		[0.018]	[0.017]
Pumps		0.013	0.008		0.022	0.038
		[0.009]	[0.007]		[0.022]	[0.023]
Competitors		-0.004	-0.006**		-0.017*	-0.007
		[0.003]	[0.003]		[0.010]	[0.011]
Website Dummy	0.15*	0.302***	0.285***	-0.226***	-0.312***	-0.303***
	[0.08]	[0.080]	[0.080]	[0.072]	[0.095]	[0.081]
Newspaper Dummy	0.09*	-0.029	-0.017	-0.048	-0.022	-0.042
	[0.04]	[0.056]	[0.056]	[0.055]	[0.093]	[0.096]
Chain Dummy		-0.004	-0.006		-0.182***	-0.164***
		[0.017]	[0.017]		[0.026]	[0.031]
Schooling			0.007			-0.035***
			[0.004]			[0.009]
Population			-0.005***			-0.007
			[0.002]			[0.018]
Constant	-1.43***	-1.782***	-1.696***	-1.116***	-1.408***	-0.733**
	[0.08]	[0.111]	[0.085]	[0.130]	[0.233]	[0.316]
ρ	0.38***	0.294***	0.252**	0.38***	0.294***	0.252**
	[0.09]	[0.101]	[0.100]	[0.09]	[0.101]	[0.100]
Municipality FE	N	Y	Y	N	Y	Y
Time Trend	N	Y	Y	N	Y	Y
Number of obs.	96588	71237	64574	96588	71237	64574
Uncensored obs.	11098	8436	7753	11098	8436	7753
Wald test (χ^2)	20.01	6.13	4.44	20.01	6.13	4.44

Note: Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

Table 1.4. Marginal effects of the probability of violation

Variables	(1)	(2)	(3)
Violation last inspection		0.077***	0.085***
		[0.018]	[0.019]
Storage capacity		0.012***	0.013***
		[0.004]	[0.004]
Pumps		0.004	0.007
		[0.004]	[0.004]
Competitors		-0.003*	-0.001
		[0.002]	[0.002]
Website Dummy	-0.041***	-0.062**	-0.063***
	[0.015]	[0.027]	[0.024]
Newspaper Dummy	-0.008	-0.004	-0.007
	[0.010]	[0.016]	[0.017]
Chain Dummy		-0.029***	-0.028***
		[0.006]	[0.007]
Schooling			-0.006**
			[0.002]
Population			-0.001
			[0.003]
Municipality FE	N	Y	Y
Time Trend	N	Y	Y
Number of obs.	96588	71237	64574
Uncensored obs.	11098	8436	7753

Note: Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

Table 1.5. Media Effects on the probability of inspection and violation: sample of violators

	Prob(Inspec=1)			Prob(Viol=1)		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Violation last inspection		0.089***	0.094***		-0.101	-0.081
		[0.021]	[0.022]		[0.065]	[0.067]
Competitors inspected	0.730***	0.648***	0.673***			
	[0.045]	[0.047]	[0.052]			
Storage capacity		0.004	0.002		0.063**	0.069***
		[0.009]	[0.010]		[0.032]	[0.034]
Pumps		0.002	-0.006		-0.03	-0.017
		[0.016]	[0.014]		[0.047]	[0.051]
Competitors		-0.008	-0.013***		-0.031**	-0.018
		[0.005]	[0.004]		[0.015]	[0.015]
Website Dummy	0.117	0.310***	0.307***	-0.503***	-0.428***	-0.397***
	[0.089]	[0.097]	[0.099]	[0.090]	[0.118]	[0.114]
Newspaper Dummy	0.042	-0.129**	-0.119**	-0.485***	-0.413***	-0.455***
	[0.041]	[0.058]	[0.059]	[0.077]	[0.125]	[0.125]
Chain Dummy		0.015	0.016		-0.208***	-0.184***
		[0.019]	[0.017]		[0.059]	[0.062]
Schooling					0.007	-0.039**
					[0.004]	[0.010]
Population					-0.005	-0.013
					[0.005]	[0.021]
Constant	-1.329***	-1.844***	-1.977***	-0.198	-0.562*	0.637
	[0.083]	[0.099]	[0.094]	[0.219]	[0.331]	[0.423]
ρ	0.304**	0.225	0.179	0.304**	0.225	0.179
	[0.133]	[0.166]	[0.168]	[0.133]	[0.166]	[0.168]
Municipality FE	N	Y	Y	N	Y	Y
Time Trend	N	Y	Y	N	Y	Y
Number of obs.	40140	31519	28023	40140	31519	28023
Uncensored obs.	5005	3890	3535	5005	3890	3535
Wald test (χ^2)	6.73	1.92	1.25	6.73	1.92	1.25

Note: Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

Table 1.6. Marginal effects of the probability of violation: sample of violators

Variables	(1)	(2)	(3)
Violation last inspection		-0.028	-0.024
		[0.019]	[0.020]
Storage capacity		0.018*	0.020*
		[0.010]	[0.011]
Pumps		-0.009	-0.005
		[0.013]	[0.015]
Competitors		-0.009**	-0.005
		[0.004]	[0.004]
Website Dummy	-0.166***	-0.137**	-0.130**
	[0.035]	[0.055]	[0.053]
Newspaper Dummy	-0.137***	-0.112***	-0.129***
	[0.036]	[0.040]	[0.043]
Chain Dummy		-0.056**	-0.052**
		[0.023]	[0.023]
Schooling			-0.012**
			[0.005]
Population			-0.004
			[0.006]
Municipality	N	Y	Y
Time	N	Y	Y
Number	40140	31519	28023
Uncensored	5005	3890	3535

Note: Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

Table 1.7. OLS estimates of gas station characteristics on Newspaper dummy: sample of violators

Variable	(1)	(2)
Violation Dummy	0.0627*	0.03
	[0.032]	[0.029]
Inspection Dummy	0.006	0.008
	[0.0074]	[0.006]
Competitors inspected	-0.009	-0.002
	[0.0077]	[0.0077]
Pumps	-15.55	9.71
	[29.7]	[25.1]
Competitors	-0.036	0.121
	[0.20]	[0.21]
Storage Capacity	-479.6	-38.01
	[367]	[308]
Schooling	-0.436	-0.444
	[0.27]	[0.28]
Population	-1278	1220
	[5919]	[5237]
Chain Dummy	-0.059	-0.085
	[0.051]	[0.054]
Municipality	N	Y
Observations	1115	1115

Table 1.8. Differentiated impacts of newspaper publications on the probability of violation

Variable	(1)	(2)	(3)
Violation last inspection		-0.029	-0.024
		[0.019]	[0.020]
Storage capacity		0.018*	0.020*
		[0.010]	[0.011]
Pumps		-0.009	-0.005
		[0.013]	[0.014]
Competitors		-0.009**	-0.006
		[0.004]	[0.004]
Website Dummy	-0.166***	-0.139**	-0.132**
	[0.036]	[0.056]	[0.054]
Nonews	-0.140***	-0.115**	-0.132***
	[0.041]	[0.042]	[0.046]
News	-0.094***	-0.074**	-0.086**
	[0.033]	[0.036]	[0.034]
Chain Dummy		-0.056**	-0.051**
		[0.023]	[0.023]
Schooling			-0.011**
			[0.005]
Population			-0.004
			[0.006]
Municipality FE	N	Y	Y
Time Trend	N	Y	Y
Number of obs.	40140	31519	28023
Uncensored obs.	5005	3890	3535
χ^2 test (News=Nonews)	0.7	0.86	1.08

Note: Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09. All regressions include time dummies. FE: fixed effects. F-test: test of equality of coefficients.

Table 1.9. Impacts of newspaper reports on total gasoline sales

	Complete Sample		Sample of violators	
Variable	1	2	3	4
Violation Dummy		-4.462		-4.488*
		[2.97]		[2.58]
Competitors Inspected		-9.729		5.9
		[10.5]		[24.9]
News	-13.16**	-12.30**	-12.36*	-12.10*
	[5.18]	[5.24]	[6.44]	[6.41]
News (t-1)		-1.064		-6.643
		[6.89]		[6.15]
News (t-2)		4.322		-1.696
		[10.4]		[8.79]
News (t-3)		6.719		3.354
		[11.8]		[10.8]
Constant	539.3***	538.4***	547.4***	532.8***
	[0.033]	[0.27]	[0.16]	[0.61]
Observations	96588	88539	40140	36795
R-squared	0.1	0.09	0.13	0.12

Note: Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09. All estimations include municipality by time and gas station fixed effects.

Table 1.10. Marginal effects of media impacts excluding outliers

Variables	(1)	(2)	(3)
Storage capacity		0.063***	0.062***
		[0.014]	[0.013]
Pumps		0.013	0.032
		[0.012]	[0.018]
Competitors		-0.002	0.002
		[0.005]	[0.006]
Website Dummy	-0.01	-0.041*	-0.056**
	[0.02]	[0.024]	[0.025]
News Dummy	-0.01	0.008	0.025
	[0.03]	[0.035]	[0.036]
Chain Dummy		-0.016	-0.016
		[0.019]	[0.022]
Schooling			-0.016***
			[0.005]
Population			-0.001
			[0.006]
Municipality FE	N	Y	Y
Time Trend	N	N	N
Number of obs.	19944	16352	31265
Uncensored obs.	2769	2215	3245

Note: Marginal effects computed from the bivariate probit with sample selection for the probability of violation. Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

Table 1.11. Marginal effects of media impacts excluding municipalities with late reports

Variables	1	2	3
Violation last inspection		0.006	0.009
		[0.008]	[0.009]
Storage capacity		0.001	0.005
		[0.007]	[0.007]
Pumps		0.003	0.003
		[0.006]	[0.007]
Competitors		-0.004	-0.003
		[0.003]	[0.002]
Website Dummy	-0.096**	-0.027	-0.027
	[0.042]	[0.035]	[0.032]
News Dummy	-0.162***	-0.050**	-0.058***
	[0.037]	[0.020]	[0.021]
Chain Dummy		-0.009	-0.007
		[0.011]	[0.010]
Schooling			-0.003*
			[0.002]
Population			-0.002
			[0.003]
Municipality FE	N	Y	Y
Time Trend	N	Y	Y
Number of obs.	19944	16352	14971
Uncensored obs.	2769	2215	2065

Note: Marginal effects computed from the bivariate probit with sample selection for the probability of violation. Standard Errors Clustered at the state-level in brackets. ***p<0.01, **p<0.05 and *p<0.09.

List of Figures

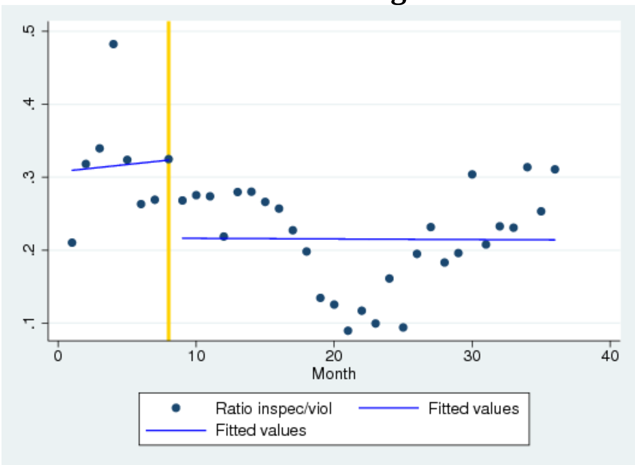

Figure 1.1: Ratio of violations to inspections before and after the website

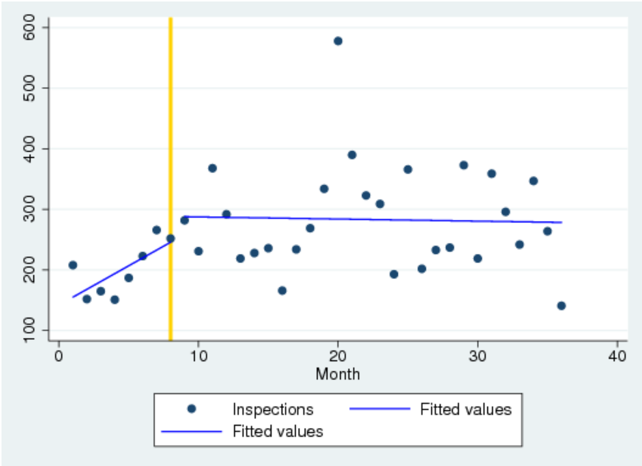

Figure 1.2: Inspections before and after the website

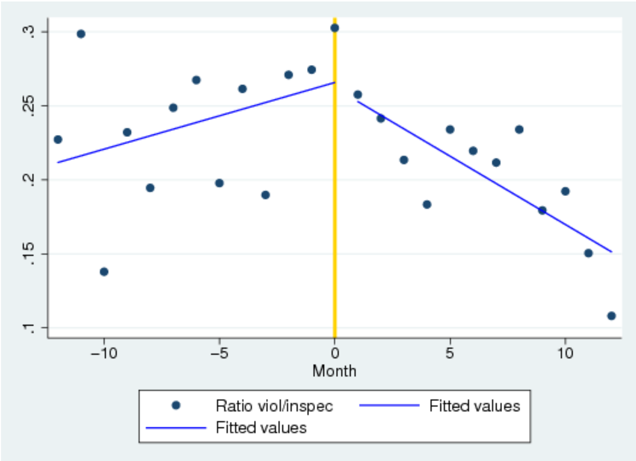

Figure 1.3: Ratio violations to inspections before and after the first newspaper publication

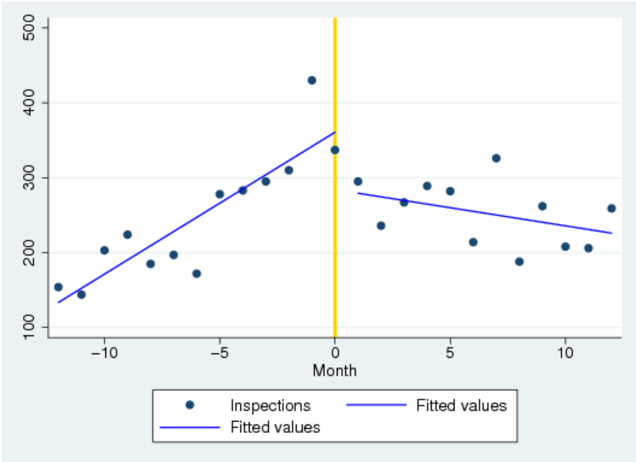

Figure 1.4: Inspections before and after the first newspaper report

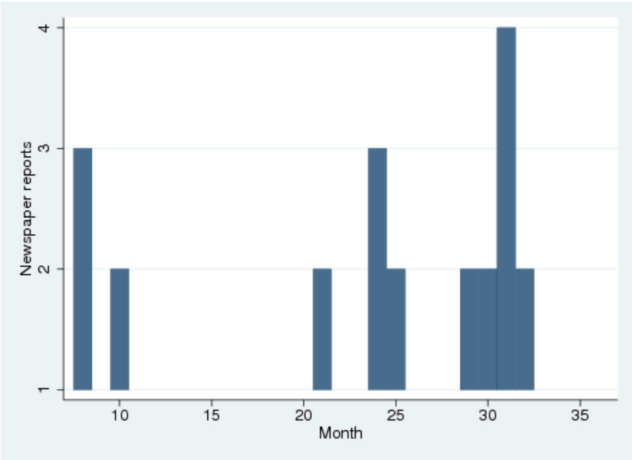

Figure 1.5: First newspaper publication

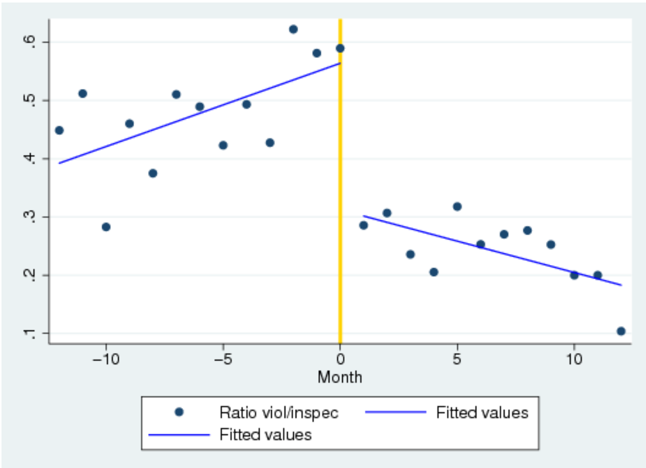

Figure 1.6: Ratio violations to inspections before and after the first newspaper publication: sample of violators

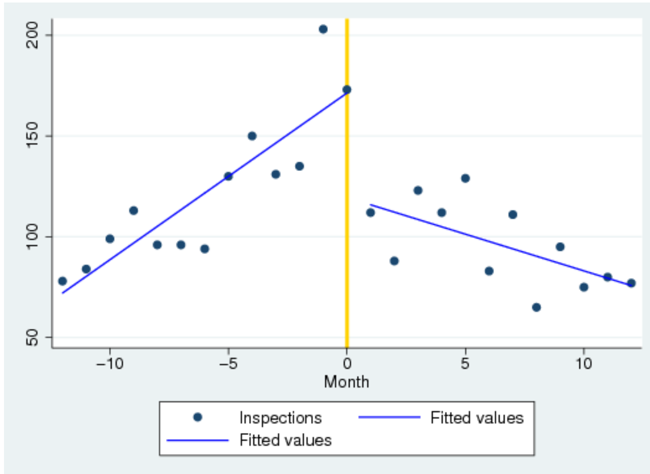

Figure 1.7: Inspections before and after the first newspaper publication: sample of violators

Chapter 2

Self-Policing Statutes: Do They Reduce Pollution and Save Regulatory Costs?

2.1 Introduction

Recent changes in environmental law enforcement encourage polluters to self-report their violations to government authorities. Across U.S. States, self-reporting inducements vary from promises of modest reductions in sanctions to complete immunity from sanctions and privilege protections for information uncovered in a firm's environmental self-audit. Environmental groups argue that many of these protections amount to a free pass for polluters that negates incentives for firms to avoid pollution violations and requires increased government oversight of firms' environmental practices (EPA, 2000). Proponents argue instead that these protections are necessary for firms to audit their own environmental performance, audits that in turn yield environmental dividends in the form of quick detection and remediation of pollution violations and potentially the identification and avoidance of pollution outbreaks before they occur (Weaver, Martineau, and Stagg, 1997). Moreover, because self-auditing firms can uncover and self-report pollution violations, enforcement of environmental laws can be achieved with less government investment in oversight and monitoring (Kaplow and Shavell, 1994; Malik, 1993).

These two perspectives offer competing empirical predictions, one that self-policing statutes raise pollution and government environmental monitoring activity, and the other that they lower them. The objective of this paper is to test these predictions, distinguishing between cross-state differences in self-policing policies in a panel of State-level industries. We estimate two equations, one for total toxic emissions and the other for the number of government environmental inspections, both aggregated across facilities to the level of State-specific industries. In doing so, we find some merit in the arguments of both environmentalists and proponents of self-policing protections. Some protections, by promoting environmental self-auditing, are found to lower levels of toxic pollution even though they also prompt lower rates of government environmental monitoring, while others deplete firms' pollution avoidance incentives to such an extent that they raise pollution and prompt compensatory increases in government oversight.

Despite the controversy surrounding self-policing policies, and a burgeoning theoretical literature on the subject,[1] there is surprisingly little empirical work studying their impact. A notable exception is a key paper by Stafford (2005), who estimates the impact of self-policing policies on the probabilities of facility-level inspection and violation using a panel of RCRA (Resource Conservation and Recovery Act) data. There are a number of crucial differences between our analysis and Stafford's (2005) that motivate our work. First, Stafford (2005) controls for overall State-level inspections in estimating her facility-level inspection equation. Hence, she implicitly controls for the effects of self-policing policies that are our primary focus, namely, impacts of self-policing statutes on government inspection policy. In order to capture State-specific inspection policy as targeted to different industries, we use data that is at a State-specific industry level. Second, we study a more direct measure of environmental performance: emissions of regulated toxic air pollutants, rather than the occurrence of a RCRA violation. Although RCRA violations may have a relationship to ultimate toxic emissions, this relationship is not clear-cut. Many violations are not directly related to emissions, including those that concern reporting and record-keeping. Those that do concern practices that affect emissions are

[1] See the initial papers of Kaplow and Shavell (1994) and Malik (1993), and recent papers by Pfaff and Sanchirico (2000), Mishra, Newman and Stinson (1997), Friesen (2006), Livernois and McKenna (1999), and Innes (1999a, 1999b, 2000, 2001a). See also the related literature on self-regulation (e.g., Maxwell, Lyon and Hackett, 2000; Maxwell and Decker, 2006).

not weighted in Stafford's (2005) violation measure. Rather, this measure is a zero-one variable that equals one if any violation occurs and does not capture effects of multiple or more serious violations. Hence, it is possible that self-policing policies yield less frequent technical violations of RCRA, even though they lead to increased toxic emissions. Third, beyond our different data and longer study period are a number of key differences in estimation method, including (for example) our accounting for fixed individual and time effects.[2]

Other related papers include Pfaff and Sanchirico (2004), who compare and contrast self-disclosed and government-detected violations; Stretsky and Gabriel (2005) and Short and Toffel (2005), who estimate an equation to explain the probability of self-disclosure; Helland (1998), who estimates a joint model explaining facility-level self-reporting and inspections; and Stafford (2006), who estimates the impact of self-policing policies on the probability of self-disclosure (generally positive). However, none of this excellent work seeks to identify the effect of self-policing policies on pollution and government inspection activity, our objective.

To frame the empirical issues addressed in this paper, we begin with a conceptual discussion of policy trade-offs that identifies opposing influences of government self-policing policies on average harm (our theoretical proxy for emissions) and government inspections. Based on educated conjectures about which influences dominate, we posit three Hypotheses on the effects of self-policing policies, which we proceed to test in our empirical work (Sections 3-4 below).

2.2 Hypotheses

Properly designed enforcement regimes that elicit self-reporting enjoy a number of potential efficiency advantages. They can yield direct enforcement economies (Kaplow and Shavell, 1994; Malik, 1993), indirect enforcement economies (such as saving on costs of imprisonment, Kaplow and Shavell, 1994), more frequent remediation / cleanup (Innes, 1999a), better tailoring of penalties to heterogeneous violators (Innes, 2000), and savings of wasteful avoidance expenditures (Innes, 2001a). To obtain benefits of self-reporting, firms must generally adopt costly environmental self-auditing programs that not only reveal pollution violations, but enable quick remediation and potentially the prevention of accidents that would otherwise occur. In doing so, self-audits also provide much cleaner and clearer documentation of a firm's environmental practices. Legal scholars have argued that this documentation can provide a roadmap for regulatory enforcement that makes prosecution of violations much easier (Hawks, 1998).

To enable self-reporting, by encouraging self-auditing, State laws variously provide two types of protections. First is a reduction in sanctions to self-reporters vis-à-vis violators who are discovered by government inspectors. Extant theory generally argues for self-reporting sanctions equal to the expected non-reporter sanction, thereby motivating firms to self-report without sacrificing incentives for the prevention of accidents / violations. Accounting for costs of self-audit programs, however, firms must be offered somewhat lower self-reporting sanctions so that they enjoy strictly positive benefits of self-reporting that can compensate for costs of self-auditing (Pfaff and Sanchirico, 2000; Mishra, et al., 1997; Innes, 2001b). Some State statutes provide self-reporters with reductions in gravity-based penalties that may or may not be in line

[2] Stafford (2005) controls for state effects, but not industry or time effects. In addition, we consider a variety of time-varying industry forces and State variables omitted in Stafford's analysis, including measures of industry scale, concentration, growth and R&D, and State population and political composition that can be important in driving environmental regulatory policy.

with those advanced by economic theorists;[3] others provide self-reporters with complete immunity from sanction.

The second type of protection afforded to self-reporters is "privilege." Many States protect the information contained in self-audits and self-reports from regulatory use beyond the narrow confine of the self-reported violation.[4] Privilege can deny regulators the enforcement economies made possible by self-audit documentation. However, privilege can also encourage firms to adopt self-auditing programs.

Both forms of protection have effects on deterrence (firms' incentives to prevent violations) and firms' adoption of self-audit programs, both of which in turn affect the two outcomes of interest in this paper, government enforcement effort and firms' environmental performance.[5]

First consider the effects of privilege. The social benefit of privilege is that it can elicit more self-auditing by protecting firms from the government's use of self-audits for prosecution. By eliciting more self-auditing, privilege can lower average accident harms due to more rapid detection of pollution accidents and pro-active management that avoids pollution outbreaks in the first instance. However, there are opposing costs. Privilege limits the government's information about a firm's environmental performance and its ability to use the information in enforcement (to the extent that it overlaps with privileged documentation). As a result, privilege limits the sanctions that the government can impose on a firm when pollution accidents occur. This reduces deterrence, which in turn raises average harms from self-auditors' accidents. Effects of privilege on average harm are thus unclear analytically, even controlling for government inspections: average harm is lowered due to more self-auditors and raised due to reduced deterrence.

Effects on government inspections are also unclear. Reduced deterrence favors increased government inspection effort as the government compensates for its limited ability to impose sanctions by more intensively monitoring and regulating firms' accident prevention strategies ("care"). However, privilege reduces the effectiveness of government inspections in achieving either accident sanctions or sanctions for insufficient care; this motivates less monitoring, a substitution of self-auditing for government inspections. We conjecture that the self-audit promotion (harm reduction) and substitution effects of privilege dominate in practice:

Hypothesis 1. Privilege lowers both emissions and government inspections.

Consider next the consequences of complete immunity. Because immunity can only be obtained when firms self-report, and self-reporting is made possible by self-audits, immunity promotes the adoption of self-audit programs. However, complete immunity exempts self-reporters from

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[3] To obtain these benefits, firms must satisfy various technical requirements, including: disclosing the violation within 21 days of discovery; correcting the violation within 60 days; taking steps to avoid a recurrence of the violation. In addition, the violation must not have been found by a third party and must not be an "imminent and substantial endangerment to public health or the environment" (EPA, 1995).

[4] Privilege makes environmental audit reports inadmissible as evidence in administrative, civil, and sometimes criminal proceedings, including those for environmental enforcement actions. However, privilege does not exclude documentation that is part of an audit report, but also contained in other reports required by law. Although States differ in the breadth of their statutes, privilege is typically voided when a violation is not diligently corrected (Weaver, et al., 1997).

[5] An expanded version of this paper (available upon request) contains a conceptual model that identifies the competing influences of self-policing statutes on government inspections and pollution, as discussed below.

accident sanctions, so that effective sanctions are reduced to only costs of cleanup and correction. This reduces self-auditing firms' incentives to invest in accident prevention (deterrence). Immunity thus lowers average pollution harm by prompting more self-auditing, but raises it by reducing deterrence.

Immunity also has opposing effects on government monitoring. Reduced deterrence raises incentives for government monitoring and regulation of self-auditors' "care." However, with immunity, government monitoring can no longer give rise to accident sanctions on self-reporting self-auditors, even though they can still be sanctioned for deficient care. As a result, the average deterrence-promoting effectiveness of inspections declines, favoring less monitoring.We expect the large deterrence depletion effects of immunity to dominate:

Hypothesis 2. Complete immunity raises emissions and government inspections.

Finally, many States have enacted an intermediate policy that provides limited immunity protection, but not "complete" immunity. These "self-policing" statutes mimic the U.S.E.P.A.'s guidance on reducing the "gravity based" penalties of self-reporting violators.[6] As with privilege, we expect that these protections spur increased self-auditing that lowers average harm, an effect that may dominate the resulting deterrence depletion. Hence, comparing the "self-policing" policy to one of no self-auditing inducements, we posit the testable speculation:

Hypothesis 3. "Self-policing" statutes lower emissions and government inspections.

2.3 The Data and the Econometric Model

Dependent Variables. We construct a U.S. panel dataset over the period 1989-2003 where the cross-section units are State-level industries measured using three-digit SIC codes. Due to missing observations and omission of outliers, this gives us an unbalanced panel of 92 manufacturing industries (SIC codes 200-399) in the fifty states for a total of 19,472 observations.

We focus principally on outcomes at a State-specific industry level (vs. State aggregates or more disaggregated facility-level) primarily because we expect State regulators and regulated firms to respond to local industry-specific environmental, political and economic circumstances, implying that regulatory (inspection) policy and emissions intensities will vary by State and, within a State, by industry. Aggregating up (to a State level) prevents us from controlling for individual State-specific industry effects and sacrifices information. Disaggregating (to a plant level) is instructive – and for this reason we present plant-level regressions below[7] – but gives us limited additional information because our explanatory variables cannot be measured at a facility level.[8]

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[6] EPA (2000) policy provides conditions under which pieces of potential environmental sanctions are reduced or eliminated. Sanctions for "economic benefits" obtained by not avoiding violations are not affected by the EPA policy. However, "gravity based" sanctions for self-reporting violators are eliminated if a firm meets nine conditions, including the presence of a systematic intra-firm self-auditing program; if all but this last condition are met, 75 percent of gravity based sanctions are voided. State-level complete immunity statutes eliminate all sanctions under weaker conditions (note 18).

[7] We are very grateful to the referees for suggesting these regressions.

[8] This is a well-known problem for empirical researchers in environmental economics. Tying EPA data (on facility-level inspections and emissions) to firm-level financial data is virtually impossible in large datasets such as ours.

A given industry's emissions for a given State are obtained by summing the reported releases of a given set of toxic chemicals from facilities that are located in the State and report the industry (three digit SIC) as their primary line of business. The release data is obtained from the EPA's Toxic Release Inventory (TRI). We consider the 172 chemicals that are regulated and monitored under the Clean Air Act's National Emission Standards for Hazardous Air Pollutants (NESHAPS, CAA Section 112(b), 40 CFR Part 61) and listed on the TRI throughout our study period.[9] We construct two measures of toxic emissions, both in total weight (millions of pounds of on-site emission), one for the 172 NESHAPS chemicals and the other for the 56 carcinogenic NESHAPS chemicals.[10] All of these chemicals are released to a common medium (air) and are subject to Federal emission standards, monitoring requirements (under the CAA) and reporting requirements under the Emergency Planning and Community Right to Know Act (EPCRA).[11] Because many fewer State-level industries release carcinogenic toxics (vs. any NESHAPS toxics), the number of observations for carcinogenic emissions is substantially lower (9550).

Our focus on chemical releases regulated under the CAA reflects our interest in firm incentives to avoid and curb pollution for which they can be monitored and sanctioned. In contrast, "unregulated" chemicals (under the CAA, even if reporting is technically required under the EPCRA) are not the object of government inspectors' attention or, therefore, the pollution abatement and avoidance efforts of firms. In addition, the regulated CAA chemicals involve tighter reporting requirements and enforcement that improve data quality and give us consistent pollution measures over our sample period.

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[9] In an earlier version of this paper, we also consider total TRI emissions and identify qualitatively similar effects to those found with our new data.

[10] Recent environmental economics studies use toxicity weighted TRI releases to measure toxic emissions (e.g., Innes and Sam, 2008). For a narrowly defined set of chemicals with common properties – such as the 17 33/50 chemicals of interest in much of this literature – toxicity weighted aggregates are arguably useful indicators of pollution harm. However, we are interested in impacts of self-auditing statutes on overall toxic air pollution at an industry level, requiring measurement of a broad set of chemical releases. For this purpose, a variety of inconsistencies between (and limitations of) methods for constructing toxicity weights for different chemicals makes toxicity weighted aggregation highly problematic (see www.peri.umass.edu for details and documentation on these methods). Toxicity weights for non-carcinogens are based on two measures (RfC and RfD) that have different units (mg/m^3 vs. mg/kg-day), each defined as "an estimate (with uncertainty spanning perhaps an order of magnitude)" of non-threatening continuous daily exposure over a lifetime. Carcinogens also have two different types of toxicity weights (IUR and SF), also measured in different units (mg/m^3 vs. mg/kg-day), both indicators of risk (vs. the no-adverse-effect indicators for non-carcinogens). The indicators for carcinogenic risk are based on the "upper bound excess lifetime cancer risk" from continuous exposure where there is a non-linear dose response. Hence, when emissions are non-continuous, or there are different variances in the estimation of cancer risks for different chemicals (with upper bounds higher when there is more variation, ceteris paribus), these measures are problematic. Also missing from these weightings are crucial determinants of risk, including stack height, human population concentrations, winds, and so forth. Perhaps the least meaningful weight is for asbestos, which has the highest toxicity index by orders of magnitude (1000000), is in completely different units than others (fibres per litre) and is based largely on qualitative judgment. For these reasons, toxicity weights are often incomparable across chemicals, whether within a class (carcinogenic and non-carcinogenic) or across classes, and even in isolation are limited as measures of relative risk. For our purposes, therefore, we believe that the most reliable and meaningful measures of toxic air pollution are based on simple weight.

[11] TRI release measures are sometimes criticized because the reporting requirements of the EPCRA are subject to limited enforcement and often represent "legal emissions" in the sense that, when monitoring is not required, reported releases are based on estimates driven by the technological requirements of pollution permits. However, NESHAPS chemicals are regulated; both EPCRA and the CAA require reporting of the true emissions when known; and the EPA prosecutes firms that do not truthfully report (see, for example, EPA New England Press Release, Oct. 5, 2001, "EPA Seeks Monetary Penalty Against Durham, Conn. Company for Violations of Environmental Regulations").

For robustness checks, we consider two additional emission measures. The first is a toxicity-weighted aggregate of NESHAPS toxic releases; for a variety of reasons, we are skeptical about the merits and interpretation of this measure (note 10). The second is an alternative to our carcinogenic release measure designed to reflect more acutely toxic emissions, namely, releases (by weight) of chemicals that have high toxicity weights (greater than 500); this measure includes 69 of our initial 172 NESHAPS chemicals.

We measure government enforcement activity with the number of State and Federal inspections under the Clean Air Act (CAA). State and industry specific inspections numbers are obtained by summing across each industry's facilities in each State. The source for inspections data is the EPA's IDEA database. These inspections are determined by State and Federal regulators (in the EPA and State environmental agencies) in view of pre-determined Federal regulations and State-level policies that have been enacted by State legislatures and voters (including the self-policing policies of central interest here).

Econometric Model. For our two endogenous variables, we posit the following structural model:

(1) $$E_{it} = X_{Eit}\,\beta_E + I^*_{it}\,\alpha_E + \varepsilon^*_{Eit},$$

(2) $$I^*_{it} = X_{Iit}\,\beta_I + E_{it}\,\alpha_I + \varepsilon^*_{Iit},$$

where E_{it} denotes emissions, $I^*_{it} = \ln(I_{it}+k)$ represents inspection intensity (with $k>0$ and I_{it} denoting inspection counts), and $(\varepsilon^*_{Eit}, \varepsilon^*_{Iit})$ are disturbances with zero conditional mean. In principle, inspection activity can promote emission reductions (as documented by Gray and Deily, 1996, and Deily and Gray, 2007, among others). In addition, the anticipation of higher emissions may spur more government enforcement scrutiny.

The structural model can be solved for the reduced form:

(3) $$E_{it} = X_{it}\,\delta + \varepsilon_{Eit},$$

(4) $$I^*_{it} = X_{it}\,\gamma + \varepsilon_{Iit},$$

where X_{it} is the union of X_{Eit} and X_{Iit}.

In what follows, we estimate the reduced form equations (3)-(4), rather than the structural forms (1)-(2), for three reasons. First and foremost, our interest ultimately is to measure the overall impact of self-policing statutes on emissions and inspections, including indirect effects of altered enforcement strategies (on emissions) and of changed emissions (on inspections). Second, as a practical matter, identification of inspections is problematic; for any instrument, a case can be made for direct relevance to emissions. And third, although we have good measures of regulated toxic air pollution, we lack measures of the "criteria air pollutants" (CO, SO2, NOX) also regulated under the CAA; the reduced form of equations (3)-(4) implicitly capture effects of the latter pollutants.[12]

Explanatory Variables. We have five general classes of independent variables: (1) individual and time effects, (2) measures of industry scale within each State, (3) State attributes, (4) industry attributes, and (5) self-policing policy variables. In all models, we incorporate fixed time effects. Subject to caveats discussed below (Section IVB), we also incorporate fixed individual effects for all cross-section (State-industry or facility) units. This treatment accounts for unobserved heterogeneity and any time trends.

[12] The additional (unobserved) class of criteria pollutants can be thought of as adding a structural equation,

$$C_{it} = X_{Cit}\,\beta_C + I^*_{it}\,\alpha_C + \varepsilon^*_{Cit},$$

and an additional argument, $C_{it}\,\alpha_2$, to equation (2). The reduced form in (3)-(4) does not change in structure.

We construct three measures of industry scale: the number of industry facilities in each state (*Facility*), and measures of State-level industry output (*Sales*) and employment (*Empl*).[13] To obtain State-level sales and employment, we use facility numbers to allocate nationwide sales and employment numbers (constructed from the financial database for publicly traded companies, COMPUSTAT).[14] We expect larger industries to have higher emissions and greater inspection scrutiny, implying positive coefficients on *Empl* or *Sales*. However, controlling for industry size, we have no prior expectation on the effects of facility numbers, whether industries with more (and hence smaller) facilities will tend to produce more or fewer emissions and be subject to more or fewer inspections.

We include a number of State attributes. First, *Pop* measures the State's population. More populous States are expected to be more sensitive to toxic pollution and, hence, due to heightened public and regulatory pressure, to elicit lower levels of pollution and higher rates of inspection. Second, *Gspmm* measures gross State product in mining and manufacturing industries. Following Alberini and Austin (1999), we include this measure of output in the more polluting activities even though we control for individual effects; the reason is that higher *Gspmm* may serve to focus regulatory efforts on pollution, potentially raising inspection activity and reducing releases. Third, we include two measures of political attitudes. *Repvote* is the ratio of votes cast for the Republican candidate to total votes in the most recent presidential election. And *Sierra* is the State's per capita Sierra Club membership, a measure of the State's environmentalist constituency. We expect *Repvot* to favor a pro-business regulatory environment, leading to higher emissions and fewer inspections. Conversely, *Sierra* may yield more public scrutiny of industry environmental performance, spurring fewer emissions and either fewer or more inspections as public scrutiny either spurs or substitutes for regulatory enforcement. Fourth, *Income* is per capita income, reflecting overall economic activity and potentially intensifying either pro-business impulses or environmental preferences. Fifth, *Expend* is overall State government expenditures and *Nrexp* measures State expenditures on natural resource programs, including conservation and regulation of exploitive industries. Higher overall expenditures may enable larger enforcement budgets and thereby enable more inspections that lead to reduced emissions. We include *Nrexp* to proxy for competition in State environmental budgeting between natural resource services and enforcement of clean air laws; for example, *Nrexp* may crowd out air-related enforcement expenditures and thus lead to fewer environmental inspections.[15] Last, *Strict* is a dummy variable indicating whether or not a State imposes strict environmental liability. Strict (vs. negligence) liability can favor higher emissions if firms are predominantly smaller (and can thus escape liability) or lower emissions if firms

[13] All financial variables in our analysis are real (1995=100).

[14] Specifically, we scale nationwide industry sales and employment by the proportion of industry facilities belonging to each state,

$$S_{ijt} = n_{ijt} / \{ \sum_{j} n_{ijt} \},$$

where the ith and jth indexes refer to industry and State, respectively, and n denotes number of facilities.

[15] Potential effects on emissions are unclear. By lowering inspections, higher *Nrexp* may lead to higher emissions. However, higher *Nrexp* may also indicate public sensitivity to environmental issues and, due to generalized community and political pressure, promote pollution abatement.

have deep pockets (Alberini and Austin, 1999), and may substitute or complement government enforcement efforts.[16]

We include four industry variables. *RD* is industry research and development expenditure. *Age* represents the "newness" of industry assets, as measured by the ratio of net to gross assets (see Khanna and Damon, 1999); industries with newer assets (and hence, less accumulated depreciation) have *Age* values closer to one. *Herf* is the four-firm Herfindahl Index, a measure of industry concentration. And *Growth* is industry sales growth over the prior year. Newer assets contain more recent pollution abatement equipment and, hence, are expected to reduce toxic releases. Similarly, more research intensive and rapidly growing industries are expected to be more facile in abating pollution. More concentrated industries may be more heavily regulated because they are perceived to be more facile in adapting to tighter emission standards; on the other hand, concentrated industries may be more effective at lobbying for more lax regulation. Hence, expected effects of concentration (*Herf*) on emissions and inspections are unclear.

Finally, our key self-policing policy variables are dummies indicating whether or not a state has a particular statute in place.[17] Three general classes of self-policing statutes are indicated. First, does a State provide privilege protections to information contained in environmental self-audits? If so, our *Privilege* variable takes a value of one. Second, does a State explicitly provide reductions in gravity-based penalties to qualified self-reported violators, consistent with the EPA's 1995 Audit Policy? If so, our *Self-Police* variable takes a value of one. Third, alternatively, does a state provide complete immunity from penalty for qualified self-reported violations? If so, our *Immunity* variable takes a value of one. For each of these variables, we also make finer distinctions. In some states, audit privilege and immunity laws only apply to civil and administrative penalties, while in others, these laws also apply to criminal penalties. Variables distinguishing these effects are denoted by the suffixes, *-ac* (for administrative and civil) or *–aco* (for administrative, civil, and other). Self-policing policies are similar in their provisions, but vary in their applicability. Some States apply self-policing benefits to all businesses (which we measure with the dummy, *SP-ab*), while others apply only to small businesses (*SP-sb*).

In practice, the distinction between "self-policing" and "immunity" statutes goes beyond language on the extent of immunity when it is granted (reducing gravity-based penalties versus waiving sanctions altogether). Immunity statutes are generally less restrictive in terms of the eligibility requirements for relief. Following EPA guidelines, "self-policing" policies are much more specific and encompassing with regard to the information that a firm must provide, including self-audit material that goes beyond the initial disclosure.[18] Unlike immunity statutes, these policies also stipulate specific timelines for disclosure and correction (note 3).

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[16] If strict liability substitutes for government oversight – spurring fewer inspections and regulatory actions – it may also indirectly spur higher emissions. In our empirical work, however, we do not find evidence for this "enforcement substitution" effect.

[17] These variables are constructed from data in Frey and McCollough (2003) and a review of State Codes.

[18] The EPA requires, at a minimum, "access to all requested documents; access to all employees of the disclosing entity; assistance in investigating the violation, any noncompliance problems related to the disclosure, and any environmental consequences related to the violations; access to all information relevant to the violations disclosed, including that portion of the environmental audit report or documentation from the compliance management system that revealed the violation; and access to the individuals who conducted the audit or review" (EPA, 1995). In contrast, immunity statutes do not define what cooperation is specifically required for relief (with the exception of Rhode Island) and in almost all cases, limit required cooperation to the investigation of the self-reported violation.

Like Stafford (2005), we treat our self-policing policy variables as exogenous. Inclusion of fixed effects mitigates any potential for endogeneity. Moreover, our industries are individually small contributors to overall State pollution; the average industry share of State emissions in our data is 3.5 percent. However, there is the potential for cross-industry (intra-State) correlation in inspections for which we account in our estimation (more later). In view of this potential, the adoption of self-auditing policies might reflect lawmakers' anticipation of tightened enforcement budgets (reduced inspection activity). In response to this point, we first note that the underpinning conjecture is belied by the analysis of Stafford (2006) who finds no significant effect of environmental budgets on the probability of State adoption of self-auditing policies. Nevertheless, beyond fixed effects, we incorporate a full set of exogenous State-level variables to control for this and other potential determinants of State adoption decisions. In particular, we control for State government spending, both overall (reflecting general budget pressures) and on natural resource programs (reflecting pressures on environmental budgets).[19]

Table 2.1 describes our endogenous and explanatory variables. Table 2.2 gives corresponding sample statistics. Table 2.3 describes which States have adopted environmental privilege and/or complete immunity for self-reported violations. Table 2.4 describes which States have officially adopted a "self-policing" policy following EPA guidance. Note that 22 States have adopted immunity laws, and all but two of these States (New Jersey and Rhode Island) have also enacted privilege protections. Four additional States have enacted privilege statutes, but not any immunity protections. Beyond timing differences in adoption of self-auditing statutes, distinct effects of privilege and immunity are thus identified in our data by 6 out of the 26 States that adopted at least one of these statutes. Among these 26 States, 18 limit the protections to administrative and civil proceedings. In addition, 19 states have enacted EPA-sanctioned "self-policing" statutes, with sixteen of these offering no additional (privilege or immunity) protections to environmental self-auditors. In all but two of these states, the enacted "self-policing" benefits apply to all regulated businesses. In all, 42 States have enacted some form of policy inducement to environmental self-auditing.

We note that most of the State statutes on self-policing were enacted in the middle of the 1990's, with some time series variation. This period follows development and promotion of Environmental Management Systems (Anton, Deltas, and Khanna, 2004) and other programs for firms to better monitor and control the effects of their activities on the environment (such as the ISO-14001 program, King, Lenox and Terlaak, 2005). Improved environmental self-auditing and control opportunities may have contributed to the spread of State policies that reward firm self-auditing initiatives. In our empirical models, we capture these forces with industry, time and in some cases, industry by time fixed effects; the latter account for general industry-specific technological change.

Ohio's statute is typical, requiring cooperation in "investigating the cause, nature, extent, and effects of the non-compliance" (Frey and McCollough, 2003).

[19] We also control for political and economic circumstances that capture relevant incentives for policy adoption (see Stafford (2006) for a detailed discussion of these determinants). For example, pressures of Republican voting and Sierra Club membership control for political impulses. Population, income, and mining and manufacturing activity control for relevant socio-economic pressures for self-auditing reform.

2.4. Methods and Models

A. The Emission Equation. For the reduced form emission equation (3) and for each of our emission measures ("total" NESHAPS chemicals and carcinogenic NESPHAPS chemicals), we estimate four linear models at a State-industry level, all with fixed cross section and time effects. The first model is a parsimonious specification that includes only the three key policy variables and measures of industry scale (*Sales*, *Empl*, and *Facility*), State income (*Income*) and population (*Pop*). The next two models include all of our posited explanatory variables, the first with our three aggregated policy variables (*Privilege*, *Immunity*, and *Selfpolice*) and the second with all six self-policing policy variables (our "base model"). The fourth "expanded" model incorporates an expanded array of regressors, including lagged inspections, squares of several key variables, and industry-by-time fixed effects that control for arbitrary technological change.[20] Finally, we present a facility-level estimation of our "base model."
Although cross-observation error covariation does not bias coefficient estimates in our linear model, it can lead to substantial bias in standard error estimates (the Moulton or "false precision" problem, highlighted in Bertrand, Duflo, and Mullainathan, 2004). In our models, there is the potential for both time series and cross-section covariation. We are particularly concerned about covariation within an industry, across States, as unobserved economic and regulatory phonomena may lead to common industry-level impacts. There is also the potential for covariation between industries within a State, as enforcement policy is decided at a State level. To account for such covariation, we cluster our errors at the multi-way (State and industry) levels (Cameron, Gelbach, and Miller, 2006). This approach accounts for arbitrary time series covariation within cross-section units and arbitrary cross-section covariation within cluster groups (within an industry across States or across industries within a State).[21]

Tables 2.5A and 2.5B present results from our emission equation estimations for total NESHAPS releases (Table 2.5A) and carcinogenic NESHAPS releases (Table 2.5B). Table 2.5C presents key coefficient estimates in "base model" estimations using toxicity-weighted NESHAPS releases and high-toxicity NESHAPS releases (those chemicals with toxicity weights greater than 500).

B. The Inspection Equation. Three central issues arise when estimating the inspection equation (4). First, inspections at each plant take a count form with no negative values and mostly zeroes and ones. Aggregating to a State-level industry gives us a somewhat more continuous distribution of values, but still one with a substantial number of zeroes (a third of our sample) and counts that are predominantly less than five (72 percent).[22] Second, as with emissions, there is the potential for cross-observation error covariation to bias standard error estimates produced by standard methods. Intuitively, one might expect that inspection policy,

[20] We are indebted to an anonymous referee for proposing this model. Industry by time effects are at a two-digit SIC level.

[21] The Moulton problem is potentially even more acute with plant-level data, and again is addressed with multi-way (State by industry) clustered errors. We have also estimated dynamic counterparts to equation (3). Because qualitative results are similar, lagged emissions are insignificant, and clustering accounts for autocorrelation, we only present the simpler non-dynamic models in Table 2.5.

[22] The truncation at zero is less pronounced when one accounts for our fixed effects. Dropping State-level industries that are never inspected over our sample period (as done with fixed effects), 12 percent of our sample has inspection numbers equal to zero. However, 63 percent of the sample has inspection numbers less than five. With plant-level data, the count structure is much more pronounced, with 70 percent of inspection counts equal to zero and over 99 percent less than five.

within a State, may be correlated across industries. Without accounting for this covariation, standard error estimates can understate the true extent of error in coefficient estimates (Bertrand, et al., 2004). Third, we have a panel structure with individual effects.

We present four types of estimation in order to account for these potential concerns, all of which include fixed time effects. First, we estimate a count model that takes the standard linear exponential form of equation (4) (Cameron and Trivedi, 1998). Because Poisson fixed effects models suffer from an equi-dispersion constraint that we reject in statistical tests, we present Negative Binomial conditional fixed effects estimations that do not impose this constraint.[23] Second, we estimate two types of models with multi-way State-by-industry clustering (Cameron, et al., 2006): (1) Negative Binomial (NB) with fixed State and industry effects, and (2) fixed effects linear models. Third, accounting for all concerns at the cost of lost information, we aggregate our data to a State level, giving us (loosely speaking) a continuous dependent variable that eliminates within-State cross-section covariation.[24] With this data, we estimate a fixed effects linear model and cluster the errors to account for generalized autocorrelation. Finally, as a robustness check, we also estimate a number of models at a facility level.

Tables 2.6A-2.6C present results from the estimations. Table 2.6A presents three NB fixed effects models: (i) a "reduced" model, (ii) a "base" model with all explanatory variables and the three unpartitioned self-policing policy variables, and (iii) an expanded model that includes lagged emissions, squares of key policy variables, and the six partitioned self-policing indicators. Table 2.6B presents clustered NB and linear fixed effects estimations; both "expanded" models include industry-by-time effects that control for technological change. Table 2.6C present results from facility-level and State aggregate regressions.[25] In all cases, we estimate dynamic models in view of generally significant lags.[26]

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[23] We have also estimated Poisson and Negative Binomial random effects models and obtained qualitatively similar results. We attempted Tobit estimations with clustered errors, but the estimations did not converge. Available software does not permit estimation of clustered conditional fixed effects count models. We did not attempt clustered explicit (unconditional) fixed effects count models because they yield inconsistent parameter estimates due to the incidental parameters problem (Hausman, et. al, 1984).

[24] To obtain State aggregates, industry variables are weighted by shares of State facilities with the exception of the dependent variables and the scale variables (*Sales*, *Empl*, and *Facility*), which are summed across the State-level industries.

[25] We again thank the referees for suggesting many of these additions. Attempts to estimate NB fixed effects and plant-level models with the industry-by-time dummies did not converge. Because the count structure of the data is particularly pronounced in the plant-level regressions, we focus on clustered NB models with fixed State, industry and time effects in Table 2.6C.

[26] For the NB models, the lagged regressor equals the log of one plus lagged inspections, consistent with the exponential model form. Dynamics alter our theoretical formulation, eq.s (1)-(4). With no dynamics in emissions (note 21), eq. (1) persists. Inspection dynamics alter equations (2)-(4) with the addition of the right-hand lag, I^*_{it-1}, and in the "expanded" models, E_{it-1}. Finding an insignificant coefficient on lagged inspections in the emission equation, this change requires no adjustment to the econometrics of that equation. However, for inspections, remaining serial correlation will generate correlation between both lags and the error; absent serial correlation, the lags can be treated as exogenous. In all NB models, we test for serial correlation and do not reject the null of no autocorrelation (see presented z statistics, distributed standard normal under the null). For the linear models, however, the Wooldridge test statistic for autocorrelation is statistically significant; we therefore instrument the lagged endogenous variables with lagged data in the presented estimations (following standard practice, see Greene, 2003).

2.5. Results

We begin by summarizing the estimation results for our main variables of interest, the self-policing policy variables *Privilege*, *Immunity*, and *Self-Police*:

Privilege reduces both total toxic air pollution and carcinogenic toxic air pollution (see Tables 2.5A-2.5B). These effects are statistically significant, but driven by protections for administrative and civil offenses and not additional protections for criminal offenses. The estimated effect of administrative and civil privilege protection is to reduce total toxics by approximately 35% and total carcinogenic toxics by approximately 90% (both as proportions of sample mean emissions). However, in our State-specific industry regressions, the estimated impact of *adding* criminal privilege is to *increase* total toxics by 23% and carcinogenic toxics by 54%, offsetting two-thirds and 60 percent of the original emission reductions, respectively. Our plant-level regressions indicate that adding criminal privilege may completely negate the emission reduction effect of the more restricted privilege protection. Qualitatively similar conclusions are obtained using high-toxicity NESHAPS emissions (Table 2.5C). For toxicity-weighted emissions (our suspect measure), we find a positive effect of broad privilege (including criminal protection), but still a negative (though insignificant) effect of privilege for administrative and civil offenses on the weighted releases.

Immunity raises both total and carcinogenic toxic air pollution (Tables 2.5A-2.5B). These effects are large and statistically significant and attenuated if criminal immunity is added to immunity in administrative and civil disputes. The estimated impact of administrative and civil immunity is to raise total toxic pollution by approximately 27 percent on average and total carcinogenic toxics by almost 90 percent (Models 3 and 4, Tables 2.5A-2.5B). However, the addition of criminal immunity offsets these increases by between 53 and 78 percent of the original increase for total toxics and between 85 and 98 percent of the original increase for carcinogenic toxics. These qualitative conclusions are robust to plant-level regressions (Model 5 in Tables 2.5A-2.5B) and alternative emission measures (Table 2.5C).

Privilege reduces inspection rates. Regardless of the data (State-industry level, facility-level, or State aggregate) or econometric model (count or linear), we estimate significant negative effects of *Privilege* on inspections (see Tables 2.6A-2.6C). In the undifferentiated (three policy) models, *Privilege* is estimated to reduce inspections by between 13 and 23 percent.[27] In all models with State-industry data (Tables 2.6A-2.6B), we estimate lesser impacts if privilege for criminal offenses is also attached; for example, in the third count (NB) model in Table 2.6B, privilege for administrative and civil offenses is estimated to reduce inspections by 18 percent, but adding criminal privilege is estimated to offset more than half (9.6 percent) of this reduction. Using the plant-level data, however, we find no significant effect of adding criminal privilege; without the additional protection, *Privilege* is estimated to reduce inspections by approximately 21 percent (Table 2.6C, Model 3); with it, the estimated effect is 24 percent, a statistically insignificant difference.

Immunity raises inspection rates in all models. In the undifferentiated (three policy) models, estimated impacts range from increases of 17 to 23 percent in the State-industry models (Tables 2.6A-2.6B) and from 32 to 60 percent in the facility-level models (Table 2.6C). These effects are statistically significant except in the State aggregated linear estimation that discards cross-industry information. In all models, we estimate smaller impacts if immunity for criminal

[27] In the Negative Binomial models, estimated proportional marginal effects equal exp(b)-1, where b is the coefficient estimate on the policy dummy.

offenses is also applied (*I-aco* vs. *I-ac*). For example, in the six-policy State-industry count model (Model 3 of Table 2.6B), immunity for administrative and civil offenses (*I-ac*) is estimated to raise inspection rates by 41 percent, and all but three percent of this increase is offset if criminal immunity is added (*I-aco*).

We generally find no statistically significant impacts of the *Selfpolice* policies on emissions or inspections, with coefficients sometimes positive and sometimes negative. There are a few exceptions, but none that are robust across different data types or econometric models.[28] When *Selfpolice* coefficients are positive, they are almost always small by comparison to counterparts for *Privilege* and *Immunity*. For example, in models with three policies, positive *Selfpolice* coefficients are between 1.8 and 4.3 percent of counterparts for *Privilege* and *Immunity* in the emission equations (Tables 2.5B-2.5C) and between .8 and 2.3 percent of counterparts in the inspection equations (Table 2.6C).

Note that *Immunity* is quite tightly associated with *Privilege* in our data, both in terms of time and space, with only two States adopting *Immunity* without *Privilege*. As a result, we interpret our estimations on the effect of immunity as impacts of *adding* immunity to privilege, as opposed to impacts of free-standing immunity.

How can one explain the impacts of criminal protections that are identified here? Intuitively, fear of criminal prosecution, even if a remote possibility, makes self-auditing particularly worrisome to firm managers as it can provide prosecutors with a roadmap to environmental crimes that managers may themselves be unaware of prior to an audit (see Starr and Cooney, 1996, for example). Both criminal privilege and criminal immunity can help in allaying these fears, but criminal immunity may be the more important of the two. If so, criminal immunity will provide a significant spur to the adoption of self-auditing, while the addition of criminal privilege will have a relatively small effect on these incentives (over and beyond the impact of privilege in administrative and civil matters). With both criminal protections depleting pollution prevention incentives, the spur to self-auditing can dominate in the case of criminal immunity – leading to a net effect of lower pollution – while the deterrence depletion effect can dominate in the case of criminal privilege. In order to counter the deterrence depletion effect of criminal privilege, the government may increase its monitoring of firms' environmental practices to ensure that they meet desired standards of "care." Conversely, self-auditing – with immunity – negates the effect of government monitoring on accident sanctions and thereby reduces inspection incentives; hence, the added spur to self-auditing provided by the addition of criminal immunity can lead in turn to reduced inspection activity.

To sum up, privilege protections have salutary effects on pollution and requisite government inspections, lowering both. In contrast, complete immunity has adverse effects on pollution and regulatory costs, raising both. Our results thus support our initial Hypotheses 1 and 2. *Privilege* is most effective in lowering pollution and inspections if it is only applied narrowly, to civil and administrative cases and not to criminal offenses. For *Immunity*,

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[28] For emissions (Tables 2.5A-2.5C), the exceptions are: 1) Broadly applied self-policing (*SP-ab*) is estimated to have a significant positive impact on plant-level total toxics (Table 2.5A, Model 5) and State-industry carcinogenic toxics in our expanded model (Table 2.5B, Model 4); (2) the narrowly applied policy (*SP-sb*) is estimated to have a significant positive impact on high-toxicity State-industry emissions (Table 2.5C); and (3) the composite (*Selfpolice*) policy is estimated to have a significant negative impact on toxicity-weighted emissions (Table 2.5C). For inspections (Tables 2.6A-2.6C), the one exception is in our three-policy fixed effects count (NB) models of Table 2.6A, where we estimate significant negative coefficients on the *Selfpolice* policy dummy; however, once we account for the Moulton problem by clustering the errors, the statistical significance of the parameter estimates vanishes (Table 2.6B).

implications for the breadth of application are less clear. *If* a State is going to enact a complete immunity statute, it may be advantageous to apply immunity broadly – to administrative, civil *and* criminal cases; the *addition* of criminal immunity is estimated to lower both toxic emissions and government inspection effort. However, relative to complete immunity, the more confined protections afforded by "self-policing" statutes that mimic EPA guidance are generally estimated to lower both pollution and inspections.[29] This suggests the tentative prescription that combining the limited immunity of the *Selfpolice* policies with administrative and civil *Privilege* protections can have the salutary effects of spurring lower toxic pollution and saving regulatory costs.[30]
In addition to self-policing and scale effects, Tables 2.5 and 2.6 reveal impacts of a few other variables. Industry research (*RD*), concentration (*Herf*), and newer assets (*Age*) are found to reduce toxic emissions (Tables 2.5A-2.5B). All of these variables are associated with greater facility to abate pollution, spurring tighter regulation, consistent with results of other studies focusing on impacts of innovation (see, for example, Carrion-Flores and Innes, 2009). As conjectured at the outset, State expenditures on natural resource programs (*Nrexp*) crowd out enforcement of clean air laws, leading to fewer inspections (Tables 2.6A-2.6B); however, we find no significant resulting impact on toxic air pollution, perhaps because State regulators compensate by focusing inspections more on toxic (vs. criteria) air pollution. Lastly, in all inspection models other than the State aggregates, per-capita Sierra Club membership (*Sierra*) and Republican vote shares (*Repvote*) are negatively associated with inspection activity (significantly so in the Negative Binomial models). These results are consistent with pro-business constituencies successfully pushing for less regulatory oversight and private environmental pressure (*Sierra*) serving as a substitute for government enforcement (see Innes and Sam, 2008, for a similar result). Neither effect is found to significantly increase toxic air pollution; indeed, private political pressure on firms' environmental performance (as measured by *Sierra*) is found to reduce toxic air carcinogens, more than compensating for reduced inspection activity. However, the size of this estimated impact is small: A doubling of per-capita Sierra Club membership is estimated to lower carcinogenic emissions by one-fifth of one percent (.002 in the "base case" six-policy model, and .0016 in the three-policy model).

2.6 Conclusion

Regulators and environmental groups criticize State-level self-policing statutes because they enable firms to hide their environmental crimes (in the case of privilege) and deny them incentives to prevent pollution outbreaks (in the case of immunity). As a result, they argue, these policies lead to more pollution and require more government monitoring of regulated firms in order to ensure that appropriate pollution abatement activity takes place. In contrast, proponents of these statutes argue that they are necessary for firms to audit their own environmental practices, auditing programs that are costly but yield substantial dividends by identifying and correcting pollution outbreaks that would not otherwise be discovered. These pollution-

[29] Comparing coefficients on broad immunity (*I-aco*) and self-policing (*SP-ab*) in the six-policy emissions equations, there is not a statistically significant difference (except with the toxicity-weighted data of Table 2.5C). However, comparing overall *Immunity* to *Selfpolice* in models with three policies, the latter is estimated to lower pollution relative to the former in all cases (differences that are statistically significant). Similarly, in our inspections models, *Selfpolice* leads to fewer inspections relative to *Immunity* counterparts (differences that are again statistically significant).
[30] In theory, *Privilege* is of no benefit absent some immunity protection; without the latter, there are no incentives for adoption of self-auditing programs.

reduction benefits of self-auditing are made possible by statutes that protect firms from thereby incriminating themselves and give regulatory rewards to the self-reporting of self-discovered violations. In addition, because firms audit themselves, the statutes may also permit environmental law enforcement to be done effectively with fewer government inspections. These two competing arguments embed incentive effects that are also competing and yield theoretically ambiguous impacts of self-policing policies. Immunity encourages firms to adopt self-auditing programs which can lower the harm from pollution outbreaks. However, immunity also reduces self-auditing firms' incentives to avoid pollution violations in the first place, thereby increasing average pollution and motivating more government monitoring of firms' pollution prevention activities. We conjecture that *complete* immunity has such powerful deterrence-depletion effects – because it gives firms no penalty at all from pollution violations, other than costs of correction – that the second (pollution raising) effect will dominate the first (self-auditing promotion) effect. Hence, we expect immunity to raise both toxic pollution and government inspections.

Similarly, privilege encourages firms to adopt self-auditing programs, but makes it more difficult for regulators to identify and sanction slovenly firm performance in pollution prevention (care). With less effective "care" regulation, firms have less incentive to exercise care and the government has less incentive to regulate it. Privilege can thus lower pollution by eliciting more environmental self-auditing, but raise pollution by reducing deterrence. If the first effect dominates – as we conjecture – then pollution will fall and governmental monitoring is also likely to decline both because harm-reduction benefits of monitoring are smaller and because monitoring is less effective in spurring pollution prevention.

Our empirical results confirm both of our conjectures to varying degrees. Privilege protections are estimated to reduce toxic emissions and government inspections. These results indicate salutary effects of these policies, spurring savings of both environmental costs and regulatory resources. In contrast, complete immunity is estimated to raise toxic emissions and government inspections. These effects tend to confirm environmentalists' criticism of self-reporting inducements, when they are too liberal, as protecting polluters to society's detriment. Overall, our results suggest that providing firms with positive incentives for environmental self-auditing, by protecting their audits from use by government prosecutors, can be a valuable component of environmental law enforcement, reducing both pollution and enforcement costs. They also suggest the need for care in the design of self-auditing inducements, arguing against blanket privilege and immunity protections and instead in favor of more targeted and limited protections.

List of Tables

Table 2.1. Definition of Variables

Level of aggregation	Variable	Description	Source
Sic-state	Emissions	Millions of pounds of toxic on-site air emissions of NESHAPS chemicals (Total or Carcinogenic only)	TRI (www.epa.gov/tri/)
Sic-state	Inspections	State and Federal Clean Air Act inspections	IDEA Database
Sic-state	Facility	Number of facilities registered in the IDEA Database	IDEA Database
State	Pop	State population (millions)	Economagic (www.economagic.com)
State	Income	State income per capita (millions of dollars)	Economagic (www.economagic.com)
State	Expend	State expenditures (billions of dollars)	Economagic (www.economagic.com)
State	Nrexp	State expenditures on natural resources (millions of dollars)	US *Statistical Abstracts*, various years
State	Gspmm	Gross State Product in mining and manufacturing (trillions of dollars)	Bureau of Economic Analysis (www.bea.gov/bea/regional/gsp/)
State	Sierra	State Sierra Club membership per capita	Sierra Club
State	Repvot	Ratio of popular vote cast for republican candidate to total votes in the most recent presidential election	US *Statistical Abstracts*, various years
Sic	RD	Industry R&D expenditures (billions of dollars)	Compustat
Sic	Age	Industry age of assets (Net assets/Gross Assets)	Compustat
Sic	Herf	Industry four-firm Herfindahl Index	Compustat
Sic	Growth	Industry growth in sales	Compustat
Sic-state	Empl	Industry number of employees by state (millions)	Compustat and IDEA
Sic-state	Sales	Industry total sales by state (trillions of dollars)	Compustat and IDEA
State	Strict	Dummy variable indicating strict liability	Environmental Law Institute (ELI)
State	P-ac	Dummy variable indicating Privilege applicable to administrative and civil penalties	State Codes, various years
State	P-aco	Dummy variable indicating Privilege applicable to administrative, civil and criminal penalties	State Codes, various years
State	I-ac	Dummy variable indicating Immunity applicable to administrative and civil penalties	State Codes, various years
State	I-aco	Dummy variable indicating Immunity applicable to administrative, civil and criminal penalties	State Codes, various years
State	SP-sb	Dummy variable indicating Selfpolicing Policies only valid for small businesses	State Codes, various years
State	SP_ab	Dummy variable indicating Selfpolicing Policies applicable to all businesses	State Codes, various years
State	Selfpolice	Dummy variable indicating Self policing Policies (SP-sb or SP-ab)	State Codes, various years
State	Immunity	Dummy variable indicating Immunity (I-ac or I-aco)	State Codes, various years
State	Privelege	Dummy variable indicating Privilege (P-ac or P-aco)	State Codes, various years

Table 2.2. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Inspections (plant-level)	251355	0.476	1.281	0	111
Inspections (sic/state-level)	19472	4.797	10.737	0	281
Emissions unweighted (sic/state-level)	19472	0.407	0.891	1.58E-10	10.63372
Emissions unweighted (plant-level)	114675	0.069	0.229	4.00E-12	6.518379
Emissions carcinogenic (sic/state-level)	9550	0.101	0.314	2.00E-11	5.836833
Emissions carcinogenic (plant-level)	24456	0.039	0.14	4.30E-12	5.727293
Emissions weighted (sic/state-level)	19418	116.845	1053.864	1.35E-06	54032.14
Emissions 500 + (sic/state-level)	11892	0.03	0.112	1.58E-10	2.854608
Expend	19472	0.31	0.067	0.178659	1.229198
Age	19472	0.764	0.116	0.073604	1
Herf	19472	5.866	2.326	2.51391	10
Sales	19472	0.001	0.003	3.12E-09	0.072215
RD	19472	0.732	2.682	0	18.16555
Growth	19472	0.294	2.068	-0.97476	29.37388
Facility	19472	6.183	9.593	1	224
Empl	19472	0.002	0.007	3.45E-08	0.202307
Sierra	19472	0.002	0.003	0.00031	0.052502
Pop	19472	7.342	6.563	0.45369	35.48445
Nrexp	19472	0.33	0.388	0.023329	2.894366
Income	19472	0.023	0.004	0.015314	0.036931
Gspmm	19472	0.038	0.032	0.000679	0.172008
Repvote	19472	0.458	0.089	0.106178	0.678899
Strict	19472	0.741	0.438	0	1
P-ac	19472	0.146	0.353	0	1
P-aco	19472	0.133	0.34	0	1
I-ac	19472	0.137	0.344	0	1
I-aco	19472	0.073	0.26	0	1
SP-sb	19472	0.011	0.104	0	1
SP-ab	19472	0.182	0.386	0	1
Privilege	19472	0.279	0.448	0	1
Immunity	19472	0.21	0.407	0	1
Selfpolice	19472	0.193	0.395	0	1

Table 2.3. Audit Privilege and Immunity Laws: Provisions and Years of Adoption

State	Year of adoption	Privilege	Immunity	Provisions: Administrative and Civil Penalties	Provisions: Other legal actions
Alaska	1997	x	x	x	
Arkansas	1995	x		x	x
Colorado	1994	x	x	x	x
Idaho	1996*	x	x	x	x
Illinois	1995	x		x	x
Indiana	1994	x		x	Criminal penalties removed in the 1999 amendments
Iowa	1998	x	x	x	
Kansas	1995	x	x	x	x
Kentucky	1996	x	x	x	
Michigan	1996	x	x	x	Criminal penalties removed in the 1997 amendments
Minnesota	1995	x	x	x	x
Mississippi	1995	x	x	x	Criminal penalties removed in the 2003 amendments
Montana	1997**	x	x	x	
Nebraska	1998	x	x	x	x
Nevada	1997	x	x	x	x
New Hampshire	1996	x	x	x	
New Jersey	1995		x	x	
Ohio	1997	x	x	x	
Oregon	1993	x		x	Criminal penalties adopted in 1997 amendments and removed in 2000
Rhode Island	1997		x	x	
South Carolina	1996	x	x	x	Criminal penalties removed in the 2000 amendments
South Dakota	1996	x	x	x	
Texas	1995	x	x	x	Criminal penalties removed in the 1997 amendments
Utah	1996	x	x	x	
Virginia	1995	x	x	x	
Wyoming	1995	x	x	x	

Source: Frey and McCollough (2003)
*In sunset since 1997
**In sunset since 2001

Table 2.4. Self-policing Policies: Provisions and Years of Adoption

State	Year of adoption	Applies only to Small Business	Applies to All Business
Arizona	2002		x
California	1996		x
Connecticut	1996		x
Delaware	1994		x
Florida	1996		x
Hawaii	1998		x
Indiana	1999		x
Maine	1996	x	
Maryland	1997		x
Massachusetts	1997		x
Minnessota	1995		x
New Mexico	1999		x
New York	1999	x	
North Carolina	1995		x
Oregon	2002		x
Pennsylvania	1996		x
Tennessee	1996		x
Vermont	1996*		x
Washington	1994		x

Source: Frey and McCollough (2003)

* In sunset from 1998 to 2000

Table 2.5A. Total NESHAPS Emissions

		State-Industry				
Variables		Reduced	3 Policies	6 Policies	Expanded	Plant-level
Inspection $_{t-1}$					0.0006	
					[0.0015]	
Expend			-0.409	-0.44	-0.413	-0.0768
			[0.43]	[-0.4]	[0.4157]	[0.0628]
Age			-0.23	-0.23	-0.204**	-0.0431**
			[0.14]	[0.14]	[0.0992]	[0.0218]
Herf			-0.0228*	-0.0228*	-0.0258**	-0.0028
			[0.013]	[0.013]	[0.0107]	[0.0023]
Sales		-35.17***	-36.09***	-36.18***	-22.45	-1.046
		[12.3]	[11]	[11.2]	[28.4238]	[1.7853]
Sales 2					-390.6	
					[473.6993]	
RD			-0.0390***	-0.0391***	-0.0597***	-0.007**
			[0.0083]	[0.0083]	[0.018]	[0.0028]
RD 2					0.0019	
					[0.0016]	
Growth			0.00209	0.00207	-0.0017	0.0005
			[0.003]	[0.003]	[0.0022]	[0.0005]
Facility		-0.00326	-0.00341	-0.00335	-0.0001	0.0001
		[0.004]	[0.0038]	[0.0039]	[0.0028]	[0.0002]
Empl		9.26	13.39**	13.38***	1.489	0.796
		[6.78]	[5.61]	[5.57]	[10.66]	[0.675]
Empl 2					89.81	
					[73.683]	
Sierra			0.414	0.68	0.493	0.0816
			[1.21]	[1.26]	[1.173]	[0.069]
Pop		-0.0288*	-0.0321	-0.0328	-0.0197	0.0114***
		[0.0156]	[0.021]	[0.023]	[0.0221]	[0.0029]
Nrexp			0.0569	0.0507	-0.0122	0.0043
			[0.036]	[0.05]	[0.0396]	[0.0123]
Income		33.37***	42.14***	38.49***	91.37*	3.911*
		[11.24]	[14.8]	[13.3]	[54.25]	[2.17]
Income 2					-876.6	
					[803.903]	
Gspmm			-1.046	-0.546	-2.47	-0.317
			[2.2]	[2.04]	[1.8059]	[0.203]
Repvot			0.262	0.286	0.147	-0.0374
			[0.2]	[0.19]	[0.1565]	[0.0394]
Strict			-0.0293	-0.0264	-0.0456	-0.018**
			[0.043]	[0.043]	[0.0469]	[0.0085]
	P-ac			-0.143***	-0.139**	-0.0171*
Privilege		-0.0936**	-0.088*	[0.065]	[0.0665]	[0.0093]
	P-aco	[0.043]	[0.046]	-0.0472	-0.0303	0.002
				[0.041]	[0.0343]	[0.0072]
	I-ac			0.113*	0.108*	0.0155*
Immunity		0.0743	0.0685*	[0.063]	[0.0635]	[0.0087]
	I-aco	[0.046]	0.05]	0.0531	0.0258	-0.009
				[0.055]	[0.0461]	[0.0086]
	SP-ab			0.0102	0.0234	0.008***
Selfpolice		-0.00979	-0.00555	[0.089]	[0.0277]	[0.0035]
	SP-sb	[0.026]	[0.027]	0.00914	-0.0028	-0.0016
				[0.026]	[0.093]	[0.0082]
Obs		19472	19472	19472	17229	114675
R2		0.12	0.13	0.13	0.18	0.04

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). All models include fixed cross-section (State-industry or plant) and time effects. The expanded model also includes industry by time dummies (in place of time effects). The first four models use State-specific industry level data, and the fifth model uses plant-level data.

Table 2.5B. Carcinogenic NESHAPS Emissions

Variables		State-Industry				Plant-Level
		Reduced	3 Policies	6 Policies	Expanded	6 Policies
Inspection $_{t-1}$					-0.0007	
					[0.0006]	
Expend			0.0422	0.0499	0.113	-0.009
			[0.2694]	[0.2995]	[0.2293]	[0.085]
Age			-0.103**	-0.106**	-0.086	-0.043***
			[0.048]	[0.048]	[0.0755]	[0.011]
Herf			-0.0193***	-0.0192***	-0.0126***	-0.005***
			[0.0052]	[0.0051]	[0.0026]	[0.002]
Sales		5.892	4.388	4.182	9.794	-0.078
		[6.285]	[3.351]	[3.342]	[8.5661]	[0.634]
Sales 2					-235.1	
					[148.5536]	
RD			-0.0265***	-0.0262***	-0.0595***	-0.014***
			[0.0075]	[0.0074]	[0.0108]	[0.004]
RD 2					0.0021***	
					[0.0005]	
Growth			0.00197	0.002	-0.0003	0.0007**
			[0.0013]	[0.0013]	[0.0007]	[0.00036]
Facility		-0.00268	-0.00269	-0.00267	-0.0008	0.00021
		[0.0027]	[0.0027]	[0.0027]	[0.0019]	[0.00032]
Empl		-2.535	0.463	0.428	-3.437	0.634**
		[3.231]	[1.642]	[1.664]	[3.1495]	[0.322]
Empl 2					38.24	
					[26.8782]	
Sierra			-1.037**	-0.829*	-0.474***	-0.051
			[0.484]	[0.506]	[0.051]	[0.103]
Pop		-0.014	-0.00489	-0.00801	0.0038	0.010*
		[0.0104]	[0.0167]	[0.0164]	[0.0152]	[0.006]
Nrexp			-0.0113	-0.00451	-0.0518*	-0.001
			[0.0377]	[0.0283]	[0.0295]	[0.012]
Income		10.26	15.32*	15.15*	22.16	1.089
		[6.337]	[8.122]	[7.819]	[29.045]	[2.781]
Income 2					-193.6	
					[470.217]	
Gspmm			-1.673	-1.439	-1.563	-0.723
			[1.435]	[1.568]	[1.1957]	[0.64]
Repvot			-0.0147	0.0323	-0.0693	-0.059*
			[0.0848]	[0.0889]	[0.0812]	[0.034]
Strict			-0.0102	-0.00891	-0.0319	-0.001
			[0.0308]	[0.0298]	[0.0447]	[0.011]
	P-ac			-0.0911*	-0.0908*	-0.036*
Privilege		-0.0639*	-0.0621*	[0.05242]	[0.0502]	[0.019]
	P-aco	[0.0357]	[0.0334]	-0.0363	-0.0312	-0.006
				[0.02811]	[0.0246]	[0.017]
	I-ac			0.104**	0.104**	0.036***
Immunity		0.0649*	0.0614*	[0.0447]	[0.0439]	[0.0126]
	I-aco	[0.0332]	[0.0316]	0.0156	0.0079	-0.007
				[0.04441]	[0.0414]	[0.022]
	SP-ab			0.0235	0.0301	0.0086
Selfpolice		-0.00817	0.00169	[0.04071]	[0.0169]	[0.0058]
	SP-sb	[0.018]	-0.0102	0.0233	0.0338	0.011
				[0.015]	[0.0614]	[0.012]
Obs		9550	9950	9550	8148	24456
R2		0.09	0.13	0.13	0.19	0.05

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). All models include fixed cross-section (State-industry or plant) and time effects. The expanded model also includes industry by time dummies (in place of time effects). The first four models use State-specific industry level data, and the fifth model uses plant-level data.

Table 2.5C. Self-Policing Policy Coefficients Using Toxicity-Adjusted NESHAPS Emissions

		Toxicity Weighted Emissions		High Toxicity (500+) Emissions	
Variables		3 Policies	6 Policies	3 Policies	6 Policies
	P-ac		-90.37		-0.012**
Privilege		26.250*	[77.854]	-0.003***	[0.006]
	P-aco	[15.069]	126**	[0.001]	0.004
			[57.933]		[0.008]
	I-ac		116.2***		0.017**
Immunity		-12.130	[43.898]	0.007*	[0.008]
	I-aco	[22.336]	-110.4**	[0.004]	0.0001
			[55.721]		[0.002]
	SP-ab		3.907		0.003
Selfpolice		-39.68**	[27.471]	0.00013	[0.004]
	SP-sb	[20.095]	-19.52	[0.004]	0.009*
			[66.748]		[0.005]
Obs		19418	19418	11892	11892
R2		0.01	0.01	0.03	0.03

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). State-industry data, "base case" models. Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). All models include fixed cross-section (State-industry) and time effects.

Table 2.6A. Inspections Equation: Negative Binomial Fixed Effects (State-Specific Industry Data)

Variables		Reduced	3 policies	Expanded
$Emission_{t-1}$				-0.0338***
				[0.0078]
$Inspection_{t-1}$		0.280***	0.261***	0.252***
		[0.011]	[0.011]	[0.011]
Expend			-0.181	-0.402
			[0.44]	[0.45]
Age			-0.0434	-0.0434
			[0.067]	[0.067]
Herf			0.00795*	0.00491
			[0.0045]	[0.0046]
Sales		0.0412	0.685	54.91***
		[6.85]	[6.96]	[12.2]
Sales 2				-885.8***
				[234]
RD			-0.00538	-0.0246*
			[0.0064]	[0.013]
RD 2				-0.000399
				[0.00071]
Growth			0.000972	0.0000452
			[0.0022]	[0.0022]
Facility		0.0286***	0.0283***	0.0268***
		[0.00088]	[0.00085]	[0.00086]
Empl		11.27***	11.62***	26.73***
		[2.32]	[2.37]	[4.00]
Empl 2				-133.4***
				[29.1]
Sierra			-5.869***	-4.960**
			[2]	[2]
Pop		0.00448	-0.0312***	-0.0361***
		[0.0058]	[0.01]	[0.01]
Nrexp			-0.228**	-0.241**
			[0.096]	[0.097]
Income		6.05	-25.49**	147.1***
		[9.35]	[10.5]	[37.7]
Income 2				-3049***
				[657]
Gspmm			12.09***	11.23***
			[1.32]	[1.34]
Repvot			-1.089***	-1.377***
			[0.2]	[0.22]
Strict			-0.00963	-0.0422
			[0.028]	[0.029]
	P-ac			-0.258***
Privilege		-0.140***	-0.154***	[0.038]
	P-aco	[0.024]	[0.023]	-0.0787***
				[0.027]
	I-ac			0.286***
Immunity		0.133***	0.157***	[0.043]
	I-aco	[0.028]	[0.028]	0.047
				[0.036]
	SP-ab			-0.013
Selfpolice		-0.0469**	-0.0745***	[0.025]
	SP-sb	[0.021]	[0.022]	-0.114
				[0.14]
Obs		13039	13039	13039
z autocorr.		1.4141732	1.45504	1.2332671

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Standard errors are in brackets. All models include fixed cross-section (State-industry) and time effects.

Table 2.6B. Inspections Equation: State-Specific Industry Estimations with Clustered Errors

		Pooled Negative Binomial			Linear	
Variables		Reduced	3 policies	Expanded	3 policies	Expanded
Emission$_{t-1}$				0.006		0.858
				[0.013]		[1.738]
Inspection $_{t-1}$		0.663***	0.661***	0.638***	0.043	0.04
		[0.064]	[0.064]	[0.059]	[0.063]	[0.062]
Expend			-1.283	-1.714	14.57	14.09
			[3.773]	[3.239]	[10.331]	[10.642]
Age			-0.0253	-0.065	0.059	-0.02
			[0.077]	[0.092]	[0.73]	[0.752]
Herf			-0.011**	-0.013**	0.011	0.025
			[0.0049]	[0.006]	[0.052]	[0.067]
Sales		-1.169	-1.097	29	-352.9**	572.5*
		[16.858]	[18.487]	[21.556]	[161.921]	[339.062]
Sales 2				-775.10**		-19107***
				[407.863]		[6182.883]
RD			-0.0086	-0.032**	0.092	0.293*
			[0.009]	[0.016]	[0.069]	[0.152]
RD 2				0.000006		-0.019**
				[0.001]		[0.008]
Growth			-0.00187	-0.004	-0.017***	-0.065*
			[0.002]	[0.004]	[0.005]	[0.035]
Facility		0.0263***	0.0262***	0.027***	0.867***	0.861***
		[0.0091]	[0.009]	[0.008]	[0.145]	[0.134]
Empl		5.397	6.724	20.390**	41.91	-203.5
		[5.461]	[5.792]	[9.411]	[63.04]	[137]
Empl 2				-155.90**		1743**
				[72.391]		[714.838]
Sierra			-8.34***	-7.917***	-7.375	-2.584
			[1.437]	[1.340]	[16.44]	[16.384]
Pop		0.0983	0.152	0.161	0.885*	0.897**
		[0.097]	[0.119]	[0.109]	[0.458]	[0.447]
Nrexp			-0.442	-0.457	-4.572***	-4.382***
			[0.406]	[0.380]	[1.631]	[1.406]
Income		15.14	-18.52	172.6	643.6**	1908.*
		[50.986]	[42.975]	[148.859]	[263.078]	[1128.309]
Income 2				-2808		-19590
				[2251.823]		[17928.603]
Gspmm			-0.474	-2.403	6.541	-2.346
			[4.946]	[4.20]	[31.809]	[31.439]
Repvot			-1.671***	-1.698***	-0.864	-1.992
			[0.644]	[0.596]	[2.787]	[3.595]
Strict			0.0631	0.045	0.163	0.034
			[0.109]	[0.095]	[0.496]	[0.478]
	P-ac			-0.197**		-1.315*
Privilege		-0.148***	-0.154***	[0.100]	-1.019**	[0.695]
	P-aco	[0.053]	[0.044]	-0.086**	[0.478]	-0.812
				[0.042]		[0.8405]
	I-ac			0.346***		1.791**
Immunity		0.189***	0.218***	[0.132]	1.393**	[0.704]
	I-aco	[0.084]	[0.076]	0.032	[0.609]	1.041
				[0.095]		[1.003]
	SP-ab			-0.019		-0.373
Selfpolice		-0.0246	-0.0658	[0.089]	-0.645	[0.597]
	SP-sb	[0.09]	[0.076]	0.014	[0.555]	0.183
				[0.052]		[0.627]
Obs		17229	17229	17229	17229	17229
z/F autocorr.		1.195	1.057	1.466	86.071	91.07
R2					0.421	0.45

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry (Cameron, et al., 2006). NB pooled models include state, industry and time effects. Linear models include fixed cross-section (State-industry) and time effects. Both "expanded" models include industry by time effects. Lag inspections and emissions are instrumented in the linear models.

Table 2.6C. Inspections Equation: Plant and State Level Data

Variables		Pooled NB (plant-level) Reduced	3 policies	6 policies	Linear (state-level) 3 policies
Inspection $_{t-1}$		0.915***	0.914***	0.914***	0.0692
		[0.034]	[0.034]	[0.034]	[0.113]
Expend			-1.968	-2.173	102.9
			[5.098]	[4.792]	[187]
Age			0.281	0.284*	224.8
			[0.172]	[0.173]	[153.1]
Herf			0.00001	0.0002	-8.658
			[0.01]	[0.01]	[6.201]
Sales		10.19	13.33	13.26	-40,221
		[19.436]	[18.757]	[18.907]	[37,874]
RD			-0.014*	-0.014*	-9.76
			[0.008]	[0.008]	[7.597]
Growth			-0.004**	-0.004**	2.553
			[0.002]	[0.002]	[4.073]
Facility		0.002*	0.002*	0.002*	33.65***
		[0.001]	[0.001]	[0.001]	[7.42]
Empl		-0.312	0.811	0.789	27,052*
		[6.408]	[6.423]	[6.482]	[16,041]
Sierra			-11.810***	-11.960***	-452
			[2.124]	[2.159]	[990]
Pop		0.243*	0.334*	0.349*	9.736
		[0.142]	[0.203]	[0.212]	[20.29]
Nrexp			-0.639	-0.663	-153.1**
			[0.589]	[0.589]	[66.61]
Income		-75.73	-71	-55.54	4,965
		[52.092]	[59.237]	[55.852]	[7,526]
Gspmm			-3.44	-4.171	1,462
			[6.374]	[6.49]	[1,327]
Repvot			-0.465	-0.27	69.47
			[0.791]	[0.725]	[93.83]
Strict			0.027	0.036	-7.667
			[0.105]	[0.105]	[17.53]
	P-ac			-0.231*	
Privilege		-0.246***	-0.266***	[0.125]	-43.91*
	P-aco	[0.062]	[0.075]	-0.273***	[23.7]
				[0.106]	
	I-ac			0.531***	
Immunity		0.434***	0.474***	[0.191]	42.33
	I-aco	[0.115]	[0.136]	0.352**	[26.16]
				[0.154]	
	SP-ab			0.031	
Selfpolice		0.056	0.039	[0.115]	-9.644
	SP-sb	[0.088]	[0.093]	0.268***	[22.1]
				[0.104]	
Obs		227141	227141	227141	696
z/F autocorr.		-0.758	-0.856	-0.961	17.677
R2					0.47

Note: *,**,*** denote significant at the 10%, 5%, and 1% levels (two-sided). Robust standard errors are in brackets, clustered multi-way State-by-industry in the plant-level regressions (Cameron, et al., 2006) and at the State level in the State aggregate regression. NB pooled models include state, industry and time effects. The State aggregate linear model includes fixed cross-section (State) and time effects, and instrumented lag inspections.

Chapter 3

The Effect of Alcohol Availability on Marijuana Use: Evidence from the Minimum Legal Drinking Age

3.1 Introduction

Economic theory suggests that when the cost of consuming a good increases, people will consume more of its substitutes and less of its complements. In the case of alcohol, these substitutes and complements are likely to include other intoxicating substances. The Minimum Legal Drinking Age (MLDA), which restricts access to alcohol for those under 21, is therefore likely to affect the consumption of other drugs among that age group, as it sharply decreases the cost of consuming alcohol for individuals just over the MLDA. When assessing the costs and benefits of policies that aim to reduce alcohol consumption - like the MLDA or alcohol taxes - we need to take possible substitution behavior into account. For example, proponents of the MLDA at age 21 argue that alcohol consumption in children and adolescents can cause long term and, sometimes, irreversible damages to the brain (American Medical Association, 2008). In particular, adolescents who drink are more likely to develop smaller hippocampi, a part of the brain that controls learning and memory, and are more likely to show alterations in their prefrontal cortex (American Medical Association, 2008).

Alcohol consumption has also been shown to induce suicides and car accidents (Carpenter and Dobkin, 2009). However, if restricting access to alcohol causes people to switch to substitutes, such as marijuana or other illegal drugs, the benefits of reduced alcohol consumption need to be weighted against the cost of increased consumption of alcohol's substitutes. The alcohol substitute (complement) we analyze in this paper is marijuana, a toxic substance made of a mixture of flowers, seeds and leaves of the hemp plant. The hemp plant contains tetrahydrocannabinol or THC, a psychoactive chemical that produces most of the intoxicating effects. Consumption of THC has been associated with cognitive deficits and changes in brain morphology and psychiatric disorders (Wilson et al. 2000; Pope et al. 2003; Hall and Degenhardt 2009). In this paper we study the substitution (complementary) effects that an increase in the consumption of alcohol produces on the consumption of marijuana.

Most previous studies of substitution between alcohol and marijuana (e.g. DiNardo and Lemieux 2001, Chaloupka and Laixuthai 1997, Pacula 1998, Williams et al. 2004, Saffer and Chaloupka 1999, and Farrelly et al. 1999) are based on cross-sectional (usually between-state) variation in the prices of alcohol and marijuana, the MLDA, alcohol taxes, or laws that partially decriminalize marijuana. A problem for these approaches is that state-level prices of alcohol and marijuana and the policies governing their consumption are likely to be correlated with unobserved characteristics of the population living in those states, making it difficult to infer causality from cross-sectional comparisons (Carpenter and Dobkin 2008).

We address the problem of causal identification that has plagued previous research through a regression discontinuity design. This approach exploits the sharply discontinuous nature of the minimum legal drinking age - the fact that a person cannot legally purchase alcohol up until the day before her 21st birthday, but can do so from her 21st birthday onwards. By comparing substance use in individuals just below and just above the age of 21, we can therefore isolate the causal effect of the MLDA on alcohol and marijuana consumption. The identifying assumption is that, apart from the ability to legally purchase alcohol, individuals just above and just below the age of 21 are similar in all characteristics that determine substance use. The regression discontinuity approach allows us to estimate the extent of substitution between alcohol and marijuana and identify the causal effect of changes in the MLDA on individuals close to 21 years of age.

Our results show that alcohol and marijuana are substitutes. The MLDA at age 21 increases the probability and the frequency of alcohol consumption but it also decreases the probability and the frequency of marijuana consumption, suggesting that restricting the age at which adolescents can legally purchase alcohol induces more marijuana consumption. Compared to baseline estimated consumption just below age 21, our estimates suggest that the MLDA at age 21 increases the probability of alcohol consumption by 16% and decreases the probability of marijuana consumption by 8%, representing an elasticity of substitution of 0.5. The elasticity of substitution of the frequency of consumption is 0.3. Both substitution effects are stronger for blacks and women, suggesting that by restricting the age at which people can legally purchase alcohol, the MLDA at age 21 induces more consumption of illicit drugs among young minorities.

The next section reviews the existing literature on the MLDA and marijuana use. Section 3.3 describes the empirical strategy in more detail. Section 3.4 presents the data and results, Section 3.5 shows the robustness of our estimates and Section 3.6 concludes.

3.2 Literature Review

Most of the previous literature on substitution between marijuana and alcohol consumption exploits between-state variation in the Minimum Legal Drinking Age (MLDA) and marijuana decriminalization during the 1970s and 1980s. DiNardo and Lemieux (2001) estimate a structural model of alcohol and marijuana consumption to test the effect of increases in the MLDA. They analyze state-level percentages of high school seniors that reported having consumed alcohol/marijuana from the Monitoring the Future Surveys (MFS) during the period 1980-1989. Their results suggest that alcohol and marijuana are substitutes and that increases in the MLDA lead to a decrease in alcohol consumption and an increase in marijuana consumption. In a similar study, Chaloupka and Laixuthai (1997) find that youths living in states where marijuana was decriminalized report having consumed less alcohol, providing some evidence of substitution between marijuana and alcohol consumption.

Other studies either find no evidence of substitution or evidence of complementarity. Using data from the National Longitudinal Survey of Youth (NLSY), Thies and Register (1993) do not find statistically significant evidence that state-level marijuana decriminalization affects consumption of alcohol or marijuana. Also using data from the NLSY, Pacula (1998) finds that state beer taxes and the MLDA are positively correlated with marijuana consumption. Focusing on college students, Williams et. al (2004) analyze alcohol and marijuana consumption reported in the Harvard School of Public Health College Alcohol Study. They find that campus regulations banning the consumption of alcohol, and to a lesser extent state policies that restrict alcohol consumption, are negatively correlated with marijuana use. Using data from the National Household Surveys on Drug Abuse (NHSDA), Saffer and Chaloupka (1999) find that, controlling for the price of marijuana, county-level alcohol prices are negatively correlated with marijuana consumption. Also using data from the NHSDA, Farrelly et al. (1999) find that increases in state-level beer prices are negatively correlated with marijuana consumption for youths aged 12 to 20, but not for young adults aged 21 to 30.

Overall, the literature on substitution between alcohol and marijuana finds contradicting results. DiNardo and Lemieux (2001) and Chaloupka and Laixuthai (1997) interpret their findings as reflecting substitution between alcohol and marijuana, while Pacula (1998), Williams et. al (2004), Saffer and Chaloupka (1999) and Farrelly et al. (1999) interpret their findings as reflecting complementarity.

One possible reason for these mixed results is that different studies use different surveys and time periods, which prevents comparability. Another reason, perhaps the most important, is that many of the previous studies are based on state-level (or in the case of Williams, 2004, campus-level) variations in prices of alcohol and marijuana and policies governing their consumption. While this approach can establish correlations between substance use, prices and policies, the correlations do not necessarily reflect causal effects, since state-level prices and policies governing alcohol and marijuana are likely to be correlated with unobserved characteristics of the population living in those states that are likely to influence alcohol and marijuana consumption (Carpenter and Dobkin 2008). In this paper, we overcome this problem by exploiting the discontinuous nature of the MLDA, which creates an abrupt change in individuals' ability to legally purchase alcohol at age 21. The empirical approach - known as a regression discontinuity design - is described in detail in the next section.

3.3 Empirical Strategy

This paper uses a regression discontinuity design (RDD) to identify the effect of the legal minimum drinking age on alcohol and marijuana use. The RDD approach exploits the sharply discontinuous nature of the minimum legal drinking age - the fact that a person cannot legally purchase alcohol up until the day before her 21st birthday, but can do so from her 21st birthday onwards. Individuals therefore switch from the control regime – being legally prohibited from buying alcohol - to the treatment regime - being allowed to do so – from one day to the next. We can therefore estimate the causal effect of the minimum legal drinking age by comparing individuals who have just turned 21 and individuals who are about to turn 21. Our identifying assumption is that, apart from the ability to legally purchase alcohol, individuals just below and just above the age of 21 are similar in all characteristics that determine substance use, so that differences between the two groups can only be explained by the effect of the minimum drinking age.

Our estimates are based on the standard regression discontinuity estimator described by Imbens and Lemieux (2008):

$$\tau_{RD} = \lim_{x \uparrow 21}[Y_i|X_i = x] - \lim_{x \downarrow 21}[Y_i|X_i = x]$$

where Y_i and X_i denote individual i's substance use and age, respectively. That is, we estimate the limit of individual's substance use on both sides of the age of 21. The difference between the limits is the regression discontinuity estimate of the effect of the minimum legal drinking age. We follow Carpenter and Dobkin (2009) and estimate the limits by local linear regression on both sides of the age of 21. In practice, this is equivalent to estimating a kernel-weighted regression of the following model (Imbens and Lemieux, 2008):

$$Y_i = \beta_0 + X_i\beta_1 + X_i * D_i\,\beta_2 + D_i\tau_{RD} + \varepsilon_i \qquad (1)$$

As before, Y_i and X_i denote individual i's substance use and age, respectively. D_i is an

indicator that takes the value 1 if individual i is 21 years old or older. The estimated coefficient τ_{RD} yields the causal effect of the MLDA at age 21 on alcohol/marijuana consumption.
In the following section we describe the data and the results from the graphical analysis and the statistical analysis of equation (1).

3.4 Data and Results

Data on alcohol and marijuana use was obtained from the National Survey of Drug Use and Health (NSDUH), which is administered annually by the U.S. Department of Health and Human Services' Substance Abuse and Mental Health Services Administration (SAMHSA) and conducted by the Research Triangle Institute. The NSDUH provides estimates of alcohol and illicit substance use among persons aged 12 and older at the national and state-level using a randomly selected sample of approximately 70,000 people.

The period of observation for our analysis is 1998-2007. The NSDUH uses two measures of substance use, whether the respondent has used the substance within the past 30 days and the number of days on which the respondent has used it. For alcohol consumption, the specific question the survey asks is "Think specifically about the past 30 days, from [30 days before the interview date], up to and including today. During the past 30 days, on how many days did you drink one or more drinks of an alcoholic beverage?" For marijuana use, the question it asks is "Think specifically about the past 30 days, from [30 days before the interview date] up to and including today. What is your best estimate of the number of days you used marijuana or hashish during the past 30 days?".[1] Since respondents' precise age is not available in the NSDUH's public-use files, aggregated data by month of age on the average number of respondents that reported having consumed alcohol/marijuana during the past 30 days, as well as on the average number of days that they reported having consumed those substances were provided to us by SAMHSA. We also obtained measures of substance use by race (black and white) and gender (male and female). To maintain confidentiality of the data, SAMHSA could not provide us with the number of individuals by month of age. Given the survey questionnaire was re-designed in 2002, which may have affected the results of the later waves, we obtained the data separately for the periods 1998-2001 and 2002-2007. We observe only the average response of cohorts, each cohort is distinguished by their birth month. For individuals between the ages of 18 and 24, there are 71 possible birth months. Each of these cohorts is observed twice, once per time period, giving us a total of 142 observations. Following Carpenter and Dobkin (2009), we drop two observations, one per time period for individuals aged exactly 21 to avoid measuring the effect of the birthday celebration itself, leaving us with 140 observations. We weighted each observation by its standard error and used a triangular kernel to estimate local linear regressions on each side of age 21.[2]

Figure 3.1 displays average alcohol and marijuana use between the ages of 18 and 24. The individual observations are averages over four months; the fitted lines are estimated by linear regressions of substance use on age on both sides of age 21. The top panels show that

[1] Hashish is produced from the resin of the hemp plant and has higher levels of THC than marijuana. The effects it has on human health are similar to the ones produced by the consumption of marijuana.
[2] Ideally we should weight the averages by the number of individuals at each month of age. In the absence of the number of individuals, we used the standard errors as weights to account for different distributions of our variables of interest at each month of age. Results are also robust to the exclusion of weights and to the exclusion of the triangular kernel.

alcohol consumption increases drastically at age 21. The probability of having consumed alcohol in the last 30 days increases by about 10 percentage points from a baseline just under 60%. A similar result has previously been found by Carpenter and Dobkin and is consistent with the hypothesis that the cost of consuming alcohol decreases significantly at age 21. The frequency of alcohol consumption increases as well, from 4 to 5.5 days drinking out of the previous 30 days. For marijuana, the effect goes in the opposite direction, though its size is notably smaller. At age 21, the probability of marijuana use decreases abruptly by about 1.5 percentage points from a baseline of about 19%. The frequency of marijuana use decreases by about 0.2 days out of a 30 day period, from a baseline of about 2.6 days.

Table 3.1 presents quantitative estimates from the local linear regression approach described in the previous section. The results reported in the table are for local linear regressions with a triangular kernel and a bandwidth of 3 years. Each observation is the mean of alcohol/marijuana consumption in the month of age and is weighted by its standard error. Robustness tests for different bandwidths are reported in Section 5. The regression results reinforce the visual impression gained from the graphs. There is a strong increase in consumption of alcohol (both probability and frequency of use) at age 21, while consumption of marijuana decreases. The changes are statistically significant and their sizes are similar to the changes visible on the graphs.

The results indicate that alcohol and marijuana are substitutes, at both the extensive and the intensive margin. The decrease in the probability of marijuana use at age 21 suggests that some individuals who use marijuana before the age of 21 stop using it (or at least use it less regularly) once they turn 21 and are able to legally consume alcohol. In absolute terms, the substitution effect of the probability of substance use is not very large - a 9.6 percentage point increase in the probability of alcohol consumption leads to a 1.6 percentage point decrease in marijuana use. However, the 9.6 percentage point increase in alcohol consumption constitutes a 16% increase from the estimated baseline consumption just below age 21, and the 1.6 percentage point decrease in marijuana use constitutes an 8% decrease from baseline use, resulting in an estimated elasticity of substitution of 0.5.

The estimated decrease in the frequency of marijuana use is 0.19 days per month which constitutes a decline of 7% from the baseline at age 21. Although the decline in the frequency of marijuana use is smaller (in percentage terms) than the decline in the probability of use, it is unlikely that the estimated drop in the frequency of use is solely driven by the decline in people using it altogether. Given the low number of days having consumed marijuana at baseline (2.8 days), one more day of reported marijuana usage represents almost a 40% increase compared to baseline consumption just before the age of 21. If some individuals who use marijuana before the age of 21 increased the frequency of use after they turn 21, it is likely that our estimates would have been resulted insignificant or positive. Hence, our estimates suggest that some individuals who use marijuana before the age of 21 use it less often after they turn 21 and can legally purchase alcohol.

Previous research suggests that MLDA at age 21 induces higher consumption of alcohol and mortality rates on whites and males (Carpenter and Dobkin, 2009). In order to address the possibility of differentiated substitution effects across race and gender, we perform separate estimates of equation 1 for blacks, whites, men and women.

Analogous figures to Figure 3.1 were obtained for blacks and whites. Figures 3.2 and 3.3 show the impacts of the MLDA at age 21 on alcohol/marijuana consumption for blacks and whites, respectively. The figures tell a similar story as the one depicted by Figure 3.1. The estimates for blacks and whites are presented in Tables 3.2 and 3.3. The signs of the coefficient of interest corroborate the graphical analysis and show that for both groups, alcohol and marijuana are substitutes. Comparing the estimates of both tables we can observe that at baseline, whites are more likely to have consumed alcohol and more frequent consumers of alcohol than blacks. However, blacks are more frequent consumers of marijuana at baseline. The impacts of MLDA on alcohol and marijuana consumption also vary across race. For blacks, the MLDA at age 21 increases by 6% the probability of alcohol consumption and decreases by 2.2% the probability of marijuana consumption. Compared to baseline consumption just below 21, blacks over 21 have a probability of alcohol consumption of 12% points higher and a probability of marijuana consumption of 11% lower, which represents an elasticity of substitution of the probability of consuming alcohol and marijuana of 0.90. The estimated substitution effect of the probability of alcohol and marijuana consumption for whites is about half of the one estimated for blacks. For whites, the MLDA at age 21 increases the probability of alcohol consumption by 10% and decreases the probability of marijuana consumption by 1.4%. Compared to baseline, whites aged 21 or older are 15% more likely to consume alcohol and 7% less likely to consume marijuana. The implied elasticity of substitution of the probability of alcohol and marijuana consumption for whites is 0.46.

Regarding the frequency of substance use, the substitution effect between alcohol and marijuana consumption induced by the MLDA at age 21 is also stronger for blacks. The MLDA increases the number of days blacks consume alcohol by 0.7 days per month and decreases the number of days they consume marijuana by 0.62 days per month. Considering blacks consume alcohol 2.66 days per month and consume marijuana 3.1 days a month at baseline, the MLDA at age 21 induces an increase of 26% on the frequency of alcohol use and a decrease of 19% on the frequency of marijuana consumption. For whites the impact on the frequency of consumption is smaller: an increase by 20% on the frequency of alcohol consumption compared to baseline consumption induces a decrease of only 8% in the frequency of marijuana consumption.
These results suggest that while MLDA at age 21 is effective at reducing both the probability and frequency of alcohol consumption for both blacks and whites, it increases the probability and frequency of marijuana consumption, particularly among young blacks.

We also replicated the analysis for men and women. Figures 3.4 and 3.5 show the estimated impacts of MLDA at age 21 on alcohol/marijuana consumption among men and women, respectively. The figures show evidence that alcohol and marijuana are substitutes for both men and women. Tables 3.4 and 3.5 show the estimates of equation 1 on men and women, respectively. The results show that men are both more likely to consume alcohol and more frequent consumers of alcohol than women at baseline. The direction MLDA affects alcohol/marijuana consumption at the age of 21 confirms the findings depicted in figures 3.4 and 3.5, although the coefficient of the dummy that indicates age 21 or older on the frequency of marijuana consumption is not statistically significant for men. The corresponding substitution effects on the probability of substance use are similar across gender. For men aged 21 or older the probability of having consumed alcohol is 9.7% higher than younger men and the probability of having consumed marijuana is 1.7% percentage points lower. Compared to a baseline probability of consumption of 65%, an increase of 9.7% in the probability of consuming alcohol corresponds to an increase of 14% on the probability of consuming alcohol, whereas a decrease of 1.7% in the probability of consuming marijuana corresponds to a decrease of 7% on the probability of consuming marijuana. The estimated elasticity of substitution of the probability of consuming alcohol and marijuana is 0.5. For women, an increase of 18% in the probability of consuming alcohol compared to baseline consumption induces a decrease of 10% on the probability of consuming marijuana, which represents an elasticity of substitution of 0.55. The substitution effect on the frequency of substance use is stronger for women. Women aged 21 or older consume alcohol 1.1 more days a month than younger women and consume marijuana 0.281 days per month less than younger women. Compared to the number of days a month women consume alcohol and marijuana at baseline, those percentages represent an increase of 33% for alcohol consumption and a decrease of 14% in the frequency of marijuana use. The implied elasticity of substitution of the frequency of alcohol and marijuana use is 0.42. For men, the MLDA at 21 induces an increase of 1 day in the frequency of alcohol consumption per month and a decrease of 0.21 days in the frequency of marijuana consumption per month. Considering men at baseline consume alcohol 5.8 days a month and consume marijuana 3.8 days a month, the MLDA at age 21 induces an increase of 17% in the frequency of alcohol consumption and a decrease of 5.7% in the frequency of marijuana consumption. The elasticity of substitution of the frequency of alcohol and marijuana consumption is 0.33, or 21% less than the one estimated for women.

3.5 Robustness to the choice of bandwidth

A crucial parameter for local linear regressions like the ones reported in Tables 3.1-3.5 is the choice of bandwidth. By choosing a bandwidth that is too small we reduce the effective sample size and obtain estimates of low precision. By choosing a bandwidth that is too large we increase the risk of mis-specification if the relationship between age and substance use is non-linear. Though some authors have suggested rules-of thumb for bandwidth choice (e.g. Fan and Gijbels 1996), no rule-of-thumb guarantees an optimal choice of bandwidth. Imbens and Lemieux (2008) therefore suggest robustness tests for different choices of bandwidth. We performed the robustness tests on the estimates of the complete sample. The results of these tests are reported in Figure 3.6.

The tests were conducted as follows: For each substance we estimated 5 local linear regressions with bandwidths of 3 years, 2.5 years, 2 years, 1.5 years and 1 year. The same bandwidth was used on both sides of age 21. The point estimates and 95% confidence intervals are plotted on the vertical axes of the graphs, against the bandwidth on the horizontal axes. If the estimates based on larger bandwidths suffer from specification bias due to a non-linear relationship between age and substance use, we would expect the point estimates to suffer large changes as the bandwidth becomes smaller, since the linear functional form better approximates the true relationship over smaller intervals. If there is no specification bias, we would expect the point estimates to have small fluctuations and hence, be robust to the choice of bandwidth. Since the estimates for smaller bandwidths are based on smaller effective samples, we naturally expect the confidence intervals to increase as the bandwidth becomes smaller.

The results in Figure 3.6 show the estimates are robust to the choice of bandwidth. The point estimates differ very little for bandwidths between 1 and 3 years. As expected, the size of the confidence intervals increases for smaller bandwidths, since fewer observations are used for estimation. Nevertheless, the estimated effect of the MLDA on alcohol use (both probability and frequency) is statistically significant for all bandwidths between 1 and 3 years. The estimated effect on the probability of marijuana use is statistically significant for bandwidths larger than 1 year; the effect on the frequency of marijuana use is significant for bandwidths larger than 2 years.

3.6 Conclusions

By exploiting the sharp decrease in the cost of alcohol consumption induced by the Minimum Legal Drinking Age (MLDA) at age 21, this paper estimates the causal effect of legal access to alcohol on marijuana consumption. Our identifying assumption is that, apart from the ability to legally purchase alcohol, individuals just above and just below the age of 21 are similar in all characteristics that determine substance use. Compared to previous research (e.g. DiNardo and Lemieux 2001, Chaloupka and Laixuthai 1997, Pacula 1998, Williams et al.2004, Saffer and Chaloupka 1999, and Farrelly et al. 1999), this approach has the advantage of not having to rely on cross-sectional (often state-level) variation in alcohol and marijuana prices and related policies, which are likely to be correlated with unobserved characteristics of the population. This allows us to cleanly identify the causal effect of the MLDA in a way that is not afflicted by omitted variable bias.

Our results show that legal access to alcohol causes a significant decrease in marijuana use among young adults close to the age of 21. The point estimates suggest that marginally lowering the MLDA would decrease the probability of marijuana consumption in the affected age group by 8%. The substitution effect is larger for blacks, representing 11%. These results suggest that marijuana and alcohol are substitutes, so that a decrease in the "full" price of alcohol (including the cost of access) has a positive effect on marijuana use. The main implication of our study is that policies - such as the MLDA - that are aimed at restricting alcohol consumption among young adults may have unintended consequences, potentially causing an increase in the use of illegal drugs such as marijuana. When assessing the net benefits of alcohol-related policies we need to take into account these substitution effects and be aware of the trade-off between the positive health effects from reduced alcohol consumption and the negative effects of increased use of marijuana.

List of Tables

Table 3.1. Effect of the MLDA on Alcohol and Marijuana Use: Regression Discontinuity estimates.

	Used in last 30 days (%)		# of days used in last 30 days	
Variables	Alcohol	Marijuana	Alcohol	Marijuana
Over21	9.618***	-1.615***	1.057***	-0.198**
	[0.84]	[0.58]	[0.12]	[0.094]
Age	4.286***	0.122	0.574***	0.0455
	[0.48]	[0.33]	[0.071]	[0.053]
Age*Over21	-5.793***	-1.632***	-0.870***	-0.294***
	[0.68]	[0.48]	[0.098]	[0.078]
Constant	59.93***	19.94***	4.581***	2.830***
	[0.66]	[0.45]	[0.095]	[0.072]
Observations	140	140	140	140
R-squared	0.86	0.49	0.79	0.53

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All models include period fixed effects. All estimates are from local linear regressions using a triangular kernel with bandwidth of 3 years, centered at age 21. Means are weighted by the standard errors.

Table 3.2. Effect of the MLDA on Alcohol and Marijuana Use: Regression Discontinuity estimates. Only Blacks

	Used in last 30 days (%)		# of days used in last 30 days	
Variables	Alcohol	Marijuana	Alcohol	Marijuana
Over21	5.931***	-2.242*	0.704***	-0.625**
	[1.61]	[1.16]	[0.19]	[0.25]
Age	4.130***	0.271	0.263**	0.214
	[0.95]	[0.67]	[0.12]	[0.14]
Age*Over21	1.493	-0.868	0.124	0.029
	[0.9]	[0.68]	[0.1]	[0.14]
Constant	45.98***	19.30***	2.661***	3.192***
	[1.26]	[0.88]	[0.15]	[0.18]
Observations	140	140	139	138
R-squared	0.58	0.17	0.45	0.16

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All models include period fixed effects. All estimates are from local linear regressions using a triangular kernel with bandwidth of 3 years, centered at age 21. Means are weighted by the standard errors. The smaller number of observations is due to the fact that for some months of age standard errors were not reported due to low number of respondents.

Table 3.3. Effect of the MLDA on Alcohol and Marijuana Use: Regression Discontinuity estimates. Only Whites

	Used in last 30 days (%)		# of days used in last 30 days	
Variables	Alcohol	Marijuana	Alcohol	Marijuana
Over21	10.06***	-1.460**	1.061***	-0.244**
	[0.93]	[0.67]	[0.14]	[0.11]
Age	4.272***	-0.0382	0.671***	0.0403
	[0.52]	[0.38]	[0.083]	[0.061]
Age*Over21	-2.139***	-1.607***	-0.377***	-0.260***
	[0.54]	[0.4]	[0.079]	[0.066]
Constant	63.60***	20.60***	5.124***	2.954***
	[0.72]	[0.52]	[0.11]	[0.083]
Observations	140	140	140	140
R-squared	0.83	0.45	0.75	0.5

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All models include period fixed effects. All estimates are from local linear regressions using a triangular kernel with bandwidth of 3 years, centered at age 21. Means are weighted by the standard errors.

Table 3.4. Effect of the MLDA on Alcohol and Marijuana Use: Regression Discontinuity estimates. Only Men

	Used in last 30 days (%)		# of days used in last 30 days	
Variables	Alcohol	Marijuana	Alcohol	Marijuana
Over21	9.756***	-1.722**	1.016***	-0.218
	[1.15]	[0.78]	[0.18]	[0.14]
Age	5.983***	0.912**	0.978***	0.103
	[0.64]	[0.45]	[0.11]	[0.08]
Age*Over21	-7.167***	-2.519***	-1.254***	-0.375***
	[0.93]	[0.64]	[0.15]	[0.11]
Constant	65.03***	24.94***	5.894***	3.802***
	[0.89]	[0.61]	[0.14]	[0.11]
Observations	140	140	140	140
R-squared	0.81	0.34	0.72	0.45

Standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. All models include period fixed effects. All estimates are from local linear regressions using a triangular kernel with bandwidth of 3 years, centered at age 21. Means are weighted by the standard errors.

Table 3.5. Effect of the MLDA on Alcohol and Marijuana Use: Regression Discontinuity estimates. Only Women

	Used in last 30 days (%)		# of days used in last 30 days	
Variables	Alcohol	Marijuana	Alcohol	Marijuana
Over21	9.834***	-1.614**	1.112***	-0.281**
	[1.02]	[0.78]	[0.11]	[0.12]
Age	2.632***	-0.591	0.168**	0.0477
	[0.58]	[0.44]	[0.065]	[0.067]
Age*Over21	-4.692***	-0.68	-0.484***	-0.219**
	[0.83]	[0.65]	[0.089]	[0.1]
Constant	54.76***	14.98***	3.274***	1.906***
	[0.81]	[0.6]	[0.088]	[0.09]
Observations	140	140	140	140
R-squared	0.75	0.35	0.74	0.24

Standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. All models include period fixed effects. All estimates are from local linear regressions using a triangular kernel with bandwidth of 3 years, centered at age 21. Means are weighted by the standard errors.

List of Figures

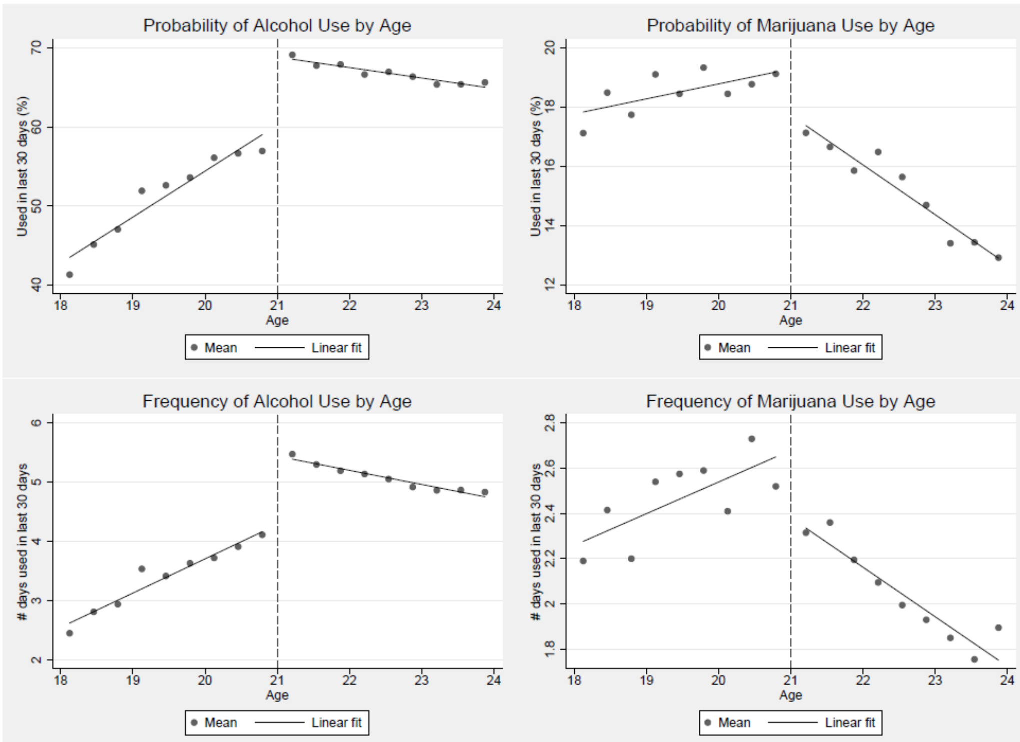

Figure 3.1 Drug use around 21

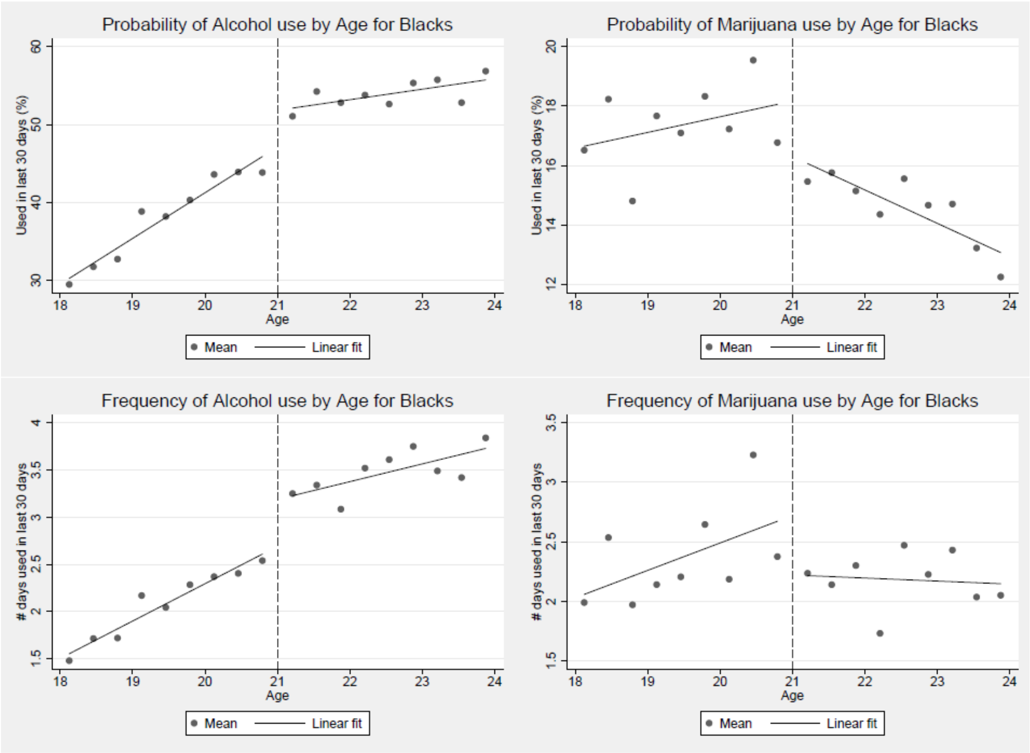

Figure 3.2 Drug use around 21. Only blacks

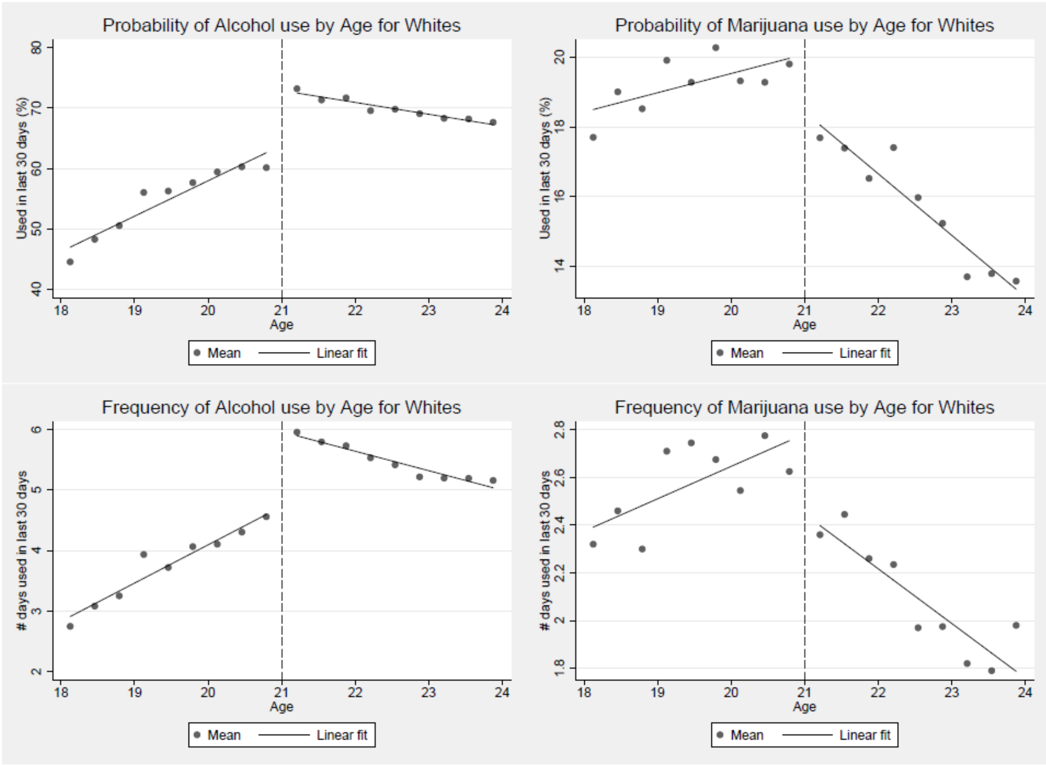

Figure 3.3 Drug use around 21. Only whites

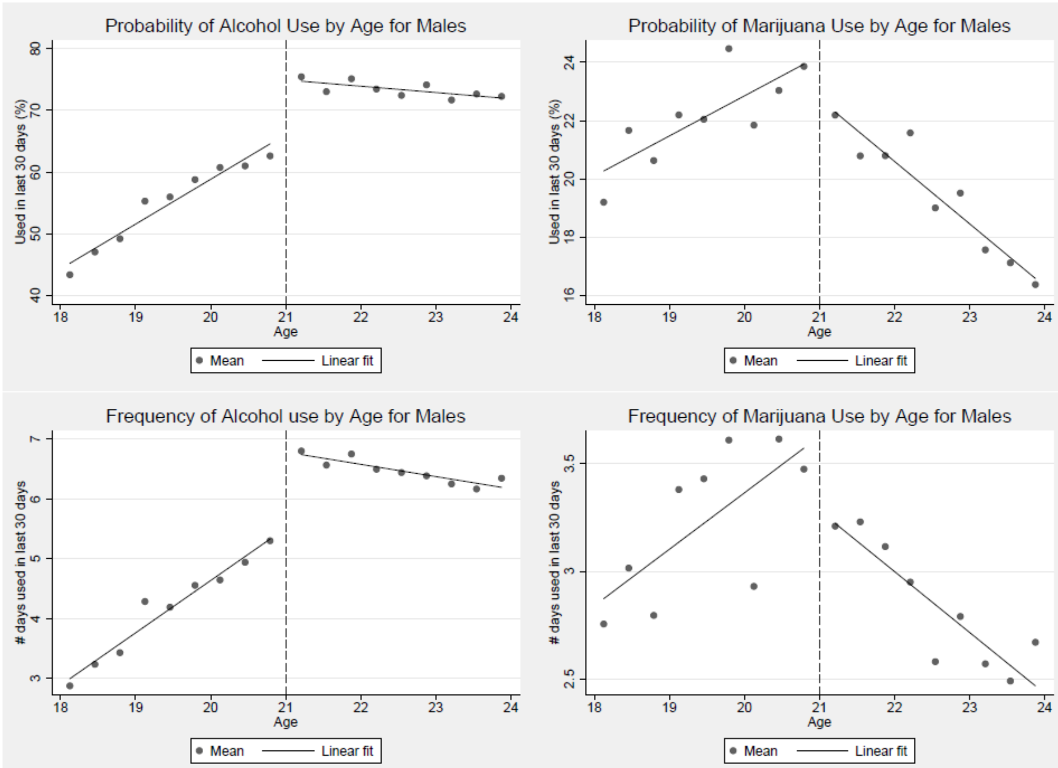

Figure 3.4 Drug use around age 21. Only men

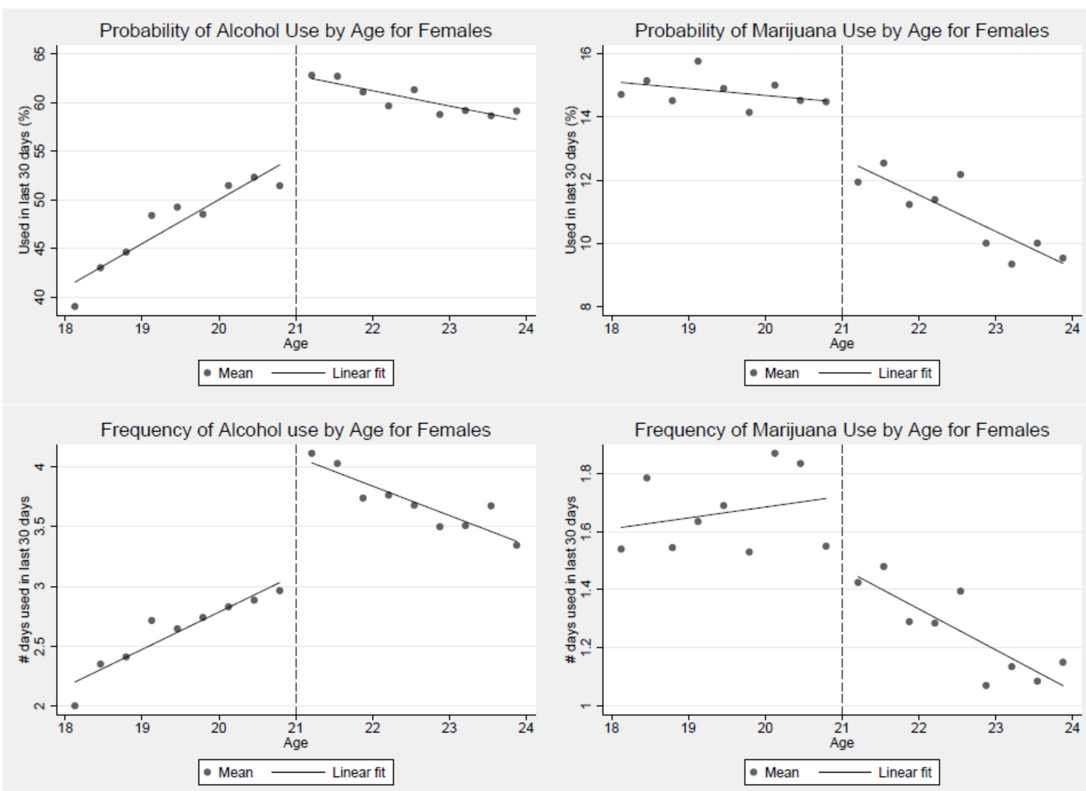

Figure 3.5 Drug use around age 21. Only women

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