ESSAYS IN APPLIED MICROECONOMICS

by

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A Dissertation Submitted to the Faculty of the

DEPARTMENT OF ECONOMICS

In Partial Fulfillment of the Requirements
For the Degree of

DOCTOR OF PHILOSOPHY

In the Graduate College

THE UNIVERSITY OF ARIZONA

2010

UMI Number: 3409087

THE UNIVERSITY OF ARIZONA
GRADUATE COLLEGE

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ACKNOWLEDGEMENTS

This dissertation would not have been possible without the guidance and insights of my advisor Robert Innes. His intellect, novelty of ideas and a broad view on the discipline of economics are of the rare nature and they elevate him to an esteemed league of true scholars. I am indebted to him for his continued passion in our work. I would also like thank my co-advisor Price Fishback. His cheerful promptness and enthusiasm for economics truly amazed and inspired me. I thank him for all his support.

I express my gratitude to Professor Gary Thompson. He always had the time to listen to my problems with an empathetic view and suggest solutions during my graduate days. I learnt a lot from his guidance on both undergraduate and graduate teaching.

Two of my friends, Subhasishda and Trevor, deserve a special mention. Subhasishda introduced me to the University of Arizona and supported me all along. I cannot thank Trevor enough for his unconditional support and calming presence. Dina and Januj shared the pain of the tormenting first year in the PhD program. I would like to say thank you to my other friends Anand, Arifbhai, Chander, Eylem, Hari, Kartik, Omjyoti, Pavan, Shourish, Sourav and Sukumar. My cousins Juin and Latu helped me come to terms in a new country.

My parents and my elder sister had their silent support from thousands of miles away and it helped me make it through the testing times. They have been my source of inspiration throughout this journey. And finally, I would like to thank my wife, Suchismita, for being with me from the days of painstaking data entry to the days of thesis-formatting.

DEDICATION

To my elder sister, Abanti.

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ABSTRACT

The first essay of this dissertation explores the role of congressional politics in environmental law enforcements in the United States. It examines if and to what extent the political affiliation of a representative politician matters for the enforcement of the Clean Air Act (CAA); in particular whether the affiliation of a representative politician to a particular party results in a higher/lower level of enforcement in his/her constituency. The period of 1989 to 2005 is considered. The analysis shows that political processes at the local, state and federal level did matter for facility level enforcements. By and large, the Republican politicians tended to reduce facility level inspections compared to their Democrat counterparts and the magnitude of such reduction marginally increased with the seniority of the Republican politicians----a finding that has important policy implications. As a result the political affiliation of a politician emerges as a key instrument for environmental enforcement in the emissions equation.

The second essay studies the potential issue of contagion in individual honesty (or, dishonesty). When an individual believes that peers are predominantly untruthful (or, truthful) in a given situation, is he/she more likely to be untruthful (or, truthful) in that situation in absence of monitoring, social sanction and reputation formation? The analysis employs an asymmetric information deception game patterned after Gneezy (2005) and reaches at the conclusion that individuals are heavily (partly) contagious when they believe that peers are predominantly dishonest (honest). The conclusion sheds some light on one of the many individual level root causes as to why the world is bipolar in the distribution of corruption (with most countries are either highly corrupt or highly honest).

The third essay discusses the complementarity that existed between the diffusion of motor vehicles usage and the construction of the network of roads in the United States during the first half of the twentieth century. With the expansion of roads, communication between two destinations became smoother, faster and more convenient and in turn attracted more and more people to use motor vehicles as a medium of communication. We empirically investigate how the expansion of the network of roads resulted in the diffusion of motor vehicles. We plan to empirically explore the impact of the diffusion of motor vehicles usage on the expansion of the road network in our future work. The complementarity that existed between the diffusion of motor vehicles and the expansion of roads in the United States in the first half of the twentieth century has important policy implications for today's developing countries that do not have a well constructed network of roads.

CHAPTER 1

INTRODUCTION

This dissertation is based on the use of micro-econometric and experimental techniques with a view to explore potential relationships in the domain of environmental economics and social preference. In particular, the first essay focuses on whether a politician's political affiliation has any bearing on the incidence of environmental enforcements in the area the politician represents. The first essay, thus, investigates the existence of potential corruption of the implicit nature. The second essay illustrates the impact of perceived norm on individual honesty and dishonesty in absence of monitoring, social sanction and reputation formation. The third essay examines the role of the expansion of road network in the diffusion of automobiles during the first half of the twentieth century in the United State.

The first essay studies the role of congressional politics in environmental law enforcements in the United States. We analyze whether there exists any implicit link between a politician's (congressman or senator) party affiliation and the number of environmental law enforcements in facilities that fall under the jurisdiction of the same politician. The presence of such implicit link between political affiliation and environmental law enforcements may have grave policy implications; it may lead to an uneven distribution of pollution resulting in public health related concerns and excessive lobbying by the interest groups. In the extant literature on environmental law enforcements, researchers have found evidence that enforcements are sensitive to the economic circumstances of the regulated firms and the surrounding areas (Deily & Gray, 1991). The literature on the common agency problems suggests, in addition, that the design and/or implementation of an environmental policy can be motivated by political considerations (Damania, Fredriksson & List, 2003, Fredriksson & Svensson, 2003). To our knowledge, there is a dearth of empirical work that bridges the gap between these two existing literatures. We try to bridge the two literatures by investigating if politicians from the two major political parties influenced the enforcements of Clean Air Act in different manners during the period of 1989-2005. Employing the techniques of panel data for count models, and using political, demographic, income data from various sources and mapping them with Environmental Protection Agency's facility level inspection data on ambient air quality, we find evidence that political affiliation at the local, state and federal level indeed mattered for environmental law enforcements. On an overall basis, the Republican politicians tended to reduce facility level inspections compared to their Democratic counterparts and the magnitude of such reduction marginally increased with the seniority of the Republican politicians. Thus, we are able to identify political affiliation as an instrument for environmental enforcements in the pollution equation—an identification strategy that can be used for future research.

The second essay investigates the role of perceived norms in individual decision making in absence of monitoring, social sanction and reputation formation. Two questions are of paramount importance in this regard: (i) Are individuals sensitive to perceived norms if no one issues explicit directives to them? If so, (ii) how can one alter such perceived norms in a manner that promotes honesty and enhances social welfare in an otherwise corrupt society? The second essay of this dissertation studies only the first question and leaves the second question as an agenda for future research. To address question (i), we follow an asymmetric information deception game patterned after Gneezy (2005) wherein we stimulate perceptions of honesty and dishonesty and study whether individuals are contagious to such perceptions. We find evidence that individuals are heavily (partly) contagious when they believe that peers are predominantly dishonest (honest) and we observe that the existing theories of social preferences (such as the guilt-aversion theory of Charness & Dufwenberg, 2006, the inequity aversion theory of Fehr & Schmidt, 1999) are unlikely to explain this finding. Therefore, the perceived norm of honesty/dishonesty turns out to be self reinforcing, which is something a social planner in a corrupt society may exploit to improve the level of individual honesty and trustworthiness. It is now important to explore alternative avenues that have the power to manipulate social norms.

The third essay talks about the role of the expansion of road network in the diffusion of automobiles during the first few decades of the twentieth century in the United States. For more than a century the United States is the world's most automobile dependent nation. Besides an independent American mindset that preferred an independent mode of communication, a set of economic factors was instrumental behind such a massive scale diffusion of automobile usage. One of such important factors was the development of road network. As early as the World War I the American engineers and policymakers understood the string of benefits that could potentially be generated by the construction and development of a widespread road network. The road network development activity got a huge boost with the emergence of the automobile as a life-changing general purpose technology. More roads encouraged individuals to buy more automobiles that generated a significant tax and registration revenues, which in a circular manner spurred further development of roads. We treat the development of roads as an endogenous factor and try to show the positive impact it had on the diffusion of automobiles in the United States. In our future work we shall trace out the role of the diffusion of automobiles on the expansion of road network for the same period.

The remainder of this dissertation is organized as follows. The next chapter discusses the first paper on congressional politics and environmental enforcements. Chapter three lays out the second paper on individuals' contagious preference. Chapter four discusses the connection between the diffusion of automobiles and the expansion of the network of roads during the first half of the twentieth century in the United States. A set of future research plans is briefly discussed as concluding remarks in the fifth chapter.

CHAPTER 2

ARE THE DEMOCRATS GREENER? A FACILITY LEVEL STUDY ON THE ENFORCEMENT OF CLEAN AIR LAWS

2.1 Introduction

The incidence of political pressure on regulatory decision making institutions has been widely discussed in the economic and political science literature (Stigler, 1971; Weingast and Moran, 1983; Scholz, 1991). Such political pressure may create biases, distort policies and result in a suboptimal outcome (Noll, 1989; Zeckhauser and Viscusi, 1990; Viscusi and Hamilton, 1999). Well known organizations like Transparency International and the PRS group (International Country Risk Guide) routinely document such pressure by their index of political corruption/risk and democratic accountability of institutions. The incidence of political pressure on autonomous institutions is more prevalent in developing countries;[1] however, developed countries like the United States are perhaps no exception.[2] In this chapter we explore the potential effects of such implicit political pressure on environmental law enforcement in the Unites States.

The issue of political affiliation and its connection to environment and health regulations in the United States pops up in television debates, newspaper reports, internet videos/blogs, town hall meetings, election campaigns and even during casual conversations among colleagues.

[1] Almost all publications of the Transparency International and the International Country Risk Guide (ICRG) document this kind of finding.
[2] For instance, for the period of January 1989 to November 2004, the United States obtains an average monthly political corruption score of 4.54 out of a (most honest) maximum score of 6. Usually, the Scandinavian countries obtain a monthly score of 6 on a regular basis. See ICRG data for details.

Figure 2.1 shows the United States Environmental Protection Agency's (henceforth, EPA) facility level yearly average inspection plus enforcement for ambient air quality for areas that were represented by either three Republican politicians (two Republican senators and a Republican congressman) or three Democrat politicians (two Democrat senators and a Democrat congressman). Without reading too much into these coarse numbers, we note a stylized fact. The average number of inspections per facility was higher in areas with full Democratic representation in every year except 2005.

A change in representation also leads to a change in enforcement and emissions. Table 2.1 presents the change in yearly average enforcements and emissions for areas that were represented by three Republican politicians in a given year and by two Republican and one Democrat politician in the next year (within the time frame of 1989-2005). After one year following a shift to include one Democratic representative of the area in Congress, the average annual number of inspections rose 2.5 percent and average emissions dropped 20 percent. Similar finding holds true if we compare the situation in any given year with the situation two years back (more on the data later). Figure 2.1 and Table 2.1 thus provide us with a rough guidance to the politics-environment nexus and also indicate that further exploration might have important policy implications.

Setting the political angle aside, let us now consider a purely environmental aspect. The EPA conducts inspections (and enforcements, if necessary) at facilities with the objective of reducing toxic emissions to a level permitted by the Clean Air Laws. Figure 2.2 shows the time-trend of facility level yearly average (self-reported) toxicity weighted release. From a level of little above 30,000 in the year 2000, the average release has almost doubled by the year 2005. This drastic increase in toxic release which may have alarming implications for public health makes the case for a study on the relation between inspection, emissions and politics more important than ever.

The remaining sections of this chapter are organized in the following manner. Section 2.2 discusses the literature and section 2.3 states the main idea and the hypotheses. Section 2.4 describes the data and the empirical model. Section 2.5 provides a discussion of the results and section 2.6 concludes the chapter.

2.2 Literature

Most work on environmental law enforcements concentrated on certain programs/policies implemented by the EPA and their impact on firm or industry specific attributes. In this chapter, we deviate from this tradition and scrutinize whether the sociopolitical conditions surrounding a facility will have any impact on environmental law enforcements at the facility level. We consider the EPA's facility level yearly inspection data as a measure of environmental law enforcement for ambient air quality, and try to investigate its relation with the political affiliations of the representatives where such facility is located.

Many researchers have contributed to the literature on environmental law enforcement and compliance. Deily and Gray (1991) documented the EPA's sensitivity to economic circumstances of the regulated firms. They found evidence that economic costs of environmental enforcement against troubled firms in high unemployment areas are high; therefore, the EPA reduces its inspection and enforcement activities. Another key work by Gray and Deily (1996) on steelmaking plants in US documented that compliance and enforcement affect each other. Higher compliance leads to less enforcement and more enforcement results in higher compliance. Helland (1998) extended the literature by showing that self reporting by a firm rises with the detection of a violation and it decreases if the cost of compliance is either too high or too low. Stafford (2002) documented that the steep increase in the penalties for violating the hazardous waste regulation introduced by EPA in 1991 have resulted in a decrease in violation and the likelihoods of such compliance and inspection are region specific.

In another paper, Stafford (2007) pointed out the strategic implications of audit policy disclosures by a facility. Such self-disclosure reduces the future probability of inspections and therefore facilities that are subjected to frequent inspection scrutiny are more likely to self-disclose to avoid future inspections. Friesen (2006) explained why self-disclosure can be suboptimal for the society when violations are small and the cost of self-disclosure is high. Khanna and Damon (1999) found that a firm's participation in the 33/50 program (a voluntary pollution reduction approach) reduces toxic releases. Innes and Sam (2008) identified manifold effects of the same program. Participation in 33/50 makes it less likely for a firm to be under scrutiny and this encourages firms to take part in the program thereby achieving its main goal.

Damania, Fredriksson & List (2003) and Fredriksson & Svensson (2003) demonstrated the impact of corruption on environmental policymaking in a common agency type setup. They considered environmental policy as a representative of the overall government policy and defined corruption as the rate at which the government trades off between bribes and public welfare maximizing policy. A straightforward prediction of their models is that pollution increases with corruption.

The above two strands of literature did not address the influence of political affiliation on environmental enforcement in the U.S. Political affiliation might be important for at least two reasons. First, a business or environmental lobby group might share proximity with a political party and such proximity may tacitly be incorporated in the political party's decision making process.[3] As a consequence, such politicians may work to influence the EPA's regulatory and research activities. It should however be noted that the exact channels of interference are difficult to trace out. Channels that are easily identified would likely attract corruption-investigations and lawsuits. A possible way one can hope to get a glimpse of such interference is by conducting anonymous surveys and interviews of the EPA personnel. A study (2008) conducted by the "Union of Concerned Scientist" found that an overwhelming 889 of nearly 1,600 EPA staff scientists reported political interference in their work in the last five years.[4]

Second, for example, knowing that the politicians in the appropriations committee in the U.S. Congress will determine its funding, the EPA might modify its actions depending upon the relative power of the political party on the committee (Furlong, 1998; Waterman, Rouse and Wright, 1998). These two reasons indicate that the political affiliation and the power (or, experience) of a politician may, supposedly, matter for environmental enforcement actions, something that the extant strands of literature have missed.

The situation, in which a politician may influence environmental policy raises some fundamental questions, even in the absence of bribery. This type of situation might be more relevant in developed countries like the U. S. where politicians typically do not accept bribe, but they may have priority interest group probably because of campaign contributions.

A third alternative is possible that has less taint for corruption. Because the power of specific members of Congress varies with respect to seniority, the party in majority, and committee assignments, a subset of the Congress could well determine the extent of enforcement nationwide and within specific districts.

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[3] Dixit et al. (1997) pointed out legislators' actions under pressure from contributors, voters, lobby groups etc.
[4] See http://www.ucsusa.org/news/press_release/hundreds-of-epa-scientists-0112.html. Also see "EPA Scientists Unhappy About Political Meddling", ScienceNOW, 23rd April, 2008.

If a plant's congressional representatives are particularly powerful, ceteris paribus, can the plant expect less enforcement scrutiny? Does the direction of any such effect depend upon the political persuasion of the congressional representatives? For example, if the representatives are Democrat in a district with a large "environmentalist" constituency, does political power translate into more enforcement, rather than less? Conversely, if the representatives are Republican, does influence translate into less enforcement? Does power in the Congressional hierarchy translate into effect on enforcement? Does commonality of party affiliation between the Congressional representatives and the Administration in the White House (both Democrat/Republican) translate into enforcement effects?

These questions are important if one wants to understand the nature and effects of environmental law enforcement in the U.S. If political influence is an important driver of enforcement activities, then models of environmental enforcement need to account for the political factors for its sheer environmental and economic impacts. The policymakers need to be informed about such impacts so that policies do not result in unevenly distributed outcomes. To our knowledge, neither extant empirical nor theoretical work on environmental law enforcement has studied the role of politics, although there is substantial theoretical work on the political economy of environmental regulation (setting of tax rates, for example).

2.3 Main Idea and the Hypotheses

Before we state the main hypotheses we want to test, a few comments are in order for their rationalization.

If political parties like the Democratic and the Republican Party differ (because of several contested reasons) in their approach towards environment, then such differentiated approach may get reflected in EPA's enforcement activities in regions where these respective parties are in power. Though none of these parties openly prefers environment over economy or economy over environment in situations where there exists a clear tradeoff between environment and economy, yet it is important to explore if the two parties' views towards this tradeoff differ to a significant extent.[5]

A facility can be subjected to environmental law enforcement for various reasons; air and water pollution are two of them. We examine if the EPA and other government agencies' enforcement activities at the facility level differ depending upon the political scenario in the surrounding area of the facility. For instance, based on figure 2.1, the EPA's monitoring activities might significantly differ between a traditionally Republican congressional district and a traditionally Democratic congressional district located in same or two different states. It is also important to explore how experience/seniority at the office translates into power; i.e., whether seniority makes a politician more influential in EPA's monitoring activities.

In addition to the political angle, it is also important to verify whether we can generalize the Deily & Gray's (1991finding of sensitivity of enforcement to local economic situations to the universe of facilities in the U.S. Finally, state level environmental awareness and strict liability statutes may also matter for environmental law enforcement. It may seem that EPA environmental enforcement would be a frequent phenomenon in states that are already conscious about environment. However, increased state awareness might allow the EPA and other government agencies to reduce enforcement activity in that state and shift resources to enforcement in other states with less awareness. Therefore, whether there exists a substitutability

[5] A potential question that arises at this juncture is the following. Since both the parties are large, is it justifiable to assume that all politicians within the same party have the same preference towards environment? We note that (i) it is difficult to model politicians' heterogeneous preferences within the same party because such preferences are somewhat subjective and not officially documented and (ii) if the politicians' preferences towards environment within the same party are truly heterogeneous, then we would be unable to make a clear-cut inference on an overall party basis. In other words, such heterogeneous preferences within the same party would bias against the results we are hoping for; that one political party has statistically significantly different impact on environmental enforcements than the other.

or complementarity between the level of enforcements and the level of existing environmental consciousness in a state is not obvious.

Our discussion so far leads to four formal hypotheses about political, economic, and environmental aspects of EPA's decision making.

Hypothesis I: *EPA's inspection activity at the facility level will differ significantly based on the political affiliation of the representatives at the local and the state level.*

Hypothesis II: *EPA's inspection activity at the facility level differ significantly in response to the political seniority of the congressional representative and the senators even after controlling for their party affiliation.*

Hypothesis III: *EPA's inspection activity at the facility level will be significantly different depending on (i) whether the state has a strict liability statute and (ii) environmental awareness (Sierra club membership) at the state level.*

Hypothesis IV: *EPA's inspection activity at the facility level will be significantly lower in areas where unemployment e is high.*

2.4 Data & the Empirical Model

To test the four hypotheses, we use an unbalanced panel dataset constructed from several sources. The air quality data come from the facility-level EPA dataset on the enforcement of the U.S. clean air laws for the years 1989-2005.[6] The time period includes the Republican administration of George H. W. Bush from 1989 through 1992, the Democratic administration of William J. Clinton from 1993 to -2000) and the Republican administration of George W. Bush from 2001 to the end of the sample. The study period, therefore, encompasses 17 years; out of which the Republican Party was in power (1989-1992 & 2001-2005) at the White House for nine years and the Democratic Party (1993-2000) was in power for eight years.

2.4.1 Data from EPA

[6] From EPA's data server, we initially extracted air quality data for the years 1987-2006. Then, based on a couple of criteria (explained shortly) on the first and last year of operation of a facility, we truncated the data from below and above and retained the remaining part for the years of 1989-2005.

EPA's air quality dataset at the facility level (called the AFS dataset) includes the yearly numbers of inspections and enforcement actions, as well as the facility's zip code, county, state, and primary SIC code for the industry for the years 1989 to 2005. The number of yearly inspections at the facility level serves as an indicator for environmental law enforcement and therefore it is the dependent variable in our model.[7]

Since the amount of the past years' toxic release by a facility can be an important factor determining whether the facility would be inspected this year, we use the EPA's TRI (Toxic Release Inventory) dataset to obtain toxic emissions (pounds/year) by each facility for the years 1989-2004.[8] The level of toxicity varies across the chemicals released by a facility, and therefore the EPA provides toxicity weights for each chemical in the TRI dataset.[9] It is also noteworthy that only a selective set of chemicals released by a facility are monitored under the provision of the Clean Air Act (CAA). Therefore, to remain consistent, we chose only those chemicals that are monitored under the provisions of the CAA. For each year and for each facility we considered the amount of CAA chemicals released and multiplied the amount of release of each chemical by its respective toxicity weight. The sum of the toxicity-weighted release for each chemical is defined as the toxicity-weighted total release. Division of the toxicity weighted total release by the sum of weights gives us the toxicity-weighted average release.[10]

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[7] We chose the number of inspections (and not the number of enforcements) as a measure for environmental quality because the incidence of enforcements are too few; in less than 6% of the times a facility gets reported for enforcement actions in the AFS dataset. We, however, control for lagged enforcements in the inspection equation.

[8] From EPA's data server, we initially extracted TRI data for the years 1987-2006. Then, based on a couple of criteria (explained shortly) on the first and last year of operation of a facility, we truncated the data from below and above and retained the remaining part for the years of 1989-2005.

[9] These weights can be found at www.epa.gov/oppt/rsei/pubs/toxwght97.pdf.

[10] Consider a facility for a given year during which, the facility has released 'n' number of toxic chemicals; the amount of release for the i^{th} chemical is given by x_i. Assume, without loss of generality, that out of these 'n' chemicals the first 'm' ($m \leq n$) chemicals are monitored under the CAA and the toxicity of the i^{th} chemical is given by w_i. The toxicity weighted average release by the facility is then given by $R_i = \sum_{i=1}^{m} w_i x_i \Big/ \sum_{i=1}^{m} w_i$

The facilities recorded in the AFS dataset do not fully correspond to the facilities recorded in the TRI dataset. In other words, there are many facilities in the AFS dataset (TRI dataset) that are not mentioned in the TRI dataset (AFS dataset). Therefore, in our basic model (more on the model shortly), we restrict our attention to only those facilities that are common to both AFS and TRI datasets. We also restrict our attention to facilities that are located in one of the 50 states of the U.S. A few important issues regarding the AFS and TRI datasets deserve attention.

In the AFS dataset, a facility is mentioned only when it is inspected or some sort of enforcement action has been taken against it. For example, if a facility is inspected in the years 1990 & 1992 but not in 1991, then the facility gets recorded for the years 1990 & 1992 and nothing is recorded about the facility for the year 1991.

Conceivably, a facility may not be inspected in a year when it is operating. The number of inspections for the facility for that year should therefore be zero.

It is imperative to know whether the facility is open or closed in a given year. However, the AFS dataset does not have historic operational status of a facility. This creates a problem. For instance, if a facility is recorded in the AFS dataset for the first time in the year 1995, it is not known whether the facility was actually operating and was never inspected between the years 1989-1994, or the facility did not exist prior to 1995. Similarly, if a facility is recorded in the AFS dataset for the last time in the year 2002, it is not clear whether the facility remained operational and was never inspected between the years 2003-2005, or it did not exist 2003 onwards. Thus, for any given facility, figuring out the start (and end) year of operation (within our study period) was not possible from the AFS dataset unless the facility is reported for inspection/enforcement action for each of the years between 1989 and 2005. We used the TRI dataset to solve this problem.

The TRI dataset has facility level information on yearly toxic releases and operational status. Like in the AFS dataset, the TRI dataset also has missing years for facilities. We merged the AFS and the TRI dataset using a unique facility identifier and year. Noting that the closure of a facility does not happen overnight, we define its first year of operation as the earlier of the first year of appearance in the AFS dataset due to inspection/enforcement reason or, the first year it is recorded as "operational" in the TRI dataset. Since our study period starts from 1989, the first year of operation was truncated from below at 1989.

Similarly, the last year of operation is the later of the last year of appearance in the AFS dataset due to inspection/enforcement reason or, the last year recorded as "operational" in the TRI dataset. The last year of operation was truncated from above at 2005.

In the AFS dataset, if nothing is recorded about a facility for *any* year between its first and last year of operation, the facility is recorded as having zero number of inspection in that year. In the merged (AFS-TRI) dataset, we have data on 17,635 facilities with an average number of year-observations per facility of 8.6. The total number of facilities in the AFS dataset is 84,101 and the average number of year-observations per facility is 7.3. For the period 1989-2005, the yearly average number of inspections per facility is 1.02 for the (AFS-TRI) dataset and 0.99 for the AFS dataset. Table 2.2 shows the average inspections per facility for each year. It indicates that the average number of inspection and enforcement activities have gone up over the years and that the facilities common to the AFS and TRI dataset are on an average subjected to higher inspection and enforcement activity compared to facilities that are part of only the AFS dataset.

The latter fact likely arises because the EPA directs its attention more towards the facilities with toxic release.

Since toxic release in the last year is an important determinant of inspection or enforcement activity by EPA in the current year, the facility-year observations for which no data on toxic release was available in the TRI dataset will ultimately get dropped during the regression analysis.

2.4.2 Data from Other Sources

From the Bureau of Labor Statistics, we have annual county-level per capita income, unemployment rates, and population density for the period 1989 to 2005. State-level demographic data on Sierra Club membership is obtained from the Sierra Club. Zip codes are used to tie facilities to the U.S. Congressional districts and to counties using information from the Missouri Census Data Center.[11] Using the U.S. Congressional Biography and the website www.wikipedia.org, we collect political data on the party affiliation and seniority of the U.S. Senators and Representatives for each congressional district and state from the 101^{st} US congress (1989-90) to the 109^{th} US congress (2005-06).

We consider a variety of different indicators of political influence. First we construct party specific dummy variable for House Representatives and the two Senators from the state. We therefore have three dummy variables for the Congressional political scenario (one for the congressman, one for the junior senator and other for the senior senator). Let us indicate these three dummy variables by 'local political dummies'. Similarly, we construct party specific dummy variables to control for the majority parties in the U.S. Senate and House of

[11] There are some zip codes that fall under multiple congressional districts. Therefore, the facilities located within the periphery of such a zip code fall under the jurisdiction of multiple congressmen and therefore may be subjected to influence from each of (each representing the concerned zip code). Note that these congressmen may or may not belong to the same party. Therefore, to avoid any overlap of political interest, we eliminate all the facilities that fall under more than one congressional district in our dataset.

Representatives and the party in power at the White House. Let us denote these two dummies respectively as the 'congress dummies' and 'administration dummy'. To test for the impact of common affiliation between the local politicians and the party that enjoys majority in the U.S. congress, we interact the 'local political dummies' with the 'congress dummies'. Similarly, to test for the impact of common party affiliation between the local politicians and the party that enjoys power at the White House, we interact the 'local political dummies' with the 'administration dummy'.

Two party-specific Congressional seniority measures are constructed. For seniority, we use the number of years a Representative (or a Senator) has served in the House (or Senate) continuously prior to a given year. In addition, we construct a measure of maximum Republican seniority for the Republican senators and a measure of maximum Democrat seniority for the Democrat senators. For instance, if a state has two Republican senators serving in tandem in a given year, then, the maximum Republican seniority for that year is equal to the seniority of the senator who has more experience (in terms of numbers of years continuously spent at the office). In contrast, if a state is served by a Democrat senator and a Republican senator in a given year, then maximum Republican seniority for that year is equal to the seniority of the Republican senator. In the third situation where a state is served by two Democrat senators in tandem in a given year, then maximum Republican seniority for that year is equal to zero.

In accordance with parts of the existing literature, the *basic inspection equation* contains the following right-hand variables. The Demographic, Environmental and Economic Variables include the toxicity weighted average release in the previous year, county level unemployment rate, county level population density, county level per-capita income, state level Sierra Club membership, state level Strict Liability Statute. The Political Variables include the local political dummies. The congressional majority and Presidential control variables are captured by year fixed effects. In addition, we include state and Standard Industrial Code (2-digit level SIC) dummies.[12]

A complete description of the variables used for the inspection equation is provided in Table 2.3.

2.4.3 Estimation Strategy for the Inspection Equation at the Facility Level

Due to the "count" nature of the dependent variable, we initially consider standard Poisson and Negative Binomial regression models for panel data (random effects).[13] The standard Poisson model for count data can be represented in terms of the following equations.

$$Y_{it} \sim \text{Poisson}[\mu_{it} = \alpha_i \lambda_{it}]$$
$$\lambda_{it} = \exp(X'_{it}\beta),\ i = 1, ..., \text{n},\ t = 1, ..., \text{T}$$

Y_{it} stands for the number of inspections at facility "i" during year "t". α_i stands for the multiplicative facility specific term that are iid random variables. X and β respectively stand for the exogenous variables discussed above and their respective coefficient. The mean of Y_{it} is given by $E[Y_{it} \mid X_{it}, \alpha_i] = \mu_{it}$

The estimation of β adopts the maximum likelihood approach.[14]

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[12] Usually a firm is comprised of several facility units; these facilities may be located in one state or across several states. The existing literature, therefore, considers the role of firm level variables (such as employment, net asset etc.) for facility level inspection and compliance equation and this requires tying a facility to its parent firm. Since the firm level financial data usually comes from a dataset known as "Compustat Data" and the facility level inspection data comes from EPA, tying a facility to its parent firm requires rigorous effort and it involves 'string matching' or 'character matching'. Since almost all the papers that dealt with an inspection equation have concentrated on only a few specific industries (such as chemical or steel) or a few set of firms (such as the ones that participate in 33/50 program), they could literally tie the facilities to its parent firm by some rigorous procedures. However, in the present study, we are not focusing on specific industries and therefore the number of facilities in our dataset is huge (17,635 in the combined AFS-TRI dataset and 84,101 in the AFS dataset) and as a result it is practically impossible for us to tie each facility to its parent firm by using 'character matching'. As such, the inspection equation in the present study does not involve any firm specific variable.

[13] Fixed effects model for Poisson distribution imposes the restriction that mean equals variance, however, random effects model allows for overdispersion in the dependent variable. For the Poisson model, our data rejects the restriction that mean equals variance. Therefore, we adopt random effects estimation technique. In addition to this, fixed effects is often known to be non-converging and therefore unworkable (Innes & Sam, 2008).

[14] A potential criticism that one may raise at this juncture is that certain individuals in certain districts may base their voting strategy on the number of environmental enforcements in the district. In other words, the voting outcome may be endogenous. We performed the standard Regression-Discontinuity analysis using congressional voting data for districts where the margins of difference between the elected and defeated representative were low. We find no evidence of the supposed endogeneity of the voting outcome. However, to maintain brevity, we do not produce the

Tables 2.3 and 2.4 define the variables and the summary statistics (for the AFS-TRI dataset) respectively. To summarize, the dependent variable of our model is $Inspection_{it}$ and it stands for the number of inspections at facility i in year t. All the independent variables also have a facility and year suffix. In order to test the hypotheses stated in section 2.3, we try various specifications of the independent variables in the right hand side of the equations.

2.5 Results

2.5.1 Basic Inspection Equations

Table 2.5 summarizes the two basic inspection models. The basic models differ in only one aspect. In basic model 2, we use a separate (Republican) dummy variable for each senator, junior and senior. In basic model 1, we combine the potential effects of both the senators in one dummy variable (ALORSD). Depending upon the party affiliation of the senators, there can be four different scenarios: (i) both senators Democrat, (ii) both senators Republican, (iii) junior senator Republican, senior senator Democrat and (iv) junior senator Democrat, senior senator Republican.[15] Comparison of these four scenarios might be confusing and therefore we introduce the condensed variable "ALORSD". This variable enables us to compare the base scenario (when both senators are Democrat) with the three other scenarios (when at least one of the senators is Republican).

The results from the random effects Poisson regression and random effects Negative Binomial regression are produced in Table 2.5. By and large, the set of marginal effects are fairly close to each other across basic models 1 and 2 with the same sign. The marginal effect of Lagged Release is positive and statistically significant under both the models. However, the

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Regression-Discontinuity results in the current version. The results will be available to interested readers upon request.

[15] The state of Vermont has two senators and only one congressman. The state has a long history of electing "Independent" representatives, both at the senate and the congress. Most of them Caucus with Democrats and therefore we consider these Independent politicians under the Democrat umbrella.

economic magnitude of the effect is tiny. The elasticity of inspections with respect to lagged release is 0.000534 and the effect of a one-standard-deviation (OSD) increase in lagged releases leads to an increase of only 0.0016 standard deviations in the number of annual inspections. An increase in toxic release by a facility this year increases the number of inspections next year, which has obvious justification under the Clean Air Act. The marginal effect of Lagged Enforcement Dummy is positive and statistically significant under both models, but again relatively small. When the EPA applied enforcement the previous year the number of inspections in the following year goes up by 0.08 or 0.129.

The marginal effects of Sierra and Strict Liability are statistically significant and negative under both models indicating that there exist strong substitution effects for EPA's monitoring activities, which lends support to hypothesis III.[16] Since Sierra is very small in magnitude (with mean being 0.0018), the marginal effect of Sierra is very high. The presence of a strict liability law reduces inspections by about 17 percent relative to the mean of one per year. Thus there exists a substitution between the level of environmental awareness and the number environmental law enforcements in a state. The impact of higher unemployment rates is negative, but the marginal effect is not statistically significant. Thus, the findings are inconsistent with Hypothesis IV and Deily and Gray's earlier findings that enforcement was laxer in high unemployment areas.

The marginal effect of population density is not statistically significant under the Poisson model, however, under the Negative Binomial model, the marginal effect is statistically significant and positive. Thus, it lends partial support to the claim that inspections happen more in densely populated areas because of high health-cost. The marginal effect of per capita income is negative and statistically significant under both basic models. Some of the other authors in the

[16] Stafford (2002) documented similar substitution effects.

literature have also found a negative sign for the coefficient of per capita income in similar circumstances; however no proper explanation for such a sign has been provided. Higher income areas usually have a higher demand for environmental quality and therefore this might act as a substitution for inspection.

The marginal effect of the Republican Congressman Dummy is negative and statistically significant under both basic models, which is consistent with Hypothesis I that Republican representatives are associated with fewer inspections. Compared to a Democratic congressman in the House, the presence of a Republican congressman reduces the number of inspections by roughly above 4%. The negative effects of the Republican senators are stronger, leading to a reduction in annual inspections of 28 or 32 percent in the basic model 1. When we separate the impact of Junior and Senior Republic senators in basic model 2, the marginal effects of Junior Republican Senator Dummy and Senior Republican Senator Dummy are each negative and statistically significant. The presence of the junior Republican senator is associated with 16 percent fewer inspections, while the senior Republican is associated with 20 percent fewer. Similarly, the marginal effect of the condensed dummy variable (ALORSD) under basic model 1 implies that the presence of at least one Republican senator reduces the number of inspections by approximately 32% (28%) under the Poisson model (Negative Binomial model) compared to the situation when both senators are Democratic.

One reason as to why the senators have stronger impact than the congressmen could be that each senator is one percent of the votes in a Senate of only 100 members while each representative in the House represents only 1/435 or 0.23 percent of the votes in the House. Thus, the senator’s share of votes in the Senate is 4.348 times the representative’s share of votes in the house. Notice that the senior senator's impact on inspections is 4.47 times as large (-0.206/-.046) as the impact on inspections of the Republican representative

Results from Table 2.5 indicate that the Republican and the Democrat politicians may approach environmental issues differently. The results possibly indicate that the Republican politicians may favor "pro business" environment, perhaps due to campaign contributions. A straightforward implication of these results would be that environmental law enforcements are, to some extent, unevenly distributed. As a result, the facilities located in areas dominated by the Republican politicians may enjoy the privilege of reduced scrutiny by EPA and other government agencies. One can therefore conjecture that the possibility of such reduced scrutiny may potentially lead these facilities to spend more on political donations and less on research and development that reduces toxic emissions. Though we are not dealing with the aspect of political donations in this chapter, it would be interesting to present an estimate of toxicity weighted average release in politically polar areas in support of the conjecture. For the entire study period of 1989-2005, the facility level toxicity weighted average release in areas represented by three Republican politicians was 33,343 and the same for the areas represented by three Democrat politicians was 22,681; approximately 33% less. Note that the difference in toxicity of releases, however, will have only a very small effect on the number of inspections. According to the elasticities estimated above, this 33% percent difference in toxic releases in the previous year would have raised the number of inspections at the facility by only 2.7 percent. The direct effect of the presence of Republican politicians is much smaller.

All subsequent inspection models that we report in this chapter are a *slight* variation of basic model 1 described above; the variation arises because in the subsequent models we either drop a few explanatory variables from basic model 1 or add a few explanatory variables to it.

When we are adding exogenous variables to basic model 1, we are mainly interested in the marginal effect and significance of the added exogenous variables.

2.5.2 Robustness of the Political Effects

To check the robustness of the political effects that we just discussed, we run a set of four separate regression models. Table 2.6(A) reports the first two results. The set of explanatory variables in the first model (Robustness Check I) does not include the variables Lagged Release and Lagged Enforcement Dummy. Recall that Table 2.5 shows regression results on those facilities that are common to both AFS dataset and TRI dataset. Now, if we do not include Lagged Release (which comes from the TRI dataset) in our regression, then we are dealing with only the AFS dataset. The non-inclusion of Lagged Release increases the number of observations and facilities to respectively 617,521 and 84,101 and this potentially includes the universe of facilities in the U.S. mainland.

Also, the variable Lagged Enforcement Dummy (LED) takes a value of unity in approximately 4.4 percent of the time and zero 95.6 percent of the time in the AFS-TRI dataset. This can lead to a suspicion that the small number of observations for which LED takes a value of 1 can have a magnified impact on the regression estimates in the basic models. Therefore to eliminate such suspicion, we conduct robustness checks I after dropping the lagged enforcement dummy from the set of covariates used in basic model I.

In the larger sample without lagged enforcement and emissions, the marginal effect of the Republican Congressman Dummy remains statistically significant and negative under both Poisson and Negative binomial models. Compared to a Democrat congressman, the presence of a Republican congressman reduces the number inspections by 3.4 percent under the Poisson model and 2.8 percent under the negative binomial model. In addition, compared to the situation when both senators from a state are Democrat, the presence of at least one Republican senator reduce(s) the number of inspections by 18.2 and 16.2 percent, respectively. These marginal effects are not much smaller than in the models in Table 2.5.

In addition to the set of explanatory variables used in basic model 1, we control for committee membership under Robustness Check II.[17] The coefficient of the Republican Congressman Dummy and ALORSD again are statistically significant and negative, although the marginal effects are slightly smaller, after controlling for committee memberships.

Table 2.6(B) reports the last two regression results for the robustness of the political effects. The set of covariates used for these two regressions is identical to the set of covariates used in basic model 1. In this case we rerun the model; however, we *could* adjust for the standard errors of the marginal effects allowing for the relatedness of the observations within a congressional district.[18] Note that the marginal effect of the covariates under Robustness Checks

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[17] Members of committees and subcommittees that are relevant for EPA are particularly powerful individuals, and without doubt they may potentially influence EPA's research and enforcement activities. Such relevant committees (subcommittees) may include the committee of Appropriations (with subcommittee: (i) Interior, Environment and Related Agencies), Energy and Commerce (with subcommittees: (i) Health, (ii) Oversight and Investigations, (iii) Environment and Hazardous materials) for US House and Appropriations (with subcommittee: (i) Interior and Related Agencies), Environment and Public Works (with subcommittees: (i) Clean Air, Wetlands and Climate Change, (ii) Superfund and Waste Management) for US Senate. We control for (a) committee chairman, (b) committee ranking member, (c) subcommittee chairman, (d) subcommittee ranking member and (e) subcommittee member for both US House and Senate. There can be three situations with respect to committee/subcommittee membership; (i) a politician (senator or congressman) may not be a member of any relevant committee, (ii) he/she may be a member and a Republican and (iii) he/she may be a member and a Democrat. Therefore, for each of the level of memberships (a)-(e) above, we introduce two dummy variables: whether a politician is 1) a member and a Republican and 2) a member and a Democrat; and the base case being non-member. Thus, in all we introduce 10 dummy variables for US House and 12 more dummy variables for US Senate. The coefficients of these dummy variables do not clearly indicate whether the effects of Republican committee members on inspections are any different from the same for the Democrat committee members. To maintain brevity, we do not display the coefficients of these 22 dummy variables under Robustness Check II. This regression result, in its entirety, is available upon request. A straightforward interpretation of this result would possibly be that at the highest level of power, the Democrats and the Republicans are indistinguishable with respect to their influence on environmental law enforcements.

[18] A potentially unavoidable issue with our dataset is that the observations within a congressional district may be related. In such case, one should try to correct for the standard errors of the marginal effects. Such correction leaves the magnitude and direction of the marginal effects unchanged but typically raises the respective standard errors. Econometric theory suggests two possible alternatives for this: (A) for nonlinear estimation, correct the standard errors by using the methods of "block bootstrapping", where each congressional district constitutes a block, (B) for linear estimation, correct the standard errors by "clustering" them at the congressional district level. For our

III & IV in Table 2.6(B) remain the same; the standard error differs for the two because they are clustered at the congressional level in Robustness Check IV. By and large, the direction of the marginal effects across Table 2.6(A) and Table 2.6(B) remain the same (with the exception of unemployment and population density), however, their magnitude differs. The difference in magnitudes arises from the difference in functional form. Under Table 2.6(A), the functional form is nonlinear and under Table 2.6(B), the functional form is linear. The marginal effect of the Republican Congressman Dummy and ALORSD still remain negative implying that the Republican politicians prompt fewer inspections even under the linear model. We next consider the issue of a politician's seniority and its impact on environmental law enforcements.

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purposes, method (A) is not applicable owing to problems with unbalanced panels and changes in the boundaries of Congressional districts. (B) is not applicable due to the changes in Congressional districts. The standard method for block-bootstrapping requires observation matrices to be conformable for multiplication and inversion purposes. Since ours is an unbalanced panel dataset, the dimensions are not identical across the blocks (congressional districts), and this makes matrices non-conformable. The number of congressional districts in the US is constant (435); however, the number of congressional districts within a state may change due to "redistricting" after a census. If a state undergoes redistricting, then the political boundaries within that state get redefined. During our study period (1989-2005), redistricting took place twice; at the start of the 103rd congress (1993) and at the start of the 108th congress (2003). For instance, the state of California had 45, 52 and 53 congressional districts for the period of 1989-1992, 1993-2002 and 2003-2005 respectively. The redistricting may make a facility (which is a "panel" in our AFS-TRI dataset) in California fall under different congressional districts (i.e. different clusters) over the study period. Therefore, the panels are not uniquely nested within a cluster if we consider each facility as one panel. The method of clustering thus fails if we want to exploit the panel structure of the dataset.

To overcome these issues we run a couple of cross-section regression models (Robustness Check III & IV) with robust and clustered-robust standard errors (at the congressional district level). Since cross-sectional regression treats each observation as a separate unit, problem (ii) above is solved. To define the clusters, we assign a separate congressional district number (before and after) to all the congressional districts within a state that underwent redistricting. For instance, if California's 52nd congressional district gets divided into two separate districts (at the start of 2003) forming the new 52nd and 53rd congressional districts, then from 2003 onwards we assign a new congressional district number to *all 53* districts in California and not just the 52nd and 53rd district. Thus we followed the policy of creating the maximum number of clusters which would raise the standard errors of the marginal effects to the maximum level thereby providing a sufficient robustness check for the marginal effects.

A potential caveat of running a cross-sectional model is that we are treating the number of inspections (which is a "count" variable) as a continuous variable. To remedy this, we also run a Probit regression by converting number of inspections into a binary variable (1; if the number of inspections in a year is greater than zero and 0; otherwise). The results from the Probit regression are in conformity with the results from the cross-sectional regressions.

For all subsequent inspection models in this chapter, we present results from the (panel data) Poisson model and OLS model with clustered robust standard error (at the assigned congressional district number). The equivalent Negative Binomial and Probit regression models with clustered robust standard errors yield similar results (with the marginal effect of the political variables still significant) to the ones we obtain from the Poisson and OLS regression models. However, to maintain brevity, we present results from only the (panel data) Poisson model and OLS model with clustered robust standard error at the assigned congressional district number. The equivalent Negative Binomial and Probit regression results are available upon request.

2.5.3 Effect of Seniority of the Politicians

A congressman or a senator keeps getting better informed about the political system as he/she spends more time at the office. Better information may translate into an increase in bargaining power with non-political agencies.

Table 2.7 summarizes the results from regressions that correspond to hypothesis II (about seniority of politicians). In order to analyze the impact of seniority on the number of inspections, we introduce four additional interaction variables in basic model 1. Two of these additional interaction variables RCDCS (dummy for Republican congressman multiplied by congressman seniority) and DCDCS (dummy for Democrat congressman multiplied by congressman seniority) capture any effect that a congressman's seniority may have on inspection. The other two additional interaction variables ALORSDMRS (dummy for at least one Republican senator multiplied by maximum Republican seniority) and ALODSDMDS (dummy for at least one Democrat senator multiplied by maximum Democrat seniority) serve the same purpose for a senator's seniority. Note that the marginal effects of the independent variables in Table 2.7, by and large, have the same sign as in basic model 1. A Republican Congressman with no seniority would be associated with a reduction of 0.022 inspections or 2.2 percent. For each year of seniority for the congressman, the number of inspections would fall another 0.002, or 0.2 percent. Thus, a Congressman with the average seniority of about 10 years would reduce inspections by about 0.044, or 4.4 percent, which is about the same size of the effect that we measured in Table 2.5.

The results for Republican senators in the Poisson regression in Table VII show that a Republican senior senator with no years in the Senate would be associated with -0.297 fewer inspections. For each additional year of seniority that he has, the number of inspections is 0.005 lower. Thus, a senior Republican Senator with the average experience of roughly 15 years would be associated with -0.372 inspections. The marginal effect of DCDCS is insignificant under both the regression models indicating that seniority of a Democrat congressman is statistically not significant for inspection activities. Therefore a more experienced Democrat congressman is statistically similar in his/her influence on environmental law enforcements to less experienced Democrat congressman.

Finally, the marginal effect of ALODSDMDS is statistically significant and positive with a value of 0.003 in the Poisson regression. Thus, increased seniority for a Democratic senator is associated with more inspections. In situations where there is one Democratic and one Republican Senator, an extra year of seniority reduces inspections more than an added year of seniority for the Democrat raises inspections. In sum, years of seniority can have sizeable effects on inspection activity.

2.5.4 Interactive Effects of Federal and State Politics

Table 2.8 shows the results from a model that tries to capture the interactive effects of federal and state politics on inspection. This model has three new exogenous variables compared to the basic model; Republican Administration Dummy, RadminRepcong (Republican Administration Dummy multiplied by Republican Congressman Dummy) and RadminALORSD (Republican Administration dummy multiplied by dummy for at least one Republican senator). RadminRepcong and RadminALORSD capture the commonality of party affiliation (between federal and local administration). These two variables can explain whether the influence of a Republican congressman and/or a senator on inspections is larger with a Republican president in office or a Republican majority in congress. In this case, we have dropped the year fixed effects.

The marginal effect of Republican Administration Dummy is statistically significant and negative under both regression models. Under a Republican president in an area where there is no Republican Congressman, the coefficient in the Poisson model in Table 2.8 implies that there are approximately 8 percent fewer inspections. When there is no Republican president, but the district is represented by a Republican congressman, the coefficient of the Republican Congressman Dummy implies that the number of inspections is 7.8 percent lower. When there is both a Republican president and a Republican congressman, the sum of the coefficients implies that the number of inspection will be -0.061 (= -0.083-0.078+0.099). Thus, there are fewer inspections when a Republican congressman is matched with a Republican president than when he is matched with a Democratic president. .

The coefficients of ALORSD in Table 2.8 show that in the absence of a Republican president, a Republican senator is associated with 0.284 fewer inspections per year. Add a Republican president to the mix and the number of inspections is -0.427 (= -0.083-.060-0.284) because the Republican president alone reduces inspections by -0.083 and the presence of the Republican president increases the negative impact of the Republican senator by -0.060 inspections. We next consider the interaction of US congress and state politics.

2.5.5 Interactive Effects of the U.S. Congress and State Politics

The influence of a politician on environmental law enforcements may change depending upon whether his/her party enjoys majority in the U.S. Congress. A majority in the U.S. Congress gives a party a clear advantage for purposes of voting, introducing a change in legislation, etc. The hierarchy within a committee depends upon the party in majority in the U.S. Congress. For instance, the chairman of a House Committee is selected from the majority party and the ranking member is selected from the minority party. As such, the advantage (disadvantage) of own party majority (minority) in the U.S. Congress may produce subtle changes in a politician's attitude to environmental enforcements.

Table 2.9 shows the interactive effects of the Republican majorities in the U.S. Congress with the specific representation in the state and Congressional district on environmental law enforcements. We consider a variation of basic model 1 by introducing four new variables. Each of these variables is the product of a dummy variable on whether the Democratic or the Republican Party is in majority in the US Congress and respective politician dummy.[19]

The results are summarized in Table 2.9. The results of the Poisson model suggest that inspections are lower by -0.134 when a Republican congressman represents the district and the Democrats are in the majority. On the other hand, there is a much smaller increase of inspections of 0.015 when the Republican congressman operates under a Republican majority in Congress. If these are causal effects the Republican congressman does more to prevent inspections when the Democrats are putting pressure on to raise inspections.

The effect of a Republican majority on the impact of a Republican Senator is somewhat similar. When the Democrats have a majority in Congress, the Republican Senator is associated with 0.38 fewer inspections in the Poisson regression. When the Republicans are in the majority, the Republican senator is associated with only 0.28 fewer inspections. Thus, he expends less effort to reduce inspections when the Republicans are in the majority.

A summary of all the results above indicate that by and large, political affiliation of a representative politician has a bearing on environmental law enforcements. The Republican politicians prompt fewer inspections and their influence on reducing inspection strengthens with seniority. From the perspective of commonality of party affiliation between the federal and local

[19] Within our study period, the Democratic Party had majority in the US Congress for years 1989-1994 and the Republican Party had majority for years 1995-2005.

administration (or the U.S. Congress and the local administration), the influence of the Republican congressmen differs from the influence of the Republican senators. The Republican congressmen engage in a switching behavior; prompting more environmental enforcements under a Republican regime and less under a Democratic regime. On the other hand, the influence of the Republican senators on environmental law enforcements is stronger and unidirectional. The difference between the influence of the Republican congressmen and senators can be attributable to the difference in (i) the relative weight of vote (1/100 for a senator as opposed to 1/435 for a congressman) (ii) size of jurisdiction, (iii) the frequency of reelection and probably (iv) campaign contributions from the business groups. We discuss the policy implication of the influence of congressional politics on environmental law enforcements in the conclusion section.

2.6 Conclusion

The literature on environmental law enforcement in the U.S. has mainly concentrated on the dynamics of enforcements and compliance and the ensuing welfare implications. Despite the existence of a substantial literature on the politics of policymaking, the role of congressional politics in environmental law enforcements, perhaps due to its indirectness in nature, has been hither-to unexplored.

In a bipartisan political system like in the U. S., it may be easier (compared to multi-party political system) for interest groups to inexplicitly align with one side of the political spectrum. We make the first attempt to explore this issue in the context of environmental law enforcement. Where Republicans represent the public in Congress, there are fewer EPA inspections. The unevenness of enforcement with respect to political representation raises some important questions about the effectiveness of the EPA.

Political interference becomes a more serious problem for nationwide pollution if there are strong spillovers of pollution from one congressional district into other congressional districts. Certainly, this is possible. For example, pollution from California has drifted over the Grand Canyon in Arizona. Political differences in enforcement by area therefore can lead to sub-optimal results from a national pollution perspective.

The variation across districts also raises worries of unwarranted lobbying; with business groups contributing more to those politicians that prompt fewer enforcements resulting in increased pollution. Political interference can therefore be connected with pollution through the former's effect on environmental law enforcements. Though pollution and enforcements are jointly determined, the existing literature has not been able to find a good instrument for any of these.[20] However, we try to solve one of these two issues by postulating political affiliation as an instrument for environmental enforcement in the pollution equation.

The magnitude of pollution (facility level toxicity weighted yearly average release) is a facility-specific phenomenon. It depends on the commodity produced, the business cycle and research and development. Unlike in the inspection equation, we would therefore need facility specific covariates to explain facility level pollution. Since it is practically impossible to map the AFS-TRI dataset with facility specific covariates (because of string-matching issues we discussed earlier), we use 4-digit SIC level variables (from the Compustat database) and map them with the AFS-TRI dataset, using 4-digit SIC code (for a facility) and year. We hypothesize that perhaps a facility will move with the tide at the 4-digit SIC level; for instance, if the last year's sales growth rate at the 4-digit SIC level was high, then this year's pollution at the facility (that falls under such 4-digit SIC code) would also be high. Similarly, if last year's research and

[20] Owing to the lack of instruments, we had to lag pollution by a year and use lagged pollution as an explanatory variable in the inspection equation.

development intensity at the 4-digit SIC level was high, then this year's facility level pollution would be lower.

Following the literature on standard pollution equation, we introduce five variables (at the 4-digit SIC level) in the pollution equation. Table 2.10 and 2.11 provide the definitions and summary statistics of these variables. To estimate pollution, we employ the techniques of two-step feasible GMM estimation.[21] The first stage estimates the number of inspections at a facility (linear estimation) and the second stage estimates pollution on estimated number of inspections and other covariates. The identification of number of inspection comes from the local political dummy variables that are not part of the (2^{nd} stage) pollution equation.

We test the efficacy of political affiliation as an instrument for inspection in the pollution equation under over and exact identification setting. Table 2.12 produces the results from two regression models; the first model is over-identified and second model is exactly indentified. The first model excludes Republican Congressman Dummy, ALORSD and 2-digit SIC level dummy variables in the 2^{nd} stage of the estimation. Since the 2^{nd} stage includes five variables at the 4-digit SIC level (that are supposed to capture a lower SIC level characteristics than the 2-digit SIC level dummies), we exclude 2-digit SIC level dummies from the 2^{nd} stage and make the model over-identified. The second model is exactly identified where we estimate the number of inspections without ALORSD and include the 4-digit SIC level dummies in the 2^{nd} stage of estimation as well. The identification comes from the 2^{nd} stage excluded variable Republican Congressman Dummy.

The results from both the models are similar. The marginal effect of number of inspections is statistically significant and it shows that a unit increase in the number of

[21] The efficient GMM estimator minimizes the GMM criterion function J = N*g'*W*g, where N is the sample size, g are the orthogonality or moment conditions and W is a weighting matrix.

inspections this year reduces the next year's facility level toxicity weighted average release by a margin of 4660.45 in the over-identified and by 5202.32 in the exactly identified model. This is consistent with the existing consensus in the literature that more enforcement leads to more compliance. The results also show that an increase in research and development intensity reduces toxicity weighted release. Since a concentrated industry is characterized by a few technologically efficient firms occupying a heavy share of the industry level sales, facility level pollution in such an industry is lower than in industries with less concentration. Higher sales imply increased business activity resulting in more pollution. On the other hand, strict liability and high per capita income reduce pollution due to a higher demand for environmental quality.

We are thus able to postulate political affiliation as an instrument for environmental law enforcements in the pollution equation. The root source behind this mechanism could be political donations from the business groups. In general, the vast portfolio of issues that a voter bases his/her decision upon before each election in the U.S., perhaps, excludes the issue of political donations and facility level environmental law enforcement. Such exclusion may however result in implicit lobbying, regional concentration of pollution and a unidirectional influence of politics on environmental law enforcements. The role of business groups' political donations and its potential trade-off with environmental law enforcement, thus, open a new vista for future research.

CHAPTER 3

IS DISHONESTY CONTAGIOUS? AN EXPERIMENT

3.1 Introduction

The importance of individual honesty and trustworthiness in economic interactions is well known. These attributes facilitate cooperative relationships, enable contracts, strengthen legal and regulatory institutions, and as a result, promote economic growth (Zak and Knack, 2001; Guiso, Sapienza and Zingales, 2004). Also well known are vast differences in these attributes across cultures and countries. Figure 3.1 illustrates these differences, showing proportions of world population and world economic activity (respectively) that derive from countries with high, medium, and low levels of corruption, as measured by Transparency International's 2005 corruption perception index (CPI). Without reading too much into these coarse numbers (which, of course, raise complex questions of cause and effect), we note a stylized fact: The distribution of corruption is largely bi-modal, with the vast majority of both population and economic activity in either the low CPI (advanced developed) or high CPI (Third World and "transition") countries.

In this chapter, we explore a possible contributing explanation for this "fact" that is rooted in individual preferences.[22] Specifically, we conjecture that honesty is *contagious* in the following sense: If a majority of one's peers are honest, an individual is more likely to suffer an aversion penalty / disutility when behaving dishonestly. If so, honesty breeds honesty and dishonesty breeds dishonesty.

[22] Another possible explanation for this "fact" is that there is a vicious cycle in which low incomes promote corruption which, in turn, deters growth and so on. There is a vast literature on the evolution of institutions and their relationship to corruption and growth (see, for example, Acemoglu, et al, 2001). One interpretation of this chapter, in the context of this literature, is that, from the level of individual preferences, there may be some self-reinforcing dynamics to the evolution of bad and good economic institutions.

We study this conjecture in the context of a simple deception experiment, wherein we stimulate different subject perceptions of the propensity for honesty in the overall group of experimental subjects. We then examine the resulting impact on an individual's choice between truthful and untruthful behavior. Our experiments mimic the original deception game designed by Uri Gneezy (2005), who studied the effects of different payoffs on individuals' aversion to untruthful behavior.[23] Unlike Gneezy, we consider a single set of payoffs in each experiment and focus on the possibility of contagion. Because a central motive for our inquiry is to determine whether a perceived norm of honesty can spur more truthful conduct in a society that is considered corrupt, we conduct our experiments in both a low CPI country (Arizona, USA) and a high CPI country (Calcutta, India). In doing so, we find evidence for contagion in two senses: (1) the perception of a strong group propensity for dishonesty promotes untruthful behavior when subjects are otherwise predominantly honest (our Arizona experiment and survey); and (2) the perception of a strong group propensity for honesty promotes truthful behavior when subjects are otherwise predominantly untruthful (our India experiment).

To our knowledge, the only study that addresses the question of contagion in honesty is Fisman and Miguel's (2007) famous paper on the tendency for diplomats to garner parking tickets in New York. They find that the immunity-protected foreigners take their home country propensities for lawlessness with them. While these results might be interpreted as evidence against contagion (because diplomats seem to ignore U.S. values in their behavior), we believe that such inferences are misplaced for two reasons. First, there is no ceteris paribus in this

[23] Other recent experimental work on deception games include Sanchez-Pages and Vorsatz (2006), who study links between a subject's willingness to punish lies of others and their aversion to lying; Hurkens and Kartik (2009), who elaborate on Gneezy's (2005) results; Ederer and Fehr (2007), who study impacts of deception and aversion to lying in a principal agent game; Sutter (2009), who identifies sophisticated deception; Rode (2008), who studies effects of competitive and cooperative priming on subjects' honesty; and Charness and Dufwenberg (2005) who provide an alternative (guilt aversion) interpretation of Gneezy's (2005) findings.

comparison; diplomats may well temper their lawless behavior, relative to what they would do if protected by immunity in their home countries. Second, the empirical observation may be a reflection of different relevant peer groups for diplomats from different countries, consistent with the contagion hypothesis. We therefore offer a direct test of contagion in this chapter.

3.2 Literature

Our study is related to recent experimental economics literature on the effects of social information on behavior and a large psychology literature on conformity (see Cialdini and Goldstein, 2004, for a review), but is distinguished from this work by its focus on subjects' truthfulness – our research question. Unlike a significant subset of the literature (but not all), our design also voids prevalent theoretical explanations for "conformity" and obeying social norms, including social sanctions for the violation of norms (see Fehr and Fischbacher, 2004), incentives to obtain social esteem (Akerlof, 1980; Bernheim, 1994), and benefits from others' information (Banerjee, 1992, Bikhchandaria, Hirshleifer and Welch, 1992). In our experiments, individual actions are unobservable to anyone other than the individual; there is no possibility of social sanction or building social esteem; and what others do has no bearing on the payoff consequences of individual decisions. In Section 3.4, we explore the scope for theories of social (other regarding) preferences to explain our findings, arguing that the behavior we observe is likely a symptom of hard-wired contagious preferences. [24]

Perhaps most closely related to the present chapter is work on social information in dictator games. In Bicchieri and Xiao (2009), dictators are given different information about the proportion of subjects in a prior session who *were* "fair" vs. "selfish," and who believe dictators

[24] In psychology, some conformist behavior is explained as "behavioral mimicry" (such as yawning) and/or "automatic activation" that provides "an adaptive shortcut that maximizes the likelihood of effective action with minimal expense of one's cognitive resources" (Cialdini and Goldstein, 2004, p. 609). We believe that our results are likely to be the outcome of such reflexive responses, as we discuss in Section 3.

should be fair vs. selfish. Their results generally suggest that fairness in actions is contagious. Krupka and Weber (2009) expose dictators to a sample of four (fair vs. selfish) allocations of prior dictators and find a significant increase in the fraction of fair allocations when the sample share is (3/4) or 1, vs. (1/2) or less. Cason and Mui (1998) find that exposing dictators to one prior dictator allocation decision (vs. irrelevant information) reduces their propensity for selfish allocations. Duffy and Kornienko (2009) show that introducing a tournament that alternately ranks subjects' givings or earnings, significantly promotes generous and selfish allocations respectively; for our purposes, these results could be interpreted as dictators' response to a norm revealed by the choice of tournament. As in our experiments, subjects' actions in these studies are private; there are no social sanctions or rewards; and what others do is irrelevant to payoffs.[25]

The major difference in our experiments is the focus on deception rather than dictator games. This distinction, we believe, is central. Selfish behavior (as in a dictator game) and dishonest behavior (as in a deception game) are very different phenomena. While "fairness" may help promote cooperative relationships (like honesty), "selfish" and acquisitive impulses can promote effort and innovation that are at the core of a thriving market economy. In contrast, the negative consequences of dishonesty and corruption for economic prosperity are well documented. Perhaps for this reason, the culture and psychology of the two phenomena are also different. Selfishness is sometimes heralded as a symptom of the drive to compete and win, as in a game, but in other contexts, scorned as an impediment to cooperative relationships. Honesty, on the other hand, is consistently promoted as a value and virtue by church and community, suggesting that contagion may be less likely. Rode (2008), for example, finds that subjects' honesty is insensitive to competitive vs. cooperative priming. Given the importance of honesty to

[25] The possible exception is Duffy and Kornienko (2009), where winning a tournament, even if the winner is only identified anonymously (by ID number), may provide some intrinsic reward.

economic growth, and differences in norms of conduct around the world, it is also important to study contagion in honesty in different countries, as we do with our twin experiments in India and the U.S. In sum, honesty in deception games and fairness in dictator games are different phenomena, and results from dictator experiments cannot be readily translated to deception experiments. Indeed, even small framing differences in dictator games are known to have significant effects on behavior, as shown by List (2007) and Bardsley (2008).[26]

3.3 Experimental and Survey Evidence

3.3.1 The Arizona Classroom Experiment

To elicit honest or dishonest decisions from subjects, we closely follow the deception game designed by Uri Gneezy (2005). In this game, there are two possible payoff distributions for each pair of players, with each pair comprised of a "Sender" and a "Receiver." The two distributions are represented by Options A and B. Only the Sender is informed about the payments associated with the two options, one of which is advantageous to the Sender and the other of which is advantageous to the Receiver. The Sender sends one of two messages to the Receiver:

Message A: "Option A will earn you (the Receiver) more money than Option B."

[26] Effects of social information have been studied in a number of other contexts. In ultimatum games, Knez and Camerer (1995) and Bohnet and Zeckhauser (2004) examine effects of information about other proposer offers on proposer and responder behavior, finding evidence of a pre-existing norm of equity. Several authors study the role of social information in achieving social learning and conditional cooperation in coordination games (Berg, Dickhaut and McCabe, 1995; Fischbacher, Gachter and Fehr, 2001; Schotter and Sopher, 2003; Chaudhuri, Graziano, and Maitra, 2006; Eckel and Wilson, 2007); in this work, unlike ours and like studies by Chen, et. al (2009) on on-line participation in MovieLens and Duffy and Feltovich (1999) on learning in ultimatum and best shot games, the social information is potentially payoff relevant. Recent field experiments on charitable contributions document that subjects contribute more often when they believe a higher fraction of their peers contribute (Frey and Meier, 2004) and contribute more when told that a prior contributor contributed more (Shang and Croson, 2008); in this work, the social information can also be payoff relevant by signaling the virtue of the charity. Overall, this work provides evidence of contagion in different contexts, but with potential channels of effect that are not at play in our experiments. Lindbeck, Nyberg and Weibull (1998) also model contagious preferences, in the context of stigma for welfare; however, they *assume* that the stigma from welfare is negatively related to the proportion of a relevant peer group on welfare and study implications of this assumption. In contrast, we are interested in testing for the presence of contagion in honesty.

Message B: "Option B will earn you (the Receiver) more money than Option A."

A message is *truthful* if it truthfully indicates the option that is advantageous to the Receiver. After receiving the message chosen by the Sender, the Receiver chooses an option, which then determines payments. Both players are fully informed about the rules of the game, but Receivers are never informed about the specific monetary consequences of either of the two options.

In our experiment, we focus on a single set of payment options (while randomly varying the A/B labels attached to the two options). In one, the Sender receives $6 and the Receiver obtains $3, while in the other, the Sender receives $4 and the Receiver obtains $6.[27]

Our objective is to study how different perceptions of the truthfulness of other Senders affect Sender behavior. To do this, we use a between-subjects design where we expose different groups of Senders to different treatments designed to alter perceptions of other Sender behavior. In the control treatment, given to an initial session of subjects, no information on other Sender behavior is given. Using outcomes from the control treatment, Senders in subsequent sessions are given information in the form of the following statement:

> "Out of 20 Sender messages from past sessions of this experiment, with identical payment options, X (=Y%) were UNTRUTHFUL and (20–X) (=(100–Y)%) were TRUTHFUL."

Four treatments of this form are considered: Y = 15% (heavily truthful), Y = 40%, Y = 60%, and Y = 85% (heavily untruthful). In all treatments, the higher percentage is reported first (so that, for example, when Y = 40%, the number and percent of truthful messages from past sessions is indicated first). Our approach is similar to that used in other experimental papers in

[27] There is no obvious choice of payment options for our experiment. We conducted preliminary surveys on alternative options that varied (1) the gain to the Sender from lying G_S (assuming Receiver acceptance of recommendations), and (2) the corresponding Receiver loss L_R. Consistent with expectations, incentives to lie rise with G_S and fall with L_R. Our survey evidence implied an approximate Sender propensity for truthfulness equal to 58 percent for $G_S = 2$ and $L_R = 3$ (our chosen options). Armed with this evidence – and the conjecture (wrong as it turned out) that actual dollar stakes would raise incentives to lie – we settled on the indicated options.

the social influence literature. Frey and Meier (2004), for example, report two different percentages of past students who contribute to a charity based on different outcomes from a recent semester and, alternatively, a ten year interval. Bicchieri and Xiao (2009) report different shares of "fair" choices (40% and 60%) from a past session and argue in their paper that the information is truthful because they can define a past session to satisfy either claimed percentage. We designed our statements to highlight the selection of a subset of Sender messages and were careful not to state or imply that the reported messages represent a general pattern.[28]

We use these treatments to test the following:

Contagion Hypothesis. The likelihood of untruthful behavior by a Sender rises with the perceived likelihood that other Senders are untruthful.

In our experiment, we are implicitly jointly testing (1) whether the treatments are believed, and (2) effects of treatment-induced perceptions of other Sender behavior. Hence, if we find no significant effect of a treatment, we cannot reject the "contagion hypothesis" per se. However, if we find a significant effect – in the predicted direction – we can reject the null hypothesis of no contagion.

In all treatments, we provide Senders with general information on the propensity of Receivers to accept their recommendations. Based on results from Gneezy's (2005) experiments (where 78 percent of Receivers followed the Sender recommendations), we tell all Senders the following:

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[28] A norm in experimental economics is that the experimenter be honest with his/her subjects. We obey this norm with our approach. However, our treatments are intended to influence perceptions. We note that experimental designs with such objectives are common in the experimental economics literature. Prominent examples are influential papers that report a subject's "awarded" status "to suggest to the (other) subjects that the high status was deserved" when in fact it was randomly assigned (Ball, et al., 2001), that expose subjects to resume's with fictitious racial profiles (Bertrand and Mullainathan, 2004), that elicit contributions for a public project given fictitious variation in seed money (List and Reiley, 2002), and that use a standard experimental protocol to not inform subjects that they will be playing in subsequent rounds or roles (e.g., Binmore, et. al, 1985; Duffy and Kornienko, 2009). See Bonetti (1998) for a lucid discussion of this topic.

"In past experiments like this one, roughly 8 out of 10 Receivers chose the Option recommended by their Senders."

Receivers are not given this information, and Senders are so informed. The instructions given to Receivers and Senders (for the heavily untruthful treatment), respectively, are provided in the Appendix.

To verify that Senders generally believe that Receivers would accept their recommendations, we follow Gneezy's (2005) approach, asking them to predict their Receiver's choice and paying them for a correct prediction. Overall, 73.4% of Senders predicted that their Receiver would accept their recommendation.[29] These results indicate that Senders generally expect their recommendations to be followed; hence, their choices reflect a concern for the "fairness" / morality of lying, and not strategic motives. As it turned out, 73 percent of our Receivers followed their Sender recommendations.

The experiment was conducted in undergraduate economics classes at the University of Arizona in Spring, 2008 and Spring, 2009. In total, there were 233 Sender/Receiver pairs. Receivers were in different classes than any of the Senders. Anonymity of all participants was ensured by identifying subjects with a randomly assigned identification number that was also used to match Senders to Receivers. Class Rosters were used to ensure that no student participated more than once.[30] The experiment took approximately 10 minutes to run. Subject participation was purely voluntary. Subjects were so informed and instructed to communicate only with the experimenter during the experiment and were carefully monitored to this end.

[29] In principle, risk aversion could motivate an "accept" prediction by truthful Senders and a "reject" prediction by untruthful Senders. However, the proportion of truthful Senders predicting Receiver accept decisions (74.8%) is essentially identical to the proportion of untruthful Senders predicting accept decisions (72.0%) in our experiment.

[30] There was no overlap between the Receiver class and any of the Sender classes. Two students who were enrolled in two of the Sender classes were not present when the second class experiment was performed.

Control treatments were run in each Sender class to control for any potential individual class/course effects.

Table 3.1 reports the number of Senders exposed to each of the different treatments, and summarizes our results. Table 3.2 reports results of a Probit regression of truthful (y = 1) vs. untruthful (y = 0) choices on the treatments and individual class fixed effects. Relative to the control, one treatment has a significant impact on subjects' propensity to be truthful. In the "heavily untruthful" treatment (Y = 85%), the proportion of untruthful messages rises from 41 percent (in the control) to approximately 81 percent (under the treatment), almost doubling. Other treatments have no statistically significant effect. *We thus find support for the contagion hypothesis in the sense that a strong propensity for untruthfulness is contagious*.

In principle, two other effects discussed in the literature might also be at play in our experiment. First is the effect our treatments might have in creating a focal point (see, for example, Crawford, et al., 2008). Although focal points generally serve as coordination mechanisms that are not relevant in our simple experiment, the mention of others' behavior may have a focusing effect (Krupka and Weber, 2009). However, any focusing effect would arise in any of our treatments. For example, both Y = 60% and Y = 85% treatments could focus subjects on the untruthful message. The absence of a significant impact of the Y = 60% treatment (vs. the control) and the presence of a significant impact of the Y = 85% treatment (vs. the Y = 60% treatment) argues against "focusing" as an explanation for our main result. A number of scholars have suggested that subjects mechanistically respond to reference (or anchoring) points (see Shang and Croson, 2008, for discussion), implying in our case that stronger treatments will be more effective in stimulating compliant behavior. Although the lack of monotonic effect in our experiments loosely argues against a monotonic "mechanistic" response, we believe the distinction between these two explanations – cognitive reference point vs. social influence – is empty. Indeed, as argued below, we believe our results are likely to be explained by hard-wired contagious preferences that spur a mechanistic response to the social information that we provide.

Second – and closely related – there is the potential for experimenter demand effects, with subjects trying to do what the experimenter appears to want them to do (see Duffy and Kornienko, 2009, for an excellent discussion). Belying such effects in our experiments is again the absence of significant impact of our other (Y = 15%, 40%, 60%) treatments which, were there an experimenter demand effect, would also influence behavior.

Finally, Sutter (2009) documents the importance of "sophisticated deception," when a Sender tells the truth anticipating that his recommendation will not be followed. Sutter (2009) proposes a different measure of deception that includes sophisticated deceivers and excludes "benevolent liars" who lie anticipating that their Receiver will reject their recommendation. In our experiment, the treatments concern raw actions of other subjects (truthful vs. untruthful); hence, we are also principally interested in effects on raw actions (as reported above), rather than on Sutter's measure of sophisticated deception. The conclusions of Table 3.1 nonetheless extend to Sutter's measure, although the impact of our treatment on "sophisticated deception" is attenuated; the proportion of Sutter-truth-tellers is 55.7 percent under the control and 30.8 percent under the heavily untruthful (Y = 85%) treatment, a difference that is statistically significant (z = 3.06).[31]

[31] The proportions of Sutter-truth-tellers are 56%, 57.7% and 60.6% under the other (Y = 15%, 40% and 60%) treatments. Note that Sutter (2009) finds almost no "benevolent liars" in his experiment. In contrast, the proportion of "benevolent liars" in our subject pool (14.2%) is roughly the same as the proportion of "sophisticated liars" (12.4%); we also find no clear pattern in this behavior across treatments (for example, in the proportion of liars who are benevolent or the proportion of truth-tellers who are sophisticated liars). These observations loosely suggest that the predictions of our Reject-predicting subjects may be random, reflecting an anticipation that the Receiver essentially flips a coin when making his choice.

3.3.2 Arizona Survey Evidence

Like Gneezy (2005), we also obtained survey evidence on our research hypothesis. In a survey of 174 economics undergraduates in the Fall of 2007, we asked their response to a hypothetical situation in which they could be untruthful (to their material advantage) or truthful (to their material disadvantage). The specific situation posed is as follows:

> "Suppose that you have been visiting a country called Bayeb. Before leaving the country permanently, you must sell your used car. A local person (unknown to you) agrees to buy the car for US $2,000 and pay you in cash. However, you know that the radiator in your car is not functioning properly and the problem will only become noticeable after 2 months. The buyer does not know about the problem. If you tell him/her about the problem, then you have to reduce the price of the car by US $250 and sell it for US $1,750. However, if you do not reveal the problem, then you can sell the car for US $2,000 and the buyer will have to fix the car after 2 months, spending US $250. Would you tell the buyer about the radiator problem?"

Because the buyer is not known to the individual, and the individual is leaving the country permanently, before the problem can be discovered, anonymity is assumed and social / institutional sanctions are impossible. We consider three treatments: (1) A control with no further information. (2) A "truthful" treatment in which the respondent is told the following:

> "Surveys in Bayeb indicate that, in a situation like yours, 9 out of 10 people would tell the buyer about the radiator problem."

(3) An "untruthful" treatment in which the respondent is told the following:

> "Surveys in Bayeb indicate that, in a situation like yours, 9 out of 10 people would not tell the buyer about the radiator problem."

Table 3.3 reports survey results. Relative to the control, the truthful treatment increases the proportion of truthful respondents by ten percent; conversely, the untruthful treatment increases the proportion of untruthful respondents by almost twenty percent. Only the second effect is statistically significant. Hence, we again have evidence of contagion in the sense that a perceived propensity for others to be highly untruthful is contagious.

3.3.3 The India Laboratory Experiment

In our Arizona experiment, we find that information indicating a strong peer propensity for dishonesty promotes untruthful behavior. A central motive for our work is to study the potential for contagion in the other direction: In a country where corruption is high, and a propensity for dishonesty correspondingly high, can subjects be spurred to more truthful conduct by information suggesting a norm of honesty? India – with a corruption index in the highest tier of countries – is arguably an ideal country in which to examine this question.

In the Spring of 2009, we conducted a deception experiment with a set of 60 Sender/Receiver pairs of undergraduate students at Jadavpur University in Calcutta. Like most university experimental labs, Jadavpur maintains a roster of willing experimental participants and regularly announces opportunities for participation in experiments. Our announcements were made in English, and were only made in Departments where English fluency is required (most of the University, excepting the Bengali major). In the India experiment, the following two payoff options were posed:

Option A: 160 Rupees to you (the Sender) and 160 Rupees to the other student (the Receiver).

Option B: 200 Rupees to you (the Sender) and 100 Rupees to the other student (the Receiver).

As in Arizona, option labels were varied randomly. The payoffs were designed to (a) have the same ratio of Receiver loss to deceit and Sender gain (3/2) as in our Arizona experiment, (b) to meet minimum payment requirements, and (c) to give substantial stakes to the choices made. Although 40 Rupees (the Sender gain from dishonesty and Receiver acceptance) are less than one U.S. dollar, average daily per capita consumption expenditures in India are less than 19 Rupees in rural areas and 35 Rupees in urban areas.[32] Put differently (quoting Fehr, Hoff and Kshetramade, 2008), "Fifty rupees are roughly equal to a day's skilled wage." The stakes in our experiment can therefore be considered substantial in context.

We conducted two treatments, a control with no information about Sender behavior in prior sessions of related experiments and a strongly truthful treatment in which Senders were given the following information:

> "Out of 15 Sender messages from a past session of this experiment here in Calcutta, 13 out of 15 (85%) were TRUTHFUL and 2 out of 15 (15%) were UNTRUTHFUL."

Our initial control treatment responses in Calcutta gave us the 15 Sender messages satisfying this statement.[33]

Table 3.4 reports results from the Calcutta experiment. We find that the honest treatment leads to a significantly higher proportion of truthful messages than in the control, although the level of significance (p = 0.067) is greater than five percent (two-sided). Under the treatment, the proportion of honest messages is more than fifty percent higher than under the control, 67.7% vs. 44.8%. The proportion of Senders predicting Receiver acceptance is high

[32] See "Household Consumption Expenditure in India (January-June 2004)," NSSO, Government of India, 23 November, 2005.
[33] As in Arizona, all Senders were told that roughly 80% of Receivers accepted their Sender recommendations in a similar prior experiment; none of this information was provided to Receivers and Senders were so informed.

(78.3%) and the proportion of Receivers accepting their Sender recommendations is also high (70.2%), although less than in Gneezy's (2005) experiments and slightly less than in Arizona.[34]

3.4 Explanations for Contagion

Our results indicate that some of our subjects have an aversion to lying that increases with the perceived propensity for honesty in a relevant peer group. Why might this be true? Because our experimental design ensures that there is no scope for social sanctions or building social esteem, and the information we provide is irrelevant to experimental payoffs for Sender and Receiver, the contagion we observe does not reflect standard theoretical motives for "conformity" (see the introduction). This leaves two alternative explanations: First, perhaps subjects have social (other-regarding) preferences. If so, then information about other Senders' behavior can potentially be relevant to the utility a subject derives from different actions (message choices). Second, alternatively, contagion may be "hard-wired": Subjects may have a built-in contagion trait that, as a reflex, prompts them to change their aversion to lying in response to what other people do (copying the majority).

3.4.1 Social Preferences

Two theories of social preferences (suitably modified) are consistent with Gneezy's (2005) findings on deception behavior and are therefore the most natural candidates for explaining our results: 1) the relative payoff preferences of Fehr and Schmidt (1999) or Bolton and Ockenfels (2000), and 2) the guilt aversion posited by Charness and Dufwenberg (CD, 2006) and Battigalli and Dufwenberg (BD, 2007).

[34] In the India experiment, a slightly higher fraction of truthful Senders predict Receiver acceptance than do untruthful Senders (82.3% vs. 73.1%), but the difference is not statistically significant (z = 0.852). Likewise, as indicated in Table 3.4, a slightly higher fraction of control subjects predict Receiver acceptance, but again the difference is not statistically significant (z = 0.806).

With Fehr-Schmidt preferences, agents are averse to inequality, whether due to obtaining a higher payoff than others or, worse, a lower payoff. Alternately, if they are "spiteful" (see Levine, 1998; or Fehr, Hoff and Kshetramade, 2008), they may benefit from a higher relative payoff. Such social preferences alone do not imply an effect of information about other Senders' propensity for honesty on a Sender's utility-maximizing decision. Necessary for such an effect is that a Sender's reference group – the group of subjects to whom a Sender compares himself – be a broader population than the Receiver who is directly affected by the Sender's decision. This property is controversial; for example, Ellingsen and Johannesson (2008, p. 994) express skepticism that agents care about the outcomes from others' actions in choosing their own conduct. Even under a "broad reference group" premise (the Sender compares himself to all subjects in the experiment), we can show that generalized Fehr-Schmidt preferences, modified to be consistent with Gneezy's (2005) findings, do not imply contagion.[35] Indeed, for our Arizona experiment, they imply the opposite: incentives for honesty to *fall* with the perceived propensity for honesty in the reference population of Senders.[36]

Perhaps guilt aversion offers more promise. CD and BD posit that subjects are averse to disappointing their partners: If a Receiver obtains a payoff that is less than he or she expects to obtain (where the "Receiver expectation" is based on the Sender's belief about the Receiver's beliefs), then the Sender suffers a guilt aversion penalty that is proportional to the extent of the

[35] Gneezy (2005) rightly points out that pure Fehr-Schmidt preferences predict that Sender incentives for dishonesty rise with the Receiver's high payoff, contrary to his experimental results. The addition of a social welfare component to preferences, and a utility penalty to deceit, cures this inconsistency.

[36] Details are available in our expanded paper. This conclusion rests on the plausible premises that (1) the reference group is the overall population of experimental subjects and (2) the inequity disutility functions are weakly convex, implying that larger inequities are not better, per unit, than smaller ones. The second premise mimics Bolton and Ockenfels (2000) Assumption 3. Intuitively, a higher propensity for Sender honesty increases the probability that Receivers obtain their high payoff and that other Senders obtain their low payoff. This raises the net benefit to dishonesty by lowering the implied cost of inequity with respect to Receivers and lowers the net benefit to dishonesty by raising the cost of inequity with respect to other Senders. In our Arizona experiment, the former (Receiver) effect dominates the latter (Sender) effect because disadvantaged Receivers obtain less than disadvantaged Senders; hence incentives are tilted toward dishonesty, contrary to the contagion hypothesis. Similar logic applies to both spiteful (Fehr, et al., 2008) and Bolton-Ockensfels (2000) preferences.

shortcoming. This logic, we believe, is likely to be important in explaining subjects' behavior in deception experiments (see Charness and Dufwenberg, 2005). The question here is whether it can explain the *contagion* that we observe.

In principle, the answer is "yes" if our treatments affect a Sender's beliefs about the Receiver's expectations. Suppose that a higher Sender expectation about the propensity for other Senders to be untruthful (as induced by our "heavily untruthful" treatment) prompts Senders to believe that Receivers also believe that there is a higher Sender propensity for untruthfulness. Then, given an assumed mechanical acceptance of Sender recommendations, the Sender expects the Receiver to expect a lower payoff, which lowers the guilt aversion penalty to lying and thus prompts more Senders to lie.

Although aspects of our experimental design mitigate such effects,[37] we nonetheless test for them directly. We do so by measuring Sender beliefs about Receiver beliefs (about Senders' propensity to lie) and evaluating their impact on Senders' decisions on whether or not to lie. We stress that this exercise is NOT a test of guilt aversion per se; guilt aversion does not predict that the Sender beliefs we measure will necessarily alter Sender decisions to lie or not.[38] However, in order for guilt aversion to fully explain the *contagion* that we observe, our treatments must not have an impact on Sender message decisions that is distinct from the impact of Sender beliefs.

[37] Receivers in our experiment are never told the payoffs available in the game and, hence, have no basis for disappointment. Senders are told this and also know that the information about Sender behavior in prior sessions – our treatment – is not provided to Receivers. Senders are also told that Receivers generally accept their recommendations mechanically, and we have evidence that this statement is believed in all treatments (Tables 3.1 and 3.4). If Senders believe that Receivers internalize the treatment information delivered only to Senders, they should also expect Receivers to revise their decisions on whether or not to accept or reject Sender recommendations.

[38] We establish this formally in our expanded paper. Intuitively, there can be two effects of Sender beliefs. The first is the pro-contagion effect described in the text (for a given Sender belief about the probability of Receiver acceptance). The second is due to a (rational) Sender belief that, with a higher Receiver assessment of the probability of Sender truthfulness, the Receiver accepts the Sender's recommendation with higher probability; this raises the Sender's (self-interested) incentive to lie, countering the first (contagion) effect. Either effect can dominate, implying no clear prediction from guilt aversion theory about the impact of Sender beliefs on Sender deception decisions.

That is, our null hypothesis – the guilt aversion explanation for contagion – is that our treatment effects are zero once we account for Sender beliefs about Receiver beliefs.

To measure Sender beliefs about Receiver beliefs, we asked Receivers the following in our India experiment and a subset of our Arizona experiments (28 control treatment Sender / Receiver pairs and 29 "heavily untruthful" treatment subject pairs):

> "We now ask you to predict the proportion of Senders in this experiment that sent truthful messages… If your prediction is correct (within five percentage points of the actual proportion, plus or minus), you will receive an addition $1 (20 Rupee) payment."

Receivers were then asked to circle one of twenty five-percentage-point bands (from 0-5% to 96-100%). The question was posed after Receivers made their option choice. Similarly, Senders were asked the following (after they made their message choice):

> "Your Receiver will indicate to us his/her belief about the proportion of Senders that are truthful. After selecting the payment option and before receiving payment from the experiment, your Receiver will indicate that out of 100 Senders, he/she believes that X percent are truthful. We now ask you to predict your Receiver's indicated belief (X). If your prediction is correct (within five percentage points of the actual choice, plus or minus), you will receive an additional $2 (20 Rupee) payment."

In the Arizona experiment, average Sender beliefs about Receiver beliefs (about the proportion of truthful Senders) were 58.6% under the control (using midpoint values) and 38.0% under the heavily untruthful (Y = 85%) treatment, a significant difference (z = 3.264). In the India experiment, in contrast, average Sender beliefs were 65.1% under the control and 69.1% under the heavily truthful (X = 15%) treatment, an insignificant difference (z = 0.85).[39]

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[39]Average Receiver beliefs were virtually identical in the two experiments, 55.8% in Arizona and 54.4% in India.

Given the surprising correlation between our treatments and Arizona Sender beliefs, Table 3.5 reports Probit estimations to test for distinct effects of Sender beliefs and our treatments on Sender message choices (virtually identical results are obtained with Logit). The Sender beliefs are statistically insignificant and, more importantly, do not confound the distinct effects of our treatments.[40] Hence, if guilt aversion underpins subjects' aversion to lying in our experiments – and nothing we have done suggests otherwise – then our treatments change the guilt aversion parameters in subjects' preferences, a contagion effect that is not itself explained by the theory.

3.4.2 Hard-wired Contagion

Contagion in honesty may be a reflexive response of subjects – a hard-wired trait that tells them to "do as others do" in these types of situations. This explanation for our findings is promising, we believe, but also shallow. It begs the deeper question: Why would such a trait evolve? That is, why is a contagion trait advantageous from an evolutionary perspective?

Two observations form the basis for a more complete inquiry into this subject (Innes, 2009), drawing broadly on the evolutionary strategy literature (e.g., Frank, 1987; Bergstrom, 2002; Guth and Kliemt, 1994; Sobel, 2005). First, there can be network effects that motivate conformity with the conduct of others (Katz and Shapiro, 1986; Banerjee and Besley, 1990). Although our experiment contains no network effects, the posed situation of moral dilemma is arguably sufficiently similar to others in which network effects are present that a contagion trait, motivated by the latter situations, kicks in. In particular, honesty can be central to advantageous

[40] In principle, subjects' expectations of Receiver beliefs could depend upon their decisions, implying that they are endogenous. Unfortunately, in our experiments, we have no exogenous instruments, distinct from our treatments, with which to identify Sender beliefs. However, the only conceivable mechanisms for endogeneity (that we can envision) – such as risk aversion or subjects projecting expectations based on their own behavior – imply a positive relationship between truth-telling and Sender beliefs that would bias our estimations against the treatment effects that we find in Table 3.5. Our estimations thus provide evidence in favor of distinct treatment effects that is robust to potential endogeneity.

outcomes in games of cooperation. In such games, honesty can be advantageous when most others are honest because honest people only engage in profitable partnerships with other honest people. Likewise, dishonesty can be advantageous when most others are dishonest because honest people are exploited in joint ventures and therefore withdraw from them. Second, however, this logic only motivates conformist equilibria in which all are honest or all are dishonest. A contagion trait – telling an individual to change preferences in response to social cues – can be advantageous when there is trade between groups that have evolved to different equilibria. Only those who are "contagious" will be able to partner with others in a different group in which a different norm predominates.

3.5 Conclusion

We find evidence that honesty can be contagious when subjects are otherwise predominantly dishonest (Calcutta) and dishonesty can be contagious when subjects are otherwise predominantly honest (Arizona). These responses shed some light on population dynamics in truthfulness and corruption that may help to explain societal tendencies to be in one camp or the other, highly honest or highly dishonest. Normatively, they suggest value to a culture of honesty in an organization by indicating the fragility of truthful behavior; even with small stakes, our Arizona subjects flocked to the dishonest course when primed with a social pass-go to do so. Conversely, they suggest promise for countering corrupt impulses in the developing world if perceptions of norms can be reversed.

Of course this begs the question: By what mechanism can norms be changed? Recent findings suggest that this may be tough. Rode (2008), for example, finds that dishonesty is insensitive to cooperative priming, and Fisman and Miguel (2007) find that foreign diplomats do not respond to American values of lawful behavior. However, empirical evidence indicates that aid and trade reduce corruption (Tavares, 2003; Gokcekus and Knorich, 2006; Innes and Mitra, 2009). Our results suggest a coarse mechanism for this effect, but leave much unanswered. For example, what determines whether "honest norm" partners bend to the norms of "dishonest norm" partners or vice versa? Our Arizona survey reveals a sensitivity to local norms, but more work is needed to determine how a subject's exposure to another country's norms effects his behavior in his own country. Moreover, if (as we suggest in Section 3.4.2) contagion in honesty is hard-wired and motivated by economic interactions between societies with different norms, then contagion will be stronger in groups that trade more with outside groups, which in turn implies links between the extent of trade and the responsiveness of local norms to trade relations. For example, if an "honest norm" agent seeks to trade in a "dishonest norm" country that is relatively closed, then the honest trader is likely to bend to local (dishonest) norms, rather than vice versa. These questions and conjectures lend themselves to further experimental work that can illuminate not only the nature of the contagion we identify, but also how it can be exploited for positive social ends and what implications it has for one of the key pillars of the globalization debate: benefits of trade in reducing corruption.

CHAPTER 4

THE DIFFUSION OF AUTOMOBILES AND ITS CONNECTION TO THE DEVELOPMENT OF ROADS IN THE UNITED STATES

4.1 Background and Literature[41]

A significant and long-lasting technological innovation that markedly improves public lifestyle is, in common parlance, referred to as the General purpose technology (henceforth, GPT). The astonishing progress of science and technology over the twentieth century led to the emergence of many prominent GPTs in the global arena and with the passing of time these GPTs shaped many associated smaller scientific innovations. An innovation in traditional technology refers to a smooth and continuous advancement of the existing scientific knowledge but the emergence of a GPT refers to a drastic and quantum leap advancement of the existing scientific knowledge that redefines certain basic acts of day-to-day human lifestyle. GPTs such as the steam engine, railroad, electricity, television, telephone, computer, internet, artificial satellite and automobile have all manifested the above characteristics and contributed to the improvement of the standard of living across the globe in a plethora of direct and indirect ways.

During the initial phase, the infrastructure that a GPT requires in order to flourish into its full functionality is seen to be relatively underdeveloped and hence requires some time for adjustment and development to fully accommodate the GPT. The main reasons include the obsolescence of the old technology and skill, the gestation period for the infrastructure to adjust (Rosenberg, 1996; Helpman and Trajtenberg, 1998), the associated learning cost (David, 1990; Bresnahan and Greenstein, 1996), the readjustment and relocation of workers. However, once its nascent phase is over, a GPT and the infrastructure that accommodates it start impacting each

[41] Throughout this chapter, we use the words 'automobile', 'motor vehicle' and 'car' in a synonymous manner.

other and this bidirectional linkage leads to further expansion and development of both of these. The diffusion of automobiles usage and the expansion of the network of roads during the first half of the twentieth century in the United States provide a classic case to study this linkage.

Ever since its inception as a GPT, the motor vehicle gradually redefined social and economic relations in the United States in a myriad of ways. The production process of motor vehicles became a major source of a stream of employment and income generation and it can be conceived as the starting point of many other twentieth century productivity-improving innovations in the technique of mass production (Lipsey et. al 2005). The motor vehicle contributed to greater and smoother user mobility both in terms of short and long distances. It made the process of internal migration comfortable, faster and more independent and thus provided individuals the option to live further away from workplace. It also led to an increase in individuals' frequency of domestic long-distance travel thereby expanding the tourism industry and all these resulted in a change in the pattern of holidays and the practice of vacations taken within short distances of individuals' residence. In addition, the diffusion of motor vehicle usage immensely contributed to the formation of suburban shopping center.

Not only people got attracted to the comforting features that an automobile brought, but also they started realizing the superiority of those features compared to the features offered by the alternative modes of transportation. Horse and rail culture were two prominent features of late nineteenth century American civilization. With the emergence of motor vehicle, the drawbacks of the horse and rail culture became more vivid. The motor vehicle was conceived of an attractive alternative with the blend of the good features of horse and rail culture. Not only was it as flexible as a horse but it also had the speed of a locomotive without the costly liability of a system of fixed rails and overhead wires (Flink, 1988). As Flink writes "….general adoption of the automobile promised to relieve taxpayers of the high cost of removing tons of excreta daily from city streets and to eliminate huge expenditures for endless miles of track, overhead wires, and networks of elevated platforms and/or tunnels, and with this the graft and corruption that too often seemed to be associated with building urban mass transit system".

The urban planners and engineers, too, started realizing that the efficacy of motor vehicles critically hinges on the condition and development of existing roads. The potential dependence between motor vehicles and road-development made people realize that the experience of driving a motor vehicle would not be as pleasant as it promised to be if the roads were not congenial enough. It soon became clear to motorists and planners that the promotion of road building and diffusion of car usage, taken together, could serve as a powerful tool for economic and social change.

With the realization of the potential benefit of such infrastructural development mainly through connectivity to unexplored places in the hinterland, people demanded more automobiles as a means for faster transportation for all purposes. In a simultaneous manner, the increased demand for automobiles generated sizeable amount of automobile tax and registration revenues that further encouraged road construction at remote places only to attract more potential consumers of automobiles. Thus, the expansion of the network of roads in the United States was critical for the diffusion of automobiles and vice versa. For instance, the grand total of existing highway mileage in 1923 was approximately 251 thousand and it more than doubled to approximately 573 thousand by 1945. Likewise, the number of registered automobiles grew by 94% from approximately 13.2 million in 1923 to approximately 25.6 million in 1945.[42]

In this chapter we try to establish that in the first half of the twentieth century the expansion of highway networks in the United States was instrumental to the diffusion of

[42] Source: Highway Statistics Summary to 1945 by Public Roads Administration & Federal Works Agency.

automobiles as a prominent GPT and such diffusion generated more registration and tax revenues only to spur further expansion of the network of highways. As seen in Table 4.1, by 1935 the United States was the world leader in automobiles per capita with almost twice as many as in New Zealand, Canada, and Australia and four times as many as most European countries. It can also be noted that passenger cars accounted for approximately 85% of the total registered motor vehicles indicating that the Americans preferred a more independent mode of transportation.

4.2 Surveys and Mechanism

The strong positive correlation and likely bi-directional linkage between automobiles and road networks is illustrated in Figure 4.1. It shows the yearly total registration of automobiles (as a multiple of 100) and the total existing highway mileage across the 48 states for the period of 1923 to 1945. It should also be noted that the expansion of road networks was taking place at a much faster pace than the diffusion of automobiles (the respective linear trend-lines indicate the difference in the slopes).

Figure 4.2 perhaps explains a rough overview of the mechanism that led to the complementarity between the diffusion of automobiles and the expansion of road networks. For the period of 1921 to 1934, Figure 4.2 shows that motor vehicle taxes had a direct connection with the financing of the expansion of roads. The huge drop in highway expenditures between 1931 and 1933 was perhaps caused by the great depression; however, there was no evidence of a concomitant decrease in the motor vehicle tax collections. The highway development planners anticipated increased tax revenues from diffusion of automobile usage and accordingly preset approximate level of outlays required for the expansion of highway networks. Here again, both the trend-lines are upward sloping.

From the micro perspective of utility maximization, the automobile owners sought improved and extended roadways. They therefore started exerting political pressure in various forums and ways, especially after World War I. They also realized that improved road would result in more people buying cars and this in turn would strengthen their lobbying ability. Vehicle operating conditions considerably improved when the existing rural highways were paved, and from the perspective of cost-benefit analysis, the car owners were willing to pay the full marginal costs of these improvements anticipating that the benefits of such improvements would be much higher. Like in most models of planning, the ratio of user benefits to user charges determined the viability of a road project dealing with road improvement and extension. The road-building boom was chiefly motivated by "traffic minded good roads associations and automobile clubs".

According to Robert Hennes (1960), a manifold increase in construction activities resulted in a road building boom during the first three decades of the twentieth century. The measurable user benefits of such construction were the reduction of vehicle operating cost, an improvement in the level of comfort and a saving in time owing to faster communication between a motorist's origin and destination. However, from the perspective of planning it became clear that building roads in any and every direction would not be beneficial because motorists had certain specific directions in mind. Conceivably, a planner had two important questions to answer: where did the motorists want to go? And, was building roads in such directions justifiable in terms of a cost-benefit analysis? These questions were respectively tackled by origin-destination surveys and formulation of benefit-cost ratios.

The successful application of the origin-destination surveys and the benefit-cost ratios resulted in the undertaking of a large scale system of road improvements mainly for those who used such facilities. Robert Hennes (1960) writes "The experience which led to the formulation and adaptation of these techniques was experience acquired, for the most part, between the wars when American engineers were completing the job of taking over a pre-existing system of roads and streets, and making it over for the use of the motor vehicle. These same tools would not have been equally valuable in the horse-and-buggy days when vehicle operating cost was a less significant factor in determining the need for road improvement".

As the development of infrastructure in terms of road construction gained momentum, motor cars became more attractive. One group of potential buyers understood how a motor vehicle could make communication easier and improve the level of comfort and independence in transportation and decided to purchase and the other group of buyers learned the same by noticing a significant improvement in the standard of living of those who already purchased. As more motorists hit the road, it sparked off a series of economic activities and consequences by a multiplier effect. Economic activities also gained momentum through a massive increase in tax dollars paid by the motorists and the introduction of several stores, marketplaces and suburbs surrounding the new roads. With the development of stores and marketplaces offering affordable goods and commodities within a medium range distance, the benefits of having a motor vehicle to a potential buyer soared even higher.

4.3 Data and Econometric Model

The motor vehicles diffusion equation can be expressed as:

(1) $M_{it} = f(P_{it}, G_{it}, H_{it}, R_{it}, Y_{it}, D, T)$;

Where M_{it} is per capita motor-vehicles registered in state "i" in year "t", P_{it} is a proxy price for motor-vehicles in state "i" in year "t", G_{it} is the average price of regular gasoline in state "i" in year "t", H_{it} is the average horse price in state "i" in year "t", R_{it} is the per capita existing road mileage in state “i" in year “t”, Y_{it} is per capita state income in state “i" in year “t”, D is a vector of 47 state dummy variables, and T is vector of 21 year dummy variables.

Table 4.2 presents the summary statistics of the variables included in the estimation. For the period of 1900 to 1946, the average number of registered automobiles was approximately 268 thousand. Florida and Nevada had the minimum number (10) of registration in the year 1900 and California had the maximum (approximately 2.7 million) number of registration in the year 1946. Between 1900 and 1946, Nevada had the lowest average number (approximately 16 thousand) of registration among all states. For the same period, New York had the highest average number (approximately 1.08 million) of registration among all states. The huge difference between the minimum average registration and the maximum average registration indicates that the average number of registrations across the states varied to a significant extent, and therefore the geographic features of each state played an important role in the number of registrations. As such, while explaining the variation in registration across the states, the unobserved state features need to be controlled for.

The highway mileage data is available from 1923 onwards. For the period of 1923 to 1945, the average existing highway mileage in a state in a given year was approximately nine thousand. Delaware had the minimum (351 miles) existing highway mileage in the year 1923 and Texas had the maximum (approximately 60 thousand) existing highway mileage in the year 1945. For the period of 1923 to 1945, Rhode Island had the lowest average (approximately 1,203 miles) existing highway mileage among all states. For the same period, Texas had the highest average (approximately 38 thousand miles) existing highway mileage among all states.

Like any typical demand function, the demand for motor vehicles depends on its price. A consistent set of direct evidence on automobile prices by state and year is not available for the entire period. Based on the information available, the price of the same motor vehicle in various cities varied with respect to the shipping costs from Detroit, which in turn was positively related to the distance from Detroit. We therefore construct a proxy price of a '*typical car*' in the following manner. Six different models of motor vehicles were in frequent use during the first half of the twentieth century. These were Buick, Cadillac, Chevrolet, Dodge, Ford and Packard. Ford accounted for the highest share and every second vehicle was either a Ford or a Chevrolet. Therefore, in order to estimate the motor vehicles usage equation, we need a 'composite price' of a 'typical car'. Using the national wholesale price index (from Bureau of Labor Statistics and the index number of price of car (1926 = 100), we constructed from the base price of a typical motor vehicle at Dearborn city in Detroit, Michigan. Then we used the distance from Dearborn to the most populous city of each state in 1910. The distance was then multiplied by the unit tariff rate and the typical weight of a car of 2,265 pounds (based on the Ford Model A).[43]

Estimate of Final Car Price at the (state level) destination = Base Price at Detroit + Weight*Distance*Tariff.

4.4 Estimation Results and Issues

4.4.1 Ordinary Least Square Estimation

Table 4.3 presents the OLS regression results for various linear specifications of equation (1) above. The dependent variable in each case is the per capita registered automobiles in state 'i' and year 't'. In specification 1 with only the per capita existing highway mileage in the equation, the coefficient on log highway mileage is positive and statistically significant. It indicates that a one unit increase in per capita existing highway mileage brings about a 1.86 unit increase in the

[43] Data on the tariff rate was available from "New Automobiles in Interstate Commerce" No. 28190 (Interstate Commerce Commission Reports), Page: 564 & 569

number of automobile registration. This implies an elasticity of 0.162, such that a one percent increase in highway mileage per capita increases automobile registrations per capita by 0.1620.

There is clearly omitted variable bias in the estimation of the raw correlation. When additional correlates and state and year fixed effects are added to the analysis, the coefficient on the per capita highway mileage is reduced to 0.625 or less and the elasticity of auto registrations per capita with respect to highway miles per capita falls to 0.054 or less. This is true whether the car price proxy or the gas price proxy are not included in the analysis in specification 2 or are included in specification 3. Note that the inclusion of the car and gas price proxies restricts the sample to the years 1927 to 1946 instead of the years 1923 to 1946.

The coefficients on most of the correlates are consistent with expectations. The statistically significant coefficient of horse price, a demand-side substitute for cars, implies that a one unit increase in the horse price results in a 0.00005 unit increase in the number of automobile registrations, which implies an elasticity of 0.06. Since automobiles are a normal good, it is no surprise that the coefficient of per capita income is positive and statistically significant in the second specification. The coefficient implies an elasticity of 0.05.

4.4.2 Potential Endogeneity and Construction of Instrument

In the previous sections we argued that the diffusion of automobiles and the expansion of the road network have a strong bi-directional relationship during the first half of the twentieth century in the United States. Therefore, the expansion of the road network was not exogenous as the above OLS regression models assumed. To correctly estimate the impact of roads expansion on automobile registrations, we need an instrument for road expansion.

The construction of the instrument is based on the perspective of a national 'social planner' whose first objective would be to connect the most populous city of each state with the most populous city of the adjacent states, something that the already existing network of railroads accomplished earlier. Accordingly, each state would be responsible for the construction of that part of the network which lies within its political boundary. Each state would receive funding from the federal government according to the amount of mileage in the road network that it would construct within its political boundary. In other words, if it so happens that the amount of mileage that one particular state would need to construct is twice the amount of mileage that another state would need to construct, then the former would receive twice as much federal funding as the latter.

The railroad miles were constructed between 1832 and 1910 in the United States. Since it is commonly believed that a significant part of the network of roads was built along the already existing network of railroads, we assume that each state, within its political boundary, would build a network of road mileages that would be equal to the existing railroad mileages in that state. Since the expansion of roads in the United States began in the early twentieth century, we considered the railroad network mileage of 1910 for each state as its road mileage responsibility for the subsequent years. Each state, therefore, faced the task of constructing a network of roads equal to its railroad network mileage of 1910.

The data on total federal funding for state highway mileage construction is available from the year 1917. However, the state specific highway mileage data is available from 1923 onwards. We assumed that if I_i stands for the road mileage responsibility for state 'i' at the first year of planning, then state 'i' would receive $\left(I_{i1} \Big/ \sum_{i=1}^{48} I_{i1} \right)$ fraction of the total federal funding to all states in that year. Using the cost of construction of one mile in each state in 1923, we calculated the road mileages that state 'i' would be able to build in 1917.[44] To avoid issues relating to endogeneity, we assumed that the cost of construction per mile for each state in subsequent years would be state-specific and remain equal to the 1923 cost.[45] Thus, the cost per mile parameter varies across states but stays constant across years. Therefore, in the first year the state 'i' would build $M_{i1} = \left\{ \left[F_1 * (I_i \Big/ \sum_{i=1}^{48} I_i) \right] / C_i \right\}$ miles of the network of roads (where F_1 stands for the total federal funding available in the first year of planning (=1917) and C_i stands for the cost of construction of one mile in state 'i'.

Consequently at the start of the second year state 'i' would face the responsibility of constructing the remaining road mileage $[I_i - M_{i1}]$. For state 'i' federal funding in the second year would be allocated according to the fraction of the remaining mileage (given by the expression in the square bracket) that it would need to construct. Thus in the second year state 'i' would construct $M_{i2} = \left\{ F_2 * \left[(I_i - M_{i1}) \Big/ \sum_{i=1}^{48} (I_i - M_{i1}) \right] \Big/ C_i \right\}$. We assumed that this process would continue for the subsequent years till a state reaches its target. It was also assumed that when a state reached its target road mileage, it would not receive any federal funding for the subsequent years. Thus, for any year T (T ≥ 1923) for state 'i', the total existing mileage can be instrumented by $\sum_{t=1923}^{T} M_{it}$.

The constructed instrument has a strong positive relationship with highway mileage per capita. For example, we ran an OLS regression of the per capita existing highway mileage on the

[44] Source for the information on per unit cost: "Highway Statistics Summary to 1945" by Public Roads Administration & Federal Works Agency

[45] The earliest year for which the state-specific cost of construction of one mile is available is 1923. The (across the state) average cost of construction of one mile in 1923 was $20,659.

per capita constructed instrument in linear form (consistent to our previous specifications) and with robust standard errors. The coefficient of the per capita instrument was 3.87 and it is statistically significant at 1% level (t-stat = 39.48, R^2 = 0.84). In Figure 4.3A we plotted the existing highway mileage on the vertical axis and the instrument on the horizontal axis and in Figure 4.3B we plotted the natural logarithm of the existing highway mileage on the vertical axis and the natural logarithm of the instrument on the horizontal axis. We also fitted a trend line in each case. The figures clearly demonstrate that a positively sloped straight line can well represent the relation between the instrument and the existing highway mileage.

4.4.3 Two Stage Least Square Estimation

Table 4.4 presents the 2SLS regression results for various linear specifications of the automobile diffusion equation. The columns of Panel A present results from the first stage and the corresponding column of Panel B presents results from the second stage. The dependent variable in the first stage is the per capita existing highway mileage in each case. The dependent variable in the second stage is the per capita (number of) registered automobiles in each case. Note that each model specification is exactly identified with one excluded explanatory variable (the per capita instrument) from the second stage regression.

The first column of Table 4.4A and Table 4.4B present the 2SLS results from a linear model specification with only one explanatory variable on the right hand side of the equation. In Panel A the coefficient of per capita instrument (existing highway miles per capita) is positive and statistically significant in the first stage. Also, the Cragg-Donald test statistic rejects the null hypothesis of weak instrument. In the second stage, the coefficient of existing highway miles per capita is positive and statistically significant. The coefficient implies an elasticity of automobile registrations per capita with respect to highway mileage per capita of 0.20.

As in the case of OLS model specifications earlier, we ran 2SLS model specification with and without the two potentially endogenous variables; auto price and gas price. We did not have any instrument for these two endogenous variables. The second column of Table 4.4A represents the first stage regression results from the linear specification without the inclusion of auto price and gas price. Correspondingly, the second column of Table 4.4B represents the second stage regression results from the same linear specifications. In the third column of either Panel, results from the linear specification of the diffusion equation with auto price and gas price on the right hand side are shown.

No matter whichever specification we use, the coefficient of the per capita instrument is positive and statistically significant in the first stage (Table 4.4A) and this indicates the validity of the instrument. The coefficient on the instrument varies from 2.58 to 3.87 indicating that a one unit increment in the instrument resulted in an increase in per capita automobile highway mileage of approximately, 2.6 to 3.9. Since we used only federal funds in determining the amount of mileage being built in the instrument, and the actual spending was much larger, it is no surprise that the coefficient is greater than one.

When we include more covariates, as expected, the coefficient of existing highway mileage reduces in margin in the second stage but remains positive and statistically significant across all specifications. For the specification without auto price and gas price, the second column of Table 4.4B indicates that a one unit increase in per capita existing highway mileage leads to approximately 1.37 unit increase in the number of per capita automobile registrations. This implies an elasticity of 0.11, so that a one percent rise in highway mileage contributed to an 0.11 percent increase in automobile registrations. For the same specification the coefficient of horse price the per capita income are all positive and statistically significant in the second stage (second column of Table 4.4B). The horse price cross-price elasticity is 0.07 while the income elasticity is 0.128.

Finally, when we include auto price and gas price in the model specification (third column of Table 4.4B), the coefficient of existing highway miles per capita further attenuates compared to the specification without auto price and gas price in the second column. The coefficient of gas price is statistically significant and negative. The elasticity implied by the coefficient implies that a one percent rise in gasoline prices reduced automobile registrations per capita by -0.25 percent. This is consistent with the economic intuition that gasoline and automobiles are complements. The horse price cross-price elasticity in the fuller specification falls slightly to 0.06, while the income elasticity implied by the coefficient falls markedly to 0.05.

In summary of the results, we find statistical evidence that by and large the expansion of existing network of roads contributed to the diffusion of automobile in the United States during the first half of the twentieth century. Owing to the lack of proper data on automobile prices across the states, we do not find any evidence that a higher auto price led to a decrease in the demand for automobile during the same period. However, we find statistical evidence that an increase in the price of gasoline resulted in a decrease in the demand for automobiles. Also, higher income states experienced higher number of automobile registrations per capita.

4.5 Conclusion and Future Work

In the current work we looked at one side of the complementarity that existed between the diffusion of automobiles and the expansion of the network of roads during the first half of the twentieth century in the United States. Perhaps the independent minded Americans had a desire for an independent mode of communication and they found an embodiment of their desire in the automobile when it came into existence. This is perhaps one of the main reasons behind the Unites States' emergence as the most automobile dependent nation on earth. The expansion of the network of roads and the multiplier effect it had on economic activities during the first half of the twentieth century had also set the platform for the diffusion of automobile.

The diffusion of automobile generated more tax and registration revenues----something that financed the expansion of the network of roads. In addition to this, perhaps the political scenario of the states played a role in such expansion. We plan to explore the causes (in addition to the diffusion of automobile) of the expansion of the network of roads in the United States in our future work. Also, we plan to create alternative instruments for the network of roads and examine whether the bi-directional linkage between the diffusion of automobiles and the expansion of the network of roads is robust to such alternative instrument specifications.

CHAPTER 5

CONCLUSION

It is commonly believed that the EPA's decision to carry out an enforcement action against a firm/facility is governed by a set of factors that typically includes the firm/facility's economic condition, its past performance, local demographic and economic characteristics. Despite allegations of political interference in EPA' activities from fringe groups, researchers have not taken up the issue; perhaps because of the intrinsic subtlety involved in such interference. In chapter two we showed that the political affiliation of the representative politician influences the level of the EPA conducted enforcement activity. As such, the models of environmental enforcement should include political factors.

The findings in chapter two indicate that a host of related issues can be explored in the future. First, the potential impact of campaign-funding by certain industrial groups on a politician's voting pattern on environmental issues in the U.S. House and Senate could further delineate the politics-environment nexus. Second, it would be interesting to analyze industry-specific political funding and the resultant industry-specific enforcement decision. Third, the extant literature has recognized the joint determination of enforcement and toxic release/pollution (i.e., endogeneity), yet the search for a proper instrument for both enforcement and toxic release is on. Chapter two indicated that the political affiliation of a representative politician can be used as an instrument for environmental enforcement in his/her jurisdiction in the release/pollution equation. Further exploration along these lines is necessary.

The second essay of the dissertation highlights the aspect of contagious preference among individuals. Given that individuals are sensitive to the perception of norms even in a one-shot anonymous interaction, it is important to investigate how norms can be altered in order to promote individual honesty in an otherwise corrupt society. This area of research offers a lot of promise in the current era of globalization with increased mobility for individuals across the globe.

If individual dishonesty is impressionable in an anonymous laboratory setup, then it offers hope in mitigating the problem of corruption in real-world individual interactions. It is therefore our future aim to design a game/framework that potentially captures the mechanism through which a prevailing corrupt/deceptive norm can be altered in a corrupt environment and propose policy implications of such alteration.

The third essay demonstrates the positive impact of road-construction in the diffusion of automobiles in the United States. Our future work will explore the reverse causation; i.e. the role played by the diffusion of automobiles in the development of roads in the United States. The complementarity between automobile-diffusion and road-expansion has important policy implications for the developing nations that do not have a properly constructed network of roads till date.

APPENDIX A: APPENDIX TO CHAPTER 2

Figure 2.1: Facility Level Yearly Average Inspection plus Enforcement in Politically Polar Areas

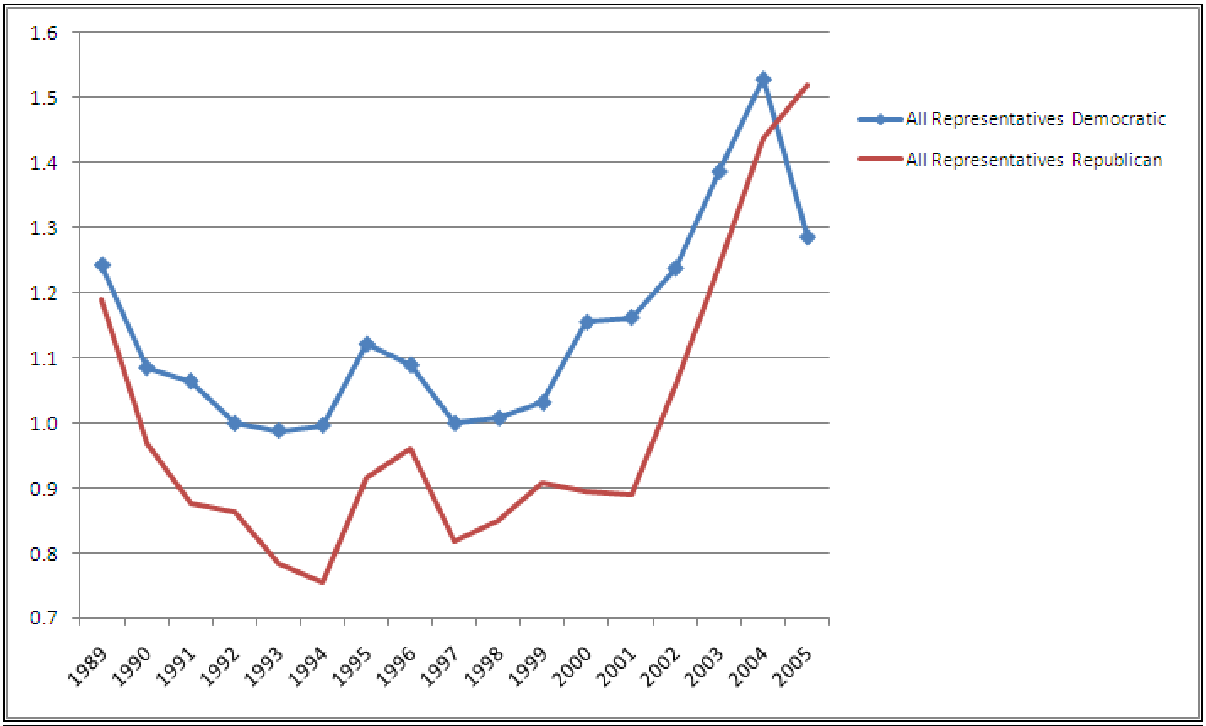

Figure 2.2: Time Trend of Facility Level Yearly Average of Toxicity Weighted Release

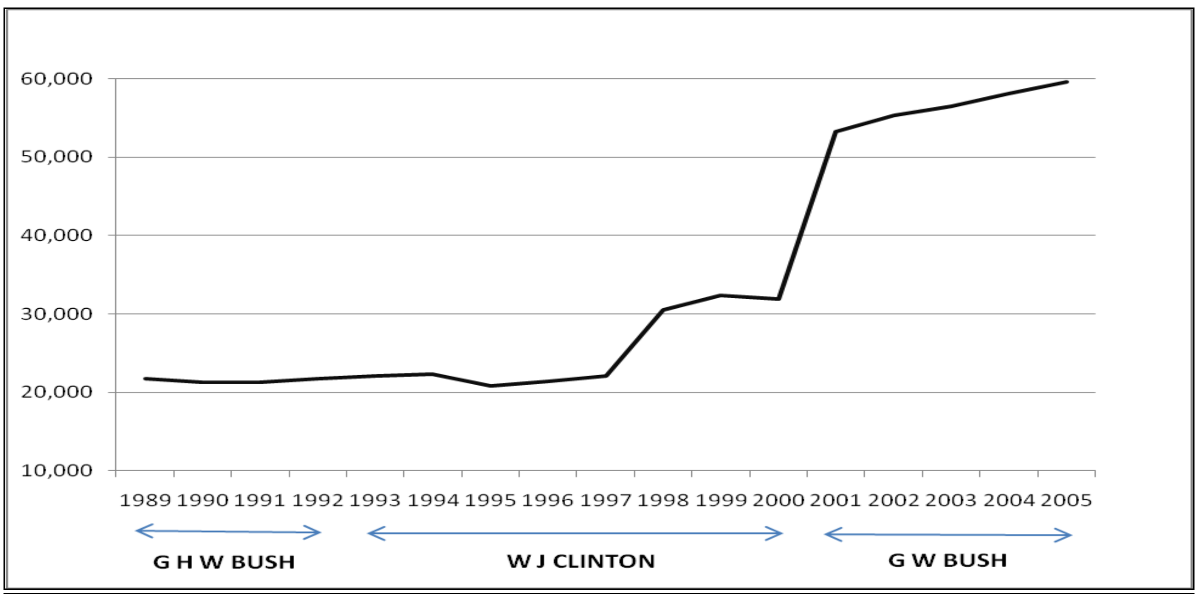

Table 2.1: Impact of a Change in Political Representation on Environmental Parameters

	Yearly Average Inspections plus Enforcements	Yearly Average Toxicity Weighted Release
Areas Represented by 3 Republican Politicians	0.999	43,800,000
The Same Areas Represented by 2 Republican Politicians & 1 Democrat Politician After ***ONE*** Year	1.024	35,000,000
Percentage Change	2.50%	–20.09%
Number of Observations	57	
Areas Represented by 3 Republican Politicians	0.985	41,900,000
The Same Areas Represented by 2 Republican Politicians & 1 Democrat Politician After ***TWO*** Years	1.016	35,400,000
Percentage Change	3.15%	–15.51%
Number of Observations	107	

Table 2.2: Facility Level Average Number of Inspections and Enforcements for Each Year

Year	Inspections		Enforcements	
	AFS-TRI Dataset	AFS Dataset	AFS-TRI Dataset	AFS Dataset
1989	0.68	1.15	0.046	0.046
1990	0.70	1.02	0.052	0.045
1991	0.70	0.91	0.044	0.043
1992	0.72	0.89	0.055	0.040
1993	0.73	0.88	0.058	0.046
1994	0.76	0.88	0.061	0.050
1995	0.88	0.93	0.064	0.043
1996	0.96	0.93	0.067	0.039
1997	1.07	0.91	0.082	0.044
1998	1.14	0.91	0.086	0.048
1999	1.25	0.95	0.094	0.048
2000	1.32	0.97	0.109	0.054
2001	1.20	0.95	0.118	0.067
2002	1.22	0.97	0.136	0.077
2003	1.32	1.11	0.156	0.084
2004	1.40	1.26	0.133	0.088
2005	1.11	1.38	0.109	0.086

Table 2.3: Description of Variables in AFS-TRI Dataset

Variable Name	Definition
Inspections	Number of yearly inspection counts for a facility
Lagged Release	Toxicity weighted average release (pounds) by facility (lagged by 1 year)
Lagged Enforcements	Binary = 1 if facility has been subjected to enforcement actions (lagged by 1 year)
Sierra	Sierra club per-capita membership in state where facility is located
Strict Liability	Binary = 1 if state has a strict liability statute
Unemployment	Annual unemployment rate in county where facility is located
Population Density	Population density in county where facility is located
PCI	Per capita yearly income in county where facility is located (in 2000 US$)
(1) Repcongdum	Binary = 1 if congressman from the area where facility is located is Republican
(2) Demcongdum	Binary = 1 if congressman from the area where facility is located is Democrat
(3) Congseniority	Seniority of congressman (years) from district where facility is located
(4) JRSD	Binary = 1 if junior senator from the state where facility is located is Republican
(5) Junsenseniority	Seniority of junior senator (years) from state where facility is located
(6) SRSD	Binary = 1 if senior senator from the state where facility is located is Republican
(7) Sensenseniority	Seniority of senior senator (years) from state where facility is located
(8) ALORSD	Binary = 1 if at least one of the two senators from facility's home state is Republican
(9) Maxrepsen	The maximum Republican seniority (years) among the two senators from the state where the facility is located; zero if both senators are Democrat
(10) ALODSD	Binary = 1 if at least one of the two senators from facility's home state is Democrat
(11) Maxdemsen	The maximum Democrat seniority (years) among the two senators from the state where the facility is located; zero if both senators are Republican
(12) Repadmindum	Binary = 1 if the Republican party is in power at the White House (yearly)
(13) RCDCS	(1) multiplied by (3)
(14) DCDCS	(2) multiplied by (3)
(15) JRSDJSS	(4) multiplied by (5)
(16) SRSDSSS	(6) multiplied by (7)
(17) ALORSDMRS	(8) multiplied by (9)
(18) ALODSDMDS	(10) multiplied by (11)
(19) RadminRepcong	(1) multiplied by (12)
(20) RadminALORSD	(8) multiplied by (12)
(21) Demmajority	Binary = 1 if the Democratic party has majority in US House (yearly), 1989-94
(22) Repmajority	Binary = 1 if the Republican party has majority in US House (yearly), 1995-2005

Table 2.4: Summary Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Inspections	1.02	2.35	0	154
Lagged Release	27094	190899	0	3.86×10^7
Sierra	0.0018	0.0016	0.0003	0.0525
Unemployment	5.51	2.00	0.90	25.80
Population Density	916	1881.95	0.04	57367
PCI	25930	6130.33	4530	83404
Congseniority	10.36	8.44	0	53
Junsenseniority	5.86	6.21	0	35
Sensenseniority	15.65	8.98	0	47
Maxrepsen	9.03	10.02	0	47
Maxdemsen	7.75	9.14	0	46

Note: Number of observations = 151,687

Table 2.5: Estimation Results of the Political Effects on Inspection (Basic Models)

	Basic Model 1 (Combined dummy for the senators)		Basic Model 2 (Separate dummy for each senator)	
Dependent Variable: Inspections	Random Effects Poisson Regression	Random Effects Negative Binomial Regression	Random Effects Poisson Regression	Random Effects Negative Binomial Regression
Independent Variables	Marginal Effect	Marginal Effect	Marginal Effect	Marginal Effect
Lagged Release	$2.01\text{x}10^{-8}$ $(1.22\text{x}10^{-8})$*	$2.90\text{x}10^{-8}$ $(1.19\text{x}10^{-8})$**	$2.26\text{x}10^{-8}$ $(1.22\text{x}10^{-8})$*	$3.05\text{x}10^{-8}$ $(1.20\text{x}10^{-8})$***
Lagged Enforcement Dummy	0.084 (0.010)***	0.129 (0.013)***	0.079 (0.010)***	0.126 (0.013)***
Sierra	–15.66 (2.66)***	–13.35 (2.94)***	–13.39 (2.58)***	–11.81 (2.88)***
Strict Liability	–0.170 (0.017)***	–0.096 (0.020)***	–0.144 (0.017)***	–0.074 (0.020)***
Unemployment	–0.003 (0.003)	–0.003 (0.003)	–0.005 (0.003)**	–0.001 (0.003)
Population Density	$5.70\text{x}10^{-6}$ $(3.97\text{x}10^{-6})$	$1.37\text{x}10^{-5}$ $(4.35\text{x}10^{-6})$***	$5.75\text{x}10^{-6}$ $(3.96\text{x}10^{-6})$	$1.53\text{x}10^{-5}$ $(4.36\text{x}10^{-6})$***
PCI	$-12.40\text{x}10^{-6}$ $(1.41\text{x}10^{-6})$***	$-12.30\text{x}10^{-6}$ $(1.44\text{x}10^{-6})$***	$-11.90\text{x}10^{-6}$ $(1.41\text{x}10^{-6})$***	$-12.30\text{x}10^{-6}$ $(1.45\text{x}10^{-6})$***
Republican Congressman Dummy	–0.047 (0.008)***	–0.042 (0.010)***	–0.046 (0.008)***	–0.040 (0.010)***
ALORSD	–0.321 (0.010)***	–0.279 (0.012)***		
Junior Republican Senator Dummy			–0.162 (0.008)***	–0.154 (0.009)***
Senior Republican Senator Dummy			–0.206 (0.010)***	–0.197 (0.014)***
Constant	1.36 (0.189)***	2.33 (0.186)***	1.27 (0.189)***	2.27 (0.186)***
State, Year, SIC dummies	YES	YES	YES	YES
Number of Facilities	17,635	17,635	17,635	17,635
Number of Observations	151,687	151,687	151,687	151,687

Note: (i) Standard errors is in parentheses, (ii) ***, ** & * respectively indicate significance at 1%, 5% & 10% level.

Table 2.6A: Robustness of the Estimates in Basic Model 1

	Robustness Check I: Using only AFS Data		Robustness Check II: Using AFS-TRI data and controlling for Committee Membership	
Dependent Variable: Inspections	Random Effects Poisson Regression	Random Effects Negative Binomial Regression	Random Effects Poisson Regression	Random Effects Negative Binomial Regression
Independent Variables	Marginal Effect	Marginal Effect	Marginal Effect	Marginal Effect
Lagged Release			2.00×10^{-8} (1.22×10^{-8})*	2.90×10^{-8} (1.19×10^{-8})**
Lagged Enforcement Dummy			0.090 (0.010)***	0.133 (0.013)***
Sierra	–8.74 (1.18)***	–7.95 (1.24)***	–18.46 (2.72)***	–16.74 (3.07)***
Strict Liability	–0.143 (0.007)***	–0.106 (0.008)***	–0.158 (0.017)***	–0.091 (0.020)***
Unemployment	–0.003 (0.001)***	–0.002 (0.001)	–0.001 (0.003)	0.004 (0.003)
Population Density	2.68×10^{-6} (1.29×10^{-6})***	1.55×10^{-6} (1.35×10^{-6})	5.54×10^{-6} (3.97×10^{-6})	14×10^{-6} (4.35×10^{-6})***
PCI	-3.37×10^{-6} (4.98×10^{-7})***	-3.70×10^{-6} (5.06×10^{-7})***	-11.8×10^{-6} (1.41×10^{-6})***	-12.4×10^{-6} (1.45×10^{-6})***
Republican Congressman Dummy	–0.034 (0.004)***	–0.028 (0.004)***	–0.025 (0.008)***	–0.019 (0.010)**
ALORSD	–0.182 (0.005)***	–0.168 (0.006)***	–0.292 (0.010)***	–0.261 (0.012)***
Committee Membership	NO	NO	YES	YES
Constant	1.17 (0.064)***	3.29 (0.064)***	1.34 (0.189)***	2.34 (0.186)***
State, Year, SIC dummies	YES	YES	YES	YES
Number of Facilities	84,101	84,101	17,635	17,635
Number of Observations	617,521	617,521	151,687	151,687

Table 2.6B: Robustness of the Estimates in Basic Model 1

	Robustness Check III: Robust Standard Errors	Robustness Check IV: Clustered-Robust Standard Errors
Dependent Variable: Inspections	Cross-Sectional Regression	Cross-Sectional Regression
Independent Variables	Marginal Effect	Marginal Effect
Lagged Release	1.02×10^{-7} (4.43×10^{-8})**	1.02×10^{-7} (6.01×10^{-8})*
Lagged Enforcement Dummy	1.04 (0.051)***	1.04 (0.085)***
Sierra	–20.12 (3.20)***	–20.12 (4.20)***
Strict Liability	–0.086 (0.024)***	–0.086 (0.047)**
Unemployment	0.002 (0.004)	0.002 (0.009)
Population Density	-6.65×10^{-6} (3.21×10^{-6})**	-6.65×10^{-6} (7.57×10^{-6})
PCI	-10.40×10^{-6} (1.48×10^{-6})***	-10.40×10^{-6} (3.64×10^{-6})***
Republican Congressman Dummy	–0.038 (0.013)***	–0.038 (0.021)*
ALORSD	–0.375 (0.019)***	–0.375 (0.055)***
Constant	3.06 (0.146)***	3.06 (0.282)***
State, Year, SIC dummies	YES	YES
Number of Observations	151,687	151,687
R^2	0.1968	0.1968
Number of Clusters		944

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Table 2.7: Estimation Results of the Effects of Seniority on Inspection

Dependent Variable: Inspections	Random Effects Poisson Regression	Cross-Sectional Regression with Clustered Robust Standard Error
Independent Variables	Marginal Effect	Marginal Effect
Lagged Release	2.02×10^{-8} (1.21×10^{-8})*	1.03×10^{-7} (6.16×10^{-8})*
Lagged Enforcement Dummy	0.083 (0.010)***	1.04 (0.085)***
Sierra	–15.74 (2.67)***	–20.16 (4.22)***
Strict Liability	–0.154 (0.017)***	–0.083 (0.037)**
Unemployment	–0.006 (0.003)**	0.002 (0.009)
Population Density	5.83×10^{-6} (3.98×10^{-6})	-6.61×10^{-6} (7.62×10^{-6})
PCI	-12.10×10^{-6} (1.41×10^{-6})***	-10.50×10^{-6} (3.75×10^{-6})***
Republican Congressman Dummy	–0.022 (0.011)**	–0.043 (0.021)**
Republican Congressman Dummy*Congressman Seniority (RCDCS)	–0.002 (0.001)***	–0.004 (0.002)*
Democrat Congressman Dummy*Congressman Seniority (DCDCS)	0.0001 (0.0006)	0.0002 (0.003)
ALORSD	–0.297 (0.010)***	–0.368 (0.054)***
ALORSDMRS	–0.005 (0.001)***	–0.004 (0.002)**
ALODSDMDS	0.003 (0.001)***	0.002 (0.001)**
Constant	1.40 (0.189)***	3.07 (0.291)***
State, Year, SIC dummies	YES	YES
Number of Facilities	17,635	
Number of Observations	151,687	151,687
R^2		0.1968
Number of Clusters		944

Table 2.8: Interactive Effects of Federal and State Politics on Inspection

Dependent Variable: Inspections	Random Effects Poisson Regression	Cross-Sectional Regression with Clustered Robust Standard Error
Independent Variables	Marginal Effect	Marginal Effect
Lagged Release	1.99x10^{-8} (1.21x10^{-8})*	1.03x10^{-7} (6.21x10^{-8})*
Lagged Enforcement Dummy	0.084 (0.010)***	1.04 (0.085)***
Sierra	–15.38 (2.66)***	–19.74 (4.18)***
Strict Liability	–0.173 (0.017)***	–0.091 (0.036)**
Unemployment	–0.003 (0.003)	0.002 (0.009)
Population Density	5.63x10^{-6} (3.96x10^{-6})	–6.56x10^{-6} (7.56x10^{-6})
PCI	–12.10x10^{-6} (1.41x10^{-6})***	–10.30x10^{-6} (3.65x10^{-6})***
Republican Administration Dummy	–0.083 (0.020)***	–0.096 (0.047)**
Republican Congressman Dummy	–0.078 (0.010)***	–0.096 (0.054)*
Republican Administration Dummy*Republican Congressman Dummy (RadminRepcong)	0.099 (0.011)***	0.112 (0.060)*
ALORSD	–0.284 (0.013)***	–0.331 (0.066)***
Republican Administration Dummy*ALORSD (RadminALORSD)	–0.060 (0.013)***	–0.075 (0.032)**
Constant	1.28 (0.189)***	3.04 (0.283)***
State, SIC dummies	YES	YES
Number of Facilities	17,635	
Number of Observations	151,687	151,687
R^2		0.1970
Number of Clusters		944

Table 2.9: Interactive Effects of U.S. Congress and State Politics on Inspection

Dependent Variable: Inspections	Random Effects Poisson Regression	Cross-Sectional Regression with Clustered Robust Standard Error
Independent Variables	Marginal Effect	Marginal Effect
Lagged Release	1.94×10^{-8} (1.23×10^{-8})	1.04×10^{-7} (6.24×10^{-8})*
Lagged Enforcement Dummy	0.085 (0.010)***	1.04 (0.085)***
Sierra	–15.79 (2.67)***	–19.80 (4.22)***
Strict Liability	–0.181 (0.017)***	–0.114 (0.059)**
Unemployment	–0.006 (0.003)**	0.0003 (0.0088)
Population Density	5.64×10^{-6} (3.95×10^{-6})	-6.45×10^{-6} (7.60×10^{-6})
PCI	-12.30×10^{-6} (1.41×10^{-6})***	-10.40×10^{-6} (3.64×10^{-6})***
Republican Congressman Dummy	NO	NO
Democratic Majority Congress Dummy*Republican Congressman Dummy	–0.134 (0.013)***	–0.105 (0.044)**
Republican Majority Congress Dummy*Republican Congressman Dummy	0.015 (0.009)*	–0.009 (0.049)
ALORSD	NO	NO
Democratic Majority Congress Dummy *ALORSD	–0.381 (0.014)***	–0.458 (0.066)***
Republican Majority Congress Dummy *ALORSD	–0.285 (0.011)***	–0.315 (0.063)***
Constant	0.921 (0.189)***	2.60 (0.281)***
State, Year, SIC dummies	YES	YES
Number of Facilities	17,635	
Number of Observations	151,687	151,687
R^2		0.1973
Number of Clusters		944

Table 2.10: Description of 4-Digit SIC Level Variables

Variable Name	Definition
Employees	Total number of employees (thousands)
R & D Intensity	Research & Development expenditure per unit of sales revenue
Age of Assets	Net assets per unit of gross asset
Herfindahl Index	Sum of squares of a company's share in sales at the 4-digit SIC level
Sales Growth Rate	Rate of growth of sales over the last year (in %)

Table 2.11: Summary Statistics of 4-Digit SIC Level Variables

Variable	Mean	Standard Deviation	Minimum	Maximum
Employees	221.13	395.39	0	3151.49
R & D Intensity	0.0184	0.1181	0	2.96
Age of Assets	0.9706	0.0750	0.3461	1
Herfindahl Index	0.2889	0.2408	0	1
Sales Growth Rate	160.30	4243.95	–59	132876.20

Note: (i) Number of observations = 67,407, (ii) The figures under the "Minimum" column are rounded off.

Table 2.12: Estimation Results of the Pollution Equation (2nd Stage)

	Dependent Variable: Toxicity Weighted Yearly Average Release	Dependent Variable: Toxicity Weighted Yearly Average Release
Independent Variables (lagged by a year)	Marginal Effect	Marginal Effect
Number of Inspections (endogenous)	–4660.45 (369.94)***	–5202.32 (652.82)***
Number of Employees (4-digit SIC level)	–1.54 (0.410)***	–1.33 (0.654)**
Research & Development Intensity (4-digit SIC level)	–118567.40 (9501.74)***	–145708.60 (9654.00)***
Age of Assets (4-digit SIC level)	8335.77 (4614.93)*	7610.33 (9582.45)
Herfindahl Index (4-digit SIC level)	–15891.45 (7090.77)**	–12508.75 (6664.00)*
Sales Growth Rate (4-digit SIC level)	0.093 (0.049)*	0.122 (0.037)***
Enforcement Dummy (2 year lagged)	12408.86 (12718.30)	11528.22 (10134.30)
Sierra	–4175.14 (205728.90)	–3398.76 (91863.90)
Strict Liability	–5134.00 (2563.26)**	–6116.73 (2918.37)**
Unemployment	157.40 (256.87)	865.54 (620.78)
Population Density	–1.56 (0.179)***	–0.978 (0.233)***
PCI	–0.447 (0.063)***	–0.274 (0.149)*
Constant	62455.19 (5135.75)***	47033.10 (27897.05)*
State, Year dummies 2-digit SIC dummies	YES NO	YES YES
Identification	Over identified	Exactly identified
Number of Observations	61,785	61,785

APPENDIX B: APPENDIX TO CHAPTER 3

Supplement to Section 3.4

Inequality Aversion and Contagion: Fehr and Schmidt (1999) assume that (i) some subjects obtain disutility when they are worse off than other subjects and also obtain disutility when they are better off, but (2) suffer more disutility when inequality is to their material disadvantage than when it is to their material advantage. Formally, consider a generalized Fehr-Schmidt subject i utility function,

$$\text{(A1)}\ U_i^*(X) = U_i(X_i) - [1/(n-1)]\{\sum_{j=1}^{i-1} \underline{V}_i(X_j - X_i) + \sum_{j=i+1}^{n} \overline{V}_i(X_i - X_j)\} + w_i(\sum_{j=1}^{n} X_j) - \delta a_i,$$

where $X = (X_1,\ldots,X_n)$ are the monetary payoffs for the reference group of $j \in \{1,\ldots,n\}$, whose payoffs (without loss) are ordered from largest to smallest, $X_1 \geq X_2 \geq\ldots\geq X_n$; the direct utility function U is increasing and weakly concave; and the inequity disutility functions $\underline{V}$ and $\bar{V}$ are increasing and weakly convex, with $\underline{V}(0) = \overline{V}(0) = 0$ and $\underline{V}(z) \geq \bar{V}(z)$ for $z > 0$ (so that a material disadvantage can yield greater disutility than a material advantage). Weak convexity of the inequity disutility function implies that the marginal disutility of inequity does not decline with the extent of inequity; that is, large inequities are not better, per unit, than small inequities. We add two components to the Fehr-Schmidt utility function, one reflecting a concern for social welfare (with w_i a non-negative constant) and the other an aversion to lying, where $\delta=0$ if agent i is honest, $\delta=1$ if agent i is dishonest, and the parameter a_i is positive. It is easily verified that, with these two additions (and the Sender caring only about his own Receiver), the Fehr-Schmidt preferences of equation (1) are consistent with Gneezy's (2005) findings.

We will assume that (1) the relevant reference group is the set of all participants in the experiment, with n a large number, and (2) a Sender subject i chooses whether to be truthful or untruthful in order to maximize his/her expected utility. Note that the reference group must include a wider population of subjects in order for others' propensity for honesty to have any impact on a Sender's choices.

We denote subject i's perceived probability that other Senders are truthful by p_S, and the probability that Receivers accept their Sender recommendations by p_R. Potential Sender payoffs for the deception game are (X_H^S, X_L^S) and potential Receiver payoffs are (X_H^R, X_L^R), where H and L denote high and low. For concreteness, we will focus on our Arizona game where X_H^S = X_H^R = X_H (=6) > X_L^S (=4) > X_L^R (=3). Finally, let B_u denote Sender i's net benefit of dishonesty (vs. honesty).

Our experimental results imply the presence of some contagion in dishonesty. Can the Fehr-Schmidt utility function, as interpreted here, explain this contagion? Specifically, does B_u *fall* when the perceived probability of honesty in the reference population (p_S) rises? Differentiating gives (see below for details):

(A2) $\partial B_u / \partial ps = (2pR - 1)^2 (1/2)[\bar{V}(X_H - X_L^R) - \bar{V}(X_H - X_L^S) - \bar{V}(X_L^S - X_L^R)] \geq 0,$

where the inequality follows from weak convexity of $\bar{V}$ (.) and $X_H > X_L^S > X_L^R$.

Hence, the Fehr-Schmidt framework does not predict the contagion in dishonesty that we observe in our Arizona experiment; in fact, it predicts the reverse!

To derive equation (A2) (dropping i subscripts for convenience), let:

p* = perceived probability that a Sender receives X_H in the reference group

$= (1 - p_S)p_R + p_S(1 - p_R)$

q* = perceived probability that a Receiver receives X_H in the reference group

$= p_S p_R + (1 - p_S)(1 - p_R)$

G_{Li} = expected utility of Sender subject i when i receives X_L^S

$$= U(X_L^S) - [(p^* + q^*)/2]\ \underline{V}(X_H - X_L^S) - [(1 - q^*)/2]\ \bar{V}(X_L^S - X_L^R)$$

G_{Hi} = expected utility of Sender subject i when i receives X_H

$$= U(X_H) - [(1 - p^*)/2]\ \bar{V}(X_H - X_L^S) - [(1 - q^*)/2]\ \bar{V}(X_H - X_L^R)$$

Hence, the net expected utility benefit of untruthful (vs. truthful) conduct is, for large n,

(A3) $B_u = (2p_R - 1)(G_{Hi} - G_{Li}) = (2p_R - 1)\{[U(X_H) - U(X_L^S)]$

$$- \ (1/2)[\bar{V}(X_H - X_L^S) - \underline{V}(X_H - X_L^S)] - [(1 - q^*)/2]\ \Delta\}$$

where we have substituted for $(p^* + q^*) = (1 - p^*) + (1 - q^*) = 1$;

(A4) $\Delta = \bar{V}(X_H - X_L^R) - \bar{V}(X_H - X_L^S) - \bar{V}(X_L^S - X_L^R) \geq 0,$

and the last inequality follows from weak convexity of $\bar{V}(.)$ and $X_H > X_L^S > X_L^R$. Differentiating gives equation (A2).

In equation (A2), we assume that when a Sender's assessment of p_S changes (with our treatments), his beliefs about p_R – the probability that Receivers accept their Sender recommendations – do not change. As noted above, this premise is consistent with our control for Sender expectations about Receiver behavior and evidence of our success with this control (Tables 3.1 and 3.4). Our conclusion here also rests on two key premises: (1) $\bar{V}$ is weakly convex; and (2) the reference group is the population of all subjects (Senders and Receivers), rather than a subset (Senders only). Reversing either of these two assumptions reverses the direction of inequality in equation (A2). Formally, if $\bar{V}$ is strictly concave, then $\Delta < 0$ in equation (A2) and hence (so long as $p_R \neq 1/2$), $\partial B_u/\partial p_S < 0$ in equation (2). Similarly, if the relevant reference group is *Senders only*, then (assuming $p_R \neq 1/2$),

$$\partial B_u/\partial p_S = (2p_R - 1)(1 - 2p_R)[\ \underline{V}(X_H - X_L^S) + \bar{V}(X_H - X_L^S)] < 0.$$

However, our premises are natural here. Even under these (plausible) premises, our observations do not imply that Fehr-Schmidt preferences cannot explain contagion in *any* deception game. In our India experiment, for example, we have

$$\partial B_u/\partial p_S \overset{s}{=} \bar{V}(100) - \bar{V}(60) - 2\bar{V}(40) - \underline{V}(40),$$

which is likely to be negative, consistent with the contagion hypothesis.

Guilt Aversion, Sender Beliefs, and Incentives for Dishonesty: Here we establish that guilt aversion implies no clear effect of Sender beliefs on a Sender's decision to be truthful or not. Let $p_S \in [0, 1]$ denote the Receiver belief about the probability that the Sender is truthful, $g(p_S; \alpha)$ the probability the Sender assigns to p_S, $p_R(p_S)$ the probability the Sender assigns to Receiver acceptance given p_S (assumed non-decreasing), and U_i the following guilt averse Sender utility function,

$$U_i = X_{Si} + \gamma \min(0, X_{Ri} - E(\tilde{X}_{Ri})),$$

where X_{Si} and X_{Ri} are Sender and Receiver payoffs, E is the Sender's belief about the Receiver's expected payoff, and $\gamma > 0$. Then the Sender's net expected benefit to an untruthful (vs. truthful) message, given payoff options (X_H^S, X_L^R) and (X_L^S, X_H^R), $X_H^S > X_L^S$, $X_H^R > X_L^R$, is

$$B_u = (X_H^S - X_L^S) \int_0^1 [2p_R(p_S) - 1] g(p_S; \alpha) dp_S$$

$$- \gamma (X_H^R - X_L^R) \{ \int_{p_s^*}^1 p_S [2p_R(p_S) - 1] g(p_S; \alpha) dp_S + \int_0^{p_s^*} (1 - p_S)[2p_R(p_S) - 1] g(p_S; \alpha) dp_S \},$$

where $p_s^* = p_S$: $p_R(p_S) >(<) (1/2)$ for $p_S >(<) p_s^*$. We assume that α shifts the p_S distribution upward in the sense of first order stochastic dominance, $G_\alpha(P_S; \alpha) < 0$ for $0 < p_S < 1$, where G is the cumulative distribution function.

Example 1: Suppose $p_R(p_S) = 1(0)$ when $p_S >(<) (1/2)$, so that the Sender expects a rational (self-interested) Receiver decision. Then $p_s^* = 1/2$ and

$$B_u = (X_H^S - X_L^S)[1 - 2G(1/2;\alpha))] - \gamma(X_H^R - X_L^R)[1 - G(1/2;\alpha) - \int_0^1 G(p_S;\alpha)dp_S].$$

Differentiating:

$$\partial B_u/\partial\alpha = -(X_H^S - X_L^S)\,2G_\alpha(1/2;\alpha) + \gamma(X_H^R - X_L^R)[G_\alpha(1/2;\alpha) + \int_0^1 G_\alpha(p_S;\alpha)dp_S].$$

The first term is positive and the second is negative. Thus, B_u can rise or fall with the Sender's assessment of Receiver beliefs about Sender propensities for truthfulness.

Example 2: Suppose $p_R(p_S) = p_R > ½$ (constant). Then

$$B_u = (X_H^S - X_L^S)[2p_R - 1] - \gamma(X_H^R - X_L^R)[2p_R - 1]\,E(p_S;\alpha),$$

where $E(p_S;\alpha) = \int_0^1 p_S g(p_S;\alpha)dp_S$. Differentiating:

$$\partial B_u/\partial\alpha = -\gamma(X_H^R - X_L^R)[2p_R - 1][\partial E(p_S;\alpha)/\partial\alpha] < 0.$$

Hence, if p_R is constant (as presumed in the discussion in the chapter), then incentives for truthful behavior rise with Sender beliefs about Receiver expectations of Sender truthfulness.

Instructions for Senders **Your Registration Number: ____________**

Introduction: Your registration number is written at the top of this page, and on the attached ribbon. This number will be used to identify you. Please tear off and keep the attached ribbon, and bring it with you to class next week for payment. We will never know the real identity of any participant in this experiment.

The Experiment: This is a short experiment in decision making.

• You will be randomly matched with another student from a different class, who we will call your "Receiver."
• Neither of you will ever know the identity of the other.
• The money that you earn will be paid to you during the last class session next week, in cash and in confidence.
• Two possible monetary payments are available to you and your Receiver in the experiment. The two payment Options are:

Option A: $6 to you and $3 to the other student (your Receiver).
Option B: $4 to you and $6 to the other student (your Receiver).

• You know what the Options are, but your Receiver **DOES NOT**.
• You will choose between **ONE** of two possible messages, which will be sent to your Receiver:

Message A: "Option A will earn you (the Receiver) more money than Option B."
Message B: "Option B will earn you (the Receiver) more money than Option A."

• Based only on your message, your Receiver will **CHOOSE ONE** of the two Options. Your Receiver's choice will determine the payments in the experiment. Receivers will never know the true Options, or the sums to be paid to you under the different Options.

• **Information from Past Sessions**:

• Out of **20 Sender messages** from past sessions of this experiment, with identical payment options,
17 out of 20 (85%) were UNTRUTHFUL, and
3 out of 20 (15%) were TRUTHFUL.

(A "TRUTHFUL" message indicates the Option that actually earns the Receiver more money.)

• In past experiments like this one, roughly **8 out of 10 Receivers chose the Option recommended by their Senders**.

• Your Receiver does not have this information.

**** I choose to send the following message (please circle ONE): ****

Message A **Message B**

(Sender Instructions continued)

To satisfy our curiosity, could you please answer the following question:

****Which option do you think your Receiver will choose based upon the message you sent? If you correctly predict your Receiver's choice, we will pay you an additional $1. Please circle ONE.****

I believe my Receiver will choose **Option A**

I believe my Receiver will choose **Option B**

Instructions for Receivers **Your Registration Number: ___________**

Introduction: Your registration number is written at the top of this page. This number will be used to identify you for payment. From now on, you should not communicate in any way with the other participants until the end of the session. If you have any question at any time, please raise your hand and one of us will help you.

The Experiment: This is a short experiment in decision making. In this experiment,

• You have been randomly matched with another student from a different class, who we will call your "Sender."

• Neither of you will ever know the identity of the other.

• The money that you earn will be paid to you after you have completed this questionnaire; payment will be private (in an envelope) and in cash.

• Two possible monetary payment options (A and B) are available to you and your Sender in the experiment. We showed the two payment options to your Sender.

• **YOU** will choose **ONE** of the two options, which will determine the payments to the two of you. The only information you will have is the message your Sender sends to you.

• Two possible messages could be sent:

Message A: "Option A will earn you more money than Option B."
Message B: "Option B will earn you more money than Option A."

• Your Sender decided to send you Message ____

• We now ask you to choose either Option A or Option B. Your choice will determine the payments in the experiment. You will never be told what sums were actually offered in the option not chosen (that is, if the message sent by your Sender was true or not). Moreover, you will never be told the sum your Sender actually receives.

We now ask you to choose ONE of the two options. **(Please circle one)**

Option A **Option B**

Figure 3.1A: Distribution of World GDP by Level of Corruption

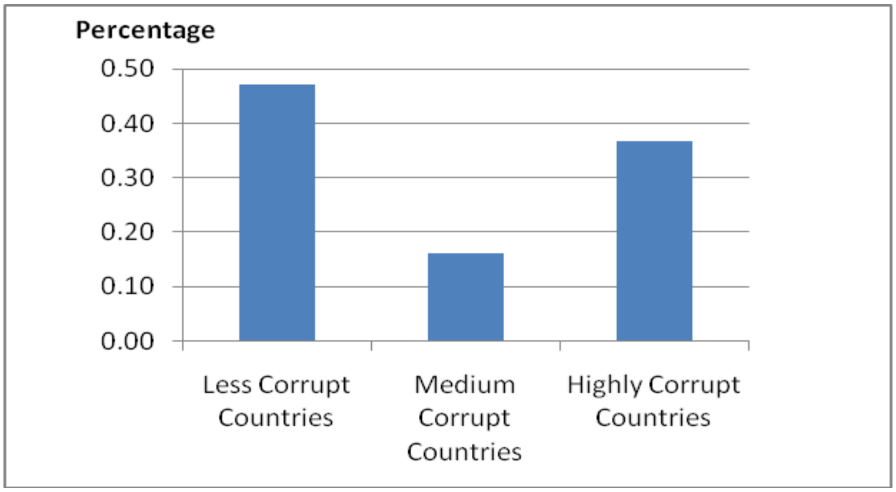
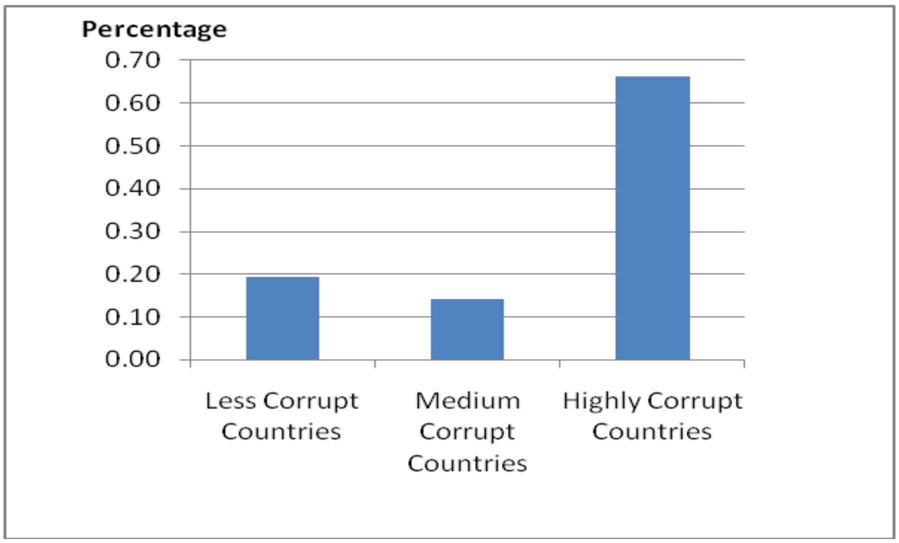


Figure 3.1B: Distribution of World Population by Level of Corruption



Note: Corruption is measured by Transparency International's Corruption Perception Index. "Less Corrupt" countries are those with CPI values in the top third of the range; "Medium Corrupt" in the middle third; and "Highly Corrupt" in the bottom third. We exclude India and China from the population distribution (Figure 3.1B), but include them in the GDP distribution (Figure 3.1A).

Table 3.1: Results of Arizona Classroom Experiment

Treatment (Reported Percentage of Untruthful Senders)	Number of Subjects	Percentage Truthful	Z-Statistic (Control-Treatment)	Percentage Predicting Receiver Acceptance
Control	97	58.8%		74.2%
Y = 15%	25	64.0%	–0.480	76.0%
Y = 40%	26	53.8%	0.455	80.8%
Y = 60%	33	54.5%	0.430	63.6%
Y = 85%	52	19.2%	5.349***	73.1%
Overall	233	49.3%		73.4%

Note: *** denotes significant at 1% level (two-sided).

Table 3.2: Probit Regression of Arizona Sender Message Choices with Course Fixed Effects

Dependent Variable: Sender Message Choice (Truthful = 1 & Untruthful = 0)	Coefficient	t-Statistic	Marginal Effect	t-Statistic
Constant	0.7004	2.06**		
Y = 15% Treatment	0.0206	0.06	0.0073	0.06
Y = 40% Treatment	–0.5554	–1.44	–0.1873	–1.58
Y = 60% Treatment	–0.4358	–1.17	–0.1480	–1.25
Y = 85% Treatment	–1.0385	–3.92***	–0.3703	–4.58***

Note: N=233. ** denotes significant at 5% level (two-sided). *** denotes significant at 1% level. Dependent variable: Sender message choice (Truthful=1, Untruthful=0). We report average marginal effects. The fixed course effects are jointly insignificant, with χ^2 (df=5) test statistic (p-value) 7.43 (0.1904); however, one course effect is individually significant.

Table 3.3: Arizona Survey Results

	Number of Subjects	Percentage Truthful	Z-Statistic (Control-Treatment)	Z-Statistic (Truthful-Untruthful)
Control	43	69.8%		
Truthful	63	79.4%	–1.108	
Untruthful	68	50.0%	2.134**	3.707***

Note: *** denotes significant at 1% level and ** denotes significant at 5% level.

Table 3.4: Results of Calcutta Laboratory Experiment

Treatment (Reported Percentage of Untruthful Senders)	Number of Subjects	Percentage Truthful	Z-Statistic (Control-Treatment)	Percentage Predicting Receiver Acceptance
Control	29	44.8%		82.7%
Y = 15%	31	67.7%	–1.836*	74.2%
Overall	60	56.6%		78.3%

Note: * denotes significant at 10% level (two-sided)

Table 3.5: Probit Regression of Arizona and Calcutta Sender Message Choices with Belief

	Dependent Variable: Sender Message Choice (Truthful = 1 & Untruthful = 0)	Coefficient	t-Statistic	Marginal Effect	t-Statistic
Arizona Subjects	Constant	–0.0215	–0.04		
	Y = 85% Treatment	–0.9275	–2.43**	–0.3396	–2.55**
	Sender Belief	0.0034	0.45	0.0012	0.45
Calcutta Subjects	Constant	–0.0980	–0.15		
	Y = 15% Treatment	0.5927	1.77*	0.2299	1.82*
	Sender Belief	–0.0005	–0.05	–0.0002	–0.05

Note: ** denotes significant at 5% level. * denotes significant at 10% level.
N (for Arizona) = 57 (common course), N (for Calcutta) = 60. Sender beliefs are each Sender's prediction of the Receiver prediction of the proportion of truthful Senders.

APPENDIX C: APPENDIX TO CHAPTER 4

Figure 4.1: The Diffusion of Automobiles and Construction of Road Network

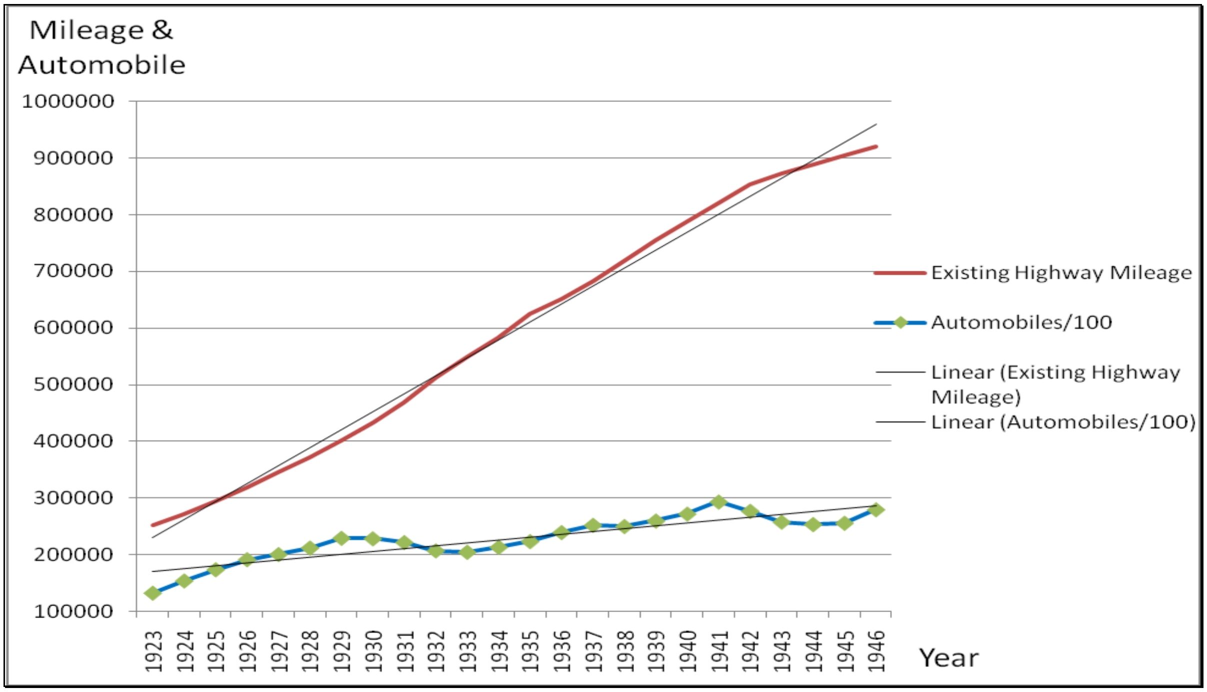


Figure 4.2: Highway Expenditure and Motor Vehicle Taxes

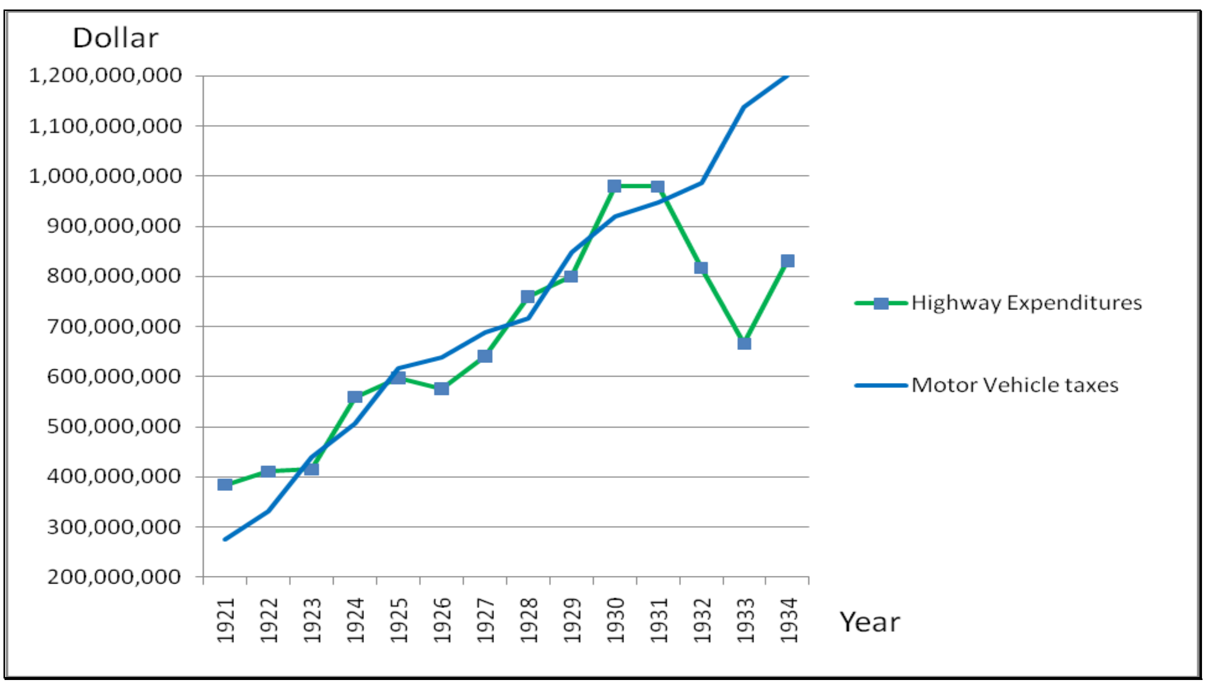


Figure 4.3A: The Existing Highway Mileage and Its Railroad Instrument

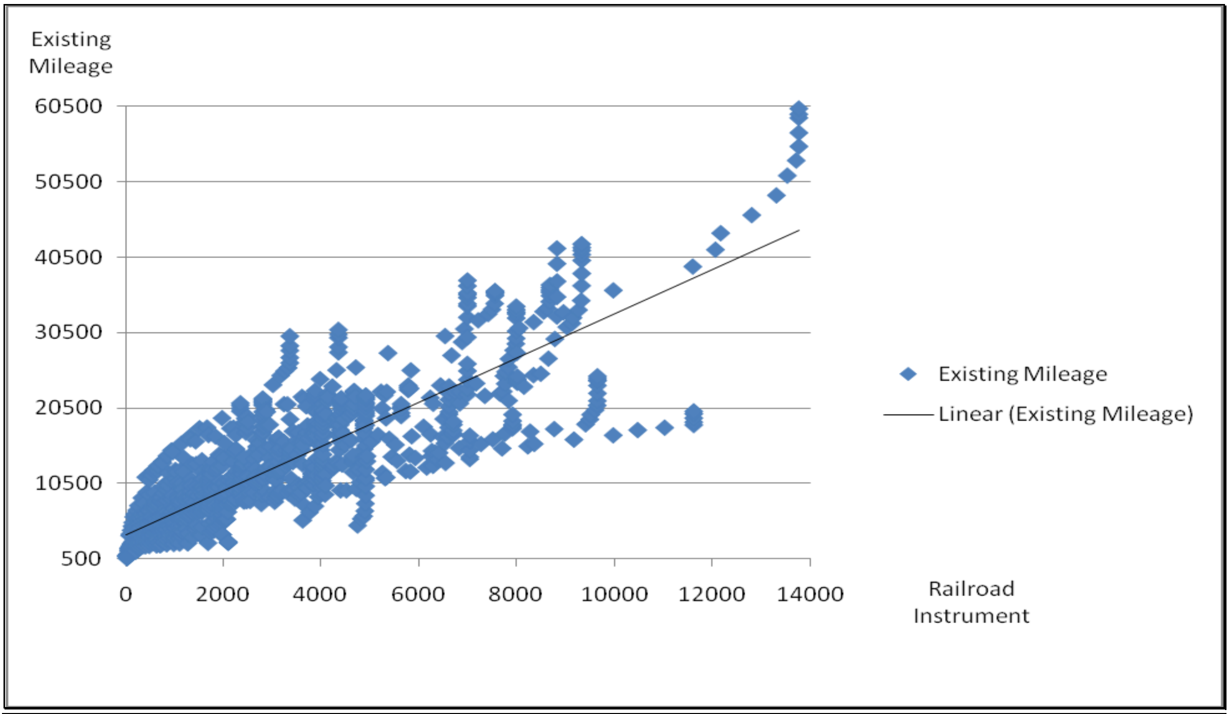


Figure 4.3B: The Log of Existing Highway Mileage and Log of Railroad Instrument

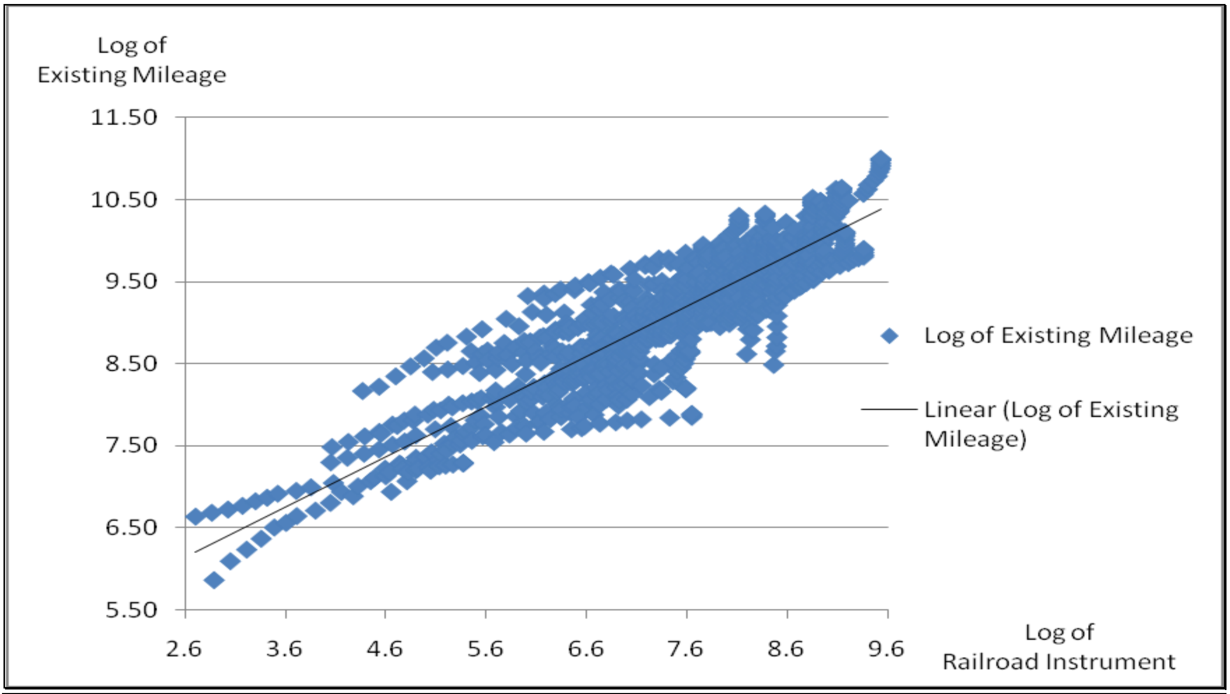


Table 4.1: Leading Automobile Dependent Countries of the World in 1935

Country	Passenger Cars	Buses	Trucks	Diesel Powered	Total Registration	Total Population	Vehicle Per 100 Persons
United State	22,455,638	115,479	3,649,935	.	26,221,052	127,521,000	20.58
Hawaiian Islands	41,300	406	9,543	.	51,249	368,336	13.91
New Zealand	149,112	559	41,205	19	190,895	1,557,043	12.25
Canada	989,963	1,892	171,001	92	1,162,948	10,835,000	10.73
Australia	473,992	.	157,862	.	631,854	6,706,000	9.43
Monaco	1,327	100	180	.	1,607	22,153	7.25
Gibraltar	905	53	135	.	1,093	17,405	6.28
Alaska	2,295	30	1,209	.	3,534	59,278	5.96
France	1,565,000	38,000	450,000	12,200	2,065,200	41,940,000	4.92
United Kingdom	1,554,155	41,367	436,814	11,115	2,043,451	46,755,000	4.37
Luxembourg	6,880	180	3,977	.	11,037	303,000	3.64
Denmark	92,300	1,600	37,700	11	131,611	3,656,000	3.60
Union of South Africa	212,767	1,184	27,694	60	241,705	8,488,000	2.85
Sweden	109,096	3,914	44,893	1,180	159,083	6,233,090	2.55
Uruguay	36,041	1,032	11,254	33	48,360	1,993,000	2.43
Norway	36,190	2,510	24,410	.	63,110	2,871,000	2.20
Switzerland	70,200	1,220	17,600	1,070	90,090	4,145,000	2.17
Argentina	182,000	7,850	57,600	330	247,780	12,164,000	2.04
Rest of the World Excluding USA	8,081,729	269,355	2,613,407	50,444	11,014,935	1,904,097,332	0.58

Source: Petroleum Facts and Figures, Fifth Edition, 1937, Page 17-19

Table 4.2: Summary Statistics

Variable	Period	Number of Observations	Mean	Standard Deviation	Minimum	Maximum
Automobile	1900-1946	2,256	267,925	425,140	10	2,661,638
Automobile Per Capita	1900-1946	2,256	0.107	0.094	0.000013	0.361
Highway Mileage	1923-1946	1,152	12,399	9,367	351	60,137
Highway Mileage Per capita	1923-1946	1,152	0.0093	0.011	0.00030	0.0723
Car Price (1926 $)	1926-1946	1,008	891.32	75.71	694.45	1081.24
Gasoline Price (cents/gallon, 1926 $)	1927-1946	960	22.59	3.47	13.97	34.72
Horse Price (1926 $)	1900-1946	2,256	86.74	33.17	16	209
Total Personal Income (1967 $)	1919-1946	1,344	3,540,000,000	4,920,000,000	99,100,000	36,100,000,000
Per Capita Personal Income (1967 $)	1919-1946	1,344	1,243.92	541.74	300.15	3,647.86
Population	1900-1946	2,256	2,283,964	2,272,425	43,000	13,500,000

Note: The variable 'Automobile' stands for the number of registered motor vehicles in a state.

Table 4.3: OLS Estimation Results of Automobile Diffusion in Linear Form

Dependent Variable: automobiles per capita	OLS	OLS without Auto Price & Gas Price	OLS with All Exogenous Variables
existing highway miles per capita	1.864 (0.105)***	0.625 (0.136)***	0.619 (0.157)***
auto price		NO	0.00078 (0.00013)***
gas price		NO	–0.0013 (0.00023)***
horse price		0.000054 (0.000026)**	0.000074 (0.000024)***
per capita income		0.000010 (0.0000041)**	0.00000436 (0.00000379)
state dummies		YES	YES
year dummies		YES	YES
N	1,152	1,152	960
R^2	0.12	0.95	0.96

Note: Standard errors are in parenthesis. *** implies significant at 1% level, ** implies significant at 5% level.

Table 4.4A: First Stage of 2SLS Estimation Results of Automobile Diffusion in Linear Form

Dependent Variable: automobiles per capita	1st Stage	1st Stage without Auto Price & Gas Price	1st Stage with All Exogenous Variables
existing highway miles per capita	3.866 (0.050)***	2.575 (0.096)***	2.622 (0.122)***
auto price		NO	–0.000071 (0.000023)***
gas price		NO	$-5.09\text{x}10^{-6}$ (0.000045)
horse price		–0.000020 $(5.41\text{x}10^{-6})$***	–0.000018 $(5.09\text{x}10^{-6})$***
per capita income		$8.66\text{x}10^{-8}$ $(5.72\text{x}10^{-7})$	$5.78\text{x}10^{-8}$ $(5.82\text{x}10^{-7})$
state dummies		YES	YES
year dummies		YES	YES
N	1,152	1,152	960
R^2	0.84	0.95	0.97
F-stat on instrument	1558.47	279.03	168.57
Cragg-Donald stat	6033.06	712.23	460.99

Table 4.4B: Second Stage of 2SLS Estimation Results of Automobile Diffusion in Linear Form

Dependent Variable: automobiles per capita	2nd Stage	2nd Stage without Auto Price & Gas Price	2nd Stage with All Exogenous Variables
existing highway miles per capita (instrumented)	2.335 (0.115)***	1.371 (0.185)***	0.722 (0.269)***
auto price		NO	0.00077 (0.00012)***
gas price		NO	–0.0012 (0.00022)***
horse price		0.000085 (0.00003)***	0.000077 (0.000024)***
per capita income		0.000011 $(4.02\text{x}10^{-6})$***	$4.42\text{x}10^{-6}$ $(3.64\text{x}10^{-6})$
state dummies		YES	YES
year dummies		YES	YES
N	1,152	1,152	960
R^2	0.12	0.95	0.96

Note: Standard errors are in parenthesis. *** implies significant at 1% level.

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