EPA United States Environmental Protection Agency

Benefits of Environmental Information Disclosure Proceedings

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U.S. Environmental Protection Agency
Benefits of Environmental Information Disclosure Meeting
Hyatt Regency Crystal City
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Executive Summary

OVERVIEW

The overarching theme of the *Benefits of Environmental Information Disclosure* meeting was how to improve or create effective information disclosure policies in the context of government-based environmental programs. Ongoing research was presented in a broad array of fields including: environmental labels, voluntary reporting, consumer willingness to pay, "greenwash," and audits.

WELCOME AND INTRODUCTION
Will Wheeler, EPA, National Center for Environmental Economics

Will Wheeler welcomed everyone and apologized for the late start. Many participants were delayed because of weather, but he anticipated they would be joining the meeting later in the day.

THE EFFECTIVENESS OF ENERGY EFFICIENCY LABELS
Session Moderator: Ann Wolverton, EPA, National Center for Environmental Economics

Consumer Willingness-to-Pay for Energy Star and Green Power Labeled Refrigerators
Christopher Clark, University of Tennessee

In the United States, environmental labeling programs usually identify both a public and a private benefit for making a particular consumer choice. Does this make sense from a policy point of view? Providing both public and private benefits could lead to more energy efficient choices, but small extrinsic rewards can sometimes "crowd out" intrinsic motivation (e.g., financial incentives for blood donors actually decreases the number of people willing to donate blood). This study analyzed consumer willingness to pay (WTP) for an environmental attribute (energy efficiency) in refrigerators based on the Energy Star label (public and private benefits), Green Power Partners (public benefits only), and Climate Leaders Program (public benefits only). Using a Knowledge Networks survey panel, participants answered questions about choosing a refrigerator when presented with one of the four following scenarios: Energy Star label; Energy Star label plus a mail-in rebate for $50; Green Power Partnership label; or Climate

Leaders Program label. The study concluded that consumers are willing to pay a premium for refrigerators that have been awarded a label by Energy Star, the Green Power Partnership, or the Climate Leadership Program. Consumer WTP is highest for Energy Star because the program: (1) is familiar to consumers; (2) is focused on consumer labeling; (3) has more explicit benefits; and (4) identifies public and private benefits. There was little or no evidence of "crowding out," but offering a rebate appeared to lower a buyer's perception of the product's quality, and thus their WTP.

Evaluating Alternative Approaches to Energy Efficiency Labeling: Designing and Implementing a Choice Experiment
Juha Siikamaki, Resources for the Future

In the United States, household valuation of energy efficiency is usually low. Possible explanations for this observation include: information problems, principal/agent problems, unobserved costs, costs of obtaining information, discount rates, and heterogeneity. Labeling programs are one option to address information barriers (and costs). Labels vary in the amount of information provided; for example, Energy Star is an endorsement and is triggered when a threshold of energy efficiency is reached, whereas Energy Guide presents the performance of the appliance relative to similar models (e.g., estimates of kWh used per year, how the appliance compares to other appliances). This study: (1) evaluates alternative labeling approaches in the context of household preferences for energy efficiency; and (2) disentangles the effects of different drivers of valuation of energy efficiency (e.g., discount rates, credit constraints, likelihood of moving). Using a Knowledge Networks computerized survey panel, participants were asked a number of questions about their preferences in water heaters. Then they chose a water heater to purchase after viewing one of the following five labeling treatments: (1) Energy Guide information; (2) Simplified Energy Guide (no range); (3) Energy Guide information plus CO_2; (4) EU-style grade; or (5) Energy Guide information plus Energy Star (multiple labels). Follow-up questions addressed their WTP for energy efficiency, payback time, individual discount rates, and current credit status. Preliminary results indicate:

- Consumers expect a 4-5 year period for payback;
- Households are willing to pay $45-$54 dollars up front for an expected $10 annual reduction in energy cost;
- Individual discount rates had a mean of 19 percent and a median of 11 percent; and
- Consumer WTP is highest for the EU label and lowest for the Simplified Energy Guide.

The next steps are to:

- Complete the survey and analyze the results;
- Thoroughly examine heterogeneity;
- Estimate a panel model;

- Examine WTP for energy efficiency in terms of discounting, credit constraints, likelihood of moving, and unobserved preference heterogeneity; and
- Evaluate policy implications of labeling practices.

Discussant
Chris Moore, EPA, National Center for Environmental Economics

Overall, the Clark paper was well organized and presented a neat and clean analysis of estimating WTP for the Energy Star label and inferring WTP for public benefits (beyond private cost savings). The discussion of the results and interpretation of interaction terms was coherent. The experimental design and econometric specification were good. The paper could be improved by: (1) discussing the implications of the results regarding alternative approaches to capturing preference heterogeneity; (2) providing a practical rather than theoretical justification for holding coefficients fixed in the RP model; (3) considering alternative specifications for attitudinal variables; (4) including an image of the design question; and (5) comparing results with other studies.

The Siikamaki paper is a more ambitious study of labeling that attempts to determine how people respond to different presentations of information and tease out factors contributing to the "efficiency gap". The energy labels used in the study are familiar to many people, thus it is very valuable to compare WTP across those treatments. The study takes a good approach by emphasizing relative values rather than dollar amounts when comparing WTP across treatments. The label with the footprint (Energy Guide information plus CO_2) had a high WTP. Could an emotional response to the footprint image be the driver? Or are people actually looking at the numbers and thinking about them?

Discussant
Maureen McNamara, EPA, Climate Protection Partnerships Division

Energy Star has about 1,500 retailers and 3,000 manufacturing partners and 60 different product categories (e.g., laptops, large appliances). An Energy Star label does not imply a price premium for efficiency; most products receiving the label have little or no difference in price compared with non-Energy Star products. Manufacturers have coupled Energy Star with other premium features. There is a perceived value of quality that comes into play with large appliances and this may contribute to consumer WTP.

Maureen McNamara is managing a national analysis of Energy Star awareness and use in the marketplace that draws upon the same Knowledge Network Panel as both of the studies within this session. The Knowledge Network Panel is widely used and provides consistent results; however, there is a concern that the panel's recruitment and refreshment rate may not be frequent enough to keep up with its use. People may "learn" because they have taken part in multiple surveys. Ms. McNamara encouraged both speakers to look at 10-12 year datasets.

The energy industry is engaged in providing financial incentives to consumers and there is a lot of interest in the effects and optimal balance of incentives. The most recent data show that 80% of consumers are aware of Energy Star. Each year, a survey is conducted to document how many consumers have bought an Energy Star product. Last year, there was a huge jump in the number of consumers saying a financial incentive was important in their decision. It is also important to look at geographical effects. Some parts of the country have been promoting Energy Star for longer or placing more emphasis on the program.

Questions and Discussion

A participant said water heater capacity and length of warranty were not included in the Siikamaki study, yet these are important factors to consumers. Dr. Siikamaki replied that water heater capacity was included and held constant in the survey choices. Warranty was not included, but it could be important to consider. Another participant asked about the observation that both studies showed people were willing to irrationally pay too much for energy efficiency (opposite of "energy efficiency gap"). Dr. Siikamaki suggested that the study might be showing a hypothetically inflated high WTP and that a field experiment could be used to validate the result. He also acknowledged the concerns about using the Knowledge Network Panel for many studies.

One participant expressed doubt about the ability of consumers to "do the math" and suggested including price and kWh in the paper. Dr. Siikamaki agreed this was a good suggestion to differentiate between the effects of the labeling versus consumer math deficits. Following a similar line of reasoning, he explained that he built the "Warm Glow" into his study to determine if the magnitude of CO_2 (Energy Guide information plus CO_2 treatment) is affecting consumers' decisions or if it is the image of the footprint. Dr. Clark postulated that WTP could be increased by combining the iconic Energy Star label with Energy Guide. This may cause consumers to think about the environment more or decide on a product because it is endorsed by Energy Star (must be the right choice).

Another participant asked if Energy Star labels (or others) are perceived as accurate. Does this influence what people are willing to pay when they consider their actual energy usage (e.g., a family of four uses more hot water than a single person)? Dr. Siikamaki asserted that the accuracy and quantity of household energy use estimates is a valid concern for consumers. His survey relies on a detailed questionnaire to collect information about the household and then uses that information to create the cost estimate on the labels in the choice questions. Consumers who are only willing to pay a little or nothing for energy efficiency usually claim they cannot afford to pay a higher price for the appliance or they are unsure of the label's accuracy.

A participant commented that for the Energy Star label, you are labeled as the most efficient product within a certain category, but not necessarily the most efficient overall. Did the surveys take this into account? Dr. Clark responded this had not been analyzed yet.

WHAT CAUSES REDUCTIONS IN TOXIC RELEASE INVENTORY (TRI) EMISSIONS?

Session Moderator: Charles Griffiths, EPA, National Center for Environmental Economics

The Impact of Quasi-Regulatory Mechanisms on Polluting Behavior: Evidence From Pollution Prevention Programs and Toxic Releases

Linda Bui, Brandeis University

Quasi-regulatory mechanisms, such as voluntary programs, are often believed to be more efficient than traditional regulatory mechanisms. Polluters may respond well to these mechanisms because quasi-regulatory mechanisms can lead to lower abatement costs by providing or lowering the cost of information and/or more formal regulations may be applied in the future if polluters do not respond by voluntarily abating. There is little empirical evidence to support either of these hypotheses. This study addresses the question, "Do quasi-regulatory mechanisms lead to lower levels of pollution?" by examining pollution prevention (P2) programs aimed at toxic releases, analyzing their effectiveness at eliciting a polluter response, and looking for evidence to explain why polluters respond to quasi-regulatory mechanisms. Drawing from the TRI, event study methodology was used to estimate the average effect of federal- and state-level P2 programs on toxic releases using facility data from 1988 to 2003. The following factors also were considered in the analyses: facilities located in "early" or "late" P2 adopting states, facilities in "low" or "high" stringency states, the distribution of facilities in "low" or "high" stringency states, and confounding effects of the 1990 Clean Air Act Amendment. The results indicated that: (1) the adoption of P2 programs can affect facility-level toxic releases (decrease of 3 to 9 percent over time); (2) spill-over effects may play an important role in the effectiveness of P2 programs with later adopters benefiting from the information and experience gained by earlier adopters; and (3) P2 programs become less effective over time.

Discussant
Ann Wolverton, EPA, National Center for Environmental Economics

The Bui paper examines the effects of state-level P2 programs on toxic releases. It finds that state P2 programs have a significant effect on emissions over time, early adopter states exhibit larger average reductions, P2 programs providing technical assistance and education are related to the largest reductions, filing fees appear to increase reported releases, and non-reporting penalties only matter for a subset of TRI releases. From a policy perspective, voluntary or partnership programs are often put in place because of a lack of authority to regulate a pollutant. The Bui paper demonstrates that some of these programs do result in real, measurable environmental benefits. Reductions are usually modest; it could be worthwhile to compare emissions reductions in a voluntary program with federal or state mandated programs.

The paper could be improved by:

- Examining other possible reasons for TRI reductions over time that are correlated with P2 programs (e.g., changes in TRI reporting methodology, advent/disappearance of regulatory threats);
- Using TRI to validate moves toward "green" practices;
- Differentiating between firms that export or import the chemical for use or sales versus those that produce it as a byproduct;
- Expanding the analyses to include data beyond 2003;
- Incorporating differences in the stringency of P2 programs as a continuous variable; and
- Considering air emissions only and weighting by toxicity.

Discussant
Sheila Olmstead, Resources for the Future

Before the Bui study, it was known that firms experienced abnormal negative downturns in their stock value immediately after the first release of TRI information in 1989. The consequence was a reduction in onsite releases, but increased offsite waste transfers. Firms with the largest stock price impact reduced their emissions more than their industry peers. Some reductions in toxic emissions may be caused by other events (e.g., command and control regulation of non-toxic pollutants). Also, it was known that state adoption of voluntary P2 programs might decrease Resource Conservation and Recovery (RCRA) violations, reduce total toxic releases, and increase source-reduction activities.

The Bui paper could be improved by:

- Paying careful attention to endogeneity;
- Dropping the falsification test from the paper;
- In the discussion, separating P2 programs that increase non-reporting penalties from programs that increase filing fees;
- Revising the discussion of the emissions response of high stringency states;
- Separately identifying year dummies and PPA_t in Eq (1);
- Explaining why a natural log is used for the dependent variable; and
- Discussing Table 4 in the text of the paper.

Questions and Discussion

Dr. Bui agreed that endogeneity is an issue. With access to census data, the study might consider only the last 10 years. Using categories (early/late, low/high) has hopefully eliminated many of the endogeneity issues. Sorting out what is going on with high stringency states is harder. It is useful to think about the size of emissions reductions and the associated costs to both polluters and regulators. During the last 16 years, the methodology used for reporting TRI estimates has changed and turnover rates for TRI reporting staff at companies is high. This calls into question the quality of the data, although the analyses indicate it to be relatively robust. More importantly, are the P2 programs changing reporting behavior or polluting behavior? There are TRI data that could answer this question, but it would be difficult to incorporate into the current analyses. The high stringency data will probably be dropped from the analysis. Releases in air, water, and land have been separately considered; analyses found similar results in air and land, but water was unusual. Weighting by toxicity has not been tried.

A participant asked if there was a correlation between participating in a voluntary program and the size or international holdings of the company. Dr. Bui acknowledged that it would be useful to control for facility information. She did examine the percentage of small manufacturing facilities within a state by two-digit Standard Industrial Classification (SIG) code and found the distribution of large and small facilities did not change much from year to year. On that basis, the changes in TRI releases per year are not being driven by changes in the size of manufacturing facilities. She did not consider companies with international holdings.

Another question was asked about controlling for manufacturing output. Dr. Bui responded that it is important to look at emissions per value output. TRI has a ratio production number, but it has huge variation and is not ideal for analyses. She normalized the data by value-added at the state level, but had problems with endogeneity. With facility information, it would be possible to control for manufacturing output.

One participant pointed out that P2 programs are less effective over time and asked if setting a sunset date for a program might influence participation. Dr. Bui was not aware of any programs disappearing over time. This could be an interesting policy to consider.

INFORMATION, AUDITS, AND ENFORCEMENT
Session Moderator: Patrick Walsh, EPA, National Center for Environmental Economics

Regulatory Enforcement With Dynamic Targeted Audit Mechanisms
Christian Vossler, University of Tennessee

In industry, there are usually high rates of regulatory compliance despite small fines for violations and a low threat of being audited. In theory, this creates two groups: "targeted," firms that have not been audited recently or who have failed a recent audit and "untargeted," recently audited firms with passing grades. This study considers how compliance may be induced through strategic interactions. Two models and a control (Random Audit) were experimentally tested. In the Dynamic Tournament model, "targeted" (audited) firms reporting the most emissions (relative to the actual emissions) are placed in the "untargeted" group; "untargeted" firms reporting the lowest emissions are transitioned into the "targeted" group. Firms now compete against each other to remain in the "untargeted" group. In the Dynamic Standards model, transitioning between the "targeted" and "untargeted" group depends on the audit probability, distribution of audit errors, and position of standards.

The results for the Random Audit showed that:
- The disclosure of emissions was much higher than theory would predict, especially in the "targeted" group;
- There was no audit cost effect; and
- Increasing the audit probabilities increases emissions disclosure.

The results for the Dynamic Standards indicated:
- Disclosure of emissions was usually higher than expected by theory without much difference between the "targeted" and "untargeted" groups;
- Increasing the audit cost did not increase emissions disclosure in the "targeted" group;
- Increasing the audit probabilities did not affect emissions disclosures;
- Increasing the transition probabilities did not decrease the emissions disclosures in both groups; and
- Targeted audits lead to higher disclosure rates.

The results for the Dynamic Tournament indicated:

- Disclosure of emissions was usually higher than expected by theory with higher disclosure rates in the "targeted" group;
- In "targeted" groups, increasing the audit cost increases the emissions disclosure;
- Increasing the audit probabilities increases emissions disclosure;
- Increasing the transition probabilities decreases the emissions disclosures in both groups; and
- Targeted audits lead to higher disclosure rates.

Overall, the study found that the Tournament and Standard mechanisms lead to identical disclosure rates. Applying the results to policy, competition to avoid audits could be beneficial to the regulatory community; firms pay attention to their competitors' behavior and potentially make similar changes in their own behavior.

Strategic Environmental Disclosure: Evidence From DOE's Voluntary Greenhouse Gas Registry
Thomas Lyon, University of Michigan

There is growing interest in using information disclosure as a policy tool. Mandatory disclosure has a modest, but significant, effect on reducing emissions; less is known about voluntary programs. Section 1605(b) of the Energy Policy Act of 1992 mandates a registry of greenhouse gas emissions and is managed by the Department of Energy (DOE). Reporting is such that companies have great flexibility in what and how they report. Using data from the greenhouse gas registry, this study investigated the participation of "clean" or "dirty" firms in the program, motivations behind participation (e.g., market opportunities, political pressure), and the effects of participation on environmental performance. A model was constructed around the premise that electric utilities may receive greenhouse gas early reduction credits (ERC) for voluntarily participating in the program. The study considered the effects of political pressures and nongovernmental organization (NGO) monitoring of "greenwash" (selectively reporting favorable data). The results indicated that: (1) all firms reported reductions in emissions but emissions were actually increasing (although at a low rate); (2) all of the firms that were not part of the program did reduce their emissions; (3) bigger emitters tend to participate in the program because they face less pressure from environmentalists (the Sierra Club negatively labeled the program as "greenwash"), face more enforcement actions, and have more to gain from ERC; and 4) firms in states with renewable energy portfolios were less likely to participate as there was no incentive to participate to stave off mandatory regulation. In terms of policy implications, information disclosure programs need to account for the tendency of firms to engage in selective disclosure. Mandating the disclosure of all relevant information, especially negative effects, should be considered as one step towards improving emissions reporting. DOE has made revisions to 1605(b) and now requires entity-wide reporting, removing the option of reporting on selective projects.

Competing Environmental Labels
Carolyn Fischer, Resources for the Future

The globalization of trade and environmental issues, and trade law restrictions makes it difficult for governments to regulate production processes outside their borders. Many groups advocate the use of market mechanisms, such as eco-labeling, to address this challenge. This study examines: (1) the incentives and behavior of industry groups versus NGOs in setting eco-label standards; (2) the effectiveness of multiple (competing) labels; and (3) the role of government in third-party voluntary labeling schemes. The study develops a formal model of rivalry between NGO and industry-sponsored labels, while considering consumer WTP for the product. The motivation behind creating the labels is different: NGOs want to minimize environmental damage while industry wants to maximize profits. The results for the single label model show that the NGO sets a higher standard than industry. In a multiple label scenario, industry lowers the standards on its label (assuming the NGO standard is higher). Industry profits increase in a multiple label setting because industry only takes actions (i.e., participates in voluntary labeling, lowers labeling standards) that will increase profits. In contrast, NGOs restrict or relax standards on a label in response to the industry label, resulting in higher or lower environmental damages depending on the distribution of firm types in the market and consumer demand for label stringency. The NGO loses a substantial amount of participation in its label when an industry label is present. The study concludes that finding a balance between NGO and industry standards and encouraging the two entities to work together instead of developing competing labels optimizes the conditions for developing effective environmental labels.

Discussant
Jon Silberman, EPA, Office of Enforcement and Compliance Assurance

Models simplify reality but must capture enough reality to be useful. Models can be improved by using real data in their design or calibrating them with actual data. The three models presented in this session would benefit from more of this. EPA is trying to improve existing information disclosure programs rather than create new ones. EPA's enforcement/inspection approach is limited because the Agency cannot inspect everyone, so how can information be leveraged effectively? Some options include building self-monitoring and reporting requirements directly into rules, making better use of e-technology to transmit data directly, and making information publicly available and easy to understand.

Overall, the authors of the papers in this session need to consider what they want to accomplish with their work and their audience. If policy makers are the target, publishing easily understandable (plain English) companion papers should be considered. Including clear examples of practical implications of the models will improve the effectiveness of its message.

The Vossler study could benefit from the addition of practical recommendations on how to improve auditing. The idea of creating a tournament where firms compete to avoid being targeted is interesting. EPA's experience suggests that this works, as long as the rules are not too transparent. Deciding who to target varies on the situation. Firm size and emissions are often two important factors.

The Fischer study's model could be of best practical use to EPA where voluntary and industry labels exist and the Agency wants to understand the pros and cons of supporting one or the other. It also may be useful for determining if policing the completeness or accuracy of a label's content is worthwhile. Label standards are an important consideration and were not addressed in this study. The model assumptions should be revisited.

Regarding the Lyon study and "greenwash," if it is important that the public receive accurate information, then mandatory reporting is necessary; however, it may not always be the best option. Environmental Management Systems also can be used to "greenwash."

Regulatory compliance rates are higher than theory predicts because: (1) culture is important (culture of compliance); (2) criminal enforcement matters; (3) in real life, people do not act like calculators; (4) people determine risk as much with their gut as they do with their head; and (5) in some sectors, most actors are not afraid of regulators because they believe they are complying. Jay Shimshack's book on "over-compliance" addresses this issue.

Discussant
Sarah Stafford, College of William and Mary

The Vossler study's development of a continuous choice model is a useful extension to existing literature. Context could be added so that it could be interpreted as an abatement model too, giving it broader application. The next step would be to add heterogeneity. The flexibility of the assumption that firms have identical emissions, as portrayed in the paper, is questionable. The experimental component of the paper is a real strength; a couple of additional experiments on audits should be considered.

The Lyon study complements theoretical papers on "greenwash." It is supported by the theory and illustrates some of the problems associated with "greenwashing." Consumers are frequently misled by "greenwashing" messages with minimal consequences. More worrisome is if early reduction credits are based on the 1605(b) reports submitted to DOE.

Regarding the Fischer study, what would happen if the NGO thought about their label more strategically? Does it make sense to make the first move in the game? Or should the NGO step back and let industry respond? How should the game be played? The study needs to do a better job of identifying the specific assumptions that are being placed on the market. Also, it may be beneficial to consider that firms are voluntarily participating in labels to preempt a mandatory label. Is there an incentive for the industry to self-label and preempt a government label? It also would be interesting to see an example of an industry label with a higher standard than the NGO label.

Questions and Discussion

Dr. Vossler indicated that his audience is academia, but he always tries to flesh out the policy implications in his papers. The paper is a preliminary draft and does not have a conclusions section yet. Competing incentives could be an important policy tool. He agreed that the theoretical model is simplified and will incorporate the suggestions from today into the paper. Realistic audit probabilities are very high and the number of audits needs to vary between groups. To lower the audit probabilities, really large groups would be needed and there are cost restrictions. However, the paper should justify why the high probabilities were used.

Dr. Lyon responded that a discussion of EPA mandated disclosure was in an earlier draft of the paper. Mandated disclosure could result in better results, but it might require large fines being imposed to get that response. Congress only allows EPA to "slap violators on the wrist," so fines may not be the most practical solution. Regarding "greenwash," the worst scenario is it could deter the government from taking action because the reports make it appear that industry has already taken action. Firms are most likely to disclose information in the presence of a Renewable Energy Portfolio.

Dr. Fischer was very interested in emphasizing the relevancy of her results. In the model, the NGO label could be a regulator or another entity. The question of strategic incentives is a good one. If the NGO recognizes that another label may come along, how would that change their incentives?

A participant asked Dr. Fischer if there is literature looking empirically at firms seeking to maximize profits and NGOs seeking to limit emissions, or literature on price premiums.
Dr. Fischer said the classic case is the forest products certification. The initiative began with the World Wildlife Foundation/Greenpeace label to protect tropical rainforests in developing countries. A year later, the U.S. industry came out with its own label and is the predominant label in developed countries. There is mixed evidence in terms of price premiums. Wood does not have much of a price premium, but researchers are still in the process of analyzing how much consumers will pay for labeled products.

PERSPECTIVES ON INFORMATION DISCLOSURE, EMISSIONS, AND COMPLIANCE

Moderator: Will Wheeler, EPA National Center for Environmental Economics

Panelist

Perspectives on Environmental Information Disclosure

Jay Shimshack, Tulane University

Environmental information disclosure programs are becoming more and more prevalent. Are these new policies delivering the intended results? This talk presents an overview of environmental information disclosure and evidence of its effects.

Environmental information disclosure programs/transparency policies are expanding and are considered by many policy makers to be the "third wave" in a series of environmental policies (first wave: command and control regulations; second wave: market-based instruments; third wave: information disclosure policies). Environmental disclosure programs can be classified broadly as: environmental advisories and hazard warnings (e.g., advisories for lead in paint, soil, and dust), pollution release registries (e.g., TRI), environmental performance ratings and rankings (e.g., Carbon Disclosure Project), eco-labels (e.g., foods), and disclosure programs that leverage traditional regulation (e.g., the Office of Enforcement and Compliance Assistance's annual enforcement reports). Effective information disclosure programs assume that: (1) people respond to the disclosed information; (2) new market or legal conditions arise such that the provided information induces the producer of the environmental harm to change their behavior; and (3) stakeholder and company responses are consistent with underlying policy objectives.

The biggest advantage offered by environmental disclosure policies is that they may correct market failures associated with incomplete or imperfect information. They are flexible, mitigate trans-boundary environmental concerns, can address pollutants where regulatory authority does not exist, mitigate risk from persistent pollutants emitted in the past, and leverage the benefits of more traditional regulations. Most importantly, they are inexpensive relative to the alternatives, quick to implement, and often politically acceptable for controversial topics.

Evidence suggests that success of transparency policies is mixed. There have been some successes in restaurant hygiene cards and the auto safety industry. Successful policies have shared the following features: (1) careful ex-ante design; (2) clear, understandable, and standardized information; (3) information provision for where and when the target audience makes a decision; and (4) persistent ex-post policy evaluations and revisions. In the environmental field, the performance of information disclosure programs varies, ranging from "effective" to "sort of effective" to "not effective." For example, mercury advisories induced at-risk consumers to reduce their mercury intakes; however, consumers did not replace mercury contaminated fish with safe Omega-3 fatty acid-containing fish. In addition, many consumers who were not at-risk also reduced their consumption of fish. Overall, the net health benefits of the policy were negative

The tangible outcomes of environmental disclosure policies indicate that they are not a panacea. They do have significant theoretical advantages relative to alternatives, but often produce mixed outcomes that are not fully consistent with public policy objectives. The most effective disclosure policies are carefully crafted ex-ante to address users and they should be adjusted ex-post to maximize their effectiveness. Evidence suggests that environmental transparency policies that target consumers, leverage existing regulations, or influence regulators may be more likely to achieve the intended results than policies aimed at firm managers, employees, investors, or activists. Much remains to be learned about how measurement error in the disclosed information influences outcomes, optimal disclosure policy designs, long-term effects of information disclosure, mechanism(s) driving environmental disclosure outcomes in the real world, and costs of disclosure programs for both regulators and firms. The gold standard remains how to balance the marginal environmental "bang per buck" for transparency policies with regulatory strategies.

Panelist
Cody Rice, EPA, Office of Chemical Safety and Pollution Prevention

Lead causes a number of health problems, especially in children. Recent scientific investigation has shown there is no "safe" exposure level for lead, even small amounts can affect IQ. In the United States, lead exposure has been greatly reduced since the 1970s by removing lead from gasoline, food cans, and other products. Most children with significant levels of lead in their body acquire it from lead-based paint. Many homes built prior to 1978 have the potential to contain lead-based paint. Amounts vary and it can be difficult to detect, but the older the building, the more likely lead-based paint is present. In 1992, Congress passed a law (Section 1018 of the Residential Lead-Based Hazard Reduction Act of 1992) that required EPA to take actions to reduce exposures to lead-based paint (e.g., hazard standards, work standards, and education). EPA required that for most houses constructed before 1978, sellers/landlords must disclose information about any known or likely sources of lead-based paint to the buyers/renters. Homebuyers are granted a 10-day period to conduct their own assessment for lead. Sales contracts and leasing agreements have to include lead disclosures. These measures provide consumers with opportunities to become informed about lead risks on the property and obtain additional information, if desired.

In terms of estimated costs, social benefit analyses show that EPA is efficient in their lead outreach program and the costs are relatively small per unit; however, costs add up when the number of home sales every year over decades are considered. The social benefit analyses are unable to factor in other lead activities that followed as a result of EPA's actions, estimate changes in the demand for older housing, and compare the "information disclosure" option versus other interventions (e.g., the cost-effectiveness of spending X on abatements). Retrospective analysis is not a high priority for staff time or the budget, unless Congress specifies it.

EPA economists are constrained by limits on their time, their budget, and available data. Ideally, they would like to thoroughly identify the benefits from a policy, but do not usually have all of the information available to do this. EPA economists would like to make better use of available academic information – especially studies that model behavior — before a policy change is made. They also would like to improve informational interventions and environmental justice by using the published literature in opportunistic ways. Improving economic analyses will help EPA to make better decisions.

Questions and Discussion

It was noted that the TRI list could be viewed as a tournament competition; firms could compete to not be listed as one of the top 10. If the TRI list, versus raw data, is effective at changing behavior, than we should be emphasizing processed data and finding people to process it.

There are many ways disclosure programs can be categorized, for example, programs designed to influence consumer behavior versus corporate behavior. Consumer-based advisories can be targeted, assuming that consumers are able to understand, process, and interpret information correctly. There is a lot of evidence that consumers react disproportionately to small risks and not enough to big risks.

One participant works with the Greenhouse Gas Reporting Program at EPA. Currently, the program is determining how the data should be published. Dr. Shimshack suggested formatting the data as summary presentations of environmental performance. These are easy to understand and readily interpretable. Other participants said that EPA has tried to make consumer-level datasets available, but access to higher levels of data is limited. Both avenues of data should be available. Providing it to a technical audience gives them the opportunity to analyze it and EPA may reap the rewards of the analyses. It would be helpful to add tags to the data to make it easier to acquire.

Another participant pointed out that environmental information can be reinvigorated by presenting it differently. For example, the TRI facility ranking could be changed to a corporate ranking. The corporate ranking uses the same information, but generates a different top 10 list, potentially leading to new discussions or evaluations of the data.

One person postulated about the actual usefulness of information disclosure. It could be promising to consider the top 10 list as a kind of tournament. Data usefulness is highly dependent on different variables, depending on the situation. Consumer preferences also must be addressed. Can new preferences be created, perhaps through advertising? Regulators may be reluctant to pursue this approach.

Another participant thought that the role of environmental activists may have been understated during the talks. Activists are effective at persuading other people to take notice of “green” products in the market. They do have an important role in reducing environmental damage, although the literature is weak in terms of defining their role and effectiveness.

One person asked if anyone has assessed the efficacy of information disclosure in the growing conditions of information overload. Another participant responded that there are some behavioral economists who have looked at limits of information absorption. In a world of too much information, consumers can become uncertain which labels are valuable and may not make the “right” choice.

The discussion ended with a participant asking what leads investors to think a certain action will be more profitable. The response was that investors react unpredictably to environmental news. Other beliefs (e.g., politics, consumers’ beliefs) probably play a role in these reactions as well, but there is much that is unknown about them.

Consumer Willingness to Pay for Appliances Produced By Green Power Partners

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Abstract

The U.S. Environmental Protection Agency's Green Power Partnership currently has over 1,200 members purchasing nearly 18 billion kilowatt hours of green power annually. One possible motivation for firms to become Green Power Partners is to increase their reputation for environmental performance among potential consumers. This research investigates the extent to which consumer preferences for a residential appliance are affected by information on whether or not the manufacturer was a Green Power Partner. Data for the study were obtained from a contingent choice exercise in an online survey of a national sample of adults. The results suggest that consumers are, on average, willing to pay an extra $53.18 to $68.66 for a refrigerator manufactured by a Green Power Partner. These amounts appear to generally exceed the additional costs associated with using green power to manufacture a refrigerator and suggest that membership in the Green Power Partnership could effectively be used to market consumer products.

Keywords: Renewable energy, Willingness to pay, Contingent choice

Classification Codes: Q21; Q42

1. Introduction

The burning of fossil fuels to produce energy is a leading source of greenhouse gas (GHG) emissions, which are widely considered to be a contributor to global climate change. Globally, fossil fuels account for more than 63 percent of energy production, while non-hydro renewable sources (NHRs) such as wind, solar, geothermal, and wood and other wastes account for only one percent (EIA, 2008a). However, while total energy consumption in the United States (U.S.) has been growing at a rate of 0.72 percent annually since 1978, energy production from NHRs has been growing at a rate of 2.82 percent annually (Schmalensee, 2010). The relatively large growth rate for NHR has been attributed, in part, to the recent rise of voluntary markets for renewable (or "green") energy (Swezey et al., 2007).

During the late 1990s and early 2000s, a large number of "green power" programs were created across the U.S. These programs, which primarily targeted residential consumers, allowed energy consumers to pay a small premium on their electric bill to subsidize the production of energy from renewable sources. More recently, however, growth in green energy sales has been attributed to increased purchases on the part of large corporations and government institutions (Bird et al., 2008; USEPA, 2010a). Many of these purchasers participate in a program initiated by the U.S. Environmental Protection Agency (USEPA) in 2001 called the Green Power Partnership (GPP). The GPP is a voluntary program in which USEPA recognizes green power purchasers in order to encourage the purchase of green power as a way to reduce the environmental impacts associated with energy use (USEPA, 2009). Currently, USEPA reports that approximately 1,200 partners are purchasing nearly 18 billion kilowatt hours (kWh) of green power annually (USEPA, 2010b).

GPP participants can, and often do, utilize GPP logos and other promotional materials in their marketing efforts. One possible motivation for purchasing green energy and participating in the GPP is to influence consumer or other stakeholder perceptions of the participant's environmental credentials. However, little research exists linking green power purchases with consumer or other stakeholder perceptions of a particular company or institution. Thus, to try to better understand how consumers would likely respond to information signifying that a company is purchasing renewable energy, we surveyed a national, online sample of adults using a contingent choice exercise for a consumer appliance in which manufacturer participation in the GPP was treated as an attribute of the appliance. Our interest is in examining how prospective consumers would react to information signifying that the product they are purchasing was produced by a manufacturer that purchases renewable energy (i.e., participates in the GPP) and how their reactions would be affected by other attributes of the product as well as by their own characteristics and attitudes about environmental issues.

2. Previous studies

It has been estimated that nearly half of the U.S. population has the option of purchasing retail green power from a local utility provider, and that everyone in the U.S. has the ability to purchase renewable energy credits (RECs) (Bird and Sumner, 2010). Actual participation in green power programs, on the other hand, is quite low. While some green power programs have as much as 21 percent of their eligible customers participating, the median participation rate among green power programs is approximately one percent. Overall, retail sales of renewable energy in voluntary purchase markets account for only 0.8% of total U.S. electricity sales (Bird and Sumner, 2010). The prices of green power for residential customers in utility programs can range from -0.17¢/kWh (a savings compared to standard service) to 10.0¢/kWh above standard electricity rates, with an average premium of about 1.8¢/kWh in 2009 (Bird and Sumner, 2010). On average, consumers who purchase green power pay about $5.40 per month above standard electricity rates for green power through utility programs (Bird and Sumner, 2010).

While little research has been conducted regarding how the purchase of green energy affects perceptions of a particular company or institution, a number of more general analyses of consumer perceptions of the provision of energy from renewable sources exist (e.g., Byrnes et al., 1999; Clark et al., 2003; Farhar and Houston, 1996; Harmon and Starrs, 2004; Holt et al., 1999; Kotchen and Moore, 2004; Kotchen and Moore, 2007; Roe et al., 2001; Rowlands et al., 2002; Rowlands et al., 2003; Whitehead and Cherry, 2007; Wiser et al., 2000 Zarnikau, 2003). The results of these studies suggest a positive willingness-to-pay (WTP) for the production of green power, and several of these studies have revealed a preference for solar and wind over other types of renewable energy.

Several of these studies also examined the effects of respondent demographics and program characteristics on WTP for green power, and/or actual participation in green pricing programs. For example, Borchers et al. (2007) conducted a contingent choice study in New Castle County, Delaware to elicit WTP for green power by generation source and as a generic label. The variables associated with green power were cost, quantity of alternative energy supplied, and source (solar, wind, biomass, farm methane, or simply "green"). Results showed that respondents over 50 or less than 30 years of age preferred green power over the status quo, as did respondents who have greater stated concern for the environment. They also found significant differences in stated WTP depending on whether the green pricing program was voluntary or non-voluntary. For a voluntary program, mean WTP was estimated to be $14.77 ($17.00) per month to replace 10 (25) percent of electricity consumption with generic green energy. For the non-voluntary program (i.e., a program where everyone was forced to purchase the same ratio of green to non-green energy), estimated mean WTP was only \$8.44 (\$11.58) per month to replace 10 (25) percent of electricity consumption with generic green energy.[1] Other findings suggest that: education has a positive impact on WTP for green power (Roe et al., 2001; Rowlands et al., 2003; Zarnikau, 2003); income is positively related to WTP for green power and actual participation in green pricing programs (Clark et al., 2003; Kotchen and Moore, 2004; Roe et al., 2001; Rowlands et al., 2003; Whitehead and Cherry, 2007; Zarnikau, 2003); and environmental concern, as evidenced by membership in environmental organizations or opinion statements about environmental issues, has a positive influence on WTP for green power (Kotchen and Moore, 2004; Roe et al., 2001; Rowlands et al., 2003).

Finally, Wiser et al. (2001) examined non-residential demand for green power, including that by businesses. Their results suggested that organizational values and civic responsibility were more important motivators than perceived green marketing opportunities in the decision to make green power purchases. For example, only about 10 percent of the respondents had used the fact that they purchased green power in their point-of-sale marketing.

3. Material and methods

The data were obtained through an online survey conducted in March and April of 2009. The survey sample and online hosting services were provided by Knowledge Networks® (KN). The sample was drawn from an online research panel maintained by KN that is designed to be representative of the U.S. population. KN recruits individuals for this panel by either random digit dialing or address-based sampling methods. If needed, panel members are provided with free access to the Internet and a laptop in exchange for agreeing to complete at least one survey

[1] Caution should be used when interpreting WTP values derived from hypothetical choice experiments as these values have been found to exceed those from binding experiments – a result that is often referred to as “hypothetical bias” (Harrison and Rutström, 2008; List and Gallet, 2001; Murphy et al., 2004).

per week. All panel members who complete longer surveys, such as the one used in this study, receive incentive points redeemable for cash. Upon being recruited to the panel, each individual completes a profile survey that collects essential demographic information. This profile is updated annually. The responses to the survey questions used in the analysis presented here were supplemented with demographic information from the panel member profile. More information on the online research panel and panel recruitment can be found in Knowledge Networks (2010).

Each panel member selected for the sample for this survey was sent an email notifying them that there was a new survey available for them to take and providing an electronic link to the survey questionnaire. After three days, automatic email reminders were sent to all non-responders. In addition, each panel member had access to a personalized “home page” that provided them with a link to this and any other surveys assigned to them.

The sample was a simple random sample of panel members 18 years of age or older that was adjusted to correct for known deviations in panel recruitment from an equal probability sample of the U.S. population, as well as non-response and non-coverage bias in panel membership, by comparing panel membership to demographic distributions from the most recent data from the Current Population Survey. A survey weight designed to compensate for non-response to the survey was calculated by comparing respondent demographics with benchmark demographics from the Current Population Survey (i.e., gender, age, race/ethnicity, education, Census Region, metropolitan area, and internet access). The weight was calculated with an iterative proportional fitting procedure. The distribution of the calculated weights was examined to identify and, if needed, trim outliers at the extreme upper and lower tails of the weight distribution. The post-stratified and trimmed weights were then scaled to the sum of the total sample size. All results presented in this paper were weighted with the resulting weights.

Preference data were obtained from a contingent choice exercise. The contingent choice methodology was used because it closely replicates the purchase decision faced by actual consumers and, thus, permits the construction of an instrument that has the look and feel of a product design task rather than an environmental-information-gathering exercise. In the choice experiment, respondents were asked to make a series of choices over different varieties of a refrigerator and a “None” option. The choice of a refrigerator as the appliance was based on a number of factors. First, refrigerators are relatively large consumers of energy, accounting for nearly 14 percent of the electricity consumed in U.S. households (EIA, 2008). Second, refrigerators are ubiquitous in U.S. households as nearly 99 percent of such households own at least one refrigerator (Barkenbus, 2006).

The contingent choice exercise was preceded by a series of general background questions on the respondents’ home and refrigerator ownership and a series of information screens providing basic information about the refrigerator attributes used in the choice experiment including, where appropriate, images of the attribute. Each of the information screens consisted of a 3-4 sentence explanation of the attribute and also provided respondents with the option to obtain more information about the attribute from an additional screen before continuing with the survey. The screen providing information on the GPP program described its aims to reduce GHG emissions by promoting the consumption of electricity produced from renewable sources. The text of both the basic and additional information screens for the GPP attribute is provided in Table 1.

The information screens were followed by the choice experiment, which consisted of 14 contingent choice tasks. In each task, respondents were asked to choose the one refrigerator they would most likely purchase out of three refrigerator options or to select a “none” option if they would not choose any of the three. Participants were asked to assume that all of the choices fit in the space they had for a refrigerator, were available in the color or finish of their choice, and had both automatic defrost and a built-in icemaker.

The refrigerator attributes used in the choice experiments were price, internal capacity, whether the refrigerator had an external ice and water dispenser, brand, configuration, and whether or not the refrigerator manufacturer was a Green Power Partner. The price options were \$879, \$929, \$979, and \$1,029. The prices were chosen based on current market prices of refrigerators that were similar to those described in the choice experiment. The internal capacity options were 23.78, 24.52, 25.34, and 25.83 cubic feet and the brand options were LG, GE, Whirlpool, and Kenmore. The internal capacity and brand options were chosen based on market popularity. The configuration options were side-by-side and French door, the choice of which was based on focus group analyses. It is worth noting that the omission of the generally lower-priced top-freezer configuration and resulting choice of price options may have served to truncate the lower-priced end of the market. The options for external ice and water dispensers were none, ice only, water only, or both ice and water. For the green power attribute, the refrigerator manufacturer was either a Green Power Partner or not.

The survey was fielded to 2,195 panel members and a total of 1,395 responses were received before the survey was closed to further responses. There were four different versions of the survey. Out of the 1,395 respondents, a total of 388 completed the version used for this analysis. Each of the 338 respondents to the survey was asked to complete 14 choice tasks yielding a possible 4,732 individual choice tasks, of which 4,721 were actually completed. Given that each choice task contained three different alternatives and a “none” option, there were a possible 18,928 individual observations (18,884 were obtained).

4. Theory

It is assumed that respondents, when presented with a choice of alternatives, will choose the alternative that possesses the combination of attributes that would provide them the highest level of utility. It can also be assumed that the utility received from a particular alternative is related to a set of observable attributes associated with the choice. Thus, the utility individual i receives from the jth alternative can be expressed as

$$U_{ij} = \beta' X_{ij} + \varepsilon_{ij} \quad (1)$$

where X_{ij} is a vector of observed attributes of alternative j for individual i, β is a vector of coefficients to be estimated, and ε_{ij} is an error term. If Equation 1 is estimated with a conditional logit (McFadden, 1972), the probability of individual i choosing alternative j can be expressed as

$$\prod_{\mathrm{ij}} = \frac{\exp(\beta' X_{ij})}{\sum_{\mathrm{j}=1}^{4} \exp(\beta' X_{ij})} \quad (2)$$

WTP for a particular attribute, k, is then calculated as

$$\mathrm{WTP_k} = -b_{\mathrm{k}}/b_P \quad (3)$$

where b_k represents the estimated coefficient for the kth attribute (*viz*., estimate of β_k) and b_P is the estimated coefficient of price.

However, the conditional logit is limited due to its assumptions of homogeneity of individuals. More specifically, the conditional logit assumes that the elements of the β vector are constant across all individuals and that the ε_{ij}'s are independently and identically distributed (iid) across all individuals and alternatives (Steckel and Vanhonacker, 1988). The model can be modified to incorporate heterogeneity of preferences across individuals by using random coefficient models such as the mixed logit (Train, 2003). The utility function for the random coefficient model can be expressed as

$$U_{ij} = \beta_P P_j + (\beta + \eta_{xi})' X_{ij} + \varepsilon_{ij} \quad (4)$$

where β is a vector of population mean parameters, and η_{xi} is a vector representing the stochastic deviation of the individual's preference from the population mean and has a mean of zero and covariance matrix Σ_β. The coefficient of price (β_P) is assumed to be a fixed (non-random) parameter (with standard deviation 0) so that estimates of WTP for the non-price attributes are normally distributed. WTP for the *k*th attribute can then be calculated using estimate b_P and estimate $\bar{b}_k$ for the mean of the random coefficients for attribute *k* as follows:

$$WTP_k = -\bar{b}_k / b_P \quad (5)$$

An additional way of incorporating heterogeneity of preferences is by explicitly relating the deterministic component of the utility function to attitudinal and/or demographic variables in a "mixed" model (Hanley et al., 2001; Steckel and Vanhonacker, 1988). With this approach, β_k becomes a function of attitudinal and demographic characteristics, *Z*, and can be expressed as

$$\beta_k^* = \beta_{k0} + \beta_{k1} Z_1 + \beta_{k2} Z_2 + \cdots + \beta_{kn} Z_n \quad (6)$$

This specification amounts to interacting the *k*th attribute variable with a constant and attitudinal/ demographic variables $Z_1, Z_2, \ldots, Z_n$ with coefficients $\beta_{k0}, \beta_{k1}, \ldots, \beta_{kn}$. The WTP for attribute *k* can then be calculated by replacing b_k in Equation (3) with $b_{k0} + b_{k1} Z_1 + b_{k2} Z_2 + \cdots + b_{kn} Z_n$, evaluated at a data point such as the sample means of the *Z*'s. This "mixed" model can be modified for use in the random parameters model as well. For the "mixed" model with random parameters, β_{k0} from Equation 6 is treated as random, becoming $\beta_{k0} + \eta_{k0}$. Parameters for the interaction terms are assumed fixed (non-random) for practical considerations, *viz*., to reduce the computational burdens due to the large number of interaction terms considered. The WTP for the *k*th attribute can be calculated by replacing $\bar{b}_k$ in Equation 5 with $b_k^* = \bar{b}_{k0} + b_{k1} Z_1 + b_{k2} Z_2 + \cdots + b_{kn} Z_n$.

Four models are used in our analysis – two fixed parameter (conditional logit) models and two random parameter models. In each case, one of the models is limited to product attributes only, while the other includes both product attributes and interactions between the demographic and attitudinal variables and the GPP attribute. Both random parameters logits are estimated with simulated maximum likelihood using Halton draws with 1,000 repetitions and are assumed to have normally distributed parameters with a diagonal covariance matrix, while only non-price attributes have random coefficients.

The product attributes, demographic characteristics, and attitudinal variable definitions, hypothesized signs, and sample means are presented in Table 2. The refrigerator attributes are *Price*, manufacturer participation in the GPP (*GPP*), *Capacity*, brand (*LG*, *GE*, and *Kenmore*, with *Whirlpool* as the base case), configuration (*Frenchdr*) with Side-by-Side (*sbs*) as the base case, and external dispenser type (*Ice* only, *Water* only, and ice and water (*IandW*), with no dispenser (*No_iw*) as the base case). In order to include the "none" responses in the analysis, an alternative-specific constant (*ASC*) was created which takes a value of 1 for the "none" option and 0 for the three refrigerator alternatives. The demographic characteristics included in the analysis are age (*Age* and Age^2), gender (*Male*), ethnicity (*White*, *Black*, or *Hispanic* with *Other* as the reference case), education (*SCollege* or *Degree* with *NCollege* as the reference case), household income (*Inc30_60*, *Inc60_85*, *Incgt85* with *Inclt30* as reference case), household size (*Hhsize*), home ownership (*Ownhome*), region of the country in which the respondent resided (*Northeast, Midwest, South,* with *West* as reference case), and whether the respondent lived in a metropolitan area or not (*Metro*). A variable (*Renewable*) was created to reflect the percentage of energy produced from renewable energy in each respondent's state of residence (EIA, 2004). Self-reported familiarity with the GPP prior to taking the survey (*Familiarity*) was also included.

The survey contained twelve Likert scale questions designed to measure respondents' attitudes toward a variety of environmental issues such as global climate change, the use of green energy, and perceived consumer effectiveness. A list of these questions can be seen in Table 3. Responses to these questions were subjected to a factor analysis. A varimax rotation of the analysis revealed two usable factors defined by seven of the twelve items. A second factor analysis was conducted on this reduced set and the results are presented in Table 4. This analysis produced weights on the seven responses defining two variables, one capturing perception of consumer effectiveness in affecting product design and manufacturing and the ambient environment (*Effect*); and the other reflecting views of the reality, severity, and need for action against global climate change (*Climate*). A Chronbach's alpha test was used to test the reliability and acceptability of each of the two factors (Cortina, 1993; Nunnally, 1967), the results of which are presented in Table 4 along with the factor loadings.

Based on prior research on green power (Byrnes et al., 1999; Clark et al., 2003; Farhar and Houston, 1996; Harmon and Starrs, 2004; Holt et al., 1999; Kotchen and Moore, 2004; Kotchen and Moore, 2007; Roe et al., 2001; Rowlands et al., 2002; Rowlands et al., 2003; Whitehead and Cherry, 2007; Wiser et al., 2000; Zarnikau, 2003), WTP for manufacturer participation in the GPP is expected to be positive. WTP for GPP participation is expected to decrease with age, but increase with income and education and be higher among females than among males. Drawing on findings by Roe et al. (2001), we expect that respondents who reside in the West will have a greater WTP for GPP participation than respondents residing in the Midwest, Northeast, or South. We also expect that respondents residing in states with a greater percentage of power being produced from renewable sources will have a greater WTP for GPP participation. Greater familiarity with the GPP program is likely to be positively correlated with

WTP for a refrigerator manufactured by a participant in the GPP program. Finally, we expect a positive relationship between WTP and those with a higher degree of perceived consumer effectiveness in affecting product design and manufacturing and the ambient environment (*Effect*) and those stating strong views toward the reality, severity, and need for action against global climate change (*Climate*). The other attributes associated with refrigerators and their hypothesized signs are listed in Table 2.

5. Results

The means of the explanatory variables are presented in Table 2. The mean age of the respondents is 47.10 years, while 47 percent of the respondents are male, 68 percent are white, 11.8 percent black and 14 percent Hispanic. A majority of the respondents either attended some college (26.2 percent) or obtained a Bachelor's degree or higher (27.4 percent). Slightly more than one-third (36.5 percent) of the respondents have household incomes between \$30,000 and \$60,000, 18.4 percent between \$60,000 and \$85,000, and 20.1 percent in excess of \$85,000. These incomes support an average of 2.47 individuals per household and 71.9 percent of the households own their own home. The greatest percentage of respondents resides in the South (36.9 percent), while 19.2 percent live in the Northeast and 21.8 percent in the Midwest. A large majority (83.6 percent) live in a metropolitan area. The mean level of familiarity is 1.37 where one was not at all familiar and three was very familiar. The mean level of energy production from renewable sources is 10.4 percent.

The results of likelihood-ratio tests reveal that the random parameter logit specifications are preferred to the conditional logit specifications between both the attributes-only models (Models 1 and 3) (LR =3592.34, df = 10, p-value < 0.0001) and between the models with interaction terms between individual characteristics and *GPP* (Models 2 and 4) (LR = 3237.42, df

= 10, p-value < 0.0001). Furthermore, the results of likelihood-rato tests also support inclusion of demographic and attitudinal variables in both the fixed parameters (LR = 695.13, df = 21, p-value < 0.0001) and the random parameters logits (LR = 340.21, df = 21, p-value < 0.0001).

Table 5 presents estimation results for all four models. As expected, the coefficient of *Price* is negative and highly significant across all models, suggesting that consumers are sensitive to price changes. The coefficient of *GPP* is positive and significant across all four models, suggesting a preference for refrigerators manufactured by participants in the GPP. Other positive and significant attribute variables included *Capacity*, *Ice*, *Water* and *IandW*, showing that respondents prefer larger refrigerators and those equipped with external ice and water dispensers. The only brand name that is significant is *LG* and it consistently has a negative coefficient, indicating consumers prefer the base case, *Whirlpool*, to *LG*.

The only two interaction variables with positive and significant coefficient estimates for both the conditional and random parameters logit are the two factor analysis scores *Effect* × *GPP* and *Climate* × *GPP*, suggesting that consumer preference for manufacturer participation in the GPP is motivated to some degree by environmental concern. The only other two interaction variables that are significant in both the conditional and random parameters models are *Midwest* × *GPP* and *South* × *GPP*, both of which are negative. *Northeast* × *GPP* is also negative in both models but only significant in the conditional logit model. Thus, the results suggest consumers in the West have a stronger preference for manufacturer participation in the GPP than consumers in other parts of the country.

A number of other interaction variables are significant in the conditional logit model but not the random parameters logit. For example, *Age* × *GPP* is negative while Age^2 × *GPP* is positive, suggesting WTP for participation in the GPP decreases with age, but at a decreasing rate. *Male* × *GPP* is positive suggesting, contrary to expectations, that males have a higher WTP than do females. *White* × *GPP*, *Black x GPP*, and *Hispanic* × *GPP* were all positive. *College Degree* × *GPP* is negative, confounding expectations. Two (*Inc60_85* × *GPP* and *Incgt85* × *GPP*) of the three income variables are positive suggesting that WTP increases with income. Home owners have a lower WTP for participation in the GPP than non-home owners. On the other hand, respondents who are more familiar with the GPP prior to the survey have a higher WTP.

It was hypothesized that the likelihood of choosing a refrigerator manufactured by a participant in the GPP would be positively correlated with the percentage of electricity produced from renewable sources in the consumer's state of residence. However, results from the fixed parameters logit suggest that there is a negative relationship between the likelihood of choosing a refrigerator manufactured by a participant in the GPP and percentage of renewable energy produced and in the random parameters logit it is not significant. One possible cause of the negative relationship revealed in the fixed parameters logit is that consumers living in states with a greater percentage of renewable energy might have more opportunities to invest in green power directly and therefore have less of a desire to support green power production through the purchase of products produced from green power.

Table 6 presents estimates of mean WTP for the refrigerator attributes derived from all four models. For the product attribute-only fixed parameters model, WTP is calculated using equation (3) while equation (5) is used to calculate mean WTP for the attribute-only random parameters model. For Models 2 and 4, which include demographic and attitudinal interaction terms, mean WTP for a refrigerator that was manufactured by a Green Power Partner as opposed to one that was not is calculated by modifying equations (3) and (5) as discussed above. These estimates are calculated at the sample means of variables and at the estimated mean parameters. Standard errors for the WTP estimates are calculated using Krinsky and Robb's (1991) re-sampling method with 15,000 draws. WTP estimates for the attributes only models (Models 1 and 3) are positive and significantly different from zero; however, the mean WTP estimates for the models that included both product attributes and interactions with *GPP* (Models 2 and 4*)*, while larger in absolute terms than the estimates from the attributes only models, are not significantly different from zero. The mean WTP for the fixed parameters and random parameters logits with attributes only (Models 1 and 3) are $53.18 and $68.66 respectively, while the mean WTP for the fixed parameters and random parameters logits with attributes and interactions (Models 2 and 4) are $93.21 and $82.90 respectively.

It would be interesting to compare these estimates to the per-refrigerator cost to a manufacturer of participating in the GPP or the per-unit cost of manufacturing refrigerators with green energy. Given that Green Power Partners are simply required to purchase enough renewable energy to meet a specified percentage of their total energy consumption, it would be difficult and not particularly useful to try to calculate a per-refrigerator cost of joining the GPP. However, the costs of producing a refrigerator with green power can be approximated using existing estimates of the energy used in the manufacturing and transportation of a new refrigerator, which range from 280 to 402 MJ (Kim et al., 2006; Horie, 2004; van Engelenburg et al., 1994), and the 2009 average and maximum price premiums for green power of $0.018 and $0.10, respectively.[2] Multiplying the lowest estimated energy use by the average price premium and the highest estimated energy use by the maximum price premium yields an estimated range

[2] Since refrigerator manufacturers would be more likely to purchase renewable energy through the renewable energy credit (REC) market than through utility green pricing program premiums it might be more appropriate to use prices from the REC market. However, since REC prices tend to be both more difficult to come by and lower than utility prices (Bird and Sumner, 2010), we use utility green pricing program prices to be conservative.

of additional costs from using green power of $2.01 to $11.17 per refrigerator. Comparing these estimated costs to the statistically significant mean WTP estimates in Table 6, which range from $53.18 to $68.66, suggests that, on average, consumers may be willing to pay more for manufacturer participation in GPP than the additional costs that would be incurred to manufacture a refrigerator using renewable instead of conventional power.

6. Conclusions

The results from this study suggest that consumers may be willing to pay more for a refrigerator produced by a Green Power Partner than a refrigerator from one that is not. More specifically, they suggest that an individual, when faced with a choice between two refrigerators, identical except that one was produced by a Green Power Partner and one was not, would be willing to pay a premium from $53.18 to $68.66 for the refrigerator produced by the Green Power Partner. As a point of comparison, it is estimated that a household participating in a green pricing program spends, on average, $5.40 extra per month (or $64.80 per year) to purchase green power from their utility provider. Furthermore, these results suggest that, on average, consumers are willing to pay more for participation in the GPP than the additional costs of manufacturing a refrigerator using renewable instead of conventional power.

Results from this study also indicate that consumer demographics and attitudes influence WTP. In particular, attitudes toward environmental issues, which were captured in the variables *Effect* and *Climate,* have a positive effect on consumer WTP for refrigerators manufactured by a participant in the GPP. Furthermore, the results indicate a negative relationship between the choice of a refrigerator manufactured by a Green Power Partner and places of residence in the *Northeast Midwest*, and *South* when compared to residence in the *West*. Hence, a consumer labeling program based on participation in the GPP might find more support in the western United States.

This paper presents estimates of the mean amount consumers are willing to pay to purchase a refrigerator manufactured by a Green Power Partner as opposed to one that was not. While these results show promise for green power labeling by manufacturers, several caveats and areas of future research exist. For example, the information provided to the respondents did not explicitly state that the refrigerator was produced using green power, nor did it describe the renewable sources used to produce the green energy, either or both of which could influence consumer WTP. In addition, the choice of an energy-using appliance such as a refrigerator might also influence consumer perception of a Green Power Partner manufacturer, given that the energy used to operate the refrigerator over its lifetime will likely be much greater than that used to produce it. Finally, consumer WTP would likely be affected by the presence of other energy-related labels, such as USEPA's Energy Star®, common to household appliances. What this effect would be is an empirical question. Thus, while this study suggests that consumers may be willing to view a manufacturer's use of green energy as a positive product attribute, much research is still needed to fully assess how use of green power in consumer product manufacturing may impact demand for these products.

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Table 1

Text of information screens for the green power partner attribute.

Another feature that you may consider is whether the refrigerator is manufactured by a company that is a GREEN POWER PARTNER.

GREEN POWER PARTNERS are businesses that purchase electricity generated from renewable sources, such as solar or wind.

By purchasing electricity generated from renewable sources, manufacturers reduce the emission of greenhouse gasses associated with refrigerator production. Studies suggest that greenhouse gases contribute to global climate change.

Would you like more information on GREEN Power PARTNERS or are you ready to proceed with the survey?

"More Information" screen for the ENERGY STAR attribute

GREEN POWER PARTNERS is a voluntary program sponsored by the U.S. Environmental Protection Agency as a way to increase the use of renewable energy and, thus, reduce greenhouse gas emission from conventionally-generated electricity.

Green power is electricity generated from environmentally preferable renewable resources, such as solar, wind, geothermal, and some biomass (i.e., plant material) and hydrological resources.

To qualify as a GREEN POWER PARTNER, a manufacturer must buy enough green power to meet specified percentages of its total electricity consumption. This percentage varies depending upon the total amount of electricity consumed by the manufacturer.

Table 2

Variable names, definitions, hypothesized signs, and sample means.

Variable	Description	Hypothesized sign	Sample mean[a]
Dependent variable			
Chosen	1 if alternative is chosen, 0 otherwise	NA	0.250
Product attribute variables			
Price	\$879, \$929, \$979, or \$1,029	–	954.536
GPP	1 if refrigerator produced by Green Power Partner, 0 otherwise	+	0.511
Capacity	23.78, 24.52, 25.34, 25.83 cubic feet	+	24.841
Frenchdr	1 if French door, 0 if side-by-side	+	0.511
LG	1 if LG brand, 0 otherwise	NA	0.267
GE	1 if GE brand, 0 otherwise	NA	0.245
Kenmore	1 if Kenmore brand, 0 otherwise	NA	0.243
Whirlp	1 if Whirlpool brand, 0 otherwise (reference case)	NA	0.245
Ice	1 if external ice dispenser only, 0 otherwise	+	0.267
Water	1 if external water dispenser only, 0 otherwise	+	0.242
IandW	1 if external ice & water dispenser, 0 otherwise	+	0.247
NoIW	1 if no external ice or water dispenser, 0 otherwise (reference case)	NA	0.243
ASC	1 if “None” option, 0 otherwise	NA	0.250
Demographic and Attitudinal Variables Interacted with GPP			
Age	Age/10	–	4.710
Age^2	Age squared/1000		2.491
Male	1 if male, 0 otherwise	–	0.470
White	1 if white, non-Hispanic, 0 otherwise		0.680
Black	1 if black, non-Hispanic, 0 otherwise		0.118
Hispanic	1 if Hispanic, 0 otherwise		0.140
Other	1 if other or 2+ races, non-Hispanic, 0 otherwise (reference case)	NA	0.062
N College	1 if did not attend college, 0 otherwise (reference case)	NA	0.464
SCollege	1 if attended some college, 0 otherwise	+	0.262
Degree	1 if earned Bachelor’s degree or higher, 0 otherwise	–	0.274
Inclt30	1 if household income < \$30,000, 0 otherwise (reference case)	NA	0.250
Inc30_60	1 if household income \$30,000 - \$60,000, 0 otherwise	+	0.365
Inc60_85	1 if household income \$60,000 - \$85,000, 0 otherwise	+	0.184
Incgt85	1 if household income > \$85,000, 0 otherwise	+	0.201
Hhsize	Total number of individuals in household	–	2.468
Ownhome	1 if owns home, 0 otherwise		0.719
Northeast	1 if lives in Northeast, 0 otherwise	+	0.192
Midwest	1 if lives in Midwest, 0 otherwise	+	0.218
South	1 if live sin South, 0 otherwise	–	0.369

West	1 if lives in West, 0 otherwise (reference case)	NA	0.221
Metro	1 if lives in metropolitan area, 0 otherwise		0.836
Renewable	State-level percentage of energy production from renewable sources	+	0.104
Familiar	Familiarity with GPP prior to survey (1 = not at all, 2 = somewhat, 3 = very)	+	1.374
Effect	Factor analysis score	+	–0.030
Climate	Factor analysis score	+	–0.019

[a] Sample means are weighted.

Table 3

Environmental concern questions.

Variables included in factor analysis [e]	
1	I will try to conserve energy only when it helps to lower my utility bills
2	When I buy products, I consider how my use of them will affect the environment
3	By choosing environmentally friendly products, I signal to manufacturers the types of products they should be producing.
4	I don't have enough knowledge to make well-informed decisions on environmental issues
5	The conservation efforts of one person are useless as long as other people refuse to conserve
6	Global climate change will have a noticeably negative impact on the environment in which my family and I live
7	There is no urgent need to take measures to prevent global climate change today.
8	The production of electricity from renewable sources such as solar, wind and biomass is an effective way to combat global climate change.
9	The most effective way to combat global climate change is to reduce energy consumption
10	Science and technology will come up with effective ways to combat global climate change
11	Most people are not willing to make sacrifices to protect the environment.
12	We need more government regulations to force people to protect the environment.

[e] Response options were 1 = strongly disagree, 2 = disagree, 3 = neutral, 4 = agree, and 5 = strongly agree.

Table 4

Rotated factor loadings with reliability scores (Chronbach's α).

	Chronbach's α	Factor weights
Perceived consumer effectiveness (effect)	0.815	
When I buy products, I consider how my use of them will affect the environment.		0.753
By choosing environmentally friendly products, I signal to manufacturers the types of products they should be producing.		0.765
Views toward climate issues (climate)	0.828	
We need more government regulations to force people to protect the environment.		0.586
There is no urgent need to take measures to prevent global climate change today.		–0.599
The most effective way to combat global climate change is to reduce energy consumption.		0.556
Global climate change will have a noticeably negative impact on the environment in which my family and I live.		0.584
The production of electricity from renewable sources such as solar, wind, and biomass is an effective way to combat global climate change.		0.585

Table 5

Estimated models of refrigerator choice.

	Conditional fixed parameter logit models				Random parameter logit models			
	Model 1		Model 2		Model 3		Model 4	
Variable	Estimate	*t*-value	Estimate	*t*-value	Estimate	*t*-value	Estimate	*t*-value
					Means of estimates			
Price	–0.006	–14.00	–0.007	–14.11	–0.011	–9.78	–0.011	–9.31
GPP	0.343	6.93	0.806	1.64	0.744	3.78	0.569	0.29
Capacity	0.069	2.07	0.070	2.11	0.141	2.60	0.092	1.76
Frenchdr	–0.262	–5.14	–0.316	–6.12	–0.484	–1.76	–0.551	–2.48
LG	–0.295	–4.17	–0.273	–3.81	–0.702	–4.74	–0.540	–4.03
GE	–0.062	–0.91	–0.056	–0.82	–0.154	–1.19	–0.077	–0.55
Kenmore	–0.104	–1.49	–0.077	–1.09	–0.147	–1.30	–0.141	–1.27
Ice	0.433	5.77	0.418	5.51	0.532	3.84	0.621	4.48
Water	0.242	3.16	0.231	2.97	0.267	2.13	0.193	1.51
IandW	1.060	14.74	1.100	15.07	1.612	8.83	1.812	5.64
ASC	–3.836	–4.18	–3.942	–4.22	–6.564	–3.81	–8.321	–4.80
Age × GPP			–0.366	–2.64			–0.142	–0.26
Age^2 × GPP			0.334	2.54			0.058	0.10
Male × GPP			0.221	2.70			–0.218	–0.83
White × GPP			0.549	2.62			0.925	1.18
Black × GPP			0.873	3.60			2.001	1.96
Hispanic × GPP			0.612	2.60			1.267	1.26
SCollege × GPP			–0.175	–1.53			0.040	–0.10
Degree × GPP			–0.210	–2.06			0.002	0.00
Inc30_60 × GPP			–0.070	–0.60			0.208	0.50
Inc60_85 × GPP			0.551	4.07			0.516	1.19
Incgt85 × GPP			0.687	4.62			0.523	0.96
Hhsize × GPP			0.016	0.54			–0.066	–0.73
Ownhome × GPP			–0.258	–2.35			–0.115	–0.29
Northeast × GPP			–0.399	–2.90			–0.570	–0.99
Midwest × GPP			–0.653	–4.42			–1.445	–2.03
South × GPP			–0.508	–3.83			–1.196	–1.95
Metro × GPP			–0.124	–1.09			0.140	0.75
Renewable × GPP			–0.868	–2.79			–1.470	–1.37
Familiar × GPP			0.284	3.71			0.408	1.34
Effect × GPP			0.203	3.81			0.582	3.09
Climate × GPP			0.312	5.91			0.560	3.03
					Standard deviations of estimates			
GPP					1.595	4.33	1.535	6.53
Capacity					0.054	1.37	0.161	7.74
Frenchdr					2.487	4.66	2.358	10.10
LG					0.913	4.86	0.874	5.88
GE					1.001	3.42	1.067	3.40

Kenmore			0.493	1.91	0.511	2.06
Ice			0.869	5.98	0.895	4.53
Water			0.624	3.67	0.561	2.46
IandW			1.997	7.11	2.102	6.65
ASC			4.173	4.83	0.263	0.53
Log likelihood	–6021.91	–5674.35	–4225.74		–4055.63	

Table 6

Estimates of WTP for refrigerator attributes.

	Conditional fixed parameter logit models				Random parameter logit models			
	Model 1: Attributes only		Model 2: Attributes and interactions		Model 3: Attributes only		Model 4: Attributes and interactions	
Attribute	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.
GPP	53.18**	16.83	93.21	92.10	68.66*	36.97	82.90	146.73
Capacity	10.62	10.13	10.63	10.05	13.08	10.09	8.43	9.54
French door	–40.66**	17.44	–47.68**	17.57	–45.39	53.59	–50.52	42.83
LG	–45.48**	21.86	–41.00*	21.60	–64.92**	29.87	–49.31*	27.60
GE	–9.55	21.31	–8.41	20.59	–13.96	23.65	–6.74	25.41
Kenmore	–15.93	21.43	–11.46	20.84	–13.63	20.80	–12.80	20.35
Ice	66.94**	23.48	62.83**	23.00	49.02*	25.79	56.31**	25.25
Water	37.57	23.31	34.68	22.93	24.66	23.16	17.44	22.95
Ice and water	164.12**	59.62	165.27**	29.51	149.24*	90.65	165.39**	70.34

Note: Asterisks ** indicate statistical significance at the 95% confidence level and * at the 90% level.

Consumer Willingness to Pay for Energy Star and Green Power Labeled Refrigerators

Benefits of Environmental Information Disclosure Workshop
National Center for Environmental Economics
U.S. Environmental Protection Agency
January 18, 2010

Christopher D. Clark, Kimberly L. Jensen, Steven T. Yen,
Clifford S. Russell, David O. Ward, and Xiaogu Li

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THE UNIVERSITY of TENNESSEE UT INSTITUTE of AGRICULTURE

Motivation

- Apparent preference in U.S. for environmental labeling programs with both *public* and *private* benefits
  - Appeal to "narrow self-interest"?
  - Motivation Crowding Out?
    - Small extrinsic reward can "crowd out" intrinsic motivation

Research Objectives

- Analyze consumer response to information on an "environmental attribute" of a consumer product
  - Attribute includes both public and private benefits
    - Energy Star
      - Reduced impact on environment
      - Energy cost savings
  - Attribute limited to public benefits only
    - Green Power Partners
    - Climate Leaders

Literature Review

- Energy "efficiency gap" papers from 1970's
- Energy Star research
  - Achieving substantial market penetration
  - Being recognized by consumers
  - Influencing decision-making
  - Generating substantial energy savings
- Sammer and Wüstenhagen (2006)
- Shen and Saijo (2009)

Data Collection

- Stated preference – contingent choice experiment
- Online survey conducted March-April 2009
- Knowledge Networks® online research panel
  - Representative of U.S. population
  - Recruited by probability-based sampling
  - Provided access to Internet and hardware, if needed
- Random sample of 2,195 panel members
- 1,395 responses before survey closed
- Four survey versions
  - Energy Star
  - Energy Star with mail-in rebate of $50
  - Green Power Partnership (GPP)
  - Climate Leaders

Choice of Consumer Product

- Energy consuming
- Capable of being described with limited number of attributes
- Familiar to all members of household
- Limited importance of aesthetic, visual qualities
- Accessible product information

Refrigerator Attributes

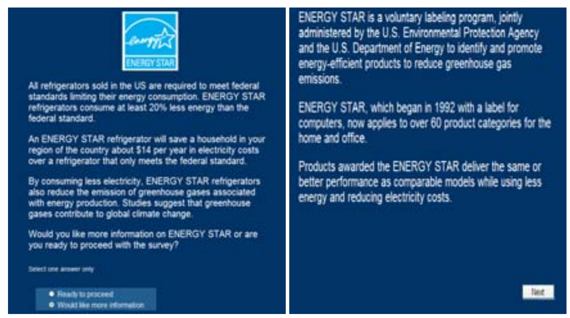
- Price
  - $879, $929, $979, $1,029
- Configuration
  - Side-by-side, French door
- Brand
  - LG, GE, Kenmore, Whirlpool
- Internal Capacity
  - 23.78, 24.52, 25.34, 25.83 ft^3
- External Ice and Water Dispenser
  - None, Ice, Water, Ice and Water
- “Label”
  - Yes, No

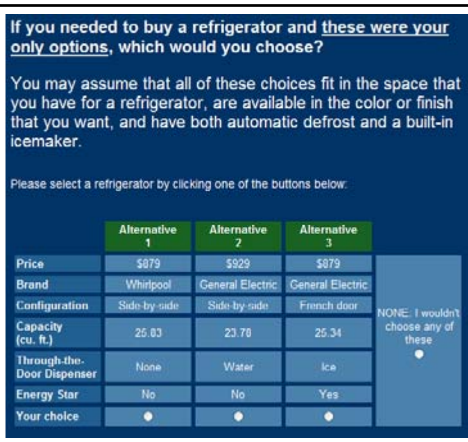
Survey Design

- Preliminary questions
  - Current refrigerator(s)
    - Type, age, number, etc.
  - Refrigerator shopping experience
- Attribute Information Screens
- Choice Experiment Tasks
- Debriefing Questions
- Attitudinal and Behavioral Questions

Configuration Information Screen

Energy Star Information Screens





Observations by Survey Version

Information Program	Respondents	Choice Tasks Completed	Individual Observations Obtained
Energy Star	355	4,965 (4,970 possible)	19,860 (19,880 possible)
Energy Star with $50 mail-in Rebate	349	4,877 (4,886 possible)	19,508 (19,544 possible)
Green Power Partnership	338	4,721 (4,732 possible)	18,884 (18,928 possible)
Climate Leaders	353	4,938 (4,942 possible)	19,752 (19,768 possible)

Empirical Model

- Utility individual *i* receives from *j*th alternative can be represented as:
  - $U_{ij} = \beta' X_{ij} + \varepsilon_{ij}$
    - Where
      - X_{ij} is vector of observed attributes of alternative *j* for individual *I*
      - β is vector of coefficients to be estimated
      - ε_{ij} is error term
  - Can estimate with conditional logit
  - Willingness to pay (WTP) for attribute *k* is
    - $WTP_k = -b_k/b_P$
    - Where b_k is estimated coefficient of for attribute *k* and b_P is estimated coefficient for *price*

Empirical Model

- Conditional logit restrictive in that assumes homogeneity of preferences across participants
  - β vector constant across individuals
  - ε_{ij} independently and identically distributed across individuals and alternatives
- Incorporate heterogeneity
  - Random parameter logit
  - Interact individual-specific characteristics with attribute variables

Empirical Model

- Utility function for random parameter logit
  $U_{tj} = \beta_P P_j + (\beta + \eta_{xt})' X_{tj} + \varepsilon_{tj}$
  - Where
    - *Β* vector of population mean parameters
    - η_{xi} vector representing the stochastic deviation of the individual's preference from the population mean
- WTP for the *k*th attribute is
  $WTP_k = -\bar{b}_k / b_p$
  - Where $\bar{b}_k$ is mean of random coefficients for attribute *k*

Empirical Model

- Interacting attitudinal and/or demographic characteristics (*Z*) with the *k*th attribute means that β_k can be expressed as
  $\beta_k^n = \beta_{k0} + \beta_{k1} Z_1 + \beta_{k2} Z_2 + \cdots + \beta_{kn} Z_n$
- Four models for each survey version
  - Attributes only conditional logit (CL)
  - Attributes only random parameters logit (RPL)
  - Attributes and interactions CL
  - Attributes and interactions RPL

Variable Name	Variable Description
Age	Age/10
Age²	Age squared/1000
Male	1 if male, 0 otherwise
White	1 if white, non-Hispanic, 0 otherwise
Black	1 if black, non-Hispanic, 0 otherwise
Hispanic	1 if Hispanic, 0 otherwise
College	1 if attended some college, 0 otherwise
Degree	1 if earned Bachelor's degree or higher, 0 otherwise
Inc30_60	1 if household income $30,000 - $60,000, 0 otherwise
Inc60_85	1 if household income $60,000 - $85,000, 0 otherwise
Incgt85	1 if household income > $85,000, 0 otherwise
Hhsize	Total number of individuals in household
Ownhome	1 if own home, 0 otherwise
Northeast	1 if lives in Northeast, 0 otherwise
Midwest	1 if lives in , 0 otherwise
South	1 if lives in South, 0 otherwise
West	1 if lives in West, 0 otherwise (reference case)
Metro	1 if lives in metropolitan area, 0 otherwise

Variable Name	Description
ASC	Alternative-specific constant (1 if "None" option, 0 otherwise)
kWh	County-level average electricity rate (¢/kWh) - Energy Star only
Renewable	State-level renewable electricity production (% of total production) - GPP only
Climate	Factor analysis score (Concern about climate change/environment)
Effect	Factor analysis score (Perceived consumer effectiveness)

Factor Loadings

		Factor Weights
Climate Change/Environment (*Climate*)	*Chronbach's α = 0.790*	
We need more government regulations to force people to protect the environment.		0.5439
There is no urgent need to take measures to prevent global climate change today.		-0.5850
The most effective way to combat global climate change is to reduce energy consumption.		0.5793
Global climate change will have a noticeably negative impact on the environment in which my family and I live.		0.6028
The production of electricity from renewable sources such as solar, wind, and biomass is an effective way to combat global climate change.		0.5684
Perceived Consumer Effectiveness (*Effect*)	*Chronbach's α = 0.794*	
When I buy products, I consider how my use of them will affect the environment.		0.7246
By choosing environmentally friendly products, I signal to manufacturers the types of products they should be producing.		0.7170

Results for Attributes Only Models

Attribute	Energy Star w/o Rebate		Energy Star w/ Rebate		Green Power Partnership		Climate Leaders	
	CL	RPL	CL	RPL	CL	RPL	CL	RPL
Price	- - -	- - -	- - -	- - -	- - -	- - -	- - -	- - -
Label	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +
Capacity	+ + +	+ + +	+ +	+ +	+ +	+ + +	+	
French Door					- - -	-		
LG	- - -	- - -	- - -	- - -	- - -	- - -	- - -	- - -
GE		- -					- -	- -
Kenmore	- -	-						
Ice	+ + +	+	+ + +	+ +	+ + +	+ + +	+ + +	+ + +
Water	+ + +	+	+ + +	+ +	+ + +	+ + +	+ + +	+ + +
Ice and Water	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +
ASC			- - -	- - -	- - -	- - -	- - -	- - -

Attributes & Interactions Models

Attribute	Energy Star w/o Rebate		Energy Star w/ Rebate		Green Power Partnership		Climate Leaders	
	CL	RPL	CL	RPL	CL	RPL	CL	RPL
Price	- - -	- - -	- - -	- - -	- - -	- - -	- - -	- - -
Label		+	+ + +	+ + +	+ +		+ + +	
Capacity	+ + +	+	+ +	+ +	+ +		+ +	
French Door					- - -	- - -		
LG	- - -	- - -	- - -	- -	- - -	- - -	- -	- -
GE							- -	- -
Kenmore	- -							
Ice	+ + +	+ +	+ + +	+ +	+ + +	+ + +	+ + +	+ + +
Water	+ + +		+ + +	+ +	+ + +		+ + +	+ + +
Ice and Water	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +
ASC			- - -	- - -	- - -	- - -	- - -	- - -

Attributes & Interactions Models

Attribute	Energy Star w/o Rebate		Energy Star w/ Rebate		Green Power Partnership		Climate Leaders	
	CL	RPL	CL	RPL	CL	RPL	CL	RPL
Age x Label	+		- - -		- - -		- - -	
Age^2 x Label	- -		+ +		+ +		+ + +	
Male x Label			- - -	- -	+ + +		+ +	
White x Label	- - -		- - -	- -	+ + +		+ +	
Black x Label			- - -	- - -	+ + +		+ +	
Hispanic x Label				- -	+ +			
College x Label			+ +	+ +				
Degree x Label			+ +		- -		+ + +	
Inc30_60 x Label							+	
Inc60_85 x Label				- -	+ + +			
Incgt85 X Label	+ + +		+		+ + +	+	+	

Attributes & Interactions Models

Attribute	Energy Star w/o Rebate		Energy Star w/ Rebate		Green Power Partnership		Climate Leaders	
	CL	RPL	CL	RPL	CL	RPL	CL	RPL
Hhsize x Label			-					
Ownhome x Label	+ + +			+ + +	- -		- - -	
Northeast x Label					- - -		- -	
Midwest x Label					- - -	- - -		
South x Label		-	+		- - -	-		
Metro x Label			- - -	- - -			+	
Effect x Label			- -	+	+ + +	+ +	+ +	
Climate x Label	+ + +		+ + +	+ + +	+ + +	+ + +	+ + +	
kWh x Label	+ + +		+ + +	+	*NA*	*NA*	*NA*	*NA*
Renewable x Label	*NA*	*NA*	*NA*	*NA*	- - -		*NA*	*NA*

WTP for "Labels"

Program	Attributes Only Models			
	Conditional Logit		Random Parameter Logit	
	Mean WTP ($)	S.E.	Mean WTP ($)	S.E.
Energy Star without rebate	244.06***	55.71	285.00***	104.01
Energy Star with rebate	172.94***	30.63	207.83**	105.67
Green Power Partnership	53.18***	16.83	68.66*	36.97
Climate Leaders	70.26***	18.43	66.15***	21.41

WTP with or without Rebate

Agreement with Statements about Energy Star (1 = Strongly Disagree. . . 5 = Strongly Agree)	ES w/o Rebate	ES w/ Rebate	t
People buy products that have the Energy Star label to save money on electric bills.	3.95	3.92	0.37
People who buy products with the Energy Star label are concerned about the environment.	3.66	3.60	0.77
Buying Energy Star-labeled products makes me feel like I'm helping to protect the environment for future generations.	3.65	3.59	0.78
When I buy a product with the Energy Star label, I can always be sure it's high quality.	3.36	3.17	2.69

Conclusions

- Consumers are willing to pay premium for refrigerators awarded Energy Star label or manufactured by a company that participates in the Green Power Partners or Climate Leaders programs (at least in hypothetical choice experiment)

Conclusions

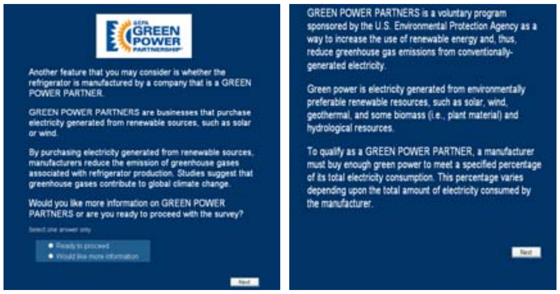
- WTP significantly greater for Energy Star labeled refrigerator than for refrigerators manufactured by participants in Green Power Partner or Climate Leader programs
  - Greater familiarity
  - Consumer labeling program – use vs. manufacture
  - More explicit benefits
  - *Public* and *private* benefits

Conclusions

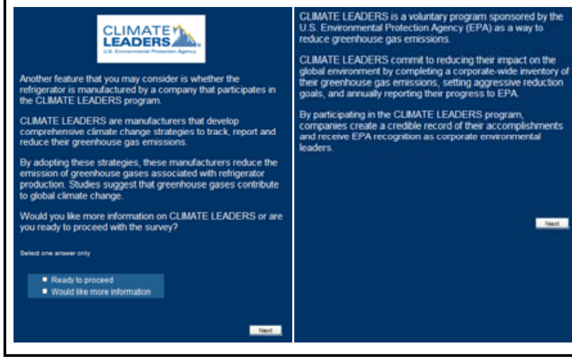
- Little or no evidence of motivation crowding out
- Evidence that rebate reduces perceptions of product quality

Energy Star with Rebate Screens

GPP Information Screens



Climate Leaders Information Screens



WTP Estimates (ES w/o Rebate)

	Attributes Only Models			
	Conditional Logit		Random Parameter Logit	
Attribute	Mean WTP ($)	S.E.	Mean WTP ($)	S.E.
Label	244.06***	55.71	285.00***	104.01
Capacity	23.08*	15.95	21.10*	16.26
French door	-15.40	24.11	-17.96	84.41
LG	- 59.54	35.60	-65.54	37.66
GE	-23.57	33.16	-24.49	26.97
Kenmore	-35.55	34.85	-31.55	35.83
Ice	70.98*	37.12	47.43	55.75
Water	57.93*	37.72	36.29	41.20
Ice & Water	239.96***	60.35	216.02**	105.95

WTP Estimates (ES w/ Rebate)

	Attributes Only Models			
	Conditional Logit		Random Parameter Logit	
Attribute	Mean WTP ($)	S.E.	Mean WTP ($)	S.E.
Label	172.94***	30.63	207.83**	105.67
Capacity	13.18	10.98	12.07	10.14
French door	-4.67	17.45	12.35	36.22
LG	-40.41	25.08	-29.04	22.19
GE	-0.40	23.46	-4.46	26.03
Kenmore	-12.94	24.87	-7.68	22.59
Ice	62.78**	26.05	44.82	35.54
Water	57.80**	26.61	40.13	32.12
Ice & Water	189.42***	35.61	189.82***	59.97

WTP Estimates (GPP)

	Attributes Only Models			
	Conditional Logit		Random Parameter Logit	
Attribute	Mean WTP ($)	S.E.	Mean WTP ($)	S.E.
Label	53.18***	16.83	68.66*	36.97
Capacity	10.62	10.13	13.08	10.09
French door	-40.66	17.44	-45.39	53.59
LG	-45.48	21.86	-64.92	29.87
GE	-9.55	21.31	-13.96	23.65
Kenmore	-15.93	21.43	-13.63	20.80
Ice	66.94***	23.48	49.02*	25.79
Water	37.57	23.31	24.66	23.16
Ice & Water	164.12***	59.62	149.24*	90.65

WTP for Attributes (Climate Leaders)

	Attributes Only Models			
	Conditional Logit		Random Parameter Logit	
Attribute	Mean WTP ($)	S.E.	Mean WTP ($)	S.E.
Label	70.26***	18.43	66.15***	21.41
Capacity	9.48	10.55	8.76	11.42
French door	5.28	16.08	-20.58	64.60
LG	-34.79	23.39	-36.52	24.74
GE	-25.57	25.86	-30.65	25.42
Kenmore	-2.20	22.75	-4.17	25.99
Ice	107.32***	27.62	86.42***	31.20
Water	75.64***	27.07	48.21**	23.94
Ice & Water	229.12***	40.91	183.53***	61.56

Evaluating Alternative Approaches to Energy Efficiency Labeling: Designing and Implementing a Choice Experiment

Juha Siikamäki

Resources for the Future

Tuesday, January 18, 2011
Washington, DC

Presentation at the
"Benefits of Environmental Information Disclosure"
U.S. EPA National Center for Environmental Economics (NCEE), National Center for Environmental Research

Study Overview

- A survey experiment to evaluate
  - Households' appliance choice and valuation of energy efficiency
  - Energy efficiency labeling alternatives
- Work-in-progress with some preliminary result from a limited sample (pilot)
- This presentation focuses on explaining
  - Study motivation and purpose
  - Survey design and implementation
  - Choice experiments
- Part of a larger EPA STAR funded project "A Conceptual and Empirical Framework for Analyzing Information Disclosure Programs," including Ian Parry, Wally Oates , Carolyn Fischer, Tom Lyon, and Richard Newell

Background and Motivation

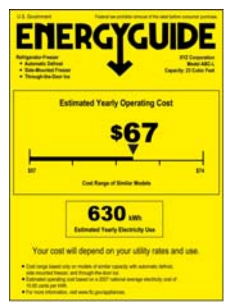
- Energy paradox
  - Households' valuation of energy efficiency low, seemingly illogical
  - Rationalizing current choices often requires 30-40% hurdle rates (Jaffe et al. 2004)
- Possible explanations (Jaffe and Stavins , 1994 Res and Energy Econ)
  - Information problems
  - Principal/agent problems
  - Unobserved costs

  → Market failures

  - Private information costs
  - Discount rates
  - Heterogeneity

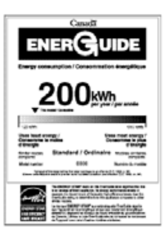
  → Not market failures
- Labeling programs to address information problems (and costs)
  - Energy Guide (lots of information)
  - Energy Star (minimal information)

Current Energy Labels: Energy Guide (relative) and Energy Star ("endorsement")

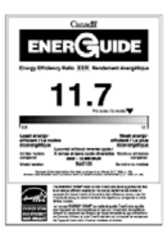


ENERGY STAR

EnerGuide Canada



Example of an EnerGuide label for an ENERGY STAR qualified appliance.

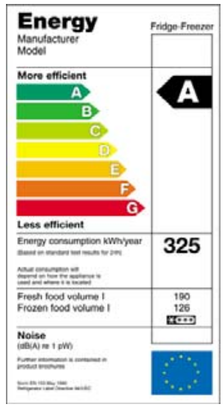


Example of an EnerGuide label for an ENERGY STAR qualified room air conditioner.

Source: Energy Efficient Strategies (EES)

Mexico: Eficiencia Energética

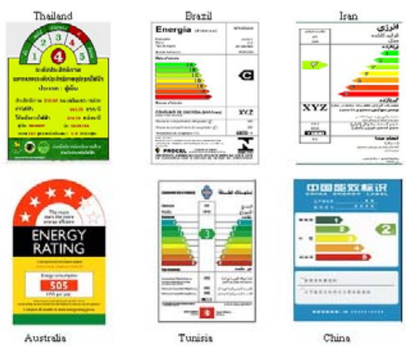
European Union



Korea

Source: Energy Efficient Strategies (EES)

More Labels



Source: Energy Efficient Strategies (EES)

Study Goals

- Evaluate alternative labeling approaches in the context of households' preferences for energy efficiency
  - Many labels in use, but systematic research lacking on whether or how they affect choices
  - Does information content and complexity matter?
  - What are the effects of multiple labels?
- Disentangle the effects of different drivers of valuation of energy efficiency
  - Discount rates (elicited in the survey)
  - Individual heterogeneity (preferences, personal/household situation; part elicited, part modeled)
  - Commonly unobserved "costs," such as income and credit constraints, cost of credit, likelihood of moving (elicited in the survey)
- Complement earlier research on both items above
  - Past research on energy efficiency relatively rich
  - Alternative labels studied by, for example, FTC (2008) and EPA (fuel economy), but not using a choice setting
  - Different drivers separately evaluated by a large number of studies; here we seek to jointly evaluate the relative importance of different factors

Study Approach

- Basic setting
  - Household survey (responses from 1,000 households)
  - Fully computerized survey instrument which is customized as each survey respondent progresses through it
  - Evaluate sudden water heater replacement decisions
  - Elicit choices between different water heater alternatives
  - Different alternatives randomly but realistically varied by price and energy use
  - Labeling approach randomly varied by respondent
  - Use elicited data to estimate households' valuation of energy efficiency under different labeling treatments
  - Elicit data on discount rates, credit situation, likelihood of moving, and so forth; use those data to examine the relative importance of different drivers of preferences for energy efficiency
- Strengths of using a survey based approached
  - Enables randomized experiments
  - Enables using a controlled, simplified, and uniform setting across different households
  - Focuses on the essential features of information disclosure
  - Enables examining labeling alternatives currently not in the market
- Possible limitations
  - Though realistic, the setting somewhat different from actual choices (for example, the label and energy information prominently displayed)
  - Hypothetical choices may differ from actual behavior, though the survey includes recommended reminders to choose as in reality
  - Data probably most robust for estimating relative treatment effects; especially the estimates of households' absolute valuation of energy efficiency must be interpreted given the overall approach

Why Water Heater?

- Practically every house has one
- Sudden replacement (imposed in the survey) is conceivable
- Investment and annual energy cost both considerable
- Relatively uniform in functionality, installation, usage, available models, quality
  - Helps abstract away "irrelevant" attributes
  - Brand considerations not central
- Also considered window AC units and clothes washers/dryers
  - Difficult to formulate a uniform yet realistic model across all households
  - Sudden replacement less realistic
  - Usage and models vary considerably
  - Occurrence of especially window AC relatively rare

Sample

- Knowledge Networks computerized survey panel
- Owners of single-family homes (detached, attached)
- Heads of household selected as respondents
- 100 households for each of treatments
- Randomized treatments enable clean identification of treatment effects

Survey Outline

1) Introduction
2) Describe your current water heater (fuel, capacity, age)
3) Considering having to suddenly replace the water heater, how importance are different considerations to your new water heater choice?
4) Choice questions (introduction + 6 choices, each with three alternatives)
5) Questions on payback time, WTP for energy savings
6) Series of questions eliciting individual discount rates
7) Questions on current credit situation, loans, loan rates

Labeling Alternatives (Treatments) Evaluated in the Pilot Stage

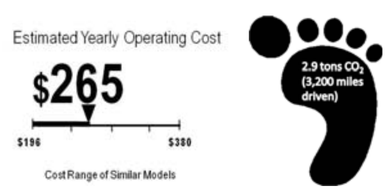
1. Energy Guide Information
2. Simplified Energy Guide (no range)
3. Energy Guide Information plus CO_2
4. Energy Efficiency Grade (EU, elsewhere)
5. Energy Guide Information plus Energy Star (multiple labels)

- Focus on information content and complexity
- Final sample will include modifications to treatments and possibly additional treatments

Expressing Annual Energy Cost: Treatment 1, Energy Guide

Treatment 2: Simplified Energy Guide

Treatment 3: Energy Guide Plus CO2



Treatment 4: Energy Efficiency Grade

Treatment 5: Energy Guide Plus Energy Star

ENERGY STAR

Choice Question Treatment 1

Consider choosing between the following three water heater options. Please think that these are the only options available to you and you have to make the purchase. Which water heater alternative would you choose from the alternatives below?

Water Heater Decision 1

	Water Heater A	Water Heater B	Water Heater C
Purchase price	$400	$650	$550
Energy Use	Estimated Yearly Operating Cost $357	Estimated Yearly Operating Cost $265	Estimated Yearly Operating Cost $288

Your choice from these options?	☐ A	☐ B	☐ C

Choice Question Treatment 2

Consider choosing between the following three water heater options. Please think that these are the only options available to you and you have to make the purchase.

Water Heater Decision 1

	Water Heater A	Water Heater B	Water Heater C
Purchase price	$400	$650	$550
Estimated Yearly Operating Cost	$357	$265	$288

Your choice from these options?	☐ A	☐ B	☐ C

Choice Question Treatment 3

Consider choosing between the following three water heater options. Please think that these are the only options available to you and you have to make the purchase.

Water Heater Decision 1

	Water Heater A	Water Heater B	Water Heater C
Purchase price	$400	$650	$550
Energy Use	Estimated Yearly Operating Cost $357	Estimated Yearly Operating Cost $265	Estimated Yearly Operating Cost $288
Carbon Footprint	3.9 tons CO_2 (4,300 miles driven)	2.9 tons CO_2 (3,200 miles driven)	3.2 tons CO_2 (3,400 miles driven)

Your choice from these options?	☐ A	☐ B	☐ C

Choice Question- Treatment 4

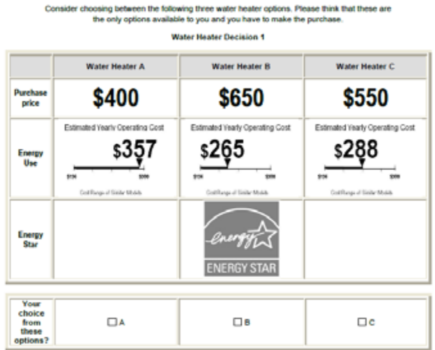
Consider choosing between the following three water heater options. Please think that these are the only options available to you and you have to make the purchase.

Water Heater Decision 1

	Water Heater A	Water Heater B	Water Heater C
Purchase price	$400	$650	$550
Energy Use	More efficient A B C D E F G Less efficient; G; Estimated Yearly Operating Cost 357	More efficient A B C D E F G Less efficient; C; Estimated Yearly Operating Cost $265	More efficient A B C D E F G Less efficient; D; Estimated Yearly Operating Cost $288

Your choice from these options?	☐ A	☐ B	☐ C

Choice Question- Treatment 5


Consider choosing between the following three water heater options. Please think that these are the only options available to you and you have to make the purchase.

Water Heater Decision 1

	Water Heater A	Water Heater B	Water Heater C
Purchase price	$400	$650	$550
Energy Use	Estimated Yearly Operating Cost $357	Estimated Yearly Operating Cost $265	Estimated Yearly Operating Cost $288
Energy Star		ENERGY STAR	
Your choice from these options?	☐ A	☐ B	☐ C

Choice Experiment Design Features

- Focuses on evaluating alternative labels and valuation of energy efficiency
  - Only the labeling treatment varies across respondents; the instrument is otherwise exactly the same across all respondents
  - Different labels designed with maximal consistency (same font size, font type, type of information presented, and so forth)
- Purchase price and annual operating costs range similarly to models currently in the market
- Water heater fuel (natural gas, electricity, propane, oil) determines annual operating cost (respondent's answers determine the survey design)
- Estimated CO2 emissions (treatments 7 & 8) also realistic
  - Expressed using pounds and miles driven equivalent (average US passenger vehicle, 21 mpg, 19.4 lbs CO_2 per gallon)
  - Emissions vary by water heater fuel (electric 0.524 lbs per kWh, natural gas 13.446 lbs per therm)
- Statistical experimental design purposed to help precisely identify treatment effects (if any exist in reality)
  - Very large number of possible overall designs exists (attribute levels are varied in each choice question, six different choices are presented to each respondents, and the sets of attribute levels and choices are varied across the respondents)
  - Chosen design maximizes statistical efficiency given the sample size (min standard errors)
  - Alternative designs evaluated using D-efficiency criterion and a Bayesian approach
  - Most efficient design systematically searched (first loop) and improved (second loop) under different starting sets (third loop)
  - Strictly dominated alternatives eliminated from each choice set
  - One hundred possible attribute-level designs, eight labeling treatments, two fuel options
  - Survey instrument individualized and populated mid-survey (using randomization and elicited responses)
  - Large number of possible survey instruments (total 1,000 with five treatments here; more will be used in the final sample)
  - Exactly similar attribute level designs across treatments helps the estimation of treatment effects

Estimating Tradeoffs

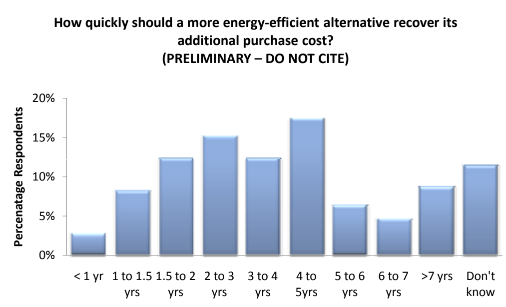
- Predict the probability of elicited choices as a function V=f(x) of the attributes of each alternative in the choice set
- Here, $V=b_{iP}*Price+b_{iEE}*EnergyUse$
  - b_{iP} estimates the effect of purchase cost
  - b_{iEE} estimates the effect of annual energy cost, conditional on labeling treatment
- WTP for energy efficiency: $\dfrac{\dfrac{dV}{dEnergyCost}}{\dfrac{dV}{d\mathrm{Price}}} = \dfrac{\beta_{EE}}{\beta_P}$

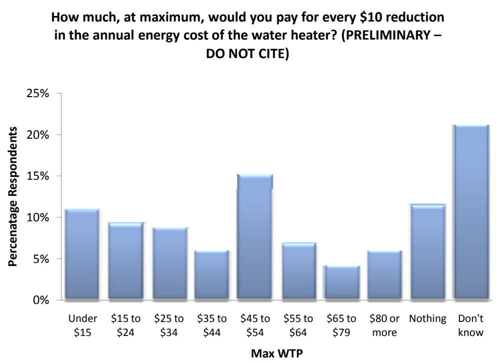
Eliciting Individual Specific Discount Rates

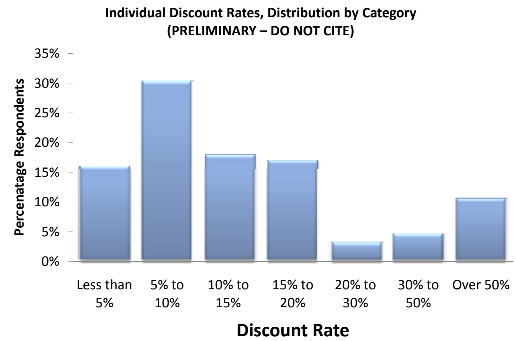
- Adapted from experimental economics (e.g., "Eliciting Individual Discount Rates," M Coller, M Williams, *Experimental Economics, 1999)*
- Elicit choices between two cash-credit alternatives
  - Credit A is delivered in one month
  - Credit B is delivered in 12 months
  - Both tax free, certain, the only difference is the delivery date and credit amount
- Credit A always equals $1,000; Credit B is greater
- Sequence of questions with varying credit B
  - Starting at $1,019 (2% annual rate), and gradually increasing to up to $2,501
  - Stop when the respondent switches to the 12-month option
- Individual discount rate (range) implicit in the choices

Some preliminary results from the pilot sample

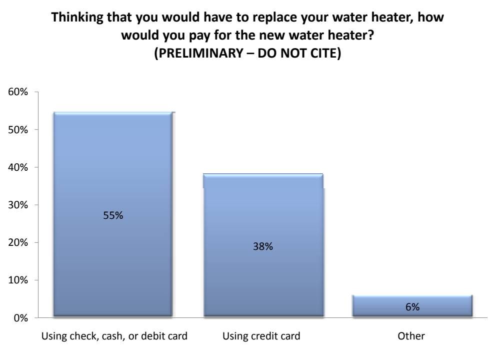
- Survey administration currently underway
- The following results are from a pilot sample of 217 households
- Presented mostly for illustration
- Do not cite -- absolute numbers will change as more data come in







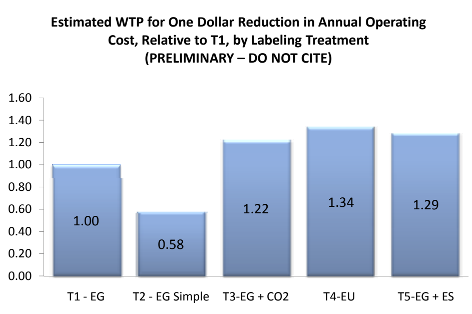
Mean 19%, Median 11%



Choice Model Parameter Estimates: Results from the Pilot Survey

Coefficient	Estimate	Standard Error	T-value	P-value
$ß_P$	-0.144	0.012	-11.721	0.000
$ß_{EE\_T1}$	-0.899	0.120	-7.469	0.000
$ß_{EE\_T2}$	-0.520	0.111	-4.689	0.000
$ß_{EE\_T3}$	-1.097	0.146	-7.492	0.000
$ß_{EE\_T4}$	-1.201	0.145	-8.313	0.000
$ß_{EE\_T5}$	-1.156	0.147	-7.868	0.000
Choices	1302			

Note
1) Early results using limited sample -- do not cite
2) Fixd logit model model with uncorrelated errors within-subject



Testing for Differences between Treatments (T1 is the baseline)

Coefficient	Estimate	Standard Error	T-value	P-value
$ß_P$	-0.145	0.012	-11.721	0.000
$ß_{EE\_T1}$	-0.899	0.120	-7.470	0.000
$ß_{EE\_T2}$	0.379*	0.140	2.712	0.007
$ß_{EE\_T3}$	-0.198	0.165	-1.200	0.230
$ß_{EE\_T4}$	-0.302*	0.164	-1.845	0.065
$ß_{EE\_T5}$	-0.257	0.166	-1.547	0.122
Choices	1302			

Note
1) * indicate statistically significantly different treatment effect relative to T1 *
2) Early results using limited sample -- do not cite

Next Steps

- Complete the first stage (comprising the five treatments discussed here) of survey administration (about one more week)
- Examine the data, estimate treatments effects, and determine additional treatments in the rest of the sample (about 750 respondents)
- Econometric modeling
  - Thoroughly examine heterogeneity:
    - observed (discount rates, likelihood of moving, credit constraints, credit rates)
    - unobserved (random coefficient models)
  - Estimate a panel model
- Decompose WTP for energy efficiency into factors related to discounting, credit constraints, likelihood of moving, unobserved preference heterogeneity
- Conduct policy evaluations (e.g., what energy use implications, if any, might different labeling programs have)

Some Early Observations

Results

- Treatment 2 (not including the range) seem to reduce WTP for energy savings
- Treatments 3 (CO_2),4 (Energy Efficiency Grade), and 5 (Energy Star + Energy Guide) all seem to increase WTP for energy savings
- Qualitative results probably somewhat robust, but the relative magnitude of the treatment effects cannot be determine yet (need more data)
- Elicited individual discount rate on average about 19% (highly variable) with median at 11%

Overall:

- Choice experiments and randomized treatments have the potential to inform the design of labeling programs
- Stated choice experiments are advantageous, but would be complemented by field experiments examining the primary labeling alternatives

Again, as a reminder, this is work-in-progress, and neither the results nor the survey design presented here are final.

Discussant Comments for:

Factors Affecting the Willingness to Pay for the ENERGY STAR Label

Chris Moore
US EPA
National Center for Environmental Economics

Benefits of Environmental Information Disclosure
18 January 2011

Purpose

- Estimate WTP for ENERGY STAR label
- Infer WTP for public benefits

Data

- Stated choice survey via Knowledge Networks
  - Right choice of format: statistical efficiency; incentive compatibility; familiar scenario for consumers...
  - National representative sample
- Question design?
  - Figure or appendix
- Truncated choice set?
  - top freezer in $500-$600 range?

Economic Model

- RUM framework
  - Appropriately brief discussion
- Modeling preference heterogeneity
  - Random parameters logit
  - Interact *ES Label* with conditioning variables

Economic Model

- *Justification* for fixing some parameters in the RP model…
  - β_p - "homogeneity is expected across individuals with respect to price..."
  - β_k - "relationships between the demographic/ attitudinal variables and preferences for the label are expected to be similar across individuals..."
- Specification of attitudinal variables
  - Factor analysis scores
  - Sensitivity to other specifications?

Results

- WTP > Private cost savings
- Interaction terms
  - Unexpected signs on *Male* and *Inc_elec*
  - Specification problems?
    - Multicollinearity, measurement error, inverse proxy for income

Results Discussion

- Preference heterogeneity
  - How much is explained by observable conditioning variables?
  - Comparison of standard deviation on β_{label} in two RP models.
- Warm Glow?
  - ES Label is qualitative → can't test sensitivity to scope...
- WTP for Public benefits
  - Debriefing questions could have explored this further

Overall Comments

- Neat and clean analysis
- Well organized paper
- Very coherent discussion of results and interpretation of interaction terms
- Speaks to good experimental design and econometric specification

Suggestions

- What do the results say about the alternative approaches to capturing preference heterogeneity?
- Practical rather than theoretical justification for holding coefficients fixed in RP model
- Alternative specifications for attitudinal variables
- Show us the question design
- If possible, compare results with other studies

Specific/Editorial Comments

- *"kWhrate"* in Table 2, "*kWh"* in text
- Include a year with dollar amounts (can we compare Revelt and Train results with yours?)

Maureen McNamara, EPA, Climate Protection Partnerships Division

For more information about this presentation, please see page 3 of the Meeting Summary included in this PDF.

THE IMPACT OF QUASI-REGULATORY MECHANISMS ON POLLUTING BEHAVIOR:

EVIDENCE FROM POLLUTION PREVENTION PROGRAMS AND TOXIC RELEASES

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The Impact of Quasi-Regulatory Mechanisms on Polluting Behavior: Evidence From Pollution Prevention Programs and Toxic Releases

Abstract:

To date, there is little convincing evidence on the effectiveness of "quasi-regulatory" mechanisms. Here I investigate how quasi-regulatory policies known as pollution prevention ("P2") programs affect toxic pollution. I construct a data base on state-level P2 programs as well as the 1990 federal Pollution Prevention Act (PPA) and exploit variation in state adoption dates and program characteristics to study their effects on facility-level releases. I find strong evidence that these mechanisms can affect pollution outcomes. In particular, I find that (1) the 1990 PPA has had a significant effect on toxic releases; (2) state programs geared to reducing the costs of P2 activities led to significant reductions in toxic releases; and (3) the response to P2 programs that increased the regulators' ability to monitor polluting behavior could either increase or decrease reported releases, depending on the regulators' ability to verify the accuracy of the reported releases.

Key Words:

TRI, quasi-regulation, voluntary programs, toxic pollution.

THE IMPACT OF QUASI-REGULATORY MECHANISMS ON POLLUTING BEHAVIOR: THE CASE OF POLLUTION PREVENTION PLANS AND TOXIC RELEASES

I. INTRODUCTION

Environmental regulation in the United States has evolved slowly from the traditional command and control strategies dominant during the early 1970s to the more market-based regimes that we see today. Those market-based approaches include voluntary programs and initiatives, or "quasi-regulations," aimed at incentivizing pollution reduction without legally requiring abatement by polluters. Quasi-regulatory mechanisms are becoming more frequently used by regulators, particularly for pollutants that are not easily regulated using command and control strategies. Currently, there are more than 50 such voluntary programs and initiatives at the federal level,[1] with several dozen more at the state level. Given the growing reliance that regulators are placing on such mechanisms it is important to understand how, (if at all) they affect polluter behavior.

To date, much of the empirical work on the effectiveness of quasi-regulatory mechanisms has been unconvincing.[2] That stems, in part, from the difficulty of separating the effects of the various elements of formal and informal environmental regulations that confront polluters. Weak identification strategies dictated by data limitations have also proved problematic. To address some of those problems, I make use of micro-level data on toxic releases, and focus on a particular set of quasi-regulatory initiatives called pollution prevention ("P2") programs. I develop a detailed data set on P2 programs and exploit variation in adoption dates to estimate their effects on facility level

[1] Brouhle, et al., 2004

[2] For an excellent overview, see Brouhle, et al., 2004. See also Bennear (2007), and Stafford (2003).

behavior. The potentially confounding effects of various formal regulatory measures, as well as international agreements, are also addressed. Different control groups are used to validate the robustness of the results.

The programs that I study include the federally mandated Pollution Prevention Act (PPA) of 1990 as well as 38 different state-level P2 programs. Those programs primarily target hazardous waste, toxic waste, and toxic releases. P2 programs aim to reduce pollution by encouraging "source reduction and other practices that reduce or eliminate the creation of pollutants through: increased efficiency in the use of raw materials, energy, water, or other resources; or [the] protection of natural resources by conservation."[3] P2 programs range from offering awards that publicly acknowledge exemplary pollution prevention initiatives to implementing non-reporting penalties; from providing free on-site technical assistance and educational outreach, to joint research initiatives between local government and industry.

Using a balanced panel consisting of more than 7100 manufacturing facilities over a 16 year period, I find strong evidence that both federal and state P2 programs have had a significant effect on polluter behavior. In particular, I find that (1) the 1990 PPA was responsible for reductions in average facility level releases of between 65%-76%; and that (2) the adoption of state P2 programs corresponded to a decline in average facility releases of between 11.5% - 12.4%. I also find that the state "adoption" effect is much larger for facilities located in early adopting states (24%) than in late adopting states (5%). Those results are robust to using either a balanced or unbalanced panel of manufacturing facilities, as well as to changes in the range of years used in the analysis. A test to determine whether the results are driven by spurious correlation is soundly rejected.

[3] EPA OPPT Overview - Draft Version 2.0.

Of the different state P2 programs that I study, programs that reduced the cost of participation, in particular, technical assistance and educational outreach programs have been the most successful at reducing toxic releases. I find, however, that the timing of the reductions depends upon a number of factors, including the length of time the program has been in place as well as whether other states have already adopted similar programs. There is strong evidence to suggest that spill-over effects play an important role in the effectiveness of these types of programs.

State P2 programs that increase the ability of regulators to monitor polluters, such as filing fees and non-reporting penalties, are also found to have had an effect on polluter behavior. Surprisingly, filing fees tend to increase reported releases. This, however, could reflect a change in *reporting* behavior, and not necessarily a change in *polluting* behavior. Non-reporting penalties, over-all, however, were mostly ineffective at altering facility behavior, except in the case of toxic substances that could be easily monitored by regulators. For those substances, non-reporting penalties led to lower levels of reported releases. I argue that the ineffectiveness of non-reporting penalties may reflect a fundamental problem facing regulators of toxic releases that arises because regulators cannot validate the accuracy of the self-reported toxic releases.

The paper is organized as follows. In Section II I provide regulatory background on federal and state level statutes. Section III describes the data used in the estimation, while Section IV discusses the model used in the estimation. Section V provides basic summary statistics for the data and in Section VI I discuss estimation results. Section VII concludes.

II. BACKGROUND

Toxic substances are those that are either known to be, or are suspected of being, hazardous to human health at low levels of exposure. Their storage, handling, transportation, and disposal are all strictly regulated. Yet, for many of these substances there is no formal regulation of their *release* into the environment. In part, this may be due to the mandate given to the EPA to set standards that protect health and human welfare. If a substance is known to be toxic at low levels of exposure, the appropriate emissions standard may be zero. Banning a substance, however, is not always feasible. Given that difficulty, regulations of toxic releases, as a whole, are not as well defined or as comprehensive as those facing conventional pollutants. Instead, toxic releases often face quasi-regulations aimed at promoting voluntary abatement. I describe below the most relevant regulations applicable to toxic releases.

Clean Air Act and Clean Water Act: A subset of TRI chemicals is regulated under the Clean Air Act, and its amendments. Such air pollutants may be regulated as hazardous air pollutants (HAPs) under the National Emissions Standard for Hazardous Air Pollutants (NESHAP), or as conventional pollutants (fine particulate matter (PM) or volatile organic compounds (VOCs)) under the National Ambient Air Quality Standards (NAAQS). In general, these regulations impose technology standards. The Clean Water Act also affects a subset of TRI chemicals, although the set of regulated chemicals is significantly smaller. Such substances also face technology based standards. In most instances, these standards are industry and (typically) state-specific.

Toxic Release Inventory: The Toxic Release Inventory was introduced by the 1986 Emergency Planning, Community Right to Know Act. Originally, only facilities in the manufacturing sector (SIC 2000-3999) that either used or manufactured more than a threshold level of a TRI "listed" substance were required to report their toxic releases to a publicly maintained data base. In 1988, approximately 300 substances were listed as TRI chemicals. The list of chemicals, threshold levels, and required TRI participants has changed over time. Currently, over 600 chemicals are listed, and the group of required participants has expanded to include such industries as electric utilities as well as government facilities

TRI 33/50 Program: The 33/50 program was initiated as a voluntary program in conjunction with TRI reporting. The EPA invited over 5000 companies to voluntarily participate in reducing releases of 17 TRI priority substances, by one-third by 1992 (from 1988 baselines) and by one-half by 1995. The program was deemed a success: target reductions were more than fully met by 1994.

1990 Pollution Prevention Act: The 1990 Pollution Prevention Act authorized the EPA to support the adoption of source reduction techniques by business, governments, and other organizations. In part, this support comes in the form of federally operated P2 programs such as Design for the Environment (DfE), which involves joint government-industry research initiatives to provide detailed information on source reduction activities. DfE has targeted industries such as dry cleaners and producers of printed wire boards, which are known to produce large volumes of toxic releases, and are dominated by small and medium sized polluters for whom investing in P2 research on their own is generally infeasible. The PPA also provides grants to states for state technical assistance programs and, promotes the exchange of information through the EPA's Pollution Prevention Resource Exchange (P2Rx), which supports 8 regional P2 information centers. Those programs are all aimed at lowering the cost to polluters of engaging in P2 activities through information dissemination.

Aside from direct support of P2 activities, the PPA requires that TRI reporters include information on source reduction and recycling activities. It also established a national awards program to "recognize a company or companies operating outstanding or innovative source reduction programs."

Pollution Prevention Programs and Toxic Use Reduction Acts (TURAs): Several states have adopted P2 legislation apart from the federal PPA. Some 27 state P2 programs were adopted prior to 1990, the first in 1984. Such programs focus on the reduction of solid and hazardous wastes as well as toxic releases. Much like the federal PPA, state P2 characteristics include programs for technical assistance, educational outreach, grants, and awards. But in contrast with the PPA, many states impose filing fees and non-reporting penalties for TRI reporters.

Another aspect unique to state programs is that some have prescribed reduction goals for toxic releases and hazardous waste production. The objectives have ranged from 30% - 80% from some baseline year. Such reduction targets are established on a state-wide basis, however, there are no penalties for non-compliance or other enforcement mechanisms in place.

Montreal Protocol: The Montreal Protocol is an international agreement, entered into in 1987 to be effective on January 1, 1989. Signatories of the Protocol agreed to a phase-out plan for the use (consumption and production) of chlorofluorocarbons (CFCs) and hydrochlorofluorocarbons (HCFCs), both of which are monitored by the TRI. The plan allowed for an increase in "Group 1 of Annex A" substances up through 1992 (with allowable increases capped at 150% of 1986 levels), but then required a rapid phase-out; to a target of no more than 25% of 1986 levels by 1994, and complete elimination by 1996. Slower phase-outs were prescribed for other substances.

III. DATA

Toxic release data are taken from the EPA-TRI website (www.epa.gov/tri/tridata) for reporting years 1988-2003. The data are given by chemical and facility. Because threshold reporting levels, reporting chemicals, and required reporters changed during this period, the bulk of my analysis is restricted to that set of chemicals that are reported for all years between 1988-2003, for which the reporting threshold did not change. I also limit myself to the balanced panel of facilities in the manufacturing sector which have been required to report to the TRI since 1988 (SIC 2000-3999).

Information on state-level pollution prevention legislation and programs are taken from a variety of sources, including the Right-to-Know Planning Guide (1997, the Bureau of National Affairs, 0-871-931-1/97), the 1999 State TRI Program Assessment, and state environmental websites. A total of six different P2 programs were found. These consisted of technical assistance programs, educational outreach programs, grants and financial aid, award programs, filing fees, and non-reporting penalties.

In addition to the different programs offered, state P2 programs also differed in one other important dimension – their level of stringency. To capture this difference, I classify states into 1 of 2 categories: low and high stringency states. Low stringency states are those that have no target reduction goals for toxic releases, whereas high stringency states are those that have state-wide numeric reduction goals for toxic releases with specific compliance dates.

IV. Basic Model and Methodological Issues

To estimate the effect of P2 programs on releases, (reduced form) releases are modeled as:

$$\text{(1)} \qquad \ln(TRI_{ijst}) = \beta_0 + \beta_1 PPA_t + \Gamma z_{st} + \delta_i + \sigma_j + \gamma_t + \epsilon_{ijst}$$

where *ln(TRI)* is the natural log of facility-level TRI releases for facility *i*, in industry *j*, state *s*, and year *t*. *PPA* is an indicator variable controlling for the 1990 PPA which takes on the value of 1 from

1990 - 2003, and 0 otherwise. *Z* is a vector of P2 state-level programs, differentiated by their basic characteristics (eg. provision of technical assistance or non-reporting penalties) that take the value of 1 if the state has a particular program in a given year, and 0 otherwise. Indicator variables are included to capture various fixed effects at the facility (δ), industry (σ), and year (γ) level. ϵ is assumed to be a well behaved random error term with a conditional mean of zero.

For the above to consistently estimate β_1 and Γ, the "treatment" variables must be uncorrelated with any time-varying unobservables that affect facility level releases: in other words, ϵ must be orthogonal to the adoption of federal and state-level P2 programs. It is important to recognize that it is unlikely that any endogeneity arises due to *facility level* releases being correlated with federal and state-level P2 adoption dates or program choices, primarily because individual facilities are not generally large enough to influence the state level regulatory environment.[4]

V. DESCRIPTIVE STATISTICS

The balanced panel of TRI facility data are from 7157 facilities, yielding 114,512 facility-year observations between 1988-2003. This consists of approximately 33% of all available facility-year observations in the TRI. Summary statistics are given in Table I.

Average, annual facility level releases of TRI substances are 251,996 pounds. Of those, by weight, 57% are air, 2% are water, and 41% are land (and underground) releases. Due to the potential confounding effects of the CAA and the 1990 CAAA, I also report descriptive statistics for toxic releases net of any CAA substances. In total, 39% of all TRI releases face formal command and control regulation under the Clean Air Act ("CAA air releases"), leaving 61% ("TRI Net of

[4] Endogeneity would be far more likely if releases were aggregated to the industry or state level.

CAA") of aggregate TRI releases primarily facing quasi-regulations. TRI 33/50 substances make up, on average, 21% of facility level TRI releases, but almost all of those substances are also regulated under the CAA. TRI 33/50 substances net of CAA substances only constitute 0.04% of average facility level aggregate TRI releases. Montreal Protocol substances contribute under 2% of aggregate TRI releases, and Montreal Protocol substances net of CAA substances make up approximately 0.3% of aggregate TRI releases.

With respect to state-level P2 programs, technical assistance programs affect 65% of facility-year observations with approximately 20% of facility-years having educational outreach opportunities. In all instances, educational outreach is offered in conjunction with a technical assistance program. Grants are offered in 43% of facility-years, and 11% have award or recognition programs. Filing fees are instituted in 61% of facility-years, and 64% have non-reporting penalties.

In columns 2 and 3 of Table 1, summary statistics are given for the facilities pre and post adoption of a state P2 program, with the average change between those periods shown in the last column. Data for the year of adoption is *not* included in either column. On average, aggregate facility level releases were more than 39% (37% for net TRI) lower by weight after the adoption of a state P2 program. Net TRI 33/50 substances were 22% lower, net Montreal Protocol substances were 86% lower, and CAA air releases were 44% lower. Although these reductions are impressive, whether they can be attributed to the adoption of P2 programs or to other factors such as improvements in abatement technology over time, changes in output levels, regulatory changes, or something else, cannot be determined from the descriptive statistics, alone.

In determining how facility level toxic releases respond to P2 programs, care should be given to the possibility of "equilibrium sorting" where firms make location choices based on compatibility with certain state characteristics. For example, "green" firms may be more likely to locate in more environmentally forward states. If so, facility response may systematically differ across groups based on these (potentially unknown) state characteristics, in which case, estimates based on the whole sample may obscure important behavioral patterns in the data. To allow for this, I also group facilities by: (1) whether they are located in a state that is an "early" or "late" adopter of P2 programs, relative to the 1990 PPA, and (2) whether they are located in a state which has a "low" P2 stringency level (no target reduction goal) or a "high" P2 stringency level (specified target reduction goal).

In Table II, Panels 1 and 2, facilities are grouped by whether they are located in a state that adopted a P2 program before 1990 ("early" adopter) or after 1990 ("late" adopter). Facilities located in states that adopted a state program in 1990 are not included in the calculations of the descriptive statistics given here. There are important differences in facility level releases across the early and late adopting states. For example, average, aggregate releases in early adopting states were only 60% as large as the releases in late adopting states. Early adopting states also had a relatively small reduction in aggregate TRI releases, with a large *increase* in TRI releases, net of CAA substances. For all other measures of toxic releases, early adopters showed reductions, post adoption, but those reductions were much smaller than those found in late adopting states. Furthermore, late adopting states showed reductions in all measures of toxic releases.

The choice of P2 programs also differs dramatically across early and late adopting states. In particular, technical assistance programs and grants were far more commonly available in early adopting states, relative to late adopting states, whereas filing fees and non-reporting penalties were more common in late adopting states.

Table III, Panel 1 summarizes releases for facilities located in states with low P2 stringency. Note that even *before* adoption, facilities located in the most stringent P2 program states have average releases that are lower than those found in other states, for all measures of toxic releases, except for CAA air releases, where the average facility level release is almost the same. And after the adoption of a P2 program, facility releases in the high P2 states fell by more than for those in low P2 states, again, with the exception for CAA air releases. CAA air releases actually fell more in the lower-stringency states so that post-adoption, average facility level releases were lower in low P2 states than in high P2 states. The most stringent P2 states also had a much higher rate of technical assistance and educational outreach, but a lower rate of grant support, filing fees and non-reporting penalties.

The differences in pre/post adoption facility level releases across early/late adopters and low/high stringency states captured by the descriptive statistics suggests that there may be important differences across these facilities. There are also important distinctions that exist at the state and industry levels between these groups. (Table IV provides data on the number of facilities found in each group.) In particular, an examination of the *unbalanced* panel of TRI reporters shows that the pattern of entering and exiting facilities (measured by the ratio of entering (or exiting) facilities to the number of facilities in the balanced panel) differs significantly in high P2 stringency states (see Table V). Here, the ratio is almost double in magnitude compared to that found in low P2 stringency states. This elevated level of entry and exit could reflect a higher level of competitiveness in the manufacturing sector. Industry composition also differs by group as evidenced by the sound rejection ($p = 0.000$) of the Kolmogorov-Smirnov test for the equality across the distribution of industries (based on 2-digit SIC). Although not presented here, data from the U.S. Census' County

Business Pattern shows that the percentage of small facilities within the manufacturing sector is stable across time within groups, where the percentage is given by the number of manufacturing facilities (by 4-digit SIC within the given state) with between 10 and 50 full time employees divided by the total number of manufacturing facilities operating in that industry-state for that year. So, although there are differences across these groups, we can rule out that both the changes in releases over time within these groups, and the differences across these groups, are attributable to changes in industry composition or structure.

IV. RESULTS

Regression results are given for three of the four different facility groupings discussed in the previous section. Results for facilities located in low stringency states are given below, broken down by adoption date (early/late). Due to both a lack of variation in, and a high level of correlation between, state P2 programs in high stringency states, results for these facilities are not included here, but are available upon request.

Four different measures of toxic releases are used in the analysis. The first consists of aggregate TRI releases for all TRI reporting chemicals in the balanced panel (as described in Section III). These chemicals are aggregated by weight across all pollution media. To address the issue of confounding effects from the CAA, the 1990 CAAA, and other potentially important policies, I also include measures for TRI releases, TRI 33/50 releases, and TRI Montreal Protocol releases, all *net* of CAA substances.

A. The Effects of State P2 Program Adoption on Facility Releases

If P2 programs affect facility level releases, it should be the case that facilities that have access to P2 programs differ from facilities that do not have such access. Although the descriptive statistics suggest that this may be the case, they do not establish a causal relationship. An event-study, however, can be used to determine whether the adoption of a state P2 program affects facility level releases, under the assumption of state-program exogeneity – which is reasonable at the *facility* level but would be much harder to defend at a higher level of aggregation. Regressing the natural log of toxic releases (at time t) on an indicator variable which takes on the value of 1 if a facility in year t is located in a state which has an active P2 program in year t, and 0 otherwise (and controlling for year, industry (at the 2-digit SIC level), and facility level fixed effects) allows for the average treatment effect to be estimated. Regression results for aggregate toxic releases and toxic releases net of CAA substances are summarized in Table VI.[5]

First, note that in all cases, the effect of the 1990 PPA is negative and statistically significant. This is consistent with the belief that the federal program was successful at helping polluters reduce pollution. With respect to the adoption of a state P2 program, for both aggregate toxic releases and toxic releases net of CAA substances, the effect on facility level releases of the adoption of a state P2 program is negative, and statistically significant (for aggregate TRI releases, $\beta = -0.115$, SE = 0.03 and for TRI releases net of CAA substances, $\beta = -0.124$, SE = 0.03), even when taking into account year, industry, and facility level fixed effects. Interestingly, when facilities are grouped by whether the state is an early or late adopter relative to the 1990 PPA, adoption is only significant in early adopting states.[6] [7] This is consistent with a story of spill-over effects. If information leaks

[5] Regression results for other subsets of TRI substances, such as CAA substances, and net air releases, as well as for the unbalanced panel, are available upon request. All of these results are consistent with those presented here.

[6] Note that this is the case even though the descriptive statistics show that facilities in early adopting states became (on average) dirtier after the adoption of a state P2 program. The descriptive statistics results are due to behavior exhibited by 15 facilities in the data set. These facilities are in

from early to late adopting areas, the effectiveness of P2 programs in late adopting states may be much smaller, with benefits having accrued prior to the adoption of a state program. The average treatment effect for facilities located in early adopting states is estimated at -23.3% (SE = 0.05) for aggregate TRI releases, and -24.2% (SE = 0.06) for TRI releases, net of CAA substances. To put these numbers into perspective, average facility releases fell by 66% and 69.5%, respectively, for aggregate TRI releases and net TRI releases over the sample period. So, in each case, approximately 35% of the reduction in releases can be attributed to the adoption of a state P2 program.[8]

Finally, the coefficient on adoption continues to be negative for facilities located in late-adopting states for both measures of toxic releases, but is imprecisely estimated in both cases.

B. Testing for Spurious Correlation.

Before continuing, it is important to rule out the possibility that the event study results are due to spurious correlation. In an ideal world, one could test for this by choosing an arbitrary adoption date taken from before the start of any state P2 program and testing for the significance of the "false" adoption date. Unfortunately, that option is foreclosed from us as TRI data only start in 1987 and 61% of the facilities in the balanced panel have adoption dates that fall on or before 1989. I can, however, conduct the experiment where I take facilities that are located in late-adopting states (adoption post 1990), and for this group, construct a false adoption date in 1987. This is a somewhat less "clean" test than the ideal one because if there are any spill-over effects from "treated" facilities

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SIC codes 28 and 33 and their "adverse" effects on releases are captured by the facility fixed effects.

[7] The event study results are robust to the inclusion of industry-year fixed effects in lieu of individual industry and year fixed effects.

[8] As another comparison, emissions reductions for the heavily regulated criteria air pollutant, PM10, was approximately 34% between 1990-2003.

to "non-treated" facilities, these effects may still be captured by the false adoption date. I would expect, however, that even in that case, both the magnitude of the coefficient as well as the level of significance would be much smaller in the false regression than in the true regression.

Results for the false adoption-date regressions are summarized in Table VII. Regression results are presented for pooled (low-stringency state) facilities and are not broken down by early/late adopters as the construction of the false adoption date reclassifies all late adopting facilities as early adopters. For both aggregate TRI releases and TRI releases net of CAA substances, the false adoption date variable is not statistically significant at any reasonable level. These results hold whether I use the balanced or the unbalanced panel of TRI reporting facilities.

One additional comment should be made regarding this test. It could be said that it is not surprising to find the "false" adoption date to be insignificant when the effect of the true adoption date is not statistically significant in late adopting states. But, given that the sign is the same and that both the difference in magnitude, as well as the difference in statistical significance is large, I would argue that this result does provide reassurance that the strategy employed here is valid and that the results are not likely to be due to spurious correlation.

C. The Effects of Individual State P2 Programs on Facility Releases

Given the evidence that state P2 program adoption affects facility level releases, I turn next to estimating the effects of individual P2 programs. As described earlier, six different state programs were identified: technical assistance programs, educational outreach, grants, awards, filing fees, and non-reporting penalties. Due to the small number of observations, awards programs are not included in the analysis.

In general, P2 programs may be classified into one of two groups: cost-reducing programs and monitoring programs. Cost-reducing programs are believed to encourage participation in P2 programs by reducing the cost to the polluter of engaging in pollution prevention activities or abatement. These would include grant programs, which would directly lower the cost of participation, as well as technical assistance and educational outreach programs, which would indirectly lower the cost of participation by providing information to polluters on pollution prevention and abatement activities. Although in theory, all three of these programs could lead to a reduction in facility level releases, the manner in which this might occur may differ depending upon whether costs are reduced directly or indirectly. In particular, when cost-reduction occurs through the provision of information, two important considerations must be taken into account. The first is that there may be a period of "learning" which takes place so reductions may not occur immediately. And second, there may be informational spill-overs that occur from areas with P2 programs to areas without P2 programs.

Monitoring programs are programs that increase the ability of a regulator to directly, or indirectly monitor polluter behavior and encourage participation in P2 programs by signaling to polluters that regulators are watching to see whether they are responding to the quasi-regulatory mechanisms. This is believed to induce polluters to engage in pollution reducing, risk-management strategies. In the case of P2, filing fees and non-reporting penalties belong to this group by (1) encouraging polluters to report to the TRI (via non-reporting penalties, which increase the cost of *non*-participation) and (2) having polluters provide summary information to state regulators on their polluting behavior (via filing fees, which increase the cost of participation). It is not clear, however, how facilities might respond to these programs in light of the fact that regulators have limited (or no) ability to validate the *accuracy* of the reported release data. On the one hand, increased regulatory scrutiny may lead to a reduction in pollution if the polluter believes that regulators will adopt more stringent regulatory measures if the polluter does not voluntarily abate. On the other hand, increased regulatory scrutiny may lead to a change in reporting behavior if the polluter believes that regulators will look more carefully at the accuracy of the reporting, in which case, reported releases may increase as over-reporting will be less likely to incur any penalty than under-reporting.

Table VIII summarizes regression results from estimating Equation (1) with indicator variables for each of the 5 identified programs, as well as an indicator variable that is used to capture the effects of the federal level 1990 PPA. Because high P2 stringency states are not included in the analysis, it does not matter whether adoption or compliance dates are used as they are identical. To capture the possibility that the effects of a particular program (in particular, technical assistance and educational outreach programs) may change over time, I have included a term which interacts the state and federal program variables with a variable that measures the time since P2 adoption.

Columns 1-3 of Table VIII, panel 1 summarize the results for aggregate TRI releases. Column 1 includes all facilities, whereas Column 2 and 3 break the sample down by early and late adopting states. In all three specifications, the variable on Federal is negative and statistically significant. The largest effect is found for facilities located in early adopting states. For these facilities, the effects of the Federal program, however, dissipate over time. After approximately 15 years, the average effect, still remains larger than the effect found for facilities located in late adopting states (holding all other factors constant).

In early adopting states, technical assistance programs have a positive effect on releases, but the effect slowly declines over time. The opposite holds true for educational programs, where there is a large, negative effect on releases initially, which dissipates over time. Of the state programs which can increase regulatory monitoring, only filing fees have a statistically significant effect, and are found to increase reported release levels. In late adopting states, of the different state level programs, only technical assistance programs have any statistically significant effect on facility level releases. And here, I find that their effects increase over time.

Because of the potential for confounding effects from the CAA and the 1990 CAAA, it is important to take care when interpreting the above results. So, to alleviate some of those issues, in Columns 4-6 I summarize regression results based on TRI releases *net* of any CAA substances.

When using only TRI releases net of CAA substances, I find that for the entire sample, (column 4), the effect of the Federal program is much larger (-1.5 versus -0.31), and dissipates somewhat more slowly over time. No state programs, however, have any statistically significant effect. When the sample is broken down into early and late adopters, however, a different picture emerges. First, facilities located in early adopting states have a much larger (negative) response to the Federal program with the effect slowly dissipating over time, the longer the state has had a P2 program in place. Technical assistance programs lead to lower facility level releases as well, and their effects increase slowly over time. Educational outreach continues to have a large, negative (and statistically significant) effect on facility releases upon adoption. Although non-reporting penalties do not have any effect on facility releases, filing fees lead to an increase in average facility level releases.[9]

When compared to facilities located in late adopting states, the Federal program has a much smaller (but still statistically significant) effect on average facility level releases. The effect of

[9] One exception is TRI CAA air releases. For this group of toxic releases, non-reporting penalties have a very strong, negative effect on facility releases.

technical assistance programs, however, is large (negative), and statistically significant with no "learning" time. In fact, the effects of the technical assistance programs decline the longer the state has had a P2 program. No other state P2 programs have any statistically significant effect on average facility releases.

It is also interesting to look at the effects of P2 programs on TRI substances that are affected by other policies, such as the voluntary TRI 33/50 program and the Montreal Protocol. So, in Table VIII, Panel 2, I re-estimate equation (1) for these two measures of toxic releases, again, net of any CAA substances. While Montreal Protocol substances appear to be unaffected by any state P2 programs, that is not the case for TRI 33/50 substances. Although this sub-category of chemicals constitute a very small portion of over-all TRI releases, technical assistance programs appear to have a strong effect on their level of releases – with a slightly larger effect being found for facilities in late adopting states. Similar to TRI CAA substances, non-reporting penalties also appear to lower the level of TRI 33/50 substances as well, although in this case, the coefficient is only precisely estimated for the entire sample of reporters, and not for early/late adopters. One possible explanation for this result is that unlike the bulk of TRI releases, for both TRI CAA and 33/50 substances, regulators are able to monitor pollution releases and validate reported releases more easily. In that case, non-reporting and under-reporting can be identified more readily and penalized, making a non-reporting penalty a more viable threat to polluters.

There may be some question as to whether the results presented here might be an artifact of using a balanced panel which only contains facilities that are in operation for a full 16 years. Although the results are not presented here, regressions based on the *unbalanced* panel of all TRI reporters (from the manufacturing sector) yield remarkably consistent results, strongly suggesting that the estimated effects of P2 programs on facility level releases are not due to special characteristics of the long-lived facilities used in the balanced panel.

VI. CONCLUDING REMARKS.

The provision of government sponsored programs that are designed to encourage pollution prevention and abatement are a growing part of the regulatory arsenal used to manage environmental quality, but are not well understood. In this study, I find strong evidence to support the proposition that both federal and state P2 programs have had an effect on facility level toxic releases, but the effects depend critically not only on the relative timing of the program's adoption but also on the changes in their effects over time. This first factor is related to the benefits associated with spill-over effects that can occur when P2 programs collect and disseminate information to polluters. Providing this public good allows facilities in late adopting states to take advantage of the information made available in early adopting states, which can translate into more rapid reductions in pollution.

The second factor is related to two different effects. The first relates to the possibility that the type of information, or how the information is used by a given facility, may change over time. For example, if facilities do not have the accounting capability to measure their toxic releases, they may want to engage in improving their accounting methodologies before tackling any actual P2 or abatement activities. If so, it would not be surprising to see a change over time in the measured effects of facility level TRI releases to the adoption of a P2 program. The second relates to changes in a facility's motivations for responding to a P2 program. If the impetus for responding to a P2 program is to minimize the risk of more stringent regulations in the future, if the perceived risk changes over time, the facility's response may also change. In the case of P2 programs and toxic releases, since the first P2 programs were first adopted in 1984, there have been no real changes in the regulation of toxic pollutants (with the exception of those that were adopted under the 1990 CAAA). The lack of any formal regulatory action may be responsible for the reduced effectiveness of P2 programs over time. This begs the question, then, of whether the changes in polluting behavior caused by P2 programs is, in fact, permanent, or temporary in nature.

Evaluating the over-all effectiveness of P2 programs on toxic polluter behavior is made even more difficult by the fact that it is almost impossible to validate the accuracy of the toxic release data. This problem is reflected in the facility level response to filing fees and non-reporting penalties. It appears to be the case that filing fees affect a polluter's reporting behavior, but not necessarily their polluting behavior. And it is precisely because regulators cannot verify the data that this response can occur. Non-reporting penalties are, likewise, affected, in that they are only effective for the subset of TRI substances that can be most easily monitored by regulators: CAA substances, and TRI 33/50 chemicals. Without the ability to validate the quality of the data, regulators cannot easily determine whether the program is affecting polluting reporting or polluting behavior. Clearly, it is the latter that is desired, but improving the ability to validate the toxic release data should be an integral part of any regulatory measure.

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Table I. Balanced Panel of TRI Reporters, Manufacturing Sector: 1988-2003

Variable	All Years Mean	All Years Std. Dev	Before Adoption Mean	Before Adoption Std. Dev	After Adoption Mean	After Adoption Std. Dev	Change
Aggregate TRI Releases (lbs)	251995.60	1883485.0	388133.40	2265480.00	233345.80	1814341.00	-39.88%
TRI CAA Air Releases (lbs)	97574.31	389993.00	158576.80	735738.70	89334.78	325800.90	-43.66%
TRI Net of CAA (lbs)	154421.3	1779415	229556.70	2019116	144011	1733725.00	-37.26%
TRI 33/50 Net of CAA (lbs)	92.38	2136.93	112.03	1127.34	87.43	2243.25	-21.96%
TRI Mtl. Protocol Net of CAA (lbs)	801.53	12471.34	3200.23	29387.37	458.23	8133.94	-85.68%
Technical Assistance	0.65	0.48			0.66	0.48	
Educational Outreach	0.20	0.40			0.19	0.39	
Grants	0.43	0.50			0.43	0.50	
Awards Program	0.11	0.31			0.12	0.32	
Filing Fee	0.60	0.49			0.60	0.49	
Non-Reporting Penalty	0.64	0.48			0.64	0.48	
Observations	114512		9077		98513		

Table II, Panel 1. Balanced Panel of TRI Reporters, Early Adopters: 1988-2003

	All Years		Before Adoption		After Adoption		
Variable	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Change
Aggregate TRI Releases (lbs)	242374.50	1991367.00	241648.50	998161.50	236563.10	1995552.00	-2.10%
TRI CAA Air Releases (lbs)	88620.71	335686.80	129319.20	781571.50	85147.10	305719.50	-34.16%
TRI Net of CAA (lbs)	153753.80	1912556.00	112329.30	561706.50	151416.00	1925640.00	34.80%
TRI 33/50 Net of CAA (lbs)	106.11	2647.93	151.23	1859.73	100.03	2710.50	-33.68%
TRI Mtl. Prot.Net of CAA (lbs)	744.23	11420.89	3038.49	27229.98	542.82	9093.68	-82.14%
Technical Assistance	0.67	0.47			0.66	0.47	
Educational Outreach	0.11	0.31			0.11	0.31	
Grants	0.53	0.50			0.52	0.50	
Awards Program	0.18	0.38			0.18	0.38	
Filing Fee	0.54	0.50			0.55	0.50	
Non-Reporting Penalty	0.63	0.48			0.63	0.48	
Observations	69600		1879		63606		

Table II, Panel 2. Balanced Panel of TRI Reporters, Late Adopters: 1988-2003

	All Years		Before Adoption		After Adoption		
Variable	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Change
Aggregate TRI Releases (lbs)	293821.60	1691795.00	405644.40	2039116.00	256799.80	1539453.00	-36.69%
TRI CAA Air Releases (lbs)	97654.76	352834.20	140994.50	467988.30	83539.67	306536.50	-40.75%
TRI Net of CAA (lbs)	196166.90	1612005.00	264649.80	1911955.00	173260.10	1477547.00	-34.53%
TRI 33/50 Net of CAA (lbs)	63.08	963.90	99.51	937.01	52.28	979.14	-47.46%
TRI Mtl. Prot. Net of CAA (lbs)	740.36	10305.54	2504.69	20054.92	167.36	2875.52	-93.32%
Technical Assistance	0.18	0.39			0.18	0.39	
Educational Outreach	0.14	0.35			0.15	0.35	
Grants	0.22	0.42			0.23	0.42	
Awards Program	0.00	0.00			0.00	0.00	
Filing Fee	0.66	0.47			0.68	0.47	
Non-Reporting Penalty	0.60	0.49			0.62	0.49	
Observations	19360		4004		14146		

Table III, Panel 1. Balanced Panel of TRI Reporters in Low P2 Stringency States: 1988-2003

	All Years		Before Adoption		After Adoption		
Variable	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Change
Aggregate TRI Releases (lbs)	233345.80	1814341.00	404880.70	2132174.00	244928.70	1920269.00	-39.51%
TRI CAA Air Releases (lbs)	89334.78	325800.90	158987.50	676098.50	88374.10	310790.40	-44.41%
TRI Net of CAA (lbs)	166621.9	1877035	245893.2	1946842.00	156554.6	1850123.00	-36.33%
TRI 33/50 Net of CAA (lbs)	104.09	2365.66	127.51	1223.90	98.29	2471.46	-22.92%
TRI Mtl. Prot. Net of CAA (lbs)	774.96	11634.13	3131.57	25665.82	466.58	8245.53	-85.10%
Technical Assistance	0.66	0.48			0.61	0.49	
Educational Outreach	0.19	0.39			0.17	0.38	
Grants	0.43	0.50			0.44	0.50	
Awards Program	0.12	0.32			0.15	0.35	
Filing Fee	0.60	0.49			0.63	0.48	
Non-Reporting Penalty	0.64	0.48			0.67	0.47	
Observations	90640		6232		78978		

Table III, Panel 2. Balanced Panel of TRI Reporters in High P2 Stringency States: 1988-2003

	All Years		Before Adoption		After Adoption		
Variable	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Change
Aggregate TRI Releases (lbs)	211663.40	1548451.00	351448.40	2533109.00	186517.20	1299935.00	-46.93%
TRI CAA Air Releases (lbs)	103566.80	476493.90	157677.10	852045.40	93218.68	380481.70	-40.88%
TRI Net of CAA (lbs)	108096.6	1344774.00	193771.30	2168960.00	93298.47	1147249.00	-51.85%
TRI 33/50 Net of CAA (lbs)	47.89	808.55	78.14	878.77	43.53	824.47	-44.29%
TRI Mtl. Prot. Net of CAA (lbs)	902.40	15236.71	3350.64	36232.21	424.46	7666.34	-87.33%
Technical Assistance	0.84	0.37			0.85	0.36	
Educational Outreach	0.29	0.46			0.28	0.45	
Grants	0.39	0.49			0.40	0.49	
Awards Program	0.00	0.00			0.00	0.00	
Filing Fee	0.50	0.50			0.49	0.50	
Non-Reporting Penalty	0.52	0.50			0.52	0.50	
Observations	23872		2845		19535		

Table IV: Tabulation of Number of Facilities by Different State Groupings[a]

	Low Stringency	High Stringency	Total
Early Adopter	3891	459	4350
Late Adopter	870	340	1210
Total	4761	799	5560

[a] Note that the table *excludes* observations from states that adopt a P2 program in 1990.

Table V: Facility Entry and Exit by Different State Groupings Using the Unbalanced Panel of TRI Reporters, 1988-2003

	Number of Entering Facilities	Number of Exiting Facilities
Early Adopting State	12372	13334
Late Adopting State	3722	3977
Low Stringency State	16558	16972
High Stringency State	4109	5104

Table VI: The Effect on Facility Level Toxic Releases from the Adoption of a State P2 Program: Facilities in Low Stringency P2 States, 1988-2003.

Variables	Aggregate TRI Releases			TRI Releases: Net of CAA		
	All	Early	Late	All	Early	Late
Federal PPA	-0.765***	-0.648***	-0.358***	-0.653***	-0.535***	-0.641***
	(0.0401)	(0.0592)	(0.0771)	(0.0515)	(0.0752)	(0.0991)
Adoption Date	-0.115***	-0.233***	-0.0716	-0.124***	-0.242***	-0.0541
	(0.0278)	(0.0505)	(0.104)	(0.0349)	(0.0641)	(0.117)
Year Indicators	✓	✓	✓	✓	✓	✓
Industry Indicators	✓	✓	✓	✓	✓	✓
Facility Indicators	✓	✓	✓	✓	✓	✓
Constant	10.63***	10.52***	11.56***	9.618***	9.478***	11.42***
	(0.115)	(0.179)	(0.213)	(0.195)	(0.243)	(0.379)
Observations	84016	57438	13198	68624	47141	10940
R-squared	0.776	0.777	0.802	0.754	0.754	0.788

Robust standard errors are in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table VII: Testing for Spurious Correlation Using False Adoption Dates

Variables	Aggregate TRI Releases	TRI Releases: Net of CAA
Federal PPA	-0.248***	-0.746***
	(0.0507)	(0.0613)
False Adoption Date	-0.00458	-0.0441
	(0.0491)	(0.0630)
Year Indicators	✓	✓
Industry Indicators	✓	✓
Facility Indicators	✓	✓
Constant	10.69***	9.659***
	(0.116)	(0.183)
Observations	79708	65150
R-squared	0.785	0.763

Robust standard errors are in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table VIII, Panel 1: The Effect of State P2 Programs on Facility Toxic Releases in Low Stringency P2 States, 1988-2003.

Variables	Aggregate TRI Releases			Aggregate TRI Releases, Net of CAA Substances		
	All	Early	Late	All	Early	Late
Federal PPA	-0.311***	-3.961***	-0.251***	-1.499***	-4.393***	-0.453***
	(0.0433)	(0.727)	(0.0915)	(0.256)	(0.937)	(0.117)
Time_PPA	0.0417***	0.189***	-0.0319	0.0346**	0.225***	-0.0385
	(0.0144)	(0.0476)	(0.0246)	(0.0174)	(0.0614)	(0.0274)
Technical Assistance	0.0967	0.348**	-0.641***	-0.0324	0.217	-1.613***
	(0.0637)	(0.153)	(0.243)	(0.0820)	(0.200)	(0.290)
Time_TA	-0.0114***	-0.0196***	0.00948	-0.00665*	-0.0104**	0.0744***
	(0.00279)	(0.00328)	(0.0191)	(0.00353)	(0.00415)	(0.0246)
Educational Outreach	-0.140*	-0.537**	0.307	-0.127	-0.607**	0.831*
	(0.0740)	(0.230)	(0.356)	(0.0984)	(0.306)	(0.473)
Time_Educ	0.00922**	0.0161***	-0.0175	0.00554	0.00310	-0.0296
	(0.00378)	(0.00479)	(0.0289)	(0.00491)	(0.00632)	(0.0380)
Grants	-0.0964	-0.246	-0.288	0.107	-0.511	-0.371
	(0.0754)	(0.313)	(0.210)	(0.102)	(0.432)	(0.296)
Filing Fees	0.193***	0.445**	0.308	0.157*	0.568**	-0.0683
	(0.0632)	(0.209)	(0.203)	(0.0844)	(0.280)	(0.285)
Non-Report. Penalties	-0.142**	0.0366	-0.296	-0.175*	0.489	-0.0480
	(0.0689)	(0.335)	(0.209)	(0.0916)	(0.461)	(0.289)
Year Indicators	✓	✓	✓	✓	✓	✓
Industry Indicators	✓	✓	✓	✓	✓	✓
Facility Indicators	✓	✓	✓	✓	✓	✓
Constant	10.58***	10.23***	11.60***	9.593***	9.063***	11.47***
	(0.117)	(0.194)	(0.221)	(0.196)	(0.301)	(0.380)
Observations	84016	57438	13198	68624	47141	10940
R-squared	0.776	0.777	0.803	0.754	0.754	0.790

Robust standard errors are in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table VIII, Panel 2: The Effect of State P2 Programs on Facility Releases in Low Stringency P2 States, 1988-2003.

Variables	TRI 33/50 Releases, Net of CAA Substances			Montreal Protocol Releases, Net of CAA Substances		
	All	Early	Late	All	Early	Late
Federal PPA	-1.426***		-1.452***		-1.935*	-5.384
	(0.433)		(0.524)		(1.075)	(3.920)
Time_PPA	-0.0108	0.138	0.0122	0.0205	0.0241	0.288
	(0.0297)	(0.109)	(0.0477)	(0.0339)	(0.0652)	(0.325)
Technical Assistance	-0.149	0.203	0.344	-0.143	-0.108	-0.294
	(0.171)	(0.535)	(0.738)	(0.0903)	(0.182)	(0.190)
Time_TA	-0.0288***	-0.0245***	-0.113***	0.00486	-0.00252	0.0649
	(0.00766)	(0.00856)	(0.0426)	(0.0213)	(0.0265)	(0.0859)
Educational Outreach	0.155	1.460	-1.257	0.147	0.246	0.803
	(0.197)	(1.030)	(1.008)	(0.135)	(0.229)	(0.539)
Time_Educ	0.00499	-0.00899	0.132**	-0.00791	0.000965	-0.342
	(0.00930)	(0.0120)	(0.0634)	(0.0297)	(0.0330)	(0.258)
Grants	0.331*	1.353	0.669	0.0629	0.413	-0.255
	(0.196)	(1.113)	(0.613)	(0.147)	(0.304)	(0.423)
Filing Fees	0.296*	-0.866	0.599	-0.107	-0.126	-0.355
	(0.168)	(1.016)	(0.575)	(0.108)	(0.168)	(0.369)
Non-Report. Penalties	-0.594***	-1.945	-1.075*	0.110	-0.282	0.241
	(0.180)	(1.221)	(0.577)	(0.141)	(0.343)	(0.347)
Year Indicators	✓	✓	✓	✓	✓	✓
Industry Indicators	✓	✓	✓	✓	✓	✓
Facility Indicators	✓	✓	✓	✓	✓	✓
Constant	7.951***	10.53***	3.946***	10.18***	9.938***	10.78***
	(1.149)	(1.524)	(1.051)	(0.279)	(0.165)	(0.476)
Observations	26750	18784	3931	5271	3509	1033
R-squared	0.816	0.812	0.840	0.834	0.835	0.839

Robust standard errors are in parentheses. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

The Impact of Quasi-Regulatory Mechanisms on Polluting Behavior: The Case of Pollution Prevention Programs and Toxic Releases

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The Role of "Quasi-Regulatory" Mechanisms in Environmental Policy

- The adoption of market-based mechanisms to address environmental issues has included the use of what is often referred to as "quasi-regulatory" mechanisms.
- These mechanisms include voluntary programs and initiatives aimed at informally incentivizing pollution reduction amongst polluters without requiring actual participation.

Examples: 33/50 Program
Design for the Environment (DfE)
TRI
Pollution Prevention Act

Why Might Polluters Respond to a Quasi-Regulatory Mechanism?

- Two main hypotheses that have been put forward:
  - Quasi-regulatory mechanisms lead to lower abatement costs by providing lowering the cost of information. (May include technical assistance or education programs, joint ventures between industry and government on P2 activities, grants.)
  - The adoption of quasi-regulatory mechanisms might lead polluters to believe that more formal regulations may be adopted in the future if polluters do not respond "voluntarily" by abating. The uncertainty associated with the potential for formal regulation leads polluters to adopt a 'risk management' strategy of abating. (May include filing fees and non-reporting penalties that allow for state level monitoring of participation).
- Very little empirical evidence on the effectiveness of such mechanisms for reducing pollution, and no evidence for which I am aware that might distinguish between the two possible explanations for polluter response (if there is one).

The Focus of this Paper

- Question: Do quasi-regulatory mechanisms lead to lower levels of pollution?
- Focus: Examine pollution prevention (P2) programs that are aimed at toxic releases.
  - Does the adoption of a P2 program affect toxic releases?
  - Are some P2 programs more effective at eliciting polluter response?
  - Is their any evidence that might explain why polluters respond to quasi-regulatory mechanisms?

Empirical Strategy

- Use an event study methodology to estimate the average effect of federal and state-level P2 programs on toxic releases using facility level data from 1988-2003.
- **Identification Strategy**: Identification comes from exploiting the variation in adoption dates of quasi-regulatory mechanisms (both "within" time change variation and those based on both time and cross-sectional variation). The adoption of regulations and program choice are assumed to be exogenous to *facility* level toxic releases.

The Basic Model

- The basic model to be estimated for facility-level TRI releases takes the form:

$$\ln(TRI_{st}) = \beta_0 + \beta_1 Federal_t + \Gamma z_{st} + \lambda_s + \gamma_t + \theta_i + \varepsilon_{ist}$$

Where ln(TRI) is the natural log of facility level TRI releases in state s in year t;
Federal is an indicator variable controlling for the 1990 PPA which takes on the value of 1 from 1990 - 2003, and 0 otherwise;
Z is a vector of indicator variables representing P2 program characteristic adoption dates;
λ are individual state indicators;
γ are individual year indicators; and
θ are individual facility indicators.
ε is assumed to be a well behaved random error term with a conditional mean of zero.

Data

- Balanced panel of 7157 TRI reporting manufacturing facilities from 1988-2003
- Facility-level releases: aggregate releases, aggregate releases net of CAA substances, TRI 33/50 chemicals (net of CAA substances), Montreal Protocol substances (net of CAA substances).
- State-level P2 program adoption/compliance data and program characteristics are taken from state statutes.
- Additional data are from U.S. County Business Patterns database.

Some Things to Consider

- There appear to be important differences in the characteristics of:
  - facilities located in "early" and "late" P2 adopting states;
  - facilities located in "low" and "high" P2 stringency states;
  - the distribution of industries in early/late adopting and low/high stringency states.
- To allow for these differences, facilities will be grouped by these characteristics when constructing control groups.
- Confounding effects of the 1990 CAAA.

Descriptive Statistics: Change in TRI Facility Releases After Adoption of State P2

	All Reporters	Early Adopters	Late Adopters	Low Stringency	High Stringency
Aggregate TRI (lbs)	-39.88%	-2.10%	-39.69%	-39.51%	-46.93%
TRI CAA (lbs)	-43.66%	-34.16%	-40.75%	-44.41%	-40.88%
TRI Net of CAA (lbs)	-37.26%	**+34.80%**	-34.53%	-36.33%	-51.85%
Net TRI 33/50 (lbs)	-21.96%	-33.68%	-47.46%	-22.92%	-44.29%
Net TRI MP (lbs)	-85.68%	-82.14%	-93.32%	-85.10%	-87.33%

The Effect of State P2 Adoption on Facility Level TRI Releases in Low Stringency States (Aggregate, Net of CAA Substances)

	All	**Early**	**Late**
Federal PPA	-1.154*** (0.0390)	-0.755*** (0.0517)	-1.1555* (0.112)
Adoption Date	-0.0875*** (0.0245)	-0.209*** (0.0426)	-0.0534 (0.0822)
Year Indicators	X	X	X
Industry Indicators	X	X	X
Facility Indicators	X	X	X
Constant	8.724*** (0.197)	8.714*** (0.245)	9.659*** (0.406)
Observations	164648	113017	26579
R-squared	0.772	0.772	0.791

Testing for Spurious Correlation Using False State Adoption Dates

	All
Federal PPA	-0.531*** (0.0433)
Adoption Date	-0.0645 (0.0451)
Year Indicators	X
Industry Indicators	X
Facility Indicators	X
Constant	8.769*** (0.201)
Observations	155816
R-squared	0.780

The Effects of Different State P2 Programs on Facility Level TRI Releases: A Selection Results Only (Aggregate, Net of CAA Substances)

	All	**Early**	**Late**
Federal PPA	-1.499***	-4.393***	-0.453***
Time_PPA	0.0346**	0.225***	-0.0385
Tech. Assist.	-0.0324	0.217	-1.613***
Time_TA	-0.00665*	-0.0104**	0.0744***
Education	-0.127	-0.607**	0.831*
Time_Educ.	0.00554	0.00310	-0.296
Grants	0.107	-0.511	-0.371
Filing Fees	0.157*	0.568**	-0.0683
Non-Reporting Pen.	-0.175*	0.489	-0.048

The Effect of State P2 Adoption/Compliance Date on Facility Level TRI Releases in High-Stringency States (Aggregate, Net of CAA Substances)

Federal PPA	-0.866*** (0.0796)	-1.460*** (0.0992)	-1.426*** (0.103)
Between	-0.184*** (0.0440)		-0.0568 (0.0523)
Compliance		0.273*** (0.0463)	0.239*** (0.0550)
Year Indicators	X	X	X
Industry Indicators	X	X	X
Facility Indicators	X	X	X
Constant	8.269*** (0.0328)	8.271*** (0.329)	8.271*** (0.329)
Observations	17410	17410	17410
R-squared	0.743	0.743	0.743

Conclusion

- Surprisingly, there is reasonably strong evidence that the adoption of a state P2 program can have an effect on facility level toxic releases.
- There is evidence to suggest that spill-over effects may play an important role in the effectiveness of these types of programs, with later adopting areas benefiting from information and experience gained in earlier adopting areas.
- P2 programs may become less effective at keeping pollution levels low over time – possibly because of changing perceptions of future regulatory actions.

The Impact of Quasi-Regulatory Mechanisms on Polluting Behavior: Evidence from Pollution Prevention Programs and Toxic Releases

Discussant: Ann Wolverton, NCEE, US EPA
January 18, 2010

The views expressed during this presentation are those of the presenter and do not necessarily represent those of the U.S. EPA.

Quick Summary

- Use differences across state-level pollution prevention programs to examine their impacts on toxic releases
- Main results:
  - State P2 programs have a significant effect on emissions over time; on average, adoption of a P2 program results in 3% – 9% reduction in pollution
  - Early adopter states exhibit larger average reductions of 15% – 21% but represent a small subset of facilities.
  - Of the P2 programs studies, technical assistance and education outreach seem be related to the largest reductions in emissions.
  - Filing fees appear to increase reported releases, and non-reporting penalties appear to matter only for a subset of TRI releases

Policy Context

- Voluntary or partnership programs are often put in place due to a lack of authority to regulate a particular pollutant.
- A real challenge has been in demonstrating whether these programs result in real, measurable environmental benefits.
- This paper shows that P2 programs can be effective in reducing emissions and points to the types of programs that tend to be more or less effective.
- Are these relatively modest reductions in emissions? (Previous studies have found that reductions tend to be relatively modest.)
  - Could put these results in context to understand how reductions compare to what might have been accomplished under a mandated program or reductions attributed to the programs by the states.

A Few Thoughts

- The results are intriguing and the study well done.
- Are there any other possible reasons for why TRI emissions would decrease over time in a way that might be correlated with the introduction of a P2 program?
  - Possible changes in TRI reporting methodology over time? ("Basis of the estimate")
  - The advent/disappearance of regulatory threats for particular sets of chemicals at the state level?
- Could use TRI to validate whether there has been a move toward "greener" practices (e.g., source reduction and recycling)
- May also be interesting to differentiate between firms that manufacture or import the chemical for use or sale vs. those that produce it as a byproduct or impurity.

Further Thoughts

- Why stop with 2003? One could argue that there was an attempt to think more carefully about the design of partnership programs post-2003.
- Can you incorporate differences in the stringency of P2 programs more directly as a continuous variable?
  - This would allow for a more direct measure of the importance of program stringency on emissions than and comparing means across samples.
  - This would allow one to include higher stringency states in all of the analyses even though there are relatively fewer of them.
- Air emissions only? Weighting by toxicity?

Comments on Linda Bui's "Impact of quasi-regulatory mechanisms on polluting behavior"

Sheila Olmstead

EPA Workshop on the Benefits of Environmental Information Disclosure

Arlington, VA, January 18, 2011

RESOURCES FOR THE FUTURE

Outline of my comments

- Quick summary of paper
- What we knew about TRI/P2 before Bui (2011).
- New contributions from Bui paper
- Comments on the methods
- Additional comments, if time.

RESOURCES FOR THE FUTURE

Quick summary of paper

- Exploits variation in state-level P2 adoption and compliance dates, stringency, and program structure/ requirements to illuminate how P2 may reduce toxic emissions.
- Data include 7100 facilities over 16 years.
- Findings:
  - Federal PPA reduces releases.
  - State adoption of P2 decreases average facility toxic releases 3-9% (more for facilities in early-adopting states; less for late-adopters).
  - Technical assistance/education most successful in reducing releases.

RESOURCES FOR THE FUTURE

What we knew before (TRI)

- Firms experienced abnormal negative stock returns immediately after the first release of TRI information in 1989 (Hamilton 1995).
- Firms with negative stock impacts subsequently reduced on-site releases, but increased off-site waste transfers (Khanna *et al*. 1998).
- Firms with largest stock price impact reduced emissions more than industry peers (Konar and Cohen 1997).
- Oil refineries' reductions in toxic emissions may be due to other changes (e.g., CAC regulation of non-toxic pollutants), not just TRI (Bui 2005).

RESOURCES FOR THE FUTURE

What we knew before (P2)

- State adoption of voluntary P2 programs may decrease firms' RCRA violations (Stafford 2003).
- State P2 programs may reduce total toxic releases and increase source-reduction activities (Bennear 2007).
- Estimating changes in reported toxic releases not accounting for reporting thresholds will bias estimates of program impact – maybe a lot (Bennear 2008).

RESOURCES FOR THE FUTURE

What Bui (2011) contributes

- Focus on P2 is useful – less work here, and more likely to be able to estimate causal impact than for TRI.
- Careful examination of the data in the paper is a real strength.
- Potential to understand impacts of different types of policies, different levels of stringency is biggest contribution.

RESOURCES FOR THE FUTURE

Comments on the paper's methods

- Biggest worry – endogeneity
  - OV is correlated with both facility-level emissions and state policy characteristics.
    - E.g., "green" state population affects both state policies (↑) and firm emissions (↓).
  - Resulting bias may erroneously attribute some changes in emissions to state policies.
  - Helps that analyses are performed primarily on low-stringency states, but more discussion needed.

RESOURCES FOR THE FUTURE

Comments on methods, cont.

- Falsification test could be dropped from paper.
  - Statistically significant effects of state policies on releases are in early-adopting states (Table 6).
  - Pooling with late-adopting states, alone, will attenuate estimate (as it does in Table 6).
  - So Table 7 results can't really be attributed to having set false adoption dates for late-adopting states.

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Additional comments

- One state P2 program classified as "monitoring" program may increase cost of non-participation (non-reporting penalties), and one may increase cost of participation (filing fees).
- Not sure these should be grouped together as they are in the discussion, and may explain different results.

RESOURCES FOR THE FUTURE

Additional comments, cont.

- Discussion of emissions response in high-stringency states at the end of the paper needs revision.
- Eq. (1): how can year dummies and PPA_t be separately identified?
- Should explain early in the paper why dependent variable is in natural logs.
- Table 4 is not discussed in the text.

RESOURCES FOR THE FUTURE

Achieving compliance with information disclosure programs through dynamic audit mechanisms

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[February 1, 2011]

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Abstract

This study builds upon the regulatory enforcement literature through the theoretical modeling and experimental testing of two dynamic targeted audit mechanisms for regulating firms based on self-reported actions. The first, similar to existing ones in the literature, is a non-strategic standards-based mechanism that targets firms (e.g., by applying a higher audit probability) based on whether they are found to have complied with regulations in the past. With the second mechanism, a dynamic tournament, firms compete with others in their (targeted or non-targeted) group to avoid being targeted. Both mechanisms are studied in the laboratory, where we find that – consistent with theory – both achieve significant leverage relative to a random audit mechanism. The comparative statics of the dynamic tournament mechanism are broadly confirmed, but this is not the case for the dynamic standards mechanism.

Keywords : Enforcement; Targeting, Tournament, Environmental regulation; Compliance; Self-reporting

JEL classification : D62; L51;Q58

1. Introduction

The resources available to enforce the compliance of regulatory standards are often limited, which mandates that the regulator use these resources effectively. This gives rise to the use of, and need for, audit mechanisms that utilize available information from which to target likely offenders. Much of the literature on regulatory compliance has focused on dynamic mechanisms that target firms (or individuals) based on compliance history (e.g., Landsberger and Meilijson, 1982; Greenberg, 1984; Harrington, 1988; Harford, 1991; Alm, Cronshaw and McKee, 1993; Raymond, 1999; Friesen, 2003; Stafford, 2008; Liu and Neilson, 2009). In dynamic targeting models, firms that are inspected and found non-compliant are transitioned to the targeted group (or remain targeted if already so), and targeted firms can "escape" back to the non-targeted group if inspected and found compliant, which yields a cost of being found noncompliant in addition to any direct fine for both targeted and non-targeted firms. Evidence exists that, at least to some extent, agencies such as the Environmental Protection Agency and the Internal Revenue Service engage in such targeting. Our study adds to the targeting literature through the development of a new competitive "tournament" mechanism as well as a more common "standards"-based mechanism, both framed in an information disclosure setting. Laboratory experiments are used to test the comparative statistics of the theory as well as provide empirical evidence on the leverage achieved from targeting relative to random audits.

With the exception of recent work by Liu and Neilson, existing dynamic targeting mechanisms examine a representative firm and do not model any strategic interaction among firms in an industry. Despite the complexity of the Markov equilibrium, firms are simply solving a dynamic optimization problem with full information of the rules governing the regulator's inspection behavior. Liu and Neilson incorporate into the dynamic targeting setting a constraint on the number of inspections the regulator can conduct each period. Because of this constraint, a fixed number of firms are targeted in each period, which generates a contest among firms to avoid being placed in targeted status, establishing a type of rank-order tournament. This introduces strategic behavior among firms in the industry, which captures an important factor that is likely to be present – or can be incorporated – in actual regulatory settings.

As a departure from Liu and Neilson, we assume there are separate compliance tournaments among the firms placed in the targeted and non-targeted groups. That is, those in the targeted group compete to be transitioned to the non-targeted group, and firms in the non-targeted group compete to avoid being moved to the targeted group. This simplifies the model dramatically, and more importantly, significantly increases the leverage achieved from targeting. In related work using a static model, Gilpatric et al. (forthcoming) provide favorable theoretical and experimental evidence on relative evaluation mechanisms such as tournaments.

One important feature of our targeting models, and in contrast to much of the literature, is that we consider a continuous choice setting. Following Harrington (1988), most models focus in on a dichotomous choice setting wherein firms choose to comply with a regulation or not. Friesen (2003) considered how the design of the targeting mechanism could be optimized with respect to inspection probabilities and fines in both states to achieve the greatest leverage. Stafford (2008) incorporates the possibility of inadvertent non-compliance (e.g. due to an accidental spill) and of self-policing, meaning a firm may choose to self-disclose an inadvertent violation. Clark, et al. (2004) and Cason and Gangadharan (2006) experimentally test the predictions of the dichotomous dynamic targeting models. Although the dichotomous framework has been the focus of the literature, in our view it has significant shortcomings.

In a dichotomous setting, targeting is only relevant to a firm if it complies when targeted but does not when not targeted. This is because if a firm complies regardless of its enforcement state then there is no value of being in the non-targeted group and targeting achieves nothing. Consequently, as Friesen (2003) points out, optimal behavior by the regulator is to never inspect firms in the untargeted group but simply randomly place firms in the targeted group. Because targeting is only consequential to firms who comply only in the targeted state, this implies that targeting is only relevant in a setting where all untargeted firms are non-compliant. This is quite at odds with the motivation of this literature toward explaining evidence that most firms are compliant most of the time despite low fines and inspection probabilities (assuming it isn't possible to target the majority of firms at any given time). Furthermore, despite the complexity of these dynamic models, they essentially can be reduced to demonstrating two rather simple points. First, if a regulator has insufficient enforcement resources (given limitations on fines that can be assessed) to achieve compliance by firms when inspection resources are spread evenly, then it is best to concentrate resources on a subset of firms so that at least this subset will comply. Second, if firms in the targeted subset are rewarded for being found compliant in the event they are audited by being moved to the non-targeted group, this creates an additional benefit of compliance and resources can be spread more thinly, making the size of the subset that can be induced to comply somewhat larger.

In addition to these theoretical issues, experimental evidence suggests the leverage achieved by targeting in the dichotomous framework may be less than predicted. Cason and Gangadharan do find that subjects in the targeted group respond as predicted to an increased probability of being transitioned to the non-targeted group if found compliant. That is, as the benefit of compliance in the targeted group increases due to a higher probability of transition to the non-targeted group, subjects comply at lower inspection probabilities. However, they find that the difference in the share of subjects complying between the targeted and non-targeted groups is not as large as predicted. Clark, et al. find weak evidence that targeted audit mechanisms achieve higher compliance than random audits, and further that targeted audits perform significantly less well than is predicted by theory.

Because of these issues with dichotomous targeting models, and because many compliance choice problems are likely better characterized by a firm's making a marginal choice of compliance effort, we believe the continuous choice dynamic targeting framework first modeled by Harford (1991) merits greater emphasis. As in Harrington and other dichotomous models, in Harford's model firms are placed into targeted or non-targeted groups and transitions occur due to compliance status determined by audits. However, firms in Harford's model make a continuous choice of compliance effort which determines the probability of being found compliant in the event of an audit. In this setting positive compliance effort is elicited from all firms (both targeted and not). The leverage generated by the fact that compliance effort affects not just the probability of a fine but also the probability of transition results in greater effort in both states than would occur with simple random audits of equal probability.

To the best of our knowledge there have been no experimental studies of behavior in a continuous choice dynamic targeting setting. The experimental results in dichotomous choice settings raise the question of whether, in a continuous choice environment, the predicted gains from leverage will be exhibited. Does subject behavior in a continuous environment more closely approximate theoretical predictions than in the dichotomous setting, and does dynamic targeting substantially outperform a simple random audit mechanism? A central purpose of this paper is to provide a first test of the continuous dynamic targeting theory.

2. Models

We frame our theory in the context of a regulation requiring disclosure of an activity which we will call the level of emissions. The basic components of our theoretical models closely follow the static enforcement models of Gilpatric et al. (forthcoming). Disclosure of emissions is assumed to have a constant marginal cost, which could result from an emissions tax, but also could incorporate other costs such as those emanating from a negative market reaction. An audited firm pays a marginal penalty on emissions determined by the audit to have been unreported, and this penalty is assumed to be at least as high as the unit cost of disclosed emissions. The penalty represents any regulatory fines imposed, but again also may entail other costs to the firm of being found non-compliant with the disclosure requirement. Actual emissions, e, are exogenously determined, and firms choose only how much to disclose. Firms may be heterogeneous in terms of their emissions. Audits are subject to error in quantifying a firm's emissions such that an audits reveals emissions of $e + t$, with *t* being a drawn from the distribution $F(t)$, which is assumed to have positive density $f(t)$ on the interval $[a, b]$. Note that we impose little structure on the distribution of audit errors at this point. If audit errors are one-sided (meaning an audit cannot reveal emissions in excess of those actually emitted, so errors involve only failure to detect emissions) then $a < 0$ and $b = 0$. If audits yield an unbiased estimate of emissions then $E[t] = 0$. We will assume $a > -e$ so that an audit cannot reveal negative emissions. We use the following notation:

α	the cost to a firm of disclosed emissions (tax)
β	the cost to a firm of revealed undisclosed emissions (penalty)
γ	the cost to a firm of being audited
e	a firm's quantity of emissions

q the quantity of emissions a firm chooses to disclose

2.1 Disclosure under random audit enforcement

Suppose a firm is simply audited at random with probability p which is independent of whether other firms are audited. Employing a standard enforcement framework similar to that developed in Evans, Gilpatric and Liu (2009), firm i chooses the optimal quantity of emissions to disclose to minimize its expected costs

$$min_{q_i}\, \alpha q_i + p\left\{\gamma + \beta \int_{q_i - e_i}^{b} (e_i + t - q_i) f(t)dt\right\}. \tag{1}$$

So long as an interior solution exists the optimal extent of over/under-reporting of emissions is *independent* of the actual quantity of emissions, and depends only on the regulatory parameters α, β and p, and on the distribution of audit errors $F(t)$. Define $z_i \equiv q_i - e_i$, so a negative z represents under-reporting whereas a positive values represents over-reporting. The reporting choice can then be restated as

$$\min_{z_i} \alpha(e_i + z_i) + p\left\{\gamma + \beta \int_{z_i}^{b} (t - z_i) f(t)dt\right\}. \tag{1'}$$

The optimal choice of disclosure, z_i^* is implicitly defined by

$$\frac{\alpha}{p\beta} = \int_{z_i^*}^{b} f(t)dt = 1 - F(z_i^*). \tag{2}$$

Under random audits an interior solution exists for z_i^* on the interval $[a, b]$ if $0 < \frac{\alpha}{p\beta} < 1$, with z_i^* defined by (2) above. For $\alpha > p\beta$ it is not optimal to report any emissions, so a corner solution at $q_i = 0$ obtains.[1] At an interior solution, the firm's optimal report is decreasing in the reporting cost; increasing in the probability of audit; and increasing in the penalty on revealed

[1] Note that the corner solution when $\alpha > p\beta$ is not $z_i = a$, which would imply $q_i = e_i + a$. If this condition holds it is not optimal to report any emissions—the firm does best simply gambling that it won't be audited. Only when the condition for any interior solution holds does (2) above define the optimum.

but unreported units (these results follow directly from the fact that F is an increasing function of z_i). The solution is independent of the fixed cost being audited. The characteristics of the enforcement regime determine the optimal extent of over/under-reporting, but this is independent of the emissions level.

2.2 Disclosure under dynamic tournament targeted enforcement

We assume N firms operate in a regulated industry for an indefinite period of time. In each period the regulator places the firms into one of two groups, the untargeted group, G_1, and the targeted group, G_2. For simplicity we will assume that the audit probability is higher in the targeted group, but more generally the targeted group may face higher expected compliance costs by differential audit probabilities, fixed audit costs (e.g. through more stringent reporting documentation) and penalties. We first model behavior when the regulator has fixed inspection capacity and the size of the groups is fixed, as in Liu and Neilson (2009). In this setting transitions between groups are driven not just by a firm's own behavior but by the behavior of other firms. Section 2.3 below models behavior when the regulator is not constrained by inspection capacity so that groups sizes can fluctuate and transitions between groups are dependent only a firm's own behavior, as in Harford (1991).

As discussed above, the optimal choice of over/under-reporting in the static random audit mechanism is independent of emissions. In the dynamic game developed here, as long as the distribution of audit errors is assumed to be identical across firms, firms that are heterogeneous in emissions will nevertheless be strategically symmetric competitors. We employ the following notation for the dynamic tournament model:

n_1 number of firms in G_1

n_2 number of firms in $G2$

m_1 number of audits conducted of firms in G_1

m_2 number of audits conducted of firms in $G2$

$\rho_1 = \frac{m_1}{n_1}$ audit probability for firms in G_1

$\rho_2 = \frac{m_2}{n_2}$ audit probability for firms in $G2$

τ number of firms transitioned each period from G_1 to $G2$ and vice-versa

δ discount factor

With a slight abuse of notation, let i index firms in G_1 and j index firms in G_2. Each firm in G_1 competes against the others in its group to avoid ranking at the bottom if audited and therefore being transitioned to G_2. Each firm in G_2 competes against the others in its group to rank first if audited and therefore be transitioned to G_1. This generates a Lazear-Rosen type rank-order tournament within each group where rankings are determined by a combination of a costly choice (here, the level of disclosure) and random noise (here, the error in the audit process). The τ firms in G_1 that are audited and found to have reported the least relative to the audit outcome (irrespective of whether it is found to have reported less than the audit outcome) are transitioned to G_2. That is, the τ firms in G_1 for which $t_i - z_i$ is largest are transitioned to G_2.The τ firms in G_2 that are audited and found to have reported the most relative to the audit outcome (irrespective of whether it is found to have reported more than the audit outcome), i.e. the firm for which $t_j - z_j$ is smallest, are transitioned to G_1. This transition process differs from that discussed by Liu and Neilson (2009), who assumed all audited firms (regardless of current group) were ranked against each other and the m_2 lowest ranked firms were placed in G_2 in the following period.

Let the probability that a firm in G_1 which is audited ranks among the bottom τ firms (and therefore gets transitioned to G_2) be represented by $Q_i(z_i, z_{-i})$ and the probability that a firm in

G_2 which is audited ranks among the top τ firms (and therefore gets transitioned to G_1) be represented by $R_j(z_j, z_{-j})$. The Markov transition matrix representing the probability that firm will be in G_1 or G_2, conditional on its current group assignment, is as follows:

	To Group	
From Group	G_1	G_2
G_1	$1-\rho_1 Q_i$	$\rho_1 Q_i$
G_2	$\rho_2 R_j$	$1-\rho_2 R_j$

Let k_{lt} be the expected cost for a firm in group l=1,2 at time t. Following from the random audit model, this is equal to $k_{lt} = \alpha(e_i + z_i) + \rho_l \left\{\gamma + \beta \int_{z_i}^{b}(t - z_i) f(t)dt\right\}$. Further, let V_{lt} be the expected present value of total costs for a firm in group l at time t. Then we have

$$V_{1t} = k_{1t} + \delta(1 - \rho_{1t}Q_{it})V_{1,t+1} + \delta\rho_{1t}Q_{it}V_{2,t+1}$$

and

$$V_{2t} = k_{2t} + \delta\rho_{2t}R_{jt}V_{1,t+1} + \delta(1 - \rho_{2t}R_{jt})V_{2,t+1}.$$

A firm in G_1 at any point in time minimizes V_{1t} and a firm in G_2 at any point in time minimizes V_{2t}. Given stationarity (which allow us to drop the t subscripts) we obtain the following first order necessary conditions:

$$G_1: \frac{\partial k_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1 \frac{\partial Q_i}{\partial z_i}$$

$$G_2: \frac{\partial k_j}{\partial z_j} = \delta(V_2 - V_1)\rho_2 \frac{\partial R_j}{\partial z_j}$$

Where

$$(V_2 - V_1) = \frac{(k_2 - k_1)}{1 - \delta(1 - \rho_2 R_j - \rho_1 Q_i)}.$$

Imposing symmetric behavior by all firms in each group yields

$$G_1\text{: } \frac{\partial k_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1 \frac{\partial Q_i}{\partial z_i}\Big|_{z_i=z_{-i}}$$

$$G_2\text{: } \frac{\partial k_j}{\partial z_j} = \delta(V_2 - V_1)\rho_2 \frac{\partial R_j}{\partial z_j}\Big|_{z_j=z_{-j}}$$

and

$$(V_2 - V_1) = \frac{(k_2 - k_1)}{1-\delta\left(1-\rho_2\left(\frac{\tau}{m_2}\right)-\rho_1\left(\frac{\tau}{m_1}\right)\right)}.$$

This set of three equations implicitly defines the solution to the dynamic game. Note that a firm minimizing its cost in the current period given its group and consequent audit probability would set $\frac{\partial k}{\partial z} = 0$. The equations above show there is a gain from leverage in dynamic enforcement because $\frac{\partial k_i}{\partial z_i} > 0$ and $\frac{\partial k_j}{\partial z_j} > 0$. Furthermore, note that the magnitude of the gain depends on the value of $(V_2 - V_1)$, the difference in the present value of expected costs to the firm in equilibrium when in G_2 versus G_1. This difference is the prize at stake in the contest, and the magnitude of the difference depends on two things: the difference in inspection probabilities between the two groups and the equilibrium transition probabilities, which determine the "stickiness" of the states (targeted or untargeted). The lower the transition probabilities, the more valuable it is to be in the untargeted group G_1 rather than the targeted group G_2.

2.3 Disclosure under dynamic standards targeted enforcement

Here we develop a model that is similar to that above but where each firm is regulated independently so that transitions are determined solely by a firm's disclosure choice relative to a standard. This model is therefore an adaptation of Harford's (1991) model to the disclosure choice setting.

The stage game here is exactly the same as for the dynamic tournament. The only difference in the mechanism is that transitions from G_1 to G_2 occur if a firm is audited and found in violation of a standard, and transitions from G_2 to G_1 occur if a firm is audited and found to have met the standard. In the disclosure choice setting a natural standard is "the truth", i.e. a firm is in violation if an audit reveals greater emissions than disclosed by the firm, i.e. $t > z$ (where t is the audit outcome, which is of course random). However, this need not be the case, and fixing the standard in this fashion constrains the regulator significantly. In particular, the level of the standard relative to actual emissions has important consequences because it affects the equilibrium transition probabilities.

We assume the standard that determines whether a firm is transitioned can be chosen by the regulator and that it is possible to apply a different standard in G_1 than in G_2. We denote the distance of the standard between the report and audit outcome in each group, respectively, by s_1 and s_2. Thus a firm in G_1 that is audited will be transitioned to G_2 only if $t > z + s_1$, i.e. if the firm is found to have *under-reported by more than* s_1. Similarly a firm in G_2 that is audited will be transitioned to G_1 only if $t < z + s_2$, i.e. if the firm is found to have *under-reported by no more than* s_2. Note that the standards can be negative or positive, i.e. the position of the standard may be "looser" or "tighter" than the truth.

With this notation, a firm in G_1 that is audited is transitioned to G_2 with probability $\int_{z_1+s_1}^{b} f(t)dt = \big(1 - F(z_1 + s_1)\big)$ and a firm in G_2 that is audited is transitioned to G_1 with probability $\int_{a}^{z_2+s_2} f(t)dt = F(z_2 + s_2)$. This gives us the following transition matrix:

	To Group	
From Group	G_1	G_2

G_1	$1-\rho_1\big(1-F(z_1+s_1)\big)$	$\rho_1\big(1-F(z_1+s_1)\big)$
G_2	$\rho_2 F(z_2+s_2)$	$1-\rho_2 F(z_2+s_2)$

As in section 2.2, Let V_{lt} be the expected present value of total costs for a firm in group l at time t. Then we have

$$V_{1t} = k_{1t} + \delta\left(1-\rho_{1t}\big(1-F(z_{1t}+s_1)\big)\right)V_{1,t+1} + \delta\rho_{1t}\big(1-F(z_{1t}+s_1)\big)V_{2,t+1}$$

$$V_{2t} = k_{2t} + \delta\rho_{2t}F(z_{2t}+s_2)V_{1,t+1} + \delta\big(1-\rho_{2t}F(z_{2t}+s_2)\big)V_{2,t+1}.$$

A firm in G_1 at any point in time minimizes V_{1t} and a firm in G_2 at any point in time minimizes V_{2t}. Given stationarity we obtain the following first order conditions:

$$G_1: \frac{\partial k_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1\big(-f(z_1+s_1)\big)$$

$$G_2: \frac{\partial k_j}{\partial z_j} = \delta(V_2 - V_1)\rho_2 f(z_2+s_2)$$

Where

$$(V_2 - V_1) = \frac{(k_2 - k_1)}{1-\delta\Big(1-\rho_2F(z_2+s_2)-\rho_1\big(1-F(z_1+s_1)\big)\Big)}.$$

These equations defining equilibrium behavior of course look very similar to those derived for the dynamic tournament model of section 2.2. There are two differences. First, the marginal effect of disclosure on the probability of being transitioned is not determined by the tournament equilibrium but instead directly by the density of the audit error distribution. Second, in the dynamic tournament model the equilibrium transition probability in each group is simply the number of transitions divided by the size of the group. In the dynamic standards model the equilibrium transition probabilities depend on equilibrium disclosure levels as well as the standards.

3. Experimental Design

The primary objectives of the compliance experiment are to test the main comparative statics of the theory and examine the leverage gained through dynamic targeted enforcement. The experiment instructions use neutral framing, but for ease of exposition we describe the experimental design and discuss results using environmental disclosure context. The compliance experiment involves 10 players who play several "games", where each game consists of a sequence of decision periods under the same treatment conditions. At the beginning of each game and based on random assignment, n_1=5 players are assigned to G_1 ("Group 1") and n_2=5 to G_2 ("Group 2"). Under the targeting mechanisms G_1 is the non-targeted group and G_2 the targeted group. In each decision period, players receive endowment E and have baseline emissions ("output" in the experiment) of 20. The decision task for each player is to choose a level of disclosure ("reported output"), at a per-unit tax ("reporting cost") of $1 in experiment currency, by selecting a whole number between 0 and 40. After all choices are made, players are selected for audit ("inspection").

Players face one of the three enforcement mechanisms discussed in the theory section: random audit, dynamic tournament, or dynamic standards. The probability of audit, or in the case of the dynamic tournament the fixed proportion of audited players, differs across the two groups. For players selected for audit, they pay a fixed audit cost ("inspection cost"). The audit is unbiased and reveals a level of emissions ("estimated output") by drawing an i.i.d. random number from the uniform distribution with supports [0, 40]. A penalty of $2 is levied on any emissions revealed by the audit to have been undisclosed.

For the dynamic tournament, from the players audited, a fixed number of players are transitioned to the other group. For the dynamic standards, players in G_1 are transitioned if they are estimated to have under-reported by more than the standard s_1. Players in G_2, the targeted group, are transitioned if they are estimated to have under-reported by less than the standard s_2 (or, equivalently, estimated to have over-reported by at least $-s_2$). Under the random audit mechanism there is no possibility of being transitioned.[2]

To capture in the lab setting the incentives of an indefinitely-repeated game (or infinitely-repeated with discounting) the number of periods in a game is uncertain from the players' perspective. In particular, players are told that after each period the computer will determine whether the game will continue an additional period and that there is a 90% chance of continuing (i.e. δ=0.9). The corresponding distribution of possible game lengths has a long right tail. Because of this, and to allow for more control over the game length and the variation in game length (for testing purposes), these are pre-determined. In particular, the possible game lengths are 6, 8, 12 and 14. The mean game length is 10, which corresponds with the mean of the underlying probability distribution. The extreme game lengths correspond with the 30th and 70th percentiles of the distribution.

The feedback given at the end of the decision period includes: (1) whether the player was audited, and if so revealed emissions; (2) all relevant earnings calculations; (3) the disclosure reports of all players in the session and whether they were audited; (4) whether the game will continue an additional period; and, for the targeting mechanisms, (5) which player(s) got transitioned. While this level of feedback helps to facilitate learning, it also reflects the field disclosure environment wherein the disclosure reports of firms are public record.

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[2] For the random audit treatments, we could have gotten rid of all the structure relevant for the dynamic targeting mechanisms, e.g. the notion of groups, a dynamic game, etc. However, we argue that our design allows for additional control when identifying the leverage effect of targeted audits. In particular, this controls for the possibility that the targeting mechanisms may motivate different behavior relative to random audits simply because players know that others have a higher (lower) audit probability. Further, it would likely be easier to learn – through observing the decisions of others – in the situation where everyone faces the same audit probability versus one where only a subset of the group faces the same audit probability.

To help facilitate learning and to undertake some targeted within-subject tests, participants play four games overall. The first two games involve a single treatment, with game lengths of 8 and 12 (or 12 and 8). The remaining two games correspond with a second treatment, with game lengths of 14 and 6 (or 6 and 14). Prior to all games, players are randomly assigned into one of the two groups.

Tables 1, 2 and 3 summarize the 16 experimental treatments. There are six treatments each of the dynamic tournament and dynamic standards, and four random audit treatments. Variable across treatments are audit probabilities (40% or 60% for G_1 and 60% or 80% for G_2), fixed audit cost ($25 or $50), and for the targeting mechanisms the (equilibrium) transition probabilities (20% or 40%). To achieve these transition probabilities with the dynamic tournament, either one or two of the members in each group (of five) are transitioned. Transition probabilities are zero for the random audit mechanism, which explains why there are only four unique random audit treatments.

The 16 distinct treatments are paired selectively with a second treatment to construct 16 sessions, which are presented in Table 2. To minimize both cognitive burdens as well as to allow for clean identification of treatment effects, with few exceptions, only one main element of the design changes across treatments within-session. The exchange rates and endowments vary by treatment. These parameters were chosen to equate the group-specific payoffs, under equilibrium play, across treatments. We note that there are also meaningful differences in expected payoffs across the untargeted and targeted groups (approximately 55 cents per period or $22 for a 40-period session).

Table 3 presents the group-specific predictions of disclosure levels (q) and disclosure rates (q/e) by treatment. Note that corresponding dynamic tournament and dynamic standards treatments have approximately equal predictions. This is deliberate, in order to place the mechanisms on theoretical equal footing. Specifically, this is achieved by first solving the set of first-order conditions for the dynamic tournament as given in the theory section. Note that for our experimental design it can be shown that $\frac{\partial Q_i}{\partial z_i}|_{z_i=z_{-i}} = -\frac{1}{2}$ and $\frac{\partial R_j}{\partial z_j}|_{z_j=z_{-j}} = \frac{1}{2}$. From our choice of the distribution of audit errors it also follows that $-f(z_1 + s_1) = -\frac{1}{2}$ and $f(z_2 + s_2) = \frac{1}{2}$, for $z_1 \in [0,40 - s_1]$ and $z_2 \in [0,40 - s_2]$. Thus, once the dynamic tournament solution is obtained, the corresponding dynamic tournament mechanism arises by setting the standards s_1 and s_2 in order to equate the equilibrium transition probabilities between the two mechanisms. Note, however, that the actual standards in the design differ slightly from those that make the two mechanisms theoretically equivalent in equilibrium. This is purely to avoid using odd "looking" standards in the experiment.

3.1 Testable hypotheses

The experimental design has several features. Common to related compliance experiments, the chosen audit probabilities are much larger than those in relevant, naturally-occurring situations for purposes of transparency and saliency. In a similar vein, the parameters generate a wide range of predicted disclosure shares, with meaningful differences between key treatment pairs, and predicted under-compliance, approximate compliance, and over-compliance. The two targeting mechanisms are parameterized to generate (approximately) equivalent symmetric equilibrium predictions for each treatment. The design allows us to test for equivalence between three components of the audit the regulator has control over: the audit cost, audit probability (both within and between groups), and transition probability. By varying the equilibrium transition probabilities the design allows us to test whether leverage is reduced by higher transition probabilities, as the theory predicts. The main hypotheses to be tested are summarized below:

Hypothesis 1 (audit cost effect). Targeted audits: increasing the fixed audit cost increases disclosure; Random audit: no audit cost effect.

Hypothesis 2 (audit probability effect). Increasing the audit probabilities leads to higher disclosure rates.

Hypothesis 3 (transition effect). Targeted audits: increasing the transition probabilities decreases disclosure rates in both groups.

Hypothesis 4 (group effect). Disclosure is higher in targeted group, G_2.

Hypothesis 5 (leverage effect). Targeted audits lead to higher disclosure than random audits.

Hypothesis 6 (mechanism equivalence). The tournament and standard mechanisms lead to identical disclosure rates.

All hypotheses can be tested based on between-subjects comparisons. Further, the transition effect, leverage effect, and mechanism equivalence hypotheses are testable by comparing within-session behavior across the two treatments.

3.2 Participant pool and procedures

Experiments were run at the University of Tennessee during the fall of 2010 in a designated experimental laboratory. There are 16 sessions, and 20 unique players participated in each. This allowed us to conduct two experiments for each treatment-pair, additional anonymity, as well as variation in the game length for each treatment-pair. The participants were drawn from a large pool of students that had previously registered to be potential participants in economics experiments. The participant pool is similar to the general undergraduate population in terms of age, gender, and academic major. Participant earnings were denominated in experimental currency, which was exchanged for dollars at the end of the session at a common and known exchange rate. The experiment lasted approximately 1 hour and 45 minutes with average earnings of approximately $35. Due to time constraints, not all periods for the second treatment conducted within-session were completed for all sessions. There are a minimum of 10 periods completed for the second treatment, as illustrated in Table 2.

The experiment was implemented using software programmed with z-Tree [Fischbacher, 2007]. The software collected all decisions and made all relevant earnings calculations. Written instructions were provided to each participant, and were read aloud by the same author. To help facilitate learning, participants were asked to work through a series of calculations questions (using pencil and paper) and were paid for providing correct answers.[3] The questions involved making a hypothetical disclosure choice and then determining earnings under three possible audit outcomes. Further, participants had to determine whether they would be transitioned to the other group based on their disclosure choice and audit outcome. Experiment moderators privately checked the calculations and re-explained procedures in the case of wrong answers. Prior to each of the two treatments, there were two corresponding practice periods. At the conclusion of the experiment, a short questionnaire was administered to assess how well instructions were understood and to elicit basic information on demographics.

4. Preliminary Results

As an initial analysis of the data, we focus on the disclosure data obtained from the first treatment in each session using a simple ordinary least squares estimator that allows the mean disclosure rates to vary fully by group assignment as well as by treatment. The disclosure rate (i.e. the proportion of emissions disclosed), q_i/e_i, is used as the basis of analysis for ease of interpretation. To control for heteroskedasticity as well as individual-specific autocorrelation, we

[3] Experiment instructions are available upon request from the authors.

compute cluster-robust standard errors for the regression coefficients. Further, heteroskedasticity-autocorrelation consistent tests are used to evaluate hypotheses.

Tables 4 through 6 present all relevant difference-of-means tests based on the linear regression model to evaluate the six hypotheses stated in Section 3.1. The main results are summarized as follows:

> **Result 1.** Both dynamic targeting mechanisms achieve significant leverage.
>
> **Result 2**. All comparative statics are confirmed for the random audit and dynamic tournament mechanisms.
>
> **Result 3**. Disclosure rates under the dynamic standards mechanism are invariant to changes in the audit cost as well as changes in the standards.
>
> **Result 4**. On average, the dynamic tournament and dynamic standards mechanisms lead to equivalent disclosure rates.

The most basic implication of the theory, that targeting leads to significant enforcement leverage, is strongly confirmed by statistical tests. As illustrated in Table 6, the leverage effect is positive and significant for every possible treatment and group of the dynamic targeting mechanisms. The leverage effects are of similar magnitude to those suggested by theory. This result contrasts those of previous experimental work, which considered a dichotomous choice framework with perfect-revealing audits. It is unclear whether the continuous decision space or the audit uncertainty is the source of the disparity. However, as these conditions likely better reflect the disclosure environment, they suggest new promise for the use of targeting mechanisms as well as their ability to explain the compliance puzzle.

The theory is further very good at explaining changes in behavior across treatment conditions for the random audit and dynamic tournament mechanisms. Specifically, consistent with theory, the disclosure rate for the random audit mechanism is invariant to changes in audit cost and increases with an increase in the audit probability across treatments, and across groups within treatment. The dynamic tournament exhibits strong audit cost, audit probability, and transition effects. These effects are statistically significant at the treatment level, although in a few cases are not significant at the group-level. Further, there are statistically significant differences between the targeted and untargeted groups for each treatment.

In contrast, the effects of treatment conditions are muddled for the dynamic standards mechanism. In contrast to the theory, there are no statistically discernable effects of audit cost or the standards themselves. There is some mixed evidence on the audit probability and group effects, with statistical differences in about half of the treatments and groups. The relative scarcity of comparative statics with the mechanism suggests a weakened effect of varying the relative cost of being in the targeted group. Nevertheless, the dynamic standards mechanism achieves, on average, equivalent compliance to the dynamic tournament.

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Table 1. Selected experiment parameters

Treatment	Audit cost (γ)	Audit probability for G_1 (ρ_1)	Audit probability for G_2 (ρ_2)	Transition probability (targeting mechanisms)	Standards (dynamic standards mechanism)
_1	25	0.4	0.6	0.2	s_1 = 10, s_2 = -15
_2	50	0.4	0.6	0.2	s_1 = 10, s_2 = -15
_3	25	0.6	0.8	0.4	s_1 = 0, s_2 = 0
_4	50	0.6	0.8	0.4	s_1 = 0, s_2 = 0
_5	25	0.6	0.8	0.2	s_1 = 10, s_2 = -15
_6	50	0.6	0.8	0.2	s_1 = 10, s_2 = -15

Table 2. Session summary (within-session treatment pairings)

Session	First treatment	Second treatment	ω	Exchange rate	Periods Completed
1	R1	S2	55 / 85	25:1 / 40:1	40
2	R2	T2	85	40:1	32
3	R3	S3	60	20:1	40
4	R4	T3	80 / 60	25:1 / 20:1	35
5	T1	S1	55	25:1	30
6	T2	R2	85	40:1	36
7	T3	S3	60	20:1	40
8	T4	T6	85	30:1	31
9	T5	R3	60	20:1	37
10	T6	T4	85	30:1	40
11	S1	T1	55	25:1	36
12	S2	R2	85	40:1	40
13	S3	T3	60	20:1	30
14	S4	S6	85	30:1	40
15	S5	R3	60	20:1	40
16	S6	S4	85	30:1	34

Notes: R≡random audit; T≡dynamic tournament; S≡dynamic standard.

Table 3. Theoretical predictions for disclosure level [disclosure *rates* in brackets]

Random audit			**Dynamic Tournament**			**Dynamic Standards**		
Treatment	q_1^*	q_2^*	Treatment	q_1^*	q_2^*	Treatment	q_1^*	q_2^*
R1	0.0 [0.00]	6.7 [0.33]	T1	4.0 [0.20]	20.7 [1.03]	S1	5.6 [0.28]	22.4 [1.12]
R2	0.0 [0.00]	6.7 [0.33]	T2	9.9 [0.49]	26.5 [1.33]	S2	11.0 [0.55]	27.6 [1.38]
R3	6.7 [0.33]	15 [0.75]	T3	11.8 [0.59]	20.1 [1.01]	S3	11.6 [0.58]	20.0 [1.00]
R4	6.7 [0.33]	15 [0.75]	T4	14.6 [0.73]	22.9 [1.15]	S4	14.3 [0.71]	22.6 [1.13]
			T5	16.1 [0.80]	24.4 [1.22]	S5	16.2 [0.81]	24.4 [1.22]
			T6	21.6 [1.08]	30.0 [1.50]	S6	20.8 [1.04]	29.2 [1.46]

Table 4. Between-subjects, within-mechanism hypothesis tests

	Difference in disclosure rate (std. err.)		
Hypothesis	Pooled	G_1	G_2
Audit cost effect			
R2 = R1	-0.040 (0.096)	-0.041 (0.127)	-0.040 (0.101)
R4 = R3	-0.011 (0.082)	0.050 (0.113)	-0.072 (0.065)
T2 = T1	0.184* (0.094)	0.174 (0.112)	0.194 (0.127)
T4 = T3	0.208** (0.058)	0.221** (0.092)	0.194** (0.070)
T6 = T5	0.159** (0.058)	0.190** (0.071)	0.128* (0.078)
S2 = S1	0.148 (0.096)	0.150 (0.121)	0.210 (0.152)
S4 = S3	-0.025 (0.074)	-0.062 (0.097)	0.060 (0.081)
S6 = S5	0.068 (0.075)	0.021 (0.098)	0.026 (0.118)
Audit probability effect			
R3 = R1	0.189** (0.088)	0.178 (0.124)	0.201** (0.064)
R4 = R2	0.219** (0.090)	0.269** (0.117)	0.169* (0.101)
T5 = T1	0.335** (0.078)	0.292** (0.082)	0.379** (0.108)
T6 = T2	0.311** (0.079)	0.308** (0.104)	0.313** (0.103)
S5 = S1	0.148** (0.072)	0.142 (0.103)	0.266** (0.101)
S6 = S2	0.068 (0.098)	0.012 (0.117)	0.082 (0.163)
Transition effect			
T5 = T3	0.236** (0.056)	0.167** (0.065)	0.306** (0.075)
T6 = T4	0.187** (0.059)	0.136 (0.097)	0.239** (0.073)
S5 = S3	-0.055 (0.071)	-0.129 (0.095)	0.062 (0.086)
S6 = S4	0.039 (0.079)	-0.046 (0.100)	0.030 (0.114)

Notes: *, ** denote estimate is statistically significant at the 10% and 5% significance levels, respectively.

Table 5. Within-subjects, within-treatment hypothesis tests

Hypothesis	Difference in disclosure rate (p-value)
Group effect	
R1	0.286** (0.104)
R2	0.287* (0.091)
R3	0.308** (0.063)
R4	0.187** (0.080)
T1	0.330** (0.090)
T2	0.350** (0.094)
T3	0.279** (0.056)
T4	0.252** (0.092)
T5	0.417** (0.060)
T6	0.355** (0.068)
S1	0.149 (0.118)
S2	0.208 (0.138)
S3	0.081 (0.091)
S4	0.202** (0.050)
S5	0.273** (0.065)
S6	0.278** (0.125)

Notes: *, ** denote estimate is statistically significant at the 10% and 5% significance levels, respectively.

Table 6. Between-subjects, between-mechanism hypothesis tests

	Difference in disclosure rate (p-value)		
Hypothesis	Pooled	G_1	G_2
Leverage effect			
T1 = R1	0.255** (0.095)	0.233** (0.125)	0.278** (0.104)
T2 = R2	0.479** (0.095)	0.448** (0.114)	0.511** (0.124)
T3 = R3	0.165** (0.068)	0.180** (0.085)	0.150** (0.063)
T4 = R4	0.384** (0.074)	0.351** (0.119)	0.416** (0.072)
T5 = R3	0.401** (0.069)	0.347** (0.079)	0.456** (0.070)
T6 = R4	0.571** (0.073)	0.487** (0.108)	0.655** (0.073)
S1 = R1	0.428** (0.095)	0.475** (0.139)	0.338** (0.100)
S2 = R2	0.617** (0.096)	0.668** (0.106)	0.587** (0.152)
S3 = R3	0.442** (0.085)	0.567** (0.109)	0.340** (0.080)
S4 = R4	0.428** (0.071)	0.455** (0.101)	0.471** (0.066)
S5 = R3	0.387** (0.062)	0.438** (0.080)	0.403** (0.065)
S6 = R4	0.466** (0.092)	0.409** (0.127)	0.500** (0.118)
Mechanism equivalence			
T1 = S1	-0.088 (0.093)	-0.162 (0.118)	0.030 (0.126)
T2 = S2	-0.083 (0.097)	-0.159 (0.115)	-0.026 (0.153)
T3 = S3	-0.287** (0.075)	-0.397** (0.099)	-0.200** (0.085)
T4 = S4	-0.064 (0.056)	-0.124 (0.089)	-0.075 (0.066)
T5 = S5	0.019 (0.050)	-0.082 (0.057)	0.053 (0.077)
T6 = S6	0.065 (0.081)	0.037 (0.107)	0.115 (0.118)

Notes: *, ** denote estimate is statistically significant at the 10% and 5% significance levels, respectively. The tests for mechanism equivalence take into account the slight differences in theoretical predictions across the two targeting mechanisms.

Achieving compliance with information disclosure programs through dynamic audit mechanisms

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Overview

- We contribute to understanding dynamic targeted enforcement by
  - Applying targeted enforcement to disclosure choice setting
  - Expanding theory of firm behavior in a strategic context
  - Experimentally testing predictions of the theory
- Two perspectives on relevance of models.
- Motivation for experiments.

Introduction

- High rates of compliance with environmental regulations a puzzle given low audit frequency and typically small fines for violations (Russell et al., 1986; Harrington, 1988; Livernois and McKenna 1999)
- Harrington (1988) first suggested this is due to leverage from "targeted" enforcement
  - Firms divided into two groups: targeted, untargeted
  - Firms are targeted based on compliance history
  - Being transitioned to targeted group if found in violation, or to untargeted group if found compliant, creates additional cost of noncompliance

Introduction

- Most literature explores a dichotomous choice setting (firms comply or not) (e.g. Friesen, 2003; Stafford, 2008).
- Issues with dichotomous framework
  - Optimal targeting entails no audits of untargeted firms
  - Does a poor job of explaining compliance puzzle.
- Experimental evidence offers weak support
  - Cason and Gangadharan (2006) find weaker comparative static effects than predicted.
  - Clark et al. (2004) find only weak evidence that they outperform random audits

Introduction

- Issues with dichotomous framework suggest focusing instead on continuous choice model of Harford (1991)
  - Firms choose abatement effort, which determines probability of being found in compliance if audited
  - Positive compliance effort is elicited from firms in both groups
  - However, differential abatement effort between groups is inefficient if cost of abatement effort is convex

Introduction

- Inducing compliance through strategic interaction.
  - Past targeting models are non-strategic games.
  - Gilpatric et al. (*forthcoming*) develop and test compliance models based on relative evaluation in static setting.
  - Liu and Neilson (WP) introduce strategic interaction among firms into dynamic targeting model.

Models

- Firms are required to disclose level of activity (emissions)
  - α the cost to a firm of disclosed emissions (“tax”)
  - β the cost to a firm of revealed undisclosed emissions (“penalty”)
  - γ the cost to a firm of being audited
  - e a firm’s quantity of emissions
  - z the share of emissions a firm chooses to disclose
- Audits are imperfect (Evans et al., 2009)
  - If audited a share t of a firm’s emissions are revealed
  - t is drawn from distribution $F(t)$ on $[0,d]$
  - Errors may be one-sided, unbiased, or otherwise

Models—Random Audits

- Firms are audited with independent probability ρ
- Firms minimize expected costs:

$$\min_{z} \alpha z e + \rho\left\{\gamma + \beta e \int_{z}^{d} (t - z) f(t)\, dt\right\}$$

  - Optimal disclosure z^* is defined by

$$\frac{\alpha}{\rho\beta} = \int_{z^*}^{d} f(t)\, dt$$

Models—Dynamic Tournament

- Regulator places each firm in a peer group of N firms into one of two groups: untargeted, G_1, or targeted, G_2
- We’ll assume firms have identical emissions levels and normalize $e\equiv1$.*
- Game is played indefinitely, with each firm’s expected cost in a particular period being

$$k = \alpha z + \rho\left\{\gamma + \beta \int_{z}^{d} (t - z) f(t)\, dt\right\}$$

Models—Dynamic Tournament

- We employ the following notation

n_1	number of firms in G_1
n_2	number of firms in G_2
m_1	number of audits conducted of firms in G_1
m_2	number of audits conducted of firms in G_2
$\rho_1 = m_1 / n_1$	audit probability for firms in G_1
$\rho_2 = m_2 / n_2$	audit probability for firms in G_2

Models—Dynamic Tournament

- Mechanics. In each decision period:
  - The $x < m_1$ firms in G_1 that are audited and found to reported the least relative to the audit outcome are transitioned to G_2
  - The $x < m_2$ firms in G_2 that are audited and found to reported the most relative to the audit outcome is transitioned to G_1
- Firms now compete to avoid being targeted in a form of rank-order tournament (Lazear and Rosen, 1981).
- In equilibrium, transition probability is simply the number of transitions divided by group size.

Models—Dynamic Standards

- Let s_1 denote the standard in G_1. An audited firm will be transitioned to G_2 if $t>z+s_1$
- Let s_2 denote the standard in G_2. An audited firm will be transitioned to G_1 if $t<z+s_2$
- In equilibrium, transition probability depends on audit probability, distribution of audit errors and position of standards.
- Given the number of tournament transitions and error distribution, can devise “equivalent” standards mechanism.

Experimental Design

- Experiment involves 10 players: (initially) 5 each in G_1 and G_2.
- Players receive an endowment and all have equal emissions ("output") of 20
- Players chose level of disclosure ("reported output") each period by selecting an integer number between 0 and 40
- Per unit tax ("reporting cost") is $1
- Players are randomly selected for audit. Audits ("inspections") are unbiased and reveal an amount ("estimated output") between 0 and 40.

Experimental Design

- Those audited pay a fixed inspection cost.
- Those audited pay a $2/unit penalty on under-reported emissions.
- Continuation probability of 0.9.
- The game is played twice.
- Players receive feedback at the end of each period.

Experimental Design

- Treatments vary the following
  - Enforcement mechanism (Random Audit, Tournament, Standards)
  - Fixed cost of being audited (γ=25 and γ=50)
  - Equilibrium transition probability (e.g. in tournament, 1 or 2 transitions each direction each period)
  - Audit probabilities (ρ_1=0.4 or 0.6; and ρ_2= 0.6 or 0.8)

Implementation

- UT undergraduates.
- Two replications per treatment (n=20).
- Sixteen sessions (one per treatment)
- Endowments & exchange rates varied to equalize payoffs.

Treatments and theoretical predictions

					Random		Tournament		Standard	
Treatment	γ	ρ_1	ρ_2	Trans. Prob. (approx.)	z_1	z_2	z_1	z_2	z_1	z_2
1	25	0.4	0.6	0.2	0.0	0.33	0.20	1.03	0.28	1.12
2	50	0.4	0.6	0.2	0.0	0.33	0.49	1.33	0.55	1.38
3	25	0.6	0.8	0.4	0.33	0.75	0.59	1.01	0.58	1.00
4	50	0.6	0.8	0.4	0.33	0.75	0.73	1.15	0.71	1.13
5	25	0.6	0.8	0.2	*	*	0.80	1.22	0.81	1.22
6	50	0.6	0.8	0.2	*	*	1.08	1.50	1.04	1.46

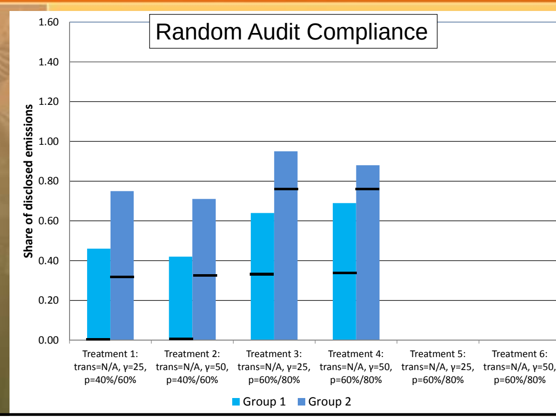
* For Random mechanism, treatments 5 and 6 are redundant.

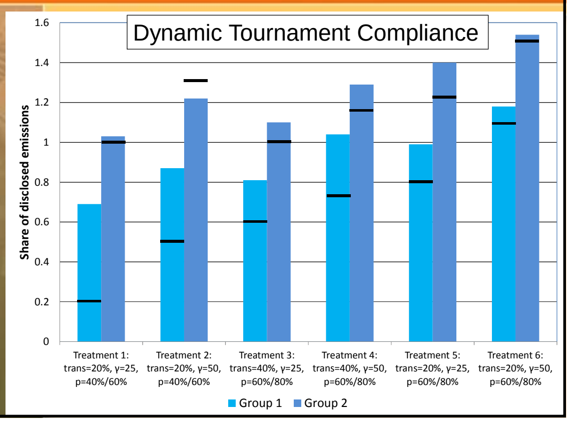
Testable Hypotheses: within-mechanism

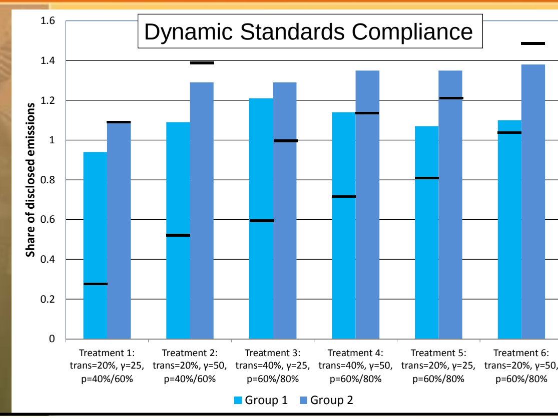
- **Hypothesis 1 (audit cost effect)**. Targeted audits: increasing the fixed audit cost increases disclosure; Random audit: no audit cost effect.
- **Hypothesis 2 (audit probability effect)**. Increasing the audit probabilities leads to higher disclosure rates.
- **Hypothesis 3 (transition effect).** Targeted audits: increasing the transition probabilities decreases disclosure rates in both groups.
- **Hypothesis 4 (group effect)**. Disclosure is higher in targeted group, G_2.

Testable Hypotheses: between-mechanism

- **Hypothesis 5 (leverage effect)**. Targeted audits lead to higher disclosure than random audits.
- **Hypothesis 6 (mechanism equivalence).** The tournament and standard mechanisms lead to identical disclosure rates.







Econometrics

- Analyze as panel data.
- Simple and general specification.
  - Allow mean disclosure rate to vary by mechanism / treatment / group.
  - Tests robust to heteroskedasticity and individual-specific autocorrelation.
  - Test hypotheses by pair-wise tests of means (separately by treatment and by group).

Hypothesis 1 – audit cost effect ($50 v. $25)

Comparison	Pooled	G_1	G_2
R2 = R1	-0.040 (0.096)	-0.041 (0.127)	-0.040 (0.101)
R4 = R3	-0.011 (0.082)	0.050 (0.113)	-0.072 (0.065)
T2 = T1	0.184* (0.094)	0.174 (0.112)	0.194 (0.127)
T4 = T3	0.209** (0.058)	0.224** (0.093)	0.194** (0.070)
T6 = T5	0.163** (0.058)	0.190** (0.071)	0.136* (0.078)
S2 = S1	0.149 (0.096)	0.152 (0.121)	0.210 (0.152)
S4 = S3	-0.025 (0.074)	-0.062 (0.097)	0.060 (0.081)
S6 = S5	0.068 (0.075)	0.021 (0.098)	0.026 (0.118)

- Results:
  - Theory confirmed for Random Audit & Tournament
  - Theory NOT confirmed for Standards

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Hypothesis 2 – audit probability effect (40%/60% v. 60%/80%)

Comparison	Pooled	G_1	G_2
R3 = R1	0.189** (0.088)	0.178 (0.124)	0.201** (0.064)
R4 = R2	0.219** (0.090)	0.269** (0.117)	0.169* (0.101)
T5 = T1	0.335** (0.078)	0.292** (0.082)	0.379** (0.108)
T6 = T2	0.315** (0.079)	0.308** (0.104)	0.322** (0.103)
S5 = S2	0.148** (0.072)	0.142 (0.103)	0.266** (0.101)
S6 = S1	0.067 (0.098)	0.011 (0.117)	0.082 (0.163)
S4 = S3	-0.025 (0.074)	-0.062 (0.097)	0.060 (0.081)
S6 = S5	0.068 (0.075)	0.021 (0.098)	0.026 (0.118)

- Results:
  - Theory confirmed for Random Audit & Tournament
  - Theory NOT confirmed for Standards

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Hypothesis 3 – transition effect (20% v. 40%)

Comparison	Pooled	G_1	G_2
T5 = T3	0.236** (0.056)	0.167** (0.064)	0.306** (0.075)
T6 = T4	0.190** (0.060)	0.133 (0.098)	0.248** (0.073)
S5 = S3	-0.055 (0.071)	-0.129 (0.095)	0.062 (0.086)
S6 = S4	0.039 (0.079)	-0.046 (0.100)	0.030 (0.114)

- Results:
  - Theory confirmed for Dynamic Tournament
  - Theory NOT confirmed for Dynamic Standards

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Hypothesis 4 – group effect ($z_2 = z_1$)

Treatment	Group effect	Treatment	Group effect
R1	0.286** (0.104)	T5	0.417** (0.060)
R2	0.287* (0.091)	T6	0.364** (0.069)
R3	0.308** (0.063)	S1	0.149 (0.118)
R4	0.187** (0.080)	S2	0.206 (0.138)
T1	0.330** (0.090)	S3	0.081 (0.091)
T2	0.350** (0.094)	S4	0.202** (0.050)
T3	0.279** (0.056)	S5	0.273** (0.065)
T4	0.249** (0.093)	S6	0.278** (0.125)

- Results:
  - Theory confirmed for Random Audit & Tournament
  - Theory somewhat confirmed for Standards

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Hypothesis 5 – Leverage effect

Comparison	Pooled	G_1	G_2
T1 = R1	0.255** (0.095)	0.233** (0.125)	0.278** (0.104)
T2 = R2	0.479** (0.095)	0.448** (0.114)	0.511** (0.124)
T3 = R3	0.165** (0.068)	0.180** (0.085)	0.150** (0.063)
T4 = R4	0.385** (0.074)	0.354** (0.120)	0.416** (0.072)
T5 = R3	0.401** (0.069)	0.347** (0.080)	0.456** (0.070)
T6 = R4	0.575** (0.073)	0.487** (0.108)	0.664** (0.074)
S1 = R1	0.428** (0.095)	0.475** (0.139)	0.338** (0.100)
S2 = R2	0.618** (0.096)	0.668** (0.106)	0.587** (0.152)
S3 = R3	0.442** (0.085)	0.567** (0.109)	0.340** (0.080)
S4 = R4	0.428** (0.071)	0.455** (0.101)	0.471** (0.066)
S5 = R3	0.387** (0.062)	0.438** (0.080)	0.403** (0.065)
S6 = R4	0.466** (0.092)	0.409** (0.127)	0.500** (0.118)

- Result:
  - Theory confirmed for Tournament & Standards

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Hypothesis 6 – mechanism equivalence

Comparison	Pooled	G_1	G_2
T1 = S1	-0.088 (0.093)	-0.162 (0.118)	0.030 (0.126)
T2 = S2	-0.083 (0.097)	-0.160 (0.115)	-0.026 (0.153)
T3 = S3	-0.287** (0.075)	-0.397** (0.099)	-0.200** (0.085)
T4 = S4	-0.063 (0.056)	-0.121 (0.091)	-0.075 (0.066)
T5 = T5	0.019 (0.050)	-0.082 (0.057)	0.053 (0.077)
T6 = S6	0.069 (0.081)	0.037 (0.107)	0.123 (0.118)

- Results:
  - Theory confirmed

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Conclusions

- Theoretical innovations
  - Extend dynamic targeting model
  - Introduce dynamic tournament
- Experimental findings
  - Tournament: comparative statics confirmed
  - Standards: achieves significant leverage but largely invariant to design.
  - Robustness
  - Noisy decision-making

Appendix -- screenshots

Period

1 of 5

Time Remaining [sec]

DECISION SCREEN

Your actual output is **20**.

Your task is to choose how much output to report.

You can choose to report any amount between, and including, 0 and 40.

Your reporting cost is equal to your reported output multiplied by **$2**.

Information about Inspections

This period you are in **Group B**.

You have a **60.00 %** chance of being randomly selected for inspection.

If you are inspected, you pay an inspection cost of **$25**.

If you are inspected, and if you are estimated to have *under-reported*, you pay a penalty equal to the amount you are estimated to have under-reported multiplied by **$2**.

Choose reported output

Time Remaining [sec]

RESULTS SCREEN

Your reported output was: **35**.

You were inspected, so you do pay an inspection cost.

Your estimated output was: **39.04**.

The difference between estimated and reported output is: **4.04**.

Your earnings for this period are:

$55 (Initial earnings)

- **35** x **$2** (Reporting cost)
- **$25** (Inspection cost)
- **4.04** x **$2.00** (Penalty)

= **$-48.08**

OK

Period

1 of 5

Time Remaining [sec]

The current game will continue in the next period.

You were not inspected.

Therefore you will remain in **Group B** prior to the upcoming period, and your chance of being inspected will remain at **60.00%**.

OK

Time Remaining [sec]

Your ID is: **3**

Player ID	Inspected	Reported	Group This Period	Group Next Period (if game has not ended)
1	N	11	A	A
8	N	12	A	A
10	N	20	A	A
6	N	12	A	A
9	Y	15	A	B
5	Y	5	B	B
4	Y	15	B	B
7	N	5	B	B
3	N	14	B	B
2	N	12	B	B

Proceed

ERB INSTITUTE

FOR GLOBAL SUSTAINABLE ENTERPRISE

UNIVERSITY OF MICHIGAN

Strategic Environmental Disclosure: Evidence from the DOE's Voluntary Greenhouse Gas Registry

Eun-Hee Kim

Thomas P. Lyon

Strategic Disclosure

- Growing interest in information disclosure as policy tool
- Most research has focused on mandatory disclosure programs. Generally found to have modest but significant impacts on reducing emissions, especially among relatively inefficient firms.
- Less is known about voluntary disclosure programs.
  - Most research has been in the accounting literature using content analysis of Annual Reports and 10ks
- Research questions:
  - Who participates? Clean firms or dirty firms?
  - Why do they participate? Market opportunities? Political pressure? Building legitimacy after a disaster?
  - Does participation improve performance?

The 1605b program

- The Department of Energy (DOE) keeps a registry of GHG reductions filed voluntarily by firms, as mandated by section 1605b of the Energy Policy Act of 1992.
- Original program design allowed firms great flexibility in reporting:
  - "Entity level" or "project level". (OK to report entity-level reductions just as the sum of project-level reductions.)
  - Baseline can be historical or hypothetical.
  - Report reductions in emissions OR emissions intensity.
  - Can report indirect reductions or sequestration as well as direct reductions.
- Why should firms participate?
  - DOE: "The voluntary reporting program provides an opportunity for you to gain recognition for the good effects of your actions--recognition from your customers, your shareholders, public officials, and the Federal government."
  - Possibility of early reduction credits (ERCs)

Our Strategy

- We develop a model based on the idea that firms disclose to 1605b in the hopes of obtaining early reduction credits (ERCs). Extends Kennedy (2002) to include
  - Uncertainty over the form of ERCs
  - Political pressures
  - NGO monitoring of greenwash
- We focus on the electric utility sector so that we can compare
  - Voluntary 1605b disclosures
  - Mandatory FERC disclosures on generation by fuel
- Thus, we can compare a firm's true change in GHGs over time against what it files with 1605b
- Data: A pooled database of 98 investor-owned electric utilities over the period of 1995-2003. Data come from combining Platt's data with data from the 1605b website, EPA, LCV, DSIRE.

Variables

B_t = Business-as-usual emissions in period t
E_t = Actual emissions in period t
δ = Growth in business-as-usual emissions each period
D_t = Demand-side reductions in period t
S_t = Supply-side reductions in period t
$K_t(S_t)$ = Cost of supply-side reductions in period t
$L_t(D_t)$ = Cost of demand-side reductions in period t

ρ = Probability of an emissions price in period 2
P = Emissions price in period 2 if one is imposed
α = Probability early reduction credits are granted if an emissions price is imposed
$\sigma(S_1)$ = Probability of state-level renewable energy legislation
S_{Reg} = Supply-side emissions reduction required under state renewable energy legislation
λ = Penalty imposed per unit by which the firm's supply-side reductions fall short of the regulatory requirement
η = Probability of NGO audit and penalty if the firm engages in selective disclosure
A = Level of harm imposed on the firm if the NGO successfully audits and attacks the firm for greenwash
R = Early Reduction Credits granted for reductions prior to imposition of a cap

How will ERCs Be Allocated?

1. Project-Based: $R^P = S_1 + D_1 + S_2 + D_2$
2. Reductions against Historical Baseline: This can be implemented in two ways, either against a baseline in period 1, in which case the credit total is $R_1^H = E_1 - E_2$, or against a baseline in period 0, in which case the credit total is $R_0^H = E_0 - E_2$. Under the 1605b program, firms had a choice of which baseline to use.

γ= Probability ERCs are allocated based on historical emissions
β=Probability ERCs are allocated based on projects

Costs under Full Disclosure

If historical reductions are measured as E0 – E2,

$$C = K_1(S_1) + K_2(S_2) + L(D_1) + L(D_2)$$
$$+ \rho P\left[\left((1+\delta)^2 B_0 - D_2 - S_1 - S_2\right) - \beta(S_1 + D_1 + S_2 + D_2)\right.$$
$$\left. - \gamma\left(-\delta(2+\delta)B_0 + D_2 + S_1 + S_2\right)\right] + \lambda\sigma(S_1)\max\{S_{Reg} - S_2, 0\}$$

If historical reductions are measured as E1 – E2,

$$C = K_1(S_1) + K_2(S_2) + L(D_1) + L(D_2)$$
$$+ \rho P\left[\left((1+\delta)^2 B_0 - D_2 - S_1 - S_2\right) - \beta(S_1 + D_1 + S_2 + D_2)\right.$$
$$\left. - \gamma(-\delta(1+\delta)B_0 - D_1 + D_2 + S_2)\right] + \lambda\sigma(S_1)\max\{S_{Reg} - S_2, 0\}$$

Costs under Selective Disclosure

If historical reductions are measured as E0 – E2,

$$C = K_1(S_1) + K_2(S_2) + L(D_1) + L(D_2)$$
$$+ \rho P\left[\left((1+\delta)^2 B_0 - D_2 - S_1 - S_2\right) - \beta(S_1 + \max\{D_1, 0\} + S_2 + D_2)\right.$$
$$\left. - \gamma\left(-\delta(2+\delta)B_0 + D_2 + S_1 + S_2\right)\right] + \lambda\sigma(S_1)\max\{S_{Reg} - S_2, 0\} + \eta A$$

If historical reductions are measured as E1 – E2,

$$C = K_1(S_1) + K_2(S_2) + L_1(D_1) + L_2(D_2)$$
$$+ \rho P\left[\left((1+\delta)^2 B_0 - D_2 - S_1 - S_2\right) - \beta(S_1 + max\{D_1, 0\} + S_2 + D_2)\right.$$
$$\left. - \gamma(-\delta(1+\delta)B_0 - D_1 + D_2 + S_2)\right] + \lambda\sigma(S_1)\max\{S_{Reg} - S_2, 0\}\} + \eta A$$

Incentives for Selective Disclosure

Key Tradeoff: Selective disclosure allows the firm to claim more ERCs, but subjects it to the risk of a penalty from NGOs opposed to greenwash.

***Proposition 1**: Growing firms are more likely to disclose selectively. The firm has incentives to engage in selective disclosure when it chooses $D_1 < 0$, which is in turn more likely when the firm is growing faster, i.e. when $\delta B_0 > D_1 + S_1$ since in this case it has strong incentives to uses a historical baseline of period 1 instead of period 0. The firm's incentives to disclose selectively are increasing in γ, the probability that early reduction credits (ERCs) will be allocated based on historical emissions reductions, and decreasing in β, the probability that ERCs will be allocated on the basis of emissions reduction projects disclosed.*

Abatement Incentives

***Proposition 2**: The possibility of earning ERCs induces a firm to increase S_1, S_2, and D_2. It has an ambiguous effect on D_1; if the firm chooses a historical baseline of period 0, or if $\beta - \gamma > 0$ then the firm increases its investment in D_1, but if the firm chooses a historical baseline of period 1 and $\beta - \gamma < 0$ then the firm decreases its investment in D_1.*

***Proposition 3**: If $|L_i(D_i)|$ is symmetric about 0,then the prospect of earning ERCs unambiguously increases the firm's incentives for abatement, regardless of uncertainty on the allocation rule. Hence, all other things equal, firms that participate in the 1605b program are expected to reduce their carbon emissions more than those that do not participate.*

Incentives to Join 1605b

***Proposition 4**: Firms with lower marginal costs of abatement have greater incentive to participate in the 1605b program.*

***Proposition 5**: A firm has greater incentive to participate in the 1605b program when it faces a greater threat of state regulation.*

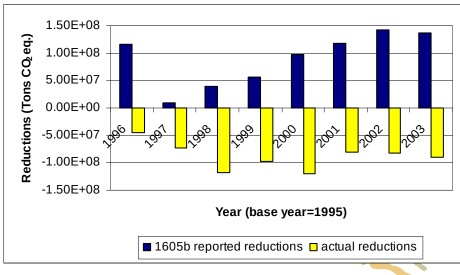
***Proposition 6**: A firm has less incentive to disclose selectively as ηA increases.*

How Do Firms Report?

Table III: Number of Firms Reporting at the Entity and Project Levels*

Reporting Mode	Year	1995	1996	1997	1998	1999	2000	2001	2002	2003
One Category Only	Project Only[1]	58	56	57	57	58	60	59	60	82
	Entity Only	6	6	5	5	5	5	5	4	4
	Sub-total	64	62	62	62	63	65	64	64	86
Two Categories	Entity ≠ Project[2]	8	10	10	10	10	10	10	10	10

Reported vs. Actual reductions (IOUs)



Participants vs. non-participants (IOUs)

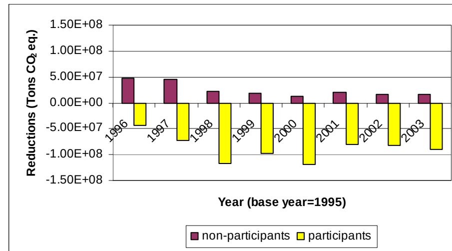
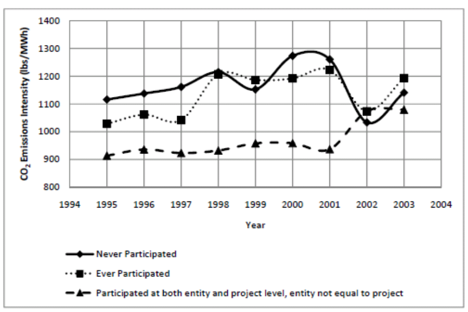


Figure 3. CO_2 Emissions Intensity over Time



Empirical Models

- Probit for discrete choice analysis (whether or not to participate in the 1605b program). Done cross-sectionally because relatively few firms (only 25) change status over time (22 of them were in 2003).
  - Explanatory variables
    - Cost/benefit: Heat rate, Capacity factor, Fraction of hydro & nuclear, Fuel switch saving, Growth in net generation, Revenue
    - External pressure: CO_2, LCV scores, RPS index
    - Greenwash: Sierra
- Propensity score matching to evaluate program effectiveness
  - 2nd stage outcome variable: CO_2 emissions intensity (CO_2 emissions per net generation)

Data

- Sample: 98 investor-owned utilities over the period 1995-2003
- Basic financial, operating and environmental data purchased from Platts
- Other data from EPA, LCV, DSIRE
- GHG emissions calculated based on fuel emissions

Descriptive Statistics

Variables	Entire Sample N=98		1605(b) participants N=45		1605(b) non-participants N=53	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	Year 2003					
CO_2 Emissions	16.88471	18.07051	26.38348	20.48595	8.116603	9.218318
CO_2 Emissions intensity	947.2028	480.0793	1016.555	456.2868	883.1851	498.2953
Sierra Subscription per thousand population	0.780096	0.531047	0.668315	0.276731	0.875005	0.664278
Electric operating revenue	1.27218	1.617865	2.132534	2.013254	0.541691	0.518223
Heatrate	8490.768	3770.109	9174.969	3011.638	7909.842	4253.28
Capacity factor	0.485825	0.178964	0.499636	0.158134	0.4741	0.195654
Fraction of hydro & nuke	0.245194	0.391585	0.238294	0.363687	0.251165	0.417667
LCV score: Senate	38.23469	33.09991	36.46667	30.23288	39.73585	35.57125
LCV score: House	39.29592	20.38911	40.22222	16.42599	38.50943	23.36222
Renewable Portfolio Standard	0.175706	0.391249	0.174713	0.427253	0.176549	0.362053
Growth in net generation	0.148871	0.477374	0.141705	0.292654	0.154946	0.594122
Fuel switch saving	0.020704	0.122529	0.015363	0.028031	0.02524	0.165212
Enforcement	0.078058	0.072319	0.096209	0.083344	0.062647	0.057891

Table IV. Participation in the 1605(b) program: Marginal Effects

VARIABLES	Probit Marginal Effects (dependant variable: binary dummy that indicates participation)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CO_2 Emissions	0.00437	0.00286			0.00453	0.00311		
	(0.00755)	(0.00762)			(0.00749)	(0.00747)		
CO_2 Emissions intensity			0.000161	8.70E-05			0.000113	3.76E-05
			(0.000108)	(0.000108)			(9.45e-05)	(9.40e-05)
Sierra Subscription per thousand population	-0.525*		-0.522*		-0.530*		-0.517*	
	(0.292)		(0.286)		(0.289)		(0.289)	
Electric operating revenue	0.397***	0.391***	0.471***	0.435***	0.416***	0.410***	0.486***	0.456***
	(0.130)	(0.127)	(0.111)	(0.103)	(0.124)	(0.121)	(0.109)	(0.100)
Heatrate	9.92E-06	3.83E-05	-4.76E-06	2.92E-05	-4.80E-06	1.21E-05	-1.61E-05	9.82E-06
	(4.25e-05)	(4.42e-05)	(4.52e-05)	(4.63e-05)	(1.99e-05)	(2.19e-05)	(2.27e-05)	(2.47e-05)
Capacity factor	-0.183	-0.279	0.0959	-0.122	-0.224	-0.345	-0.101	-0.277
	(0.429)	(0.450)	(0.449)	(0.477)	(0.421)	(0.441)	(0.428)	(0.453)
Fraction of hydro & nuke	0.152	0.258	0.175	0.247				
	(0.423)	(0.414)	(0.448)	(0.420)				
LCV score: Senate	-0.00128	-0.00353	0.000288	-0.0027	-0.00072	-0.00272	0.000341	-0.00256
	(0.00364)	(0.00361)	(0.00380)	(0.00377)	(0.00355)	(0.00353)	(0.00371)	(0.00367)
LCV score: House	0.00754	0.00504	0.0044	0.00333	0.00695	0.00434	0.00451	0.00371
	(0.00596)	(0.00575)	(0.00627)	(0.00617)	(0.00593)	(0.00571)	(0.00625)	(0.00614)
Renewable Portfolio Standard	-0.464**	-0.464*	-0.515**	-0.477**	-0.475**	-0.462*	-0.502**	-0.461*
	(0.236)	(0.244)	(0.240)	(0.243)	(0.234)	(0.241)	(0.237)	(0.239)
Growth in net generation	-0.0876	-0.0799	-0.114	-0.0903	-0.109	-0.103	-0.129	-0.105
	(0.121)	(0.114)	(0.125)	(0.115)	(0.113)	(0.109)	(0.119)	(0.111)
Fuel switch saving	-0.537	-0.513	-0.6	-0.552	-0.7	-0.666	-0.668	-0.61
	(2.745)	(2.137)	(2.533)	(2.088)	(4.408)	(3.188)	(3.059)	(2.451)
Enforcement		3.140***		3.080***		2.904***		2.839**
		(1.148)		(1.180)		(1.112)		(1.134)

Key Findings: Probit

- Firms that participate have significantly
  - Higher revenues
  - Less NGO pressure
  - No renewable portfolio standard
  - Greater enforcement pressure
- They also tend to have
  - Higher emissions
  - Higher emissions intensity

Why Didn't Firms Whose Emissions Declined File with 1605b?

- Empirical results suggest:
  - They are smaller (30% the revenue of participants), so total amount of ERCs may not have justified the cost.
  - They are already less GHG-intensive than participants and have fewer opportunities to reduce.
  - They face more potential backlash from environmentalists

Propensity Score Matching

$$\text{Outcome variable} = \frac{(CO_2 \text{ emissions intensity in 1995} - CO_2 \text{ emissions intensity in 2003})}{CO_2 \text{ emissions intensity in 1995}}$$

Firms are matched based on propensity to join 1605b, as determined by observables. Conditional independence implies

$$E[Y_0 | D=1, P(\mathbf{X})] = E[Y_0 | D=0, P(\mathbf{X})]$$

If variables like managerial preferences influence both propensity to join and reductions over time, the technique is biased towards finding an effect.

We impose the common support condition, so matching is only done over the propensity region that is common to both joiners and non-joiners.

After calculating propensity scores, we perform the balance test to ensure there is no significant difference in either propensities or covariates across the two groups.

Table V. Impact of the 1605(b) program

Matching	Average treatment effect on the treated (participation in 1605(b) program)			
	Kernel	Caliper/Radius (0.10)	Stratification/ Interval	Nearest Neighbor
(1)[†]	0.121	0.152	0.193	-0.137
	(0.180)	(0.197)	(0.220/0.246)	(0.286/0.256)
Number of Treatment	39	26	24	39
Number of Controls	23	28	38	14
(2)[†]	0.311	0.365	0.201	-0.046
	(0.355)	(0.231)	(0.228)	(0.516/0.510)
Number of Treatment	39	24	20	39
Number of Controls	23	28	42	14
(3)[†]	0.158	0.333[‡]	0.322	0.348
	(0.215)	(0.236)	(0.294)	(0.538/0.290)
Number of Treatment	39	36	24	39
Number of Controls	27	29	42	12
(4)[†]	0.182	0.381[‡]	0.256	0.059
	(0.341)	(0.214)	(0.227)	(0.285/0.452)
Number of Treatment	39	31	25	39
Number of Controls	24	28	38	15
(5)[†]	0.127	0.219	0.174	-0.042
	(0.172)	(0.202)	(0.211/0.228)	(0.466/0.252)
Number of Treatment	39	26	24	39
Number of Controls	24	27	39	13
(6)[†]	0.186	0.192[§]	0.225	0.009
	(0.321)	(0.163)	(0.209/0.227)	(0.140/0.425)
Number of Treatment	39	28	23	39
Number of Controls	21	26	37	14
(7)[†]	0.184	0.234[§]	0.332	0.128
	(0.187)	(0.205)	(0.303)	(0.443/0.212)
Number of Treatment	39	39	23	39
Number of Controls	27	26	39	15
(8)[†]	0.300	0.266[‡]	0.322	0.247
	(0.388)	(0.209)	(0.247/0.273)	(0.493/0.430)
Number of Treatment	39	27	26	39
Number of Controls	24	27	37	16

* Standard errors in parenthesis. When there are two standard errors, the second one is a bootstrapped standard error. When there is only one, it is bootstrapped (the raw standard errors are not available) except for Caliper/Radius matching, where it is a raw standard error.

[†] (1)–(8) refer to models displayed in the probit table (Table V).

[‡]Caliper:0.15, [§]Caliper:0.2, [¶]Caliper:0.25, and [‖]Caliper:0.35.

Impact of participation is usually positive, but never significant.

1605(b) Revisions

- In 2006, DOE revised the program to require firms to participate at the "entity" level, thereby preventing "project level" disclosure and the possibility of greenwash.
- NRDC:
  - "Without full and transparent entity-wide emissions accounting, project-based reporting weakens the system and undermines the value of real reductions by providing opportunities for gaming the system and claiming hypothetical reductions while emissions are actually increasing..."
- Edison Electric Institute:
  - "If the purpose is to obtain transferable credits...the reporting under the revised guidelines may need to be more rigorous in the criteria to be applied..." Even then, however, it maintains that (p. 7) "these criteria should not, and need not, be dependent on entity-wide reporting."

1605(b) Revisions

- DOE
  - "...Because most large companies and institutions regularly take actions that have as one of their effects the reduction of greenhouse gas emissions, there are always many candidates for project-based emission reductions. But the net effect of such project-based reductions on an entity's total emissions is often questioned, because large entities may be taking actions that reduce emissions, while simultaneously taking other actions that increase emissions. Furthermore, it is impossible to evaluate the significance of a particular entity's actions to reduce emissions unless the total emissions of that entity are known."

Policy Implications

- Information disclosure programs need to account for firms' tendency to engage in selective disclosure
- Consider mandating disclosure of all relevant information, especially negative impacts
- Disclosure is a step in the right direction, but is likely not enough by itself to accomplish large impacts.

Conclusions

- Disclosure tends to be highly selective
  - Participants disclose successful projects but seldom disclose whole carbon footprint
- Disclosure is NOT simply an economic issue---political pressures matter, too. 1605b participants tend to be in states
  - Without RPSs
  - Facing weak NGO pressure
- The format of a firm's 1605b participation can be viewed as a rhetorical device arguing for a particular way of allocating early reduction credits, i.e. project-based vs. entity-level.
- Participation could be called a form of greenwash
  - Fits with notion of greenwash as selective disclosure
  - Participation does not mean firms are clean or improved carbon intensity more than non-participants.
  - More NGO pressure deters firms from participating, consistent with the greenwash theory
- Revisions to 1605(b) make it more rigorous, and encourage entity-wide reporting.
  - This suggests that upon reflection, DOE concluded that selective disclosure was welfare-reducing

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Incentives under Full Disclosure

If baseline year is 0, FOCs are

$$\frac{\partial C}{\partial S_1} = \frac{\partial K_1}{\partial S_1} - \rho P(1+\beta+\gamma) + \lambda\sigma'(S_1)\max\{S_{Reg} - S_2, 0\} = 0$$

$$\frac{\partial C}{\partial D_1} = \frac{\partial L_1}{\partial D_1} - \rho P\beta = 0$$

If baseline year is 1, FOCs are

$$\frac{\partial C}{\partial S_1} = \frac{\partial K_1}{\partial S_1} - \rho P(1+\beta) + \lambda\sigma(S_1)\max\{S_{Reg} - S_2, 0\} = 0$$

$$\frac{\partial C}{\partial D_1} = \frac{\partial L_1}{\partial D_1} - \rho P(\beta-\gamma) = 0$$

Incentives under Full Disclosure

For S_2 and D_2, the baseline year is irrelevant. FOCs are

$$\frac{\partial C}{\partial S_2} = \frac{\partial K_2}{\partial S_2} - \rho P(1+\beta+\gamma) - \lambda\sigma(S_1)I[S_{Reg} > S_2] = 0$$

$$\frac{\partial C}{\partial D_2} = \frac{\partial L_2}{\partial D_2} - \rho P(1+\beta+\gamma) = 0$$

Incentives under Selective Disclosure

Only the incentives for D^1 are affected.

If baseline year is 0, the FOC is

$$\frac{\partial C}{\partial D_1} = \frac{\partial L_1}{\partial D_1} - \rho P(\beta I[D_1 > 0]) = 0$$

If baseline year is 1, the FOC is

$$\frac{\partial C}{\partial D_1} = \frac{\partial L_1}{\partial D_1} - \rho P(\beta I[D_1 > 0] - \gamma) = 0$$

Competing Environmental Labels

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Benefits of Environmental Information Disclosure

January 18, 2011

School of Natural Resources & Environment
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Introduction

- Globalization of trade and environmental issues create problems difficult for governments to address with standard policy tools
- Trade law makes it difficult for governments to regulate attributes of production processes outside their borders
- Many groups have put increasing effort into international market mechanisms such as ecolabeling

Labels Promulgated by a Non-Governmental Organization (NGO)

- Swedish Society for the Conservation of Nature
- Forest Stewardship Council (FSC)

Industry-led Labels

- Pulp and paper
- Tuna canning
- American Forest & Paper Association (AF&PA) Sustainable Forestry Initiative

Research Questions

- How do the incentives and behavior of industry groups and environmental NGOs compare in setting ecolabel standards?
- Is society made better off by multiple ecolabels in an industry, or do competing labels reduce overall effectiveness?
- Is there a role for government intervention in third-party voluntary labeling schemes?

Previous Literature

- Still quite sparse, but growing
- Heyes and Maxwell (2004) compare a mandatory standard adopted by a "World Environmental Organization" (WEO), subject to political pressures, with an NGO-led voluntary ecolabel
  - NGO label may reduce welfare by pre-empting the more socially desirable WEO label.
  - If the two labels coexist, then the NGO label is beneficial
- Baksi and Bose (2007) compare NGO labels with self-labeling by individual firms
  - Self-labels can be better if the government is willing to engage in costly monitoring

Our Analysis

- Formal model of rivalry between NGO and industry-sponsored labels
- Each chooses a standard of stringency
  - NGO wants to minimize damages
  - Industry wants to maximize profits

RESOURCES FOR THE FUTURE

Consumers

- Willingness-to-pay (WTP) is a continuous, increasing function of the standard stringency, s:

  $p(s)$ with $p'(s) > 0$ and $p''(s) < 0$.
- Assumptions
  - Standard is credible
  - Presence of one standard does not affect WTP for another standard

RESOURCES FOR THE FUTURE

Firms

- Each chooses abatement level s at cost θs

$$\pi(\theta, s) = p(s) - \theta s.$$

- Firms distributed across a spectrum of costs of complying with a standard $[\underline{\theta}, \overline{\theta}]$
  - Probability density function $f(\theta)$

RESOURCES FOR THE FUTURE

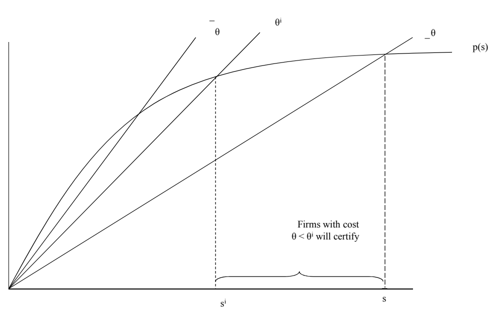
The Firm Decision

- Firms elect to join a labeling program if the net benefits outweigh the alternatives
- For a single label:
  - if the price premium outweighs the costs of meeting the standard
  - i.e., below a cutoff level of the cost parameter

$$\theta < \theta^i \equiv \frac{p(s^i) - p(0)}{s^i}.$$

RESOURCES FOR THE FUTURE

Basic Model Structure



RESOURCES FOR THE FUTURE

Objectives and Standards

- NGO maximizes environmental benefits

$$V(s^N) = s^N F(\theta^N)$$

$$p'(s^N) = \theta^N - \frac{F(\theta^N)}{f(\theta^N)}.$$

- Industry maximizes profits

$$\Pi(s^I) = \int_{\underline{\theta}}^{\theta^I} (p(s^I) - \theta s^I) f(\theta) d\theta + \int_{\theta^I}^{\overline{\theta}} p(0) f(\theta) d\theta.$$

$$p'(s^I) = \theta^I - \frac{\int_{\underline{\theta}}^{\theta^I} F(\theta) d\theta}{F(\theta^I)}.$$

RESOURCES FOR THE FUTURE

Result with Single Labels

- NGO sets a higher standard than industry.
  - Proof applies results from Bagnoli and Bergstrom (2004).
  - Assumes log-concavity
    - encompasses many standard distributions, including the uniform, normal, exponential and logistic distributions.

RESOURCES FOR THE FUTURE

The Firm Decision: Multiple Labels

- Firm chooses less stringent label:
  - if the price premium outweighs the costs of meeting the standard
  - i.e., below a cutoff level of the cost parameter

$$\theta_B^L \equiv \frac{p(s_B^L) - p(0)}{s_B^L}.$$

- Firm chooses more stringent of two labels:
  - if the price premium outweighs the costs of meeting the standard
  - *And* if the additional price premium outweighs the additional costs
  - i.e., *above* a cutoff level of preferring the looser standard

$$\theta_B^H \equiv \frac{p(s_B^H) - p(s_B^L)}{s_B^H - s_B^L}$$

RESOURCES FOR THE FUTURE

Response of Cutoff Costs to Changes in Standards

- Cutoff for higher standard declining in both standards
- Cutoff for weaker standard declining only in that standard.

$$\frac{d\theta_B^H}{ds_B^H} = \frac{p'(s_B^H) - \theta_B^H}{(s_B^H - s_B^L)} < 0; \qquad \frac{d\theta_B^L}{ds_B^L} = \frac{p'(s_B^L) - \theta_B^L}{s_B^L} < 0;$$

$$\frac{d\theta_B^H}{ds_B^L} = -\frac{p'(s_B^L) - \theta_B^H}{s_B^H - s_B^L} < 0. \qquad \frac{d\theta_B^L}{ds_B^H} = 0$$

RESOURCES FOR THE FUTURE

Objectives with Multiple Standards
(Assuming NGO's is Higher)

- NGO

$$V(s_B^N; s_B^I) = s_B^I F(\theta_B^I) + (s_B^N - s_B^I) F(\theta_B^N)$$

$$p'(s_B^N) = \theta_B^N - \frac{F(\theta_B^N)}{f(\theta_B^N)}$$

- Industry

$$\Pi(s_B^I; s_B^N) = \int_{\underline{\theta}}^{\theta_B^N} (p(s_B^N) - \theta s_B^N) f(\theta) d\theta + \int_{\theta_B^N}^{\theta_B^I} (p(s_B^I) - \theta s_B^I) f(\theta) d\theta + \int_{\theta_B^I}^{\overline{\theta}} p(0) f(\theta) d\theta$$

$$\frac{\partial \Pi}{\partial s_B^I} = \int_{\theta_N}^{\theta_A^I} (p'(s_A^I) - \theta) f(\theta) d\theta < \int_{\underline{\theta}}^{\theta_A^I} (p'(s_A^I) - \theta) f(\theta) d\theta = 0.$$

RESOURCES FOR THE FUTURE

Main Results for Industry

- Industry further relaxes its label if the two labels coexist.
  - The NGO standard draws off some firms, so industry loosens to increase participation.
  - Assuming NGO standard is higher
- Industry profits increase with multiple labels.
  - Firms only volunteer if it increases profits
  - Industry only changes its standard if it increases profits

RESOURCES FOR THE FUTURE

Main Results for NGO and Environment

- NGO may tighten or loosen its standards in response to an industry label
- Environmental damages may be higher or lower with both labels than with the NGO label alone.
- Specific results depend on the distribution of types of firms in the market and consumer demand for label stringency.

RESOURCES FOR THE FUTURE

Changes in Environmental Benefits

$$
\begin{aligned}
& V(s_B^N; s_B^I) - V(s_A^N; 0) \\
= & \int_{\underline{\theta}}^{\theta_B^N} s_B^N f(\theta) d\theta + \int_{\theta_B^N}^{\theta_B^I} s_B^I f(\theta) d\theta - \int_{\underline{\theta}}^{\theta_A^N} s_A^N f(\theta) d\theta \\
= & \underbrace{(s_B^N - s_A^N) F(\theta_B^N)}_{\substack{\text{Change in Stringency} \\ ?}} + \underbrace{(s_B^I - s_A^N)\left(F(\theta_A^N) - F(\theta_B^N)\right)}_{\substack{\text{Lost Participation to Industry} \\ -}} + \underbrace{s_B^I\left(F(\theta_B^I) - F(\theta_A^N)\right)}_{\substack{\text{Additional Participation} \\ +}}
\end{aligned}
$$

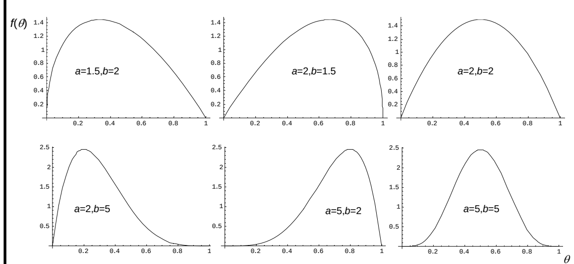
Simulations

- Explore role of firm-type distribution, consumer willingness-to-pay functions, and range of costs
  - Abatement cost parameter $\theta = (c + g\phi)s$
  - Beta distribution function

$$f(\phi; a, b) = \frac{\phi^{a-1}(1-\phi)^{b-1}}{\int_0^1 u^{a-1}(1-u)^{b-1} du} = \frac{\Gamma(a+b)}{\Gamma(a)\Gamma(b)} \phi^{a-1}(1-\phi)^{b-1}$$

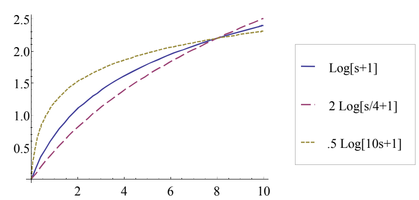
  - Log price premium function

Distribution Function Examples



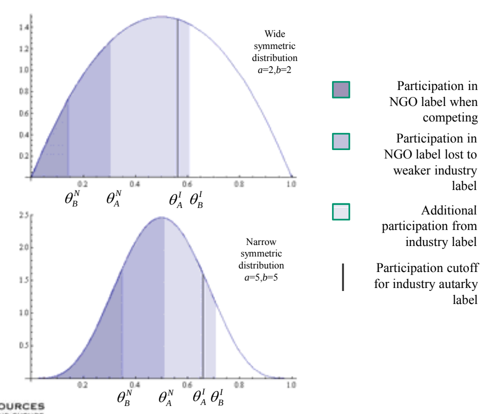
(Beta distribution function)

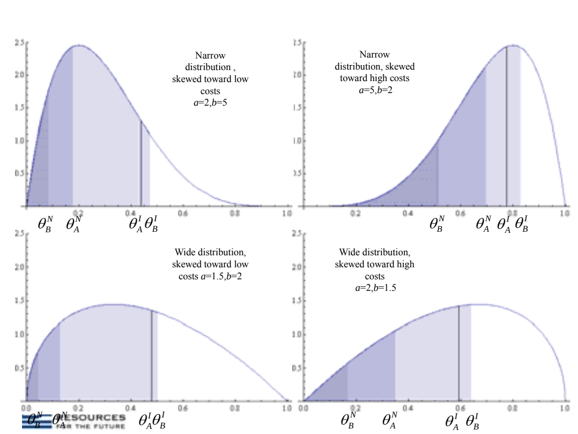
Price Premium Functions
"Regular", "Bowed", and "Flat"



Simulation Results

- Find both kinds of NGO and damages response
- NGO loses substantial participation when industry label present





Simulation Results

	Standards				Participation Rates				Environmental Benefits		
Distribution	sIA	sIB	sNA	sNB	%IA	%IB	%NA	%NB	VIA	VNA	VB
a=5,b=5	1.19	0.92	2.37	3.00	84%	74%	53%	18%	1.00	1.26	1.21
a=2,b=5	3.35	2.84	16.06	26.75	82%	78%	29%	8%	2.75	4.62	4.41
a=5,b=2	0.63	0.44	0.99	1.53	59%	60%	41%	13%	0.38	0.41	0.46
a=2,b=2	1.88	1.52	6.68	13.53	59%	60%	22%	6%	1.12	1.49	1.69
a=1.5,b=2	2.77	2.49	25.41	66.31	59%	60%	11%	2%	1.64	2.71	3.10
a=2,b=1.5	1.64	1.30	5.26	11.36	51%	53%	20%	5%	0.83	1.05	1.25

- "Regular" price premium
- [0,1] cost range

RESOURCES FOR THE FUTURE

Green = Environment benefits from competing labels

Yellow = NGO loosens standard in competition

			Standards				Participation Rates				Environmental Benefits		
Price Premium	Cost Range	Distribution	sIA	sIB	sNA	sNB	%IA	%IB	%NA	%NB	VIA	VNA	VB
Regular	.1 to 1.1	a=5,b=5	0.84	0.63	1.52	1.83	79%	68%	52%	19%	0.66	0.79	0.77
Regular	.1 to 1.1	a=2,b=5	2.00	1.54	5.44	5.59	84%	71%	45%	19%	1.67	2.44	2.15
Regular	.1 to 1.1	a=5,b=2	0.50	0.33	0.72	1.09	45%	46%	33%	10%	0.22	0.23	0.27
Regular	.1 to 1.1	a=2,b=2	1.30	0.94	3.07	4.19	56%	55%	29%	11%	0.73	0.90	0.96
Regular	.1 to 1.1	a=1.5,b=2	1.73	1.27	5.20	6.18	59%	56%	27%	11%	1.03	1.39	1.42
Regular	.1 to 1.1	a=2,b=1.5	1.17	0.83	2.63	3.85	47%	47%	25%	9%	0.55	0.64	0.72
Regular	.1 to .9	a=5,b=5	1.07	0.86	2.05	2.22	93%	74%	63%	23%	0.55	0.64	0.72
Regular	.1 to .9	a=2,b=5	2.30	1.82	6.27	5.97	91%	73%	51%	22%	0.99	1.30	1.14
Regular	.1 to .9	a=5,b=2	0.58	0.43	0.91	1.26	80%	73%	57%	18%	2.09	3.22	2.64
Regular	.1 to .9	a=2,b=2	1.41	1.04	3.40	4.41	73%	68%	38%	14%	0.47	0.52	0.54
Regular	.1 to .9	a=1.5,b=2	1.85	1.37	5.56	6.36	72%	67%	33%	14%	1.02	1.29	1.33
Regular	.1 to .9	a=2,b=1.5	1.23	0.89	2.80	3.99	65%	64%	34%	12%	1.34	1.86	1.83
Flat	0 to 1	a=5,b=5	2.66	1.81	4.21	6.70	23%	24%	16%	5%	0.80	0.96	1.05
Flat	0 to 1	a=2,b=5	8.74	7.09	36.01	67.29	50%	50%	17%	5%	0.62	0.66	0.77
Flat	0 to 1	a=5,b=2	2.01	1.31	2.73	4.77	4%	5%	3%	1%	4.38	6.23	6.84
Flat	0 to 1	a=2,b=2	6.33	4.95	20.18	44.04	22%	23%	8%	2%	0.09	0.09	0.11
Flat	0 to 1	a=1.5,b=2	9.46	8.42	81.18	221.74	27%	28%	5%	1%	1.37	1.70	2.05
Flat	0 to 1	a=2,b=1.5	5.95	4.62	17.87	40.40	16%	17%	6%	2%	2.60	4.01	4.82
Bowed	0 to 1	a=5,b=5	0.90	0.80	2.57	1.84	100%	79%	81%	21%	0.93	1.13	1.39
Bowed	0 to 1	a=2,b=5	1.65	1.50	9.13	12.37	100%	90%	46%	10%	0.90	2.08	1.01
Bowed	0 to 1	a=5,b=2	0.60	0.60	1.48	1.48	100%	92%	94%	8%	1.65	4.21	2.63
Bowed	0 to 1	a=2,b=2	0.90	0.83	3.03	5.61	100%	91%	60%	9%	0.60	1.40	0.67
Bowed	0 to 1	a=1.5,b=2	1.07	1.02	7.63	23.19	100%	96%	32%	4%	0.90	1.82	1.28
Bowed	0 to 1	a=2,b=1.5	0.78	0.72	2.07	4.48	100%	91%	72%	9%	1.07	2.40	1.97
Bowed	.1 to 1.1	a=5,b=5	0.73	0.73	2.01	15.32	100%	100%	84%	0%	0.78	1.50	1.06
Bowed	.1 to 1.1	a=2,b=5	1.20	1.03	4.46	3.13	100%	78%	64%	22%	0.73	1.69	0.73
Bowed	.1 to 1.1	a=5,b=2	0.51	0.51	1.25	17.20	100%	100%	95%	0%	1.20	2.85	1.50
Bowed	.1 to 1.1	a=2,b=2	0.73	0.65	2.08	2.24	100%	83%	71%	17%	0.51	1.19	0.51
Bowed	.1 to 1.1	a=1.5,b=2	0.85	0.74	3.18	3.27	100%	84%	55%	16%	0.73	1.47	0.91
Bowed	.1 to 1.1	a=2,b=1.5	0.64	0.58	1.56	1.99	100%	85%	80%	15%	0.85	1.75	1.16
Flat	.1 to 1.1	a=5,b=5	1.67	1.11	2.34	3.46	12%	13%	9%	3%	0.64	1.25	0.79
Flat	.1 to 1.1	a=2,b=5	4.13	2.95	8.71	10.18	45%	41%	26%	11%	0.20	0.21	0.25
Flat	.1 to 1.1	a=5,b=2	1.38	0.89	1.78	2.82	2%	2%	1%	0%	1.86	2.27	2.33
Flat	.1 to 1.1	a=2,b=2	3.42	2.36	6.50	8.83	17%	17%	10%	4%	0.02	0.02	0.03
Flat	.1 to 1.1	a=1.5,b=2	4.42	3.10	9.97	11.91	25%	24%	13%	6%	0.58	0.66	0.73
Flat	.1 to 1.1	a=2,b=1.5	3.30	2.26	6.13	8.57	12%	12%	7%	3%	1.09	1.33	1.41
Flat	.1 to .6	a=5,b=5	2.42	1.77	4.06	4.88	69%	61%	48%	18%	0.39	0.44	0.50
Flat	.1 to .6	a=2,b=5	5.33	4.06	12.00	11.71	82%	66%	49%	22%	1.68	1.93	1.95
Flat	.1 to .6	a=5,b=2	1.56	1.04	2.15	3.20	31%	32%	23%	8%	4.36	5.93	5.27
Bowed	.4 to 2.4	a=2,b=2	0.26	0.23	0.56	0.43	100%	75%	81%	25%	0.48	0.50	0.58
Bowed	.4 to 2.4	a=1.5,b=2	0.42	0.35	1.17	0.87	100%	73%	67%	27%	0.26	0.46	0.28
Bowed	.4 to 2.4	a=2,b=1.5	0.17	0.16	0.32	0.29	100%	79%	92%	21%	0.42	0.78	0.49
Regular	.1 to .6	a=5,b=5	1.86	1.61	3.82	3.26	100%	75%	78%	25%	0.17	0.30	0.18
Regular	.1 to .6	a=2,b=5	3.16	2.64	8.54	6.76	99%	73%	64%	26%	1.86	2.98	2.03
Regular	.1 to .6	a=5,b=2	1.19	1.04	2.02	2.12	100%	79%	87%	21%	3.13	5.45	3.73
Regular	.4 to .9	a=5,b=5	0.55	0.46	0.94	0.86	98%	70%	76%	29%	1.19	1.77	1.27
Regular	.4 to .9	a=2,b=5	0.86	0.72	1.67	1.33	98%	65%	73%	34%	0.54	0.71	0.57
Regular	.4 to .9	a=5,b=2	0.34	0.26	0.50	0.60	93%	75%	71%	24%	0.84	1.22	0.92

RESOURCES FOR THE FUTURE

Lessons: Responses

- In all cases, the NGO loses market share under competition.
- In most cases, the industry loses market share under competition
  - but not always.
- In all cases, the industry responds to competition by loosening its standard.
- In most cases, the NGO responds to competition by raising its standard
  - but not always.

RESOURCES FOR THE FUTURE

Lessons: Benefits under Competition

- When the NGO does respond by loosening its standard, environmental benefits are always lower
- When the industry expands its label participation rate, environmental benefits are almost always higher.
- Dueling labels more likely to be beneficial to the environment when
  - price premium is flatter
  - when costs are skewed toward the high end, and the distribution is relatively wide.
    - Else competing within a tight range of firms

RESOURCES FOR THE FUTURE

Thinking About Welfare

- Societal objective function would likely balance profits and environmental damages (and consumer surplus)
- Profits and consumer options increase with more labels, but environmental benefits may decrease
- Role for influencing the number of labels and their criteria
- Incentives for NGOs to work *with* industry groups to avoid excess competition

RESOURCES FOR THE FUTURE

Caveats and Extensions

- Consumer willingness to pay for one label may depend on the qualities of the other labels
  - additional interactions between competing labeling schemes
- We assume standards set targets for reductions in damages; absolute standards may create twin distributions of firms by costs and emissions

Further Research

- Use discrete firm types
  - Explore reverse equilibria (i.e., industry standard could be higher than the NGO's in competition)
  - Explore when the NGO wants fewer labels
    - Industry always wants as many labels as firms

Thanks!

- To EPA-STAR
  - RD-83285101
- To Mistra Foundation
  - ENTWINED Program
- For more information:
  - Resources for the Future www.rff.org
  - Erb Institute for Global Sustainable Enterprise http://www.erb.umich.edu/

FINAL V3: 01/18/11 Benefits of Environmental Disclosure Panel Discussion

Good afternoon. I'm Jon Silberman. I'm an attorney with EPA's Office of Enforcement and Compliance Assurance. This is my 28th year with EPA. My present focus is on quantitative compliance and deterrence measurement.

An opening disclaimer: my comments are my own. They're based on my professional experience, but do not necessarily represent formal Agency positions. Also, because I'm not an economist or statistician, with Will Wheeler's concurrence, my observations will be policy focused – no math!

An occupational hazard of being a lawyer is that people can't resist telling you their favorite lawyer jokes. This being a conference of economists, I thought I'd begin with my favorite economist joke. Here's the joke:

Q. An economist was trapped in a solid steel cage. How did he get out?

A. He assumed the existence of a door, opened the door, and left.

Now, I didn't begin with this joke merely for a laugh. Most of my work is with data or field anecdotes but I do, now and then, use models. I understand that, by design, models simplify reality. A key way they do this is through reasonable assumptions. Indeed, I have to reinforce to my own colleagues that our own deterrence models <u>also</u> don't capture every possible intervention, confounder, and outcome. They can't. When it come to modeling, "If it could perfectly capture reality, it would BE reality!"

However, for a model to be of practical use to those who issue the rules, write the policies, do the inspections, or take the enforcement actions, it does need to accurately capture ENOUGH of the underlying reality to be reliable. Basing key assumptions on preexisting empirical data, or calibrating with hard data after-the fact, are two ways in which models can be made more persuasive. All three of the models presented in this session would benefit from one or both of these approaches.

Improving the environment through information disclosure is a hot topic today. I think it's fair to say that, today, EPA is less emphasizing developing new voluntary programs than seeking to improve our rules, permits, and enforcement. We continue, though, to support an array of important existing voluntary programs. And labeling and information disclosure are important across-the-board.

EPA's current approach to regulatory compliance and enforcement is rooted largely in the traditional inspection and enforcement model. This model has shown substantial environmental and human health benefits but will not be able to keep up with expanding universes of regulated sources. Put bluntly, there are too darn many of them, and too few of us. We can't inspect everyone – we've got to leverage reporting and disclosure approaches to address our challenges.

> "While the evidence suggests that information provision should not replace traditional enforcement, new incremental transparency policies may leverage current enforcement efforts to achieve greater impacts."

This quote comes from Professor Jay Shimshack's testimony, last year, before the Congressional Committee on Transportation and Infrastructure. In fact, EPA's new Strategic Plan for FY 2011-2015 commits us to do exactly this:

1. [One] By building self-monitoring and reporting requirements directly into rules.

2. [Two,] By making better use of 21st century e-technology to transmit data directly from regulated sources to, and among, regulators. – and –

3. [Three,] By making more information available to the public in an easy-to-use, understandable format so the public can demand better facility <u>and</u> government performance.

Today's papers on labeling, greenwash, and inspection targeting can help move us forward. I would begin by asking all of the authors two opening questions:

1. Who's your audience for your papers? – and –
2. What do you want to accomplish with them?

If your goal is to get <u>policymakers</u> to read and apply your papers, consider publishing companion articles in plain English. With due respect, in my office, we don't all handle well sentences like, "To control for possible unspecified heteroskedasticy and autocorreclation, we use robust standard errors with clustering at the participant level."

My colleagues and I may enjoy an occasional "gin and tonic," but we don't know from "monotonic." My point is, if you want your work to be read and applied, why not speak to policymakers in their own language? If it's a penalty, call it that – not an "audit cost." Same with an inspection. And so forth.

Take this as a caring suggestion from someone who himself is willing to read dense economic analyses but too often finds it difficult convincing his colleagues to do the same.

My second comment, also for all of the authors: include in your papers the practical policy implications of your models. We can try to infer them from your analyses. But it helps us when you get the ball rolling in your publications.

The Greenwash paper provides an example of how to do this. The authors use their modeling results to call for increased public policy pressures to induce firms to adopt EMSs to improve their environmental self-awareness, thereby prompting more accurate disclosures. I have some issues with their reasoning, which I'll explain shortly. And the Agency already promotes EMS use. But the suggestion to reconsider our policies in the context of the paper's modeling is helpful.

In the Labeling paper, in addition to showing us how, when, and why (according to the models) it's good – or less good – to have NGO-promoted environmental labeling; industry-sponsored labeling; or both, the authors highlight a potential role for government in promoting or discouraging industry-sponsored labeling.

The Achieving Compliance Through Dynamic Auditing Systems paper could benefit from more in the way of practical recommendations from the authors as to how you advise EPA to employ your findings.

EPA already relies heavily on targeted, rather than random inspections. Our targeting is based on many factors and considerations. These are often sector and facility-specific. In fact, we may target based specifically on nondisclosure or inaccurate disclosure. And we're increasingly driven by performance-based goals and measures.

Can a relatively simple targeting model like yours one inform EPA's practical field work? If yes, where and how? In choosing our National Initiatives? In targeting individual field inspections within given sectors? In prioritizing when and where to issue statutory information request letters? Share your recommendations!

Some additional paper-specific comments. For the Labeling Paper, I'm not a labeling expert, but I discussed the papers with my more experienced colleagues. They shared these reactions.

Where the model could perhaps be of best practical use to EPA might be where voluntary industry, NGO, or both types of labels exist, and the Agency wants to better understand the pros and cons of supporting or endorsing one or both of the labels. The modeling results, alone, do not support generalizable label competition rules, but they do help frame the issue.

We might also consult the paper in considering whether, in a given case, some type of government involvement could be useful in policing – I'm using that term generically, now – the completeness or accuracy of the labels' content.

The authors focus on the labels themselves, rather than on underlying standards on which they're based. In practice, these aspects are linked. Take ISO 14001, for example. Here, an NGO created a standard which industries, other NGOs, <u>and</u> governments have designed into their programs. At some point, as the authors note, these linkages will need greater attention.

The authors might consider revisiting some of their models' assumptions. An example: the assumption that, in the absence of any labels, consumers cannot distinguish the abatement levels of any individual firm. In practice, because TRI, ECHO, and other relevant data sources are accessible on line, I'm not sure this is always a fair assumption in 2011.

Concerning the Greenwash paper, if it's truly important that the public receive accurate, comparable, and prompt data, perhaps what we <u>really</u> need is mandatory reporting. There are areas, however, where mandatory reporting is impossible, unnecessary, or inadvisable. Continued research on greenwashing could prove useful to EPA's voluntary and assistance programs in more effectively promoting green information, as opposed to green-wash.

I do have a problem with the assumption in the Greenwash paper that, just because a facility has an Environmental Management System, management's access to internal information will be improved. Based on litigation-related EMS experience and the EMS literature, I don't believe this can be assumed.

EMS adoption can just as readily be a cause of – as an antidote to – greenwashing. An EMS is just a tool. Firms adopt EMSs for all sorts of reasons. One might do so from a sincere desire to minimize its environmental footprint; another to improve its compliance; a third because its supplier requires it; a fourth expressly to greenwash. That's why EMS adoption alone – or even certification – is a weak metric for consumers <u>or</u> governments to use to distinguish between firms' disclosures or performance.

The Achieving Compliance Through Dynamic Audit Mechanisms paper resonated with me because my office's mission is to manage EPA's compliance targeting. The idea of a "dynamic tournament in which firms compete to avoid being targeted" especially grabbed my attention – it could be a great concept for a new environmental compliance reality show!

In fact, we should consider ways apply the dynamic tournament concepts modeled in the paper to how we arrange and display the data we make public. If it is true, as other research suggests, that communities and firms respond more strongly to "Top 10" lists than raw data alone, perhaps EPA should give more thought to ways we can rank facilities and states to promote competition to improve performance.

The most important finding, in the Targeting paper, may be the observation on page 21:

> "That targeting leads to significant enforcement leverage is strongly confirmed by statistical tests."

EPA's experience suggests this as well ... as long as the targeting rules aren't so transparent to enable people to determine with certainty when they have no real chance of being inspected at all. That would undermine deterrence.

I hope the targeting finding will be broadly shared. My program is often asked, by OMB and others, whether we can conduct more random inspections in order to generate statistically valid compliance rates. The question is a fair one, but the performance tradeoffs must not be overlooked.

Another finding is equally interesting: that [some] "targeting schemes which combine penalties with transitioning facilities between targeted and non-targeted groups" can produce greater efforts to comply than would occur through targeting with no transition opportunities.

As a practical matter, EPA is loath to assure anyone they've been removed from the pool of potential future inspection targets. Explore the history of the Performance Track program's "low priority for routine inspections" incentive to understand the politics and practicalities of why. Still, there may be areas where the approach would be helpful.

A concern: in real life, how we actually target may not lend itself well to broadly applied, 1-size-fits-all, algorithmic rules of the type that facilitate highly-generalized facility-leveling modeling.

When EPA's compliance and enforcement program target our resources, we begin by identifying the most significant national environmental problems. Then we identify the causes of the problems. We ask, are these causes due to noncompliance with <u>federal</u> standards? If yes, is this an area where a <u>federal</u> response is appropriate? And we proceed from there.

EPA often takes firms' compliance histories, and the size of their regulated emissions or discharges, into account in planning, targeting, and measuring – especially on the initiative or sector level. But again, when you tunnel down to the facility level, there are few hard and fast rules.

An example. Recidivism – violating; returning to compliance; and then violating again – is bad. It's a legitimate concern for the criminal <u>and</u> civil programs. Yet, as discussed in my office's 2009 recidivism report to OMB, to target specifically to minimize recidivism could, in some instances, yield perverse and inefficient outcomes. Such burning an inordinate amount of resources on just a few firms with the highest marginal costs of compliance.

The absolute level of a firm's discharges and emissions are important, too. But whether they exceed rule or permit limits, for how long, and their root causes also matter.

EPA or a state might respond differently to facilities with management-based root causes, versus others requiring significant capital expenditures to return to compliance. The latter could become subject to judicial consent decrees with long term compliance schedules. These firms would continue to be noncompliant, but could become EPA's <u>lowest</u> priority for new targeted inspections – especially if the decrees provide for compliance auditing – another form of information disclosure.

The authors introduce their paper by observing how, in many settings, compliance rates are actually significantly higher than classic economy theory would predict. They discuss Harrington's and others' efforts to address the conundrum, but here are 6 more factors worth considering:

(1) <u>Culture is important</u>. Our country has an exceptionally strong culture of compliance.

(2) <u>Criminal enforcement matters</u>. What price tag do <u>you</u> put on your liberty interest?

(3) <u>In real life, people often *don't* act like rational calculators</u>. Or perhaps they value wacky or counterintuitive things. Or maybe they're just *really* bad at math. This is why I like to see theoretical economists collaborate with behavioral scientists.

(4) <u>People determine risk with their guts as much as with their heads</u>. It's been demonstrated empirically that most, people significantly <u>over</u>estimate their odds of being audited.

(5) <u>In some sectors, most actors are not be deterred in the classic sense at all</u>. They don't fear enforcement because they believe they're good apples who are complying. For these folks, research suggests monitoring and enforcement serve valuable "reminder" and "reassurance" functions.

(6) <u>The potential for stochastic spikes in emissions and jointness in the production of pollutants</u>. I would refer you to Professor Shimshack's *Enforcement and Overcompliance* paper for additional background on these effects.

I look forward to Sarah's comments and our subsequent discussion. Thank you.

Sarah Stafford, College of William and Mary

For more information about this presentation, please see page 11 of the Meeting Summary included in this PDF.

Economic Perspectives on Environmental Information Disclosure

February 2011

Prepared by Jay P. Shimshack for the US Environmental Protection Agency
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Benefits of Information Disclosure Workshop

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DISCLAIMER:

1. Introduction

Information disclosure programs for environmental protection are proliferating rapidly. Growth has become so widespread that policy observers often refer to information programs as the “third wave” of environmental policy, following a first wave of command and control regulation and a second wave of market-based regulation. Given this rapid growth, natural questions arise. What do we know about environmental transparency policies? Are existing environmental disclosure policies delivering intended results? What don’t we know about information programs? What research is necessary to fill critical knowledge gaps?

This paper takes up these issues. First, I develop a taxonomy of environmental disclosure programs. Second, I discuss the potential advantages of transparency policies for environmental disclosure relative to alternatives. Third, I investigate the possible theoretical mechanisms linking information policies to enhanced environmental performance. Fourth, I review the empirical evidence on how well transparency policies deliver intended results in practice. Fifth, I review the relatively rare empirical evidence on the mechanisms driving policy outcomes in the real world. Finally, I review policy lessons, knowledge gaps, and directions for future research.

This review focuses on information disclosure programs only, and not on related voluntary programs, compliance assistance, and other pollution prevention activities. Additionally, I focus on lessons from North American contexts, and especially from U.S. policies. Those interested in environmental information policies in developing country contexts should consult Blackman (2010).

My key conclusions are straight-forward. Environmental information programs are not a panacea. Transparency policies have significant theoretical advantages relative to alternatives, but more often than not environmental information programs produce nuanced outcomes in the real world. These outcomes are often inconsistent with public policy objectives. Scholars and policy-makers have a lot to learn about the design of optimal transparency policies. Moreover, scholars and policy-makers have a lot to learn about the social efficiency of transparency policies relative to alternative regulatory approaches.

2. Information disclosure programs: a taxonomy

Table 1 presents a broad characterization of the possible forms of environmental information disclosure. I do not claim uniqueness, as other taxonomies are possible. Nevertheless, this simple classification highlights the increasing aims and applications of environmental transparency policies.

Table 1. Common environmental information disclosure programs

Program Type	Mandatory or Voluntary?
Environmental Advisories and Hazard Warnings	Mandatory
Pollution Release Registries	Mandatory & Voluntary
Performance Ratings and Rankings	Mandatory
Eco-Labels and Certifications	Mandatory & Voluntary
Information to Leverage Traditional Regulation	Mandatory

The common feature of mandatory environmental information programs is exogeneity from the entity perspective. These programs are externally imposed by governmental agencies, non-governmental organizations (NGOs), or the media. Individual entities responsible for environmental outcomes are unable to opt in, opt out, or systematically influence the content of the disclosed information. Examples of advisories and hazard warnings include: advisories for lead in paint, soil, and dust; advisories for radon in homes; and advisories for methyl-mercury in fish. Examples of mandatory pollution release registries include the national Toxic Release Inventory (TRI) and state-level carbon reporting rules. Examples of externally imposed performance ratings include state-sponsored fuel mix disclosure programs in the electricity industry, NGO-sponsored performance ratings like Greenpeace scorecards and Climate Counts climate scores, and media-sponsored ratings like Newsweek's "the Greenest Big Companies in America" rankings. The most prominent externally imposed environmental certification program is the EPA-sponsored Energy Star program. An illustrative example of a transparency policy to leverage traditional regulation is the EPA's "Annual Report on Enforcement and Compliance Assurance Accomplishments," which names companies that receive major civil or criminal enforcement actions for pollution violations. Other examples of transparency policies that leverage traditional regulation include consumer confidence reports of Safe Drinking Water Act violations by public water systems.

The unifying feature of voluntary information disclosure programs is the endogeneity of program participation. Individual entities responsible for environmental outcomes may voluntarily opt in and opt out of the program, and entities may often directly influence the content or magnitude of disclosed information. These programs may be especially flexible regulatory tools, but they may also be difficult to evaluate because of challenges related to selective participation, information accuracy, and selective reporting. Examples of voluntary pollution release registries include industry/NGO partnerships like the Carbon Disclosure Project. Examples of voluntary eco-label or certification programs abound. These include: forest certification programs; Leadership in Energy and Environmental Design (LEED) standards for construction; eco-labeled foods like wine, potatoes, and seafood; and "green" power products.

3. Potential advantages of transparency policies

In order to understand the potential advantages of information disclosure policies, it is useful to consider the desirable features of an effective transparency policy. First, an effective information program spurs a behavioral response. Information users, including consumers, investors, employees, activists, and other stakeholders, respond to the disclosed information.

Second, new market or legal conditions arise such that the provided information induces the entity responsible for the environmental harm to change their environmental behavior. Most often, this condition involves improved corporate environmental behavior. Third, stakeholder and firm responses are consistent with underlying public policy objectives.[1] This last condition is often overlooked by observers, as *any* observed response is often assumed to be consistent with policy goals.

Table 2. Potential advantages of environmental information disclosure

Classification	Potential Advantages. Transparency Policies …
Comparative Regulation Issues	…are flexible and easily targeted to specific groups …can mitigate trans-boundary environmental concerns …can mitigate risks from persistent environmental concerns …can leverage existing traditional regulatory programs …can address concerns where regulatory authority is absent or incomplete
Political Economy Issues	…may be relatively inexpensive …may be relatively quick to implement …may be politically expedient, especially when the socially desirable level of environmental harm is controversial

Table 2 reviews the potential advantages of environmental information disclosure programs. Many potential advantages pertain to comparative regulation issues. First, transparency policies are flexible and easily targeted to specific groups. Lead disclosures can advise households with young children about the dangers of lead paint and lead dust. Such policies reduce risk exposure without imposing socially costly regulations like mandatory lead-based paint removal in all older residences. Second, information disclosure policies can mitigate trans-boundary environmental concerns. While U.S. government agencies cannot mandate eco-friendly fishing practices in Mexico, dolphin-safe tuna labels can leverage domestic consumer power to enhance foreign producer’s environmental performance. Third, transparency policies

[1] This paragraph parallels a more detailed discussion in Fung et al. (2007).

can mitigate risks from persistent environmental concerns. Much of the existing stock of polychlorinated biphenyls (PCBs) was emitted in the past. We cannot regulate old PCB discharges, but fish consumption advisories can help prevent current risk exposure to pre-existing contamination. Fourth, information disclosure programs can leverage existing traditional regulatory regimes. The deterrence impacts of traditional civil and criminal sanctions can be potentially magnified by publishing lists of penalty recipients and their violations. Fifth, transparency policies can address environmental concerns where regulatory authority is absent or incomplete. EPA and state authority to regulate carbon dioxide and other greenhouse gas emissions is notoriously controversial, so many states have proceeded with mandatory carbon reporting rules designed to influence non-governmental stakeholder pressure.

Political economy factors have importantly influenced the proliferation of information disclosure programs as well. First, transparency programs may be inexpensive relative to alternatives. This is increasingly true as the costs of information dissemination technologies decrease. Second, disclosure policies may be quick to implement relative to alternatives. Most information programs require significantly less infrastructure than traditional regulatory alternatives. Third, transparency policies may be politically expedient, especially when the socially desirable level of environmental harm is controversial. Command and control regulations, market-based policies, and even many voluntary environmental programs involve caps or thresholds for environmental harm. Information disclosure programs most often do not.

4. Potential mechanisms

In order to understand *how well* transparency policies may work, it is useful to consider *how* transparency policies may work. This section takes up the issue. The discussion addresses corporate environmental behavior to ease interpretation, although parallel arguments could be made for individuals rather than firms. The discussion also focuses on negative disclosed environmental information, but natural parallel arguments could be made for positive disclosed environmental information.

Table 3. Six Theories Linking Disclosure and Environmental Performance

1. Managerial Information
2. Investor Preferences
3. Employee Preferences
4. Consumer Preferences
5. Private Politics
6. Public Politics

Table 3 presents six channels that might link information disclosure programs and environmental performance. First, the managerial information hypothesis suggests that external information helps firms identify areas where they are generating environmental harm by using inputs wastefully. Disclosed negative performance may highlight areas where management improvements can be made, and therefore disclosure may spur improved environmental behavior. Second, under the investor preference hypothesis, investors with green preferences avoid investment in facilities with identifiably poor environmental performance. Disclosed information may provide incentives for improved environmental behavior because negative information may raise capital acquisition costs. Third, under the employee preference hypothesis, employees with green preferences are less loyal, demand higher wages, and are more difficult to hire at firms with identifiably poor environmental performance. Transparency programs may provide incentives for improved environmental behavior because negative information may raise labor costs. Fourth, under the consumer preference hypothesis, consumers with green preferences have a positive willingness to pay for environmentally differentiated products or products from socially responsible producers. Disclosed information may provide incentives for improved environmental behavior because negative information may reduce sales revenues.

The final theories linking information disclosure and environmental outcomes entail "private politics" and "public politics." Under the private politics hypothesis, NGOs and activists target protests, boycotts, letter-writing campaigns, proxy votes, and/or citizen suits towards firms with identifiably poor environmental performance. Transparency programs may provide incentives for improved environmental behavior because firms wish to avoid these external pressures. Under the public politics hypothesis, public regulators target future regulation, increase current and future monitoring and enforcement attention, and complicate future permit applications at firms with identifiably poor environmental performance. Disclosed information may provide incentives for improved environmental behavior because firms prefer less rigorous regulatory oversight.

5. Empirical evidence: effectiveness

Do transparency policies deliver intended results in practice? The broader disclosure literature cites a few examples where the answer appears to be largely 'yes.' Examples include restaurant hygiene grade cards and auto safety ratings. See, for example, reviews by Dranove and Jin (2011) and Fung et. al (2007). Successful policies tend to share several features: careful ex-ante design; clear, understandable, and standardized information; information provision where and when the target audience makes decisions; and persistent ex-post evaluations and revisions (Weil et al. 2006).

In contrast, the evidence on environmental information disclosure programs is nuanced. Some environmental transparency policies generate no response. Some environmental disclosure programs generate responses that are inconsistent with policy objectives. Some environmental transparency policies generate desired responses, but in incomplete or socially inefficient ways.

Finally, some environmental disclosure programs generate desired responses for some groups, but socially undesirable outcomes for other groups.

The remainder of this section reviews select evidence on the effectiveness of information disclosure. The discussion is not intended to be comprehensive, and many important studies are not reviewed. The goal of the section, rather, is illustrative. I focus on what I see as the key strengths and weaknesses of disclosure in the real world.

The literature on information advisories highlights several cautionary notes about environmental information disclosure. Desvousges et al. (1992) found that information advisories influenced self-reported attitudes favorable towards radon testing. This is consistent with policy goals. However, radon testing itself only increased when mass media dissemination was coupled with community-based implementation programs. Information alone did not achieve the social objectives. Shimshack et al. (2007) and Shimshack and Ward (2010) found that mercury advisories for commercial seafood induced at-risk consumers to reduce harmful methyl-mercury intakes. Taken alone, this result is again consistent with policy goals. However, observed mercury reduction benefits came with substantial countervailing costs from reductions in beneficial nutrients like omega-3 fatty acids. Consumers did not differentially avoid high mercury fish, nor did they substitute from high mercury fish into low mercury/high omega-3 fish. On net, the estimates in Shimshack and Ward (2010) indicated that the public health benefits of a national mercury-in-seafood advisory were *negative*. Also, Shimshack et al. (2007) found that many consumers not considered at-risk reduced consumption in response to advisories. Such outcomes may be rational, but they may have pronounced market implications.

The pollution release registry literature produces inconsistent conclusions. Some early studies found that stock movements associated with Toxic Release Inventory (TRI) announcements led to increased abatement and reduced emissions (Konar and Cohen 1997; Khanna et al. 1998). However, more recent work by Bui (2005) and others showed that some of effects may have been at least partially attributable to unobserved regulation coinciding with TRI information releases. Pollution reductions may also have been partially offset by increased off-site transfers. TRI data, at least in raw form, is also unlikely to systematically affect households' decisions about where to live, where to work, or where to engage in activism. See, for example, Bui and Mayer (2003). A final concern is that TRI-induced pollution reductions may be especially likely in high income areas and may even be reversed in low income areas (Powers 2010).

One emerging consensus in the pollution registry literature is that processed environmental information is significantly more likely to produce desirable outcomes than unprocessed information. For example, Bae et al. (2010) found that state-sponsored TRI data dissemination efforts alone did not reduce health risks. In contrast, state-sponsored TRI data processing efforts like risk analyses and customized reports reduced both pollution emissions and health risks. Similarly, Scorse (2010) found that firms placed on Top 10 TRI polluter lists reduced emissions more than they would have had they not appeared on this list – even though their total emissions were still publicly disseminated in unprocessed formats.

The literature exploring externally imposed environmental performance ratings draws stronger first-order conclusions than the pollution registries literature. Event studies typically show that stock prices declined in response to negative environmental news and increased in response to positive environmental news (e.g. Hamilton 1995; Klassen and McLaughlin 1996). However, even this strand of the literature suggests some potential limitations. Delmas, Montes, and Shimshack (2009) found that mandatory fuel mix and emissions performance information reduced the equilibrium proportion of fossil fuels and increased the equilibrium proportion of cleaner fuels in disclosure states. These results are consistent with policy goals. However, the programs made “clean” firms cleaner while leaving “dirtier” firms unchanged. The evidence suggests it may be more socially efficient to clean up dirtier areas first. Beatty and Shimshack (2010) showed that climate performance ratings produced statistically significant and large impacts on stock market returns. However, results were restricted to penalties for poorly rated firms; firms receiving good ratings received no significant benefit. These asymmetric responses may hint at limits to the long-term potential for performance ratings to improve average environmental performance.

The literature exploring voluntary disclosure programs is small. However, Kim and Lyon (2010) highlight the fundamental concern with such policies. They found that participants in the Department of Energy’s voluntary greenhouse gas registry engaged in significant selective reporting. On average, participants selectively reported reduced pollution emissions while actually increasing aggregate emissions over time. On net, the program produced no significant impacts on overall carbon intensity. The key point is that voluntary disclosure programs may be especially susceptible to “greenwashing” or at least to selective reporting.

The largest empirical economics literature related to environmental transparency addresses eco-labels and certification programs. A growing stated preference literature finds that consumers, on average, express an incremental willingness to pay for environmentally friendly and socially responsible products including food and timber products. Electricity consumers state that they are willing to pay 0.6 to 2 cents/kWh, or about a 5 to 20 percent premium, for renewable energy (Goett et al. 2000, Roe et al. 2001). Quantitative evidence indicates that buildings with green ratings earn rental rates that are 3 percent higher per square foot, after controlling for other attributes. Sales prices are 16 percent higher (Eichholtz et al. 2010). In short, the evidence suggests that consumers' assessment of firms, evaluation of products, final consumption decisions, and willingness to pay are influenced by environmental information disclosure.

However, the intensity of demand for 'green' products and products produced by 'green' firms varies significantly across context. Further, the demand for these goods is far from universal. Relatively small subgroups with strong feelings about socially responsible consumption drive most observed green consumption outcomes and green price premiums, so environmental gains from disclosure alone may be limited. Further, the literature suggests that market impacts are extremely sensitive to how the information is presented and the specific characteristics of the target consumers. See, for example, Leire and Thidell (2005).

The small literature examining disclosure programs designed to leverage the effectiveness of traditional regulatory programs suggests these approaches may be effective at achieving policy goals. Foulon et al. (2002) found that inclusion on a public list of noncompliant pulp and paper mills in British Columbia produced similar incentives for pollution control to a regulatory fine. In large survey of plant managers, Thornton et al. (2005) found that 63 percent of respondents took an environmental action in response to learning about a sanction at another facility. This suggests that greater information disclosure may lead to greater deterrence spillover effects from each individual sanction.[2] Bennear and Olmstead (2008) showed that consumer confidence reports that summarize compliance status and specific violations under the Safe Drinking Water Act spurred fewer health and other violations. While the literature on disclosure

[2] These deterrence spillover effects are often referred to as general deterrence. Shimshack and Ward (2005) empirically assessed the magnitude of this effect in an environmental context.

policies for leveraging traditional regulation is encouraging, relatively few studies directly examine the approach and both scholars and policy-makers still have a lot to learn.

6. Empirical evidence: mechanisms

How do transparency policies affect environmental performance in practice? The short answer is that our knowledge is very limited. More research is needed. Nevertheless, insights into mechanisms linking disclosure and environmental performance can be obtained from diverse empirical settings. In this section, we review the existing evidence.

Evidence from the environment and competitiveness literature suggests that the managerial information hypothesis is unlikely to systematically explain disclosure-induced environmental improvement. The managerial information argument involves a win-win scenario where environmental performance spurs innovation that enhances overall profitability. In other words, environmental improvements come at no (long-term) cost to the firm. However, most empirical studies suggest that exogenously-imposed environmental improvements do not induce innovation and do not enhance competitiveness on average (Jaffe et al. 1995, Pasurka 2008).

Causal empiricism and the existing quantitative evidence suggest that investor preferences are unlikely to drive disclosure-induced environmental improvements. As noted in the previous section, investors do respond to disclosed environmental information. However, it is likely that these investor responses occur via changes in beliefs about expected profitability through other channels like consumption or politics. Sophisticated investors often know as much or more about firms' aggregate environmental performance as disclosing regulators or NGOs, so they may be unlikely to learn much from disclosure. Further, Davidson et al. (1995) found no significant financial market impact when small groups of investors publicly announced stock divestitures for social purposes; other investors were immediately willing to buy divested stock.

Similarly, empirical studies of non-profit and public labor markets provide little support for the notion that employee preferences could drive disclosure-induced environmental improvements. In short, the evidence indicates that employees do not appear to systematically sacrifice wages to work at socially responsible organizations. Observed wage differences between non-profit/public sector employment and private sector employment become small and typically insignificant after controlling for worker, job, and workplace characteristics (Frye et al. 2006; Goodeeris 1988; Leete 2001; Ruhm and Borkowski 2003). Observed non-profit/public sector wages are lower than private sector wages on average, but it appears that much of the observed difference may be driven by differences in job requirements and working conditions. If workers are not systematically willing to sacrifice wages, or "donate labor," employee preferences are unlikely to importantly link disclosure to subsequent environmental improvement.

In contrast to hypotheses related to managerial information, investor preferences, or employee preferences, hypotheses related to consumer preferences may provide a compelling link between disclosure and environmental performance. Consumers regularly express positive willingness to pay for environmentally differentiated products and products from socially responsible firms. We do observe consumers paying premiums for these goods in the real world (Goett et al. 2000; Roe et al. 2001; Bird et al. 2009; Eichholtz et al. 2010). Therefore, product markets may provide incentives for enhanced environmental performance in the presence of disclosed information. However, the evidence also suggests that current socially responsible consumption comes from small subgroups of socially responsible consumers. So, this channel's potential to influence environmental outcomes via disclosure may be limited in the long-run unless preferences themselves are evolving.

Private and public politics channels may also provide compelling links between disclosure and environmental performance. One third to one half of firms targeted by protests, boycotts, letter writing campaigns, proxy votes, or citizen suits publicly announce subsequent behavioral changes that are broadly consistent with activist aims (Davidson et al. 1995; Eesley and Lenox 2006). Other companies voluntarily implement environmental management systems in response to proxy actions and other activist pressures (Gupta and Innes 2009). Similarly, evidence indicates that good environmental performers receive permits more quickly and receive less regulatory attention (Decker 2003; Innes and Sam 2008). If disclosure enhances public and NGO pressure, improved environmental performance may result. Nevertheless, little research directly links transparency policies to pressures from public or private agents, so support for these mechanisms is more speculative than definitive.

7. Discussion

So what have we learned? At a minimum, this review highlights the fact that environmental information disclosure programs are not a panacea. Environmental transparency policies have significant theoretical advantages relative to alternatives, but they frequently produce nuanced outcomes in the real world that are inconsistent with public policy objectives. Scholars and policy-makers are well served by remembering that the classical economic belief that information provision improves welfare relies on strong assumptions about how target audiences access, understand, and process information.

Broad implications follow from these basic lessons. First, disclosure policies must be very carefully crafted ex-ante to address the psychological and behavioral realities of users' responses to information. Second, disclosure policies must be evaluated and adjusted ex-post to maximize their effectiveness and social efficiency. Third, early evidence on mechanisms suggests that environmental disclosure policies that target consumers, activists, and public regulators may be more likely to produce socially desirable outcomes than those aimed at firm managers, investors, and employees.

Notwithstanding the above lessons, we have much to learn about environmental information disclosure programs. Several key questions are poorly understood.[3] First, how does measurement error influence outcomes? The evidence suggests that disclosed environmental information can sometimes be inaccurate and misrepresented, especially when disclosed content or disclosure itself is voluntary. Second, what do optimal disclosure program designs look like? The complete answer is likely context dependent. At present, we have learned some general lessons about what *not* do to, but less about what to do in any given setting. Third, what are the long-run effects of information disclosure policies? Nearly all existing empirical studies focus on the short-run implications of transparency programs. Fourth, what links disclosure and environmental outcomes in the real world? As discussed, evidence from other literatures provides some insight into environmental disclosure mechanisms, but a more definitive understanding of real world links is essential for the design of effective and efficient transparency policies. Fifth, how much do disclosure programs costs firms and regulators? Scholars and policymakers often simply assume that disclosure policies are socially cheaper than natural alternatives, but this may not be accurate. Finally, the most fundamental question: How does the marginal "bang per buck" from a dollar devoted to environmental disclosure compare to the marginal dollar devoted to alternative, and more traditional, regulatory approaches?

[3] Dranove and Jin (2011) explored unresolved issues in quality disclosure, and several of their directions for future research parallel the discussion here.

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AGENDA

8:00 a.m. – 8:30 a.m.	**Registration**	
8:30 a.m. – 8:45 a.m.	**Welcome and Introduction:** *Will Wheeler, EPA, National Center for Environmental Economics*	
8:45 a.m. – 10:10 a.m.	**Session I: The Effectiveness of Energy Efficiency Labels**	
	Session Moderator:	*Ann Wolverton, EPA, National Center for Environmental Economics*
	8:45 a.m. – 9:10 a.m.	Consumer Willingness-to-Pay for Energy Star and Green Power Labeled Refrigerators *Christopher Clark, University of Tennessee*
	9:10 a.m. – 9:30 a.m.	Evaluating Alternative Approaches to Energy Efficiency Labeling: Designing and Implementing a Choice Experiment *Juha Siikamaki, Resources for the Future*
	9:30 a.m. – 9:40 a.m.	Discussant: *Chris Moore, EPA, National Center for Environmental Economics*
	9:40 a.m. – 9:50 a.m.	Discussant: *Maureen McNamara, EPA, Climate Protection Partnerships Division*
	9:50 a.m. – 10:15 a.m.	Questions and Discussion
10:15 a.m. – 10:40 a.m.	**Break**	
10:40 a.m. – 11:45 a.m.	**Session II: What Causes Reductions in TRI Emissions?**	
	Session Moderator:	*Charles Griffiths, EPA, National Center for Environmental Economics*
	10:40 a.m. – 11:05 a.m.	The Impact of Quasi-Regulatory Mechanisms on Polluting Behavior: Evidence From Pollution Prevention Programs and Toxic Releases *Linda Bui, Brandeis University*
	11:05 a.m. – 11:15 a.m.	Discussant: *Ann Wolverton, EPA, National Center for Environmental Economics*
	11:15 a.m. – 11:25 a.m.	Discussant: *Sheila Olmstead, Resources for the Future*
	11:25 a.m. – 11:45 a.m.	Questions and Discussion
11:45 a.m. – 1:00 p.m.	**Lunch** (On Your Own)	

1:00 p.m. – 3:15 p.m.	**Session III: Information, Audits, and Enforcement**		
	Session Moderator:	*Patrick Walsh, EPA, National Center for Environmental Economics*	
	1:00 p.m. – 1:25 p.m.	Regulatory Enforcement With Dynamic Targeted Audit Mechanisms *Christian Vossler, University of Tennessee*	
	1:25 p.m. – 1:50 p.m.	Greenwash: Corporate Environmental Disclosure Under Threat of Audit *Thomas Lyon, University of Michigan*	
	1:50 p.m. – 2:15 p.m.	Competing Environmental Labels *Carolyn Fischer, RFF*	
	2:15 p.m. – 2:30 p.m.	Discussant: *Jon Silberman, EPA, Office of Enforcement and Compliance Assurance*	
	2:30 p.m. – 2:45 p.m.	Discussant: *Sarah Stafford, College of William and Mary*	
	2:45 p.m. – 3:15 p.m.	Questions and Discussion	
3:15 p.m. – 3:30 p.m.	**Break**		
3:30 p.m. – 5:00 p.m.	**Session IV: Panel: Perspectives on Information Disclosure, Emissions, and Compliance**		
	Moderator:	*Will Wheeler, EPA, National Center for Environmental Economics*	
	Panelist:	*Jay Shimshack, Tulane University* *Cody Rice, EPA, Office of Chemical Safety and Pollution Prevention*	
5:00 p.m.	**Adjournment**		