

The Identification of Regulatory Impacts Using Panel Data

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Abstract

This dissertation broadly explores the use of panel data methods for identifying the effects of regulation. Chapter 1 applies dynamic panel models to control for potential feedback effects when examining the relationship between environmental regulations and firm performance. Specifically, this chapter uses data from the U.S. manufacturing industry to explore the effects of the Clean Air Act (CAA) on firm performance outcomes related to profitability, employment, and capital expenditures. The evidence suggests that feedback effects are present in the data and, more critically, appropriately modeling these effects can alter conclusions regarding the effects of environmental regulation.

Chapter 2 uses panel data logit-class models to analyze whether the stringency of environmental regulation motivates mergers among regulated firms. Using comprehensive data set that capture both environmental and financial behaviors from a variety of data sources, results in this chapter provide new evidence on the impact of environmental regulation on manufacturing firm M&A activities, suggesting that more stringent environmental regulation (CAA) will motivate manufacturing firms conduct more M&As, and market value of M&A firms will increase.

In Chapter 3, my coauthor and I use Monte Carlo experiments to provide insight on how power is influenced by data analysis methods, such as the choice of econometric estimator or whether all or a subset of data is used. Surprisingly, except in very few extreme cases, we find that power does not vary much if one relies on simple difference-of-means tests or instead on robust or structured (i.e. assumption-laden) panel data estimation. However, the simulations provide strong evidence of power loss when uses a subset of the collected data, and power gains through the inclusion of non-experimental control variables (e.g. demographic variables).

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Introduction

This dissertation broadly explores the use of panel data methods for identifying the effects of regulation. Panel data methods have been widely used in economic analysis. Relative to cross-section data, the analysis of panel data provides opportunities for better identification when omitted variables are a concern. Moreover, panel data provides opportunities to explore important dynamic relationships. Chapter 1 applies dynamic panel models to control for potential feedback effects when examining the relationship between environmental regulations and firm performance. Previous empirical studies have treated regulation intensity as exogenous, and find that more stringent environmental regulations negatively impact firm performance measures such as profits. However, theoretical research suggests that potential feedback effects exist in the sense that the stringency of environmental regulations may be endogenous to firm performance, and further that such feedback effects may positively affect firm performance. Specifically, this chapter uses data from the U.S. manufacturing industry to explore the effects of the Clean Air Act (CAA) on firm performance outcomes related to profitability, employment, and capital expenditures. The evidence suggests that feedback effects are present in the data and, more critically, appropriately modeling these effects can alter conclusions regarding the effects of environmental regulation. Empirical results support the notion that more stringent environmental policies can provide dividends to regulated firms.

Chapter 2 uses panel data logit-class models to analyze whether the stringency of environmental regulation motivates mergers among regulated firms. Extant theoretical analyses studying how environmental regulation affects merger and acquisition decisions suggest that polluting firms have more incentives to cooperate under more stringent environmental regulations. Coincident with this, although the related empirical literature is sparse, the common finding is that

the likelihood of mergers increases with stringency. These studies have ignored the effects of financial characteristics such as stock market return and market/book ratio, however, which are commonly found in the finance literature to be important determinants of mergers. As such, this chapter uses insights from both the environmental economics literature and the broader finance literature to investigate whether existing findings are sensitive to the omission of these key financial variables. Firm-level panel data models are estimated using merged data from COMPUSTAT/CRSP, Securities Data Corporation (SDC), PACE, and NEI. Results in this chapter provide new evidence on the impact of environmental regulation on manufacturing firm M&A activities, suggesting that more stringent environmental regulation (CAA) will motivate manufacturing firms conduct more M&As, and market value of M&A firms will increase.

An increasingly common approach for identifying the effects of potential and existing policy interventions is to use randomized experiments. As highlighted by some high-profile articles (e.g. Maniadis, Tefano and List, 2014), one often ignored yet important consideration when analyzing data from randomized experiments is statistical power: the ability to reject a false null hypothesis. In Chapter 3, my coauthor and I use Monte Carlo experiments to provide insight on how the power of statistical tests based on panel data methods compares with more traditional (and simple) parametric and non-parametric tests commonly used by researchers when analyzing data from randomized experiments. Further, we compare the power across a class of panel data estimators that vary in terms of the structure imposed on the model. Surprisingly, except in some extreme cases, we find that power does not vary much if one relies on simple difference-of-means tests or instead on robust or structured (i.e. assumption-laden) panel data estimation. However, the simulations provide strong evidence of power loss when uses a subset of the collected data, and power gains through the inclusion of non-experimental control variables (e.g. demographic

variables). Although the structured panel estimator we explore (random effects model), which adheres to the underlying data generating process, is in fact the most efficient according to the simulations, we find considerable evidence of bias in small samples. The extent of the bias, in combination with small efficiency gains, suggests that other approaches may be more appropriate in some settings. Although focused on data from randomized experiments, the results are also informative for the analysis of observational panel data using quasi-experimental identification approaches.

**Chapter 1: Re-examining the Relationship between Environmental
Regulations and Firm Performance: Evidence from a Dynamic Panel Model
of the U.S. Manufacturing Sector**

Abstract:

An important empirical question for both policy makers and firms is whether more stringent environmental regulations have positive or negative impacts on firm performance. In order to gain new insight on this question, and in contrast to related work, this chapter employs dynamic panel models that allow for potential feedback effects (i.e. reverse causality) between measures of performance and regulation. Specifically, this chapter uses data from the U.S. manufacturing industry to explore the effects of the Clean Air Act (CAA) on firm performance outcomes related to profitability, employment, and capital expenditures. The evidence suggests that feedback effects are present in the data and, more critically, appropriately modeling these effects can alter conclusions regarding the effects of environmental regulation. Empirical results support the notion that more stringent environmental policies can provide dividends to regulated firms.

1.1 Introduction

An important empirical question for both policy makers and firms is whether more stringent environmental regulations have positive or negative impacts on firm performance. Accumulated empirical evidence suggests that increasing environmental regulation has tended to negatively affect firms in terms of profitability, employment and capital expenditures. Such a result is consistent with traditional economic theories that mainly consider the short-run costs of regulation. However, the so-called Porter Hypothesis (Porter, 1991; Porter and van der Linde, 1995) challenged such conventional wisdom by considering benefits and costs systematically, asserting that more stringent but well-designed environmental regulation could generate net private benefits for firms. The basic intuition is that managers have present-biased preferences, which

serves to retard the adoption of expensive but more efficient green technology, while environmental regulation forces them to correct such preferences (Ambec and Barla, 2006). In addition, if there are opportunities such as better access to green markets and product differentiation, this can serve to enhance the private benefits from environmental regulations (Ambec and Lanoie, 2008).

The theoretical model underlying the Porter Hypothesis implies that potential dynamic feedback effects (i.e. reverse causalities) exist. The basic intuition is as follows. Changes in the regulatory status affect production costs and incentivize firms to re-optimize by innovating more on green technology. On the flipside, economic activity typically correlates with pollution, which affects the intensity of environmental regulations. Econometric work by Chamberlain (1993), Ahn and Schmidt (1995), Chay and Hyslop (1998), and Wooldridge (2000) suggest that exogeneity assumption about conditioning variables will cause bias when dynamic feedback effects are present but ignored. If dynamic feedback effects are in fact present in the relationship between environmental regulation and firm performance, this calls into question any analysis that ignores these effects.

Previous empirical work on environmental regulation, summarized in Table 1-1, has neglected possible feedback effects. Most studies employ static econometric models, and identify regulation effects using: (1) simple regressions (Greenstone 2002, Greenstone et al. 2012); (2) propensity matching estimates (List et al. 2003); and (3) difference-in-difference estimation (Holladay 2010). Negative effects of environmental regulations dominate the results.¹ A few

¹ The magnitudes and the significance levels are small, some even find no impacts. The major problem in those estimators is ignoring the feedback effects. The first two methods could help address the endogenous problem to some degree. Even so, in Greenstone (2002), only estimator based on CO is significant. Later research by Holladay (2010) discusses that it may be due to firms transferring from non-attainment to attainment, which leads to concerns about the main limitation of the empirical methodology, the feedback problem.

studies have considered dynamics but ignore possible endogeneity issues.² For example, Lanoie et al. (2008) empirically study the dynamic effect of regulation on manufacturing by adding time lags of regulation, while Greenstone (2002) applies a dynamic model by including the lag of dependent variable as a robustness check for the simple static regression models. Similar results to those from static analyses are found.

Distinct from earlier empirical work, this chapter uses two-step system Generalized Methods of Moments (GMM) estimation of dynamic panel models that address potential feedback effects by treating measures of regulation as endogenous. In particular, using county-level data from the U.S. manufacturing industry, this chapter explores the effects of the Clean Air Act (CAA) on firm performance outcomes such as profitability, employment and capital expenditures.³ For each county, manufacturing profitability is measured by return on sales (ROS), real profits, and value added. Under the CAA, regulatory stringency increases based on increases in pollution activity.⁴ For example, counties with pollution levels above regulatory standards are often required to adopt particular abatement technologies, which would then affect future regulatory status. Further, when firms are in counties in attainment status, they have a relative cost advantages, which can serve to incentivize increased production and pollution, and possible lead to a violation of

² From industrial organizational theory, Ericson and Pakes (1995) provide a formal framework allowing feedback effects in dynamic competition model. Ryan (2012) recovers the entire cost structure of U.S. Portland cement industry by adopting the Ericson and Pakes (1995) framework. He finds that static analysis would obtain wrong sign of the welfare effects on incumbent firms.

³ Employment and capital are two key elements of firm's decision making process, so similar to previous literature; this study includes them to shed light on the environmental regulatory impacts more broadly under the same dynamic feedback effects framework. More importantly, results based upon these two variables show that feedback effects do present in the data.

⁴ In the United States, the CAA requires the Environmental Protection Agency (EPA) to set ambient pollution standards for airborne pollutants such as ozone, sulfur dioxide (SO₂), and carbon dioxide (CO₂) to every locale. EPA designates the regulatory status to every county annually according to those standards, providing variations in regulatory stringency that fit the purpose of the study. Counties whose pollution levels are above the standards are under non-attainment status with more stringent regulation than those under attainment status.

regulatory standards. Exploiting county-level panel data compiled from Economic Census and Environmental Protection Agency (EPA), panel dynamic models that allow for feedback effects can capture such dynamics appropriately.

The evidence of this chapter suggests that feedback effects are present in the data and, more critically, appropriately modeling these effects can alter conclusions regarding the effects of environmental regulation and lend support to the Porter Hypothesis. Ignoring feedback effects, the estimates of regulatory impacts from simple panel dynamic models are negative and not significant in the ROS equation, which is similar to extant estimates from naïve static panel models. However, the estimates are positive and significant when the potential feedback effects are considered. On average, non-attainment status leads to an annual increase in ROS by about 1.5 percentage points. Similar results are obtained when using different measurements of profitability, emphasizing the importance of feedback effects in the estimation. For real profits, dynamic feedback effects models show that CAA non-attainment status could increase profits by about 250 million in 2005 dollars, compared to a loss about 634 million from a naïve static model. More stringent CAA regulation increases value added by about 47 million in 2005 dollars, rather than a loss of 261 million implied by static models. Although no significant impacts of CAA regulation on employment growth or capital expenditures are found, signs of estimated coefficients are switched from negative to positive when dynamic feedback effects are considered, providing some suggestive evidence of feedback effects.

1.2 Background

1.2.1 *Feedback Effects: Intuition and Theoretical Background*

Underlying dynamics indicated by the Porter Hypothesis suggest feedback effects. Intuitively, feedback effects exist because of mutual influences between environmental regulations and production processes, as in the supply-and-demand system. In the case of environmental regulations under CAA, the regulatory status is determined by whether a county's ambient pollution exceeds one of the ambient thresholds established for criterion pollutants, where the thresholds acts as the equilibrium price in supply-and-demand model. If the pollution levels exceed the standards, then firms therein will be found with more stringent regulation, thus in turn affect the production cost of firms. Most of the previous empirical studies assume the regulatory status is exogenous to firms and use quasi-experimental design methods by constructing counterfactuals of regulated and unregulated counties. However, with changes in production costs, firms can either change the level of production or production technology, and either of these changes will affect the byproduct, the pollution that ultimately affect the regulatory status. Thus, the process starts over again, leading to the existence of feedback effects.

Theoretically, both environmental regulations and economic performance could be endogenous. Most previous literatures adopt static analyses without considering the dynamic property of Porter Hypothesis.⁵ Contrast to static studies, Xepapadeas and Zeeuw (1999) notice

⁵ The focus of those static studies is to find out why the firms not choose the more efficient production frontier by themselves if there is indeed an opportunity of producing with higher efficiency. This is the main doubt against the Porter Hypothesis. Two sets of literature using static models over the last 20 years provide some insights to answer the question. The one set focuses on the organizational and behavioral economics, pointing out that the limited rationality of the firm is driven by its manager present-biased preferences. Due to the present-biased preferences of managers, Ambec and Barla (2006) argue that managers tend to postpone any costly investments in innovation of abating pollution. However, environmental regulations force managers to change their self-control problem, which enhances firm profits. The other set turns back to study the firms' profit maximization problem. Andre et al. (2009) show that a minimum standard for environmental product quality might benefit all firms under a setting where there

the importance of dynamic behaviors behind environmental regulations and production decisions. They analyze the impact of emission tax on the dynamics of a representative firm's choice of capital expenditures, showing that the environmental regulations will lead to an increase in average productivity. In addition, there are two strands of dynamic analyses recently: (1) how transitory environmental policy can engender permanent changes in the sustainability of economic performance; (2) how transitory fluctuations in productivity over time interact with the performance of environmental policies. On one hand, the model about effects of environmental regulation on manufacturing performance follows the Xepapadeas (2005) model allowing endogenous technological growth. The planning problem is to maximize utility by choosing optimal consumption and pollution level. Xepapadeas (2005) shows that with the introduction of the abatement, it is possible to have sustained economic growth in the long run, if there is increasing return on pollution abatement. On the other hand, Heutel (2012) argues from the other direction: the effects of an exogenous productivity shock on the performance of environmental policies. He proposes a dynamic programming model by including pollution and abatement, and treats productivity shock as exogenous. And he finds that the optimal emissions are higher when there are positive productivity shocks, as a result, he argues that the optimal environmental policies like the emission taxes or quotas should be more stringent in responding to such positive technology shocks.

However, these models account for only one part of the underlying dynamic interactions between environmental regulation and economic performance. Combining those two together

is imperfect competition in differentiated products. In short, environmental regulations allow them to reach a Pareto-improving equilibrium. Lambertini and Tampieri (2012) replace Bertrand with Cournot behavior, and then argue that a Porter-type result takes place only within a certain region.

leads to feedback effects. Heutel and Fischer (2013) clearly point out that ignoring the interactions between environmental policies and economic performance risks overlooking the feedback effects.

1.2.2 CAA and the Porter Hypothesis

CAA is the most fundamental air pollutants regulation in the United States, and it is widely studied because it provides rich regulatory variations. Under CAA, Environmental Protection Agency (EPA) sets ambient pollution levels of different air pollutants, classifying U.S. counties into two categories based on the standards: counties whose pollution levels are below the standards will be designated as in attainment; otherwise will be in non-attainment. Counties in non-attainment are required to develop a State Implementation Plan (SIP) to reduce pollution below the EPA standards; both new and existing establishments are subjected to stricter regulations than those in counties in attainment. For example, new establishments in non-attainment counties are required to obey “lowest achievable emissions rate” rule, with no reference to cost, while those in attainment counties are required to implement “best available control technologies”. If firms in attainment counties do not adopt control technologies voluntarily, higher probability will be that they are forced to implement higher-requirement technology after being regulated, thus regulation is endogenous, and feedback effects exist. In addition to introduce dynamic feedback effects, the designation of regulatory status under CAA generates rich variations in regulation among and within county. Since different counties may have different regulatory status, and a county in different years may be designated into different status dynamically as well.

Therefore, CAA fits the purpose of testing the Porter Hypothesis. The basic idea of the Porter Hypothesis is that more stringent environmental regulations will lead to win-win situations, where both social welfare and net benefits of firms will be increased. In order to test the Porter Hypothesis, variations in the degree of regulatory stringency are fundamental. A number of

studies, including Greenstone (2002), List et al. (2003), Holladay (2010), and Greenstone et al. (2012), etc., take advantages of regulation variations from CAA. In addition, the use of the CAA regulations should get rid of the concerns in Jaffe et al. (1995) about too aggregated measures of regulation (e.g., state-level measures) to detect the differences in stringency. Moreover, CAA regulatory variations also overcome the shortcomings of using abatement costs to measure stringency, under which it might be the case where inefficient firms spend more while under similar regulatory situations. This chapter is the first to study CAA effects on U.S. manufacturing by allowing for dynamic feedback effects in panel dynamic models.

1.3 Empirical Methods

1.3.1 Data

The main data sources are the Economic Census data containing county-level manufacturing performance with SIC/NAICS information about sub industries under manufacturing sector,⁶ and the Greenbook from EPA showing the regulatory status of counties over years 1978 to 2012. The Economic Census data comes from surveys conducted every five years, and include data on the manufacturing value of shipments, value added, employment, payroll, costs of materials, new capital expenditures, production workers, and the number of establishments of each county.⁷ Industrial classification information is included as well. For year 1982, 1987, and 1992, the industrial classification was based on SIC, after 1992, it was based on NAICS. In the final data, all NAICS codes are converted to SIC based on the SIC-NAICS

⁶ The Economic Census is the U.S. Governments' official five-year measure of American business and economy. Is it conducted by the U.S. Census Bureau, and response is required by law.

⁷ More details about the variable definitions can be found in Appendix A.

comparison information from Economic Census. The Greenbook contains regulatory status of specific pollutants, including 1-hr Ozone, 8-hr Ozone, CO, PM10, PM2.5, and SO₂.⁸ When identifying separate regulatory effects of different pollutants, this chapter combines 1-hr Ozone and 8-hr Ozone as Ozone, PM2.5 and PM10 as PM. The raw data contains 3128 counties from reporting years 1982, 1987, 1992, 1997, 2002, 2007, and 2012, with gaps and missing values in key variables, especially the dependent variables. The missing values appear to be unrelated to county characteristics or reporting years. Necessarily omitting observations with missing data, the estimation sample consists of at most 2601 counties, each with up to 6 observation years.

Three sets of indicator variables are constructed to capture potential size effects, industry effects, and time effects.⁹ Different manufacturing sizes may have different behaviors, similar to Greenstone (2002), manufacturing size of each county in each survey year is classified into four categories based on the value of shipments: below the median, between the median and the 75th percentile, above the 75th percentile but below the mean, greater than mean. Table 1-2 presents summary statistics of total manufacturing shipments for each county, and there seems to be clear patterns as over time, and thus time dummies are created to capture time fixed effects. Additionally, to distinguish polluted industries from clean ones, this chapter uses SIC information and 12 polluting industries classified in Greenstone (2002) and List (2003) to generate 13 industry dummies, 12 for the polluted industries, one additional for the remaining clean industry. The

⁸ There are additional two pollutants that are reported in Greenbook: lead and NO₂, but very few counties are under non-attainment for those two, 58 and 24 counties separately. Besides, CAA, especially the 1970 amendments focused more on the high concentrations in CO, SO₂, O₃ and TSPs, so this chapter focuses on regulations on these four.

⁹ Including size and industry dummies could control for the entry-exit effects that may affect the estimates. Intuitively, firms may sort automatically into attainment counties to get rid of higher abatement costs, thus the manufacturing sizes and the structure of industries components may change over time. Controlling size and industry dummies could help eliminate some of the entry-exit effects. In addition, including 13 industry dummies could also distinguish polluted industries from clean ones, making the estimates of CAA effects more precise than treating them all as the same. Refer to Appendix B for more discussions about the entry-exit effect.

classification of 12 polluting industries is based on the emission reports from EPA, and these 12 industries emit different combination of air pollutants, which may cause them face different requirements under same regulatory status. These 12 industries (with SIC) are: Printing 2711-89, Organic Chemicals 2861-9, Rubber and Miscellaneous Plastic Products 30, Fabricated Metals 34, and Motor Vehicles, Bodies, and Parts 371, for emitting O_3 ; Inorganic Chemicals 2812-9 for emitting SO_2 ; Lumber and Wood Products 24 for emitting TSPs; Nonferrous Metals 333-4 for CO/SO_2 ; Petroleum Refining 2911 for $CO/SO_2/O_3$; Stone, Clay, Glass, and Concrete 32 for $SO_2 / O_3 /TSPs$; and Pulp and Paper 2611-31, Iron and Steel 3312-3 and 3321-5 for $CO/SO_2/O_3/TSPs$.

Since the data are collected every five years, while the designation of regulatory status is determined annually, this chapter follows Greenstone (2002) and assigns the attainment/non-attainment status for each five-year period based on the first year of the period. This is a reasonable approximation as technological development is likewise a slow process, and it takes time to have the status changed from one to another, especially when one county is under non-attainment status. The data show that the average time it takes to move from non-attainment to attainment status is about 14 years. One of the key variables, profitability, can be measured by return on sales (ROS) and level of profits from previous literature. Additionally, this chapter analyses value added, for which the data directly comes from Economic Census. Consistent findings are obtained from different measurements of profitability, which enhance the final results about the importance of feedback effects.

The industrial organization literature has identified capital intensity and market share as two important variables that affect a firm's profitability (Geroski et al., 1993; Machin and Reenen, 1993; Goddard et al., 2005). Capital intensity is calculated as the proportion of capital expenditures

divided by the total value of shipments. Market share is measured as the proportion of a county's shipments divided by total shipments of all counties. Table 1-3 contains descriptive statistics of key variables. As there are a large number of counties, the manufacturing market share is relatively small for each county. What is more, value added is greater than profit on average, because of the inclusion of inventories. Employment growth rate is calculated by natural log of current employment minus natural log of previous employment, which is commonly used as calculation of growth rate. On average, there is 0.52% decrease in employment growth from 1982 to 2007.

1.3.2 Estimation Framework

In order to address dynamic feedback effects, this chapter employs dynamic panel models. The omnibus econometric model specification I explore is given by:

$$[1.1] \quad y_{it} = \alpha_0 + \rho y_{it-1} + \theta \mathbf{z}_{it} + \beta \mathbf{x}_{it} + c_i + \gamma_t + v_{it}, (i = 1, \dots, N; t = 1, \dots, T)$$

Where y_{it} is the dependent variable; $\mathbf{z}_{it}$ are regulatory status indicators for four pollutants (PM, Ozone, CO, and SO₂); $\mathbf{x}_{it}$ is a vector of control variables, including size and industry indicators; the c_i are county fixed effects; the γ_t are time fixed effects; and v_{it} is a mean-zero error term.

Lagged dependent variables are unlikely to meet standard strict exogeneity assumptions, given correlation with unit fixed effects and, if there are serially correlated, the model error term. That is to say, even when one can ignore possible feedback/simultaneity between regulation and firms performance there is nevertheless an endogeneity concern and an instrumental variables approach is needed. A common approach is to use panel-style instruments (i.e. instruments that are time-period specific) based on lagged values and lagged first-differences of the (potentially) endogenous variable with a GMM estimation framework (for efficiency). This approach relies on the assumption that these lagged values are predetermined in the sense that they are uncorrelated

with the current and future period errors. Arellano and Bond (1991) provide an example of treating employment as predetermined and estimate the dynamic employment equations with GMM.

A predetermined variable by definition is not correlated with current or future values of the error term, but there may be some correlations between the lagged error and lagged dependent variable due to feedback effects. If y_{it-1} is considered predetermined, this means that $E(y_{t-s}v_{it}) = 0$ for $t = 2, \dots, T; s = 1, \dots, T-1$. Because of the feedback effects, variables capturing difference in regulation intensity are potentially predetermined as well. A county's regulatory status is determined by its pollution concentrations, which are determined by the manufacturing activities. Thus, the current regulatory status may have mechanical correlation with the lagged unobserved components of manufacturing activities (the error terms) or the lagged manufacturing activities (the dependent variables).¹⁰ Then for all $s \leq t$, $E(z_{is}v_{it}) = 0$, and this allows potential feedback effects on z_{it} from lagged v_{it} or lagged y_{it} .

In order to identify the panel dynamic model, a two-step system GMM estimators defined by available moments of the level and first-differenced errors is implemented. Comparing to difference GMM and one-step system GMM, two-step system GMM is more efficient and robust, since the optimal weighting matrices are corrected, so that the statistical tests are more reliable (Windmeijer, 2005; Roodman, 2013). Proper instruments for the predetermined variables are needed. Such instruments can be obtained within the model, more specifically, from the

¹⁰ There may be concerns about weather patterns and wind that will affect the pollution concentrations of a certain county, but it will not affect the predetermined property of the regulatory status. If we think about the regulatory status as a linear function of manufacturing activities, those factors will affect the coefficient of the function, but will not cut the correlation out unless the coefficient becomes zero. Previous literature only finds one county that does not have firms produce o-zone pollutants but with high-level of such pollutants. However, there are five more pollutants that are regulated by EPA, so the coefficient is hardly to be zero. So it is reasonable to treat the weather factors as unobservable errors, which will not dramatically change the predetermined regulatory status of a county into a non-predetermined variable.

predetermined variables themselves. A rich set of instruments includes either the lagged level predetermined variables based on first-difference equation, or the lagged first-difference predetermined variables based on level equation. Some of the instruments may not be valid if the error term is serially correlated, after dropping those invalid instruments, there are still rich enough instruments for estimation. This chapter carefully selects a set of instruments that is contingent on the tests of instruments validity. Related specification tests are: the Arellano-Bond test of autocorrelation, Hansen over-identification tests provide tests of the model specification. More specifically, in order to get rid of the first-order autocorrelation and over-identification problems, we make the instruments for predetermined variables start from the third lag when applying GMM dynamics. When estimating the GMM dynamic with feedback effects, in addition to the same set of instruments as GMM dynamics, all the regulatory indicators are treated as predetermined, so the GMM-type instruments starting with the first lag are also included. More details are given in Appendix C. Moreover, Hausman type test shed light on whether regulatory status is indeed endogenous or not. Baum et al. (2003) and Roodman (2013) provide details of these tests.

For comparison purposes, specifications characteristic of previous work in this area are included. Models 1 – 4 are estimated using standard fixed effects procedures. Model 1 and Model 2 are static models, with the difference being that the latter model includes time fixed effects. Model 3 and Model 4 add one-period lag of the dependent variable. Model 3 is close to the robustness check in Greenstone (2002). Models 5 – 8 are estimated using system GMM. Model 5 and Model 6 treat the one-period lag of the dependent variable as predetermined. Finally, Model 7 and Model 8 treat both the one-period lag of dependent variable as well as regulatory status as predetermined variables. Comparisons across models distinguish whether simple dynamics or dynamic feedback effects matter the most in identifying the regulatory impacts. I note that Lanoie

et al. (2008) include lags of regulatory status to see whether there is long-run effects of regulation. I also explored models with one-period lag of regulatory status and found that this has little effect on the results. In all cases, exclusion of this lag is statistically justified. This finding might be due to the fact that data is measured only every five years.

1.4 Results

1.4.1 Profitability

Three measurements of profitability are used in this chapter: ROS, profits, and value added. To obtain ROS, I need to know the profit margin first, follow Alpay et al. (2002), based on the data I have, the manufacturing profit of each county can be calculated as:

$$Profit_{it} = Shipments_{it} - Payroll_{it} - Materials_{it} - Capital_{it}$$

This is a reasonable measure of the total profits, since $Shipments_{it}$ provides information about the total revenues, while the other three variables of the left hand side provide information about the total costs of labor, raw materials, and capital expenditures. Then ROS is the proportion of profits over total value of shipments, specifically $ROS_{it} = Profit_{it}/Shipments_{it}$. Another measurement about the manufacturing performance is value added, which could be obtained directly from the data. The major difference between value added and level of profit is that value added is production based, while profit is sales based. So value added includes not only current products that are sold in the markets, but also those unsold as inventories. To control price inflation, values of variables in the data set are divided by the manufacturing PPI deflator provided by the U.S. Bureau of Labor Statistics (BLS), using 2005 as the base year.

Moreover, Conyon and Machin (1991) argue that labor markets' characteristics affect the profit margins in UK manufacturing, so the control variables also include the proportion of production workers over total employees of each county at each survey year. Since the labor characteristics variable may correlate with unobserved error terms, such as the manufacturing entrepreneurs' preferences or managerial abilities, in order to identify the model, production worker rate is treated as an endogenous variable.

Table 1-4 presents the estimated models using ROS, including only the current pollutant-specific regulatory status. The first four columns are results from fixed effects (FE) models:¹¹ column (1) and (2) do not include the first lag of ROS, while column (3) and (4) include one lag of it, which are similar to robustness check in Greenstone (2002). Except CO regulatory status, the other three pollutant-specific regulatory statuses do not have significant impacts on ROS from static or "exogenous" dynamic models, although the signs of the first two columns are consistent with findings from Rassier and Earnhart (2010): negative impacts from static models. Columns (5)-(6) are results from panel dynamic models, which is different from the robustness check models from Greenstone (2002) by treating the lag of ROS as predetermined. Simple panel dynamic models return similar results to the FE estimates, no significant impacts from environmental regulations on the ROS, except CO regulation. The last two columns are the results from dynamic feedback effects models, where the regulatory status is treated as predetermined as well, since it is correlated with previous pollution level. The results show that the CAA regulation, especially regulation on CO emissions, has statistically significant and positive impacts on ROS, with or without inclusion of time fixed effects. The model from column (8) is optimal, comparing to

¹¹ King and Lanox(2001) compare FE and RE, and find that FE could be more reliable, so this chapter applies FE as baseline estimates.

negative impacts from static estimates and simple dynamic models, the dynamic feedback effects model shows that non-attainment status in CO regulation increases ROS by 1.74%, which is about a \$33.48 million profit margin (2005 dollars).¹² The impacts from capital intensity, market share, and production worker rate are reasonable across models, although from dynamic models, the impacts of production worker are no longer significant. This may be a result of the first lag of ROS capturing most of the variation from history.

Table 1-5 presents results for profits, while Table 1-6 presents results for manufacturing value added. Similar results can be found, which enhance the fact that feedback effects are important in testing positive impacts from CAA. In Table 1-5, the static and simple dynamic models return significantly negative impacts from CAA regulations, especially regulations based on PM, Ozone, and CO. Even the GMM dynamic models suggest a negative although insignificant impacts on profits, except CO regulation. Allowing for feedback effects, the impacts of CO regulation on profits become positive and statistically significant. Based on Model 8, profits are increased about \$28.73 million (2005 dollars); in contrast the parallel static model, Model 2, suggests a decrease of \$67.14 million, solely from non-attainment under CO regulation. For manufacturing value added, the GMM dynamic feedback model implies an increase of \$8.01 million versus a \$40.52 million decrease estimated from the static model for CO regulation.

Taken together, the results from Table 1-4 to Table 1-6 suggest that estimates from simple dynamic models and static models are downward biased. The signs of the coefficients on pollutant-specific regulatory statuses change dramatically from negative (insignificant) in simple dynamic models to positive (significant) in dynamic feedback effects models, showing that the feedback

¹² 36.75 million dollars is obtained by multiplying the regulatory impacts by the average total value of shipments, which is $1.74\% \times 1.9243 \text{ billion} = 33.48 \text{ million}$.

effects do affect the estimates significantly. Intuitively, without considering feedback effects, treating the profitability as predetermined leaves the effect of $Status_{it}$ static. Thus it proves that current instantaneous effect of regulation on profitability is negative because of the increase in abatement costs. However, allowing feedback effects introduced by the nature of CAA regulations would take past regulatory effects into account. As a result, the effects on profitability turn to positive. Under this circumstance, it also lends some supports to the applicability of the data with every five-year frequency, since it takes time for both the technology and the status to change. Moreover, Hausman tests for the exogeneity of regulatory status reject the null, indicating that feedback effects exist in the data.

1.4.2 *Employment and Capital Expenditures*

Employment and capital expenditure are parts of the firms' decision making targets; moreover, to have a more broad idea about the environmental regulatory effects on different outcomes, this chapter analyzes these two variables as well. Similar to profitability equation [1.1], we have the following dynamic models for employment and capital expenditures:

$$[1.2] \quad Growth_emp_{it} = \alpha_0 + \rho Growth_emp_{it-1} + \theta status_{it} + \beta x_{it} + c_i + \gamma_t + v_{it}$$

$$[1.3] \quad Capital_{it} = \alpha_0 + \rho Capital_{it-1} + \theta status_{it} + \beta x_{it} + c_i + \gamma_t + v_{it}$$

Employment equation [1.2] has similar specification to the pure dynamics of employment without environmental regulations in Arellano and Bond (1991); control variables in x_{it} vector include payroll, capital expenditures, and size and industry dummies. Xepapadeas and Zeeuw (1999) indicates capital expenditures equation [1.3], since regulatory status will affect the capital expenditure decisions made by producers about whether to spend more on abatement or on R&D to improve the production efficiency. Results are presented in Table 1-7.

As shown in Table 1-7, the naïve models return negative estimates of environmental regulation impacts on employment growth and capital expenditures, only CO regulation is significant. Since regulations cause instantaneous increase in costs, and reductions in output, job loss will increase as well. Different from studying the overall employment growth, Walker (2012) adopts establishment-level longitudinal database to analyze the distributional consequences of environmental regulations, and finds that jobs are reallocated among firms in close related industry. Although negative effect on employment is found for regulated firms, such worker migration process may lend support to no significant effect at the economy wide level, because workers could move from lower efficiency firms to higher efficiency ones. For the results on capital expenditures, the pattern is similar, except that GMM dynamic with feedback effect model returns statistically significant and positive CO regulation impacts. Although CAA does not significantly affect employment growth, feedback effects do exist in the data, since signs of coefficients that with the most interests change from negative to positive when feedback effects are modeled. More formally, endogeneity tests suggest that regulation measures should not be treated as exogenous, which enhance the findings about regulatory effects on profitability.

1.5 Discussion

Using county-level manufacturing data, results from feedback dynamic panel models suggest that feedback effects are critical in testing whether more stringent environmental regulations, at least for CAA, will help firms improve the profitability. Porter and Van der Linde (1995) argue that such regulations "must create the maximum opportunity for innovation, leaving the approach to innovation to industry and not the standard-setting agency...the regulatory process should leave as little room as possible for uncertainty at every stage". To correct the present-biased

preferences about innovations, it is better for government to leave firms free to choose technologies via proper guidance than to set up unique technological standard. Government may consider about providing proper subsidies for R&D or for adopting greener technologies, at the same time when providing more stringent environmental regulations.¹³ In addition, patent policies about innovations on green technologies could act as complement to environmental regulations, and protect the enthusiasm of innovations in low-cost firms. Training programs for small and medium enterprises about the regulations and technologies could help them survive without wasting too many resources.¹⁴ Above all, policies that aim at encouraging and guiding profitable innovations will enhance the regulatory impacts of stringent environmental policies.

Several extensions of this chapter in the future could help understand the relationship between environmental regulations and economic performances better, thus policy recommendations could be more reliable. First of all, this chapter focuses on the effects of CAA on manufacturing performance. Extensions about CAA effects on other industries, or other environmental regulations (e.g. clean water regulations) on manufacturing performance using similar methodologies could help to draw a more general conclusion about whether environmental regulations could induce win-win situation for society. Since feedback effects exist in most of the dynamic procedures, the methodology employed in this chapter could be applied to other dynamic studies. Secondly, more detailed information could help identify the underlying mechanism of positive impacts from CAA on manufacturing firms performance. For example, the job-flow

¹³ So far, numerous states provide favorable tax treatment for the construction and installation of pollution control equipment. This kind of subsidy does not affect the empirical results in this chapter dramatically. Since the tax-benefit subsidy is taken as part of the decisions on capital expenditures. There may be innovation incentives due to subsidy, and such incentive effects may exceed the requirements of green technology. The feedback effects still perform important role in estimating the impacts.

¹⁴ See Lyon and Van Hoof (2009) and Rochon-Fabien and Lanoies (2010).

patterns could be tracked more precisely with longitudinal data using feedback panel dynamic models. Thus it could help to understand how dynamics in employment affect profitability under environmental regulation. Moreover, if different investments data are available, especially the R&D investment data, and if feedback effects dynamic models could return significantly positive effects from environmental regulation, the channels through which the Porter Hypothesis works could be identified more clearly. In addition, more detailed data could shed light on how environmental regulations affect the location decisions of firms, as well as the spatial spillovers.¹⁵ The last but not the least, studies about the optimal degree of stringency could get rid of coming to the hasty conclusion about implementing more and more stringent environmental regulations. Since this chapter only compares regulated to unregulated without more detailed distinguishes about the degrees of stringency, it might be the case that in some intervals of stringency, increasing stringency will increase profitability, while in some other intervals, increasing stringency decreases profitability even when feedback effects are considered. These are beyond the scope of this chapter, and could serve as future extensions under the same feedback effects framework.

¹⁵ See Millimet and Roy (2011) about the spatial endogeneity of environmental regulations.

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Appendix A: Tables

Table 1-1: Related Previous Empirical Studies

Reference	Data	Model	Results
Walker(2012)	Panel data: about 150,000 plants of U.S. over 10 years, with gaps	Triple-difference estimators on wages and employment <u>Controls</u> : county, industry, sector, nonattainment, year fixed effects, and their interactions	Negative effects on wages and employment
Lanoie et al. (2011)	Cross-section data: mailed survey, 4,200 manufacturing facilities in 7 OECD countries, 2003	IV-probit model <u>Controls</u> : technology standards, performance standards, country, sector, age, log(emp), etc	Negative effect on economic performance
Rassier and Earnhart(2010)	Panel data: 73 U.S. chemical firms, 1995-2001	Dependent variable: ROS <u>Controls</u> : capital intensity; age of assets; size; market share; industry concentration	Negative effect on ROS (Return on Sales)
Alpay et al. (2002)	Panel data: Mexican and U.S. processed food sectors, plant-level data from 1962 to 1994	Regress ln(profit) on ln(prices), regulation, time, and their squares and interactions.	U.S.: no effect Mexico: negative effect
Greenstone (2002)	Panel data: 1.75 million U.S. manufacturing plant observations over four census periods (1967-1987)	Dependent variables: plant growth of shipments, employment, and capital expenditure <u>Controls</u> : plant size, age, ownership, wages, industry indicators	Negative effects on all three dependent variables
King and Lenox(2001)	Panel data: 652 U.S. manufacturing firms over 1987-1996.	Dependent variable: Tobin's Q <u>Controls</u> : size, capital intensity, sales growth, leverage, R&D	No effect
Khanna and Damon (1999)	Panel data: 123 U.S. chemical firms over 1991-1993, participating in 33/50 program or not	Dependent variables: ROI, EV/S <u>Controls</u> : sales, capacity utilization, sales growth, age, innovativeness.	Negative effect on ROI Positive effect on EV/S
Brannlund et al. (1995)	Panel data: 41 Sweden pulp mills for the period 1986-1990	Estimation of regulated and unregulated profit using a non-parametric model of the technology for each mill <u>Control</u> : output, categories, size	No effect on most of the mills Average: negative effect

Table 1-2: Summary Statistics of Total Value of Shipments (in \$Millions)

Year	Observations	Min	Median	75 th percentile	Mean	Max	Std. Dev.
1982	2,586	0.31	208.92	713.63	1,130.48	134,719	4,346.08
1987	2,492	0.15	276.94	954.92	1,401.80	149,366.6	4,949.45
1992	2,595	0.26	310.84	1,048.31	1,424.31	132,357.5	4,748.21
1997	1,992	29.94	652.46	1,782.37	2,182.60	126,220.4	5,963.19
2002	1,814	41.06	709.40	1,917.94	2,269.19	121,923.3	5,740.96
2007	1,549	52.68	857.08	2,232.60	2,666.78	156,710.9	7,281.32
2012	1,945	0.27	578.11	1,606.44	2,011.54	156,240.1	6,209.90

Table 1-3: Descriptive Statistics of Key Variables

Variable	Observations	Mean	Std. Dev.	Min	Max	Units
Profit	12,877	0.5406	1.6744	-2.8983	45.7724	Billion dollars
Value added	12,878	0.9075	2.7808	-0.3864	76.1208	Billion dollars
Capital expenditures	13,010	0.0622	0.2028	0.0001	6.3414	Billion dollars
Total shipments	13,010	1.9576	5.8868	0.0003	156.7109	Billion dollars
Total payroll	13,002	0.3075	1.0172	0.0000	34.5641	Billion dollars
Costs of materials	12,880	1.0502	3.2795	0.0000	109.3977	Billion dollars
Employment growth rate	10,098	1.0434	2.4560	-2.2014	7.6733	Percentage
Capital intensity	13,010	0.0307	0.0218	0.0000	0.3996	Percentage
Market share	13,010	0.0005	0.0016	0.0000	0.0461	Percentage
Production worker rate	13,010	0.7573	0.1187	0.0000	5.7143	Percentage
ROS	12,877	0.2595	0.1128	-0.7321	0.9762	Percentage

Note: The full sample has 11120 observations. The number of observations reported here correspond to the estimation samples, which differs from 11120 due to missing values of some controls, e.g. production worker rate.

Table 1-4: Estimated Models for ROS

	Static ((1)-(2))		Simple Dynamic ((3)-(4))		GMM Dynamic ((5)-(6))		GMM Dynamic with Feedback Effects ((7)-(8))	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ROS_{it-1}	-	-	0.068*** (0.012)	0.034*** (0.012)	0.478** (0.190)	-0.278 (0.496)	0.461*** (0.075)	0.437*** (0.142)
Status_PM	0.023*** (0.005)	0.007 (0.005)	0.011** (0.005)	0.005 (0.005)	0.013 (0.016)	0.026 (0.024)	-0.002 (0.007)	0.002 (0.006)
Status_ozone	-0.014*** (0.004)	-0.003 (0.003)	-0.011** (0.004)	-0.008* (0.004)	-0.005 (0.019)	-0.044 (0.035)	-0.007 (0.009)	-0.007 (0.009)
Status_CO	-0.040*** (0.007)	-0.024*** (0.007)	-0.028*** (0.006)	-0.018*** (0.006)	-0.019 (0.022)	-0.074* (0.046)	0.019** (0.009)	0.015* (0.008)
Status_SO2	-0.015 (0.009)	-0.001 (0.009)	-0.018 (0.012)	-0.012 (0.011)	-0.020 (0.024)	-0.053 (0.036)	-0.013 (0.014)	-0.018 (0.017)
Cap_intensity	-1.285*** (0.048)	-1.230*** (0.057)	-1.121*** (0.050)	-1.117*** (0.050)	-1.179*** (0.077)	-1.129*** (0.094)	-1.166*** (0.066)	-1.208*** (0.073)
Market share	7.567* (3.944)	6.227** (3.139)	5.490*** (1.879)	4.403** (1.860)	3.976 (2.593)	3.843* (2.379)	10.245** (4.758)	5.037** (2.490)
Production worker rate	0.019 (0.012)	0.025* (0.014)	0.049*** (0.013)	0.046*** (0.012)	-0.398 (0.330)	-1.008 (0.671)	0.005 (0.077)	-0.057 (0.078)
# of Obs	12,877	12,877	8,778	8,778	8,778	8,778	8,778	8,778
# of counties	2,679	2,679	2,222	2,222	2,222	2,222	2,222	2,222
# of instruments	-	-	-	-	27	32	93	98
Arellano-Bond test	-	-	-	-	-0.73 (0.467)	-0.70 (0.482)	1.87 (0.061)	1.34 (0.180)
Hansen test for overid	-	-	-	-	2.44 (0.296)	0.56 (0.755)	64.87 (0.585)	80.89 (0.136)
Hausman test for exogeneity	-	-	-	-	46.26 (0.005)	49.19 (0.001)	95.38 (0.000)	87.09 (0.000)
Size dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** represents 1% significance level, ** represents 5% significance level, and * represents 10% significance level. Standard errors are reported in the parentheses. For test statistics, those in the parentheses are the p-values. Column (1)-(4) are estimated results from static FE models with or without dynamics, and with or without time fixed effects; column (5)-(6) are results from panel dynamic models using Arellano-Bond estimates, with or without time fixed effects; similarly, the last two columns reports the dynamic feedback effects models, with or without time fixed effects. Hausman tests for endogeneity of ROS_{it-1} in column (5) and (6) significantly reject the null of exogeneity, confirming that ROS_{it-1} is indeed endogenous. Hausman tests for endogeneity of both ROS_{it-1} and $Status_{it}$ in column (7) and (8) also significantly reject the null, confirming that feedback effects do exist, and will affect the estimates.

Table 1-5: Estimated Models for Profits

	Static (1)-(2)		Simple Dynamic (3)-(4)		GMM Dynamic (5)-(6)		GMM Dynamic with Feedback Effects (7)-(8)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Profit_{it-1}$	-	-	0.170*** (0.009)	0.167*** (0.009)	-0.409* (0.221)	-0.409** (0.206)	0.125* (0.074)	0.086 (0.073)
Status_PM	0.230*** (0.066)	0.200*** (0.071)	0.047 (0.030)	0.044 (0.031)	0.659 (0.417)	0.433 (0.340)	0.112 (0.089)	0.060 (0.077)
Status_ozone	-0.195*** (0.046)	-0.162*** (0.048)	-0.150*** (0.028)	-0.146*** (0.027)	-0.734 (0.448)	-0.758 (0.0476)	0.051 (0.068)	0.026 (0.066)
Status_CO	-0.634*** (0.082)	-0.604*** (0.106)	-0.316*** (0.038)	-0.316*** (0.038)	-1.326** (0.558)	-1.229** (0.548)	0.243** (0.118)	0.250** (0.120)
Status_SO2	-0.269** (0.122)	-0.229 (0.144)	-0.242*** (0.072)	-0.240*** (0.071)	-0.778* (0.486)	-0.770 (0.472)	-0.061 (0.187)	-0.060 (0.193)
Cap_intensity	-1.642*** (0.221)	-1.724*** (0.217)	-1.247*** (0.310)	-1.662*** (0.313)	-0.975 (0.2818)	-1.975** (0.817)	-1.275*** (0.214)	-1.415*** (0.257)
Market share	939.158*** (168.648)	932.511*** (160.694)	1,000.192*** (12.350)	996.874*** (12.294)	1,457.673*** (135.955)	1,455.99*** (134.859)	1,049.98*** (80.025)	1,063.067*** (93.753)
Production worker rate	0.247*** (0.094)	0.206** (0.084)	0.404*** (0.078)	0.331*** (0.078)	-7.588 (9.108)	-5.90 (7.149)	1.579** (0.657)	1.152 (0.847)
# of Obs	12,877	12,877	8,778	8,778	8,778	8,778	8,778	8,778
# of counties	2,679	2,679	2,222	2,222	2,222	2,222	2,222	2,222
# of instruments	-	-	-	-	27	32	93	98
Arellano- Bond test	-	-	-	-	0.19 (0.848)	0.71 (0.475)	1.33 (0.184)	1.27 (0.204)
Hansen test	-	-	-	-	8.77 (0.362)	4.83 (0.090)	48.56 (0.491)	45.47 (0.617)
for overid	-	-	-	-	35.92 (0.004)	40.45 (0.004)	111.03 (0.000)	71.66 (0.000)
Hausman test for exogeneity	-	-	-	-				
Size dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** represents 1% significance level, ** represents 5% significance level, and * represents 10% significance level. Standard errors are reported in the parentheses. For test statistics, those in the parentheses are the p-values. Column (1)-(4) are estimated results from static FE models with or without dynamics, and with or without time fixed effects; column (5)-(6) are results from panel dynamic models using Arellano-Bond estimates, with or without time fixed effects; similarly, the last two columns reports the dynamic feedback effects models, with or without time fixed effects. Hausman tests return similar results as in Table 1-4, supporting that the regulatory status is indeed endogenous.

Table 1-6: Estimated Models for Manufacturing Value Added

	Static (1)-(2))		Simple Dynamic (3)-(4))		GMM Dynamic (5)-(6))		GMM Dynamic with Feedback Effects (7)-(8))	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Valadd_{it-1}$	-	-	0.087*** (0.008)	0.084*** (0.008)	-1.884* (1.128)	-0.702 (0.891)	0.191** (0.077)	0.154** (0.076)
Status_PM	0.097 (0.078)	0.093 (0.070)	-0.079** (0.032)	-0.056* (0.032)	3.615 (2.721)	0.616 (1.526)	-0.005 (0.078)	-0.016 (0.078)
Status_ozone	-0.183*** (0.040)	-0.138*** (0.044)	-0.117*** (0.027)	-0.117*** (0.028)	-3.490 (3.025)	-1.244 (2.691)	0.118 (0.089)	-0.045 (0.066)
Status_CO	-0.261** (0.123)	-0.248** (0.125)	0.044 (0.039)	0.023 (0.040)	-4.390 (3.595)	-1.587 (3.128)	0.217* (0.130)	0.047* (0.028)
Status_SO2	-0.250* (0.139)	-0.209* (0.120)	-0.165** (0.073)	-0.171** (0.072)	-1.341 (1.753)	-0.759 (1.314)	-0.064 (0.150)	-0.112 (0.131)
Cap_intensity	0.071 (0.163)	-0.134 (0.214)	0.859*** (0.317)	0.220 (0.319)	3.935 (5.821)	-2.049 (5.647)	0.542** (0.237)	0.110 (0.217)
Market share	1,369.588*** (167.253)	1,362.826*** (176.898)	1,463.75*** (13.954)	1,461.37*** (13.846)	2,943.42*** (853.755)	2,166.94*** (686.705)	1,500.46*** (169.531)	1,508.99*** (168.72)
Production worker rate	0.058 (0.048)	-0.040 (0.053)	0.095 (0.073)	0.006 (0.073)	-132.222 (97.918)	-24.669 (63.699)	1.817** (0.897)	0.269 (0.664)
# of Obs	12,878	12,878	9,620	9,620	9,620	9,620	9,620	9,620
# of counties	2,679	2,679	2,389	2,389	2,389	2,389	2,389	2,389
# of instruments	-	-	-	-	27	32	93	98
Arellano-Bond test	-	-	-	-	-1.36 (0.173)	-0.33 (0.739)	1.09 (0.275)	0.99 (0.324)
Hansen test for overid	-	-	-	-	0.65 (0.721)	11.16 (0.193)	58.63 (0.163)	55.27 (0.250)
Hausman test for exogeneity	-	-	-	-	69.85 (0.0000)	3.03 (0.220)	91.36 (0.0000)	61.83 (0.0000)
Size dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** represents 1% significance level, ** represents 5% significance level, and * represents 10% significance level. Standard errors are reported in the parentheses. For test statistics, those in the parentheses are the p-values. Column (1)-(4) are estimated results from static FE models with or without dynamics, and with or without time fixed effects; column (5)-(6) are results from panel dynamic models using Arellano-Bond estimates, with or without time fixed effects; similarly, the last two columns reports the dynamic feedback effects models, with or without time fixed effects. Hausman tests are similar to previous tables.

Table 1-7: Pollutant-specific Regulatory Effects on Employment Growth and Capital Expenditure

	Static ((1)-(2))		Simple Dynamic ((3)-(4))		GMM Dynamic ((5)-(6))		GMM Dynamic with Feedback Effects ((7)-(8))	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Employment Growth								
Status_PM	-0.598*** (0.094)	-0.018 (0.013)	-0.513*** (0.127)	-0.009 (0.016)	-0.699 (6.57)	-0.037 (0.077)	1.862*** (0.475)	-0.061*** (0.019)
Status_ozone	-0.092 (0.082)	0.007 (0.012)	-0.094 (0.125)	-0.006 (0.015)	-7.800** (3.653)	-0.060 (0.087)	-1.705*** (0.622)	-0.033 (0.024)
Status_CO	-0.424*** (0.133)	-0.025 (0.016)	-0.375** (0.169)	-0.036* (0.021)	-2.002 (3.976)	-0.189 (0.133)	1.830** (0.743)	0.083*** (0.028)
Status_SO2	0.194 (0.241)	0.003 (0.021)	-0.307 (0.326)	0.015 (0.040)	-6.938** (3.121)	-0.003 (0.064)	-0.341 (1.608)	0.044 (0.041)
B. Capital Expenditure								
Status_PM	0.004 (0.007)	0.007 (0.007)	0.001 (0.005)	0.003 (0.006)	0.012 (0.035)	0.025 (0.037)	0.001 (0.009)	-0.001 (0.008)
Status_ozone	-0.017** (0.007)	-0.015* (0.009)	-0.025*** (0.005)	-0.025*** (0.005)	-0.080* (0.031)	-0.051** (0.024)	0.028** (0.012)	0.017* (0.011)
Status_CO	-0.027 (0.017)	-0.024* (0.013)	-0.044*** (0.007)	-0.039*** (0.007)	0.095 (0.094)	0.096 (0.1112)	0.052*** (0.020)	0.041** (0.018)
Status_SO2	-0.001 (0.013)	-0.000 (0.013)	-0.004 (0.013)	-0.001 (0.013)	-0.031 (0.039)	-0.041 (0.038)	-0.002 (0.035)	-0.004 (0.033)
Size dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *** represents 1% significance level, ** represents 5% significance level, and * represents 10% significance level. Standard errors are reported in the parentheses. Column (1)-(4) are estimated using FE, in which (1) and (2) are static models without one lag of dependent variables, while (3) and (4) are simple dynamic models by including one lag of the dependent variables.

Appendix B: Key Variable Definitions¹⁶

Total Value of Shipments (Billion 2005 Dollars) Includes the received or receivable net selling values, "Free on Board (FOB) plant (exclusive of freight and taxes), of all products shipped, both primary and secondary, as well as all miscellaneous receipts, such as receipts for contract work performed for others, installation and repair, sales of scrap, and sales of products bought and sold without further processing. Included are all items made by or for the establishments from material owned by it, whether sold, transferred to other plants of the same company, or shipped on consignment. The net selling value of products made in one plant on a contract basis from materials owned by another was reported by the plant providing the materials.

Employment (in 1000) This item includes all full-time and part-time employees on the payrolls of operating manufacturing establishments during any part of the pay period which included the 12th of the months specified on the report form. Included are all persons on paid sick leave, paid holidays, and paid vacations during these pay periods. Officers of corporations are included as employees; proprietors and partners of unincorporated firms are excluded. The "all employees" number is the average number of production workers plus the number of other employees in mid-March. The number of production workers is the average for the payroll periods including the 12th of March, May, August, and November.

Production Workers (in 1000) Included in this item are workers (up through the line-supervisor level) engaged in fabricating, processing, assembling, inspecting, receiving, storing, handling, packing, warehousing, shipping (but not delivering), maintenance, repair, janitorial and guard services, product development, auxiliary production for plant's own use (e.g., power plant), recordkeeping, and other services

¹⁶ Definitions are from Economic Census.

closely associated with these production operations at the establishment covered by the report. Employees above the working-supervisor level are excluded.

Payroll (Billion 2005 Dollars) This item includes the gross earnings of all employees on the payrolls of operating manufacturing establishments paid in the calendar year 1992. Respondents were told they could follow the definition of payrolls used for calculating the Federal withholding tax. It includes all forms of compensation, such as salaries, wages, commissions, dismissal pay, bonuses, vacation and sick leave pay, and compensation in kind, prior to such deductions as employees' Social Security contributions, withholding taxes, group insurance, union dues, and savings bonds. The total includes salaries of officers of corporations; it excludes payments to proprietors or partners of unincorporated concerns. Also excluded are payments to members of Armed Forces and pensioners carried on the active payrolls of manufacturing establishments.

Cost of Materials (Billion 2005 Dollars) This term refers to direct charges actually paid or payable for items consumed or put into production during the year, including freight charges and other direct charges incurred by the establishment in acquiring these materials. It includes the cost of materials or fuel consumed, whether purchased by the individual establishment from other companies, transferred to it from other establishments of the same company, or withdrawn from inventory during the year.

Value Added by Manufacture (Billion 2005 Dollars) This measure of manufacturing activity is derived by subtracting the cost of materials, supplies, containers, fuel, purchased electricity, and contract work from the value of shipments (products manufactured plus receipts for services rendered). The result of this calculation is adjusted by the addition of value added by merchandising operations (i.e., the difference between the sales value and the cost of merchandise

sold without further manufacture, processing, or assembly) plus the net change in finished goods and work-in-process between the beginning- and end-of-year inventories.

New and Used Capital Expenditures (Billion 2005 Dollars) For establishments in operation and any known plants under construction, manufacturers were asked to report their new expenditures for (1) permanent additions and major alterations to manufacturing establishments, and (2) machinery and equipment used for replacement and additions to plant capacity if they were of the type for which depreciation accounts were ordinarily maintained.

Appendix C: Entry-exit Effect

Firms' entry-exit effects will not change the results dramatically. First of all, the size dummies and industry dummies could capture such effects in some degrees. Secondly, the number of establishments (total or whose employees are more than 20) does not change dramatically in the data as shown in Table 1-8:

Table 1-8: Summary Statistics of Establishments

Variable	Description	Total Counties		Attainment Counties		Non-attainment Counties	
		Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
ESTAB_growth	Growth rate of the total number of establishments	-0.008	0.237	0.005	0.157	-0.011	0.249
EST20E_growth	Growth rate of the number of establishments who have more than 20 employees	-0.024	0.284	-0.029	0.225	-0.023	0.295
P_estab	Proportion of establishments having more than 20 employees over total number of establishments	0.329	0.130	0.325	0.095	0.330	0.136

Note: For each county, there are relatively large establishments with more than 20 employees, which are supposed to behave differently from relatively smaller ones. Results show that the numbers of both types of establishments in attainment counties or non-attainment counties do not change dramatically. The underlying dynamic changes of establishments need more detailed data.

Last but not the least, Table 1-9 provides some robustness checks by using average on profitability, showing that using average measurements does not change the general patterns of different estimates. The conclusions in this chapter still hold. Note that the average measurements may be problematic because of different types of firms in a certain county will be treated as the same. That is part of the reason why there are no impacts of regulatory statuses on average profits and value added. However, without distinguishing big firms from small ones by treating them as different departments of a huge company, "the manufacturing sector company", the use of total

county-level measurements will be more accurate as proxies to firm performances. And the estimates of the coefficient on regulatory status could be interpreted as the average total effects on all the manufacturing firms. With more detailed data, different effects on big firms and small firms will be captured more precisely, and could be extended in the future analysis.

Table 1-9: Estimated Models Based on Average Measurements of Profitability

	Static ((1)-(2))		Simple Dynamic ((3)-(4))		Panel Dynamic ((5)-(6))		Dynamic Feedback Effects ((7)-(8))	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Average ROS								
Status_PM	0.0232*** (0.0050)	0.0080 (0.0053)	0.0094* (0.0055)	0.0039 (0.0057)	-0.0355 (0.0258)	0.0037 (0.0273)	0.0078 (0.0087)	0.0111 (0.0086)
Status_ozone	-0.0118*** (0.0033)	-0.0003 (0.0031)	-0.0082* (0.0047)	-0.0057 (0.0046)	-0.0010 (0.0328)	0.0296 (0.0254)	-0.0092 (0.0094)	0.0042 (0.0090)
Status_CO	-0.0345*** (0.0055)	-0.0201*** (0.0046)	-0.0296*** (0.0061)	-0.0198*** (0.0061)	-0.0367* (0.0195)	-0.0136 (0.0159)	0.0255*** (0.0081)	0.0201** (0.0081)
Status_SO2	-0.0188** (0.0085)	-0.0042 (0.0085)	-0.0158 (0.0120)	-0.0094 (0.0118)	-0.0311 (0.0291)	-0.0322 (0.0283)	-0.0142 (0.0138)	-0.0091 (0.0152)
B. Average Profits								
Status_PM	0.0005 (0.0004)	-0.0002 (0.0002)	0.0004 (0.0003)	-0.0000 (0.0002)	-0.0002 (0.0002)	-0.0004 (0.0010)	-0.0011 (0.0002)	-0.0004 (0.0002)
Status_ozone	-0.0007*** (0.0002)	-0.0003** (0.0002)	-0.0005 (0.0006)	-0.0004 (0.0003)	-0.0009** (0.0005)	-0.0008 (0.0015)	0.0002 (0.0009)	0.0001 (0.0003)
Status_CO	-0.0005** (0.0002)	0.0001 (0.0002)	-0.0004* (0.0002)	-0.0003** (0.0001)	-0.0013*** (0.0004)	-0.0004 (0.0006)	0.0002* (0.0001)	0.0002* (0.0001)
Status_SO2	-0.0002 (0.0003)	0.0003 (0.0021)	0.0001 (0.0004)	-0.0002 (0.0004)	-0.0006 (0.0004)	-0.0007 (0.0021)	-0.0004 (0.0010)	-0.0003 (0.0004)
C. Average Value Added								
Status_PM	0.0005*** (0.0002)	-0.0001 (0.0002)	-0.0003 (0.0003)	-0.0001 (0.0001)	-0.0003 (0.0003)	-0.0016 (0.0008)	-0.0004 (0.0003)	-0.0003 (0.0002)
Status_ozone	-0.0008*** (0.0002)	-0.0003** (0.0002)	-0.0004*** (0.0002)	-0.0003** (0.0001)	-0.0010** (0.0005)	-0.0003 (0.0010)	0.0001 (0.0003)	0.0001 (0.0003)
Status_CO	-0.0005** (0.00002)	-0.0002 (0.0002)	-0.0003* (0.0002)	-0.0002* (0.0001)	-0.0015*** (0.0006)	-0.0001 (0.0014)	0.0001* (0.0000)	0.0003* (0.0002)
Status_SO2	-0.0002 (0.0003)	0.0004 (0.0004)	-0.0000 (0.0004)	-0.0002 (0.0004)	-0.0008 (0.0005)	-0.0005 (0.0011)	-0.0007 (0.0005)	-0.0007 (0.0007)
Size dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	No	Yes	No	Yes	No	Yes	No	Yes

Note: Estimated models are similar to those using total measurements, and * represents 10% significance level. Though some noises are present in FE and simple dynamic models, which may due to different behaviors of big establishments and small establishments since size is discussed as an important control variable in previous literature, the results present a similar pattern to the main results from Table 1-4 to Table 1-7: the dynamic feedback effects models return significantly positive regulatory impacts.

Appendix D: Selection of Instruments

In order to find proper instruments for dynamic panel model, one can either use the lagged level predetermined variables based on first-difference equation, or the lagged first-difference predetermined variables based on level equation. Based on the estimated model, the level equation is:

$$y_{it} = \alpha_0 + \rho y_{it-1} + \theta z_{it} + \beta x_{it} + c_i + \gamma_t + v_{it}, (i = 1, \dots, N; t = 1, \dots, T)$$

And differenced equation is:

$$\Delta y_{it} = \rho \Delta y_{it-1} + \theta \Delta z_{it} + \beta \Delta x_{it} + \Delta \gamma_t + \Delta v_{it}, (i = 1, \dots, N; t = 3, \dots, T)$$

$T=6$. Considering the level equation, there are 4 GMM-type instruments based on $y_{i,t-1}$:

$$E(\Delta y_{i,t-1} v_{it}) = 0 \text{ for } t = 3, 4, \dots, 6.$$

Since an intercept is included, there is an additional instrument included: $E(v_{it}) = 0$. Considering the first-differenced equation, there are 10 GMM-type instruments based on y_{it-1} :

$$E(y_{i,t-s} \Delta v_{it}) = 0 \text{ for } t = 3, \dots, 6 \text{ and } s = 2, \dots, t-1,$$

In addition, $Employment_{i,t}$ in x_{it} is predetermined and needs to be instrumented. Similar to $y_{i,t-1}$, there are 19 instruments available:

$$E(\Delta Employment_{i,t} v_{it}) = 0 \text{ for } t = 2, 4, \dots, 6.$$

$$E(Employment_{i,s} \Delta v_{it}) = 0 \text{ for } t = 3, \dots, 6 \text{ and } s = 1, \dots, t-1,$$

Finally, there are 26 standard instruments from the equation's assumed exogenous variables (considering the GMM dynamic without feedback effects, all four regulation indicators are exogenous, there are another 3 size indicators, 13 industry indicators, 5 year indicators, and capital intensity and market share). Overall, there are 61 instruments available when estimating the GMM dynamic model with time effects. However, including them all does not pass the

autocorrelation test nor the over-identification test, indicating that some of the instruments, especially the GMM-type instruments are not valid. So this chapter applies further lags of the predetermined variables to obtain the GMM-type instruments. For both $Employment_{i,t}$ and $y_{i,t-1}$, the GMM-type instruments start with the fourth lag, which will make a 5-instrument set for each of them. Thus for GMM dynamic without feedback effects but with time effects, there are 32 instruments in total; if no time effects, then 27 instruments in total. And in this case, the models pass all the specification tests. Considering GMM dynamic model with feedback effects where all the regulation indicators are predetermined as well, GMM-type instruments starting with the first lag are included. Thus there are 66 more instruments, making Model 7 have 93 instruments, and Model 8 have 98 instruments in total.

Chapter 2: New Evidence on the Impact of Environmental Regulation on Firm Mergers

Abstract:

This chapter estimates the impact of environmental regulation on firm merger and acquisition (M&A) activity in regulated and unregulated industries. Using a comprehensive firm-level panel dataset on U.S. manufacturing firms and a variety of estimation frameworks, this chapter provides new evidence suggesting that stringency of environmental regulation motivates mergers among regulated firms. Using two measures of environmental regulations, pollution abatement costs and expenditures (PACE) and exogenous variation in Clean Air Act (CAA) regulations, we find that increases in regulation are associated with increased probability of being acquired or acquiring a firm. A \$10 million increase in industry abatement expenditure will have positive impact of around 0.7%, on the probability of being an acquirer and 0.2% on being a target of M&A activity.

2.1 Introduction

Traditional empirical and theoretical analyses focused on the effects of environmental regulations suggest that firms can optimally respond to more stringent regulations by undertaking internal actions such as abatement efforts and technology adoption. However, especially for significant regulatory changes, the optimization can give rise to strategic responses across firms.¹⁷ One potentially important inter-firm strategic behavior is mergers and acquisitions (M&As, hereafter), which occur frequently during the last 100 years and has been referred to as a “least-cost means” of responding to industrial shocks (Mitchell and Mulherin, 1996). The focus of this

¹⁷ One example is the debate on whether environmental regulations have positive or negative impacts on firms’ profitability, or competitiveness, which is discussed in Chapter 1.

chapter is to empirically identify how industrial shocks, especially shocks due to environmental regulations, affect firm M&As.

There appears to be a consensus among theoretical studies that more stringent environmental regulations will induce more M&As. A number of theoretical articles in environmental economics study the relationship between mergers and environmental regulations such as pollution permit, pollution taxation, etc., by bringing pollution into traditional Cournot oligopoly competition models (Bencheikroun and Chaudhuri, 2007; Enrnart, Hoope, and Löschel, 2008; Hennessy and Roosen, 1999; Roy Chowdhury, 2006; Lambertini and Tampieri, 2012). They find that under environmental regulations, firms have incentives to merge, one of the important incentives is profitability, and mergers are in general more profitable.¹⁸ Viscusi, Harrington, and Vernon (2005) highlight four incentives to merge: (1) to obtain monopoly power; (2) to produce pecuniary economies, e.g. monetary saving on production costs¹⁹; (3) to have real economies, e.g. real resource saving to generate scale of economies; (4) and to reduce management inefficiencies. The theoretical impacts of mergers on pollution abated is either positive (Enrnart, Hoope, and Löschel, 2008) or ambiguous (Hennessy and Roosen, 1999).

Turning to empirics, the majority of the work on M&As appears in the finance literature, and does not focus on environmental regulations (Bergman, Jakobsson, and Razo, 2005; Gorton, Kahl, and Rosen, 2005; Harford, 2005; Stewart, Harris, Carleton, 1984). Many of these studies endeavor to identify factors that can affect decisions on M&As, such as firm size, stock market return, cash flow, etc., controlling for deregulation-induced merger waves (see Harford, 2005, and

¹⁸ This applies to most of the industries except eco-industry, which is an industry attempting to reduce waste and pollution. Canton, David, and Sinclair-Desgagné (2007) and Canton, Soubeyran, and Stahn (2008) find that environmental regulations have negative impacts on mergers and acquisitions in eco-industry.

¹⁹ Enrnart, Hoope, and Löschel (2008) find that after mergers and acquisitions, firms tend to purchase less pollution permits;

Viscusi, Harrington, and Vernon, 2005). The regulations studied include price controls and entry barriers stemming from antitrust regulations, which are typically analyzed in the context of specific industries.²⁰ With few exceptions, empirical analyses have not considered possible interactions between environmental regulations and merger behavior. For instance, Cook and Chapple (2000) find that the Environmental Protection Act in UK did increase the probability of a waste disposal firm to merge. One concern about this and other studies focused on environmental regulation (Berchicci, Dowell, and King, 2012) are that, unlike the broader literature, important financial control variables are absent thus giving rise to possible misspecification and incorrect inferences.

Through integrating insight from both the environmental economics and finance literatures, this chapter presents new evidence about the relationship between environmental regulations and firms M&As, focusing on the impact of Clean Air Act (CAA) on manufacturing firms. CAA provides rich variation of regulatory stringency by annually designating county whose pollution level exceeds the national ambient air quality standards as non-attainment county, and emitters in such county will be subject to more stringent regulation than those in attainment county. In order to measure CAA regulatory stringency, we adopt a measure generally used in literature: pollution abatement cost and expenditures (PACE); furthermore, we introduce a new measure by taking advantages of emissions and CAA pollution redefinitions. Though it might be problematic to use abatement cost to proxy environmental regulatory stringency, since such cost are correlated with firms' productivity and efficiency, Brunel and Levinson (2013) suggest that overall the abatement cost is sufficient for distinguishing the difference of environmental regulatory stringencies across industries. Redefinitions of regulations are based on the fact that CAA revised

²⁰ For example, starting from 1978, price controls on natural gas have been gradually removed until 1989.

the standards for pollutants over time, e.g. for Ozone, there are 1979 standard, 1997 standard, and 2008 standard. Each change of standard leads to the number of counties being regulated changes. If the number increases dramatically after standard redefinition, we can conclude that CAA regulation of such standard is overall more stringent.

We construct a comprehensive data set that capture both environmental and financial behaviors from a variety of data sources. In particular, we construct panel data of U.S. manufacturing firms from COMPUSTAT/CRSP that includes all the financial characteristics. Mergers and acquisition information can be obtained from Securities Data Corporation (SDC), which contains the date of announcement of a merger, and when the merger completed. Detailed target and acquirer information (name, SIC, company id, etc.) are available for each transaction. Pollution abatement cost and expenditures, one of the measures of CAA regulatory stringency can be found from Environmental Protection Agency (EPA)'s PACE surveys, which covers years from 1979 to 1994 annually. The information of the other measure of environmental regulatory stringency, redefinitions of regulations, can be found from EPA's Greenbook database.²¹ In order to track emitters and non-emitters for industries, we aggregate facility-level emission data from National Emissions Inventories (NEI) to be industry-level, and following similar procedure of Greenstone (2002) to classify emitters and non-emitters. Since NEI covers years 1990, 1996-2002, 2005, 2008, 2011, which is more recent than PACE data but no intersection, thus at the end, we have two analysis samples based on two different measures of regulatory stringency: PACE (1979-1994) and redefinitions (1990-2011, with gaps). By taking advantage of emissions and

²¹ EPA Greenbook provides county-level attainment/nonattainment designations starts from 1978 annually. Due to the structure of our data set, which is firm-level, we do not use attainment/nonattainment designation for counties as a measurement of regulatory stringency directly. The use of redefinitions enable us to adopt diff-in-diff analyses.

redefinitions information, we create a new measure via principal component analysis. We then adopt logit, Poisson, as well as diff-in-diff analyses.

Results in this chapter provide new evidence on the impact of environmental regulation on manufacturing firm M&A activities, suggesting that more stringent environmental regulation (CAA) will motivate manufacturing firms to conduct more M&As, and market value of M&A firms will increase. Logit models using PACE as measure of environmental regulations indicate that the marginal impact of CAA regulatory stringency on being acquirer is 0.7%, which means that at the mean of PACE, one-unit (10 million 1995 dollars in pollution abatement costs) increase in regulatory stringency will increase the probability of being an acquirer by 0.7%. Such marginal impacts are 0.2% and 0.3% on being target and being either acquirer or target. As for the M&As frequency, using both PACE and redefinitions as measures of environmental regulations return positive and significant results on target counts, such marginal impact using PACE is 29%, and about 6.0% using redefinitions.

2.2 Literature review

M&As have been studied for more than 40 years because of their distinct frequency and volume. Most of the M&As research is published in finance literature, only recently, environmental economics researchers become more interested in how environmental regulations would affect M&As activity, e.g. Hennessy and Roosen (1999), and Lambertini and Tampieri (2012) provide theoretical support that environmental regulations will motivate more M&As, but lack of reliable empirical studies, especially on U.S. manufacturing sector.

2.2.1. *General M&As researches in finance*

There is limited theoretical research on M&As in the finance literature. Despite general discussion on motives of M&As from Viscusi, Vernon, and Harrington (2005), the existing theories are to explain why mergers occur in waves, clustering of mergers, based on the observation of merger waves started from 1893. The neoclassical one argues that merger waves occur because firms react to economic disturbances (Jovanovic and Rousseau, 2001, 2002); the behavioral one suggests that stock valuations cause overvalued firms purchase undervalued firms (Shleifer and Vishny, 2003). Related empirical studies provide evidence for both theories, e.g. Harford (2005) supports neoclassical explanation, while Rhodes-Kropf, Robinson, and Viswanathan (2005) provide evidence consistent with behavioral theory. Besides, the remaining majority of empirical M&As research in finance literature looks more closely at firms M&As from two aspects in general: the effects of M&As on stock prices and shareholders' wealth and the determinants of conducting M&As (Golubov, Petmezas and Travlos, 2013).

The wealth effect of M&As on shareholders is of primary interests for finance scholars. Profusion of event study on value creation provide mixed results on shareholder wealth, stock prices of acquirer and target. For example, Andrade, Mitchell, and Stafford (2001) analyzes stock market response to M&As of publicly traded U.S. firms during 1973-1998 period, and finds that acquirers achieve negative to zero abnormal returns. However, Alexandridis, Petmezas and Travlos (2010) shows that acquirers do realize gains. As for the combined entity (acquirer and target), Mulherin and Boone (2000), Bhagat, Dong, Hirshleifer and Noah (2005) suggest positive abnormal returns, while Alexandridis, Mavrovitis and Travlos (2012) does not find superior combined gains. Some of the empirical studies also explore long-run returns of M&As. They either

find negative abnormal returns of acquirer over three years following an acquisition (Loderer and Martin, 1992), or no significant underperformance (Franks, Harris and Titman, 1991).

In order to understand and explain why there is no consensus finding on wealth effect of M&As, and since several firm characteristics affect acquisition returns, researchers spend lots of efforts to identify factors lead firms to conduct M&As. Commonly accepted firm characteristics that will potentially affect M&As probability in the literature are firm size (Gorton, Kahl and Rosen, 2009; Moeller, Schlingemann and Stulz, 2004), stock market return, market-to-book ratio, equity-to-asset ratio (Rau and Vermaelen, 1998), cash flow (Harford, 1999), asset growth rate, profitability, R&D intensity (Mitchell and Mulherin, 1996; Spiegel and Tookes, 2013), leverage, and productivity (Harford, 2005; Siegel and Simons, 2010), which covers most of the determinants of domestic M&As discussed in Golubov, Petmezas and Travlos (2013).²²

2.2.2. *Deregulatory impacts on M&As*

Besides the characteristics of M&As participants, policies like anti-trust deregulations will affect M&As activity as well. Intuitively, investors take anticipated regulatory intervention into account when considering a business combination (Aktas, Bodt and Roll, 2004), more specifically, deregulatory events will ease the monitoring pressure of M&A transactions, which motivates more M&As indirectly. Empirically, Harford (2005) shows that deregulatory event has significantly positive coefficient in predicting merger waves. The positive relationship between deregulation and M&As is also true for cross-border M&As, e.g. Boudier and Lochard (2013) finds that the

²² Due to measurement and sample differences, and inclusion of control variables, these paper find slightly different results. For example, the effect of firm size on the probability of being target is negative in Moeller, Schlingemann and Stulz (2004). however, Gorton, Kahl and Rosen (2009) suggests that larger target will be more attractive. In this paper, we include as many potential determinants as possible. If considering cross-border acquisitions, there are some more country-related factors that may affect M&As probability such as FDI, GDP growth, investor protection, trading volume, etc. (Brar, Giamouridis, and Liodakis, 2009; Breinlich, 2008; Rossi and Volpin, 2004).

impact of service deregulation in the target country on M&As is positive and large. Harford (2005) and Viscusi, Vernon and Harrington (2005) construct tables of major deregulatory events and corresponding industries affected. However, very few of the researches take economic regulations into consideration.

2.2.3. *Environmental regulation impacts on M&As*

Apart from economic regulations, environmental regulations are more often neglected in previous finance researches. However, from environmental economics perspective, environmental regulations are found to affect firm behaviors and performances significantly, most of which focuses on analyzing: (1) how environmental regulations affect firms' abatement and location decision, e.g. Holladay (2010); (2) the relationship between environmental regulatory stringency and firms' competitiveness, Jaffe, Peterson, Portney and Stavins (1995) and Shadbegian and Wolverton (2010) provide surveys of related literature.²³

A recently developed topic is about environmental regulatory impacts on M&A decisions, and several theoretical studies suggest that tighter environmental policy increases the incentives to merger, especially in polluting firms. Hennessy and Roosen (1999) adopts stochastic polluting setting to study the effects of uncertainty on emissions trading, and finds that permit management incentives may motivate merger, and merger is likely to increase production. Cournot models with pollution suggest similar results. Ehrnart, Hoppe and Loschel (2008) studies the tacit collusion induced by European Union emissions trading law, proposing that higher allowance price will induce higher firm profits, and "pooling" could trigger monopolization. Canton, David and Sinclair-Desgagne (2007) and Canton, Soubeyran and Stahn (2008) focus on pollution tax impacts

²³ Though there are different kinds of environmental regulations and industries, most of the empirical studies focus on Clean Air Act (CAA) impacts on manufacturing. Additionally, Brunel and Levinson (2013) provides comprehensive literature review on measurement of environmental regulatory stringency.

on M&As, and find positive impacts in polluting firm M&As but negative impacts in eco-industry, industry providing abatement techniques. Because more stringent environmental regulations lead more firms to abate pollution, providing higher demand in eco-industry, thus firms are less likely to cooperate with each other. Bencheikroun and Chaudhuri (2007) suggests that mergers are generally more profitable under higher emission tax. Furthermore, Lambertini and Tampieri (2012) argues that the higher the negative externality from pollution, the more likely that the horizontal mergers could be socially efficient.

Comparing to the development in theoretical studies on this topic, empirical analyses are far away from adequate. Cook and Chapple (2000), the only empirical paper explicitly does research on this topic, though find that the Environmental Protection Act in UK did increase the probability of a waste disposal firm to merge, neglect potentially important financial control variables as mentioned above. Berchicci, Dowell, and King (2012) implicitly considers the impacts of environmental regulation on M&As when studying how environmental capabilities affect M&As, suggesting significantly positive impacts yet without controlling financial variables like market-to-book ratio, stock market return, etc. By combining findings in both finance and environmental economics literature, our biggest contribution of this chapter is to provide relatively comprehensive and reliable empirical evidence on whether more stringent environmental regulation will motivate more M&As.

2.3. Data and methodology

2.3.1. Data

We merge two databases, SDC platinum from Thomson Reuters and COMPUSTAT/CRSP by Wharton Research Data Services, to build a firm-level panel of M&A transactions and financial

performance characteristics for publicly-traded firms in the U.S. manufacturing sector.²⁴ SDC platinum reports mergers and acquisitions announcement date, effective date, acquirer and target identification and their primary industry, deal status, etc. Though SDC Platinum provides some financial data for acquirer and target firms, they are incomplete in both data observations and data variables, e.g. common shares outstanding, comparing to COMPUSTAT/CRSP data set. Thus, we collect additional financial characteristics that could affect M&A decisions from COMPUSTAT/CRSP. To merge SDC Platinum data with COMPUSTAT/CRSP, we use historical CUSIP, the universally recognized identifier for financial instruments. Table 2-10 in Appendix B provides descriptions of the financial data used in our study.

When assess the relationship between M&A activities and environmental regulations, the key variable of interests is environmental regulatory stringency. We firstly adopt the traditional measure of environmental regulatory stringency, the Pollution Abatement Costs and Expenditures (PACE). PACE from United States Census Bureau provides statistics on abatement costs at 4-digit SIC industry level (manufacturing sector only) from 1978 to 1994, except 1987. Each company could sell multiple products, to assign a certain firm's 4-digit SIC code, COMPUSTAT/CRSP uses the SIC code of the product that has greater than 50% of total sales, if none of the products accounts for more than 50% of the total sales, then choose a more general SIC code, which could be a 3-digit or even 2-digit SIC code, but transferred into 4-digit by adding zero(s). In order to keep as many companies in COMPUSTAT/CRSP and SDC as possible, we break down the general SIC code into more specific 4-digit SIC code via either looking up into the corresponding NAICS code,

²⁴ SDC Platinum from Thomson Reuters collects detailed information on M&A transactions for both private and public companies. However, financial data on privately-traded firms are not available in COMPUSTAT/CRSP. Though data of publicly-traded firms in all industrial sectors can be found in both SDC and COMPUSTAT/CRSP, manufacturing sector firms dominants, and with the most environmental data available, so we restrict our focus to U.S. domestic and publicly-traded firms in manufacturing sector.

or searching for the company's detailed information about their products if NAICS is missing. At the end, the SIC codes are uniformed to be specific 4-digit ones, thus merging with PACE data will not drop firms unnecessarily. Thus the data is a firm-level panel from 1978 to 1994 containing information about publicly-traded companies in U.S. manufacturing sector. Since COMPUSTAT/CRSP data is deflated into 1995 dollars as default, we deflate PACE data into 1995 dollars using PPI.

Because PACE data ends in 1994, we employ a second measure of exposure to environmental regulation. We take advantage of industry-level polluting data from the National Emission Inventory (NEI) database and EPA's redefinitions of the Clean Air Act (CAA) on designating attainment and non-attainment status. First established in 1970, CAA has modified air quality standards for the six criteria pollutants several times: one-hour ozone (1979 standard), eight-hour ozone (1997 standard and 2008 standard), carbon monoxide (1971 standard), lead (1978 standard and 2008 standard), nitrogen dioxide (1971 standard), PM-10 (1987 standard), PM-2.5 (1997 standard and 2006 standard), and sulfur dioxide (1971 standard and 2010 standard). Counties that have ambient pollutant concentrations above the required standards will be designated as in non-attainment and be regulated. We use the EPA Green Book database to observe how these modifications affect the number of non-attainment counties, so that we can identify pre/post regulation indicator representing the overall regulatory stringency. Figure 2-5 in Appendix B illustrate how redefinitions of the standards for Ozone and PM increased the number of non-attainment counties significantly. Specifically, there is a sharp jump in the number of non-attainment counties in 2004 due to Ozone regulations and in 2005 due to PM regulations.²⁵

²⁵ Besides environmental regulation, there are some other economic regulations, especially those related to antitrust ones will directly affect firms M&A decisions. Following Harford (2005) and Viscusi et al. (2000), we control for major deregulations and related industries affected by those deregulations in Appendix Table 2-3. In this study, we

NEI provides detailed information about facility emission levels for criteria air pollutants, including CO, NH₃, NO_x, VOC, SO₂, and PM, for years 1990, 1996-2002, 2005, 2008, and 2011. We aggregate facility level emissions of an industry to represent the total pollution level of that industry, by different pollutants. And then use a 7% assignment rule similar to Greenstone (2002) to classify industries into chemical-specific polluting and non-polluting categories. Figures 2-1 – 2-4 in the Appendix B demonstrate the percentage pollution of 4-digit SIC industries for four pollutants across sample years. Very few industries emit more than 7% pollution, thus we also explore assignment rules at 2% and 3% to capture as much variation in regulation exposure as possible.

With polluters and redefinitions indicator variables, we can take advantage of diff-in-diff analyses. However, bringing those dummies into non-linear regression models makes the estimation difficult to converge, so we limit our difference-in-difference analyses to be linear. For the non-linear logit and Poisson regressions, similar to Harford (2005) of measuring the economic shock, we adopt Principal component analysis (PCA). PCA is used to convert a set of possibly correlated variables into a comprehensive index that could capture most of the variance of those variables. Suppose a data matrix $\mathbf{X}$ consists of data observations for a vector of variables (with d dimensions), each column is one variable. To utilize PCA, we need to compute the covariance matrix of the data matrix, and then compute the eigenvectors and corresponding eigenvalues, sort and choose k eigenvectors with the largest eigenvalues to form a $d \times k$ dimensional matrix $\mathbf{W}$, where every column represents an eigenvector, and then use $\mathbf{W}$ to transform the corresponding eigenvector. And then use the weights vector to map each row of $\mathbf{X}$ to a new subspace by

focus on those deregulations that affect manufacturing industries (SIC from 2011 to 3999), e.g. Staggers Rail Act in 1980 affected industry with SIC code 3743 (Railroad Equipment).

equation $\mathbf{Y} = \mathbf{W}^T \times \mathbf{X}$. Elements of $\mathbf{Y}$ represent principal component scores. Usually, the first principal component captures most of the information of $\mathbf{X}$. Thus, we create an environmental regulatory stringency index by extracting the first principal component from a set of variables that cover industry share of emissions, polluter/non polluter indicators for different pollutants, and pre/post regulation indicators.²⁶ Some descriptive statistics of key variables of analysis samples defined be measures of environmental regulatory stringency – PACE and redefinitions – are as following Table 2-1.²⁷ Table 2-2 provides key variables descriptions.

2.3.2. Empirical Methodology

By treating an M&A occurrence as a binary dependent variable, we undertake logit-class estimation:

$$[2.1] \quad P_{it} = \text{prob}(y_{it} = 1 | ER_{it}, \mathbf{x}_{it}) = \frac{1}{1 + \exp(-\beta_0 - \gamma ER_{it} - \mathbf{x}_{it}\boldsymbol{\beta})}$$

where y_{it} is an indicator that equals 1 if firm i at time t is involved in M&As. We are interested in the coefficient of environmental regulation stringency ER_{it} , which could be measured by abatement cost or regulatory index using principal component analysis. $\mathbf{x}_{it}$ is a vector of control variables including finance characteristics and deregulation events that could affect firms' M&A decisions. We use fixed effects estimator by explicitly modeling time-invariant firm fixed effects. More specifically, to eliminate the incidental parameters problem, we follow Chamberlain (1980)

²⁶ Principal components analysis (PCA) is a popular tool of reducing the dimensionality of a data set, while keeping most of the information. Besides Harford (2005), in M&A related studies, researchers tend to apply PCA to identify common variation in several different proxies measuring the same variable, e.g., Schimdt (2015) employs PCA to create index of monitoring needs and advising benefits. Earlier related examples are Choi and Philippatos (1984), Lubatkin, Schweiger, and Weber (1999), Nikolaev (2010), and Gu and Lev (2011). In this chapter, including too many dummies may lead the matrix to be singular, while PCA could help find a score to capture the major characteristics of the environmental characteristics matrix without worrying about non-concavity of estimating logit or Poisson models.

²⁷ More information considering about the structure of these two samples can be found in Table 2-12 and 2-13 in Appendix B.

conditional fixed effects estimator.²⁸ We provide both pooled estimator and conditional fixed effects estimator in results section. Partial effects of environmental regulatory stringency on the response probability $prob(y_{it} = 1|ER_{it}, \mathbf{x}_{it})$ is obtained from the partial derivative $\frac{\partial prob(y_{it} = 1|ER_{it}, \mathbf{x}_{it})}{\partial ER_{it}} = \frac{\gamma \exp(-\beta_0 - \gamma ER_{it} - \mathbf{x}_{it}\boldsymbol{\beta})}{[1 + \exp(-\beta_0 - \gamma ER_{it} - \mathbf{x}_{it}\boldsymbol{\beta})]^2}$, thus the sign of the partial effect is the same as the sign of γ . If we observe statistically positive estimates on γ , then we can conclude that more stringent environmental regulation will raise the probability of M&A transactions.

Firms occasionally conduct several M&A transactions in a single year, in fact, we observe more than 10 percent of the firms involving in M&As make more than two M&A transactions, and the highest number of M&As in the sample is 13. To identify environmental regulatory impacts on the number of transactions, rather than just whether any transaction occurred, we employ Poisson models as this is appropriate for events counts. The Poisson regression model specifies that y_{it} given $\mathbf{z}_{it} = (ER_{it}, \mathbf{x}_{it})$ is Poisson distributed with density

$$[2.2] \quad f(y_{it}|\mathbf{z}_{it}) = \frac{\exp(-\lambda_{it})\lambda_{it}^{y_{it}}}{y_{it}!}, \quad y_{it} = 0, 1, 2, \dots$$

Given the mean parameter follows $E(y_{it}|\mathbf{z}_{it}) = \lambda_{it} = \exp(\mathbf{z}_{it}'\boldsymbol{\theta})$, where $\boldsymbol{\theta} = (\gamma, \boldsymbol{\beta})$, the conditional mean parameter is multiplicative when considering firm-specific fixed effects, which is $E(y_{it}|\mathbf{z}_{it}, c_i) = \lambda_{it} = c_i \exp(\mathbf{z}_{it}'\boldsymbol{\beta}), i = 1, \dots, n, t = 1, \dots, T$. Similar to logit models, we can use pooled estimator assuming away heterogeneity, and fixed effects estimator when considering potential heterogeneity. The marginal effect of environmental regulatory stringency

²⁸ The incidental parameters problem is when using cross-sectional unit dummy variables to the logit specification, the estimates of coefficients will be biased unless the number of time periods is large. Katz (2001) use Monte Carlo experiments to compare unconditional and conditional maximum likelihood estimators of fixed-effects logit models, and find that when $T < 16$, the conditional estimator outperforms the unconditional estimator, having very small amount of bias.

is $\frac{\partial E(y_{it}|\mathbf{z}_{it})}{\partial ER_{it}} = \gamma \exp(\mathbf{z}_{it}'\boldsymbol{\beta})$, thus the sign of the marginal effect is determined by γ . For example, if γ is positive, then a one-unit change in ER_{it} is associated with an increase in the number of times being involved in M&As y_{it} , after controlling for the other regressors. Similar to Logit models, ER_{it} is measured by PACE and by regulatory index. Compared to binary logit models, Poisson models provide more information about firm M&A behaviors, identifying whether more stringent environmental regulation will motivate firms to engage in M&A activity more intensely.

Though linear probability models may have predicted values outside $[0, 1]$, the estimates are theoretically consistent and can provide general information about the trend of the coefficients. Assume the same underlying profit function as linear probability models of firm i at time t ; and let y_{it} be an indicator that equals 1 if firm i at time t is involved in a merger and acquisition, else 0. Since PACE contains continuous measure of regulatory stringency, then the probability of $y_{it} = 1$ would follow:

$$[2.3] \quad P_{it} = \text{prob}(y_{it} = 1 | ER_{it}, \mathbf{x}_{it}) = \beta_0 + \gamma PACE_{it} + \mathbf{x}_{it}\boldsymbol{\beta} + u_i + \varepsilon_{it}$$

We are interested in γ , the coefficient of environmental regulation measured by PACE. $\mathbf{x}_{it}$ is a vector of control variables that are posited to affect merger and acquisition decisions, refer to Table 2-2 for a detailed list.

As for the measure of regulatory stringency based on CAA redefinitions, we employ diff-in-diff linear probability analyses similar to [2.3] as following:

$$[2.4] \quad P_{it} = \text{prob}(y_{it} = 1 | ER_{it}, \mathbf{x}_{it}) = \beta_0 + \mathbf{x}_{it}\boldsymbol{\beta} + \boldsymbol{\theta}_1 \text{polluter}_{it} + \boldsymbol{\theta}_2 \text{redefinition}_{it} + \boldsymbol{\theta}_3 \text{polluter}_{it} * \text{redefinition}_{it} + \varepsilon_{it}$$

where **polluter**_{it} is a vector of pollutant-specific dummy indicating whether a firm is in a polluting industry or not (they are defined by either 7% rule or 3%, 2% rules, and only VOC, NOX,

PM10 and PM2.5 are included, since we care Ozone and PM); and *redefinition_{it}* is a vector of dummy indicating whether it is a year post redefinition of regulation that brings significantly more counties being regulated. We are interested in θ_3 , which is a measure of the regulatory impact.

We further look at how CAA affect market value of M&A firms and the value of M&A transactions. Market value is measured by market capitalization calculated as common shares outstanding multiplies stock price of fiscal year, and M&A transactions are reported by SDC. Thus the linear models follow:

$$[2.5] \quad y_{it} = \beta_0 + \gamma ER_{it} + x_{it}\beta + \varepsilon_{it}$$

Where y_{it} is market value, or M&A transactions; x_{it} is the vector of control variables; ER_{it} is PACE and regulatory index.

2.4. Results

Estimated logit and Poisson models using different measures of environmental regulations provide similar and consistent results, suggesting that more stringent environmental regulation will increase the probability of M&As extensively and intensively, especially the probability of being target firm.

2.4.1. General impacts on M&A activity

We estimated general impacts of CAA on U.S. manufacturing firms M&A activity via logit regressions. Table 2-3 presents the estimated results from Logit models using PACE as the measure of environmental regulations, in which the analysis sample has annual data from 1980 to 1994 (except 1987). We investigate firms' decision on being acquirer, target, and either of them, and denote the dummy dependent variable as acquirer, target, and M&A separately. Pooled logit and conditional fixed-effects logit estimators are reported, and the results demonstrate that

environmental regulation measured by PACE indeed has positive impacts on M&A activity. Considering about whether or not to buy another firm under environmental regulation, the estimated coefficient of PACE is 0.027 from fixed-effects logit model, which means that one-unit (10 million 1995 dollars) increase in abatement cost will increase the log odds of being acquirer (versus not being acquirer) by 0.027, holding all other factors constant. More specifically, according to the calculation of marginal impact based on fixed-effects logit model, such marginal impact of PACE on the probability of being an acquirer is 0.7% at the mean of PACE. Statistically positive impacts are also found when studying on whether firms choose to be a target or not, and whether to involve in M&A transactions, from both pooled logit estimator and fixed-effects logit estimator, and the magnitudes of marginal impacts are 0.2% and 0.3% separately (see Table 2-5, Panel a, column (1)).

Anti-trust deregulation does not significantly affect the acquirer decision making, but has significant negative impacts on probability of being acquired. Economic shocks seem to have little impact on M&A decision making, however, the market share does have significantly positive impacts, which means that firms with larger market share will behave more actively in M&A transactions. Coefficients of other control variables that are supposed to affect M&As decisions behave similarly to those found elsewhere in the literature (e.g. Harford, 2005).²⁹

The results are similar when using regulatory index based on information of NEI and CAA redefinitions. The analysis sample contains data of more recent years from 1990 to 2011, with gaps. Since Hausman tests of choosing between pooled logit and fixed-effects logit models suggest

²⁹ We estimate models that are similar to Harford (2005) by excluding environmental regulations from [2.1], and the results return similar impacts of financial characteristics on M&As, e.g., asset growth and leverage have positive impacts. See Appendix Table 2-5 for estimated logit and Poisson models for sample when use PACE as a measure of environmental regulations. Harford (2005) includes logit models only.

that fixed-effects logit model is preferable, Table 2-4 presents fixed-effects estimators only, and the first three columns are fixed-effects logit estimators, using 2% rule.³⁰ Regulation index is generated as described in data section using principal component analysis. Variables that are used to generate the regulation index are polluting dummy, percentage of pollutants level, and redefinition of CAA on Ozone and PM. Table 2-5 shows that the impacts of environmental regulation on acquirer, target, and M&A decisions are positive, though not statistically significant. Compared to results in Table 2-3, there are no significant impacts from other control variables, except asset growth, market share, and productivity.

Corresponding marginal impacts of environmental regulation based on different models and measures are reported in Table 2-5.

2.4.2. *Intensive impacts on M&A frequency*

Since firms may conduct multiple M&A transactions in certain years, we study further on how environmental regulation will affect the frequency of being involved in M&As. Table 2-6 presents the estimated Poisson models based on PACE measure, and the last two columns of Table 2-4 reports the estimated fixed-effects Poisson models based on regulatory index measure. Results suggest that environmental regulation will increase M&A frequency, both on the number of times being acquirer and the number of times being target. Thus from an intensive point of view, more stringent environmental regulation will motivate firms to seek for M&A transactions more actively.

³⁰ We also conduct random-effects logit regressions and random-effects Poisson regressions, Hausman tests of fixed-effects and random-effects also reject the null, and suggest that fixed-effects models are preferable. Results about using 7% and 3% polluter/non-polluter rules are presented in Table 2-15 and 2-16 in Appendix B. Results are consistent across different designation rules.

2.4.3. *Robustness Checks*

The results are robust to a variety of robustness checks. First of all, estimated linear probability models [2.3] and [2.4] provide evidence supporting that environmental regulations have positive impacts on the probability of being acquirer, target, and of being involved in M&As. More specifically, *ceteris paribus*, one-unit (10 million 1995 dollars) increase in abatement cost will increase the probability of a firm being acquirer by 0.2%, by 0.3% if a firm chooses to sell, and by 0.3% if a firm is involved in M&A activities. It also increases the market value of firms conducting M&As if PACE is higher. Ten million dollars increase in PACE will lead to about 31 million dollars increase in M&A firms' market value, and the value of M&A transactions will increase by 0.3 million dollars. Table 2-7 provides estimated results of linear models for both measures of environmental regulations.

When estimate difference-in-difference model [2.4] using CAA redefinitions as measure of environmental regulatory stringency, we vary the specifications by including variable represents percentage pollutants a 4-digit industry produces to total pollution of each pollutants or not, combining pollutants VOC and NOX into Ozone or not, and combining PM10 and PM2.5 into PM or not. Results in Table 2-7, Panel b show that there is no significant impact of CAA on M&As for most of the cases, but there is positive effect on M&A firms market value. Table 2-17 and 2-18 in Appendix B provide estimated linear models using 7% rule and 3% rule separately. The results are sensitive to polluter/non-polluter designation rules. More specifically, when the designation rule is 7%, more stringent of the regulation on VOC, manufacturing firms will conduct more M&As, and behave as acquirer more often; when the designation rule is 3%, such effect is only significant on the probability of being target. This might be because that the variation of polluters and non-polluters are not enough. Though the results vary because of different

designation rules, the estimates on M&A firms' market value are significantly positive for most of the cases, which indirectly proves that environmental regulation does have some impacts on M&As.

Since our measures of environmental regulatory stringency are at the industry-level, we aggregate our firm-level data into industry-level, and then employ similar analyses as we undertook using firm-level data.³¹ Table 2-8 presents similar results to previous results based on firm-level data. Results of linear models based on PACE measure suggest that more stringent environmental regulation will increase market value and M&A transactions of manufacturing industries. When dependent variables are binary indicator of whether an industry conducts M&A or not, the impacts of environmental regulation on M&As are positive but not significant. Non-significance is because M&As are among firms instead of industries, and industry-level binary indicator are over-generalized. Differing from binary dependent variables, Poisson regressions of industry-level acquirer/target counts on regulation and control variables are more reliable, and they return positive and significant coefficients of interests based on PACE measure. The corresponding marginal impacts of environmental regulation on industry-level acquirer and target counts are 0.060 and 0.045 when use PACE measure. When use regulatory index measure, such impact on the number of times being target is positive and statistically significant, and robust to polluter/non-polluter designation rules.

Moreover, when control variables of financial characteristics are excluded, and only environmental regulations, anti-trust deregulation, market share and productivity are included as

³¹ To aggregate firm-level data into industry-level data, all the level data of firms such as PACE, market value, transactions, capital expenditures, and sales, are summed. As for the ratio data, especially the financial ratios, such as market-to-book ratio, equity/asset ratio, and cash flow, we use the mean of all firms sharing the same four-digit SIC, weighted by firms' market shares, to represent the market-to-book ratio of that industry.

explanatory variables, linear models return similar results but with slightly higher magnitudes when other financial controls are included. And this is especially significant for logit models using PACE measure, e.g., the marginal impacts of environmental regulations on target is 0.007 when finance controls are excluded, which is about 3.5 times of such impacts when finance controls are included. However, marginal impacts on frequency of being target is smaller than when finance controls are included. When use regulatory index, such pattern exists but the differences are with much smaller magnitudes (refer to Panel c in Table 2-5). Thus, comparing to logit models without controlling for firms' finance characteristics, such positive impacts are lower in magnitudes, suggesting upward bias when neglecting financial control variables; while Poisson models without control variable induce downward bias. This gives us an idea that when firms are making one-shot decision on whether to conduct M&A transactions, financial performance seems to be more important than environmental regulations; however, when firms are considering about M&As repeatedly, environmental regulatory impacts tend to be growing.

Another issue is that there may be dynamic effects. To consider this, we include the lag of regulation and lag of dependent variables in the regressions. Table 2-9 provides results from dynamic models based on firm-level data. Linear models based on PACE measure bring both the first lag of PACE and the first lag of dependent variables, and then use Arellano-Bond methods by treating PACE and lag of dependent variables as endogenous. Results suggest positive impacts of environmental regulations on M&As. Since the coefficients on lagged dependent variables are not significant in most of the linear regressions, and because it is difficult to conduct Arellano-Bond in non-linear models, we include only lag of regulation in Logit/Poisson models, PACE measure returns similar results to Table 2-3, suggesting that the positive regulation impacts are not due to dynamics, and that the first lag of PACE has positive impacts on M&As as well. Results based on

regulatory index measure are similar to results in Table 2-4 except that the regulation impacts are significantly positive on M&A.

2.5. Conclusion

Prior to implementing environmental regulations, it is important to understand the ways in which firms are likely to respond. This chapter examines manufacturing firms' M&A activities under CAA regulation while controlling comprehensively for relevant environmental and financial characteristics. Results suggest that more stringent environmental regulations will motivate manufacturing firms to conduct more M&As, and that the market value of M&A firms will increase. Though difference-in-difference estimates based on redefinition measure varies slightly due to different polluter/non-polluter designation rules, the impacts of the CAA on the market value of M&A firms are consistently positive and significant, which supports the idea that firms will be motivated by more stringent environmental regulation indirectly. Different measurements of environmental regulatory stringency do not impact the results dramatically, especially when dependent variable is the number of times behaving as target during a certain year. This is true when we aggregate firm-level data to the industry-level, and when we allow for dynamics into the models.

Two potential extensions of this chapter include: (1) environmental regulatory impacts on demand side; and (2) environmental regulatory impacts on cross-border M&As. This chapter studies how supply side will react under environmental regulations through M&As. One potential result of more M&As generated monopoly power is that consumers will pay higher price, thus there might be consumer loss when environmental regulations become more stringent. To identify both supply-side and demand-side welfare can help understand how environmental regulations

will affect the social welfare. There are papers studying economic regulation impacts on international M&As, but it is not clear whether environmental regulations across countries play a role of motivating firms to conduct cross-border M&As. This will help to identify the global impacts of environmental regulations.

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Appendix A: Tables

Table 2-1: Descriptive statistics of analysis samples defined by environmental regulatory measures

(PACE measure and CAA Redefinitions measure)

Panel a: Summary statistics of key variables (PACE)				
Variable	Mean	Std. Dev.	Min	Max
Abatement Costs	9.242	24.205	0	322.964
Market share	0.11	0.22	0.00	1
Stock Market Return	0.18	2.20	-0.99	150.61
Market/book ratio	2.97	27.95	-1,480.26	1,985.29
Equity/assets ratio	0.48	0.48	-25.55	1.00
Asset growth	0.13	0.59	-0.92	22.77
R&D intensity	0.92	22.31	-10.01	1,639
Leverage	0.50	0.38	0.00	15.55
Labor productivity	123.69	112.41	-3,014.5	4,331.625
Cash flow	-1.27	28.77	-1,788.46	26.63
Profitability	0.06	0.30	-7.99	0.82
Panel b: Summary statistics of key variables (CAA Redefinitions)				
Variable	Mean	Std. Dev.	Min	Max
Regulation index	-0.28	1.83	-0.76	19.42
Market share	0.08	0.19	0.00	1
Stock Market Return	0.21	5.49	-1.00	427.57
Market/book ratio	3.50	69.54	-2,500	6,526.26
Equity/assets ratio	0.21	7.32	-666.90	1.00
Asset growth	0.18	1.85	-0.98	159.65
R&D intensity	4.79	127.65	-90.38	12,522.1
Leverage	0.74	7.17	0.00	667.47
Labor productivity	237.50	281.23	-5.5	8,422.50
Cash flow	-7.25	174.79	-14,675.3	116.03
Profitability	-0.12	1.51	-126.31	8.83

Note: The total sample when use PACE measure contains about 2,800 companies for each observed year, making the total company-year observation be 54,329. However, due to missing values of key variables, the final analysis sample contains 14,000 observations. Those missing values mainly come from COMPUSTAT/CRSP finance characteristics, which is generally due to older data and inactive firms, thus will not affect our estimates dramatically. The total sample when use CAA redefinitions measure contains 39,481 company-year observations. However, due to missing values of key variables, the final analysis sample contains 17,924 observations.

Table 2-2: Control Variables

Model Variable	Data Variable
Market share of the firm	<u>Market value of the firm: $cs_{ho} * prcc_f$</u> common shares outstanding*stock price of fiscal year, then divide it by total value of the industry in fiscal year
Stock market return	the increase in stock price <u>$prcc_f$</u>
Market/book ratio	<u>$prcc_f/bkvlps$</u> : stock price of fiscal year/book value per share
Equity/asset ratio	<u>ceq/at</u> : total common or ordinary equity/ total assets or <u>$(bkvlps/at) * cs_{ho}$</u> : (book value per share/total assets)*common shares outstanding
Cash flow	<u>$ebitda/sale$</u> : the ratio of earnings before interest, taxes, depreciation, and amortization to total sales
Asset growth rate	The rate of growth of total assets: <u>at</u>
Profitability	<u>$ebitda/at$</u> : the ratio of earnings before interest, taxes, depreciation, and amortization to total assets
R&D intensity	<u>$xrd/sale$</u> : R&D expenditure/total sales
leverage	<u>lt/at</u> : total liabilities/total assets
productivity	<u>$sale/emp$</u> : total sales/employees
Deregulation event dummy	Refer to Table 2-11 in Appendix B
Economic shock index	First principal component from seven economic shock variables: cash flow, asset turnover ($at/sale$), R&D (xrd), capital expenditures ($capx$), employee growth, productivity, and sales growth

Table 2-3: Logit models when use PACE measure

	acquirer		target		M&A	
	pooled	FE	pooled	FE	pooled	FE
PACE	0.001 (0.73)	0.027*** (4.31)	0.003** (2.21)	0.034*** (4.98)	0.001 (1.04)	0.034*** (5.31)
Deregulation	0.226 (0.79)	0.166 (0.45)	-1.402* (-1.95)	-1.306** (-2.09)	-0.094 (-0.32)	-0.112 (-0.33)
Economic shock	0.175* (1.93)	0.026 (0.46)	0.140** (1.99)	-0.015 (-0.28)	0.149* (1.85)	-0.029 (-0.54)
Market share	1.421*** (9.97)	1.525*** (3.58)	1.332*** (9.24)	0.573 (1.26)	1.436*** (10.04)	1.223*** (3.01)
Market/book	-0.000 (-0.48)	-0.000 (-0.01)	0.001 (1.26)	0.011*** (2.70)	0.000 (0.57)	0.005* (1.91)
Equity/asset	1.110** (2.06)	-0.882 (-1.09)	0.590*** (2.62)	1.508*** (2.94)	0.779*** (2.56)	1.040** (2.34)
R&D intensity	-0.005 (-0.35)	-0.001 (-0.15)	0.005** (2.39)	-0.000 (-0.10)	0.005** (1.97)	-0.000 (-0.01)
Leverage	1.185** (2.10)	-0.647 (-0.77)	1.057*** (3.60)	2.436*** (3.95)	1.088*** (3.17)	1.739*** (3.29)
Stock market return	-0.008 (-0.67)	-0.014 (-0.68)	0.011 (1.17)	0.026* (1.76)	0.010 (1.16)	0.031* (1.66)
Asset growth	0.405*** (5.33)	0.875*** (9.38)	-0.118 (-0.96)	-0.092 (-1.12)	0.252*** (4.58)	0.454*** (6.22)
Productivity	0.001** (2.49)	0.001** (2.17)	0.001*** (3.05)	0.004*** (6.04)	0.001*** (3.18)	0.004*** (6.00)
Intercept	-3.124*** (-5.72)	-	-3.100*** (-12.08)	-	-2.542*** (-7.95)	-
Obs	14,000	6,911	14,000	6,219	14,000	7,804

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. Comparing to pooled logit models, FE logit will drop observations where the dependent variables are all 0 or 1 across years, meaning no status change over time. Pooled logit models are reported here as a baseline, Hausman tests suggest that FE logit is preferable.

Table 2-4: Estimated models when use regulatory index based on information of NEI and CAA redefinitions (2% rule)

	Logit models (fixed effects)			Poisson models (fixed effects)	
	Acquirer	Target	M&A	Acquirer counts	Target counts
Regulation index	0.010 (0.23)	0.062 (1.39)	0.027 (0.68)	0.029 (1.12)	0.071** (2.32)
Deregulation	-0.321 (-1.05)	0.284 (0.88)	0.033 (0.13)	-0.255 (-1.32)	-0.108 (-0.43)
Economic shock	0.047 (0.86)	-0.015 (-0.48)	-0.039 (-1.35)	0.040** (2.36)	-0.007 (-0.36)
Market share	0.726* (1.83)	-0.791* (-1.83)	0.439 (1.14)	0.607*** (3.23)	-0.888*** (-3.32)
Market/book	-0.000 (-0.65)	-0.000 (-0.41)	-0.001 (-1.16)	-0.000 (-0.60)	-0.000 (-0.18)
Equity/asset	0.861 (1.50)	0.060 (0.29)	0.255 (0.77)	0.396 (0.97)	0.008 (0.04)
R&D intensity	0.000 (0.33)	0.000 (0.08)	-0.000 (-0.96)	0.000 (0.49)	0.000 (0.44)
Leverage	0.515 (0.87)	0.068 (0.33)	0.259 (0.79)	0.153 (0.36)	0.014 (0.08)
Stock market return	-0.033 (-1.50)	0.001 (0.16)	-0.014 (-1.11)	-0.021 (-1.39)	0.006 (1.04)
Asset growth	0.610*** (11.58)	-0.033 (-0.77)	0.384*** (9.14)	0.252*** (12.48)	-0.027 (-0.80)
Productivity	-0.000 (-0.60)	-0.001** (-2.13)	-0.000 (-0.92)	-0.000** (-2.13)	-0.000** (-2.04)
Obs	8,949	7,345	9,847	9,231	7,440

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses.

Table 2-5: Marginal effects of environmental regulation by estimated models and measures

Model	PACE (1)	Index (7%) (2)	Index (3%) (3)	Index (2%) (4)
Panel a: Firm-level data (control financial characteristics)				
FE-Logit (acquirer)	0.007*** (4.31)	-0.011 (-1.01)	-0.006 (-0.55)	0.002 (0.22)
FE-Logit (target)	0.002* (1.85)	0.015 (1.06)	0.012 (0.92)	0.015 (1.39)
FE-Logit (mergers)	0.003** (2.27)	-0.000 (-0.02)	0.000 (0.01)	0.007 (0.68)
FE-Poisson (acquirer)	0.044 (1.37)	0.002 (0.04)	0.028 (0.63)	0.040 (1.02)
FE-Poisson (target)	0.290** (1.99)	0.051** (2.20)	0.070** (2.25)	0.060** (2.19)
Panel b: Industry-level data (control financial characteristics)				
FE-Logit (acquirer)	0.001 (1.05)	0.012 (0.91)	0.001 (0.06)	0.007 (0.35)
FE-Logit (target)	0.001 (0.51)	0.006 (0.38)	-0.001 (-0.08)	0.026 (1.41)
FE-Logit (mergers)	0.001 (0.65)	0.038 (0.86)	0.023 (0.61)	0.065 (1.55)
FE-Poisson (acquirer)	0.060*** (2.82)	-0.001 (-0.16)	0.008 (1.16)	0.008 (1.11)
FE-Poisson (target)	0.045*** (2.64)	0.021** (2.21)	0.031** (2.00)	0.024* (1.67)
Panel c: Firm-level data (without financial characteristics)				
FE-Logit (acquirer)	0.006*** (4.39)	-0.012 (-0.94)	-0.006 (-0.53)	0.003 (0.32)
FE-Logit (target)	0.007*** (5.68)	0.014 (1.06)	0.012 (0.92)	0.015 (1.39)
FE-Logit (mergers)	0.007*** (5.89)	-0.000 (-0.02)	-0.000 (-0.02)	0.007 (0.67)
FE-Poisson (acquirer)	0.026*** (4.28)	0.004 (0.13)	0.021 (0.68)	0.030 (1.15)
FE-Poisson (target)	0.035*** (4.79)	0.050** (2.40)	0.070** (2.45)	0.060** (2.37)

Note: Measures of environmental regulations are marked by PACE and Index in the first row of the table. The marginal effects are calculated at the sample means of the explanatory variables. The prediction rule for FE-Logit models is to predict the probability of a positive outcome assuming that the fixed effect is zero. The prediction rule for FE-Poisson models is the number of events. Thus a 0.007 marginal effect from FE-Logit model is interpreted as: at the mean of environmental regulatory stringency, one-unit increase in regulatory stringency will increase the probability of changing the dependent variable from 0 to 1 by 0.7%. And a 0.290 marginal effect from FE-Poisson model represents that one unit increase in regulatory stringency will increase the counts of M&As by 0.290.

Table 2-6: Poisson models when use PACE measure

	Number of times being acquirer		Number of times being target	
	pooled	FE	pooled	FE
PACE	0.001 (1.25)	0.016*** (4.51)	0.005*** (6.21)	0.020*** (5.63)
Deregulation	-0.267 (-0.83)	-0.201 (-0.70)	-1.024 (-1.54)	-0.993** (-2.03)
Economic shock	0.133*** (7.25)	0.045** (2.34)	0.127*** (7.19)	0.018 (0.86)
Market share	1.346 *** (20.01)	1.058*** (4.23)	1.443*** (16.81)	0.253 (0.87)
Market/book	0.000 (0.25)	0.001 (0.23)	0.001* (1.77)	0.007*** (2.76)
Equity/asset	0.906*** (2.70)	0.411 (0.60)	1.085*** (2.78)	1.637*** (3.79)
R&D intensity	-0.014 (-0.69)	-0.000 (-0.00)	0.004*** (3.81)	0.001 (0.45)
Leverage	1.024*** (2.90)	0.618 (0.88)	1.387*** (3.39)	2.721*** (5.34)
Stock market return	0.001 (0.14)	-0.007 (-0.62)	0.004 (0.47)	0.014 (1.24)
Asset growth	0.162*** (8.31)	0.380*** (8.87)	-0.209 (-1.60)	-0.099 (-1.47)
Productivity	0.001*** (4.63)	0.001*** (3.34)	0.001*** (4.85)	0.002*** (6.97)
Intercept	-2.740*** (-8.00)	-	-3.292*** (-8.27)	-
Obs	14,000	7,003	14,000	6,269

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses.

Table 2-7: Linear models when use PACE measure

	Acquirer	Target	M&A	Market value	M&A transactions
Panel a: PACE Measure					
PACE	0.002*** (4.64)	0.003*** (6.32)	0.003*** (6.16)	31.0*** (3.59)	0.3 (1.08)
Panel b: Regulatory Index Measure					
With Percentage, Four Pollutants					
Inter_VOC	-0.068 (-1.38)	0.005 (0.11)	-0.045 (-0.86)	12808.94** (2.00)	-1173.01 (-1.37)
Inter_NOX	0.124 (1.35)	-0.065 (-0.82)	0.093 (0.94)	9394.666 (1.41)	819.807 (0.65)
Inter_PM10	-0.254* (-1.92)	0.120 (1.06)	-0.199 (-1.41)	11144.92 (1.55)	1786.433 (1.30)
Inter_PM25	0.110 (1.13)	-0.109 (-1.31)	0.052 (0.50)	-751.118 (-0.14)	-
Without Percentage, Four Pollutants					
Inter_VOC	-0.084* (-1.72)	-0.002 (-0.04)	-0.061 (-1.18)	7702.518 (1.14)	-1130.552 (-1.36)
Inter_NOX	0.088 (0.98)	-0.080 (-1.03)	0.054 (0.57)	-2368.109 (-0.34)	667.58 (0.54)
Inter_PM10	-0.180 (-1.42)	0.150 (1.37)	-0.127 (-0.94)	17187.97** (2.00)	1791.071 (1.39)
Inter_PM25	0.104 (1.08)	-0.109 (-1.32)	0.048 (0.47)	-3697.118 (-0.66)	-
Without Percentage, combine PM					
Inter_VOC	-0.091* (-1.87)	0.005 (0.13)	-0.064 (-1.24)	8640.993 (1.29)	-1130.552 (-1.36)
Inter_NOX	0.050 (0.59)	-0.041 (-0.56)	0.037 (0.41)	-462.644 (-0.06)	667.58 (0.54)
Inter_PM	-0.030 (-0.40)	-0.008 (-0.12)	-0.057 (-0.74)	9620.064 (1.56)	1791.071 (1.39)
Without Percentage, Ozone and PM					
Inter_Ozone	-0.067 (-1.52)	0.034 (1.10)	-0.063 (-1.36)	9183.563* (1.74)	56.753 (0.08)
Inter_PM	0.002 (0.03)	0.024 (0.59)	-0.018 (-0.34)	6830.128 (0.93)	1346.996 (1.01)
Without Percentage, all in one					
Inter_env	-0.047 (-1.54)	-0.029 (-1.10)	-0.058* (-1.78)	10487.21** (2.02)	964.226 (1.12)

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. PACE varies at industry level. The analysis sample covers year from 1980 to 1994, except 1987. Dependent variables Acquirer, Target, and M&A are binary variables; Market value is measured by market capitalization calculated by common shares outstanding multiplies stock price of fiscal year, with units as million 1995 dollars; M&A transactions' units are million 1995 dollars. Analysis sample when use CAA redefinitions measure covers manufacturing firms from year 1996 to 2002, and year 2005, 2008, and 2011.

Table 2-8: Estimated models using industry-level data for different measures of environmental regulations (PACE and regulatory index)

Regulations (PACE and Regulatory Index)							
	Acquirer	Target	PACE			Acquirer	Target
			M&A	Market Value	M&A transactions	Counts	Counts
Linear Models							
PACE	0.002* (1.80)	0.001 (0.64)	0.001 (0.81)	107.308*** (4.70)	2.125 (0.96)	-	-
Obs	2,214	2,214	2,214	1,548	1,192	-	-
Logit/Poisson Models							
PACE	0.008 (1.37)	0.003 (0.53)	0.003 (0.72)	-	-	0.011*** (4.39)	0.012*** (4.65)
Obs	1,977	1,997	1,884	-	-	2,133	2,078
	acquirer	target	Index (2% rule)			Acquirer	Target
			M&A	Market Value	M&A transactions	Counts	Counts
Logit/Poisson Models							
Regulation index	0.029 (0.35)	0.107 (1.42)	0.272 (1.60)	-	-	0.014 (1.13)	0.026* (1.80)
Obs	1,109	1,375	912			1,776	1,731

Note: The units of PACE is 10 million dollars. *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. Linear models based on NEI sample return similar pattern as using firm-level data, but none of the coefficients of interaction terms (coefficients of interests) is significant except when the dependent variable is market value, Ozone regulation has positive impacts, thus we do not report here. The full NEI industry-level sample contains 4,924 observations, PACE industry-level sample contains 7,724 observations. Due to missing values in key variables used in analyses, the analysis sample of NEI sample has 1,776 observations, while PACE sample has 2,214 observations.

Table 2-9: Estimated models using firm-level data for different measures of environmental regulations (PACE and regulatory index)

		PACE				Acquirer Counts	Target Counts
	Acquirer	Target	M&A	Market Value	M&A transactions		
Linear Models							
PACE	0.001* (1.89)	0.001* (1.91)	0.001** (2.06)	59.423*** (2.73)	-1.433 (-0.35)	-	-
Lag (1)	0.001 (1.38)	0.003*** (4.93)	0.003*** (3.95)	42.004* (1.94)	5.551 (1.39)	-	-
Obs	11,475	11,475	11,475	2,724	1,543	-	-
Logit/Poisson Models							
PACE	0.015 (1.61)	0.061 (1.13)	0.010* (1.71)	-	-	0.005 (1.05)	0.003 (0.82)
Lag (1)	0.006 (0.72)	0.048*** (5.34)	0.035*** (4.36)	-	-	0.010* (1.78)	0.022*** (4.47)
Obs	5,516	4,980	6,224	-	-	5,591	5,003
		Index (2% Rule)				Acquirer Counts	Target Counts
	acquirer	target	M&A	Market Value	M&A transactions		
Logit/Poisson Models							
Regu_index	0.045 (0.37)	0.132 (0.97)	0.344** (2.19)	-	-	-0.024 (-0.26)	0.055 (0.68)
Lag (3)	0.027 (0.21)	-0.029 (-0.21)	-0.080 (-0.54)	-	-	0.044 (0.47)	-0.093 (-1.10)
Obs	4,798	3,788	5,265	-	-	5,137	3,922

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. Linear models based on NEI sample return similar pattern as using firm-level data, but none of the coefficients of interaction terms (coefficients of interests) is significant, thus we do not report here.

Appendix B: Other Information

Tables and figures in the Appendix are to provide: (1) some background information about the data, deregulation events, structure of analysis samples, polluter/non-polluter related distributions, and CAA redefinitions; (2) some more details of estimates from models with pure financial control variables by excluding environmental regulations as a first step to see whether our data by itself behaves dramatically different from that used in finance literature; (3) estimated models based on other polluter/non-polluter assignment rules (7% and 3%).

a. Tables: background information

Firm-level financial characteristics can be found in COMPUSTAT/CRSP. COMPUSTAT/CRSP includes many variables from firms' balance sheets. Table 2-10 lists the data variables and corresponding descriptions that are used to generate our model control variables of firms' financial characteristics in Table 2-2, which are commonly adopted in M&A empirical literature.

Table 2-10: Data variables and related measurement of units:

Data variable	Description	Units
cusip	Company identifier	id
ebitda	Earnings before interests	millions
at	Total value of assets	millions
bkvlp	Book value per share	dollars
capx	Capital expenditures	millions
ceq	Common/ordinary equity-total	millions
csho	Common shares outstanding	millions
emp	Number of company workers	thousands
lt	Liabilities – total	millions
sale	Net sales	millions
xad	Advertising expense	millions
xrd	R&D expense	millions
Prcc_f	Price close, annual, fiscal year	dollars

Source: Wharton Research Data Services, <https://wrds-web.wharton.upenn.edu/wrds/>

Harford (2005) and Viscusi et. al. (2005) point out that anti-trust related deregulations will trigger merger waves – during when the probability of M&As is higher than the unconditional expected probability. Table 2-11 lists major economic deregulatory events from 1978 to 2007, and will be used to generate deregulation indicator by time and industry affected. For example, an industry with 4-digit SIC 3743 in 1980 would have a deregulation indicator equal 1. Deregulation indicator is used to control impacts from other regulations, and could capture some of the time and industrial effects.

Table 2-11: Major Economic Deregulatory Events: 1978-2007

year	Deregulatory event	Industry affected (4-digit SIC)
1978	Airline Deregulation Act	4512,4513,4522,4581;4729
	Natural Gas Policy Act	1311,1321,1389;4922,4923,4924
1979	Deregulation of satellite earth stations (FCC)	4899
	Urgent-mail exemption (Postal Service)	4311
1980	Motor Carrier Reform Act	4213,4214
	Household Goods Transportation Act	
	Staggers Rail Act	3743;4011,4013
	Depository Institutions Deregulation and Monetary Control Act	6021,6022
	International Air Transportation Competition Act	4512,4513,4522
	Deregulation of cable television (FCC)	3663,4833,4841
	Deregulation of customer premises equipment and enhanced services (FCC)	
1981	Decontrol of crude oil and refined petroleum products (executive order)	1311, 2911
	Deregulation of radio (FCC)	2517,3663, 4832, 5064, 5731
1982	Garn-St. Germain Depository Institutions Act	6021,6022,6082,6091,6099
	AT&T settlement	3663
1984	Cable television deregulation act	3663,4833,4841
	Shipping act	4412,4424,4432,4449,4481,4489
1987	Elimination of fairness doctrine (FCC)	3651
1989	Natural gas wellhead decontrol act of 1989	1311,1321,1389;4922,4923,4924
1991	Federal deposit insurance corporation improvement act	6021,6022,6082,6091,6099
1992	Cable television consumer protection and competition act	3663,4833,4841
	Energy policy act	1311,1321,1389;4922,4923,4924
	FERC order 636	3699,4911
1993	Elimination of state regulation of cellular telephone rates	3661,4812
	Negotiated rates act	4212,4213
1994	Trucking industry and regulatory reform act	4212,4213
	Interstate banking and branching efficiency act	6021,6022,6082,6091,6099
1995	Interstate commerce commission termination act	40-45
1996	Telecommunications act	3663,4812,4813,4822
	FERC order 888	3699,4911
1999	FERC order 2000	3699,4911
2000	FERC order 637	3699,4911
2005	Energy Policy Act 2005	3699,4911
2007	FERC order 890	3699,4911

Note: All the deregulation events and industries affected from 1978 to 1996 are summarized by Harford (2005) and Viscusi et. al. (2005). After 1996, most active deregulations are in energy, and information is obtained from Federal Energy Regulatory Commission (FERC).

After merging data from SDC, COMPUSTAT/CRSP, PACE and NEI, we first count the proportion of firms involved in mergers to all firms, and further among all M&As by each year, the number of M&As occurring within the same industry, the number of polluting acquirer, and polluting target. Moreover, the number of observations in analysis sample is different from total sample because of missing values, we test whether our analysis sample could be a representative random sample from population by comparing to the remaining sample. Table 2-12 provide those information for sample when use PACE measure, Table 2-13 provide such information for sample when use regulatory index measure. From these two tables, we can see that there are big enough variations in M&As for identification, and the analysis samples are approximately representative random samples, since the analysis samples are approximately identical to excluded samples.

Table 2-12: Descriptive statistics when use PACE measure

Panel a: Sample with full information for regression								
Year	Mergers		Same industry		Polluting acquirer		Polluting target	
	No	Yes	No	Yes	No	Yes	No	Yes
1980	547	4	4	0	2	0	3	0
1981	464	46	36	10	35	6	13	4
1982	764	105	75	30	83	8	43	3
1983	728	186	127	59	141	9	84	7
1984	740	265	151	114	194	21	137	11
1985	858	142	91	51	89	18	69	17
1986	821	189	116	73	132	16	92	15
1988	827	210	144	66	130	14	108	14
1989	769	271	161	110	174	18	159	17
1990	738	291	178	113	188	20	160	18
1991	751	279	178	101	172	19	146	18
1992	775	301	185	116	187	27	156	20
1993	1,055	394	240	154	254	23	193	23
1994	1,040	440	299	141	293	21	215	24

Panel b: Comparison of means for analysis sample and excluded sample					
Variables	Analysis Sample		Excluded Sample		ttest of means (p-value)
	mean	obs	mean	obs	
Market share	0.11	14,000	0.11	26,992	-1.40 (0.16)
Stock Market Return	0.18	14,000	0.31	21,363	1.03 (0.30)
Market/book ratio	2.97	14,000	2.41	26,806	-0.80 (0.43)
Equity/assets ratio	0.48	14,000	-0.25	35,903	-0.95 (0.34)
Asset growth	0.13	14,000	0.38	29,000	5.12 (0.00)
R&D intensity	0.92	14,000	1.17	17,072	0.62 (0.54)
Leverage	0.50	14,000	1.15	35,931	0.95 (0.34)
Labor productivity	123.69	14,000	127.42	31,643	0.60 (0.55)
Cash flow	-1.27	14,000	-1.10	34,930	0.47 (0.64)
Profitability	0.06	14,000	-0.03	35,670	-1.48 (0.14)

Note: Panel a provides description of the structure of M&As in analysis sample, especially the number of mergers, the number of mergers in same industry, the number of polluting acquirer and target. Whether an acquirer or a target is a polluter or not is based on NEI information using 2% assignment rule. Panel b compares the means of analysis sample and excluded sample. The null hypothesis of t-test in Panel b is that the means of the two samples are the same, tests results suggest that the analysis sample and excluded sample are statistically similar in terms of market share, stock market return, market/book ratio, equity/assets ratio, R&D intensity, leverage, labor productivity, cash flow, and profitability.

Table 2-13: Descriptive statistics when use regulatory index measure

Panel a: Sample with full information for regression								
Year	Mergers		Same industry		Polluting acquirer		Polluting target	
	No	Yes	No	Yes	No	Yes	No	Yes
1996	1,247	541	343	198	357	35	281	25
1997	1,398	559	329	230	384	35	277	32
1998	1,339	547	323	224	396	32	235	26
1999	1,296	532	304	228	380	33	232	32
2000	1,265	501	313	188	375	23	185	21
2001	1,365	433	275	158	310	19	185	16
2002	1,395	397	241	156	279	17	166	17
2005	1,420	413	256	157	304	11	164	14
2008	1,403	376	213	163	275	17	148	14
2011	1,198	299	179	120	228	19	92	11

Panel b: Comparison of means for analysis sample and excluded sample					
Variables	Analysis Sample		Excluded Sample		ttest of means (p-value)
	mean	obs	mean	obs	
Market share	0.08	17,924	0.07	15,567	-0.13 (0.90)
Stock Market Return	0.21	17,924	2.31	11,406	2.83 (0.00)
Market/book ratio	3.50	17,924	0.95	15,305	-2.48 (0.01)
Equity/assets ratio	0.21	17,924	-3.77	19,391	-2.60 (0.01)
Asset growth	0.18	17,924	4.62	14,221	3.74 (0.00)
R&D intensity	4.79	17,924	6.52	7,381	1.01 (0.31)
Leverage	0.74	17,924	4.51	19,411	2.59 (0.01)
Labor productivity	237.50	17,924	245.29	14,811	1.19 (0.23)
Cash flow	-7.25	17,924	-6.07	17,675	0.75 (0.45)
Profitability	-0.12	17,924	-1.30	19,168	-4.96 (0.00)

Note: Panel a provides description of the structure of M&As in analysis sample, especially the number of mergers, the number of mergers in same industry, the number of polluting acquirer and target. Whether an acquirer or a target is a polluter or not is based on NEI information using 2% assignment rule. Panel b compares the means of analysis sample and excluded sample. The null hypothesis of t-test in Panel b is that the means of the two samples are the same, tests results suggest that the analysis sample and excluded sample are statistically similar in terms of market share, R&D intensity, labor productivity and cash flow.

b. Tables: excluding environmental regulations

Table 2-14 provide estimated models that are similar to those generally used in finance literature focusing on M&As by excluding environmental regulations, in this case PACE, and analyzing the same sample when use PACE as a measure of environmental regulations. As shown in the table, results are very similar to Harford (2005), though magnitudes vary, signs are the same, especially for market share, leverage, and asset growth.

Table 2-14: Non-linear models without PACE as a measure of environmental regulations					
	Logit models (fixed effects)			Poisson models (fixed effects)	
	Acquirer	Target	M&A	Acquirer counts	Target counts
Deregulation	0.205 (0.55)	-1.243** (-1.99)	-0.052 (-0.15)	-0.177 (-0.62)	-0.954* (-1.95)
Economic shock	0.011 (0.20)	-0.018 (-0.35)	-0.036 (-0.69)	0.043** (2.24)	0.010 (0.45)
Market share	1.508*** (3.53)	0.581 (1.27)	1.221*** (3.00)	1.034*** (4.13)	0.260 (0.89)
Market/book	0.000 (0.07)	0.011*** (2.71)	0.005* (1.93)	0.001 (0.32)	0.007*** (2.73)
Equity/asset	-0.832 (-1.03)	1.578*** (3.06)	1.105** (2.48)	0.338 (0.50)	1.670*** (3.84)
R&D intensity	-0.001 (-0.18)	-0.000 (-0.10)	-0.000 (-0.06)	0.000 (0.01)	0.000 (0.32)
Leverage	-0.562 (-0.67)	2.570*** (4.15)	1.856*** (3.49)	0.595 (0.86)	2.769*** (5.41)
Stock market return	-0.014 (-0.68)	0.026* (1.76)	0.031* (1.65)	-0.008 (-0.67)	0.013 (1.19)
Asset growth	0.867*** (9.35)	-0.096 (-1.18)	0.450*** (6.18)	0.384*** (8.98)	-0.095 (-1.41)
Productivity	0.002*** (3.44)	0.005*** (7.55)	0.005*** (7.60)	0.001*** (4.91)	0.003*** (8.53)
Obs	6,911	6,219	7,804	7,003	6,269

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. The estimated models are the same as [2.1] for logit models, and [2.2] for Poisson models, except that environmental regulations are excluded, which leads to models close to those in finance literature when analyze M&As.

c. Tables: other robustness checks

When combining CAA redefinitions and pollution information as a measure of environmental regulations, we define polluter/non-polluter by setting a threshold of percentage emissions, those industries produce pollution above such threshold will be designated as polluter, vice versa. In order to identify whether the polluter/non-polluter assignment rule affects the estimates or not, we vary such rule to be 7%, which is used in Greenstone (2002), and 2%. Table 2-15 – 2-18 provide estimated results for both non-linear and linear models for each rule. More specifically, Table 2-15 and 2-16 are non-linear models, comparing to Table 2-4, the results are with similar pattern. Results of linear models using different rules in Table 2-17 and 2-18 present similar pattern to results in Panel b in Table 2-7.

Table 2-15: Estimated models when use regulatory index measure (7% rule)

	Logit models (fixed effects)			Poisson models (fixed effects)	
	Acquirer	Target	M&A	Acquirer counts	Target counts
Regulation index	-0.053 (-1.03)	0.059 (1.06)	-0.001 (-0.02)	0.001 (0.04)	0.059** (2.34)
Deregulation	-0.321 (-1.05)	0.285 (0.88)	0.033 (0.13)	-0.254 (-1.31)	-0.108 (-0.43)
Economic shock	0.046 (0.86)	-0.015 (-0.48)	-0.039 (-1.35)	0.040** (2.37)	-0.005 (-0.28)
Market share	0.723* (1.82)	-0.780* (-1.81)	0.444 (1.16)	0.608*** (3.24)	-0.870*** (-3.25)
Market/book	-0.000 (-0.65)	-0.000 (-0.41)	-0.001 (-1.16)	-0.000 (-0.60)	-0.000 (-0.17)
Equity/asset	0.860 (1.50)	0.061 (0.0.29)	0.256 (0.78)	0.391 (0.96)	0.009 (0.05)
R&D intensity	0.000 (0.33)	0.000 (0.08)	-0.000 (-0.96)	0.000 (0.49)	0.000 (0.45)
Leverage	0.511 (0.86)	0.069 (0.33)	0.260 (0.79)	0.146 (0.35)	0.016 (0.09)
Stock market return	-0.033 (-1.49)	0.001 (0.16)	-0.014 (-1.10)	-0.021 (-1.38)	0.006 (1.04)
Asset growth	0.611*** (11.58)	-0.034 (-0.78)	0.384*** (9.14)	0.252*** (12.48)	-0.028 (-0.82)
Productivity	-0.000 (-0.59)	-0.001** (2.12)	-0.000 (-0.92)	-0.000** (-2.15)	-0.000** (-2.41)
Obs	8,949	7,345	9,847	9,231	7,440

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. The results are based logit and Poisson models when use CAA redefinitions related regulatory index measure, and the polluter/non-polluter indicator is assigned using 7% rule, according to which, those industries who produce pollution higher than 7% of total emissions of all industries are treated as polluter, otherwise non-polluter. This is the rule that is used in Greenstone (2002).

Table 2-16: Estimated models when use regulatory index measure (3% rule)

	Logit models (fixed effects)			Poisson models (fixed effects)	
	Acquirer	Target	M&A	Acquirer counts	Target counts
Regulation index	-0.027 (-0.56)	0.048 (0.92)	0.000 (0.01)	0.020 (0.65)	0.083*** (2.40)
Deregulation	-0.320 (-1.05)	0.285 (0.88)	0.033 (0.13)	-0.254 (-1.31)	-0.108 (-0.44)
Economic shock	0.047 (0.87)	-0.151 (-0.49)	-0.039 (-1.35)	0.040** (2.36)	-0.001 (0.948)
Market share	0.725* (1.82)	-0.780* (-1.81)	0.444 (1.16)	0.608*** (3.24)	-0.007 (-0.37)
Market/book	-0.000 (-0.65)	-0.000 (-0.41)	-0.001 (-1.16)	-0.000 (-0.60)	-0.871*** (-3.26)
Equity/asset	0.861 (1.50)	0.060 (0.29)	0.256 (0.78)	0.392 (0.97)	-0.000 (-0.18)
R&D intensity	0.000 (0.34)	0.000 (0.08)	-0.000 (-0.96)	0.000 (0.49)	0.008 (0.04)
Leverage	0.513 (0.86)	0.068 (0.33)	0.260 (0.76)	0.149 (0.35)	0.015 (0.08)
Stock market return	-0.033 (-1.49)	0.001 (0.16)	-0.014 (-1.10)	-0.021 (-1.39)	0.006 (1.04)
Asset growth	0.611*** (11.58)	-0.033 (-0.78)	0.384*** (9.14)	0.252*** (12.48)	-0.027 (-0.81)
Productivity	-0.000 (-0.60)	-0.001** (-2.12)	-0.000 (-0.92)	-0.000** (2.15)	-0.000** (-2.15)
Obs	8,949	7,345	9,847	9,231	7,440

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. The results are based logit and Poisson models when use CAA redefinitions related regulatory index measure, and the polluter/non-polluter indicator is assigned using 3% rule, according to which, those industries who produce pollution higher than 3% of total emissions of all industries are treated as polluter, otherwise non-polluter.

Table 2-17: Linear models when use regulatory index measure (7% rule)

	Acquirer	Target	M&A	Market Value	M&A transactions
With Percentage, Four Pollutants					
Inter_VOC	0.324** (2.29)	0.140 (1.15)	0.332** (2.20)	75127.06* (1.76)	7904.333 (0.84)
Inter_NOX	-0.068 (-0.85)	0.004 (0.05)	-0.124 (-1.46)	47571.77*** (3.16)	-377.112 (-0.08)
Inter_PM10	0.027 (0.20)	-0.133 (-1.14)	-0.104 (-0.72)	46694.96 (0.89)	-5153.41 (-0.73)
Inter_PM25	-0.232* (-1.95)	-0.106 (-1.04)	-0.180 (-1.42)	-47812.09 (-0.91)	-
Without Percentage, Four Pollutants					
Inter_VOC	0.234* (1.78)	0.072 (0.64)	0.232* (1.66)	85801.1** (2.12)	7802.98 (0.84)
Inter_NOX	-0.051 (-0.78)	0.012 (0.21)	-0.086 (-1.24)	26773.07* (1.67)	180.173 (0.03)
Inter_PM10	0.052 (0.39)	-0.113 (-0.97)	-0.069 (-0.48)	55867.2 (1.06)	-5547.296 (-0.79)
Inter_PM25	-0.163 (-1.48)	-0.078 (-0.82)	0.136 (-1.15)	-63155.09 (-1.19)	-
With Percentage, combine PM					
Inter_VOC	0.218* (1.74)	0.104 (0.96)	0.263** (1.97)	71611.2** (2.07)	3869.33 (0.56)
Inter_NOX	-0.055 (-0.85)	0.020 (0.35)	-0.080 (-1.15)	22947.46 (1.47)	733.879 (0.15)
Inter_PM	-0.132 (-1.55)	-0.143* (-1.95)	-0.181** (-1.99)	-13203.78*** (-2.87)	-
Without Percentage, Ozone and PM					
Inter_Ozone	-0.006 (-0.11)	0.030 (0.60)	-0.024 (-0.38)	12148.08 (0.78)	2520.306 (0.61)
Inter_PM	-0.069 (-0.91)	-0.100 (-1.55)	-0.101 (-1.26)	73154.31** (2.31)	63.743 (0.01)
Without Percentage, all in one					
Inter_env	-0.050 (-1.05)	-0.030 (-0.74)	-0.082 (-1.62)	35706.1** (2.46)	2584.098 (1.01)

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. The results are based logit and Poisson models when use CAA redefinitions related regulatory index measure, and the polluter/non-polluter indicator is assigned using 7% rule, according to which, those industries who produce pollution higher than 7% of total emissions of all industries are treated as polluter, otherwise non-polluter. This is the rule that is used in Greenstone (2002). Market value is measured by market capitalization calculated by common shares outstanding multiplies stock price of fiscal year. For models on acquirer, target and M&A, the number of observations in regressions is 17,924, while for market value, it is 4,598, and for M&A transactions, it is 2,843.

Table 2-18: Linear models when use regulatory index measure (3% rule)

	Acquirer	Target	M&A	Market Value	M&A transactions
With Percentage, Four Pollutants					
Inter_VOC	-0.059 (-0.99)	0.089** (1.75)	-0.058 (-0.92)	19060.01*** (2.84)	-3597.744 (-1.27)
Inter_NOX	0.100 (0.97)	-0.036 (-0.41)	0.159 (1.45)	8354.274 (1.50)	-766.138 (-0.80)
Inter_PM10	-0.951*** (-2.58)	0.235 (0.74)	-0.5745 (-1.46)	20973.24 (1.44)	6927.122*** (2.71)
Inter_PM25	0.782** (2.18)	-0.297 (-0.96)	0.322 (0.84)	9245.38** (2.36)	-31.386 (-0.11)
Without Percentage, Four Pollutants					
Inter_VOC	-0.050 (-0.85)	0.083* (1.67)	-0.048 (-0.77)	10223.42 (1.24)	-3297.934 (-1.26)
Inter_NOX	0.065 (0.65)	-0.024 (-0.27)	0.130 (1.21)	222.854 (0.04)	-967.304** (-2.29)
Inter_PM10	-0.918** (-2.50)	0.223 (0.71)	-0.545 (-1.39)	13060.36 (0.81)	6778.235** (2.55)
Inter_PM25	0.812** (2.26)	-0.297 (-0.96)	0.344 (0.90)	16088.98*** (4.58)	58.259 (0.62)
With Percentage, combine PM					
Inter_VOC	-0.052 (-0.89)	0.082* (1.64)	-0.052 (-0.83)	12900.22 (1.55)	-1038.817 (-0.37)
Inter_NOX	0.029 (0.30)	-0.033 (-0.40)	0.081 (0.79)	2807.6 (0.65)	-998.025** (-2.49)
Inter_PM	-0.061 (-0.62)	-0.063 (-0.75)	-0.141 (-1.36)	21309.71* (1.66)	4396.905 (1.59)
Without Percentage, Ozone and PM					
Inter_Ozone	-0.104* (-1.85)	0.038 (0.79)	-0.074 (-1.24)	10673.77** (1.98)	-1124.149 (-1.02)
Inter_PM	0.036 (0.52)	-0.073 (-1.23)	-0.022 (-0.29)	25929.87* (1.89)	3661.708* (1.76)
Without Percentage, all in one					
Inter_env	-0.082*** (-2.30)	-0.003 (-0.08)	-0.089** (-2.33)	20120.42** (2.52)	1730.776 (1.19)

Note: *, **, and *** represent 10%, 5%, and 1% significance level. Corresponding test statistics are in parentheses. The results are based logit and Poisson models when use CAA redefinitions related regulatory index measure, and the polluter/non-polluter indicator is assigned using 7% rule, according to which, those industries who produce pollution higher than 7% of total emissions of all industries are treated as polluter, otherwise non-polluter. Market value is measured by market capitalization calculated by common shares outstanding multiplies stock price of fiscal year. For models on acquirer, target and M&A, the number of observations in regressions is 17,924, while for market value, it is 4,598, and for M&A transactions, it is 2,843.

d. Figures: background information

Figure 2-1 – 2-4 are based on data of percentages of 4-digit SIC industrial emissions to total emissions that are used to designate polluter/non-polluter industries for four pollutants by each year marked at the top of each sub-graph: NOX, VOC, PM-10 and PM-2.5. As these four figures present, very few industries emit more than 7% pollution, thus we also explore polluter/non-polluter assignment rules at 2% and 3% to capture as much variation in regulation exposure as possible. Data source is NEI. Figure 2-5 provides information about the overall impacts of CAA redefinitions. Each sub-graph represents the trend of the number of non-attainment counties for one specific pollutant marked at the top. Each line in the graph represents a specific redefinition of pollutants standard. For example, the red line in Ozone graph is for the trend of the number of non-attainment counties under 1997 Ozone pollution standard. And as we see from the graph, there is a sharp jump at 2004, thus we can conclude that post 2004, the Ozone regulation is overall more stringent. And this is at 2005 for PM.

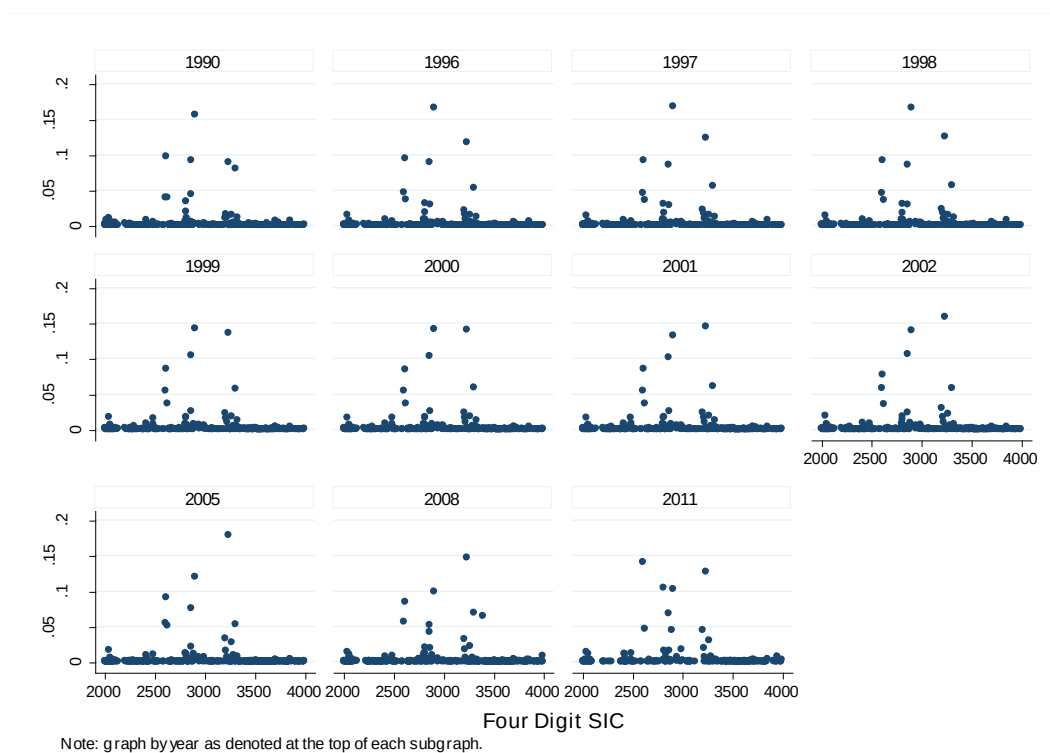


Figure 2-1: Scatter Plot of Percentage 4-digit SIC Polluters for NOX



Figure 2-2: Scatter Plot of Percentage 4-digit SIC Polluters for VOC

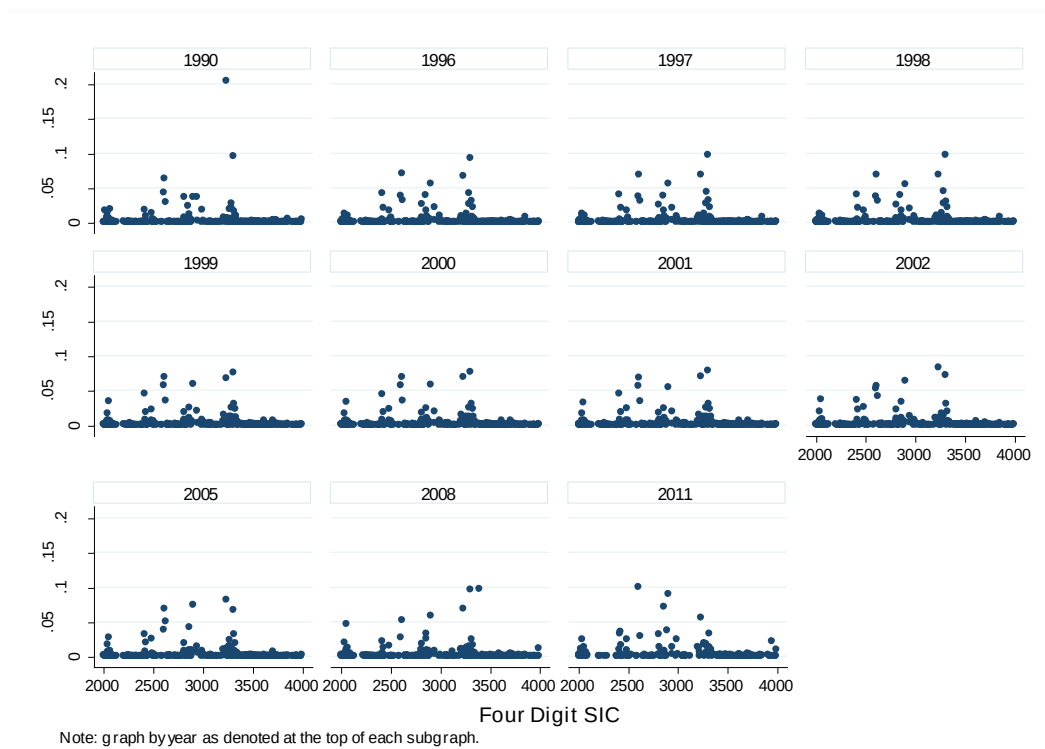


Figure 2-3: Scatter Plot of Percentage 4-digit SIC Polluters for PM-10

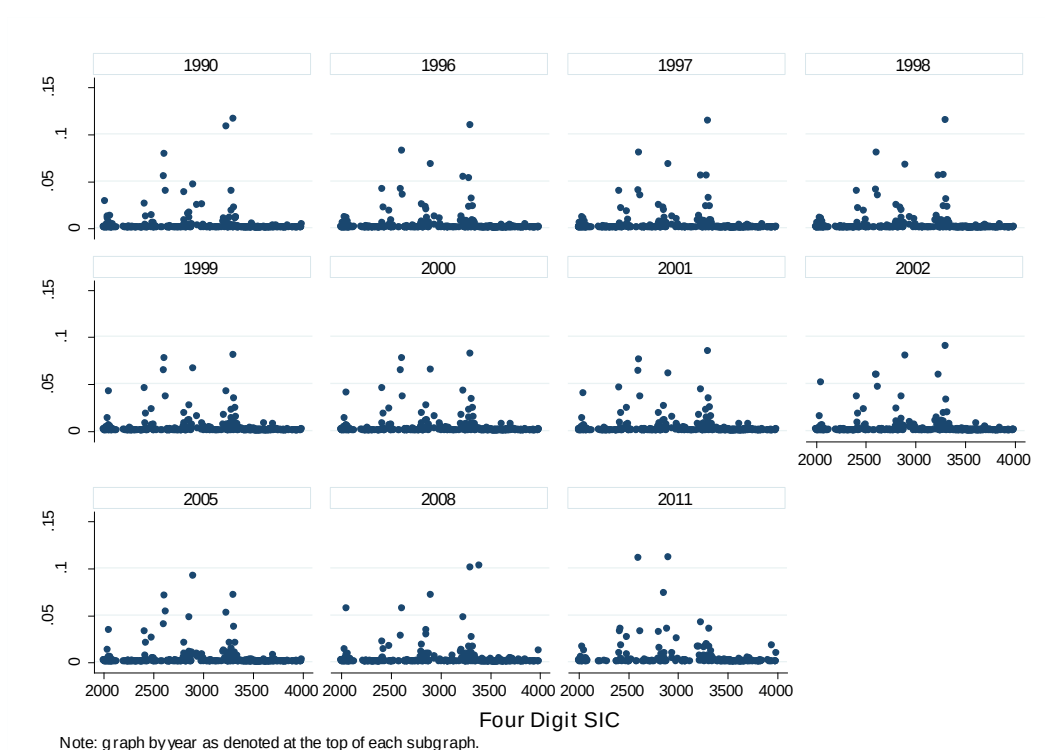
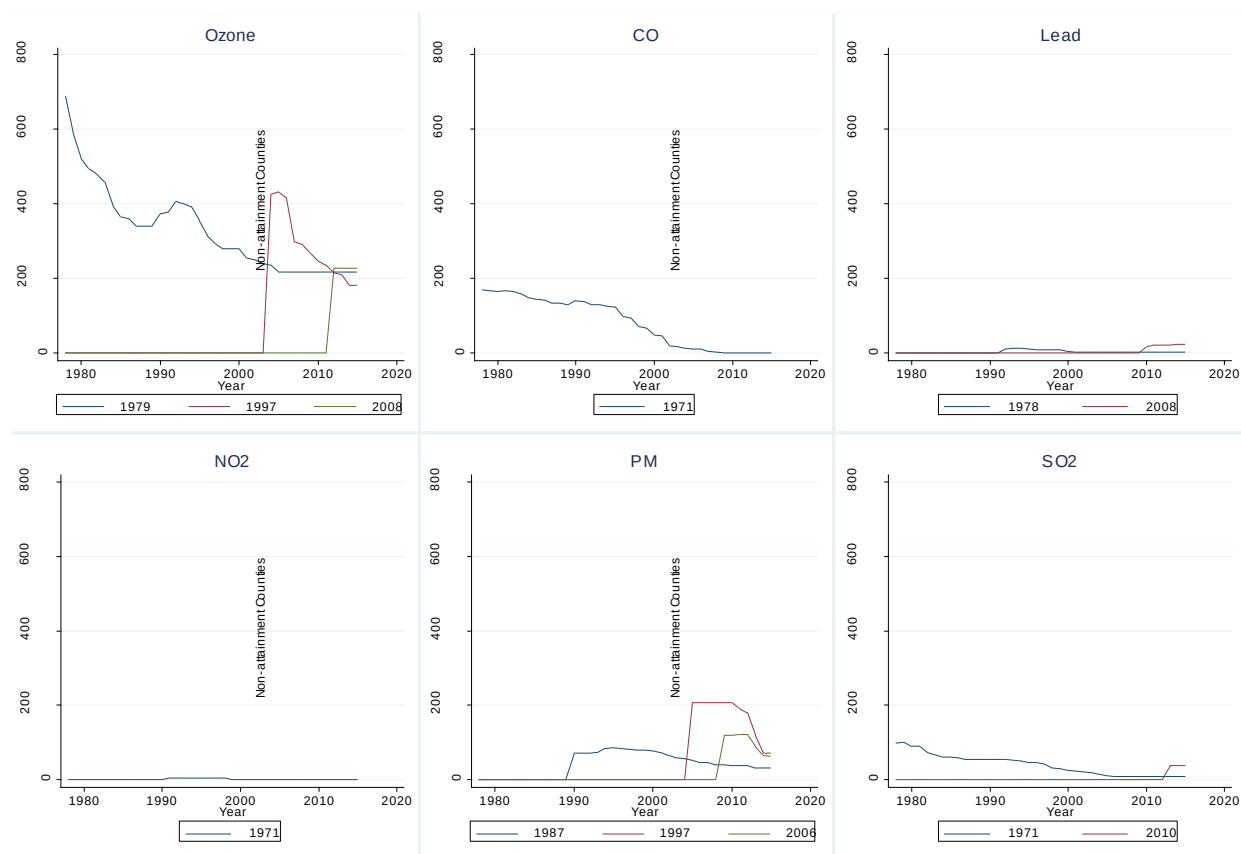


Figure 2-4: Scatter Plot of Percentage 4-digit SIC Polluters for PM-2.5



Source: EPA Green Book Database (http://www.epa.gov/airquality/greenbook/data_download.html).

Figure 2-5: Non-attainment Counties by Year and Regulation Vintage for Six Criteria Pollutants

Chapter 3: Power and Model Selection for Randomized Experiments: Monte Carlo Evidence

Abstract:

An increasingly common approach for identifying the effects of potential and existing policy interventions is to use randomized experiments. An often ignored yet important consideration when analyzing data from randomized experiments is statistical power: the ability to reject a false null hypothesis. This chapter uses Monte Carlo experiments to provide insight on how power is influenced by data analysis methods, such as the choice of econometric estimator or whether all or a subset of data is used. Surprisingly, except in very few extreme cases, we find that power does not vary much if one relies on simple difference-of-means tests or instead on robust or structured (i.e. assumption-laden) panel data estimators. However, the simulations provide strong evidence of power loss when one uses a subset of the collected data, and power gains through the inclusion of non-experimental control variables (e.g. demographic variables). Although the structured panel estimator we explore (random effects model), which adheres to the underlying data generating process, is in fact the most efficient according to the simulations, we find considerable evidence of bias in small samples. The extent of the bias, in combination with small efficiency gains, suggests that other approaches may be more appropriate in some settings. Although focused on data from randomized experiments, the results are also informative for the analysis of observational panel data using quasi-experimental identification approaches.

3.1. Introduction

Many randomized experiments in economics give rise to panel data sets. For instance, the effects of a policy intervention in the field may be examined over a number of years; or, even when testing a static theory, in the experimental laboratory it is common to repeat the game to achieve behavioral convergence. From examining the current state of the literature regarding the analysis

of experiment panel data, two observations emerge.³² First, it is common for experimentalists to treat the average outcome of the cross-sectional unit (e.g. an individual or a group) as the unit of analysis and then perform simple tests, such as t-tests or nonparametric Mann-Whitney tests. The potential merit of this approach is that one can avoid econometric models, thus avoiding possible specification errors. A second observation is that, when used, econometric models employed tend to be conventional ones, making heavy use of random effects and related mixed effects models. To an outside observer, the second observation appears at odds with the first. That is, why use such a *robust* approach when using a simple test yet such a *structured* (i.e., assumption heavy) approach when employing econometric modelling? Further, even when panel methods are employed, they are often framed as robustness checks and in this sense are relegated to a lower status, with simple tests being heavily relied upon to support conclusions drawn.

In this chapter we use Monte Carlo experiments to provide insight on how the choice of analytical methods used to draw inferences affects statistical power, that is, the ability to correctly reject a false null hypothesis. This investigation appears especially cogent given the variation in methods used to analyze data from randomized experiments. As an ancillary benefit, although establishing rules for optimal sample sizes for repeated experiments is beyond the scope of this study, the simulations provide insight on the gains in power one achieves through multi-period versus one-shot experimental design. This line of investigation can be seen as a complementary to recent research in economics and psychology that emphasizes the important role that statistical

³² For example, in the survey of laboratory public goods experiments by Chaudhuri (2011), roughly 80% of the studies adopt simple (i.e. non regression based) tests based on unit means of all periods or selected periods, or at least use such tests to draw their main conclusions; some of these studies employ some panel regression methods as robustness checks. Beginning around 2001, researchers have tended to use more panel regression methods but still rely on simple tests.

power has in advancing proper research conclusions (e.g., List, Sadoff and Wagner, 2011; Maniadis, Tufano and List, 2014; Simmons, Nelson and Simonsohn, 2011).³³

In our exploration, we focus on four representative testing approaches for analyzing panel data from randomized experiments. Two “simple” approaches included are the Mann-Whitney test and simple *t*-test. Given the panel data, these are implemented by using unit-specific averaged outcomes. The other two approaches utilize panel regression methods. The first is a simple OLS regression with cluster-robust standard errors. This modelling approach is increasingly used in the empirical economics literature in the analysis of quasi-experimental data and, as discussed by Wooldridge (2010, p. 915), regression adjustment estimators nonparametrically identify treatment effects. In the context of randomized experiments, given the traditional view in experimental economics is to favor robust testing procedures, this estimator appears especially relevant; specifically, the approach is robust to general forms of heteroskedasticity and within-unit serial correlation. The fourth approach is a random effects model. As our simulations consider within-unit serial correlation – inherent to most repeated experiments – we apply Prais-Winsten transformations on the data prior to random effects estimation. In fact, the underlying data generating process (DGP) we consider is that of a random effects model with within-unit serial correlation and so this approach is theoretically efficient. This thus provides a benchmark from which to compare the other, more robust, approaches.

Within these testing frameworks, we further investigate power differences based on whether all or a subset of the data are used. For instance, many studies use only the last period or last few periods when drawing inferences (e.g., Anderhub, Engelmann, and Güth, 2002;

³³ Studies as early as Cohen (1952) point out that the average statistical power in many research studies in psychology is too low to ensure the empirical findings are reliable.

Fischbacher and Gächter, 2010; Grosskopf and Sarin, 2010; Duffy and Muñoz-García, 2012). A casual observation one can make in this literature is that there are often discrepancies between inferences drawn when all versus a subset of data are used. While some of this may stem from outcome trends, another possible explanation is that using a subset of data leads to meaningful power losses, favoring non-rejection of true null hypotheses.

Finally, for the regression-based approaches, we investigate the merit of including non-experimental control variables; e.g., individual or group demographic variables. Such control variables are often available in the context of randomized experiments, e.g., through post-experiment questionnaires in lab experiments or census data in field experiments, and are increasingly included to account for possible sampling differences that naturally arise when randomizing over small samples. However, through randomization into treatment, control variables based on participant characteristics should be uncorrelated with treatment variables, and inclusion of controls that help explain outcomes can increase efficiency (Wooldridge, 2010, p. 927).

Much to our surprise, our primary result is, ironically, a null finding: except in extreme cases, and in the absence of non-experimental control variables, there are generally little differences in power between the four testing approaches we investigate. As our design, given the assumed DGP, includes the theoretically efficient estimator, one way to interpret this result is that there is little cost to using “robust” estimation approaches for experimental panel data. When significant control variables are included, however, meaningful power differences do emerge between simple and regression-based approaches, however, when significant control variables are included. There are further meaningful power losses when one uses a subset of the available data. As an ancillary result, we find considerable small-sample bias in the “size” of statistical tests for

the random effects estimator; in particular, there is a severe tendency to over-reject a true null hypothesis.³⁴ Such small samples arise commonly when group-level data are analyzed. Although related small-sample biases for the OLS estimator with cluster-robust standard errors are well-documented, we find very modest bias, even when the number of clusters is only 10, when one uses a limiting distribution based on the number of clusters rather than the sample size, as advocated by Cameron and Miller (2015) and others.³⁵

3.2. Monte Carlo Experiments

3.2.1. Basic setting and data-generating process

The basic setting we consider can be characterized as a between-subject, multi-period experiment with a single binary treatment and equal sample sizes in each treatment. We assume that there is within-unit serial correlation across periods. Units are assumed independent. Thus, in the event that outcomes are observed at the individual-level and there are no important interactions between individuals, the “unit” is best thought of as an individual. Alternatively, if outcomes are observed at the group-level and/or there are significant correlations in outcomes among group members, the unit is best thought of as the group. The testable hypothesis of interest is whether the treatment effect is zero.

Specifically, the outcome variable, y_{it} , is assumed to follow the DGP:

$$[3.1] \quad y_{it} = \beta_0 + \beta_1 x_{1it} + \beta_2 x_{2i} + u_i + \varepsilon_{it},$$

³⁴ To be clear here, the size of a test refers to the probability of rejecting a true null hypothesis. For test statistics with the correct “size”, this probability is equal to the significance level one specifies when undertaking the test.

³⁵ Simulations from some studies (e.g., Bertrand, Duflo and Mullainathan (2004) have given rise to notions that the number of clusters needs to be “at least 50” for tests based on this approach to be approximately correct. Such rules of thumb are generated based on the use of traditional limiting distributions, e.g., t -distribution with $NT - K$ degrees of freedom).

where $x_{1,it}$ is a binary treatment indicator; $x_{2,i}$ is a time-invariant explanatory variable that may be correlated with the outcome but is uncorrelated with treatment; u_i is a normally-distributed, unit-specific random effect; and ε_{it} is a normally distributed, mean-zero error term. Given the specification, β_1 is the average treatment effect. We employ the normalization $\beta_2 \equiv 1$. β_0 is assumed to be 10, although the choice of β_0 has no impact on the results as long as we include an intercept in the regressions. Instead, what matters for power is the size of the treatment effect as well as the variation in outcomes in the two treatments. As a simple model of within-unit error correlation, the error term is assumed to follow a first-order serial correlation (AR(1)) process:

$$[3.2] \quad \varepsilon_{it} = \rho \varepsilon_{it-1} + \eta_{it}$$

where ρ is the correlation coefficient.

In order to make *ceteris paribus* interpretations across a variety of parameter choices, we hold fixed across simulations the variation in outcomes left unexplained by treatment. Denote this variance by σ^2 . Given the model structure and the fact that the sources of variation ($x_{2,i}, u_i, \varepsilon_{it}$) are assumed to be additive and independent, we can define this unexplained variance as $\sigma^2 = \sigma_x^2 + \sigma_u^2 + \sigma_\varepsilon^2$. Let $r_1 = \frac{\sigma_x^2}{\sigma^2}$, which is the proportion of the variation that can be explained by $x_{2,i}$. Thus a value of r_1 close to 0 suggests that inclusion of non-experimental controls does little to capture unexplained outcome variation. Further, given the definition of r_1 , we can define the ratio of between-unit variation to overall unexplained variation as $r_2 = \sigma_u^2 / (1 - r_1) \sigma^2$. It follows that we can define the three error components in our DGP as:³⁶

$$[3.3a] \quad \sigma_u^2 = r_2 * (1 - r_1) \sigma^2$$

$$[3.3b] \quad \sigma_\varepsilon^2 = (1 - r_2) * (1 - r_1) \sigma^2$$

³⁶ σ_η^2 is derived from the AR(1) process using Yule-Walker methods.

and

$$[3.3c] \quad \sigma_{\eta}^2 = (1 - \rho^2)(1 - r_2) * (1 - r_1)\sigma^2.$$

Further, to complete the DGP we assume the following normal distributions:

$$[3.4a] \quad x_{2,i} \sim \text{Normal}(0, r_1 \sigma^2)$$

$$[3.4b] \quad u_i \sim \text{Normal}(0, r_2 * (1 - r_1)\sigma^2)$$

and

$$[3.4c] \quad \eta_{it} \sim \text{Normal}(0, (1 - \rho^2)(1 - r_2) * (1 - r_1)\sigma^2).$$

Note that with the assumed distribution for $x_{2,i}$ there is zero linear correlation between the control variable and the treatment indicator, which would be expected with random treatment assignment.

Also, importantly, σ^2 is held fixed across various values of r_1 , r_2 and ρ . We normalize $\sigma^2 \equiv 1$.

Using the DGP, we vary key design parameters as follows. First, the panel dimensions vary both in the terms of the total number of units, $N \in \{10, 20, 30, 60, 100\}$ and periods, $T \in \{5, 15, 20, 25\}$. Since N is the total number of independent units, there are $N/2$ units in each of the two treatment samples. Thus, $N = 10$ represents a rather small sample, more characteristic of experiments where outcomes are at the group-level (e.g., efficiency, units traded, etc.). We further vary the sources of variation unexplained by treatment: the relative between-unit variation, $r_2 \in \{0.0, 0.5, 0.9\}$; the within-unit serial correlation, $\rho = \{0.0, 0.5, 0.9\}$; and, the relative variation explained by the control variable, $r_1 \in \{0.0, 0.5, 0.9\}$.³⁷ Finally, we consider variation in the treatment effect size, $\beta_1 \in \{0, 0.3, 0.6, 0.9, 1, 1.2, 2\}$.³⁸ Given the normalized σ , what matters is the ratio of treatment effect size to σ (Treatment/ σ), and in our case such ratio is equal to β_1 . We

³⁷ When $r_1 = 0.0$, no control variable is included.

³⁸ The choice of β_1 is related to σ . In our case, β_0 is set to be 10, however, assuming normal distribution, the power of statistical tests is determined by relative distance of β_1 and β_0/σ , which is normalized to be 1, thus our treatment effect can be treated as one standard deviation, two standard deviations, etc.

selected 2 as the largest value of β_1 given that values greater than this largely yielded power near 100%. The number of simulations used for each design cell is 1000. Table 3-1 summarizes the design variables.

3.2.2. Statistical tests

Here we provide some additional details of the four testing procedures. First, as a representative non-parametric testing procedure, we conduct Mann-Whitney test (M-W, hereafter). Since the data are of a simulated panel, we apply the test to unit-specific average outcomes, which is common in the literature. We note that although this is a test of medians, given the DGP the outcome distributions are symmetric such that the median will equal the mean. Second, also based on unit-specific averaged data, we apply simple difference-of-mean t -tests for two independent samples (SIMPLE-T). The third approach employs a simple linear regression, in tandem with cluster-robust standard errors (OLS-CRSE) where errors are clustered at the unit level. A t -test is then applied to the null hypothesis $\beta_1 = 0$. As recommended by Bester, Conley, and Hansen (2011) and Cameron and Miller (2015), we assume the test statistic has a limiting t -distribution with $C - 1$ degrees of freedom, where C denotes the number of clusters. Fourth, we estimate a random effects model (RE) as a representative structured panel regression approach. In simulations involving serial correlation, we first apply a Prais-Winsten transformation on the data. A t -test is then used to test the null hypothesis $\beta_1 = 0$, using the standard limiting t -distribution with $NT - K$ degrees of freedom, where K denotes the number of estimated regression parameters. The simulations are coded in Stata v.13, and testing procedures make use of standard Stata commands.³⁹ We use 5% significance levels for all tests.

³⁹ The OLS regression is implemented using the “regress” command with the “cluster” option. The random effects estimator is implemented using “xtregar”.

All four testing procedures lead to theoretically consistent tests given the assumed DGP. By construction, the random effects estimator is efficient as it incorporates directly the true assumptions about the error structure. Tests based on OLS with cluster-robust standard errors are consistent in the presence of within-unit serial correlation (and heteroskedasticity). As the assumed unit (random) effects structure is a specific form of within-unit serial, this OLS approach is robust to the presence of the random effect. The Mann-Whitney and simple t-tests rely on the assumption that units are independent, which is consistent with the DGP. Further, as required by simple t-tests the normality assumption holds by construction.

3.2.3. *Calculation of size-adjusted power*

For a valid test, the size of a test, i.e. the probability of rejecting a true null hypothesis, should be (approximately) equal to the assumed significance level (here, 5%). Hence, with the treatment effect set to 0 we should reject the null hypothesis $\beta_1 = 0$ about 5% of the time. There are two reasons why the size of simulated tests might differ from the assumed true size in our application. One reason is that, with a finite number of simulated values, there will naturally be variation introduced by the set of random numbers used in a particular run. Second, both regression-based tests are biased, with the bias going to zero as $N \rightarrow \infty$. For instance, a test with incorrect size might reject 10% rather than the assumed 5% of the time. As a result, for any non-zero treatment effect one will likewise have a tendency to over-reject and this will masquerade as increased power.

We thus compute size-adjusted power when making comparisons to provide a clearer view of relative power. In particular, holding fixed the values of other design parameters, we set the treatment effect to equal 0 and then identify the critical value from the simulated distribution of test statistics; i.e., for a 5% test we rank-order the statistics and identify the 95th percentile of the

distribution. This size-adjusted critical value is then used to determine rejections for parallel tests based on non-zero treatment effects.

3.3.Results

In order to illustrate power differences among tests, we provide both power curves and pair-wise power differences for informative cases. Each power graph contains multiple sub-graphs representing the relationship of statistical power and treatment effect size for a specific case defined by (r_1, r_2) denoted at the top of each sub-graph, thus comparing sub-graphs provides information about how r_1 and r_2 affect power, *ceteris paribus*, e.g., Figure 3-1. Power difference graphs are similar to power graphs except that power differences graphs are providing information about magnitudes of power loss or gains between a pair of tests. To save space, we provide several representative cases instead all of them. We also provide, in Table 3-2, summary statistics of power differences based on data from the simulations described above. In Table 3-3, we further use simulated power data of all cases, and conduct OLS regressions of power and pairwise-power differences on the parameters of our experimental design (i.e., $N, T, r_2, \rho, r_1, \beta_1$). More specifically, we employ these graphs and tables to compare simple tests versus regression-based tests, tests based on structured versus robust regression models, inclusion of non-experimental control variables versus not, and using subsets of data versus using full data set.

3.3.1. Simple tests versus regression-based tests

In order to gain insight on the power losses associated with using simple difference-of-means tests rather than tests based on panel methods, we first compare simple tests (M-W and SIMPLE-T) with regression-based tests (OLS-CRSE and RE). As clearly the regression approaches have an advantage when meaningful control variables are available and used, we focus

here on comparisons without controls, specifically, the first four rows in Table 3-2. Across all cases defined by N, T , treatment effect sizes (greater than 0), r_1 , and r_2, ρ , rows 1-4 establish that, on average, there are no meaningful differences between the two simple tests, nor between the simple and regression-based tests. Looking at particular cases, given that averaging across all design cells can mask important information, we note that there are a few cases where the difference is on the order of 10%. Such power loss happens when sample size is small, and when the coefficient of AR(1) process ρ is greater than 0. Results in Table 3-3 suggest that when comparing non-parametric tests and tests based on panel methods, increasing time periods will increase the statistical power, but has little impacts on power differences, *ceteris paribus*. Though there is no meaningful power differences among tests, besides treatment effect size, r_1 and r_2 affect the power the most among all other parameters. And in general, power of all tests are decreasing as between-unit variation increases, *ceteris paribus*.

Figure 3-1 and 3-2 demonstrate the largest power differences (all in percentages) among M-W, SIMPLE-T, OLS-CRSE, and RE when $r_1 = 0$. More specifically, Figure 3-1 is the power graph of tests for $N = 10, T = 20, r_1 = 0$, and each sub-graph is for case (ρ, r_2) marked at the top. Holding ρ constant, and increasing r_2 , the between-unit variation ratio, will decrease statistical power for all four tests. Similarly, increasing ρ , the serial correlation coefficient, will decrease power, *ceteris paribus*. Though in Figure 3-2, the power loss (gains) of simple tests comparing to regression-based tests can be as large as about 10% (11%) when sample size is small, serial correlation exists, and most of the variations is from between-unit variations, in general, there are no meaningful power differences for most of the cases.

By convention, the acceptable level of power is 80% (Royall, 1997, p.109-110). Table 3-4 presents the initial combination of N and T , when β_1 is small, say 0.3, and when simulated power

is greater than 80% for each case defined by explanatory power r_1 , between-unit variation r_2 , and serial correlation ρ . For each case, any combination that has N greater than that of the initial combination, and/or T greater than the initial combination will return power higher than 80%. For example, when $r_1 = 0$, increasing either between-unit variation ratio r_2 or ρ , the degree of serial correlation, we need to increase the sample size by either increasing N or T to make sure the tests can have power of 80% or higher. Those without any combination means that even for $N = 100$, power is lower than 80%. From Table 3-4, for small β_1 , the treatment/ σ ratio, even in the absence of serial correlation, we need to have a sample with at least 60 units participating in a 15-period experiment to ensure our tests, either simple tests or regression-based tests, have power greater than 80%. However, when β_1 is greater than 0.9, even a very small sample size, e.g. 10 units participating repeated experiment 5 times, yields power greater than 80% for all tests, no matter how r_1, r_2 and ρ are defined.

3.3.2. Tests based on structured versus robust regression models

We then compare the power of tests using robust regression models (OLS-CRSE) and tests based on structured models, in our case, the RE. Theoretically, RE is the correct model that would be efficient, however, as illustrated by Vossler (2013), there may be severe size distortions in the direction of over-rejecting the null hypothesis.

3.3.2.1. Size distortions

Figure 3-3 presents the simulated rejection probabilities (i.e., sizes) of different tests for $r_1 = 0$, and $T = 15$ for varying N and combinations of (ρ, r_2) , as indicated at the top of each sub-graph. The patterns are similar for other values of r_1 and T . When $\rho = 0, r_2 = 0$, which means there is no serial correlation and no between-unit variation, all the tests studied in this chapter have approximately correct size, lying between 0.04 and 0.06. However, from Figure 3-3, especially

when there is between-unit variation, the RE-based approach tends to over-reject the null hypothesis when N is small. Across all cases, the average size of RE-based tests is about 0.098, approximately twice the significance level when $N = 10$. The other three tests return similar and approximately correct sizes. When N increases, all tests tend to have similar (correct) size, especially when N is greater than 60.

3.3.2.2. Power similarity

The size of tests based on RE have severe small-sample bias, implying that one reason a RE might lead to rejection whereas a simple t-test based on CRSE does not is not likely because of efficiency but because of bias. However, after adjusting power by size, in general, OLS-CRSE and RE have little difference in powers. Row four in Table 3-2 shows that across all cases, the average power differences between OLS-CRSE and RE is about -0.22%, which is negligible given the variation induced by simulations. The largest power loss from using OLS-CRSE, -10.1%, occurs when the sample size is very small, and either there is severe serial correlation, or β_1 is small. In our simulation, this is when $N = 10, T = 25, r_1 = 0, r_2 = 0$, and $\rho = 0.9, \beta_1 > 0.9$, or when $N = 10, T = 25, r_1 = 0, r_2 = 0$, and $\rho = 0, \beta_1 = 0.3$. This suggests that except in extreme cases when the sample size is extremely small, where RE could be slightly more powerful than OLS-CRSE, the unstructured panel methods (OLS-CRSE) and structured panel methods (RE) have approximately the same power. This can also be found in Figure 3-1 and 3-2, which provides as an example of power similarity between OLS-CRSE and RE.

3.3.3. Inclusion of non-experimental control variables

We then compare statistical power of tests in tandem with panel models with and without control variable to further explore potential advantages of panel methods. When x_{2i} has explanatory power, which means $r_1 > 0$, inclusion of a non-experimental control variable will

increase the goodness-of-fit of regression models. Figure 3-4 provides an example of the R^2 of panel models with and without x_{2i} when $N = 10$ and $T = 15$; the other cases present a similar pattern. Moreover, in terms of power, inclusion of non-experimental control variables can increase statistical power. From rows 5 and 6 of Table 3-2, across all cases, average power gains when including non-experimental control variables is about 3%, with largest power gains more than 60%, which is when the sample size is small, and the control variable x_{2i} explains 90% of the variation in outcomes unexplained by treatment. Figure 3-5 and 3-6 provide power and power differences graphs when $N = 10, T = 5, r_1 = 0.9$. When treatment effect size is small, such power gains, on average, will be larger, e.g., when $\beta_1 = 0.3$, the average power gains is about 5%. Increasing sample size will decrease such power gains, *ceteris paribus*. Moreover, Figure 3-5 contains M-W and SIMPLE-T demonstrating that simple tests have approximately the same power as OLS-CRSE and RE, and tests in tandem with regressions with control variables are superior to simple tests. Thus we can conclude that, in general, when one ignores controls that are important in explaining outcome variation, there are meaningful power losses.

3.3.4. Using subsets of data

Finally, we explore whether using subsets of data leads to meaningful power differences. We consider two cases – tests based on using observations from the last period, and tests based on the last five periods – and denote the corresponding tests as M-W-Last, M-W-Sub, SIMPLE-T-Last, SIMPLE-T-Sub, OLS-CRSE-Last, OLS-CRSE-Sub, RE-Last, RE-Sub, and further OLS-CRSE-Last-Control, OLS-CRSE-Sub-Control, RE-Last-Control, and RE-Sub-Control. M-W-Last and SIMPLE-T-Last use last period only, whereas, M-W-Sub and SIMPLE-T-Sub are based on unit-specific averages constructed from the last five periods. In order to have comparable regression-based tests to simple tests using subsets of data, we bring indicators of whether the

period is the last period (d_l) or not, and whether the periods are the last five periods (d_5) or not separately to regressions based on the full data set. More specifically, when excluding controls, the regression model is $y_{it} = \beta_0 + \beta_1 x_{1,it} + \beta_2 d_T + \beta_3 d_T x_{1,it} + u_i + \varepsilon_{it}$, where d_T is d_l for testing the last period, and d_5 for testing the last five periods. And then the relevant test is of the null hypothesis: $\beta_1 + \beta_3 = 0$. Similarly, the regression is $y_{it} = \beta_0 + \beta_1 x_{1,it} + \beta_2 x_{2i} + \beta_3 d_T + \beta_4 d_T x_{1,it} + u_i + \varepsilon_{it}$ when including the control variable, and the relevant test is whether: $\beta_1 + \beta_4 = 0$.

Rows 7 to 18 in Table 3-2 summarize the power differences between tests using the full data set and subsets of data. Using subsets of data, especially using only one period of observation will generate meaningful power loss, in some cases such power loss could be as large as 81%, e.g. power of SIMPLE-T-Last when compared to SIMPLE-T. Using five periods of data leads to higher power than using one period, but still worse than using full data set. See Figure 3-7 – 3-10 for relevant examples. When $\beta_1 = 1$, across all cases simulated, average power loss of tests based on the last period when compared to tests based on full data set is 14.6%, 14.7%, 15.1%, and 12.8% for M-W, SIMPLE-T, OLS-CRSE, and RE, respectively. Average power losses of basing tests on the last five periods is about 5% for all four testing approaches. Such power losses are even larger when $\beta_1 < 1$. Increasing β_1 of course increases power of all tests, which leads to differences that approach zero for large values.

3.4 Discussion

The results from the Monte Carlo experiments suggest that in many cases there are rather negligible differences in power across the different testing procedures we explored. This thus provides some suggestive evidence that, even when more sophisticated regression models are

desired, simple tests can be informative without a meaningful loss in power. One exception is when control variables are available, and when such controls explain a significant fraction of the outcome variation. Here, regression methods have the advantage of increased power. Thus, in applications where simple tests support the null hypothesis and regression-based tests (with controls) suggest rejection, the latter tests – assuming underlying assumptions are met – are more likely to support the correct conclusion. A second exemption is when only a block of the data is analyzed, which also can lead to meaningful power losses. Certainly such a practice can be justified on the basis of a DGP that varies over time. However, even then, there is potential advantage to modelling this process.

The underlying DGP process explored here of course abstracts from several data issues that may be present.⁴⁰ For example, the results presented assume homoskedasticity. Heteroskedasticity should theoretically decrease the power of all tests considered, although this could lead to meaningful differences across tests. In this setting, of course, a *t*-test based on the RE estimator is no longer consistent and the efficient estimator would incorporate both the underlying heteroskedasticity and serial correlation processes. One trending estimator in the experimental literature is a RE estimator with cluster-robust standard errors, which in our setting would lead to a consistent test. However, by definition, this approach is no longer efficient. Further, given negligible differences between the RE estimator and OLS with cluster-robust standard errors in the homoskedastic case suggests there are unlikely to be efficiency gains with such an approach. As suggested by our simulations involving small numbers of cross-section units, the size distortion of RE-based tests suggests that OLS with cluster-robust standard errors is likely to be preferred.

⁴⁰ We explored a simple extension to our DGP where the error variance decreased over time. This did not lead to any additional variation across testing approaches considered.

Other data issues include trending outcomes due to learning or other sources of behavioral dynamics. Moreover, the serial correlation process considered in this chapter is AR(1), and we see significant impacts of serial correlation on the statistical power of tests, and it even affects the power difference between OLS-CRSE and RE negatively. It is not clear whether this is true when there are higher-order serial correlation processes. These and other extensions that reflect the realities of data from randomized experiments are left as fodder for future explorations.

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Appendix: Tables and Figures

Table 3-1: Monte Carlo experiment design variables

Variable	Description
β_1	Treatment effect
σ^2	Outcome variance unexplained by treatment
r_1	Proportion of the unexplained variance captured by non-experimental control variable (if present)
r_2	Proportion of the unexplained variance due to between-unit variation.
ρ	Coefficient of within-unit serial correlation
N	Total number of cross-section units (across both treatments)
T	Total number of time periods

Table 3-2: Average power differences among tests

	Mean	Std. Dev.	Min.	Max.
(M-W)–(SIMPLE-T)	0.22	2.27	-6.10	12.2
(M-W) – (OLS-CRSE)	0.11	2.87	-9.40	11.7
(M-W) – (RE)	-0.11	2.84	-10.3	11
(OLS-CRSE) – (RE)	-0.22	1.06	-10.1	3.70
(OLS-CRSE) – (OLS-CRSE-Control)	-2.56	6.64	-61.8	2.60
(RE) – (RE-Control)	-2.54	6.56	-64.3	3.60
(OLS-CRSE-Last) – (OLS-CRSE-Last-Control)	-18.86	20.28	-70	0
(RE-Last) – (RE-Last-Control)	-18.59	20.20	-75.40	0
(OLS-CRSE-Sub) – (OLS-CRSE-Sub-Control)	-5.65	10.58	-69	2.9
(RE-Sub) – (RE-Sub-Control)	-5.74	10.76	-72.2	2.1
(M-W) – (M-W-Last)	16.43	19.32	-3.40	80.9
(SIMPLE-T) – (SIMPLE-T-Last)	16.40	19.67	-2.90	81
(OLS-CRSE) – (OLS-CRSE-Last)	16.72	19.83	-2.20	80.1
(RE) – (RE-Last)	14.98	19.14	-3.20	79.10
(M-W) – (M-W-Sub)	4.93	9.05	-10.2	56.1
(SIMPLE-T) – (SIMPLE-T-Sub)	5.04	9.14	-5	55.3
(OLS-CRSE) – (OLS-CRSE-Sub)	5.14	9.22	-2.20	56.6
(RE) – (RE-Sub)	4.24	8.54	-2.9	52.4

Note: The number of cases defined by N , T , treatment effect sizes (greater than 0), r_1 , and r_2, ρ is 3,240. First four rows are across cases when $r_1 = 0$, and there are 1,080 cases. Rows 5 – 8 are across all cases when $r_1 > 0$, and there are 2,160 cases. Rows 9 and 10 are across all cases when $r_1 > 0$ and $T \neq 5$, and there are 1,620 cases. Rows 11 – 14 are based on all 3,240 cases. And the last four rows are based on cases when $T \neq 5$, and there are 2,430 cases. All power data is adjusted by size, and are in percentages.

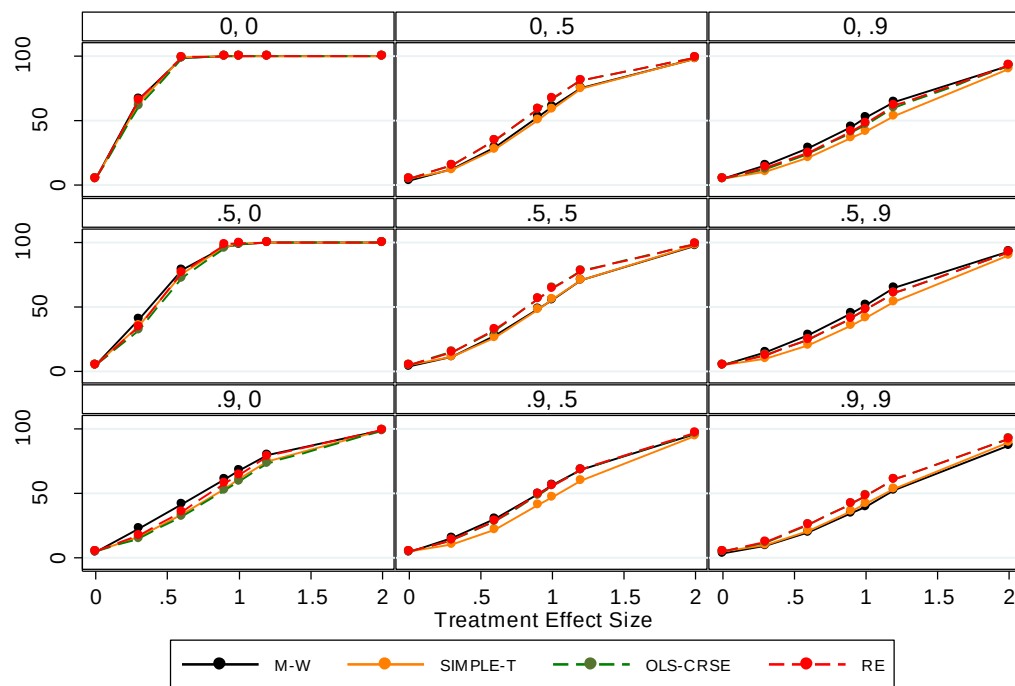


Figure 3-1: Statistical power of tests for $N = 10, T = 20, r_1 = 0$

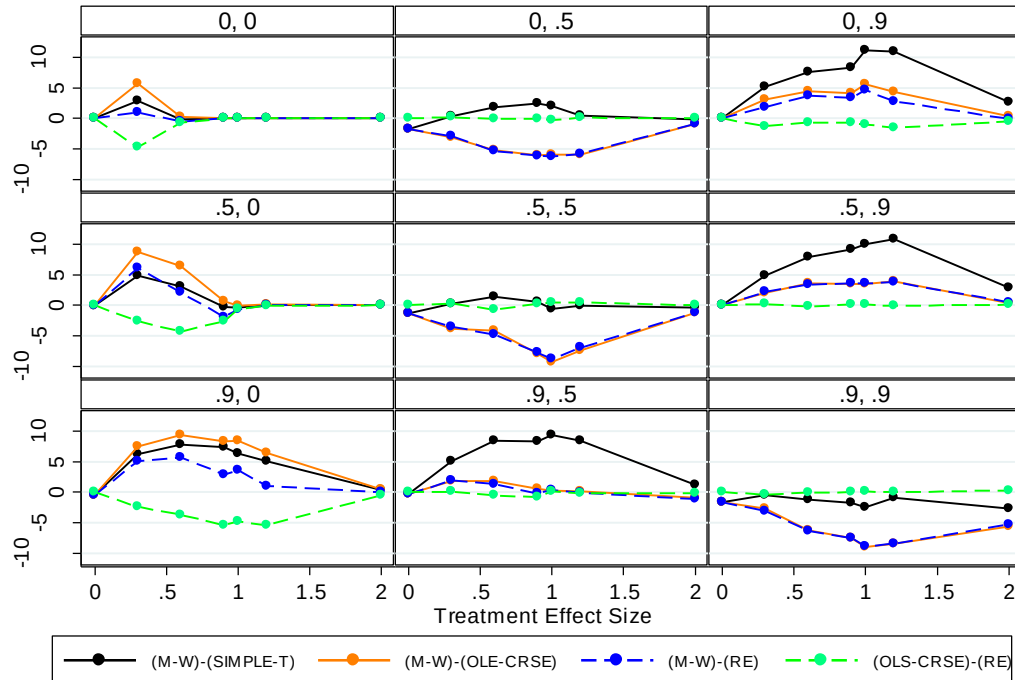


Figure 3-2: Statistical power differences of tests for $N = 10, T = 20, r_1 = 0$

Table 3-3: Regressions of power and power differences on N , T , β_1 , r_1 , r_2 , and ρ

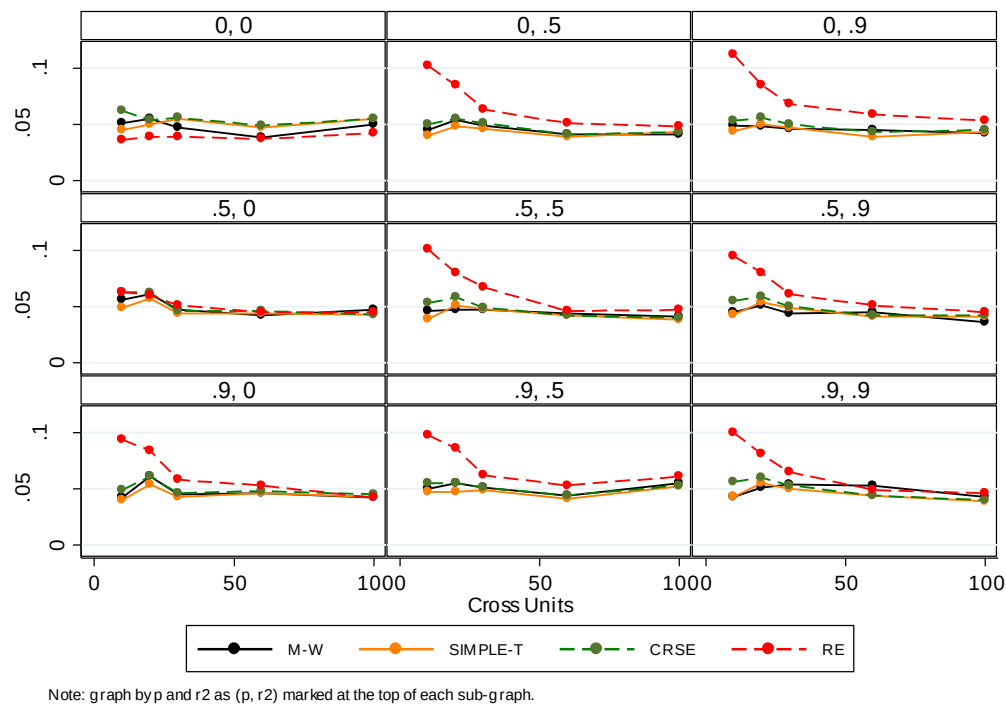
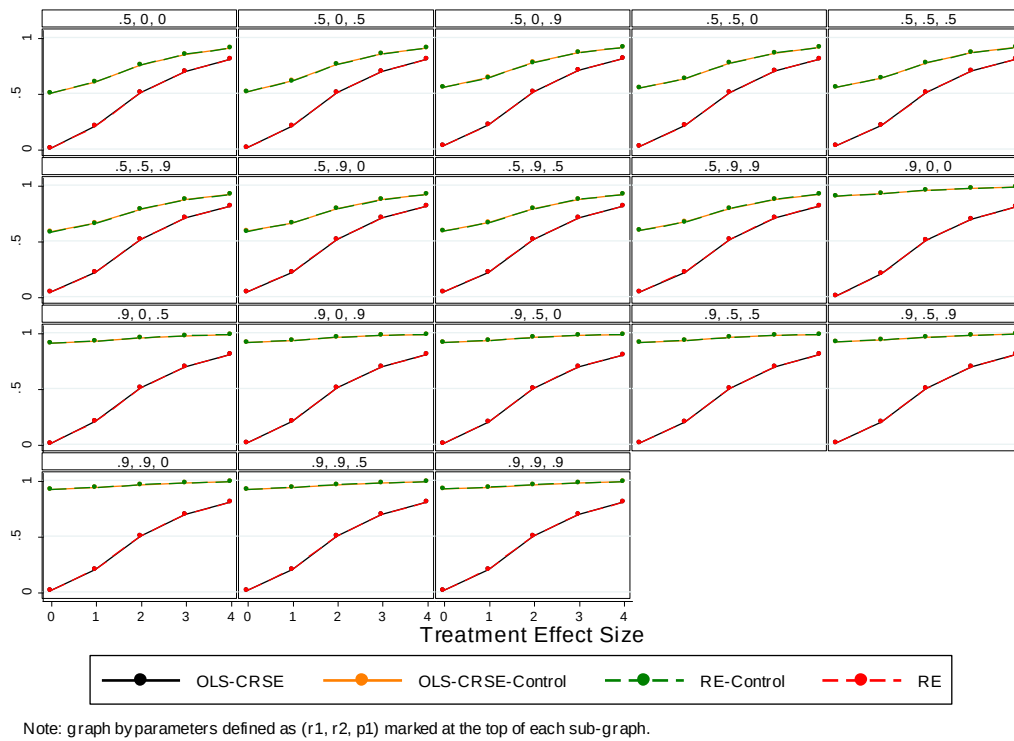
Panel a: statistical power of tests				
	M-W	SIMPLE-T	OLS-CRSE	RE
N	0.242*** (0.009)	0.254*** (0.009)	0.248*** (0.009)	0.243*** (0.09)
T	0.290*** (0.040)	0.278*** (0.041)	0.279*** (0.040)	0.277*** (0.040)
r_1	16.682*** (0.790)	16.876*** (0.811)	17.125*** (0.799)	17.083*** (0.800)
β_1	23.822*** (0.611)	23.903*** (0.621)	23.627*** (0.616)	23.416*** (0.616)
r_2	-11.603*** (0.783)	-11.857*** (0.802)	-11.624*** (0.792)	-12.076*** (0.790)
ρ	-5.720*** (0.788)	-5.754*** (0.809)	-5.705*** (0.796)	-5.618*** (0.795)
constant	47.009*** (1.386)	46.513*** (1.42)	47.070*** (1.412)	47.846*** (1.409)
Panel b: statistical power differences of tests				
	(M-W)-(SIMPLE-T)	(M-W)-(OLS-CRSE)	(M-W)-(RE)	(OLS-CRSE)-(RE)
N	-0.012*** (0.001)	-0.006*** (0.001)	-0.002 (0.001)	0.004*** (0.000)
T	0.012*** (0.004)	0.012* (0.006)	0.013** (0.006)	0.001 (0.002)
r_1	- (0.086)	- (0.109)	- (0.111)	0.042 (0.041)
β_1	-0.081* (0.049)	0.195*** (0.066)	0.406*** (0.067)	0.211*** (0.023)
r_2	0.254** (0.082)	0.021 (0.106)	0.473*** (0.108)	0.452*** (0.043)
ρ	0.035 (0.085)	-0.015 (0.107)	-0.101 (0.110)	-0.087** (0.036)
constant	0.496*** (0.132)	-0.061 (0.194)	-0.837*** (0.190)	-0.777*** (0.078)

Note: *, **, *** represent 10%, 5%, and 1% significance level separately. All power data is adjusted by size, and in percentages. Data when treatment effect size is zero are excluded, since they are actually the statistical size of tests, thus number of observations used in regressions is 3,240. Standard errors are reported in parentheses. $(M-W) - (SIMPLE-T)$ is the power difference between Mann-Whitney test and t-test based on subject means; $(M-W) - (OLS-CRSE)$ is the power difference between Mann-Whitney test and t-test in tandem with CRSE; $(M-W) - (RE)$ is the power difference between Mann-Whitney test and t-test in tandem with RE; $(OLS-CRSE) - (RE)$ is the power difference between t-test in tandem with CRSE and t-test in tandem with RE.

Table 3-4: Initial Combination of N, T when Power $> 80\%$, and $\beta_1=0.3$

(r_1, r_2, ρ)	M-W	SIMPLE-T	OLS-CRSE	RE
(0, 0, 0)	(20,15)	(20,15)	(20,20)	(20,20)
(0, 0, 0.5)	(60,15)	(60,15)	(60,15)	(60,15)
(0, 0, 0.9)	-	-	-	-
(0, 0.5, 0)	-	-	-	-
(0, 0.5, 0.5)	-	-	-	-
(0, 0.5, 0.9)	-	-	-	-
(0, 0.9, 0)	-	-	-	-
(0, 0.9, 0.5)	-	-	-	-
(0, 0.9, 0.9)	-	-	-	-
(0.5, 0, 0)	(20,20)	(20,20)	(20,15)	(20,15)
(0.5, 0, 0.5)	(30,20)	(30,20)	(30,20)	(30,20)
(0.5, 0, 0.9)	(100,20)	(100,20)	(100,15)	(100,15)
(0.5, 0.5, 0)	(100,15)	(100,15)	(100,5)	(100,15)
(0.5, 0.5, 0.5)	(100,15)	(100,15)	(100,15)	(100,15)
(0.5, 0.5, 0.9)	-	-	-	-
(0.5, 0.9, 0)	-	-	-	-
(0.5, 0.9, 0.5)	-	-	-	-
(0.5, 0.9, 0.9)	-	-	-	-
(0.9, 0, 0)	(20,20)	(20,20)	(20,15)	(20,15)
(0.9, 0, 0.5)	(20,25)	(20,20)	(20,20)	(20,20)
(0.9, 0, 0.9)	(30,25)	(30,20)	(30,20)	(30,20)
(0.9, 0.5, 0)	(30,20)	(30,20)	(30,20)	(30,20)
(0.9, 0.5, 0.5)	(30,20)	(30,20)	(30,20)	(30,20)
(0.9, 0.5, 0.9)	(60,15)	(60,15)	(60,15)	(60,15)
(0.9, 0.9, 0)	(60,15)	(60,15)	(60,15)	(60,15)
(0.9, 0.9, 0.5)	(60,15)	(60,15)	(60,15)	(60,15)
(0.9, 0.9, 0.9)	(60,15)	(60,15)	(60,15)	(60,15)

Note: Each cell from column 2 to 5 represents a combination of (N, T) when under situation (r_1, r_2, ρ) the power of tests studied is greater than 80%.

Figure 3-3: Size of tests when $T = 15, r_1 = 0$ Figure 3-4: R-squares of panel models when $N = 10, T = 15$

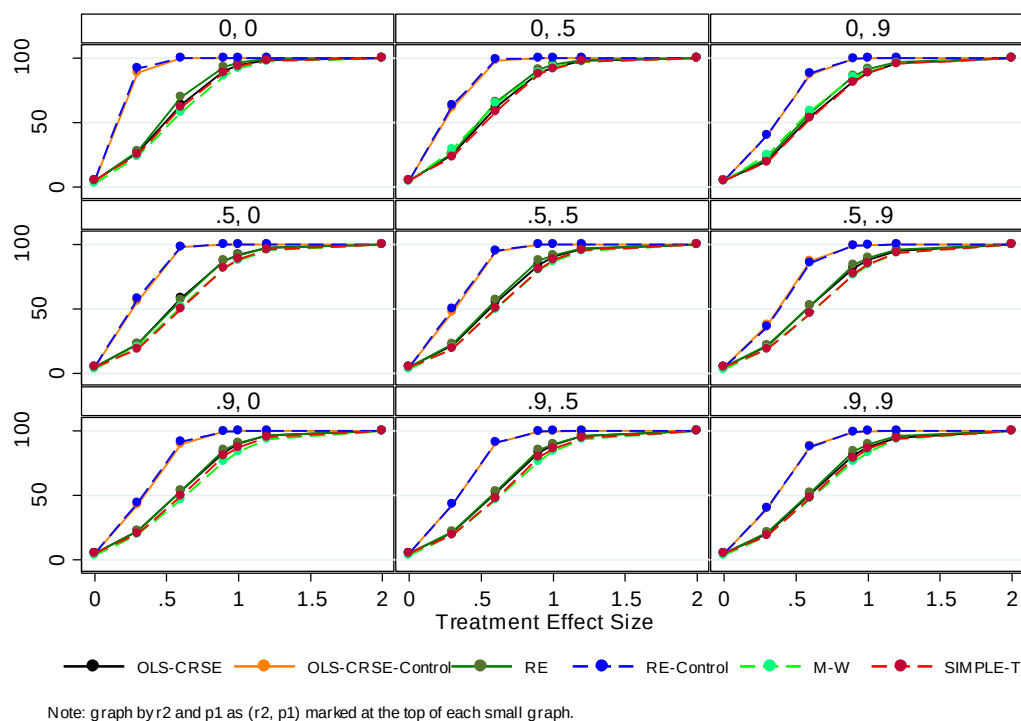


Figure 3-5: Statistical power of tests for $N = 10, T = 5, r_1 = 0.9$

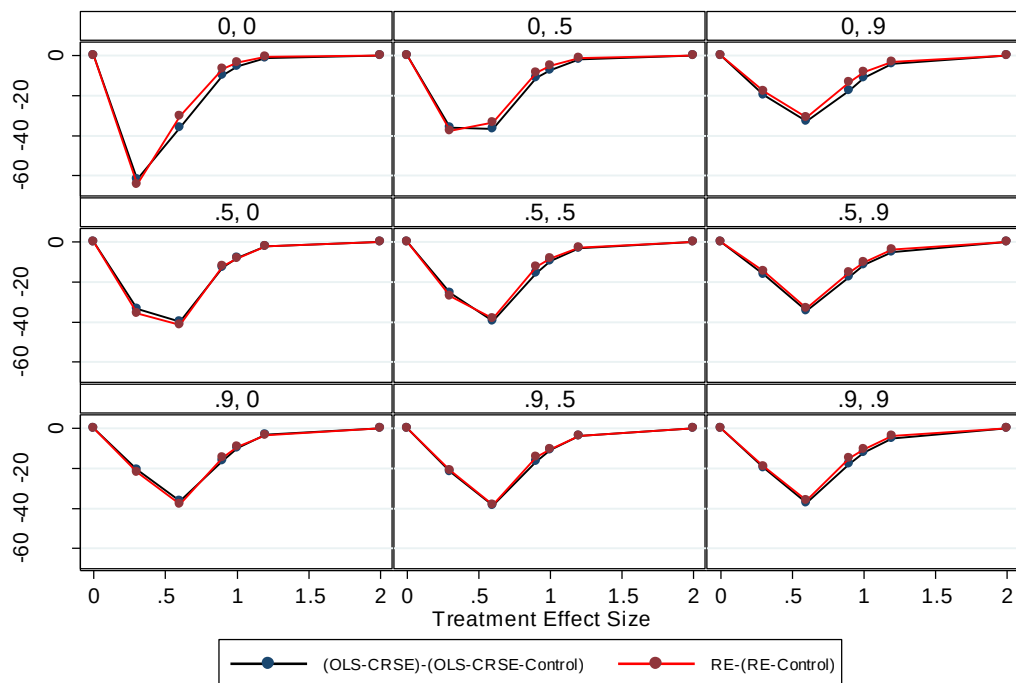


Figure 3-6: Statistical power differences of tests for $N = 10, T = 5, r_1 = 0.9$

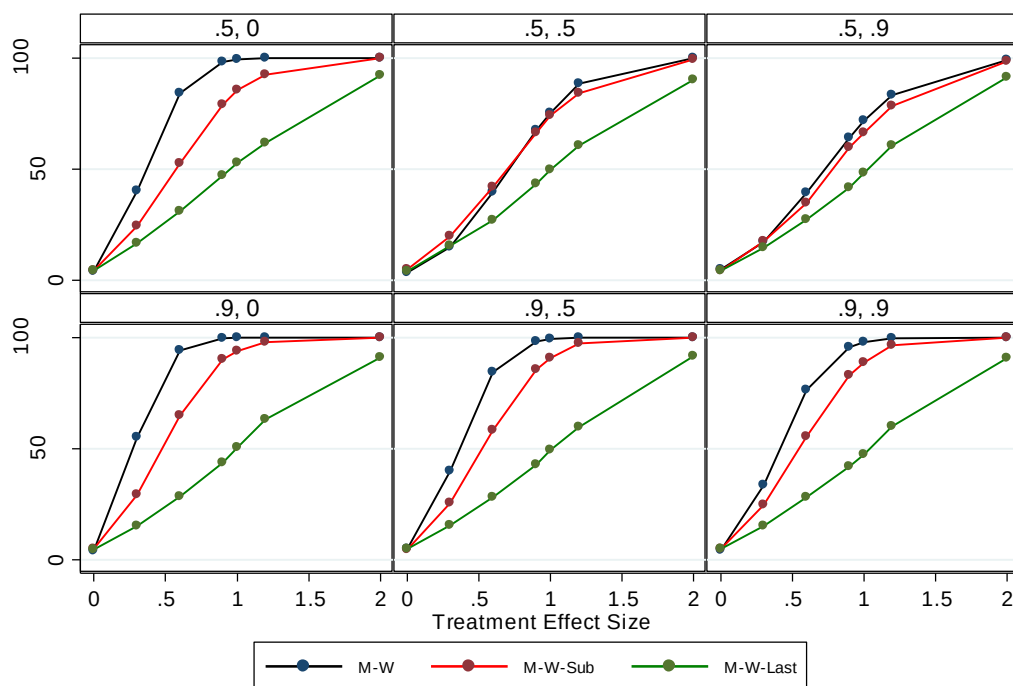


Figure 3-7: Statistical power of M-W when $N=10, T=15, \rho=0.5$

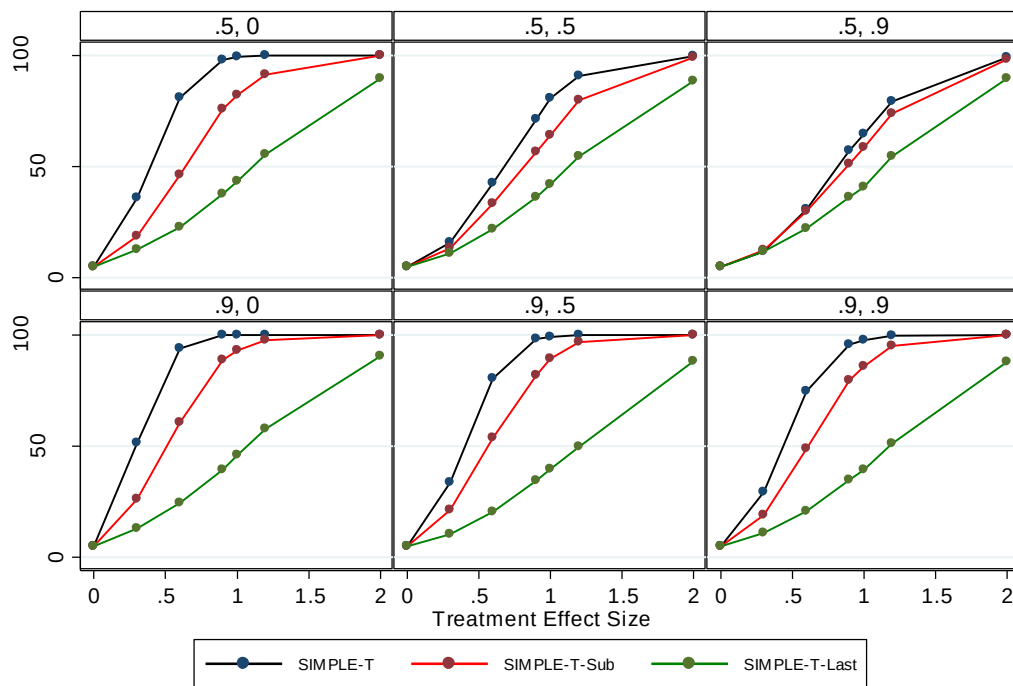
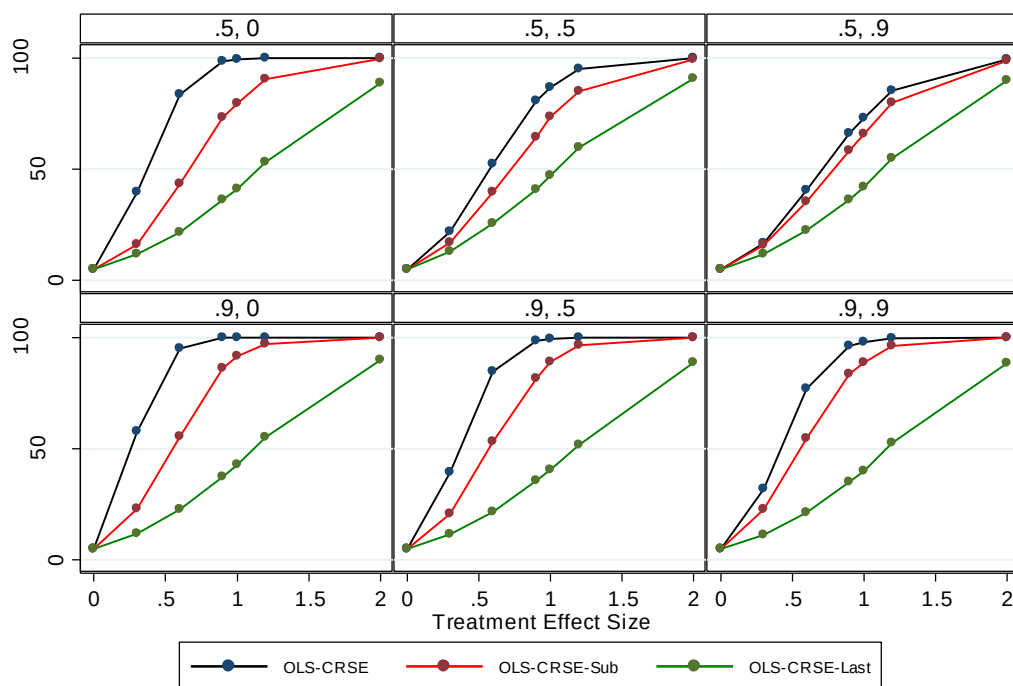
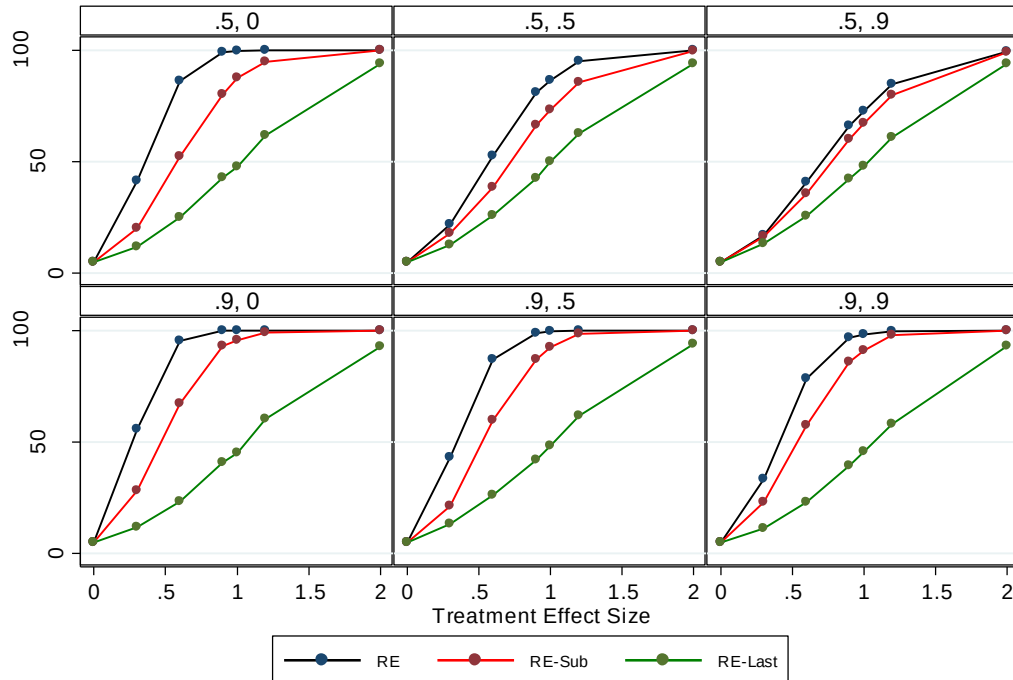


Figure 3-8: Statistical power of SIMPLE-T when $N=10, T=15, \rho=0.5$



Note: graph by r_1 and r_2 as (r_1, r_2) marked at the top of each small graph.

Figure 3-9: Statistical power of OLS-CRSE when $N=10, T=15, \rho=0.5$



Note: graph by r_1 and r_2 as (r_1, r_2) marked at the top of each small graph.

Figure 3-10: Statistical power of RE when $N=10, T=15, \rho=0.5$

Conclusion

Panel data methods are widely used in empirical analysis, this project is aiming at analyzing regulatory impacts using panel data. More specifically, in order to gain new insight on this question, and in contrast to related work, Chapter 1 employs dynamic panel models that allow for potential feedback effects (i.e. reverse causality) between measures of performance and regulation. This is an important empirical question for both policy makers and firms is whether more stringent environmental regulations have positive or negative impacts on firm performance. Specifically, Chapter 1 uses data from the U.S. manufacturing industry to explore the effects of the Clean Air Act (CAA) on firm performance outcomes related to profitability, employment, and capital expenditures. The evidence suggests that feedback effects are present in the data and, more critically, appropriately modeling these effects can alter conclusions regarding the effects of environmental regulation. Empirical results support the notion that more stringent environmental policies can provide dividends to regulated firms.

Chapter 2 uses insights from both the environmental economics literature and the broader finance literature to investigate whether existing findings are sensitive to the omission of these key financial variables. Using comprehensive firm-level panel data and a variety of estimation frameworks, this chapter provides new evidence suggesting that stringency of environmental regulation motivates mergers among regulated firms. Using two measures of environmental regulations, pollution abatement costs and expenditures (PACE) and combination of emissions and Clean Air Act (CAA) pollution redefinitions, we find that every 10 million 1995 dollars increase in PACE will have positive marginal impact, about 0.7%, on the probability of being acquirer, and such marginal impacts are 0.2% and 0.3% on being target and being involved in

M&A activities separately. Moreover, firms under more stringent environmental regulations tend to behave as target firms more frequently.

An increasingly common approach for identifying the effects of potential and existing policy interventions is to use randomized experiments. An often ignored yet important consideration when analyzing data from randomized experiments is statistical power: the ability to reject a false null hypothesis. Chapter 3 uses Monte Carlo experiments to provide insight on how power is influenced by data analysis methods, such as the choice of econometric estimator or whether all or a subset of data is used. Surprisingly, except in some extreme cases, we find that power does not vary much if one relies on simple difference-of-means tests or instead on robust or structured (i.e. assumption-laden) panel data estimation. However, the simulations provide strong evidence of power loss when uses a subset of the collected data, and power gains through the inclusion of non-experimental control variables (e.g. demographic variables). Although the structured panel estimator we explore (random effects model), which adheres to the underlying data generating process, is in fact the most efficient according to the simulations, we find considerable evidence of bias in small samples. The extent of the bias, in combination with small efficiency gains, suggests that other approaches may be more appropriate in some settings. Although focused on data from randomized experiments, the results are also informative for the analysis of observational panel data using quasi-experimental identification approaches.

Vita

Jie Deng is from Sichuan, China. She graduated from Southwestern University of Finance and Economics in 2010 with a Bachelor's degree in international trade and economics. Jie then studied Economics at the University of Tennessee, and earned her Master of Arts degree in economics in summer 2013. She completes her Ph.D. in economics in August 2015 and will begin working at Southwestern University of Finance and Economics in September 2015. Although her major fields are environmental economics, industrial organization and applied econometrics, she is also interested in various topics related to experimental economics and public economics.